

FROM LABORATORY TO JURYROOM:
A REVIEW OF EXPERIMENTS ON JURY DECISION-MAKING

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ABSTRACT

This paper reviews recent experiments in the field of jury decision-making. The experiments are discussed and evaluated in terms of the quality of their simulation and their conceptualization of the research problem. A review of the literature indicates that most of the recent experimental work has been flawed to such an extent that the results should not be generalized to the jury system.

We report the results of a jury experiment which avoids many of the previous external validity problems. We show that quite different conclusions can be drawn about the rationality of the jury system when the simulation technique and conceptualization are improved.

We also report a replication of the experiment using nondeliberating students as subjects. By comparing these results with the results obtained when actual jurors are used as subjects and allowed to deliberate, we can more accurately assess the consequences of different methodological approaches. Several hypotheses are proposed to explain the effects of inexperienced subjects and nondeliberating jurors on experimental results.

INTRODUCTION

In the last decade or so numerous social psychologists have studied jury behavior by simulating trials in the laboratory. Many of these studies have had a dual purpose. On the one hand they were designed to test hypotheses developed in social psychology about how individuals and groups make decisions and the way in which they use information to make these decisions. On the other hand, the studies have hoped to tell us something about how juries in real trials make decisions.

This proliferation of jury studies logically follows from the development of the sub-field commonly known as attribution theory. Ever since Heider's early work (Heider: 1958) and Kelly's important theoretical contributions (Kelly: 1967) a main branch of attribution theory has concerned itself with causal attributions. These are very closely tied to the attributions which juries make in criminal and civil trials. One of the primary tasks of the jury is to attribute responsibility to one party or the other. A significant part of the attribution of responsibility is the decision as to whether the defendant "caused" an untoward event in some legally relevant fashion. Thus the study of jury decision making is a natural place to examine attribution processes.

Given the dual purpose of much of the research, however, compromises have been made which we believe have seriously limited the generalizability of the research to the way in which real juries decide real cases in real courts. These compromises have been made in terms of the adequacy of the simulation and the

conceptualization of decision making within the context of a legal system.

In this paper we will review recent experiments and report the results of an experiment which attempts to correct some of the simulation problems and clarify the conceptual issues. We will compare these findings with previous work and with results obtained in a replication of the present experiment using the more typical methodology. We hope to show that typical simulation methods do indeed greatly distort the laboratory findings, and that a clearer conception of how real juries decide may only be obtained through more realistic simulation and refined conceptualization.

REVIEW OF RECENT EXPERIMENTS

The following is a list of some recent articles which discuss decision making within the context of a jury decision.¹

Landy & Aronson, 1969; Efran, 1974; Sigall & Ostrove, 1975; Kaplan & Kemmerick, 1974; Nemeth & Sosis, 1973; Mitchell & Byrne, 1972 and 1973; Stephan, 1974; Sue, Smith & Caldwell, 1973; Griffitt & Jackson, 1973; Izzett & Leginski, 1974; McComas & Noll, 1974.

While it is impossible to summarize these articles in a few words, the general thrust of the work is that it has followed the legal realist line of looking for ways in which the law in action deviates from the law on the books. In 1930 the noted legal scholar Leon Green commented that there was a "great difference between law in statement and law in operation; between jural postulates and jury judgements" (1930:178). Dean Green drew this conclusion from personal observation, with no systematic data to back up his argument. The social psychologists working in the

laboratory have been about the business of proving the truth of Green's statement.

The experiments have examined the effect of such variables as victim and defendant attractiveness (Landy & Aronson, 1969; Efran, 1974; Izzett & Leginski, 1974; Sigall & Ostrove, 1975), defendant character traits (Kaplan & Kemmerick, 1974; Nemeth & Sosis, 1973), jury attitudes and personality (Griffitt & Jackson, 1973; Mitchell & Byrne, 1973) and sex of defendant and juror (Stephan, 1974). Most of these variables are arguably either irrelevant or marginally relevant in the law. Thus if a jury decides a case according to the strict legal criteria these factors should not produce variance in jury verdicts.

The results of most of the experimental work are that in fact such factors do produce variance in the dependent variable. The conclusion one might draw is that there is a significant use of legally irrelevant criteria in the jury decision-making process.

To their credit none of the authors of these experiments baldly assert that real juries are irrational in this fashion. They qualify and moderate their conclusions by recognizing that the laboratory is not a real courtroom. They note that the subjects knew that no one's life, money or freedom was really at stake. Nevertheless, the studies do imply that the results have some relevance to the courtroom.

Thus, Landy and Aronson are "encouraged by the fact that in the absence of extraneous stimuli, our results paralleled some of the findings of Kalven and Zeisel (1966) in their examination of actual cases" (1969: 151-152). Mitchell and Byrne find: "some comfort about the comparability of the laboratory and the 'real world'

[because of] the correspondence between experimental findings and the correlational and descriptive data gathered in legal settings" (1973:128). Efran concludes that, "neither legalistic analyses, statements based on faith, or even jurors' own post-trial reflections accurately portray the importance of seemingly irrelevant influences on the judicial process" (1974: 52). Kaplan and Kemmerick even imply that the experimental setting may produce less dramatic results than would the real thing. "To underscore the biasing power of defendant attributes, note that this experiment employed trait descriptions as characterizations; consider the magnitude of such effects in real-life settings where defendant behavior is in the flesh" (1974: 498).

It seems that most of the experimenters would agree with Griffitt and Jackson that what they are examining is the operation of general processes of evaluative behavior that should affect real jury behavior (1973: 7); and further agree with Sigall and Ostrove that "simulations constitute legitimate avenues for investigating person perception and interpersonal judgement, and there is no obvious reason to believe that these processes would not have the effects in trial proceedings that they do elsewhere" (1975: 413).

We do not wish to argue with the statement that laboratory simulations constitute legitimate avenues of research. Since the Chicago Jury study caused the United States Congress to pass legislation making it a crime to wiretap juryrooms, even with the consent of the parties, their attorneys and the judge, people interested in jury research have been compelled to use alternate methods. The laboratory simulation is probably the best substitute,

but it is clear that this setting leaves much to be desired. Most obviously, the subjects know that no one's fate rests in their hands. Nevertheless, simulations can be more or less similar to the real thing. What we wish to argue is that the simulations under consideration are so different from the "real thing" that there is in fact very good reason to doubt that the processes would operate the same way in real trial proceedings. This has been due, primarily to four factors: a) the instrument used, b) the nature and obtrusiveness of the independent variable, c) the nature of the dependent variable, and d) the presence of judicial instructions.

1) Instrument Problems

In all but one experiment the stimuli were presented by having the subjects read a short script setting out the facts. Usually the script was a few paragraphs describing the facts of the case which varied only in the insertion of appropriate paragraphs describing the defendant's attitudes, sex or attractiveness. In some attractiveness manipulations a photograph purporting to be a picture of the defendant was included. In two studies, (Vidmar and McComas & Noll) although a written instrument was used, the evidence was presented in the form of the testimony of the parties and other witnesses. Only in the Griffitt and Jackson study was another method used. In that study the subjects were shown a videotape. The tape, however, was that of an individual reading a script. From other studies (e.g. Miller et. al, 1974:3) we have strong reason to suspect that the method of having subjects read a short vignette is a far different stimuli from the actual courtroom proceeding. These same studies indicate that a videotape reproduction is a far superior simulation.

2) Independent Variable Problems

The most serious consequence of using scripts which present only a bareboned fact situation is to dramatically heighten the importance of the independent variable since the manipulation constitutes a disproportionate share of the total stimulus. For example, in the second Landy & Aronson experiment (they report two experiments in their 1969 article) approximately four inches of type is devoted to background information, (i.e. the facts of the case). The manipulation is two inches long and it is the last thing that the subjects read. One-third of the entire stimulus is devoted to the manipulation. In the 1973 Mitchell & Byrne study the manipulation is approximately three inches long and the remaining information is about 7 1/2 inches. In the Nemeth and Sosis article the ratio is approximately 1 1/2 inches of manipulation to 4 1/4 inches of other information. For several articles it is impossible to reconstruct even this crude estimate of the salience of the manipulation, but one can hazard the guess that it usually varies between one-third and one fifth of the entire stimulus. An exception are some studies where the manipulation is a photograph (Efran; Sigall & Ostrove). Clearly one cannot make easy inferences about the importance of pictures versus words, but if the old Chinese saying is correct this makes the manipulation the most important stimulus in the experiment. There are some studies where the manipulation is not so overwhelming but in most cases the manipulation is far more salient than any one piece of evidence would be in a real trial.²

In addition to making the manipulation very salient the experiments frequently maximize the variance on the variable.

Maximizing the variance on the independent variable is not an unreasonable procedure for social psychologists. It does, however, lead to a departure from the reality of many, if not most trials. Through the process of cross-examination and opening and closing arguments the attorneys often attempt to reduce variance on some of the things the experiments maximize. Few witnesses appear as bad or good as the parties in the experiments after both counsel have had a chance to question them.

Moreover, the rules of evidence would make it difficult for some of the evidence to be admitted at all. In the Landy and Aronson case for example, much of the evidence concerning the unattractive defendant and victim is clearly and blatantly inadmissible. In no American jurisdiction could the jury be told that the victim in an automobile negligent homicide case was a notorious gangster who allegedly was responsible for the massacre of five men, or that he had been indicted for mail fraud and income tax evasion, or even that he was carrying a .32 caliber pistol at the time of the accident. Nor could they be told over the defense's objection that the defendant was a two-time divorcee. Several other studies have, like Landy and Aronson, run roughshod over the rules of evidence.

Most importantly, all but two of the studies manipulated only an "irrelevant" factor. In doing so they have committed a ceteris paribus fallacy. The crucial issue for jury deliberations is whether "irrelevant" factors overwhelm relevant issues so as to make jury decision making irrational. This is a very serious question, but it is not the question addressed by most of the studies. By failing to manipulate anything but "irrelevant" factors these studies are

asking a different question, that is, if nothing else about a case varies will individuals base their decision upon "irrelevant" criteria? Regardless of the answer to this question, it cannot address the first and central issue concerning jury deliberations.

The results of the two studies which avoid this central problem are revealing. In the Sue, Smith and Caldwell study the evidence of guilt was manipulated (weak or strong evidence against the defendant). In addition, the study manipulated judicial instructions as to the admissibility of some wiretap evidence. The evidence derived from the wiretap was clearly probative as to the defendant's guilt, but might have been inadmissible on Fourth Amendment grounds.

The strength of evidence manipulation had a significant impact upon the verdicts ($\chi^2=13.86$ $df=1$ $p = .005$). The judicial instruction manipulation apparently had no significant impact on jury verdicts in the weak evidence condition, but did influence verdicts in the strong evidence condition (1973:350-351)^{3, 3}

In the Kaplan & Kemmerick study both the character of the defendant and the level of incrimination of the evidence were manipulated. While the authors do not report the data in such a way that we can compare the proportion of variance on guilt or sentence explained by these two factors, an examination of the F ratios indicates that the latter manipulation was by far the more significant.⁴

Thus in the two cases where relevant factors were manipulated they significantly influenced the jury verdict, and in one study overwhelmed the effect of irrelevant evidence.

3) The Dependent Variable Problem

Landy & Aronson's early (1969) jury simulation has been quite important in the jury decision-making literature. It was the first article of recent vintage to examine the issue of attractiveness within the context of jury settings. As a consequence of its primacy in the literature the general facts of the case have been used by subsequent investigators in their own research. The Landy & Aronson case, (hereinafter called the Sander case after the name of the defendant) was used by Griffitt & Jackson, Izzett & Leginski, Mitchell & Byrne and Nemeth & Sosis. The reasons for the widespread use of this case are understandable. The case has produced significant results in the past and there is something to be said for replication. Nevertheless, the choice of this case is extremely unfortunate since there is little or no doubt but that the defendant is legally responsible for the crime with which he is charged. The issue that the "jurors" are asked to decide is how severely the defendant should be punished, and not whether he is guilty of the crime charged.

Even in some of the experiments where the Sander case was not used the same problem emerges. Thus in the Sigall & Ostrove experiment the jurors were only asked to decide the sentence. In Mitchell & Byrne and in the Stephan experiment the cases were strongly biased toward guilty.⁵ Thus in 8 of the 11 studies the primary dependent variable was sentence, not guilt, and in 4 of the studies the guilt issue was not even raised.

Moreover, even in the studies where there was an issue of guilt, this was confounded by asking the jurors to also sentence the defendant. The standard form of this data collection was to

give the juror a short questionnaire which asked whether the defendant was guilty, and directly below asked what sentence the defendant should receive. The subjects could, and presumably some did, look ahead to the next question and thereby entertain the issue of appropriate sentence while arriving at a decision as to the defendant's guilt.⁶ Only in the Kaplan & Kemmerick study were these two issues apparently kept completely separate for the subjects.

In substituting sentence for guilt or confounding the two issues for the jurors the experiments have committed a groundwater mistake. Neither in law nor in common sense are these synonymous issues, and except in the narrow circumstance of the death penalty real jurors are never permitted to even recommend a sentence. They are in fact often admonished that this is not their business. Their business is to decide whether the defendant is guilty of the crime charged; no more, no less.

This issue, however, even goes beyond the question of external validity to actual juries. It seriously compromises the findings of the experiments since the relevant criteria for determining sentences given an individual's guilt are much broader than the criteria for determining guilt. Among relevant issues are the probability that the individual would engage in the activity in the future, whether the commission and consequences of the act were punishment in themselves and whether the defendant is the type of person that should be exiled from the community for a period of time. This is not meant to argue that defendant attractiveness per se, as from a photograph, is an appropriate criteria upon which to sentence someone to jail. Clearly it

should not be, and one should not draw inferences from physical attractiveness to make judgements on the types of issues mentioned above. It could be argued with only slightly less conviction that a person's social status should not play a role. Thus whether John Sander is a well liked insurance adjustor or a little liked janitor should make no difference. The fact that it does, however, must be interpreted in light of other considerations. First of all, it is not clear that these jurors consider this type of gross status distinction or physical attractiveness distinction as "best evidence" as to the need for deterrence or the possibility of future illegal activity. Most people would like better evidence, but given the necessity for a decision, it is not unreasonable that inferences are made from these factors. When the only information is tangentially probative it may be relied upon. This does not prove that if more probative evidence were available, (e.g. defendant's remorse, past driving record) that it would not overwhelm the attractiveness data. The answer to the latter question is still open.

Second, we should consider the results of the Izzett and Leginski study discussed below (p. 14). After group discussion the sentences for attractive and unattractive defendants did not significantly differ. One of the consequences of group deliberation may be to heighten the awareness of jurors that such factors should not be relied upon in setting sentences.

And finally we must recall that in all but one experiment the two issues of guilt and sentence were probably considered simultaneously by many of the subjects. Thus it is difficult to infer

whether attractiveness would have assumed the same importance if guilt were completely separated from sentence. Where both guilt and sentence were at issue it appears that attractiveness played less of a role on verdict outcomes than on sentence outcomes (e.g. Efran, 1974:49).⁷

4) Judicial Instructions

The final difficulty with the design of the experiments is the general absence of any judicial instructions as to how jurors should decide the case. In the McComas and Noll and Vidmar experiments part of the judicial instruction, the crimes for which the defendant could be found guilty, was in fact the manipulation. Only four of the remaining 11 experiments had any judicial instructions at all. One of these (Efran) involved an instruction advising the subjects as to the rule of law under which they should decide the case. This instruction, however, was not manipulated in the experiment. The other three studies (Kaplan & Kemmerick, Mitchell & Byrne 1972 and Sue, Smith & Caldwell) involved instructions as to the admissibility or the probativeness of some piece of evidence.

In the Sue, et al, experiment the issue is certainly artificial. The jurors were allowed to hear evidence which might be challenged as violating the Fourth Amendment and then were instructed either that the evidence was or was not allowable. In actual proceedings all of this would have taken place outside of the jury's presence.

The other two studies are more realistic in that the judicial instructions attempt to have jurors attend to or disregard certain attractiveness information. Since such information might in fact appear in an actual trial, the findings of these experiments have

relatively important implications. Mitchell & Byrne found, with respect to sentencing, that the instructions appeared to have an effect on low authoritarian personality jurors, but no effect on high authoritarian personality jurors. Unfortunately, because the Sander case was used there was no variance on guilt and one cannot, therefore, conclude whether such an instruction could affect the verdict.

In the Kaplan & Kemmerick experiment, the instructions had no significant effect on either the sentencing or the verdicts of the jurors. (1974:497). These results suggest that instructions may not cause jurors to disregard defendant character evidence. However, the character manipulations did not have a great effect on verdicts to begin with. The table on p. 496 indicates that the extreme cases of positive and negative character produced only about 1 1/2 unit shift on a 20 unit guilt scale. Thus, there simply was not too much bias that the instructions could have corrected. Also, the judicial instructions were not as mandatory in nature as they might have been. Rather than ordering the jurors to disregard any character evidence, the instruction said that such characterizations are often inaccurate and not useful (1974:496).

Juror Experience

There are problems with these experiments that go beyond the inadequacy of the courtroom simulation. The first and most obvious difficulty is that all the experiments employ student subjects. We certainly do not wish to argue that students are inappropriate

subjects for use in the social sciences. Research on this question is limited and we do not have data that addresses this issue directly. However, it is reasonable to suspect that using student subject responses to infer the decisions of actual jurors raises serious questions of external validity. Most students have had no courtroom experience and are probably less likely to "play" the role of juror in the same way that actual jurors will.

Deliberations

A much more serious difficulty is that the experimental jurors are rarely allowed to deliberate. Only in the Izzett and Leginski study did the students deliberate in a jury-like fashion.⁸ And even here the deliberation took place only after the subjects had publicly stated their individual conclusions. This kind of prior public statement probably has consequences for the deliberation process.⁹

The lack of a deliberation, more than any other single factor, puts in serious question the external validity of the results. In fact, without a deliberation it is misleading to use the words juror or jury at all since these words connote a group decision-making process. The group decision allows individuals to consolidate and organize the stimuli and to argue with each other about the proper grounds of decision. As a consequence, we might hypothesize that some types of information will be considered largely irrelevant, and other information will be considered important. This seems to be a fair conclusion of what happened in the Izzett & Leginski study. The consequence of a group deliberation was to wash out any sentencing effects caused by an attractive defendant - unattractive defendant manipulation. Sentencing differences had been significant

in the pre-deliberation, individual verdict judgements (1974:275-76).

Table 1 presents a summary of the studies and the nature of their simulation.

What conclusions should we draw from the above review? First, some of the problems of previous designs are easy to correct, and relatively inexpensive. The technology for producing and using videotapes of trials is now available in most social psychology laboratories. There appears to be little justification for the use of the less realistic method of having jurors read scripts.

Second, some of the simulation design problems are such that the findings are basically irrelevant for actual jury deliberations. For instance, some of the evidence introduced in the experiments simply would never come before a jury. It is not clear that examining the effects of such information under better simulation designs is really a worthwhile undertaking given that the goal of the research is to learn something about jury decision-making. The rules of evidence already attempt to control the types of data the jury is allowed to consider, and to see what effect such evidence would have is to discuss a counter-factual. Likewise, it seems at best very artificial to test designs which make the sentence the primary dependent variable. This simply is not a jury task.

Third, there are conceptual problems with some of the designs. One simply cannot conclude anything about the relative importance of "irrelevant" factors from experiments where only these factors are manipulated. Experiments which ignore such a central issue should no longer be done as jury studies.

Finally, there is a set of empirical questions to which we

should direct our attention if we wish to do laboratory research which has relevance for the jury system in American society. For example, what effect does the deliberation have on results? What are the consequences of using students as subjects?

Below we will present the results from an experiment explicitly designed to assess the importance of relevant and irrelevant criteria in jury decision-making. As Table 1 indicates, this experiment attempts to correct most of the simulation problems that flawed the previous jury experiments. This enables us to compare the conclusions about the rationality of the jury system which are derived using different simulation techniques. In addition the present experiment allows us to begin to assess the effects of the deliberation and type of subject on the results of jury decision-making experiments.

THE EXPERIMENT

The experiment reported here is a criminal case loosely based on the facts and legal issues of *Morrisette v. United States*, 342, U.S. 246, 96 L.Ed. 288, 72 S. Ct. 240 (1952). Briefly, the case involves the nature of the intention which an individual must have to be guilty of the federal crime of conversion of government property 18 USC Sec. 641. Morrisette took \$84.00 worth of old army dummy bombshell casings from some land in northern Michigan which the Air Force used as a practice bombing range. There was no argument as to whether Morrisette took the casings and sold them for scrap. He did so in broad daylight and freely admitted he had done so. Morrisette claimed, however, that he believed the casings to be abandoned property and that therefore he did not intend to

steal the casings.

The trial judge refused to allow the jury to consider this excuse, saying (in our terminology) that the appropriate responsibility rule only required that the defendant have the general intent to take. He did not have to possess the specific intent to steal. This position was upheld by the court of appeals, but the Supreme Court, per Justice Jackson, unanimously reversed.

A case was devised which would incorporate, among other considerations, the issue of intent raised in *Morissette v. U.S.* The facts of the experimental case are that approximately 9 months after a building has burned down on a piece of property on the outskirts of a town, a retired citizen, named William Harris, who lived in the area, removed a large pile of uncleaned bricks and used some of them to build an outdoor barbecue. As the act of taking the bricks occurred during the afternoon, it was observed by a woman who lived across the street from the property. She recognized Harris and reported the event to the police when they questioned her in the course of their investigation about the missing bricks. As a result, Harris was arrested and charged with the theft of the bricks. At the time of his arrest, and throughout his testimony, Harris claimed that he thought that the bricks had been abandoned.

The trial was presented to the subjects in the experiment by means of a videotape of a courtroom procedure. Attempts were made to assure the believability of the trial but the subjects were informed at the outset that it was staged and that certain aspects of an actual trial (for example, the attorney's opening and closing arguments) were omitted.

The first part of the videotape consisted of the testimony (including some cross-examination testimony) of four witnesses. The witnesses were: the owner of the property, the woman who lived across the street from the property, the arresting officer and the defendant. The testimony was identical for all versions of the experiment with the exception of the identity of the owner of property and certain circumstances relating to his use of the property and how he came to discover that the bricks were missing. In half of the trials the owner of the property was the State of Michigan and a representative for the State presented testimony in the trial. In the remainder of the trials, the property was owned by a private individual and he testifies. Through these manipulations, the facts of the case remained virtually unchanged while the "size" of the victim varied.

After the presentation of the witnesses' testimony (which takes about 10 minutes), the judge gave his instructions to the jury as to the law applicable in this case. These instructions were detailed and covered a wide range of legal considerations. Among these instructions, two additional experimental variables were introduced.¹⁰

One variable consists of the degree to which the jurors are expected to follow the judge's instructions. At the beginning of his instructions the judge either informs the jury that it is their duty to determine the facts, and to determine them only from the evidence in this case, and that it is their duty to apply the law, as the judge states it, to the facts, and in this way decide the case. (Hereinafter, this instruction is called judge law.) Or the judge says that whatever he tells them about the law, while it is intended to be helpful to them in reaching a just and proper

verdict in the case, it is not binding upon the members of the jury, and the jury may accept the law as they apprehend it to be in the case. (Hereinafter this instruction is called jury law.)

At a later point in the instructions, the judge presents the definition of the intent necessary to find the defendant guilty. The alternatives of general or specific intent are defined, respectively, as:

In determining the defendant's intention, the law assumes that every person intends the natural consequences of his voluntary acts. Therefore, the defendant's intention is inferred from his voluntary commission of the act forbidden by law, and it is not necessary to establish that the defendant knew that his act was a violation of law.

Or, the crime charged in this case requires proof of specific intent before the defendant can be convicted. Specific intent means more than the general intent to commit the act. To establish specific intent the government must prove that the defendant knowingly did an act which the law forbids, purposely intending to violate the law. Such intent may be determined from all facts and circumstances surrounding the case. The word "knowingly" means that the act was done voluntarily and purposely, and not because of mistake or accident. Knowledge may be proven by the defendant's conduct, and by all the facts and circumstances surrounding the case. No person can intentionally avoid knowledge by closing his eyes to facts which should prompt him to investigate.¹¹

Again, each jury receives the same instructions, except for the slight variations described above. The experimental design can therefore be summarized as follows:

INTENT

General

Specific

VICTIM	Judge Law	Jury Law	Judge Law	Jury Law
State of Michigan				
Private Individual				

The two variables on the heading relate to the importance of the responsibility-rule. The dimension of judge law-jury law establishes whether, in general, the jury is bound by the set of responsibility rules presented by the judge. The specific intent-general intent variable defines explicitly one key element of the responsibility rule in this case. Finally, the state vs. individual variable introduces one irrelevant factor which may produce variance in jury verdicts. This distinction has produced different assessments of responsibility among respondents in a large scale attitude survey (Smigel, 1956).

SUBJECTS
SUBJECTS

The subjects were drawn from two sources. The majority of subjects were from the Circuit and District Courts of Washtenaw County (Ann Arbor) Michigan. These individuals were actual jurors who had just completed a two month term in these courts. We requested participation from all the jurors we were able to contact

and achieved a participation rate of about 35% of all eligible jurors.¹²

The second pool of subjects were college students enrolled in introductory sociology classes at the University of Michigan. Participation by these subjects was associated with the fulfillment of the laboratory requirement, but the subjects were not compelled to participate.

All subjects were scheduled to participate in groups of eight. After a brief introduction, each person received a one-page summary of the facts of the case and the charge against the defendant. This document was read aloud to the subjects and they then viewed the video-tape. A short break occurred between the testimony and the judicial instructions, where the experimenter explained what was to follow. After the videotape was shown, the Washtenaw County subjects were asked to deliberate and reach a verdict. The student subjects did not deliberate.

In the case of the Washtenaw County jurors the juries consisted of 5 or 6 members.¹³ Therefore, if more than six subjects came for the experiment these additional members were drawn off randomly and were interviewed immediately concerning their individual opinion. In addition, extra juries were run, none of whose members deliberated. This was done in order to increase the number of non-deliberating jurors. These jurors, like those randomly drawn off, and like the student jurors were interviewed immediately after viewing the videotape. Thus each cell of the experimental design contained six deliberating juries and 10 to 14 student subjects. In addition data was collected on a total of 40 non-deliberating Washtenaw County jurors.

The deliberating jurors were interviewed individually after the deliberation. The deliberation was taped and this fact was made known to the participants before they began deliberating. If a verdict was not reached in 30 minutes the subjects were informed that they would have approximately 5 additional minutes to make a decision. If no decision was reached after this additional period the jury was considered to be hung.

ANALYSIS

To repeat, a recurring conclusion throughout much of the previous jury research is that jurors use evidence that is not legally relevant in deciding cases. By presenting the results from our own experiment, we will examine whether these conclusions could have been due to the conceptualization of the research and the quality of the simulation.

The Decisions of Deliberating Jurors

Table 2 presents a contingency table of the three experimentally manipulated variables by verdict for deliberating Washtenaw County jurors.¹⁴ The chi-square values for main effects are also presented. The dependent variable in this table is a dichotomy of guilty verdicts versus not guilty verdicts.¹⁵

It is clear from the table that the intention variable is most strongly related to juror verdict. The instruction variable is next in importance, while the "size" of victim variable is marginally important.

Jurors under the general intent instruction are more likely to find the defendant guilty. This follows from the fact that

the judge has instructed the jury that the only relevant issue is whether or not the defendant took the bricks and this is freely admitted by all the witnesses in the testimony. Under the specific intent instruction, however, the juror has to decide further questions about the defendant's intention. This is disputed in the the trial. Given this additional constraint one would expect fewer guilty verdicts if jurors are actually basing their verdicts on legally relevant criteria (i.e., deciding the case according to the rules which the judge sets out for them). This expectation is confirmed by the data.

Similarly, when the judge informs the jurors that they must follow the instructions that he gives them, we would expect more guilty verdicts than when he allows each juror to decide both the law and the facts of the case. Again, our expectations are confirmed by the data -- jurors are more likely to find the defendant guilty under the "judge law" instruction.

While we would have predicted that the "size" of victim (a legally irrelevant fact) should have no relationship with verdict, the data show that when the victim is a private individual, jurors are slightly more likely to find the defendant guilty.¹⁶

We also have some data which addresses the question of whether the choice of a dependent variable affects results. We asked our subjects (regardless of their own verdict) to specify the sentence that they thought the defendant should receive in this case. Table 3 presents the cross tabulations of recommended fine, first by "size" of victim and then by intent for deliberating jurors. Note that the effect for the irrelevant factor of "size" of victim now

exhibits a stronger relationship with the dependent variable than does intent. This finding is consistent with our reasoning that additional criteria (e.g., seriousness of the offense) may be more relevant for jurors making sentencing decisions than for jurors deciding guilt or innocence. Sentence is not an appropriate substitute for verdict in experiments involving decision-making where the results will be generalized to the jury system.

Therefore, as we expected (and contrary to most of the conclusions reached by previous researchers on jury decision-making), the data show that legally relevant criteria are those most important to jurors in deciding responsibility. In reaching a verdict jurors do use the rules set out for them by the judge.

It is possible that we are actually underestimating the amount of juror rationality when we examine the simple relationship between verdict and legal rules for decision. To get a more complete picture of the decision-making process it is also necessary to examine the way in which jurors interpret evidence within the context of legal rules. For example, if a juror did not believe that the defendant thought the bricks were abandoned, then even under the specific intent condition, the defendant should be found guilty. Or, a juror may have believed the defendant's assertions, but yet thought that a reasonable person would have acted differently in the same situation. In this case also, the juror would have been acting within the legal framework set up by the judge by finding Harris (the defendant) guilty.

In the post-experimental interview we asked all subjects whether they thought that Harris believed the bricks to be abandoned

(reasonable defendant variable) and whether a reasonable person would have thought that the bricks were abandoned (reasonable person variable). Both questions were coded on a seven point scale from strongly agree (7) to strongly disagree (1).

When the means on the two variables are examined for groups constructed by combinations of intent and verdict, the importance of relevant criteria in juror decision-making is considerably increased. Note in Table 4 that the mean on the reasonable defendant variable is 6.22 in specific intent cases where the defendant was found not guilty and only 4.13 in specific intent cases where he was found guilty. This means that those jurors who thought Harris was guilty had doubts about his intent. They were less likely to believe that he thought the bricks were abandoned. Similarly, the means on the reasonable person variable are 5.42 and 2.62 for specific intent cases where the defendant was found not guilty and guilty, respectively.¹⁷

Furthermore, it is possible for a juror to consider "size" of victim in reaching a verdict in a way that is consistent with a legal model of the jury decision process. There are circumstances under which such information about the victim is legally relevant. For example, the juror may want to use this information in the specific intent cases to determine whether the defendant was acting reasonably by thinking that the bricks were abandoned. A juror might reason that bricks on state land look abandoned but that this is an unreasonable conclusion when the bricks are on private property. In fact, it does appear from the data that this is the case. When the specific and general intent conditions

are considered separately we find that there is a significant relationship of the "size" of victim with verdict only in specific intent cases ($\chi^2 = 5.566$, $df = 1$, $p < .02$). For general intent cases, there is no relationship ($\chi^2 = .0938$, $df = 1$, $p > .70$).

Given these results, we conclude that jurors do use evidence in a legally relevant fashion and that they conform to the model of jury decision-making set forth in the law. Our own view contrasts greatly with the conclusions reached by many of the previous social scientists doing research in this area. While we do not wish to challenge the validity of their results, we think that it is necessary to question the implications they draw from their results to the jury system. The methodology and conceptualization of the earlier work is flawed in such a way that we cannot accept the conclusions regarding the process of jury decision-making. We have shown that improving the simulation technique and designing an experiment with relevance for the jury system, results invasively different conclusions about the quality of jury decision-making.

The Decisions of Non-deliberating Students

To emphasize the importance of the methodological design, we will now discuss the results of a replication using student subjects who did not deliberate to reach their verdicts. With this data we can begin to address some empirical questions which should be of concern to researchers doing jury studies. Specifically, we will comment on the centrality of the deliberation as a tool of the jury and on the ability of student subjects to substitute for real jurors.

Table 5 presents the contingency table of verdict by the three experimentally manipulated variables for the students. None of

these variables demonstrates a significant relationship with the dependent variable. An examination of this table alone would lead to the conclusion that jury decision-making is a totally random process. We even failed to replicate the relationship between the legally irrelevant factor ("size" of victim) and verdict. One might have expected that since our replication with students was closer in design to the earlier attempts to study jury decision-making, that the earlier findings of juror irrationality (i.e., use of legally irrelevant evidence) would have been reproduced. They were not.¹⁸ However, we also did not reproduce our own findings concerning juror use of legal rules in decision-making. The contrast of the student data with that for the "real" jurors is remarkable.

When we introduce the reasonableness variables however, the attributions of the students begin to look less random and more like the attributions of the deliberating jurors. Table 6 presents the means for students on the reasonable defendant and reasonable person variables. Again, these means are computed on groups made up of combinations of intent and verdict. Similar to the jurors, the means on the two variables were higher for the specific intent-not guilty group than for the specific intent-guilty group (6.0 vs. 4.68 for the reasonable defendant variable, and 5.38 vs. 3.8 for the reasonable person variable).¹⁹

Therefore, it appears from this data that the students are using some relevant criteria in making decisions about responsibility. However, they are not using these criteria to the extent that the deliberating jurors do. Furthermore, they are completely ignoring the legal rules that the judge provides in his instructions.

The type of subject used in the experiment and whether or not the jurors are allowed to deliberate to reach a verdict together are crucial factors in the determination of the results of the study. Unfortunately, our own data do not permit us to assign relative importance to each of these two factors. In order to do this, it would be necessary to replicate our experiment using non-deliberating "real" jurors and deliberating students. We tried to do this on a small scale with the nondeliberating jurors. The small number of cases and uneven distribution of cases to cells in the experimental design (despite random assignment of subjects to cells) however, do not permit us to draw conclusions about how the nondeliberating jurors decide the case. We nevertheless present the contingency table for nondeliberating jurors in Table 7.

In general it seems that the process of decision-making for these jurors falls somewhere in between that of the students and the deliberating jurors.

Even though we do not have comparable data on the decision-making processes of groups of student deliberators we do have additional evidence that will allow us to specify some of the ways that students are different from experienced jurors and deliberators are different from jurors deciding the case individually.

Ability to Remember Instructions

Recall the hypothesis that it is more difficult for non-deliberators to correctly remember all of the judge's instructions. Without the benefit of a group discussion some points will be missed or misunderstood. When jurors are allowed to discuss the case among themselves to arrive at a verdict, there is a greater chance that

each of the significant parts of the judge's instructions will be remembered. This will occur because different individuals will attend to different parts of the testimony and instructions.

In the post-experimental interview we ask all of our subjects to recall key points of the judge's instructions.²⁰ Thirty-nine percent of the deliberating jurors, 47% of the non-deliberating jurors and 45% of the students either did not remember the judge's instruction about intent or remembered it incorrectly. These percentages are even more striking when we consider the fact that all of the students and nondeliberating jurors were asked this question immediately following the videotape of the judge's instructions. There was on the average a 5-10 minute wait for these subjects to be interviewed. The deliberating jurors had to complete their deliberation before any of them were interviewed and their wait after that averaged between 15-20 minutes. This piece of evidence emphasizes the usefulness of the deliberation for remembering and understanding the judge's instructions.

When we consider how well the subjects remembered what the judge had to say about the duty of jurors to obey his instructions, we get a slightly different picture. Forty percent of the deliberating jurors, 34% of the nondeliberating jurors and 52% of the students either did not remember or incorrectly repeated the meaning of this part of the instructions. This finding is best interpreted as being due to the previous courtroom experience of the jurors. In conversations with the jurors after the experiment and during the deliberation, we learned that jurors hearing the jury law instruction were quite surprised. For two months they had been accustomed to hearing

the judge say that they must follow his instructions. The novelty of the jury law instruction therefore made it relatively easy to remember. Using the same logic we conclude that the judge law instruction was fairly automatic for jurors to repeat and they could do this without having to recall the words the judge used in our particular case. The students, who lacked this prior experience, had no cause to either attend particularly to this instruction or to recognize it as an intrinsic part of the instructions to the jury.

Playing the Role of Juror

With the preceding explanation we have introduced a second hypothesis that, to a certain extent, students are either unwilling or unable to play the role of juror. For the most part, they lack the experience of being in a courtroom and have never listened to a judge inform them about the responsibilities of a juror. Every juror in our experiment (even those who did not sit on any cases during their time as jurors in Washtenaw County) had this experience.

During the post-experimental interview we asked the subjects, "What is the extent of the jury's obligation to obey the instructions the judge gives as to the law?" The responses to this closed question were as follows: 68% of the deliberating jurors said that jurors must obey the instructions the judge gives, 23% thought that jurors should obey the instructions except under special circumstances, while only 10% thought that jurors should do justice between the parties regardless of the judge's instructions.²¹ The corresponding distribution for the nondeliberating jurors was 62%, 22% and 16%, and for students, 52%, 24%, and 24%, respectively. This data clearly

supports the position that the prior courtroom experience of a subject affects his or her perception of the role of juror. It is further possible that prior experience also could affect the willingness of the subject to seriously accept that role in a laboratory experiment, but we have no data which address the latter issue.

DISCUSSION

We must reiterate our contention that the methodology of these experiments is a factor that must be seriously considered when evaluating the interpretations that researchers place upon their data. We have shown that the quality of the simulation (i.e., the use of videotapes and realistic presentation) and the conceptualization of the problem under investigation (i.e., the choice of a dependent variable, the relevance of the manipulations) affect the conclusions one reaches about the rationality of the jury system. In addition, we have demonstrated that other methodological considerations -- the type of subject used, the presence or absence of deliberations -- will affect the results of decision-making experiments.

There is a need for more research of this type. While our own work provided some clues as to the effects of deliberations and inexperienced subjects, it is not conclusive. We have found that both factors make a difference in the way subjects act as jurors to decide a case. Our feeling is that having a deliberation is by far the most crucial consideration in the design of a jury experiment (beyond the obvious necessity of a proper simulation). However, before this can be stated with conviction, it is necessary that data be collected on groups of student deliberators.

Our data for deliberating jurors suggest that judicial instructions can exert considerable control over the decisions of juries. Certainly, an important area of future research (and one that would provide useful information to lawyers and judges) is to specify the conditions under which judges can affect jury outcomes and to indicate which types of instructions are important in determining the types of evidence that the jury will consider.

The issues of external validity and careful methodological design that we have raised throughout this paper are important precisely because the research we have discussed purports to have implications for the jury system. If researchers are going to try to exert influence in areas outside of social psychological theory, they must begin to conduct their research more responsibly and be aware of the requirements of external validity before making broad generalizations from their results. If this research is to be done it must be done correctly. If it cannot be done correctly, change the title!

TABLE 1
EXPERIMENTS AND SIMULATION TECHNIQUES

Experiment	Threats to External Validity									
	Student Juror	Non-Deliberator	Use of Script	Independent Variable	No Cross Exam.	No Opening & Closing Argu. of Attorneys	No Relevant Variable Manipulation	Sentence as Dependent Variable	No Judicial Rule	No Instruction About Relevance of Evidence
Efran	x	x	x	x ¹	x	x	x	o.k. ²	o.k.	x
Griffitt & Jackson	x	x	x ³	x	x	x	x	o.k. ⁴	x	x
Izzett & Leginski	x	o.k. ⁵	x	x	x	x	x	x	x	x
Kaplan & Kemmerick	x	x	x	o.k.	x	x	o.k.	o.k. ²	x	o.k.
Landy & Aronson	x	x	x	x	x	x	x	x	x	x
Mitchell & Byrne (1973)	x	x	x	x	x	x	x	o.k. ⁶	x	x
Mitchell & Byrne (1972)	x	x	x	x	x	x	x	o.k. ²	x	o.k.
McComas & Noll	x	x	x ⁷	o.k. ⁸	x	x	x	x	o.k.	x
Nemeth & Sosis	x ⁹	x	x	x	x	x	x	x	x	x
Sigall & Ostrove	x	x	x	x ¹	x	x	x	x	x	x
Stephan	x	o.k. ¹⁰	x	o.k.	x	x	x	o.k. ²	x	x
Sue et. al.	x	x	x	o.k.	x	x	o.k.	o.k. ²	x	o.k.

TABLE 1 (continued)

Vidmar	x	x	x ⁷	o.k. ⁸	x	x	x	x	o.k.	x
Colasanto & Sanders	o.k.	o.k.	o.k.	o.k.	o.k.	x	o.k.	o.k.	o.k.	x

1. The independent variable was a photograph of the defendant.
2. The questionnaire allowed subjects to look ahead to the sentencing question while answering the guilt question.
3. A video tape was used, but it presented a person reading a script.
4. The experiment used the Sander case, heavily biased toward guilt.
5. The subjects arrived at individual verdicts before group deliberation.
6. The effect of the manipulation on guilt was not significant, and the case was skewed toward guilt.
The mean on guilt was approximately 6.00 on a 7 point scale.
7. The script was the testimony of several witnesses.
8. The manipulation is the crime the defendant is charged with.
9. There were two separate students samples from two schools.
10. The group deliberation was among three person groups, all of the same sex.

x = threat to external validity is present
o.k. = threat to external validity is absent

TABLE 2: VERDICT BY INTENT, INSTRUCTION AND VICTIM FOR JURORS

	Specific Intent				General Intent			
	Jury Law		Judge Law		Jury Law		Judge Law	
	State Indiv.	State Indiv.	State Indiv.	State Indiv.	State Indiv.	State Indiv.	State Indiv.	State Indiv.
Guilty	18 (51%)	20 (57%)	19 (56%)	29 (91%)	26 (76%)	30 (77%)	33 (92%)	32 (97%)
Not Guilty	17	15	15	3	8	9	3	1

N = 279

	x ²	p
Intent	17.97	.0000
Instruction	11.56	.0007
Victim	4.12	.0425

TABLE 3: SENTENCE (FINE) BY INTENT AND VICIM FOR JURORS (N=279)

	State	Individual
None	9.4%	10.2%
Under \$200	22.3	9.5
\$200	56.1	67.2
\$201-500	10.1	12.4
Over \$500	2.2	.7
N=	138	135

$\chi^2=9.83$
 $p = .0434$

	Specific	General
None	12.7%	7.0%
Under \$200	20.9	11.3
\$200	55.2	67.6
\$201-500	10.4	12.0
Over \$500	.7	2.1
N=	133	141

$\chi^2=9.00$
 $p = .0611$

TABLE 4: Means and Variances on Reasonable Defendant and Reasonable Person Variables for Jurors

	Reasonable ^a Defendant	Reasonable ^b Person	N
Specific Intent Guilty	Mean = 4.13 Variance = 3.81	2.62 3.71	86
General Intent Guilty	5.02 3.11	3.26 3.50	121
General Intent Not Guilty	6.62 .448	4.62 5.35	21
Specific Intent Not Guilty	6.22 .624	5.42 1.15	50

All differences between means are significant beyond the .05 level.

- a. "The defendant really thought the bricks were abandoned."
(1 = very certain the statement is not true. . . . 4 = un-
certain. . . . 7 = very certain the statement is true)
- b. "A reasonable person would have thought that the bricks were
abandoned." (same scale as above)

TABLE 5: VERDICT BY INTENT, INSTRUCTION AND VICTIM FOR STUDENTS

	Specific Intent				General Intent			
	Jury Law		Judge Law		Jury Law		Judge Law	
	State	Indiv.	State	Indiv.	State	Indiv.	State	Indiv.
Guilty	5 (42%)	7 (58%)	7 (64%)	6 (43%)	4 (36%)	5 (56%)	8 (67%)	6 (46%)
Not Guilty	7	5	4	8	7	4	4	7

N=94

	X ²	P
Intent	.0001	.9930
Instruction	.3685	.5438
Victim	.0444	.8331

TABLE 6: Means and Variances on Reasonable Defendant and Reasonable Person Variables for Students

	Reasonable ^a Defendant	Reasonable ^b Person	N
Specific Intent Guilty	Mean = 4.68 Variance = 2.48	3.80 3.17	25
General Intent Guilty	5.09 2.54	4.52 3.90	23
General Intent Not Guilty	6.32 .703	5.82 1.11	22
Specific Intent Not Guilty	6.00 .696	5.38 1.72	24

The only comparisons which yield significant (.05 level) differences are specific intent-not guilty vs. specific intent-guilty and general intent-not guilty vs. general intent-guilty.

a. see note a for Table 4.

b. see note b for Table 4.

TABLE 7: VERDICT BY INTENT, INSTRUCTION AND VICTIM FOR
NON-DELIBERATING JURORS

	Specific Intent				General Intent			
	Jury Law		Judge Law		Jury Law		Judge Law	
	State	Indiv.	State	Indiv.	State	Indiv.	State	Indiv.
Guilty	4 (44%)	3 (75%)	1 (50%)	2 (40%)	5 (83%)	3 (75%)	1 (100%)	1 (17%)
Not Guilty	5	1	1	3	1	1	0	5

N=37

	χ^2	P
Intent	.288	.5915
Instruction	3.05	.0807
Victim	.703	.4018

FOOTNOTES

1. Although we have not been exhaustive, we believe this list includes most of the important studies that had been published as of January, 1976. We have limited ourselves only to articles which explicitly refer to juries, jurors or judicial processes in their title, and have by implication or express statement conceived of the research as relating to jury decision-making. We have, therefore, excluded many studies which have obvious implications for jury deliberations, but which have not attempted to make the analogy. Thus, for instance, Walster's well known piece on assignment of responsibility for an accident is not included because she did not attempt to make any direct comparisons to juries.

In addition we have generally restricted ourselves to published studies. The only exception is the 1972 Mitchell and Byrne study which is generally cited in the published literature. For a more comprehensive examination of published and unpublished jury studies see Davis, et al, 1975.

2. For the purposes of the present discussion we have set aside the McComas & Noll and Vidmar studies. They concern a different issue; the relationship of offense charged with verdict. In a recent article Lantz (1975:123) has cast serious doubt upon the conclusions of these studies.
3. See the further discussions of judicial instructions below.
4. Character traits of defendant on guilt $F=11.8$ $df=2, 186$.
Evidence factors on guilt $F=299.1$ $df=1, 93$.

5. In Mitchell & Byrne (1973) on a seven point scale from guilty to not guilty the mean score for all subjects was approximately 6.
6. In the Stephan study the subjects ranged between 36% and 0% not guilty, depending upon the treatment. The average appears to be around 10-12% with the remaining 88-90% finding the defendant guilty of some degree of homicide.
6. See Sue, et al, for an example (1973:349).
7. Moreover, even in these cases we should not forget that there really was little doubt as to guilt. Thus in the Griffitt & Jackson piece defendant-subject attitude similarity did correlate significantly with guilt, but even the similar subjects tended to find the defendant guilty (Mean = 2.66 on a 7 point scale where 1 equals very certain guilty and 7 equals very certain not guilty). Guilt after all is a dichotomous variable in law--one is or one is not. It may be that variables like defendant attitudes affect the certainty of the verdict, but may have less effect on guilt per se.
8. One other study, by Stephan, did allow groups to decide a case. However, the groups were of three persons, purposefully all of the same sex. This does not constitute a deliberation in the legal sense.
9. See Hamilton, 1974 and Gaskell, 1973.
10. The manipulations were embedded in the body of the instructions. We attempted to make them no more obtrusive than they would be in a real trial. Thus, out of about 7 minutes of instructions, the manipulations accounted for only 30 to 45 seconds.

11. These instructions are those now given to federal juries in light of Morrissette.
12. Although comparable data was not available for the population of jurors in Washtenaw County, an examination of the distribution of our sample of jurors on certain demographic variables seemed to indicate that our sample was not seriously biased. 46.6 percent of the jurors were male and 53.4% female. Blacks were somewhat underrepresented (94.3% white, 5% black and 0.7% another race). As would be anticipated in a city such as Ann Arbor, professionals and students were overrepresented (31.8% professional and technical workers and 12.4% students), and the education and income distributions were more to the high end of the scales than is true for the population in general (median income was about \$20,000 and median education was just over 3 years of college, with almost one quarter of the sample having some graduate training). The mean age of the sample was 39 years.

Census data for the city of Ann Arbor in 1970 shows the population was 91% white and the median years of schooling was reported to be 15.4. Also, the largest proportion of workers was in the category of professional and technical workers.

13. While eight subjects were always scheduled, we found it to be extremely difficult to always get at least six jurors to come. We, therefore, decided to run the experiment with five person juries when necessary. A comparison of five and six person

juries indicates that there are differences in their verdicts. The primary difference is that, as one might expect, 5 person juries were more likely to reach a unanimous verdict than were six person juries (82% vs. 49%). Other differences, however, lead us to conclude that the use of the smaller juries does not affect our findings. In all cases where there are differences, the five person juries are operating against our hypotheses. Moreover, the main effects in the experiment remain significant when we only look at six person juries. Given these results, we will treat all juries as a single data set.

14. The present analysis uses the individual juror as the unit of analysis. Although we believe that the jury is the unit which should be examined in any study of jury decision-making, we have decided to use the individual data for comparability with the student data presented below. In an earlier paper, "The Use of Responsibility Rules in Jury Decision-Making: Morissette Revisited", we consider the jury as a whole. The results at the individual level are consistent with results obtained using juries as the unit of analysis.
15. Each individual in the jury was asked to sign a verdict form after the deliberation. For juries that hung, two verdict forms were provided -- one for those voting guilty and one for those voting not guilty. Subjects who did not deliberate were asked to give their individual verdict at the start of the post-experimental interview.

16. The relationship between victim and verdict using juries as the unit of analysis is not statistically significant.
17. For jurors, the interaction between the reasonable defendant variable and intent is significant beyond the .05 level ($F = 6.27, df = 1,273$). The interaction of the reasonable person variable and intent is significant beyond the .01 level ($F = 16.0, df = 1,271$).
18. However, we have some evidence that the students consider the "size" of victim to be more important than the jurors do. In the post-experimental interview we asked, "How important was it to you personally that the victim in this case was a private individual rather than a large organization like the State of Michigan in reaching your decision as to Harris' guilt or innocence?" The question was reversed for those conditions where the victim was the State of Michigan. Subjects responded in terms of a five point scale. Seventy-seven percent of the jurors and only 55% of the students stated that the "size" of the victim was not important. On the other hand, 17% of the jurors and 28% of the students stated that this piece of information was very important to them. These differences are significant at the .001 level.
19. Neither the interaction between the reasonable defendant variable and intent nor the interaction between the reasonable person variable and intent is significant for students. ($F = .4285$ and $F = .114$, respectively, with 1,90 degrees of freedom.)
20. We ask, "Do you recall what the judge said in his instructions about the intention the defendant must have to be guilty of the

crime?" and "Do you recall what the judge said in his instructions about the duty of jurors to obey and follow his instructions as to the law in this case?".

21. See Mortimer R. Kadish and Sanford H. Kadish, "The Institutionalization of Conflict: Jury Acquittals," Journal of Social Issues 27 (2, 1971), for a theoretical discussion of these three interpretations of the juror role.

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