Blindfolding the Jury to Verdict Consequences: Damages, Experts, and the Civil Jury

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This research examines the behavior of jurors as active information processors. Our experimental examination of the performance of the civil jury in response to a complex price-fixing case varies the information provided to jurors about the consequences of their damage award decisions (i.e., the treble damage rule) and the type of expert testimony (statistical models vs. concrete yardstick models). We find, consistent with a picture of the jury as active rather than passive, that jurors are more likely to follow judicial instructions when they are given explanations rather than bald admonitions. In addition, complex expert testimony neither overpowers the jurors nor is dismissed by them. The expert presenting a statistical model is viewed as having higher expertise but lower clarity; as a result the statistical expert and the expert presenting a more concrete model are not significantly different in their persuasiveness. Finally, in contrast to most research on the criminal jury, we find that deliberations do affect jury awards.

The American justice system confers extraordinary power on the jury. It allows the jury to determine guilt or innocence in criminal cases and to decide whether an offender should be sentenced to death. It also permits the jury to determine liability and set damages in civil cases. Yet if the system offers the jury great responsibilities, it is also ambivalent about the ability

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of laypersons adequately to perform their assigned tasks. Thus, critics and even admirers of the jury often express doubt that the jury can find its way through the labyrinth of complex and inconsistent facts or that the jury is able or willing to follow the legal rules it is asked to apply.

Early research portrayed the jury as a decisionmaker generally responsive to the evidence presented at trial (Kalven & Zeisel 1966). More recently, critics of the jury have raised a variety of questions about jury competence, voicing doubts about the ability of juries to analyze complex data logically and to return verdicts based on evidence rather than irrelevant considerations (see, e.g., Burger 1979). Critics have advocated a variety of methods of reining in and controlling the jury, from a complexity exception to the constitutional right to a jury trial (*In re Japanese Electronic Products Antitrust Litigation* 1980:1086) to less drastic measures like specially qualified juries (Nordenberg & Luneberg 1982) and rules that limit the information juries receive and are permitted to use in making their decisions (see Diamond et al. 1989).

I. Images of the Jury

In this research, we explore how juries react to two types of legal strategies designed to control decisionmaking: blindfolding the jury by denying it certain types of information, and channeling its decisions by providing information accompanied by directions as to how the information should be used. We focus on controlling jury decisionmaking in situations in which jurors' expectations about the consequences of their decisions may influence the way they evaluate evidence and apply legal standards.

We also examine the jurors' responses to a common and growing type of complex evidence: the testimony of experts. Some observers of the jury have suggested that jurors are incapable of evaluating the credibility of expert witnesses and of understanding the complex information contained in their testimony. Here we focus on how jurors assess and weigh statistical evidence.

Finally, we examine the process by which the predeliberation preferences of the individual jurors are transformed into the jury's final award. We analyze jury deliberations to explore the effects of the structure of initial preferences on jury awards, the selection and influence of the foreperson, and an inflation effect by which jury deliberation appears to increase the jury's award.

While there is a large body of research on the jury, much of it fails to go beyond studying the production of a verdict to examine the jury as an active information processor attempting

to make sense out of its task (for an example that does focus on information processing, see Hastie et al. 1983). By treating the jury as an active collaborator helping to develop a meaningful basis for decision rather than as a passive recipient simply accepting information and instruction, we can develop a clearer understanding of how this powerful institution performs.

A. The Jury as a Passive Participant

Much legal doctrine proceeds on the assumption that the jury is a passive participant in the trial until it is asked to deliberate and render its verdict. The jury is selected and placed in the jury box where it is presented with testimony elicited by questions from lawyers and occasionally from judges. Jury members typically are not permitted to clarify points by asking questions.[1] They are instructed to listen carefully to testimony but not to form impressions or make judgments about their ultimate verdict until all the testimony is completed and judicial instructions have been issued. Jurors are told not to discuss the evidence or possible verdicts until the end of the trial and after they have received final instructions on the law. While common sense suggests that few jurors will actually achieve the nearly complete passivity envisioned by these norms, the legal system proceeds as though these expectations are attainable goals. After hours, often days, and occasionally months of reviewing testimony and instructions, the jury is finally mobilized to deliberate. Metaphors like "sponge" or "tape recorder" seem accurately to capture the passive role that formal legal doctrine assumes the jury will fill until deliberations begin.

An image of the jury as tape recorder or sponge is also exemplified by rules dealing with such matters as pretrial publicity or instructions to disregard testimony. In both instances, the jurors are instructed to set aside information that is already available to them and to reach their verdicts based simply on the evidence that has been appropriately presented at the trial. While legal norms recognize that sometimes jurors simply cannot be expected to proceed in this fashion—a conclusion that may lead to a change of venue or a declaration of a mistrial—these rules emphasize the perception of the jury as a blank slate on which the trial testimony can be written—and sometimes even erased (Tanford 1991).

[1] While some audiences have been receptive to the idea of allowing jurors to ask questions, many attorneys have objected on the grounds that parties should have the right to control the production of testimony. For a judge's report on his experiences with allowing jurors to ask questions, see Wolfson 1987, and for some experiments in which jurors were allowed to ask questions, see Heuer & Penrod 1988.

B. The Jury as Active Information Processor

There is substantial evidence that like other human decisionmakers (Wong & Weiner 1981), jurors do not conform to these assumptions of passivity (e.g., Pennington & Hastie 1986). Rather, jurors bring expectations and preconceptions with them to the jury box, actively search for causal explanations to make sense of events about which they are told, and consciously or unconsciously process information, filling in blanks or interpreting ambiguities in testimony in ways that may strongly influence their decisions.

Perhaps the clearest example involves the role of juror expectations. The idealized legal model presumes that jurors simply listen to the testimony and proceed as though it contains most or all of what they know about the case. For example, parties generally cannot tell jurors whether the defendant in an automobile accident case carries liability insurance (see, e.g., Fed. R. of Evid. 411), for such information is held to be irrelevant to a determination of negligence.[2] Yet jurors are likely to bring to their deliberations the expectation that most defendants are insured, for most states require such insurance. To the extent that jurors are influenced by belief that the damage award will be paid not by the defendant alone but by the deep pocket of the insurance company, these expectations may have a substantial impact on jury decisions (Broeder 1959; Kalven 1957).

To make sense of the testimony they hear, jurors also search diligently for explanations for the conduct described to them at trial. As the Pennington-Hastie "story model" research suggests (Hastie et al. 1983; Pennington & Hastie 1986), jurors do not simply listen to testimony and apply verdict categories to the facts that have been related. The jurors they studied listened to the testimony, brought to bear their knowledge about the world and general rules of inference, and produced logically coherent "stories" of what happened in the set of events that led to the court proceeding—stories that included such elements as who did what, motivation, or intention. Jurors, according to Pennington and Hastie, then tested these stories against the set of verdict categories the judge provided. These mental representations were said to be the best predictors of verdict. Thus, under this view jurors are quite energetic information processors who actively (although of course often unconsciously) organize the testimony and make crucial inferences about motivation and causation.

Knowledge structures or information-processing heuristics may also influence the ways jurors interpret ambiguous testi-

[2] Note, however, that the existence and extent of insurance may affect the parties' incentives and thus such information may be relevant in judging their credibility.

mony and fill in "blanks"—information about which there is no testimony at all. In the work by Casper et al. (1988, 1989) on the role of the hindsight bias (Fischhoff 1975), jurors hearing evidence in a civil damage suit against officers alleged to have engaged in an illegal search were influenced by knowledge about the outcome of the search (whether evidence of crime was found in the search of the suspect-become-plaintiff's apartment). The influence of this outcome information operated primarily through its effect on jurors' recall of the testimony at the trial. Jurors hearing a suit by a defendant whose search produced evidence of a crime were more likely to interpret ambiguous testimony and to recall "facts" about which there had been no testimony (e.g., How experienced were the police officers making the search? How reliable was the informant who gave them a tip?) in ways favorable to the police than those who heard a case in which no evidence of illegal conduct was found. Processes like the hindsight bias or knowledge structures like scripts (Abelson 1981) may produce important interpretive activity on the part of the jurors.

Thus, jurors and juries play an important and active role in evidence interpretation. Such activities take place during the trial itself, affecting what is perceived and the way evidence is understood, not simply during deliberations.

Based on the evidence that jurors are active information processors, we can make a number of predictions about their response to the various approaches to channeling their decisionmaking. Some of the techniques we have examined are in common practice today: blindfolding, instructions to ignore information, and limiting instructions. Others are less often used—for example, directly discussing information that jurors may be troubled by and explaining why it ought to be used in a certain fashion or attempting to deal with jury concerns and inclinations by specifying that they will be addressed by the actions of the judge.

C. Efforts to Control the Jury

Blindfolding the Jury

One of the most commonly employed techniques for controlling juror decisionmaking is blindfolding, that is, withholding certain information. Juries typically cannot be told of the criminal record of a defendant who does not testify (Fed. R. Evid. 609); whether and how much liability insurance a defendant in a civil case has (Fed. R. Evid. 411); arrangements for payment of attorneys' fees; whether any original parties have settled, and for how much (Burns 1965); settlement offers that were rejected (Fed. R. Evid. 407); and that a jury award in a

private antitrust suit will by law automatically be tripled by the court (e.g., *Pollack & Riley, Inc. v. Pearl Brewing Co.* 1974).[3] Such rules about blindfolding typically are justified on several grounds, including the possible bias that might be introduced by the undisclosed information; the possibility that some facts are so complicated that they might confuse rather than inform the jury; and the common exclusion of "irrelevant" evidence that by definition lacks probative value and will thus at best waste the jury's time and at worst improperly bias its decision.

Blindfolding is unlikely to achieve its purposes when juries hold strong expectations about the information they are given. As noted above, if most jurors believe that defendants in automobile cases have substantial liability coverage, not telling them that the insured defendant has such coverage does not eliminate a possible deep-pocket effect. By the same token, if the defendant actually has little or no insurance, a blindfolded jury operating under a presumption that the defendant *is* insured may award more than true compensation because it incorrectly assumes a deep pocket will pay.

In a similar fashion, blindfolded jurors in private antitrust cases who are unaware of the trebling rule may add amounts for punishment and deterrence. In doing so, they may thus award damages greater than the amount they believe would be sufficient to achieve compensation.

Finally, blindfolding may produce undesired inter-jury variation. Some juries may have correct expectations (e.g., that damage awards will be trebled), while others operate on incorrect assumptions (e.g., that the total award to the plaintiff will be the amount chosen by the jury or that the jury itself is supposed to award triple damages).

Thus, both correct and incorrect expectations may lead the blindfolded jury astray. Moreover, by pursuing a policy limited to blindfolding, the legal system may ignore or rule out more effective alternative techniques like *voir dire* or judicial instructions to deal with the problems which blindfolding is intended to address.

Instructions to Ignore Evidence

Another common method of juror control that emerges from the assumptions of jury passivity involves instructing juries to ignore available evidence. Perhaps the most studied case concerns instructions to disregard testimony (e.g., Sue et al. 1973; Wolf & Montgomery 1977). This approach is perhaps the

[3] Treble damage provisions are not limited to antitrust suits. The Racketeer Influenced and Corrupt Organizations Act (1988) (RICO) also provides that any person injured by a RICO violation may sue in federal court and recover treble damages. 18 U.S.C. 1964(c) (1988).

clearest example of the crude tape-recorder or sponge model of jury behavior. After information is provided and a successful objection is lodged, the judge simply instructs the jurors to disregard the testimony. A similar process is employed in cases in which pretrial publicity has provided information to prospective jurors (see, e.g., Sue et al. 1974; Kramer et al. 1990). They are instructed to attend only to evidence presented in court, and jurors who assert they will base their decision solely on such information can be seated on juries even when they have been exposed to potentially biasing information. Both social science evidence and the advice of experienced attorneys who tell us that "you can't unring the bell" suggest that neither of these instructional approaches is effective. Jurors' search for causation and the attribution of motives make it difficult for them to ignore evidence that they find useful in making sense of the facts of the case, and an admonition to ignore cannot overcome this information-processing activity. Moreover, certain types of outcome information may directly affect jurors' recall and interpretation of testimony so that even the conscientious juror may be unable to obey the judicial instruction, for the encoding and recall of other information are affected by the very testimony that the juror is supposed to ignore (Casper et al. 1988, 1989).

Limiting Instructions

A third approach designed to harness jurors involves limiting instructions. Jurors are given information and are told that they are to consider it for one purpose but not for others. One frequently examined instance involves the prior criminal record of defendants who take the stand and testify in their own behalf (see, e.g., Wissler & Saks 1985). In most federal and state courts, the prosecutor is entitled in such a case to introduce the prior criminal record of the testifying defendant. When such information is introduced, the judge gives the jurors a limiting instruction telling them that they may consider such evidence only in evaluating the credibility of the defendant's testimony and that a prior conviction is not to be considered as evidence of guilt in the current case.

Common sense suggests that following this instruction is difficult, and substantial research indicates that it is probably impossible (Doob & Kirshenbaum 1972; Hans & Doob 1975; Wissler & Saks 1985). Jurors told about the defendant's prior record tend to convict at a higher rate than those not told, particularly if a defendant has a prior conviction for a crime much like the current one. Moreover, there is persuasive evidence that jurors do not simply discount the defendant's exculpatory testimony, but rather they use the criminal record itself as evi-

dence of probable guilt. Again, information-processing models that focus on attribution and the search for coherent stories may explain jurors' inability to use the information in the prescribed limited fashion.

All of these commonly employed methods of controlling the jury are based on rather crude models of jurors as passive information absorbers. All may be defeated by the information-processing activities in which jurors and juries actively engage. In this study of juries in antitrust cases, we examine the effects of some alternative approaches to jury control that were designed to be more consistent with what is known about how jurors handle information.

D. The Evaluation of Expert Testimony

The complexity and potential influence of expert testimony has been the subject of a good deal of recent debate about jury competence (e.g., Vidmar 1989; Imwinkelreid 1981; Hosch 1980; Tribe 1971). Many observers of the jury at work, and some researchers, have questioned the ability of jurors to comprehend and employ the many types of complex information that trials increasingly entail. Complex statistical evidence appears in suits involving antitrust violations, trademark infringements, deceptive advertising, race and gender discrimination in employment, and estimates of losses in a range of tort suits (Fienberg 1989; Saks & Van Duizend 1983). Medical malpractice, product liability, and criminal cases have brought a variety of complex medical and technological evidence into the courtroom.

This debate about whether typical lay juries can sift through expert testimony in an adversary setting implicates the traditional "sponge" theory. It revolves around the issue of whether the jury is able to soak up, retain, and accurately apply an assortment of complicated information, distinguishing complex but untrustworthy information from more reliable data. There has been a good deal of comment and argument about these issues (e.g., Rosenthal 1983), but little systematic research.[4]

One concern often raised is the weight jurors attach to statistical evidence. While some authors have predicted that jurors will overvalue the apparent precision that statistical results appear to provide (e.g., Tribe 1971), others have suggested that jurors are, if anything, likely to discount statistical information inappropriately (e.g., Saks & Kidd 1981; for a review of the evidence concerning jury evaluation of statistical data on base rates and error rates, see Thompson 1989).

Evidence from research on information processing and per-

[4] For notable recent exceptions to this neglect, see Thompson & Schumann 1987; Raitz et al. 1990.

suasion suggests that concrete or clinical models should be more influential than statistical models. Work on decision heuristics (Nisbett & Ross 1980; Kahneman et al. 1982) would predict that availability produced either by vividness or other sources of accessibility favors the more anecdotal approach of concrete and clinical models. More recent work on vividness tends to question its power as an explanatory concept (Taylor & Thompson 1982), but there does seem to be general agreement that, as McGuire (1985) suggests, anecdotal examples do tend to be more persuasive than statistics. If not a vividness effect, this outcome may occur because concrete evidence facilitates attention and comprehension more effectively, making it more available than abstract, statistical information (Taylor & Thompson 1982). This work would characterize the greater appeal of anecdotal over statistical evidence as a "bias" in decisionmaking. Attaching greater weight to a striking or easily recalled story as opposed to the more broadly sampled evidence from a statistical model violates traditional notions of rationality.

Substantial evidence also exists that concrete case studies are somewhat more persuasive than abstract statistical arguments (Ginosar & Trope 1980; Hamill et al. 1980; Reyes et al. 1980). The literature on persuasion also indicates that use of obscure and unusual words appears to reduce persuasiveness (Bowers 1963; Carmichael & Cronkite 1965), again suggesting that statistical models may suffer in comparison to the more concrete approaches.

The expert testimony we examine in this research was designed to test these predictions. In this study of antitrust juries, we compared a statistical model with another common method used to prove damages, the more concrete yardstick model.

II. Research Design and Methods

We showed 12 versions of a simulated videotaped antitrust price-fixing case to 1,022 jurors in a Cook County (Ill.) courthouse over a period of 8 months. We used six sets of judicial instructions to test the effects of blindfolding on the jury. The design also included two versions of expert testimony, creating a 6×2 factorial design, shown in Figure 1.

A. Instruction Conditions

In the first five conditions, jurors were instructed to compensate the plaintiff for any injury caused by the defendants' antitrust violations. The amount required to compensate the plaintiff is the standard for jury damage awards in antitrust

	Concrete Testimony for Plaintiff vs. Statistical Testimony for Defendants	*Statistical Testimony for Plaintiff vs. Concrete Testimony Defendants*
Trebling with No Admonition	5 juries 40 nondeliberators	5 juries 42 nondeliberators
Trebling with an Admonition	5 juries 41 nondeliberators	5 juries 46 nondeliberators
Trebling with an Explanation	5 juries 48 nondeliberators	5 juries 44 nondeliberators
Motive Control	5 juries 39 nondeliberators	5 juries 44 nondeliberators
No Information	5 juries 40 nondeliberators	5 juries 42 nondeliberators
Unconstrained	5 juries 79 nondeliberators	5 juries 100 nondeliberators
Total	**35 juries** 209 nondeliberators 287 nondeliberators	**35 juries** 208 nondeliberators 318 nondeliberators
	1022 jurors	

Figure 1. Basic design for antitrust study

cases, and the judge then trebles this amount to produce the final award. In experimental *condition 6* ("unconstrained"), the jurors were instructed simply to award the amount they felt was appropriate and reasonable in the circumstances. We introduced this condition in order to obtain a measure of juror preference unfettered by judicial instruction on the standard for a damage award.

In the first three conditions, the judge informed the jurors that their verdict would automatically be trebled:

In *condition 1* ("trebling without admonition"), the jurors were simply told that their verdict would automatically be trebled. They were told neither to disregard nor to consider this information in their decision.[5] The policy of blindfolding the jury to trebling assumes that jurors in this condition will give reduced awards to avoid a plaintiff windfall.

[5] The instruction in the trebling with no admonition condition on the issue of trebling was:

> Now, under the antitrust laws, the judge will award to Granite Road three times the amount of damages which the jury finds. That is, if you find that Granite Road suffered X dollars in damages, the judge will order the defendants to pay a total of 3 times that amount to Granite Road.

In *condition 2* ("trebling with admonition"), the jurors were told that their verdict would automatically be trebled but that this information should not influence the size of their damage award.[6] This condition was the general practice prior to the mid-1970s when courts began to advocate blindfolding the jury to the trebling provision of the statute (*Pollock & Riley, Inc. v. Pearl Brewing Co.* 1974). Research on limiting instructions in other contexts suggests that such a simple admonition is a crude and ineffective way to control jury behavior (see, e.g., Wissler & Saks 1985).

In *condition 3* ("trebling with explanation"), the jurors were told that their verdict would automatically be trebled and were instructed that this information should not influence the size of their award. In addition, the instructions explained the reasons for the trebling provision of the statute, pointing out that the provision was designed to deter and punish price-fixing agreements; jurors were thus provided with a rationale for awarding full compensation even when that amount would be tripled by the judge.[7] Although several researchers have tested the effects of juror instructions and found only a modest impact on jurors (e.g., meaning of negligence—Elwork et al. 1977; burden of proof—Severence et al. 1984), in all previous cases the instructions have amounted to bald directives. No prior work has tested the effect of reasoned admonitions. Thus, in condition 3, trebling with explanation, we gave jurors the information that their damage awards would be trebled and explained why they should not employ this information to go below their assessment of the amount required for compensation.[8]

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[6] The instruction in the trebling with an admonition condition on the issue of trebling began with the language quoted in note 5, followed by:

> Your job, however, is to decide only on the amount of damages, if any, suffered by Granite Road. The fact that the damage award will be tripled should in no way affect your decision. It is the judge's job to multiply the amount you award by 3 and to order the defendants to pay that amount.

[7] The instruction in the explanation condition on this issue was:

> Now, under the antitrust laws, the judge will award to Granite Road three times the amount of damages which the jury finds. That is, if you decide that Granite Road suffered X dollars in damages, I will order the defendants to pay a total of 3 times that amount to Granite Road. Your job, however, is to decide only on the amount of damages, if any, suffered by Granite Road. The fact that the damage award will be tripled should in no way affect your decision.
>
> If you reduce your damage award below what you believe to be the appropriate compensation amount in anticipation of its being tripled, you will be defeating Congress's purpose in providing for triple damages. Congress decided to have jury compensation awards tripled in order to provide for punishment of the defendants for their law violation and to deter them and others from future law violation.

[8] Some of our earlier research suggested that jurors may understand and accept the rationale for trebling of damage awards if they are given an explanation for trebling (Diamond et al. 1989). In a telephone survey of 192 jury-eligible citizens, we asked respondents if they thought trebling was a good idea or a bad idea. Half the respondents were asked to evaluate the trebling rule before they were told about the purposes

In *condition 4* ("motive control"), jurors were not told about the automatic trebling provision of the statute but instead were informed that the judge would add an amount for punishment and deterrence if appropriate. This approach was designed to counteract any juror inclination to add to the compensatory award in order to punish or deter, without fully removing the blindfold as to how the damages would be calculated (keeping them in the dark about automatic trebling).[9] The instructions do not guarantee an award for punishment or deterrence but indicate that the judge will address such concerns if appropriate.[10]

In *condition 5* ("no information"), jurors received no indication that the judge would add to their damage award.[11] This is the form of instruction used by most courts.

Finally, in *condition 6* ("unconstrained"), jurors were asked simply to award an amount that was appropriate and reasonable in the circumstances.[12]

Effects of Trebling Information

We designed the study to test five hypotheses about the ef-

of the rule, and half evaluated the rule after they were told of its suggested purposes. Support for the rule was significantly higher—75% versus 60%—among respondents who were told the purposes of the rule ($\chi^2=4.12, p<.05$). These preliminary data suggested that an explanation of purposes might encourage jurors not to reduce awards to avoid a plaintiff windfall.

9 This approach to jury control was suggested in Michigan Law Review 1983.

10 The judicial instruction on this issue in the motive control condition was:

> In deciding upon damages to be awarded to the plaintiff, you should consider only the amount necessary to compensate the plaintiff for the damages caused by the price-fixing agreement. After you have decided the appropriate amount necessary to compensate the plaintiff for harm done, my job as the judge will be to add an additional amount to the plaintiff's award, if such an addition is deemed necessary to punish the defendant companies for their law violation and to deter them and others from similar acts in the future.

11 The judicial instruction in the no information condition included the general instructions from conditions 1–4 informing jurors to focus on compensation and providing general principles about how damages were to be computed (e.g., they should not be based on mere guesses or estimates of witnesses; damages should be awarded only if they flowed necessarily and immediately from the wrong). The jurors were then told to deliberate and set a damage award, with no reference to trebling or any other additional amounts that might be added by the judge.

12 The judicial instruction for computing damages in the unconstrained condition read:

> It is now my duty to instruct you as to how to compute the measure of damages in this case. You must determine the amount of money which it is reasonable and fair for the plaintiff to receive in light of the actions of the defendant companies.
>
> The general rule of the subject of damages is that the amount of money to be awarded shall be appropriate and reasonable in light of the actions of the defendants. The law does not require that the plaintiff make exact proof, in dollars and cents. But the law does require that the amount of your award be based upon the testimony that you have heard in this case or upon inferences that reasonably flow from such testimony.

Note: The last two sentences appeared in all instruction conditions.

fects of judicial instructions. The first and most general deals with the effect of knowledge about trebling on juror awards, while the remainder explore in more detail how such knowledge might influence juror verdicts.

Hypothesis 1: Jurors told about the automatic trebling provision of the antitrust statute (conditions 1–3) will award less than jurors who are not told that their verdict will automatically be trebled (conditions 4–6).

This result might be the product of two quite different juror reactions, windfall avoidance or punishment–deterrence. If windfall avoidance is operating, the trebling information should cause jurors informed of trebling to reduce their award to avoid giving the plaintiff a windfall. Thus, these jurors would *lower the award* they would have given had they not been informed of the trebling rule.

But a difference in awards by jurors told or not told about trebling can also arise in another way. Jurors not informed that their award will be trebled may include in their award not only the amount they feel is necessary to compensate the plaintiff but also an additional amount aimed at punishing the defendants and deterring them and others from future similar behavior. Unaware that the statute itself provides for punitive damages by tripling the jury's award, they may *add to the award* they otherwise would have given.

Hypotheses 2–5 test the operation of these two potential influences on awards: windfall avoidance and punishment-deterrence.

Controlling Windfall Avoidance Effects with Judicial Instructions

If jurors informed about the automatic trebling rule are motivated to reduce their damage awards to avoid a plaintiff windfall, judicial explanation of the trebling rule may reduce this motivation.

Hypothesis 2: Jurors admonished to disregard the fact that their verdict will be automatically trebled (condition 2) will award no more than jurors who are told that their verdict will automatically be trebled and who receive no admonition to disregard that information (condition 1).

Hypothesis 3: Jurors informed about the automatic trebling provision and given an explanation for this policy (condition 3) will give larger awards than jurors informed of trebling but given no explanation for the policy (conditions 1 and 2).

Hypothesis 2 predicts that a judicial admonition to disregard the fact that the award will be trebled will not dissuade jurors from lowering their damage awards. Such admonitions to disregard generally have little or no effect, presumably because they offer little incentive to comply or, in this case, to change views on the appropriate damage amount. In contrast, hypothesis 3 predicts that a judicial instruction informing jurors about the trebling rule but explaining why they ought not let this information cause them to lower their award (i.e., that such behavior would defeat Congress's purposes of adding punishment and deterrence to the appropriate compensation amount) will alleviate some or all of the tendency to reduce damage awards.

Punishment and Deterrence Hypotheses

If blindfolding jurors to the automatic trebling provision leaves them with a motive to punish and deter, their verdicts will exceed the amounts they would award strictly to compensate the plaintiff:

> *Hypothesis 4:* Jurors instructed to award an amount necessary to compensate the plaintiff for the damages caused by the defendant's antitrust violation (condition 5) will award less than jurors who are told to award an amount that is reasonable and fair (condition 6).

> *Hypothesis 5:* Jurors who are told the judge will, if necessary, add to their award for the purposes of punishment and deterrence (condition 4) will award less than if they are simply told to compensate (condition 5).

Hypothesis 4 suggests that jurors *not* told to focus on compensation (condition 6) will give higher awards than those told to focus on compensation (condition 5). Hypothesis 5 predicts that informing jurors that the judge may add amounts for punishment and deterrence will prevent jurors from adding to their own award in order to punish and deter. The hypothesis is based on the assumption that learning that the judge will take care of punishment and deterrence will reduce the motivation to add such amounts.

As these hypotheses indicate, we predicted that awards would generally increase from condition 1 through condition 6, with no difference expected between conditions 1 and 2.

B. Alternative Models of Damages in Antitrust Cases

The plaintiff and the defendant in an antitrust suit generally propose quite different estimates of the damages allegedly suffered as a result of the defendant's alleged antitrust violation. Thus, in a price-fixing case like the one we used, if the jury finds that the defendants have engaged in an illegal agreement to fix prices, it must determine how much damage the agreement caused the plaintiff. This task involves a "but for" calculation of the extent to which the plaintiff's profits would have been higher had there been competitive rather than monopoly pricing. Two common approaches employed by parties in such cases are the "yardstick" and regression models.

Yardstick models employ comparative data from similar firms that conducted their business in competitive markets at the time of the defendant's anticompetitive activity and are based on the premise that the difference in prices paid or profits made by the benchmark firms and the plaintiff will index the excess costs imposed on or profits lost by the plaintiff company (e.g., *Zenith Radio Corp. v. Hazeltine Research, Inc.* 1969; *Moore v. Jas. H. Matthews & Co.* 1982). Valid comparisons are, of course, quite difficult to obtain,[13] for a valid comparison involves acquiring data from a firm or firms similar in nearly every respect other than the experience of the alleged antitrust violations. But such yardstick measurements are concrete and relatively easy to understand.

Another common approach involves the use of regression models, typically employing time-series analyses of pricing patterns before, during, and sometimes after the price-fixing agreement. These models attempt to predict what prices would have been like during the period of price fixing had there been no illegal agreement (see Rubinfeld 1985:1087–94 for a description of forecasting methods). Such an approach is more abstract and technically complex than the yardstick method.

We constructed two versions of the case to test the effects of different expert models. In version 1, the expert presenting the yardstick model testified for the plaintiff while the expert presenting the statistical model testified for the defense; in version 2, the statistical model was presented on the plaintiff's side and the yardstick model was presented for the defense. If the two expert models were equally persuasive, the two versions

[13] Note the similarity of this approach to the nonequivalent control group quasi-experimental design

$$\frac{O \times O}{O \quad O},$$

where X is the price-fixing agreement and O is the measure of profit. The strength of the design depends on the comparability of the nonequivalent groups (Cook & Campbell 1979).

should have produced similar awards. If one of the models was more persuasive, awards should have been higher when that model was presented on behalf of the plaintiff. (See Figure 2 and the accompanying section below for a more detailed presentation of the expert hypotheses.)

C. The Price-fixing Case

The case we used involved a price-fixing agreement in the road construction business. Two suppliers of crushed rock who controlled 70% of the business in Colorado agreed to set the same price for their product. The plaintiff was a road construction company, a long-time customer of the two suppliers. The owner of the company sued, claiming that the price-fixing agreement had caused $490,000 in damages. An earlier trial had established that the illegal price-fixing agreement had indeed occurred, and the issue for the jurors in this trial was to determine damages. The defendants claimed that any damages that had occurred probably amounted to only $35,000. The case presented testimony from opposing expert witnesses who presented conflicting damage models. The damage claimed by the plaintiff's expert was $490,000, although he conceded that the figure might be as low as $420,000. The defendant's expert presented a model which produced a damage amount of $35,000 but also admitted that the number might be as high as $105,000.

The simulated trial lasts about an hour and 15 minutes and contains all of the basic elements of an actual trial. It includes opening statements by plaintiff and defense attorneys, direct and cross examination of witnesses by both sides, closing arguments by both sides, and instructions by the judge. Professional actors following a script played all roles in the trial, which was videotaped in the courtroom at Loyola Law School in Chicago.

D. Procedures

The jurors who participated in the study were randomly selected from those called for jury service at a Cook County (Ill.) courthouse. They were informed of the nature of the study and were told that their participation was completely voluntary. They filled out a brief pretape questionnaire on demographic attributes (e.g., age, race, education, occupation), prior jury experience, and attitudes toward business and toward expert witnesses. After completing the first questionnaire, they viewed the simulated trial. At the end of the trial, the jurors were asked to fill out an individual verdict form, indicating the dollar amount they would award to the plaintiff in the case. They were

then randomly divided into two groups, which we will refer to as *deliberators* and *nondeliberators*. Nondeliberators then filled out a 17-page questionnaire, which had a few open-ended questions and a large battery of closed-ended questions, dealing with such matters as their reasons for their verdict, their strategies in arriving at their damage award, evaluations of the plaintiff and defendant companies, evaluations of the price-fixing agreement and the motives of the defendants in taking part in it, assessment of all the witnesses and the attorneys, items testing their comprehension of the testimony and of the judicial instructions, items designed to assess the general "story" the jurors had constructed of what happened in the case (cf. Pennington & Hastie 1986), and items dealing with their assessment of the difficulty of the testimony and of making a decision in the case. The nondeliberators were excused after completing their questionnaires.

The six randomly selected deliberators in each panel were first taken to lunch, allowing them some time to become acquainted with one another and simulating in a modest way the types of interpersonal interactions that a real jury might experience in the course of an extensive trial. Deliberators were then sent to a jury room and asked to deliberate to a verdict on the size of the damage award. After completing their deliberations, the deliberators were asked to fill out a somewhat truncated version of the questionnaire given to nondeliberators and were then excused.[14] The deliberations were videotaped; they took an average of 34 minutes.

The original research design called for the simulated trials to be shown to 70 groups of jurors, divided between 420 (6×70) who took part as deliberators and an additional 480 nondeliberators. Because of the one-day/one-trial system used in Cook County, court officials know relatively early in the day how many members of the jury venire are needed for trials that will take place, and were very cooperative in allowing those not likely to be called for jury service to be invited to participate. Jurors themselves proved very willing to participate in the study as well; 91% of those invited to participate accepted the invitation and a total of 1,022 adults called for jury service[15]

[14] This research strategy of random assignment to deliberation or nondeliberation conditions was designed to facilitate understanding the process by which individual verdicts are transformed into jury awards. By comparing the perceptions and evaluations of nondeliberators with responses to the same questions made by jurors after they have completed deliberations, we can examine the effects of the deliberation process itself. See sec. III.E below.

[15] Of the jurors, 49% were females and 51% were males; 22% were less than 30 years old, 49% were between 30 and 49, and 29% were 50 or older; 6% had less than a high school education, 24% had a high school diploma or technical training, 28% had some college, 23% had a college degree and 18% had graduate school experience; 70% classified themselves as white, 23% as black, and 7% as Hispanic, Asian, or Native

participated in the antitrust jury research, 417 deliberators and 605 nondeliberators.[16]

III. Results

The case provided conflicting evidence on the amount of damage caused by the price-fixing agreement. As a result, when the jurors began deliberating, there was generally substantial disagreement among them, a pattern common for jury deliberations (e.g., Kalven & Zeisel 1966). Despite the rather dry testimony and complex expert evidence that is typical of trials like this one, the jurors showed substantial interest in the trial and demonstrated lively involvement during deliberations. We begin here with the predeliberation awards of all jurors, deliberators and nondeliberators. Later, we will discuss the patterns of awards among juries that deliberated to verdict.

A. Variability in the Dependent Variable

The dependent variable, award size, is characterized by high variance. The testimony by the two sides implied that damages caused by the price-fixing agreement might range from 0 to $490,000. The individual juror verdicts had an overall mean of $208,905, a range of 0 to $1,470,000, and a standard deviation of $182,701; as indicated in Table 1, the high variance occurred in all six instruction conditions. In the primary statistical analyses reported here, we will focus on the damage award as measured in terms of the jurors' actual dollar awards.[17] When we transformed the damage awards into ranked data, there was no change in the results reported here.

B. Data Sources

The results reported here come from two primary sources. The first are the questionnaires filled out by all 1,022 subjects, including the pretape questionnaire, the predeliberation verdict, and the posttrial questionnaire dealing with recall and

American. There were no differences in the characteristics of deliberators and nondeliberators.

[16] Three of the juries had five members. The number of nondeliberators was determined in part by the number of jurors available for participation on any particular day.

[17] Although log transformations are often appropriate for variables involving dollar amounts because of their strong positive skew, a log transformation is not appropriate here. Rather than having a positive skew, the distribution is rather lumpy, with substantial nodes at award amounts emphasized by the two sides in the testimony (e.g., $35,000, $105,000, $420,000, $490,000). We did, however, test the sensitivity of our results to extreme values by truncating the awards at $500,000 to reduce the effect of the 4 extreme awards ($1 million or above) on the results. This produced no change in the findings.

Table 1. Tested Contrasts

	Mean Award (Standard Deviation)	Hypothesis 1 (Trebling)	Hypothesis 2 (Admonition)	Hypothesis 3 (Explanation)	Hypothesis 4 (Reasonable to Compensate)	Hypothesis 5 (Motive Control vs. No. Info.)
Condition 1						
Trebling with no admonition ($N=141$)	$155,281 ($173,919)	1	1	1	0	0
Condition 2						
Trebling with admonition ($N=141$)	$176,067 ($163,161)	1	−1	1	0	0
Condition 3						
Trebling with explanation ($N=150$)	$213,722 ($202,028)	1	0	−2	0	0
Condition 4						
Motive control ($N=142$)	$259,172 ($187,424)	−1	0	0	0	1
Condition 5						
No information ($N=139$)	$211,960 ($174,898)	−1	0	0	1	−1
Condition 6						
Unconstrained ($N=296$)	$221,101 ($179,042)	−1	0	0	−1	0
		$p<.001$	$p=.334$	$p=.009$	$p=.585$	$p=.029$

evaluation of witnesses, testimony, instructions, and the like. For some purposes (e.g., characterizing the whole sample or examining the relationship between personal attributes and predeliberation verdicts), we employ data from the whole sample. Since the deliberators filled out their main questionnaire only after deliberations and thus their responses could have been affected by what occurred in their jury, we employ the subsample of 605 nondeliberating jurors when we explore the ways in which individual attitudes relate to other case-related judgments (e.g., the relationship between evaluations of the experts and damage awards). In exploring the transformation process by which predeliberation judgments are turned into a jury decision, we focus on the subsample of 417 deliberators, who filled out the main questionnaire after they had completed their deliberations. In examining some aspects of the transformation process, we compare the nondeliberators to the deliberators, using the data from the nondeliberators as a baseline for assessing the effects of deliberation on judgments and decisions of those who participated in a jury. Finally, for some of the analyses dealing with the jury itself, we focus on aggregate attributes of the 70 juries. Thus, the sample sizes vary, depending on which samples are appropriate. The second major source of data comes from the deliberations of the 70 juries. These deliberations were videotaped, and complete and verified verbatim transcripts of 60 of the deliberations were prepared.[18] These transcripts were subjected to various forms of content analysis, some performed by individual coders and some employing computer-based text analysis.

C. Effects of Judicial Instructions

Table 1 shows the planned contrasts corresponding to the five tested hypotheses. As the table indicates, there was substantial variation in the level of predeliberation awards across the six instruction conditions. The level generally increased from condition 1 to condition 6, as the hypotheses predicted, although not all of the expected differences were found.

Hypothesis 1

Jurors who were told about trebling gave significantly lower awards than jurors who were not told ($t=-4.19$, df$=1{,}003$, $p<.001$),[19] as hypothesis 1 predicted.[20] Thus, either trebling information was causing jurors to reduce their awards below

[18] As indicated above, the original 70 juries which deliberated to verdict included twice as many in the unconstrained condition (20) as in the other instruction conditions (10 each). As a result, we randomly selected 10 of those 20 juries, producing 10 transcriptions for each of the six instruction conditions.

[19] A Bartlett-Box test indicated no violation of the assumption of homogeneity of

what they believed would be necessary to compensate the plaintiff or the desire to punish and deter was causing jurors not informed of the trebling rule to elevate their awards above what they believed would be necessary to compensate the plaintiff. The third possibility was that both forces were operating, the first reducing awards and the second increasing them.

Hypothesis 2

Although jurors told about automatic trebling but not admonished to ignore the trebling rule gave somewhat lower awards ($155,281) than did jurors who were told but admonished to disregard it ($176,067), the difference did not approach significance ($t=-.97$, $p>.10$). The absence of a significant difference is consistent with other research which has shown that simple admonitions to disregard are ineffective methods of jury control (e.g., Wissler & Saks 1985 on the defendant's prior criminal record; Broeder 1959 on defendant insurance).[21]

variance in verdicts across the six instruction conditions, so the pooled variance estimate was used; degrees of freedom for each contrast were 1,003.

[20] We conducted the same analyses on the *jury* as well as the *juror* awards. As indicated below, the order of the average jury awards in the six conditions mirrored the order of the average juror awards.

Instruction Condition	Mean Jury Award
Trebling without admonition	$201,000 ($n=10$)
Trebling with admonition	223,850 ($n=10$)
Trebling with explanation	237,100 ($n=10$)
Motive control	317,778 ($n=9$)[a]
No information	250,750 ($n=10$)
Unconstrained	330,231 ($n=20$)

[a] The jury in this condition not included in the jury analysis could not reach a verdict. At the end of the deliberation, 5 of the jurors agreed on $490,000, but the lone hold-out was unwilling to award more than $250,000.

Although the small number of juries in each cell of the design meant that the power of the comparisons was low, the test of hypothesis 1 did produce a significant difference ($t=-2.69$, df=63, $p=.009$).

[21] Most work on the effect of admonitions has examined the effects of admonitions to disregard testimony jurors thought important (e.g., the criminal record of the defendant) and/or which was likely to be directly implicated in the jurors' "stories" or understanding of what happened in the case (e.g., whether a search produced evidence of illegal conduct). In such cases, either jurors may consciously choose to ignore an admonition to disregard testimony they see as important, or the material may be so embedded in the "story" they have arrived at that even a conscientious juror may be unable to comply with the admonition (Pennington & Hastie 1986; Casper et al. 1989). Here we deal with an admonition to disregard on an issue that seems less likely to be the subject of conscious juror nullification or to the information-processing effects observed in prior studies. Yet the ineffectiveness of bald instructions to disregard is again observed. It is only when the admonition is coupled with an explanation that the admonition influences verdicts.

Hypothesis 3

If jurors informed about trebling do tend to lower their awards to avoid a windfall for the plaintiff, can this effect be overcome or reduced by an instruction that informs the jurors about trebling but also provides an explanation to justify the larger award? The average award of $213,722 given by jurors in condition 3 (trebling with an explanation) was significantly higher than the average award of $165,674 given by the jurors in conditions 1 and 2 who were told about trebling but given no explanation for it ($t=-2.63, p<.01$). The fact that the explanation raised awards provides evidence that windfall avoidance was indeed occurring in conditions 1 and 2.

Our preliminary analysis of the deliberations by the juries in conditions 1, 2, and 3 reveals that the admonition *did* affect what was discussed during the deliberations. We examined the jury deliberations for mentions of the trebling rule.[22] Juries in condition 1 who were told about trebling but received no admonition averaged 20.2 relevant mentions, significantly more than the juries in conditions 2 and 3, who averaged 4.5 and 3.2 mentions ($F_{(2,27)}=11.57, p<.001$). Thus, the admonition alone, without the explanation for the rule, failed to remove the effect of the trebling information on juror verdict preferences, even though it was quite successful in controlling jury discussion.

The tests of the first three hypotheses thus indicate that trebling information does produce a reduction in awards but that the reduction is either partly or fully avoided when jurors receive an explanation for the trebling provision. The tests of the final hypotheses (4 and 5) assess juror inclination to punish and deter, inflating jurors' awards when they are not told about trebling.

Hypothesis 4

If jurors are inclined to punish antitrust violators and to mete out substantial awards designed to deter them and others from future antitrust violations, jurors unconstrained by an instruction to focus exclusively on compensation should give higher awards than jurors instructed to restrict their awards to compensation. Yet the average award of $221,101 by the unconstrained jurors in condition 6 was not significantly higher than the average award of $211,960 given by the jurors in condition 5 who were simply instructed to compensate the plaintiff for his antitrust losses ($t=-.55, p>.10$). One possible explana-

[22] Using the text analysis software, we searched the 30 jury deliberation transcripts from the first three conditions to identify all mentions of the following words and phrases: (1) "three times"; (2) "3 times"; (3) "treble" or a word with "trebl-" as a root; or (4) "triple" or a word with "tripl-" as a root. We identified 436 mentions of any of these words and phrases and coded 293 as referring to the treble damage provision.

tion for these similar responses is that neither set of jurors was inclined to add to their awards in order to punish and/or deter. Alternatively, both may have added to the awards, and the directive to award only the amount necessary to compensate in condition 5 may have been unsuccessful in controlling the desire to punish and/or deter. As we shall see below, the first explanation appears more consistent with other findings.

Hypothesis 5

If the jurors in condition 5 were motivated to punish and/or deter, we expected that their awards would be higher than those of the jurors in condition 4 who were given information designed to control that motivation. We designed the motive control instruction to examine a policy that has been proposed (Michigan Law Review 1983) to prevent inflated awards by jurors who wish to punish defendants in antitrust cases. The notion behind the proposal is that if jurors are told that the judge will mete out appropriate punishment, the jurors should be more inclined to confine their damage award to the amount they believe is necessary to achieve compensation. If the instruction had functioned as expected, jurors given the motive control instruction (condition 4) should have given awards that were lower than those of jurors in condition 5 who were provided with no information about trebling. Certainly they should not have given awards that were significantly higher. But the jurors in the motive control condition awarded an average of $259,172, an amount significantly higher than the average award of $211,960 in condition 5 ($t=2.19$, $p<.05$). It appears that something in the motive control instruction inflated rather than controlled jurors' awards.

Two factors appear to have combined to produce this inflation: framing and a response to uncertainty. Our analyses indicate that by mentioning punishment and deterrence, the motive control instructions *framed* the case as more serious and the behavior of the defendants as more blameworthy, or at least made punishment and deterrence more available to the jurors as goals in setting awards.[23]

To test for a framing effect, we compared juror ratings on a 12-item index of defendant blameworthiness for jurors in conditions 3 and 4 in which punishment and deterrence were mentioned in the judicial instructions with the ratings of jurors in other conditions (in which punishment and deterrence were

[23] If the mention of punishment and deterrence made these goals cognitively more *available* to jurors (Kahneman et al. 1982), jurors might have taken these goals more into account in their individual decisions and been more likely to employ them in arguing for higher awards during deliberation. Our data do not enable us to distinguish between framing and availability effects.

not mentioned).[24] Jurors who heard an instruction that mentioned punishment and deterrence did tend to see the defendants as somewhat more blameworthy ($M=64.4$) than those who heard no mention of punishment and deterrence in the instruction ($M=62.3$; $t=2.04$, df$=544$, $p<.05$).[25]

Framing, however, cannot be the entire explanation. Despite the fact that the instructions in both conditions 3 and 4 mentioned punishment and deterrence, subjects in the motive control condition gave awards that were on average $45,000 higher than those in condition 3. The reason for this difference, we believe, lies in the uncertainty of what the judge will award in the motive control condition. In the trebling with explanation condition, the judge both discussed punishment and deterrence and at the same time described a clear formula by which these goals would be achieved—the trebling of the jury's award for compensation. In the motive control condition, the judge also introduced the issues of punishment and deterrence, but in this instruction condition he offered a vague and contingent method for dealing with them.[26] As a result, jurors in the motive control condition were confronted with a case that had been framed as more serious (than in all other conditions except condition 3) but without clear guidance about how much of an award the plaintiff would ultimately receive. Their response was to take control of the damage award process themselves, ensuring that the plaintiff would receive an award commensurate with the defendants' harmful acts. This unanticipated effect for motive control not only produced a result contrary to our fifth hypothesis—damage awards larger than those in the condition 5 (no information)—but produced awards in the motive control condition that were larger than those in *any* of the other five.

Comparing the Effects of Instructions against True Compensation

Much of the discussion of the effects of blindfolding or informing jurors about the treble damage rule turns on the notion of "true compensation." Those favoring the policy of blindfolding fear that knowledgeable jurors might dip below this level to avoid providing a perceived windfall to the plaintiff. An alternative view suggests that blindfolded jurors will go

[24] The blameworthiness index was the total of 12 seven-point rating scales which assessed jurors' perceptions of defendants' fairness, justness, lack of greed, and honesty; the price-fixing agreement's rightness, fairness, morality; and the defendants' lack of intent to harm and lack of careful planning, how much harm the price-fixing agreement did to the plaintiff, whether it was appropriate for the plaintiff to sue the defendants, and whether jurors thought that price fixing should be a criminal offense. Cronbach's Alpha for the composite scale was .84.

[25] The potential impact of framing on awards is indicated by the correlation of .43 between the index of defendant blameworthiness and the size of juror awards.

[26] See last sentence of the instruction quoted in note 10.

above true compensation to provide a measure of punishment and deterrence in their damage award.

Our original assumption was that the true compensation level would fall between conditions 3 (trebling with explanation) and 4 (motive control). We reasoned that the three trebling conditions would tend to produce a windfall effect moving awards below the compensation level, with the explanation condition reducing this effect and moving awards up toward true compensation, thus placing true compensation at or above average awards in condition 3. On the other side of the coin, we reasoned that the three conditions not mentioning trebling (motive control, no information, and unconstrained) would produce the highest awards because subjects would be most inclined to add amounts for punishment or deterrence in these conditions, raising awards above the compensation level. We assumed that the motive control condition would be most effective at removing this inflation effect and thus that true compensation would fall at or below awards in condition 4.

As indicated above, if inflation occurred in conditions 4, 5, and 6, the motive control instruction did not remove it, for awards by these subjects were the highest of any group. If we then set aside the motive control condition, our estimate of true compensation should fall between condition 3 (trebling with explanation) and condition 5 (no information). The results indicate that these two conditions are nearly identical ($211,960 and $213,722, respectively), and we thus conclude that true compensation lies in the $210,000–$215,000 range.

One puzzle remains: What of the expectations that jurors not informed about trebling would raise their awards substantially above true compensation in order to add amounts for punishment and deterrence? Although we and others assumed that the motive to punish and deter would operate, our evidence does not suggest that it played a significant role in our jurors' consideration and decisionmaking. Our analysis of the transcripts of jury deliberations reveals that very little discussion focused on the need or desirability of punishing the defendants or deterring them or others.[27]

Even when jurors were set free to employ any criteria they wished (condition 6), there is little evidence that the motivation to punish and deter played a significant role in individual or

[27] Using the text analysis software, we searched the 60 deliberation transcripts for all statements mentioning words likely to index the motive to punish or deter, and examined all instances in which the following words occurred: "penalty"; "punish" and other words with "punish-" as a root; "lesson"; "deter" and other words with "deter-" as a root. We identified 280 mentions of these words or phrases and coded 83 of them as indicating the desirability of punishing or deterring the defendants. The bulk of the other mentions of punishment and deterrence came in the trebling conditions and involved speculations by jurors about the reasons for the treble damage policy. The average number of mentions across all juries was 1.4 per jury.

group decisionmaking. Thus, the evidence from the deliberations suggests that the expected strong motivation to punish does not appear to be present in the jurors we studied.[28] This absence of an apparent inflation over compensation in the nontrebling conditions produces the similarity between the awards in the no information condition and those in the trebling with explanation condition.

Evaluating Current Policy

The most common instruction in current use makes no attempt to control jurors' punitive impulses or to inform them of the consequences of their decision: the jurors cannot be told, by the judges or by the parties, that their verdict will be trebled. This no information condition produced juror awards that were nearly identical to juror awards in the trebling with explanation condition. The instruction in the explanation condition attempts to recognize the jurors as active information processors, both by telling them that their verdict will be trebled and explaining the purposes of the statutory trebling provision. If no information and a more complete explanation produce comparable results, are there any reasons to prefer one over the other?

One value of the full disclosure approach is that it deals with the potential "wild-card" juror who happens to know that antitrust awards are automatically trebled. The blindfolded jury that hears about the automatic trebling provision from a fellow juror will not receive a judicial admonition warning that trebling should not be considered, let alone a judicial explanation of why it should not affect the jury's decision. The official blindfold may thus lead to a reduced award to eliminate the perceived windfall—just as it does in condition 1 (trebling without admonition) of this experiment.

While jurors knowledgeable about trebling are rare, the odds of getting at least one on a jury are not insignificant. In a survey we conducted of jury-eligible Chicagoans, 2 of 192 knew about automatic trebling in private antitrust suits. Extrapolating from that rate of knowledge, 6% of six-person juries would be expected to have at least one member with knowledge of trebling when the case begins.[29] In the course of the trial,

[28] It is possible that this lack of motivation to punish and deter might in part be an artifact of our case and that in cases with more egregious violations or larger damages to the plaintiff, such a motivation might play a more significant role.

[29] We examined the deliberations of all 20 juries in conditions 4 and 5 and 10 of the juries in condition 6. None of these juries received a trebling instruction, but the topic of trebling came up in 3 of the juries. Although it is unclear whether the discussion influenced any jury verdicts, in one case the foreperson (an attorney) hesitantly raised the topic, saying "I don't think you're allowed to take that under consideration, but I think we should." In the other two cases, the topic was briefly mentioned but did not produce any discussion.

others may learn from uncontrolled sources outside the courtroom. Moreover, the likelihood may be growing because a number of states in recent years have eliminated the exemption of attorneys from jury service (e.g., Illinois Rev. Stat. 1987). A policy of blindfolding may thus be a policy favoring blinders that are insufficient to the task.

Worse yet, a "knowledgeable" wild-card juror actually may be misinformed about the precise meaning of the policy of trebling. Judge William Schwarzer (1990) provides a compelling example of wild-card mischief in an antitrust case in which the jury was not instructed about treble damages. After trial, a juror told the judge that he had heard from his daughter, a law student, that the damages would be *quadrupled* by the court. As Judge Schwarzer (ibid., p. 134) suggests, "Surely in that case it would have been better to tell the jury the whole truth."

D. Effects of the Expert Testimony

To test the effects of statistical versus concrete expert testimony, two videotaped versions of the trial were prepared. In both versions, one expert testified for the plaintiff and a second expert testified for the defense. In version 1, the plaintiff's expert presented a statistical model and the defendants' expert presented a concrete model of the damages produced by the price-fixing agreement; in version 2, the plaintiff's expert presented a concrete model and the defendants' expert presented a statistical model.[30] The actor who presented the concrete model did so in both versions; the same was true for the actor who presented the statistical model.

Statistical models are in general more complicated and harder to understand than the more homely and concrete yardstick models. The concrete yardstick model in this case was based on the actual experience of another road-building company operating in a different state and thus not a victim of the price-fixing agreement that allegedly injured the plaintiff. The owner of the other company testified about his experience during the time of the price-fixing agreement.[31] The primary issue about the yardstick model was whether the plaintiff's company and the yardstick company and their marketing environments were similar enough to draw the inference that their prices,

[30] When the statistical expert testified for the plaintiff, his model attributed $490,000 in damages to the price-fixing agreement; when he testified for the defendants, the same model allegedly revealed $35,000 in damages. In each version, the statistical expert's damage estimate was disputed by the expert presenting the concrete model who offered the lower damage figure ($35,000 when the statistical expert said it was $490,000) or the higher damage figure ($490,000 when the statistical expert said it was $35,000).

[31] When he appeared for the plaintiff, he testified that his costs for crushed rock had not risen during this period; when he appeared for the defendants, he testified that he had experienced a price increase similar to that experienced by the plaintiff.

which had been similar in the past, would have been similar during the conspiracy period if not for the price-fixing agreement. In contrast, the statistical expert built a model of the plaintiff's past price performance and, based on that earlier performance, projected what prices would have been in the absence of the price-fixing agreement. The adequacy of the statistical model as a basis for projecting prices hinged in large measure on the completeness of the model in including and properly measuring all relevant variables.

If statistical evidence is more persuasive than concrete evidence, as Tribe (1971) and others have claimed, damage awards should have been higher when the plaintiff's expert put forward a statistical model than when the plaintiff's expert presented a concrete model of damages (see Fig. 2, *A*). Alternatively, if concrete evidence is more persuasive than statistical testimony, as studies comparing the persuasiveness of concrete case studies versus abstract statistical arguments (e.g., Ginosar & Trope 1980) would predict, damage awards should have been higher when the plaintiff's expert put forward a concrete model than when the plaintiff's expert presented a statistical model (see Fig. 2, *B*).

The jurors did give somewhat higher awards when the statistical expert testified for the plaintiff than when the expert presenting the concrete model did so ($216,515 versus $200,813),[32] but the difference was not statistically significant ($F_{(1, 1007)}=1.86$, $p>.15$).[33] To assess juror reaction to the experts' testimony, we looked at the way the jurors evaluated the two experts. The comparisons indicate that the jurors reacted quite differently to the statistical and concrete models, and that those different reactions cut against each other, so that, on balance, the experts exerted substantially equivalent influence on the jurors' awards.

We compared the jurors' ratings of the two experts on four dimensions: persuasiveness, expertise, clarity, and trustworthiness,[34] using a repeated measures analysis of variance because

[32] There was no evidence of an interaction between the expert and instruction conditions ($F<1$).

[33] There is some additional evidence that the statistical expert was slightly more convincing. When the juror awards are categorized according to whether they were in the range claimed in the statistical model, in that claimed in the concrete model, or somewhere in between, the results suggest a slight advantage for the party presenting a statistical model (i.e., 38% of the awards in the range of the statistical model vs. 32% in that of the concrete model). Moreover, when jurors were asked to indicate how they computed their verdicts, they cited the statistical model 18% of the time and the concrete yardstick model 14% of the time. (The majority said they used both models or selected an amount and adjusted it because it seemed too high or too low.)

[34] Persuasiveness was computed by summing seven-point items rating how believable and convincing the expert was; expertise by summing items rating how well informed, knowledgeable, and competent he was; clarity by summing how clear, easy to understand, and simple he was; and trustworthiness by summing three five-point items measuring (1) disagreement that he twisted the evidence to suit his own purpose, (2)

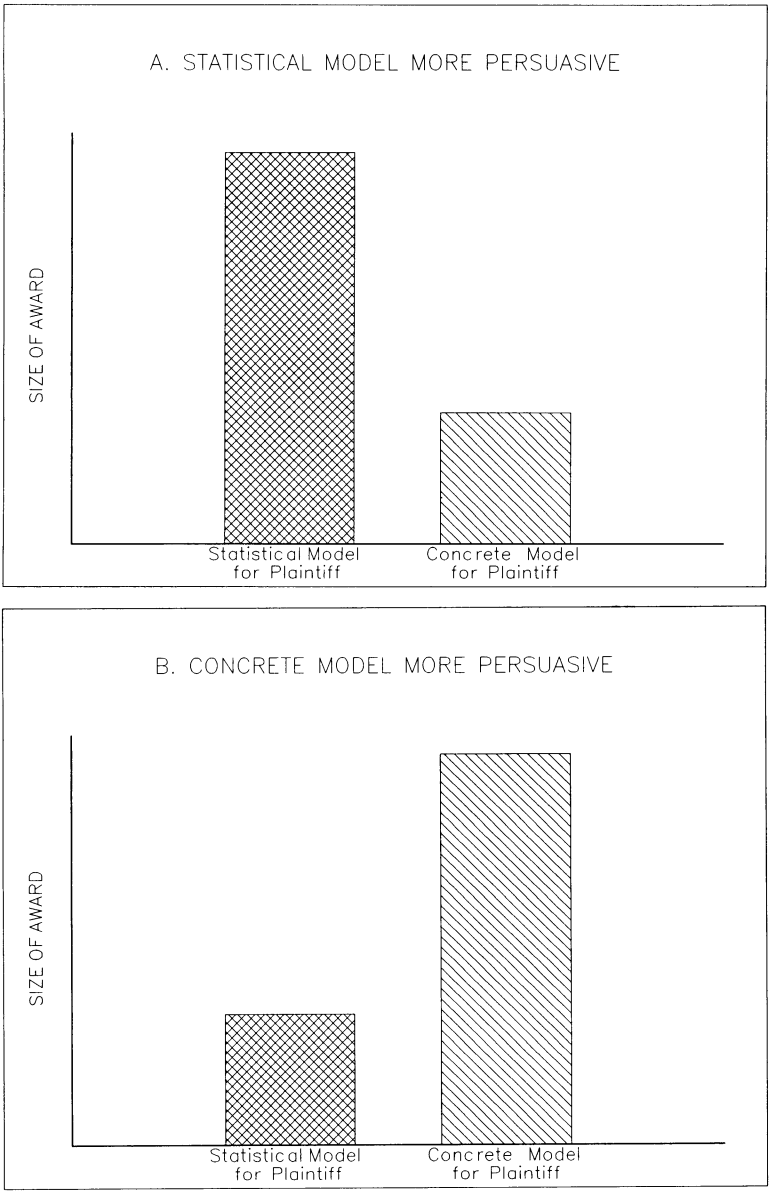

Figure 2. Hypothetical effects of expert damage models on juror awards

each juror rated both experts on each dimension. Thus, the within-subjects variable (EXPERT) represented each juror's two ratings of the two experts. The between-subjects variable (VERSION) indicated whether the statistical or the concrete model

agreement that he seemed eager to explain in a fair and balanced way, and (3) agreement that he was someone who could be trusted in other matters. The Cronbach's Alphas for the scales, computed separately for each expert, were for the statistical expert, .68, .76, .77, and .70; for the expert presenting the concrete model, .72, .74, .65, and .70.

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was presented first, that is, for the plaintiff. The results of the repeated measures analysis of variance show that the jurors perceived the experts differently. The mean differences are displayed in Table 2. While the statistical expert was not rated as significantly different on persuasiveness or trustworthiness, he was seen as more expert and less clear than the expert who presented the concrete yardstick model ($p < .001$). This pattern of differences and similarities held in both versions of the trial, that is, whether the statistical expert or the expert presenting the concrete model testified for the plaintiff.[35]

Table 2. Juror Evaluations of Experts Presenting Statistical and Concrete Models

Expert Characteristic	Expert Presenting Statistical Model	Expert Presenting Concrete Model	Difference	F	p
Persuasiveness ($N=558$)[a]	4.70[b]	4.53	0.17	1.71	.19
Testifies for plaintiff	4.97	4.89			
Testifies for defendants	4.39	4.22			
Expertise ($N=562$)	5.26	4.84	0.42	39.49	.001
Testifies for plaintiff	5.44	5.06			
Testifies for defendants	5.06	4.65			
Clarity ($N=568$)	3.69	4.89	−1.20	208.89	.001
Testifies for plaintiff	3.96	5.18			
Testifies for defendants	3.38	4.63			
Trustworthiness ($N=574$)	3.09	3.14	−0.05	1.91	.17
Testifies for plaintiff	3.27	3.36			
Testifies for defendants	2.90	2.94			

NOTE: Significant order effects were indicated by an interaction between VERSION (Statistical Model for Plaintiff-Concrete Model for Defendants vs. Concrete Model for Plaintiff-Statistical Model for Defendants) and EXPERT (Presents Statistical vs. Presents Concrete Model); for all four expert characteristics, the order effect was significant at $p < .001$.

[a] N indicates nondeliberators who rated all items composing the scale.

[b] Higher scores indicate more favorable evaluations of the expert.

These differences in jurors' perceptions on expertise and clarity provide some insight as to why the statistical and concrete damage models had relatively comparable effects on the jurors' damage awards. It appears that while the statistical expert's greater perceived expertise made him *more* convincing,

[35] Order, too, affected jurors' ratings. When a witness testified for the plaintiff, he was rated more persuasive, more expert, clearer, and more trustworthy than when he testified for the defendants ($p < .001$ in each case). This order effect is consistent with the primary effects typically observed in studies of person perception (e.g., Anderson & Jacobson 1965). More recent research suggests that early information frames the issues and modifies the way the receiver of a message processes later information. The adversary structure of trial proceedings does alert jurors to expect later disconfirming evidence and thus partially to reserve judgment (Lind et al. 1976), but the strong order effect observed here suggests that early influence by the plaintiff's witness may be difficult to counteract.

the lower clarity of his model made him *less* convincing. The net result was that the statistical and concrete models were equally persuasive, because both perceived expertise and perceived clarity are associated with expert influence. As the regression equations in Table 3 reveal, trustworthiness, expertise, and clarity all were significant predictors of the persuasiveness of each expert.

Table 3. Determinants of Expert Persuasiveness

Expert Rating	Expert Presenting Statistical Model Beta	Expert Presenting Concrete Model Beta
Expertise	.363*	.412*
Trustworthiness	.328*	.386*
Clarity	.278*	.115*
Variance accounted for (R^2)	.537	.571
N of cases	548	553

*$p < .001$

The role of lack of clarity in reducing the influence of the statistical model on juror awards can be seen most clearly by examining the awards in cases in which the experts presenting the statistical and concrete models were perceived as roughly equivalent in clarity. When the two experts were rated as less than two points apart on the seven-point clarity scale, the mean juror award when the statistical expert testified for the plaintiff was \$220,517 ($N=201$), but when the expert presenting the concrete model testified for the plaintiff, the mean award dropped to \$168,223 ($N=139$). Thus, with clarity controlled, the statistical expert exerted greater influence on awards ($t=-2.64$, $p<.01$). He was also seen as higher in persuasiveness ($t=-8.02$, $p<.001$).

In raising questions about the limits of juror competence, some have suggested that jurors will be overpowered by complicated testimony that they do not understand; sometimes this argument is pressed to its extreme, suggesting that the less jurors understand, the more they may be influenced by the magic of statistics. The analyses presented thus far suggest that lack of clarity, that is, perceived complexity and difficulty, discourages the jurors from accepting an expert's position rather than inducing them to accept it. Another way to test the extent to which the jurors naively adopt the position of experts they do not understand is to look at the relationship between their comprehension of expert testimony and their evaluation of the expert's persuasiveness. If jurors are being persuaded to adopt positions because they are impressed by what they do not understand, we should see a negative correlation between comprehension of the expert's testimony and the evaluation of the

expert. In fact, the correlation between juror comprehension of the statistical expert and his judged persuasiveness was positive ($r = .12$, $n = 564$, $p < .01$) as was the parallel correlation for the expert presenting the concrete model ($r = .09$, $n = 569$, $p < .05$). Thus, there is no evidence that lack of understanding is associated with greater expert persuasiveness.

E. From Juror to Jury Awards

Kalven and Zeisel (1966) suggested that the role of individual preferences of jury members in the process of deliberation is akin to a photographic negative waiting to be developed. Although the appearance of the final picture may be different from the negative, its critical elements and much of its form are already established before deliberations commence. This metaphor suggests that the transformation process is relatively straightforward and that the decisions of the individual members of the jury prior to deliberation are the critical elements in the ultimate jury verdict. Most of the work on this individual to group transformation process has explored criminal cases (e.g., Kalven & Zeisel 1966; Hastie et al. 1983). The antitrust case discussed here presents one of the first opportunities to explore this issue in the context of civil damage awards. Moreover, in criminal cases, the individual to jury transformation process has often been found to produce consistent changes in the direction of leniency toward the defendant (see MacCoun & Kerr 1988). Here we examine such asymmetric effects in the civil context and find a systematic inflation of award size as a result of jury deliberations.

We examine several aspects of the process by which juror predeliberation preferences are converted through group interaction into a jury verdict.[36] The first involves the ability of various attributes of the initial distribution of preferences to predict the ultimate jury award. The second focuses on the role of the foreperson in the deliberation process, first describing the process of foreperson selection and then discussing the conditions under which those who occupy this role are likely to exert particular influence on the jury's verdict. The third deals with a general inflation effect by which the group verdict appears to be systematically increased by group interaction.

[36] In this report, we examine data from the questionnaires filled out by all 417 deliberators and a content analysis of 60 jury deliberations. The verbatim transcripts of these juries were content analyzed using computer-based text analysis software. In addition, for the analysis of foreperson selection and proposals of awards in the juries discussed in this section, we performed a similar and parallel content analysis of portions of videotaped deliberations of the remaining 10 juries for which a transcript had not been prepared. Thus, the content analysis of deliberations is based on 60 juries, except for the discussions of foreperson selection and award proposals, which are based on all 70 juries.

Some Group Predictors of Jury Verdict

If we look at the verdict preferences of the jurors before they begin deliberations, can we observe patterns—as Kalven and Zeisel suggested we would—that predict the final award of the jury? We examined several potential predictors of final award, employing data from the 69 juries that deliberated to verdict in the antitrust case, excluding the one jury that was unable to resolve its differences.

In Table 4 we display the predictors we examined. The top half of the table (panel A) focuses on individual award preferences. If the first award proposal offered during deliberations acted as an anchor, providing a reference point for subsequent proposals, we would expect it to be particularly good predictor of the eventual group award. The highest and lowest awards might also have special influence: by framing the range of acceptable awards, they might affect subsequent proposals or an eventual compromise.

Table 4. Predictors of the Transformation of Individual Juror Predeliberation Awards into Jury Verdicts

Predictor	Correlation with Jury Verdict[a]	*N*
A. *Individual award amounts*		
All jurors' predeliberation awards	.26**	(404)
First award proposed during deliberations	.20*	(56)
Highest individual predeliberation award	.25*	
Lowest individual preliberation award	.20*	
B. *Measures of average individual awards*		
Mean predeliberation award	.54***	
Median predeliberation award	.62***	
Mode predeliberation award	.48**	(34)

[a] $N=69$ unless otherwise indicated.

* $p<.05$ ** $p<.01$ *** $p<.001$

Panel B of the table focuses on the configuration of the initial preferences of the members prior to deliberation rather than on particular individual awards. It reveals the impact of various measures of the initial central tendency (the mean, the median, and mode) of the group on final award decisions.

The measures of individual verdicts produce rather modest effects. Taking the correlation between all predeliberation verdict preferences and the final jury awards as a baseline, neither the first proposed award during deliberations nor the highest or lowest predeliberation preferences improve our ability to predict the jury's final verdict.

The measures of central tendency are better predictors of final award. Although these measures do not significantly differ

from one another, the median is the best single predictor of the jury's final verdict, with mean and mode nearly as strong.[37] Thus, the central tendency of the jury's members prior to deliberation appears to drive the group's decision more than any distinctive individual vote. Moreover, the contributions of the potential anchors examined above appear to make no independent contributions to predicting the jury award apart from their influence on the group's central tendency: when we take the strongest group predictor—the predeliberation median—and first regress jury award on this variable and then add any of the individual verdict variables (first proposed award; highest and lowest predeliberation preferences), none makes a significant independent contribution to our ability to predict the final award.

The importance of the group's initial central tendency as a predictor of its ultimate award might be taken as suggesting a notion sometimes put forth by those critical of civil juries, that juries actually engage in some form of very crude averaging. To examine this possibility we analyzed the 60 verbatim transcripts of deliberating juries. The text analysis software employed enabled us to search for words that might indicate that the jury actually averaged individual awards at some point in deliberation (we identified and examined all references to "average," "averaging," "middle," "split," "splitting," and "compromise"). Although these words appeared often (562 times in 56 of the juries), the reference usually referred to other aspects of the testimony and not to combining individual juror award preferences. The idea of averaging individual awards to produce a verdict was proposed in only 8 of the 60 juries; in one the idea was rejected, and in the remaining 7 an actual average of individual award preferences was computed.[38] In only 4 of these juries was the eventual award the same as the average that had been computed. Even the 7 juries that did compute an average at some point did not fit a crude image in which the jury simply got together and quickly exchanged preferences, averaged, and then accepted the result as their verdict. Rather all debated on matters of substance for a substantial time before actually computing an average. Indeed, the average deliberation time in the 4 juries in which the computed average became the jury award did not differ from the average deliberation time for those that did not engage in averaging at all. This pattern indicates that even in the few instances in which it was

[37] In her study of damage awards in an automobile accident case, Sonaike (1978) also reports that the median of individual juror predeliberation awards is a better predictor of the juries' damage awards than the mean. This result was obtained by assigning a value of 0 to verdicts for the defendant.

[38] When averaging actually occurred, six juries computed the arithmetic mean of the set of six expressed verdict preferences, while one jury computed the average of the four remaining preferences after the highest and lowest had been discarded.

clear that averaging did have an influence on jury awards it was not employed as a shortcut that avoided consideration of the evidence and the judge's instructions. Thus, the ability of measures of central tendency in initial preferences to predict jury award occurs not because the juries engaged in crude averaging of their preferences but because the *deliberation process* itself is more strongly influenced by those in the middle rather than by outliers.

We have focused thus far in this section on the jurors as a group. The foreperson, however, occupies a leadership role and as a result has significant opportunity to play an especially influential role in the damage-setting process.

The Foreperson

We can see a first indication of the foreperson's influence in the substantial correlation between the foreperson's predeliberation verdict and the jury's award ($r = .44$, $p < .001$).[39] It is significantly greater than the correlation between the predeliberation verdicts of each of the other jury members and their respective jury awards ($r = .22$, $p < .001$; $z_{diff} = 1.82$, $p < .05$, one-tailed).

Before concluding that the foreperson influences the jury's verdict, however, we must consider an alternative source for the foreperson's higher predeliberation award–jury verdict correlation. The correlation might also be high if the foreperson is chosen *because* his or her preferred verdict is particularly representative of the group's view prior to deliberation. The latter explanation assumes that the jury has at least some sense of the foreperson's preferred verdict before selection occurs and then chooses the foreperson based on that information. While it is possible in principle for jurors to share such information before deliberation, jurors are usually admonished not to discuss the case until deliberations begin. Moreover, the jurors in our study, as in other research (e.g., Strodtbeck et al. 1957; Ellsworth 1989), selected the foreperson quickly, nearly always completing selection before the group turned to a discussion of the case. Almost two-thirds of our juries chose a foreperson within 10 statements of the beginning of deliberations, and nearly 90% chose within 20 statements.[40] Nonetheless, jurors do have the opportunity to learn something about one another before deliberations begin and may indirectly signal their reactions to the evidence without explicitly discussing the case. Therefore, we examined the predeliberation verdicts of the

[39] This correlation is based on 67 juries. The foreperson did not indicate a predeliberation verdict on two of the juries. The remaining jury ended in deadlock.

[40] A new statement began when one juror stopped speaking or was interrupted by another juror. The average jury deliberation contained 363 statements.

forepersons versus those of other jurors to see whether the foreperson reflected the sense of the group more accurately than other jurors did.

We found no evidence that the forepersons were particularly good reflections of the jury's position before deliberations began. The correlation between the predeliberation awards of the forepersons and the mean and median predeliberation awards of the juries they came to lead were similar to the correlations for other jurors.[41] The correlation of the foreperson's verdict with the jury's mean preliberation award was .42 ($n=68$, $p<.001$), and their correlation with the median was .43 ($n=68$, $p<.001$). But the correlations for the forepersons were no different from the correlations for other members of the jury, indicating that forepersons were no better representatives of the central tendency of their jury than were any other jurors.

Thus, the foreperson's apparent influence is more than simply representational. Several pieces of evidence suggest a more active leadership role for the foreperson. By the end of deliberations, forepersons were viewed by their fellow jurors as more influential than other jurors. After the juries completed their deliberations, the final question on their postdeliberation questionnaire asked each juror to rate the influence of each member of the jury.[42] Forepersons received an average rating of 5.49, while other jurors averaged 4.66 ($t=7.24$, df$=129$, $p<.001$).[43] This difference in ratings reflects juror perceptions and is only one of several possible proxies for actual influence. However, there is some indication that the ratings difference reflects more than the higher activity level of the foreperson. Although forepersons talked substantially more than other jurors ($M=1770.22$ words vs. $M=789.35$ words, $t=5.29$, df$=67$, $p<.001$),[44] they were seen as more influential than their fellow jurors, even when we controlled for the number of words spoken. Number of words spoken explained 14% of the variation in the average influence rating a juror received ($p<.001$). Whether the juror was foreperson accounted for an additional 3% ($p<.01$).

[41] Thus, e.g., for jurors who occupied seat 1 at the jury table, the correlation of their predeliberation award with their jury's mean predeliberation award was .57 ($n=56$, $p<.001$) and it was .49 ($n=56$, $p<.001$) with their jury's median predeliberation award. The corresponding correlations with the mean awards for jurors who occupied the other five seat positions varied from .40 to .56; the correlations with the median awards varied from .37 to .57.

[42] Jurors rated each member of their jury on a 7-point scale on which 1=not very influential and 7=very influential.

[43] The degrees of freedom are adjusted because the variance of ratings for the nonforepersons was significantly greater than the variance of the ratings for the forepersons ($F=1.90$, $p<.01$).

[44] The degrees of freedom are adjusted because the variance in the number of words used by the forepersons was significantly greater than the variance in word usage by the nonforepersons ($F=3.02$, $p<.001$).

We also have evidence that the foreperson's predeliberation vote independently contributed to the verdict over and above its contribution to the initial sense of the jury as captured in its mean or median predeliberation award. Thus, while the mean predeliberation award accounted for 27% of the variance in jury awards, the foreperson's award significantly increased the explained variance ($p < .05$), bringing the total explained variation to 32%. And while the median award accounted for 36%, the foreperson's award brought the explained variance to 39% ($p = .056$).

We have shown that forepersons appear to exert actual influence on other jurors in the course of the deliberation process rather than merely representing the predeliberation preferences of the jury. To identify attributes that may help to explain the foreperson's influence over the jury, we turn now to a consideration of the characteristics of forepersons and the process by which they are selected.

We coded the jury deliberations for jurors' mentions of attributes they thought should be considered in choosing their foreperson. The attributes that were mentioned (Table 5), emerged primarily in the context of discussing the qualifications of particular individuals, rather than being mentioned as abstract qualifications that forepersons ought to possess. Thus, among the three most commonly mentioned attributes were (1) occupying the seat at the head of the table,[45] and (2) the occupation, education, or expertise of a particular member of the group. The third often-mentioned attribute, prior jury service, was the one factor that jurors did discuss in the abstract (e.g., by asking if anyone had been on a jury before and suggesting

Table 5. Attributes Discussed during Selection of the Foreperson

	N of Juries Mentioning	
At head of table	25	
Previous jury service	12	
As juror		9
As jury foreperson		3
Occupation/education/expertise	12	
Gender	4	
Age	5	
Talks a lot/good talker	4	
Took notes/has paper and pen	4	
Other	11	

NOTE: Based on the deliberations of 67 juries; 3 juries began discussions before the tape was started.

[45] The jury table was rectangular, with three chairs arranged on one side, two on the other, and one at one end. The seat position of the juror on the end was thus distinctive.

that such experience might be useful for the foreperson). Of course, other attributes not explicitly discussed also might have affected the choice of foreperson. For example, although gender was mentioned only a few times, it still could have influenced who volunteered to serve or who was nominated.[46]

To examine the effect of both implicit and explicit criteria used in foreperson selection, we tested the predictive power of various juror attributes on the choice of foreperson.[47] As Table 6 shows, four of the attributes tested were significant. Two of them, position at the table and occupation, were attributes specifically mentioned during deliberations. Jurors at the head of the table and those who had professional or managerial occupations were more likely to become forepersons. Other researchers have also identified these attributes as significant predictors in choosing a foreperson (e.g., Strodtbeck & Lipinski 1985).

Table 6. Logistic Regression Coefficients for Selecting as Foreperson

	Regression Coefficient
Seated at head of table	1.786***
	(.344)
Had taken a statistics course	1.111**
	(.340)
Spoke first	.895*
	(.359)
Professional or managerial occupation	.801*
	(.340)
Prior juror	−.430
	(.438)
Male	.321
	(.312)
χ^2	64.71
Degrees of freedom	6
N of cases	390

*$p < .05$ **$p < .01$ ***$p < .001$

A third predictor, experience with statistics, relates to a type of juror expertise that was likely to be particularly valuable in determining damages in this antitrust case. Jurors who indicated on the pretrial questionnaire that they had taken a statistics course were significantly overrepresented among forepersons. Finally, one way a juror can subtly express interest or

[46] Nearly all of the forepersons (96%) were nominated rather than volunteering to serve.

[47] Correlations among these 6 predictor variables were less than .20 in all cases with the exception of a .41 correlation between occupation and having taken a statistics course. Educational level was not included as a predictor because it correlated .54 with occupation and .60 with statistics experience.

ability to serve as foreperson is to open the conversation, either with the bailiff before deliberations formally begin or with the jury at the start of deliberations. We recorded the identity of the first juror who spoke on each jury, and found that jurors who spoke first were significantly more likely to be chosen as forepersons than jurors who did not. Unlike earlier researchers, we did not find gender[48] and prior jury experience[49] to be significant predictors of foreperson selection.[50]

We have now identified a number of factors that are associated with the choice of foreperson, and we have seen that the foreperson has an influence on the jury's award beyond the influence exerted by other jurors. But does the foreperson's enhanced influence stem from personal characteristics, the same qualities that led this individual to be selected by the other jury members? Or can the foreperson's enhanced influence be explained only by the deference of other jurors to whoever becomes foreperson or by the opportunities to control deliberations that accrue to the person in the foreperson's position? One possibility is that the foreperson is influential because the same characteristics that led to the foreperson's selection tend to make *any* juror more influential, whether or not that juror becomes foreperson. That is, all jurors who sit at the head of the table may have enhanced influence, and forepersons may appear more influential simply because they are more likely to be selected from the set of jurors who sit at the head of the table. In the comparisons shown in Table 7, we use the correlation between jurors' predeliberation verdicts and their jury awards as a measure of juror influence.[51] By comparing

[48] Strodtbeck et al. (1957) found that men, at least in the 1950s, were far more likely to become forepersons than women.

[49] Strodtbeck and Lipinski (1985) as well as Kerr et al. (1982) found that jurors with previous jury experience were more likely than inexperienced jurors to become forepersons, while Ellsworth (1989) did not find that prior jury experience increased the probability of becoming a foreperson. In the jurisdictions studied by Strodtbeck and Kerr, jury service lasted several weeks, giving jurors the opportunity to serve on multiple juries. As a result, jury experience was common (34% and 42% of their jurors had served on juries previously), recent, and salient. In contrast, under the one day/one trial system of jury service currently in use in Cook County, only 16% of our jurors reported that they had been jurors previously, and because by law none of them would have been eligible to serve if they had served within the past year, the potential salience of that experience was probably reduced.

[50] Because 21 of the juries did not include any jurors who indicated on their questionnaire that they had prior jury experience, we repeated the logistic regression reported in Table 6 excluding these juries. The same four factors were significant, and gender and prior jury experience remained not significant.

[51] Assessing the influence of jurors and their attributes on the jury verdict is complicated by the fact that many attributes (e.g., prior jury service, sex, position at the table, prior statistical training) make no directional prediction about the size of the award that might be preferred by those with one or another attribute. Thus, in this analysis we examine the correlations between the individual predeliberation awards and jury verdicts for sets of individuals possessing various attributes. These correlations permit us to evaluate whether influence was exerted differently by jurors with and without these attributes.

Table 7. Correlations between Individual Predeliberation Verdicts and Jury Awards for Jurors with Characteristics Associated with Leadership

	Forepersons		Not Forepersons	
All jurors	.44**	(67)	.22**	(337)
Position at table				
At head of table	.43*	(26)	.20	(40)
Not at head of table	.46**	(41)	.23**	(295)
Spoke first or later				
Spoke first	.46*	(17)	.20	(46)
Spoke later	.40**	(46)	.22**	(272)
Occupation				
Professional or manager	.40*	(43)	.31**	(119)
Not professional or manager	.49*	(24)	.18*	(218)
Taken statistics course				
Had course	.63**	(42)	.25*	(78)
Did not have course	.08	(25)	.20*	(238)

*$p < .01$ **$p < .001$

forepersons and nonforepersons who do and do not possess these characteristics, we can test whether any of these attributes can account for the influence of the foreperson. If the foreperson appears influential simply because, for example, he or she is more likely to sit at the head of the table, then we would expect (1) that the correlation between the foreperson's predeliberation verdict and the jury's award would be reduced when the foreperson selected was *not* seated at the head of the table, and (2) that the predeliberation verdicts of nonforepersons seated at the head of the table would correlate more highly with their jury verdicts than would the verdicts of nonforepersons not seated at the head of the table. The results in Table 7 indicate that neither speaking first nor being at the head of the table modified the apparent influence of the foreperson. Nor did those characteristics affect the influence of nonforepersons. Similarly, forepersons who had professional or managerial occupations were no more influential than those who did not, and although the correlation for nonforepersons with a professional or managerial background was somewhat higher than for those in other occupations, the difference was not significant ($z = 1.22$, $p > .10$). These results indicate that while forepersons were more likely to speak first, sit at the head of the table, and have a professional or managerial occupation, these attributes do not explain the foreperson's greater influence on the jury's verdict.

The pattern associated with having a statistics course tells a very different story. Having a statistics course significantly increased the influence of forepersons ($z = 2.48$, $p < .05$). It had no apparent effect on the influence of the nonforepersons. The power of this indicator of expertise was apparently so crucial

for the foreperson that if the foreperson *lacked* that experience, the foreperson was no more influential than any other juror. Task-relevant expertise thus increased the likelihood that a person would be selected as foreperson, and when such a person was chosen, the foreperson appeared to exert a substantial impact on the jury's outcome.[52]

Forty-three of the 70 juries faced with a case involving antitrust damages selected a foreperson with relevant expertise, as measured by some statistics training.[53] The probability of being selected foreperson for jurors who said they had taken a statistics course was .30 ($n=145$), while the probability for jurors without such a course was .10 ($n=270$; $\chi^2_{(1\ df)}=24.61$, $p<.001$).[54] These results may suggest that jurors are able to select leaders who will help them reach rational decisions, but it is also possible that they are simply willing to choose a leader and be influenced by the advice of other jurors who have apparent expertise even when that expertise is not associated with a more reasoned approach to the evidence and the law. We expect to learn more from future analyses of the deliberations about how this statistical experience actually was used during deliberations.

Inflation Effects of the Deliberation Process

A clear inflation of damage awards occurred between the individual and the group level. On average the juries produced awards about $56,000 (or 26%) higher than the average of their members prior to deliberation.[55] Thus, the deliberation process produced a substantial inflation of the award size, and its effect occurred across all conditions and both orders of expert testimony.[56]

[52] A somewhat similar pattern of correlations was obtained comparing jurors who had at least a bachelor's degree with those who had less education: for forepersons with a B.A., $r=.56$ ($n=43$, $p<.001$); for forepersons without a B.A., $r=.16$ ($n=24$, not signif.); for nonforepersons with a B.A., $r=.24$ ($n=123$, $p<.01$); and for nonforepersons without a B.A., $r=.20$ ($n=212$, $p<.01$).

[53] Thus, over 60% of the juries had forepersons who said on their questionnaires that they had taken a statistics course. Four of the juries had no member with such a background; 65% of the juries which included a member with a statistics course chose one as their foreperson.

[54] Note that 35% of the jurors claimed to have taken a statistics course. In view of this unexpectedly high figure, we suspect that jurors were including other mathematics classes in this account.

[55] The mean juror predeliberation award is the average of the mean individual verdicts obtained for each of the 69 sets of jurors prior to their deliberation, excluding the jury that hung. The mean jury award consists of the average of the awards made by the 69 juries after deliberation. The mean juror predeliberation award was $213,440; the mean jury award was $269,444 ($t=4.42$, df=68, $p<.001$).

[56] Similar effects have appeared in other studies of damage awards as well, including Zuehl (1982), Wasserman & Robinson (1980), and Kaplan & Miller (1987), in which an inflation effect was observed for punitive but not compensatory awards. Hastie et al. (1983) report an analogous inflation effect in the criminal context—by which

One possible explanation for this effect might come from the literature on the "risky shift" or group polarization (Stoner 1968; Lamm & Myers 1978). This process, well established in the group decisionmaking literature, involves situations in which the initial tendency of group members' preferences become exaggerated in the ultimate group decision (e.g., members of a group composed of moderate opponents of abortion might, after discussion, become much more intense in their views). The effects of group polarization have been attributed to a variety of processes, including the operation of persuasion within the group and an interaction between individual impulses to be close to the group mean and the updating effects of information obtained from learning others' positions during discussion. (For a recent review of this literature, see Isenberg 1986.) Such a group polarization process would seem on theoretical grounds to be a fertile source for explaining the inflation process we are observing. For example, if a subgroup of jurors began the deliberation with awards consistently higher or lower than those of their peers, the jury deliberation might pull the ultimate award toward the preferred verdict of the subgroup, moving it beyond the mean of the individual jurors' initial preferences. If juries tended to begin deliberation with subgroups of jurors more supportive of the plaintiff's than the defendants' case, then this group polarization might tend to produce systematic inflation across juries.

Our analysis indicates that this explanation does not account for the inflation process we are observing. We have classified the 69 juries reaching a verdict on the basis of the configuration of initial preferences, identifying those juries having subgroups of at least three with initial awards in the plaintiff or defense ranges. Of the 47 juries with initial configurations favoring one of the parties, three-quarters initially favored the *defendants* rather than the plaintiff. Thus, a group polarization effect should have produced a deflation effect. We also examined the relationship between the mean of each jury's individual predeliberation awards and its ultimate award. The juries do not appear to be systematically influenced by their initial polarization. Although juries with an initial configuration favoring the plaintiff's range are more likely to produce a verdict above their initial mean (as opposed to below it or at the same level), those that begin with a predisposition toward the defendants' range are twice as likely to end up above their

deliberating jurors tended to move toward a more serious conviction charge than their initial predeliberation verdict preference. The Hastie model predicts this inflation effect on the basis of the distribution of faction sizes favoring various verdict alternatives. Its applicability to a civil context like ours is limited by the lack of clear definitions of "factions" when initial damage award positions have such a large range compared to the smaller number of verdict categories available to jurors in the criminal context.

mean predeliberation award as they are to produce a verdict at or below their mean.[57] Thus, even juries predisposed in favor of the defendants' damage estimate tended to move toward the plaintiff's damage request, which is inconsistent with the shifts predicted by the group polarization literature.

Three other aspects of the deliberation process *do* appear to provide some insights into the inflation effect. First, deliberation appears to increase the jurors' sense that the defendants' behavior was blameworthy. The 12-item defendant blameworthiness scale used in the analysis of the framing effects of mentioning punishment and deterrence contained five items that were asked of both nondeliberators and deliberators. These five items were used to form a second defendant blameworthiness scale.[58] Because jurors were randomly assigned to be deliberators or nondeliberators, and because deliberators completed the questionnaires after deliberation, we were able to compare the two groups on this index to test whether deliberation itself affected assessments of the defendants' conduct. When we controlled for the effects of condition on blameworthiness assessments, we found that deliberators scored significantly higher on the scale of defendant blameworthiness than jurors who did not engage in deliberation ($F_{1,993}=20.2$, $p<.001$).[59] Assessments of defendant blameworthiness were associated with higher damage awards at the individual level for both nondeliberators and deliberators,[60] and the mean score on the scale within juries was related to the jury award.[61] Thus, deliberation appears to increase a sense of blameworthiness, and this attitude tends to increase verdict size. This effect for deliberation may occur in part because many jurors are unfamiliar with the issues in antitrust cases. Thus, they may begin

[57] Note that regression toward the mean cannot explain these results. Two-thirds of the juries reached verdicts that were higher than their average predeliberation verdict. This increase occurred as often for juries that began with an average above their mean as for juries that began below their mean.

[58] The items asked respondents to evaluate the defendants' behavior on such dimensions as whether they intended to harm the plaintiff, how much harm they did to the plaintiff, whether their act was carefully planned, whether price fixing ought to be a criminal offense, and whether the plaintiff was justified in taking the matter to court. These five items each had a seven-point response scale (e.g., Did Rocky Mountain Crushed Rock and Western Rock Supply intend to harm Granite Road Company? 1=definitely did; 7=definitely did not) and formed a satisfactory scale (Cronbach's Alpha=.63).

[59] The analysis of variance included two factors: (1) whether the juror was assigned to instruction condition 3 or 4, in which the instructions mentioned punishment and deterrence or was assigned to another instruction condition; (2) whether they were deliberators or nondeliberators. The first factor was also significant ($F=7.79$; df=1,990; $p<.005$). No interaction was obtained.

[60] The Pearson correlations between the blameworthiness scale and predeliberation verdicts for nondeliberators and deliberators were .39 ($p<.001$; $n=582$) and .34 ($p<.001$; $n=400$), respectively.

[61] The Pearson correlation between the jury award and mean score for the jury as a whole on the blameworthiness scale was .51 ($p<.001$; $n=69$).

their jury experience with only a tentative sense of how serious such violations are. If deliberation exposes such individuals to opinions which suggest that such activities are blameworthy, it may promote the inflation effect observed. And, indeed, the deliberations contain numerous references to the inappropriateness of the defendants' price-fixing behavior. Moreover, in the civil context we are examining, the jurors are not exposed, as they are in a criminal case, to extensive instructions stressing that they should err on the side of the defendant if they are uncertain, so the leniency effect observed in criminal cases is given no justification in this context. (On the effects of standards of proof on individual-to-group transformation processes, see Kaplan & Miller 1987; MacCoun & Kerr 1988.)

A second factor contributing to the inflation effect emerges from a content analysis of the jury deliberation transcripts. For each jury, we coded the first four proposed awards suggested by jurors.[62] Jurors who came to the deliberation with preferences for relatively low verdicts were substantially less likely to mention their proposed awards than those who had higher damage award preferences when they began deliberating. This pattern is most clearly suggested by comparing the mean and median of the predeliberation preferences of each jury with the mean and median of the first four proposals made in that deliberation. The mean of the first four awards proposed during deliberation was $25,000 higher than the predeliberation mean and $45,000 higher than its median.[63] This suggests a process by which inflation of jury awards might have been enhanced. In many juries, the "central tendency" actually displayed by the jurors was somewhat higher than the group's predeliberation preferences. Thus, the first four proposals provided a frame for further discussion and anchor points that were higher than the initial preferences brought to the deliberation.

[62] If a juror suggested more than one award before three other jurors had made award proposals, we coded only the juror's first proposal. Two coders were trained to identify proposals of award amounts in the transcribed deliberations of 60 juries. In addition, the coders used the same conventions to analyze the videotapes of the remaining 10 juries whose deliberations were not transcribed. Thus, the data on award proposals reported here are based on all 70 juries. The coders made independent judgments and then met and discussed coding decisions with the two authors. After discussion, a consensus decision was made about the appropriate coding for each jury. The following conventions were employed for proposals which did not involve a single specific dollar amount. Proposals which suggested a range (e.g., $150,000 to $250,000) were coded at their midpoint. The 28 proposals which were not codable into a specific amount (e.g., less than $490,000; greater than $35,000) were not included in the calculations of mean proposals.

[63] Using a correlated t-test to compare the mean of the first four proposals with the median of the predeliberation awards, we found that the difference between them is significant ($190,408 vs. 235,639; $t=3.05$; df=60; $p<.003$) while the difference between the mean of the first four proposals and the mean of the predeliberation verdict preferences is marginally significant ($235,639 vs. $210,731; $t=1.88$; df=60; $p<.07$). The mean of the first four proposals was calculated only for the 62 juries in which at least 3 codable proposals were made prior to conclusion of the deliberations.

A third factor that may have contributed to an inflation effect involves the influence of the foreperson. As noted above, when forepersons had expertise in the subject area, they appeared to exercise greater influence on verdicts than did other members of the jury. Forepersons brought to deliberations award preferences which were on average $40,000 higher than the average of their group.[64] As indicated above, forepersons typically exercised more influence on jury awards than other jurors, and as a result their higher initial predispositions may have contributed to the inflation process.

Thus, the inflation process observed may be accounted for by the effects of deliberation on judgments of defendant blameworthiness, the process by which lower predeliberation award preferences were less likely to be proposed as awards, and the fact that forepersons were both especially influential and likely to bring a preference for higher awards to the deliberation process.

IV. Conclusion

The jury evokes an ambivalent and inconsistent set of responses. Ambivalence emerges in the tension expressed between vesting great responsibility in groups of laypersons asked to decide important and often complex issues and nagging concerns about whether the institution is up to its tasks. Inconsistency arises in the variety of different jury images that range from the legal fiction that jurors are passive participants in the process of evidence presentation and assessment to the view that they bring to their tasks expectations, biases, and other attributes that affect their decisions and often lead them to resist efforts to control their behavior by legal instruction.

The evidence we present is quite inconsistent with the model of the jury as a passive and malleable recipient of testimony. Moreover, even when the testimony is arcane, complex, and difficult to follow, jurors make conscientious and often successful efforts to deal with the substance of what they hear, and their decisions reflect such activity. Yet, even if juries seem to pay close attention to both facts and law, they do not always follow the letter of the law. Rather, the jury portrayed in this research is a receptive but not uncritical audience for legal instruction, more responsive to the law when an acceptable rationale for the legal rule is provided.

The image of the jury as an active information processor emerges from a variety of our findings. The windfall avoidance effect when jurors are informed that their damage awards will

[64] $248,250 for all forepersons vs. $206,579 for all other deliberating jurors ($t = 1.78$; df=408; $p < .075$).

be trebled suggests that legal rules and their consequences play an important role in juror decisionmaking. The responses to expert testimony we observe also suggest that jurors play an active role in assimilating and assessing testimony. Jurors did not simply adopt the view of a witness they rated high on expertise, using apparent expertise as a peripheral cue to conclude that the expert must be correct (Petty & Cacioppo 1986). Rather, consistent with deeper processing of information which produces attitude change when the listener is highly involved, the jurors appeared to consider and evaluate the content of what the expert was presenting, and were less likely to be persuaded if they did not feel they understood it.

This approach not only suggests active evaluation and perhaps even subtlety in dealing with expert testimony, but it also indicates the care jurors use in evaluating evidence to reach their decisions. When presented with complex statistical testimony, jurors were not simply overpowered by material they did not understand. Rather, the persuasive force of such testimony appears to depend in substantial measure on the ability of the expert to express clearly the basis for the conclusions it is being used to support. Our results thus suggest that concerns about jurors' uncritical willingness to accept statistical evidence may be overstated.

The relationship between active information processing and sensible decisionmaking is further illustrated by the difference observed among the conditions in which jurors learned about the treble damage rule. When jurors were simply informed of the rule but not admonished to disregard it, the windfall avoidance effect operated strongly; a bald admonition to ignore the effect produced less discussion in the jury of the trebling policy but no significant increase in damage awards. But when judicial instructions acknowledged jurors' inclinations to reduce their awards and jurors were given a clear justification for *not* reducing their awards, the windfall avoidance effect was substantially meliorated. When jurors are taken seriously and efforts are made to deal with their concerns and expectations, that is, when they are treated as active co-participants rather than passive sponges, they appear to be willing and able to respond more appropriately to the dictates of legal rules. Studies such as this one examine the sources of jury reactions to legal constraints, especially the individual and collective processes which produce such reactions. Such investigations can both increase our understanding of jury behavior (and lay response to legal rules more generally) and explore alternative approaches to jury control.

But establishing the conditions and procedures to achieve such jury control is not a simple matter. The results of our motive control condition dramatically show how an intuitively

plausible instruction, grounded in equity theory, produced unintended consequences: the effects of framing or availability that occurred in this condition appeared to lead jurors away from rather than toward the goal that the instruction was designed to achieve. Although the most commonly used policy—our no information condition—turned out to approximate true compensation, this outcome should not simply reassure legal policymakers that their common sense is as good as or better than systematic empirical research. More appropriately, this finding—taken in conjunction with the unexpected inflation in the motive control condition—suggests that plausible predictions will sometimes, but not always, receive empirical support. Thus, policymaking by legislators or appellate judges is best served not by reliance on common wisdom, casual observation, or even promising theory but rather by careful exploration of the likely effects of policies prior to their implementation. Moreover, such empirical work provides an opportunity to compare multiple approaches to jury control simultaneously. Thus, we found that the trebling with explanation condition also produced awards quite close to our true compensation level, suggesting that carefully crafted efforts that both provide jurors with important information and give them reasons why they ought not employ it may, indeed, be successful. In addition, because full disclosure to all jurors may also avoid the potential unpredictability that can occur if only some juries learn about trebling, there may be good policy arguments to prefer this approach to simple blindfolding. More generally, the ways in which the various instruction conditions affect juror and jury decisionmaking suggest that expectations, attention to consequences of their verdicts, and conscious and unconscious information processing all play significant roles in juror and jury decisionmaking.

The last set of issues we discuss centers on the process by which deliberation transforms initial individual preferences into a jury decision. Clearly the group verdict is the product of the preferences, expectations, inferences, and stories that individual jurors bring to the deliberations, but the algorithms that produce this transformation are not well understood. Some of the patterns emerging here—the special influence exercised by the foreperson and the inflation from juror to jury awards—are all in need of further analysis. Moreover, they contradict an image of the jury that has permeated the literature, namely, the view that deliberations have relatively little effect on jury outcomes. Kalven and Zeisel (1966) argued that the study of jury deliberations could add little to our understanding of juries because the final verdict was implicit in the distribution of the jury's predeliberation votes. The social decision theory literature on juries continued this theme (e.g., Davis et al. 1975;

Stasser et al. 1982). Most of this research developed from the criminal context in which juries are faced with few verdict choices. In the civil context, when money damages are at issue and more extensive avenues for compromise are available, the deliberation phase may have a substantial impact on outcomes. The effects of deliberation on perceptions of defendants' behavior and the attendant inflation effect we have found here suggest that research on deliberations provides a fertile ground for understanding jury decisionmaking in civil trials.

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