

Law and Economics in the Laboratory

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Experimental methods have become increasingly prominent in the social sciences. Of course this methodology has been common for decades in psychology, but economists and political scientists have also found it to be a fruitful resource for examining behavior and testing theory. Yet, why should researchers in the area of law and economics care about laboratory experiments? After all, there are plenty of field data available for empirical tests. While field data are indeed rich and abundant, they reflect a variety of environmental factors; disentangling these factors is difficult, if not impossible. The intertwining of potential causal factors in the field is particularly acute in law and economics.

Laboratory experiments have some important advantages over other approaches. One key advantage is the feasibility of controlling conditions more tightly than in any other context. It is possible to assign subjects randomly to treatments and to replicate results. Random assignment helps control for alternative explanations.. Replicability is valuable both in providing a solid baseline from which to test the effects of varying factors in the environment and in allowing for robustness checks. One of the great strengths of laboratory experiments is that one can keep constant all elements of the environment save one. The researcher can then readily test the impact of a particular manipulation, cleanly identifying a potential treatment effect.

Particular applications and issues potentially make laboratory experiments especially valuable to the field of law and economics. One can implement changes in punishment and monitoring technology—in the law, the size of sanctions, probability of apprehension and so

on—without having to worry about simultaneity issues.¹ One can readily extract information such as the role of risk preferences or uncertainty, and can potentially crack open the black box of decision-making by judges and juries during trials. Furthermore, one can learn about the role of morality (or lack thereof) in decisions by juries, judges, etc.

Lab experiments are particularly useful for testing theory. For example, one may wish to learn whether a theoretical prediction that, holding the expected cost of malfeasance constant, a higher penalty combined with a lower probability of detection will more effectively deter undesirable behavior. It is straightforward to test this (within some bounds in terms of the size of the penalty) by comparing the predictions of the model to actual behavior in the lab. In addition, in some environments theoretical predictions might be embryonic or absent; in this case, gathering behavioral data in the lab may lead to new (or more precise and accurate) theories. One way to think about a lab experiment is as a first link in a longer chain running from theory to actual field behavior.

Of course, external validity, the extent to which behavior observed in the laboratory can be generalized to behavior in field environments, is a serious issue.² Indeed, our view is that one must take with several grains of salt any extrapolation of the choices observed in the lab to field contexts. Nevertheless, the ability to clearly identify experimental treatment effects provides a major advantage over the field. While we may not completely trust, for example, that a particular rate of contractual adherence observed in the lab will be the same in a field context, knowing the

¹ In the field, changes in levels of punishment and monitoring can impact crime levels, but changes in crime levels can also impact levels of punishment and monitoring. For this reason, using field data to identify the impact of punishment and monitoring on crime rates is difficult.

² Critics of laboratory experiments assert that little can be learned from inexperienced undergraduate students, who typically act as subjects in economics experiments, making choices in unfamiliar settings. Levitt and List (2007) provide a critique, and Falk and Heckman (2009) strongly dissent, pointing out the advantages of the laboratory for careful control and tests of theory. Plott (1982) argues that the simple environments used to study behavior in the lab are well suited for theory testing.

magnitude or the direction of a change that results in simple contexts is still quite useful.

In any case, laboratory experiments are just one of a variety of complementary methodological approaches that are useful for research in law and economics; one should use the tool that is best suited for the task at hand.³ At a minimum, this approach should be viewed as one arrow in the researcher's quiver.

Laboratory experiments have become an important research tool in economics. The number of experimental papers published in high-ranking economics journals has grown exponentially in recent years. These experiments have been used to study everything from social preferences and reciprocity to behavior in large-scale markets. Literally thousands of laboratory experiments have been conducted in economics in the past 20 years. We do not perform a recital of these but refer the reader to recent surveys such as Kagel and Roth (1995), Camerer, Lowenstein and Rabin (2003), and Kagel and Roth (forthcoming).

More recently, researchers in the fields of political science and anthropology have embraced laboratory experiments. For example, Thomas Palfrey and Rebecca Morton have examined voter participation, the curse of the swing voter, strategic voting, and information aggregation in committees.⁴ Some of these areas have immediate application to jury decision-making.⁵ Cultural anthropologists and development economists have been conducting simple laboratory-style experiments in primitive areas for perhaps a decade. Henrich et al. (2005) report such experiments conducted in 15 small-scale societies to study behavioral features such as social preferences, risk-aversion, and informal risk sharing.

³ For a detailed discussion of the advantages and disadvantages of laboratory experiments, the interested reader may wish to consult Charness and Kuhn (2011).

⁴ See e.g., Battaglini, Morton and Palfrey (2010) and Battaglini, Morton and Palfrey (2008).

⁵ For a survey of the literature in this area, see Morton and Williams (2008, 2011).

The purpose of this chapter is to familiarize the reader with a handful of the vast array of techniques experimentalists use to explore theories related to law and decision-making in (mock) legal environments. Part I describes a set of experiments conducted to study decisions of judges, juries and attorneys. Part II reviews experiments designed to study the effects of law enforcement. Part III describes a set of experiments that study bargaining behavior of principals and their agents and the role that communication plays in negotiations. Our objective is to demonstrate the usefulness of employing economics experiments to study law and legal institutions. It is important to note that the summaries of studies we provide are not meant to reflect the bigger picture of the literatures on the studied topics. The literatures in these areas are vast, and we do not draw general conclusions about what we can learn from the broad literatures here. Our goal simply is to provide a window into how experiments can be used to study economics principles relevant to law.

I. Experiments Studying Decision-Making by Actors in Legal Environments

Researchers are starting to employ the tools of experimental economics in many areas of law and economics. From topics pertaining to judge and jury decision-making to the appropriate punishment regimes, the laboratory has proved to be a fruitful environment for understanding the role of economic agents in legal environments. In what follows, we briefly discuss several areas in which experiments that study law and economics issues have been particularly prevalent in the past ten years, including the behavior of judges, juries, attorneys and litigants.

Judges

The study of judges in their natural environment – the courtroom – is difficult because

researchers cannot observe directly what drives judicial decision-making. In particular, understanding what sorts of factors influence decisions is limited by the fact that judges hear each case only once. This makes it impossible to observe a judge making the same decision repeatedly under slightly different conditions. Additionally, our ability to observe judicial decisions in similar environments is constrained by the number of cases that settle out of court – approximately 97 percent of cases (Rosenberg 2003). Decisions also are likely biased by previous decision and precedents. Thus, conducting empirical research on the judicial decision-making process using field data is severely limited.

Given the problems with examining judges in their natural habitat, researchers have begun examining judges in controlled environments where multiple treatments can be implemented. This research has proven to be useful in understanding heuristics and biases that might influence judges' decisions and might be difficult for judges to overcome when handling a trial and determining a verdict.

Chris Guthrie, Jeffrey Rachlinski and Andrew Wistrich have played an integral role in examining judges in the past ten years. They have a knack for gathering large numbers of judges from many different courts and examining their decisions under various conditions. Observing decisions across multiple treatments has allowed the authors to examine how anchoring, framing, hindsight and racial biases might impact judicial decision-making.⁶

Rachlinski, Guthrie and Wistrich (2007) and Guthrie, Rachlinski and Wistrich (2007) examine the impact of anchoring, framing, egocentricity and hindsight bias on decisions by

⁶ Anchoring and framing are components of decision-making that result in individuals attaching too much importance to one aspect of a decision, potentially yielding an error in judgment (see Tversky and Kahneman 1974). Hindsight bias (see Roese and Vohs 2012) can impact decision-making when individuals believe that an outcome was likely *ex ante* despite any objective evidence to support the claim that the outcome actually was predictable.

bankruptcy and federal magistrate judges. The decisions by the judges are compared to laypersons' decisions to determine if judges appear to be better able to resist the influence of biases. In general, results from experiments suggest that judges are impacted by these biases in approximately the same ways that laypersons are. Additionally, the judges who were examined do not appear to be any more or less affected by the representativeness heuristic and cognitive illusions than laypersons when making rulings.⁷

In follow-up research, Rachlinski et al. (2009) and Rachlinski, Guthrie and Wistrich (2011) examine hindsight and racial biases in judges. Rachlinski et al. (2009) examines whether judges carry and potentially implement racial biases that laypersons tend to suffer from – notably that white Americans harbor implicit biases toward black Americans.⁸ To examine this bias, the authors recruited 133 trial judges in a study of “the psychology of judging.” The main finding was that judges suffer from the same invidious biases that most adults suffer from when assessing the outcome of a hypothetical trial. However, in treatments in which the judges were told the race of a hypothetical defendant (e.g. “Caucasian” or “Black”), the judges appeared to overcome their implicit biases. The judges produced nearly identical decisions in cases in which they were faced with a Black defendant and with a Caucasian defendant. In effect, judges tend to make judgments that do not appear to be correlated with their implicit associations.

Rachlinski, Guthrie and Wistrich (2011) examine whether judicial decision-making is subject to hindsight bias. This question is important because judges can issue warrants to permit

⁷ Tversky and Kahneman (1974) define the representativeness heuristic as “the degree to which [a single event] (i) is similar in essential characteristics to its parent population, and (ii) reflects the salient features of the process by which it is generated.” For example, if I’m told a coin is fair and ten flips of the coin result in seven heads, then I believe that the probability that the next flip will result in a head is higher than the objective probability (50%).

⁸ The authors define “implicit bias” as “stereotypical associations so subtle that people who hold them might not even be aware of them.” (p. 1196)

the gathering of data (foresight) and can also permit a piece of information to be presented at trial as evidence (hindsight). Because the judge knows whether a search without a warrant produced incriminating evidence, hindsight bias might encourage him to permit evidence obtained using unconstitutional methods. The authors use a data set they gathered by surveying over 900 state and federal judges that make judgments of probable cause in circumstances involving both foresight and hindsight. Surprisingly, judges appear to make approximately the same ruling in both foresight and hindsight conditions. This suggests that they somehow override biases that might cloud their judgment.

Juries

Although judges oversee the vast majority of court cases, a trial by a jury of one's peers is a legal right in many legal systems. Juries are interesting subjects to study because jurors typically do not have formal legal training. This lack of expertise gives rise to questions related to the components of the legal process that might impact jury decisions. Jury trials also produce greater uncertainty relative to bench trials about the outcome of the trial (see Harel and Segal 1999), which makes research on jury trials all the more interesting.

One of the most widely publicized determinations juries make is how much to award plaintiffs in compensatory and punitive damages. Hastie, Schkade and Payne (1999) designed a mock jury experiment that questioned whether variation in anchors (i.e., variations in plaintiff's request for a punitive damage award of a specific amount) and whether the plaintiff was local or remote affected the jury's decision.⁹ They found that both high anchors and the plaintiff being local (regardless of whether the defendant was local or remote) were positively correlated with

⁹ The experiment tested the conjecture that "some juries are motivated by their power to redistribute wealth from remote (usually corporate) coffers into local (usually individual citizens') pockets." (Hastie, Schkade and Payne 1999, p. 449).

the size of punitive damage awards. Similarly Greene, Coon and Bornstein (2001) used a mock trial experiment to test whether caps on the amount of awardable punitive damages would affect compensatory damages. They found that compensatory damages were inflated only when punitive damages were completely prohibited. Kahneman, Schkade, and Sunstein (1998) examined personal injury cases and how the plaintiff's severity of injuries affected the punitive damage award. This study found that the participants agreed on outrage and punishment scales, but hypothetical award amounts were random. Greene, Johns and Smith (2001) used a hypothetical automobile negligence case to look into whether injury severity might affect damage awards and found that the magnitude of hypothetical awards was positively associated with hypothetical injury severity rather than the degree of negligence of the hypothetical defendant.¹⁰ The results suggest that irrelevant aspects of cases can influence juries' decisions in problematic ways.¹¹

The factors that impact how juries decide have also been examined fairly extensively. For example, Inbar, Pizzarro and Cushman (2012) examined the role of moral blameworthiness on a jury's propensity to convict. Interestingly, experiments conducted using jury-eligible subjects suggest that juries are prone to moral judgments even when the individuals they convict have not caused or intended to inflict harm on other individuals. For instance, a jury might convict an individual that it deems to have been morally at fault when betting, for example that a company's stock will decline or that a natural disaster will occur, if the betting individual might benefit from

¹⁰ Half the subjects participated in a mild injury treatment and the other half in a severe injury treatment. Similarly, half the subjects were told that negligence was certain (e.g., the defendant was traveling faster than the speed limit) and the other half were told that the defendant was not negligent (e.g., the defendant was traveling at the speed limit).

¹¹ These and other similar experimental designs assume that subjects' decisions in hypothetical settings are correlated with the decisions of actual jurors in real cases. They might not be for a variety of reasons. The assignment of subjects to value-laden roles and the elicitation of hypothetical decisions are considered shortcomings that limit our confidence in placing weight on the results. For a general discussion of best practices in experiment design, see Davis and Holt (1993).

another individual's misfortune.¹²

Others have studied how adversarial competition among expert witnesses at trial impacts the accuracy of jury decision-making. In a laboratory experiment where subjects solve problems as groups, Boudreau and McCubbins (2008) chose two subjects to be fully informed experts. By giving their expert opinion, the experts can influence their payouts, which depend on the choice of the other group members. They find that even in the absence of monitoring and possible punishment for misrepresentation, competition fostered by the adversarial nature of trials, by itself, can increase truth telling and accuracy of jury decisions.

While experiments that examine how individuals process the quality and value of the information they receive from others yield insights about individual jurors, the collective intelligence of the group is also an object of study since collective decision-making is an essential part of the decision-making process for juries. Bornstein and Greene (2011) find that both individual and collective cognitive reasoning influence juries. Results reported by Minson and Mueller (2011) provide insight into how individuals display *dissent neglect* by systematically placing greater weight on judgments that are similar to their own. *Dissent neglect* is hypothesized to stem from the fact that individuals believe that their own judgments are fundamentally more objective than those of others.¹³ Additionally, Minson and Mueller (2011) show that individuals will systematically underweight their peers' judgments when making group decisions. Finally, Yaniv (2004) finds that individuals tend to discount the information they receive from others. Specifically, the more knowledgeable the information receiver, the greater the discount attached to the information received from others.

¹² See also Monin, Pizarro and Beer (2007).

¹³ See also Carlson and Russo (2001).

Particular insights from experiments designed to test theories from political science are also relevant for understanding juror behavior. For example, under certain conditions, theory predicts that rational voters sometimes do not vote sincerely (in line with their private information) because one's vote matters only if it is the tie-breaking or deciding vote. Recent experimental studies on juries, committees and legislatures have attempted to test this prediction by applying it to particular types of decisions (see for example, Fréchette, Kagel, and Lehrer 2003; Fréchette, Kagel, and Morelli 2005; Levine and Palfrey 2007; Feddersen, Gailmard, and Sandroni 2009; Battaglini, Morton, and Palfrey 2010; Goeree and Yariv 2011). These studies suggest that the voting institution can substantially impact the outcome.

Many experimental studies find compelling evidence that voters may vote strategically in some settings. This is problematic if we assume that the most reliable jury decisions occur when all jurors vote non-strategically. A non-strategic juror votes according to his true beliefs given the information he received at trial and through discussions with his co-jurors during the deliberation process. Experimental studies have found that the voting institution can impact whether jurors vote strategically. For example, Guarnaschelli, McKelvey and Palfrey (2000) find that jurors are more likely to vote strategically under a unanimity rule. Battaglini, Palfrey and Morton (2010) also identify strategic voting behavior in the form of the so-called swing-voter's curse—the prediction that “[a] poorly informed voter may be better off...to leave the decision to informed voters because his uninformed vote may go against their choice and could decide the outcome in the wrong direction.” (p. 62).

The voting-aggregation rules clearly seem key in predicting jurors' behavior. The issue of strategic voting, however, is most applicable to environments in which voters cannot communicate amongst themselves and thereby aggregate individually received signals. This

theory is applicable only in settings where jurors are unable to aggregate these signals through the deliberation process. The typical deliberation process, however, involves communication. Jurors are at least somewhat informed of the potential distribution of votes by other jurors. This knowledge potentially impacts the voting decision of each individual juror. In other words, the pivotal juror will have learned, through the deliberation process, about the distribution of votes from other jurors, which is likely to impact her own vote.

In the past decade, several papers have analyzed the potential impact of communication on collective choice outcomes. Coughlan (2000) and Austen-Smith and Feddersen (2006) were among the first to report evidence suggesting that the availability of particular communication protocols can dramatically alter collective decisions. A recent paper by Goeree and Yariv (2011) represents the state of the art. They find that people vote in a sophisticated, strategic manner when free-form communication is not feasible. However, free-form communication greatly improves efficiency, virtually eliminates strategic voting, and substantially diminishes the distorting effects of different voting institutions. The findings from these experiments support predictions that communication in deliberation on collective decisions consistently reveals private information, does a good job of predicting eventual verdicts, and improves efficiency. The results suggest that collective decision-making by juries is likely to be preferable to individual decisions from a welfare point of view.

Attorneys

As noted above, 97 percent of civil court cases are decided outside the courtroom through settlement, making the role of attorneys all the more important. Unlike courtroom decisions, the impact that attorneys have on settlements are not as easily observable. In fact, little is known

about the decisions that settling parties make because most settlements in civil cases are kept private. Some describe settlements as resulting from “bargaining in the shadow of the trial” or occurring inside a black box of decision-making.¹⁴ Bushway and Redlich (2012) offer one of the first empirical tests of bargaining in the shadow of the trial, which is somewhat surprising given the large body of work on what drives trial outcomes (Devine et al. 2001). Unlike trial outcomes, however, data on settlements are hard to come by. For example, case data on the plea process in criminal cases are hard to find, and the relevant counterfactuals for individuals who enter pleas (i.e., what would have happened if they went to trial) are hard to identify.

In an attempt to make progress in this area, Bushway, Redlich and Norris (2014) used a web-based protocol to survey judges, prosecutors and defense attorneys who were randomly assigned to 16 different evidence conditions. The survey, completed by over 1,600 respondents, was employed to test predictions generated by applying the expected utility model framework to decision-making during plea bargain sessions.¹⁵ The survey questions relate to the expected outcome of the trial, including the probability of conviction and the expected punishment. Responders were also asked to state the plea deal they would advocate for (all within their own jurisdiction).¹⁶ Contrary to the predictions of Bibas (2004), they found that, on average, the simple expected utility theory appears to fit the data quite well. They also found little difference in responses by role. Prosecutors, defense attorneys, and judges responded similarly to the survey

¹⁴ Researchers have studied the general question whether individuals account for future uncertainties when making choices in the current period. For example, Dal Bo (2005) used infinitely repeated prisoner’s dilemma games with a random continuation rule (so that subjects were uncertain about whether each current round was the final round) to study whether the shadow of the future impacts current choices. He found that subjects were increasingly more cooperative as the likelihood of future interaction increased.

¹⁵ Basic expected utility models predict that defendants will accept a plea bargain if the punishment offered is less than the expected punishment at trial discounted by the likelihood of a guilty verdict. If the defendant is risk averse, he might be willing to accept an amount of punishment that exceeds his expectation over the trial outcome.

¹⁶ Miller, McDonald and Cramer (1978) provide details about how plea-bargaining works. The institutional details inform economics models of decision-making during bargaining sessions over pleas.

questions.

Bushway et al. did, however, find a surprisingly large amount of unexplained deviation from the expected utility theory prediction at the individual level. This is documented in Figure 1, taken from their study, which presents a scatter plot of the relationship between the expected value of the trial and the best plea deal offered by prosecutors. As the authors explain:

“The diagonal line [see Figure 1] represents the slope expected by the shadow of the trial model. Although the relationship is clearly positive and strong, considerable variation is found around the line. Some prosecutors offer deals that are below the expected value of the trial (below the diagonal line), whereas other prosecutors offer deals that are above the expected value of the trial (above the diagonal line).... This variation could be random noise or measurement error induced by the clumping of the responses in the probability of conviction,¹⁷ but it could also reflect other causal mechanisms. In addition, [the figure]¹⁸ shows that the relationship is likely to be nonlinear, with a slope that seems to flatten as the expected value of the trial increases.” (p. 742-43)

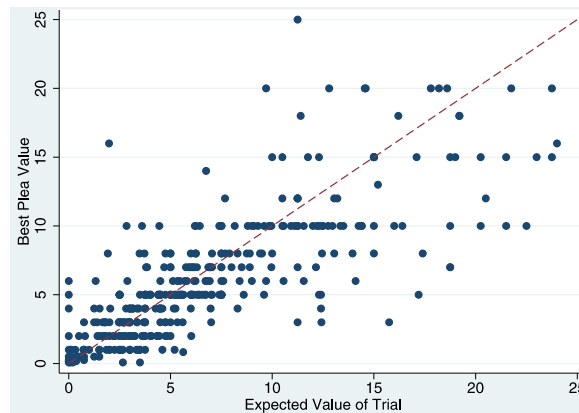
Anecdotally at least, this large amount of variation in the plea discount is consistent with other observations researchers have reported that suggest that very similar cases result in very different plea discounts (Smith 1986; Ulmer and Bradley 2006; Spohn and Fornango 2009).

¹⁷ “Although we used a method designed to minimize clumping, the probability of conviction has substantial bunching at 25 percent, 50 percent, and 75 percent.” (Bushway, Redlich and Norris 2014, p. 742).

¹⁸ The authors refer to their figure 2 at this point, but they obviously intend to refer to figure 1.

Figure 1: Bushway, Redlich and Norris (2014)

Scatter Plot of Best Plea Value and Expected Value of Trial for Prosecutors



Two broad classes of explanations attempt to account for the types of variation found in the Bushway, Redlich and Norris (2014) experiment: different types of people and different types of bargaining environments. The attorney-respondents could have varied over at least three key decision-making dimensions – attitudes towards risk, attitudes towards uncertainty and attitudes towards loss. In each case, experimental economists have shown that these attitudes can explain sizeable variation in the degree to which individual decisions deviate from the simple expected utility model tested by Bushway, Redlich and Norris (2014). It is at least possible that the observed variation could be explained by differences in individual preferences.¹⁹ Another

¹⁹ The authors collected no systematic information about differences in preferences about risk, attitudes towards uncertainty or attitudes towards loss in the surveyed case (armed robbery).

possibility is that this variation is the natural result of key characteristics of the bargaining game, such as uncertainty over the outcome at trial (Stuntz 2004). We believe the key question is the extent to which the nature of the bargaining game itself, over and above the individual differences in preferences, can help explain the wide variation in plea discounts observed in both case data (Bushway and Redlich 2012, Smith 1986)²⁰ and experimental data (Bushway, Redlich and Norris 2014).

Others have studied the impact of information on attorneys' estimates of jury verdicts. One notable example is a study by Jacobson, Dobbs-Marsh, Liberman, and Minson (2011), in which law students were paired with other law students and experienced trial attorneys were paired with other experienced trial attorneys. The subjects were asked to guess the amounts of jury awards in actual cases. After guessing, each subject was then told his partner's guess and asked if he wished to revise his own guess. Jacobson et al. found that the accuracy of the second, informed guess was better than the first for both students and experienced attorneys. While both students and attorneys placed less weight on the information received from their partners relative to the weight assigned to their own guesses, the attorneys discounted the information more heavily than the students. The attorneys' guesses would have been more accurate if they would have placed equal weight on their guesses and their partner's guesses. This suggests that attorneys "should seek out a second opinion and give that opinion more weight than they might be initially inclined to do." (p. 115)

Experiments on Litigation and Settlement

Judges might do more harm than good when they intervene in settlement negotiations. Research

²⁰ Bargaining in case data entails bargaining over plea deals in the shadow of a trial as well as bargaining over the charges that will be brought to a trial.

suggests that a decision maker's subjective perceptions of probabilities over potential outcomes differ from objective reality (see e.g., Tversky and Kahneman 1974). If judges' beliefs are consistent with this common finding, the divergence could have an unfortunate impact when judges interject themselves into settlement talks. In addition, experimental evidence suggests that judges are likely to (correctly or incorrectly) perceive settlement to be more attractive to plaintiffs than to defendants in ordinary litigation (Guthrie 2003). This, in turn, suggests that judges are likely to advocate settlement more strenuously to plaintiffs than to defendants even though the experimental evidence suggests that plaintiffs are more likely than defendants to be attracted to settlement in the first place (Rachlinski 1996-1997; Guthrie 2003). By urging plaintiffs to accept a relatively small amount or by failing to urge defendants to offer some larger amount, judges could promote unfair settlements that under-compensate plaintiffs and under-deter defendants.

Attorneys can also wreak havoc on settlement negotiations under some conditions for a different set of reasons. Rachlinski (1996) reported experimental evidence demonstrating that a substantial number of subjects assuming the role of lawyer chose to behave (arguably) unethically when faced with an ethical dilemma. Rachlinski (1996) presented a litigation problem to law student-subjects assigned to the role of counsel for a defendant pharmaceutical company in a hypothetical products liability suit. The subjects learned that the parents of a child allegedly injured by a drug manufactured by the defendant had sued the defendant for damages. The subjects further learned that the defendant had offered the parents \$3 million to settle the case. Unknown to the parents, however, the defendant had discovered and withheld several relevant, incriminating documents during the discovery process. The subjects learned that they could be sanctioned if they agreed to settle the case without disclosing the documents to the

plaintiffs.

To study the impact of framing on subjects' choices, Rachlinski (1996) randomly assigned half of the subjects to a "gains" condition. Subjects in this group learned that their client, who had originally expected to have to pay plaintiffs \$5 million to settle the case, believed the case was "going well." Subjects assigned to the "losses" condition learned that their client, who had originally expected to pay plaintiffs only \$1 million, believed that the case was "going poorly." Rachlinski asked subjects in both groups to indicate whether they would agree to accept the plaintiffs' offer and settle the case. Although the subjects faced the same decision—whether to settle the case for \$3 million prior to disclosing relevant and incriminating documents—the framing of the decision problem affected subjects' willingness to engage in risky, and arguably unethical, behavior. Only 12.5% of the subjects assigned to the gains condition (case is going well) indicated that they would engage in the ethically risky behavior of settling prior to disclosing. By contrast, 45% of those assigned to the losses condition (case is going poorly) indicated that they would settle before disclosing.

Consistent with the predictions of Kahneman and Tversky's (1979) Prospect Theory,²¹ Rachlinski found that subjects prompted about hypothetical clients facing hypothetical losses were nearly four times as likely to adopt a risk-seeking, and ethically dicey, litigation strategy. Although the subjects might not have known whether settling before disclosing violated the governing ethical rules, "settling before a party can find out unpleasant facts about one's case smacks of impropriety and unfairness." (p. 142) Many of the subjects appeared willing to sacrifice ethical principles in an attempt to avoid incurring losses. It is worth emphasizing,

²¹ Prospect Theory is an alternative to Expected Utility Theory. It assumes that individuals assign value to gains and losses relative to some reference point (as opposed to assigning value to outcomes) and that they are risk averse in the face of potential gains but risk seeking in the face of potential losses (as opposed to being consistently risk averse in the face of any type of uncertainty).

though, that attorneys do not directly incur losses when their clients pay. Moreover, Prospect Theory assumes that the decision maker will incur actual losses (as opposed to hypothetical losses). This calls into question the suitability of the experiment as a test of the theory. This research does, however, report an interesting finding about reference effects and their possible impact on decision-making during settlement that highlights a need for further investigation.

Others have studied factors that might explain why settlement negotiations sometimes fail. For example, Gilliland, Dunn and Navarro (2008) and Miettinen, Ropponen and Saaskilahti (2011) conducted experiments involving computer-based questionnaires to explore reasons why settlement fails. Specifically, the experimenters place subjects in roles (plaintiff, defendant or one party in a negotiation game), give them a settlement offer and then ask them to decide whether to accept the offer or continue to negotiate. Gilliland, Dunn and Navarro (2008) found that subjects were more likely to hypothetically settle out of court when the outcome was framed in a positive way,²² when they perceived the probability of winning the case as low and when the outcome of trial was uncertain. In a separate study, Miettinen, Ropponen and Saaskilahti (2011) studied the impact of inequity aversion on settlement behavior. They found that, contrary to conventional wisdom that greater variance in possible court outcomes leads to higher settlement rates, high variance reduces the likelihood of settlement. The authors rule out asymmetric information and self-serving biases as explanations and attribute the result to subjects' distaste for inequality in outcomes.

²² All subjects were told that a settlement offer of \$10,000 had been made and that their attorney estimated a 50% chance of a \$20,000 award and a 50% chance of getting nothing. Subjects playing the plaintiff role in the positive-frame treatment were told, "If you accept this offer, you will receive \$10,000 in compensation." Subjects in the negative-frame treatment were told, "If you accept this offer, you will lose \$10,000 in income." Despite the obvious problems with the language (i.e., one cannot actually "lose" \$10,000 by accepting \$10,000 in lieu of playing the lottery—one merely loses the *chance* to gain \$20,000), the results might teach us something about framing.

A Note on Methods

Before moving on to Part II, it's important for us to highlight possible methodological issues with the experiments summarized in Part I. First, experimentalists generally avoid asking the subjects to engage in "role play," where they attempt to place themselves into the shoes of some actor with particular skills or characteristics or interests. Many are critical of this technique given the difficulties subjects likely have in making authentic decisions during role play.²³ Second, many of the experiments described in this section are hypothetical in nature. This violates a basic principle of experiment design. Specifically, experimental environments are valid tests of theory only if they include all necessary assumptions that drive the theory's predictions. The experiments described in this section all purport to test theories that assume non-hypothetical outcomes (e.g., an actual verdict with actual consequences). Thus, whether these experiments stand as valid tests of the theories is questionable.²⁴ While these concerns should not compel us to ignore the results, they might substantially reduce the amount of weight we place on the evidence as support for the tested theories' predictions. More practically, it would be prudent for us to wait to apply the tested theories until other sorts of evidence gives us better confidence in the theories' predictions as they relate to legal system actors.

²³ Croson (2002, pp. 929-930) explains:

"Experimental economists, generally, prefer very little context when they are testing theories for three reasons. First, the theory being tested often does not rely on context, so the experiments should not either. Second, context often adds variance to the data. For example, if some subjects think that going to court is a bad thing, then describing the experimental decision as 'going to court' as opposed to 'choosing option A' could change an individual's decision. These changes might not affect the average or aggregate decision, but it can impact the variance of those decisions, reducing the likelihood of detecting statistically significant different between treatment of the experiment. Finally, and most importantly, context can add systematic bias or demand effects. For example, if subjects want to be seen as kind, gentle types by their professor, then describing the decision in terms of going to court might reduce everyone's likelihood of choosing that option. Such systematic changes in the data will significantly change the conclusions reached, thus context should be avoided in theses types of experiments. In theory testing experiments, there are only low costs associated with avoiding context."

²⁴ Davis and Holt (1993, p. 24) note: "In designing an experiment, it is critical that participants receive salient rewards that correspond to the incentives assumed in the relevant theory or application."

II. Experiments on Legal Remedies and Enforcement

Many behavioral experiments produce results that are potentially relevant to choices over institutional settings and laws and to predicting the impacts of perceptions of fairness and punishment. We focus specifically on papers published after 2000, emphasizing the relevance to the field of Law and Economics.

Laboratory experiments help researchers isolate the effects of institutional settings, the characteristics of information and the structure of legal rules on subjects' behavior and outcomes. Croson and Johnston (2000), for example, study the impact of vagueness in the legal definition of property rights. They find that vagueness strongly influences the tendency of subjects to take property from others without consent and reduces the likelihood of a consensual exchange. The results suggest that alternative legal approaches to the definition and protection of entitlements can lead to dramatic differences in behavior and in allocations.

In addition to studying the impact of law vagueness, researchers have also studied the impact of information vagueness. Standard litigation theory assumes that information—produced during the discovery phase, for example—leads to a convergence of the parties' expectations over trial outcomes, which results in a higher likelihood of settlement. Loewenstein and Moore (2004) conducted experiments to determine whether all such information discovery is likely to produce these results. They use a conventional bargaining experiment (unrelated to litigation) to test the predictions of the standard model and to differentiate between information that can give rise to multiple interpretations and information that leads to an undisputed interpretation. They find that, contrary to the standard theory, vague information leads to a divergence of

expectations. This result provides support for models that assume that individuals are subject to self-serving biases - e.g., plaintiffs interpret vague information in the best light for their case, and defendants interpret the same information in the best light for theirs. Even though subjects were paid only if they could agree to trade, negotiations failed more often, and took longer, when information was vague as opposed to precise. They also found that a working communication institution that helped to reduce uncertainty increased the efficiency of the bargaining process and outcomes.

Others have found that allowing communication in settings in which law typically disallows it can actually increase efficiency. For example, Santore, McKee and Bjornstad (2010) design an experiment to study whether allowing communication about pricing strategies, typically a violation of antitrust law, can help to achieve socially optimal trades between multiple holders of complementary patents²⁵ and producers who wish to purchase licenses to use the legally protected intellectual property. The results support theoretical models that predict increased efficiency when communication is allowed. These findings suggest that loosening up on enforcement of collusion prohibitions might be optimal under some conditions. Deck and Farmer (2006) design experiments that produce results supporting a similar theory around information structures of final offer arbitration.²⁶ They find that allowing subjects to decide publicly how much to invest in building their cases to convince the arbitrator to side with them results in more efficient outcomes than those produced under private investment choices.

Researchers have also employed experiments to study the justifications for strong

²⁵ Complementary patents refer to a set of patents held by different parties, all of which are required to incorporate the collective technology into useable products. When a product manufacturer wishes to license all the technology protected by the set of patents, inefficiencies such as coordination and hold out problems can arise.

²⁶ Final offer arbitration (Stevens 1966) "requires that agents submit a final offer and the arbitrator must choose one of the two: there is no splitting of the difference." (Deck and Farmer 2006, p. 361)

enforcement of particular legal rules. Bilz (2012) designed an experiment to determine whether the exclusionary rule²⁷ is justified by concerns over protecting the integrity of the judicial system and not just deterrence of illegal searches and seizures. Employing hospital cafeteria diners, law students and lawyers as subjects, she elicited responses to questions about hypothetical vignettes to measure motives. She also draws inferences from whether subjects chose a small bottle of hand-sanitizer or a highlighter pen as a thank-you gift as evidence of the dirty-evidence motive. She finds that subjects prefer exclusion even when law enforcement officials did not realize they were violating the suspect's constitutional rights, a type of situation in which the motive for exclusion cannot be deterrence. In addition, participants who were obliged to use unconstitutionally obtained evidence at trial disproportionately chose the sanitizer over the pen. From these choices, Bilz concluded that subjects were motivated by a desire to maintain the system's integrity.

Others have studied similar issues in civil settings. Darley et al. (2010), for example, explore whether and how we should punish wrongdoers when wrongful acts result only in the possibility of future harm. Just as Bilz did, the authors use subject responses to a variety of vignettes that vary the details related to responsibility of the wrongdoer, the wrongdoer's state of mind, causation, risk levels and actual harm versus an increased risk of harm. The authors conclude that individuals prefer to award damages even in cases in which the injurer's conduct was not negligent or intentional, taking a stronger strict liability stance than current tort doctrine seems to take. The subjects also more stringently punished actions that actually resulted in harm relative to actions that did not result in harm, which seems to be in line with modern tort doctrine.

²⁷ The exclusionary rule is a rule of evidence that courts apply to forbid the government from using, during criminal proceedings, evidence collected or analyzed in a manner that violates a defendant's constitutional rights.

A long line of experimental economics research examines free riding and the punishment of such behavior in public goods games. Some have used public goods games in the lab to study sanctions as an element of justice when information about choices, and therefore who deserves punishment, is incomplete. Grechenig, Nicklisch and Thoni (2010) use a standard repeated voluntary contribution mechanism²⁸ game to examine how punishment is doled out when subjects can punish participants who under-contribute but “reasonable doubt” exists about the amount each subject contributed. Interestingly, the authors find that subjects are willing to punish in the face of highly inaccurate information about contribution amounts. Their results suggest that sufficiently accurate information about others’ behavior is crucial for achieving socially efficient outcomes using sanctions.²⁹

Experimentalists have also studied methods of enforcement in the lab. Guttentag, Porath and Fraidin (2008) study methods for preventing corporate fraud. They separated subjects into small groups and put them into a situation that made it possible to engage in behavior that was not necessarily disallowed but might be perceived as fraudulent. Each group was given time to complete a difficult task and then given the right answers and asked whether they wanted to change their answers. They earned more for right answers but, if an answer change was detected, they would earn less than they would have for answering incorrectly. Subjects were required to disclose explanations for why they answered the way they answered, analogous to financial statement reporting. The authors found that requiring disclosure of explanations reduced “fraud” (the answer-change rate). Levels of intra-group trust and the strength of group cohesion were

²⁸ The repeated voluntary contribution mechanism allocates to each subject a fixed number of tokens that the subject can either keep or contribute to a public good, entirely or partially. Choices are made simultaneously. The contributed tokens are then multiplied by some known number and divided evenly amongst all of the participants in the experiment (see Fehr and Gächter 2000). The socially efficient outcome requires all subjects to contribute all tokens to the public good, but individual rationality compels subjects to keep all their tokens and free ride on others’ contributions.

²⁹ See also Andreoni and Gee (2012), Andreoni and Gee (2015) and DeAngelo and Gee (2015).

associated with an increase in the number of changed answers. The results help us understand both the causes of fraud and the methods for deterring it.³⁰

Law enforcement regimes have also been examined to determine the impact of different enforcement tools on proscribed behavior. The laboratory is a particularly good environment for studying enforcement regimes given that it's difficult, if not impossible, to untangle causal mechanisms in the field (e.g., enforcement efforts are typically higher in high crime areas) and observing outcomes such as recidivism rates is impossible. Friesen (2012) and DeAngelo and Charness (2012) study different components of law enforcement mechanisms on the level of deterrence in experimental environments.³¹ Friesen (2012) examines whether an increase in the probability of getting caught is better at deterring crime than an increase in fines. Her results suggest that increases in punishment severity are more effective.

DeAngelo and Charness (2012) examine the role of uncertainty over the enforcement regime (fine and probability of apprehension) on individuals' choices to commit a proscribed criminal activity—hypothetical speeding in this case. Each subject was paid \$0.60 if he refrained from speeding and \$1 if he chose to speed and didn't get caught. Detection of the "law violation" was possible with some probability, and the subject suffered a fine upon detection. Multiple experiments were run, and treatments varied by the fine imposed on offenders and the probability of getting caught. As theory predicts, subjects were less likely to speed when the expected cost of speeding increased. In addition, the researchers studied the impact of uncertainty about the likelihood of detection and the fine. The results suggest that greater uncertainty over the enforcement regime yields increased deterrence at no additional cost to the enforcer.

³⁰ Others have studied similar phenomena. Mullen and Nadler (2008), for example, design an experiment to study whether people are more likely to act immorally if they witness another's immoral behavior. They find evidence of moral spillovers; witnessed immoral behavior can trigger others' deviant behavior.

³¹ See also Schildberg-Horisch and Strassmair (2012).

Finally, DeAngelo and Freeborn (2012) examine the role of uncertainty over the deterrence mechanism (probability of apprehension and/or fine) on the willingness to recidivate. Variations between treatments mimicked the DeAngelo and Charness (2012) design, with subjects making choices over repeated rounds. Rather than focusing on the likelihood of offending given uncertainty over the enforcement regime, the researchers examine how such uncertainty impacts the likelihood that one will offend again after getting caught and fined. Consistent with DeAngelo and Charness (2012), higher expected costs of speeding reduce speeding. With respect to recidivism, subjects who are caught and fined in one period are less likely to ever speed again relative to those who skirt detection. Interestingly, uncertainty over the legal regime increases the willingness to recidivate. Subjects re-offended sooner after being caught if they were uncertain about which enforcement regime would be applied relative to facing a known risk of detection.

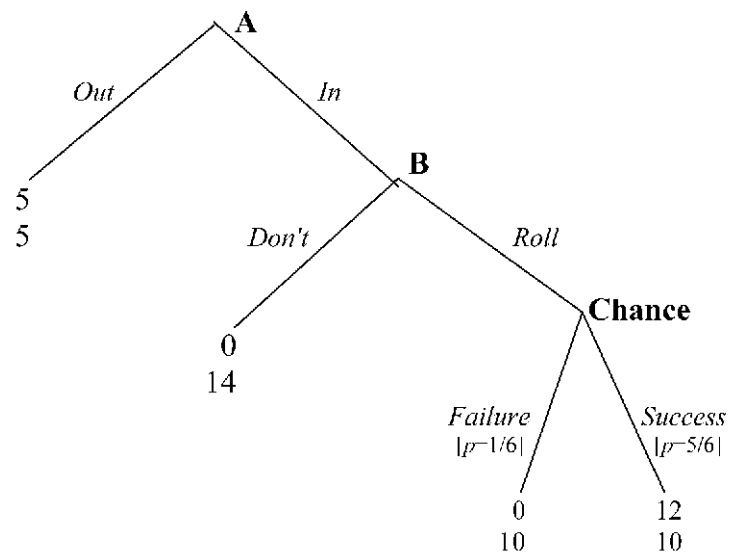
III. Experiments Studying Bargaining of Principals and Agents

Behavioral contract theory predicts how people will respond in contract negotiations; for example, holding to one's reservation price may very well not lead to good social outcomes, as individuals are prone to reject very low offers (as in the ultimatum game). One particular research area that has been developing in recent years is how communication influences decisions and outcomes in principal-agent environments. A series of experimental studies finds that non-binding and costless messages (cheap talk) can effectively induce optimal social outcomes. These studies also shed light on how the nature of the message can play an important role.

Charness and Dufwenberg (2006) examine the effect of cheap talk messages on behavior in

environments with either hidden action (moral hazard) or hidden information (adverse selection). This study considers a situation in which a principal (or potential employer) can hire an agent (potential employee) to complete a project, but cannot observe with certainty whether the agent, if hired, exerts costly effort. The payoff structure in one calibration of the game is the following (see Figure 2): If the principal chooses not to offer the position (Out), then the principal and the agent each receive \$5. If the principal hires the agent (In) and the agent exerts no effort (Don't), the project is not successful; in this case, the principal receives \$0 and the agent receives \$14. If the agent does exert effort (Roll), there is a $\frac{5}{6}$ chance that the project will be successful; if it is unsuccessful (if a die rolled individually for the agent comes up 1), the principal receives \$0 and the agent receives \$10, while if it is successful (the die comes up 2-6), the principal receives \$12 and the agent receives \$0. Since the principal who hires an agent can receive the payoff of \$0 even if the agent exerts effort, the principal cannot verify whether the agent failed to exert effort. A second payoff calibration involves the principal and agent each receiving \$7 if the agent is not hired (all else is the same).

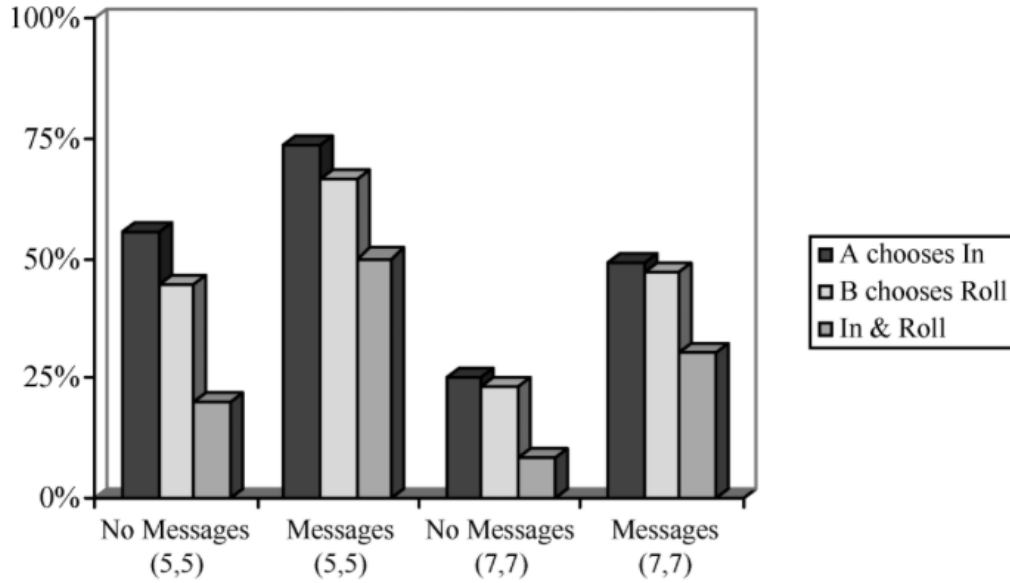
Figure 2: Game Form from Charness and Dufwenberg (2006)



Play is one-shot and anonymous, so that an agent who cares only about his or her payoff has no reason to exert effort. Knowing this, according to standard game theory assumptions, an own-payoff-maximizing principal should choose to avoid paying a wage for no work. This leads to each person receiving \$5, instead of each receiving (in expectation) \$10. Since this is a suboptimal social outcome, there is scope for an intervention to improve matters. The intervention studied is a free-form message that the agent is allowed to send to the paired principal, who sees the message before making a choice. Figure 3 summarizes the results. “A” refers to the principal and “B” refers to the agent.

Figure 3: Results from Charness and Dufwenberg (2006)

Percentage of Outcomes by Tree Branch



Standard game theory, as noted, would predict that A always chooses Out in all treatments. Completely selfish behavior is not observed in any treatment, however. This is unsurprising given a large body of other work finding that people do have social preferences (see, for example, Bolton and Ockenfels 2000, Fehr and Schmidt 1999, and Charness and Rabin 2002). Nevertheless, the authors observe a strong effect from communication. When the outside option for the principal leads to \$5 for each (the (5,5) treatment) and no messages are permitted, 25 of 45 (56%) A's choose to hire the agent ("In") and 20 of 45 (44%) B's choose to exert costly effort ("Roll").³² When B is able to send a message to A, considerably more outcomes are socially optimal: 31 of 42 (74%) A's choose In and 28 of 42 (67%) B's choose Roll. The (In, Roll) socially optimal profile occurs 20% of the time (9 of 45 pairs) without communication,

³² In this experiment, the strategy method was used. Each agent made a choice contingent on the principal's choosing to hire the agent. In this manner, a choice for every agent is elicited, which increases the amount of data produced by a relatively small number of recruited subjects.

compared to 50% (21 of 42 pairs) with possible communication. Similar effects are observed in the (7,7) treatment. Without communication, 12 of 48 (25%) B's choose Roll and 11 of 48 (23%) A's choose In. When B is able to send a message to A, once again the authors observe considerably more socially optimal outcomes: 24 of 49 (49%) B's choose Roll and 23 of 49 (47%) A's choose In. The (In, Roll) optimal profile occurred 8% of the time (4 of 48 pairs) without communication, compared to 31% (15 of 49 pairs) with possible communication. All of the differences mentioned in this paragraph are statistically significant at the 5% level.

The content of the messages helps us understand how communication might have changed the results. The effectiveness of communication seemed to be driven by promises (non-binding statements of intent). In the (5,5) treatment with messages, 22 of 24 A's (92%) choose to hire after receiving a promise, and 18 of 24 B's (75%) exert costly effort after making a promise; this leads to the (In, Roll) profile occurring 67% of the time. Similarly, in the (7,7) treatment with messages, 16 of 24 A's (67%) choose to hire after receiving a promise, and 20 of 24 B's (83%) exert costly effort after making a promise; this leads to the (In, Roll) profile occurring 58% of the time. Note that these numbers are much higher than in the corresponding treatments where no messages are permitted. The results also provide strong support for the notion of guilt aversion, whereby one wishes to avoid anticipated guilt triggered by disappointing another party. These results suggest that, contrary to conventional theory, judicial enforcement of contracts is not required in all cases to achieve the efficient outcome.

Charness and Dufwenberg (2010) performed another set of experiments to more closely examine the impact of communication. The results reported provide a cautionary note regarding the effectiveness of messages per se. In the same (5,5) framework as above, free-form messages were not employed; instead, each B was given two sheets of paper. One stated: "I promise to

choose Roll;” the other was blank.³³ B placed one of the two sheets in an envelope that was conveyed to the paired A. These experimenter-generated and impersonal messages were largely ineffective. Twenty-three of 41 A’s (56%) chose In after receiving a bare promise from B, and 25 of 41 B’s (61%) chose Roll after making a bare promise. These rates did not differ from those observed in Charness and Dufwenberg (2006) in the (5,5) treatment without messages; neither difference is statistically significant at the 5% level. These results suggest that individuals do not have preferences over promise-keeping per se. It seems that something beyond a preference for truth-telling is driving behavior in experiments that allow for free-form communication.

Finally, Charness and Dufwenberg (2011) test the effect of communication in an environment of hidden information (i.e., the principal is unable to observe the ability of the agent) as opposed to hidden action (i.e., the principal is unable to observe the choice of the agent). There is a 2/3 chance that an agent has low talent and a 1/3 chance the agent has high talent, and both players know this probability distribution, but only the agent knows his type. The design varies the payoffs and the ability of the agent to send a message to the principal.

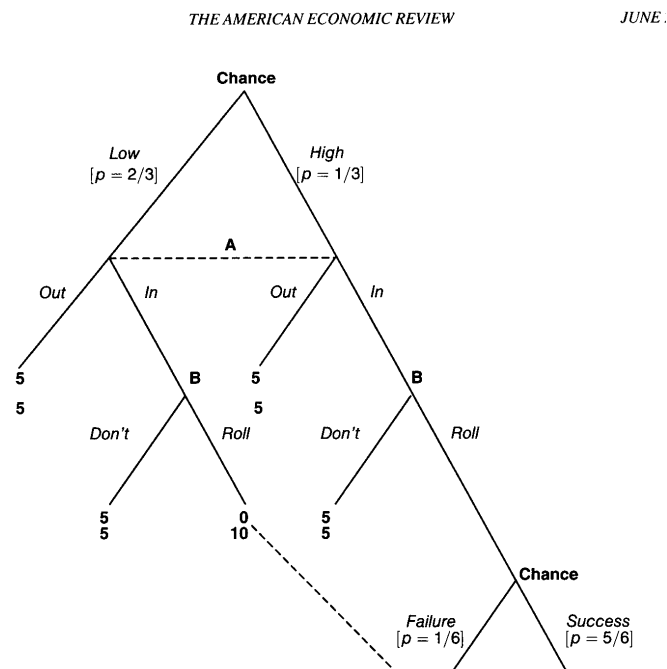
In the (5,5) treatment (see Figure 4), the principal can choose not to hire (Out), in which case the principal and the agent each receive \$5. If the principal hires the agent (In), the outcome depends on the agent’s type and choice. If the agent has high talent and chooses Roll (to exert effort), the agent receives \$10 and there is a 5/6 chance that the principal receives \$12 and a 1/6 chance that he receives \$0. If the high-talent agent chooses Don’t (to not exert effort), each person receives \$5. If the hired agent has low ability and chooses Roll, the agent receives \$10 and the principal receives \$0; if the hired low-ability agent chooses Don’t, each person receives

³³ The instructions mentioned that a promise was not binding as otherwise some B’s might have felt compelled to choose *Roll* if they promised they would, believing that this was their only choice.

\$5.

The game-theoretic prediction, which assumes that players care only about their material payoffs, can be determined using backward induction. Theory predicts B will choose Roll regardless of his ability because he will receive 10 in any event, which is larger than 7, the amount he would receive if he chose Don't. Knowing this, A's expected payoff from choosing In is \$3.33 ($\$12 \times 1/3 \times 5/6$), which is less than \$5, what he gets if he chooses Out. So, A will choose Out even though this choice fails to achieve the socially optimal outcome. This prediction is one inspiration for our system of legal enforcement of contracts.

Figure 4: The (5,5) Game from Charness and Dufwenberg (2011)



Note: A represents the principal and B represents the agent. The (5,7) game is identical except that each player receives \$7 if either agent type chooses Don't. In the (7,7) game, Out choices by A and Don't choices by both agents result in a payout of \$7 to each player.

The (5,7) treatment is nearly identical, but there is one difference, which in fact turns out to be crucial: If an agent is hired, both the agent and the principal receive \$7 if the agent chooses Don't. The theoretical predictions (when we assume that one cares only about one's own material payoffs) is the same as in the (5,5) treatment, but here even the low-ability agent can participate in an outcome that is a Pareto improvement over the principal's outside option.³⁴ The authors also conducted a (7,7) treatment, where the outside option for the principal leads to \$7 each, while each of the principal and the agent receives \$7 if the agent is hired and chooses Don't. The game theoretic prediction is the same as in the other two environments.³⁵

Note that in all treatments, if the principal (A) chooses In and the agent (B) chooses Don't, both players are better off than if the principal and agent decide in the game-theoretic predicted way (A chooses Out). So, again, we have room to intervene in some way to help the players achieve what's in their joint best interest. The source of the problem is the hidden information related to the agent's ability, so an opportunity to communicate is a potential

³⁴ A Pareto improvement is an outcome that makes at least one person better off without making anyone worse off, relative to some other outcome.

³⁵ A parallel to the (5,7) environment is the Spence (1973) signaling game, except without a materially-costly signal. In Spence's (1973) design, the agent can assume a managerial position or a clerical position. While both types of agents can perform the clerical job, only the high-ability agent can succeed at the managerial position. The principal would like to assign the agent to the appropriate position, but the agent receives more money if she chooses the managerial position (by choosing Roll). In contrast, in the (5,5) treatment and the (7,7) treatment, only a managerial position is available, so that the principal can profitably hire only the high-ability agent. A low-ability agent who is offered a position can give the principal a break only by declining the position (choosing Don't).

solution. Since any promise communicated by the agent to the principal is not binding (in the sense that the principal is not punished if he promises and then breaches his promise), theories that assume that individuals care only about material outcomes would produce the same predictions as those that arise from similar environments in the absence of information. As mentioned, however, existing evidence suggests people have social preferences. Communication might promote trust and cooperation. This might depend, though, on whether the agent has the ability to increase his payout by making a promise and then keeping the promise, as in the (5,7) treatment.

The results are summarized in Table 1. Note that the results turn on whether the low-ability agent can participate in a Pareto-improving outcome. Summarizing the results, the only case in which communication led to a significant increase was for low-talent B's in the (5,7) game, where the Don't rate nearly doubles, to 78%. Note that this rate is more than quadruple the Don't rates with communication in the two non-participation games, with statistical significance at $p < 0.001$ for each comparison. The proportions of Don't are very close in the (5,5)- and (7,7)-games, regardless of whether communication is possible. In general, it seems that low-talent B's refuse to step aside when there is no available Pareto improvement over A's outside option, but are often willing to accept lower payoffs than high-talent B's when participation is feasible.

Table 1: Results from Charness and Dufwenberg (2011)

Rates by Treatment and Tests for the Effect of Communication

Treatment	Low B's Don't		Z-stat	A's In		Z-stat
	M	NM		M	NM	
(5,7)	18/23 (78%)	8/20 (40%)	2.56***	33/41 (80%)	28/40 (70%)	1.09
(5,5)	3/16 (19%)	2/13 (15%)	0.24	24/47 (51%)	20/45 (44%)	0.64
(7,7)	2/11 (18%)	3/13 (23%)	-0.29	21/42 (50%)	18/40 (45%)	0.45

The patterns in the messages sent reveal that the effect of communication is driven by promises by low-ability agents in the (5,7) treatment. While high-ability agents typically state that they have high-ability (as do about 20% of the low-ability agents in each of the three treatments), many low-ability agents in the (5,7) treatment state that they have low ability but that they will choose Don't, leading to \$7 for each of A and B. It turns out that A's who receive such messages choose In 93% of the time. What is perhaps more surprising is that each and every low-ability agent who sent this message chooses Don't when this promise has been made.

These results provide some 'useful lessons' that, on extrapolation, may offer guidance for those who wish to detect whether someone else is being honest. A claim that the agent has high talent should be viewed with some suspicion, as it may well be "the big lie." However, when participation is possible regardless of the agent's talent, the claim that someone has low talent but will do his best turns out to be completely reliable, and is in fact almost always believed by the principal; it seems that one can trust people who confess imperfections. Perhaps people are substantially more prone to be cooperative when they can participate by having a voice and choosing an action that yields improvements in material payoffs for all parties involved than

when the only way to gain is at the expense of others. This may help to explain many market situations, which differ over the availability of Pareto-improvements. For example, e-commerce furnishes settings in which the quality of the good traded is not readily observable, and it may or may not be the case that all sellers have the ability to provide a good that buyers value.

Brandts, Charness and Ellman (2012) consider the effect of communication in a buyer-seller framework, in which a cost shock occurs after the parties agree to an initial price. Two individuals are randomly paired, and the buyer proposes a price to the seller. The seller can choose to accept or reject this price. If rejected, each party receives the disagreement payoff of 5. If the seller accepts the offer, she then chooses a quality level, which determines the buyer's gross trade value (10, 30 or 45). However, before this quality choice is made, the seller observes a cost of either 0 or 20 (with equal likelihood).

Two forms of contract can be offered. The first is a rigid contract, in which the price is set in the beginning and cannot be changed. The second is a flexible contract, in which an initial price is set, but the price can be augmented by an additional payment after the cost shock is realized (and observed by the buyer). In either case, the seller observes the final price and decides on quality level -1, 0, or 1. A quality level of 0 is costless, while choosing -1 or 1 costs the seller one unit. With a quality level of -1, the buyer receives the original endowment of 5 plus an additional 10 payoff units; with a quality level of 0, the buyer receives the original endowment of 5 plus an additional 10 payoff units plus 20 more units for the higher quality; with a quality level of 1, the buyer receives the original endowment of 5 plus an additional 10 payoff units plus 35 more units for the highest quality.

In principle the flexible contract seems clearly superior, as the buyer and seller can in effect

share the cost shock. However, the seller has no particular reason to believe that a buyer who offers a flexible contract will in fact pay an additional amount after the cost shock is revealed. This uncertainty can lead to a rigid contract being more profitable for the buyer, as found in Fehr, Hart and Zehnder (2015). Brandts, Charness and Ellman (2012) test for the effect of communication (free-form chat) on the type of contract offered, the quality chosen, and buyer and seller profits. Two treatments (chat and no chat) were conducted, each with four sessions.

The rigid contract is in fact more prevalent (chosen 55% of the time) and leads to slightly higher earnings for the buyer when no communication is permitted. The buyer receives an average payoff of 10.80 and the seller receives an average payoff of 7.81 when a rigid contract is offered. By comparison, the buyer receives an average payoff of 9.73 and the seller receives an average payoff of 7.96 when a flexible contract is offered. The distribution of $(-1,0,1)$ quality choices, given an accepted contract, was (31%, 65%, 4%) with rigid contracts and (40%, 56%, 4%) with flexible contracts.

However, matters change dramatically when communication is allowed. In treatment 2, the proportion of rigid contracts offered was only 25%. The buyer receives an average payoff of 12.66 and the seller receives an average payoff of 16.68 when a rigid contract is offered. Earnings are substantially higher with a flexible contract—the buyer receives an average payoff of 17.02 and the seller receives an average payoff of 20.95 when a flexible contract is offered. The distribution of $(-1,0,1)$ quality choices, given an accepted contract, was (14%, 36%, 50%) with rigid contracts and (6%, 20%, 74%) with flexible contracts. The differences in the proportion of rigid contracts offered, the payoffs for the buyer and the seller, and the distribution of quality choices are all significantly different across treatments. These results are further evidence that communication, even when non-binding, can foster trust and trust-worthiness.

In an additional treatment with restricted chat (the buyer is allowed to state only how much he would pay with and without a cost shock), the authors find no effect on quality or profits compared to the baseline with no chat. This holds for both rigid and flexible contracts. This result adds to the growing evidence that a richer and endogenous form of communication is needed to move behavior away from a unique, but socially-inferior, equilibrium when own-payoff maximization is assumed. These sorts of studies refine our understanding of the need for judicial enforcement of agreements.

CONCLUSION

The role and importance of laboratory experiments in research related to law and economics has been and will continue growing. As expectations for reliable estimates from field data move further in the direction of natural experiments, the laboratory will continue to be an environment where manipulation of context and parameters can be profitably undertaken. We once again point out the value of incentivized experiments rather than hypothetical ones, whenever this is feasible.

In our distillation of previous research, we discussed several law and economics contexts that have been applied in the laboratory. The last decade of research has produced a considerable amount of work from legal scholars, psychologists, political scientists and economists in the area of law and economics. This research has examined the role and magnitude of cognitive and social biases in decision-making by many legal actors (attorney, judges and juries). Additionally, the heuristics that legal actors/agencies utilize in reaching their decisions/conclusions and enforcing the legal code are examined in order to determine the role that information, risk, uncertainty and bargaining play in these processes.

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