


6-1-1988

A Coasean Experiment on Contract Presumptions

Stewart J. Schwab

Cornell Law School, sjs15@cornell.edu

Follow this and additional works at: <http://scholarship.law.cornell.edu/facpub>

 Part of the [Contracts Commons](#), and the [Property Law and Real Estate Commons](#)

Recommended Citation

Schwab, Stewart J., "A Coasean Experiment on Contract Presumptions" (1988). *Cornell Law Faculty Publications*. Paper 535.
<http://scholarship.law.cornell.edu/facpub/535>

This Article is brought to you for free and open access by the Faculty Scholarship at Scholarship@Cornell Law: A Digital Repository. It has been accepted for inclusion in Cornell Law Faculty Publications by an authorized administrator of Scholarship@Cornell Law: A Digital Repository. For more information, please contact jmp8@cornell.edu.

A COASEAN EXPERIMENT ON CONTRACT PRESUMPTIONS

STEWART SCHWAB*

DESPITE the theoretical importance of the Coase Theorem, scholars have given surprisingly little attention to verifying its predictions empirically. Supporters often accept the theorem as dogma, while armchair critics assail its assumptions. In an exciting series of recent articles, however, Elizabeth Hoffman and Matthew Spitzer¹ have presented experi-

* Associate Professor, Cornell Law School. I thank Greg Alexander, Kevin Clermont (for the title and its spelling), George Hay, Bob Hillman, Jon Macey, Peter Railton, John Siliciano, and Fred Zacharias. I also received helpful comments from Richard Thaler and other participants at a collective bargaining workshop at Cornell's Industrial and Labor Relations School. I thank John Burton for suggesting that I expand the study and David Lipsky for making that possible. Keith Shugarman provided able research assistance.

Lest anyone think the spelling in the title be anything less than the result of herculean research, let me give the following analysis of "Coasean" versus "Coasian." The suffix at issue is "-ian," meaning "of or pertaining to." The general rule is to drop a silent "e" before adding a suffix beginning with a vowel, as in collegian. This would suggest "Coasian." But *Webster's Third New International Dictionary (Unabridged)*, at 22a, notes that proper names are usually treated differently. A "Burkean" might suggest that tradition or usage rather than principle might control. In the hope that a "Lockean" right would accrue to the investment of labor, I searched Westlaw. Neither "Coasian" nor "Coasean" has ever appeared in a state or federal case. Westlaw, with a limited data base, reports eleven law review articles using "Coasean" and eleven using "Coasian"—not decisive. Turning to Lexis, with its different but overlapping data base, a search revealed eleven articles using "Coasian" and twenty-one articles using "Coasean." Authors in this data base using "Coasean" include Ackerman, Alexander, Coffee, Coleman, Ellickson, Epstein, Frug, Grey, Gilson, Kelman, Peller, and Priest. Authors using "Coasian" include Calabresi, Carney, Fletcher, Haddock and Macey (in a title), Hoffman and Spitzer (in a mimeographed title), Lowi, Levmore, and Romano. The Coaseans seem to have it in numbers. From all this I conclude that the Schwabian practice, at least, will be "Coasean." But as a Shakespearean might say, "A Coasean by any other name would question the Pigovian tax."

¹ Elizabeth Hoffman & Matthew Spitzer, *The Coase Theorem: Some Experimental Tests*, 25 J. Law & Econ. 73 (1982) (hereinafter *Some Experimental Tests*); Elizabeth Hoffman & Matthew Spitzer, *Entitlements, Rights, and Fairness: An Experimental Examination of Subjects' Concepts of Distributive Justice*, 14 J. Legal Stud. 259 (1985) (hereinafter *Distributive Justice*); Elizabeth Hoffman & Matthew Spitzer, *Experimental Tests of the Coase Theorem with Large Bargaining Groups*, 15 J. Legal Stud. 149 (1986) (hereinafter

[*Journal of Legal Studies*, vol. XVII (June 1988)]

© 1988 by The University of Chicago. All rights reserved. 0047-2530/88/1702-0007\$01.50

mental evidence, as have others,² that largely supports the Coasean prediction that bargainers will negotiate around inefficient property rights to reach a Pareto-optimal³ solution. The methodology has even gained sufficient attention to have its detractors.⁴

The existing experiments analyze the results of bargains when one side has the power to impose unilaterally one outcome but can negotiate with others for other outcomes. As discussed below, the unilateral power of one side makes these experiments most insightful to the world of property and tort. The present article, by contrast, analyzes the efficiency and distributive effects of a contract presumption, whereby the nominal beneficiary must obtain the contractual consent from the other side before benefiting from the rule. The experiment tends to confirm the Coasean prediction that contract presumptions do not affect the efficiency of bargains. The results question, however, the Coasean wisdom that contract presumptions should not affect the distribution of wealth between the parties.

I. PROPERTY RIGHTS, CONTRACT RULES, AND THE COASE THEOREM

Part of the concept of private property is that the owner may use or consume his entitlement without the permission of others. The classic illustration, from Coase himself, is the property right of the rancher or farmer under an open-range or enclosure law.⁵ The Coase Theorem predicts that, absent transaction costs, the entitlement holder will use the entitlement only if he is the efficient user. If not, the Coase Theorem predicts, he will make himself better off by trading the entitlement (for a price) to someone who values it more highly. The initial placement of the

Large Bargaining Groups); Don Coursey, Elizabeth Hoffman, & Matthew Spitzer, *Fear and Loathing in the Coase Theorem: Experimental Tests Involving Physical Discomfort*, 16 *J. Legal Stud.* 217 (1987). See also Elizabeth Hoffman & Matthew Spitzer, *Experimental Law and Economics: An Introduction*, 85 *Colum. L. Rev.* 991 (1985) (hereinafter *Experimental Law and Economics*).

² Glenn Harrison & Michael McKee, *Experimental Evaluation of the Coase Theorem*, 28 *J. Law & Econ.* 653 (1985).

³ A Pareto-optimal solution is one where no party can be made better off without making another party worse off. If the parties can make side payments as well as trade rights (the typical bargaining setting), a Pareto-optimal solution will maximize the joint gains to the parties. A Pareto-optimal solution is often called an "efficient" solution, terminology I will adopt for this paper.

⁴ See Mark Kelman, *Comment on Hoffman and Spitzer's Experimental Law and Economics*, 85 *Colum. L. Rev.* 1037 (1985). See also Michael Gordon, Neal Schmitt, and Walter Schneider, *Laboratory Research on Bargaining and Negotiations: An Evaluation*, 23 *Industrial Relations* 218 (1984).

⁵ For a detailed study of these laws, see Kenneth R. Vogel, *The Coase Theorem and California Animal Trespass Law*, 16 *J. Legal Stud.* 149 (1987).

property right thus does not affect the efficient use of the property.⁶ The initial placement has created wealth for the holder, however, and thus affects the distribution of wealth between the parties. The price the holder extracts for waiving his property right allows the holder to increase his wealth created by the property right.⁷

A contract rule differs from a property entitlement in that the nominal beneficiary of the contract rule must obtain the other party's signature before benefiting. If no contract exists, no one can gain from the rule. Because both sides to a contract simultaneously agree to create and distribute wealth, the distributive effect of contract rules is muted.

Contract rules are of two types. Some rules are coercive, mandating that certain terms appear in any contract. For example, a housing code may require that landlords furnish air conditioners in all apartments. Other rules are presumptions. Presumptions interpret silent or ambiguous contracts but allow parties to alter or waive the presumed interpretation with specific language. For example, a court might presume that apartment leases warrant habitability unless specific language suggests otherwise.

Coercive contract rules either have no efficiency or distributive effect or are inefficient with uncertain distributive effects. If the nominal beneficiary would have obtained the coerced benefits without legal regulation, the law is superfluous. For example, suppose in an unregulated market all tenants would rent air conditioners for \$30 per month for their \$500-per-month apartments. A rule mandating that landlords supply "free" air conditioners will not benefit (or hurt) tenants. The rent will simply rise by \$30.

⁶ Calabresi and Melamed, in a famous article, distinguish between property rules: whereby the entitlement holder maintains the entitlement until he sells it at a price he chooses—and liability rules—whereby tortfeasors can interfere with the entitlement by paying court-awarded damages. They conclude that property rules are more efficient unless significant transaction costs or holdup or free rider problems make private trades difficult. Guido Calabresi & Douglas Melamed, *Property Rules, Liability Rules, and Inalienability: One View of the Cathedral*, 85 Harv. L. Rev. 1089 (1972). Under either a property or a liability rule, the entitlement holder can use the entitlement without the consent of others. In the present article, I lump both types of rules together as property rules and contrast them with contract rules (that is, rights or presumptions which require a contract before being exercised). My framework would divide Calabresi and Melamed's third category of entitlements, inalienable entitlements. Some inalienable entitlements can be enjoyed without others' consent, even though they cannot be sold (Calabresi and Melamed suggest the right to one's kidney). These create immediate wealth in the holder, so I would classify them as property rights. Other inalienable entitlements cannot be enjoyed without another's consent (Calabresi and Melamed suggest the right against due-on-sale clauses). These I would treat as coercive contract rules.

⁷ See Harold Demsetz, *When Does the Rule of Liability Matter?* 1 J. Legal Stud. 13 (1972) (hereinafter *Liability*); Harold Demsetz, *Wealth Distribution and the Ownership of Rights*, 1 J. Legal Stud. 223 (1972) (hereinafter *Wealth Distribution*).

A coercive rule is inefficient if it mandates more benefits than some unregulated contracts would call for. For example, suppose some tenants would not purchase an air conditioner, showing that they prefer (and landlords are willing to supply) non-air-conditioned, \$500 apartments to air-conditioned \$530 apartments. The coercive air-conditioning law forbids this mutually desired transaction. Unless the transaction affects third parties or the market suffers from limited information or some other imperfection, the coercive rule should be less efficient than the unregulated market. How the parties share the efficiency losses depends on the shape of the supply-and-demand curves.⁸

In sharp contrast to coercive contract rules, law-and-economics scholars view contract presumptions as innocuous when transaction costs are low. A well-designed contract presumption is thought to be a standard, off-the-rack clause that parties put into a typical contract. If the parties value another clause more highly, they can simply substitute that clause in the contract.⁹ Which side the presumption favors thus has no efficiency or distributive effects. Table 1 summarizes the standard views.

A nonunionized, competitive labor market nicely illustrates a contract presumption's theoretical inability to influence the distribution of wealth.¹⁰ First, suppose the competitive labor market in East State operates under a "tenure" rule of contract interpretation. If an individual worker's contract says nothing about job security, the law forbids a firm to replace the worker during the contract term with someone willing to work for less. Suppose further that workers value job security more highly than employers value the freedom to change workers. If so, says the Coase Theorem, employees will not negotiate away the tenure rule's presumption of job security. Suppose, in equilibrium, labor contracts in East State call for a wage of \$6 (plus the tenure rule). Equilibrium implies that any other wage/benefit package that workers would prefer is less profitable to employers.

⁸ The theoretical and empirical issues involved in determining the actual distributive impact of coercive housing codes are well summarized in Werner Hirsch, *Law and Economics: An Introductory Analysis* 43-58 (1979).

⁹ For a redirection of this standard view of contract presumptions, see Charles Goetz & Robert Scott, *The Limits of Expanded Choice: An Analysis of the Interactions between Express and Implied Contract Terms*, 73 *Calif. L. Rev.* 261 (1985).

¹⁰ Demsetz, in his classic article on wealth distribution and the Coase Theorem, uses a rule requiring employers to compensate workers for on-the-job accidents in order to illustrate that coercive contract rules will not affect the distribution of wealth. Demsetz, *Wealth Distribution*, *supra* note 7. Demsetz notes that this coercive rule will affect distribution if workers would have self-insured rather than bought insurance in the absence of the rule, for the rule prevents this choice. *Id.* at 225-26.

TABLE 1
STANDARD THEORY OF PROPERTY AND CONTRACT RULES
(Assuming No Transaction Costs)

	Property Rights	Coercive Contract Rules	Contract Presumptions
Effect on efficiency	None	Adverse, unless rule is superfluous	None
Effect on distribution	Increase holder's wealth	Unclear which direction, unless none	None

West State has a no-tenure rule, so that firms may replace employees with cheaper workers during the contract term unless the contract specifies otherwise. (Workers do not migrate between the two states.) Workers must bargain to obtain the job protection presumed by law in East State. According to the Coase Theorem, however, if the two states are otherwise identical (for example, workers value job security equally, technology is identical, and labor contracts are equally costless to transact and enforce), the different legal presumptions will not alter the labor contract. The implicit tenure clause in East State contracts will simply be an express clause in West State contracts. Any West State firm that refused to waive its nontenure presumption would have no applicants for work unless the firm paid more than \$6. A high-wage, no-tenure firm that attracts workers, however, would be uncompetitive because this package is less profitable than the equilibrium package of \$6 with tenure. The contract presumption has no effect because it has not changed how workers and firms value various wage/tenure packages.

The preceding argument relied on competitive markets to show that a legal rule would not alter the wealth of otherwise identical workers. In a noncompetitive labor market, such as exists with unions, the question remains whether initial entitlements affect the distribution of wealth. Consider the *Milwaukee Spring* cases,¹¹ which mirror the tenure/no-tenure example already discussed. The issue there was whether a company could transfer work from the unionized plant to its nonunionized plant during the collective-bargaining contract term to avoid the high union wage. The contract had no explicit clause on point. In *Milwaukee I*, the National

¹¹ *Milwaukee Spring Div.*, 265 N.L.R.B. 206 (1982) (*Milwaukee I*), rev'd on reh'g, 268 N.L.R.B. 601 (1984) (*Milwaukee II*), aff'd sub nom. *UAW v. NLRB*, 765 F.2d 175 (D.C. Cir. 1985).

Labor Relations Board held that, absent express language in the collective bargaining contract, a company could not transfer work to avoid the high union wage during the contract term. In *Milwaukee II*, the Board reversed itself and allowed the company to transfer work absent an express must-stay clause.

Suppose, under *Milwaukee I*, a strong union threatened a crushing strike and thereby obtained a wage of \$8 and also kept the prohibition on work transfer. *Milwaukee II* does not diminish the union's control over the labor supply and thus does not reduce its economic bargaining power. Thus, under the standard theory, an equally strong union under *Milwaukee II* should be able to obtain an \$8 wage and force the company to waive its right to transfer work. Under this view, then, the initial contract presumption influences neither efficiency nor distribution.

The present experiment was designed in part to test the effect of the *Milwaukee Spring* cases on wealth distribution between firms and unions. To anticipate the results, it finds, contrary to the standard model, that a party with the contract presumption in its favor will be more successful in negotiations.

II. HYPOTHESES TO BE TESTED

As previously discussed, the Coase Theorem asserts that a change in contract presumption affects neither the efficiency of contracts nor the distribution of wealth between the parties. To turn the efficiency assertion into testable hypotheses requires some refinements.

Probably the most common formulation of the Coase Theorem asserts that, absent transaction costs, interacting parties will reach an efficient outcome even if the law awards initial legal entitlements to less valued uses.¹² I will call this formulation the "strong efficiency hypothesis."

STRONG EFFICIENCY HYPOTHESIS: 100 PERCENT EFFICIENCY. All bargainers reach an efficient outcome, regardless of the initial contract presumption under which they bargain.

It focuses on the efficiency of the bargain rather than the effect of the law. Most experimental work (which has examined the efficiency of property rules rather than contract presumptions) has tested this hypothesis. Hoffman and Spitzer find that over 90 percent of the bargainers make an

¹² For example, Mitchell Polinsky states as a simple version of the Coase Theorem: "If there are zero transaction costs, the efficient outcome will occur regardless of the choice of legal rule." Polinsky, *An Introduction to Law and Economics* 12 (1983).

efficient bargain.¹³ Harrison and McKee reach similar results.¹⁴ These results tend to support the 100 percent efficient hypothesis.¹⁵

An alternate formulation of the Coase Theorem would focus on the effect of changes in legal rules rather than the efficiency of bargains. I term this formulation the "weak efficiency hypothesis." This hypothesis asserts that rule makers will not alter the level of efficiency in the market when they award an entitlement to party A or party B.

WEAK EFFICIENCY HYPOTHESIS: PRESUMPTION DOES NOT AFFECT EFFICIENCY. The initial contract presumption does not affect the proportion of bargains that are efficient, and a substantial proportion of bargainers reach efficient bargains, regardless of the initial contract presumption under which they bargain.

The weak efficiency hypothesis is more modest because it does not insist that all bargains will reach an efficient outcome. Some inefficient bargains may occur, perhaps because of inertia, imperfect information, or other vagaries of bargaining. But, claims the weak efficiency hypothesis, any deviations from efficiency will not arise from differences in contract presumptions. All situations satisfying the strong efficiency formulation will also satisfy the weak formulation because if all bargains are always efficient the contract presumption cannot increase or decrease efficiency. But even if not all bargainers reach efficient outcomes (thus violating the strong efficiency hypothesis), the weak efficiency hypothesis can be satisfied as long as the legal rule does not alter the overall level of inefficiency in the market.

Coase's classic parable of farmers and ranchers can illustrate the two formulations. Suppose West State has an open-range law and East State

¹³ In their initial experiments, Hoffman & Spitzer found that 102 of 114 sets of bargainers reached the joint-profit maximum. Some Experimental Tests, *supra* note 1, at 92. In their experiments with large bargaining groups, Hoffman & Spitzer found that "93 percent of the experimental decisions chose the profit-maximizing outcome." Large Bargaining Groups, *supra* note 1, at 156. In their study focusing on methods of selecting the controller, seventy-eight of the eighty-six decisions reached the joint-maximizing outcome. Distributive Justice, *supra* note 1, at 276.

¹⁴ Harrison & McKee found that thirty-nine of the forty-one decisions where the controller had a unilateral property right reached the joint-maximizing result. Harrison & McKee, *supra* note 2, at 663.

¹⁵ Technically, observing even a single nonefficient bargain would falsify the 100 percent efficiency prediction, unless one posits error in observation as the source of deviation from the predicted results. Normal hypothesis testing of samples from a population breaks down when the null hypothesis assumes that every element of the population has the same characteristic.

has an enclosure law. In both states land is more valuable at the margin when used to grow corn than when used to graze cattle. The strong efficiency hypothesis of the Coase Theorem predicts that no corn will be trampled in either state, by enforcing the enclosure rule in East State and by private bargain between ranchers and farmers in West State. The weak efficiency hypothesis of the Coase Theorem predicts that the same amount of corn will be trampled in each state. For example, if 80 percent of the corn is not trampled in East State and 80 percent not trampled in West State (a few ranchers and farmers in each state making inefficient bargains), the strong efficiency hypothesis would be rejected, but the weak efficiency hypothesis would not be rejected.

The choice of the 80 percent figure, while only illustrative, is selected to convey a clear message. The weak version of the efficiency hypothesis is *not* confirmed if the percentage of contracts that reach the efficient solution is at best random. Take the extreme case where no contract is efficient. In such a case of massive inefficiency, it is true but uninteresting that the legal rule does not affect efficiency. If bargainers tend toward efficiency, however, confirmation of the weak efficiency hypothesis that the contract presumption will not increase or decrease this tendency has important policy implications.

Previous experiments have not carefully distinguished between these formulations, and the structure of those experiments has not allowed for distinct testing of the weak efficiency hypothesis. As discussed below, previous experiments have only a single legal rule (the controller can unilaterally pick any number), while testing this second hypothesis necessarily requires contrasting a legal rule with its converse (for example, workers have a favorable contract presumption, or firms do).¹⁶ A major purpose of the present experiment is to test the weak efficiency hypothesis of the Coase Theorem.

The third Coasean hypothesis to be tested concerns distribution. As we have seen, standard Coasean theory predicts that contract presumptions have no wealth effects.

NO DISTRIBUTIVE EFFECT HYPOTHESIS: CHANGE IN PRESUMPTION DOES NOT AFFECT WEALTH DISTRIBUTION. Buyers (and sellers) will be as well off with bargains struck under a waivable contract presumption favoring buyers.

¹⁶ Harrison & McKee, *supra* note 2, have conducted experiments contrasting situations where a single controller can unilaterally pick a number (a unilateral property right) from situations where both parties must agree on a number or else a number will be chosen randomly or no payoff is given (a joint property right). Parties in both situations seemed equally likely to reach a joint-maximizing result.

III. PRIOR EXPERIMENTS ON PROPERTY RIGHTS

The basic design of the Hoffman/Spitzer experiments is that an experiment presents two bargainers¹⁷ with a series of numbers and tells each party the payoff for each number. One person, labeled the controller,¹⁸ picks the number. The controller may pick the number that maximizes his individual payoff, but the experiment is constructed so that this number gives little to the other party. The experiment allows and enforces side payments if the controller chooses another number.

Although the Hoffman/Spitzer experiments are abstract, they tend to support the accuracy of the Coase Theorem as applied to property rights. One can view the controller in these experiments as having a property right to pick any number with impunity. The entitlement holder can unilaterally exercise this right or trade it to a higher-valued use. The experiments find that the controller overwhelmingly picks the number that maximizes the joint gains, rather than the personal gain of the controller.¹⁹ From this result, Hoffman and Spitzer find support for the Coase Theorem's prediction that bargainers, under certain conditions,²⁰ will reach an efficient bargain regardless of the initial legal entitlement.

Greater debate exists on whether the experimental evidence supports the Coasean hypothesis that property rules alter the distribution of wealth between the parties. Hoffman and Spitzer found in their initial experiments that most bargainers split the total payoff essentially equally, even though this meant that the controller received a smaller payoff than he could receive without bargaining.²¹ This finding would suggest that unilateral property rights have little effect on distribution and accordingly casts doubt on whether bargainers try to maximize individual payoffs. Harrison and McKee, expanding the Hoffman and Spitzer experiment specifically to examine this point, found that, after "training" subjects about the significance of being a unilateral controller by having them bargain first without a controller, most controllers demanded at least their individual

¹⁷ The initial Hoffman & Spitzer experiments involved two and three bargainers. In later experiments they have found that the Coase Theorem might apply accurately to disputes involving as many as thirty-eight parties. See *Large Bargaining Groups*, *supra* note 1, at 162.

¹⁸ Usually, the controller is designated randomly by a coin flip. Hoffman & Spitzer have explored whether meritocracy methods of designating the controller affect the results. They find that bargainers reach the efficient outcome under either method but that divisions of the surplus are more even when a nonmerit method of designating the controller is used. See *Distributive Justice*, *supra* note 1.

¹⁹ See note 13 *supra*.

²⁰ Hoffman & Spitzer delineate eight assumptions of the basic Coase Theorem, the most familiar being zero transaction costs. See *Experimental Tests*, *supra* note 1, at 73. Much of Hoffman & Spitzer's work tests whether the Coasean prediction can be maintained when these assumptions are relaxed.

²¹ Sixty-two of 114 bargainers split the payoffs equally or within \$1 of an even split. See *Experimental Tests*, *supra* note 1, at 92.

no-agreement maximum.²² Hoffman and Spitzer, in a later experiment, similarly found that controllers who "earned" the right to be a controller by winning a prebargaining trigger game were more likely to receive a payoff at least equal to their individual no-agreement maximum. These later results thus suggest, consistent with predictions, that recipients of a unilateral property right will receive larger payoffs. In sum, the experiments tend to confirm the Coasean efficiency and distributive predictions for property rights as presented in Table 1.

IV. DESIGN OF THIS EXPERIMENT

As part of my regular law school labor law classes²³ and David Lipsky's industrial and labor relations (ILR) classes on collective bargaining theory,²⁴ 222 students were paired²⁵ and asked to bargain²⁶ over a collective bargaining contract. The study obtained 108 usable contracts.²⁷ Students were given a regularly scheduled class period for their initial bargaining session, and about half the students completed negotiations and signed their contract in this period. Students were allowed to schedule additional bargaining sessions on their own if needed. They were told that the union

²² They found that thirteen of seventeen controllers demanded at least their unilateral maximum. Harrison & McKee, *supra* note 2, at 663.

²³ The experiment was conducted with three law school classes: spring 1985, spring 1986, and fall 1986. Enrollment ranged from twenty to fifty students.

²⁴ These classes were the fall 1985 and fall 1986 ILR 601 classes of the New York State School of Industrial and Labor Relations at Cornell University. Enrollment was about seventy students in each class, about 8 percent graduate students and the rest junior and senior undergraduates.

²⁵ In the first class, students sitting next to each other in the assigned seats were paired, with the first name being the union representative and the second name being the company representative. Because students were assigned the seat they chose on the third day of class, they could well be bargaining against a friend. To avoid this problem, pairings were done alphabetically in the other four classes. Results from the first class did not differ significantly from other classes. A general problem of conducting in-class experiments, however, is that bargainers will often know each other. This problem is less severe when the experiment obtains subjects by advertisement and pays them for their time. Nevertheless, using in-class students is probably preferable overall because it avoids the severe problem that paid students may want to milk as much money as possible from the experiment regardless of self-interest, a goal that is difficult to disentangle from the basic Coasean hypothesis. (I thank Richard Thaler for this point.)

²⁶ We were careful to say nothing about the purpose of the negotiating session until it was over and implied that it was simply an exercise to gain experience in collective bargaining. Nevertheless, because the bargainers came from these classes, more subjects probably had a general awareness of the Coase Theorem and game theory than the typical subject in experiments of the Coase Theorem. See Harrison & McKee, *supra* note 2, at 656 ("All subjects were recruited from the economics undergraduate program at the University of Western Ontario. None had received any formal exposure to the Coase Theorem or game theory.").

²⁷ Three contracts were excluded from the analysis because of ambiguities that could not be resolved by examining the postbargaining student description of the bargaining session.

would strike twenty-four hours after the bargaining began and that each side would lose ten points for every hour the contract signing was delayed after that. Thus, the negotiations were under a limited, but rather generous, time constraint.²⁸ All students were asked to submit a week later a two-page written description of the bargaining process, outlining bargaining strategies, stumbling blocks, and ambiguities. These descriptions allowed the researcher to check for problems and validate the results. Unlike most bargaining experiments, students were not paid to come nor given monetary payoffs based on their performance. Sufficient motivation was present, however, because negotiations were part of a regular class in which class performance was a factor in the grade. Law students, in particular, are motivated by such incentives.

Each student was given a preference sheet that showed how many points he or she would receive for various outcomes. Students could tell their counterpart how much they valued an item but were forbidden from proving their credibility by showing their counterpart their preference sheet. (See the Appendix for copies of general instructions, the contract, and union and management preference sheets given to students.) Thus, unlike most previous experiments, bargainers here did not have perfect information of the other side's payoffs,²⁹ and bluffing and other forms of strategic behavior were possible.³⁰

²⁸ Harrison & McKee suggest that a fixed amount of time for bargaining is preferable to an indeterminate amount of time. See Harrison & McKee, *supra* note 2, at 658 n.13. A time constraint avoids the problem of extraneous pressures on the time allowed for bargaining (if one bargainer has a pressing appointment) and ensures that potential payoffs are commensurate with the opportunity cost of the time spent in the experiment. Because students in this experiment were not paid, the second concern is irrelevant here. Because the present experiment gave bargainers twenty-four hours, however, extraneous time pressure was a complicating factor. This was especially true for the ILR collective-bargaining classes, where the experiment was given on the Thursday at 12:20 P.M. before fall break began on Friday at 5:00 P.M. With buses and planes to catch at various times, negotiators could easily be under different time pressures; however, given the random assignments, there is no reason to think these extraneous time pressures varied systematically between union and management negotiators.

²⁹ Hoffman & Spitzer have included as one of the necessary assumptions of the Coase Theorem that bargainers know each other's preferences. Some Experimental Tests, *supra* note 1, at 73. Jules Coleman has suggested on theoretical grounds that this highly restrictive assumption is unnecessary. See Coleman, Efficiency, Exchange, and Auction: Philosophic Aspects of the Economic Approach to Law, 68 Calif. L. Rev. 221, 223 n.6 (1980). Hoffman & Spitzer have studied the effect of bargainers not knowing the payoff of the other side but being free to tell the other participant anything he or she wished about the value of each number to him or her. In two-party bargaining, they found no reduction in the (very high) percentage of parties reaching the joint-maximizing result. In three-party bargaining where two parties are joint controllers, limited information did lessen the percentage of bargains that reached the joint-maximizing result. See Some Experimental Tests, *supra* note 1, at 92.

³⁰ Students were allowed to file unfair labor practice charges for illegal bargaining tactics. Three students filed such charges, but (as typically occurs) the charges were resolved

Unlike previous experiments that ask bargainers to choose a single number, these negotiations were significantly more complex. The setting for the experiment was the negotiation of a collective bargaining agreement between an employer and a new union that had just been elected as bargaining representative for its workers. Because the union was newly chosen, no history of past collective-bargaining agreements could set the stage for this round of negotiations or make legitimate, if only implicitly, one approach to the exclusion of another.³¹ Instead, the preference sheet of management negotiators revealed that, at the outset of negotiations, the current, nonunion wage at the plant was \$5.80/hour with five vacation days per year. Labor law rules, familiar to the students, require management to divulge this basic information to union negotiators to enable meaningful bargaining. This information provided a starting point for negotiations, but no other information was given suggesting whether any proposed wage/vacation/transfer package was high or low. Unlike many previous experiments, then, bargainers were operating with limited information about what the other side thought a good contract was.

With the framework thus set, the students bargained over three items in the contract.³² First, negotiators bargained over the wage. Between \$6 and \$12, the wage negotiations were straight competitive bargaining: the union gained one point for every penny the wage increased, while management lost one point.³³

The second subject of negotiations was vacation time, which allowed joint gains from bargaining. The union received twenty points for each vacation day obtained, up to fifteen days, while management lost only ten

informally. See Stewart Schwab, *Collective Bargaining and the Coase Theorem*, 72 *Cornell L. Rev.* 245 (1987), for an argument that labor law's requirement of good faith bargaining mitigates some of the inefficiencies that can occur from bluffing and other strategic behavior.

³¹ For a general discussion explaining how prior "reference transactions" may influence prices or wages when conditions change, see Daniel Kahneman, Jack L. Knetsch, & Richard Thaler, *Fairness as a Constraint on Profit Seeking: Entitlements in the Market*, 76 *American Econ. Rev.* 728 (1986).

³² A fourth item of negotiations benefitted both parties equally if included in the contract. Both union and management received 100 points for a noise reduction clause. (Management was told on its preference sheet that a recent study had shown that productivity increased if noise were reduced.) This item was included for pedagogical reasons, to illustrate most forcefully in subsequent lectures that mutual gains were possible in labor negotiations. Student write-ups revealed that some management negotiators bluffed that they were not interested in noise reduction. Nevertheless, all bargainers included a noise reduction clause in their contracts.

³³ Outside this range, one side lost more than the other side gained. Below \$6, the union lost ten points for every penny drop in the wage, while management gained only one point. Above \$12, management lost ten points for every penny rise in the wage, while the union gained only one point. The experiment resulted in no contracts with a wage above \$12 and only two contracts with a wage below \$6.

points for each vacation day granted. Beyond fifteen days, mutual gains from bargaining were impossible; the union received ten points while management lost ten points for each additional vacation day.³⁴

The third subject of bargaining, the relocation clause, was the focal point of the experiment. Bargainers were to negotiate whether the company had the right to transfer work to its nonunion plant during the three-year contract term³⁵ if the bargained wage proved excessive. The bargainers were divided into two preference groups. In group A, the union received 300 points for a clause forbidding the company from transferring work (a stay clause), while management received only 200 points for a clause allowing the company to transfer work (a go clause). In group B, management received 300 points for a go clause while the union received only 200 points for a stay clause.

Both groups A and B were subdivided according to the legal presumption that would govern the contract in the absence of an express contract provision. Subgroup 1 negotiators bargained under the shadow of *Milwaukee I*, which presumes that a company must stay with union workers for the contract term unless the contract explicitly says otherwise.³⁶ Subgroup 2 negotiators bargained under *Milwaukee II*, which presumes that a company may transfer work during the contract term unless the contract explicitly states otherwise.

The experiment thus had a range of Pareto-optimal outcomes. For those in group A, a Pareto-optimal contract would give the union its stay clause, have a wage anywhere in the \$6–\$12 range, and provide at least fifteen vacation days. For those in group B, a Pareto-optimal contract would give management its go clause, again have a wage anywhere in the \$6–\$12 range, and provide at least fifteen vacation days.³⁷ The parties to any efficient contract would receive 3,050 total points.

V. RESULTS

Table 2 presents the basic results of the experiment. The third column shows that 57 percent of the contracts bargained under *Milwaukee I* had a stay clause, while 52 percent of the contracts bargained under *Milwau-*

³⁴ Vacation days were not the primary focus of the experiment but were included in the negotiations to provide an additional check on whether negotiators were bargaining efficiently.

³⁵ All bargainers were told that the contract term was three years. This was not a subject of negotiations.

³⁶ All students had recently studied the holdings of the *Milwaukee Spring* cases but had not been told anything about the possible application of the Coase Theorem to it.

³⁷ All contracts should have the noise control provision as well.

TABLE 2
 CONTRACT OUTCOMES BY PREFERENCE GROUP AND LEGAL RULE

	Wage (\$)	Vacation Days	Contracts with Stay Clause (%)	Union Points	Management Points	Total Points	No. of Contracts
Group 1 (<i>Mil I</i> controls):							
Subgroup A (stay clause efficient)	7.82	11.6	72	1,320	1,657	2,977	29
Subgroup B (go clause efficient)	8.23	11.6	40	1,229	1,741	2,970	25
Group 1 mean	<u>8.01</u>	<u>11.6</u>	<u>57</u>	<u>1,278</u>	<u>1,696</u>	<u>2,973</u>	<u>54</u>
Group 2 (<i>Mil II</i> controls):							
Subgroup A (stay clause efficient)	7.58	9.9	65	1,250	1,712	2,963	26
Subgroup B (go clause efficient)	7.38	11.2	39	1,137	1,832	2,969	28
Group 2 mean	<u>7.48</u>	<u>10.6</u>	<u>52</u>	<u>1,192</u>	<u>1,774</u>	<u>2,966</u>	<u>54</u>
Grand mean	7.74	11.1	55	1,235	1,735	2,970	108

NOTE.—*Mil I* = *Milwaukee I*; *Mil II* = *Milwaukee II*.

kee II had a stay clause. Columns 1 and 4 show that unions received a higher wage and scored more total points when bargaining under the favorable legal rule of *Milwaukee I*. Conversely, column 5 shows that management scored more points under the promanagement presumption of *Milwaukee II*.

A. Data Question Strong Efficiency Hypothesis

In this multiple-item bargaining, substantial cooperative gains from trade were possible for two items.³⁸ Yet neither item was bargained to a fully efficient result. Only thirty-three of the 108 contracts (31 percent) called for at least fifteen vacation days. The mean was 11.1 days, a loss of nearly forty total points from a contract with an efficient vacation clause. Similar inefficiencies were observed in bargaining over the relocation clause, where one side received 300 points for a favorable clause while the other side received only 200 points for a favorable clause. Seventy of the 108 contracts (65 percent) contained a 300-point relocation clause. Because every inefficient relocation clause lost the parties 100 points, the mean loss from the relocation clause was about thirty-five points.

The contracts as a whole reflect these inefficiencies in individual clauses. A fully efficient contract would give the parties 3,050 points. Only 20 percent of the contracts were fully efficient, although the efficient contract was the mode. The mean contract had 2,970 total points, with a standard deviation of sixty-three points.

These results cast more doubt than previously reported experiments do on the Coasean prediction that parties always reach efficient bargains. In the various Hoffman and Spitzer tests, for example, over 90 percent of the negotiators reached the efficient solution. Three differences in the experimental design may explain the lower levels of efficiency obtained here. First, the bargainers here were negotiating on multiple elements.³⁹ In an experimental situation with time constraints,⁴⁰ bargainers may have difficulty focusing on all items and making efficient choices. Second, in

³⁸ Inefficient contracts were possible for the other two items, wages and the noise reduction clause, as well. All contracts included the efficient noise reduction clause and all but three contracts contained an efficient wage clause. See notes 32 and 33 *supra*.

³⁹ In a "real-world" negotiation, multiple-item bargaining may not hinder efficiency. The marginal costs of bargaining on the item of interest are lower when the parties are already assembled to bargain on other issues. Further, information about preferences may be easier to signal and assimilate when multiple items are on the trading table. See Schwab, *supra* note 30, at 268; John Cross, *Negotiation as a Learning Process: Theories and Applications*, in *The Negotiation Process* 29, 52 (I. Zartman ed. 1978).

⁴⁰ See note 28 *supra* and accompanying text.

most previous experiments of the Coase Theorem, bargainers were given perfect information, including the other side's preferences, so signaling and bluffing about valuations were impossible. When negotiations include signaling about valuations, one would expect that not all negotiators will reach mutually advantageous bargains.⁴¹ Third, unlike previous experiments, where negotiators were presented with a clear best outcome, bargainers in this experiment were given only a general idea of starting points, and there were no finite limits on individual payoffs. One might expect greater deviations from efficient bargains when the bargaining range is so large. Given the greater relaxation of conditions of this experiment, the deviations from efficiency are not surprising.

B. Data Support Weak Efficiency Hypothesis

The alternate formulation of the Coase Theorem does not predict that all bargainers will reach efficient solutions but more modestly predicts that the rule of law under whose shadow they bargain does not influence whether they reach efficient solutions. In our context, the prediction is that bargainers are equally likely to reach an efficient bargain when operating under *Milwaukee I* or *Milwaukee II* as the rule of law.

The cross-tabulation of Table 3 confirms this prediction. Thirty-one of the 54 contracts bargained under *Milwaukee I* contained a union-favorable stay clause. Twenty-eight of the 54 contracts bargained under *Milwaukee II* contained the stay clause. As the chi-square statistic reveals, this minor difference is statistically insignificant.

Further breakdown of the results confirms the insignificant effect of *Milwaukee I* and *II* on the efficiency of contracts. Panel A of Table 3 examines the fifty-five group A contracts bargained when the union valued a stay clause more than the company valued a go clause. Sixty-seven percent of those contracts contained the efficient stay clause. A stay clause was hardly more likely to occur under *Milwaukee I*—the efficient presumption for group A—than under *Milwaukee II*—the inefficient presumption for group A. As panel A reveals, 72 percent of the contracts under *Milwaukee I* contained the efficient stay clause, while 65 percent of the contracts under *Milwaukee II* contained the efficient stay clause, a statistically insignificant difference.

⁴¹ An interesting theoretical literature explores the trade-offs between maximizing the surplus from trade and lying about one's preferences to maximize the individual gains from trade. See Edward Saraydar, *Bargaining Power, Dissimulation, and the Coase Theorem*, 139 *Zeitschrift für die gesamte Staatswissenschaft* 599 (1983). Saraydar's model suggests that the optimal self-interested strategy will include understating one's preferences. See also Schwab, *supra* note 30, at 278–80.

TABLE 3
EFFICIENCY EFFECT OF LEGAL RULE

	Stay Clause	Go Clause	No. of Contracts
<i>Milwaukee I</i>	31 (57.4)	23 (44.4)	54
<i>Milwaukee II</i>	28 (51.9)	26 (48.1)	54
Column total	59 (54.6)	49 (45.4)	108
Significance .6991 χ^2 (1 df) .14943			
A. WHEN STAY CLAUSE IS EFFICIENT			
<i>Milwaukee I</i>	21 (72.4)	8 (27.6)	29
<i>Milwaukee II</i>	17 (65.4)	9 (34.6)	26
Column total	37 (67.3)	17 (30.9)	55
Significance .7864 χ^2 (1 df) .07342			
B. WHEN GO CLAUSE IS EFFICIENT			
<i>Milwaukee I</i>	10 (40.0)	15 (60.0)	25
<i>Milwaukee II</i>	11 (39.3)	17 (60.7)	28
Column total	21 (39.6)	32 (60.4)	53
Significance 1.0000 χ^2 (1 df) .0000			

NOTE.—Row percentages are in parentheses.

Panel B presents analogous results for parties bargaining when management valued a go clause more highly than the union valued a stay clause. It shows even more clearly that, while not all contracts called for the efficient go clause, the efficiency or inefficiency of the presumption did not affect whether the efficient go clause appeared in a contract.

The lack of influence of the legal rule is also seen by examining the total points of contracts. The mean total point score was 2,973 points under *Milwaukee I* and 2,966 points under *Milwaukee II*, again an insignificant difference.⁴²

⁴² The *t*-statistic with a pooled estimate of the variance is 0.62 with 106 degrees of freedom, significant only at the .54 level.

In sum, the data are consistent with the reformulated Coasean hypothesis that the legal rule does not affect whether parties reach an efficient result.

C. Data Question No Distributive Effect Hypothesis

The data cast doubt on the standard Coasean hypothesis that contract presumptions should not affect distribution. Instead, the evidence suggests that bargainers in this experiment acted as if they must purchase the right when the legal presumption favored the other party and thus were in a weaker bargaining position.

Table 4 (analogous to Table 3 on efficiency) reports the effect of *Milwaukee I* and *II* on distribution. It reveals that the average union negotiator gained eighty-six more points under the favorable *Milwaukee I* presumption than under *Milwaukee II* and that the average management negotiator lost seventy-eight points. Both differences are statistically significant at the 5 percent level.

Panel A of Table 4 reports the results for those contracts in which a union stay clause was the more highly valued clause. Contrary to the

TABLE 4
EFFECT OF LEGAL RULE ON DISTRIBUTION

	Union Points	Management Points
<i>Milwaukee I</i> mean	1,278	1,696
(SD)	(177)	(152)
<i>Milwaukee II</i> mean	1,192	1,774
(SD)	(189)	(180)
Difference	86	-78
(<i>t</i> -statistic)	(2.44)*	(-2.45)*
A. WHEN STAY CLAUSE IS EFFICIENT		
<i>Milwaukee I</i> mean	1,320	1,657
(SD)	(189)	(156)
<i>Milwaukee II</i> mean	1,250	1,712
(SD)	(222)	(189)
Difference	70	-55
(<i>t</i> -statistic)	(1.25)	(-1.20)
B. WHEN GO CLAUSE IS EFFICIENT		
<i>Milwaukee I</i> mean	1,229	1,741
(SD)	(152)	(136)
<i>Milwaukee II</i> mean	1,137	1,832
(SD)	(134)	(153)
Difference	92	-91
(<i>t</i> -statistic)	(2.34)*	(-2.27)*

* = significant at .05 level, two-tailed tests.

TABLE 5
REGRESSION ANALYSIS OF DISTRIBUTIVE EFFECT OF LEGAL RULE

INDEPENDENT VARIABLES	DEPENDENT VARIABLE	
	Union Points	Management Points
MILWAUKEE1	78.09 (2.32)**	-69.96 (-2.34)**
UNION VALUES MORE	103.53 (3.07)***	-103.30 (-3.46)***
LAW STUDENTS	-62.86 (-1.87)*	79.20 (2.65)***
Constant	1,173.16	1,784.36
R ²	.16	.20

NOTE.—*t*-statistics are in parentheses.

* = significant at .10 level, two-tailed tests.

** = significant at .05 level, two-tailed tests.

*** = significant at .01 level, two-tailed tests.

standard economic view that contract presumptions do not influence the distribution of wealth, the average union negotiator obtained seventy more points, and the average management negotiator obtained fifty-five fewer points, when bargaining under *Milwaukee I*. The point differentials for this subgroup are not statistically significant, however.

Panel B reports analogous results when the go clause was the efficient result. Again, management negotiators did much better when bargaining under *Milwaukee II* than under *Milwaukee I* (1,832 points compared with 1,741 points), while union negotiators did much worse (1,137 points compared with 1,229 points). The differences for this subgroup are statistically significant at the 5 percent level.

The total distributive influence of the rule of law is perhaps best seen in Table 5, which reports two regression equations where the dependent variables are union points and management points. The three explanatory variables are MILWAUKEE1 (a dummy variable that equals one for contracts bargained under *Milwaukee I* and equals zero for contracts bargained under *Milwaukee II*), UNION VALUES MORE (a dummy variable that equals one for contracts where the union receives 300 points for a stay clause and equals zero for contracts where the union receives only 200 points for a stay clause), and LAW STUDENTS (a dummy variable that equals one when the bargainers were law school students and equals zero when they were ILR school students).⁴³

⁴³ The significant coefficients on the LAW STUDENT variable indicate that union negotiators receive fewer points, and management negotiators receive more points, in a law school class than an industrial and labor relations school class. This is not surprising because many students attending the ILR school have a definite sympathy for unions.

The standard Coasean hypothesis that contract presumptions do not affect distribution would predict that MILWAUKEE1 would not be a significantly explanatory variable. The coefficient of 78.09 for MILWAUKEE1 in the first equation indicates that union negotiators obtain 78.09 more points when bargaining under the contract presumption (*Milwaukee I*) that favors unions. Similarly, the coefficient (-69.96) in the second equation indicates that management negotiators obtain 69.96 fewer points when bargaining under the presumption that favors unions. These large and statistically significant coefficients strongly suggest that the bargainers acted as if they had to buy the relocation clause when the contract presumption lay with the other party. These results are inconsistent with the hypothesis that legal contract presumptions do not alter the distribution of wealth between bargainers.

VI. POSITIVE EXPLANATIONS

How can one explain experimental evidence that a contract presumption does not affect the efficiency of the contract but does increase the division of the gains from trade toward the contract beneficiary? In assessing experimental results that seem to refute a central hypothesis of a theory, the first place to look is with the methodology of the experiment. In particular, one can ask whether the experiment captures the essence of the theory and the spirit of real-world transactions.⁴⁴ I have already described the experiment in some detail and will now highlight only the key points suggesting that the results be taken seriously. An obvious weakness with experiments such as these is that the subjects are students with little at stake rather than experienced, worldly negotiators in real situations. Further, the experimenter can never be sure that the subjects are not perceiving unintentional cues that make them behave in ways that appear inconsistent or unexplainable. On the other hand, the strength of bargaining experiments is that one can eliminate other influences on bargaining that make it difficult to interpret the results when observing real negotiations.

Most important, this experiment can control for the bargaining power of the two sides. Nothing but the shift in contract presumption changed the bargaining power of parties negotiating under *Milwaukee I* from the power of parties negotiating under *Milwaukee II*. In actual labor negotia-

⁴⁴ See generally Hoffman & Spitzer, *Experimental Law and Economics*, *supra* note 1, for a discussion of the concept of parallelism or external validity. As they state, "only if one can safely assume that the essential features of actual institutions have been incorporated into *parallel* laboratory institutions will experimental results be of interest." *Id.* at 993 (emphasis in original).

tions, bargaining power depends largely on the ability of labor to engage in, and management to withstand, a strike, as well as on negotiating skill, risk aversion, degree of solidarity on goals and aims, and other factors.⁴⁵ The experiment mirrors the threat of a strike by the threatened loss of ten points to each side for every hour without a contract after the strike deadline.⁴⁶ The experimental subjects undoubtedly differ in risk aversion and bargaining skill as well, which explains the diversion in scores. Presumably, though, with large numbers of subjects randomly assigned to bargain under *Milwaukee I* or *Milwaukee II*, the bargaining power of the various negotiators is independent of the contract presumption under which they bargain. The explanation this leaves for the systematic difference in results is that the contract presumption increases bargaining power. Bargainers do better when the contract presumption favors them.

More generally, Coasean theorists cannot casually dismiss this experiment because in important ways the bargainers did behave according to Coasean efficiency predictions. Almost two-thirds of the bargainers (thirty-two of fifty-one) waived an inefficient presumption while only one-third (nineteen of fifty-seven) waived an efficient presumption. Further, the experiment revealed no stickiness in trades owing to the contract presumption. Parties operating under an efficient presumption were hardly more likely to reach an efficient result than parties operating under an inefficient presumption (confirming the weak efficiency hypothesis).⁴⁷ Thus, as Coase predicts, these bargainers seem driven toward efficiency rather than toward the presumption.

A. *The Labor Context*

Accepting the experimental results, one might suppose they apply only to a limited class of real-world bargaining situations. One explanation might be that contract presumptions have a distributive effect in the labor context of the experiment but not elsewhere.⁴⁸ Several aspects of labor

⁴⁵ For a general discussion of the factors influencing relative bargaining power in the collective bargaining process, see Terry Leap & David Grigsby, *A Conceptualization of Collective Bargaining Power*, 39 *Indus. & Lab. Rel. Rev.* 202 (1986); Samuel Bacharach & Edward Lawler, *Bargaining: Power, Tactics, and Outcomes* (1981).

⁴⁶ See text at note 28 *supra*.

⁴⁷ Rearranging panels A and B of Table 3 reveals that thirty-eight of the fifty-seven contracts with an efficient presumption (66.7 percent) reached an efficient result, while thirty-two of the fifty-one contracts with an inefficient presumption (62.7 percent) reached an efficient result. The difference is statistically insignificant at the .01 level.

⁴⁸ Douglas Leslie, while accepting the argument that contract entitlements will not affect distribution in competitive markets, suggests that legal entitlements will sometimes affect distribution in "the special case of union/management collective bargaining." Leslie, *Cases*

negotiations, captured in the experiment, may lead the contract presumption to increase the bargaining power of the nominal beneficiary.⁴⁹

First, labor negotiators, like the experimental subjects, often face a time constraint. The contract presumption may give one side an edge when bargaining comes to the eleventh hour and both sides are desperate to sign a contract. If they simply ignore the issue while battling over more central matters, the beneficiary of the presumption gains the term without cost. Or a last-minute demand to waive may by necessity be accompanied by concessions in other areas.

Second, these negotiations, like labor negotiations generally, involve a situation of bilateral monopoly. Unlike the participants in competitive markets, the parties in a bilateral monopoly (and in this experiment) are faced with an all-or-nothing choice of reaching agreement with this party or foregoing trade in this product. Indeed, labor law puts positive pressure on parties to reach a contract.⁵⁰ If some bargain must be struck (even one that silently relies on a contract presumption for one item), the beneficiary may be able to parlay the pressing need for agreement on basic terms into payment for waiving the presumption on a side issue.

Third, labor negotiations are noted for the complexity of the bargains being struck. Collective bargaining rarely entails one-for-one trades but,

and Materials on Labor Law 371 (2d ed. 1985). Suppose, suggests Leslie, the union's valuation of the entitlement exceeds its minimum settlement point had the board not awarded it this entitlement. If managers cannot injure the union and thereby force the union to give back the asset, the board's decision will redistribute wealth from managers to the union. Leslie does not clearly distinguish between contract mandates and contract presumptions.

⁴⁹ I have previously discussed how contract presumptions may have distributive effects in labor bargaining in Schwab, *supra* note 30, at 261-65.

⁵⁰ The National Labor Relations Board monitors bargaining for compliance with the Congressional mandate to bargain "in good faith." See sections 8(a)(5) and 8(d) of the NLRA. More generally, many labor scholars assume that a company and union cannot easily refuse to reach some agreement. See Morris Stone, Managerial Freedom and Job Security 4 (1964) (unlike other contracts, "labor and management can never escape one another; both are under obligations imposed by law, by self-interest, and by the very nature of their relationship to bargain until they *do* reach agreement."); Archibald Cox, The Legal Nature of Collective Bargaining Agreements, 57 Mich. L. Rev. 1, 3 (1958) ("In fact neither the employer nor the employees collectively have the freedom to disagree which characterizes typical contracts between business firms and individuals. Sooner or later the employer and employees must strike some kind of a bargain."); Clyde Summers, Collective Agreements and the Law of Contracts, 78 Yale L. J. 525, 530 (1969) ("Selection of a majority union establishes a bargaining relationship which is compulsory for both the employer and all individual employees."); Paul Weiler, Striking a New Balance: Freedom of Contract and the Prospects for Union Representation, 98 Harv. L. Rev. 351, 366 (1984) (labor law assumes that employer and union must eventually reach some contract). Nonetheless, almost one-quarter of first-contract negotiations fail to reach agreement. See William Cooke, The Failure to Negotiate First Contracts: Determinants and Policy Implications, 38 Indus. & Lab. Rel. Rev. 163, 164 (1985).

rather, involves a multiplicity of issues (captured in the experiment by bargaining over wages, noise, vacation days, and the relocation clause). Bargainers may have difficulty determining (or justifying to their constituents) what they received in exchange for waiving a presumption. This difficulty may make bargainers reluctant to waive a clear favorable presumption for uncertain gains.⁵¹ One problem with applying the “complicated bargaining” explanation to this experiment is that complicated deals should lead to stickiness and inefficient refusals to waive contract presumptions, as well as distributive effects. This we did not see.

B. *Bargaining in General*

Perhaps the results apply more generally to contract presumptions in all bargaining contexts (that is, also to single-item bargaining without time constraints and with rival buyers and sellers available). The argument, then, is that a contract presumption in general may increase the beneficiary’s bargaining power. To those not initiated in the mysteries of Coase, the point may even seem obvious. Ask a negotiator whether she would prefer to have a contract presumption favor her or the other side and she will certainly respond, “With me.” She would probably maintain this view even when it was pointed out that the law does not regulate other clauses in the contract and she is free (and the other side is free to induce her) to waive the presumption by inserting specific language in the contract. Certainly litigants fight long and hard to have contract presumptions favor their side.⁵²

This view suggests that a party is better off when it can waive a presumption than when it must induce the other side to waive. For example, under this view a union has two things to sell under *Milwaukee I*—its labor and the presumption of a stay clause—whereas the union has only its labor to sell under the promanagement presumption of *Milwaukee II*. Most economists would suggest that the difference between buying and selling is illusory (absent wealth effects, which are often minor). When the

⁵¹ Leslie has coined the term “batch theory” to capture the idea that labor negotiations are often complex and interrelated. The batch theory suggests that parties will be more reluctant to waive inefficient presumptions when the corresponding gains are unclear. See Douglas Leslie, *supra* note 48, at 372. For an argument that multiple-item bargaining will promote efficiency because preferences are easier to signal, see note 39 *supra*.

⁵² Demsetz has explained the interest of litigants in contract rules (despite any long-term distributive effect) as the short-term interest in having already-written contracts interpreted in a favorable way. See Demsetz, *Wealth Distribution*, *supra* note 7. The present results suggest that the explanation may be that litigants are also fighting over long-term distributive effects. Certainly the lengthy battles over presumptions such as warranty of habitability and the arbitrability of labor disputes belie the suggestion that only short-term distributive gains are at stake.

union has an entitlement, it incurs an opportunity cost (equaling the amount the company would pay for it) if it refuses to waive the entitlement. Correspondingly, when the entitlement favors management, the union must incur a real cost (equaling the amount the company will sell it for) to obtain the right. But scholars of various viewpoints have questioned the standard economic view, and perhaps it is misleading or incomplete here.

The radical critique rejects the standard economic view that people form preferences independently of legal presumptions.⁵³ For example, Mark Kelman suggests that the value placed on pure water may increase if the law awards a right to pure water. To return to the labor context a moment, presumptions favoring arbitrability may gradually cause persons to value arbitration more than if the law had no presumption or (as it once did with general commercial arbitration) refused to enforce promises to arbitrate.

A less radical explanation emphasizes that contract presumptions may signal useful information to the beneficiaries. Parties are often uncertain which clause will suit them best, particularly when the clause deals with remote contingencies. The parties may believe that contract presumptions reflect the standard, widely accepted solution to a contracting situation. This is particularly plausible when contract presumptions are specifically designed to reflect the customary or optimal structure for most people. Under a public interest view of legislation, or an efficiency view of the common law, contract presumptions are the efficient terms for most transactions. A nominal beneficiary, then, aware of his uncertainty about the value of various clauses, must be induced to take the risk that waiving the standard clause is in his interest. For this reason, beneficiaries will demand more when waiving an entitlement than they would pay to purchase the entitlement. If so, we should see that contract presumptions distribute wealth toward the beneficiary.

These explanations would suggest that parties in general hesitate (that is, demand extra compensation) to waive presumptions. Let me term this the "general hesitation effect" of contract presumptions. Two problems arise in using the general hesitation effect to explain our experimental results that presumptions alter distribution but not efficiency.

⁵³ See Mark Kelman, *Consumption Theory, Production Theory, and Ideology in the Coase Theorem*, 52 S. Cal. L. Rev. 669 (1979). For a critical legal studies critique of compulsory terms rather than waivable presumptions, see Duncan Kennedy, *Distributive and Paternalist Motives in Contract and Tort Law, with Special Reference to Compulsory Terms and Unequal Bargaining Power*, 41 Md. L. Rev. 563 (1982). For experimental evidence that willingness to pay may differ substantially from willingness to accept, see Jack L. Knetsch, Richard Thaler, & Daniel Kahneman, *Experimental Tests of the Endowment Effect and the Coase Theorem* (mimeo September 1987).

First, a general hesitation effect from imperfect information should lead to (ex post) inefficiencies as well as distributive changes; a party with a presumption that is inefficient in his situation (even if efficient in the typical case) will hesitate to waive and, at the margin, will not waive. We found, however, that parties conformed to the weak efficiency hypothesis. The problem may be with the finding rather than the hesitation effect, however, in that the experiment may not have picked up possible marginal hesitation effects on efficiency. The item at issue in the experiment was an all-or-nothing stay or go clause. Perhaps subjects in this experiment waived inefficient presumptions because of the large increase in surplus from an efficient clause (100 points), even though subjects would be reluctant at the margin to waive inefficient presumptions. Thus, the experiment cannot rule out the hypothesis that contract presumptions might marginally affect efficiency, as well as affect distribution.

Second, the present experiment was not designed to detect any possible hesitation effect on efficiency or distribution, and it seems unlikely it could do so. The hesitation effect requires that a party internalize the values suggested by the contract presumption or believe that the contract presumption conveys information about the most efficient clause in the typical bargain. Subjects here, however, were told unambiguously how they valued the relocation clause (at either 200 or 300 points). The contract presumption would not make the valuation more precise. Further, half the class was bargaining under a rule they knew had been overturned. It seems implausible that students would use overturned presumptions to value clauses, even if the value of the clause was uncertain. Further refinement of the experimental design is required to test the possible information-signaling feature of contract presumptions.

VII. IMPLICATIONS, POSITIVE AND NORMATIVE

The findings suggest that contract presumptions are not always innocuous, even in settings with low transaction costs, but rather have important distributive effects. The question that is left open by this experiment is, In what contexts do we expect these distributive effects to be most important? Labor contracts, as noted above, are usually negotiated from scratch, and the bargaining takes place in a context in which the holder of the presumption can hold out against the demands of the other side. It is unclear whether the distributional consequences that attach to waivable presumptions in this bilateral monopoly context will carry over to other settings. Thus, the ordinary contract for the purchase of consumer goods and services may well be prepared in advance by the seller and used in a competitive market. Because there is no give and take of negotiation, the

seller may fasten on the term that yields the efficient solution and direct all subsequent negotiations over the distributive matter of price. If that is the case, then these consumer contracts may be immune from the pressure at work in the labor area. If so, the original waivable presumptions need not yield any distributional consequences.

Yet waivable presumptions may have distributional effects in many markets. The differences between consumer and labor contracts may be differences in degree rather than kind, so that distributional consequences might attach to waivable presumptions, but these could be of smaller magnitude than the ones observed in these experiments. Further work is needed to determine just how robust these results turn out to be.

Regardless of whether these experimental results are fully generalizable to all markets, they should be of interest to both judges and legislators, who at the very least should be aware that presumptions may enable the beneficiary to capture greater gains from the contract. One suspects that judges and legislators already thought they could alter bargaining power by creating presumptions. The modern trend to ameliorate the harshness of the common law uses presumptions in a wide range of areas. Witness the explosive growth of implied warranties to protect buyers,⁵⁴ the creation of the substantial performance doctrine to protect contractors, and the parallel relaxation of the rigors of the perfect tender rule to protect sellers more generally.⁵⁵ These developments very often were expressly designed to protect a weaker party, although in general the protections are waivable. The legislators may well have sensed something about distributional effects that the standard law-and-economics approach has overlooked.

One salient message from these results, then, is to caution against the common view that distributional objectives cannot be obtained by varying standard contractual presumptions. More generally, the results may suggest the need to rethink the sharp theoretical division between coercive contract rules and contract presumptions. The standard law-and-economics view sees nonwaivable coercive terms as inefficient and paternalistic at best and more likely misguided and harmful (absent some spectacular form of externality in a particular market). Contract presumptions, by contrast, are seen as innocuous and therefore ineffective in achieving distributive goals, although beneficial in lowering the costs of transactions to parties with typical needs and values. The results

⁵⁴ See U.C.C. § 2-312 (warranty of title and against infringement); U.C.C. § 2-314 (implied warranty of merchantability); U.C.C. § 2-315 (implied warranty of fitness for a particular purpose).

⁵⁵ See E. Allan Farnsworth, *Contracts* 590-96 (1982).

of this experiment suggest, however, that at least some contract presumptions have a real impact, even when transaction costs are low. In some settings, contract presumptions may have either a distributive effect similar to property rights, or a hesitation effect on efficiency and distribution similar in kind, if not degree, to coercive contract terms.

The range of legal areas implicated is potentially enormous. Whether rights should be waivable is an issue in landlord-tenant law,⁵⁶ labor law,⁵⁷ basic contract law,⁵⁸ and elsewhere. Rule makers sometimes hesitate to make rights waivable when they sense unequal bargaining power. Under the traditional economics view, they are then torn between creating inefficient coercive terms or not regulating the transaction. This experiment suggests, however, that in some settings, at least, creating even a waivable presumption increases the bargaining power of the beneficiary.

Let me push the potential scope of the findings with a brief speculation about their application to intestate succession laws. One might wonder what intestate laws have to do with a theory of contract presumptions' effect on bargaining. Traditional bargaining does not occur in the making of wills. Nevertheless, estate planners know well the maneuvering and posturing that occurs between testator and potential legatees (what Buchanan has characterized as rent-seeking behavior).⁵⁹

The question for us is whether intestate succession laws have any effect

⁵⁶ For example, the Restatement of Property allows tenants to waive the implied warranty of habitability unless the waiver is unconscionable or significantly against public policy. See Restatement (Second) of Property (Landlord & Tenant) § 5.6 (1977). By contrast is the issue of enforcing clauses that permit landlords to use self-help in repossessing apartments when tenants have overstayed the lease. The Restatement allows some self-help but declares that agreements allowing further self-help are void as against public policy. See Restatement (Second) of Property (Landlord & Tenant) § 14.3 (1977). A third landlord-tenant issue that divides the courts is whether to recognize exculpatory clauses that limit landlord tort liability for physical injury from unsafe conditions. Compare *O'Callaghan v. Waller & Beckworth Realty Co.*, 15 Ill. 2d 436, 155 N.E.2d 545 (1958) (exculpatory clause valid despite lessee's argument that shortage of housing created disparity in bargaining power), with *Kuzmiak v. Brookchester, Inc.*, 33 N.J. Super. 575, 111 A.2d 425 (1955) (exculpatory clause invalid because of unequal bargaining power).

⁵⁷ See generally 1 *The Developing Labor Law* 640-50 (Charles Morris 2d ed. 1983). A classic example comes from *NLRB v. J. Weingarten, Inc.*, 420 U.S. 251 (1975), where the Court expressly relied on the goal of equalizing bargaining power in holding that unionized workers have a right to have a shop steward present at investigatory interviews. Justice Powell in dissent questioned whether the newly found right would be waivable. *Id.* at 275 n.8. After some vacillation, later board decisions have held the right to be waivable. See Schwab, *supra* note 30, at 280-82.

⁵⁸ In contract law, the issue usually is discussed in terms of whether the contract clause is unconscionable (and therefore unenforceable). See generally Farnsworth, *supra* note 55, at 307-19.

⁵⁹ James Buchanan, *Rent Seeking, Noncompensated Transfers, and Laws of Succession*, 26 *J. Law & Econ.* 71 (1983).

on the relative ability of various legatees to curry favor with the testator. For example, compare the Uniform Probate Code with the Georgia Code's distribution between spouse and children. The Uniform Probate Code bequeaths the first \$50,000 to the spouse, with the spouse also getting half the remainder and the rest being divided among the children.⁶⁰ The Georgia Code treats the spouse less favorably; the spouse takes equally with the children, down to a minimum of a one-fourth share.⁶¹

These laws have an obvious distributive impact when transaction costs of one kind or another prevent persons from writing a will. But even when a Georgia testator writes a will, and so can freely write around the intestate presumption that treats the spouse equally with each child, the hesitation effect of contract presumptions may keep the testator from increasing the spouse's share as much as he otherwise would. The prediction, then, if we extrapolate wildly from our results, is that children in Georgia should receive more in wills relative to the spouse than children in states following the Uniform Probate Code, even when testators think similarly on the appropriate share to be left the spouse. To use Buchanan's terminology, one might predict that the presumption created by Georgia's intestate succession laws will increase the rent-seeking ability of children as compared to the spouse.

VIII. SUMMARY

Unlike previous bargaining experiments, where the task of the bargainers was to choose among numbers with various payoffs, this experiment attempted to mirror typical items of real-world labor negotiations and to have the parties bargain under the shadow of an actual legal rule. One should expect in this setting more "noise" than in the more pristine, abstract experiments. Indeed, only 20 percent of the contracts were efficient in all respects, and in the clause to which differing legal presumptions applied, only two-thirds of the contracts included an efficient relocation clause. These results seem far less supportive of the strong Coasean hypothesis of 100 percent efficiency.

Because the experiment included opposite versions of an actual rule of collective-bargaining contract interpretation, it allows rather precise study of the more modest Coasean claim that legal rules do not affect whether parties will reach efficient contracts. This "weak efficiency hypothesis" is strongly supported. Parties bargaining under *Milwaukee I*

⁶⁰ Unif. Probate Code § 2-102(3). If some of the children are not the issue of the spouse, the spouse gets one-half the entire estate without the \$50,000 initial bequeathment.

⁶¹ Georgia Code Ann. 1987 Cum. Supp., vol. 40, Title 53-4-2(2) (amended 1985).

and *Milwaukee II* were equally likely to include an efficient relocation clause in the contract and to write a contract that maximized the total number of points.

Finally, the experiment offers evidence that questions the Coasean hypothesis that rules of contract interpretation do not affect the distribution of wealth between bargainers. On the contrary, a party operating under a favorable contract presumption obtained, on average, over 70 points more than a similarly situated party bargaining under the opposite presumption.

The distributive effect of contract presumptions may apply only to contexts like labor negotiations, where complex bargaining under the constraints of time and bilateral monopoly may allow the beneficiary to turn a contract presumption into increased bargaining power. But the distributive effect may apply more generally. The information conveyed in contract presumptions may induce beneficiaries in a world of uncertainty to demand compensation for waiving presumptions or may produce a hesitation effect that inhibits efficient exchanges and alters distribution.

These results have potentially dramatic implications for judges and legislators drafting contract presumptions and theorists analyzing their effects. Far from being innocuous, contract presumptions may in some situations increase the bargaining power of the beneficiary. If so, the division between coercive terms and waivable presumptions may be far murkier than traditionally thought. Indeed, contract presumptions may frequently operate more like property entitlements, in that absent transaction costs the holder will trade them to higher valued uses, but the holder will receive positive rewards for them.

One should hesitate to speculate more widely than this until the experimental results are confirmed in other contexts. Nevertheless, the results are powerful enough to raise questions about the common wisdom that contract presumptions have little distributive effect.

APPENDIX

INFORMATION GIVEN TO STUDENT BARGAINERS

General Instructions

The United Workers (UW) has just won its first election at the Ithaca plant of ABC Company (ABC operates another, nonunionized plant in Elmira), and the board has certified UW as the authorized representative of this bargaining unit. The company currently pays \$*x*/hour (known to management only) and gives employees *y* vacation days per year (known to management only).

The goal of the union negotiator is to satisfy the membership as much as possible (that is, to obtain as many union points as possible from the contract). The goal of the management negotiator is to satisfy stockholders as much as possible (that is, to obtain as many management points as possible from the contract). In other words, do not stop at some number you think is a threshold or plateau. The number of points for each side depends on its success at the bargaining table.

Your counterpart does not know the preferences of your constituency. You may reveal as much or as little as the law permits. You can NOT, however, show your counterpart your preference sheet. All lawful bargaining tactics are permitted. If you wish to file a ulp (unfair labor practices) charge with the board, photocopy the form from pp. 78-81 of your statutory supplement and file it in the mailbox of Regional Director Schwab. He will act upon charges as quickly as reasonable.

The workers and shareholders hope that a contract can be reached by 11 A.M., Thursday, November 13. If the contract is not signed and handed in by that time, the union will strike. For every hour that the contract is late, the union and company each lose ten points, unless one side is vindicated by a successful ulp charge.

I will also ask each negotiator to write a one-to-two-page description of the bargaining process: tactics used, major stumbling blocks, ambiguities. This description (typed is preferable but not essential) can be turned in any time before Monday, November 17, at 2 P.M.

- WARNING: 1) Carefully read the contract and your preference sheet before you begin negotiating. The notes on the contract and preference sheet are worth noting.
 2) DO NOT discuss this problem with anyone other than your bargaining counterpart.
 3) DO NOT show your counterpart your preference sheets.

Contract

PREAMBLE: This agreement is made this ____th day of November, 1986, at ____ o'clock __.M., between the ABC Company, hereinafter referred to as the "Company," and United Workers, hereinafter referred to as the "Union." The agreement is to remain in force for three years.

RECOGNITION: The Company recognizes the Union as the sole and exclusive agent for all production and maintenance employees at the Ithaca plant.

WAGE: The Company promises to pay all workers \$_____ per hour.

VACATION: Each worker is entitled to ____ vacation days per year.

OTHER: 1) _____

2) _____

Legibly Signed,

 for United Workers

 for ABC Company

NOTE (given to group 1): In judging the negotiating conduct of parties and in interpreting this contract, the regional director will rely on current board and case law, except that *Milwaukee Spring I*, 265 NLRB 206 (1982), is valid, and *Milwaukee Spring II*, 268 NLRB 601 (1984), does not exist.

NOTE (given to group 2): In judging the negotiating conduct of parties and in interpreting this contract, the regional director will rely on current board and case law, including *Milwaukee Spring II*, 268 NLRB 601 (1984).

Union Preferences

(CONFIDENTIAL: May not be revealed to anyone)
 (Given to union negotiators only)

- A. *Wages.*
 Wages of \$6/hour—union receives 600 points.
 Every penny above \$6—union receives one additional point.
 Every penny below \$6/hour—union loses ten points.
 Examples: 1) If the contract specifies \$6.84/hour, union receives 684 points.
 2) If the contract specifies \$5.84/hour, union receives 440 points.
 3) If the contract specifies \$5.40/hour, union receives zero points.
- B. *Relocation Clause.* If the contract prevents the company from avoiding its labor costs by transferring work to other plants during the contract term, the union receives 300 points (200 points for group B).
 NOTE: The contract can specify this, or the rule of law may prevent the company from transferring work to avoid labor costs. In either case, the union receives 300 points (200 points for group B).
- C. *Vacation Days.* Union receives twenty points for each vacation day up to fifteen. Each additional day beyond fifteen gives the union ten points.
 Example: Seventeen days' vacation gives the union 320 points.
- D. *Noise Reduction.* Union receives 100 points if contract calls for the company to institute a program to reduce noise in the plant to eighty decibels.

GENERAL NOTE: The workers will refuse to ratify any contract worth less than 1,000 points to the union. If the union signs a contract less than 1,000 points, there is no contract, and both sides receive zero points. Remember, though, the union wants more than 1,000 points.

Management Preferences

(CONFIDENTIAL: Not to be revealed to anyone)
(Given to management negotiators only)

A. Wages.

Wage of \$12/hour—management receives 1,200 points.

Every penny above \$12/hour—management loses ten points.

Every penny below \$12/hour—management gains one additional point.

- Examples:
- 1) If the contract specifies a wage of \$8/hour, management receives 1,600 points.
 - 2) If the contract specifies a wage of \$12.63/hour, management receives 570 points.
 - 3) If the contract specifies a wage of \$13.20/hour, management receives zero points.

B. Relocation Clause. If the contract allows the company to avoid its labor costs by transferring work to other plants during the contract term, the company receives 200 points (300 points for group B).

NOTE: The contract can specify this, or the rule of law may allow the company to avoid labor costs by transferring work. In either case, the company receives 200 points (300 points for group B).

C. Vacation Days. For every vacation day, company loses ten points. Thus, if the contract calls for three vacation days, the company loses thirty points.

D. Noise Reduction. A new study indicates that productivity increases by 12 percent if noise is reduced to eighty decibels. Therefore, management receives 100 points if the contract institutes a program to reduce noise in the plant to eighty decibels.

GENERAL NOTE: ABC Company currently pays wages of \$5.80/hour and gives employees five vacation days/year.