An Experimental Study of Settlement Delay in Pretrial Bargaining with Asymmetric Information

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Abstract

In the United States legal system, tort disputes often exhibit protracted delay between injury and settlement. That is, parties to a dispute tend to agree on settlement conditions only after engaging in lengthy legal sparring and negotiation. Resources committed to settlement negotiation are large and economically inefficient. Even small reductions in average settlement delay stand to affect large reductions in socially inefficient spending.

This research contributes to the understanding of settlement delay by carefully exploring one popularly advanced hypothesis for the phenomenon: the idea that asymmetric information over the value of a potential trial verdict might help to drive persistent settlement delay. A large-scale laboratory experiment is conducted with payment-incentivized undergraduate and law school subjects. The experiment closely implements a popular model of settlement delay in which litigants attempt to negotiate settlement under asymmetric information about the value of a potential trial verdict. The experiment is designed to address two broad research questions: (i) can asymmetric information over a potential trial verdict plausibly contribute to the protracted settlement delay observed in the field, and (ii) can specific policies be identified which might mitigate the settlement delay associated with asymmetric information?

In response to the first broad research question, experimental results strongly confirm the plausibility of asymmetric information contributing to settlement delay. Starting from a baseline of symmetric information, settlement delay in the laboratory is increased by as much as 95% when subjects are exposed to a controlled information asymmetry over the value of the potential trial verdict. This observation is found strongly robust to perturbations in the underlying bargaining environment.

In response to the second broad research question, experimental results do not strongly confirm the capacity of reasonable policy changes to affect large reductions in settlement delay. Collected data fail to indicate that any explored reform policy obviously reduces average settlement delay, though estimators are sufficiently imprecise that substantial effects on average delay cannot be ruled out. Settlement delay in the laboratory is responsive to changes in bargaining costs, but does not obviously respond to changes in the distribution of damages available at trial.

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Chapter I

Introduction

Contrary to popular belief and media depiction, the vast majority of claims arising in the United States tort system actually resolve in private settlements—not trial verdicts. But although most litigants ultimately agree to settle their disputes, few do so without first engaging in protracted legal posturing and negotiation. The accepted norm of long and costly settlement delay is puzzling, as the time and resources litigants invest in settlement negotiations are *ex post* inefficient expenditures.

The underlying causes of systematically delayed settlement are poorly understood. This is largely attributable to the nature of the subject: tort disputes are private, decentralized, and complicated events for which accurate field data are difficult to find and expensive to collect. Large social stakes, elusive data, and numerous unanswered questions motivate an experimental approach to the topic. The present study uses controlled laboratory experiments to investigate the popular hypothesis that asymmetric information between litigants may be one potential factor contributing to systematically delayed settlements.

This first chapter provides background and motivation for the remaining chapters of the study. Section 1 discusses legal and economic background concepts relevant to an investigation of settlement delay in tort disputes. Mechanics of the United States tort system are summarized along with extant research on tort disputes and settlement delay. Section 2 provides a broad overview of the present study, motivating the chosen research design and road-mapping progression through the remaining chapters.

1 Background

To properly frame analysis, this section provides a detailed background on settlement bargaining in tort disputes. Section 1.1 describes the legal context, explaining the mechanics of a tort claim and summarizing noteworthy characteristics of the current tort system. Section 1.2 introduces important economic puzzles concerning tort disputes and summarizes previous research on these puzzles.

1.1 Legal Background

In the United States legal system, tort law governs the resolution of disputes over most civil harms not arising from contract. Examples of such harms include traffic collisions, injuries from product defects, adverse outcomes from medical treatment, premise-related injuries, slander, assault and battery. Every tort dispute involves an injured party (the *plaintiff*), and an alleged injurer (the *defendant*). For any plaintiff with a valid cause of action (i.e. a claim for which legal relief may be sought), tort law provides a means of seeking redress for the harm sustained.

To seek legal redress, the plaintiff files a formal complaint against the defendant. The complaint alleges facts relevant to the dispute and provides legal arguments in support of a right to recovery. If the defendant contests any part of the plaintiff's claim, the disputed issues are raised in a formal answer to the complaint. Issues unresolved by pleadings, discovery, or various pretrial motions are eventually argued before a judge or jury, who renders a verdict on the defendant's liability and the legal relief owed to the plaintiff. To be found liable, the defendant must be shown causally responsible for the plaintiff's injury in breach of a legal duty of care.[1]

[1] The duty of care depends on the subject matter of the dispute. For an unintentional tort (e.g. traffic collision), the plaintiff must at least show that the defendant acted negligently. For an intentional tort (e.g. battery), the plaintiff must show the defendant either intended to cause harm,

If the plaintiff is entitled to relief, the trial verdict specifies the form of redress. In most cases, relief will involve money damages the defendant must pay to compensate the plaintiff for the injury sustained. Depending on the tort, damages may be either pecuniary (covering measurable harms such as lost wages, expenses or property damage), non-pecuniary and non-punitive (covering non-measurable harms such as pain and suffering), or punitive (damages assigned to punish the defendant and deter similar behavior in the population). The legal remedy may also include positive or negative injunctions on future behavior: i.e. a judicial mandate, enforceable by action of the plaintiff, requiring the defendant to act (or not act) in some specified way.

According to the national *Civil Justice Survey of State Courts, 2005* (CJSSC), conducted by the Bureau of Justice Statistics (BJS) at the U.S. Department of Justice, tort disputes account for around 60% of all civil cases decided by trial verdicts (BJS, 2009).[2] Plaintiffs are awarded damages in approximately half of all verdicts, with a median award of $24,000 (Cohen, 2009, pp. 4–5), though outcomes vary considerably by dispute type.[3] Of the approximately 10% of tort disputes in which punitive damages are sought, such damages are awarded only about 12% of the time (BJS, 2009).[4] While the actual trial of a tort dispute lasts an average of only about 4 days, average delay from complaint to verdict is about 22.3 months (Cohen, 2009, pp. 8–9).

Although a trial verdict is the only legal remedy for a tortious harm, an alternative means of dispute resolution is private settlement. A settlement occurs when litigants

or acted with knowledge that harm was likely to result. For a strict liability tort (e.g. injury from certain types of product defect), the plaintiff need only show a harm was caused by the defendant.

[2]The CJSSC sample is restricted to disputes falling under the major civil law categories of torts, contracts, and real property, with classification according to the plaintiff's principal claim.

[3]Cohen (2009, pp. 4–5) estimates that plaintiffs win about 64.3% of automotive liability cases and about 54.9% of asbestos product liability cases, but only win about 22.7% of medical malpractice disputes and 19.6% of general product liability cases. The median product liability award in excess of $500,000 compares with the median automotive liability award of $15,000.

[4]Punitive damages can be large, with 25%, 50%, and 75% quantiles of approximately $20,000, $55,000, and $179,000 respectively (BJS, 2009).

agree to a compensation package—to be provided to the plaintiff by the defendant—which is mutually preferred to continuation with the legal remediation process. In settlement, the plaintiff contractually waives the right to a trial verdict in exchange for a compensatory payment plan to be executed by the defendant. Non-monetary contractual obligations may also be included: e.g. a non-disclosure clause, which prevents disputants from revealing the terms of settlement to other parties.

Contrary to popular belief, settlement is by far the most common path to dispute resolution in United States tort cases. Only around 3–5% of all cases are resolved by a verdict on the merits (Smith et al., 1995, p. 3; Cohen, 2009, p. 14). Approximately 55–73% of cases conclude in some form of private settlement (Smith et al., 1995, p. 3; BJS, 2009),[5] and these frequency estimates are almost certainly lower bounds, as disputes settled without at least the formal filing of a complaint are unobserved in contemporary national surveys. Residual cases are mainly dismissed, abandoned, or disposed in alternative tribunals (see Smith et al., 1995, p. 3).

Because most settlements are private, decentralized arrangements in which at least one of the disputants has incentives to guard the secrecy of settlement details, the collection of data on tort dispute settlements has proven extremely difficult. No detailed nationwide survey records systematic data on settlements, though some limited data are available in certain states. Texas, for example, has for the past 20 years required liability insurance companies operating within its borders to report on the resolution of bodily injury tort claims levied against policy holders (see Texas Department of Insurance, 2009b). When a dispute is settled, liability insurance companies in Texas are obligated to disclose information about the date and terms of the settlement.

[5] The lower bound of 55% is computed with data from the CJSSC and includes disputes voluntarily dropped by the plaintiff. The upper bound of 73% is reported by (Smith et al., 1995, p. 3) and excludes dropped cases; allowing for dropped cases increases the percent of settled disputes to about 83%, but also includes cases dismissed by a court.

While most tort disputes are ultimately settled, anecdotal accounts suggest that settlement often occurs only after protracted delay: in the intervening time, disputants negotiate potential terms of settlement and proceed through steps in the legal remediation process. The qualitative observation of lengthy settlement delay is corroborated by available data. In the *2007 Texas Liability Insurance Closed Claim Survey* (TLICCS), compiled by the Texas Department of Insurance (TDI), median delay between injury and resolution was about 29.9 months for settled disputes, and about 38.2 months for disputes disposed by trial verdict (TDI, 2009a).[6] As indicated in Table 1, average delay-to-settlement varies systematically across dispute types. There is also considerable variation in delay within dispute types, as illustrated in Figure 1. In the 2007 TLICCS, the average settlement transfer was about $65,000.[7]

Tort litigation implies a number of private and social costs. Private costs include the opportunity cost of time invested by the disputants, attorney fees and various litigation costs. Attorney representation generally requires payment of legal fees accruing on an hours-worked basis.[8] Litigation costs include the special costs of discovery and depositions, and compensation for expert witness testimonies. Depending on the jurisdiction of the complaint, the legal remediation process may also require payment of additional fees: e.g. to file a complaint, to retrieve and copy court files, to enter witnesses, to enter attorney representatives, and to appeal judicial decisions.

[6]In some cases, such as asbestos disputes and other instances of delayed-onset injury, it may be more relevant to count settlement delay from the point at which a complaint was filed. In the 2007 TLICCS, the median delay between claim and resolution was 14.2 and 22.5 months, for settled and verdict-disposed disputes respectively (TDI, 2009a).

[7]Delay and settlement calculations are based on resolved disputes reported in the 2007 TLICCS for which both parties were represented by attorneys and settlement entailed a positive transfer. Care is needed in comparing settlement amounts in the TLICCS with court awards in the CJSSC. Whereas CJSSC data cover trial dispositions for all types of tort claims, TLICCS data cover only a subset of tort disputes—principally automotive, product liability, and medical malpractice cases involving a claim of bodily injury and in which the defendant is holder of a liability insurance policy.

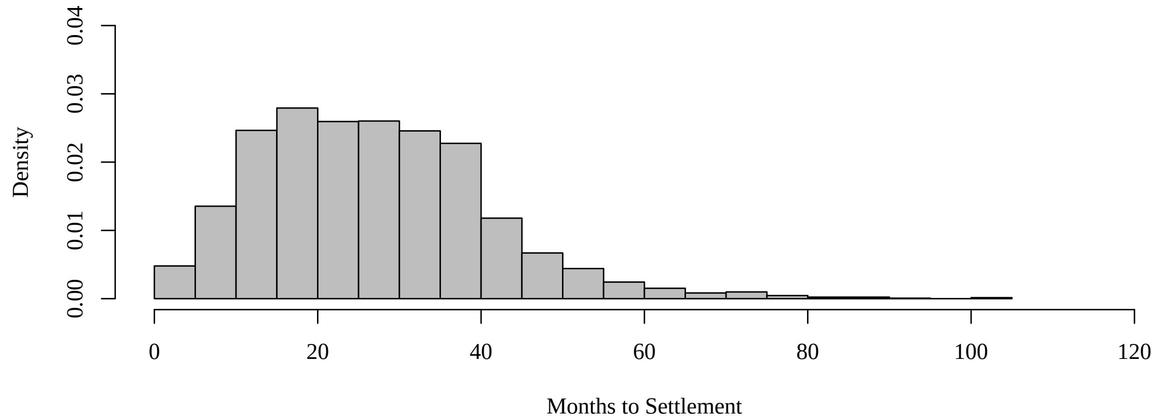
[8]In certain areas of tort law it is common for plaintiff attorneys to be compensated on a contingency basis: i.e. compensated only in the event that the plaintiff obtains some form of reward. In such cases, attorney compensation is usually a fixed percentage of any award obtained.

Table 1: Median Delay-to-Settlement by Dispute Type and Year, TLICCS

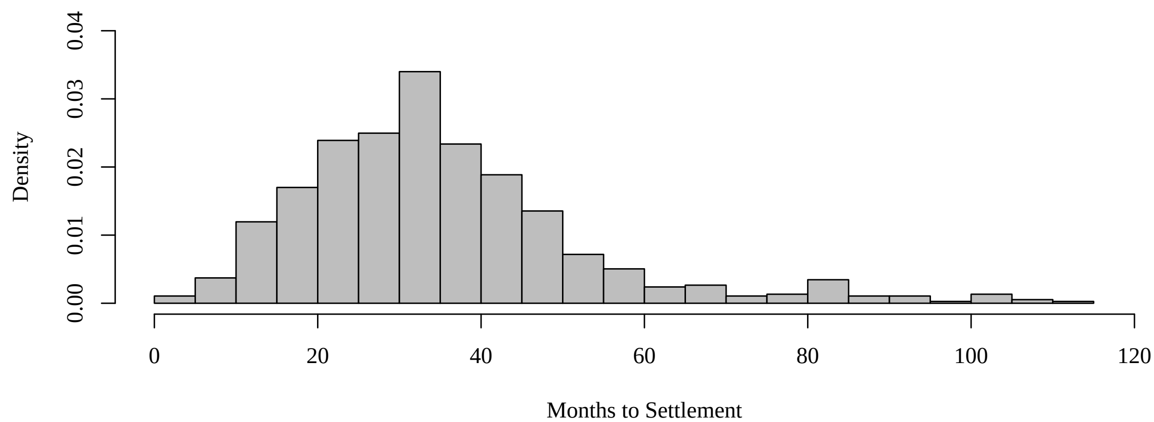
Year	Auto. Liability[a,b]	Product Liability[a,b]	Med. Mal. Liability[a,b]	All Types[a,c]
1997	26.78	34.29	38.93	33.30
1998	28.22	36.18	37.74	31.87
1999	27.39	33.40	37.05	31.10
2000	27.58	33.83	38.56	31.71
2001	27.30	34.59	39.09	32.09
2002	26.25	30.82	38.37	30.31
2003	27.42	33.24	39.65	31.86
2004	28.14	33.96	36.23	31.17
2005	27.72	35.49	37.51	32.28
2006	25.82	33.62	41.92	30.97
2007	25.50	33.01	41.38	29.90

[a] Sample definition provided in note 7.
[b] Median delay-to-settlement in months from injury.
[c] Median delay-to-settlement in months from injury for all auto, product liability, and medical malpractice disputes.

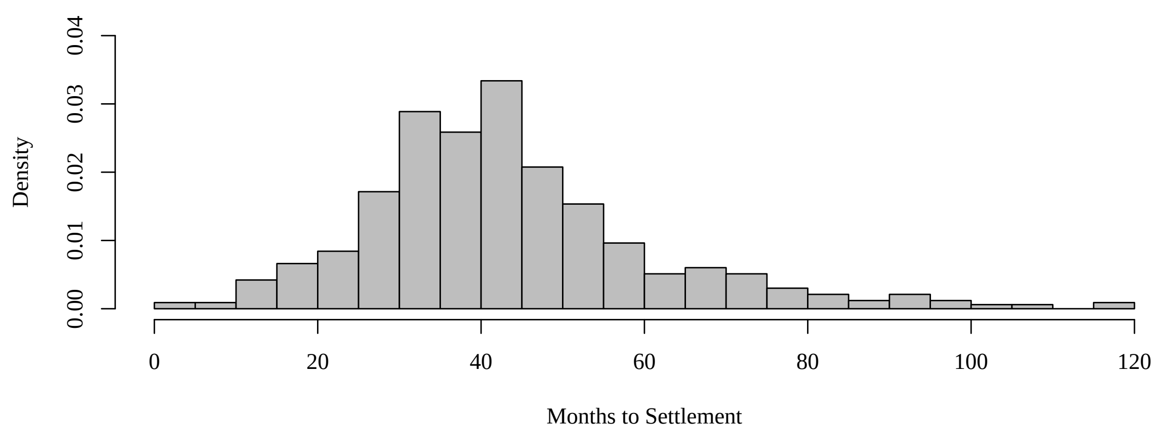
Figure 1: Delay-to-Settlement by Dispute Type, 2007 TLICCS[a]



(a) Automotive Liability Disputes



(b) Product Liability Disputes



(c) Medical Malpratice Disputes

[a]Sample definition provided in note 7. A small number of disputes with delay-to-settlement in excess of 120 months are omitted from the illustration.

Social costs of tort litigation include the opportunity costs of time and resources invested in the dispute by all involved actors. To see this, note that the transfer of wealth being negotiated in a tort dispute creates no economic value outside of affecting potential deterrence in the population. Holding the deterrent effects of tort disputes constant (see Section 17.2), the time and resources of the disputants as well as the opportunity costs of court officials, attorneys, and witnesses, approximate socially inefficient rent-seeking costs.

Industry estimates place the aggregate "direct" cost of the U.S. tort system at $252 billion per annum in 2007: roughly 1.8% of annual GDP or $825 per capita (Towers Perrin, 2008, pp. 5–6).[9] Over 97% of allocated defense expenditures reported in the 2007 TLICCS are attributable to settled disputes, with defense expenditures averaging about $1,000 per month from the point of injury to settlement.[10] Plaintiff costs are not observable in available settlement data, but are arguably comparable to defendant costs (see, e.g., Sieg, 2000, p. 1010). I am not aware of any systematic study of the indirect costs of the U.S. tort system.

The high costs associated with tort litigation have motivated many attempts to "reform" current tort law. For example, many states currently place limitations on the kind of damages a court may award: e.g. eliminating the potential for punitive or non-economic damages in certain circumstances. Another common policy change is imposition of a cap on the total amount of damages that can be awarded, either in

[9]This cost estimate excludes federal and state administrative costs (Towers Perrin, 2008, p. 10). It is "direct" in the sense of failing to account for "indirect" costs such as distortionary behavior (e.g. taking inefficient precautions in order to avoid litigation) and pure rent-seeking behavior (e.g. the formation of industries around tort litigation which only affect the potential size of transfers).

[10]Calculations are based on defendant/insurers' allocated loss adjusted expenditures: e.g. amount paid to in-house defense counsel and outside defense counsel, as well as court costs, stenographer costs, etc (see Texas Department of Insurance, 2009b, p. 47). Average expenditures are based on settled disputes in which both parties are represented by attorneys and settlement entails a non-zero transfer, with a small number of disputes having delay-to-settlement in excess of 120 months omitted. When measured from the time of complaint to settlement, rather than from injury to settlement, average defense expenditure is about $1,500 per month.

terms of specific categories or gross damages.[11] In contrast to the previous *defendant-favoring* policy changes, a popular *plaintiff-favoring* reform measure is a prejudgment interest rule, which compels defendants to pay damages with interest accrued from the date of injury if found liable in a trial verdict.[12]

Other reform policies, proposed by academics and policy makers, have not been as widely implemented. An example is the "early offers" reform proposed by O'Connell (1982). Under early offers reform, a defendant who early in the litigation processes offers to settle for at least remuneration of economic damages will face a more favorable standard of proof if the dispute ultimately proceeds to trial. This gives the defendant a positive incentive to make a reasonable settlement offer early in negotiation, and gives the plaintiff a negative incentive to reject any such an offer. Though not widely adopted, a variation on early offers reform is narrowly employed in Maryland (Schukoske, 1994, pp. 38–40, n. 63).

While all attempts at tort reform share the common objective of reducing average dispute costs, there is considerable heterogeneity in the way proposed policy changes approach this goal. Different reform measures target different parties, and affect different incentives. Due to the complicated social costs of the tort system and limitations of available data, the practical efficacies of even widely implemented reform policies are largely unknown.

[11] I define a damage *limit* and a damage *cap* as respectively corresponding to a truncated and censored award distribution: further discussion is provided in Sections 13.1 and 13.2. Either policy may apply to specific categories of damages, or to a gross award. The American Tort Reform Association (ATRA) lists 32 states as restricting punitive damages, and 23 states as restricting non-economic awards (ATRA, 2009), but such restrictions differ widely by state. A variation on award restrictions is a "split-award" reform, where the defendant pays the full amount of the award, but the plaintiff splits receipt of the award with the state (Nikitin and Landeo, 2004).

[12] The ATRA lists 16 states as having some form of prejudgment interest law (ATRA, 2009).

1.2 Economic Background

Economists conceptualize dispute resolution as a bargaining process between litigants. The abstract model involves a plaintiff and defendant bargaining over a potential settlement (i.e. monetary transfer), with default payoffs determined by a trial verdict in the event that the litigants fail to reach agreement. In the bargaining framework, litigation costs map to the delay costs of failure to settle.[13]

Dispute resolution has been studied with both cooperative and non-cooperative models, though the latter approach dominates contemporary research. Under non-cooperative analysis, it is assumed that parties to a dispute behave according to a cohesive set of equilibrium strategies. While most economic models rest predictive authority on equilibrium analysis, an important lesson of experimental economics is that game theoretic equilibria are not always reliable descriptors of actual behavior (e.g. Davis and Holt, 1993, pp. 507–508). The relevance of equilibrium analysis to tort disputes has not been systematically studied, and there are compelling arguments both for and against its application.

The most important potential obstacle to equilibrium in tort disputes is the limited opportunity for learning afforded to the players. Tort disputes are infrequent and highly heterogeneous events in which past and community experience may provide little opportunity for emulation or learning. Using only introspection, disputants unfamiliar with the process may not necessarily coordinate on a set of equilibrium strategies (cf. Holt, 2007, p. 317). On the other hand, the frequent use of attorneys and liability insurance companies—who gain institutional knowledge through repeated play and can pass this knowledge on to their clients—may act to mitigate problems

[13] In a two-period settlement-or-trial model, the aggregate costs of bargaining and trial are lumped into the delay-cost of taking a dispute to trial (e.g. Bebchuk, 1984). In a multi-period bargaining model, aggregate bargaining costs become the cost of delay for all but the final period of the game, and final-period delay costs reflect the special costs of trial (e.g. Spier, 1989, 1992).

caused by the infrequency of tort disputes. Though the issue demands future scrutiny, the present study follows the extant literature in assuming that actions of litigants in the field can be fairly characterized as equilibrium behavior.

The critical detail in economic study of dispute resolution is the potential settlement of a dispute. From the perspective of both plaintiff and defendant, private settlement has several advantages over disposition by trial verdict. First, settlement eliminates uncertainty over the size of a potential transfer of wealth. Second, it excuses litigants from paying the opportunity costs of continuing to litigate the dispute—this is especially relevant in light of the considerable dispute costs observed in Section 1.1. Substantial costs imply two important economic puzzles in terms of the way disputants are observed to behave in the data: (i) the trial verdict puzzle, and (ii) the settlement delay puzzle.

1.2.1 Trial Verdict Puzzle

Much of the work on settlement bargaining focuses on what I term the *trial verdict puzzle*: the puzzle of explaining why tort disputes are ever disposed by trial verdict. To see the problem, suppose a verdict awards damages $x \geq 0$ to the plaintiff, and let the costs of trial be $k_p > 0$ for the plaintiff and $k_d > 0$ for the defendant. Just to clarify analysis, ignore inter-temporal discounting and assume preferences are limited to the size of a monetary transfer. A trial verdict is Pareto dominated by a range of settlement amounts $s \in (x - k_p, x + k_d)$. Because settlement is feasible in every dispute, the puzzle is to explain why trial verdicts are ever observed in the data.

In the extensive literature on the trial verdict puzzle, the most widely studied hypothesis is that trial verdicts can be explained as rational failures to settle in the presence of asymmetric information between litigants.[14] In most models, litigants are

[14] Cf. Kennan and Wilson (1993) for similar arguments in other bargaining contexts.

assumed to be asymmetrically informed about the details of the potential verdict, such that one party has superior information about the likelihood or size of a trial award.[15] With exogenously fixed potential settlement amounts, P'ng (1983) and Hylton (1993) demonstrate rational failures to settle under models where the defendant is privately informed about his/her true liability. With endogenous settlement amounts, Bebchuk (1984) and Reinganum and Wilde (1986) describe screening and signaling models, respectively, where asymmetric information over trial outcomes leads to trial verdicts through rational failures to settle.

Some researchers raise the question whether asymmetric information over the facts of a dispute could survive the discovery processes (e.g. Hay, 1995). The validity of this concern is unclear: a number of procedural restrictions obstruct complete disclosure, and disputants have strong incentives to selectively hide and reveal different types of information.[16] Of course, even if discovery were to compel full disclosure of the facts, it would not rule out other forms of information asymmetries. Asymmetric information over preferences such as risk aversion and litigiousness (Farmer and Pecorino, 2005), costs (Hay, 1995), and taste for fairness (cf. Andreoni et al., 2003) may also drive rational failures to settle.

Despite extensive theoretic agreement, there is only modest empirical evidence that asymmetric information leads to trial verdicts. Considering the Nalebuff (1987) model of settlement bargaining when information is asymmetric and not all claims are credible, Sieg (2000) shows that structural estimation moments closely match observed empirical moments. Noting that comparative statics can be highly sen-

[15]Disputants may also be differently informed about aspects of the potential verdict. One party may have superior information about the size of an award, the other superior information about the likelihood of a plaintiff-verdict (e.g. Daughety and Reinganum, 1994; see also Schweizer, 1989).

[16]Many classes of information are privileged, and thus exempt from the discovery processes. Even nonexempt information may remain concealed if the opposing party fails to ask the right questions in the right way. It is theoretically unclear that this process necessarily improves upon voluntary disclosures in reducing information asymmetries: see, e.g., discussion in Shavell (1989).

sitive to model specification, Fournier and Zuehlke (1989) comment that estimates from a reduced-form model of settlement are nominally more consistent with a model involving questions of claim credibility (Nalebuff, 1987) than with a model of asymmetric information alone (Bebchuk, 1984). Laboratory experiments with settlement bargaining observe a rather fragile increase in the frequency of trials when asymmetric information is included as a treatment (cf. Babcock and Landeo, 2004; Inglis et al., 2005). Such fragility is not terribly surprising, as asymmetric information has demonstrated mixed results in predicting rates of disagreement in experimental tests of several generic bargaining models (see Roth, 1995).

The literature on the trial verdict puzzle has also explored a number of other hypotheses. Popular in the legal community, the Priest and Klein (1984) model conceptualizes settlement bargaining as a cooperative game with trial verdicts resulting from absence of a contract zone when commonly known but divergent beliefs about the potential trial outcome foreclose Pareto improving settlement transfers.[17] A related hypothesis, based on the cognitive limitations of overconfidence and self-serving bias, has shown some promise in experiments (Loewenstein et al., 1993; Babcock et al., 1995, 1997) and field data (Farmer et al., 2004).[18]

Regardless of the underlying cause, empirical evidence suggests that the trial verdict puzzle may be of limited practical importance. As noted in Section 1.1, only a small fraction of tort disputes—less than 3–5%—actually culminate in a trial verdict. Most disputes do in fact end in private settlement. A second puzzle remains, however, and is the subject of study in the following chapters.

[17] This *divergent expectations* model of settlement failure is similar to early non-strategic models of the dispute resolution process which arguably culminated in the work of Shavell (1982). A difficult question is *why* beliefs diverge when litigants are modeled as acting cooperatively.

[18] A curiosity of this behavioral explanation is that these biases appear to be mitigated by introspection (Babcock et al., 1997) and learning through repeated play (Farmer et al., 2004). It is therefore unclear why lawyers and liability insurance companies would be subject to such biases.

1.2.2 Settlement Delay Puzzle

Closely related to the trial verdict puzzle, but less well studied, is what I term the *settlement delay puzzle*: the puzzle of explaining how litigants facing substantial negotiation costs can reach delayed settlement in equilibrium. Unlike trial verdicts—which are only infrequently observed in the data—lengthy settlement delays are pervasive.

To clarify the settlement delay puzzle, let monthly litigation costs be $c_p > 0$ for the plaintiff and $c_d > 0$ for the defendant, and suppose the parties settle for a monetary transfer $s \geq 0$ after t months of negotiation. Again just to simplify analysis, ignore inter-temporal discounting and assume preferences are limited to the size of a transfer. Any settlement s at time $t > 1$ is Pareto dominated by a range of feasible settlements $s' \in (s - c_p, s + c_d)$ at time $t - 1$. Iteration on this logic argues that if a dispute is settled, it should be resolved immediately at time $t = 1$. The onus is to explain the reverse, in light of the protracted delays evident in the data.

As in the trial verdict puzzle, the most popular hypothesis is that asymmetric information explains the empirical observation of settlement delay.[19] The most widely cited model in this vein is presented by Spier (1989, 1992). Spier extends the one-period settlement/trial model in Bebchuk (1984) to a series of concatenated ultimatum games, showing that with one-sided asymmetric information over trial outcomes and an exogenous trial date, there exists a pure strategy equilibrium with a positive probability of settlement in every period. As in the trial verdict puzzle, asymmetric information need not be over the facts of a dispute. For example, Miceli (1999) considers a signaling equilibrium with potential settlement delay when asymmetric information regards legal costs.[20]

[19] Not all research assumes asymmetric information. For example, Watanabe (2006) uses a multi-period extension of the "divergent expectations" model to motivate delayed settlement.

[20] Asymmetric information over costs may not be applicable in every dispute: see Kennan and Wilson (1993, p. 50) for an interpretation of lawyers as experts on predicting legal costs.

A complication in addressing the settlement delay puzzle is deciding when settlement bargaining ends. The previous models assume disposition by trial verdict at the end of an exogenous number of bargaining periods. Attempts to endogenize the trial date in asymmetric information models have encountered mixed success. Modeling trial as an outside option of the plaintiff in each period, Spier (1992) demonstrates a continuum of equilibria, many involving delays. In a similar model, however, Wang et al. (1994) find that with the addition of infinitely repeating alternating offers, equilibrium settlement occurs no later than the second period of bargaining. Thus, the theoretical consequences of an endogenous trial date are currently unclear.

Empirical evidence that asymmetric information contributes to settlement delay is relatively thin. Fournier and Zuehlke (1996) and Fenn and Rickman (1999, 2001) use survival models to empirically investigate several comparative static predictions of the Spier (1992) model of settlement delay. Using field data, these studies find directional conformity with several predicted effects: for example, greater expected trial awards are associated with increased average settlement delay, while greater negotiation costs are associated with reduced average delay. Kessler (1996) uses a survival model to study institutional sources of settlement delay. Results implicate legal system congestion as an important contributor,[21] but also indirectly corroborate the predictions of Spier's model with regard to the delay consequences of increased potential awards through prejudgment interest laws.[22]

I am not aware of any experimental study of settlement delay in tort disputes, though delayed agreement is a robust characteristic of many generic multi-period bargaining games in the laboratory. For example, experimental studies by Roth

[21]Some portion of the observed settlement delay in field data is undoubtedly attributable to exogenous factors: e.g. incompatible schedules, heavy attorney workloads, and legal system congestion. Exogenous delay is irrelevant to theoretical study, but complicates analysis of field data.

[22]See Section 13.3, on the effects of prejudgment interest rules in the Spier model.

et al. (1988) and Gneezy et al. (2003) observe that in games with finite negotiation periods, agreements often tend to be made only as the exogenous stopping point draws near—this behavioral regularity is usually termed the “deadline effect” (see Güth et al., 2005). Experiments involving models of generic bargaining with incomplete information have encountered mixed success in describing delayed agreement (e.g. Forsythe et al., 1991; Rapaport et al., 1995; see also Roth, 1995, pp. 312–322).

2 Research Overview

This section explains the present study's use of laboratory experiments as a tool for investigating settlement delay under asymmetric information. Section 2.1 summarizes the policy relevance of settlement delay and describes the comparative advantage of controlled laboratory experiments in application to this topic. Section 2.2 outlines the progression of research throughout the remaining chapters of this study.

2.1 Motivation

In the United States tort system, large social costs accumulate as litigants engage in protracted settlement negotiation. Such costs are *ex post* inefficient in the sense that when litigants reach settlement after any significant delay, both parties must realize that a mutually preferred settlement was feasible at an earlier time. All else equal, efficiency gains exist for any policy change that reduces the average duration of bargaining in settled tort disputes.[23] The magnitude of costs associated with the tort system means that even small reductions in average settlement delay could imply large reductions in socially inefficient spending.

An important obstacle to achieving efficiency gains is the difficulty of changing tort law at either the state or federal level. First, public opinion about "tort reform" is divided along strongly held ideological lines. Second, deeply interested and influential parties lobby for and against various reform measures. Third, visibly disastrous consequences await the enactment of ill-conceived reforms. Fourth, policy changes which are subsequently discovered unappealing may prove politically difficult to reverse. And fifth, the powerfully emotional nature of many tort disputes drives concern about the morality and fairness of various reforms, increasing the complexity of se-

[23]For discussion of model limitations and dynamic analysis, see Section 17.2.

lecting a policy change and arguing against real-world policy experiments as a viable way to learn about the performance of competing reform proposals.

The high stakes and formidable costs of attempting to change tort policy make academic research on this topic especially valuable. From a practical perspective, no significant policy change is likely to occur without the support of both research and experience. But while the tort system has long been the focus of academic scrutiny, the critical detail of settlement delay remains under-explored. Neither the causes of settlement delay nor potential solutions to the problem are presently well understood.

One problem is that empirical research on settlement delay lags behind theoretic study. This is basically attributable to the difficulty of working with currently available field data. Detailed empirical research on settlement delay requires rich data on the terms and timing of settlement, and on the disposition of trial verdicts whenever they occur. But as noted in Section 1.1, field data on settled tort disputes are difficult to find and expensive to collect; even in relatively detailed data, critical details such as asymmetries in information, expectation, preference, and costs are generally opaque. It is consequently difficult to employ available field data in exploring theoretic models of settlement delay in great detail.

Limitations in field data affect policy analysis as well. While it is obviously hard to predict the consequences of policy changes with the underlying causes of settlement delay unknown, remaining agnostic about the causes of delay by simply focusing on how observed delay responds to changes in extant tort policy introduces its own complexities. First, only a very small number of states attempt to collect systemtic field data on settled tort disputes. As a practical matter, this makes exploitation of interstate variation in tort law an unrealistic strategy for identifying the effects of most policy changes. Second, with so few data sources and no obvious instruments available, concern about endogeneity between settlement delay and adopted tort policy may limit confidence in derived predictions. Third, many promising policy proposals have yet to be implemented in any jurisdiction and so would not be observable even if ideal field data were available.

A research strategy which sidesteps many of the limitations in current field data is the collection of data on mock dispute bargaining in controlled laboratory experiments with payment-incentivized subjects. Laboratory experiments are frequently motivated by research topics for which satisfactory field data are either unavailable or prohibitively costly to collect. At a cost much lower than would be incurred in securing similarly detailed field data, laboratory experiments provide a valuable starting point for empirical research on both the theory and policy of settlement delay.

Data from laboratory experiments are not, however, without their own limitations. Particular concern attaches to the *external validity* of experimental results: i.e. the ability of behavior in an abstract laboratory setting to reasonably approximate actual behavior in the field. This concern councils for cautious attention to both the proper design of experimental methodology and the proper interpretation of behavior.

2.2 Outline

The remaining chapters of this study employ a large laboratory experiment to empirically investigate the properties of settlement delay when subjects are asymmetrically informed about the outcome of a potential trial verdict in mock tort disputes. Focus on the asymmetric information hypothesis is driven by its plausibility and popularity in theoretic models.[24] Research addresses two broad empirical questions important to understanding settlement delay and crafting efficient tort policy.

[24] The pervasive settlement delay observed in field data is almost certainly driven by a complicated mix of factors. It remains for future studies to explore causes other than asymmetric information. The basic research design employed in the following chapters may be modified to test other hypotheses in subsequent studies.

Research Question 1. Can asymmetric information over trial verdicts plausibly contribute to the protracted delay observed in tort disputes?

This question is academically interesting as an empirical examination of theory. To my knowledge, this is the first experimental study of settlement delay in dispute resolution, and results may contribute to a range of other dispute resolution models. The practical relevance of the question is in identifying a potential contributor to the protracted settlement delay observed in tort disputes in the field.

Research Question 2. Can specific policies be identified which might mitigate the settlement delay caused by the presence of asymmetric information over trial verdicts?

Assuming Research Question 1 is affirmed, the answer to this second question is of practical importance to policy-makers interested in changing United States tort policy. Study of how settlement bargaining behavior responds to changes in the bargaining environment also provides insights into the robustness of theoretic models which attribute settlement delay to asymmetric information.

The remainder of this study address these two broad research questions in detail. Though conceptually distinct, the questions share many common dependencies: in particular, the underlying theoretic model and experimental environment are effectively the same for both. This close relationship is exploited in construction of a complicated, but resource-efficient experimental design that addresses both research questions simultaneously. The following chapters are structured as follows.

Chapter II presents a theoretic model of settlement bargaining where delayed settlement results from asymmetric information over a potential trial verdict. The model is a minor variation on the popular model of settlement delay proposed by Spier (1989, 1992). Recognizing that theoretic predictions from bargaining models are often too stark to reliably describe behavior in the lab, this chapter also discusses a number of observations from the behavioral economics of bargaining. Particular note is given to the inefficient disagreements and costly delays which pervade even simple bargaining models with complete information. Juxtaposed against theoretic predictions, behavioral caveats recommend a cautious interpretation of laboratory data and results.

Chapter III presents the experimental design employed in the present research. Payment-incentivized laboratory subjects interact in an experimental bargaining game closely based on the theoretic model described in Chapter II. A repeated measurement cross-over design exposes different subjects to different pairs of experimental treatments. This flexible design addresses a range of exploratory and confirmatory research questions while also providing strong experimental controls against various sources of potential design bias.

Chapter IV describes and summarizes the results of Sub-Experiment 1 (SE1), the objective of which is exploratory analysis of settlement bargaining behavior under a control treatment which closely adheres to the theoretic model of settlement bargaining with asymmetrically informed litigants. Data suggest mixed conformance between observed behavior and theoretic prediction. Settlement delay is pervasive in SE1, with resolution times distributed close to the theoretic prediction.

Chapter V describes and summarizes the results of Sub-Experiment 2 (SE2). The objective of SE2 is broadly confirmatory: experimental data are used to assess the causal influence of a controlled information asymmetry on average settlement delay. Under a variety of treatments designed to test the sensitivity of results, asymmetric information is affirmed to causally induce settlement delay.

Chapter VI describes and summarizes the results of Sub-Experiment 3 (SE3). The broad objectives of SE3 are both confirmatory and exploratory. Experimental data address the confirmatory question whether various "tort reform" policies might reduce the settlement delay associated with asymmetric information. A subsequent inquiry looks to the wealth-distributive effects each policy. Investigated policies include a damages limit, damages cap, prejudgment interest rule, and Early Offers reform. No reforms are found to affect obvious reductions in settlement delay, but most reforms do cause clear changes in the relative earnings of tort dispute litigants.

Chapter VII provides concluding remarks about the findings of the present research. Implications of research findings are discussed (i) for future academic study of settlement delay in tort disputes, and (ii) for practical relevance in informing future tort policy. Limitations of the research design are also addressed, including concerns about the dynamic effects of imposing various reform measures, and the external validity of laboratory data to real-world tort disputes. Finally, extensions of the present research are suggested for the future study of settlement delay with laboratory experiments.

Chapter II

Model of Settlement Bargaining

A fully satisfactory model of settlement bargaining and the timing of dispute resolution is difficult to imagine. Legal bargaining—with all its uncertainty, heterogeneity, intricacy, and emotional drama—is a far cry from the abstract bargaining environments studied in non-cooperative game theory. But disengaging from the structure of negotiation to focus solely on axiomatic bargaining outcomes is also unsatisfying.[25] In abandoning the structure of bargaining, one concedes the ability to study settlement delay—effectively discarding the baby with the bathwater.

Understanding that no approach is perfect, the model of settlement bargaining adopted in the present study errs on the side of abstraction. For the broad research questions at hand, the capacity of a model to predict patterns of settlement delay is more important than generality in bargaining structure. Section 3 presents the model of non-cooperative settlement bargaining explored in this study. Equilibrium strategies predict rationally delayed settlement, with an uninformed defendant screening information from an informed plaintiff over multiple periods of bargaining.

Although theoretic bargaining models can provide strong predictions under reasonable assumptions, such predictions are often too stark to reliably describe bargaining behavior in the laboratory. To present a more rounded profile of predictions, Section 4 discusses important findings from the behavioral economics of bargaining. Several behavioral regularities oppose theoretic predictions in various classes of bargaining models. These regularities recommend a careful interpretation of theoretic predictions and their comparison to experimental results.

[25]Nash (1950) is archetypal axiomatic bargaining. See also Davis and Holt (1993, pp. 244–255).

3 Theoretic Model

Theoretic predictions are derived from a slight modification of the asymmetric information "pre-trial" model of settlement bargaining presented by Spier (1989, 1992).[26] The model provides an intuitive context for understanding how settlement delay may result from asymmetric information over a potential trial verdict. Attractive properties of the model include (i) wide citation, meaning experimental results may pertain to many extant discussions, (ii) unique equilibrium play under reasonable refinements, (iii) robustness to an effectively continuous interpretation of bargaining, and (iv) concrete predictions for the effects of various remedial tort policies.

Presentation of the model proceeds as follows. Details of the settlement bargaining game, rules of play, and associated notation are described in Section 3.1. Section 3.2 describes equilibrium strategies in the (standard) bargaining game with asymmetric information over a potential trial verdict. Section 3.3 describes equilibrium strategies in the special case of settlement bargaining with symmetric information over the potential verdict.

3.1 Model Description

The model characterizes settlement bargaining as follows. Litigants negotiate during at most T discrete periods of bargaining. In period $t = 1, \ldots, T$, the defendant makes a proposal S_t for the size of a settlement wealth transfer, and the plaintiff decides whether to accept or reject the proposal. Acceptance represents settlement, ending the game in period t with an immediate transfer of S_t from the defendant to the

[26] Relative to the Spier (1989, 1992) model, the present model reverses the bargaining roles of plaintiff and defendant and introduces uncertainty over liability in the trial verdict (i.e. the chance of a plaintiff verdict). The Spier model is a multi-period concatenation of the Bebchuk (1984) model of settlement bargaining under asymmetric information, with the information asymmetry changed from liability to damages and with bargaining costs distinct from trial costs.

plaintiff; rejection means the game proceeds to period $t + 1$. If the parties fail to settle within T periods, the dispute is resolved by a trial verdict in period $T + 1$.

Trial verdicts are modeled as the product of two exogenous events: (i) the determination of liability, i.e. whether the defendant is legally liable for causing the plaintiff's injury, and (ii) the determination of damages, i.e. the size of compensatory wealth transfer a liable defendant must make to the plaintiff. I model the liability decision as a simple probability $\pi \in [0, 1]$: the plaintiff wins positive damages with probability π and loses the case (zero damages) with probability $1 - \pi$. The value of potential damages x is modeled as a random draw from the continuous distribution $F(x)$ with positive support on $[\underline{x}, \overline{x}] \subset [0, \infty)$, and with $(d/dx)F(x) = f(x)$. A victorious plaintiff is awarded a wealth transfer of x in period $T + 1$.

Other aspects of the model include delay costs, and risk and time preferences. To represent the costs of bargaining, the plaintiff and defendant (indexed p and d respectively) incur negotiation costs of c_p and c_d at the beginning of each bargaining period $t = 1, \ldots, T$. If the dispute goes to trial, one-time trial costs are k_p and k_d in period $t = T + 1$. To avoid trivial special cases, I define $c_p, c_d, k_p, k_d > 0$. Disputants are rational, risk neutral, and have time preferences represented by the common discount rate $\delta \in (0, 1)$ per period.

In the standard (asymmetric information) model, the value of potential damages x is the private information of the plaintiff. That is, the plaintiff starts a dispute knowing his or her *type*, in terms of what damages the court would award if the plaintiff were to win at trial. The defendant does not know the plaintiff's type, but instead maintains beliefs over the type of plaintiff being faced. Conditional on rejection of all prior proposals, the defendant's period t beliefs are given by the probability density $\rho(x|S_1, \ldots, S_{t-1})$. All costs, π, $F(x)$, δ, the structure of information, and the structure of game-play are common knowledge.

Under this model of settlement bargaining, the plaintiff and defendant's settlement preferences in each period, $U_p(S_t)$ and $U_d(S_t)$, are the first-period net present value of a period t settlement transfer of size S_t:

$$U_p(S_t) = \delta^{t-1} S_t - c_p \sum_{i=1}^{t} \delta^{i-1} \qquad U_d(S_t) = -\delta^{t-1} S_t - c_d \sum_{i=1}^{t} \delta^{i-1} \tag{1}$$

Note that because neither $U_p(S_t)$ nor $U_d(S_t)$ depends on the plaintiff's type, all plaintiffs and defendants have symmetric preferences over settlement options in every period. Note also that social utility, defined as $U_p(S_t) + U_d(S_t)$, is monotonically decreasing in t. Delayed settlement is always socially inefficient.

The plaintiff and defendant's preferences over a trial verdict, $W_p(x)$ and $W_d(x)$, are defined by the size of potential damages x, net of bargaining and court costs and discounted to period $t = 1$:

$$W_p(x) = \delta^T(\pi x - k_p) - c_p \sum_{i=1}^{T} \delta^{i-1} \qquad W_d(x) = -\delta^T(\pi x + k_d) - c_d \sum_{i=1}^{T} \delta^{i-1} \tag{2}$$

In contrast to settlement preferences, trial preferences are explicitly a function of the plaintiff's type. Informed about the value of potential damages, the plaintiff knows trial preferences with certainty. The defendant does not know the value of potential damages, and so operates off the expected value of trial preferences given beliefs:

$$\mathrm{E}\left[W_d(x)\right] = \int_{-\infty}^{\infty} W_d(x) \rho(x|S_1, \ldots, S_{t-1}) dx \qquad t = 1, \ldots, T+1. \tag{3}$$

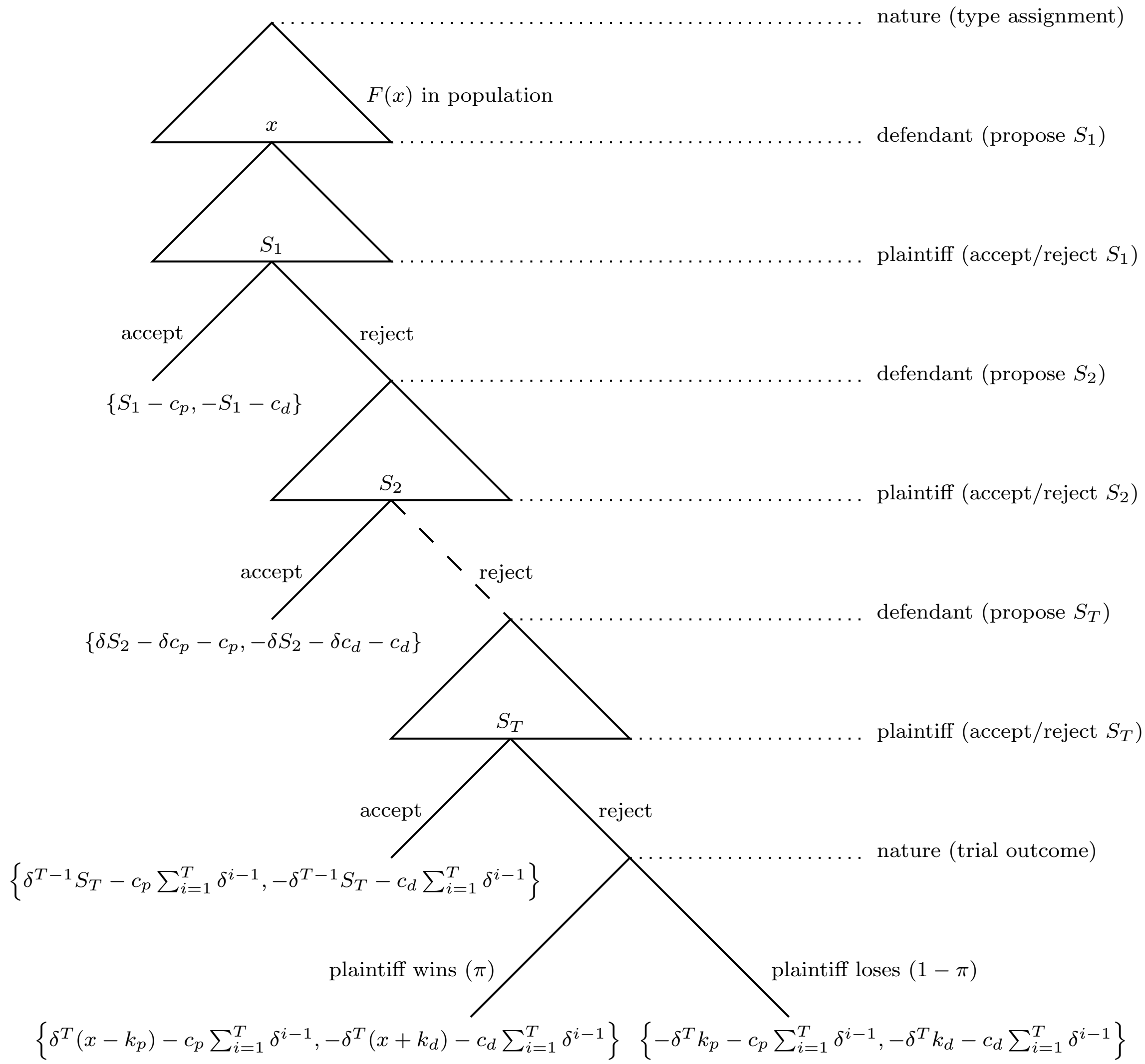
A summary of game notation is consolidated in Table 2. Figure 2 illustrates the basic structure of the game. Because continuous choice spaces and multiple bargaining periods make the accounting intractable, information sets are omitted from Figure 2. It suffices to say that there are no proper subgames to the standard game.

Table 2: Summary of Notation

Notation	Description[a]
p, d	index *plaintiff* and *defendant* respectively
x	potential damages; private information of the plaintiff; $x \geq 0$
$F(x)$	distribution of potential damages in the population
$f(x)$	density function of potential damages; $f(x) = (d/dx)F(x)$
$\overline{x}$, $\underline{x}$	upper/lower bounds on the support of potential damages; $\overline{x} = \sup\{x : f(x) > 0\}$; $\underline{x} = \inf\{x : f(x) > 0\}$; $[\underline{x}, \overline{x}] \subset [0, \infty)$
π	probability that plaintiff wins damages at trial; $\pi \in [0, 1]$
T	final period of bargaining
$T + 1$	period of trial verdict
c_p, c_d	negotiation costs paid at the start of every round of bargaining (i.e. periods $1, \ldots, T$); $c_p, c_d > 0$
k_p, k_d	one-time court costs paid only if dispute is resolved by a trial verdict (i.e. period $T + 1$); $k_p, k_d > 0$
δ	common per-period discount factor; $\delta \in (0, 1)$
S_t	settlement proposal made by defendant in period $t = 1, \ldots, T$
$U_p(S_t)$, $U_d(S_t)$	plaintiff and defendant preferences over settlement at S_t in period $t = 1, \ldots, T$
$W_p(x)$, $W_d(x)$	plaintiff and defendant preferences over a trial verdict when the plaintiff is of type x; period $T + 1$
$\rho(x \mid S_1, \ldots, S_{t-1})$	defendant's period t beliefs about the plaintiff's type (probability density over x), given observed play in prior periods

[a] Unless stated otherwise, parameters are common knowledge of the disputants.

Figure 2: Illustration of the Settlement Bargaining Game[a]



[a]Values in braces are the net present value for the plaintiff (left) and defendant (right).

3.2 Equilibrium with Asymmetric Information

As the present model of settlement bargaining involves sequential decision-making with asymmetric information, an appropriate choice of equilibrium concept is Perfect Bayesian Equilibrium (PBE).[27] Beyond the PBE refinement, I focus exclusively on equilibria in pure strategies. Several assumptions are employed to simplify analysis and further concentrate on relevant equilibria. Under these restrictions, the model admits a unique equilibrium path of play.

Before equilibria are derived, an initial assumption is made to rule out *nuisance suits*, which occur when a plaintiff pursues a dispute without credible intent to take the case to trial. Though a potentially important factor in explaining dropped cases in the field (see, e.g., Nalebuff, 1987; Bebchuk, 1996), this detail is ancillary to the present study's focus on settlement delay. I follow both Bebchuk (1984) and Spier (1989, 1992) in defining the population to contain only credible disputes.[28]

Assumption 1. In every period, the plaintiff expects the net present value of a trial verdict to exceed zero.

In practice, a sufficient condition for Assumption 1 is that the expected net present value of a trial verdict is positive for even the lowest-type plaintiff at the start of a game: $W_p(\underline{x}) \geq 0$. It is easy to see from equations (2) that if $W_p(\underline{x}) \geq 0$ holds at the start of a game of length T, then it must also hold in every continuation game of length $T' < T$, and if the condition holds for a plaintiff of type $\underline{x}$, then it must also hold for every $x > \underline{x}$ as well.

[27] Define an information set as *on the equilibrium path* if it is reached with positive probability under equilibrium strategies. The PBE concept requires (i) that a player maintain beliefs about the node reached in any non-singleton information set, (ii) that such beliefs be determined by Bayes' Rule on the equilibrium path, and (iii) that given beliefs, strategies are sequentially rational.

[28] An exclusive focus on credible disputes makes this model inappropriate as a descriptor of disputes where the defendant is uncertain about the credibility of the complaint. Assumption 1 can be interpreted as a restriction of the model to describing only the subset of disputes in which the plaintiff has an unambiguously credible claim.

3.2.1 Single-Period Game

Critical to understanding the equilibrium in a game of length $T > 1$, the PBE for a single-period game is given separate treatment. With $T = 1$, the defendant makes only a single ultimatum settlement proposal, S_1; the plaintiff's strategy dictates whether to accept the settlement proposal, or reject it and proceed to trial.

Proposition 1. *Implicitly define the interior-solution settlement proposal, S_1^I, as*

$$S_1^I: \quad -F(\pi^{-1}\{\delta^{-1}S_1^I + k_p\}) + \pi^{-1}(k_d + k_p)f(\pi^{-1}\{\delta^{-1}S_1^I + k_p\}) = 0.$$

Let the boundary-solution settlement proposal, S_1^B, be defined as

$$S_1^B = \delta(\pi\overline{x} - k_p).$$

In a game of length $T = 1$, the defendant's PBE strategy is to make the proposal S_1^* defined as

$$S_1^* = \min\,\{S_1^I, S_1^B\}.$$

The strategy for a plaintiff of type x is to accept any settlement proposal S_1 such that $U_p(S_1) \geq W_p(x)$, and to otherwise reject.

Proof. The proof is essentially the same as those of Bebchuk (1984) and Spier (1989, 1992), but is included in Appendix A.1 for completeness. □

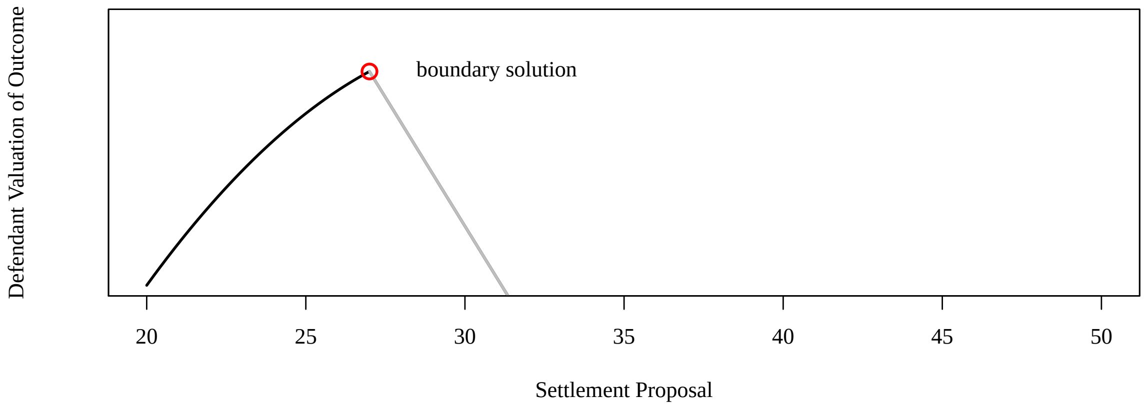
Intuition for the plaintiff's equilibrium strategy is straightforward: a plaintiff of type x accepts any proposal that is weakly preferred to a trial verdict, and otherwise rejects. The two components of the defendant's strategy correspond to interior and boundary solutions. A proposal of S_1^I corresponds to an interior solution: the defendant equates the marginal benefit of proposing a smaller S_1, so that the payout is smaller for the measure of plaintiff types that settle (the first term on the left-hand side of S_1^I), with the marginal benefit of proposing a larger S_1^I, so that a greater measure of plaintiff types are willing to settle (the second term on the left-hand side of S_1^I). A proposal of S_1^B corresponds to a boundary solution, where trial costs are sufficiently large in relation to the distribution of potential damages that the marginal benefit of proposing a larger S_1^I (so that a greater measure of plaintiff types settle) exceeds the marginal cost of a greater payout in settlement all the way up to full settlement with every type of plaintiff.

Whether the interior or boundary solution is optimal depends on model parameters. The relationship between interior and boundary solutions is illustrated over a range of parameter values in Figure 3. Figure 3(a) illustrates a situation where equilibrium involves the boundary solution: i.e. a situation in which trial costs are sufficiently great that the defendant can do no better than make a proposal just large enough that every type of plaintiff settles. Equilibrium involves the boundary solution identically when the boundary-solution settlement proposal is less than the interior-solution settlement proposal. Figures 3(b) and 3(c) illustrate equilibria involving the interior solution.

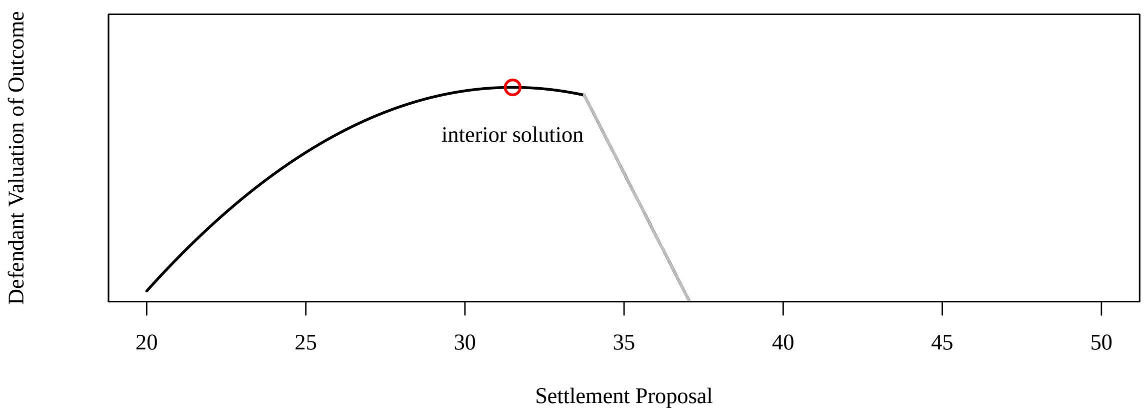
Since all types of plaintiff settle for the boundary-solution proposal, the boundary equilibrium predicts perfect settlement with no incidence of trial. By contrast, the settlement proposal in an interior equilibrium screens plaintiffs into two types. Types $x \leq \pi^{-1}\{\delta^{-1}S_1^* + k_p\}$ prefer the equilibrium settlement proposal and accept S_1^*, while types $x > \pi^{-1}\{\delta^{-1}S_1^* + k_p\}$ net greater expected returns from a trial verdict and so reject S_1^*. *A priori*, the interior solution places positive probability on both settlement and trial outcomes.[29]

[29]Provided the information structure is preserved, additional nuances such as risk aversion and dis-equal discount rates should not generally change the basic structure of the equilibrium.

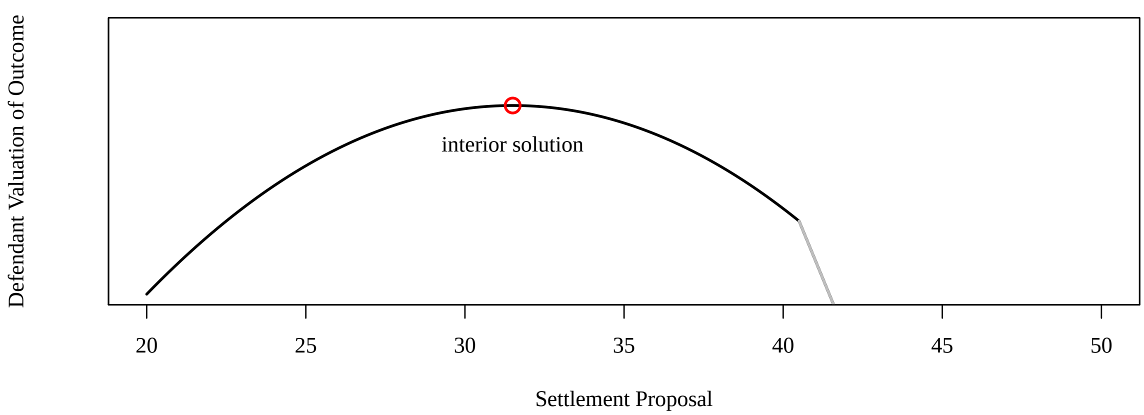
Figure 3: Illustration of Interior and Boundary Solutions[a]



(a) Example Boundary Solution: $\overline{x} = 80$



(b) Example Interior Solution: $\overline{x} = 95$



(c) Example Interior Solution: $\overline{x} = 110$

[a]Defendant objective function with x distributed uniformly on $[50, \overline{x}]$, $c_p = c_d = 1$, $k_p = k_d = 10$, $\delta = 0.9$, and $\pi = 0.5$. The black portion of the line indicates the value of a settlement proposal which some plaintiff types reject; the gray portion indicates the value of the boundary solution.

3.2.2 Multiple-Period Game

For a game of length $T > 1$, three additional restrictions are imposed to improve tractability and eliminate unreasonable equilibria. The first additional restriction is a refinement requiring that the plaintiff's settlement decision be a monotone function of type. As formalized in Assumption 2, acceptance is assumed to be decreasing in x.

Assumption 2. If S_t is accepted by a plaintiff of type x', then it is also accepted by a plaintiff of type $x < x'$.

The restriction imposed by Assumption 2 is intuitive: if a plaintiff of type x' accepts proposal S_t, it seems only reasonable that a plaintiff of lower type $x'' < x'$ should accept S_t as well, the lower-type plaintiff having weakly less to gain from rejection than the higher type. The refinement is maintained to rule out unintuitive strategies in which settlement timing is a non-monotone function of type. For example, if proposals S_1 and S_2 have the same net present value, so that $U_p(S_1) = U_p(S_2)$, Assumption 2 forecloses strategies in which a higher-type plaintiff settles for S_1 while a lower-type plaintiff settles for S_2. An immediate implication of the refinement is that the distribution of plaintiff types remaining at any point in the negotiation process is a truncation of the population distribution.[30]

The second additional restriction imposed in solving a game of length $T > 1$ is a refinement requiring that a plaintiff settle immediately when the defendant proposes the maximum transfer that could credibly be expected in light of parameter values and the distribution of potential damages. As formalized in Assumption 3, a plaintiff must settle when the proposal is weakly preferred to the value of a trial verdict for the highest-type plaintiff.

[30]Spier (1989, 1992) imposes the slightly weaker restriction that if a type x' plaintiff accepts S_t with positive probability, then a plaintiff of type $x < x'$ accepts with probability one. In restricting attention to pure strategy equilibria, Assumption 2 is an equivalent condition.

Assumption 3. A proposal S_t such that $U_p(S_t) \geq W_p(\overline{x})$ is always accepted.

Assumption 3 is a weak formulation of the familiar bargaining-model assumption that an agent indifferent between acceptance and rejection chooses to accept. The refinement rules out unintuitive boundary equilibria involving settlement delay: e.g. equilibria where all types of plaintiff reject a first-period settlement proposal such that $U_p(S_1) = W_p(\overline{x})$, but accept a later proposal of equal net present value.

The third additional restriction imposed in solving a game of length $T > 1$ specifies a particular distribution for $F(x)$. As formalized in Assumption 4, potential damages are uniformly distributed in the population.

Assumption 4. The population of plaintiff types has potential damages x distributed uniformly on support $[\underline{x}, \overline{x}]$.

The main benefits of assuming a uniform distribution are simplified computation of truncated distributions and admittance of closed-form expressions for the defendant's equilibrium strategy.[31] The uniform distribution is also used in experimental environments throughout the following chapters, where it provides an approachable concept of randomness for subjects who may have difficulty understanding more complicated probability distributions.

The symmetry of settlement preferences across plaintiff types is a critical element of the model for deriving equilibrium strategies in a game of length $T > 1$. Intuition for the importance of preference symmetry can be gained by noting that plaintiff preferences place strong bounds on the sequence of settlement proposals that can be made in any equilibrium. Enumerated in Lemma 1, proposal-sequence bounds are relied upon extensively in deriving the PBE for a multiple-period game.

[31] For example, with $T = 1$ and x uniform on $[\underline{x}, \overline{x}]$, the interior-solution settlement proposal is $S_1^I = \delta(\pi \underline{x} + k_d)$. This compares with the general expression for S_1^I in Proposition 1.

Lemma 1. *With $T > 1$, the sequence $S_1^*, \ldots, S_T^*$ satisfies the following properties:*

1. *$U_p(S_1^*) \geq \ldots \geq U_p(S_T^*)$ in any equilibrium;*
2. *$U_p(S_1^*) \leq \ldots \leq U_p(S_T^*)$ in any equilibrium where not all types of plaintiff settle.*

Proof. Provided in Appendix A.2. □

The intuition for these bounds is instructive. Consider a two-period game and suppose, contrary to the first proposition of Lemma 1, that $U_p(S_1^*) < U_p(S_2^*)$ so that *every* type of plaintiff prefers settlement at S_2^* to settlement at S_1^*. Not all types of plaintiff will necessarily settle, but those that do so will uniformly reject S_1^* and settle for S_2^*. This sequence of proposals cannot be optimal for the defendant, since there exists a first-period proposal S_1' such that $U_p(S_1') = U_p(S_2^*)$ which all plaintiff types would be equally willing to accept and which is strictly preferred by the defendant because it allows for collection of the bargaining costs that would otherwise have been committed in the second period of bargaining. A first-period proposal of S_1' is always feasible, so it must be that $U_p(S_1^*) \geq U_p(S_2^*)$ in any equilibrium.

Now suppose, contrary to the second proposition of Lemma 1, that $U_p(S_1^*) > U_p(S_2^*)$ with a positive measure of plaintiff types rejecting both S_1^* and S_2^*. Since *every* type of plaintiff prefers settlement at S_1^* over S_2^*, every type of plaintiff that opts to settle does so for S_1^*. As not all types of plaintiff settle by assumption, the continuation game starting in the second period is reached with *ex ante* positive probability. The continuation game is just a one-period game with the population of plaintiff types adjusted to remove types that settled for S_1^*. This means S_2^* is characterized by Proposition 1, which requires settlement with a positive measure of remaining types. But the given sequence of proposals fails to entice any additional settlement, because S_1^* is universally preferred. Thus, it must be that $U_p(S_1^*) \leq U_p(S_2^*)$ in any equilibrium in which a positive measure of plaintiff types refuse to settle.

A surprising implication of Lemma 1 is that the sequence of equilibrium settlement proposals in any interior equilibrium is subject to both the upper and lower bounds. Thus, in any equilibrium in which not all types of plaintiff settle, every type of plaintiff must be indifferent between settlement in every period of the game. This result is fundamental in deriving the PBE for a game of length $T > 1$.

The other important insight in deriving the PBE is recognition that only the first period of play in a game of length $T > 1$ needs to be solved in order to construct PBE for the full game. In the first period of a game of length $T > 1$, the defendant's strategy specifies a settlement proposal S_1 and the plaintiff's strategy dictates conditions for accepting the settlement proposal, and for rejecting the proposal in favor of the continuation game. Subject to Assumptions 1 through 4, the first-period PBE for a game of length $T > 1$ is characterized by Proposition 2.

Proposition 2. *Let the interior-solution settlement proposal, S_1^I, be defined as*

$$S_1^I = \delta^T(\pi\underline{x} + k_d) + c_d \sum_{i=1}^{T-1} \delta^i.$$

Let the boundary-solution settlement proposal, S_1^B, be defined as

$$S_1^B = \delta^T(\pi\overline{x} - k_p) - c_p \sum_{i=1}^{T-1} \delta^i.$$

In a game of length $T > 1$, the defendant's PBE strategy is to make the first-period proposal S_1^* defined as

$$S_1^* = \min\,\{S_1^I, S_1^B\}.$$

If the defendant proposes $S_1^* = S_1^B$*, the plaintiff's strategy is to accept* S_1^**. If the defendant proposes* $S_1^* = S_1^I$ *and the plaintiff prefers settlement at* S_1^* *to a trial verdict,* $U_p(S_1^*) \geq W_p(x)$*, then the plaintiff's strategy is to accept* S_1^* *if and only if* $x \leq \underline{x} + \pi^{-1}\delta^{-T+1}(c_p + c_d)$*. Otherwise, the plaintiff's strategy is to accept any settlement proposal strictly greater than the value of the continuation game, and to reject any settlement proposal strictly worse than the value of the continuation game.*

Proof. The proof is essentially the same as that of Spier (1989, 1992) for closely related models, but is included in Appendix A.3 for completeness. □

The PBE for the first-period of bargaining recursively defines the PBE of the full game. Under maintained assumptions, every possible continuation game is simply a game of length $T' < T$, with the population of plaintiff types distributed uniformly on support determined by prior play. PBE strategies for continuation games starting in periods $t = 2, \ldots, T-1$ are thus given by Proposition 2; PBE strategies for continuation games starting in period T are given by Proposition 1.

Intuition for the boundary solution is analogous to that in the single-period game. When trial and negotiation costs are sufficiently large relative to the distribution of potential damages, the defendant can do no better than to make a settlement proposal just equal to the value of a trial verdict to the highest type plaintiff—i.e. an offer just large enough that no type of plaintiff can refuse it.

Understanding the interior solution is more challenging. As established in Lemma 1, symmetry of settlement preferences across plaintiff types requires that the plaintiff be indifferent between all equilibrium settlement proposals made in an interior solution. For such a sequence of settlement proposals to be sequentially rational, however, it must be that the defendant selects each proposal as an optimal response to changes in belief about the distribution of plaintiff types remaining in each period.

The sequence in which types of plaintiff settle is thus determined not by plaintiff preferences—in fact, it is premised on the plaintiff's indifference between all equilibrium proposals—but by the need to make this sequence of settlement proposals sequentially rational from the defendant's perspective.

Whether the interior or boundary solution is optimal depends on model parameters, the relationship between interior and boundary solutions being basically the same as discussed previously for a single-period game. Whereas the boundary solution predicts universal settlement in the first period of bargaining, the interior solution explains both delayed settlement and trial verdict dispositions as the result of rational equilibrium play. Because of these properties, theoretic predictions from the interior solution are the more relevant to analysis throughout the following chapters.

Although PBE strategies for the interior-solution equilibrium are rather opaque for a game of length $T > 1$, the equilibrium path of play is straightforward.

Corollary 1. *The interior-equilibrium sequence of settlement proposals is as follows:*

$$S_t^* = \begin{cases} \delta^T(\pi \underline{x} + k_d) + c_d \sum_{i=1}^{T-1} \delta^i & t = 1 \\ \delta^{-1} S_{t-1}^* + c_p & t = 2, \ldots, T. \end{cases}$$

When the plaintiff prefers settlement to trial, $U_p(S_1^*) \geq W_p(x)$*, settlement timing is a deterministic function of the plaintiff's type. A plaintiff of type* x *settles for proposal* S_t^* *where* t *is the unique period in which* $\underline{x}_t \leq x < \underline{x}_{t+1}$ *for*

$$\underline{x}_t = \begin{cases} \underline{x} & t = 1 \\ \underline{x}_{t-1} + \pi^{-1}\delta^{-T+t-1}(c_p + c_d) & t = 2, \ldots, T \\ \underline{x}_{t-1} + \pi^{-1}(k_p + k_d) & t = T + 1. \end{cases}$$

To illustrate with a concrete example, let $T = 10$, $\underline{x} = 75$, $\overline{x} = 215$, $\pi = 0.5$, $c_p = c_d = 1.6$, $k_p = k_d = 5$, and $\delta = 0.9$. The Corollary 1 equation for S_t^* gives a first-period equilibrium settlement proposal of $S_1^* = 23.64$. Plaintiff types that settle in equilibrium are those such that $U_p(S_1^* = 23.64) \geq W_p(x) \iff x \leq 196.20$. The timing of settlement is determined by the lower bounds given in the Corollary 1 equation for $\underline{x}_t$: $\underline{x}_1 = 75.00$, $\underline{x}_2 = 91.52$, $\underline{x}_3 = 106.39$, $\underline{x}_4 = 119.77$, $\underline{x}_5 = 131.81$, $\underline{x}_6 = 142.65$, $\underline{x}_7 = 152.40$, $\underline{x}_8 = 161.18$, $\underline{x}_9 = 169.08$, $\underline{x}_{10} = 176.20$, $\underline{x}_{11} = 196.20$. A plaintiff of type x settles in period t where $\underline{x}_t \leq x < \underline{x}_{t+1}$. For example, a plaintiff of type $x = 80$ settles for S_1^*, a plaintiff of type $x = 145$ settles for S_6^*, and a plaintiff of type $x = 190$ settles for S_{10}^*.

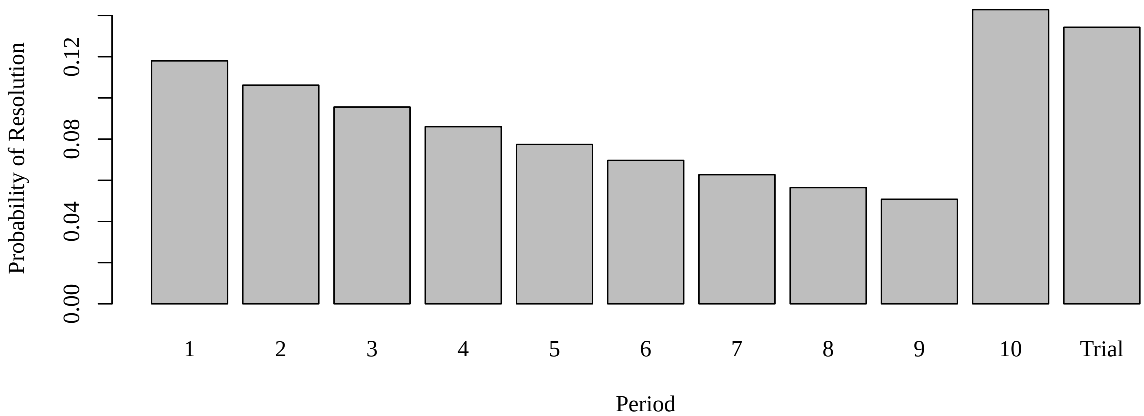
The *ex ante* probability of settlement in period $t = 1, \ldots, T$ of an interior equilibrium, p_t, is the measure of types that fall between $\underline{x}_t$ and $\underline{x}_{t+1}$ as defined in Corollary 1: $p_t = (\underline{x}_{t+1} - \underline{x}_t)/(\overline{x} - \underline{x})$. The probability of resolution by trial verdict in period $T + 1$ is the complement of the probability of settlement in some period.

Corollary 2. *Let p_t be the ex ante probability that a dispute is resolved in period t. In an interior equilibrium, p_t is defined as follows:*

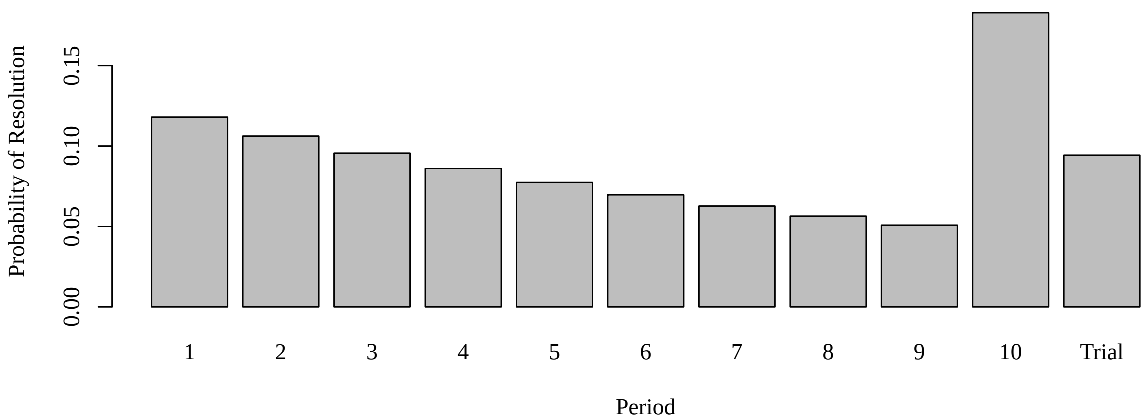
$$p_t = \begin{cases} \pi^{-1}\delta^{-T+t}(c_p + c_d)/(\overline{x} - \underline{x}) & t = 1, \ldots, T-1 \\ \pi^{-1}(k_p + k_d)/(\overline{x} - \underline{x}) & t = T \\ 1 - \sum_{i=1}^{T} p_i & t = T + 1. \end{cases}$$

Figure 4 illustrates the distribution of resolution time (p_t) for variations on the example parameter values given above. The decreasing probability of settlement over periods $t = 1, \ldots, T-1$ is a result of the discount factor δ in the top term of the equation for p_t in Corollary 2. With $\delta = 1$, the probability of settlement would be equal in all but the final period of bargaining. The dissimilarly of settlement probability in

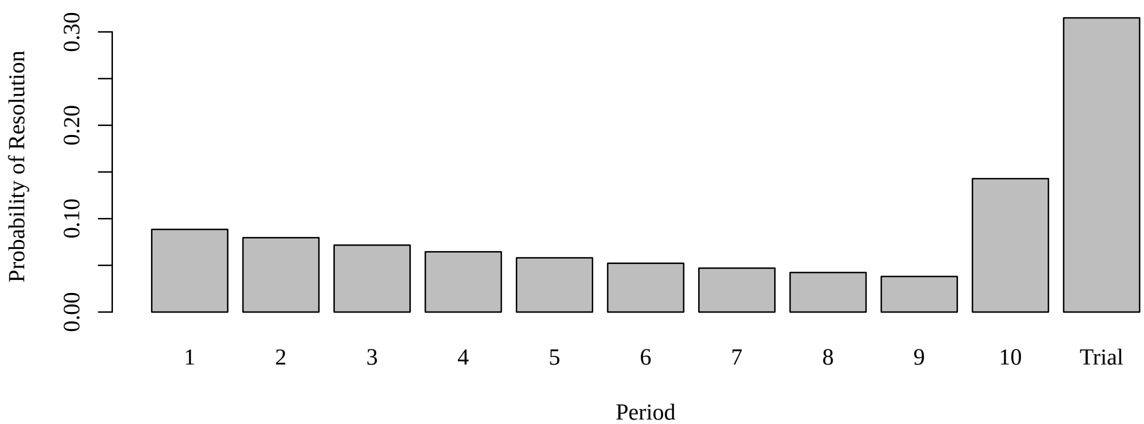
Figure 4: Illustration of Probability of Resolution by Period



(a) $T = 10$, $\underline{x} = 75$, $\overline{x} = 215$, $\pi = 0.5$, $c_p = c_d = 1.6$, $k_p = k_d = 5$, $\delta = 0.9$
(reference parameters)

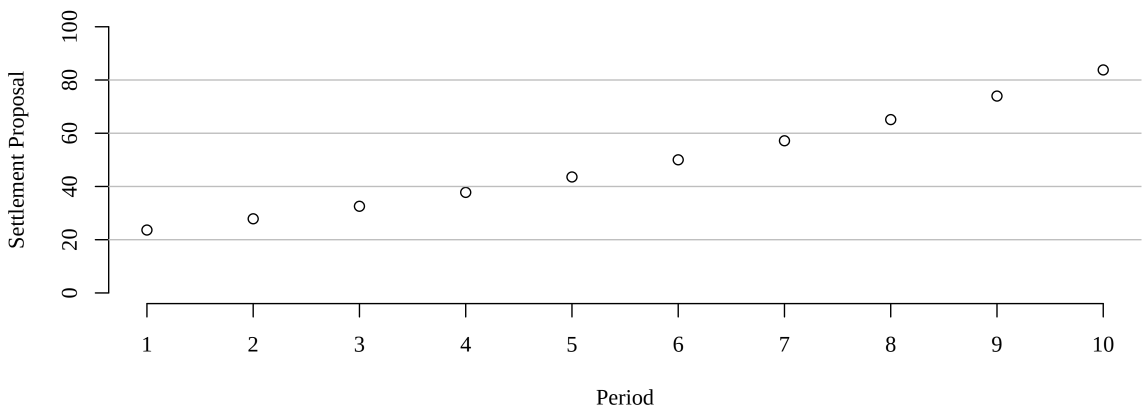


(b) $T = 10$, $\underline{x} = 75$, $\overline{x} = 215$, $\pi = 0.5$, $c_p = c_d = 1.6$, $\boldsymbol{k_p = k_d = 6.4}$, $\delta = 0.9$
(increased trial costs)

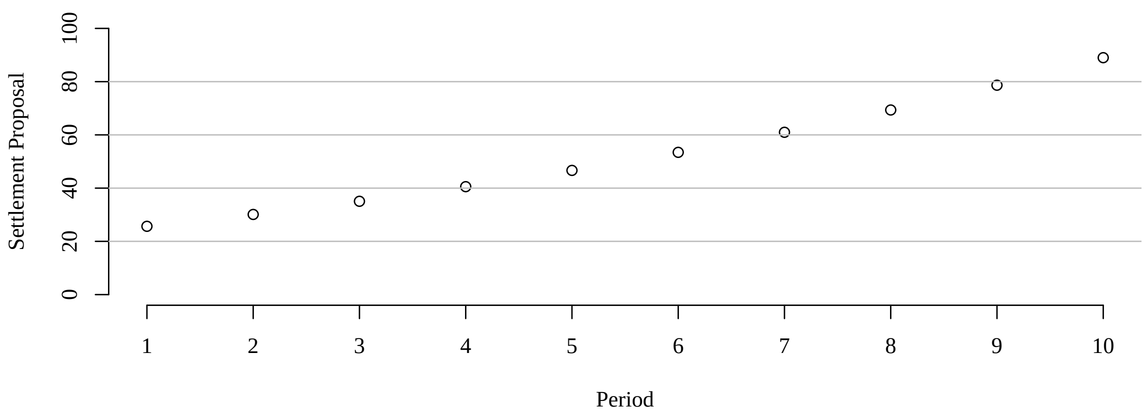


(c) $T = 10$, $\underline{x} = 75$, $\overline{x} = 215$, $\pi = 0.5$, $\boldsymbol{c_p = c_d = 1.2}$, $k_p = k_d = 5$, $\delta = 0.9$
(decreased negotiation costs)

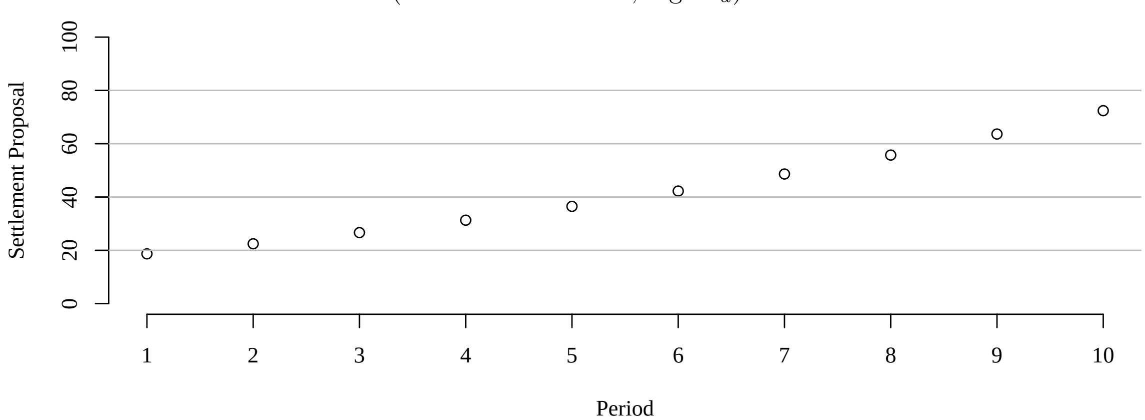
Figure 5: Illustration of Sequence of Equilibrium Settlement Proposals



(a) $T = 10$, $\underline{x} = 75$, $\overline{x} = 215$, $\pi = 0.5$, $c_p = 1.6$, $c_d = 1.6$, $k_p = 5$, $k_d = 5$, $\delta = 0.90$
(reference parameters)



(b) $T = 10$, $\underline{x} = 75$, $\overline{x} = 215$, $\pi = 0.5$, $c_p = 1.6$, $c_d = 1.6$, $\boldsymbol{k_p = 2}$, $\boldsymbol{k_d = 10.8}$, $\delta = 0.90$
(increased trial costs, high k_d)



(c) $T = 10$, $\underline{x} = 75$, $\overline{x} = 215$, $\pi = 0.5$, $\boldsymbol{c_p = 1.7}$, $\boldsymbol{c_d = 0.7}$, $k_p = 5$, $k_d = 5$, $\delta = 0.90$
(decreased negotiation costs, high c_p)

period T corresponds to the structural difference between this period (the one-period game immediately preceding a trial verdict) and those prior to it (multiple-period games preceding continuation games). In the final period of bargaining, the measure of plaintiff types that settle is a function of the measure of remaining types that prefer settlement to trial, rather than a function of the need to make a subsequent proposal sequentially rational. For comparable parameter values, Figure 5 illustrates the relatively unresponsive shape of equilibrium settlement proposal sequences (S_t^*).

A distinguishing characteristic of the interior-solution equilibrium is its robustness to an effectively continuous interpretation of the bargaining process. Unlike a wide class of common asymmetric information bargaining models (see, e.g., Gul and Sonnenschein, 1988), the prediction of settlement delay does not generally vanish as the duration of bargaining periods becomes arbitrarily small.

Proposition 3. *Consider transforming a game of length $T > 1$ into a game of $J > T$ bargaining periods a fraction T/J the normal duration. Provided that*

$$\delta^T[\pi(\overline{x} - \underline{x}) - (k_d + k_p)] - (c_d + c_p)\frac{\delta^T - 1}{\log \delta} > 0,$$

settlement delay persists as $J \to \infty$, making period granularity arbitrarily fine.

Proof. Provided in Appendix A.4. □

Intuition for this result is apparent in Proposition 2. The interior solution to a game of length $T > 1$ involves a positive probability of delay regardless of the number and duration of bargaining periods; only the boundary solution leads to a prediction of zero settlement delay. The condition for persistent delay in Proposition 3 is simply the limit, as the duration of bargaining periods becomes arbitrarily small, of the condition under which equilibrium involves the interior solution.

Sensitivity to period granularity is an important property of any model of settlement bargaining. Unlike other bargaining contexts, where negotiation might plausibly be limited to a small number of discrete interactions, legal bargaining is generally informal and unconstrained. Relative insensitivity of the Spier (1989, 1992) model to changes in period duration is exploited to construct continuous-time bargaining environments in the experimental design discussed in the following chapters.

3.3 Equilibrium with Symmetric Information

When the plaintiff and defendant are symmetrically informed about potential damages, an appropriate choice of equilibrium concept is Subgame Perfect Equilibrium (SPE).[32] For the settlement bargaining game with symmetric information, there exists a unique SPE in which all disputes settle in the first period of bargaining for exactly the plaintiff's expected net present value of a trial verdict. Only Assumption 1 is maintained in deriving this result.

Proposition 4. *In a game of length $T \geq 1$, with potential damages common knowledge, the defendant's SPE strategy is to make a first-period settlement proposal $S_1^*(x)$ such that $U_p(S_1^*(x)) = W_p(x)$:*

$$S_1^*(x) = \delta^T(\pi x - k_p) - \sum_{i=1}^{T-1} \delta^i c_p.$$

The plaintiff's strategy is to accept any settlement proposal such that $U_p(S_1) \geq W_p(x)$, and to otherwise reject.

Proof. The proof is essentially the same as that of Spier (1989, 1992), but is included in Appendix A.5 for completeness. □

[32]The SPE concept requires strategies to constitute a Nash Equilibrium in every subgame.

Intuition for the equilibrium in Proposition 4 is straightforward. In the final-period subgame, symmetric information over potential damages means the defendant can make an ultimatum proposal exactly equal to the plaintiff's discounted expected value of a trial verdict. The plaintiff always accepts such a proposal, and the defendant prefers this settlement to trial. In the penultimate-period subgame, the defendant can make a similar proposal, this time equal to the plaintiff's discounted valuation of settlement for the final-period proposal just derived. The plaintiff always accepts such a proposal, and the defendant prefers this settlement to continuation. By backward induction, the defendant's first-period proposal is exactly the plaintiff's expected net present value of a trial verdict. The plaintiff always accepts such a proposal, so settlement is never delayed in equilibrium.[33]

[33] Similar to the asymmetric information case, additional nuances such as risk aversion and disequal discount rates should not generally change the basic structure of the equilibrium.

4 Behavioral Caveats

Under reasonable refinements, equilibria to many structured bargaining models afford sharp predictions about optimal behavior. This is certainly true of the settlement bargaining model presented in Section 3. Under asymmetric information, theory predicts a specific sequence of settlement proposals, leading to a unique pattern of settlement with extensive delay. Under symmetric information, theory predicts universal first-period settlement with no delay at all. Either way, refinements and maintained assumptions afford a unique equilibrium path of play.

As is often true of bargaining models, the sharp predictions of Section 3 are bought at the cost of a strong reliance on model assumptions, a demanding concept of equilibrium, and a heavy dose of rationality. These are exacting requirements for which departures are inevitable in even the most tightly controlled laboratory environment. Observed behavior in laboratory experiments is thus likely to be much noisier than theory would predict.

While theoretic predictions are the focus of the following chapters, a pragmatic experimental design must anticipate and accommodate ways in which subject behavior may deviate from prediction. Toward this end, the remainder of this section comments on the behavioral economics of bargaining in a laboratory environment.[34] Section 4.1 reviews several behavioral regularities in the experimental and behavioral literatures on bargaining. Section 4.2 comments on how these regularities may (or may not) apply in a settlement bargaining context. Section 4.3 discusses implications for identification and experimental design.

[34]Consistent with Thaler (1992) and Camerer and Thaler (1995), I use *behavioral economics* to mean research centered on observed *regularities* or *anomalies* in behavior relative to typical neoclassical economic models. This contrasts with an alternative meaning: research explicitly focused on introducing elements of cognitive psychology to economic modeling. Comments are limited to bargaining *in a laboratory environment* insofar as cited literature mainly concerns observations from artificial bargaining environments rather than real-world negotiation settings.

4.1 Behavioral Regularities

Experimental data abound with bargaining behavior inconsistent with theoretic prediction.[35] Several such inconsistencies have been observed with sufficient frequency to become topics of study in their own right. Robust to various bargaining environments and more than the noisy actions of confused or inexperienced subjects, these *regularities* are powerful descriptors of certain aspects of observed bargaining behavior. Enumerated below are three behavioral regularities helpful in contextualizing the experimental analysis of the following chapters.

Regularity 1. *Responders often reject highly inequitable offers in favor of strictly lower payoffs than would have resulted from acceptance.*

In structured bargaining models with complete information—such as ultimatum, sequential-offer, and alternating-offer games—Subgame Perfect Equilibrium (SPE) requires responders to accept any offer that exceeds the expected monetary payoff of rejection. For example, in an ultimatum game to divide \$10 with \$0 default payments from disagreement, a responder is predicted to accept any offer of \$0.01 or more: a payout of as little as \$0.01 is still more than \$0, so the responder has nothing to gain from rejecting the offer. In defiance of this reasonable and intuitive rule, the first behavioral regularity is that offers in excess of the rejection value are frequently refused when the proposed division is highly inequitable to the responder.

This pattern of rejection was first observed in a one-period bargaining game by Güth et al. (1982), where it was noted that responders in ultimatum games opt for rejection, and receipt of a \$0 payoff, in favor of accepting small to even moderate offers that involve inequitable divisions. By framing the ultimatum game in terms of binding contingent demand questions, Kahneman et al. (1986) find the average

[35] See, e.g., surveys by Davis and Holt (1993), Roth (1995), and Güth (1995).

responder prefers a \$0 rejection payment (where the proposer also receives \$0) to acceptance of offers to keep as much as \$2 from a \$10 pie.

This regularity characterizes bargaining behavior in multi-period games as well. Summarizing several studies of alternating-offer games in which SPE implies first-offer acceptance, Ochs and Roth (1989, Table 6) observe first-offer rejection rates of around 15%, with as many as 65–86% of resulting counter-offers being *disadvantageous* in the sense that they propose a smaller monetary payoff than would have resulted from acceptance of the rejected offer. In a large and detailed experiment, Ochs and Roth (1989) find fully 81% of counter-offers to be disadvantageous.

Rejections inconsistent with payout maximization have proven robust to many manipulations of the bargaining environment. Such rejections are evidently not mitigated by learning through repeated play (Roth et al., 1991), and remain even when the scale of payoffs is increased (Forsythe et al., 1994; Hoffman et al., 1996).[36] Disadvantageous rejections have been observed in structured bargaining games played by subjects from a wide variety of diverse countries and ethnicities (Roth et al., 1991). Interestingly, such rejections are less frequent when the responder is informed that the offer is made by a computer rather than a human partner (Sanfey et al., 2003).

These results are commonly interpreted as an indication that the preferences of human bargainers are poorly approximated by monetary payoff alone (e.g. Ochs and Roth, 1989; Thaler, 1992, p. 23). In addition to monetary incentives, subjects in these experiments appear to be influenced by concerns about the social norm of fairness. For example, Sanfey et al. (2003) observe that both receipt and rejection of inequitable offers cause heightened neurological activity in an area of the brain associated with emotion. Kahneman et al. (1986) find that subjects in ultimatum

[36] But cf. Slonim and Roth (1998), providing evidence that with high payoffs, responders may learn to reject offers less frequently.

games are willing to incur personal cost in order to punish proposers of inequitable offers and reward proposers of fair divisions. Of course, if a responder's preferences include the proposer's payoff through some type of inequity aversion (see, e.g., Fehr and Schmidt, 1999), then a common payment of $0 may actually be preferred to a positive but inequitable division of the pie—meaning a rejection inconsistent with payout maximization may nevertheless be consistent with utility maximization.

While these experiential studies are cumulatively convincing that human bargainers care about the fairness of a bargaining outcome, exactly how the equity of an outcome is measured and processed remains frustratingly unclear. There is particular uncertainty over the mechanical definition of equity in a bargaining environment.[37] It may be, as Andreoni et al. (2003) conclude, that pronounced preference heterogeneity means no single model of inequity aversion adequately describes bargainer preferences. Concerns about fairness may also conflate with cognitive failures—such as the inability of responders to appropriately backward induct in multi-period games—further complicating the interpretation of experimental data. This behavioral regularity thus serves to discount a sharp theoretic prediction without providing a particularly clear behavioral prediction in its place.

Regularity 2. *Proposers often make offers more generous than payoff maximizing equilibria allow.*

In many structured bargaining games of complete information, SPE predicts proposers to offer the smallest possible amount that is greater than the responder's value of rejection. For example, in an ultimatum game to divide $10, with $0 default payments from disagreement, the proposer is predicted to offer $0.01; in an alternating-

[37] Goeree and Holt (2000) discuss some competing ideas of how fairness might influence preferences. The story becomes more complicated when more than two parties are involved in bargaining. For example, Güth and Van Damme (1998) conclude that responders do not obviously care about the payoffs of helpless third parties in a three-way division of a pie.

offer game, the first proposer is predicted to offer the smallest increment more than what the responder would net from being proposer in the second period of the game. In comparison to these theoretic predictions, the second behavioral regularity is that proposers often make surprisingly generous offers.

Studying behavior in ultimatum games, Güth et al. (1982) find the modal offer to be an equal (50/50) division, and the mean offer to be around 35% of the pie. Qualitatively similar results have been observed when subjects play the ultimatum game multiple times, when experiments are designed to ensure understanding through careful instructions and methodology (Kahneman et al., 1986; Forsythe et al., 1994), and when the game is played in different populations (Roth et al., 1991). The distribution of ultimatum offers is basically the same throughout: the modal offer is around a 40–50% portion of the pie, with most of the remaining support increasing in probability for offers on the range of 10–50% and a small frequency of offers for more than 50% of the pie.[38] Offer generosity does, however, appear sensitive to the context of property-rights (Hoffman et al., 1994), and to limitation of the proposer's choice set to exclude exactly equitable offers (Güth et al., 2001).

The regularity characterizes offers in multi-period alternating-offer games as well. As Davis and Holt (1993, pp. 270–273) and Goeree and Holt (2001) observe, first-proposer offers in two-round alternating-offer experiments tend to be close to SPE predictions when the predicted offer is approximately fair (e.g. 50–75% of the pie), but diverge from SPE predictions as the predicted offer becomes less fair. First-proposer offers tend to be too high in games where the SPE offer is lower than a fair

[38] The distribution of ultimatum offers varies under certain environments. For example, Marlowe (2004, p. 176 and Figures 6.2, 6.3) notes that in experiments conducted with certain small-scale societies (e.g. the hunter-gatherer Hadza of Tanzania), the distribution of offers can be skewed to the right, with a modal offer of as little as 15-20% of the pie. Offers from other small scale societies look more like those of developed countries. Ensminger (2004) comments that, in small-scale societies, ultimatum offer generosity increases with the market-integration of the proposer.

division, and tend to be too low in games where the SPE offer is higher than fair.[39] In alternating-offer games with more than two periods, Neelin et al. (1988) observe that average first-proposer offers continue to be more generous than predicted, though the effect of game length on offers is somewhat complicated.

The exact interpretation of this behavioral regularity is not immediately obvious. Generous offers are consistent with both fairness considerations on the part of the proposer, and with selfish payoff maximization if the proposer correctly foresees that highly inequitable offers will be rejected.[40] To disaggregate these factors, a number of studies compare offers between the ultimatum game, where responders can reject an offer, and the dictator game, where responders cannot reject an offer.

Average offers are lower in the dictator game, but surprisingly generous offers remain common. For example, in a dictator game to divide \$5, Forsythe et al. (1994) observe the modal offer of \$0 was made by 36% of proposers, but 30% of proposers still offered \$1, and just under 20% of proposers actually offered an equal division. Qualitatively similar observations have been made in a number of other dictator game experiments (e.g. Kahneman et al., 1986; Hoffman et al., 1994), but experiments conducted with small-scale societies have found fewer proposers willing to keep the entire pie (Ensminger, 2004; Marlowe, 2004). The context effects of greater anonymity and perceived social distance (Hoffman et al., 1994), and of a pie perceived to be earned (Cherry et al., 2002), result in substantially less generous offers.[41]

To summarize, previous study suggests that offers in bargaining games are often more generous than theory predicts. Neither inequity aversion nor strategic prediction of offer rejection seem individually capable of explaining offer generosity, but some

[39]In fact, in a particular design where SPE and fair offers are negatively correlated, Goeree and Holt (2000) observe that first-proposer offers track more closely with fair offers than with SPE offers.

[40]The case is even more complicated in multi-period (e.g. alternating-offer) games. Here the proposer may also need to consider limitations in the responder's capacity to backward induct.

[41]See also, discussion by Camerer and Thaler (1995).

combination of these factors and noisy decision-making appears promising (Goeree and Holt, 2000). From a practical perspective, this regularity affords the following behavioral observations: (i) an equitable division of the pie is highly "focal," and tends to dominate when bargainers are perceived to be equal in either a strategic or social sense, (ii) when equilibrium offers involve unfair divisions of the pie, deviations will usually be in the direction of a fair division.

Regularity 3. *Bargainers often fail to reach efficiency-improving agreements, and agreements are often reached only after inefficient delay.*

Nearly all bargaining games studied by economists begin with the assumption that gains exist from parties reaching (rapid) agreement. In the previously discussed ultimatum game to divide $10, for example, any agreement in which the responder collects more than $0 and less than $10 provides strictly greater payoffs to both parties than the $0 default payoff from disagreement. In multi-period bargaining games, where the size of the pie shrinks with each rejection, the cost of delay similarly implies that for any feasible division in subsequent periods, there exists an earlier division that is a Pareto improvement in payoffs.

When both parties prefer some division of the pie to the default payoff from disagreement, agreement seems intuitively certain. Similarly, in multi-period games with costly delay, it seems intuitive that agreements should always occur in the first period of bargaining. Both conclusions are true of the SPE for standard ultimatum, sequential-offer, and alternating-offer bargaining games, where neither delay nor disagreement is predicted along the equilibrium path of play. Contrary to intuition, however, the third behavioral regularity is that efficiency-improving agreements often fail to be reached, or are reached only after inefficient delay.

Experimental evidence of inefficient disagreement has already been presented. The rejections observed in one-period games in Regularity 1 all involve disagreements. Inefficient disagreements occur in multi-period bargaining games (e.g. Ochs and Roth, 1989) and non-structured bargaining games as well (Malouf and Roth, 1981; Roth, 1985). Although parties to a disagreement must certainly experience some degree of regret, knowing that a more preferred outcome was feasible but forgone, disagreements are not obviously mitigated by careful instructions or repeated play.

Partial evidence of costly delay has also been presented. Disagreements in multi-period games are necessarily preceded by delay, but games that end in agreement can also involve inefficient delay. Ochs and Roth (1989) and Neelin et al. (1988) provide experimental data on delayed agreements in alternating-offer games with two or more periods. In structured bargaining games with small delay costs, Güth et al. (2005) observe a *deadline effect*—where bargainers in games of finite length delay agreement until just before the game ends.[42]

As with previously discussed regularities, the interpretation of inefficient disagreement and delay is unclear. Roth (1985) interprets inefficient disagreements as coordination failures resulting from inconsistent expectations about the set of reasonable divisions. Roth proposes that bargaining games with highly "focal" offers—such as clearly equitable ways to divide the pie—should experience relatively few disagreements, while games without focal offers should experience more frequent bargaining failures. There is some experimental evidence for this hypothesis (Malouf and Roth, 1981; Roth, 1985). A related interpretation by Babcock et al. (1995) and Babcock and Loewenstein (1997) attributes disagreements to coordination failures where over-

[42]The deadline effect is a robust behavioral characteristic of bargaining in games without delay costs (Roth et al., 1988; Gneezy et al., 2003). In the long structured bargaining games explored by Güth et al. (2005), small delay costs appear to weakly attenuate the deadline effect relative to no delay cost.

confidence and self-serving assessment bias cause bargainers to form incompatible ideas about the set of equitable, likely, or reasonable bargaining outcomes.[43]

An alternative interpretation attributes disagreement and delay to information asymmetries. Recall, for example, that inefficient rejections are commonly attributed to inequity aversion. Non-monetary preferences can rationalize the responder's decision to reject unfair offers, but cannot explain the proposer's decision to make such offers in the first place: with complete information, the proposer should have foreseen that an inequitable offer would be rejected, and so should not have made it. By this reasoning, disagreement and delay might be seen as evidence of asymmetric information over preferences—a consequence of the experimenter's inability to fully control non-monetary preferences in the experiment (Kennan and Wilson, 1993).

The asymmetric information hypothesis is easy to appreciate: since the experimenters themselves are ignorant of their subjects' non-monetary preferences, why should subjects in the experiment be somehow better informed? Experiments designed to test whether asymmetric information can describe disagreement and delay range from supportive (Forsythe et al., 1991) to critical (Rapaport et al., 1995). Surveying several asymmetric information bargaining experiments, Roth (1995, pp. 312–322) finds limited support for the hypothesis.[44]

Similar to previously discussed regularities, experimental study provides clear evidence of inefficient disagreements and delays, without suggesting a clear motivation for this behavior. The predictive power of this regularity is thus negative: even in highly structured and controlled experiments with ostensibly complete information, disagreements and costly delays frequently result in inefficient bargaining outcomes.

[43]But cf. Galasso (2010), providing a model where overconfidence leads to more rapid agreement.

[44]Gul and Sonnenschein (1988) note that delay in many asymmetric information models vanishes as the time between offers becomes small—an important caveat for the external validity of this rationalization.

4.2 Application to Settlement Bargaining

Similarities between the settlement bargaining model described in Section 3.1 and ultimatum and sequential-offer bargaining models discussed in Section 4.1 make it tempting to apply behavioral regularities of the latter to the former. This would lead, for example, to a conclusion that settlement proposals should tend to be more generous than predicted by theory. While it is reasonable to think that these regularities may be instructive, several distinctive characteristics of settlement bargaining suggest behavior may be substantively different in this context.

For example, disputants in the settlement game are bargaining over a payment to be made from one party to the other, whereas agents in standard bargaining games negotiate the division of an exogenous pie. The defendant's *ownership* of the pie creates a contextual distinction: unlike the standard bargaining environment, where one party's gain is the other party's opportunity cost, the plaintiff's gain in settlement bargaining is a concrete cost paid out-of-pocket by the defendant. In the settlement context, it is not *a priori* obvious that a defendant will tend to make generous proposals. Cognitive biases such as endowment effects and loss aversion may cause a defendant to be less generous than agents in standard bargaining games.

There may also be a distinction in the concept of fairness between settlement bargaining and standard bargaining contexts. In standard bargaining games to divide an exogenous pie, the size of the pie is a focal part of the game and payoff equalization is obviously equitable. In the settlement bargaining game, the pie is abstractly the defendant's stock of resources (e.g. personal income) and is not a focal part of the game. It is unclear what defines an equitable offer in this context. Does a fair offer equalize payoffs, or just compensate the plaintiff for the injury sustained? In the latter case, does a fair offer compensate the plaintiff for the amount of the injury, or the amount of the expected trial award? While a plaintiff may reject inequitable proposals in accordance with Regularity 1, it is not *a priori* obvious what an equitable proposal looks like in a settlement bargaining context.

Default payoffs from rejection are also different between settlement bargaining and more standard bargaining games. In ultimatum, sequential-offer, and other bargaining games commonly studied in laboratory experiments, the default payoff from rejection is usually $0 for both agents: neither party has any monetary incentive to disagree. In the settlement bargaining context, default payoffs reflect the outcome of a trial verdict with a positive transfer of wealth in expectation. Asymmetric, non-zero, and negative (for the defendant) default payoffs are more complicated than uniform $0 payoffs, and may interact differently with known cognitive biases such as loss aversion, overconfidence, and optimism bias. It is not *a priori* obvious how this may affect the frequency of disagreement in a settlement bargaining context relative to more standard bargaining games.

4.3 Implications for Experimental Design

Research outlined in Section 4.1 shows that even in simple bargaining games of complete information, deviations from theoretic prediction are common. Disagreements and inefficient outcomes are pervasive, and bargainers frequently adopt strategies inconsistent with payoff maximization. Though Section 4.2 suggests that these behavioral regularities need not directly characterize behavior in a settlement bargaining context, the lesson is still apt that substantial deviations from theory are likely.

As discussed in Section 4.1, many behavioral regularities appear to result from a failure to fully control preferences and information in laboratory bargaining experiments. Payoff-inefficient rejections, for example, appear motivated by a taste for fairness that is present in subject preferences, but is typically absent from neoclassical bargaining models. Inefficient outcomes are likewise hypothesized to result from asymmetric information over bargaining preferences. Such information asymmetry is not controlled by the experimenter, and is often an unwanted aspect of the experimental design.

Lack of control is actually desirable when simply testing the point predictions of a theoretic model. For example, experiments on ultimatum and alternating-offer games have revealed standard payout-maximizing models provide poor predictions of behavior in the lab. When the research question is merely whether theory provides accurate predictions, hypothesis tests need only contrast predicted and observed outcomes.

On the other hand, lack of experimental control complicates experimental tests of more nuanced aspects of theory than simple point prediction. With uncontrolled preferences, apparently irrational rejections in ultimatum games can result from either a failure of the SPE concept, a failure of predictions to appropriately model preferences over payoff inequities, or some combination of the two. Inefficient outcomes in complete information bargaining games are likewise unsurprising if preference heterogeneity means that strategy-relevant information is actually incomplete.

While laboratory experiments offer greater control than comparable field and natural experiments, it seems unlikely that any realistic experimental design could fully control the preferences and information of the voluntary human subjects from which data are collected. In most cases, tests of bargaining theory face the basic identification challenge of separating failures of theoretic mechanics (e.g. an equilibrium refinement that does not accurately represent strategy or behavior) from failures of assumption (e.g. an incorrect assumption that subjects have common risk preferences). A degree of pragmatism is therefore needed in both the design and interpretation of bargaining experiments intended to test specific aspects of theoretic predictions.

As a concrete example, the observation of delayed agreements in a multiple-period settlement bargaining game with asymmetric information does not necessarily signal that delayed agreement was the result of controlled asymmetric information. Even if the theoretic hypothesis that asymmetric information drives delayed agreement were true, delayed agreements could result from a similar game with complete information if preferences and information are insufficiently controlled. In this case, a more appropriate test of theory may be to compare observed outcomes in treatments where the controlled aspect of information is asymmetric, with outcomes in treatments where it is complete. If the only difference between treatments is the presence or absence of an information asymmetry, then the empirical observation of greater delay with asymmetric information would seem consistent with the theoretic hypothesis that asymmetric information causes settlement delay.

Similar identification arguments will be discussed in the following chapters, but the general motivation for such careful interpretation can be summarized succinctly. Behavioral analysis of bargaining games reveals that many behavior-relevant factors remain unknown, uncontrolled, or both. Even direct tests of theory must therefore take care in determining what is identified by observed deviations from prediction.

A Technical Appendix

A.1 Proof of Proposition 1

Proof. The proof is essentially the same as those of Bebchuk (1984) and Spier (1992) for closely related models.

Begin with the plaintiff's strategy. In choosing whether to accept or reject the settlement proposal S_1, the plaintiff's optimal strategy must be to reject the proposal if and only if the expected net present value of a trial verdict exceeds the value of settlement. That is, a plaintiff of type x cannot credibly reject a proposal of S_1 unless $U_p(S_1) < W_p(x)$. To break ties, assume a plaintiff indifferent between settlement and trial chooses to settle.

In a PBE, beliefs are consistent with strategies along the equilibrium path. With complete information, the plaintiff has trivial beliefs over singleton information sets. The defendant lacks complete information about the value of potential damages and so maintains a non-degenerate belief profile over the plaintiff's type. The defendant's initial beliefs weight plaintiff types according to the population density: $\rho(x) = f(x)$. If S_1 is rejected, however, beliefs update to $\rho(x|S_1)$ in the following manner.

Under the above strategy, a plaintiff of type x rejects settlement if and only if a trial verdict is preferred to settlement. Expanding and rearranging the inequality $U_p(S_1) < W_p(x)$ reveals a rejecting plaintiff to have type $x > \pi^{-1}(\delta^{-1}S_1 + k_p)$. For notational convenience, let $\underline{x}_2(S_1) = \pi^{-1}(\delta^{-1}S_1 + k_p)$ denote the cutoff between the highest-type plaintiff that would just accept proposal S_1, and the lowest-type plaintiff that would just reject.[45] The defendant's updated beliefs following rejection of S_1 are accordingly the truncated density $\rho(x|S_1) = f(x|x > \underline{x}_2(S_1))$.

[45]Imprecise interpretation of $\underline{x}_2(S_1)$ is excused by the assumption that $F(x)$ is a continuous distribution. Notation underscores that $\underline{x}_2(S_1)$ is analogous to $\underline{x}$ in the period following rejection: i.e. $\underline{x}_2(S_1)$ is a lower bound on the support of plaintiff types remaining after rejection of S_1.

Let $V_d(S_1)$ denote the defendant's expected valuation of the dispute resolution resulting from a proposal of S_1:

$$\begin{aligned} V_d(S_1) &= \mathrm{P}\left[x \leq \underline{x}_2(S_1)\right] U_d(S_1) + \mathrm{P}\left[x > \underline{x}_2(S_1)\right] \mathrm{E}\left[W_d(x) | x > \underline{x}_2(S_1)\right] \qquad (4) \\ &= F(\underline{x}_2(S_1))\, U_d(S_1) + (1 - F(\underline{x}_2(S_1))) \int_{\underline{x}_2(S_1)}^{\overline{x}} W_d(x)\rho(x|S_1)\, dx \\ &= -F(\pi^{-1}(\delta^{-1}S_1 + k_p))(S_1 + c_d) - \int_{\pi^{-1}(\delta^{-1}S_1+k_p)}^{\overline{x}} (\delta(\pi x + k_d) + c_d)\, f(x)\, dx. \qquad (5) \end{aligned}$$

The first term in equation (4) is the defendant's valuation of settlement at S_1 weighted by the measure of plaintiff types that accept S_1. The second term is the expected net present value of a trial verdict given that the plaintiff is a type that rejects S_1, weighted by the measure of types that reject S_1. Equation (5) has the same interpretation, following from (4) by simple term-expansion and cancellation.

The defendant selects a settlement proposal S_1 in order to maximize $V_d(S_1)$. At an interior solution, the FOC for (5) implicitly characterizes the optimal proposal:

$$S_1^I: \quad -F(\pi^{-1}(\delta^{-1}S_1^I + k_p)) + \pi^{-1}(k_d + k_p) f(\pi^{-1}(\delta^{-1}S_1^I + k_p)) = 0. \qquad (6)$$

The FOC will be assumed to locate a unique maximum, though in practice this will depend on the specification of $F(x)$.[46] All distributions discussed in these essays admit a unique interior maximum at S_1^I.

In addition to the interior solution, two boundary conditions on S_1^* must be considered: an equilibrium settlement proposal must satisfy $W_p(\underline{x}) \leq U_p(S_1^*) \leq W_p(\overline{x})$. The lower bound prohibits equilibrium proposals that are rejected by every type of

[46]See Spier (1992) for general conditions under which a unique maximum obtains.

plaintiff, $W_p(\underline{x}) \leq U_p(S_1^*) \iff S_1^* \geq \delta(\pi\underline{x} - k_p)$, but never binds since every interior solution entails settlement with a positive measure of plaintiff types. This result follows from the definition of defendant preferences in equations (1) and (2), as a rational defendant always settles with at least the lowest-type plaintiff in order to recoup own and opponent court costs: $U_d(\delta(\pi\underline{x} - k_p)) = W_d(\underline{x}) + \delta(k_p + k_d) > W_d(\underline{x})$.

The upper bound binds when trial costs are sufficiently high that the defendant can do no better than settle with every type of plaintiff in order to avoid any possibility that the dispute proceed to trial. In this case, the defendant makes a settlement proposal S_1^B such that $U_p(S_1^B) = W_p(\overline{x})$, so the highest-type plaintiff is just indifferent between settlement and trial; expanding and rearranging the equality yields

$$S_1^B = \delta(\pi\overline{x} - k_p). \tag{7}$$

The equilibrium proposal depends on parameter values. When $V_d(S_1^I) \geq U_d(S_1^B)$, the defendant prefers the interior solution—balancing the marginal benefit of a lower settlement proposal against the marginal cost of more frequent trial outcomes—and accordingly proposes $S_1^* = S_1^I$. When $V_d(S_1^I) < U_d(S_1^B)$, litigation costs are sufficiently high that the defendant can do no better than to recoup such costs by settling with every type of plaintiff and so proposes $S_1^* = S_1^B$.

For continuous $F(x)$, $V_d(S_1)$ is continuous at the boundary solution S_1^B, so the interior solution is preferred identically when the interior proposal is less than the boundary proposal: $V_d(S_1^I) \geq U_d(S_1^B) \iff S_1^I \leq S_1^B$.[47] This allows S_1^* to be expressed parsimoniously:

$$S_1^* = \min\{S_1^I, S_1^B\}. \tag{8}$$

□

[47] See Section 3.2, Figure 3, for an illustration of this relationship.

A.2 Proof of Lemma 1

Proof. Each part of the proposition

1. $U_p(S_1^*) \geq \ldots \geq U_p(S_T^*)$ in any equilibrium,

2. $U_p(S_1^*) \leq \ldots \leq U_p(S_T^*)$ in any equilibrium where not all types of plaintiff settle,

is demonstrated by induction, starting with a game of length $T = 2$.

Verification Step: $\boldsymbol{T = 2}$

1. The claim that $U_p(S_1^*) \geq U_p(S_2^*)$ in any equilibrium is established by contradiction. Suppose the reverse: $U_p(S_1^*) < U_p(S_2^*)$. Because settlement preferences are not type-dependent (i.e. do not depend on a plaintiff's type in terms of potential damages), all types of plaintiff prefer settlement at S_2^* over S_1^*. Any plaintiff that decides to settle will therefore reject S_1^* and settle for S_2^*.

   Now define an alternative first-period proposal, S_1', such that the plaintiff is indifferent between S_1' and S_2^*: i.e. $U_p(S_1') = U_p(S_2^*) \iff S_1' = \delta(S_2^* - c_p)$. The plaintiff is indifferent between S_1' and S_2^* by construction, but the defendant strictly prefers settlement at S_1', since earlier settlement allows the defendant to recoup the delay costs that would otherwise be paid in the second period of bargaining: $U_d(S_1') = U_d(S_2^*) + \delta(c_p + c_d) > U_p(S_2^*)$.

   If S_1' would be accepted by a positive measure of plaintiff types, then feasibility of S_1' means that any sequence of proposals $\{S_1^*, S_2^*\}$ such that $U_p(S_1^*) < U_p(S_2^*)$ cannot be a best response.[48] Thus $U_p(S_1^*) < U_p(S_2^*)$ cannot hold in equilibrium, and it must be that $U_p(S_1^*) \geq U_p(S_2^*)$ in any equilibrium.

[48] Though ancillary to discussion and so not proven here, the requirement that S_1' be accepted with positive probability will in practice be satisfied in every interesting equilibrium.

2. The claim that $U_p(S_1^*) \leq U_p(S_2^*)$ in any equilibrium where not all types of plaintiff settle is also established by contradiction. Suppose the reverse: $U_p(S_1^*) > U_p(S_2^*)$ with a positive measure of plaintiff-types rejecting both proposals. Settlement preferences are not type-dependent, so all types of plaintiff prefer settlement at S_1^* over settlement at S_2^*. Any plaintiff that settles will accept S_1^*, and any plaintiff that rejects S_1^* will also reject S_2^*.

   The continuation game starting in period $t = 2$ is reached with positive probability, since a positive measure of plaintiff types reject S_1 by assumption. This continuation game is just a game of length $T = 1$ with the distribution of plaintiff types adjusted to remove those types that settled for S_1^*. By Proposition 1 the defendant's optimal strategy in the second period is to make a settlement proposal S_2^* that is accepted by a positive measure of the remaining types of plaintiff. But a proposal sequence $\{S_1^*, S_2^*\}$ such that $U_p(S_1^*) > U_p(S_2^*)$ fails to induce any plaintiff to settle for S_2^*. A contradiction being reached, it must be that $U_p(S_1) \leq U_p(S_2)$ in any equilibrium where not all types of plaintiff settle.

Inductive Step

Suppose the proposition holds for a game of length T. The following shows that it must also hold for a game of length $T + 1$.

1. In a game of length $T + 1$, the continuation game starting in the second period is just a game of length T with the population of plaintiff types adjusted to remove types that settle for S_1. By the assumption that the proposition holds for a game of length T, the sequence of proposals in the continuation game must conform to the inductive hypothesis: $U_p(S_2^*) \geq \ldots \geq U_p(S_{T+1}^*)$. This means the proposition only remains to be established for S_1^*.

To show $U_p(S_1^*) \geq \ldots \geq U_p(S_{T+1}^*)$ in any equilibrium, suppose the reverse: $U_p(S_1^*) < U_p(S_2^*) \geq \ldots \geq U_p(S_{T+1}^*)$. From this point on the proof parallels that given in the verification step. Because settlement preferences are not type-dependent, any plaintiff that decides to settle will reject S_1^* and settle for some subsequent proposal valued at $U_p(S_2^*)$.[49]

Define an alternative first-period proposal, S_1', such that the plaintiff is indifferent between S_1' and S_2^*: i.e. $U_p(S_1') = U_p(S_2^*) \iff S_1' = \delta(S_2^* - c_p)$. The plaintiff is indifferent between S_1' and S_2^*, but the defendant strictly prefers settlement at S_1', as explained in the verification step. Assuming S_1' would be accepted by a positive measure of plaintiff types, feasibility of S_1' means that any sequence of proposals such that $U_p(S_1^*) < U_p(S_2^*) \geq \ldots \geq U_p(S_{T+1}^*)$ cannot be a best response, and it must be that $U_p(S_1^*) \geq \ldots \geq U_p(S_{T+1}^*)$ in any equilibrium.

2. As the continuation game starting in the second period of a game of length $T+1$ is itself a game of length T, the continuation-game sequence of proposals conforms to both the inductive hypothesis, $U_p(S_2^*) \leq \ldots \leq U_p(S_{T+1}^*)$, and the first proposal in this Lemma (already shown to hold in every equilibrium), $U_p(S_2^*) \geq \ldots \geq U_p(S_{T+1}^*)$. This shows $U_p(S_2^*) = \ldots = U_p(S_{T+1}^*)$, which only leaves the proposition to be established for S_1^*.

   To show that $U_p(S_1^*) \leq \ldots \leq U_p(S_{T+1}^*)$ in any equilibrium where not all types of plaintiff settle, suppose the reverse: $U_p(S_1^*) > U_p(S_2^*) = \ldots = U_p(S_{T+1}^*)$ with a positive measure of plaintiff-types rejecting every proposal. The remainder of the proof parallels that given in the verification step. Since settlement preferences are not type-dependent, any type of plaintiff that settles will accept S_1^*, and any plaintiff that rejects S_1^* will also reject every subsequent proposal.

[49] Awkward language is the result of weak inequalities in the inductive hypothesis.

Given that a positive measure of plaintiff types reject every proposal up to S_T^*, the continuation game starting in period $T+1$ is reached with positive probability. This continuation game is just a game of length $T = 1$ with the distribution of plaintiff types adjusted to remove those types that settled for S_1^*. By Proposition 1 the defendant's optimal strategy in this final period is to make a proposal that is accepted by a positive measure of remaining types, but a proposal sequence such that $U_p(S_1^*) > U_p(S_2^*) = \ldots = U_p(S_{T+1}^*)$ fails to induce any further settlement. It therefore must be that $U_p(S_1^*) \leq \ldots \leq U_p(S_{T+1}^*)$ in any equilibrium where not all types of plaintiff settle.

□

A.3 Proof of Proposition 2

Proof. The proof is essentially the same as that of Spier (1989, 1992) for closely related models. The proposition is established by induction starting with a game of length $T = 2$.

Verification Step: $T = 2$

Begin with the plaintiff's strategy: decision rules for accepting and rejecting values of the first-period settlement proposal S_1. Three cases must be considered. First, if the expected net present value of the continuation game following rejection exceeds the value of settling at the first-period proposal S_1, the plaintiff's optimal strategy must be to reject S_1. Second, if the expected net present value of the continuation game is exceeded by the value of settling at S_1, the plaintiff's optimal strategy must be to accept S_1. The remainder of the plaintiff's equilibrium strategy concerns the third case, where the plaintiff is indifferent between accepting and rejecting S_1.

Let $S_2^*(S_1)$ denote the equilibrium settlement proposal that the defendant would make in the second period of the game if the first-period proposal S_1 were rejected. A plaintiff is indifferent between accepting and rejecting the first-period settlement proposal when $U_p(S_1) = \max\{U_p(S_2^*(S_1)), W_p(x)\}$: that is, when settlement at S_1 yields the same payoff as the better of settlement in the continuation game, or receipt of a trial verdict. Similar to the assumption made in proving Proposition 1, break ties by assuming a plaintiff indifferent between settlement and trial chooses to settle in some period.

The tie-breaking assumption compels first-period settlement when indifference concerns only settlement at S_1 or rejection in favor of an eventual trial verdict: i.e. when $U_p(S_1) = W_p(x) > U_p(S_2^*(S_1))$. When indifference concerns settlement across multiple periods, i.e. $U_p(S_1) = U_p(S_2^*(S_1)) \geq W_p(x)$, the plaintiff's preferences require settlement in some period, but do not alone specify *which* period. Timing of acceptance in this aspect of the plaintiff's equilibrium strategy is dictated by the PBE concept, which restricts the plaintiff's strategy to prescribe a pattern of settlement for which the defendant's strategy is sequentially rational.[50] This point will be revisited after the interior-solution to the defendant's equilibrium strategy is derived.

For the defendant's strategy, begin by considering two potential boundary solutions: (i) a solution in which no types of plaintiff settle, and (ii) a solution in which all types of plaintiff settle. There exist no equilibria of the first type, as a positive measure of plaintiff types must settle in every equilibria. This result was established for a single-period game in Proposition 1, and generalizes to a two-period game because the continuation game reached in the second period is itself just a single-period game and therefore characterized by Proposition 1.

[50]This is intuitively comparable to a randomized equilibrium, where randomization is only possible as a consequence of the randomizing player's indifference between actions, and where behavior of the randomizing player is defined by the need to make the opposing player's actions optimal.

Next consider a solution in which all types of plaintiff eventually settle. By the first proposition of Lemma 1, no equilibrium proposal can be more valuable to a plaintiff than S_1^*: $U_p(S_1^*) \geq \ldots \geq U_p(S_T^*)$ in any equilibrium. Thus, if all plaintiff types eventually settle, it must be that S_1^* is as valuable as the expected net present value of a trial verdict to the highest-type plaintiff: $U_p(S_1^B) = W_p(\overline{x})$.[51] Substituting terms and solving for S_1^B yields the equilibrium settlement proposal for the boundary solution in which all types of plaintiff settle:

$$S_1^B = \delta^2(\pi\overline{x} - k_p) - \delta c_p. \tag{9}$$

All types of plaintiff immediately accept S_1^B under Assumption 3, so the value of the boundary solution to the defendant is $U_d(S_1^B)$.[52]

With the boundary solution out of the way, consider the interior solution where some but not all types of plaintiff settle. A convenient way to construct the equilibrium is by working backwards from the continuation game following rejection of S_1. Since some types of plaintiff never settle by assumption, the second-period continuation game is necessarily reached with positive probability in an interior equilibrium.

Assumption 2 restricts the plaintiff's strategy to be monotone in type, so in any interior solution there exists some cutoff type $\underline{x}_2(S_1)$ under which types of plaintiff accept S_1 and above which types of plaintiff reject S_1. Note also that since Assumption 4 specifies the population distribution of plaintiff types $\rho(x) = f(x)$ to be uniform on $[\underline{x}, \overline{x}]$, existence of a cutoff type means the distribution of plaintiff types remaining in the second-period continuation game, $\rho(x|S_1) = f(x|x > \underline{x}_2(S_1))$, is simply the uniform distribution with support $[\underline{x}_2(S_1), \overline{x}]$.

[51] All plaintiff types would also settle for $U_p(S_1^*) > W_p(\overline{x})$, but this would not be an equilibrium as the defendant could profitably deviate in the direction of a lower first-period proposal.

[52] Assumption 3 is a refinement ruling out unintuitive boundary equilibria where, e.g., all types of plaintiff reject every value of S_1 including S_1^B but accept a proposal S_2 such that $U_p(S_2) = U_p(S_1^B)$.

Optimal play in the continuation game can be expressed as a function of S_1 alone. The continuation game is a one-period game to which Proposition 1 applies, simplified by the assumption that potential damages are uniform on $[\underline{x}_2(S_1), \overline{x}]$. Substituting the distributional specification into the interior solution to Proposition 1 yields the optimal continuation game settlement proposal as a function of S_1:

$$S_2^*(S_1) = \delta(\pi \underline{x}_2(S_1) + k_d). \tag{10}$$

The cutoff type at which the equilibrium continuation-game proposal $S_2^*(S_1)$ is just rejected, $\underline{x}_3(S_1)$, is also given by Proposition 1 with $\underline{x}_2(S_1)$ substituting for $\underline{x}$:

$$\underline{x}_3(S_1) = \pi^{-1}(\delta^{-1} S_2^*(S_1) + k_p). \tag{11}$$

Finally, the value of $\underline{x}_2(S_1)$ can be derived from optimal play in the continuation game. Note that since some but not all types of plaintiff settle in an interior equilibrium, both propositions of Lemma 1 apply to the sequence of equilibrium settlement proposals: i.e. $U_p(S_1^*) \geq U_p(S_2^*)$ and $U_p(S_1^*) \leq U_p(S_2^*)$. Combining the Lemma 1 implication that $U_p(S_1) = U_p(S_2^*(S_1)) \iff S_2^*(S_1) = \delta^{-1} S_1 + c_p$ with the specification of $S_2^*(S_1)$ in equation (10) allows $\underline{x}_2(S_1)$ to be solved in terms of S_1:

$$\underline{x}_2(S_1) = \pi^{-1}(\delta^{-2} S_1 + \delta^{-1} c_p - k_d) \tag{12}$$

The above terms can be used to represent the defendant's problem as a function of S_1 alone. As expressed in equation (13), the defendant chooses a value of S_1 in order to maximize the sum of (i) the value of settlement at S_1, weighted by the measure of plaintiff types that accept S_1, (ii) the value of settlement at $S_2^*(S_1)$, weighted by the measure of plaintiff types that reject S_1 but accept $S_2^*(S_1)$, and (iii) the expected net present value of a trial verdict conditional on the plaintiff being a type that rejects both S_1 and $S_2^*(S_1)$, weighted by the measure of types that reject both proposals:

$$\begin{aligned} V_d(S_1) &= \mathrm{P}\left[x \leq \underline{x}_2(S_1)\right] U_d(S_1) \\ &\quad + \mathrm{P}\left[\underline{x}_2(S_1) < x \leq \underline{x}_3(S_1)\right] U_d(S_2^*(S_1)) \\ &\quad + \mathrm{P}\left[x > \underline{x}_3(S_1)\right] \mathrm{E}\left[W_d(x) | x > \underline{x}_3(S_1)\right] \end{aligned} \tag{13}$$

$$\begin{aligned} &= \frac{\underline{x}_2(S_1) - \underline{x}}{\overline{x} - \underline{x}} \left[-S_1 - c_d\right] \\ &+ \frac{\underline{x}_3(S_1) - \underline{x}_2(S_1)}{\overline{x} - \underline{x}} \left[-\delta S_2^*(S_1) - c_d - \delta c_d\right] \\ &+ \frac{\overline{x} - \underline{x}_3(S_1)}{\overline{x} - \underline{x}} \left[-\delta^2 \left(\pi \frac{\overline{x} + \underline{x}_3(S_1)}{2} - k_d\right) - c_d - \delta c_d\right]. \end{aligned} \tag{14}$$

Equation (14) follows from (13) by definition of the uniform distribution and expansion of defendant valuation terms.

It is possible but tedious to mechanically derive the FOC for maximization of equation (14) by simply taking the derivative of every term with respect to S_1. An easier approach invokes the envelope theorem with respect to $S_2^*(S_1)$ and $\underline{x}_3(S_1)$, capitalizing on the definition of $S_2^*(S_1)$ as optimal behavior in the continuation game following rejection of S_1.[53] Either way, the FOC provides a simple expression for the interior-solution equilibrium proposal S_1^I:

$$S_1^I = \delta^2(\pi \underline{x} + k_d) + \delta c_d. \tag{15}$$

[53] Abusing notation, let $V_d(S_1, S_2)$ represent the defendant's objective function over simultaneous choice of both S_1 and S_2, and let $S_2^*(S_1)$ be the argmax of $V_d(S_1, S_2)$ with respect to S_2. If $V_d(S_1) = V_d(S_1, S_2^*(S_1))$, the envelope theorem provides a simplifying result:

$$\frac{dV_d(S_1)}{dS_1} = \left.\frac{\partial V_d(S_1, S_2)}{\partial S_1}\right|_{S_2 = S_2^*(S_1)}.$$

This result also encompasses $\underline{x}_3(S_1)$, itself a simple function of $S_2^*(S_1)$ as defined in equation (11).

To complete the interior solution, it remains to establish the plaintiff's equilibrium strategy for the timing of settlement in cases where the plaintiff prefers settlement to trial, but is indifferent between accepting any of multiple settlement proposals: i.e. $U_p(S_1^I) = U_p(S_2^*(S_1^I)) \geq W_p(x)$. Evaluating the definition of $\underline{x}_2(S_1)$ in equation (12) at S_1^I provides the upper bound on types that accept S_1^I in an interior equilibrium:

$$\underline{x}_2 = \underline{x} + \pi^{-1}\delta^{-1}(c_p + c_d). \tag{16}$$

The equilibrium strategy of a plaintiff of type $x \leq \underline{x}_2$ is accordingly to accept S_1^I. The strategy of plaintiff types $x > \underline{x}_2$ is to reject S_1^I, either in favor of subsequent settlement for the equally preferred second-period proposal $U_p(S_2^*(S_1^I))$, or in favor of an eventual trial verdict.[54]

Note again that the timing of settlement is not a result of plaintiff preferences—in fact, it is premised on the plaintiff's indifference between settlement in either period. Equilibrium rules for settlement timing tailor the support of plaintiff types remaining in each period so that satisfaction of Lemma 1, requiring $U_p(S_1^I) = U_p(S_2^*(S_1^I))$, is a natural consequence of sequentially rational play by the defendant.

Similar to the one-period game, the equilibrium first-period settlement proposal in the two-period game depends on parameter values. When $V_d(S_1^I) \geq U_d(S_1^B)$, the defendant prefers the interior solution—balancing the marginal benefit of a lower settlement proposal against the marginal cost of bargaining and more frequent trial outcomes—and accordingly proposes $S_1^* = S_1^I$. When $V_d(S_1^I) < U_d(S_1^B)$, bargaining and trial costs are sufficiently high that the defendant can do no better than to recoup costs by settling with all types of plaintiff and so proposes $S_1^* = S_1^B$.

[54]Types of plaintiff that reject S_1^I divide into two classes. A plaintiff of type $\underline{x}_2 < x \leq \underline{x}_3(S_1^I)$ rejects S_1^I, but subsequently accepts the equally preferred second-period proposal, $S_2^*(S_1^I)$. A plaintiff of type $x > \underline{x}_3(S_1^I)$ rejects both S_1^I and $S_2^*(S_1^I)$ in favor of a trial verdict.

Since $V_d(S_1)$ is continuous at the boundary solution, S_1^B, the interior solution is preferred identically when the interior proposal is less than the boundary proposal: $V_d(S_1^I) \geq U_d(S_1^B) \iff S_1^I \leq S_1^B$. This allows S_1^* to be expressed parsimoniously:

$$S_1^* = \min\{S_1^I, S_1^B\}. \tag{17}$$

It is simple to verify by substitution and simplification that all terms defined in solving the game of length $T = 2$ adhere to the general definitions provided in Proposition 2.

Inductive Step

Inductive logic is used to demonstrate the *interior* solution to a general multi-period game. Though grouped under the heading of the inductive step for narrative convenience, all other aspects of the equilibrium can be derived without induction. To fit the framework of an inductive proof, these aspects of the equilibrium (including much of the plaintiff's strategy and the boundary solution where all types of plaintiff settle) are established directly for a game of length $T + 1$.

Begin with the plaintiff's equilibrium strategy in a game of length $T+1$. The first proposition of Lemma 1 establishes that the plaintiff must weakly prefer S_2^* to every subsequent equilibrium settlement proposal: $U_p(S_2^*) \geq \ldots \geq U_p(S_{T+1}^*)$. The value of rejecting S_1 in a game of length $T+1$, $\max\{U_p(S_2^*(S_1)), \ldots, U_p(S_{T+1}^*(S_1)), W_p(x)\}$, is thus equivalent to $\max\{U_p(S_2^*(S_1)), W_p(x)\}$. But this last expression for the value of continuation is exactly the expression that was used in deriving the plaintiff's strategy in the two-period game of the verification step. Aside from the timing of settlement in an interior solution, the plaintiff's strategy is thus exactly the set of rules derived in the verification step for a game of length $T = 2$.

Turning to the defendant's strategy, begin by considering two potential boundary solutions: (i) a solution in which no types of plaintiff settle, and (ii) a solution in which all types of plaintiff settle. There exist no equilibria of the first type, since every equilibrium involves settlement with a positive measure of plaintiff types. This result was established for a single-period game in Proposition 1, and generalizes to any multi-period game as the continuation game reached in the final period is itself just a single-period game and therefore characterized by Proposition 1.

A solution of the second type does exist, where the defendant makes a settlement proposal accepted by all types of plaintiff. By the first proposition of Lemma 1, S_1 must be weakly preferred to every subsequent equilibrium settlement proposal: $U_p(S_1^*) \geq \ldots \geq U_p(S_{T+1}^*)$. Thus, if all plaintiff types eventually settle, it must be that S_1^* is as valuable as the expected net present value of a trial verdict to the highest-type plaintiff: $U_p(S_1^B) = W_p(\overline{x})$. Substituting terms and solving yields the equilibrium settlement proposal for the boundary solution in which all types of plaintiff settle:

$$S_1^B = \delta^{T+1}(\pi\overline{x} - k_p) - c_p \sum_{i=1}^{T} \delta^i. \tag{18}$$

All types of plaintiff immediately accept S_1^B under Assumption 3, so the value of the boundary solution to the defendant is $U_d(S_1^B)$.[55]

Next consider the interior solution where some but not all types of plaintiff settle. Derivation of the defendant's equilibrium strategy is based on inductive reasoning. Suppose the equilibrium asserted in Proposition 2 holds for a game of length T: specifically, assume that for a game of length T, the interior solution involves a first-

[55]See note 52 for a discussion of Assumption 3.

period settlement proposal of

$$S_1^I = \delta^T(\pi \underline{x} + k_d) + c_d \sum_{i=1}^{T-1} \delta^i, \tag{19}$$

which is accepted by a plaintiff of type

$$x \leq \underline{x} + \pi^{-1}\delta^{-T+1}(c_p + c_d). \tag{20}$$

The following shows this solution then also holds for a game of length $T + 1$.

As in the verification step, the easiest way to construct the interior solution is by working backwards from the continuation game following rejection of S_1. Since some types of plaintiff reject every equilibrium proposal by assumption, the continuation game is necessarily reached with positive probability in an interior equilibrium.

As reasoned in the verification step, the Assumption 2 requirement that the plaintiff's strategy be monotone in type means there must exist some cutoff type $\underline{x}_2(S_1)$ under which types of plaintiff accept S_1 and above which types of plaintiff reject S_1. With plaintiff types distributed uniform in the population, the distribution of plaintiff types remaining in the continuation game following rejection of S_1, $\rho(x|S_1) = f(x|x > \underline{x}_2(S_1))$, is accordingly uniform on support $[\underline{x}_2(S_1), \overline{x}]$.

Optimal play in the continuation game can be expressed as a function of S_1 alone. The continuation game is just a game of length T, and so is characterized by the proposed equilibrium under the assumption that the proposition holds for a game of length T. Substituting $\underline{x}_2(S_1)$ for $\underline{x}$ in the interior solution given by equation (19) yields the optimal continuation game settlement proposal as a function of S_1:

$$S_2^*(S_1) = \delta^T(\pi \underline{x}_2(S_1) + k_d) + c_d \sum_{i=1}^{T-1} \delta^i. \tag{21}$$

The cutoff type at which the continuation game proposal $S_2^*(S_1)$ is just rejected, $\underline{x}_3(S_1)$, is given by inequality (20) with $\underline{x}_2(S_1)$ substituting for $\underline{x}$:

$$\underline{x}_3(S_1) = \underline{x}_2(S_1) + \pi^{-1}\delta^{-T+1}(c_p + c_d). \tag{22}$$

Finally, the Lemma 1 restriction that $U_p(S_1) = U_p(S_2^*(S_1)) \iff S_2^*(S_1) = \delta^{-1}S_1 + c_p$ in an interior equilibrium, combined with the specification of $S_2^*(S_1)$ in equation (21), allows $\underline{x}_2(S_1)$ to be solved in terms of S_1:

$$\underline{x}_2(S_1) = \pi^{-1}(\delta^{-2}S_1 + \delta^{-1}c_p - k_d). \tag{23}$$

The defendant's problem for a game of length $T+1$ is an intuitive generalization of the two-period problem described in the verification step. Without formal definition, let $S_t^*(S_1)$ denote the equilibrium settlement proposal made in period $t > 2$, and let $\underline{x}_t(S_1)$ be the lower bound on plaintiff types remaining under equilibrium play in period $t > 3$. As expressed in equation (24), the defendant chooses a value of S_1 in order to maximize the sum of (i) the value of settlement at S_1, weighted by the measure of plaintiff types that accept S_1, (ii) the value of settlement at $S_t^*(S_1)$, weighted by the measure of plaintiff types that reject all prior proposals but accept $S_t^*(S_1)$ for all $t \in \{2, \ldots, T+1\}$, and (iii) the expected net present value of a trial verdict conditional on the plaintiff being a type that rejects all equilibrium proposals, weighted by the measure of types that reject all proposals:

$$
\begin{aligned}
V_d(S_1) = {} & \mathrm{P}\left[x \leq \underline{x}_2(S_1)\right] U_d(S_1) \\
& + \mathrm{P}\left[\underline{x}_2(S_1) < x \leq \underline{x}_3(S_1)\right] U_d(S_2^*(S_1)) \\
& + \ldots \\
& + \mathrm{P}\left[\underline{x}_{T+1}(S_1) < x \leq \underline{x}_{T+2}(S_1)\right] U_d(S_{T+1}^*(S_1)) \\
& + \mathrm{P}\left[x > \underline{x}_{T+2}(S_1)\right] \mathrm{E}\left[W_d(x) | x > \underline{x}_{T+2}(S_1)\right] \qquad (24)
\end{aligned}
$$

$$
\begin{aligned}
& = \frac{\underline{x}_2(S_1) - \underline{x}}{\overline{x} - \underline{x}} (-S_1 - c_d) \\
& + \frac{\underline{x}_3(S_1) - \underline{x}_2(S_1)}{\overline{x} - \underline{x}} (-\delta S_2^*(S_1) - c_d - \delta c_d) \\
& + \ldots \\
& + \frac{\underline{x}_{T+2}(S_1) - \underline{x}_{T+1}(S_1)}{\overline{x} - \underline{x}} \left(-\delta^T S_{T+1}^*(S_1) - c_d \sum_{i=1}^{T+1} \delta^{i-1} \right) \\
& + \frac{\overline{x} - \underline{x}_{T+2}(S_1)}{\overline{x} - \underline{x}} \left(-\delta^{T+1} \left(\pi \frac{\overline{x} + \underline{x}_{T+2}(S_1)}{2} - k_d \right) - c_d \sum_{i=1}^{T+1} \delta^{i-1} \right). \qquad (25)
\end{aligned}
$$

Equation (25) follows from (24) by substitution and expansion of terms.

In deriving the FOC for maximization of equation (25), informality in the definition of $S_t^*(S_1)$ for $t > 2$ and $\underline{x}_t(S_1)$ for $t > 3$ is circumscribed by application of the envelope theorem. These terms are defined by optimal behavior in nested continuation games and so can be treated as constants when taking the derivative with respect to S_1; they are subsequently absent at the point of evaluation.[56] The resulting FOC

[56]The reasoning is analogous to that of note 53. Define the T-vector $\vec{S}_{-1} = [S_2, \ldots, S_{T+1}]$ and let $V_d(S_1, \vec{S}_{-1})$ be the defendant's objective function over simultaneous choice of S_1 and $\vec{S}_{-1}$, with $\vec{S}_{-1}^*(S_1)$ the argmax of $V_d(S_1, \vec{S}_{-1})$ with respect to $\vec{S}_{-1}$. If $V_d(S_1) = V_d(S_1, \vec{S}_{-1}^*(S_1))$, the envelope theorem provides a simplifying result:

$$
\frac{dV_d(S_1)}{dS_1} = \left. \frac{\partial V_d(S_1, \vec{S}_{-1})}{\partial S_1} \right|_{\vec{S}_{-1} = \vec{S}_{-1}^*(S_1)}.
$$

Since each $\underline{x}_t(S_1)$ for $t > 3$ is by definition a function of a $S_{t-1}^*(S_1)$ for $t \geq 2$, the former are also subject to the envelope theorem simplification.

provides a simple expression for the interior-solution equilibrium proposal S_1^I:

$$S_1^I = \delta^{T+1}(\pi \underline{x} + k_d) + c_d \sum_{i=1}^{T} \delta^i. \tag{26}$$

Evaluating the definition of $\underline{x}_2(S_1)$ in equation (23) at S_1^I provides the upper bound on plaintiff types that accept S_1^I in an interior equilibrium:

$$\underline{x}_2 = \underline{x} + \pi^{-1}\delta^{-1}(c_p + c_d). \tag{27}$$

The equilibrium strategy for plaintiff types $x \leq \underline{x}_2$ is accordingly to accept S_1^I. The strategy for plaintiff types $x > \underline{x}_2$ is to reject S_1^I, either in favor of subsequent settlement for the equally preferred second-period proposal $U_p(S_2^*(S_1^I))$, or in favor of an eventual trial verdict.

As discussed in the verification step, the interior solution is preferred identically when the interior proposal is less than the boundary proposal: $V_d(S_1^I) \geq U_d(S_1^B) \iff S_1^I \leq S_1^B$. This allows S_1^* to be expressed parsimoniously:

$$S_1^* = \min\{S_1^I, S_1^B\}. \tag{28}$$

Exactly the hypothesized solution for a game of length $T+1$, the solution in equation (28) completes the inductive proof.

□

A.4 Proof of Proposition 3

Proof. Spier (1992) provides a *sufficient* condition for settlement delay to persist as the duration of bargaining periods becomes arbitrarily small. By the same reasoning, the following proof derives the *necessary and sufficient* condition for persistent delay.

For a game of length $T > 1$, Proposition 2 predicts delayed settlement only when parameter values induce an interior equilibrium—the boundary solution in which all plaintiffs settle involving no probability of delayed agreement. Existence of equilibrium settlement delay is accordingly identical to the condition that equilibrium involves an interior solution. Under Proposition 2, an interior equilibrium obtains exactly when the interior settlement proposal is less than the boundary proposal: $S_1^I \leq S_1^B$. Expanding terms provides the condition for delayed settlement as a function of model parameters alone.

$$\delta^T(\pi\underline{x} + k_d) + c_d \sum_{i=1}^{T-1} \delta^i \leq \delta^T(\pi\overline{x} - k_p) - c_p \sum_{i=1}^{T-1} \delta^i. \tag{29}$$

To assess the persistence of settlement delay as the duration of bargaining periods becomes arbitrarily small, consider the model where a game of length T is transformed to a game of length $J > T$, but with periods reduced to a fraction T/J of the normal duration. Reduced period duration translates to transformed negotiation costs and discounting as follows:

$$c_p \to c_p \frac{T}{J} \tag{30}$$

$$c_d \to c_d \frac{T}{J} \tag{31}$$

$$\delta \to \delta^{T/J} \tag{32}$$

Applying the transformed parameter values in equations (30) through (32) to inequality (29) provides the condition for persistent delay in the transformed model:

$$\delta^T(\pi\underline{x} + k_d) + c_d\frac{T}{J}\sum_{i=1}^{J-1}\delta^{iT/J} \leq \delta^T(\pi\overline{x} - k_p) - c_p\frac{T}{J}\sum_{i=1}^{J-1}\delta^{iT/J}. \tag{33}$$

Rearranging terms in inequality (33) provides a more convenient expression for the existence of settlement delay in the transformed model with reduced period duration:

$$\delta^T[\pi(\overline{x} - \underline{x}) - (k_d + k_p)] - (c_d + c_p)\frac{T}{J}\sum_{i=1}^{J-1}\delta^{Ti/J} > 0. \tag{34}$$

Settlement delay persists as the duration of bargaining periods becomes arbitrarily small when condition (34) is satisfied in the limit as $J \to \infty$. Taking this limit provides the necessary and sufficient condition for delay to persist as period granularity becomes vanishingly fine:

$$\delta^T[\pi(\overline{x} - \underline{x}) - (k_d + k_p)] - (c_d + c_p)\frac{\delta^T - 1}{\log\delta} > 0. \tag{35}$$

Because condition (35) is satisfied for a range of parameter values, equilibrium settlement delay under the asymmetric information model does not generically vanish as the duration of bargaining periods becomes arbitrarily small.

□

A.5 Proof of Proposition 4

Proof. The proof is essentially the same as that of in Spier (1989, 1992) for closely related models. A simple recursion on the well known SPE for an ultimatum game, the following argument errs on the side of brevity.

In a game of length $T > 1$ with the value of potential damages x common knowledge, the subgame beginning in the final period of bargaining is simply an ultimatum game in which the defendant proposes settlement at S_T and default payoffs are determined by a trial verdict. By backwards induction, the plaintiff accepts any settlement proposal at least as good as the value of rejection: $U_p(S_T) \geq W_p(x)$. The defendant thus proposes S_T^* such that $U_p(S_T^*) = W_p(x) \iff S_T^* = \delta(\pi x - k_p)$. The defendant always prefers settlement at S_T^* to trial, as settlement allows the defendant to recoup own and opponent trial costs: $U_d(S_T^*) = W_d(x) + \delta(k_p + k_d) > W_d(x)$.

Given play in the final period, the subgame beginning in the penultimate period of bargaining is like an ultimatum game with the defendant proposing settlement at S_{T-1} and default payoffs determined by play in the final-period game. Again by backwards induction, the plaintiff accepts any settlement proposal at least as good as the value of continuation: $U_p(S_{T-1}) \geq U_p(S_T^*)$. The defendant accordingly proposes S_{T-1}^* such that $U_p(S_{T-1}^*) = U_p(S_T^*) \iff S_{T-1}^* = \delta^2(\pi x - k_p) - \delta c_p$. The defendant always prefers settlement at S_{T-1}^* to settlement at S_T^*, as earlier settlement allows the defendant to recoup own and opponent negotiation costs: $U_d(S_{T-1}^*) = U_d(S_T^*) + \delta(c_p + c_d) > U_d(S_T^*)$.

Iteration on this logic reveals the plaintiff's equilibrium strategy in the first period must be to accept any settlement proposal S_1 such that $U_p(S_1) \geq W_p(x)$. The defendant's equilibrium strategy is correspondingly to propose S_1^* such that $U_p(S_1^*) = W_p(x) \iff S_1^* = \delta^T(\pi x - k_p) - c_p \sum_{i=2}^{T} \delta^{i-1}$.

□

Chapter III

Experimental Design

The remaining chapters of this study concern the use of a large laboratory experiment in addressing the potential for asymmetric information to explain systematic settlement delay. The mechanics of settlement bargaining in the experiment have already been described: the experiment closely adheres to the theoretic model presented in Chapter II. With the finer details of settlement bargaining out of the way, the present chapter focuses on high-level aspects of the experimental design.

The design phase is a critical step in experimental study. Experimental design choices both empower and constrain subsequent analysis of results. Two guiding principles underlie the present experiment's framework for investigating settlement bargaining with asymmetrically informed litigants. The first principle is that the experiment should be flexible enough to address a range of both exploratory and confirmatory research questions. The second principle is that the experiment should be resource-efficient, applying data to multiple inquiries wherever possible.

The remainder of this chapter describes the experimental design employed in the following chapters. Section 5 covers formal elements of the experiment design. Presentation emphasizes the flexibility of the adopted design in addressing a range of diverse research questions while also providing strong experimental controls. Section 6 explains the proceedures followed in conducting the experiment. Points of particular importance include adaptation of the theoretic model of settlement bargaining to an appropriate experimental environment, and procedural details relevant to replication of the experiment.

5 Design Elements

This section presents formal elements of the experimental design applied in the remaining chapters. Definitions are provided for most of the design language, with particularly esoteric terms (emphasized by italics on first use) explained in greater detail in Appendix B. The remainder of the section proceeds as follows. Section 5.1 defines experimental units. Section 5.2 defines experimental factors. Section 5.3 defines experimental treatments and Section 5.4 defines treatment sequences. Section 5.5 discusses experimental replication. Section 5.6 covers randomization in the design. Finally, Section 5.7 explains measurements and recorded data.

5.1 Units

An experimental *unit* is an identifiable entity from which measurements are collected during the experiment. Four experimental units are relevant to the present design and analysis: (i) subjects, (ii) disputes, (iii) rounds, and (iv) sessions. These units are nested such that the full experiment is divided into a number of sessions, sessions are divided into multiple rounds, rounds consist of multiple disputes, and disputes involve matched pairs of subjects.

Subjects in the experiment are voluntary student-participants recruited from the undergraduate class and law school at the University of Virginia. All subjects are compensated for participating in the experiment according to their performance in a series of (settlement bargaining) disputes during a session. Subjects volunteer to participate without prior knowledge of the experiment's methodology, objective, or topic (i.e. subjects are *uninformed*). A total of 12 subjects participate in each session of the experiment and no subject is allowed to participate in more than one session (i.e. subjects are *inexperienced*).

Disputes are the individual settlement bargaining games played by pairs of subjects in the experiment. Within a session-round, measurements are collected as 6 randomly matched pairs of disputants interact in an experimental settlement bargaining game closely based on the theoretic model of settlement bargaining described in Section 3. Disputes are presented as independent events, with no mechanical dependence of one dispute on the outcome of any another dispute in a session.

Rounds are independent repetitions of disputes. For example, in the first round of a session, subjects are randomly matched into plaintiff/defendant pairs to form a set of 6 disputes. Each dispute is then played to conclusion, resulting in either settlement or a trial verdict. In the following round, the same set of subjects are again randomly matched into 6 pairs, and a new set of disputes is played to conclusion. This is repeated for a total of 14 rounds in each session.

Sessions are the logistical units in which the experiment is conducted. Due to its fairly large size, the present experiment was actually divided into 36 separate sessions. With 12 subjects and 14 rounds, each session collects measurements from $6 \times 14 = 84$ distinct settlement bargaining disputes.

5.2 Factors

In standard design terminology, a *factor* is any variable considered relevant to a measured outcome of an experiment. Factors can be either *experimental*, being under the control of the researcher, or *observational*, being observed but uncontrolled by the researcher. A *factor level* is a particular value or classification of a factor.

Aside from the identities of experimental units (i.e. subjects, disputes, rounds, and sessions), no observational factors are tracked in this experiment. Four experimental factors are manipulated, as enumerated below.

To avoid confusion with model notation, experimental factors are denoted by calligraphic letters, with specific factor levels denoted by subscripts. A *control* level of a factor is a reference value against which other factor levels can be informatively compared; notation adopts the convention of denoting control levels by subscript 0. For example, the information environment factor (defined below) is denoted $\mathcal{I}$ and the control level of this factor (asymmetric information) is denoted $\mathcal{I}_0$.

5.2.1 Information Environment

The information environment factor, $\mathcal{I}$, relates to the (binary) availability of potential damages information in the experimental settlement bargaining game. The control level of this factor, $\mathcal{I}_0$, corresponds to a bargaining environment in which potential damages information is asymmetrically available to the plaintiff. The other level of the factor, $\mathcal{I}_1$, corresponds to a complete information environment in which both plaintiff and defendant are symmetrically informed about potential damages. Level definitions are consolidated in Table 3.

In terms of the theoretic settlement bargaining model, factor level $\mathcal{I}_0$ corresponds to the (standard) model with asymmetric information (Section 3.2). Level $\mathcal{I}_1$ corresponds to the (special case) model with symmetric information (Section 3.3). Variation in the information environment factor helps to isolate the causal effects of controlled information asymmetries on settlement bargaining behavior.

5.2.2 Parameter Values

The parameter values factor, $\mathcal{P}$, corresponds to the specific parameter values substituted into the abstract theoretic model to form a concrete experimental bargaining environment. Each level of the factor corresponds to a unique numeric value of the 9-vector, $[\underline{x}, \overline{x}, \pi, T, \delta, c_p, c_d, k_p, k_d]$. Four factor levels are explored in this study, with the first level, $\mathcal{P}_0$, corresponding to a control environment of the settlement bargaining game against which various modifications are compared.

Relative to the control environment, three additional factor levels are defined by perturbing one aspect of the control parameters at a time. Factor level $\mathcal{P}_1$ perturbs control parameters by reversing negotiation and trial costs: i.e. the values of c_d and c_p are swapped, as are the values of k_d and k_p. Level $\mathcal{P}_2$ perturbs control parameters by halving negotiation costs for both plaintiff and defendant: i.e. $c_p \to \frac{1}{2}c_p$ and $c_d \to \frac{1}{2}c_d$. Finally, factor level $\mathcal{P}_3$ corresponds to a bargaining environment with a compacted potential damages support resulting from a reduction in $\overline{x}$. Level definitions for the parameter values factor are consolidated in Table 3.

The numeric values of parameters under each factor level are provided in the relevant parts of Chapters IV and V. Perturbations in costs help to explore conformity to theoretic prediction (e.g. regarding proposal sequences and acceptance decisions) and help to test the robustness of control environment behavior to modest changes in the bargaining environment. Variation in the support of potential damages helps to gauge sensitivity to the degree of information asymmetry facing the defendant.

5.2.3 Reform Environment

The reform environment factor, $\mathcal{R}$, defines the legal context for settlement bargaining. The control level of this factor, $\mathcal{R}_0$, is meant to reflect current tort policy. Since the theoretic model of settlement bargaining presented in Section 3 is itself based on current policy, the control level of the reform environment factor requires no modification to the control settlement bargaining environment.

Four additional levels of the reform environment factor are meant to reflect the implementation of various "tort reform" policies through modest changes to the control environment. Level $\mathcal{R}_1$ represents implementation of a damages limit, where the distribution of potential damages is compacted by a reduction in $\overline{x}$. As explained in Section 6.1.3, the experiment distinguishes between injury and potential damages; in contrast to the $\mathcal{P}_3$ factor level, which compacts the distribution of both injury *and* potential damages, the $\mathcal{R}_2$ level leaves the injury unchanged, while compacting the distribution of potential damages.[57] Level $\mathcal{R}_2$ represents implementation of a damages cap, truncating the distribution of potential damages at a value less than $\overline{x}$. Level $\mathcal{R}_3$ represents implementation of a prejudgment interest rule, where any transfer awarded in a trial verdict is paid with interest accrued from the start of the game. Finally, level $\mathcal{R}_4$ represents implementation of the "Early Offers" reform proposal discussed previously in Section 1.1 and subsequently in Section 13.4.

For narrative simplicity, the specific values of parameters under each factor level are provided in the relevant parts of Chapter VI. Variation in the reform environment factor helps to determine how bargaining behavior responds to the implementation of simple reform polices. Observing behavior under various reform environments also provides a robustness check for behavior under the control environment.

5.2.4 Subject Pool

The subject pool factor, $\mathcal{U}$, defines the University of Virginia subpopulation from which experimental subjects are recruited. Similar to most laboratory experiments, the control level of the subject pool factor, $\mathcal{U}_0$, corresponds to the recruitment of undergraduate-student subjects. An additional factor level, $\mathcal{U}_1$, corresponds to subjects recruited from the University of Virginia School of Law. Factor levels for the subject-pool are consolidated in Table 6.

A potential concern in comparing undergraduate and law student behavior is the relative incentivization of subjects recruited from each pool. To control for dif-

[57] For example, an injury of x', can be transformed into potential damages of $\alpha x'$ with $0 < \alpha < 1$.

Table 3: Factor Levels for the Information Environment

Factor Level	Description
$\mathcal{I}_0$	Asymmetric Information: value of x known only by plaintiff.
$\mathcal{I}_1$	Symmetric Information: value of x common knowledge.

Table 4: Factor Levels for Parameter Values

Factor Level	Description
$\mathcal{P}_0$	Control: reference parameter values.
$\mathcal{P}_1$	Reverse Costs: swap $c_p \leftrightarrow c_d$ and $k_p \leftrightarrow k_d$.
$\mathcal{P}_2$	Low Costs: reduce negotiation costs by decreasing c_p and c_d.
$\mathcal{P}_3$	Low Asymmetry: reduce asymmetry by decreasing $\overline{x}$.

Table 5: Factor Levels for the Reform Environment

Factor Level	Description
$\mathcal{R}_0$	Control: no reform policy imposed.
$\mathcal{R}_1$	Damages Limit: limit imposed on potential damages.
$\mathcal{R}_2$	Damages Cap: cap imposed on potential damages.
$\mathcal{R}_3$	Prejudgment Interest: prejudgment interest rule imposed.
$\mathcal{R}_4$	Early Offers: model of Early Offers reform imposed.

Table 6: Factor Levels for the Subject Pool

Factor Level	Description
$\mathcal{U}_0$	Undergraduate: subjects compensated at 0.05% earnings.
$\mathcal{U}_1$	Law School: subjects compensated at 0.075% earnings.

ferences in the perceived opportunity cost of participation, subjects recruited from the law school are provided greater compensation for participation in the experiment: undergraduate-student subjects are compensated at 0.05% of their experimental earnings, whereas law student subjects are compensated at 0.075%.[58] Variation in the subject pool affords a robustness test for the external validity of data collected from the (control) undergraduate subject pool.

5.3 Treatments

Experimental *treatments* are combinations of factor levels relevant to the research objectives of an experiment. Particular interest is in the *treatment effect* that exposure to a treatment has on the measurements collected from experimental units.

To avoid confusion with model or factor notation, treatments in the present design are denoted by boldface upper-case letters and distinguished by subscripts; notation continues to index the control by subscript 0. The control treatment is defined by the control levels of all experimental factors: $\mathbf{T}_0 = [\mathcal{I}_0, \mathcal{P}_0, \mathcal{R}_0, \mathcal{U}_0]$. Additional treatment levels consist of alternative combinations of factor levels.

Many commonly used schemes for selecting the set of experimental treatments are impractical for the present design. For example, treatments defined by a *fully crossed* set of factor levels (i.e. every possible permutation of factor levels) would involve $2 \times 4 \times 5 \times 2 = 80$ distinct treatment levels—well beyond the resource capacity of the present study. Instead, a set of 14 treatment levels is defined to isolate effects of particular interest in addressing the broad research questions at hand. This set of treatment levels is consolidated in Table 7.

[58]Greater compensation of law students is intended to maintain approximate parity in the incentives of subjects from the different subject pools. The *ad hoc* compensation differential was informed by discussion with faculty and students at the University of Virginia School of Law.

Table 7: Experimental Treatment Levels

Treatment	Factor Levels	Difference from Control Treament
$\mathbf{T}_0$	$[\mathcal{I}_0, \mathcal{P}_0, \mathcal{R}_0, \mathcal{U}_0]$	none (control treatment)
$\mathbf{T}_1$	$[\mathcal{I}_1, \mathcal{P}_0, \mathcal{R}_0, \mathcal{U}_0]$	Symmetric Information
$\mathbf{T}_2$	$[\mathcal{I}_0, \mathcal{P}_1, \mathcal{R}_0, \mathcal{U}_0]$	Reverse Costs
$\mathbf{T}_3$	$[\mathcal{I}_1, \mathcal{P}_1, \mathcal{R}_0, \mathcal{U}_0]$	Reverse Costs & Symmetric Information
$\mathbf{T}_4$	$[\mathcal{I}_0, \mathcal{P}_2, \mathcal{R}_0, \mathcal{U}_0]$	Low Costs
$\mathbf{T}_5$	$[\mathcal{I}_1, \mathcal{P}_2, \mathcal{R}_0, \mathcal{U}_0]$	Low Costs & Symmetric Information
$\mathbf{T}_6$	$[\mathcal{I}_0, \mathcal{P}_3, \mathcal{R}_0, \mathcal{U}_0]$	Low Asymmetry
$\mathbf{T}_7$	$[\mathcal{I}_1, \mathcal{P}_3, \mathcal{R}_0, \mathcal{U}_0]$	Low Asymmetry & Symmetric Information
$\mathbf{T}_8$	$[\mathcal{I}_0, \mathcal{P}_0, \mathcal{R}_0, \mathcal{U}_1]$	Law School
$\mathbf{T}_9$	$[\mathcal{I}_1, \mathcal{P}_0, \mathcal{R}_0, \mathcal{U}_1]$	Law School & Symmetric Information
$\mathbf{T}_{10}$	$[\mathcal{I}_0, \mathcal{P}_0, \mathcal{R}_1, \mathcal{U}_0]$	Damages Limit Reform
$\mathbf{T}_{11}$	$[\mathcal{I}_0, \mathcal{P}_0, \mathcal{R}_2, \mathcal{U}_0]$	Damages Cap Reform
$\mathbf{T}_{12}$	$[\mathcal{I}_0, \mathcal{P}_0, \mathcal{R}_3, \mathcal{U}_0]$	Prejudgment Interest Reform
$\mathbf{T}_{13}$	$[\mathcal{I}_0, \mathcal{P}_0, \mathcal{R}_4, \mathcal{U}_0]$	Early Offers Reform

Two groups of non-control treatments are distinguished by a horizontal dividing line in Table 7. The first group, $\mathbf{T}_1, \ldots, \mathbf{T}_9$, involves disjoint variation of the parameter factor, $\mathcal{P}$, and subject pool factor, $\mathcal{U}$. Each level of either factor is crossed with the information factor, $\mathcal{I}$, meaning treatments cover the control environment with and without asymmetric information, the reverse costs environment with and without asymmetric information, the low costs environment with and without asymmetric information, etc. Treatments in this group isolate the effect of the controlled information asymmetry on behavior under various bargaining environments.

The second group of treatments, $\mathbf{T}_{10}, \ldots, \mathbf{T}_{13}$, varies the reform environment factor, $\mathcal{R}$, with all other factors fixed at the control level. In comparison to the control treatment, $\mathbf{T}_0$, treatment $\mathbf{T}_{10}$ imposes a limit on the support of potential damages, treatment $\mathbf{T}_{11}$ imposes a cap on the support of potential damages, etc. Treatments in this group isolate the effect of various “tort reform” policies on settlement bargaining behavior when the plaintiff is asymmetrically informed about the value of a potential trial verdict.

5.4 Sequences

The present experiment adopts what is usually referred to as a *cross-over* design. In contrast to a *parallel* design, where each experimental unit is exposed to only a single treatment, a cross-over design exposes each experimental unit to multiple different treatments. In the present experiment, experimental units are exposed to two treatments (i.e. every subject *crosses over* from one treatment to another during a session). The identity and order of treatments to which an experimental unit is exposed is referred to as a treatment *sequence*.

Like treatments, sequences are denoted by boldface upper-case letters, with specific treatment levels denoted by subscripts. Sequence subscripts initialize at 1 (not 0), as there is no meaningful concept of a control sequence in this design. A fully crossed set of sequences (i.e. a full permutation of all $14 \times 13 = 182$ ordered pairs of treatments) is beyond the resource capacity of the present study. Instead, a total of 18 sequences are defined to isolate treatment effects of particular interest in addressing basic research questions. Let $\mathbf{T}_A$ and $\mathbf{T}_B$ denote the first and second treatments to which an experimental unit is exposed in a sequence. The set of sequences employed in the present design is consolidated in Table 8.

The set of sequences in Table 8 evinces two noteworthy properties. First, every non-control treatment in Table 7 appears in two different sequences while the control treatment appears in ten. Second, every sequence isolates a single experimental factor for variation. These properties allow the set of sequences in Table 8 to encompass three separable sub-experiments within a common design.

5.4.1 Sub-Experiment 1

Sub-Experiment 1 (SE1) explores measurements collected during exposure to the control treatment, $\mathbf{T}_0$, in experimental sequences $\mathbf{S}_1, \mathbf{S}_2, \mathbf{S}_{10}, \ldots, \mathbf{S}_{13}$. Assigned as $\mathbf{T}_A$ in some sequences and as $\mathbf{T}_B$ in others, all data in SE1 concerns exposure to the control treatment only. The basic objective of this sub-experiment is exploratory: data reveal patterns of behavior when payment-incentivized laboratory subjects take the roles of litigants in the settlement bargaining game with asymmetric information.

By assigning the control treatment across many experimental sequences, sufficient data are collected to provide a rich profile of observed behavior in the control environment. Observed patterns of behavior compare with theoretic predictions to determine points of agreement and discord between theory and observation.

Table 8: Experimental Sequence Levels

Sequence	$\mathbf{T}_A$	$\mathbf{T}_B$	Common Environment	Difference
$\mathbf{S}_1$	$\mathbf{T}_0$	$\mathbf{T}_1$	Control	Asymmetric → Symmetric Information
$\mathbf{S}_2$	$\mathbf{T}_1$	$\mathbf{T}_0$	Control	Symmetric → Asymmetric Information
$\mathbf{S}_3$	$\mathbf{T}_2$	$\mathbf{T}_3$	Reverse Costs	Asymmetric → Symmetric Information
$\mathbf{S}_4$	$\mathbf{T}_3$	$\mathbf{T}_2$	Reverse Costs	Symmetric → Asymmetric Information
$\mathbf{S}_5$	$\mathbf{T}_4$	$\mathbf{T}_5$	Low Costs	Asymmetric → Symmetric Information
$\mathbf{S}_6$	$\mathbf{T}_5$	$\mathbf{T}_4$	Low Costs	Symmetric → Asymmetric Information
$\mathbf{S}_7$	$\mathbf{T}_6$	$\mathbf{T}_7$	Low Asymmetry	Asymmetric → Symmetric Information
$\mathbf{S}_8$	$\mathbf{T}_7$	$\mathbf{T}_6$	Low Asymmetry	Symmetric → Asymmetric Information
$\mathbf{S}_9$	$\mathbf{T}_8$	$\mathbf{T}_9$	Law School	Asymmetric → Symmetric Information
$\mathbf{S}_{10}$	$\mathbf{T}_9$	$\mathbf{T}_8$	Law School	Symmetric → Asymmetric Information
$\mathbf{S}_{11}$	$\mathbf{T}_0$	$\mathbf{T}_{10}$	Control	Control → Damages Limit
$\mathbf{S}_{12}$	$\mathbf{T}_{10}$	$\mathbf{T}_0$	Control	Damages Limit → Control
$\mathbf{S}_{13}$	$\mathbf{T}_0$	$\mathbf{T}_{11}$	Control	Control → Damages Cap
$\mathbf{S}_{14}$	$\mathbf{T}_{11}$	$\mathbf{T}_0$	Control	Damages Cap → Control
$\mathbf{S}_{15}$	$\mathbf{T}_0$	$\mathbf{T}_{12}$	Control	Control → Prejudgment Interest
$\mathbf{S}_{16}$	$\mathbf{T}_{12}$	$\mathbf{T}_0$	Control	Prejudgment Interest → Control
$\mathbf{S}_{17}$	$\mathbf{T}_0$	$\mathbf{T}_{13}$	Control	Control → Early Offers
$\mathbf{S}_{18}$	$\mathbf{T}_{13}$	$\mathbf{T}_0$	Control	Early Offers → Control

5.4.2 Sub-Experiment 2

Sub-Experiment 2 (SE2) explores measurements collected from sequences $\mathbf{S}_1, \ldots, \mathbf{S}_{10}$. These sequences isolate the effect of changes in information under various bargaining environments and with different subject pools. The basic objective of this sub-experiment is confirmatory: data are investigated to determine (i) whether information asymmetry over a potential verdict tends to increase settlement delay, and (ii) how much average settlement delay increases under asymmetric information.

The causal effects of information asymmetry are identified by within-unit variation: e.g. by comparing average settlement delay before and after the introduction of the controlled information asymmetry. The effects of various environment perturbations are identified by between-unit variation: e.g. by comparing average settlement delay between the control environment and the reversed costs environment.

5.4.3 Sub-Experiment 3

Sub-Experiment 3 (SE3) explores measurements collected from sequences $\mathbf{S}_{11}, \ldots, \mathbf{S}_{18}$. These sequences isolate the effects of imposing various "tort reform" policies with all other factors fixed at control levels. The basic objectives of this sub-experiment are both confirmatory and exploratory. An initial confirmatory question is whether any reform policy tends to reduce settlement delay. Subsequent questions explore the relative properties of behavior under each reform policy.

The causal effects of each reform policy are identified by within-unit variation: e.g. by comparing average settlement delay before and after imposition of a cap on damages. The comparative effects of the reform policies are identified by between-unit variation: e.g. by comparing average settlement delay between the damage cap environment and damage limit environment.

5.5 Replication

The experiment follows an intuitive replication scheme. First, every sequence in Table 8 is replicated twice: each sequence is assigned to 2 separate experimental sessions. Second, within a session, each treatment in the assigned sequence is replicated 7 times: treatment $\mathbf{T}_A$ is assigned to the first 7 rounds of the session, and treatment $\mathbf{T}_B$ is assigned to the second 7 rounds.

Given this replication scheme, sample size determination is straightforward. Each sequence is assigned to 2 sessions and each non-control treatment, $\mathbf{T}_1, \ldots, \mathbf{T}_{13}$, appears in 2 sequences. Each non-control treatment is thus assigned to $2 \times 2 = 4$ sessions, $2 \times 2 \times 7 = 28$ rounds, and $2 \times 2 \times 7 \times 6 = 168$ disputes. The control treatment, $\mathbf{T}_0$, appears in 10 separate sequences, and is thus assigned to $2 \times 10 = 20$ sessions, $2 \times 10 \times 7 = 140$ rounds, and $2 \times 10 \times 7 \times 6 = 840$ disputes. Each of the 36 sessions involves 12 subjects, so the experimental design requires 432 unique subjects: 384 undergraduate students, and 48 law students.

In standard design terminology, this type of replication scheme is usually referred to as a *repeated measurement* design. The practice of collecting data as subjects interact in multiple rounds of settlement bargaining compares with an alternative design in which each experimental unit engages in only a single round of bargaining. Collecting repeated measurements makes the present design more resource efficient, but also has the potential to introduce several sources of design bias.

A noteworthy property of the sequences in Table 8 is that treatment order is orthogonal for every pair of treatments. Thus for any sequence such as $\mathbf{S}_1 = [\mathbf{T}_0, \mathbf{T}_1]$, the design includes a dual sequence $\mathbf{S}_2 = [\mathbf{T}_1, \mathbf{T}_0]$. Orthogonal treatment assignment provides an experimental control for two important sources of potential design bias introduced by a repeated measurements cross-over design.

The first source of potential bias is an *order effect*: the effect of taking repeated measurements on the value of measurement itself. An intuitive subclass of order effects are learning effects, such as differences in behavior as subjects become more comfortable with the experimental environment. The present cross-over design controls for order effects by assigning treatments orthogonally across every round of the experiment: i.e. for every experimental unit exposed to treatment $\mathbf{T}_0$ during the early rounds of a session, there is another experimental unit exposed to treatment $\mathbf{T}_0$ during the later rounds of a session.

The second source of potential bias is a *carryover* or *sequence effect*: the effect of exposure to a prior treatment on measurements taken during exposure to a subsequent treatment. An intuitive example of a carryover effect is when a subject develops a particular strategy during bargaining under treatment $\mathbf{T}_A$, and continues to adhere to this strategy even when the bargaining environment is changed to $\mathbf{T}_B$. Orthogonal treatment assignment also controls for carryover effects. For example, for every session in which $\mathbf{T}_0$ is the second treatment to which subjects are exposed, there is another session in which $\mathbf{T}_0$ is applied first.

5.6 Randomization

Randomization serves two important roles in the present design. First, it affords additional controls against potential sources of design bias. Second, it represents several important aspects of the theoretic settlement bargaining game. The role of randomization is approximately distinguished by the experimental unit to which it is applied: bias control involves randomization at the session level, whereas elements of modeling are served by randomization at the round and dispute levels.

5.6.1 Session Level

One aspect of session-level randomization is the assignment of experimental sequences to sets of subjects. As practical constraints (e.g. the size of the available subject pool and the need for voluntary participation) prevent true random assignment of subjects to sequences, the following quasi-random assignment technique is employed. First, empty sessions are defined and assigned to experimental sequences. Second, subjects volunteer to participate in a given session without knowing (i) the experiment being conducted, (ii) the sequence that has been assigned, or (iii) the identities of other subjects in the session. This voluntary but uninformed sorting of subjects into sessions/sequences approximates true random assignment by the experimenter.

Another aspect of session-level randomization is the assignment of persistent litigation-roles to individual subjects. Again, a quasi-random assignment technique is employed. First, role assignment is programmed to be deterministic: within a session, the first 6 subjects to sign-in to the experiment-software are assigned to be plaintiffs, the second 6 are assigned to be defendants. Second, subjects are instructed to complete the sign-in process in discrete steps, such that the order of actual sign-in (i.e. final button press) is approximately random across subjects. Reliance on mechanically deterministic role assignments facilitates the separation of subjects by role in situations where the physical distance between subjects is limited.

Quasi-random assignment of subjects to sequences and litigation-roles provides an experimental control for the effects of untracked observational factors: e.g. competitiveness, formal exposure to game theory, familiarity with other subjects in the session, etc. Session-level randomization supports the assumption that no unobserved factors tend to correlate with the assignment of any particular experimental treatment or bargaining role.

5.6.2 Round and Dispute Level

Round-level randomization concerns subjects matchings. With persistent roles, randomized matching provides 6! possible sets of pairs, affording a low probability of repeat pairing. For any plaintiff-defendant pair, the probability of being matched in a given round is $1/6$; the probability of being matched in a given round *and* the following round is $1/36$, etc. Combined with a bargaining interface that limits communication and conceals subject identities, randomized matchings approximate the theoretic interpretation of settlement bargaining as a one-shot game.

Dispute-level randomization concerns two random variables in the experimental settlement bargaining game: (i) a random draw from a uniform distribution representing the size of injury sustained, and (ii) a random draw from a Bernoulli distribution representing the (binary) liability determination of a trial verdict. Other parameters such as potential damages are deterministic functions of these basic random variables.

The present experiment adopts the practice of using a common random number sequence in the round-level and dispute-level randomization of every session. At the round level, this means that dispute-matchings are the same in every session: i.e. if the subject assigned to ID 1 is randomly matched with the subject assigned to ID 7 in the first round of *any* session, then the subject assigned to ID 1 is randomly matched with the subject assigned to ID 7 in the first round of *every* session.

At the dispute level, reliance on a common random number sequence means that injury and liability draws are assigned by treatment-specific transformations of common draws from an underlying standard uniform distribution. As an example, consider the above hypothesized dispute between ID's 1 and 7 that occurs in every session. Every session employs the same underlying standard uniform draw $d' \in [0, 1]$ to assign the injury for this dispute. The value of the injury in a given session is constructed as $\underline{x} + (\overline{x} - \underline{x})d'$ for whatever values of $\underline{x}$ and $\overline{x}$ the relevant treatment specifies. Every session's liability draw for the hypothesized dispute is constructed similarly. In a given session, liability is assigned by awarding a plaintiff-verdict when the value of a common standard uniform draw $d'' \in [0, 1]$ satisfies $d'' \leq \pi$, for whatever value of π the relevant treatment specifies. Reliance on a common sequence of standard uniform draws means that between any two sessions, the injury and liability draws assigned to a given dispute are either the same or (if treatments differ between sessions) derived from appropriate transformations of the same primitive values.

Relying on a common sequence of random numbers has both advantages and disadvantages. The use of common random number sequences increases internal validity by reducing noise between sessions and treatment sequences. A disadvantage of the practice is potentially decreased external validity, as the use of different random number sequences across sessions may result in a more complete randomization of matchings and draws. The use of common sequences in the present design reflects the perception that internal validity is the more pressing concern.

5.7 Measurements

Experimental measurements include the time-stamped values of every action taken by a subject in the experiment: i.e the timing and value of every proposal, acceptance, and trial verdict. Time stamps are accurate to seconds, reflecting the same granularity as the experimental bargaining environment itself. All measurements are tagged with associated data including session-unique ID numbers for the subjects involved in a dispute and information on the round number, treatment, values of bargaining variables (e.g. the value of the standing settlement proposal), and values of random variable draws (i.e. injury and liability draws).

Measurements are recorded in session-specific rectangular files. Because every action and outcome in the experiment is recorded and time stamped, collected data can be used to fully reconstruct exact bargaining behavior for every dispute. Bargaining can literally be rewound and replayed in continuous-time post-experiment (see Appendix D.1). Detailed tagging also allows records to be matched against subject-specific stocks such as current-round and cumulative earnings, and against the outcomes of previous disputes to which either of the litigants was a party.

Data collection is unobtrusive insofar as the online bargaining interface records actions without any explicit signal of measurement or interruption to the bargaining process. Subjects are aware that the experiment is designed to study settlement bargaining, but are never told what aspects of bargaining are being explored. The experiment is not blindly conducted, and subjects are aware that the experimenter is observing their behavior during the session.

Errors in measurement are possible, but are thought to be of minor importance. Timing measurements may include small errors resulting from network glitches and human limitations such as the temporary loss of concentration or slow reaction speeds of subjects.[59] Such errors tend to imply a positive bias, but are mechanically unavoidable. Informal *ex post* discussions with subjects suggest that the frequency and magnitude of timing errors are most likely small.

Value measurements such as proposal and acceptance decisions may also include errors due to accidental typos and mouse clicks. Such errors do not imply a clear bias. Informal *ex post* discussions with subjects suggest that these errors can have substantial magnitude, but are very infrequent after the first two rounds of a session.

[59]It is ambiguous whether these "human limitations" should be considered as generating errors. Taking the data as a simple recording of behavior, all measurements are precise. But if data are interpreted as a reflection of *intended behavior*, even precise measurements of imperfectly intended actions may be more satisfyingly labeled data errors. Note that this nuance of data interpretation is an inevitable aspect of the data collected in any continuous-time experiment.

6 Proceedure

Given the centrality of procedural details to the validity of a laboratory experiment, this section describes experimental procedures adopted in the present study. Section 6.1 discusses research methods, particularly design choices in adaptation of the theoretic model of settlement bargaining to an appropriate experimental environment. Section 6.2 describes the materials used in this experiment which would be needed for replication studies. Section 6.3 discusses practices and standards in the conduct of experimental sessions.

6.1 Methods

The method of investigation is experimental adaptation of the theoretic model of settlement bargaining presented in Section 3. Data are collected as payment-incentivized subjects interact in an experiment-appropriate version of the theoretic model. Collected data are then used to assess properties and predictions of the theoretic model and related policy inquiries. Careful bargaining environment adaptation is essential to the validity and interpretation of research findings under this method of investigation.

The bargaining environment in this design is closely based on the underlying theoretic model of settlement bargaining. While almost all aspects of the bargaining environment mirror the theoretic model exactly, a full duplication of the theoretic model would be inappropriate. Theoretic abstraction from behaviorally relevant details leaves room for improvements in both internal and external validity. To increase validity, the bargaining environment adaptation introduces the following four prediction-neutral modifications to the theoretic model.

6.1.1 Exogenous Wealth Injections

The first modification introduces exogenous wealth injections. Loss aversion and related discontinuities in behavior on the domains of positive and negative earnings suggest internal validity is increased when all subjects in an experiment are limited to a single domain of earnings (see, e.g., Thaler, 1992, pp. 70–74). The motivation for wealth injections is to insure all subjects achieve positive earnings in all rounds.[60]

To insure strictly positive earnings, subjects begin an experiment with initial stocks of wealth: both plaintiff and defendant begin with $50 experimental dollars. Subjects also "earn" exogenous incomes each round: a plaintiff receives $225 each round, and a defendant receives $300. These wealth injections are fixed across all sessions and sequences. Set by exploration in pilot studies, the injections effectively prevent subjects from experiencing negative earnings.[61] The injections also tend to approximately equalize average experimental earnings between plaintiff and defendant roles in the control treatment.

Subjects in the experiment were aware that all subjects received an initial endowment and a per-round exogenous income, but were not provided information on the size or distribution of these injections. The instructions provided no indication that the same injection was assigned to all subjects of a given role. Exogenous injections are prediction-neutral if subjects are only motivated by profit maximization, but might affect changes in behavior if subjects have non-monetary preferences. Concealment of injection size and distribution is intended to minimize potential behavioral consequences from the injections' influence on fairness considerations.

[60] The theoretic model involves only costs and wealth-neutral transfers. A defendant in the model always ends a game at a strict loss. A plaintiff ends at either a loss or a gain, depending on the size of the wealth transfer.

[61] While it remains possible for subjects to achieve negative earnings under extreme strategies, no subject in the present experiment experienced negative earnings in any round.

6.1.2 Interest Rate Substitution

In theoretic models, intertemporal discounting is a convenient tool for neatly summarizing various sources of opportunity cost and time preference associated with delayed action. Unfortunately, the concept does not translate well to an experimental environment: abstract discount rates are unintuitive and can be difficult to explain to subjects. The problem is compounded for the settlement bargaining model, since explicit discounting cannot be framed as a simple additive delay cost.[62]

To affect a common discount rate without imposing explicit discounting, the experimental bargaining environment introduces interest accumulation on stocks of wealth. An interest rate defined as $r = (1-\delta)/\delta$ and compounded each period is theoretically isomorphic with an explicit discount rate of δ. The idea of interest accumulation should be familiar to most subjects, and so requires little motivation.

An alternative technique for imposing a common discount rate is exogenous termination of bargaining with some probability each bargaining period. With appropriately set default payoffs and termination probability, this technique can theoretically affect any arbitrary discount rate for risk neutral subjects. Interest accumulation is preferred in this study, as it avoids several negative features of the exogenous termination technique. In particular, interest-earning avoids artificial censoring of experimental data and exaggeration of information asymmetries when subjects have different subjective valuations of small-probability events.

[62] Many experimental bargaining environments frame inter-temporal discounting as a shrinking pie: e.g. a pie of size \$5 in the first round of bargaining which shrinks to size \$2 by the second round. Lacking an exogenous pie, the settlement bargaining game has no obvious analogue.

6.1.3 Injury and Potential Damages

Agents in the theoretic model of settlement bargaining negotiate in the context of potential damages, not injuries: any injury sustained is irrelevant except as it relates to potential damages, so the model abstracts from the injury altogether. Though theoretically irrelevant, the pain (in terms of lost wages, etc) of an injury may be a behaviorally important aspect of settlement bargaining.

As actual tort disputes involve painful injuries, including an injury in the experimental bargaining environment argues for increased external validity. The experiment introduces injuries to disputes in the form of a subtraction from the plaintiff's exogenous income at the start of a round. Potential damages are then defined by the injury according to the treatment-appropriate reform policy. For example, in the control treatment, $\mathbf{T}_0$, potential damages are identically the size of the injury; in $\mathbf{T}_{12}$, potential damages are equal to the smaller of the injury and damage cap; etc.

To improve intuition and accommodate the study of Early Offers reform, a plaintiff's injury is described to subjects as the sum of two components: (i) an *economic* component, which is common knowledge, and (ii) a *pain and suffering* component, which is the private information of the plaintiff.[63] Rich terminology (e.g. "pain and suffering") clarifies the model of information asymmetry and helps subjects better understand the bargaining process. The commonly known value of the economic injury is fixed at $50 in every session of the experiment, while the privately known pain and suffering injury is uniformly distributed with support determined by treatment.[64] The total injury, x, is the sum of the economic and pain-and-suffering injuries.

[63] Early Offers reform exploits the legal distinction between economic and non-economic damages discussed in Section 1.1. A model of Early Offers reform thus requires such a distinction in the definition of an injury and potential damages.

[64] Note that a commonly known but variable economic injury could also be used; conditional on the economic injury, the total injury would remain uniformly distributed. The present injury definition is preferred on the grounds that a less noisy distribution may improve internal validity.

6.1.4 Continuous Bargaining

A final modification is actually just a special case of the theoretic bargaining model described in Section 3: subjects in the experiment bargain in continuous time. The motivation for continuous bargaining is increased external validity. Unlike bargaining in other contexts, where negotiation might plausibly be limited to a small number of discrete interactions, legal bargaining is fully unconstrained, with litigants negotiating as frequently or infrequently as they desire. The relevance of differences in bargaining-period granularity in strategic interaction is increasingly evident in experimental studies (see, e.g., Güth et al., 2005; Friedman and Oprea, 2009).[65]

To affect perceptually continuous-time bargaining, the experimental adaptation defines $T = 120$ in every session, with each period of bargaining lasting one second. Bargaining rounds are correspondingly 2 minutes long. To accommodate second-long period durations, default actions are assigned to each bargaining role: the default action of a defendant is repetition of the most recent proposal, and the default action of a plaintiff is rejection of the current proposal. Subjects in the experiment are able to easily override default actions at any time during bargaining.

As noted in Section 3.2, with appropriate parameter values, the theoretic prediction of delayed settlement persists as period granularity becomes arbitrarily fine. For all levels of the parameter value factor in the experimental design, delayed settlement is theoretically persistent, satisfying the requirements of Proposition 3 in Chapter II. Focus on continuous-time bargaining is thus prediction-neutral.

[65] Güth et al. (2005) find increases in bargaining length (i.e. offer granularity) induce complicated, but systematic, changes in bargaining behavior. Friedman and Oprea (2009) find reductions in response granularity (i.e. more rapid response time) support monotonically greater cooperation in a prisoners' dilemma environment.

6.2 Materials

Bargaining is conducted with an online interface programmed specifically for this experiment. PHP, MySQL, and Asynchronous JavaScript And XML (AJAX) are used to create a continuous-time bargaining interface which subjects access with a web browser. The interface manages interaction in the structured bargaining game while invisibly recording data on subject actions to MySQL tables on a centralized secure-server. Integrated into the V*e*conLab Experimental Economics Suite, the bargaining interface will be made publicly available at the conclusion of the present research.

Access to the online bargaining interface requires a computer for each subject and access to the V*e*conLab website at `http://veconlab.econ.virginia.edu`. The interface is fully portable, functioning on any modern Windows, Mac, or Linux/Unix operating system with JavaScript enabled. The interface is best viewed at a resolution of 1024x768 or higher on Internet Explorer 8 or W3C-compliant browsers such as Firefox and Opera. Undergraduate subjects in the experiment used desktop computers provided by the Department of Economics at the University of Virginia. Law school subjects in the experiment provided their own laptop computers.

The experiment is best conducted with all subjects in audible distance of the experimenter. Sessions with law school subjects were conducted in an empty classroom at the University of Virginia School of Law; the small size of the classroom motivated physical separation of subjects by role in order to increase anonymity. Sessions with undergraduate subjects were conducted in the V*e*conLab laboratory at the University of Virginia and did not require separation by role. In either case, subjects were seated at isolated computer terminals and separated by foam-blinders to increase anonymity and decrease the likelihood of uncontrolled communication.

6.3 Practices

All sessions of the experiment were conducted at the University of Virginia between October 2009 and July 2010. Subject participation in the experiment was voluntary. Sessions were announced 1–5 days in advance, and recruiting software allowed at most 12 subjects to register for participation in a given session. The software provided no indication of what other subjects had registered for a session, and enforced the requirement that no subject participate in more than one session of the experiment.

Upon arriving to a session, subjects were seated at isolated computer terminals. Subjects were told their own role and knew there were a total of 6 plaintiffs and 6 defendants, but did not know the particular role assignment of any other subject. Large foam blinders separated terminals to increase subject anonymity, and subjects were asked not to speak, except as necessary to ask clarifying questions.

After roles were assigned, instructions for the session were simultaneously read aloud and displayed on the subjects' computer screens. In every session, subjects were provided 6 pages of instructions describing the settlement bargaining environment. Example instructions are provided in Appendix C.1. Instructions used rich terminology—i.e. plaintiff, defendant, injury, trial, settlement—to help subjects clearly understand the decision-making process.

Subjects were then presented with the online bargaining interface to be used for the duration of the experiment. The interface, illustrated in Figures 6 and 7, provided no means of communication outside of proposal and acceptance actions in the structured bargaining game. Controls for both plaintiff and defendant were displayed on each subject's interface (regardless of role), but only role-specific controls were active during bargaining; this is thought to increase the clarity of the bargaining process by making visual the possible actions of both roles (cf. Norman, 1988). Upon first seeing the online bargaining interface, subjects were provided a detailed verbal description of the displayed controls and information. An example of the interface description is provided in Appendix C.2.

After the interface was explained, subjects began interacting in distinct rounds of bargaining. Subjects were informed that they would participate in "a number of distinct rounds," but were not aware that a treatment change would occur after round 7, or that the session would end after round 14. In either case, the bargaining interface indicated the interruption only after full completion of the relevant round. At the point of a treatment change, the bargaining interface automatically loaded a second set of instructions, with changes to the bargaining environment marked in red. Example treatment-change instructions are provided in Appendix C.3.

Sessions usually lasted 60 to 75 minutes. Initial logistics, instructions, and subject questions took from 20 to 25 minutes on average. Bargaining usually required between 17 to 22 minutes per treatment. Instructions corresponding to the treatment change were kept brief, requiring less than 5 minutes. Subjects were promptly paid and released at the end of a session.

All subjects were compensated with cash payments. Subjects received a $6 "show-up fee" for arriving on-time to the scheduled session. In addition, subjects were paid a fixed proportion of their experimental earnings, as announced in the instructions. Average total payments were around $23.50 for subjects from the undergraduate student subject pool, and $31.00 for subjects from the law school subject pool, though earnings varied by treatment and role. Informal discussions with subjects after session completion suggest that, given the short session-length and rapid pace of bargaining, this level of compensation was sufficient to maintain subject interest throughout all 14 rounds of a session.

Figure 6: Defendant Interface for Settlement Bargaining Game

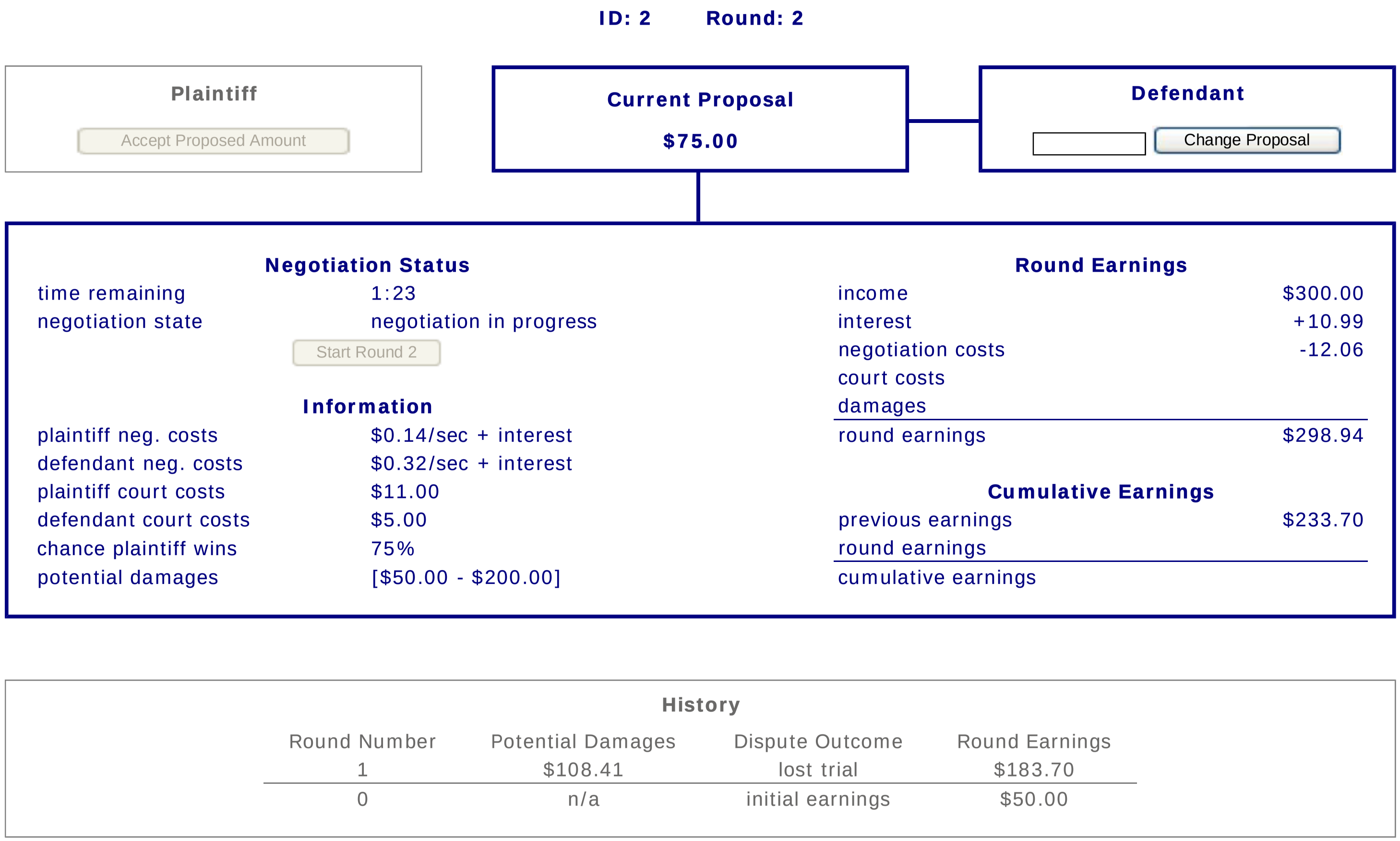
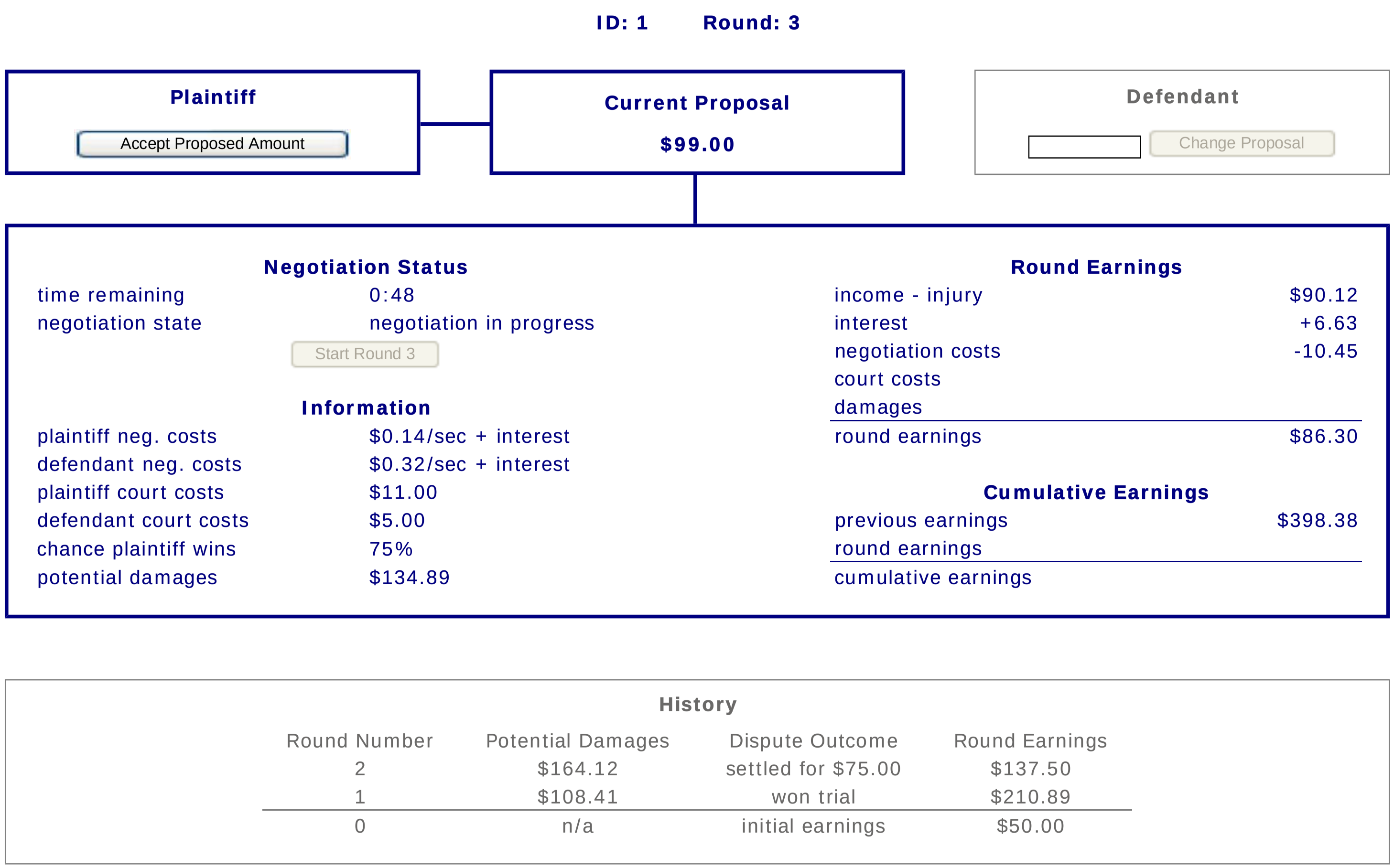


Figure 7: Plaintiff Interface for Settlement Bargaining Game



Finally, several practices were taken to insure subjects fully understood the settlement bargaining game. First, subjects in pilot sessions were asked to give general comments about the instructions and to answer comprehension-testing questions about the bargaining process. The results of subject responses in pilot sessions were then used to improve and clarify instructions for experimental sessions. Second, subjects in the experiment were given multiple opportunities to ask clarifying questions. Questions were invited after each page of instructions was read, after subjects were first taken to the online bargaining interface, and immediately following the first round of bargaining. Subjects were also repeatedly informed that they could raise their hand to ask a question at any time during a session.[66] Informal discussions with subjects after session completion indicate that subjects had a mature understanding of litigant incentives and the settlement bargaining process.

[66] Questions were infrequent, and generally raised during brief between-round waiting periods. More than two such questions per session was unusual.

B Glossary Appendix

B.1 Basic Elements of Design

Unit An entity to which an experimental treatment may be assigned in an experiment; a statistical observation. Generally an element from a set of initially common entities, differentiated by exposure to different treatments.

Factor A variable considered relevant to a measured outcome of an experiment. An experimental factor is a variable controlled by the researcher. An observational factor is a variable that is observed but not controlled.

Factor Level An identifiable categorization or value of a factor. A factor level may be categorical, ordinal, or continuous.

Treatment A unique permutation of factor levels to which an experimental unit may be exposed.

Sequence An ordered set of treatments to which an experimental unit may be exposed in series.

Measurement Collection of data or recording of an observed outcome when an experimental unit is exposed to a treatment.

B.2 Design Terminology

Repeated Measurement Design A design in which multiple measurements are taken in sequence from an experimental unit. Repeated measurements may be taken as an experimental unit is exposed to a single treatment. Alternatively, repeated measurements may be taken as an experimental unit is exposed to a sequence of treatments.

Parallel Design A design in which each experimental unit is exposed to only a single treatment.

Cross-over Design A repeated measurement design in which each experimental unit is exposed to multiple different treatments in sequence. Each unit may be exposed to the full set of experimental treatments, or to a subset thereof. The order of treatment exposure is often orthogonal across experimental units.

Fully Crossed An element of design defined by an orthogonal permutation of constituent parts. For example, a set of treatments may be defined by a fully crossed set of factor levels. A set of sequences may be defined by a fully crossed set of ordered sets of treatments.

B.3 Effect Terms

Treatment Effect The causal effect that exposure to a treatment has on measured outcomes. The effect is usually cast as a difference, either in terms of difference from a control treatment, or difference from an alternative treatment.

Order Effect The causal effect of experimental measurement on the value of subsequent measured outcomes. Order effects are only relevant in repeated measurement experiments.

Carryover Effect The causal effect of exposure to a treatment on the value of measured outcomes during exposure to subsequent treatments. Carryover effects are only relevant in repeated measurement experiments. Also commonly referred to as a "sequence effect."

B.4 Terms of Art

Control A sequence, treatment, or factor level against which alternative sequences, treatments, or levels may be informatively compared. A common control is a (no-effect) placebo, or *status quo* value against which alternative values of interest may be compared.

Uninformed Subject A subject who volunteers to participate in an experimental study without prior knowledge of the experiment to be conducted.

Inexperienced Subject A subject who has not previously participated in any part of an experiment, and whose behavior is therefore not conditioned on prior experience.

C Instructions Appendix

C.1 Example Instructions for First Treatment

Instructions for the first treatment in a sequence, $\mathbf{T}_A$, are simultaneously displayed on subjects' computer screens and read aloud by the experimenter. The instructions are generated by the experimental software, and vary by treatment to reflect changes in experimental factor levels. As an example, screenshots of instructions for the control treatment, $\mathbf{T}_0$, follow.

Screenshot 1: Example Instructions $\mathbf{T}_A = \mathbf{T}_0$: Page 1 of 6

Instructions (Page 1 of 6): Experiment Overview

This experiment involves bargaining during a lawsuit. The lawsuit is a legal dispute between an injured party (the **plaintiff**) and an injurer (the **defendant**). Parties bargain over a potential settlement---a payment the defendant may make to the plaintiff in order to avoid going to trial. The experiment has several rounds; think of each round as a **completely separate lawsuit**.

In every round of this experiment, you will play the role of the **plaintiff**. Each round you are assigned to bargain with a randomly selected defendant. All bargaining is anonymous, so no one will ever know who they were assigned to bargain with in any round.

All bargaining is done with **experimental money**. How you bargain determines how much experimental money you gain each round. At the end of the experiment, you will be paid **0.50%** of your accumulated experimental money in U.S. dollars.

To Summarize:

- In this experiment, you will play the role of the **plaintiff**.
- Each round, you are assigned to bargain with a **randomly selected** defendant; you will not usually be paired with the same defendant in consecutive rounds.
- At the end of the experiment, you will be paid **0.50%** of your total experimental earnings.

Screenshot 2: Example Instructions $\mathbf{T}_A = \mathbf{T}_0$: Page 2 of 6

Instructions (Page 2 of 6): Income and Injury

You start the experiment with an initial stock of **$50.00** in experimental money. You also earn a fixed income of **$225.00** each round. No one but you knows how much money you start with, and how much you earn as income each round.

At the start of every round, each plaintiff is **injured by a defendant**: this injury is the source of the lawsuit. The cost of the plaintiff's injury is randomly determined at the start of every round. The plaintiff always suffers an economic injury of **$50.00** (something like the cost of a medical bill), but also suffers a pain and suffering injury which is equally likely to be any amount between **$0.00** and **$150.00** (something like discomfort and depression due to the injury). The plaintiff's total injury is thus equally likely to be any amount between **$50.00** and **$200.00**.

Interactive Example:

- Click the following button a few times to see example injuries: Generate Example

Economic Injury	+	Pain and Suffering Injury	=	**Total Injury**
$50.00	+	$131.19	=	**$181.19**

The full amount of the total injury is subtracted from the plaintiff's income at the start of a round. Everyone knows the size of the economic injury is **$50.00**, but only the plaintiff knows the actual size of his/her pain and suffering injury. After bargaining in each round is over, the defendant learns what the plaintiff's actual pain and suffering injury was.

To Summarize:

- You begin the experiment with **$50.00** and get at most **$225.00** in income each round.
- The defendant always receives exactly his/her income at the start of a round. The plaintiff, on the other hand, receives the amount of his/her income **minus** the amount of his/her total injury in that round.
- The size of the plaintiff's injury varies from round to round. The plaintiff suffers an economic injury of **$50.00**, and a pain and suffering injury which is equally likely to be any amount between **$0.00** and **$150.00**. The plaintiff's total injury is thus equally likely to be any amount between **$50.00** and **$200.00**.
- Only the plaintiff knows the size of the pain and suffering injury in a given round.

Screenshot 3: Example Instructions $\mathbf{T}_A = \mathbf{T}_0$: Page 3 of 6

Instructions (Page 3 of 6): Bargaining

Rounds in this experiment last **2 minutes**. During each round, you and the randomly assigned defendant bargain over the size of a possible **settlement**---a payment the defendant may make to the plaintiff in order to avoid going to **trial**. If you and the defendant agree on a settlement amount, the defendant immediately pays the agreed amount to the plaintiff and there is no trial. If agreement is not reached by the end of **2 minutes**, then the round ends with a trial.

Plaintiffs and defendants have different roles during bargaining.

- The defendant can make and revise proposals to pay any amount in settlement; the defendant can **change the current proposal** at any time, by submitting a new proposal.
- The plaintiff cannot make settlement proposals, but instead decides whether to accept or reject the defendant's current proposal at any given time; to reject a proposal, the plaintiff simply does not accept it until either the defendant changes the proposal or the lawsuit goes to trial.

To Summarize:

- In each round, you and the defendant have **2 minutes** to bargain over a possible settlement.
- During bargaining, the defendant makes (possibly many) settlement proposals. The plaintiff decides whether or not to accept the current proposal at any given time.
- If you and the defendant do not agree on a settlement amount, the round ends with a trial.

Screenshot 4: Example Instructions $\mathbf{T}_A = \mathbf{T}_0$: Page 4 of 6

Instructions (Page 4 of 6): Interest and Negotiation Costs

Everyone **earns interest** on current stocks of money at a rate of **0.10%** (compounded every second). Interest is earned only on current-round money, not on cumulative earnings from previous rounds. Because money earns interest, it is worth more early in a round than it is late in a round. For example, **$100.00** at the start of a round is worth **$112.63** by the end of the round, while **$100.00** gained in the middle of the round is worth only **$106.07**. If a settlement is made, the plaintiff immediately begins earning interest on the settment amount and the defendant immediately stops earning interest on this amount.

On the other hand, time spent bargaining is **costly**. Every second of bargaining costs the plaintiff **$0.14** and the defendant **$0.32** in legal fees. Bargaining costs are stored up and paid with interest when bargaining ends (either at settlement or at trial). While settlement immediately freezes bargaining costs, interest continues to accrue on your current stock of money.

To Summarize:

- Everyone earns interest on current-round money at the rate of **0.10%** (compounded every second).
- Every second of bargaining costs the plaintiff **$0.14** and the defendant **$0.32**, plus interest.
- Settlement freezes bargaining costs, but you will continue to earn interest for the rest of the round.

Screenshot 5: Example Instructions $\mathbf{T}_A = \mathbf{T}_0$: Page 5 of 6

Instructions (Page 5 of 6): Trial

If you and the defendant have not agreed on an acceptable settlement amount by the end of **2 minutes**, the lawsuit goes to trial where a judge **decides who wins the case**. Going to trial is **costly**. It costs the plaintiff **$11.00** and the defendant **$5.00**. Trial costs are paid at the end of the round (without interest) in addition to accumulated negotiation costs. Rules of the trial follow:

- There is always a **75%** chance that the plaintiff will win at trial, and a **25%** chance that the plaintiff will lose.
- If the plaintiff wins, the defendant is ordered to pay the exact amount of the platinff's total injury (i.e. an amount between **$50.00** and **$200.00**). If the plaintiff loses, no payment is ordered.

Interactive Example:

- Click the following button a few times to see example potential damages: Generate Example

Total Injury	=	**Potential Damages**
$184.26	=	**$184.26**

Although no one knows who would win if the case went to trial, the plaintiff does know the exact amount that could be won at trial. (This is because only the plaintiff knows the size of his/her pain and suffering injury in a given round.)

To Summarize:

- Going to trial costs the plaintiff **$11.00** and the defendant **$5.00**.
- There is always a **75%** chance that the plaintiff will win at trial, and a **25%** chance that the plaintiff will lose.
- If the plaintiff wins at trial, the defendant is ordered to pay the exact amount of the platinff's total injury (i.e. an amount between **$50.00** and **$200.00**). If the plaintiff loses, no payment is ordered.
- Neither party knows who would win a trial, but the plaintiff does know exactly how much could be won.

Screenshot 6: Example Instructions $\mathbf{T}_A = \mathbf{T}_0$: Page 6 of 6

Instructions (Page 6 of 6): Summary

- In this experiment, you will play the role of the **plaintiff**.
- Each round, you are assigned to bargain with a **randomly selected** defendant; you will not usually be paired with the same defendant in consecutive rounds.
- You begin the experiment with **$50.00** and get at most **$225.00** in income each round.
- The defendant always receives exactly his/her income at the start of a round. The plaintiff, on the other hand, receives the amount of his/her income **minus** the amount of his/her total injury in that round.
- The size of the plaintiff's injury varies from round to round. The plaintiff suffers an economic injury of **$50.00**, and a pain and suffering injury which is equally likely to be any amount between **$0.00** and **$150.00**. The plaintiff's total injury is thus equally likely to be any amount between **$50.00** and **$200.00**.
- Only the plaintiff knows the size of the pain and suffering injury in a given round.
- Everyone earns interest on current-round money at the rate of **0.10%** (compounded every second).
- In each round, you and the defendant have **2 minutes** to bargain over a possible settlement.
- Every second of bargaining costs the plaintiff **$0.14** and the defendant **$0.32**, plus interest.
- Settlement freezes bargaining costs, but you will continue to earn interest for the rest of the round.
- If you and the defendant do not agree on a settlement amount, the round ends with a trial.
- There is always a **75%** chance that the plaintiff will win at trial, and a **25%** chance that the plaintiff will lose.
- If the plaintiff wins at trial, the defendant is ordered to pay the exact amount of the platinff's total injury (i.e. an amount between **$50.00** and **$200.00**). If the plaintiff loses, no payment is ordered.
- Going to trial costs the plaintiff **$11.00** and the defendant **$5.00**.
- At the end of the experiment, you will be paid **0.50%** of your total experimental earnings.

C.2 Example Interface Explanation

The interface explanation is read aloud immediately following initial instructions, but before the first round of bargaining. The explanation is generated by the experimental software and varies by treatment to account for differences in the information environment factor, $\mathcal{I}$. An example screenshot for the control treatment follows.

Screenshot 7: Example Interface Explanation $\mathbf{T}_A = \mathbf{T}_0$

- Everyone sees basically the same screen. If you are a plaintiff, the top left box will be blue, and you will be able to perform actions in it during bargaining. If you are a defendant, the top right box will be blue, and you will be able to perform actions in it during bargaining. The screen is otherwise the same for both roles.
- The top middle box labeled **Current Proposal** displays the current settlement proposal—in real time—through a round of bargaining. The current proposal defaults to **$0.00**, but changes anytime the defendant decides to change the current proposal.
- If you are a defendant, you have two action fields in the **Defendant** box. In the text-box you may enter the amount of the settlement proposal you want to make at any time. To submit the proposal you type into the text box, press the **Change Proposal** button. This will immediately update the value displayed in the **Current Proposal** box for you and whatever plaintiff you are paired with in a given round. As a defendant, you may change the current proposal as frequently, or infrequently, as you want during a round.
- If you are a plaintiff, you have only a single action field in the **Plaintiff** box. The button says **Accept Proposed Amount.** Clicking this button at any time marks a settlement agreement---stopping negotiation and transferring the amount displayed in the **Current Proposal** box from the defendant to yourself. If you want to settle for the current proposal, click this button; if you do not want to settle for the current proposal, simple do not click this button; if you intend to take a dispute to trial, then never click this button. There is no request for confirmation, so only click the **Accept Proposed Amount** button if you want to settle for the current proposal.
- The large middle box keeps track of information that may be relevant to you during bargaining.
    - Under the **Negotiation Status** heading, **time remaining** keeps track of the time remaining in a given round. It starts at **2:00** and counts down to **0:00**.
    - Under the **Information** heading you will see reminders of costs and probabilities described in the previous instructions. At the bottom of the **Information** section is a field labeled **potential damages:**
        - If you are a plaintiff, this field indicates exactly the value of damages to be assigned if the plaintiff wins at trial (it is called potential damages because no one knows who would win if the case went to trial).
        - If you are a defendant, you will not be able to see the value of potential damages during bargaining. Instead, you will see a generic placeholder that reads **[$50.00 - $200.00]** , to remind you the range of values on which potential damages can lie.
    - Under the **Round Earnings** heading, the bargaining interface automatically keeps track of cash flows such as interest and negotiation costs during a round. If you are a plaintiff, the top field labeled **income** will switch to **income – injury** as soon as a round begins; this is a reminder that the extent of the injury is subtracted from the plaintiff's income at the start of a round.
    - Under the **Cumulative Earnings** heading, the bargaining interface automatically keeps track of total experimental earnings so far.
- At the bottom of the screen is a gray box labeled **History.** This box provides quick reminders of the outcomes of previous rounds of bargaining, in case you ever want to check what happened previously.

C.3 Example Instructions for Second Treatment

Instructions corresponding to the second treatment in a sequence, $\mathbf{T}_B$, are simultaneously displayed on subjects' computer screens and read aloud by the experimenter. Differences from the prior bargaining environment are marked in red. The instructions are generated by the experimental software, and vary by treatment to account for differences in factor levels. As an example, screenshots of instructions for the symmetric information treatment, $\mathbf{T}_1$, follow. Pages without changes are omitted.

Screenshot 8: Example Instructions $\mathbf{T}_B = \mathbf{T}_1$: Page 1 of 6

Instructions Part II (Page 1 of 6): Experiment Overview

(Changes marked in red.)

This experiment involves bargaining during a lawsuit. The lawsuit is a legal dispute between an injured party (the **plaintiff**) and an injurer (the **defendant**). Parties bargain over a potential settlement---a payment the defendant may make to the plaintiff in order to avoid going to trial. The experiment has several rounds; think of each round as a **completely separate lawsuit**.

In every round of this experiment, you will play the role of the **[your role]**. Each round you are assigned to bargain with a randomly selected **[other party]**. All bargaining is anonymous, so no one will ever know who they were assigned to bargain with in any round.

All bargaining is done with **experimental money**. How you bargain determines how much experimental money you gain each round. At the end of the experiment, you will be paid **0.50%** of your accumulated experimental money in U.S. dollars.

To Summarize:

- In this experiment, you will play the role of the **[your role]**.
- Each round, you are assigned to bargain with a **randomly selected [other party]**; you will not usually be paired with the same **[other party]** in consecutive rounds.
- At the end of the experiment, you will be paid **0.50%** of your total experimental earnings.

Screenshot 9: Example Instructions $\mathbf{T}_B = \mathbf{T}_1$: Page 2 of 6

Instructions Part II (Page 2 of 6): Income and Injury

You start the experiment with an initial stock of **[some amount]** in experimental money. You also earn a fixed income of **[some amount]** each round. No one but you knows how much money you start with, and how much you earn as income each round.

At the start of every round, each plaintiff is **injured by a defendant**: this injury is the source of the lawsuit. The cost of the plaintiff's injury is randomly determined at the start of every round. The plaintiff always suffers an economic injury of **$50.00** (something like the cost of a medical bill), but also suffers a pain and suffering injury which is equally likely to be any amount between **$0.00** and **$150.00** (something like discomfort and depression due to the injury). The plaintiff's total injury is thus equally likely to be any amount between **$50.00** and **$200.00**.

Interactive Example:

- Click the following button a few times to see example injuries: Generate Example

Economic Injury	+	Pain and Suffering Injury	=	**Total Injury**
$50.00	+	$36.90	=	**$86.90**

The full amount of the total injury is subtracted from the plaintiff's income at the start of a round. At the beginning of a round, both the plaintiff and defendant are told the size of the plaintiff's injury for that round.

To Summarize:

- You begin the experiment with **[some amount]** and get at most **[some amount]** in income each round.
- The defendant always receives exactly his/her income at the start of a round. The plaintiff, on the other hand, receives the amount of his/her income **minus** the amount of his/her total injury in that round.
- The size of the plaintiff's injury varies from round to round. The plaintiff suffers an economic injury of **$50.00**, and a pain and suffering injury which is equally likely to be any amount between **$0.00** and **$150.00**. The plaintiff's total injury is thus equally likely to be any amount between **$50.00** and **$200.00**.
- At the beginning of a round, both the plaintiff and defendant are told the size of the plaintiff's injury for that round.

Screenshot 10: Example Instructions $\mathbf{T}_B = \mathbf{T}_1$: Page 5 of 6

Instructions Part II (Page 5 of 6): Trial

If you and the **[other party]** have not agreed on an acceptable settlement amount by the end of **2 minutes**, the lawsuit goes to trial where a judge **decides who wins the case**. Going to trial is **costly**. It costs the plaintiff **$11.00** and the defendant **$5.00**. Trial costs are paid at the end of the round (without interest) in addition to accumulated negotiation costs. Rules of the trial follow:

- There is always a **75%** chance that the plaintiff will win at trial, and a **25%** chance that the plaintiff will lose.
- If the plaintiff wins, the defendant is ordered to pay the exact amount of the platinff's total injury (i.e. an amount between **$50.00** and **$200.00**). If the plaintiff loses, no payment is ordered.

Interactive Example:

- Click the following button a few times to see example potential damages: Generate Example

Total Injury	=	**Potential Damages**
$142.01	=	**$142.01**

Although no one knows who would win if the case went to trial, both plaintiff and defendant know the exact amount that could be won at trial. (This is because both parties know the size of the plaintiff's total injury.)

To Summarize:

- Going to trial costs the plaintiff **$11.00** and the defendant **$5.00**.
- There is always a **75%** chance that the plaintiff will win at trial, and a **25%** chance that the plaintiff will lose.
- If the plaintiff wins at trial, the defendant is ordered to pay the exact amount of the platinff's total injury (i.e. an amount between **$50.00** and **$200.00**). If the plaintiff loses, no payment is ordered.
- Neither party knows who would win a trial, but both parties know exactly how much could be won.

Screenshot 11: Example Instructions $\mathbf{T}_B = \mathbf{T}_1$: Page 6 of 6

Instructions Part II (Page 6 of 6): Summary

- In this experiment, you will play the role of the **[your role]**.
- Each round, you are assigned to bargain with a **randomly selected [other party]**; you will not usually be paired with the same **[other party]** in consecutive rounds.
- You begin the experiment with **[some amount]** and get at most **[some amount]** in income each round.
- The defendant always receives exactly his/her income at the start of a round. The plaintiff, on the other hand, receives the amount of his/her income **minus** the amount of his/her total injury in that round.
- The size of the plaintiff's injury varies from round to round. The plaintiff suffers an economic injury of **$50.00**, and a pain and suffering injury which is equally likely to be any amount between **$0.00** and **$150.00**. The plaintiff's total injury is thus equally likely to be any amount between **$50.00** and **$200.00**.
- At the beginning of a round, both the plaintiff and defendant are told the size of the plaintiff's injury for that round.
- Everyone earns interest on current-round money at the rate of **0.10%** (compounded every second).
- In each round, you and the **[other party]** have **2 minutes** to bargain over a possible settlement.
- Every second of bargaining costs the plaintiff **$0.14** and the defendant **$0.32**, plus interest.
- Settlement freezes bargaining costs, but you will continue to earn interest for the rest of the round.
- If you and the **[other party]** do not agree on a settlement amount, the round ends with a trial.
- There is always a **75%** chance that the plaintiff will win at trial, and a **25%** chance that the plaintiff will lose.
- If the plaintiff wins at trial, the defendant is ordered to pay the exact amount of the platinff's total injury (i.e. an amount between **$50.00** and **$200.00**). If the plaintiff loses, no payment is ordered.
- Going to trial costs the plaintiff **$11.00** and the defendant **$5.00**.
- At the end of the experiment, you will be paid **0.50%** of your total experimental earnings.

Chapter IV

Sub-Experiment 1: Settlement Bargaining Profile

Sub-Experiment 1 (SE1) explores measurements collected during exposure to the control treatment, $\mathbf{T}_0$. Assigned as the first treatment, $\mathbf{T}_A$, in some sequences and as the second treatment, $\mathbf{T}_B$, in others, all data in SE1 concerns exposure to the control treatment only. The basic objective of this sub-experiment is exploratory: data reveal patterns of behavior when payment-incentivized laboratory subjects take the roles of litigants in the settlement bargaining game with asymmetric information.

Section 7 completes the definition of $\mathbf{T}_0$ begun in Chapter III. Values are provided for the control parameter vector, $\mathcal{P}_0$. Theoretic predictions for the control treatment are discussed and illustrated. Caveats to data interpretation are noted for practical design limitations and previously discussed behavioral regularities.

Section 8 discusses the results of SE1. Collected data provide a detailed profile of observed settlement bargaining behavior. Points of particular interest include patterns of play in individual settlement bargaining disputes, properties of average proposal sequences, properties of average settlement decisions, and properties of average resolution delay. A few areas of serious discord aside, SE1 data are generally quite consistent with theoretic prediction.

Section 9 provides concluding discussion. Emphasized comparisons include the consistency of observed and predicted behavior, and the consistency of behavior across assignments $\mathbf{T}_A$ and $\mathbf{T}_B$. Side commentary is also provided on the design of proposal sequence elicitation.

7 Treatments

SE1 concerns the settlement bargaining behavior of payment-incentivized laboratory subjects when assigned to the control treatment, $\mathbf{T}_0$. Half of the SE1 data are collected with $\mathbf{T}_0$ assigned as the first treatment in a sequence, $\mathbf{T}_A$; the other half are collected with $\mathbf{T}_0$ assigned as the second treatment, $\mathbf{T}_B$. Section 7.1 presents the control treatment and associated theoretic predictions.

7.1 Control

The control treatment, $\mathbf{T}_0$, corresponds closely to the theoretic model of settlement bargaining with asymmetric information presented in Section 3.2. The treatment defines an experimental bargaining environment wherein (i) the plaintiff is asymmetrically informed about the value of a potential trial verdict, (ii) parameter values are set to control values as defined below, (iii) no reform regime is imposed, and (iv) subjects are drawn from the undergraduate-student subject pool.

The control level of the parameter values factor, $\mathcal{P}_0$, is defined in the first and second columns of Table 9. Set by exploration in pilot studies, the control vector of parameter values admits several attractive properties. First, it corresponds to the more relevant theoretic prediction of delayed settlement (i.e. an interior solution as opposed to a boundary solution with immediate settlement of all disputes). Second, control parameter values leave room for substantial perturbations in other treatments. Third, chosen parameter values afford predictable bounds on subject earnings under reasonable play (providing additional safeguards against negative earnings). Fourth, constituent values are easy for subjects to understand and remember during a session (e.g. round numbers are preferred to whole numbers, whole numbers are preferred to fractions, fractions are preferred to irrational numbers, etc).

Table 9: Control Parameter Values

Parameter	Value	Translation to Experimental Environment
$\underline{x}$	\$50.00	economic injury = \$50.00
$\overline{x}$	\$200.00	pain and suffering $\in [\$0.00, \$150.00]$
π	0.75	(direct translation)
T	120	continuous bargaining
δ	$^{1000}/_{1001}$	$r = 0.001$
c_p	\$0.14	(direct translation)
c_d	\$0.32	(direct translation)
k_p	\$11.00	(direct translation)
k_d	\$5.00	(direct translation)

Details on the translation of $\mathcal{P}_0$ parameter values to the experimental bargaining environment are provided in the third column of Table 9. The lower bound on potential damages, $\underline{x}$, is presented as a commonly known economic injury which is the same in every dispute. The difference $\overline{x} - \underline{x}$ is presented as the upper bound on the size of a random pain and suffering injury; the pain and suffering injury is uniformly distributed on $[0, \overline{x} - \underline{x}]$, varies by dispute, and is the private information of the plaintiff. To affect an inter-temporal discount rate of δ, stocks of wealth accumulate interest at a rate of r, compounded each second during a dispute.

Equilibrium strategies under control parameter values are defined by the interior solutions in Propositions 1 and 2 of Chapter II. As $\mathcal{P}_0$ parameters satisfy the persistent-delay condition in Proposition 3, settlement delay is predicted not just for a game of length $T = 120$, but also as period duration becomes arbitrarily fine.[67] The

[67]Satisfaction of Proposition 3 addresses the rather esoteric concern that subjects perceive the settlement bargaining game to be played in continuous time. The distinction from a game of 120 second-long periods is probably of limited practical importance.

predicted equilibrium path of play in the control treatment follows from substituting $\mathcal{P}_0$ parameter values into Corollary 1; the *ex ante* probability of settlement over time is given by Corollary 2. Rather than give the equations themselves, resulting predictions for equilibrium play under $\mathbf{T}_0$ are illustrated in Figure 8.

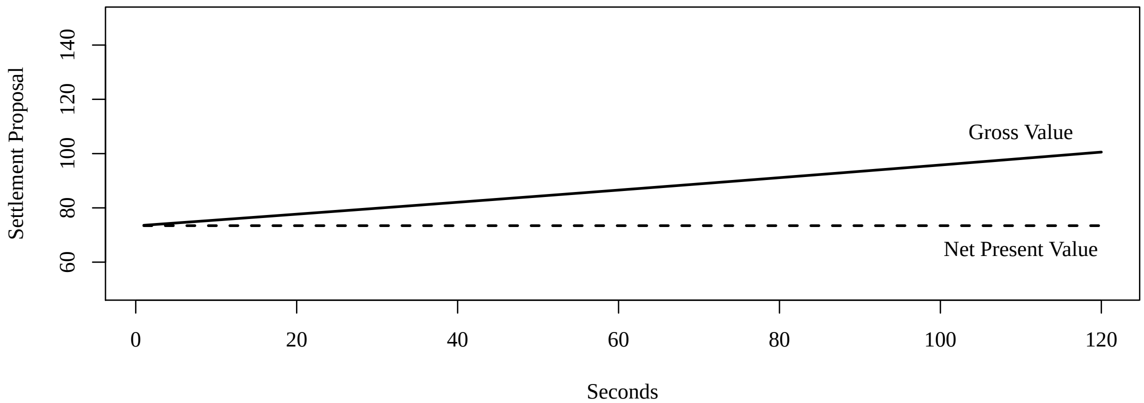
Figure 8(a) illustrates the equilibrium sequence of settlement proposals. In gross value (GV), a defendant makes an initial proposal of \$73.58. The defendant slightly increments the proposal each second, leading to a final proposal of \$100.56 in the last second of bargaining. The dashed line in Figure 8(a) illustrates the net present value (NPV) of each settlement proposal from the perspective of a plaintiff. Equilibrium proposals afford a constant NPV of \$73.44 in every period (i.e. second) of a dispute.

As noted in the discussion of Lemma 1 in Section 3.2, the symmetry of settlement preferences across all types of plaintiff drives the flat trajectory of NPV settlement proposals. A plaintiff's type only affects the value of a trial verdict, so if *any* type of plaintiff strictly prefers settlement at a particular proposal in a sequence, then *every* type of plaintiff must also strictly prefer settlement at this point. This intuition, combined with the need for a positive measure of plaintiff-types to settle in each period in an interior equilibrium, explains the shape of the NPV proposal sequence.

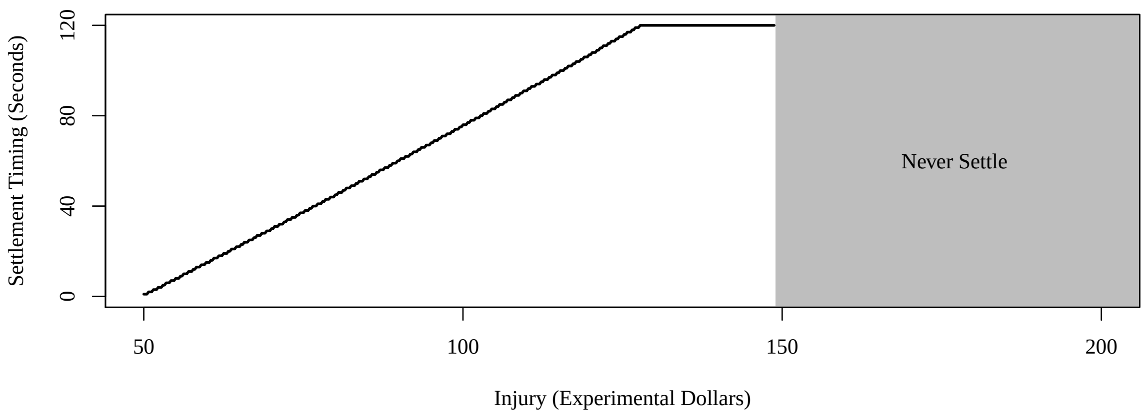
For the present experimental design, observed NPV proposal sequences are unlikely to be anywhere near perfectly flat. It would be physically impractical (or at least very tiring) for subjects in the laboratory to type and submit a new proposal every second.[68] In light of practical constraints, a sequence of proposals that is approximately level and distributes around a NPV of \$73.44 would tend to indicate strong consistency with theory.

[68] In fact, following each submission of a settlement proposal, the online bargaining interface imposes a one-second moratorium on further proposals in that dispute. This built-in delay acts as a safeguard against artificial race conditions wherein very rapid submission of settlement proposals can result in a plaintiff's acceptance of an unintended proposal. Observed data and informal *ex post* discussions with subjects suggest that this brief delay was very rarely a binding constraint.

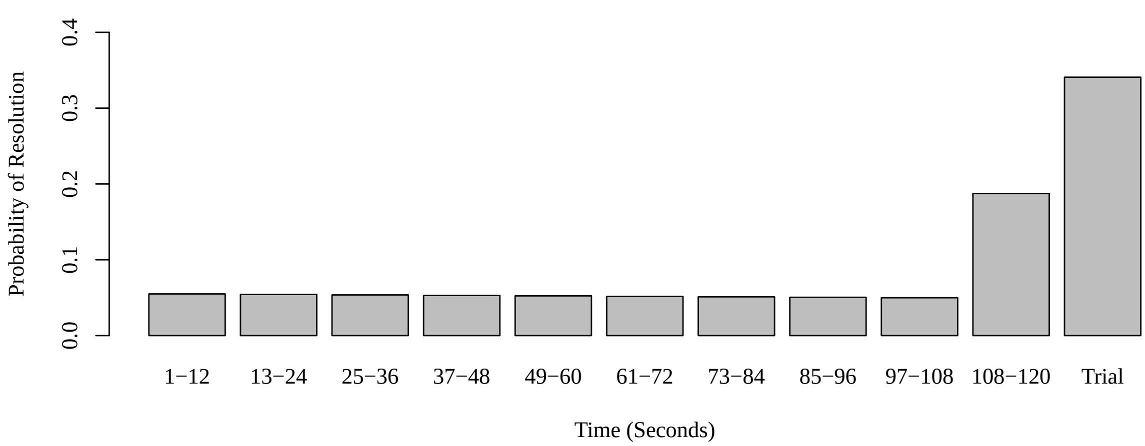
Figure 8: Theoretic Predictions for the Control Treatment



(a) Sequence of Settlement Proposals in Gross Value and Net Present Value



(b) Equilibrium Settlement Decision/Timing by Injury



(c) Distribution of Resolution Timing

Remark 1. The equilibrium sequence of settlement proposals admits both weak and strong interpretations. The *weak interpretation* is that observed settlement proposals (those preceding a settlement) have a constant NPV of $73.44. The *strong interpretation* is that all proposals (even those foreclosed from being made by prior settlement) have a constant NPV of $73.44.

Figure 8(b) illustrates equilibrium acceptance decisions. In the control treatment, potential damages are identically a plaintiff's injury. When presented with the equilibrium proposal sequence, each plaintiff with injury less than $148.88 settles at the illustrated time. Recall from discussion in Section 3.2, that the sequence in which types of plaintiff settle is determined not by plaintiff preferences—in fact, it is premised on the plaintiff's indifference between all equilibrium proposals—but by the need to make this sequence of settlement proposals sequentially rational from the defendant's perspective. Although the timing of settlement is a deterministic function of potential damages (i.e. a pure strategy), the intuition is that of a randomized-strategy equilibrium.

Two noteworthy characteristics of Figure 8(b) are the mass of plaintiff-types that settle in the final second of bargaining, and the mass of types that never settle. Any plaintiff with injury draw between about $127.54 and $148.88 waits until the final period of bargaining to settle. The discontinuity in settlement in the final second (i.e. the large mass of plaintiff types that settle in this period as opposed to any other) owes to the one-time trial costs associated with receipt of a trial verdict in the following period. A plaintiff with injury draw in excess of about $148.88 refuses to settle for any equilibrium proposal. The intuition is straightforward: with all settlement proposals sharing a common NPV of $73.44, a plaintiff with injury in excess of $148.88 nets a greater expected return from a trial verdict than from settlement.

It should be clear these equilibrium settlement rules are specific to realization of the equilibrium proposal sequence. If a plaintiff is presented with proposals that differ from equilibrium predictions, individual rationality simply requires settlement for the greatest NPV proposal unless the expected trial verdict is preferred to every proposal. Because sharp predictions depend heavily on the anticipated sequence of settlement proposals, it is difficult to be precise about the types of observed settlement decisions that are strongly consistent or inconsistent with theoretic behavior.

Remark 2. For the equilibrium proposal sequence, a plaintiff with injury draw in excess of \$148.88 never settles. The delay-to-settlement for a plaintiff with injury draw less than \$148.88 is monotone increasing in the size of the injury draw.

Remark 3. Regardless of the realized sequence of settlement proposals, a plaintiff never settles for a proposal with NPV less than $W_p(x)$, the expected net present value of a trial verdict for the plaintiff's injury draw.

Figure 8(c) illustrates the equilibrium distribution of settlement delay. Settlements occur in slightly decreasing frequency over the course of a dispute, with a spike in settlement at the end of bargaining and with a large mass of plaintiff types failing to settle at all. The end-of-bargaining spike in settlement frequency corresponds to the mass of plaintiff types that settle in the final period of bargaining.

Corollary 3. *Let D_R denote delay-to-resolution, the time between initiation of a dispute and either settlement or trial verdict. Let D_S denote delay-to-settlement, the former definition when restricted to the subset of disputes that settle. Probability mass functions follow immediately from the definition of p_t in Corollary 2:*

$$f_{D_R}(t) = \begin{cases} p_t & t = 1, \ldots, T+1 \\ 0 & \textit{otherwise} \end{cases} \qquad f_{D_S}(t) = \begin{cases} p_t/(1 - p_{T+1}) & t = 1, \ldots, T \\ 0 & \textit{otherwise.} \end{cases}$$

Substituting $\mathcal{P}_0$ parameter values into Corollaries 2 and 3, equilibrium play involves an average delay-to-resolution of about 89 seconds, with around 34% of disputes resolved by trial verdicts.[69] Average delay-to-settlement (by definition conditional on settlement of a dispute) is around 72 seconds. These averages are derived as simple functions of the prior comments on equilibrium play—i.e. as the result of interactions between equilibrium proposal and acceptance strategies.

Remark 4. Each second of a dispute corresponds to a unique settlement proposal. On average, equilibrium play implies 89 proposals per dispute; conditional on settlement, equilibrium play implies an average of 72 proposals.

As explained in Section 4.2, there are no clear behavioral predictions for the settlement bargaining game. Behavioral research on other bargaining games does, however, suggest several qualifications for the interpretation of experimental data. For example, if information is imperfectly controlled in this experiment, then deviations from the sharp theoretical predictions for proposals do not necessarily indicate a failure of theory: e.g. deviations may reflect rational strategies in attendance to uncontrolled information asymmetry. Prior research also suggests that with imperfectly controlled preferences, apparent deviations from theory may have rational motivations: e.g. disadvantageous trial outcomes may reflect a taste for fairness if trial is perceived to be more equitable than settlement, or is intended to punish an inequitable defendant.

These qualifications recommend a cautious interpretation of observed bargaining behavior. In the following section, results are pointedly presented as *consistencies* and *inconsistencies* between observed behavior and theoretic prediction. Inconsistencies constitute sound rejections of theoretic-model point predictions, but do not generally suggest the reason for disagreement between theory and practice.

[69] Note that in calculating average delay-to-resolution, a trial verdict is assumed to occur in period $T+1$: i.e. at 121 seconds.

8 Results

The objective of SE1 is broadly exploratory: collected data are used to provide a general profile of settlement bargaining behavior under the control treatment, $\mathbf{T}_0$. Results highlight observed patterns of behavior with particular emphasis on comparison to theoretic predictions. Three comments on data analysis are generally applicable.

First, the following analysis takes care to distinguish measurements by the order of treatment assignment: i.e. distinguish between data collected when $\mathbf{T}_0$ is assigned as $\mathbf{T}_A$ versus $\mathbf{T}_B$. Measurements collected when $\mathbf{T}_0$ is assigned as $\mathbf{T}_B$ are arguably less reliable than those collected during $\mathbf{T}_A$ assignment, owing to the $\mathbf{T}_B$ assignment's greater sensitivity to order and carryover effects (see Section 5.5). Second, data from the first two rounds of a treatment assignment are omitted from analysis: i.e. rounds 1 and 2 are dropped when $\mathbf{T}_0$ is assigned as $\mathbf{T}_A$ and rounds 8 and 9 are dropped when $\mathbf{T}_0$ is assigned as $\mathbf{T}_B$. Dropping the initial rounds of an assignment provides an experimental control for potential design bias introduced by rapid learning and strategy-adjustment in the early rounds of exposure to a treatment.[70] Third, when data from a single round are needed to illustrate behavior, the following analysis adopts the convention of using the final round of an assignment. For notational convenience, let $\mathbf{T}_{A,7}$ denote the final round of a $\mathbf{T}_A$ assignment (i.e. round 7) and let $\mathbf{T}_{B,14}$ denote the final round of a $\mathbf{T}_B$ assignment (i.e. round 14).

The remainder of this section proceeds as follows. Section 8.1 provides informal commentary drawn from viewing replays of all SE1 disputes. Sections 8.2 and 8.3 summarize properties of average proposal sequences and settlement decisions, respectively. Section 8.4 discusses the average timing of dispute resolution.

[70] Reliance on common random number sequences across sessions renders stability untestable, because changes in play between rounds are not identifiable from changes in parameters and matchings. As a simple heuristic, play is presumed stable by the third round of a treatment assignment.

8.1 Individual Disputes

Presentation of results begins with an informal look at individual disputes. As in any exploratory analysis, the objective is to discover patterns of behavior which may be more rigorously examined in subsequent confirmatory studies. Informal assessment is aided by continuous-time replays of SE1 disputes, available in an online appendix at `http://people.virginia.edu/~sps2d/settlement_bargaining_replays/`. Details of the online appendix are provided in Appendix D.1, and static examples of dispute replays are illustrated in Figure 9.

An important nuance in the analysis of dispute replays is the distinction between explicit and implicit proposals. A defendant submits an *explicit* proposal by typing a dollar value into the proposal field of the online interface and pressing the submit button (see Figure 6). Explicit proposals stand until revised or accepted, defining *implicit* proposals as the value of the last explicit proposal prior to revision.[71] Intuitively, this makes gross value (GV) proposal sequences look like step functions, and makes net present value (NPV) proposal sequences look like saw-tooth functions.

In dispute replays, explicit proposals are represented by black dots, and implicit proposals are represented by black connecting lines between dots. Settlement is indicated by a vertical orange line drawn at the time of agreement, and failure to settle is indicated by a vertical red line at the conclusion of the round. Context lines illustrate controlled private information: one line (labeled "E [Trial]") indicates the expected value of a trial verdict given the plaintiff's injury draw, while the other (labeled "Injury") indicates the size of the injury itself. Finally, predicted proposals and settlement decisions are represented by blue (unlabeled) lines of appropriate shape.

[71] For example, in a dispute lasting 120 seconds and ending in a trial verdict, a defendant's submission of 5 explicit proposals implies $120 - 5 = 115$ implicit proposals.

Static examples of dispute replays are provided in Figure 9. Figure 9(a) illustrates a dispute where behavior approximates theoretic predictions. The sequence of settlement proposals is approximately level at the predicted value and the timing of settlement is close to prediction for the plaintiff's injury draw. By contrast, the dispute illustrated in Figure 9(b) is inconsistent with theoretic predictions. The defendant makes a monotone decreasing sequence of settlement proposals; the plaintiff settles early (rather than in the last second of bargaining as predicted for the injury draw) and not for the largest NPV proposal made by the defendant.

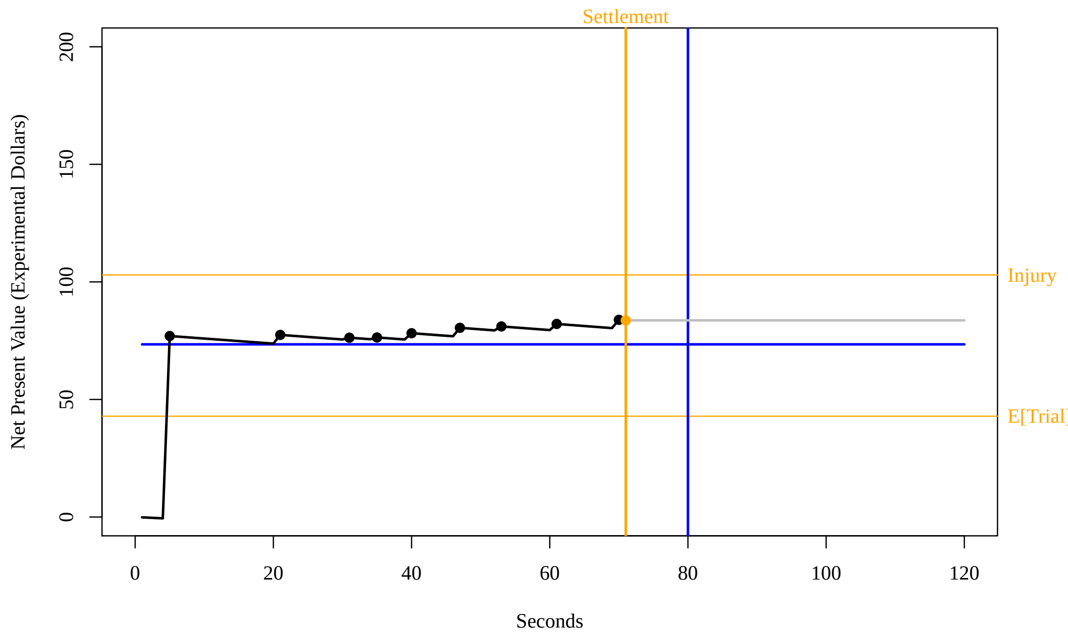
Though Figure 9 reflects the variety of play observed in SE1 disputes, neither illustration is particularly representative of average play. A better feel for typical modes of play is afforded by the 600 continuous-time dispute replays available in the online appendix. An informal look reveals several interesting patterns of play.

Result 1. Assessed individually, few dispute replays conform to the predicted equilibrium patterns for proposal and acceptance.

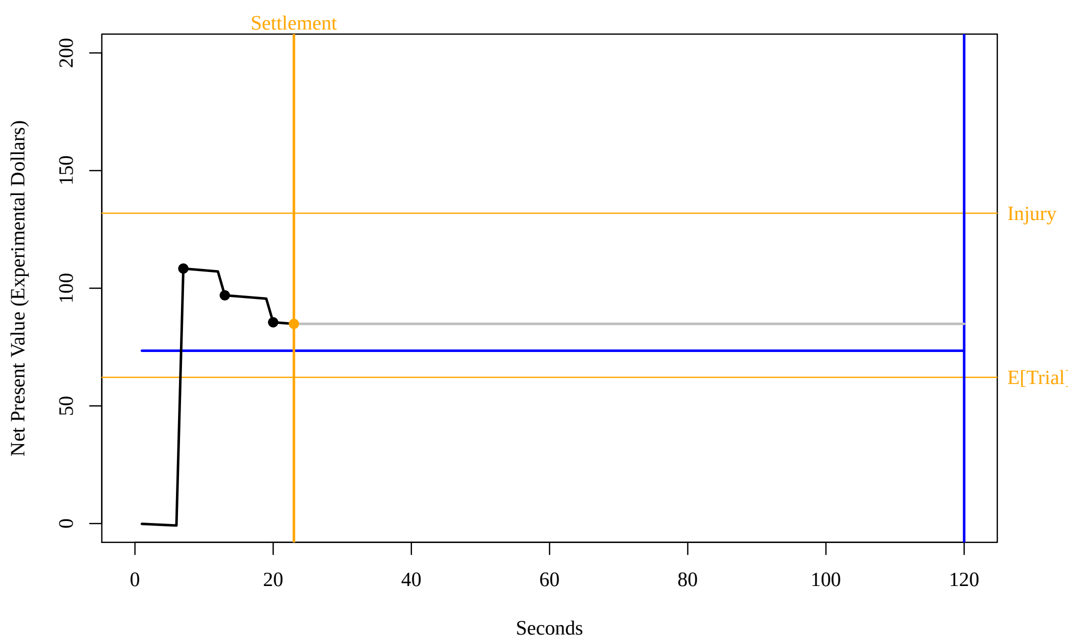
In contrast to equilibrium proposal sequences, NPV proposals in most SE1 disputes are generally increasing over time. Although it is difficult to definitively categorize the direction of many proposal sequences, by my judgment about 1/3 of SE1 disputes involve a sequence of proposals that is clearly increasing over the course of a round (i.e. roughly monotone increasing at an appreciable rate).

The payoff maximizing response for a plaintiff facing a monotone increasing sequence of NPV proposals is to settle just before the trial verdict, if at all. While many disputes with monotone increasing proposal sequences do involve last minute settlements, a non-trivial number actually involve early to mid-game settlement. These anomalously early settlements often evince the common property of occurring just as the sequence of proposals reaches the value of the plaintiff's injury draw.

Figure 9: Example Illustrations of Individual Disputes[a]



(a) Dispute in which plaintiff and defendant act in rough accordance with theoretic predictions.



(b) Dispute in which play is inconsistent with theory. The defendant makes a decreaseing sequence of settlement proposals. The plaintiff settles earlier than predicted, and for less than the initial proposal.

[a]Black dots represent explicit proposals; black connecting lines represent implicit proposals. Settlement is illustrated by a vertical orange line drawn at the time of agreement. Context lines illustrate controlled private information. Predicted proposals and settlement decisions are represented by blue (unlabeled) lines of appropriate shape.

Among other explanations, such behavior is consistent with the plaintiff having a taste for fairness centered on full recovery of the total injury. In contrast to theoretic focus on the expected net present value of a trial verdict, my impression from watching SE1 disputes is that the nominal injury value (i.e. the injury draw unweighted by bargaining costs or the probability of a plaintiff-verdict) is the more obvious focal point in many disputes.

Less frequent than strictly increasing sequences of settlement proposals, a number of disputes (perhaps 10%) instead involve a roughly monotone-decreasing sequence of NPV proposals. A short example is the game in Figure 9(b). Declining sequences of proposals elicit strong responses. While settlements occur with some frequency during protracted runs of declining proposals, more frequently settlement is affected by an even moderately more generous proposal after an extended declining run.

A similar number of disputes (again, maybe 10%) involve approximately flat NPV proposal sequences. An example is illustrated in Figure 9(a). Even in this subset of disputes, behavior is not generically consistent with theoretic prediction. Overall, these proposal sequences appear to be more generous than predicted by theory, and settlement decisions do not usually correspond with predicted timing.

Finally, the remaining mass of disputes involve proposal sequences that are not clearly increasing, decreasing, or level over time. Such sequences are often sharply non-monotone, with either saw-tooth patterns of spikes followed by declining runs, or seemingly random jumps between series of high and low proposals apropos of nothing.

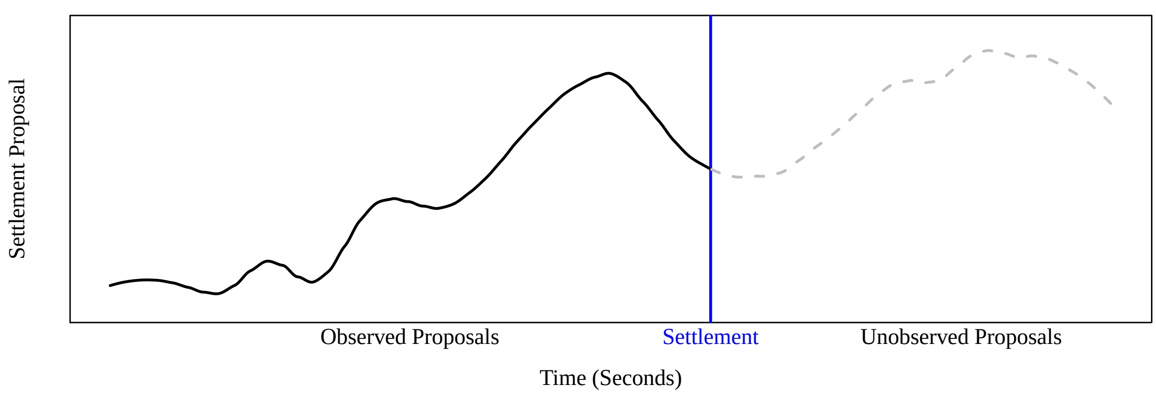
While it is difficult to draw any definite conclusions about litigant behavior in this remaining mass of disputes, an interesting speculation is that experience with highly erratic proposal sequences could tend to induce early settlement in subsequent disputes. For example, a plaintiff facing an increasing sequence of settlement proposals may elect to settle early out of concern that the defendant may suddenly change to a low series of proposals. Experience with erratic sequences of settlement proposals (even if only from a subset of defendants) might then help to explain some of the anomalously early settlements noted previously.

8.2 Average Proposal Sequences

In contrast to Section 8.1, which seems to suggest that SE1 proposal sequences are largely incompatible with theoretic predictions, results from formal analysis of average proposal sequences are more demure. Though statistically distinguishable from equilibrium play in several regards, average proposal sequences tend not to stray too far from predictions and actual inconsistencies with theory are modest at best.

Analysis in the present section maintains the terminology of Section 8.1 distinguishing between *explicit* and *implicit* settlement proposals. Additionally, a distinction is drawn between *observed* settlement proposals (those made prior to settlement) and *unobserved* proposals (those foreclosed from being made as a result of settlement). Figure 10 illustrates this definition of observed and unobserved proposals. For narrative convenience, the *observed* descriptor is omitted where contextually obvious.

Figure 10: Illustration of Observed and Unobserved Proposals



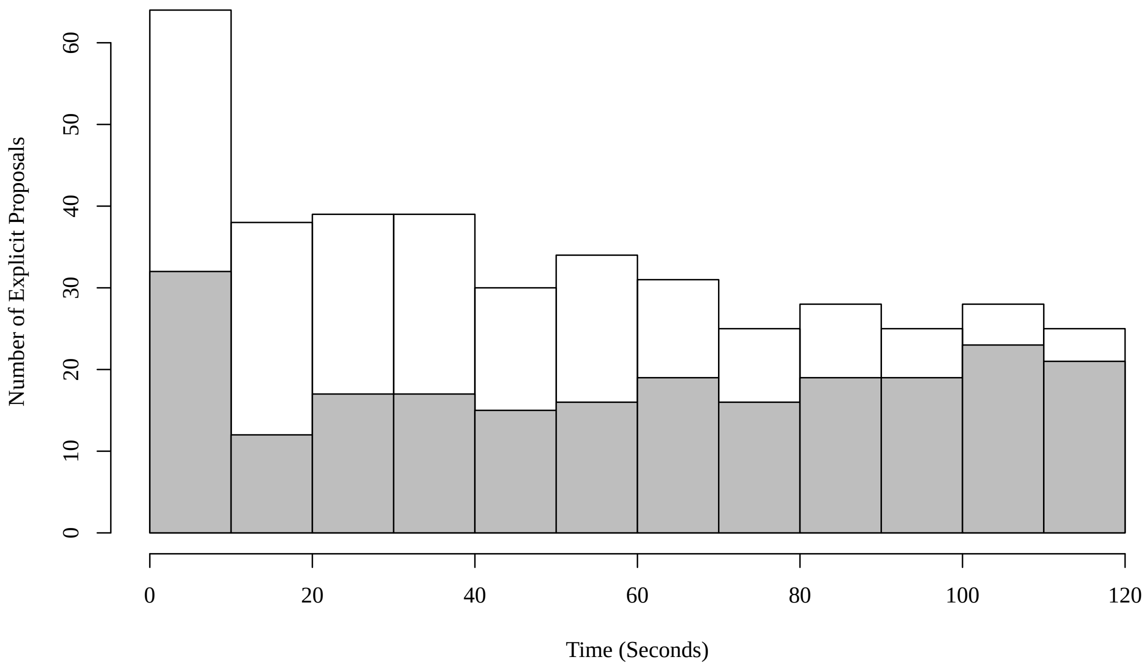
A starting point is summary analysis of explicit proposals. As noted in Remark 4, the theoretic equilibrium demands a different proposal in every second of bargaining, leading to an expectation of 89 explicit proposals per dispute: i.e. one proposal each second for the *ex ante* expected duration of a dispute. The prediction of a typed and submitted proposal every second is admittedly unrealistic, and the relevant inquiry is whether experimental data even begin to approximate theory in this regard.

In SE1, initial explicit proposals are usually submitted within about the first 5–7 seconds of bargaining.[72] Following that, defendants submit an average of only around 5 more explicit proposals over the course of a dispute, with little variation between rounds or treatment assignments. As SE1 disputes last an average of about 84 seconds (see Section 8.4), this equates to around one explicit proposal every 15 seconds. Even judged by the time taken to submit initial proposals (around 6 seconds), intervals of 15 seconds are arguably too long to comport with theoretic prediction.

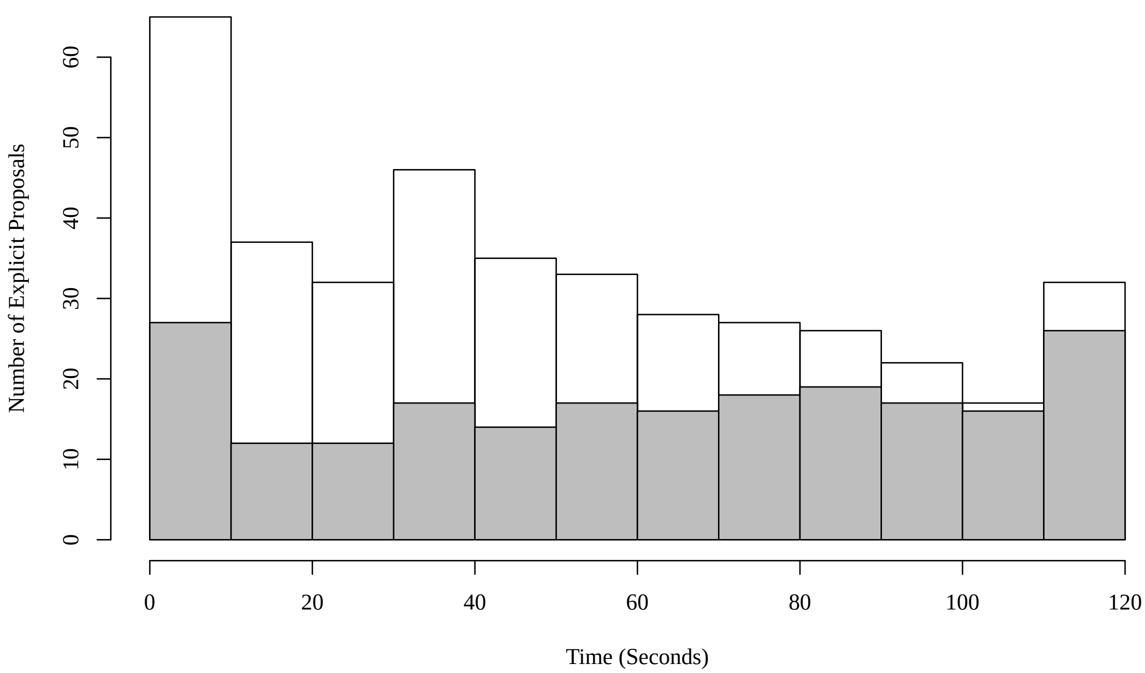
Aggregated by round, data on observed explicit proposals can also used to characterize the average frequency of proposals throughout a dispute. The *absolute frequency* (i.e. number) of explicit proposals over time is illustrated in Figure 11 for assignments $\mathbf{T}_{A,7}$ and $\mathbf{T}_{B,14}$; illustrations for all other rounds are provided in Appendix D.2. White bars illustrate the frequency of explicit proposals across all disputes, while superimposed gray bars illustrate the frequency of explicit proposals for only the subset of disputes that end in a trial verdict (i.e. disputes that never settle). Marginal of ultimate resolution, explicit proposal frequency is greatest in the first 10 seconds of a dispute, evincing a downward trend thereafter as more and more disputes settle. Conditional on failure to settle, explicit proposal frequency admits a modest "U"-shape, with the greatest frequency of proposals being made in the first 10 and last 20 seconds of bargaining.

[72] The interquartile mass of first-proposal times is between about 4 and 8 seconds in every round.

Figure 11: Absolute Frequency of Explicit Proposals in SE1[a]



(a) Absolute Frequency of Explicit Proposals in $\mathbf{T}_{A,7}$



(b) Absolute Frequency of Explicit Proposals in $\mathbf{T}_{B,14}$

[a]White bars illustrate the absolute frequency of explicit settlement proposals across all disputes. Superimposed gray bars illustrate the absolute frequency of explicit proposals for only the subset of disputes that end in a trial verdict.

A more nuanced view of explicit proposal frequency is provided by Figure 12, which illustrates the *relative frequency* (i.e. proportion) of proposal-time combinations in $\mathbf{T}_{A,7}$ and $\mathbf{T}_{B,14}$; illustrations for all other rounds are provided in Appendix D.3. The background gradient is a heat map of proposal-time relative frequencies, with light colors representing greater frequency (see Venables and Ripley, 2002).[73] The solid black line in each plot shows the average explicit proposal over time, and the dashed black line is the flat theoretic prediction of \$73.44.[74]

Figure 12(a) plots explicit proposal data collected in $\mathbf{T}_{A,7}$. Proposals cluster heavily around an NPV of \$75 for about the first 30 seconds of bargaining, trailing off thereafter at an NPV of about \$100.[75] Nearly all explicit proposals fall above the \$73.44 prediction, with most below-prediction proposals occurring early in a dispute. Explicit proposals appear to increase in generosity over about the first 25 seconds of bargaining, stabilizing at an approximately constant level thereafter. Explicit proposal data for other rounds in $\mathbf{T}_A$ are comparable, as illustrated in Appendix D.3.

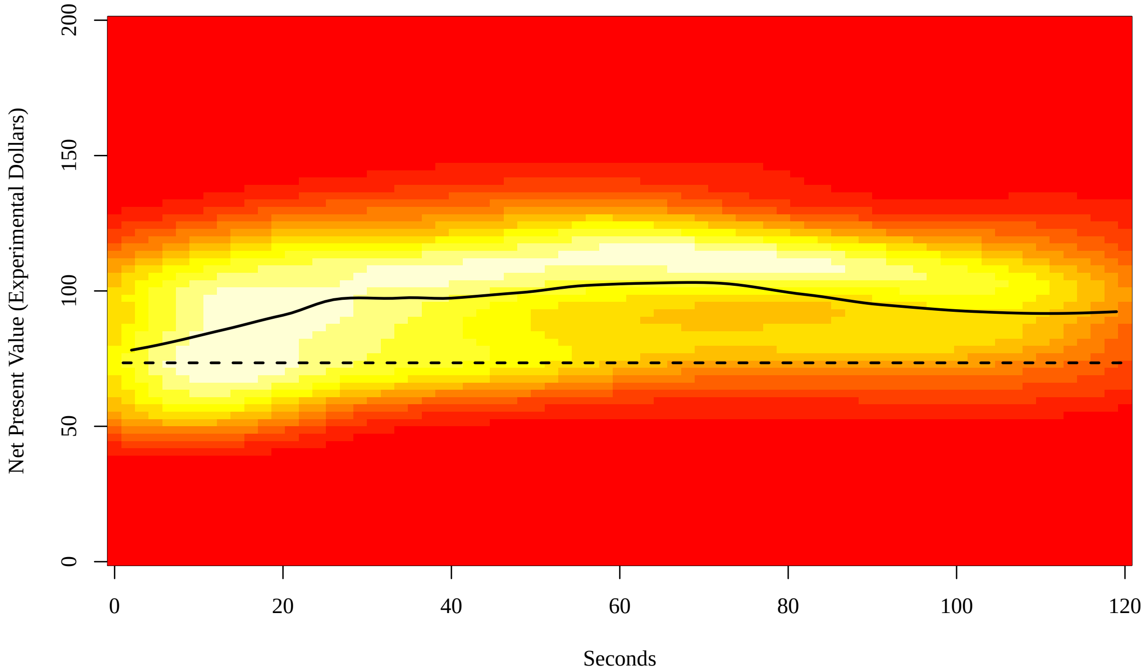
Figure 12(b) plots explicit proposal data collected in $\mathbf{T}_{B,14}$. Relative to Figure 12(a), explicit proposals are more heavily concentrated in the first 40 seconds of bargaining. Most explicit proposals falls above prediction, though below-prediction proposals occur with appreciable relative frequency throughout the dispute. Proposals are less homogeneous by round in $\mathbf{T}_B$, as illustrated in Appendix D.3. Overall differences between assignments $\mathbf{T}_A$ and $\mathbf{T}_B$ are modest, however, and may in part reflect the effects of different random numbers and matchings.

[73]Conceptually, this graphing technique is like a smoothed histogram over proposal-time combinations. Whereas a histogram uses height to illustrate frequency over one dimension, a heat map uses brightness to illustrate frequency over two dimensions.

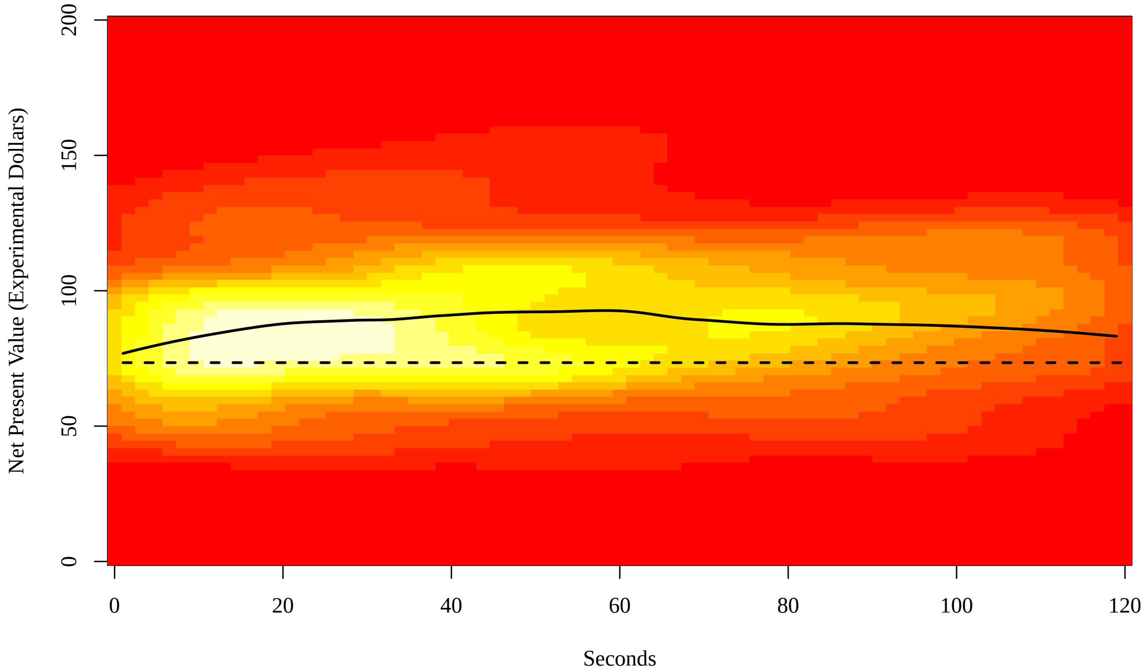
[74]The conditional average is estimated under a Loess model with smoothing parameter $\alpha = 0.5$. Loess is a robust, nonparametric (nearest neighbor) estimator of conditional expected value (see, e.g., Cleveland and Devlin, 1988).

[75]Note that relative frequencies are marginal of dispute resolution. Interpretation of Figure 12 should account for a reduction in the relative frequency of proposals as disputes settle over time.

Figure 12: Relative Frequency of Proposal-Time Observations in SE1[a]



(a) Relative Frequency of Proposal-Time Combinations in $\mathbf{T}_{A,7}$



(b) Relative Frequency of Proposal-Time Combinations in $\mathbf{T}_{B,14}$

[a]The background gradient is a heat map of proposal-time combinations. The solid black line is a robust estimate of average observed explicit proposals over time. The dashed black line represents the theoretic prediction for the NPV proposal sequence.

Though valuable as a measure of bargaining intensity, explicit proposal sequences may be misleading as a measure of average proposal generosity. Since explicit proposals stand (in gross value) until explicitly revised or accepted, explicit proposal sequences tend to overstate the generosity of full NPV proposal sequences. The appropriate measure of average generosity is thus the full sequence of explicit *and* implicit NPV proposals made in SE1 disputes.

Result 2. In some rounds of SE1, average NPV proposals modestly exceed the theoretic prediction.

As noted in Remark 1, equilibrium play pegs the average NPV settlement proposal at $73.44. This theoretic prediction is tested against the average observed NPV proposal in every round of SE1.[76] An *ad hoc* normal-theory test is used to draw inferences from SE1 data, with results illustrated in Figure 13; details on the statistical test are presented in Appendix E.1.

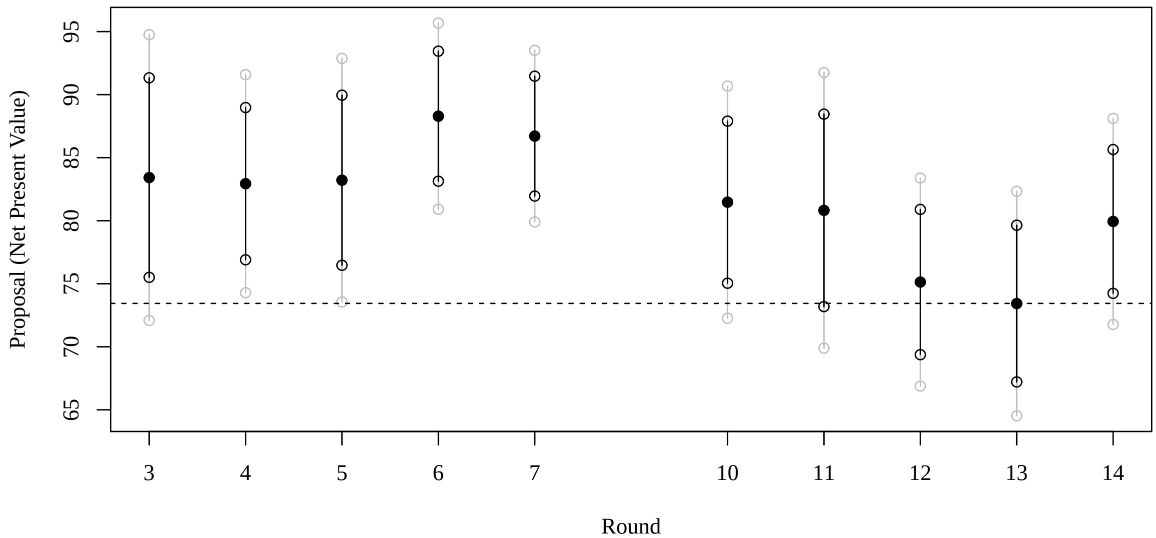
Solid black dots in Figure 13 indicate round-average NPV proposals for $\mathbf{T}_A$ rounds 3–7 and $\mathbf{T}_B$ rounds 10–14. Hollow black dots with vertical connecting lines illustrate individual 95% confidence intervals associated with the underlying expected NPV proposal each round. Hollow gray dots and connecting lines illustrate simultaneous 95% confidence intervals for all 10 expected NPV proposals. Finally, the flat dotted line represents the theoretic prediction of $73.44.

As Figure 13 illustrates, sample-average NPV proposals generally exceed the theoretic prediction, but never by more than $15.[77] In most rounds, a 95% confidence

[76] As a control against potential design bias, analysis omits all NPV proposal data for the first 5 seconds of bargaining. The concern is that initiation of GV proposals at $0 in the bargaining interface may interact with the average delay of 5–7 seconds before the first explicit proposal to create the appearance of artificially low proposals at the very start of a dispute. *Ex post* investigation suggests a 5 second buffer as a reasonably heuristic for distinguishing artificial $0 proposals from intentional $0 proposals affected by a defendant's strategy.

[77] To provide some context for this difference, the minimum and maximum NPV proposals in SE1 are −$15.84 and $171.60, respectively. The interquartile range on NPV proposals is $[\$66.74, \$99.16]$.

Figure 13: Average Net Present Value Proposal by Round in SE1[a]



[a]Solid dots illustrate the sample-average NPV proposal by round. Hollow black dots with a vertical connecting line represent an 95% confidence interval on the expected NPV proposal each round. Hollow gray dots with a vertical connecting line represent simultaneous 95% confidence intervals. The dashed line illustrates the theoretic average NPV proposal of $73.44. In omitting data for the first 5 seconds of bargaining, less than 1% of SE1 disputes were dropped from analysis; these disputes settled within the first 5 seconds, and thus contributed no data.

interval on the expected NPV proposal fails to contain the predicted value—equivalent to rejection of the null that the expected proposal equals the predicted value. Similar to sample-averages, confidence intervals do not indicate extreme deviations from theory, always falling within about $20 of prediction.

Individual confidence intervals control the expected rate of Type-I errors (e.g. false rejections), and are appropriate for drawing inferences in specific rounds of SE1. For simultaneous inference on all rounds, a more conservative control is the familywise error rate: the probability of even a single Type-I error in the *family* of all 10 inferences (cf. Benjamini and Hochberg, 1995). Figure 13 illustrates simultaneous 95% confidence intervals constructed by Bonferroni correction (see, e.g., Miller, 1997, pp. 74–75). Simultaneous inference fails to statistically distinguish predicted and observed proposals in $\mathbf{T}_B$, but still rejects the null of equality for $\mathbf{T}_A$ rounds 4, 6, 7, and (narrowly) 5.

Result 3. Average NPV proposal sequences are statistically distinguishable from the flat-line prediction, but the difference from theory is modest.

A second implication of Remark 1 is that equilibrium settlement proposals have a flat average NPV throughout a dispute. Even if average observed NPV proposals do not exactly comport with the predicted level of equilibrium proposals ($73.44), there remains the question whether they comport with the flat-line shape of the theoretic prediction. To address this question, the shape-over-time of observed and predicted NPV proposals is compared in every round of SE1. Comparison is formalized as equality to zero for time-term parameters in the regression of observed NPV proposals on a fourth-order polynomial of time, with results consolidated in Table 10.[78]

At every interesting level of significance and in every round of SE1, the shape over time of observed NPV proposals is statistically distinguishable from the flat-line prediction. Similar to Result 2, however, the practical difference from equilibrium play is mild. This is easy to see in Figure 14, where the average observed NPV proposal in assignments $\mathbf{T}_{A,7}$ and $\mathbf{T}_{B,14}$ is plotted over time as the central black line; plots for other rounds of SE1 are generally comparable, as illustrated in Appendix D.4. The distinction from theoretic prediction is largely confined to the first 20 seconds of bargaining, after which observed NPV proposals are approximately flat as predicted.

Analysis thus far has concerned only the weak interpretation in Remark 1: i.e. comparison to theory has been limited to observed proposals. The strong interpretation of theory in Remark 1 demands more, as equilibrium play describes not just observed proposals, but also the unobserved proposals foreclosed by SE1 settlements.

[78]Again, proposal data for the first 5 seconds of bargaining are omitted from analysis (see n. 76).

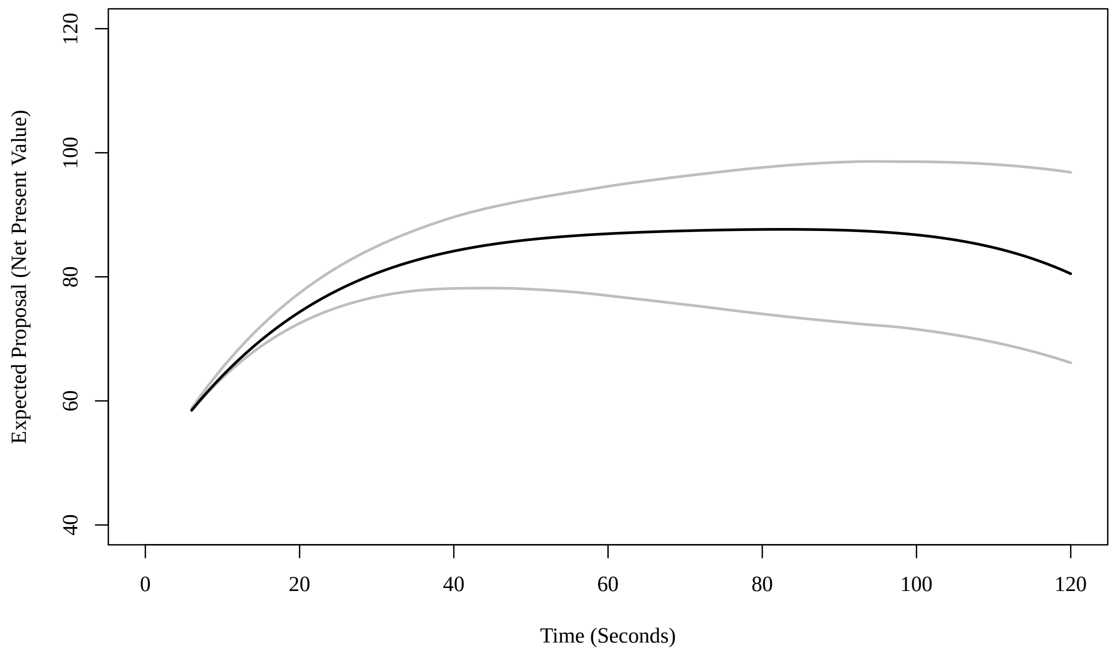
Table 10: Regression of Net Present Value Proposal on Time in SE1[a]

	Rounds in Assignment $\mathbf{T}_A$				
	3	4	5	6	7
$time^1$	3.617*** (0.649)	2.772*** (0.451)	3.808*** (0.688)	2.523*** (0.565)	1.924*** (0.533)
$time^2$	−0.088*** (0.020)	−0.059*** (0.013)	−0.092*** (0.020)	−0.056*** (0.016)	−0.037* (0.015)
$time^3$	$9.47{\times}10^{-4}$*** ($2.56{\times}10^{-4}$)	$5.54{\times}10^{-4}$*** ($1.38{\times}10^{-4}$)	$9.53{\times}10^{-4}$*** ($2.40{\times}10^{-4}$)	$5.78{\times}10^{-4}$*** ($1.69{\times}10^{-4}$)	$3.30{\times}10^{-4}$* ($1.67{\times}10^{-4}$)
$time^4$	$-3.66{\times}10^{-6}$** ($1.14{\times}10^{-6}$)	$-1.89{\times}10^{-6}$*** ($5.26{\times}10^{-7}$)	$-3.49{\times}10^{-6}$*** ($9.95{\times}10^{-7}$)	$-2.20{\times}10^{-6}$*** ($6.21{\times}10^{-7}$)	$-1.14{\times}10^{-6}$† ($6.29{\times}10^{-7}$)

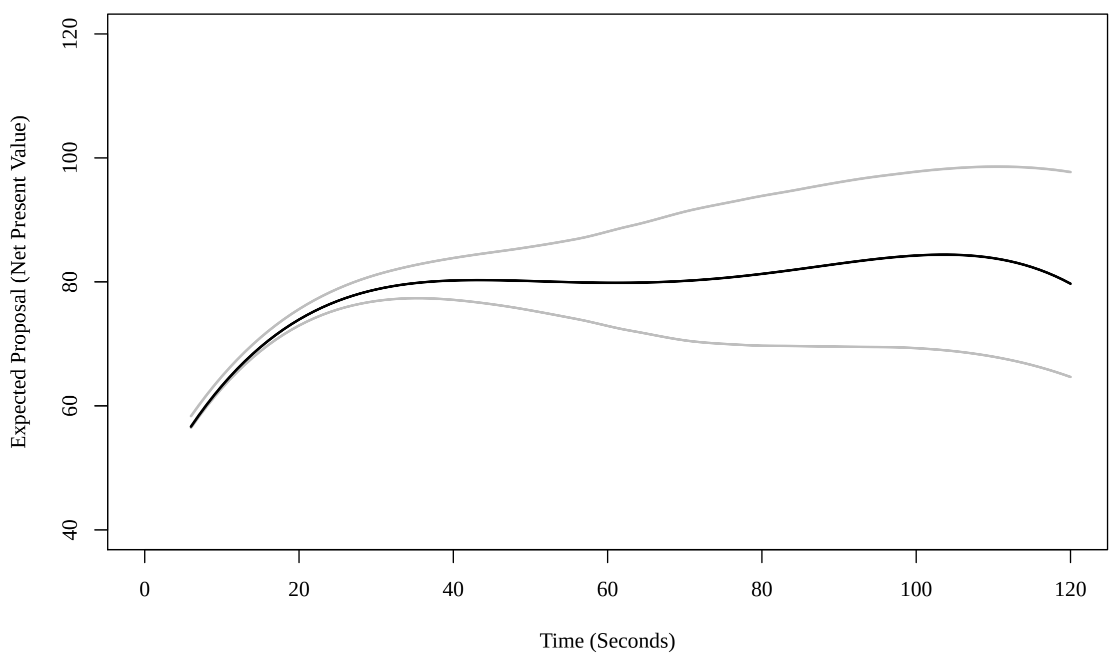
	Rounds in Assignment $\mathbf{T}_B$				
	10	11	12	13	14
$time^1$	3.542*** (0.626)	2.391*** (0.483)	2.364*** (0.568)	1.763** (0.538)	2.505*** (0.607)
$time^2$	−0.084*** (0.017)	−0.053*** (0.015)	−0.049** (0.018)	−0.037* (0.015)	−0.061*** (0.018)
$time^3$	$8.26{\times}10^{-4}$*** ($1.89{\times}10^{-4}$)	$5.10{\times}10^{-4}$** ($1.78{\times}10^{-4}$)	$4.06{\times}10^{-4}$† ($2.20{\times}10^{-4}$)	$3.63{\times}10^{-4}$* ($1.68{\times}10^{-4}$)	$6.33{\times}10^{-4}$** ($2.13{\times}10^{-4}$)
$time^4$	$-2.89{\times}10^{-6}$*** ($7.45{\times}10^{-7}$)	$-1.74{\times}10^{-6}$* ($7.12{\times}10^{-7}$)	$-1.24{\times}10^{-6}$ ($9.01{\times}10^{-7}$)	$-1.29{\times}10^{-6}$* ($6.41{\times}10^{-7}$)	$-2.28{\times}10^{-6}$** ($8.55{\times}10^{-7}$)

[a] Time-term parameter estimates from fixed dispute-effects regression of NPV proposals on a fourth-order polynomial of time (in seconds) by round. Values in parentheses are heteroskedasticity and cluster-robust standard errors (Arellano, 1987). In omitting data for the first 5 seconds of bargaining, less than 1% of SE1 disputes were dropped from analysis; these disputes settled within the first 5 seconds, and thus contributed no data. Qualifiers ∗∗∗, ∗∗, ∗, and † denote significance from zero at levels < 0.001, 0.01, 0.05, and 0.1, respectively.

Figure 14: Average Net Present Value Proposal over Time in SE1[a]



(a) Average NPV Proposal over Time in $\mathbf{T}_{A,7}$



(b) Average NPV Proposal over Time in $\mathbf{T}_{B,14}$

[a]The central black line illustrates the predicted observed NPV proposal over time arising from the relevant regression in Table 10. Gray outer bounds are non-parametric worst-case-scenario bounds on the average value of *all* (observed and unobserved) SE1 proposals; details of the bound estimators are provided in Appendix E.2.

The gray bounding lines in Figure 14 (and Appendix D.4) represent an effort to address the strong interpretation in Remark 1. It should be noted at the outset that the present experimental design fails to identify the expected value of proposals in all periods following settlement of a dispute (i.e unobserved proposals). To at least illustrate the extent of identification problem regarding the expected value of *all* (observed and unobserved) NPV proposals over time, the gray lines in Figure 14 are constructed as worst-case-scenario bounds on the conditional expected value (Manski, 1989); details of the estimator are provided in Appendix E.2.

Intuitively, this bounding technique replaces the mass of unobserved proposals with the values of worst-case upper and lower bounds on the possible expected value of the unobserved proposals. Illustrated bounds are calculated under the assumption that the expected value of unobserved proposals falls somewhere between the 10% and 90% empirical quantiles of all observed NPV proposals in SE1: $[\$50.08, \$115.19]$. As illustrated in Appendix D.4, capacity to address the strong interpretation of theory as predicting the value of both observed and unobserved proposals varies by round and fades rapidly as disputes settle over time. SE1 proposal data are not obviously consistent or inconsistent with the strong interpretation of theory in Remark 1.

8.3 Average Settlement Decisions

Analysis of settlement decisions is complicated by strategic dependence on realized sequences of settlement proposals. For example, the rules for settlement timing in an interior equilibrium are wholly irrelevant when a plaintiff faces a monotone-increasing sequence of NPV proposals—in which case delaying settlement is always individually rational. Fortunately, some aspects of settlement behavior are less sensitive to realized proposal sequences than others.

A starting point is assessment of Remark 3, the very general maxim that—regardless of the realized proposal sequence—a plaintiff should not accept any NPV proposal smaller than the NPV of a trial verdict. In terms of model notation, theory prohibits settlement at any S_t where $U_p(S_t) < W_p(x)$ for the plaintiff's particular injury draw, x. For narrative clarity, let a *disadvantageous settlement* be one in which the Remark 3 prediction is violated.[79]

Result 4. Plaintiffs in SE1 rarely agree to disadvantageous settlements. When such settlements do occur, the loss relative to the expected NPV of a trial verdict is modest.

Of the 600 disputes observed in SE1, 400 terminate in settlement. Of these 400 disputes, only 12 involve disadvantageous settlements.[80] Among these 12 disputes, the median difference from the expected NPV of a trial verdict is \$8.34, with minimum and maximum differences of \$1.19 and \$26.73, respectively.

The proper interpretation of disadvantageous settlements is not obvious. Small losses relative to the expected NPV of a trial verdict may simply reflect noisy comparison of similarly valued alternatives: the online bargaining interface provides subjects with all the signals needed to understand the expected net present values of alternative transfers, but does not present the NPV comparisons themselves. Large losses may reflect a variety of influences, from errors in using the bargaining interface, to errors in understanding relative values, to the presence of idiosyncratic risk aversion in some subjects. The low frequency of disadvantageous settlements (approximately 3% of all disputes) forecloses formal study of these speculations.

[79]Note that this definition is under-inclusive by the standards of a *disadvantageous rejection* in the behavioral bargaining literature discussed in Section 4.1. The present focus on acceptance of NPV proposals smaller than the NPV of a trial verdict insures that all such settlements are obviously and contemporaneously disadvantageous. Alternative definitions of disadvantageous settlements depend on the accuracy of plaintiff expectations about the sequence of proposals being made.

[80]The 12 disadvantageous settlements are made by 10 different subjects and in 7 sessions of the 20 sessions contributing data to SE1.

A related inquiry concerns the Remark 2 observation that only plaintiffs with injury draws in excess of about \$148.88 opt for a trial verdict in equilibrium play. This prediction is dependent on the realized sequence of settlement proposals in a dispute, but the modest difference between average observed and predicted NPV proposals (see Results 2 and 3) suggests that SE1 data may still be informatively compared to this theoretic prediction.

In the following analysis, three considerations motivate aggregation of verdict-disposed disputes by assignment: i.e. combining data from $\mathbf{T}_A$ rounds 3–7 and from $\mathbf{T}_B$ rounds 10–14. First, the relative infrequency of trial verdicts—exactly 1/3 of SE1 disputes—renders round-level sample sizes impractically small. Second, assignment-level aggregation helps to expand the support of injury draws, since fixed random number sequences imply only 6 different injury draws per round across all sessions. Third, aggregating across rounds helps to attenuate noise introduced by the dependence of settlement decisions on realized proposal sequences.

It should be noted that aggregating verdict-disposed disputes by assignment may result in weakly dependent samples. Dependence may be within-subject, as a result of repeated measurements, or within-session, as a result of possible inter-dependence between within-session disputes.[81] Neither source of dependence seems likely to be very strong. Within-subject dependence is mitigated by a large number of subjects, each contributing a relatively small number of observations: while nearly all SE1 defendants experience at least one trial verdict, most experience two or less. Within-session dependence is mitigated by independence of disputes across the 10 sessions contributing to each assignment's sample. Overall, the following analysis seems unlikely to be heavily affected by at most weakly dependent samples (cf. Tran, 1989).

[81] An example of the inter-dependence concern is a situation where play between subjects A and B in round 1 of a session affects subsequent play between subjects A and C or B and D in round 2.

Result 5. The distribution of injuries in verdict-disposed disputes roughly conforms to prediction, but many disputes predicted to end in trial verdicts actually settle.

Figure 15 illustrates the observed and predicted distributions of injuries in verdict-disposed disputes. The observed distribution of injury draws in verdict-disposed disputes is illustrated by a background histogram and (solid-line) kernel density estimate. A dashed line illustrates the predicted distribution of injuries: i.e. a kernel density estimate of the distribution of all injuries in excess of \$148.88 in SE1 disputes.

Results appear comparable between assignments $\mathbf{T}_A$ and $\mathbf{T}_B$. The distribution of injuries in observed verdict-disposed disputes roughly conforms to the predicted distribution, with injuries in observed verdict-disposed disputes exceeding the theoretic cutoff of \$148.88 about 85% of time. Not apparent in Figure 15 is that of all SE1 disputes predicted to end in a trial verdict, only 60% of $\mathbf{T}_A$ disputes and 67% of $\mathbf{T}_B$ disputes are actually observed to be verdict-disposed.[82]

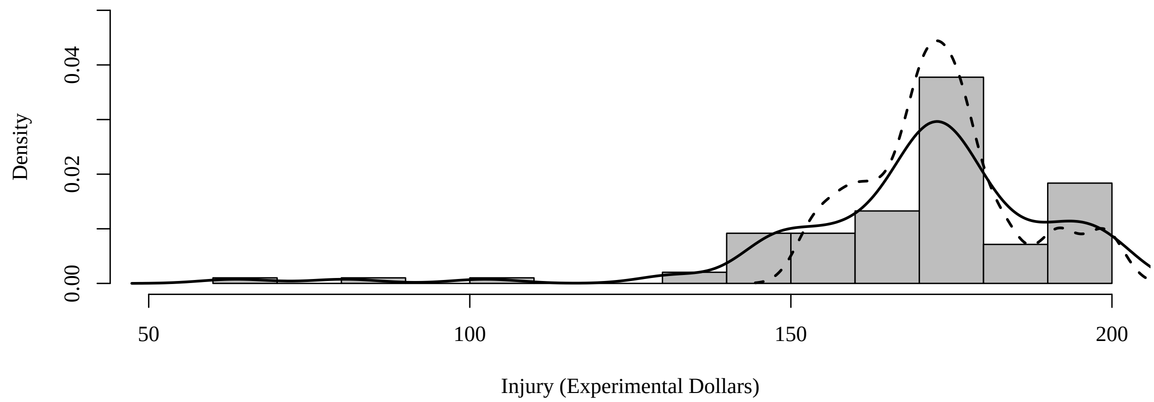
A second implication of Remark 2 is that equilibrium delay-to-settlement is monotone increasing in the injury draw of a plaintiff with injury less than \$148.88. This prediction is tied to a demanding equilibrium concept, and depends heavily on the assumption that realized proposal sequences adhere closely to theoretic prediction. As observed settlement proposals deviate from prediction in several regards, the validity of theoretic predictions for settlement timing is questionable.

Result 6. The observed relationship between settlement timing and injury draws in SE1 disputes is incompatible with theoretic prediction.

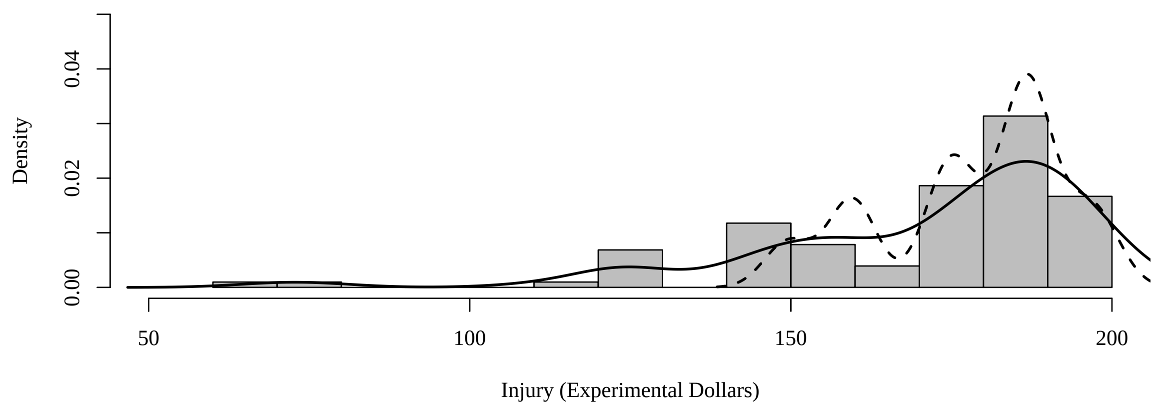
Figure 16 compares observed settlement timing with theoretic prediction. The dashed line represents the predicted timing of settlement by injury draw for disputes that are predicted to settle: i.e. for injury draws less than \$148.88. The gray region of

[82] These unpredicted settlements are illustrated in the gray region of Figure 16.

Figure 15: Distribution of Injuries in Verdict-Disposed Disputes in SE1[a]



(a) Distribution of Injuries in Verdict-Disposed Disputes in $\mathbf{T}_A$



(b) Distribution of Injuries in Verdict-Disposed Disputes in $\mathbf{T}_B$

[a]Background histograms illustrate the relative frequency of injury draws conditional on failure to settle. Solid black lines illustrate kernel density estimates of the injury distribution conditional on failure to settle. For comparison to theoretic predictions, dashed lines illustrate kernel density estimates of the injury distribution conditional on the theoretic prediction that a dispute should be resolved by trial verdict: i.e. conditional on an injury draw in excess of $148.88.

the plot represents the range of injury draws for which a plaintiff is not predicted to settle. These theoretic predictions are exactly those illustrated previously in Figure 8(b). Superimposed over theoretic predictions, hollow dots illustrate observed injury and settlement-time pairs from disputes in SE1 and a solid black line illustrates a robust estimate of average observed settlement time conditional on injury.[83]

Aggregating settlement-disposed disputes by assignment, Figures 16(a) and 16(b) illustrate the wide range of settlement times observed at every level of injury draw.[84] Average observed settlement times appear to be slightly increasing in injury size, but to a much smaller degree than predicted by theory. Also illustrated are the previously noted mass of settlements involving injury draws for which a trial verdict resolution is predicted. Differences between assignments $\mathbf{T}_A$ and $\mathbf{T}_B$ appear modest, and may in part reflect differences in the support of injury draws.

A related inquiry concerns the proposal value for which disputes settle. As implied by Remarks 1 and 2, every type of plaintiff that settles in equilibrium does so for a common NPV proposal of \$73.44. Similar to predictions concerning settlement proposals, consistency between average observed and predicted settlement amounts can be assessed in both level and shape.

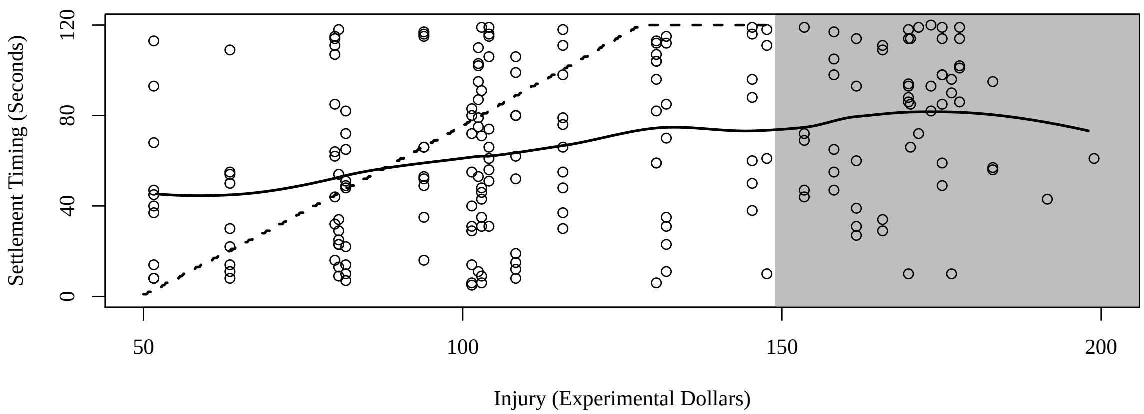
Result 7. The average observed settlement amount is higher in SE1 than predicted by theory, but is consistent with theory in being basically flat across injury draws.

Figure 17 plots observed NPV settlement amounts against the value of a plaintiff's injury draw. Dashed lines represent the predicted settlement amount, a NPV of

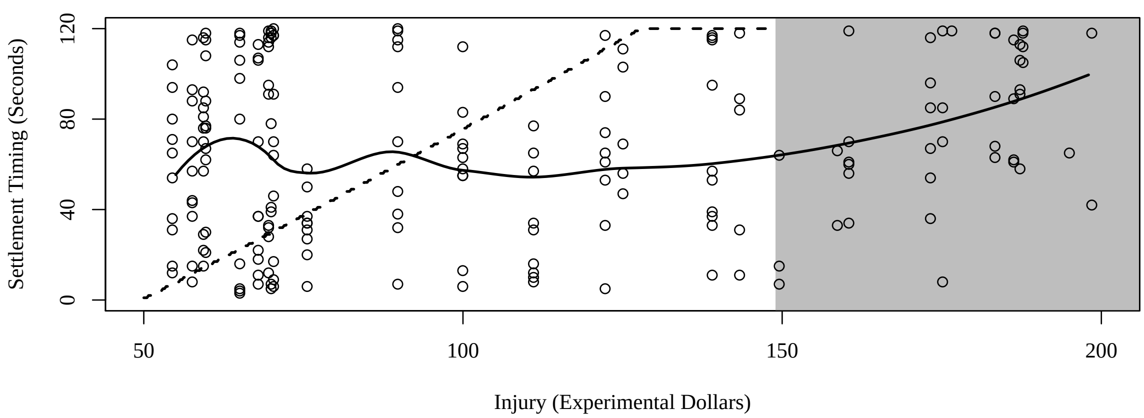
[83] A flexible conditional average is estimated under a Loess model with smoothing parameter $\alpha = 0.5$ (see n. 74).

[84] As in the previous discussion for verdict-disposed disputes, aggregation across multiple rounds of a session introduces potential dependencies between sample observations. Similar arguments to those discussed previously motivate the decision to aggregate data, and counsel that potential sample dependence is probably mild. Subjective analysis of robust conditional expected value illustrations seems unlikely to be strongly influenced by weakly dependent observations.

Figure 16: Timing of Settlement by Injury in SE1[a]



(a) Timing of Settlement by Injury in $\mathbf{T}_A$



(b) Timing of Settlement by Injury in $\mathbf{T}_B$

[a]Dashed lines represent the predicted timing of settlement by injury draw for disputes that are predicted to settle. The gray region of the plot represents the range of injury draws for which a plaintiff is not predicted to settle. Hollow dots illustrate observed injury and settlement-timing pairs and a solid black line illustrates a robust average settlement time conditional on injury. The average is flexibly estimated under a Loess model with smoothing parameter $\alpha = 0.5$ (see n. 74).

\$73.44 for all types of plaintiff that are predicted to settle. The gray region of the plot represents the range of injury draws for which a plaintiff is not predicted to settle. Hollow dots illustrate observed injury and settlement-amount pairs and a solid black line illustrates a robust estimate of average settlement amount conditional on injury.[85]

Aggregating settlement-disposed disputes by assignment, Figures 17(a) and 17(b) illustrate comparable patterns of settlement amount by injury across assignments. As these figures indicate, the average NPV settlement amount is higher in SE1 than predicted by theory. Data peg the average NPV settlement amount at \$106.66 in $\mathbf{T}_A$ and \$99.96 in $\mathbf{T}_B$: corresponding 95% confidence intervals are $[\$102.70, \$110.61]$ and $[\$95.83, \$104.09]$.[86] These averages substantially exceed the \$73.44 prediction.

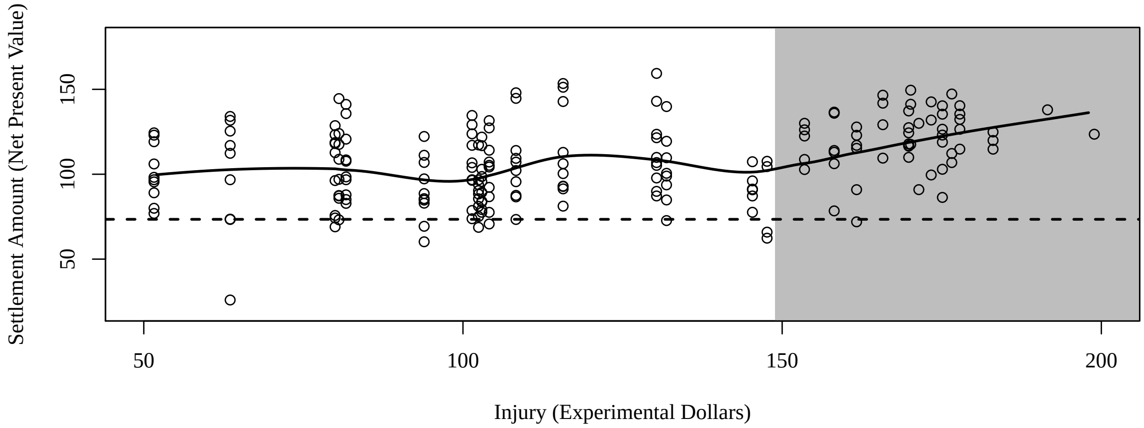
But while the level of average SE1 settlement amounts is inconsistent with predicted settlements, Figures 17(a) and 17(b) suggest that the shape of average settlements amounts by injury draw is basically consistent with theory. Over all injury draws between \$50 and about \$148.88 (i.e. the range of injury draws for which theory predicts settlement at all) robust estimates of conditional average settlement amount show little variation by injury draw. Formalized in a regression framework, F-tests for the parameters of a third-degree polynomial of injury fail to reject the null of invariance in injury at every interesting level of significance: F-test p-values are 0.1806 in $\mathbf{T}_A$ and 0.3472 in $\mathbf{T}_B$.[87] Interestingly, Figures 17(a) and 17(b) both indicate at least a modest positive relationship between settlement amount and injury draw on the domain of injuries for which settlement is *not* predicted.

[85] A flexible conditional average is estimated under a Loess model with smoothing parameter $\alpha = 0.5$ (see n. 74).

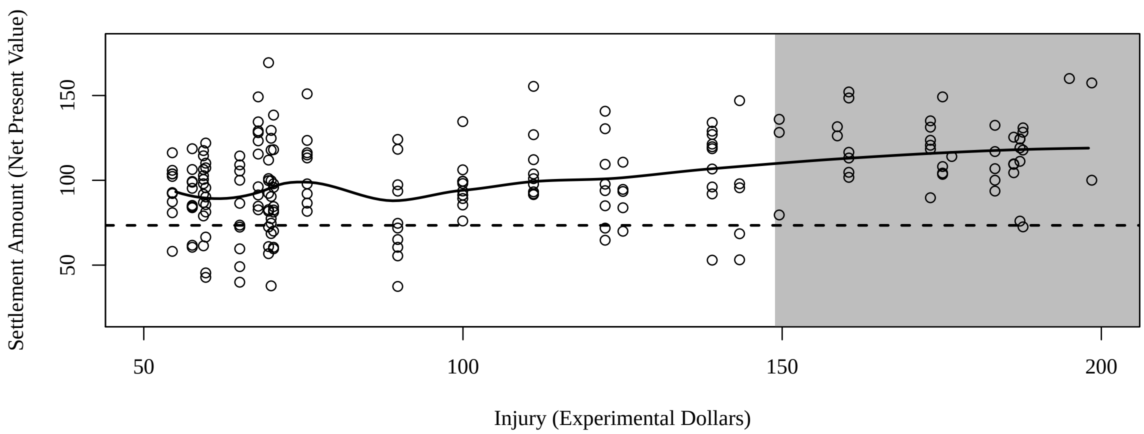
[86] Confidence intervals are based on the asymptotic sampling distribution of mean-prediction from regression of NPV settlement amounts on fixed round-effects with random defendant-effects (cf. Kutner et al., 2005, 229); variances and covariances are estimated robustly (Arellano, 1987).

[87] F-tests are derived from regression of observed settlement amount on a third-degree polynomial of injury, with fixed subject and round-effects, and employ a heteroskedasticity and cluster-robust estimator of the variance-covariance matrix (Arellano, 1987).

Figure 17: Settlement Amount by Injury in SE1[a]



(a) Settlement Amount by Injury in $\mathbf{T}_A$



(b) Settlement Amount by Injury in $\mathbf{T}_B$

[a]Dashed lines represent the predicted settlement amount, a NPV of $73.44 for all types of plaintiff that are predicted to settle. The gray region of the plot represents the range of injury draws for which a plaintiff is not predicted to settle. Hollow dots illustrate observed injury and settlement-amount pairs and a solid black line illustrates a robust average settlement amount conditional on injury. The average is flexibly estimated under a Loess model with smoothing parameter $\alpha = 0.5$ (see n. 74).

8.4 Average Timing of Resolution

As delay is the central theme of the present research, consistency of observed and predicted delay in SE1 disputes is of particular interest. This section preserves the terminology of Corollary 3, distinguishing between *delay-to-resolution* (the time between initiation of a dispute and either settlement or trial verdict) and *delay-to-settlement* (delay-to-resolution restricted to the set of settlement-disposed disputes).

To simplify discussion, let *population predictions* denote theoretic predictions attaining with injuries exactly distributed according to the uniform distribution on $[\underline{x}, \overline{x}]$. Let *sample predictions* denote the theoretic predictions implied by simulated equilibrium play for the set of realized injury draws in SE1 disputes.

Result 8. In both average and distribution, the timing of resolution in SE1 disputes is more consistent with population predictions than it is with sample predictions.

Population predictions place average delay-to-settlement at about 72 seconds, and place average delay-to-resolution at about 89 seconds. Sample predictions peg average delay-to-settlement and delay-to-resolution at about 76 and 90 seconds, respectively. In SE1 disputes, the observed average delay-to-settlement is about 66 seconds and the observed average delay-to-resolution is about 84 seconds.

Tables 11 and 12 consolidate summary statistics and inferences concerning population prediction, sample prediction, and observed average delays by assignment. Statistical inference is based on session-average delays, which are independent and plausibly identically distributed (deriving from a common data-generating process). To accommodate small sample sizes—10 session-average observations per assignment—inference is based on the exact permutation distribution of Wilcoxon's Signed-Rank test statistic (see, e.g., Miller, 1997, pp. 22–26). Table cells provide p-values for tests of locational equality and 95% confidence intervals on expected delay.

Table 11: Consistency of Observed and Predicted Delay-to-Settlement in SE1

Assignment	Population Prediction[a]	Sample Prediction[a]	Observed Average[b]
$\mathbf{T}_A$	72.021 0.2754	86.441 0.0020**	66.065 [56.362, 76.068]
$\mathbf{T}_B$	72.021 0.0840†	64.823 0.6250	65.910 [56.147, 73.602]

[a] On top is the theoretic prediction for the expected delay. On bottom is the p-value corresponding to an exact Wilcoxon Signed-Rank test of the null hypothesis that average observed delay equals the theoretic prediction. Qualifiers ∗∗ and † denote significance at the 0.01 and 0.1 levels, respectively.
[b] On top is the observed average across all sessions in an assignment: i.e. the grand mean. On bottom is a 95% confidence interval on the expected observed delay constructed by inversion of the Wilcoxon Signed-Rank test.

Table 12: Consistency of Observed and Predicted Delay-to-Resolution in SE1

Assignment	Population Prediction[a]	Sample Prediction[a]	Observed Average[b]
$\mathbf{T}_A$	88.714 0.1602	96.967 0.0020**	84.080 [77.983, 90.617]
$\mathbf{T}_B$	88.714 0.2324	83.067 0.4922	84.383 [76.617, 90.967]

[a] On top is the theoretic prediction for the expected delay. On bottom is the p-value corresponding to an exact Wilcoxon Signed-Rank test of the null hypothesis that average observed delay equals the theoretic prediction. Qualifiers ∗∗ and † denote significance at the 0.01 and 0.1 levels, respectively.
[b] On top is the observed average across all sessions in an assignment: i.e. the grand mean. On bottom is a 95% confidence interval on the expected observed delay constructed by inversion of the Wilcoxon Signed-Rank test.

All 95% confidence intervals in Tables 11 and 12 cover the population-prediction delay values. Observations from assignment $\mathbf{T}_A$ are inconsistent with sample predictions for both average delay-to-settlement and delay-to-resolution, but the same is not true of $\mathbf{T}_B$ observations (which appear basically consistent with sample predictions). Observed average delay is otherwise nearly identical between assignments.

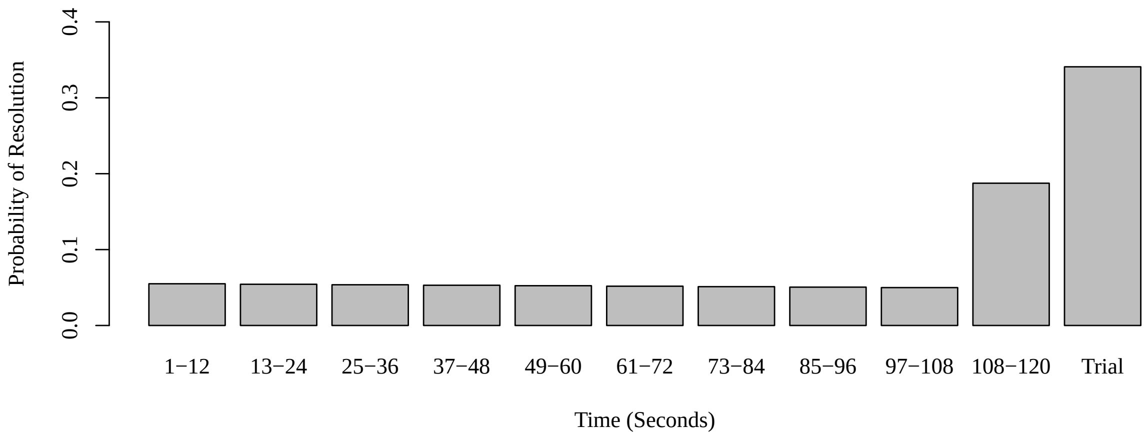
As a more detailed comparison of delays, Figures 18 and 19 illustrate approximate delay-to-resolution distributions for $\mathbf{T}_A$ and $\mathbf{T}_B$, respectively.[88] Note that the shape of delay-to-settlement is simply that of the delay-to-resolution distribution prior to the trial verdict period. Figures 18(a) and 19(a) illustrate the population prediction for the delay-to-resolution distribution. Figures 18(b) and 19(b) illustrate sample predictions: coarseness is attributable to the small number of distinct injury draws in an assignment.[89] Figure 18(c) and 19(c) plot observed delay-to-resolution distributions. Note that sample-prediction and observed delay-to-resolution plots involve the same set of random injury draws by assignment.

Immediately apparent in Figures 18 and 19 is the high degree of consistency between population-prediction and observed delay-to-resolution distributions. Interestingly, the timing of resolution in SE1 data appear more consistent with population predictions than with sample predictions tailored to the particular injury distributions in each assignment. This may be the combined result the coarseness of the injury distribution in a given sample—leading to a very discrete distributional prediction—and the smoothing effects of noisy settlement bargaining behavior.

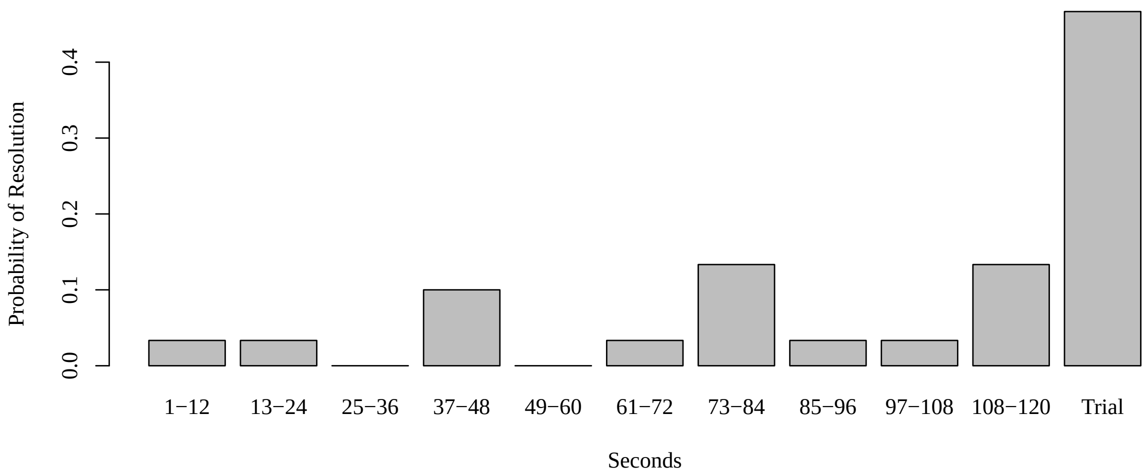
[88]Samples aggregate observed delay-to-resolution measurements across rounds and sessions within an assignment, again introducing the possibility of within-sample dependencies. As discussed previously (see Section 8.3) potential dependence seems unlikely to be very strong. For a balanced design (i.e. the same number of observations from each experimental unit) and without the intent to derive inferential sampling distributions, it seems reasonable to think the following analysis should be robust to weak dependencies between observations (cf. Tran, 1989).

[89]Recall that random number sequences are common across sessions. Each of the 5 rounds in $\mathbf{T}_A$ involves 6 unique injury draws, so Figure 18(b) is based on only 30 distinct injury draws.

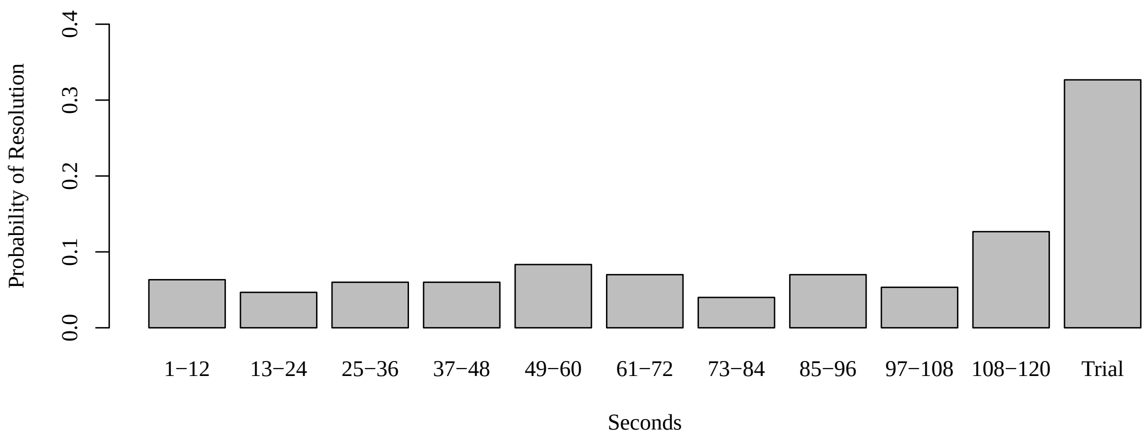
Figure 18: Predicted and Observed Delay-to-Resolution Distributions in SE1, $\mathbf{T}_A$



(a) Population Prediction for the Delay-to-Resolution Distribution in $\mathbf{T}_A$

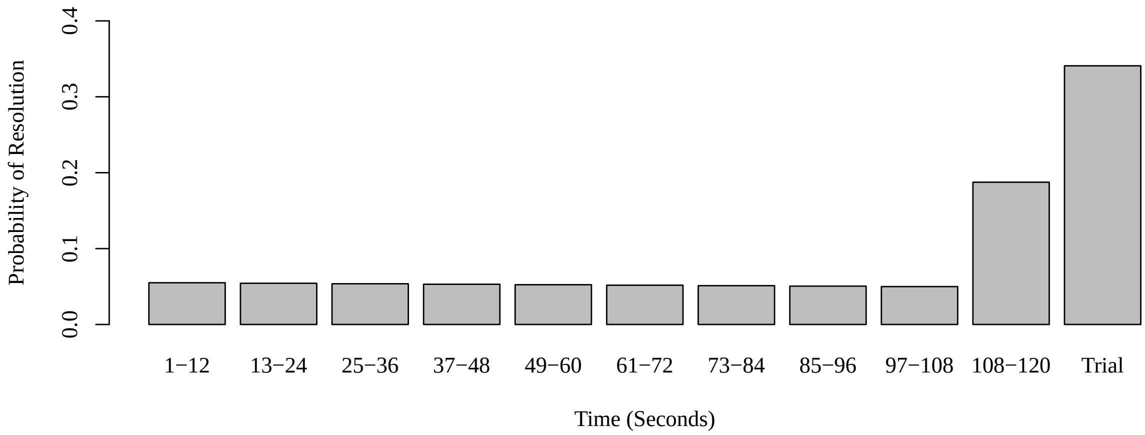


(b) Sample Prediction for the Delay-to-Resolution Distribution in $\mathbf{T}_A$

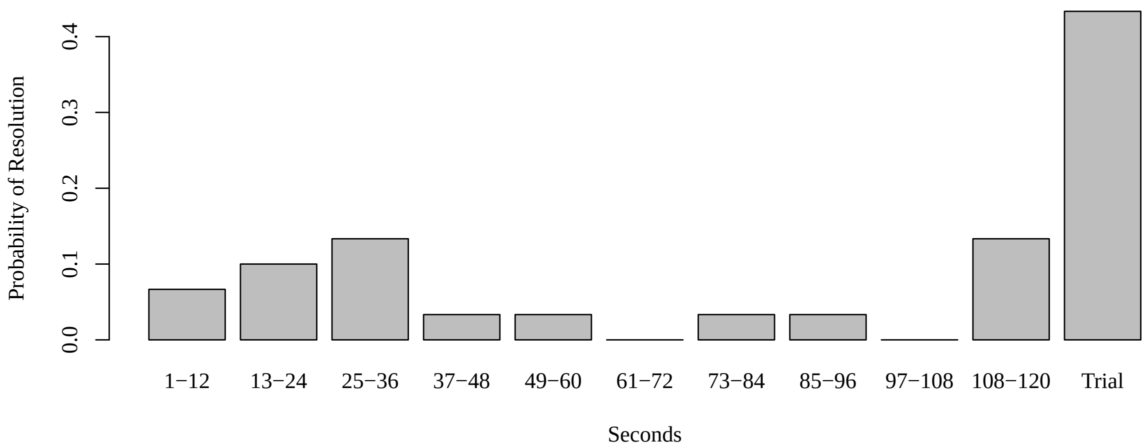


(c) Observed Delay-to-Resolution Distribution in $\mathbf{T}_A$

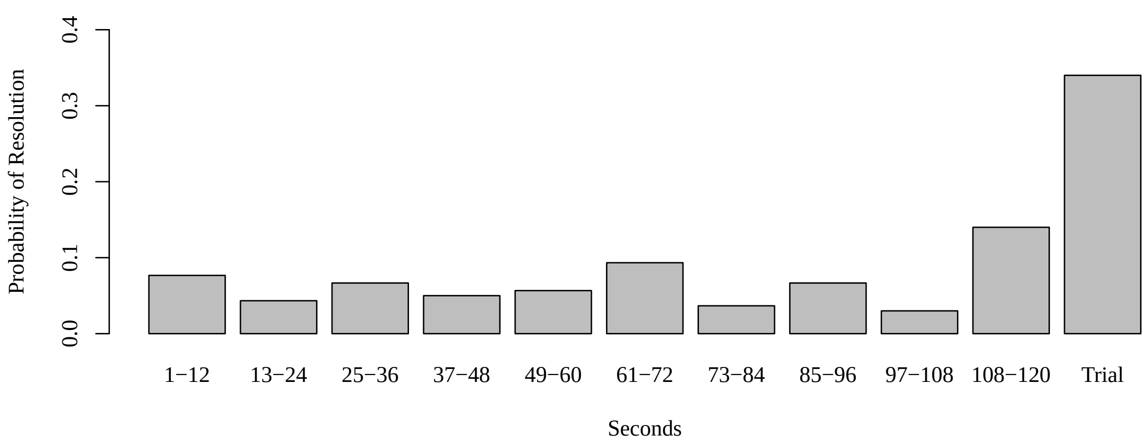
Figure 19: Predicted and Observed Delay-to-Resolution Distributions in SE1, $\mathbf{T}_B$



(a) Population Prediction for the Delay-to-Resolution Distribution in $\mathbf{T}_B$



(b) Sample Prediction for the Delay-to-Resolution Distribution in $\mathbf{T}_B$



(c) Observed Delay-to-Resolution Distribution in $\mathbf{T}_B$

An alternative means of visualizing distributional differences uses predicted and observed hazard functions for settlement in SE1 disputes by assignment. The (discrete) hazard rate

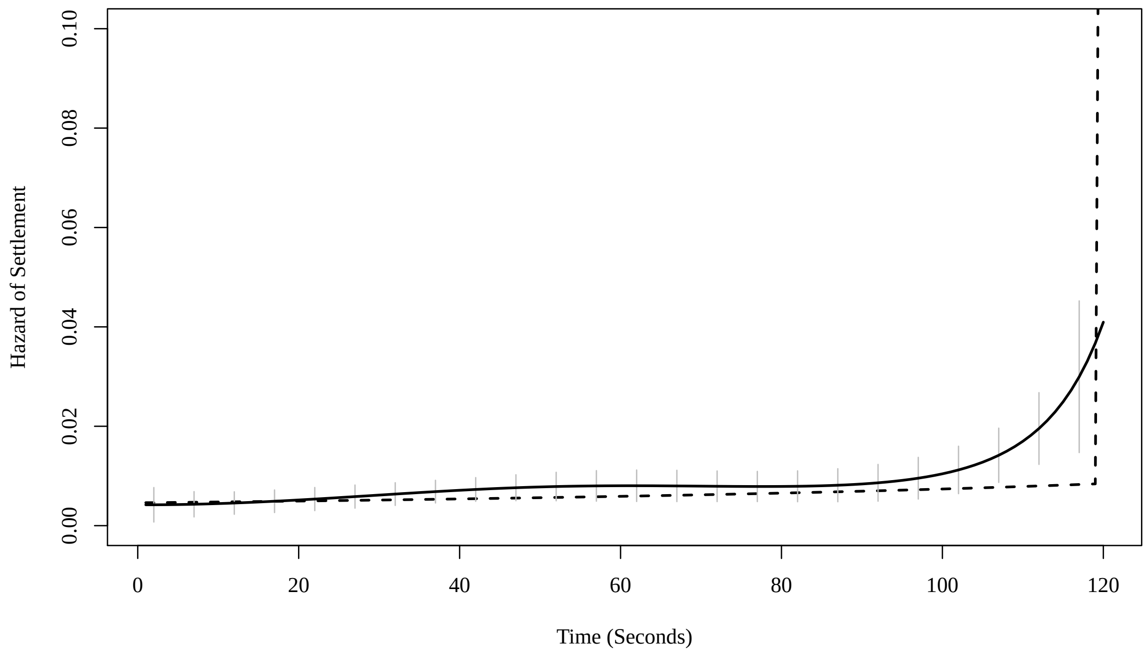
$$h(t) = \frac{p_t}{1 - \sum_{i=1}^{t-1} p_i} \qquad t = 1, \ldots, T+1 \tag{36}$$

is the probability of settlement at time t conditional on failure to settle prior to t. The hazard function is only interesting up to period T, being identically 1 at the point of a trial verdict.

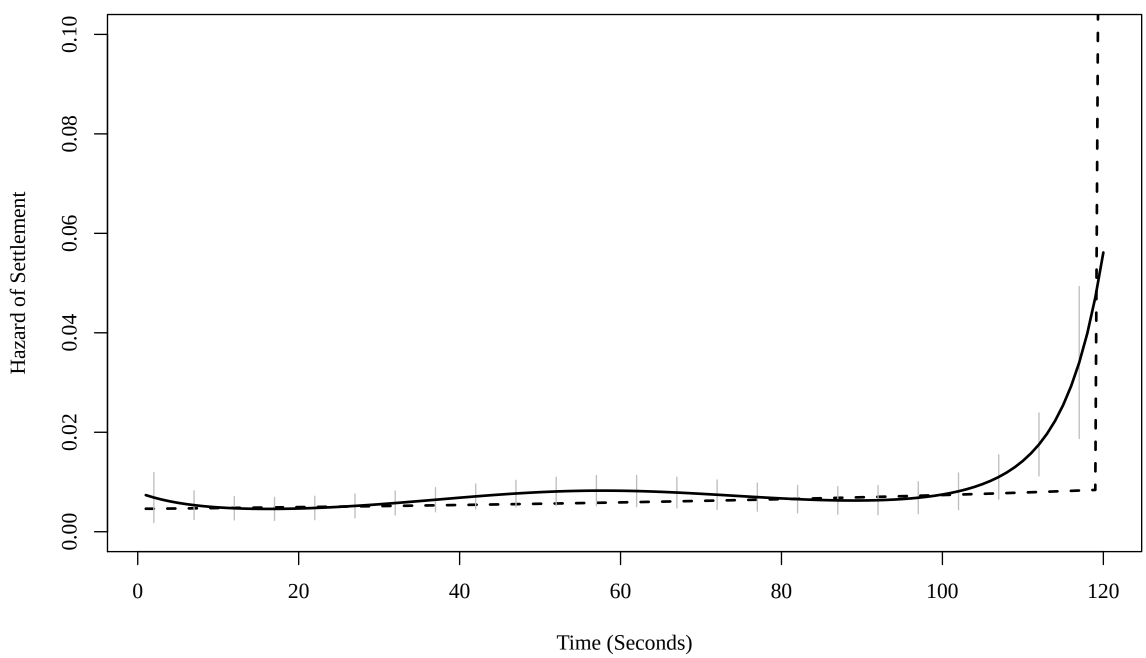
Figure 20 illustrates observed and predicted hazard functions. Dashed black lines represent the (population) hazard of settlement under equilibrium play: the predicted hazard at time T (about 0.294) falls outside of the plot window. Solid black lines represent robust estimates of the observed hazard. The precision of the hazard function estimator is suggested by a series of simultaneous 95% confidence intervals (constructed by Bonferroni correction) drawn as gray vertical bars. Details on the estimator and associated inference are provided in Appendix E.3.

In light of prior analysis, Figures 20(a) and 20(b) are predictably similar. The estimated observed hazard closely tracks the predicted hazard for at least the first 100 seconds of bargaining. Differences in observed and predicted hazards at the very end of bargaining may be a simple artifact of the continuous bargaining design. Unlike litigants in the theoretic model, subjects in SE1 need to physically press a button in order to accept a settlement proposal. Since a miss-timed button-press in the final second of bargaining results in an unintended trial verdict, subjects in the experiment may settle slightly before the final second as a means of insuring against imperfect actions, network glitches, etc.

Figure 20: Predicted and Observed Hazard of Settlement in SE1[a]



(a) Predicted and Observed Hazard of Settlement in $\mathbf{T}_A$



(b) Predicted and Observed Hazard of Settlement in $\mathbf{T}_B$

[a]Dashed black lines represent theoretic hazard functions while solid black lines represent a robust estimate of the observed hazard over the duration of bargaining (i.e. seconds 1–120). Gray vertical bars illustrate simultaneous 95% confidence intervals, constructed by Bonferroni adjustment for multiple comparisons. Details of the estimator are provided in Appendix E.3.

9 Discussion

Exploratory analysis of SE1 data provides a detailed profile of settlement bargaining behavior under the control treatment, $\mathbf{T}_0$. Two points of comparison summarize results. Section 9.1 addresses the first point of comparison: consistency of observed and predicted behavior. Section 9.2 addresses the second point of comparison: consistency of behavior between treatment assignments. Finally, Section 9.3 provides side commentary on issues in the design of proposal sequence elicitation.

9.1 Consistency of Observed and Predicted Behavior

Despite a few specific instances of obvious discord, the overall consistency of SE1 data and theoretic prediction is high by the standards of many bargaining games studied in laboratory experiments. Conformance is certainly greater than I anticipated prior to conducting this experiment (see Section 1.2).

Informal analysis of individual disputes reveals a variety of bargaining patterns—some basically consistent with prediction, many startlingly inconsistent. Distressing observations include NPV proposal sequences that are monotone decreasing, or ostensibly random over time. Other aspects of behavior are more consistent with expectations: disputes rarely settle for less than the plaintiff's expected net present value of trial, and disputes involving high injuries usually proceed to trial.

Though statistically distinguishable from the theoretic equilibrium, average proposal sequences in SE1 are at most only modestly different than predicted by theory. The frequency of explicit proposals is arguably lower than predicted. Averaged proposals tend to exceed the predicted proposal level—being more generous than predicted by theory—but roughly conform to the expected shape of NPV proposals, at least after the first 20 seconds of bargaining.

Settlement decisions vary in consistency with different aspects of prediction. On one hand, the distribution of injuries in observed trial verdicts is consistent with the theoretic prediction. On the other hand, only around 63% of disputes predicted to end in trial verdicts actually do so. Settlement amounts by injury exceed the theoretic prediction in level (i.e. settlements involve larger average transfers than predicted by theory), but are consistent with the prediction that average settlement amount not vary with the injury draw of the plaintiff. The timing of settlements by degree of injury is plainly inconsistent with the theoretic prediction, but the exact interpretation of this inconsistency is muddied by differences from theoretic prediction in observed proposal sequences.

Given mixed conformity between the observed behavior and equilibrium strategies, the distribution of resolution delay is surprisingly consistent with theoretic predictions. The average frequency of observed trial verdicts differs from the (population) theoretic prediction by only a few percentage points, and average delay-to-resolution and delay-to-settlement are both within a few seconds of prediction. The shape of the delay-to-resolution distribution is also basically consistent with prediction.

Comparative results in this chapter are best interpreted as taking an exploratory posture: the theoretic model acts as a reasonable predictor of behavior in certain aspects of observed settlement bargaining behavior, and as a valuable point of reference in all other aspects. It is important to note that the approximate consistency of observed and predicted results should not itself be interpreted as direct evidence for or against the theoretic model of settlement bargaining with asymmetric information. For example, nothing in SE1 attempts to identify the portion of observed settlement delay attributable to the influence of asymmetric information. These types of inquiries are addressed in detail in Chapter V.

9.2 Consistency of Results between Assignments

A secondary point of comparison concerns differences in behavior between assignments $\mathbf{T}_A$ and $\mathbf{T}_B$. As discussed in Section 5.5, the concern is that sequential assignment of treatments in the cross-over design may introduce sources of artificial bias in measurements taken from the treatment assigned second. Order effects may distort results if learning, boredom, or other influences cause subjects to behave differently across different rounds. Sequence effects may affect behavior if play in assignment $\mathbf{T}_B$ is framed by the treatment assigned as $\mathbf{T}_A$. The design element of orthogonal treatment assignment is meant to mitigate these potential design biases, but greater confidence would be provided by lack of obvious design bias in the first place.

Observed results are generally positive. Across all aspects of bargaining behavior studied in this chapter, similar results obtain under assignments $\mathbf{T}_A$ and $\mathbf{T}_B$. Observed differences—e.g. in the level of average NPV proposals—are not large, and may be partly attributable to differences in random sequences between assignments. In combination with orthogonal treatment assignment, consistency of results between SE1 assignments $\mathbf{T}_A$ and $\mathbf{T}_B$ does much to mitigate concern about the above sources of design bias in the inferential analysis of the following chapters.

9.3 Comment on Proposal Sequence Design

Though not a critical aspect of the experimental design or interpretation of results, complications introduced by the inability to measure proposals following settlement of a dispute invite side commentary on the methodology of proposal elicitation. The objective of this methodological discussion is actually substantive. The following commentary underscores the relative reliability of proposal and bargaining data collected under the present experimental design.

Analytical complications relating to proposal elicitation (e.g. the difference between strong and weak interpretations of theoretic predictions) raise the question whether the present experimental design could be improved upon. For example, an alternative elicitation scheme with many analogues in the experimental literature would be to ask defendants to submit, *ex ante*, full sequences of proposals to cover the entire the duration of bargaining. With full schedules of proposals in hand at the start of bargaining, settlement would not preclude subsequent measurement of proposal sequences. For narrative simplicity, I refer to this as a *menu design* approach.

The use of a menu design approach to proposal elicitation has much to recommend it. For one thing, it would greatly simplify subsequent statistical analysis. Additionally, the menu design approach may tend to induce greater subject introspection. Treating the decision schedule as a cohesive problem comports with theoretic analysis and may tend to clarify the implications of various strategies: e.g. that offering an increasing sequence of NPV settlement proposals may incentivize settlement delay on the part of a plaintiff.

The present experimental design eschews the menu design approach out of concern for experimental validity. An initial concern is that introducing a menu of proposals changes both the rules and information structure of the problem. It provides a defendant with an artificial capacity to constrain future action, and it provides a plaintiff with information on counter-factual proposals. I doubt many litigants in the field consummate a final settlement by enumerating the actions they would have taken under various unrealized contingencies! While these concerns can admittedly be mitigated by careful structure of the bargaining environment—e.g. requesting a full menu of proposals upfront but only revealing proposals in the sequence at appropriate times in the dispute—a more fundamental concern remains.

The remaining concern attaches to the way that a menu design approach defines the decision structure of the game. Like a game of chess, the decision structure of the menu design approach is both deep (requiring repeated inductions) and wide (involving many alternatives and contingencies). Subjects' daily lives familiarize them with decision structures which are either shallow (approximating contemporaneous decisions) or narrow (involving few alternatives); deep *and* wide decision structures are often alien (Norman, 1988, pp. 119–127).

The chess analogy is apposite. If placed in the role of the advantaged player near the end of a game of chess, a subject may easily win the game within a few moves—making each move in sequence, watching how the opponent responds, and then making another move. If given the same task, but instead asked to provide, *ex ante*, a menu of moves for every (factorial) progression of board positions until the end of play, I suspect an average subject would falter. The problem is not that the underlying game is difficult (the subject wins without difficulty in the first hypothetical), but that the latter decision environment involves an unnatural cognitive process, and is therefore difficult. In the settlement bargaining context, as in the chess example, the menu design approach seems both artificial and unlikely to generate the more internally and externally valid data.

D Graphical Appendix

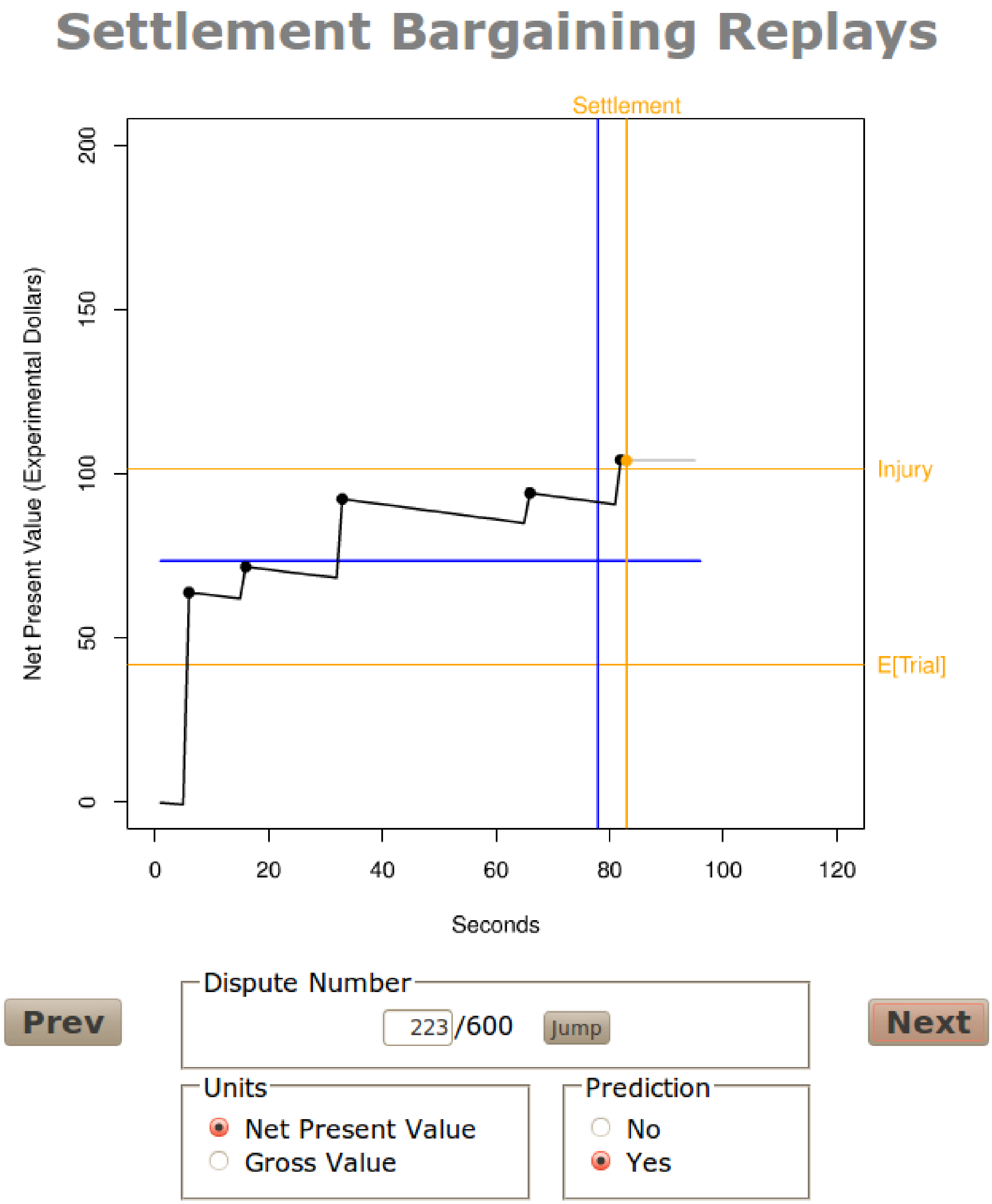
D.1 Online Appendix

An online appendix, providing continuous-time replays of all SE1 disputes, is available at `http://people.virginia.edu/~sps2d/settlement_bargaining_replays/` or by request of the author. Experimental measurements are used to reconstruct the exact play of each game, and reconstructed games are then converted to continuos-time Shockwave Flash (*.swf) graphics files. The online appendix will be maintained by the author until the supporting technology (XHTML, JavaScript, php5, Shockwave Flash) becomes unsupported.

Illustrated below, the online appendix allows a user to watch the progression of bargaining (i.e. proposals and acceptance decisions) throughout all 600 SE1 disputes. Explicit proposals are represented by black dots, and implicit proposals are represented by black connecting lines between dots. Settlement is indicated by a vertical orange line drawn at the time of agreement, and failure to settle is indicated by a vertical red line at the conclusion of the round. Context lines illustrate controlled private information: one line (labeled "E [Trial]") indicates the expected value of a trial verdict given the plaintiff's injury draw, while the other (labeled "Injury") indicates the size of the injury itself. Finally, predicted proposals and settlement decisions are represented by blue (unlabeled) lines of appropriate shape.

The user can select whether units are dispayed in gross value or in net present value. Unit control is crossed with an option to overlay theoretical predictions, allowing for a total of 4 ways to replay each bargaining game. To protect subject anonymity, subject and session data are surpressed in the construction of bargaining-game replays. The order in which games are displayed is static, but has been randomized so that the sequence of replays does not reveal identifying information.

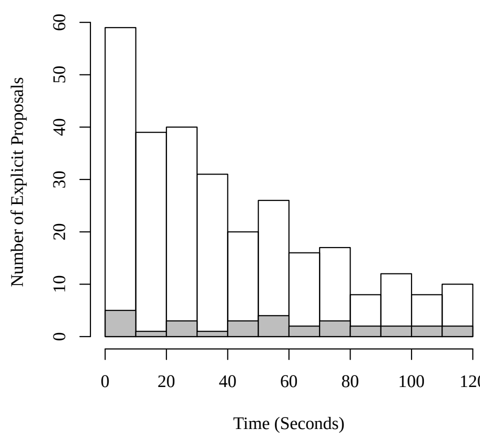
Screenshot 12: Online Appendix



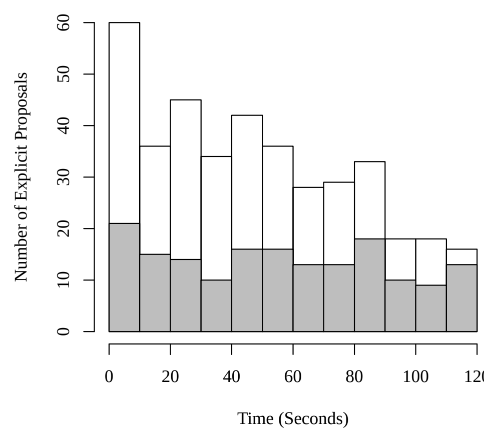
D.2 Explicit Proposal Frequencies for All Rounds

Figure 21 illustrates the absolute frequency of explicit proposals over time in every round of SE1. Construction of these illustrations is the same as those in Figure 11. White bars illustrate the frequency of explicit proposals across all disputes in a round, while superimposed gray bars illustrate the frequency of explicit proposals for only the subset of disputes that end in a trial verdict (i.e. disputes that never settle).

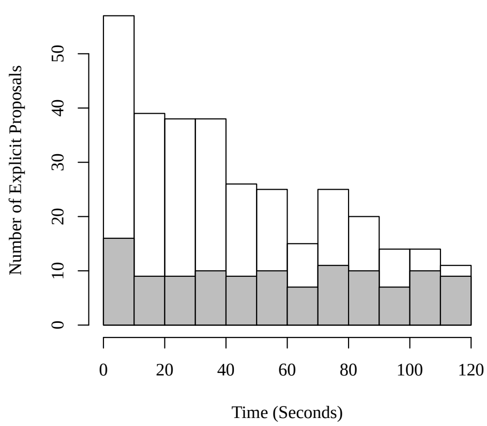
Figure 21: Absolute Frequency of Explicit Proposals by Round



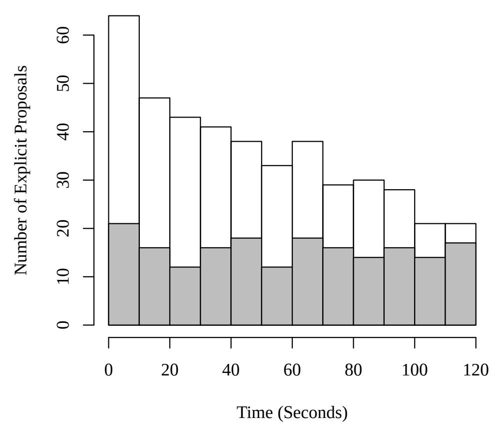
(a) Round 3



(b) Round 4

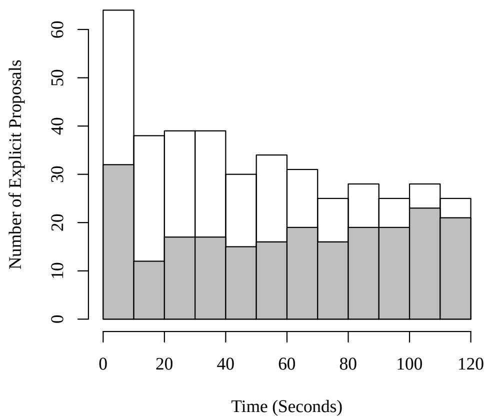


(c) Round 5

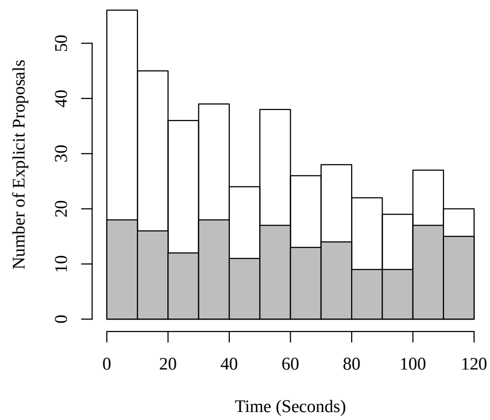


(d) Round 6

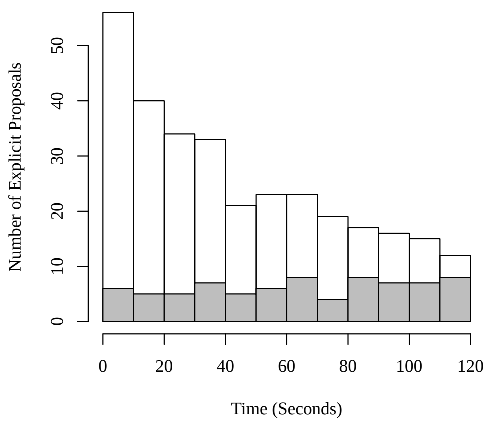
Figure 21: Absolute Frequency of Explicit Proposals by Round (Continued…)



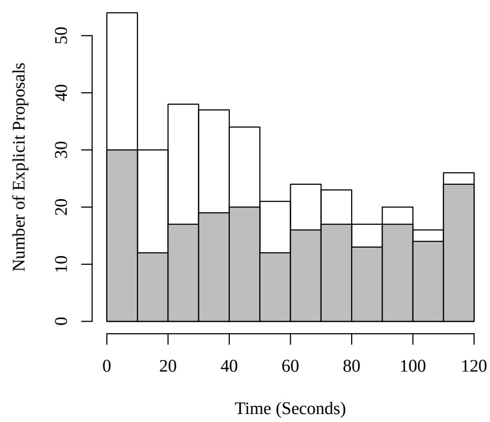
(e) Round 7



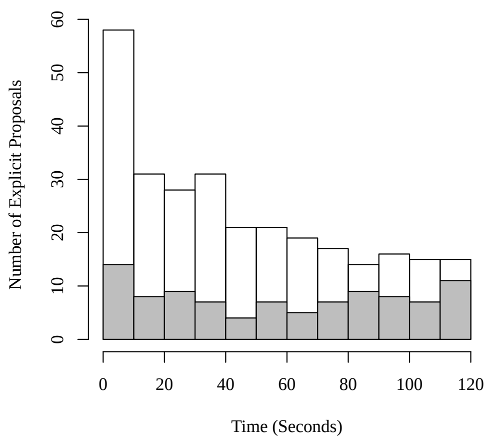
(f) Round 10



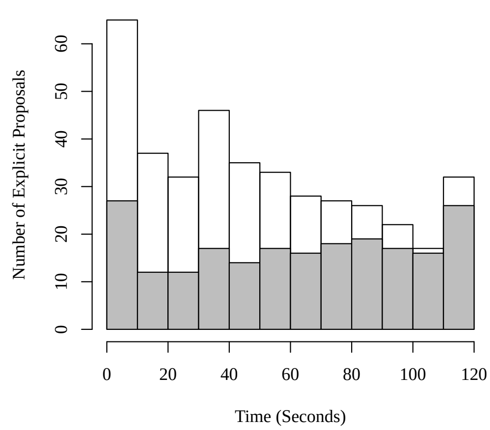
(g) Round 11



(h) Round 12



(i) Round 13

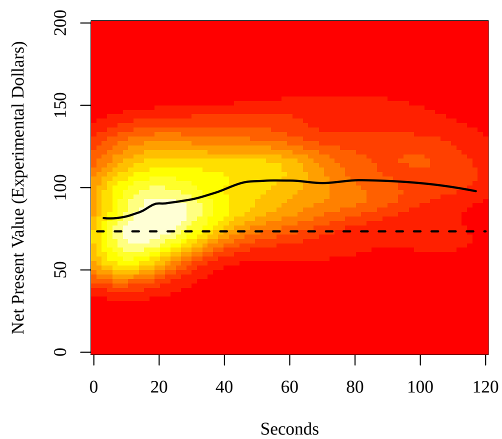


(j) Round 14

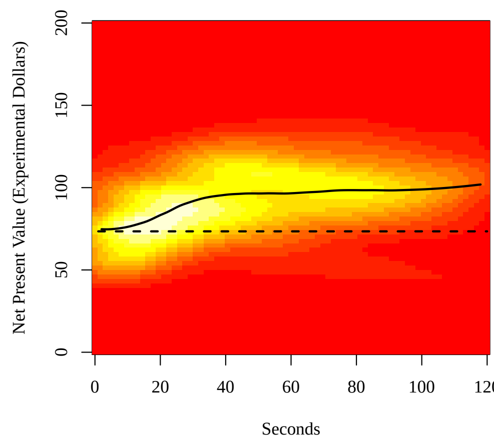
D.3 Explicit Proposal-Time Frequencies for All Rounds

Figure 22 illustrates the relative frequency of proposal-time combinations over time in every round of SE1. Construction of these illustrations is the same as those in Figure 12. The background gradient is a heat map of proposal-time relative frequencies, with light colors representing greater frequency (see Venables and Ripley, 2002). The solid black line in each plot shows the average explicit proposal over time—estimated under a Loess model with smoothing parameter $\alpha = 0.5$—and the dashed black line is the flat theoretic prediction of \$73.44.

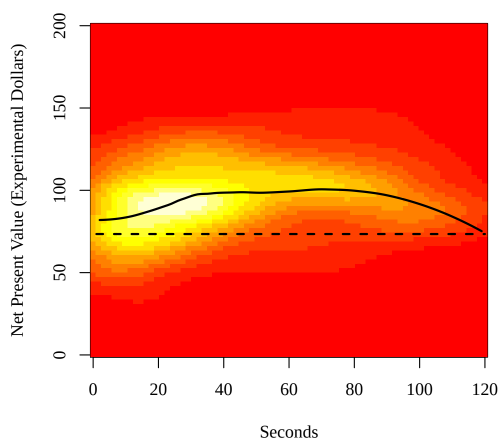
Figure 22: Relative Frequency of Proposal-Time Combinations by Round



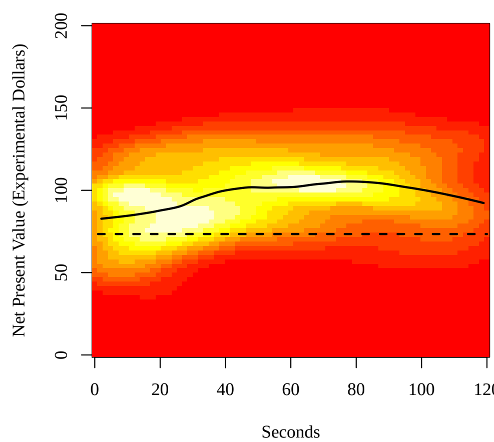
(a) Round 3



(b) Round 4

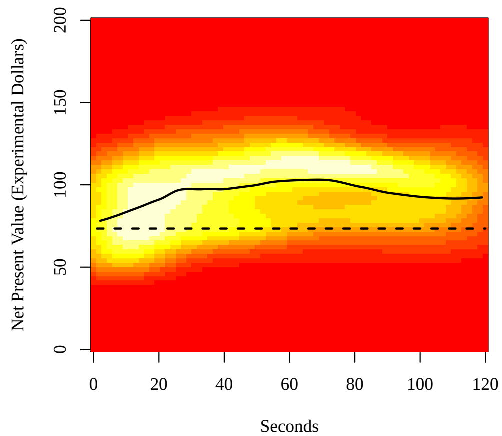


(c) Round 5

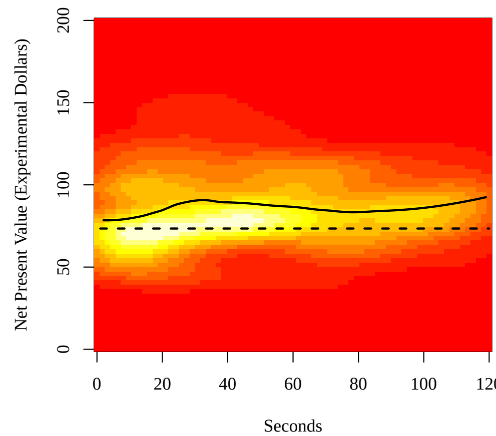


(d) Round 6

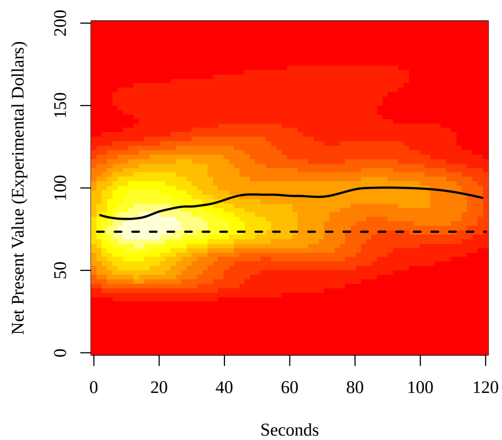
Figure 22: Relative Frequency of Proposal-Time Combinations by Round (Cont...)



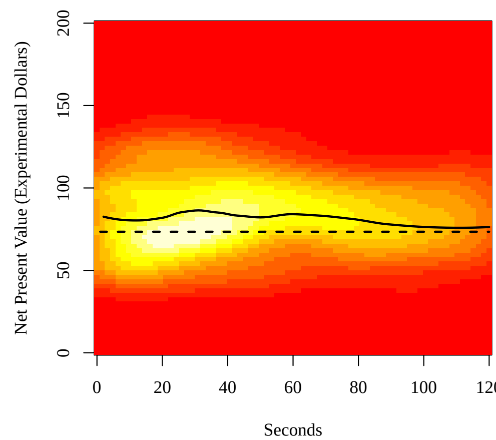
(e) Round 7



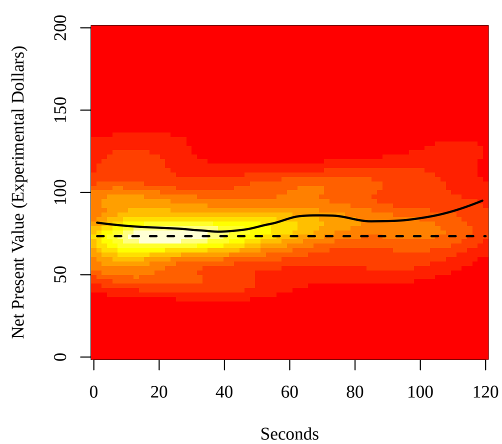
(f) Round 10



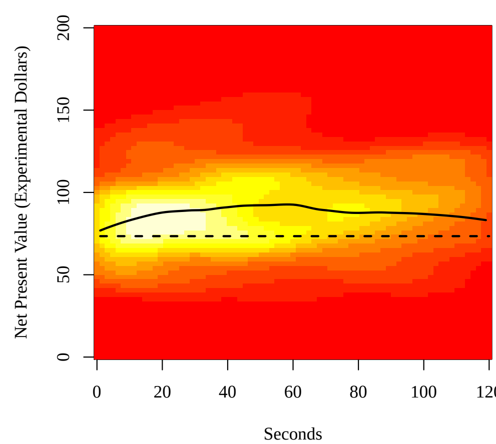
(g) Round 11



(h) Round 12



(i) Round 13

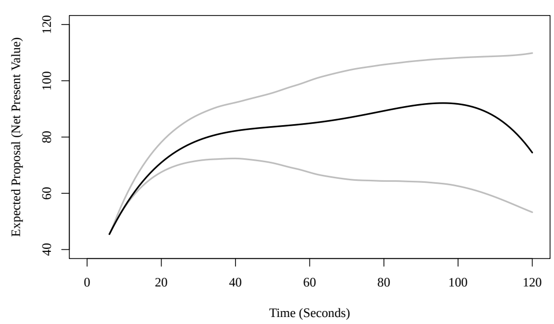


(j) Round 14

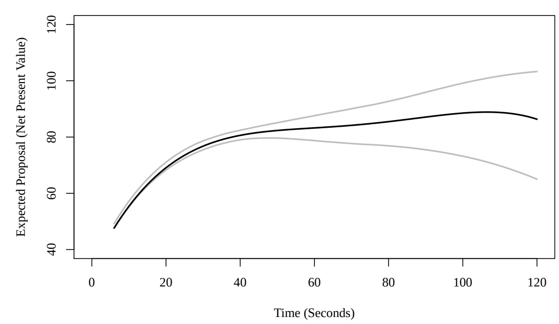
D.4 Average NPV Proposal over Time for All Rounds

Figure 23 illustrates the average NPV proposal in all rounds of SE1. Construction of these illustrations is the same as those in Figure 14. The central black line represents the average observed NPV proposal as estimated by the relevant polynomial regression in Table 10. Gray bounding lines are constructed as worst-case-scenario bounds on the conditional expected value of all (observed and unobserved) proposals (Manski, 1989); details of the estimator are provided in Appendix E.2.

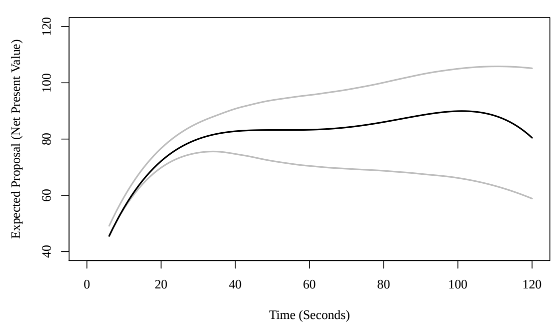
Figure 23: Average NPV Proposal over Time by Round



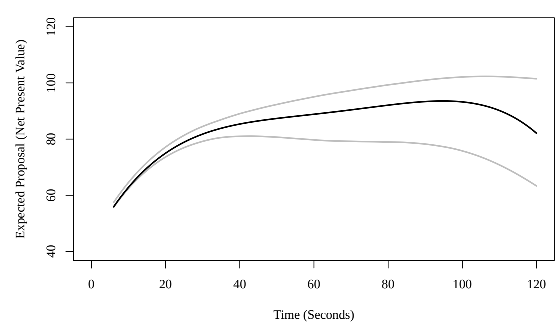
(a) Round 3



(b) Round 4

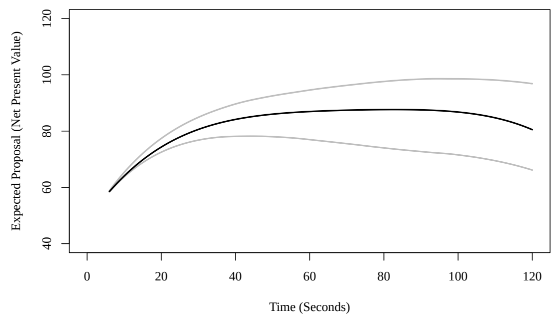


(c) Round 5

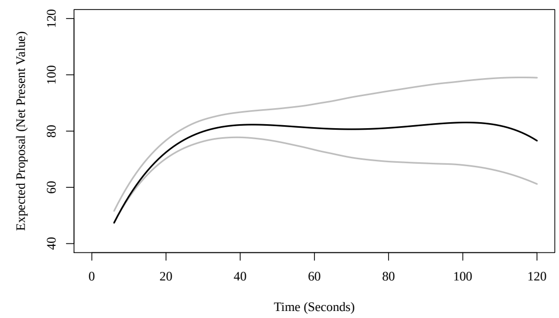


(d) Round 6

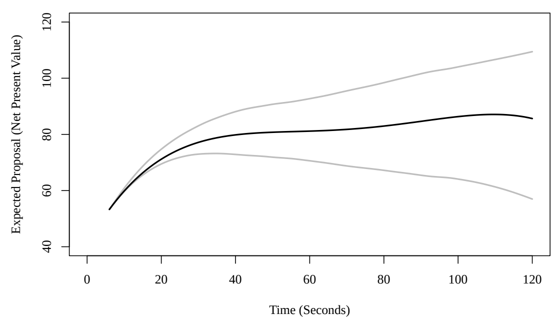
Figure 23: Average NPV Proposal over Time by Round (Continued...)



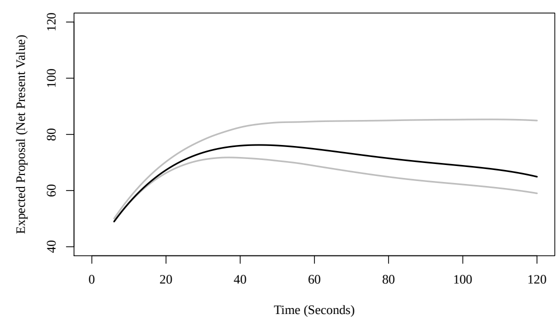
(e) Round 7



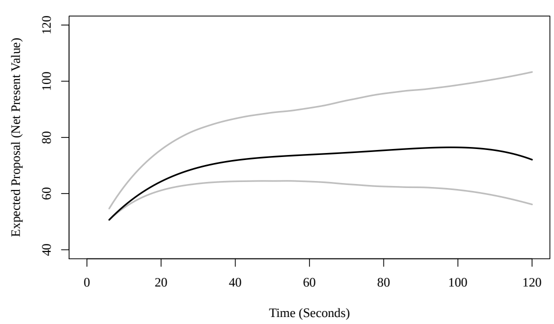
(f) Round 10



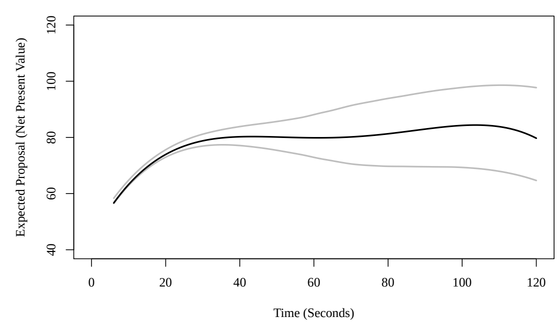
(g) Round 11



(h) Round 12



(i) Round 13



(j) Round 14

E Technical Appendix

E.1 Average Proposal Inference

An *ad hoc* normal theory test statistic is used in comparing average observed and predicted NPV settlement proposals. Abusing notation for simplicity, let $S_{i,t}$ denote the NPV proposal made at time t of dispute i.[90] Observed proposals for the $m = 60$ disputes in a single round of SE1 can be represented as

$$\begin{array}{ccccc} S_{1,1} & S_{1,2} & \cdots & S_{1,n_1} & \text{(dispute 1)} \\ S_{2,1} & S_{2,2} & \cdots & S_{2,n_2} & \text{(dispute 2)} \\ \vdots & \vdots & \ddots & \vdots & \\ S_{m,1} & S_{m,2} & \cdots & S_{m,n_m} & \text{(dispute } m\text{)} \end{array}$$

where $1 \leq n_i \leq 120$ represents the number of observed proposals in dispute i and varies by dispute according to the time of resolution.

Between-dispute proposals are plausibly independent, so that $S_{i,t}$ is assumed independent of $S_{j,r}$ for all $i \neq j$, but within-dispute proposals may tend to serially correlate. The full sample of proposals are thus not iid observations within the ambit of conventional one-sample normal-theory or permutation tests.

To address the complications posed by this data structure, define dispute-average proposals:

$$\overline{S}_i = \frac{1}{n_i} \sum_{t=1}^{n_i} S_{i,t} \quad \text{for all } i = 1, \ldots, m. \tag{37}$$

Dispute-average proposals are independent, but unequal sequence lengths (the n_i terms) suggest that the variances of dispute-average proposals $\sigma_1^2, \ldots, \sigma_m^2$ will not

[90]Note carefully that this definition of $S_{i,t}$ is inconsistent with the definition of S_t in the theoretic model. The former represents a NPV settlement proposal observed in the experiment; the latter represents a GV settlement proposal contemplated in the theoretic model.

generally be the same for all $i = 1, \ldots, m$. Dispute-average proposals are thus independent, but not identically distributed.

Let $\overline{S}$ be the grand mean of observed proposals

$$\overline{S} = \frac{1}{m} \sum_{i=1}^{m} \overline{S}_i. \tag{38}$$

Since dispute-average proposals are independent, the variance of $\overline{S}$ can be expressed as a sum of variances:

$$\sigma^2 = \mathrm{V}\left[\overline{S}\right] = \frac{1}{m^2} \sum_{i=1}^{m} \sigma_i^2. \tag{39}$$

Under either the Lyapunov or Lindeberg regularity conditions (see, e.g. Ash and Doléans-Dade, 2000, pp. 307-315), a central limit theorem provides convergence in distribution for the usual test statistic:

$$T_n = \frac{\overline{S} - \mathrm{E}\left[\overline{S}\right]}{\sigma} \xrightarrow{d} N(0, 1). \tag{40}$$

The remaining complication in this approach is definition of the distribution-average proposal variances: $\sigma_1^2, \ldots, \sigma_m^2$. These variances are (by definition) simple functions of proposal sequence covariances:

$$\sigma_i^2 = \mathrm{V}\left[\overline{S}_i\right] = \frac{1}{n_i^2} \sum_{t=1}^{n_i} \sum_{r=1}^{n_i} \mathrm{C}\left[S_{i,t}, S_{i,r}\right] \quad \text{for all } i = 1, \ldots, m. \tag{41}$$

Restrictions on the form of serial correlation are needed if equation (41) is to do any work. The explanation of NPV proposal sequences provided in Section 8.1 does not obviously recommend any standard model serial correlation, but does tend to suggest the plausibility of assuming autocorrelation is constant across disputes—downward runs are, after all, common linear functions of prior explicit proposals.

Assuming serial correlation is common across disputes, data on all observed settlement proposals in a round can be used to estimate the elements of a flexible variance-covariance matrix by any consistent estimator $\widehat{\mathrm{C}}\,[S_t, S_r]$ for all $t, r \in \{1, \ldots, 120\}$. Substituting estimated covariances into equation (41) provides a consistent estimator for σ_i^2, and normal theory inferences regarding the location of $\overline{S}$ can then be conducted using the convergence rule for T_n in equation (40).

E.2 Bound Estimator for Expected Proposal over Time

Worst-case-scenario bounds on the expected value of all (observed and unobserved) proposals over time are constructed using an estimator proposed by Manski (1989). Abusing notation for simplicity, let $S_{i,t}$ denote the NPV proposal made at time t of dispute i.[91] Further, let $Z_{i,t}$ be a binary variable valued 1 if proposal $S_{i,t}$ is observed (i.e. precedes settlement) and valued 0 if $S_{i,t}$ is unobserved.

The experimental design identifies the expected value of observed proposals over time, $\mathrm{E}\,[S_t|Z_t = 1]$, and the probability of observing a proposal, $\varphi_t = \mathrm{P}\,[Z_t = 1]$, but not the expected value of unobserved proposals over time, $\mathrm{E}\,[S_t|Z_t = 0]$, and thus not the expected value of all (observed and unobserved) proposals over time, $\mathrm{E}\,[S_t]$. Provided upper and lower bounds on the expected value of unobserved proposals over time, $\alpha_t \leq \mathrm{E}\,[S_t|Z_t = 0] \leq \beta_t$, a simple exercise in the law of total probability places identification bounds on $E[S_t]$:

$$\mathrm{E}\,[S_t|Z_t = 1]\varphi_t + \alpha_t(1 - \varphi_t) \leq \mathrm{E}\,[S_t] \leq \mathrm{E}\,[S_t|Z_t = 1]\varphi_t + \beta_t(1 - \varphi_t). \tag{42}$$

In the present application, bounds on the expected value of unobserved proposals over time are set equal to the 10% and 90% empirical quantiles of the distribution of all

[91]See n. 90 on comparison to notation in the theoretic model.

observed proposals in SE1 disputes: $\alpha_t = 50.08$ and $\beta_t = 115.19$ for all $t = 1, \ldots, 120$. An estimator of the worst-case-scenario bounds on $E[S_t]$ then follows from replacing the remaining terms in equation (42) with sample estimators. The expected value of observed proposals over time, $\mathrm{E}[S_t|Z_t = 1]$, is estimated by the relevant polynomial regression in Table 10. A Loess model with smoothing parameter $\alpha = 0.5$ is used to provide a flexible, nonparametric estimator of φ_t.

E.3 Estimation and Inference for Hazard Functions

Hazard rates are fitted by a robust parametric estimator. Abusing notation, let i denote a unique pair of plaintiff and defendant subjects in SE1. Let $d_{i,m}$ denote the observed value of delay-to-resolution in repetition $m \leq M$ of matching i: randomization results in an uneven panel with $M = 1, \ldots, 4$. Observed resolution times are not censored. The desired estimand is some version of the discrete hazard,

$$h(t) = \frac{\mathrm{P}[d = t]}{1 - \sum_{i=1}^{t-1} \mathrm{P}[d = t]} \qquad t = 1, \ldots, T + 1. \tag{43}$$

When a general estimate of the average hazard function is desired, a common approach is to rely on a non-parametric technique such as the Kaplan-Meier estimator (see, e.g., Lancaster, 1990; Miller, 1981). The Kaplan-Meier estimator offers attractive flexibility, but is burdened by two significant limitations for purposes of the present application: (i) potentially poor power characteristics relative to parametric hazard models (Miller, 1983), and (ii) absence of any obvious way to adjust inference for data sampled in a panel structure.[92] To avoid these limitations, the present analysis relies on a slight modification of the robust "partial logistic regression" estimator of the hazard function suggested by Efron (1988).

[92]But cf. Meier et al. (2004) for a very delayed response to power criticisms.

In terms of the hazard of settlement, the partial logistic regression estimator is based on the assumption that the frequency of resolution at time t is binomial distributed with size parameter equal to the number of disputes unresolved at time t and with probability parameter $h(t)$. Adopting the notation of Efron (1988), let λ_t be the logistic parameter (i.e. the log odds of settlement at time t) and define the logistic regression model as $\lambda_t = \alpha x_t$ (in practice, the columns of x_t should probably contain some polynomial or spline of time, but may also contain other covariates). If $\widehat{\alpha}$ is a maximum likelihood estimator (MLE) of the coefficient vector α, then by invariance $\widehat{\lambda}_t = \widehat{\alpha} x_t$ is a MLE of the logistic parameter.

A MLE for the hazard rate, $h(t)$, is constructed by inverting the identity

$$\lambda_t = \log\left(\frac{h_t}{1-h_t}\right) \tag{44}$$

to produce the following estimator (again by invariance):

$$\widehat{h}(t) = (1+\exp(-\widehat{\lambda}_t))^{-1}. \tag{45}$$

If $\widehat{\mathrm{C}}$ denotes the estimated variance-covariance matrix associated with $\widehat{\alpha}$, then the variance of $\widehat{h}(t)$ is as follows (Efron, 1988, equation (3.3)):

$$\widehat{\mathrm{V}}\left[\widehat{h}(t)\right] = \left(\widehat{h}(t)(1-\widehat{h}(t))\right)^2 x_t \widehat{\mathrm{C}} x_t'. \tag{46}$$

Note that both $\widehat{h}(t)$ and $\widehat{\mathrm{V}}\left[\widehat{h}(t)\right]$ are easily computed with the output of logistic regression procedures in standard statistical programs. As a (conditional) MLE, $\widehat{h}(t)$ has an asymptotically normal and efficient sampling distribution under standard regularity conditions (see, e.g., Fahrmeir and Kaufmann, 1985; Gourieroux and Monfort, 1981; Amemiya, 1985).

With slight modification, the Efron (1988) partial logistic regression estimator can be coerced into accommodating various forms of dependence introduced by panel-data sampling. As a Generalized Linear Model (GLM), most common statistical packages have procedures for appropriately modeling the influence of repeat or group observations in logistic regression. Controls for within-cluster correlation, e.g. as a result of neglected heterogeneity, can be introduced in a Generalized Estimating Equation (GEE) framework (e.g. Liang and Zerger, 1986).[93] Explicit models of unobserved effects can also be accommodated in the framework of a Generalized Linear Mixed Model (GLMM) (e.g. Breslow and Clayton, 1993) or Hierarchical Generalized Linear Model (HGLM) (e.g. Lee and Nelder, 1996, 2006).

In the present application, collected data of the form $d_{i,m}$ are transformed into sequences of binary observations. For example, if $d^i_{i,m} = 7$ then seven observations are created: $\{y_1 = 0, \ldots, y_6 = 0, y_7 = 1\}$. Logistic regression fits the log odds of settlement ($y = 1$) to a constant term and fourth-order polynomial in time ($t = 1, \ldots, 120$). As plaintiff/defendant-pair heterogeneity is neglected in the regression model, a cluster-robust estimator of the variance-covariance matrix is employed based on a GEE framework (Halekoh et al., 2006).[94]

[93]Wooldridge (2006a) provides helpful commentary on cluster-robust techniques for various nonlinear models, and on the GEE perspective in particular.

[94]The present application employs the `geeglm` estimator in the `geepack` library for R. Comparable procedures in other software packages are `proc genmod` in SAS and `XLISP-STAT` in Stata (Halekoh et al., 2006). The option to compute cluster-robust standard errors in the Stata `logistic` procedure makes it another alternative.

Chapter V

Sub-Experiment 2: Asymmetric Information & Delay

Sub-Experiment 2 (SE2) explores measurements collected from sequences $\mathbf{S}_1, \ldots, \mathbf{S}_{10}$, isolating the effects of information asymmetry under various bargaining environments. The basic objective is confirmatory: collected data are used to determine (i) whether asymmetric information increases settlement delay, and (ii) whether the effect of asymmetric information differs under variations in the bargaining environment.

Section 10 defines the five treatment environments explored in SE2: a control environment, a reverse costs environment, a low costs environment, a low asymmetry environment, and an environment with law student subjects. Theoretic predictions compare expected resolution delay within and between treatment environments. The presence of asymmetric information is always predicted to increase settlement delay, though not necessarily to the same degree in each environment.

Section 11 discusses the results of SE2. Asymmetric information over the potential trial verdict is confirmed to induce an increase in delay. For control parameters, for example, exposure to the controlled information asymmetry increases delay-to-settlement by about 32 seconds—around a 95% increase in delay over symmetric information. Differences in delay between environments evince few obvious deviations from theory, but are not estimated with great precision.

Section 12 provides concluding discussion. Comments include (i) the importance of a causal link between information asymmetry and settlement delay, and (ii) the interpretation of modest differences between SE2 bargaining environments.

10 Treatments

Sub-Experiment 2 (SE2) concerns 5 different treatment *environments*: pairs of related sequences such as $\{\mathbf{S}_1, \mathbf{S}_2\}$, $\{\mathbf{S}_3, \mathbf{S}_4\}$, etc. Each environment consists of two treatments which differ only in the information factor, $\mathcal{I}$ (see Section 5.2). In one treatment, the plaintiff has asymmetric information about the value of a potential trial verdict; in the other treatment, information about the potential verdict is symmetric. Sequence pairing achieves an orthogonal order of treatment assignment within each environment. In half the sessions, the asymmetric information treatment is assigned first; in the other half, the symmetric information treatment is assigned first.

The remainder of this section describes the set of SE2 treatment environments. Section 10.1 explains the control environment: the control treatment (see Chapter IV) and a symmetric information version thereof. All other treatment environments consist of single-factor perturbations of the control environment. Section 10.2 explains the reverse costs environment. Section 10.3 covers the low costs environment. Section 10.4 discusses the low asymmetry environment. Finally, Section 10.5 explains the law school environment.

10.1 Control

The control treatment environment consists of sequences $\mathbf{S}_1$ and $\mathbf{S}_2$, in turn consisting of treatments $\mathbf{T}_0$ and $\mathbf{T}_1$ (see Tables 7 and 8 in Chapter III). Exhaustive description of the control treatment, $\mathbf{T}_0$, is provided in Chapter IV. As a brief summary, the control treatment defines a settlement bargaining game with potential damages information asymmetrically available to the plaintiff. Equilibrium involves the interior solution of Proposition 2 with persistently delayed settlement; the predicted distribution of delay is illustrated in Figure 8(c) of Chapter IV.

Treatment $\mathbf{T}_1$ differs from the control treatment only in dispensing *symmetric* information about the value of the potential trial verdict: i.e. the injury draw, x, is visible to both the plaintiff and the defendant. Equilibrium strategies in $\mathbf{T}_1$ are defined by Proposition 4 of Chapter II. In the theoretic equilibrium, the defendant makes an initial settlement proposal exactly equal to the plaintiff's expected net present value of a trial verdict. The plaintiff always accepts such a proposal, so every dispute settles and settlement is never delayed past the initial proposal. The full-settlement and no-delay predictions of Proposition 4 generalize to *every* treatment with the symmetric information level of the information environment factor, $\mathcal{I}_1$.

As predictors of settlement bargaining behavior in SE2, theoretic implications for treatments $\mathbf{T}_0$ and $\mathbf{T}_1$ suggest the following *strong* hypothesis about settlement delay: delay should be extensive when information is asymmetric, and should be non-existent when information is symmetric. The naïvete of this proposition is demonstrated in behavioral regularities 1 and 3 of Section 4.1: laboratory experiments almost universally reject the proposition that immediate agreement obtains when structured bargaining games are played with symmetrically informed subjects.

Unexplained sources of settlement delay make reliance on the strong hypothesis problematic. Because settlement may be delayed even when information about the potential trial verdict is symmetric, not all of the settlement delay observed under asymmetric information is necessarily attributable to the effects of the controlled information asymmetry. Identifying the treatment effect of asymmetric information on settlement delay thus requires a more flexible working hypothesis on the effects of exposure to the controlled information asymmetry. Rather than attempting to gain flexibility by formalizing unexplained sources delay, the present analysis relies on the following *weak* implication of theoretic results.

Remark 5. If $\mathbf{T}_i$ and $\mathbf{T}_j$ are treatments that differ only in respective information factor levels $\mathcal{I}_0$ (asymmetric information) and $\mathcal{I}_1$ (symmetric information), then delay-to-resolution and delay-to-settlement are stochastically larger in $\mathbf{T}_i$ than $\mathbf{T}_j$.[95]

Remark 5 is a weak implication of theoretic results in the sense that it contains the naïve theoretic implication as a special case. The underlying logic is that, with all else equal, introducing a controlled information asymmetry to a bargaining environment should not *decrease* any extant resolution delay. This relationship seems reasonable as a first order approximation, and suggests that within-session differences in expected delay identify the treatment effect of the controlled information asymmetry even in the presence of unexplained sources of settlement delay.

Remark 6. Let $D_R^{\mathbf{T}}$ and $D_S^{\mathbf{T}}$ denote treatment-specific delay-to-resolution and delay-to-settlement. For any $\mathbf{T}_i$ and $\mathbf{T}_j$ which differ only in respective information factor levels $\mathcal{I}_0$ and $\mathcal{I}_1$, Remark 5 implies the following inequalities:

$$\mathrm{E}\,[D_R^{\mathbf{T}_i}] > \mathrm{E}\,[D_R^{\mathbf{T}_j}] \qquad \mathrm{E}\,[D_S^{\mathbf{T}_i}] \geq \mathrm{E}\,[D_S^{\mathbf{T}_j}].$$

The prediction expressed by Remark 6 is intuitive, plausible, and testable under conventional statistical tests of location. It also includes as a special case the more demanding theoretic predictions of the strong hypothesis given above. A noteworthy limitation of relying on the weak theoretic implication in Remark 5 is lack of a concrete prediction for the magnitude of differences in expected delay. Though presented under the heading of the control environment, Remark 6 predicts the treatment effect of asymmetric information in all other SE2 treatment environments as well.

[95] Stochastic dominance is a generalization of the concept of inequality for deterministic variables. Let X and Y be random variables with cumulative distribution functions F_X and F_Y. By definition, X is stochastically larger than Y if and only if $F_X(t) \leq F_Y(t)$ for all t, with strict inequality for some t. Shift and scale models of distributional difference are special cases of stochastic dominance. An immediate implication is that $\mathrm{E}\,[X] > \mathrm{E}\,[Y]$.

10.2 Reverse Costs

The reverse costs treatment environment consists of sequences $\mathbf{S}_3$ and $\mathbf{S}_4$, in turn consisting of treatments $\mathbf{T}_2$ and $\mathbf{T}_3$. The structure is the same as that of the control environment: treatments differ only in the information factor, $\mathcal{I}$. Relative to the control environment, the reverse costs treatment environment changes the parameter values factor from $\mathcal{P}_0$ to $\mathcal{P}_1$. Parameter values in the $\mathcal{P}_1$ level are the same as those in $\mathcal{P}_0$, but with cost terms swapped between litigants: i.e. $c_p \leftrightarrow c_d$, $k_p \leftrightarrow k_d$. Differences between parameter values under the control and reverse costs treatment environments are consolidated in Table 13.

Parameter values in the asymmetric information treatment, $\mathbf{T}_2$, satisfy the requirement for persistent delay with continuous bargaining (Proposition 3). An interior equilibrium obtains in which settlement is systematically delayed. By contrast, the symmetric information treatment, $\mathbf{T}_3$, is characterized by Proposition 4: all disputes settle in equilibrium, and the distribution of delay is degenerate at $t = 1$. In common with all other SE2 treatment environments, greater expected delay is predicted under the asymmetric information treatment (Remark 6). An additional inquiry concerns the relative distribution of resolution delay between $\mathbf{T}_2$ and the control treatment $\mathbf{T}_0$.

Corollary 4. *With asymmetric information, the following sum-invariance properties characterize the (Corollary 2) ex ante probability of dispute resolution, p_t:*

1. *The values of $p_1, \ldots, p_{T-1}$ are constant over all sum-invariant combinations of negotiation costs $\{c_p, c_d : c_p + c_d = C\}$.*

2. *The value of p_T is constant over all sum-invariant combinations of trial costs $\{k_p, k_d : k_p + k_d = K\}$.*

3. *The probability of a trial verdict, p_{T+1}, is constant over all combinations of sum-invariant costs $\{(c_p, c_d), (k_p, k_d) : (c_p + c_d, k_p + k_d) = (C, K)\}$.*

Table 13: Non-Control Parameter Values

Parameter	$\mathcal{P}_0$ (Control)[a]	$\mathcal{P}_1$ (Reverse Costs)[b]	$\mathcal{P}_2$ (Low Costs)[b]	$\mathcal{P}_3$ (Low Asymmetry)[b]
$\underline{x}$	\$50.00	—	—	—
$\overline{x}$	\$200.00	—	—	\$150.00
π	0.75	—	—	—
T	120	—	—	—
δ	$^{1000}/_{1001}$	—	—	—
c_p	\$0.14	\$0.32	\$0.07	—
c_d	\$0.32	\$0.14	\$0.16	—
k_p	\$11.00	\$5.00	—	—
k_d	\$5.00	\$11.00	—	—

[a] See Table 9 for details on parameter value translation to the experimental bargaining environment.
[b] A "—" indicates equality to the corresponding $\mathcal{P}_0$ parameter value.

As implied by Corollary 4, sum-invariant perturbations in costs (like the change in costs between $\mathcal{P}_0$ and $\mathcal{P}_1$) have no theoretic effect on the *ex ante* probability of resolution at any point in time. Intuitively, this follows from casting the model as a sequence of concatenated ultimatum games. The defendant's ability to exploit the bargaining power of making an ultimatum offer means full appropriation of all cost savings from settlement. The timing of resolution thus concerns sums of own negotiation or trial costs and reciprocal plaintiff cost terms, and is invariant to sum-neutral changes in these costs.

Remark 7. Expected resolution delay is the same in the control and reverse costs treatment environments:

$$\mathrm{E}\,[D_R^{\mathbf{T}_2}] = \mathrm{E}\,[D_R^{\mathbf{T}_0}] \qquad \mathrm{E}\,[D_S^{\mathbf{T}_2}] = \mathrm{E}\,[D_S^{\mathbf{T}_0}].$$

Data collected under $\mathbf{T}_2$ allow for empirical assessment of the comparative statics in Remark 7. Comparing average delay under $\mathbf{T}_0$ and $\mathbf{T}_2$ also provides a robustness check for the concern that $\mathcal{P}_0$ parameter values might somehow be inducing a deceptive distribution of resolution delay: e.g. that the directions of cost asymmetry under $\mathcal{P}_0$ parameter values for some reason greatly promotes resolution delay. A roughly comparable pattern of behavior with costs reversed serves to mitigate concern about this potential source of design bias.

The reverse costs treatment environment also contributes in assessing the Remark 6 prediction that exposure to asymmetric information increases delay in dispute resolution. Observed satisfaction of the prediction across a variety of treatment environments provides understandably stronger evidence of validity than satisfaction of the inequality under control parameters alone.

10.3 Low Costs

The low costs treatment environment consists of sequences $\mathbf{S}_5$ and $\mathbf{S}_6$, in turn consisting of treatments $\mathbf{T}_4$ and $\mathbf{T}_5$. The control environment is perturbed by changing the parameter values factor from $\mathcal{P}_0$ to $\mathcal{P}_2$. Parameters in the $\mathcal{P}_2$ level are the same as those in $\mathcal{P}_0$, but with negotiation costs set to half their control value: i.e. $c_p \to {}^1\!/\!{}_2\, c_p$, $c_d \to {}^1\!/\!{}_2\, c_d$. Specific parameter values are consolidated in Table 13.

Parameter values in the asymmetric information treatment, $\mathbf{T}_4$, satisfy the requirement for persistent delay with continuous bargaining (Proposition 3) so that an interior equilibrium obtains. Proposition 4 characterizes the full-settlement and no-delay equilibrium in the symmetric information treatment, $\mathbf{T}_5$. As in other treatment environments, Remark 6 predicts greater expected delay under the asymmetric information treatment. Any difference in equilibrium behavior from changing $\mathcal{P}_0$ to $\mathcal{P}_2$ is limited to the asymmetric information treatment, $\mathbf{T}_4$.

Corollary 5. *Let $C = c_p + c_d$ denote aggregate negotiation costs. The (Corollary 2) ex ante probability of dispute resolution, p_t, responds to changes in C as follows:*

$$\frac{\partial p_t}{\partial C} = \begin{cases} \pi^{-1}\delta^{-T+t}/(\overline{x} - \underline{x}) > 0 & t = 1, \ldots, T-1 \\ 0 & t = T \\ -\sum_{i=1}^{T-1} \pi^{-1}\delta^{-T+i}/(\overline{x} - \underline{x}) < 0 & t = T+1. \end{cases}$$

The implication of Corollary 5 is that a reduction in bargaining costs decreases the instantaneous probability of settlement throughout nearly the entire duration of settlement bargaining (periods $t = 1, \ldots, T-1$), but has no effect on the probability of settlement in the final second of bargaining (period T). The probability of a trial verdict increases by the sum of the decrease in settlement probabilities. This conforms to intuition: as the costs of delay decrease, it seems reasonable that the probability of (rapid) settlement should likewise decrease. Combined with the definitions of delay-to-resolution and delay-to-settlement in Corollary 3, Corollary 5 implies both forms of delay in the control treatment stochastically dominate the same in the the low costs treatment. Concrete predictions for experimental parameter values are as follows.

Remark 8. Expected resolution delay is greater in the low costs treatment environment than in the control environment:

$$\mathrm{E}\left[D_R^{\mathbf{T}_4}\right] - \mathrm{E}\left[D_R^{\mathbf{T}_0}\right] \approx 16.07 \qquad \mathrm{E}\left[D_S^{\mathbf{T}_4}\right] - \mathrm{E}\left[D_S^{\mathbf{T}_0}\right] \approx 8.51.$$

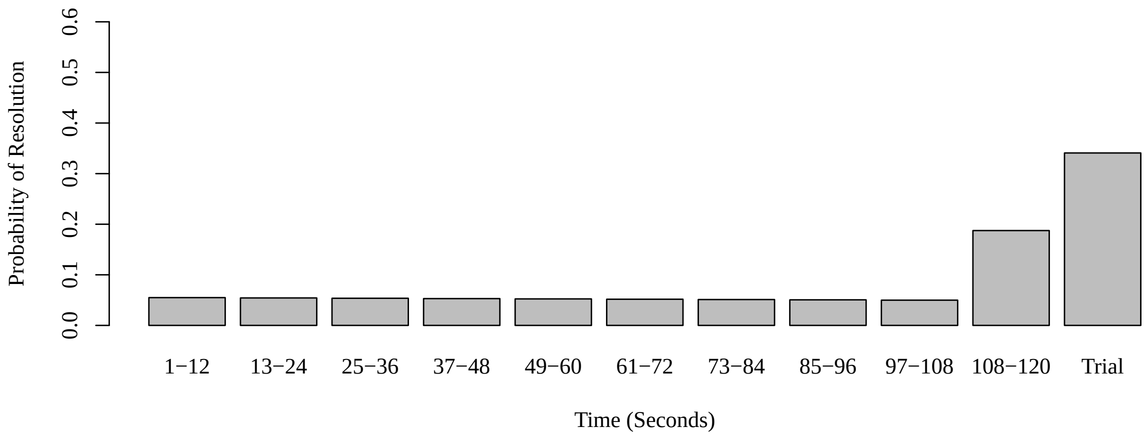
Figure 24 illustrates the predicted distribution of resolution delay under various SE2 treatments. Figure 24(a) shows resolution delay under the control treatment. Figure 24(b) shows the same under the $\mathbf{T}_4$ treatment with asymmetric information and $\mathcal{P}_2$ parameter values. Comparison illustrates the implication of Corollary 5: the probability of settlement is (almost) everywhere greater under the control treatment environment than it is under the low costs environment.

As an alternative way to visualize distributional differences, Figure 25 illustrates theoretic hazard functions for the same SE2 treatments.[96] The hazard of settlement in the low costs treatment environment is everywhere lower than the hazard in the control environment. With a greater measure of disputes ending in a trial verdict, the low costs hazard function also increases more modestly over time.

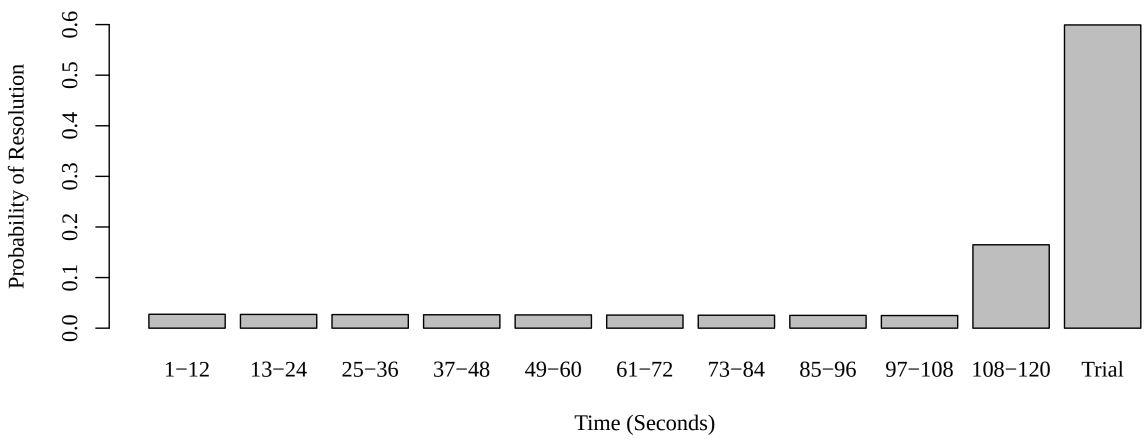
Comparing average delay in $\mathbf{T}_0$ and $\mathbf{T}_4$ affords an empirical test of the Remark 8 predictions about delay sensitivity to negotiation costs. The low costs environment also acts as a robustness check in assessing the causal relationship between presence of a controlled information asymmetry and resolution delay.

[96] See Section 8.4 for the definition of a hazard function.

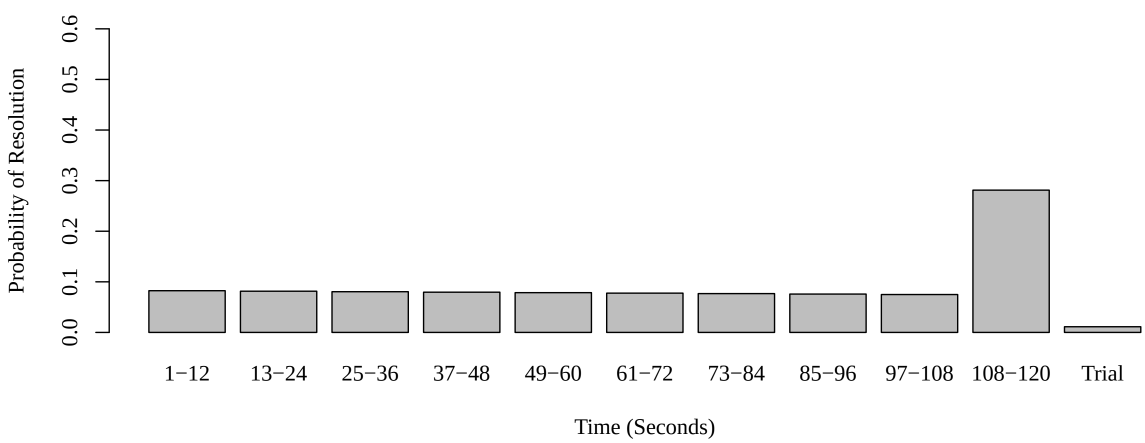
Figure 24: Predicted Delay-to-Resolution Distribution in SE2



(a) Distribution of Resolution Timing in $\mathbf{T}_0$ (Control)

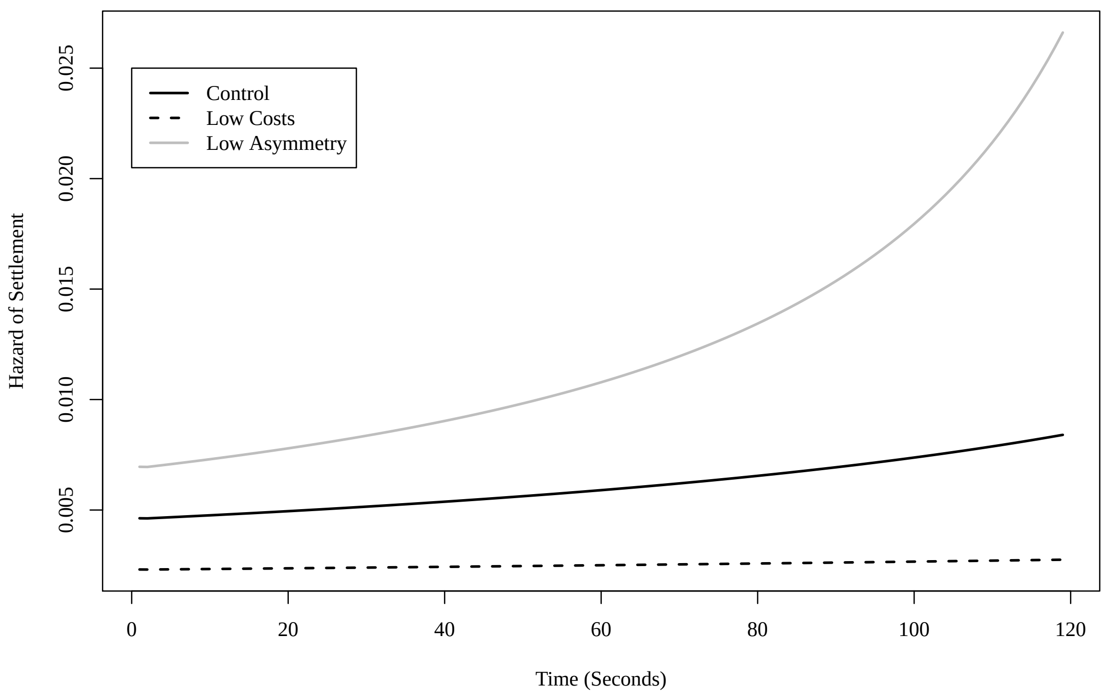


(b) Distribution of Resolution Timing in $\mathbf{T}_4$ (Low Costs)



(c) Distribution of Resolution Timing in $\mathbf{T}_6$ (Low Asymmetry)

Figure 25: Theoretic Hazard of Settlement in Select Treatments[a]



[a]Hazard rates for dispute resolution are illustrated up-to-but-excluding the final second of bargaining and the trial verdict phase: i.e. the illustration covers seconds 1 through 119 of a dispute. Truncating the illustration at 119 seconds improves legibility, as the sharp spike in the hazard function in the final second of bargaining swamps all other variability.

10.4 Low Asymmetry

The low asymmetry treatment environment consists of sequences $\mathbf{S}_7$ and $\mathbf{S}_8$, in turn consisting of treatments $\mathbf{T}_6$ and $\mathbf{T}_7$. This treatment environment perturbs the control environment by changing the parameter values factor from $\mathcal{P}_0$ to $\mathcal{P}_3$. The support of potential damages is compacted by a reduction in the upper bound on potential damages, from $\overline{x} = 200$ to $\overline{x} = 150$. Comparison to other treatment environments is provided in Table 13.

Like other asymmetric information treatments in SE2, equilibrium in $\mathbf{T}_6$ involves the interior solution to Propositions 1 and 2, and like other symmetric information treatments, the equilibrium in $\mathbf{T}_7$ involves full settlement without any delay (Proposition 4). Greater expected delay is predicted under the asymmetric information treatment (Remark 6). An additional inquiry concerns the relative distribution of resolution delay between $\mathbf{T}_6$ and the control treatment $\mathbf{T}_0$.

Corollary 6. *The (Corollary 2) ex ante probability of dispute resolution, p_t, responds to changes in $\overline{x}$ as follows:*

$$\frac{\partial p_t}{\partial \overline{x}} = \begin{cases} -\pi^{-1}\delta^{-T+t}(c_p + c_d)/(\overline{x} - \underline{x})^2 < 0 & t = 1, \ldots, T-1 \\ -\pi^{-1}(k_p + k_d)/(\overline{x} - \underline{x})^2 < 0 & t = T \\ -\sum_{i=1}^{T} \partial p_i / \partial \overline{x} > 0 & t = T+1. \end{cases}$$

Reducing the upper limit on potential damages increases the probability of settlement in each period $t = 1, \ldots, T$ and decreases the probability of a trial verdict. Intuitively, this is like squeezing one part of a plastic bag of water (specifically, the trial verdict part), which leads the water level to rise proportionally over the unsqueezed portion of the bag (i.e. the settlement part). Combining Corollaries 2, 3, and 6, delay-to-resolution in $\mathbf{T}_0$ should stochastically dominate that in $\mathbf{T}_6$, with delay-to-settlement the same in both. A brief "proof" is provided in Appendix F.1. Concrete predictions for experimental parameter values are as follows.

Remark 9. Expected delay-to-resolution is greater in the control environment than the low asymmetry environment, but expected delay-to-settlement is the same:

$$\mathrm{E}\,[D_R^{\mathbf{T}_6}] - \mathrm{E}\,[D_R^{\mathbf{T}_0}] \approx -16.14 \qquad \mathrm{E}\,[D_S^{\mathbf{T}_6}] = \mathrm{E}\,[D_S^{\mathbf{T}_0}].$$

Figure 24 illustrates the prediction of Remark 9: Figure 24(c) corresponds to the distribution of resolution delay under the low asymmetry treatment, $\mathbf{T}_6$. The probability of settlement is everywhere greater under the low asymmetry treatment than the control. An alternative illustration is provided by Figure 25, which shows the hazard of settlement in the low asymmetry environment to be everywhere higher and more rapidly increasing over time than the hazard in the control environment.

Comparing average delay in $\mathbf{T}_0$ and $\mathbf{T}_6$ affords an empirical test of the comparative statics in Remark 9. The low asymmetry treatment environment also acts as a robustness check for the Remark 6 prediction that the controlled information asymmetry causes settlement delay. It does so by repeating the test under different parameter values, and by providing delay data from simultaneous variation in both the presence and degree of controlled information asymmetry.

10.5 Law School

The law school treatment environment consists of sequences $\mathbf{S}_9$ and $\mathbf{S}_{10}$, in turn consisting of treatments $\mathbf{T}_8$ and $\mathbf{T}_9$. The control treatment environment is perturbed by changing the subject pool factor from $\mathcal{U}_0$, (undergraduate subjects) to $\mathcal{U}_1$ (law student subjects). Subject compensation is simultaneously manipulated as a control for perceived differences in the opportunity cost of participation. Though *ad hoc* and of uncertain efficacy, the intent of differential compensation is to offset extant differences in incentivization between subject populations (see Section 5.2). In analysis, data collected from law school subjects are treated as comparable to data collected from undergraduate student subjects.

The subpopulation from which subjects are recruited has no theoretic effect on equilibrium behavior. Equilibria in treatments $\mathbf{T}_8$ and $\mathbf{T}_9$ are correspondingly the same as the respective control treatments $\mathbf{T}_0$ and $\mathbf{T}_1$. Remark 6 predicts greater expected delay under the asymmetric information treatment than the symmetric information treatment. No difference in average delay is predicted between asymmetric information treatments $\mathbf{T}_8$ and $\mathbf{T}_0$.

Remark 10. Expected resolution delay is the same in the control and law school treatment environments:

$$\mathrm{E}\,[D_R^{\mathbf{T}_8}] = \mathrm{E}\,[D_R^{\mathbf{T}_0}] \qquad \mathrm{E}\,[D_S^{\mathbf{T}_8}] = \mathrm{E}\,[D_S^{\mathbf{T}_0}].$$

Data collected from the law school treatment environment provide a rough test of external validity for measurements taken from undergraduate subjects. The underlying concern is that heavy reliance on data collected from undergraduate subjects in most treatments of SE2 (and all treatments of SE1 and SE3) may undermine experimental validity if undergraduate subjects are somehow unrepresentative of the population of litigants. Abstract presentation of settlement bargaining in the experiment mitigates any obvious need for subjects to be real-world litigants, but does not provide a compelling argument for relying on undergraduate students in particular.

A robustness check for the use of undergraduate subjects is achieved by assigning law-student subjects to what is otherwise a control treatment environment. Intensive exposure to legal analysis and practice makes subjects from the law school subpopulation subjectively different from undergraduate-student subjects, and arguably more representative of litigants in the field.[97]

Differences in behavior between the control and law school treatment environments identify the effects of population-differences in perception, strategy, etc. The law school treatment environment also contributes in assessing the Remark 6 prediction that information asymmetry causes greater delay in dispute resolution. Observed satisfaction of the prediction with subjects from different populations provides increased confidence in prediction validity.

[97]Though a potentially more appropriate subpopulation, the small size of the law school foreclosed conducting more than a handful of experimental sessions with law student subjects. Law students were recruited from 1st, 2nd, and 3rd year classes at the University of Virginia School of Law. All sessions involving law student subjects were conducted at the end of the academic year. Discussions with law school faculty suggest that by the end of first-year courses, first-year law students have the same basic legal knowledge as their more senior peers.

11 Results

The objective of SE2 is broadly confirmatory: collected data are used to determine whether and to what extent asymmetric information over a potential trial verdict tends to induce delay in the resolution of disputes. Data are also used to compare observed resolution delay under various models of settlement bargaining, with resolution delay analyzed in both expectation and distribution. Two comments on data analysis are generally applicable.

First, data from the first two rounds of a treatment assignment are omitted from analysis except as needed in constructing lag terms. Dropping initial rounds is meant to control for rapid learning and strategy-adjustment in the early rounds of exposure to a treatment.[98] Second, each SE2 treatment is assigned as $\mathbf{T}_A$ (the first treatment in a session) in two of the sessions for a particular environment, and as $\mathbf{T}_B$ (the second treatment in a session) in the other two sessions. The orthogonal order of treatment assignment combines with other experimental controls to mitigate any serious concern about design bias from order or sequence effects.[99]

The remainder of this section proceeds as follows. Section 11.1 assesses the fundamental question of the present study: whether asymmetric information causes delay in the resolution of disputes. Results strongly confirm that it does. Section 11.2 compares resolution delay between the different SE2 treatment environments. Few compelling differences from theoretic prediction are observed. Finally, Section 11.3 assesses the distribution of resolution delay under the different SE2 treatments. Again, differences between treatments are modest.

[98]For additional discussion, see Section 8, particularly n. 70.

[99]These potential sources of design bias are discussed in Section 5.5. As noted in Section 9.2, data collected in SE1 suggest differences between assignments are muted. An additional control is the inclusion of fixed round-effects in regression analysis.

11.1 Causal Delay

Presentation of results begins with an empirical assessment of Remark 6: the prediction that asymmetric information about the potential trial verdict increases average delay-to-resolution and delay-to-settlement. The proposition that asymmetric information might help explain systematic settlement delay is both the launching point of a considerable theoretic literature, and an assertion for which supporting empirical evidence is remarkably scarce (see Section 1.2). Addressing Remark 6 raises the inquiries posed in Research Question 1: (i) whether asymmetric information causes delayed resolution, and (ii) to what extent average resolution is delayed.

The identification strategy in this section is comparison of average delay within SE2 treatment environments. As each environment consists of two treatments that differ only in the information factor (asymmetric information or symmetric information), within-environment differences in resolution delay are attributable to the causal effect of the controlled information asymmetry.

A starting point is assessment of session-average delay. Averaging delay measurements across all disputes in a session provides a matched pair of observations for each session: one corresponding to the treatment with asymmetric information, the other corresponding to the treatment with symmetric information. Session-average observations may be dependent within a matched pair (as the same group of subjects are assigned to each treatment), but are independent across sessions and plausibly identically distributed (at least, within-treatment) as the products of a common data generating process.

Aggregating across the different treatment environments in SE2, the average increase in delay-to-resolution (D_R) caused by the controlled information asymmetry is about 25.18 seconds. This is statistically distinguishable from the null hypothesis of no treatment effect, with an exact p-value of 1.907×10^{-6} under a paired-sample application of Wilcoxon's Signed-Rank test.[100] The average increase in delay-to-settlement (D_S) is 23.78 seconds with the same p-value. In fact, the presence of asymmetric information causes a strict increase in resolution delay in every one of the 20 experimental sessions that compose SE2.

For lower level study—e.g. estimating the increase in resolution delay by treatment environment—session-averages are a less satisfying unit of analysis. Appropriately treating each session as a matched pair of observations leaves only 4 unique observations per environment; too few to provide useful power in detecting treatment effects under conventional matched-pairs permutation tests (cf. Miller, 1997).[101] One way to proceed is to ignore matchings, instead hoping that positive within-pair correlation and the general conservatism of permutation tests in small samples will give validity to independent-sample tests of location. Such results are consolidated in Table 14, but should be interpreted with a grain of salt because validity is not guaranteed.

A better approach is to estimate average treatment effects using appropriate dispute-level analysis: i.e. treating the outcome of each dispute as a separate observation. Observations on the outcomes of individual disputes may be dependent within repetitions of a particular matching, but are independent and plausibly identically distributed after accounting for potential sources of dependence. Table 15 contains parameter estimates and associated inferences for several regressions of dispute-level resolution delay on experimental and observational controls. Columns 1 and 3 regress delay-to resolution and delay-to-settlement, respectively, on an interacted set of indicators for the treatment environment (one of reverse costs, low costs, low asymmetry,

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[100] The paired-sample test is just an application of the one-sample Wilcoxon Signed-Rank test to the vector-difference of matched pairs (see, e.g., Miller, 1997).

[101] For example, Wilcoxon's Signed-Rank test with 4 observations has a minimum p-value of $^2/_{4^2} = 0.125$. There exists no sample for which the test would reject the null at even the 0.1 level.

Table 14: Asymmetric Information Treatment Effect in SE2, Session Level[a]

Treatment Environment	ΔD_R	ΔD_S
Control	27.525	30.095
	0.0286*	0.0286*
Reverse Costs	22.008	18.026
	0.0286*	0.0286*
Low Costs	27.992	23.521
	0.0286*	0.0286*
Low Asymmetry	20.592	19.171
	0.0286*	0.0286*
Law School	27.783	28.095
	0.0286*	0.0286*

[a] On top are average treatment effects (in seconds). On bottom are exact p-values corresponding to application of (independent sample) Wilcoxon-Mann-Whitney permutation tests. The qualifier ∗ denotes significance from zero at the nominal 0.05 level.

or law school with the control environment as reference) and the presence of asymmetric information. Columns 2 and 4 provide the same, but with two lags of delay for the plaintiff, $D(p)$, and defendant, $D(d)$, as additional controls.

Randomized matchings produce an unbalanced panel with $n = 620$ pairs, and $M \in \{1, \ldots, 4\}$ repeat observations per-pair for an effective sample of $N = 1200$ observations in delay-to-resolution regressions. Conditioned on settlement of a dispute, delay-to-settlement regressions involve $n = 532$ pairs with $M = \{1, \ldots, 4\}$ repetitions for an effective sample of $N = 842$ observations. Random pair-effects account for potential correlation within unique pairs of subjects (using the Swamy and Arora (1972) transformation); fixed round-effects are included in the regressions, but omitted in presentation.

Table 15: Regression of Delay on Asymmetric Information in SE2, Dispute Level[a]

	D_R		D_S	
Parameter	(1)	(2)	(3)	(4)
Constant	46.876***	10.586†	35.484***	12.164*
	(5.7243)	(6.1978)	(5.1419)	(5.7191)
Asymmetric Information	27.728***	15.467**	31.836***	23.358***
	(5.6439)	(5.2354)	(4.9396)	(4.8077)
Reverse Costs	2.079	2.060	9.930†	9.067†
	(6.5875)	(5.7408)	(5.3102)	(4.8697)
Low Costs	10.854	6.208	18.345**	15.865**
	(6.6874)	(5.8895)	(5.7531)	(5.3255)
Low Asymmetry	6.479	4.610	6.020	5.891
	(6.7911)	(5.9048)	(5.4769)	(5.1481)
Law School	7.197	4.085	9.847†	6.719
	(6.4392)	(5.9475)	(5.3682)	(5.1446)
Reverse Costs × Asymmetric	−6.546	−4.688	−15.397*	−13.062*
	(7.9447)	(7.1764)	(6.6202)	(6.3036)
Low Costs × Asymmetric	−1.176	−0.552	−6.974	−8.001
	(7.7787)	(7.0566)	(7.6844)	(7.1361)
Low Asymmetry × Asymmetric	−9.160	−6.623	−12.435	−11.581
	(8.0635)	(7.4130)	(7.5727)	(7.3887)
Law School × Asymmetric	0.033	−1.906	−1.435	−2.073
	(7.5962)	(7.0301)	(7.3410)	(7.0941)
Lag(1) D(p)		0.043		0.073**
		(0.0285)		(0.0270)
Lag(2) D(p)		0.139***		0.103***
		(0.0315)		(0.0304)
Lag(1) D(d)		0.159***		0.082**
		(0.0301)		(0.0297)
Lag(2) D(d)		0.199***		0.087**
		(0.0297)		(0.0282)
σ_ϵ^2	1269.39	1255.71	698.74	701.62
σ_η^2	479.44	152.01	531.5	381.84

[a] Parameter estimates from random pair-effects regression of delay-to-resolution and delay-to-settlement on treatment indicators and lagged dependent variables (Swamy and Arora, 1972). Values in parentheses are heteroskedasticity and cluster-robust standard errors (Arellano, 1987). Parameter estimates for fixed round-effects are omitted. Variances σ_η^2 and σ_ϵ^2 correspond to pair and idiosyncratic error terms, respectively. Qualifiers $***$, $**$, $*$, and † denote significance from zero at levels < 0.001, 0.01, 0.05, and 0.1, respectively.

Result 9. Asymmetric information over the potential trial verdict increases settlement delay in every SE2 treatment environment.

For the control environment, the increase in delay due to asymmetric information is easiest to see as the parameter on the asymmetric information indicator in columns 1 and 3 of Table 15. The estimated increase of 27.7 seconds in delay-to-resolution is about a 50% increase over delay with symmetric information; the 31.8 second increase in delay-to-settlement is about a 95% increase.[102] A 30 second delay constitutes 1/4 of the maximum duration of settlement bargaining.

Confidence that asymmetric information causes resolution delay is bolstered by similar findings in other environments. For columns 1 and 3 of Table 15, Figure 26 illustrates the average treatment effect of asymmetric information in all SE2 bargaining environments. Solid center dots illustrate average treatment effects, with vertical lines and hollow black dots representing asymptotic 95% confidence intervals. Gray lines and hollow dots illustrate simultaneous 95% confidence intervals constructed by Bonferroni correction (see, e.g., Miller, 1997, pp. 74–75).[103] The increase in delay is statistically distinguishable from zero in every case.

Alternative estimates of the asymmetric information treatment effect are provided in columns 2 and 4 of Table 15. Parameter estimates in these columns benefit from the inclusion of lagged dependent variables—which capture potential sources of serial dependence—but are more difficult to interpret than their column 1 and 3 analogues. Lag terms indicate significant positive partial-correlation between past and present delay, probably by acting as a proxy for litigant-specific fixed effects.[104]

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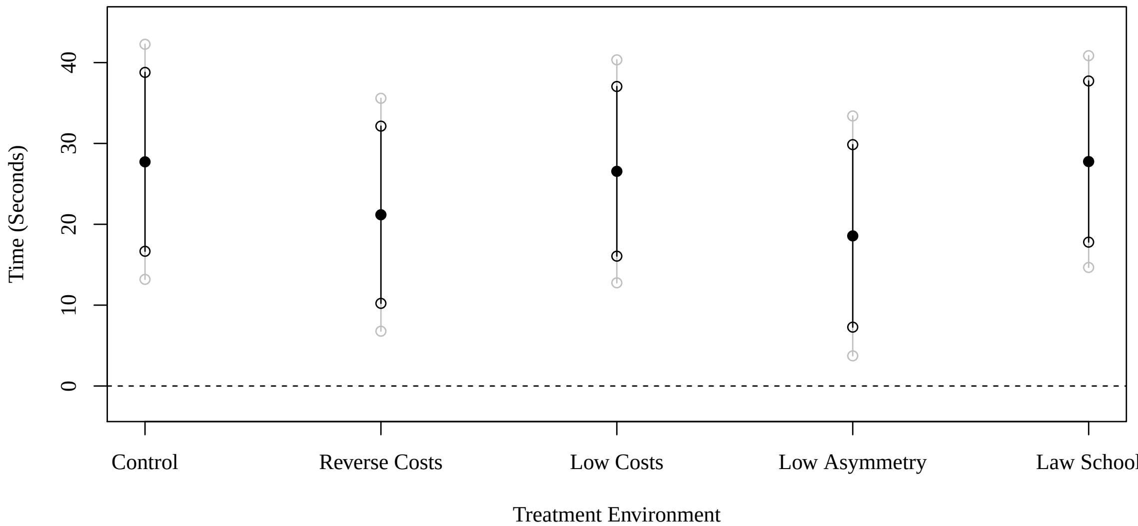
[102] Account for fixed round-effects, average delay-to-resolution and delay-to-settlement are about 55.3 and 33.3 seconds, respectively, with potential verdict information symmetrically dispersed.

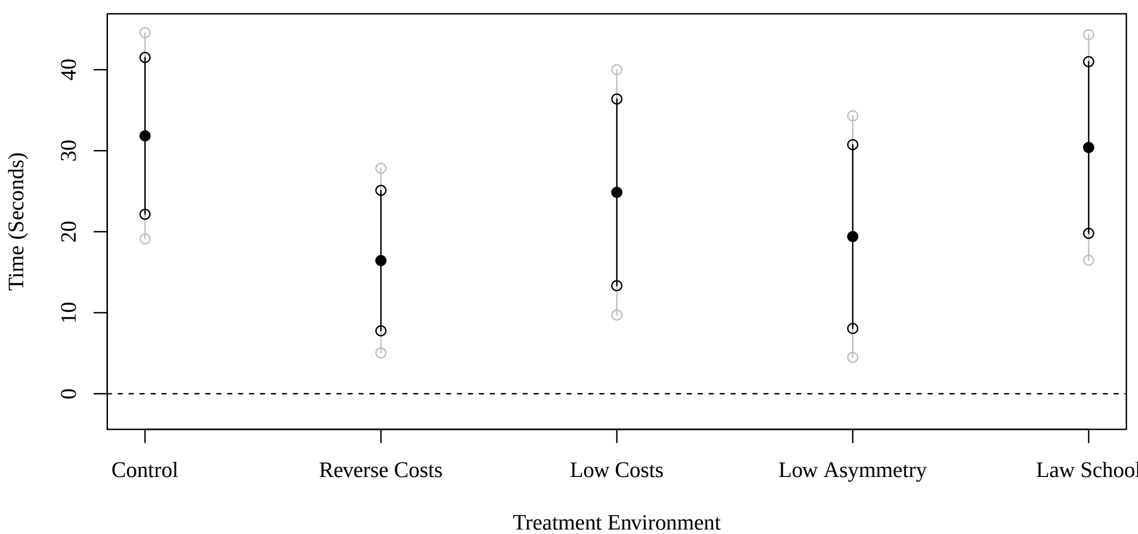
[103] Simultaneous 95% confidence intervals have the interpretation of being generated by a process that bounds all five expected values at least 95% of the time. Greater efficiency, but also greater interpretive complexity, is provided by a confidence *region* (see, e.g., Draper and Guttman, 1995).

[104] This interpretation is suggested by the reduced effect of lagged terms in alternative regression

Figure 26: Asymmetric Information Treatment Effect in SE2, Dispute Level[a]



(a) Average Increase in Delay-to-Resolution under Asymmetric Information



(b) Average Increase in Delay-to-Settlement under Asymmetric Information

[a]Solid dots illustrate observed average treatment effects (in seconds). Hollow black dots with a vertical connecting line represent 95% confidence intervals. Hollow gray dots with a vertical connecting line represent simultaneous 95% confidence intervals. The dashed line illustrates the no-effect hypothesis where exposure to asymmetric information causes no increase in resolution delay.

Differences in parameter estimates between columns 1 and 3 and columns 2 and 4 are attributable to a difference in what is being estimated in each regression model. For example, in columns 1 and 3, the parameter for "Asymmetric Information" represents the average treatment effect of asymmetric information in the control environment. In columns 2 and 4, the same parameter represents the *contemporaneous effect* of introducing asymmetric information in the control environment, holding prior experience constant. The subtle complication is that prior experience is itself a function of the information environment, so the contemporaneous effect of information asymmetry is not generally the same as its average effect over time. Appropriate functions of column 2 and 4 parameter estimates peg average treatment effects close to the column 1 and 3 estimates: 33.6 seconds for delay-to-resolution and 35.6 seconds for delay-to-settlement. Details on the estimation of average treatment effects with lagged dependent variables are provided in Appendix F.2.

11.2 Comparative Delay

Having addressed the Remark 6 prediction that asymmetric information increases resolution delay, a subsequent inquiry is how delay differs between environments. This section focuses on the comparative static predictions in Remarks 7 through 10 dealing with changes in expected delay when the control treatment is perturbed along various dimensions. Reverse costs and law school treatments provide robustness checks for data collected under control parameter values and with undergraduate-student subjects. Low costs and low asymmetry treatments provide additional perspective on how and why asymmetric information increases settlement delay.

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models with fixed pair-effects: see Appendix F.3. See Wooldridge (2006b, pp. 315–317) for an accessible discussion of lagged dependent variables as a proxy for unobserved heterogeneity.

The identification strategy in this section is comparison of delay between asymmetric information treatments in the control and 4 non-control SE2 environments. Holding the information environment factor fixed at the asymmetric information level, between-environment differences in resolution delay are attributable to the causal effect of changes in the settlement bargaining environment.

As in the previous section, one possible approach is analysis of session-average data. When focusing solely on the asymmetric information treatment of each bargaining environment, session-average observations are independent and plausibly identically distributed (at least within a treatment). With only four observations per treatment, independent-sample permutation tests are far from powerful, but are capable of detecting strong differences in average delay where they exist.[105] Average treatment effects from exposure to a non-control bargaining environment and associated tests of locational equality are consolidated in Table 16.

Also as in the previous section, a better approach is probably to rely on appropriate dispute-level analysis. At the cost of imposing additional structure on the data, dispute-level regression analysis provides stronger controls and greater inferential power than comparable session-average tests. Average treatment effects can be constructed from parameter estimates in Table 15: e.g. the treatment effect of changing to the reverse costs environment is equal to the sum of the parameter on "Reverse Costs" and the parameter on the interaction term "Reverse Costs × Asymmetric." Average treatment effects and associated Wald tests of the no-effect hypothesis are provided in Table 17.[106]

[105] In contrast to the matched-pairs tests contemplated in Section 11.1 (see n. 101), independent-sample permutation tests are capable of rejecting the null hypothesis at interesting levels of significance. For example, the Wilcoxon-Mann-Whitney test applied to a balanced pair of 4 observation samples has a minimum p-value of $2/\binom{8}{4} = 0.0286$.

[106] Since each treatment effect is the sum of two parameters, e.g. $\theta_0 + \theta_1$, the null hypothesis of no-effect corresponds to the zero sum hypothesis: $\theta_0 + \theta_1 = 0$.

Table 16: Change in Environment Treatment Effect in SE2, Session-Level[a]

Treatment Comparison	ΔD_R	ΔD_S
Control → Reverse Costs	-4.708	-4.592
	0.4857	0.3429
Control → Low Costs	10.233	10.946
	0.0286*	0.1143
Control → Low Asymmetry	-1.100	-5.358
	0.6857	0.3429
Control → Law School	7.425	8.109
	0.3429	0.3429

[a] On top are average treatment effects in seconds. On bottom are exact p-values corresponding to application of Wilcoxon-Mann-Whitney permutation tests. The qualifier ∗ denotes significance from zero at the nominal 0.05 level.

Table 17: Change in Environment Treatment Effect in SE2, Dispute-Level[a]

Treatment Comparison	ΔD_R	ΔD_S
Control → Reverse Costs	-4.468	-5.467
	0.3822	0.3168
Control → Low Costs	9.679	11.372
	0.0481*	0.0549†
Control → Low Asymmetry	-2.681	-6.415
	0.6048	0.2640
Control → Law School	7.229	8.412
	0.1475	0.1547

[a] On top are average treatment effects in seconds. On bottom are p-values corresponding to Wald tests of the no-effect null hypothesis (see n. 106). The qualifiers ∗ and † denote significance from zero at the nominal 0.05 and 0.1 levels, respectively.

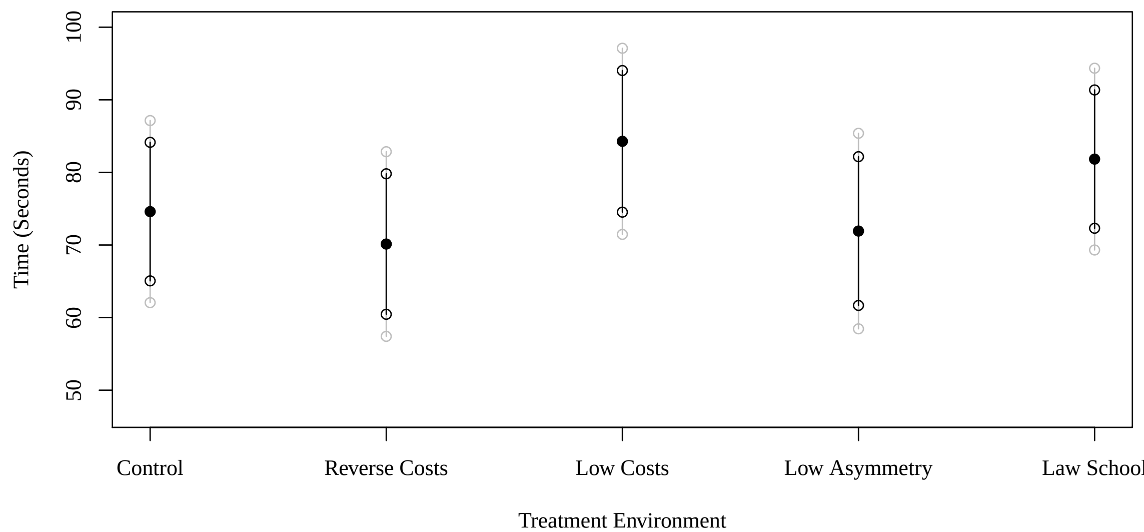
Result 10. Robustness checks involving cost reversal and the use of an alternative subject pool reveal no obvious evidence of bias in the experimental design.

Provided the experimental design adequately represents the theoretic model of settlement bargaining, exposure to the reverse costs and law school treatment environments should imply no change in average resolution delay relative to the control treatment environment (Remarks 7 and 10). Starting with the reverse costs environment, the estimated average effect of exposure to reversed costs suggests a modest decrease in average delay, but the magnitude of the decrease is not statistically distinguishable from zero at any interesting level of significance. As illustrated in Figure 27—which plots confidence intervals for average delay under the asymmetric information treatment of each SE2 environment—there is broad overlap between confidence intervals for the control and reverse costs environments.

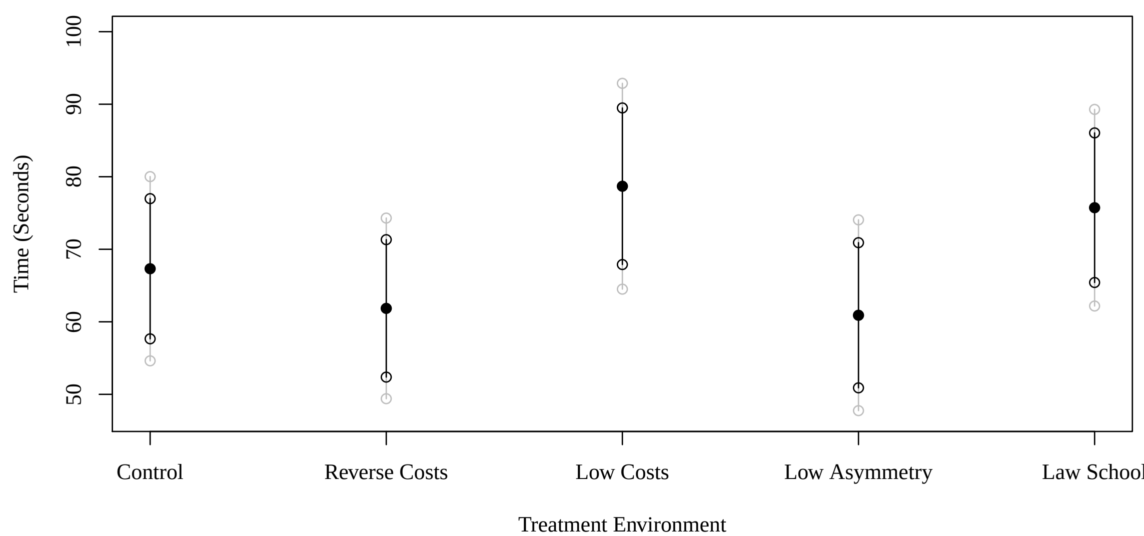
Turning to the law school treatment environment, the average effect of changing to the law student subject pool is positive at about 7 and 8 seconds for delay-to-resolution and delay-to-settlement respectively; again, the difference is not significantly distinguishable from zero. Even assuming that the change to a law school subject pool increases average delay under asymmetric information, confidence intervals in Figure 26 suggest that the increase in delay is across-the-board for symmetric information as well (since the treatment effect of exposure to asymmetric information is nearly identical to that of the control environment).

Trepidation about the force of these results grows from the classic statistical dilemma of predicting the null hypothesis. It must be remembered that failing to find strong evidence against the null hypothesis is not the same as finding strong evidence in favor of it. The wide sampling distributions associated with all treatment effect estimates preclude ruling non-trivial effects as entirely out-of-hand. The most

Figure 27: Average Delay under Asymmetric Information in SE2[a]



(a) Average Delay-to-Resolution under Asymmetric Information



(b) Average Delay-to-Settlement under Asymmetric Information

[a]Solid dots illustrate observed average treatment effects (in seconds). Hollow black dots with a vertical connecting line represent 95% confidence intervals. Hollow gray dots with a vertical connecting line represent simultaneous 95% confidence intervals. The dashed line illustrates the no-effect hypothesis where exposure to asymmetric information causes no increase in resolution delay.

honest interpretation of these estimates is simply that they fail to provide obvious evidence of any serious design bias owing to the asymmetric structure of costs or to reliance on an undergraduate-student subject pool.

Result 11. The estimated treatment effect of exposure to the low costs treatment environment is not obviously different from prediction.

Under the prediction of Remark 8, the $\mathbf{T}_4$ reduction in negotiation costs increases average delay-to-resolution and delay-to-settlement by 16.07 and 8.51 seconds, respectively. As the results in Table 17 indicate, the average treatment effect of exposure to the low costs treatment in SE2 is indeed positive and significantly different from zero at the 0.1 level for both measures of delay.[107] Individual 95% confidence intervals on the average increase in delay are $[0.08, 19.28]$ and $[-0.24, 22.98]$ for delay-to-resolution and delay-to-settlement, respectively. Each confidence interval includes the predicted increase in delay, but also includes a treatment effect of practically (or exactly) zero change in delay between environments.

Despite considerable noise, estimated treatment effects are roughly consistent with theoretic prediction. As predicted, decreases in negotiation costs are clearly found to increase average delay. This observation provides greater confidence in experimental validity by reproducing an important feature of the empirical model in the data.

Result 12. The estimated treatment effect of exposure to the low asymmetry treatment environment appears consistent with theory for delay-to-settlement, but inconsistent with theory for delay-to-resolution.

Under the prediction of Remark 9, the $\mathbf{T}_6$ reduction in the degree of information asymmetry should decrease average delay-to-resolution and delay-to-settlement by

[107] Reported results are limited to Table 17, which was selected *ex ante* as the more powerful set of tests. Note that results differ when inference regarding the change in delay-to-settlement is conducted at the session-average level (Table 16). Statistical difference from zero is maintained at the familywise 0.1 level under the Hochberg (1988) correction for simultaneous inference.

-16.14 and 0 seconds, respectively. Table 17 shows that Wald tests based on columns 1 and 3 of Table 15 fail to reject the no-effect null hypothesis at every interesting level of significance. Individual 95% confidence intervals on the change in average delay under the low asymmetry environment are $[-12.84, 7.47]$ and $[-17.67, 4.84]$ for delay-to-resolution and delay-to-settlement, respectively. Discrepancy is observed between the empirical and predicted treatment effect on delay-to-resolution: the delay-to-resolution interval fails to contain the predicted change in delay, with an associated Wald test p-value of 0.0094.

Inconsistency of observed and predicted delay-to-resolution in the low asymmetry environment is interesting. While it is tempting to excuse this finding as a design-consequence of choosing a treatment effect too modest to be detected empirically, two observations suggest an alternative interpretation. First, note that a predicted delay of about 16 seconds is actually fairly large (e.g. compared with an average delay-to-resolution of 55.3 seconds in the control environment). Second, the $\mathbf{T}_6$ perturbation setting $\overline{x} = 150$ is about as extreme a change as parameters will allow: for even $\overline{x} = 140$, the predicted equilibrium switches from an interior solution to a boundary solution. Given these observations, the most honest interpretation of the data is probably that while subjects in SE2 appear very responsive to the *existence* of asymmetric information, they do not appear particularly responsive to marginal changes in the *extent* of the information asymmetry.

11.3 Distribution of Delay

In contrast to Section 11.1, which found a large treatment effect of asymmetric information on average delay, Section 11.2 noted only muted differences in average delay between treatment environments. A reasonable concern is that the exclusive focus of Section 11.2 on average delay may obscure more nuanced differences in delay distributions between treatment environments.

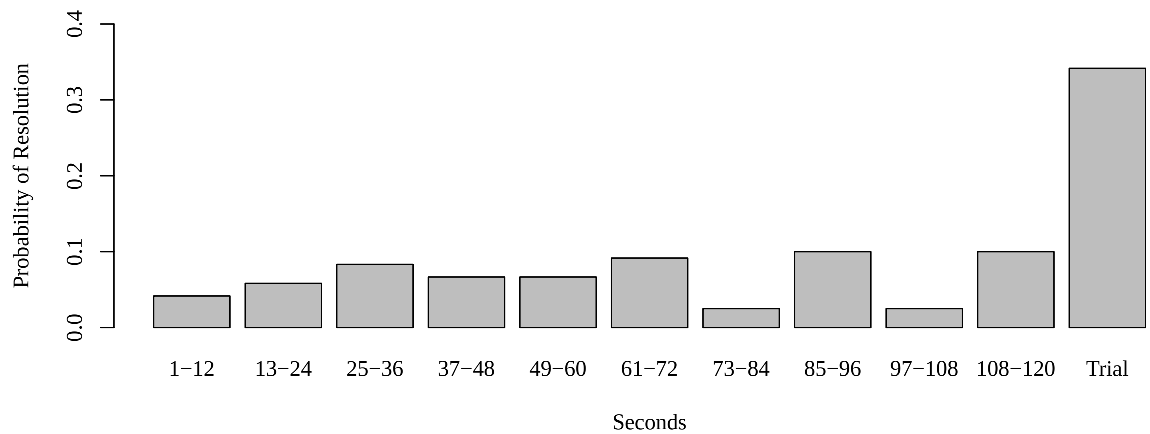
To address this concern, Figure 28 illustrates the observed distribution of resolution delay under the asymmetric information treatment for each SE2 treatment environment. Figure 28 is the empirical analogue of the theoretic distribution of delay illustrated in Figure 24 in Section 11.1. Figure 28(a) shows resolution delay under the control treatment. Figures 28(b) through 28(e) show the same under the reverse costs, low costs, low asymmetry, and law school treatment environments, respectively.[108]

Subject to a fair amount of noise, delay-distributions appear basically comparable across treatment environments. Control and reverse costs delay distributions have basically identical shape, and correspond closely to the predicted distribution of delay for these environments (see Figure 24(a)). Delay in the low asymmetry environment is about the same as that in the control environment, with a much greater probability of trial verdict than predicted by theory (see Figure 24(c)). The difference in rate of trial verdicts between observed and predicted outcomes in the low asymmetry environment is probably the major reason for inconsistency in observed and predicted average delay-to-resolution noted in Result 12.

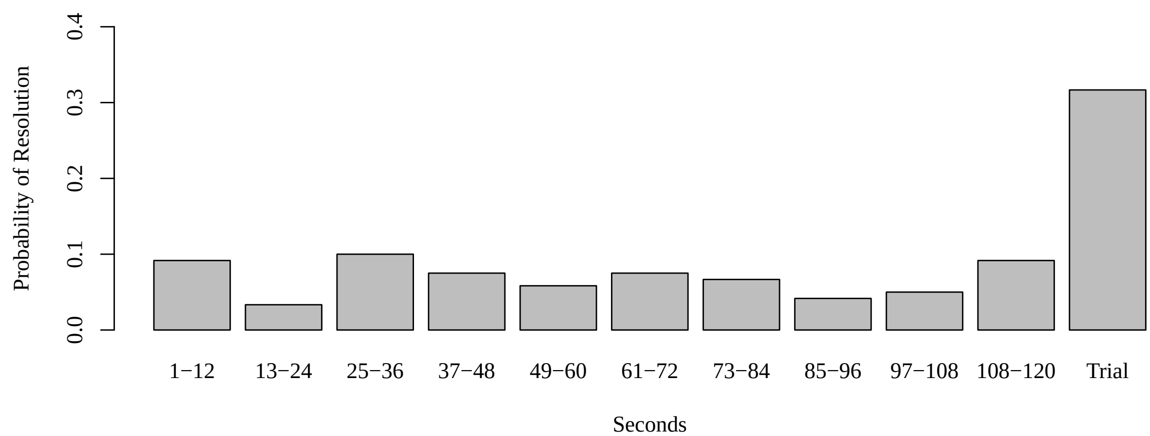
Delay distributions for the low costs and law school treatment environments are similar and distinctive against other delay distributions. The rate of trial verdicts in the low costs environment is lower than predicted (see Figure 24(b)), and the distribution of delay in the law school environment appears qualitatively different from the control distribution. Both low costs and law school delay distributions exhibit an interesting non-monotonicity in the rate of settlement at 61–72 seconds. It is presently unclear whether the jump in settlement at this point is a framing-

[108]Note that the data used to construct Figure 28 include repeat observations for some randomly matched pairs of litigants, and thus may involve within-sample dependencies (see n. 88). For purposes of drawing statistical inferences about delay distributions, Figure 29 is a more satisfying approach.

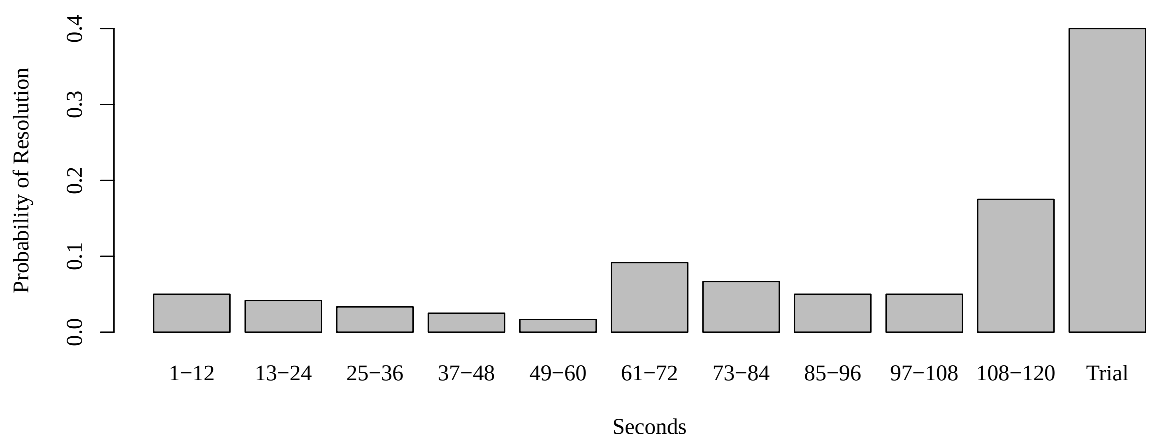
Figure 28: Observed Delay-to-Resolution Distributions in SE2



(a) Distribution of Resolution Timing with Control Parameters

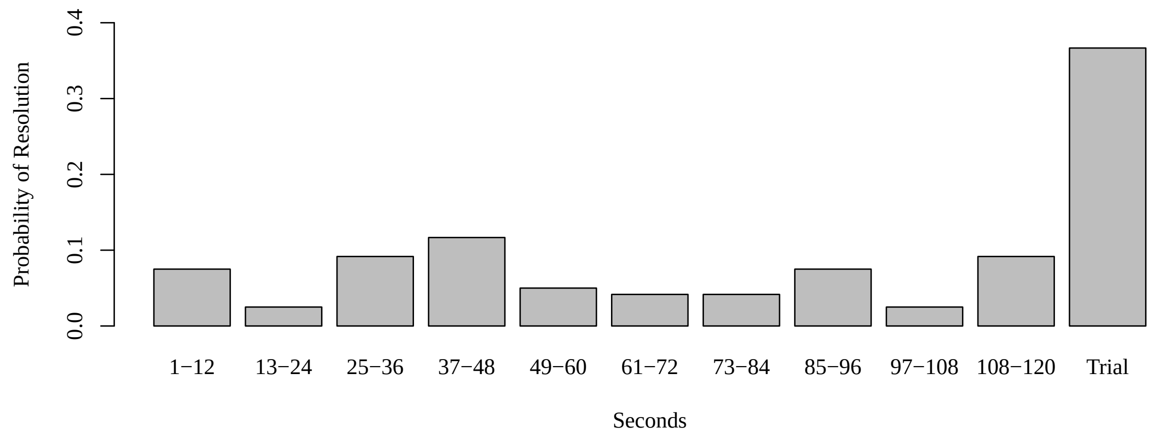


(b) Distribution of Resolution Timing with Reverse Costs

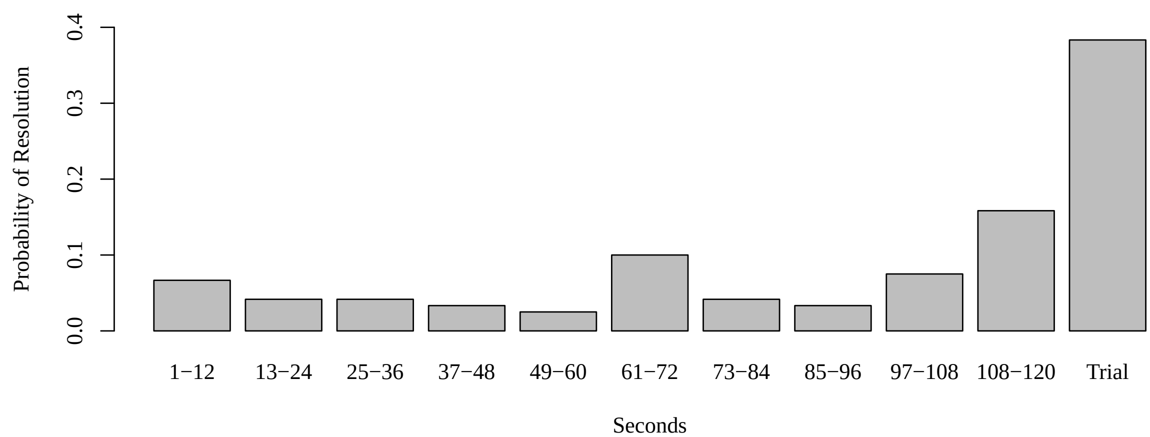


(c) Distribution of Resolution Timing with Low Costs

Figure 28: Observed Delay-to-Resolution Distributions in SE2 (Cont...)



(d) Distribution of Resolution Timing with Low Asymmetry



(e) Distribution of Resolution Timing with Law School Subjects consequence of the one-minute mark in bargaining, or is a spurious characteristic of the sampling distribution.

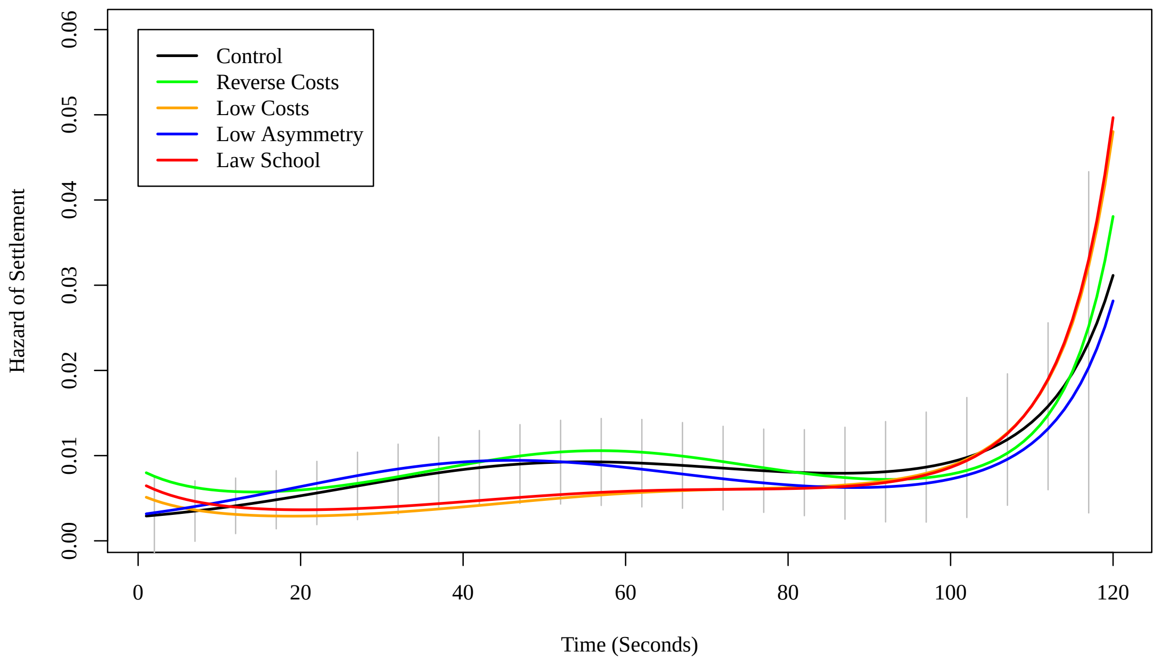
As an alternative way to visualize distributional differences, Figure 29 illustrates estimated empirical hazard functions for all asymmetric information treatments in SE2.[109] The black curve illustrates the empirical hazard of settlement in the asymmetric information treatment of the control environment, with colored curves showing empirical hazards in the non-control SE2 environments. Gray vertical lines are simultaneous 95% confidence intervals on the control hazard. Details on the hazard function estimator and associated inference are provided in Appendix E.3

Estimated hazard functions reveal many of the same relationships noted previously. The hazards of settlement in the control, reverse costs, and low asymmetry environments appear nearly identical. The hazards of settlement in the low costs and law school environments appear nearly identical to each other, and different from the other SE2 hazard functions. For about the first 80 seconds of bargaining, low costs and law school hazards fall clearly below the control hazard.

In addition to estimating observed hazard rates, Figure 29 includes inferential material to suggest the precision of hazard function estimators. Two comments are noteworthy. First, the precision of all empirical hazard estimators falls precipitously after about the first 100 seconds of bargaining. Second, even the low costs and law school empirical hazard functions fall (barely) within the set of simultaneous intervals on the control hazard function. This suggests that for the amount of noise observed in SE2 settlement bargaining, additional data may be required to speak with much confidence about specific features of the delay distributions in different treatment environments.

[109]See Section 8.4 for the definition of a hazard function.

Figure 29: Estimated Hazard of Settlement by Environment in SE2[a]



[a]Hazard rates for dispute resolution are illustrated up-to-but-excluding the final second of bargaining and the trial verdict phase: i.e. the illustration covers seconds 1 through 119 of a dispute. Truncating the illustration at 119 seconds improves legibility, as the sharp spike in the hazard function in the final second of bargaining swamps all other variability.

12 Discussion

Confirmatory analysis of SE2 data addresses the role of asymmetric information in explaining persistent settlement delay in the resolution of tort disputes. Results are best summarized in two observations. Section 9.1 raises the first observation: the capacity of asymmetric information over a potential trial verdict to explain some, but not all, settlement delay. Section 9.2 addresses the second observation: robustness of average delay to substantial changes in the settlement bargaining environment.

12.1 Settlement Delay from Asymmetric Information

Data collected in SE2 strongly confirm that asymmetric information over a potential trial verdict can induce considerable delay in the resolution of settlement bargaining. For the control SE2 bargaining environment, exposure to asymmetric information causes average delay-to-settlement to almost double. Similar results obtain under various modifications to the bargaining environment, indicating an impressively robust result. The message of SE2 is clear: asymmetric information about a potential trial verdict *can* substantially increase average settlement delay.

This empirical demonstration has considerable theoretic importance. First, it contributes a solid affirmation to the thus far indeterminate experimental economics literature on the capacity of asymmetric information to explain delayed agreement (see Section 1.2). Second, SE2 speaks to a sizable theoretic literature specifically focused on asymmetric information as an answer the settlement delay puzzle. Results validate prior focus on asymmetric information, and recommend further study of this topic. Third, the results of SE1 and SE2 combine to provide a fairly comprehensive validation of the popular Spier (1989, 1992) model of settlement bargaining with asymmetric information.

On the practical question whether asymmetric information is an important contributor to settlement delay in the field, the results of SE2 cut both ways. While collected data confirm a causal link between exposure to asymmetric information and increased delay, it must be noted that nothing in the present study should or can be interpreted as direct evidence that asymmetric information explains settlement delay in the field. The appropriate interpretation of SE2 is as a proof of concept. The experiment confirms that asymmetric information *can* cause a substantial increase in settlement delay in the field. It does not establish that asymmetric information *does* cause settlement delay in the field, or even that the predicate assumption of asymmetrically informed litigants is satisfied in practice. These important empirical demonstrations are left to future research.

Even interpreted as a proof of concept, however, results in SE2 provide a powerful *negative* answer to the hypothesis that asymmetric information is solely responsible for the pervasive settlement delay observed in the field. SE2 involves a highly abstract, highly controlled, and highly sterilized bargaining environment for which only asymmetric information should be able to explain settlement delay. But as results show, substantial delay is nevertheless measured when litigants are symmetrically informed about the value of the potential trial verdict. Even under ideal conditions, asymmetric information about a potential verdict can apparently only explain *part* of observed settlement delay. Results thus recommend further study of alternative answers to the settlement delay puzzle as well.

Given the complexity of settlement bargaining in the field, many different factors probably combine to explain the the existence and extent of systematic settlement delay. Based on my own conversations with subjects, practicing attorneys, and legal scholars, I suspect that apt areas for future study are (i) agency problems between lawyer and client, and (ii) cognitive biases associated with regret avoidance.

12.2 Sensitivity of Delay to Bargaining Environment

In contrast to the substantial change in average settlement delay when SE2 subjects are exposed to asymmetric information, fairly large perturbations of the control asymmetric information settlement bargaining environment elicit surprisingly modest changes in average delay. The implications of this observation are mixed, depending on both the prediction for delay under the change in environment and the motivation for including each non-control environment in SE2.

For robustness check modifications of the bargaining environment (the reverse costs and law school treatment environments), average delay is theoretically identical before and after the change in settlement bargaining environment. The approximate similarity of observed delay under these environments thus lends support to experimental validity. This and the remarkable consistency of asymmetric information treatment effects across SE2 environments provides confidence in the causal relationship described in the previous section.

The only treatment environment in SE2 to show statistically significant sensitivity to a modification of the bargaining environment, the low costs treatment environment evinces an increase in average delay when negotiation costs are reduced from their control levels. This result conforms to theoretic prediction, but is of questionable practical importance. One interesting policy implication is that reductions in average delay may be possible under reform policies which tend to increase average negotiation costs. Provided that the increase in costs involves a welfare-neutral transfer (e.g. additional fees paid to the government), the efficiency of tort dispute resolution would be correspondingly increased under such a reform.

Relative imprecision of all treatment effect estimators aside, the only troubling observation in comparisons between SE2 treatment environments is the apparent insensitivity of delay-to-resolution to changes in the degree of asymmetric information. The theoretic prediction for the low asymmetry treatment environment involves no change in delay-to-settlement, but a substantial reduction in delay-to-resolution. In defiance of this prediction, neither observed treatment effect is statistically distinguishable from zero for the low asymmetry environment, and the change in delay is if anything larger for delay-to-settlement than for delay-to-resolution.

As noted in Section 11.2, the change in information asymmetry between control and low asymmetry treatments is about as large as possible without inducing a simultaneous change in the class of equilibrium strategies (from an interior solution to a boundary solution). The most obvious interpretation of these results—that subjects in SE2 are strongly sensitive to the existence of asymmetric information, but not to the degree of asymmetry—is inauspicious for Sub-Experiment 3, which looks at the treatment effects of various tort reform policies. Most popular tort reforms attempt to influence litigants through manipulation of the distribution of potential damages. To the extent that subjects appear relatively insensitive to even the significant change in potential damages introduced by the low asymmetry treatment environment, it is difficult to imagine that reform policies affecting similar changes in potential damages will somehow experience greater efficacy in reducing average delay.

F Technical Appendix

F.1 Proof of Remark 9

Proof. Predictions in Remark 9 result from simple algebra; the following "proof" is provided for convenience only. Since $f_{D_R}(t) = p_t$ for all $t = 1, \ldots, T+1$, the comparative static for D_R is exactly that given for p_t in Corollary 6. Determining the marginal effect of $\overline{x}$ on the distribution of D_S is only slightly more complicated.

For example, the probability that D_S equals $t < T$ seconds is

$$f_{D_S}(t) = \frac{p_t}{1 - p_{T+1}}. \tag{47}$$

Substituting the definition of p_t from Corollary 2,

$$f_{D_S}(t) = \frac{\dfrac{\pi^{-1}\delta^{-T+t}(c_p + c_d)}{\overline{x} - \underline{x}}}{1 - \left(1 - \displaystyle\sum_{i=1}^{T-1} \frac{\pi^{-1}\delta^{-T+i}(c_p + c_d)}{\overline{x} - \underline{x}} - \frac{\pi^{-1}(k_p + k_d)}{\overline{x} - \underline{x}}\right)}. \tag{48}$$

Simplifying an cancelling terms,

$$f_{D_S}(t) = \frac{\delta^{-T+t}(c_p + c_d)}{\sum_{i=1}^{T-1} \delta^{-T+i}(c_p + c_d) + (k_p + k_d)}. \tag{49}$$

As $\overline{x}$ does not appear in this expression for $f_{D_S}(t)$, its marginal effect must be zero:

$$\frac{\partial}{\partial \overline{x}} f_{D_S}(t) = 0. \tag{50}$$

Similar logic applies in period T. □

F.2 Treatment Effect with Lagged Dependent Variable

For narrative convenience, focus on the control treatment environment and ignore fixed round-effects in regression models. Let D_0 and D_1 denote delay with asymmetric and symmetric information, respectively. For the regression model in columns 1 and 3 of Table 15, expected delay in the control environment is

$$\mathrm{E}[D_0] = \alpha + \theta \tag{51}$$

$$\mathrm{E}[D_1] = \alpha \tag{52}$$

for α a constant term and θ the coefficient on the asymmetric information indicator. The average treatment effect of exposure to asymmetric information is identically the value of the asymmetric information parameter:

$$\begin{aligned} ATE &= \mathrm{E}[D_0] - \mathrm{E}[D_1] \\ &= \theta. \end{aligned} \tag{53}$$

For the regression model in columns 2 and 4 of Table 15, expected delay is a more complicated function of model parameters:

$$\mathrm{E}[D_0] = \alpha + \theta + \phi_1 \text{Ł} D(p) + \phi_2 \text{Ł}^2 D(p) + \phi_3 \text{Ł} D(d) + \phi_4 \text{Ł}^2 D(d) \tag{54}$$

$$\mathrm{E}[D_1] = \alpha + \phi_1 \text{Ł} D(p) + \phi_2 \text{Ł}^2 D(p) + \phi_3 \text{Ł} D(d) + \phi_4 \text{Ł}^2 D(d) \tag{55}$$

where Ł is the lag operator and $D(p)$ and $D(d)$ denote delay terms (i.e. the dependent variable) for the plaintiff and defendant respectively. In the collected data, present and lagged delay terms all involve a common information environment:

$$\mathrm{E}\left[D_0\right] = \alpha + \theta + \phi_1 \mathrm{L} D_0(p) + \phi_2 \mathrm{L}^2 D_0(p) + \phi_3 \mathrm{L} D_0(d) + \phi_4 \mathrm{L}^2 D_0(d) \tag{56}$$

$$\mathrm{E}\left[D_1\right] = \alpha + \phi_1 \mathrm{L} D_1(p) + \phi_2 \mathrm{L}^2 D_1(p) + \phi_3 \mathrm{L} D_1(d) + \phi_4 \mathrm{L}^2 D_1(d). \tag{57}$$

Assuming the model is wide-sense stationary (e.g. Hamilton, 1994, "covariance stationary"), expected delay is the same over time. Defining $\mathrm{E}\left[D_0\right] = \mu_0$ and $\mathrm{E}\left[D_1\right] = \mu_1$, wide-sense stationarity means that expected delay can be written as

$$\mu_0 = \alpha + \theta + \phi_1 \mu_0 + \phi_2 \mu_0 + \phi_3 \mu_0 + \phi_4 \mu_0 \tag{58}$$

$$\mu_1 = \alpha + \phi_1 \mu_1 + \phi_2 \mu_1 + \phi_3 \mu_1 + \phi_4 \mu_1. \tag{59}$$

Solving for μ_0 and μ_1 gives

$$\mathrm{E}\left[D_0\right] = \frac{\alpha}{1 - \phi_1 - \phi_2 - \phi_3 - \phi_4} + \frac{\theta}{1 - \phi_1 - \phi_2 - \phi_3 - \phi_4} \tag{60}$$

$$\mathrm{E}\left[D_1\right] = \frac{\alpha}{1 - \phi_1 - \phi_2 - \phi_3 - \phi_4}. \tag{61}$$

The average treatment effect of exposure to asymmetric information is thus

$$\begin{aligned} ATE &= \mathrm{E}\left[D_0\right] - \mathrm{E}\left[D_1\right] \\ &= \frac{\theta}{1 - \phi_1 - \phi_2 - \phi_3 - \phi_4}. \end{aligned} \tag{62}$$

Average treatment effects for these regression models are thus given by equation (62) above, and other treatment effects can be computed analogously. Re-introducing fixed round-effect does not materially change the analysis; like the constant term, α, round-effect terms cancel during differencing.

F.3 Alternative Regression Models

As a robustness check on the parameter estimates in Table 15, Table 18 reports parameter estimates and associated inference when the regression models in columns 2 and 4 of Table 15 are conducted with fixed pair-effects (instead of random effects). Data on all pairs without repeat matchings are necessarily omitted.

Table 18: Alternative Regression of Delay on Asymmetric Information, Dispute Level[a]

Parameter	D_R	D_S
Asymmetric Information	26.986*** (7.3772)	37.054*** (6.6230)
Reverse Costs × Asymmetric	-6.579 (9.7389)	-22.748** (8.0331)
Low Costs × Asymmetric	-1.625 (9.9164)	-4.159 (10.8945)
Low Asymmetry × Asymmetric	-14.194 (9.9158)	-17.957† (10.3557)
Law School × Asymmetric	2.086 (9.3865)	-1.385 (9.5416)
Lag(1) D(p)	-0.055 (0.0412)	-0.007 (0.0358)
Lag(2) D(p)	-0.079 (0.0439)	-0.067 (0.0454)
Lag(1) D(d)	0.022 (0.0418)	0.026 (0.0420)
Lag(2) D(d)	0.082 (0.0410)	0.002 (0.0396)

[a] Parameter estimates from fixed pair-effects regression of delay-to-resolution and delay-to-settlement on treatment indicators and lagged dependent variables. Values in parentheses are heteroskedasticity and cluster-robust standard errors (Arellano, 1987). Parameter estimates for fixed round-effects are omitted. Qualifiers $***$, $**$, and † denote significance from zero at levels < 0.001, 0.01, and 0.1, respectively.

Chapter VI

Sub-Experiment 3: Reducing Settlement Delay

Sub-Experiment 3 (SE3) explores measurements collected from sequences $\mathbf{S}_{11}, \ldots, \mathbf{S}_{18}$, isolating the effects of various "tort reform" policies. The basic objectives are both confirmatory and exploratory. An initial confirmatory question is whether any reform policy tends to reduce settlement delay. Additional questions concern the wealth-distributive consequences of each reform policy.

Section 13 defines the four non-control treatments explored in SE3. Each corresponds to the implementation of a different type of "tort reform" policy: a damages limit, a damages cap, a prejudgment interest rule, and Early Offers rules. Theoretic predictions are provided for each environment, with emphasis on the reduction in resolution delay predicted under each reform policy.

Section 14 discusses the results of SE3. Treatment effects are not estimated with great precision, and imposing the studied reform policies does not obviously achieve predicted reductions in average settlement delay. The distribution of delay is subjectively similar across reform policies, though minor variations are apparent. Several reform policies induce large changes in relative earnings between litigants.

Section 15 provides concluding discussion. Comments include (i) the proper interpretation of results in light of imprecise treatment effect estimates for average delay, and (ii) the importance of wealth-distributive consequences as practical context for the feasibility of implementing reform policies.

13 Treatments

Sub-Experiment 3 (SE3) concerns 4 different treatment *pairs*: sets of sequences such as $\{\mathbf{S}_{11}, \mathbf{S}_{12}\}$, $\{\mathbf{S}_{13}, \mathbf{S}_{14}\}$, etc. Each pair consists of two treatments, one being the control treatment, $\mathbf{T}_0$, and the other being one of $\mathbf{T}_{10}, \ldots, \mathbf{T}_{13}$. Treatments differ in only the reform environment factor, $\mathcal{R}$ (see Section 5.2). The order of treatment assignment is orthogonal within each pair of sequences. In one sequence the control treatment is assigned first and the reform treatment second; in the other, the reform environment treatment is assigned first and the control second.

Exhaustive description of the control treatment is provided in Chapter IV. With asymmetrically informed litigants, control parameter values, subjects drawn from the undergraduate subject pool, and no reform regime ($\mathcal{R}_0$), equilibrium in $\mathbf{T}_0$ involves the interior solution of Propositions 1 and 2. Settlement is persistently delayed, with predicted delay illustrated in Figure 8(c) of Chapter IV.

The remainder of this section describes the set of non-control SE3 treatments: models of various "tort reform" policies imposed on the control settlement bargaining environment.[110] Section 13.1 explains the damages limit treatment. Section 13.2 covers the damages cap treatment. Section 13.3 discusses the prejudgment interest treatment. Finally, Section 13.4 explains the Early Offers treatment. In describing non-control SE3 treatments, emphasis is on changes in resolution delay and differences in litigant earnings relative to the control treatment.

[110] Field examples of several such reform policies are provided by the American Tort Reform Association (2009). Literature from the American Tort Reform Association (ATRA) is repeatedly cited in this chapter as a convenient aggregation of various tort reform policies in the field. Citation to ATRA material relates to the presentation of objective facts, and is not meant as endorsement or adoption of any policy recommendations contained therein.

13.1 Damages Limit

Damages limit treatment sequences $\mathbf{S}_{11}$ and $\mathbf{S}_{12}$ consist of treatments $\mathbf{T}_0$ and $\mathbf{T}_{10}$ (see Tables 7 and 8 in Chapter III). The only difference between treatments is that $\mathbf{T}_{10}$ involves the $\mathcal{R}_1$ level of the reform environment factor, corresponding to the imposition of a limit on potential damages. The term *damages limit* is non-standard. It is used in the present study to highlight a structural distinction between those reform policies that truncate the distribution of potential damages (damages limits) and those that censor the potential damages distribution (damages caps).

As a non-standard policy categorization, practical examples of damages limits may help to solidify the concept. Note that a damages award is usually a convolution of various sub-categories of damages: e.g. a sum of compensatory and punitive damages, a sum of economic and non-economic damages, etc. Limits on damages remove or restrict access to one or more sub-categories of damages with the effect of moving probability density toward the lower tail of the potential damages distribution.

One practical example of a damages limit is a complete bar to legal relief under some sub-categories of damages. A policy might withdraw all access to punitive damages from an identifiable subset of disputes, or make the infliction of emotional distress a non-compensable harm. In both cases, the plaintiff is entirely foreclosed from a previously accessible source of redress.

Generalizing the complete bar to damages, a second example of a damages limit is a down-scaling of one or more categories of potential damages. In abstract terms, awarded damages could be defined as a fixed proportion, $0 < \theta < 1$, of all or part of the assessed injury. Though I am unaware of any *explicit* down-scaling policy in the field, reforms that impose heightened burdens on punitive damages awards may achieve a similar effect in practice (cf. ATRA, 2009).[111] Scaling-type damages limits reduce, but do not entirely foreclose, access to some part of a damages award.

For purposes of describing equilibrium strategies, a damages limit is modeled as a truncation of the pre-reform damages distribution. Let $\widetilde{x}_{\text{DL}}$ denote the damages limit: an upper bound on potential damages following imposition of the reform policy. If $f(x)$ is the density function of pre-reform damages, then the post-reform density is $f_{\text{DL}}(x) = f(x|x \leq \widetilde{x}_{\text{DL}})$. In the interesting case where $\widetilde{x}_{\text{DL}} < \overline{x}$, it follows that $x \sim f(x)$ stochastically dominates $x \sim f_{\text{DL}}(x)$.[112]

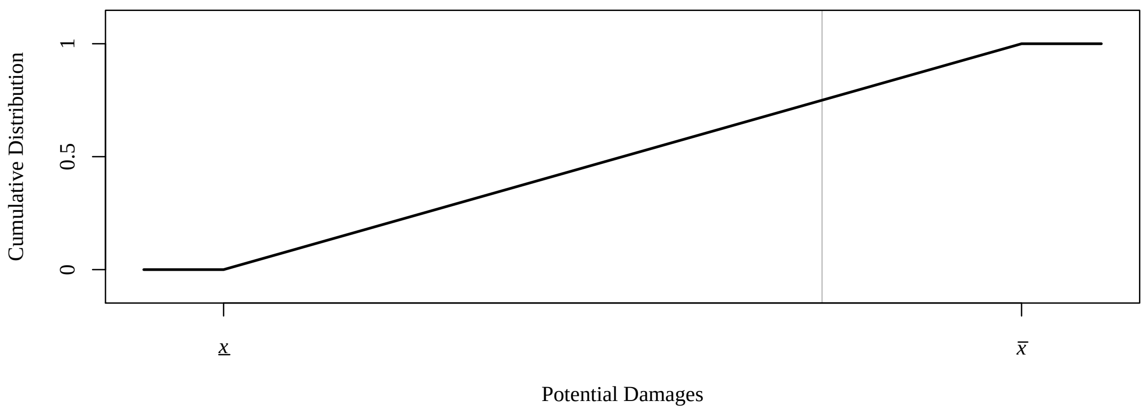
Figure 30 compares the distributional effects of a damages limit and damages cap. Figure 30(a) shows the cumulative distribution function (CDF) of potential damages without any reform policy. Figure 30(b) illustrates the CDF of damages following imposition of a damages limit at $\widetilde{x}_{\text{DL}}$. Figure 30(c) illustrates the same when damages are instead capped at $\widetilde{x}_{\text{DC}}$ (see Section 13.2). Unlike a damages cap—which results in an atomic mass-point at the cap value—a damages limit simply narrows the support of potential damages with the probability mass of the foreclosed range redistributed evenly over the remaining support of potential damages.

The damages limit reform policy imposed in treatment $\mathbf{T}_{10}$ is of the scaling type. Recall that total injuries in the experiment are presented as the sum of an economic injury and a pain-and-suffering injury: the economic injury is fixed at \$50 in every dispute and the pain and suffering injury is randomly distributed on support $[\$0, \$150]$. Under the $\mathcal{R}_1$ damages limit reform environment, subjects are informed that potential damages are equal to the full value of the economic injury plus 73.33% of the pain and suffering injury, leaving total damages distributed uniformly between

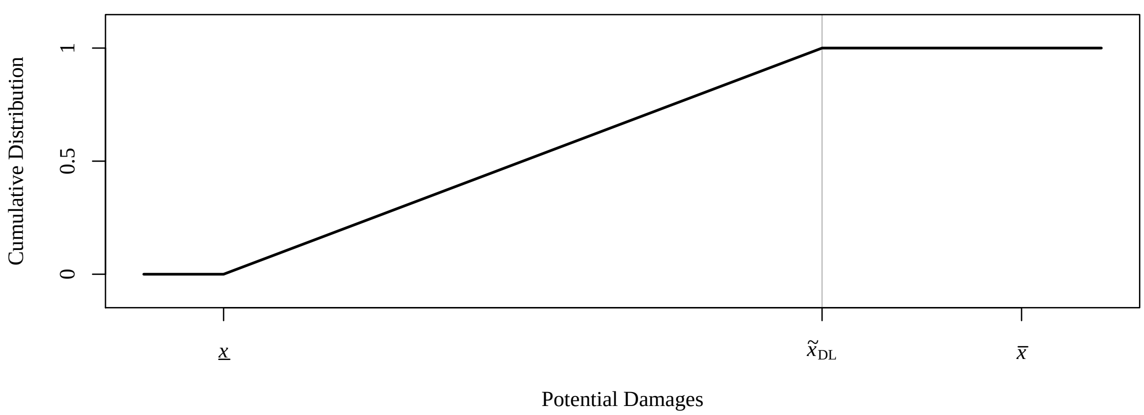
[111] A related policy is a split-award reform wherein a defendant pays out a full award but the plaintiff is compelled to split an explicit portion of the reward with the state (see Nikitin and Landeo, 2004). This *is* an explicit down-scaling of compensation for an assessed injury, but is not properly a damages limit as the defendant still pays the full amount of the injury in a plaintiff-verdict.

[112] See n. 95 for the definition of stochastic dominance.

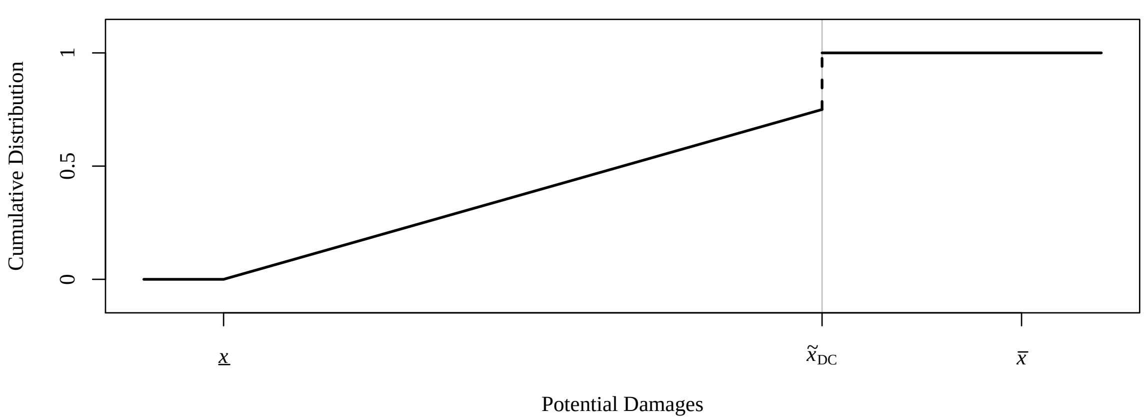
Figure 30: Illustration of Limited and Capped Damages Distributions



(a) Example CDF of Potential Damages (Control)



(b) Example CDF of Potential Damages with Damages Limit Imposed



(c) Example CDF of Potential Damages with Damages Cap Imposed

\$50.00 and \$160.00 by a linear transformation of the plaintiff's random injury draw. Example instruction are provided in Appendix H.1.

Imposing the $\mathbf{T}_{10}$ damages limit restricts the damages support to $[\$50, \$160]$ while leaving the support of total injuries at the control level, $[\$50, \$200]$. The distinction between injury and potential damages is consistent with damages limits in the field and may be a behaviorally important aspect of the reform policy. From a theoretic perspective, however, only the distribution of potential damages is material, so that a damages limit is theoretically equivalent to a simple reduction in $\overline{x}$.

With control parameter values and a potential damages limited at $\widetilde{x}_{\mathrm{DL}} = \$160$, the Proposition 3 requirement for an internal solution (even with arbitrarily fine period duration) remains satisfied in treatment $\mathbf{T}_{10}$. Relative to the control treatment, the predicted effects of the damages limit are exactly analogous to those of the simple reduction in $\overline{x}$ studied in the low asymmetry treatment environment of Chapter V (see Section 10.4). Delay-to-resolution is stochastically smaller after a limitation is placed on damages, but the distribution of delay-to-settlement is unchanged.[113]

Remark 11. Imposing a limit on potential damages decreases expected delay-to-resolution, but does not change expected delay-to-settlement:

$$\mathrm{E}\,[D_R^{\mathbf{T}_{10}}] - \mathrm{E}\,[D_R^{\mathbf{T}_0}] = -11.74 \qquad \mathrm{E}\,[D_S^{\mathbf{T}_{10}}] = \mathrm{E}\,[D_S^{\mathbf{T}_0}].$$

Figure 31 illustrates delay-to-resolution under various SE3 treatments. Figure 31(a) illustrates delay under the control treatment. Figures 31(b) to 31(d) illustrate the same under imposition of a damages limit, prejudgment interest rule, and Early-Offers rules, respectively; no distributional prediction is available for the damages cap treatment. Together, Figures 31(a) and 31(b) illustrate the treatment effect of a

[113]Details and intuition are provided in Section 10.4 and Appendix F.1.

damages limit: the probability of a trial verdict is reduced with the probability mass redistributed to scale up the probability of settlement in every period of bargaining.

As an alternative way to visualize distributional differences, Figure 32 illustrates theoretic hazard functions for the same SE3 treatments.[114] The hazard of settlement in the damages limit treatment is everywhere above the control hazard. The damages limit hazard function also increases more rapidly over time.

From the perspective of an actual policymaker, the effects of various "tort reform" policies on the relative distribution of wealth are at least as important as effects on economic efficiency. While the present study takes no normative position on desirable distributive results, it would be remiss to ignore this aspect of tort reform altogether. Wealth-distributive context for the reform policies and treatment effects in SE3 is provided by differences in earnings between control and reform treatments.

Predicted differences in average earnings are consolidated in Table 19 for all SE3 reform policies. Consistent with its popular conception as a "defendant-favoring" policy, a damages limit is predicted to reduce average plaintiff earnings and to increase average defendant earnings. The efficiency gain from imposing a damages limit (i.e. reduced average delay) is reflected in the positive net change in earnings.

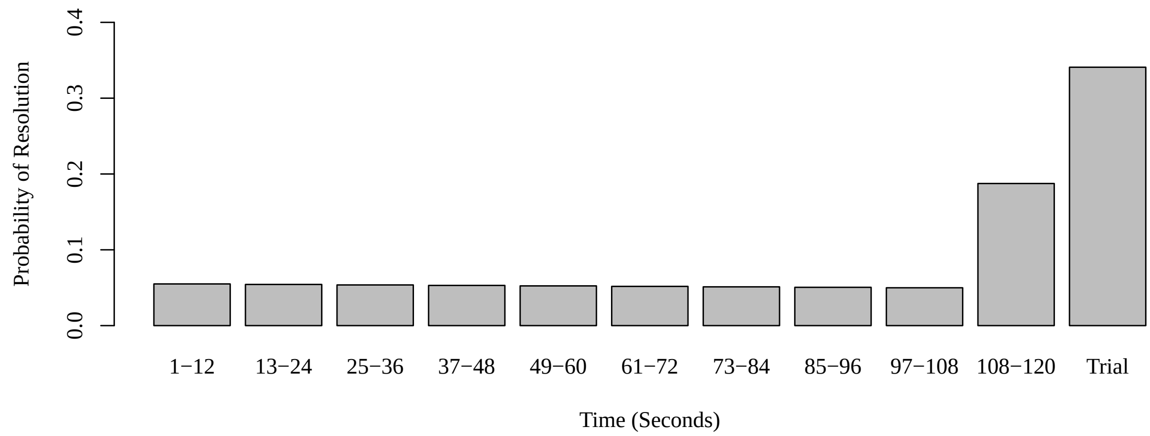
Table 19: Predicted Reform Policy Effect on Earnings in SE3[a]

Treatment Comparison	Plaintiff	Defendant	Net Change
Control → Damages Limit	−$7.64	$16.82	$9.18
Control → Damages Cap	$31.57	$19.18	$50.75
Control → Prejudgment Interest	$7.82	−$10.26	−$2.44
Control → Early Offers	−$20.73	$33.14	$12.41

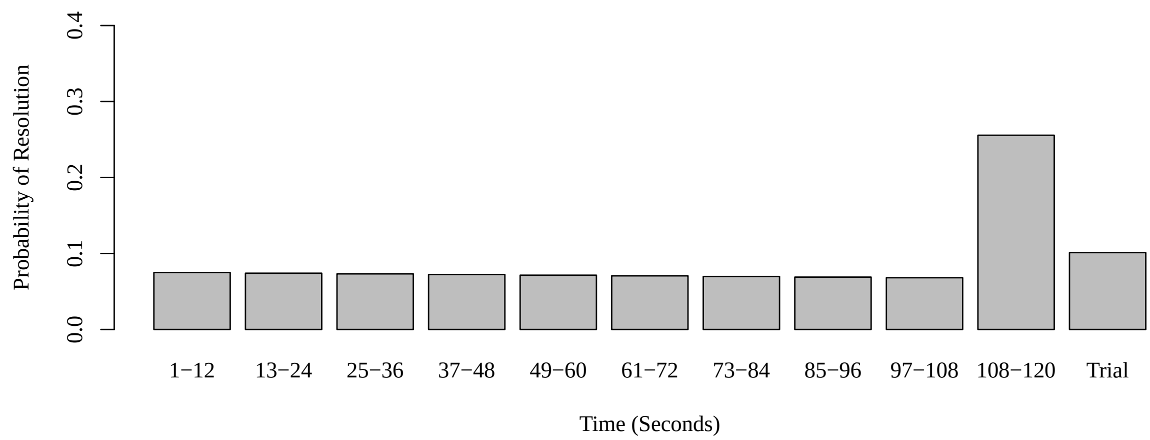
[a] Average changes in end-of-game earnings (in experimental dollars) are calculated under simulated equilibrium play with injury and trial verdict draws distributed as in SE3.

[114] See Section 8.4 for the definition of a hazard function.

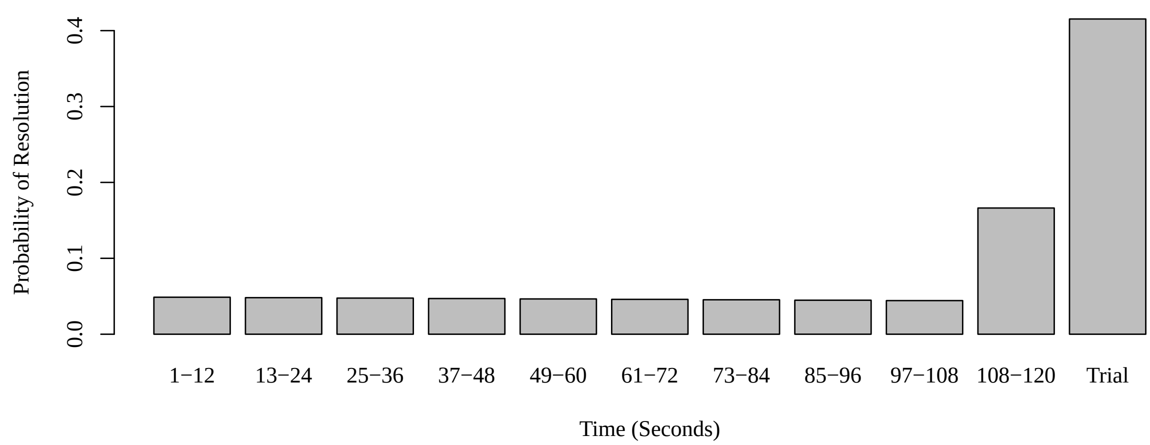
Figure 31: Predicted Delay-to-Resolution Distributions in SE3



(a) Distribution of Resolution Timing in $\mathbf{T}_0$ (Control)

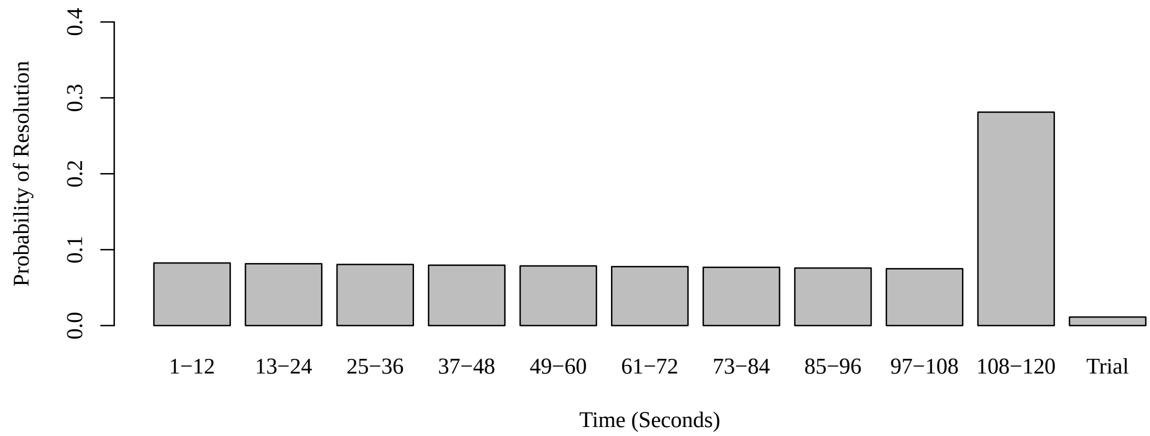


(b) Distribution of Resolution Timing in $\mathbf{T}_{10}$ (Damages Limit)



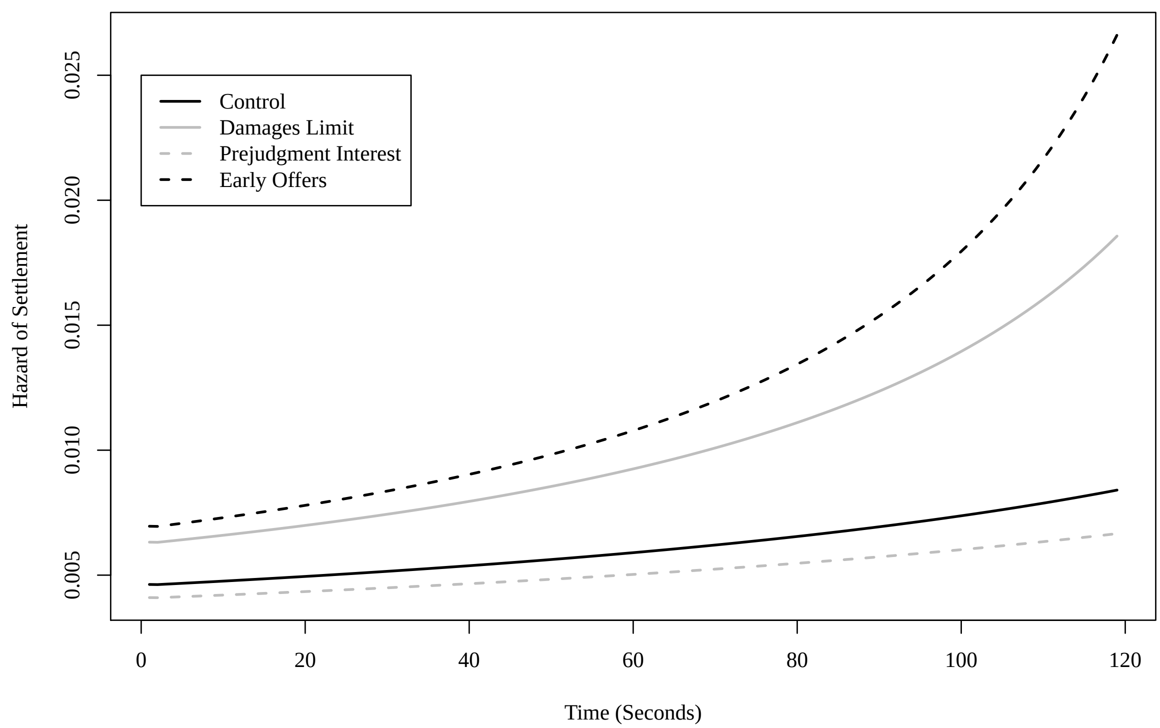
(c) Distribution of Resolution Timing in $\mathbf{T}_{12}$ (Prejudgment Interest)

Figure 31: Predicted Delay-to-Resolution Distributions in SE3 (Cont...)



(d) Distribution of Resolution Timing in $\mathbf{T}_{13}$ (Early Offers)

Figure 32: Predicted Hazard of Settlement in SE3[a]



[a]Hazard rates for dispute resolution are illustrated up-to-but-excluding the final second of bargaining and the trial verdict phase: i.e. the illustration covers seconds 1 through 119 of a dispute. Truncating the illustration at 119 seconds improves legibility, as the sharp spike in the hazard function in the final second of bargaining swamps all other variability.

13.2 Damages Cap

Damages cap treatment sequences $\mathbf{S}_{13}$ and $\mathbf{S}_{14}$ consist of treatments $\mathbf{T}_0$ and $\mathbf{T}_{11}$. The only difference between treatments is that $\mathbf{T}_{11}$ involves the $\mathcal{R}_2$ level of the reform environment factor, corresponding to imposition of a cap on potential damages. In contrast to damages limits, damage caps are a common categorization of reform policy.

Damage caps are frequently but heterogeneously imposed at the state level (see ATRA, 2009). Many practical examples involve the placement of caps on punitive damages awards. Such policies may cap punitive damages in absolute level (e.g. at \$500,000), in relative level (e.g. at three-times the size of compensatory damages), or by a combination thereof (e.g. at the lesser of an absolute and relative cap). Similar examples involve caps on non-economic compensatory damages: e.g. damages to compensate for pain and suffering or emotional distress.

Let $\widetilde{x}_{\mathrm{DC}}$ denote a damages cap: the maximum value of potential damages following imposition of the reform policy. Imposing a cap on damages censors the pre-reform damages distribution at $\widetilde{x}_{\mathrm{DC}}$. If $F(x)$ and $f(x)$ are the pre-reform distribution and density functions for un-capped potential damages, then the post-reform distribution and density/mass functions are as follows:

$$F_{\mathrm{DC}}(x) = \begin{cases} F(x) & x < \widetilde{x}_{\mathrm{DC}} \\ 1 & x \geq \widetilde{x}_{\mathrm{DC}} \end{cases} \qquad f_{\mathrm{DC}}(x) = \begin{cases} f(x) & x < \widetilde{x}_{\mathrm{DC}} \\ 1 - F(\widetilde{x}_{\mathrm{DC}}) & x = \widetilde{x}_{\mathrm{DC}} \\ 0 & x > \widetilde{x}_{\mathrm{DC}}. \end{cases} \tag{63}$$

In the interesting case where $\widetilde{x}_{\mathrm{DC}} < \overline{x}$, imposing a cap on potential damages moves all probability mass from values above $\widetilde{x}_{\mathrm{DC}}$ onto an atomic mass-point at $\widetilde{x}_{\mathrm{DC}}$.

Figure 30(c) illustrates two noteworthy distributional effects of a cap on damages. First, unlike the pre-reform distribution in Figure 30(a) or the post-reform damages limit distribution in Figure 30(b), the damages distribution following imposition of a cap is not continuous over its full support. Second, for all values less than $\widetilde{x}_{\text{DC}}$, the capped damages distribution is identical to the pre-reform distribution.

The subtle distinction between a damages limit and damages cap leads to a striking difference in equilibrium strategies. In deriving the damages cap equilibrium, start with a game of length $T = 1$. Consistent with the approach taken in Chapter II, only Assumption 1 is maintained in arriving at the following result.

Proposition 5. *Implicitly define the interior-solution settlement proposal,* S_1^I*, as*

$$S_1^I: \quad -F(\pi^{-1}\{\delta^{-1}S_1^I + k_p\}) + \pi^{-1}(k_d + k_p)f(\pi^{-1}\{\delta^{-1}S_1^I + k_p\}) = 0.$$

Let the boundary-solution settlement proposal, S_1^B*, be defined as*

$$S_1^B = \delta(\pi\widetilde{x}_{\text{DC}} - k_p).$$

For $V_d(S_1)$*, the defendant-valuation of arbitrary proposal* S_1 *(see proof for definition), the defendant's PBE strategy in a game of length* $T = 1$ *is to make proposal* S_1^**:*

$$S_1^* = \begin{cases} S_1^I & V_d(S_1^I) \geq U_d(S_1^B) \\ S_1^B & V_d(S_1^I) < U_d(S_1^B). \end{cases} \tag{64}$$

The strategy for a plaintiff of type x *is to accept any settlement proposal* S_1 *such that* $U_p(S_1) \geq W_p(x)$*, and to otherwise reject.*

Proof. Provided in Appendix G.1. □

Proposition 5 reaches a surprising conclusion: unless it causes a boundary solution to obtain, imposing a cap on damages induces no change in equilibrium strategies. The intuition for this result is that a cap on damages only affects the upper-tail distribution of plaintiff types—types that do not settle in an interior equilibrium anyway. The lower expected damages award for these types is a windfall for the defendant, but as the damages distribution is undisturbed at the margin, no change in equilibrium strategies results.

Relative to Proposition 1, the final expression for S_1^* in Proposition 5 is complicated by discontinuity in the defendant's objective function at the boundary solution. This discontinuity is illustrated in Figure 33, as compared with Figure 3 for an uncapped damages distribution. The problem is that the atomic mass of plaintiff types at $\widetilde{x}_{\mathrm{DC}}$ causes a jump in the probability of settlement from

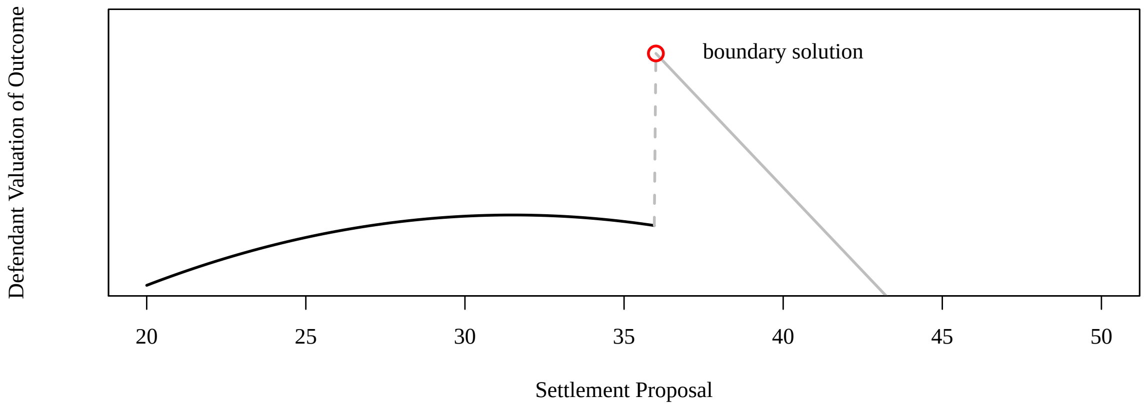
$$\lim_{\varepsilon \to 0^+} \mathrm{P}_{\mathrm{DC}}[x \leq \widetilde{x}_{\mathrm{DC}} - \varepsilon] = F(\widetilde{x}_{\mathrm{DC}}) \tag{65}$$

for any proposal with net present value getting arbitrarily close to $W_p(\widetilde{x}_{\mathrm{DC}})$ from the left, to $\mathrm{P}\left[x \leq \widetilde{x}_{\mathrm{DC}}\right] = 1$ when $U_p(S_1)$ exactly equals $W_p(\widetilde{x}_{\mathrm{DC}})$.

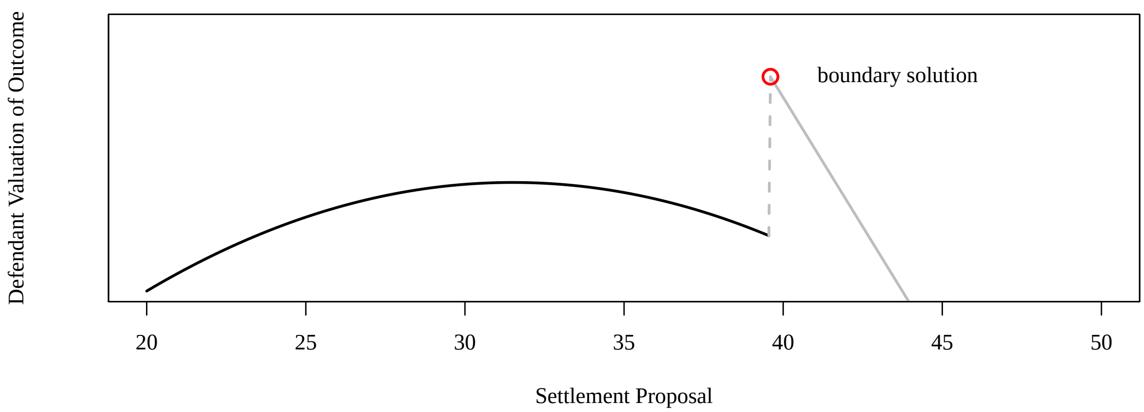
As Figures 33(a) and 33(b) illustrate, this discontinuity destroys the simplifying result in Propositions 1 and 2 that the interior solution obtains identically when $S_1^I < S_1^B$. After imposition of a damages cap, the most obvious means of determining whether equilibrium involves an interior or boundary solution is to manually check which provides a greater expected payoff to the defendant.

Next consider a multiple-period model of settlement bargaining. Equilibrium strategies in the first period of a game of length $T > 1$ follow easily from Proposition 1. Consistent with the approach taking in Chapter II, Assumptions 1 through 4 are maintained in deriving the following equilibrium strategies.

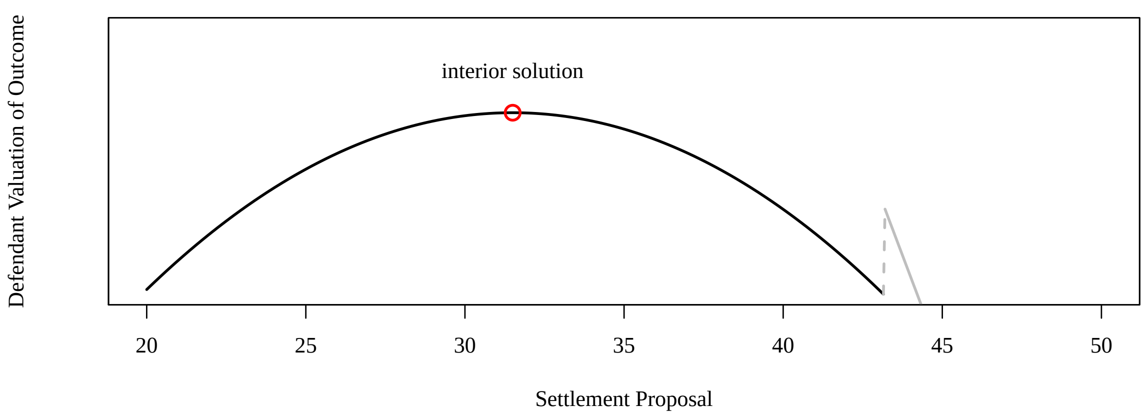
Figure 33: Illustration of Damages Cap Interior and Boundary Solutions[a]



(a) Example Boundary Solution: $\widetilde{x}_{\mathrm{DC}} = 100$



(b) Example Interior Solution: $\widetilde{x}_{\mathrm{DC}} = 108$



(c) Example Interior Solution: $\widetilde{x}_{\mathrm{DC}} = 116$

[a]Defendant objective function with x distributed uniformly on $[50, 120]$, $c_p = c_d = 1$, $k_p = k_d = 10$, $\delta = 0.9$, and $\pi = 0.5$. The black portion of the line indicates the value of a settlement proposal which some plaintiff types reject; the gray portion indicates the value of the boundary solution.

Proposition 6. *Let the interior-solution settlement proposal, S_1^I, be defined as*

$$S_1^I = \delta^T(\pi \underline{x} + k_d) + c_d \sum_{i=1}^{T-1} \delta^i.$$

Let the boundary-solution settlement proposal, S_1^B, be defined as

$$S_1^B = \delta^T(\pi \widetilde{x}_{\text{DC}} - k_p) - c_p \sum_{i=1}^{T-1} \delta^i.$$

For $V_d(S_1)$, the defendant-valuation of arbitrary proposal S_1 (see proof for expansion), the defendant's PBE strategy in a game of length $T > 1$ is to make proposal S_1^*:

$$S_1^* = \begin{cases} S_1^I & V_d(S_1^I) \geq U_d(S_1^B) \\ S_1^B & V_d(S_1^I) < U_d(S_1^B). \end{cases} \tag{66}$$

The plaintiff's PBE strategy is the same as that given in Proposition 1, except that all plaintiff types settle immediately for a proposal of S_1^B or greater.

Proof. Provided in Appendix G.2. □

To be consistent with the damages limit treatment, the damages cap in experimental treatment $\mathbf{T}_{11}$ restricts potential damages to lie at or below $\widetilde{x}_{\text{DC}} = \$160$. A boundary solution obtains at this cap-point, so the theoretic prediction for treatment $\mathbf{T}_{11}$ is full and immediate settlement at S_1^B, the net present value of a trial verdict to a plaintiff with potential damages equal to the cap-value of \$160. This boundary solution equilibrium explains the absence of resolution-delay predictions for the damages cap treatment in Figures 31 and 32: the predicted distribution of resolution delay is degenerate at $t = 1$ for treatment $\mathbf{T}_{11}$.

Like the zero-delay prediction for symmetric information in Chapter V, the *strong* theoretic implication of full and immediate under a boundary solution in $\mathbf{T}_{11}$ is a naïve predictor of settlement bargaining behavior in SE3. Imperfectly controlled information, imperfectly modeled subject preferences, or other unexplained influences may lead to systematic resolution delay even under a boundary solution. Rather than attempting to formalize unexplained sources of delay, the present analysis relies on the following *weak* implication of theory in framing the damages cap treatment.

Remark 12. Delay-to-resolution and delay-to-settlement are stochastically smaller in $\mathbf{T}_{11}$ than in $\mathbf{T}_0$.

Note that Remark 12 contains as a special case the strong theoretic prediction of zero delay under the damages cap treatment. The more flexible prediction captures the intuition that a damages cap should not tend to *increase* delay and is better suited to formal testing. A limitation is lack of concrete predictions for the distribution and expectation of delay under the damages cap treatment.

Remark 13. Imposing a cap on potential damages decreases expected delay-to-resolution and expected delay-to-settlement:

$$\mathrm{E}\left[D_R^{\mathbf{T}_{11}}\right] < \mathrm{E}\left[D_R^{\mathbf{T}_0}\right] \qquad \mathrm{E}\left[D_S^{\mathbf{T}_{11}}\right] \leq \mathrm{E}\left[D_S^{\mathbf{T}_0}\right].$$

Predicted changes in average earnings under imposition of a damages cap are consolidated in Table 19. The startling observation that earnings increase for both litigants (and actually increase more for the plaintiff than for the defendant) owes to reliance on the strong prediction of zero settlement delay and no inefficient expenditures on bargaining and trial costs. Predicted changes in average earnings are not available for the more realistic prediction in Remark 13.

13.3 Prejudgment Interest

Prejudgment interest sequences $\mathbf{S}_{15}$ and $\mathbf{S}_{16}$ consist of treatments $\mathbf{T}_0$ and $\mathbf{T}_{12}$. The only difference between treatments is that $\mathbf{T}_{12}$ involves the $\mathcal{R}_3$ level of the reform environment factor, corresponding to imposition of a prejudgment interest rule for damages awarded by a trial verdict. In contrast to caps and limits on damages—conventionally considered defendant-favoring reform policies—a prejudgment interest rule is usually thought to be plaintiff-favoring.

Under a prejudgment interest rule, damages awarded in a trial verdict may (or must) include interest accrued on the value of the assessed injury from some prior point in time: e.g. from the point of injury or from the point of formal complaint. A number of states (e.g. Georgia, New Hampshire, Ohio, West Virginia) currently impose some form of prejudgment interest rule, though there is considerable heterogeneity in what interest rate is used (e.g. federal reserve, U.S. treasury, or commercial interest rates) and in what limitations are imposed on interest accrual (e.g. interest unavailable on future damages or total interest capped at a certain amount).[115]

The popularity of prejudgment interest rules stems, at least in part, from their perceived ability to dissuade delayed resolution of disputes.[116] The thinking is that a defendant retaining interest on the amount of a future compensation payment has marginal incentives to delay remuneration in order to accumulate greater interest. By this reasoning, elimination of the defendant's ability to retain interest on a potential award would seem to decrease incentives for delay, and therefore marginally increase the chances of more rapid settlement of tort disputes.[117]

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[115] Prejudgment interest policies in the field policies vary widely (see ATRA, 2009).

[116] "In addition to seeking to compensate the plaintiff fully for losses incurred, the goal of such statutes is to encourage early settlements and to reduce delay in the disposition of cases, thereby lessening congestion in the courts" (ATRA, 2009, p. 40). See also Kessler (1996, p. 434).

[117] Prejudgment interest rules also have the equitable justification of compensating the plaintiff for the full amount of an assessed injury, including the time-value of a loss.

A simple model of prejudgment interest maps an injury draw, x, to a potential damages draw of $\delta^{-T}x$. Note that this reform model is equivalent to simply rescaling the entire potential damages distribution from $[\underline{x}, \overline{x}]$ to $[\delta^{-T}\underline{x}, \delta^{-T}\overline{x}]$: i.e. increasing the range of potential damages by a scalar multiple, $\delta^{-T} > 1$. Marginal changes in the range of potential damages have the same predicted effect on resolution delay as marginal changes in $\overline{x}$ with $\underline{x}$ held constant (see Corollary 6). Note that the implied result is exactly counter to the motivation for a prejudgment interest rule given above: prohibiting the defendant from retaining interest on compensation owed to the plaintiff actually *increases* average resolution delay.

Corollary 7. *At an interior equilibrium, the (Corollary 2) ex ante probability of dispute resolution, p_t, responds to changes in $(\overline{x} - \underline{x})$ as follows:*

$$\frac{\partial p_t}{\partial(\overline{x} - \underline{x})} = \begin{cases} -\pi^{-1}\delta^{-T+t}(c_p + c_d)/(\overline{x} - \underline{x})^2 < 0 & t = 1, \ldots, T-1 \\ -\pi^{-1}(k_p + k_d)/(\overline{x} - \underline{x})^2 < 0 & t = T \\ -\sum_{i=1}^{T} \partial p_i/\partial(\overline{x} - \underline{x}) > 0 & t = T+1. \end{cases}$$

To get some intuition for the surprising result in Corollary 7, think of settlement timing terms, $\underline{x}_1, \ldots, \underline{x}_{T+1}$ (Corollary 1), as a grid to be laid over the distribution of potential damages draws. A prejudgment interest rule causes no change in the settlement-timing grid: the spacing of $\underline{x}_t$ values reflects the need to make settlement proposals sequentially rational and depends only on discounting and cost terms. By expanding the support of damages, the prejudgment interest rule reduces the density of all plaintiff types and thus shrinks the measure of plaintiff types falling into each pretrial bin of the grid. The probability of settlement in every period is decreased and the likelihood of a trial verdict is increased.[118]

[118]Kessler (1996) provides an alternative explanation in terms of the variance of potential damages.

Treatment $\mathbf{T}_{12}$ models a prejudgment interest rule by exact analogy to the above theory; example instructions are provided in Appendix H.1. With control parameter values and a prejudgment interest rule, the Proposition 3 requirement for an internal solution (even with arbitrarily fine period duration) remains satisfied. Equilibrium strategies in $\mathbf{T}_{12}$ are thus described by the interior solutions in Propositions 1 and 2, but with the support of potential damages redefined from $[\underline{x}, \overline{x}]$ to $[\delta^{-T}\underline{x}, \delta^{-T}\overline{x}]$.

Remark 14. Imposing a prejudgment interest rule increases expected delay-to-resolution, but expected delay-to-settlement is unchanged:

$$\mathrm{E}\,[D_R^{\mathbf{T}_{12}}] - \mathrm{E}\,[D_R^{\mathbf{T}_0}] = 3.65 \qquad \mathrm{E}\,[D_S^{\mathbf{T}_{12}}] = \mathrm{E}\,[D_S^{\mathbf{T}_0}].$$

The small predicted change in delay-to-resolution reflects an inherent limitation of prejudgment-interest reform: the magnitude of its effect is determined by an exogenous interest rate. The relatively low interest rate under control parameter values (see Section 7.1) explains the relatively small change in expected delay. Explanation for lack of effect on delay-to-settlement is basically the same as for the asymmetric information (Section 10.4) and damages limit (Section 13.1) treatments: a prejudgment interest rule has no effect on the relative shape of settlement timing, so there is no change in expected delay when conditioning on settlement of a dispute.[119]

Popular conception of a prejudgment interest rule as plaintiff-favoring is born out by average earnings predictions in Table 19. Exposure to treatment $\mathbf{T}_{12}$ is predicted to increase average plaintiff earnings and decrease average defendant earnings relative to the control treatment. The small net decrease in earnings reflects the modest predicted decrease in efficiency caused by imposition of a prejudgment interest rule.

[119] This is formally evident in equation (49) of Appendix F.1. The probability density of delay-to-settlement only depends on three types of parameter values: cost terms, the rate of inter-temporal discounting, and the maximum duration of settlement negotiation.

13.4 Early Offers

Early Offers treatment sequences $\mathbf{S}_{17}$ and $\mathbf{S}_{18}$ consist of treatments $\mathbf{T}_0$ and $\mathbf{T}_{13}$. The only difference between treatments is that $\mathbf{T}_{13}$ involves the $\mathcal{R}_4$ level of the reform environment factor, corresponding to imposition of the "Early Offers" reform policy proposed by O'Connell (1982). Early Offers rules are similar to other reform policies considered in this study in that the manipulation concerns the plaintiff's access to damages awards, but differ from other reform policies in that access to potential damages is made to depend on litigant behavior during settlement bargaining.[120]

Under Early Offers reform, a defendant who early in the litigation processes (e.g. within the first 60 days) offers to settle for at least remuneration of economic damages, faces a more favorable standard of proof (e.g. *gross negligence* instead of *negligence*) if the dispute ultimately proceeds to trial.[121] Supporting the emphasis on payment of economic damages is the proposition that these are generally easier to predict than non-economic damages. Early Offers rules are meant to give the defendant a positive incentive to make a reasonable settlement offer early in the negotiation process, and to give the plaintiff a (strong) negative incentive to reject any such early offer.[122] Though not widely adopted, elements of Early Offers reform are present in Maryland laws regulating lead paint in rental properties (Schukoske, 1994, pp. 38–40, n. 63).

[120]Cf. Kessler (1996, p. 435), commenting on negotiation-dependent implementations of prejudgment interest rules in some states.

[121]Intuitively, economic or "special" damages are those for which monetary remuneration is simple to determine: e.g. lost wages, doctors' bills, repair costs, etc. This contrasts with non-economic or "general" damages, such as compensation for pain and suffering, emotional distress, etc. The distinction between negligence and gross negligence is more ethereal. Negligence is often defined as less care than would be taken by a reasonably prudent person. Gross negligence requires a greater showing of carelessness—possibly something like moral culpability.

[122]One objective of Early Offers rules is to prevent a defendant's early proposal to settle for a reasonable amount from being interpreted as a signal of weakness (O'Connell, 1982, p. 604). The present theoretic model of settlement bargaining provides the defendant with no private information to divulge, so this consideration does not appear in theoretic analysis. The signal-obscuring aspect of the reform policy may nevertheless be reflected in experimental results if, e.g., experimental settlement bargaining is affected by uncontrolled aspects of asymmetric information.

A simplified model of Early Offers reform follows. Let e be some specified *early* point in settlement bargaining, $e < T$, and let x_{E} be the economic part of the plaintiff's total damages draw, $x_{\mathrm{E}} \leq x$. If in any period $t = 1, \ldots, e$, the defendant makes a settlement proposal for at least remuneration of economic damages, $\max\{S_1, \ldots, S_e\} \geq x_{\mathrm{E}}$, then the probability of a plaintiff verdict drops to $\pi_{\mathrm{EO}} < \pi$.

Early Offers rules are more complicated than the simple manipulations of damage awards considered in Sections 13.1 through 13.3. Combined with the delicacy of equilibria that admit both boundary and interior solutions, the discontinuity in valuation introduced by satisfaction of the Early Offers condition makes characterizing the full set of candidate equilibria a tedious endeavor. Fortunately, a special case result provides a unique and parsimonious equilibrium prediction for a subset of parameter values containing $\mathbf{T}_{13}$. A brief sketch of other candidate equilibria under Early Offers rules is provided in Appendix G.4.

Proposition 7. *Let $S_1^*(\pi)$ denote the first equilibrium settlement proposal in a game of length $T > 1$ with the probability of a plaintiff verdict is equal to π. Provided*

$$U_p(S_1^*(\pi_{\mathrm{EO}})) \leq U_p(S_e = x_{\mathrm{E}}),$$

equilibrium strategies under Early Offers rules are given by Propositions 1 and 2, but with the value of π_{EO} substituted for π.

Proof. Provided in Appendix G.3. □

The intuition for Proposition 7 is simple. For sufficiently small values of x_{E} relative to π_{EO}, the defendant rationally adopts a sequence of settlement proposals that exceed $S_e = x_{\mathrm{E}}$ along the equilibrium path. Since equilibrium proposals in this sequence always satisfy the Early Offers condition, strategies in this special case are simply those of the standard settlement bargaining game but with π replaced by the lower Early Offers probability of a plaintiff-verdict, π_{EO}.

Corollary 8. *The (Corollary 2) ex ante probability of dispute resolution, p_t, responds to changes in π as follows:*

$$\frac{\partial p_t}{\partial \pi} = \begin{cases} -\pi^{-2}\delta^{-T+t}(c_p + c_d)/(\overline{x} - \underline{x}) < 0 & t = 1, \ldots, T-1 \\ -\pi^{-2}(k_p + k_d)/(\overline{x} - \underline{x}) < 0 & t = T \\ -\sum_{i=1}^{T} \partial p_i/\partial \pi > 0 & t = T+1. \end{cases}$$

Intuition for Corollary 8 is basically the same as that for Corollary 7, concerning the marginal effect of a change in $(\overline{x} - \underline{x})$. A marginal decrease in π effectively shrinks the support of potential damages, increasing the probability of settlement and decreasing the probability of a trial verdict. As discussed in Appendix G.4, a decrease in π can cause an interior solution to switch to boundary solution, but cannot cause a boundary solution to become an interior one. Thus, even extra-marginal decreases in π lead to decreases in the probability of settlement in each period.

Treatment $\mathbf{T}_{13}$ makes use of the stylized presentation of damages in the experiment: i.e. damages defined as the sum of a (common knowledge) fixed economic injury of \$50 and a (private knowledge) variable pain-and-suffering injury between \$0 and \$150. The experimental adaptation of Early Offers reform sets the economic injury according to the presentation of damages, $x_E = 50$, requires the proposal be made within the first $e = 30$ seconds of bargaining, and reduces the probability of a plaintiff-verdict from $\pi = 3/4$ to $\pi_{\text{EO}} = 1/2$ following a satisfactory "early" proposal.[123]

[123]Note that setting $x_E = \underline{x}$ is consistent with the proposition that economic damages are easy for either party to determine. Parameter choices $e = 30$ and $\pi_{\text{EO}} = 1/2$ are conservative compared to the suggestions of O'Connell (1982): more appropriate parameters may be more like $e = 5$ and $\pi_{\text{EO}} = 1/20$. The more conservative parameter values preferred in defining $\mathbf{T}_{13}$ have the advantage of

Example instructions for the Early Offers treatment are provided in Appendix H.1. With $\pi_{\mathrm{EO}} = 1/2$ and other parameters set to control levels, the condition in Proposition 7 is satisfied so that equilibrium is given by Propositions 1 and 2, but with π_{EO} substituted for π. An interior solution obtains in which delay-to-resolution is stochastically smaller than in the control treatment.

Remark 15. Imposing Early Offers rules decreases expected delay-to-resolution, but not expected delay-to-settlement:

$$\mathrm{E}\,[D_R^{\mathbf{T}_{13}}] - \mathrm{E}\,[D_R^{\mathbf{T}_0}] = -16.143 \qquad\qquad \mathrm{E}\,[D_S^{\mathbf{T}_{13}}] = \mathrm{E}\,[D_S^{\mathbf{T}_0}].$$

As illustrated by comparison of delay distributions in Figures 31(a) and 31(d), exposure to the Early Offers treatment is predicted to substantially decrease expected delay-to-resolution. The same relationship is illustrated in Figure 32, where the hazard of settlement in the Early Offers treatment is everywhere below that of the control treatment. Like the damages limit and prejudgment interest reform proposals, however, expected delay-to-settlement is not affected by Early Offers reform.[124]

For the particular parameter values imposed in treatment $\mathbf{T}_{13}$, Early Offers rules are strongly defendant-favoring.[125] Under simulated equilibrium play, average plaintiff earnings fall by about \$21 (experimental dollars) relative to the control treatment; average defendant-earnings increase by almost \$33. The net increase in earnings reflects the efficiency gain of the Early Offers equilibrium in terms of reduced average delay-to-resolution.

maintaining a comparison between interior solutions under both control and Early Offers treatments. This provides a potentially more valid initial assessment of the reform policy than a treatment involving both a change in π and a simultaneous change in solution-type.

[124] Formal reasoning is the same as that in n. 119.

[125] This result will generally differ under alternative values of the Early Offers parameters e, x_E, and π_{EO}.

14 Results

The objectives of SE3 are both confirmatory and exploratory: if reform policies are found to mitigate settlement delay, a subsequent inquiry is how mitigation compares across policies. To address concerns about the distributive effects of different reform policies, collected data are also used to assess changes in litigant earnings during exposure to different reform environments.

Two comments on data preparation are generally applicable. First, data from the first two rounds of a treatment assignment are omitted from analysis to control for rapid learning and strategy-adjustment in the early rounds of exposure to a treatment.[126] Second, each SE2 treatment is assigned as the first treatment in a session ($\mathbf{T}_A$) in two of the sessions for a treatment pairing, and as the second treatment in a session ($\mathbf{T}_B$) in the other two sessions. The orthogonal order of treatment assignment combines with other experimental controls to mitigate serious concern about design bias from order or sequence effects.[127]

The remainder of this section proceeds as follows. Section 14.1 assesses the capacity of select reform policies to mitigate settlement delay. Results are not optimistic about the capacity of reasonable policy changes to affect large reductions in delay. Section 14.2 compares resolution delay between the different SE3 treatments. Consistent with the comparability of average delay between treatments, few compelling differences are observed. Finally, Section 14.3 assesses the wealth-distributive effects of SE3 reform policies. The direction and magnitude of changes in wealth are mostly consistent with theoretic predictions.

[126]For additional discussion, see Section 8, particularly n. 70.

[127]See discussion in n. 99.

14.1 Effect of Reform Policies on Delay

Presentation of results begins with empirical assessment of Remarks 11 through 15: the predicted effects of various tort reform policies on expected delay. The identification strategy in this section is comparison of average delay between SE3 treatments. Each treatment-pair in SE3 consists of the control treatment, and one *reform treatment* which perturbs the control only by imposing one of the four reform policies described in Section 13. Differences in resolution delay between control and reform treatments are attributable to the causal effect of implementing a given reform policy.

A starting point is analysis of session-average delay. Averaging delay measurements across all disputes in a session provides a matched pair of observations for each session: one corresponding to assignment of the control treatment, and one corresponding to assignment of a reform treatment. Session-average observations may be dependent within a matched pair (as the same group of subjects are assigned to each of the two treatments), but are independent across pairs (as each session involves a unique group of subjects).[128] Treatment effects are estimated by comparing the 4 session-average delay measurements collected from a given reform treatment with the 12 session-average control treatment measurements collected from *other* SE3 treatment pairings. Session averages are plausibly identically distributed (at least, within-treatment) as the products of a common data generating process.

The estimated treatment effects of each reform policy on delay-to-resolution (D_R) and delay-to-settlement (D_S) are consolidated in Table 20. As indicated by the associated Wilcoxon-Mann-Whitney tests, session average data fail to statistically distin-

[128]For example, the 4 session-average observations for the damages limit treatment may tend to correlate with the 4 control treatment observations in the damages limit treatment pairs. Damages limit observations are plausible independent of control treatment observations for sessions involving damages cap, prejudgment interest, and Early Offers treatment pairs. Observations for each reform treatment can thus be compared to $3 \times 4 = 12$ independent observations of the control treatment.

Table 20: Reform Policy Treatment Effects in SE3, Session Level[a]

Treatment Comparison	ΔD_R	ΔD_S
Control → Damages Limit	−5.914	−7.690
	0.2615	0.1703
Control → Damages Cap	2.689	6.147
	0.9527	0.5989
Control → Prejudgment Interest	−2.561	−4.721
	1.0000	0.8615
Control → Early Offers	−1.089	−0.813
	0.8615	0.8615

[a] Comparison of delay between session-average observations for a reform treatment and the control treatment of all other SE3 sessions: i.e. sample sizes $n = 4$ and $m = 12$. On top are average treatment effects (in seconds). On bottom are exact p-values corresponding to application of Wilcoxon-Mann-Whitney permutation tests.

guish any reform policy form the no-effect null hypothesis. In contrast to theoretic predictions, no reform policy is found to obviously affect either measure of average delay and delay-to-settlement is, if anything, the more responsive measure.

As noted in Section 11.1, the experimental design is better suited to dispute-level analysis, and appropriate regression models may provide considerably greater power than simple session-average comparisons. At the dispute level, observations on the outcomes of individual disputes may be dependent within repetitions of a particular matching, but are independent and plausibly identically distributed after accounting for potential sources of dependence. Results from several regressions of delay on treatment indicators and random pair-effects are provided in Table 21. Columns 1 and 3 regress delay-to resolution and delay-to-settlement, respectively, on a set of reform policy indicators (damages limit, damages cap, prejudgment interest, and Early Offers, with the control environment as reference). Columns 2 and 4 provide

Table 21: Regression of Delay on Reform Policy in SE3, Dispute Level[a]

	D_R		D_S	
Parameter	(1)	(2)	(3)	(4)
Constant	71.015*** (4.3256)	33.830*** (6.5213)	62.321*** (4.5106)	19.024* (7.3830)
Damages Limit	−5.864 (3.8949)	−2.780 (3.7372)	−5.912 (4.0513)	−2.737 (3.9057)
Damage Cap	2.697 (3.7847)	0.855 (3.5530)	6.605† (3.9525)	4.778 (3.7507)
Prejudgment Interest	−1.955 (4.0113)	−0.351 (3.8360)	−2.033 (4.4562)	−0.543 (4.3496)
Early Offers	−1.084 (4.0554)	−0.469 (3.6884)	−0.590 (4.6152)	1.184 (4.2237)
Lag(1) D(p)		0.043 (0.0332)		0.092* (0.0363)
Lag(2) D(p)		0.237*** (0.0366)		0.252*** (0.0356)
Lag(1) D(d)		0.090** (0.0328)		0.066† (0.0355)
Lag(2) D(d)		0.050 (0.0334)		0.066† (0.0385)
σ^2_ϵ	1077.69	1042.12	831.62	809.08
σ^2_η	406.09	128.86	515.22	258.81

[a] Parameter estimates from random pair-effects regression of delay-to-resolution and delay−to−settlement on treatment indicators and lagged dependent variables (Swamy and Arora, 1972). Values in parentheses are heteroskedasticity and cluster-robust standard errors (Arellano, 1987). Parameter estimates for fixed round-effects are omitted. Variances σ^2_η and σ^2_ϵ correspond to pair and idiosyncratic error terms, respectively. Qualifiers $***$, $**$, $*$, and † denote significance from zero at levels < 0.001, 0.01, 0.05, and 0.1, respectively.

the same, but with two lags of delay for the plaintiff, $D(p)$, and defendant, $D(d)$, as additional controls.

Result 13. No reform policy studied in SE3 achieves a reduction in average delay that is statistically distinguishable from a null hypothesis of no effect.

Regression results in Table 21 are comparable to session-average results. The average treatment effect of each policy is most easily seen as the parameter estimate on the respective reform indicator in columns 1 and 3. Despite having signs that are mostly consistent with theory—the damages limit treatment notwithstanding—the only treatment effect found to be statistically distinguishable from zero at even the 0.1 level is that of the damages cap treatment on delay-to-settlement in column 3.

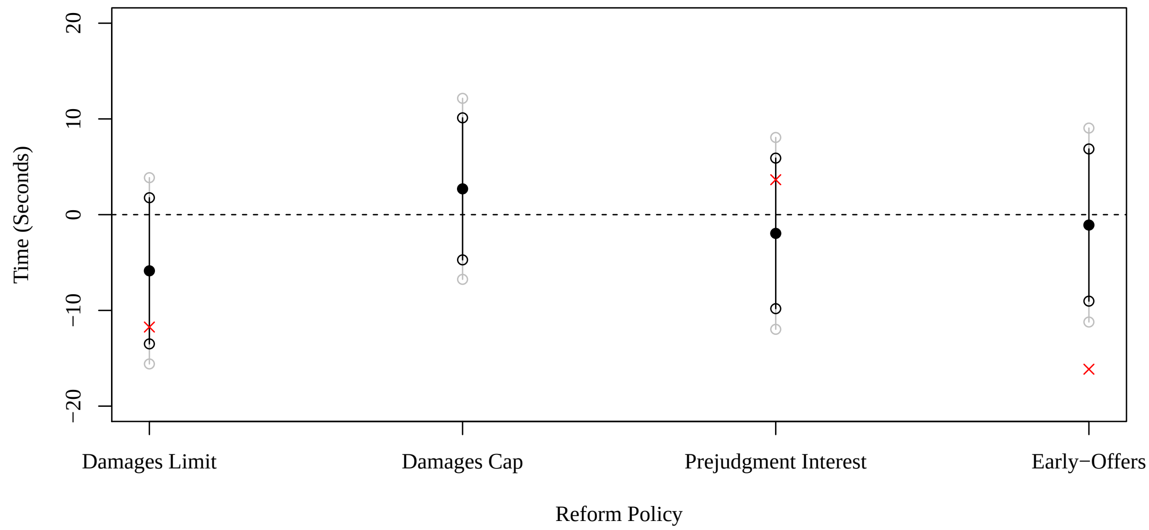
The knee-jerk impulse to focus on this weakly "significant" parameter should be avoided. Given the large number of individual hypothesis tests reported in Table 21 and the low precision of the single parameter estimate in question, the potential for spurious rejection is probably substantial. The most honest reading of results is correspondingly that the data fail to strongly distinguish any observed treatment effect from the null hypothesis of no effect.

Result 14. Few reform policies involve a reduction in average delay that is statistically distinguishable from prediction.

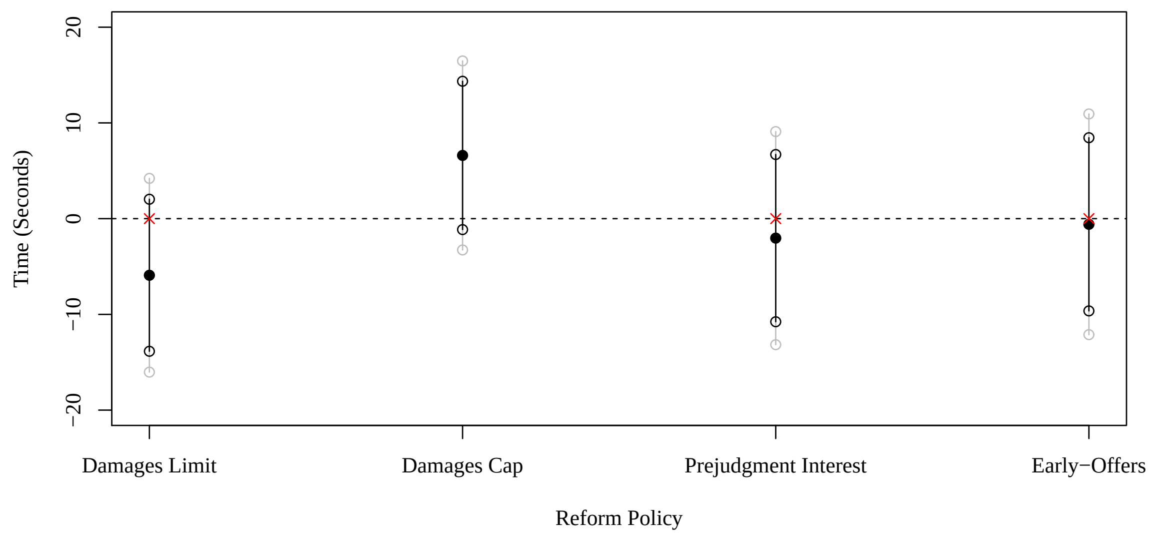
A more refined interpretation of the data is provided by Figure 37, which plots individual (black) and simultaneous (gray) confidence intervals for the average treatment effect of each reform policy as characterized by columns 1 and 3 of Table 21.[129] A dashed black line represents the no-effect null hypothesis at reference in Result 13; red crosses illustrate theoretic predictions for each treatment effect (but are omitted for the damages cap treatment as no viable point predictions are available).

[129] Simultaneous intervals employ Bonferroni corrections (see, e.g., Miller, 1997, pp. 74–75).

Figure 34: Reform Policy Treatment Effects in SE3, Dispute Level[a]



(a) Average Change in Delay-to-Resolution under Reform Policy



(b) Average Change in Delay-to-Settlement under Reform Policy

[a]Solid dots illustrate observed average treatment effects (in seconds). Hollow black dots with a vertical connecting line represent 95% confidence intervals. Hollow gray dots with a vertical connecting line represent simultaneous 95% confidence intervals. The dashed line illustrates the no-effect hypothesis where imposition of a reform policy causes no change in average resolution delay. Red crosses illustrate the average treatment effects predicted by theory in Section 13.

With respect to delay-to-resolution, estimated average treatment effects are more attenuated than theory would predict, but only the Early Offers treatment is statistically distinguishable from its theoretic prediction. Estimated average treatment effects on delay-to-settlement are close to prediction for Early Offers and prejudgment interest treatments, but are farther from prediction for the damages limit and damages cap treatments. Imprecise estimation of average treatment effects combines with modest observed effects to produce a frustratingly ambivalent set of statistical inferences: most treatment effects are not obviously different than either the no-effect *or* predicted effect null hypotheses.

A reasonable question is whether such ambivalent results are an accurate reflection of underlying patters of behavior, or are a simple artifact of an experimental design that specifies treatment effects too modest to be easily detected in the data. Although the imprecision of treatment effect estimators is disquieting, results are probably *not* artificially limited by insufficiently extreme experimental treatments. Specified treatment effects are already about as extreme as realistically possible within the present experimental design.

The damages limit treatment, for example, is already close to the point where asymmetric information is so compacted that equilibrium strategies switch to those of a boundary solution.[130] The same is true of the reduction in plaintiff-verdict probability in the Early Offers treatment, and the damages cap treatment actually *does* predict a boundary solution. As discussed previously, the prejudgment interest reform policy is tied to an exogenous interest rate, and thus not directly manipulatable in extremity. Changes in delay-to-resolution such as the predict decrease of almost 12 seconds under the damages limit treatment or about 16 seconds under the Early

[130] Compare results when asymmetric information is manipulated directly in Chapter V, Sections 10.4 and 11.2.

Offers treatment are quite large compared to both predicted and average resolution delay under the control treatment.

In light of methodological limitations on the ability to specify more extreme treatment effects, as well as practical limitations on the policy-relevance of increasingly draconian reform rules, collected data are probably a reasonable reflection of actual patterns of behavior. The conclusion to be drawn from this aspect of SE3 is modestly negative: at least within the confines of the current laboratory setting, results fail to provide strong evidence that reasonable "reform" policy changes will affect large reductions in average delay.

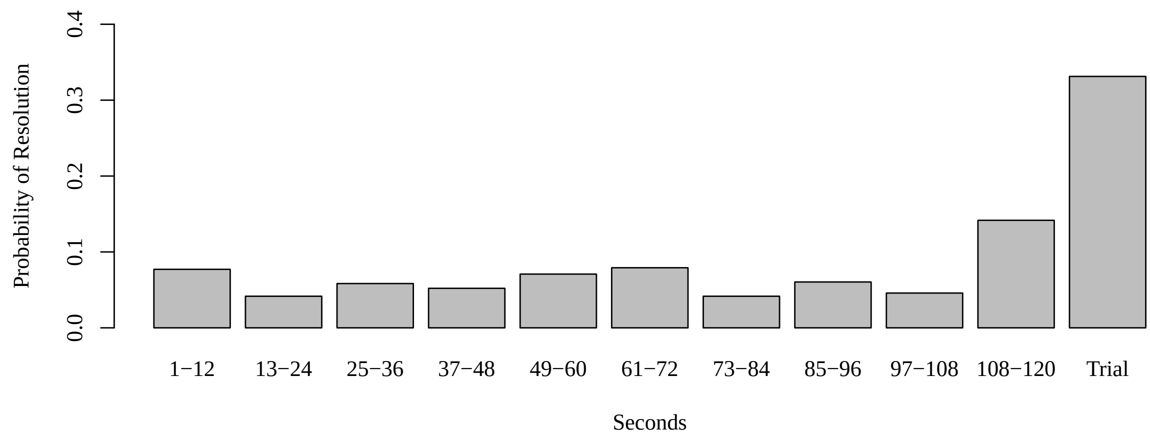
14.2 Distribution of Delay under Reform Policies

A question un-addressed in Section 14.1 is whether the distribution of resolution delay differs dramatically under different reform policies. Theoretic predictions about the effect of reform treatments on delay distributions are provided in Figures 31 and 32 of Section 13. With the exception of the damages cap treatment—for which reasonable distributional predictions are unavailable—reform policies in SE3 change the distribution of settlement delay by moving probability mass into or out of the event of a trial verdict; imposing these reform policies does not change the shape of resolution delay during settlement bargaining.

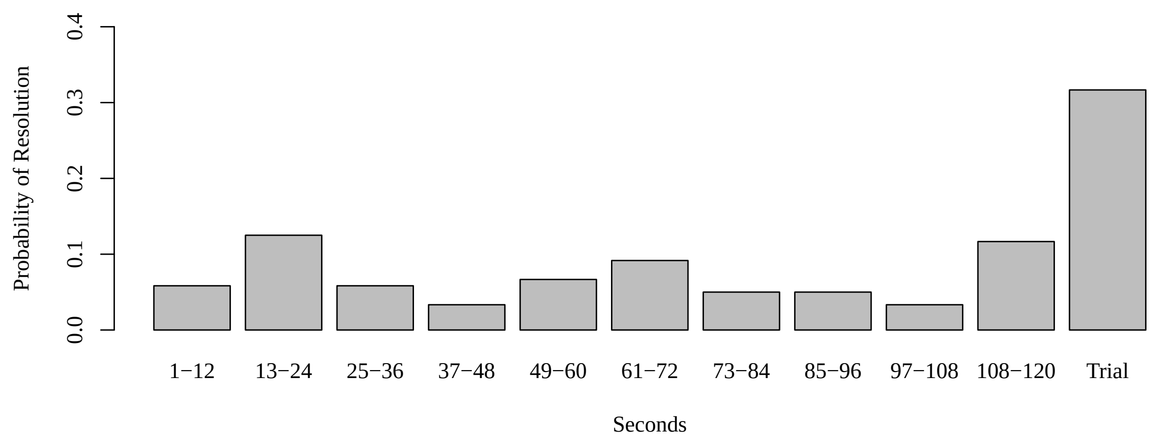
An empirical analogue of the theoretic predictions in Figure 31, Figure 35 illustrates the observed distribution of resolution delay under each SE3 treatment. Figure 35(a) shows resolution delay under the control treatment. Figures 35(b) through 35(e) show the same under the damages limit, damages cap, prejudgment interest, and Early Offers treatments, respectively. Though an accurate descriptor of delay in SE3, the inferential value of Figure 35 is questionable.[131]

[131] The interpretive caveats of n. 108 apply: due to the potential dependence of repeat observations

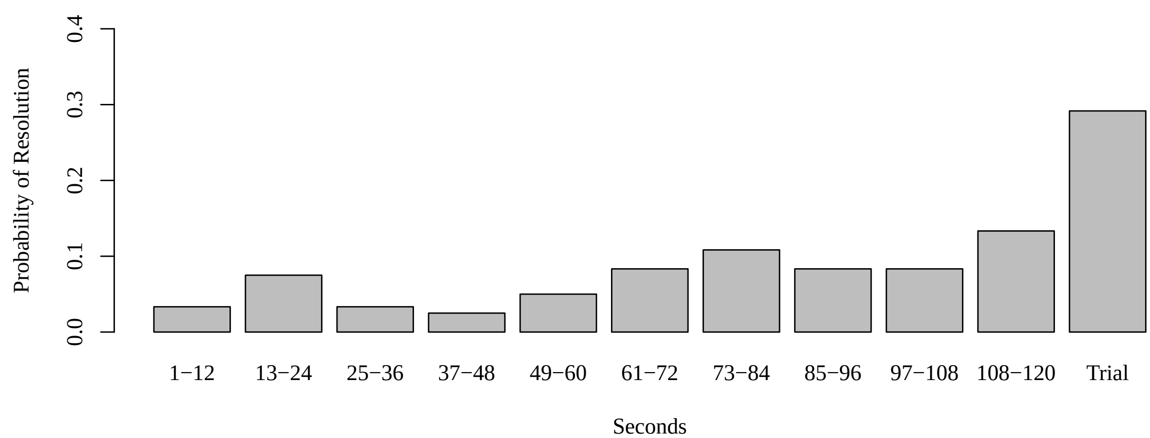
Figure 35: Observed Delay-to-Resolution Distributions in SE3



(a) Distribution of Resolution Delay (Control)

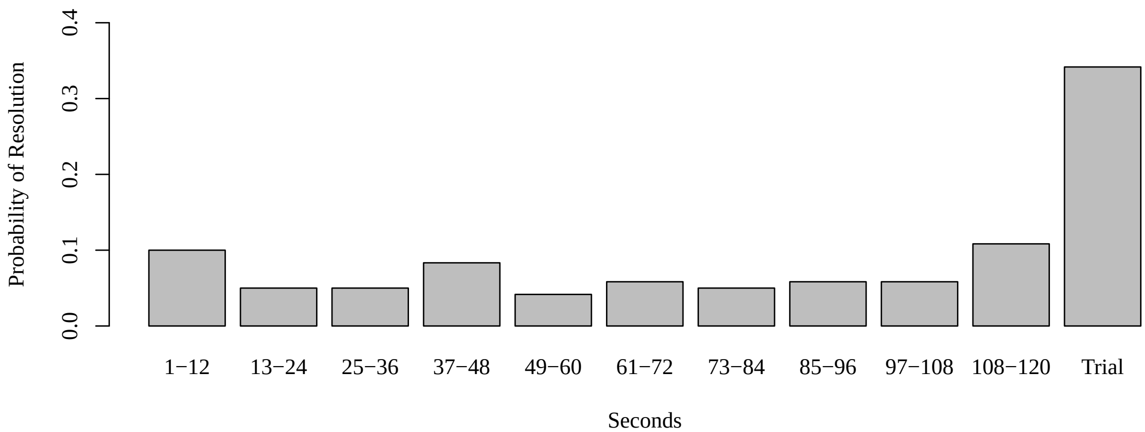


(b) Distribution of Resolution Delay under a Damages Limit

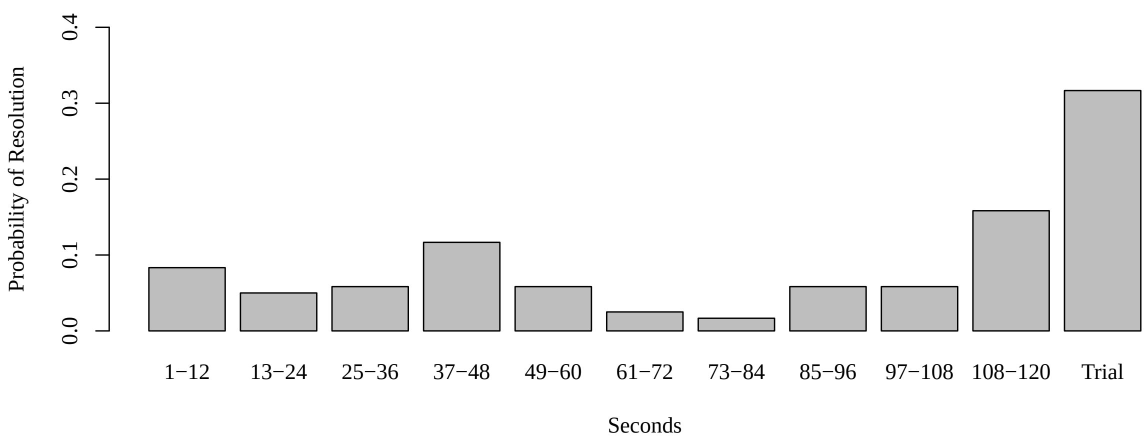


(c) Distribution of Resolution Delay under a Damages Cap Reform

Figure 35: Observed Delay-to-Resolution Distributions in SE3 (Cont...)



(d) Distribution of Resolution Delay under a Prejudgmennt Interest Rule



(e) Distribution of Resolution Delay under Early Offers Reform

Subject to a fair amount of noise, Figure 35 delay distributions appear roughly comparable across treatments. The distributional shapes for the control and prejudgment interest treatments are subjectively similar. Relative to these distributions, resolution delay under the damages cap reform policy seems to exhibit greater mass toward the end of the bargaining interval. The damages limit treatment seems to evidence a greater probability of early settlement, and Early Offers reform appears to induce a modest increase in the probability of settlement around the policy's prescribed cutoff-point for making an "early offer" ($e = 30$ seconds).

To provide inferential context, and as an alternative way to visualize distributional differences, Figure 36 illustrates estimated empirical hazard functions for all SE3 treatments.[132] The black curve illustrates the empirical hazard of settlement in the control treatment, with colored curves showing empirical hazards in each of the SE3 reform treatments. Gray vertical lines are simultaneous 95% confidence intervals on the control hazard. Details on the hazard function estimator and associated inference are provided in Appendix E.3

Consistent with the mild change in rules under the prejudgment interest policy, empirical hazards in this and the control treatment appear basically identical. Damages limit and Early Offers hazard functions fall outside the simultaneous confidence intervals on the control hazard during early bargaining (about 10–40 seconds into bargaining) in conformance with the previously noted tendency for greater early-in-dispute settlement under each reform policy. The apparently greater end-of-dispute probability of settlement under the damages cap policy is also visible in the empirical hazard illustrations: the damages cap estimated hazard function falls outside control hazard confidence intervals over the range of about 80–115 seconds into bargaining.

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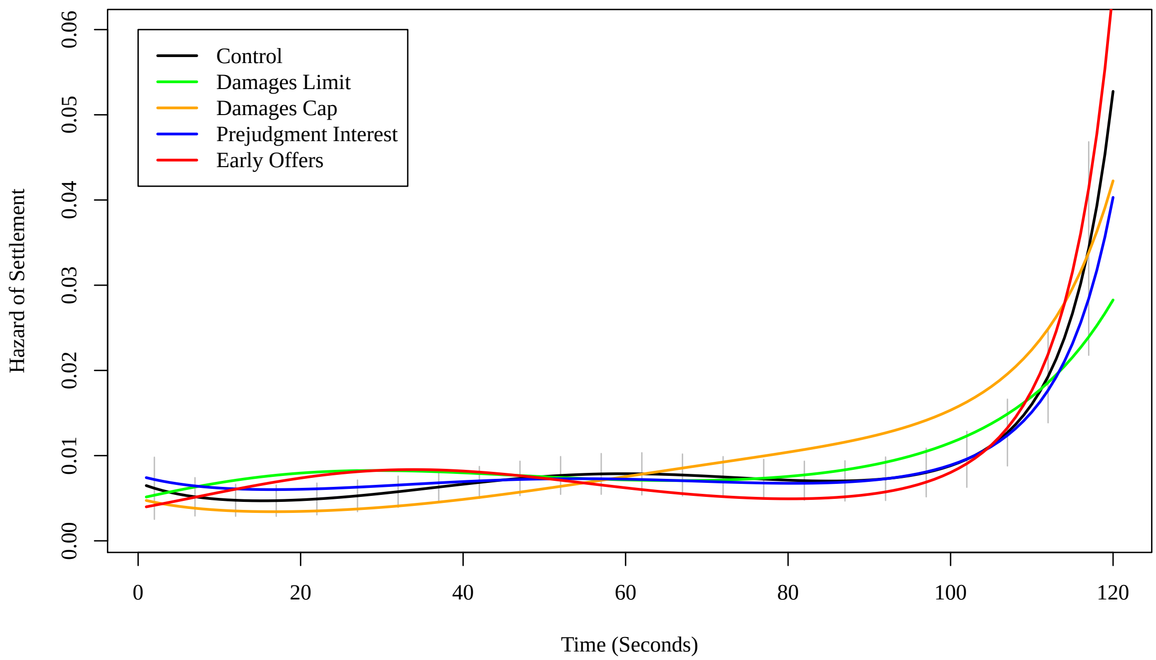
for many randomly matched pairs of litigants, statistical inferences are probably better reserved to the interpretation of empirical hazard functions in Figure 36.

[132] See Section 8.4 for the definition of a hazard function.

Figure 36: Estimated Hazard of Settlement by Reform Policy in SE3[a]



[a]Hazard rates for dispute resolution are illustrated up-to-but-excluding the final second of bargaining and the trial verdict phase: i.e. the illustration covers seconds 1 through 119 of a dispute. Truncating the illustration at 119 seconds improves legibility, as the sharp spike in the hazard function in the final second of bargaining swamps all other variability.

Empirical hazard functions for SE3 treatments do not obviously share a common shape, as predicted by theory. In fact, collected data seem to suggest modest differences in the frequency of disputes resolution when different reform policies are imposed on the control settlement bargaining environment: e.g. more early-round settlements under damages limit and Early Offers reform policies and more late-round settlement under imposition of a cap on damages. These apparent relationships are subject to considerable noise, and future work will be needed to determine whether and why such relationships actually exist.

14.3 Effect of Reform Policies on Wealth Distribution

Following assessment of average delay in Sections 14.1 and 14.2, a subsequent inquiry concerns the effects of various "tort reform" policies on the relative distribution of wealth between litigants. Wealth-distributive effects are relevant to the political feasibility of many reform policies, providing practical context for the types of policies considered in SE3. It should be emphasized, however, that the present study is agnostic about the welfare consequences of monetary transfers of wealth, and takes no normative position on the desirable distribution of wealth between litigants.

Similar to assessment of average delay, the identification strategy in this section is comparison of average delay between SE3 treatments. Differences in average earnings between control and reform treatments are attributable to the causal effect of implementing a given reform policy. Changes in earnings are assessed by role (plaintiff or defendant) and depend on a variety of factors: e.g. changes in average delay, changes in settlement proposal size, changes in trial verdict awards. Though negatively related through the structure of settlement bargaining, changes in earnings are not generally symmetric across roles (see Table 19).

Result 15. Several reform policies induce clear changes in the distribution of wealth between litigants.

A starting point is analysis of session-average earnings. Comparison of session-average earnings is exactly analogous to comparison of session-average delay in Section 14.1. Treatment effects are estimated by comparing the 4 session-average earnings measurements collected from a given reform treatment with the 12 session-average control treatment measurements collected from *other* SE3 treatment pairings. Session averages are independent and plausibly identically distributed (at least, within-treatment) as the products of a common data generating process.

Estimated session-average treatment effects and associated permutation tests are consolidated in Table 22. This is the empirical analog of Table 19, which presents *predicted* treatment effects on average earnings. Damages limit and Early Offers reform policies cause clear changes in earnings. Differences in earnings under a prejudgment interest policy are statistically distinguishable from zero when assessed individually, but not when tests are adjusted to account for simultaneous inference.[133] Interestingly, earnings do not obviously change when a cap is placed on potential damages.

As discussed previously, analysis is probably more appropriately conducted at the dispute level for the present experimental design. The independence and distributional properties of dispute-level earnings observations are the same as those for delay measurements discussed in Section 14.1. Results from several regressions of earnings on treatment indicators and random pair-effects are provided in Table 23. Columns 1 and 3 regress average earnings for the plaintiff and defendant, respectively, on a set of reform policy indicators (damages limit, damages cap, prejudgment interest, and Early Offers, with the control environment as reference). Columns 2 and 4

[133] Using the Hochberg (1988) algorithm to control the familywise error rate of all inferential tests in Table 22, changes in relative wealth under the prejudgment interest policy are statistically distinguishable from zero at only the 0.2 level.

Table 22: Observed Reform Policy Effect on Earnings in SE3, Session Level[a]

Treatment Comparison	Plaintiff	Defendant	Net Change
Control → Damages Limit	−\$30.18 0.0011**	\$33.35 0.0011**	\$3.17
Control → Damages Cap	−\$6.81 0.3791	\$6.32 0.3791	−\$0.49
Control → Prejudgment Interest	\$25.73 0.0297*	−\$24.80 0.05824†	\$0.93
Control → Early Offers	−\$22.61 0.0077**	\$23.30 0.0132*	\$0.69

[a] Comparison of earnings between session-average observations for each reform treatment and the control treatment of all other SE3 sessions: i.e. sample sizes $n = 4$ and $m = 12$. On top are average earnings differences (in experimental dollars). On bottom are exact p-values corresponding to application of Wilcoxon-Mann-Whitney permutation tests. Qualifiers $**$, $*$, and † denote significance from zero at levels 0.01, 0.05, and 0.1, respectively.

provide the same, but with two lags of average earnings for the plaintiff, $Y(p)$, and defendant, $Y(d)$, as additional controls.

Regression results in Table 23 are comparable to session-average results. The average treatment effect of each policy is most easily seen as the parameter estimate on the appropriate reform indicator in columns 1 and 3. The signs of all treatment effects are consistent with theory, but it is not immediately obvious how closely the size of observed treatment effects approximate theoretic predictions.

A convenient comparison of observed and predicted treatment effects is provided by Figure 34, which plots individual (black) and simultaneous (gray) confidence intervals for the average treatment effects of reform policies as characterized by columns 1 and 3 of Table 23.[134] A dashed black line represents the no-effect null hypothesis while red crosses illustrate theoretic predictions for each reform policy.

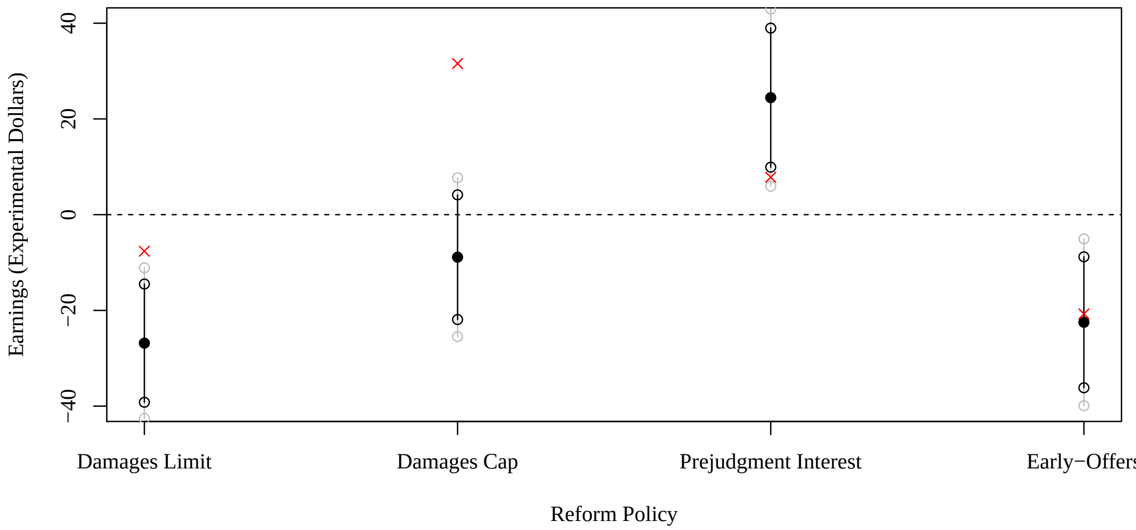
[134]Simultaneous intervals employ Bonferroni corrections.

Table 23: Regression of Earnings on Reform Policy in SE3, Dispute Level[a]

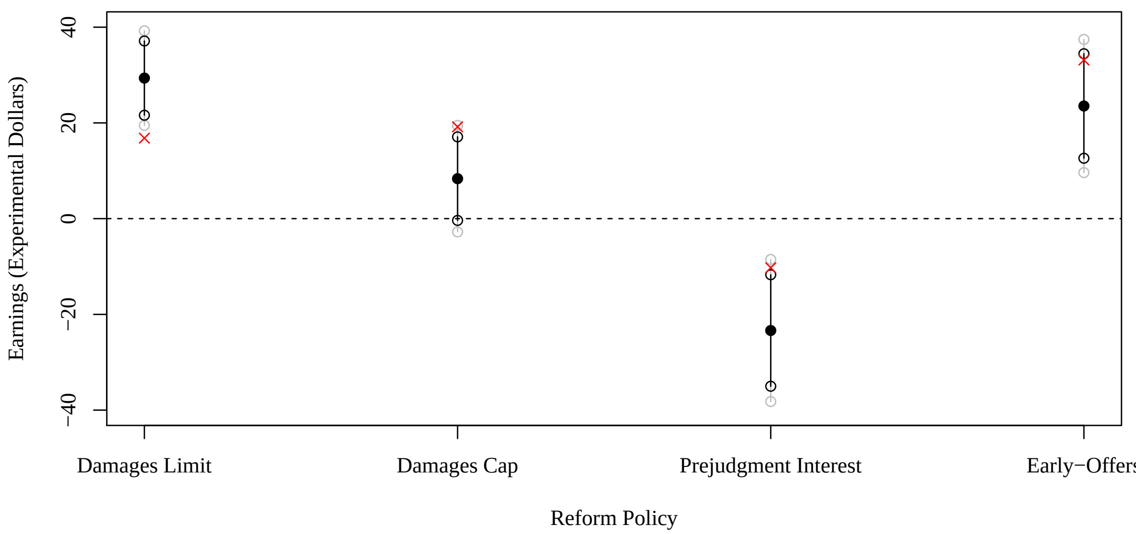
	Plaintiff		Defendant	
Parameter	(1)	(2)	(3)	(4)
Constant	237.872***	267.693***	199.471***	190.480***
	(7.4532)	(18.5382)	(4.4988)	(12.4678)
Damages Limit	−26.835***	−26.564***	29.362***	27.552***
	(6.3008)	(6.4509)	(3.9571)	(4.4639)
Damage Cap	−8.882	−8.764	8.358†	7.920†
	(6.6439)	(6.5250)	(4.4527)	(4.4871)
Prejudgment Interest	24.449**	24.397**	−23.364***	−21.676***
	(7.4108)	(7.9279)	(5.9427)	(6.0925)
Early Offers	−22.481**	−22.442**	23.536***	21.344***
	(6.9783)	(7.2440)	(5.5700)	(5.7691)
Lag(1) Y(p)		−0.121***		−0.048†
		(0.0333)		(0.0263)
Lag(2) Y(p)		0.031		0.034
		(0.0370)		(0.0300)
Lag(1) Y(d)		−0.173***		−0.053
		(0.0477)		(0.0354)
Lag(2) Y(d)		0.078		0.112**
		(0.0537)		(0.0374)
σ_ϵ^2	3424.86	3254.08	2013.59	1954.05
σ_η^2	153.29	150.35	218.68	176.10

[a] Parameter estimates from random pair-effects regression of earnings on treatment indicators and lagged dependent variables (Swamy and Arora, 1972). Values in parentheses are heteroskedasticity and cluster-robust standard errors (Arellano, 1987). Parameter estimates for fixed round-effects are omitted. Variances σ_η^2 and σ_ϵ^2 correspond to pair and idiosyncratic error terms, respectively. Qualifiers $***$, $**$, $*$, and † denote significance from zero at levels < 0.001, 0.01, 0.05, and 0.1, respectively.

Figure 37: Reform Policy Effects on Earnings in SE3, Dispute Level[a]



(a) Average Change in Plaintiff Earnings under Reform Policy



(b) Average Change in Defendant Earnings under Reform Policy

[a]Solid dots illustrate observed average treatment effects (in seconds). Hollow black dots with a vertical connecting line represent 95% confidence intervals. Hollow gray dots with a vertical connecting line represent simultaneous 95% confidence intervals. The dashed line illustrates the no-effect hypothesis where imposition of a reform policy causes no change in average earnings. Red crosses illustrate the average treatment effects predicted by theory in Table 19.

Result 16. Changes in relative earnings are statistically distinguishable from prediction for some reform policies, but few observed effects are too far from prediction.

Looking comprehensively at Figure 37, it is clear that observed effects on the relative distribution of wealth are not necessarily very different from prediction. In cases where simultaneous 95% confidence intervals do not contain the predicted change in earnings (e.g. the damages limit policy), the distance of the prediction from the endpoints of the associated confidence interval is small. One apparent anomaly in Figure 37 is the observed changes in plaintiff earnings when a cap is placed on potential damages. Because the damages limit prediction is based on a boundary solution equilibrium of dubious practical relevance, however, it is uncertain that this difference from prediction is analytically informative.

15 Discussion

Analysis in SE3 addresses the effects of various "tort reform" policies on settlement delay and the relative distribution of wealth between litigants. Results are summarized along these lines of inquiry. Section 15.1 discusses the uncertain capacity of reasonable reform policies to achieve large reductions in settlement delay. Section 15.2 notes the non-trivial effect of SE3 reform policies on the relative distribution of earnings between litigants.

15.1 Reducing Settlement Delay through Tort Reform

Analysis in SE3 is not optimistic about the capacity of reasonable policy changes to affect large reductions in average settlement delay. From a theoretic perspective, many reforms reduce average delay-to-resolution, but not delay-to-settlement. Put another way, these reforms affect reductions in average delay by moving probability mass away from the event of a trial verdict. As trial verdicts are an empirical rarity in the field, the practical relevance of this mode of delay reduction seems questionable. It is also noteworthy that the only SE3 reform with even a *theoretic* capacity to reduce delay-to-settlement is the damages cap policy—and this reduction in delay is achieved through inducement of a boundary solution of doubtful practical relevance.

Empirical results are no more favorable. This is largely attributable to imprecision in the estimation of SE3 reform policy treatment effects on delay. Despite specifying reform treatments with large predicted changes in delay-to-resolution, collected data are sufficiently noisy that no treatment effect is obviously different from either the predicted effect on delay or the no-effect null hypothesis. The distribution of delay looks generally similar across control and reform policies, though a few modest distributional differences are apparent.

Two important caveats are needed. First, failure of SE3 results to show strong evidence of reform policy efficacy in reducing settlement delay is emphatically *not* the same as finding strong evidence against this capacity. Future study—including the collection of additional data within the present experimental framework—may provide the needed precision to speak more informatively on the effects of the studied reform policies. Second, it should be carefully noted that SE3 results are specific to the studied specifications of each class of reform policy. Alternative specifications, especially of the highly flexible Early Offers reform policy, may yield larger or more obvious results in reducing settlement delay.

15.2 Reform Policy Effects on the Distribution of Wealth

In contrast to the general ambivalence of SE3 on the ability of reform policies to affect large changes in settlement delay, the effects of these reform policies on the relative distribution of wealth are quite clear. Non-trivial and generally one-sided effects on the distribution of wealth are not surprising, but do provide important practical context for the efficacy of reform policies in increasing economic efficiency. As a practical matter, a reform policy which achieves a modest decrease in settlement delay at the cost of a large redistribution of wealth may be a political non-starter.

An interesting question is whether reform policies which evenly burden both plaintiffs and defendants might be more politically palatable than those which burden one litigant to the benefit of the other. Rather than manipulate the distribution of damages, for example, a reform policy might induce more rapid settlement by artificially increasing the costs of settlement negotiation for both litigants evenly. A related inquiry is posed in SE2, Section 12.2.

G Technical Appendix

G.1 Proof of Proposition 5

Proof. The plaintiff's strategy is the same as that given in Proposition 1. In choosing whether to accept or reject settlement proposal S_1, the plaintiff's optimal strategy must be to reject the proposal if and only if the expected net present value of a trial verdict exceeds the value of settlement. That is, a plaintiff of type x cannot credibly reject a proposal of S_1 unless $U_p(S_1) < W_p(x)$. To break ties, assume a plaintiff indifferent between settlement and trial chooses to settle.

Lacking complete information about the value of potential damages, the defendant maintains a non-degenerate belief profile over the plaintiff's type. Under the above strategy, a plaintiff of type x rejects settlement if and only if a trial verdict is preferred to settlement. Expanding and rearranging the inequality $U_p(S_1) < W_p(x)$ reveals a rejecting plaintiff to have type $x > \pi^{-1}(\delta^{-1}S_1 + k_p)$. For notational convenience, let $\underline{x}_2(S_1) = \pi^{-1}(\delta^{-1}S_1 + k_p)$ denote the cutoff between the highest-type plaintiff that would just accept proposal S_1, and the lowest-type plaintiff that would just reject.

Let P_{DC} and E_{DC} denote probability and expectation operators associated with the capped distribution of potential damages, $F_{\mathrm{DC}}(x)$. The defendant's problem is to maximize, $V_d(S_1)$, the expected valuation of resolution following a proposal of S_1:

$$V_d(S_1) = \mathrm{P}_{\mathrm{DC}}[x \leq \underline{x}_2(S_1)]\, U_d(S_1) + \mathrm{P}_{\mathrm{DC}}[x > \underline{x}_2(S_1)]\, \mathrm{E}_{\mathrm{DC}}[W_d(x)|x > \underline{x}_2(S_1)] \quad (67)$$

The first term in equation (67) is the defendant's valuation of settlement at S_1 weighted by the measure of plaintiff types that accept S_1. The second term is the expected net present value of a trial verdict given that the plaintiff is a type that rejects S_1, weighted by the measure of types that reject S_1.

The Proposition 1 equilibrium to a one-period settlement bargaining game with asymmetric information is given for a general distribution of potential damages, depending critically on the full continuity of $F(x)$ only in the final simplifying statement of S_1^*. Interior and boundary solutions in Proposition 1 thus characterize equilibrium for a capped distribution of potential damages as well. There are two cases to consider: an interior solution in which not all types of plaintiff settle, and a boundary solution in which every type of plaintiff settles.

Start with the interior solution. Substituting the capped distribution of damages into the general result in Proposition 1 for S_1^I gives the following:

$$S_1^I: \quad -F_{\mathrm{DC}}(\pi^{-1}(\delta^{-1}S_1^I + k_p)) + \pi^{-1}(k_d + k_p)f_{\mathrm{DC}}(\pi^{-1}(\delta^{-1}S_1^I + k_p)) = 0. \qquad (68)$$

Note that the argument, $\pi^{-1}(\delta^{-1}S_1^I + k_p)$, is just the expansion of the cutoff type evaluated at the interior solution: $\underline{x}_2(S_1^I) = \pi^{-1}(\delta^{-1}S_1^I + k_p)$.

In an interior equilibrium, it must be that the cutoff type $\underline{x}_2(S_1^I)$ is less than $\widetilde{x}_{\mathrm{DC}}$. If this were not the case, then every type of plaintiff would settle (since $\widetilde{x}_{\mathrm{DC}}$ is the highest type in the post-reform distribution of potential damages) and the equilibrium could not be interior by definition. But since $F_{\mathrm{DC}}(x) = F(x)$ and $f_{\mathrm{DC}}(x) = f(x)$ for all $x < \widetilde{x}_{\mathrm{DC}}$, it follows that the interior solution given by (68) is exactly the solution that would have obtained with potential damages un-capped:

$$S_1^I: \quad -F(\pi^{-1}(\delta^{-1}S_1^I + k_p)) + \pi^{-1}(k_d + k_p)f(\pi^{-1}(\delta^{-1}S_1^I + k_p)) = 0. \qquad (69)$$

This is an intuitive result: imposing a cap on damages affects the far upper tail of the potential damages distribution, but has no effect on the distribution of potential damages at the interior margin.

Next consider the boundary solution. To make the highest-type plaintiff just indifferent between settlement and trial requires a settlement proposal S_1^B such that $U_p(S_1^B) = W_p(\widetilde{x}_{\mathrm{DC}})$. Expanding and rearranging gives the boundary solution:

$$S_1^B = \delta(\pi\widetilde{x}_{\mathrm{DC}} - k_p). \tag{70}$$

The equilibrium proposal depends on parameter values. When $V_d(S_1^I) \geq U_d(S_1^B)$, the defendant prefers the interior solution—balancing the marginal benefit of a lower settlement proposal against the marginal cost of more frequent trial outcomes—and accordingly proposes $S_1^* = S_1^I$. When $V_d(S_1^I) < U_d(S_1^B)$, litigation costs are sufficiently high that the defendant can do no better than to recoup such costs by settling with every type of plaintiff and so proposes $S_1^* = S_1^B$.

A distinction between equilibria with and without a cap on damages relates to the discontinuity in potential damages introduced by a cap. The discontinuity in $F_{\mathrm{DC}}(x)$ at $\widetilde{x}_{\mathrm{DC}}$ leads to a corresponding discontinuity in $V_d(S_1)$ at S_1^B. This is easy to see when $V_d(S_1)$ is expressed in terms of the un-capped distribution of potential damages:

$$\begin{aligned} V_d(S_1) = {} & \mathrm{P}\left[x \leq \underline{x}_2(S_1)\right] U_d(S_1) \\ & + \mathrm{P}\left[\underline{x}_2(S_1) < x < \widetilde{x}_{\mathrm{DC}}\right] \mathrm{E}\left[W_d(x) | \underline{x}_2(S_1) < x < \widetilde{x}_{\mathrm{DC}}\right] \\ & + \mathrm{P}\left[x \geq \widetilde{x}_{\mathrm{DC}}\right] W_d(\widetilde{x}_{\mathrm{DC}}) \end{aligned} \tag{71}$$

The first term in equation (71) is the defendant's probability-weighted valuation of settlement at S_1. The second term is the probability-weighted expected net present value of a trial verdict given that the plaintiff rejects S_1 but has type less than $\widetilde{x}_{\mathrm{DC}}$. The third terms is the expected net present value of a trial verdict given that a plaintiff is at the cap-point, $\widetilde{x}_{\mathrm{DC}}$, weighted by the probability mass of this type. This third term demonstrates the discontinuity in preferences. For any value of $S_1 < S_1^B$, the interior equilibrium involves a positive, additive, and proposal-invariant term $\mathrm{P}\left[x \geq \widetilde{x}_{\mathrm{DC}}\right] W_d(\widetilde{x}_{\mathrm{DC}}) > 0$.

The discontinuity in $V_d(S_1)$ at S_1^B destroys the simplified expression for S_1^* in Proposition 1: i.e. that $S_1^* = \min\{S_1^I, S_1^B\}$. Following imposition of a cap on damages, the most concise statement of S_1^* is that given previously:

$$S_1^* = \begin{cases} S_1^I & V_d(S_1^I) \geq U_d(S_1^B) \\ S_1^B & V_d(S_1^I) < U_d(S_1^B). \end{cases} \tag{72}$$

□

G.2 Proof of Proposition 6

Proof. Equilibrium strategies for a game of length $T > 1$ follow easily from Proposition 5. The following argument correspondingly errs on the side of brevity.

As established in Proposition 2, there are only two cases to consider: an interior solution in which not all types of plaintiff settle, and a boundary solution in which all types of plaintiff settle immediately. The following demonstrates that the interior solution is equivalent to that given in Proposition 2 for an un-capped distribution of potential damages, and that the boundary solution is the same as that of Proposition 2 but with $\widetilde{x}_{\mathrm{DC}}$ substituted for $\overline{x}$.

Start with the interior solution. By definition the final period of settlement bargaining is reached with positive probability. The continuation game starting in the final period of bargaining is just a game of length $T = 1$, so equilibrium strategies in this period are given by Proposition 5. In an interior solution, the equilibrium proposal dictated by Proposition 5 is the same as when no damage cap is applied.

By Lemma 1, the plaintiff must be indifferent between all equilibrium settlement proposals $S_1^I, \ldots, S_T^I$, and since S_T^I is the same with and without a cap on potential damages, it follows that the entire sequence of settlement proposals must be the same in either case. Since the timing of plaintiff settlement is a deterministic function of the sequence of settlement proposals (see proof of Proposition 2), it follows that the interior solution under imposition of a damages cap is exactly that given by Proposition 2 for an un-capped distribution of potential damages.

Next consider the boundary solution. If all types of plaintiff eventually settle, then by Lemma 1, the defendant can do no better than to propose S_1^B just high enough to make the highest-type plaintiff indifferent between settlement and trial: $U_p(S_1^B) = W_p(\widetilde{x}_{\mathrm{DC}})$. Expanding and rearranging gives the boundary solution:

$$S_1^B = \delta^T(\pi\widetilde{x}_{\mathrm{DC}} - k_p) - c_p \sum_{i=1}^{T-1} \delta^i. \tag{73}$$

Whether equilibrium involves the interior or boundary solution depends on which is preferable to the defendant. The value of the boundary solution is simply $U_d(S_1^B)$. Let the interior solution proposal and settlement-timing sequences $S_1^I, \ldots, S_T^I$ and $\underline{x}_1, \ldots, \underline{x}_{T+1}$ be defined by Corollary 1. The value of the interior solution, $V_d(S_1^I)$, is as follows:

$$\begin{aligned} V_d(S_1^I) = &\sum_{i=1}^{T} \frac{\underline{x}_{t+1} - \underline{x}_t}{\overline{x} - \underline{x}} U_d(S_t^I) \\ &+ \frac{\widetilde{x}_{\mathrm{DC}} - \underline{x}_{T+1}}{\overline{x} - \underline{x}} \left(-\delta^T \left(\pi \frac{\underline{x}_{T+1} + \widetilde{x}_{\mathrm{DC}}}{2} - k_d \right) - c_d \sum_{i=1}^{T} \delta^{i-1} \right) \\ &+ \frac{\overline{x} - \widetilde{x}_{\mathrm{DC}}}{\overline{x} - \underline{x}} \left(-\delta^T (\pi\widetilde{x}_{\mathrm{DC}} - k_d) - c_d \sum_{i=1}^{T} \delta^{i-1} \right). \end{aligned} \tag{74}$$

The first term in equation (74) is the sum of the defendant's probability-weighted valuations of settlement at $S_1^I, \ldots, S_T^I$. The second term is the probability-weighted expected net present value of a trial verdict given that the plaintiff rejects S_1 but has type less than $\widetilde{x}_{\mathrm{DC}}$. The third terms is the expected net present value of a trial verdict given that a plaintiff is at the cap-point, $\widetilde{x}_{\mathrm{DC}}$, weighted by the probability mass of this type.[135]

The equilibrium proposal depends on parameter values. When $V_d(S_1^I) \geq U_d(S_1^B)$, the defendant prefers the interior solution—balancing the marginal benefit of a lower settlement proposal against the marginal cost of more frequent trial outcomes—and accordingly proposes $S_1^* = S_1^I$. When $V_d(S_1^I) < U_d(S_1^B)$, litigation costs are sufficiently high that the defendant can do no better than to recoup such costs by settling with every type of plaintiff and so proposes $S_1^* = S_1^B$. A consolidated expression for S_1^* is as follows:

$$S_1^* = \begin{cases} S_1^I & V_d(S_1^I) \geq U_d(S_1^B) \\ S_1^B & V_d(S_1^I) < U_d(S_1^B). \end{cases} \tag{75}$$

□

G.3 Proof of Proposition 7

Proof. Recall that the Early Offers condition for reduction in the probability of a plaintiff-verdict is as follows:

$$\max\{S_1, \ldots, S_e\} \geq x_{\mathrm{E}}. \tag{76}$$

Suppose that some proposal in the sequence of equilibrium settlement-proposals $S_1^*, \ldots, S_T^*$ satisfies the Early Offers condition. Since the sequence of equilibrium

[135]Note that unlike the corresponding equation in Proposition 5, there is no indicator function attached to the third term in equation (74). The indicator function would be redundant, as equation (74) is limited to evaluation of interior equilibria.

proposals is guaranteed to be satisfied at some point prior to the trial verdict (by assumption), the effective probability of a plaintiff-verdict (if a dispute were to be resolved in trial) is π_{EO} at *every* instance during settlement bargaining.

Subject to satisfaction of the Early Offers condition along the equilibrium path, the model of settlement bargaining is thus the control model, but with π_{EO} in place of π. Since the problem is the same as that in Section 3.2 of Chapter II, the solution must also be the same. Equilibrium is thus characterized by Propositions 1 and 2, but with π_{EO} substituted for π.

Now consider conditions under which the equilibrium settlement proposal sequence satisfies the Early Offers condition. For notational clarity, let $S_t^*(\pi)$ denote the equilibrium settlement proposal in period t of a model with probability π of a plaintiff-verdict; allow analogous notation for other periods and interior/boundary solutions. In an interior solution, the Early Offers condition is satisfied when

$$S_e^I(\pi_{\text{EO}}) \geq x_E. \tag{77}$$

As all equilibrium settlement proposals provide equal net present value to the plaintiff in an interior solution (Lemma 1), an equivalent expression for equation (77) is

$$U_p(S_1^I(\pi_{\text{EO}})) \geq U_p(S_e = x_E), \tag{78}$$

which is consistent with the proposed condition for the equilibrium.

Now consider a boundary solution. Sufficient for satisfaction of the Early Offers condition is a boundary proposal

$$S_1^B(\pi_{\text{EO}}) \geq x_E. \tag{79}$$

Note, however, that proposal $S_1^B(\pi_{\text{EO}})$ need not itself satisfy the Early Offers condition so long as equilibrium play in the continuation game following rejection of $S_1^B(\pi_{\text{EO}})$ would eventually lead to satisfaction of the condition. Since the sequence of settlement proposals must satisfy $U_p(S_1^*) \geq \ldots \geq U_p(S_T^*)$ in any equilibrium (Lemma 1), it follows that a necessary and sufficient condition for satisfaction of the Early Offers condition is

$$U_p(S_1^B(\pi_{\text{EO}})) \geq U_p(S_e = x_E), \tag{80}$$

which is also consistent with the proposed condition for the equilibrium.

□

G.4 Sketch of Candidate Equilibria under Early Offers

When combined with the need to account for possible boundary and interior solutions within every candidate equilibrium, the discontinuity in the defendant's problem introduced by Early Offers reform makes specifying the exact conditions under which various equilibria obtain a remarkably tedious task. Rather than devote several pages to the exercise, this section attempts to provide high-level intuition for possible effects of imposing Early Offers rules through an informal sketch of several interesting candidate equilibria.

One candidate equilibrium involves a sequence of settlement proposals which fail to satisfy the Early Offers condition for reduction in the probability of a plaintiff-verdict. For sufficiently large values of x_E, the defendant simply does better by ignoring the reform-option altogether. Equilibrium strategies are correspondingly characterized by Propositions 1 and 2 of Chapter II.

A second interesting candidate equilibrium is that described in Proposition 7. For sufficiently low values of x_E, the defendant does better by proposing settlement at a value above the Early Offers requirement. The defendant's optimization is the same as in Propositions 1 and 2, but with π_{EO} substituted for π because the Early Offers condition is sure to by satisfied under equilibrium play.

A third interesting candidate equilibrium results from the discontinuity in the defendant's optimization when $S_e = x_E$. Let $W_p(\overline{x}, \pi_{\text{EO}})$ denote the net present value of a trial verdict to a plaintiff of type $\overline{x}$ with the reduced probability of a plaintiff-verdict. For intermediate values of x_E satisfying the condition that $x_E \geq W_p(\overline{x}, \pi_{\text{EO}})$, a *discontinuity solution* may result in which $S_1^* = x_E$, and this proposal is accepted by all types of plaintiff.[136] That is, the discontinuity equilibrium occurs where the Early Offers condition is just satisfied *and* all types of plaintiff settle.

Importantly, there does not generally exist such a discontinuity equilibrium in which fewer than the full support of plaintiff types settle. To see why, note that any such equilibrium would be characterized by the Lemma 1 requirement that $U_p(S_1^I) = \ldots, U_p(S_T^I)$. Since the equilibrium must pass through $S_e = x_E$, it follows that $U_p(S_t^I) = U_p(S_e = x_E)$ for all $t = 1, \ldots, T$. But since it is certain that the Early Offers condition will be satisfied by construction, the defendant's problem at the start of a given continuation game is exactly the same as that described in Proposition 7: i.e. the defendant's standard problem, but with π_{EO} substituted for π. Except in the special case where parameter values simply happen to specify $U_p(S_1^I) = U_p(S_e = x_E)$, it follows that no discontinuity equilibrium can exist where fewer than all types of plaintiff settle.

[136] This is a sufficient condition for the equilibrium. See Appendix G.3 for the logic behind a necessary and sufficient condition.

H Instructions Appendix

H.1 Example Instructions for Reform Treatments

Instructions in reform treatments $\mathbf{T}_{10}, \ldots, \mathbf{T}_{13}$ are nearly identical to instructions for the control treatment (see Appendix C). Differences from control instructions are limited to the page 5 description of damages awards from a trial verdict. Example instructions follow for each reform treatment when assigned second in a sequence. First-assignment instructions are the same, but without red highlighting.

Screenshot 13: Example Instructions $\mathbf{T}_B = \mathbf{T}_{10}$: Page 5 of 6

Instructions Part II (Page 5 of 6): Trial

If you and the **[other party]** have not agreed on an acceptable settlement amount by the end of **2 minutes**, the lawsuit goes to trial where a judge **decides who wins the case**. Going to trial is **costly**. It costs the plaintiff **$11.00** and the defendant **$5.00**. Trial costs are paid at the end of the round (without interest) in addition to accumulated negotiation costs. Rules of the trial follow:

- There is always a **75%** chance that the plaintiff will win at trial, and a **25%** chance that the plaintiff will lose.
- If the plaintiff wins, the defendant is ordered to pay the exact amount of the plaintiff's economic injury, plus **73.33%** of the platinff's pain and suffering injury (i.e. a total amount between **$50.00** and **$160.00**). If the plaintiff loses, no payment is ordered.

Interactive Example:

- Click the following button a few times to see example potential damages: Generate Example

Economic Injury	+ (0.7333 *	Pain and Suffering)	=	Potential Damages
$50.00	+ (0.7333 *	91.31)	=	$116.96

Although no one knows who would win if the case went to trial, the plaintiff does know the exact amount that could be won at trial. (This is because only the plaintiff knows the size of his/her pain and suffering injury in a given round.)

To Summarize:

- Going to trial costs the plaintiff **$11.00** and the defendant **$5.00**.
- There is always a **75%** chance that the plaintiff will win at trial, and a **25%** chance that the plaintiff will lose.
- If the plaintiff wins at trial, the defendant is ordered to pay the exact amount of the plaintiff's economic injury, plus **73.33%** of the platinff's pain and suffering injury (i.e. a total amount between **$50.00** and **$160.00**). If the plaintiff loses, no payment is ordered.
- Neither party knows who would win a trial, but the plaintiff does know exactly how much could be won.

Screenshot 14: Example Instructions $\mathbf{T}_B = \mathbf{T}_{11}$: Page 5 of 6

Instructions Part II (Page 5 of 6): Trial

If you and the **[other party]** have not agreed on an acceptable settlement amount by the end of **2 minutes**, the lawsuit goes to trial where a judge **decides who wins the case**. Going to trial is **costly**. It costs the plaintiff **$11.00** and the defendant **$5.00**. Trial costs are paid at the end of the round (without interest) in addition to accumulated negotiation costs. Rules of the trial follow:

- There is always a **75%** chance that the plaintiff will win at trial, and a **25%** chance that the plaintiff will lose.
- If the plaintiff wins, the defendant is ordered to pay the exact amount of the platinff's total injury up to a maximum of **$160.00**: i.e. if the plaintiff's total injury **exceeds $160.00**, then the defendant only has to pay **$160.00**. If the plaintiff loses, no payment is ordered.

Interactive Example:

- Click the following button a few times to see example potential damages: Generate Example

Smaller of {	Total Injury	and $160.00 }	=	Potential Damages
Smaller of {	$179.35	and $160.00 }	=	$160.00

Although no one knows who would win if the case went to trial, the plaintiff does know the exact amount that could be won at trial. (This is because only the plaintiff knows the size of his/her pain and suffering injury in a given round.)

To Summarize:

- Going to trial costs the plaintiff **$11.00** and the defendant **$5.00**.
- There is always a **75%** chance that the plaintiff will win at trial, and a **25%** chance that the plaintiff will lose.
- If the plaintiff wins at trial, the defendant is ordered to pay the exact amount of the platinff's total injury up to a maximum of **$160.00**: i.e. if the plaintiff's total injury **exceeds $160.00**, then the defendant only has to pay **$160.00**. If the plaintiff loses, no payment is ordered.
- Neither party knows who would win a trial, but the plaintiff does know exactly how much could be won.

Screenshot 15: Example Instructions $\mathbf{T}_B = \mathbf{T}_{12}$: Page 5 of 6

Instructions Part II (Page 5 of 6): Trial

If you and the **[other party]** have not agreed on an acceptable settlement amount by the end of **2 minutes**, the lawsuit goes to trial where a judge **decides who wins the case**. Going to trial is **costly**. It costs the plaintiff **$11.00** and the defendant **$5.00**. Trial costs are paid at the end of the round (without interest) in addition to accumulated negotiation costs. Rules of the trial follow:

- There is always a **75%** chance that the plaintiff will win at trial, and a **25%** chance that the plaintiff will lose.
- If the plaintiff wins, the defendant is ordered to pay the exact amount of the platinff's injury plus interest accrued on that amount since the beginning of the round: depending on the plaintiff's injury, the defendant will be ordered to pay an amount between **$56.32** (if the plaintiff's injury was **$50.00**) and **$225.26** (if the plaintiff's injury was **$200.00**). If the plaintiff loses, no payment is ordered.

Interactive Example:

- Click the following button a few times to see example potential damages: Generate Example

Total Injury	+	Interest on Injury	=	Potential Damages
$153.20	+	$19.35	=	$172.56

Although no one knows who would win if the case went to trial, the plaintiff does know the exact amount that could be won at trial. (This is because only the plaintiff knows the size of his/her pain and suffering injury in a given round.)

To Summarize:

- Going to trial costs the plaintiff **$11.00** and the defendant **$5.00**.
- There is always a **75%** chance that the plaintiff will win at trial, and a **25%** chance that the plaintiff will lose.
- If the plaintiff wins at trial, the defendant is ordered to pay the exact amount of the platinff's injury plus interest accrued on that amount since the beginning of the round: depending on the plaintiff's injury, the defendant will be ordered to pay an amount between **$56.32** (if the plaintiff's injury was **$50.00**) and **$225.26** (if the plaintiff's injury was **$200.00**). If the plaintiff loses, no payment is ordered.
- Neither party knows who would win a trial, but the plaintiff does know exactly how much could be won.

Screenshot 16: Example Instructions $\mathbf{T}_B = \mathbf{T}_{13}$: Page 5 of 6

Instructions Part II (Page 5 of 6): Trial

If you and the **[other party]** have not agreed on an acceptable settlement amount by the end of **2 minutes**, the lawsuit goes to trial where a judge **decides who wins the case**. Going to trial is **costly**. It costs the plaintiff **$11.00** and the defendant **$5.00**. Trial costs are paid at the end of the round (without interest) in addition to accumulated negotiation costs. Rules of the trial follow:

- By default, there is a **75%** chance that the plaintiff will win at trial, and a **25%** chance that the plaintiff will lose.
- The chance that the plaintiff will win at trial drops from **75%** to **50%** if the defendant ever makes a settlement proposal that satisfies both of the following requirements:
  - The proposal must be at least as large as the plaintiff's economic injury of **$50.00**.
  - The proposal must be made within the first **30 seconds** of bargaining.

  Note: if a defendant makes an proposal that qualifies for the reduced chance of the plaintiff winning at trial, no subsequent settlement proposal can be made for less than **$50.00** for the rest of the round.
- If the plaintiff wins, the defendant is ordered to pay the exact amount of the platinff's total injury (i.e. an amount between **$50.00** and **$200.00**). If the plaintiff loses, no payment is ordered.

Interactive Example:

- Click the following button a few times to see example potential damages: Generate Example

Total Injury	=	**Potential Damages**
$150.30	=	**$150.30**

Although no one knows who would win if the case went to trial, the plaintiff does know the exact amount that could be won at trial. (This is because only the plaintiff knows the size of his/her pain and suffering injury in a given round.)

To Summarize:

- Going to trial costs the plaintiff **$11.00** and the defendant **$5.00**.
- By default, there is a **75%** chance that the plaintiff will win at trial, and a **25%** chance that the plaintiff will lose.
- The chance that the plaintiff will win at trial drops from **75%** to **50%** if the defendant ever makes a settlement proposal that satisfies both of the following requirements:
  - The proposal must be at least as large as the plaintiff's economic injury of **$50.00**.
  - The proposal must be made within the first **30 seconds** of bargaining.
- If the plaintiff wins at trial, the defendant is ordered to pay the exact amount of the platinff's total injury (i.e. an amount between **$50.00** and **$200.00**). If the plaintiff loses, no payment is ordered.
- Neither party knows who would win a trial, but the plaintiff does know exactly how much could be won.

Chapter VII

Discussion

The preceding chapters comprise a detailed experimental analysis of settlement bargaining and delay when litigants are asymmetrically informed about the value of a potential trial verdict. Experimental results are categorized by sub-experiment (SE). SE1 compares experimental results to the predictions of an important theoretic model of settlement delay. SE2 uses collected data to confirm the causal effect of asymmetric information on settlement delay. SE3 compares observed results under a variety of promising "tort reform" policies in an effort to determine whether reasonable changes to tort policy can affect substantial reductions in settlement delay. This chapter provides brief concluding remarks, summarizing important aspects of observed behavior and commenting on the design of this and future studies.

The remainder of this chapter proceeds as follows. Section 16 discusses implications of the present study. Commentary is divided between implications for academic research and real-world policy. Section 17 discusses noteworthy limitations of the present study. Obstacles to external validity are discussed, along with alternative conceptions of economic efficiency in tort disputes. Finally, Section 18 comments on potential extensions to the present study. Relaxation of the present bargaining structure is recommended, and different types of future replications are discussed. Additional details on much of the material in this chapter can be found in the discussion sections of Chapters IV through VI.

16 Implications

Specific implications of each sub-experiment are provided in the discussion sections of Chapters IV through VI. A summary of basic conclusions can be neatly categorized according to the two broad research questions posed in Section 2.1. Section 16.1 addresses the first research question, relating to the academic value of the present study. Section 16.2 addresses the second research question, regarding the policy implications of results.

16.1 Academic Study

Research Question 1 asks whether asymmetric information over a potential trial verdict can plausibly contribute to the protracted delay observed in tort disputes in the field. Taken as an experimental proof of concept, the results of the present study strongly suggest that it can. This observation has several implications for the academic study of settlement delay.

The affirmation that asymmetric information causes a substantial increase in settlement delay confirms the basic hypothesis of a sizable theoretic literature on the topic. Although experimental results are tailored to asymmetric information over a potential trial verdict, the underlying principle generalizes to a range of asymmetric information models. The unmistakable causal effect of asymmetric information on delay also contributes a strong affirmative finding to the thus far indeterminate experimental economics literature on the capacity of asymmetric information to explain delayed agreement in the context of a variety of general bargaining models (see Section 1.2).

Validation of many predictions of the Spier (1989, 1992) "pre-trial" model of settlement bargaining with asymmetric information provides further confidence in the utility of this important theoretic model. This is of interest to a number of empirical studies which cite Spier's model for the proposition that asymmetric information over a trial verdict leads to delayed resolution with predictable properties. It should be remembered, however, that not all predictions of the Spier model are obviously consistent with observed behavior. For example, the timing of settlement by injury tracks poorly against prediction. Results also raise the disturbing question whether theory adequately describes the behavior of individual litigants in a given dispute, or only average behavior across many litigants in many disputes.

It is likewise noteworthy that even under the ideal conditions of an abstract laboratory experiment, exposure to a controlled information asymmetry is only found to explain *part* of observed settlement delay. Results thus recommend theoretic study of answers to the settlement delay puzzle beyond the well-traveled asymmetric information hypothesis. As noted in Section 12.1, my own impression is that principal-agent problems and regret avoidance are worthy candidates for future study.

16.2 Policy

Research Question 2 asks whether specific policies can be identified which might mitigate the settlement delay caused by asymmetric information over trial verdicts. The results of SE2 are not optimistic on this point. While exposure to asymmetric information clearly causes a substantial increase in settlement delay, the increase in delay is not obviously responsive to marginal changes in the degree of information asymmetry. Similarly pessimistic observations are made in the analysis of SE3. Imposing a variety of promising "tort reform" policies fails to obviously achieve substantial reductions in average settlement delay in the laboratory.

Particularly when read with an eye toward policy, it is important that these findings be interpreted correctly. Failure to find strong evidence of an effect on settlement delay is not the same as an affirmative finding of strong evidence against such effects. Results are also limited to the range of reform policies explored in the present experiment. It remains to future research to determine whether additional data or alternative treatments will suffice to establish the clear evidence of efficacy in reducing delay that is wanting in the present study.

It should also be noted that the present study takes the posture of a proof-of-concept. While it is firmly concluded that asymmetric information *can* induce substantial settlement delay, nothing in the present study speaks to the empirical question whether asymmetric information *does* cause delayed resolution in the field. An important initial step in reliance on this study for policy purposes is determination of the existence and extent of trial-verdict-relevant informational asymmetries in tort disputes in the field.

Finally, it is worth reiterating that the present study finds substantial settlement delay even in the absence of asymmetric information over a potential trial verdict. While information asymmetries are an important consideration in seeking to reduce the average delay to dispute resolution, too narrow a focus on asymmetric information may be to the detriment of effective policy development.

17 Limitations

Accurate interpretation of experimental results requires attention to both the strengths and weaknesses of experimental design and analysis. This section comments on two noteworthy limitations of the present study. Section 17.1 discusses areas in which the external validity of the experiment may be questionable. Section 17.2 raises the thus far unaddressed issue of dynamic efficiency and its interaction with the present study's focus on minimizing resolution costs.

17.1 External Validity

An ubiquitous concern in the conduct of laboratory experiments is external validity: the ability of behavior in an abstract laboratory setting to reasonably approximate actual behavior in the field. The measure of external validity is rarely full replication of the institution being studied. For example, it would be infeasible for the present experiment to fully reproduce every potential nuance of settlement bargaining in the field. Rather, the hope is that appropriate abstraction and modeling choices will permit treatment effects in the laboratory to identify the signs and possibly relative magnitudes of patterns of behavior in the field.

The present experimental design includes several attempts to support and control for external validity. For example, the use of continuous-time settlement bargaining is motivated by the recognition that period granularity is important to bargaining behavior (see, e.g., Güth et al., 2005; Friedman and Oprea, 2009), and that legal bargaining involves no obvious constraints which might support a more discrete model of bargaining.[137] Various aspects of the experimental framework such as the use of interest accrual to represent inter-temporal discounting and subtraction of injuries

[137] See discussion in Section 6.1.4, particularly n. 65.

from a plaintiff's income are similarly motivated by a desire to accurately reproduce as many aspects of settlement bargaining as possible. Explicit experimental controls for external validity include a parallel experiment conducted with law students in SE2 to test the specificity of results to the undergraduate student sub-population.

Despite efforts to support external validity, a few important limitations should be noted. A particularly troubling obstacle is the inability of laboratory experiments to adequately model the emotional nature of many tort disputes in the field. Discussions with legal scholars and practicing attorneys convince me that painful injuries, hurt feelings, anger, desire for revenge, and the pursuit of moral vindication are important impediments to the rapid settlement of many tort disputes in the field. While I see no obvious reason to think that these emotional factors would interact with asymmetric information to bias experimental results, neither can I present a very convincing argument to the contrary.[138]

A second important limitation is the present experiment's reliance on a stylized model of settlement bargaining in which asymmetric information is one-sided and only the relatively uninformed litigant is able to make settlement proposals. As discussed in Section 18.1, this design seems plausible as a model of many tort disputes in the field and has the practical advantage of providing determinate theoretic predictions for behavior in the experiment. The cost of imposing this structure on bargaining is the potentially low validity of experimental results in describing patterns of behavior for disputes to which these assumptions are inapposite. While the structure of concatenated ultimatum offers lacks the fluidity and communication-potential of settlement bargaining in the field, it is presently unclear how results might differ under more flexible rules.

[138]There is a growing psychological literature on the effect of emotions on perception and behavior (see, e.g., Norman, 2004). A deep interaction between emotion, cognition, and decision frustrates efforts to convincingly distinguish between factors contributing to settlement delay.

A third limitation of the present analysis is its narrow focus on only the subset of obviously credible tort disputes.[139] Although fairly standard in the literature (see, e.g., Bebchuk, 1984; Spier, 1989, 1992), this restriction is a limitation in the sense that results are of uncertain validity in describing tort disputes for which the plaintiff does not have an obviously credible threat of relief. Complications in relaxing this assumption are of little consequence to theoretic research, but should be carefully considered in applying present results to the design of actual tort policy in the field.

17.2 Dynamic Efficiency

The present study of settlement bargaining focuses exclusively on the *static efficiency* of bargaining outcomes: given the existence of some catalyzing injury, efficiency is increased by reductions in the aggregate negotiation and court costs committed to resolution of a dispute. For the model of settlement bargaining presented in Chapter II, aggregate costs are a simple function of the time spent negotiating a settlement. The study is thus concerned with understanding and possibly reducing the average delay between initiation and settlement of a dispute.

Another common efficiency concept relating to tort disputes concerns the capacity of tort law to act as a deterrent against negligence and injury. In what I term *dynamic efficiency* analysis, the objective is not to minimize the costs of dispute resolution, but to manipulate tort law to induce a socially optimal level of care in the population (see, e.g., Calabresi, 1975). A simple model of the deterrent role of tort disputes involves a rational actor choosing the level of care to invest in an activity by balancing the marginal cost of diligence (e.g. slower production, more safety measure, etc) against the marginal benefit of reduced exposure to potential tort liability.

[139] That is, analysis ignores the possibility of *nuisance suits* in which the plaintiff does not have a credible threat of taking a claim to trial: see Assumption 1 in Section 3.2.

A reasonable objection to the present study is that an exclusive focus on static efficiency may fail to recognize negative effects on dynamic efficiency. For example, a reform policy which serves to reduce average settlement delay will tend to reduce the average dispute-costs that a potential defendant can expect to invest in the resolution of a dispute, possibly reducing the marginal benefit of exercising care and thus increasing the incidence of injuries and disputes in the population. This line of reasoning suggests that improvements in the static efficiency of dispute resolution may in fact do violence to the dynamic efficiency of the tort system as a whole.

Abstraction from dynamic efficiency concerns is probably not, however, a tremendous practical or theoretic limitation of the present study. From a practical perspective, it is not even clear that a reduction in costs would necessarily translate into a reduced incentive to exercise care. To the extent that potential plaintiffs can also expect to invest fewer resources in settlement bargaining, it may well be that the decreased cost to a plaintiff of instituting a dispute would tend to increase the marginal benefit of caution, leading defendants to exercise greater care than before.

From a theoretic perspective, the effective costs of settlement bargaining can be fixed at any arbitrary level through the imposition of appropriate welfare-neutral fees or other institutional adjustments. The basic idea is that settlement negotiation involves socially inefficient investments of resources. If a certain level of costs is needed to achieve desirable deterrence incentives, such costs are more efficiently introduced through welfare-neutral transfers than through socially inefficient commitments of resources to rent seeking, costly screening/signaling strategies, etc.

18 Extensions

Specific suggestions for extension of the present research are provided throughout the previous chapters of this study. As a summary, this section comments on two classes of extension. Section 18.1 discusses relaxation of the bargaining structure. Section 18.2 suggests further replications of the experiment.

18.1 Flexible Bargaining

A question raised by many experimental subjects during informal *ex post* interviews (generally in the form of a complaint when the subject had been assigned the role of plaintiff) is how results would differ if the rules of bargaining were changed so that both plaintiff and defendant could make and accept settlement proposals during negotiation. The current model of settlement bargaining imbues the plaintiff with an informational advantage, but allows only the defendant to make settlement proposals during bargaining. This structure seems plausible as a model of many tort disputes in the field, but is more restricted than the fully unstructured model of bargaining which could be made available to subjects in the experiment.

An advantage of the present model of settlement bargaining is the availability of determinate—in fact unique—equilibrium strategies for both plaintiff and defendant. It seems doubtful that this would be true of a continuous and unstructured model of settlement bargaining with asymmetric information. A known limitation of non-cooperative game theory is that the strategy space of many games can explode under even ostensibly modest relaxations of structure and assumption. This is clearly the case for many bargaining models: with continuous interaction and no restrictions on the form of bargaining, the strategy space and set of potential equilibria quickly become unmanageable (see, e.g., Davis and Holt, 1993, p. 244).

Note, however, that even if equilibrium strategies are indeterminate under the more flexible model of settlement bargaining, laboratory experiments can still be used to provide potential insights into the properties of average behavior. Modifying the present experiment to accept proposals and acceptances from both litigants is simple. With only minor changes, the present experimental design could be employed to perform many of the same tests as in the current experiment, but with less structure imposed on the form of settlement bargaining. This type of analysis would be especially valuable as a robustness check on results of the present study.

18.2 Future Replication

Throughout analysis of SE1, SE2, and SE3, suggestions are made for possible extensions of the experiment. Rather than recite these suggestions here, this section comments on two basic classifications of future extensions.

The first class of extensions involves simple replication of treatment sequences for which desired precision is currently lacking. In Sections 11.2 and 14.1, noisy decision-making combines with modest apparent treatment effects to produce a frustratingly ambivalent set of inferential conclusions. Statistical inferences could be tightened by the collection of further data on the offending treatment sequences.

The second class of extensions involves replication of the experiment with alternative treatment designs. Due to the extensible nature of laboratory experimentation, little effort would be required to conduct parallel experiments with, for example, alternative sets of reform policies. Using the framework of the present experiment with alternative treatments, newly collected data could be fairly compared with present results to provide an even more detailed tour of the sensitivity of settlement delay to perturbations in the bargaining environment.

A particularly promising example of the second class of extension is further exploration of Early Offers reform under alternative bargaining environments. As noted in Section 13.4, the present experiment's implementation of Early Offers is heavily weighted against the policy—the objective being to establish a conservative baseline against which more plausible future implementations of the policy may be compared. Results may change dramatically when Early Offers parameters are set to values more consistent with the proposal (e.g. a shorter window in which an early offer may be tendered), when the early offer involves more substantial relative compensation of the plaintiff (e.g. a larger economic injury relative to the average size of the non-economic injury), and when the bargaining environment is modified to encompass additional elements of the reform policy (e.g. a model in which tender of a generous proposal by the defendant might otherwise signal negative private information). Like other examples of the second class of extension, the value of these additional treatments is in further mapping the landscape of reform efforts—providing both guidance for future research and direction for future policy development.

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