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WEB-BASED EXPERIMENTAL RESEARCH IN PSYCHOLOGY AND LAW:

METHODOLOGICAL VARIABLES THAT MAY AFFECT DROPOUT RATES,

SAMPLE CHARACTERISTICS, AND VERDICTS

by

Kevin M. O'Neil

A DISSERTATION

Presented to the Faculty of

The Graduate College at the University of Nebraska

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For the Degree of Doctor of Philosophy

Major: Psychology

Under the Supervision of Professors Steven D. Penrod and Brian H. Bornstein

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DISSERTATION TITLE

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that May Affect Dropout Rates, Sample Characteristics, and Verdicts

BY

Kevin M. O'Neil

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GRADUATE COLLEGE
UNIVERSITY OF NEBRASKA

WEB-BASED EXPERIMENTAL RESEARCH IN PSYCHOLOGY AND LAW: METHODOLOGICAL VARIABLES THAT MAY AFFECT DROPOUT RATES, SAMPLE CHARACTERISTICS, AND VERDICTS

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University of Nebraska, 2002

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The development of the Internet and the Web has allowed all researchers to employ a new method of collecting data, but questions remain about methodological threats to the validity of research findings. There are many variables associated with what is termed "Web-based research," each of which might influence the results of Web-based studies. Four studies examined the effects of different sample types, methods of soliciting participants, financial incentives, manners of collecting personal or demographic information, designs of tables on Web pages, and methods of obtaining informed consent on dropout rates, sample characteristics, and the ultimate dependent variable in these studies—verdicts. Each variable showed some effect on dropout rates but two patterns emerged. Some variables had consistent effects, as certain conditions led to increased dropout at several points in the study. Other variables had effects on dropout at one point in the study but then effects in the opposite direction appeared at subsequent stages of the study. Sample type and table design had consistent effects on sample characteristics, reflected in mean differences and differences in variance on attitude measures. Sample type was related to verdict in one study, in which there were also several interactions. Other methodological variables generally did not affect results.

Web-based Experimental Research in Psychology and Law:

Methodological Variables that May Affect Dropout Rates,

Sample Characteristics, and Verdicts

Researchers seeking to conduct Web-based psychology experiments face many

challenges. Following the development of the Internet and the World-Wide Web,

researchers took advantage of the new medium to conduct email and Web-based surveys,

the first being by Kiesler and Sproull (1986). Despite changes and advances in the

Internet and the Web since then, including increases in the number of people using the

Internet and Web, the vast majority of psychological research that is done over the Web

still consists of survey research (for examples, see Buchanan, 2000; Buchanan & Smith,

1999; Stanton, 1998).

It took until 1996 for the first psychological experiment, outside of the classroom

and with manipulation of some variable, to be completed by John Krantz and colleagues

(Krantz, Ballard & Scher, 1997) over the Web (see Musch & Reips, 2000), placing Web-

based experimentation far behind research into Web-based surveys. Although more have

been completed since then, and in growing numbers, a recent survey (Musch & Reips,

2000) that searched for researchers who had completed an experiment over the Web

could only find thirty-five such researchers, excluding the present author.

One possible explanation for the disparity is that survey research is easier to

accomplish over the Web than experimental studies. A researcher need only have basic

familiarity with Web pages in order to construct a form to collect survey data, and several

readily available software programs make survey research over the Web extremely easy.

A few published articles and books (Birnbaum, 2000a; Morrow & McKee, 1998;

Schmidt, 1997) have addressed issues involved in Web-based experimental research.

Several online sources aim to aid researchers in constructing Web pages for Web-based

studies (see factorWiz by Michael Birnbaum [2000c] at http://psych.fullerton.edu/

mbirnbaum/programs/factorWiz.htm, WEXTOR by Ulf-Dietrich Reips at

http://www.genpsylab.unizh.ch/wextor/index.html. and my own at http://psych.unl.edu/

psychlaw/guide/guide.asp). However, even the most complex online guide cannot

automate the construction of all necessary Web pages. and each researcher is left to make

many decisions on his own. decisions that may influence the results of the study.

Validity of Web-based Research

The decision to collect data over the Web causes concerns about validity. The

majority of research regarding the validity of Web-based research has compared Web-

based data collection to other methods. In general, survey methodologists have reported

equivalent results from Web-based surveys and other methods of collecting data (Best,

Krueger. Hubbard & Smith. 2001; Frey. 2000; Miller. Neal. Roberts. Cressler. Metrik.

Marlatt. & Baer. 2002; Pealer. Weiler. Pigg, Miller. & Dorman. 2001; Yun & Trumbo.

2001). Krantz and Dalal (2000) summarized nine previous psychology experiments that

have allowed comparisons between methods (all paper-and-pencil versus Web). Although

the different studies used different tests of equality between the samples, all studies

concluded that the Web data were comparable (in terms of main effects) to traditional

laboratory findings. In addition, cognitive and perceptual experiments running at

PsychExperiments have produced similar results to previous laboratory data (McGraw,

Tew, & Williams, 2000). More recent studies have continued to find mostly equivalent

results (Simon, Snow, Brownstein, & Read, 2002; Slawson & Bornstein, 2002)

Penrod and O'Neil (2000) presented a summary of two studies that compared Web-based data to pencil-and-paper data in the context of psychology and law (see also Simon et al., 2002; Slawson & Bornstein, 2002). Both studies addressed capital jury decision-making. One study (Patry, O'Neil, & Penrod, 2002) solicited non-student participants for a paper version from Texas and Nebraska and participants for a Web-based version from Texas through direct mail and email solicitations. The second study (Claussen-Schulz, O'Neil, & Penrod, 2002) solicited undergraduates for a paper version of the study and non-students from a national sample for a Web-based version. Although this did not allow a simple comparison between methods, a comparison was made between the standard method of data collection and the new, Web-based method. Analyses did not reveal any main effects of method or sample type or any interactions involving these variables in either study. Analyses did show more variance in the continuous dependent variable in the second study among the Web sample.

<center>Methodological Variables</center>

A researcher should not compare data collected over the Web to data collected by other methods without also considering specifically how the Web-based research is conducted. Comparing previous Web-based experiments, there is a wide variety of what is termed "Web-based research." Web-based researchers can use several methods to solicit participants and there are numerous methodological variables that affect how research is done and that could affect the data collected. The present research focuses on different sample types, financial incentives, Web site designs, methods of asking for personal information, and methods for obtaining informed consent.

These methodological variables may be related to dropout rates, characteristics of the sample, and/or results of the study. Since response rates are difficult to compute for Web-based studies, previous research has focused on dropout rates—of those participants who begin the study how many do not finish it. Many methodological variations attempt to reduce dropout in order to increase sample size. However, these variations may affect the representativeness of the completed sample and thus the external validity of the research. The present research investigates whether methodological variables affect the demographic, attitudinal, and motivational characteristics of the sample. Methodological variables may also affect the substantive results of the study through main effects on the dependent variable and/or interactions with other variables. These effects, if any, may be attributable to demographic, attitudinal, and motivational variables.

Population and Sample

Psychological research frequently uses undergraduates as research participants because of their relative accessibility and cost. Researchers can easily recruit large samples of undergraduates, and dropout concerns are infrequently noted. One advantage of Web-based research is the ability to reach large samples of non-students, and dropout concerns are more prevalent (e.g., Reips, 2000). Researchers expect less dropout among non-students, though research has not directly tested the hypothesis. Prior research has addressed demographic, attitudinal, and motivational differences between samples.

Undergraduates vs. non-students

Several articles have commented on the potential problems of using undergraduate students as a sample of the general population (e.g., Jung, 1969; Oakes, 1972; Sears, 1986; Smart, 1966). These problems include that undergraduates may differ

from non-students in their motivation to participate in the study, may have less well-formed attitudes that less strongly influence their behavior, or may have a weaker definition of the self (Sears, 1986). Some researchers go as far as to say that undergraduate participants are "coerced," while non-student participants are truer volunteers who are more interested in the study (Oakes, 1972).

Undergraduates' attitudes are less well-formed in that they are still developing and are malleable in response to recent events and peer pressure, and whatever attitudes undergraduates do hold may exert less influence on behavior than adults. For instance, two meta-analyses of jury studies have found a weaker relationship between attitudes and guilt verdicts among undergraduates than non-students (Narby, Cutler, & Moran, 1993 [authoritarianism]; Nietzel, McCarthy, & Kern, 1999 [death penalty attitudes]).

Yet still, as pointed out by Sears, many researchers feel that results produced by studies with different samples are usually equivalent, a feeling that has been supported by several analyses (but see Oakes, 1972). For instance, Bornstein (1999) summarized jury decision-making studies that used multiple samples and reported that few found any main effects or interactions involving sample type. However, such findings may not replicate in Web-based experiments. First, undergraduates willing to participate in Web-based research may not be representative of all undergraduates. The former will most likely be more comfortable or experienced with computers or may be looking for the easiest way to complete their required research participation hours. This may cause undergraduates over the Web to have different motivations and attitudes. Second, non-students reached through the Web are not the same as the entire population, in terms of demographic variables, attitudes and motivation (Hewson, Laurent, & Vogel, 1997).

Penrod and O'Neil's (2000) conference presentation described a study (O'Neil &

Penrod, 2002) that compared data from non-students (all Texas residents) recruited by

email solicitations and undergraduates in a study addressing capital jury decision-making.

There was a significant bivariate difference in the distribution of verdicts between the

two samples (56.2% of non-students voting for death vs. 39.7% of undergraduates, $\chi^2(1)$

= 8.57, p = .003) but in a multivariate analysis this effect was not independent of the

difference in support for the death penalty between the two samples. There were no

significant interactions involving sample type. Overall, the study showed that sample

type could be related to attitudes that in turn influence the dependent variable of a study.

O'Neil and Penrod (2001) reported another study on capital jury decision-making

that compared data from non-students solicited by posting links on Web sites that collect

such links and undergraduates. Analyses revealed no main effect of different samples and

no significant difference between attitude factor scores. However, there were two

significant interactions between sample type and attitudes. A subscale measuring

retributive attitudes had a stronger positive relationship to death verdicts among the

undergraduates (β = .37) than the non-students (-.03), while the subscale measuring

"general support" of the death penalty had a weaker relationship (β = .17 for

undergraduates and .61 for non-students). This study shows that sample type may

moderate the relationship between attitudes and behavior.

Different non-student samples

The same concerns about using undergraduates instead of non-students apply to

the different sources of non-student samples that can be tapped. Web-based research has

employed a variety of methods to solicit non-student participants. Researchers may

"troll" online sources to collect a list of email addresses, randomly select addresses from that list, and then email a solicitation to potential participants. Researchers have trolled addresses from online directories, newsgroups, bulletin boards, listservs, and chat forums (for a summary see Kaye & Johnson, 1999). Also, there are Web sites that collect links to ongoing Web-based psychology research. Similarly, the study's URL may be submitted to popular search engines, or a solicitation can be posted on relevant newsgroups or advertisements could be placed on popular Web pages.

Each of the above methods may attract participants with different demographic characteristics, attitudes, and motivations. Each method depends on the self-selection of participants, which depends on each participant's characteristics (Groves, Singer & Corning, 2000). Self-selection has been a noted concern for Web-based researchers (e.g., Goeritz & Schumacher, 2000; Kent & Lee, 1999; Reips, 2000), but may be moderated by solicitation method. Participants who reach an experiment through links on certain Web sites may be the most interested in participating in a Web-based study. Participants reached through email solicitation may also be interested in participating, but there is also an increased risk of reaching participants who want to skew the results of the study, either because they are angered by the unsolicited email or because they are the most interested in the subject matter. That is, participants solicited by email may have the strongest and most influential attitudes relevant to the study's subject matter. Participants' motivation and attitudes may be more influential when the subject matter of the experiment is controversial or appealing or if the study is demanding of time and effort. Also, "volunteer bias" has been found in areas such as sexuality research (Catania, Binson, Van der Straten, & Stone, 1995) and high-demand tasks (Waite, Claffey & Hillbrand, 1998).

Further, salience has been shown to be related to response rates of email surveys (Sheehan & McMillan, 1999).

Psychological researchers have only recently begun to employ the solicitation method most often used by survey researchers—large sample lists compiled in advance. Once a sufficiently large list is assembled, a more representative sample can be chosen. For example, Terhanian (2000) and Taylor (2000) describe how Harris Interactive, Inc. chooses samples from a panel pool of over 5.5 million people. From this panel, 56 percent had Internet access and therefore were eligible to participate in Web-based surveys. This group showed some demographic differences from the non-Internet users, but the panel was sufficiently large so that all demographic categories and combinations were included in good amounts (Terhanian, 2000). Two recently developed Web sites (www.experimentcentral.org and www.studyresponse.com) offer researchers access to a pre-compiled participant pool. Still, the motivation and attitudes of a large panel that expects to participate in Web-based studies (probably for money) may differ from those reached by other solicitation methods.

Maximizing and Controlling Responses

There are several things that Web-based researchers may choose to do in order to maximize response rates or minimize dropout rates and, possibly at the same time, to control the quality of participants' responses. Response rates among adults solicited by email in previous studies were extremely low (around 2.5%). Musch and Reips (2000) found that dropout rates in previous Web-based studies averaged 66% and ranged from 13 to 99%. The present research focuses on three variables involved in most psychological research that may influence dropout—the use of financial incentives or

rewards for participation, how researchers collect personal information from participants, and Web site design. Any effort to minimize dropout rates could also affect the characteristics of the sample and participants 'responses.

Financial Incentives

Offering some amount of financial incentive has long been used to increase response rates of mail surveys (Church, 1983), and has received much attention by survey researchers (for a review see Dillman, 2000). Financial incentives have also been used in Web-based studies: Musch & Reips (2000) found three studies that paid each participant and ten that included payment through a lottery. Previous research has addressed whether financial incentives cause some who would not otherwise participate to participate, increase the effort of those who do participate, or affect the performance of participants.

Research by survey methodologists has shown that a token financial incentive sent along with the solicitation clearly increases response rates, but making a reward contingent upon the completion and return of the survey does not (Church, 1983). Even one or two dollars can increase response rates (James & Bolstein, 1992; Dillman, 2000) for even relatively large surveys, though longer surveys still lead to lower response rates (Dillman, Sinclair & Clark, 1993). Also, there are generally diminishing returns on response rates when larger incentives ($5 or $10) are given (James & Bolstein, 1992). Finally, some studies have found that donations to charity or lotteries have no effect on response rates (Warriner, Goyder, Gjertsen, Hohner, & McSpurren, 1996).

O'Neil and Penrod (2001) manipulated whether a chance at payment through a lottery was offered and looked for any effects on response rate. Participants in the lottery condition were told on the first page of the site that they would be eligible for a lottery

with prizes of $50, $25, and $10; all then proceeded through a page with a consent form and then to the research materials, allowing dropout to be monitored across several stages of the study. Offering a chance at payment did not influence the number of people who began the study or who reached the research materials, but of those who reached the research materials, more people in the lottery condition dropped out of the study than those in the no-lottery condition (36.9% versus 27.0%; $p = .054$). This finding contradicts that by Frick, Bächtiger and Reips (1999), who found a significant decrease in dropout if payment by lottery was offered. However, the increase in dropout in the former study may be attributable to having to ask for the participant's name, address and social security number on the same page as the research materials in order to get participants paid. In addition, size of the study may be important: the study by Frick et al. (1999) was very short, while O'Neil and Penrod's (2001) study took over 15 minutes.

The largest concern about offering financial incentives for participation is that a participant's intrinsic motivation---the motivation to read or view what is required, perform the necessary tasks (e.g., decision making), and then honestly answer the questions---will be replaced by the extrinsic motivation of participating for money and such a change will influence the results of the study. First, financial incentives may result in less-motivated participants in that participants who are otherwise tired of the study may continue only for the money, not taking the time to carefully respond to all questions. Second, people may participate only for the money (or may try to participate several times) and not take the study seriously. Singer, Van Hoewyk and Maher (1998) describe another, unpublished study that found that survey respondents who had received a ten dollar incentive felt that they had completed the survey because of the incentive,

whereas those who received a small gift responded because of interest in the survey. However, the authors did not note whether these different motivations influenced the results of the survey.

The commonly cited research for such a concern deals with children's play activities, where extrinsic rewards did replace intrinsic motivation and changed children's play behavior (Deci, 1975; Lepper, Greene & Nisbett, 1973). However, other social psychological theory predicts that financial incentives may increase intrinsic motivation to participate in a survey or study. Specifically, the literature on persuasion and compliance with requests indicates that the rule of reciprocation---a felt obligation to respond to positive behavior by another with positive behavior toward that person---is extremely strong (Cialdini, 1993). As such, participants' receipt of a financial incentive will increase the participants' motivation to participate in the study in such a way as to benefit the person or group that is conducting the survey. This rule applies most strongly to financial incentives included with the survey or study, but even the opportunity to receive a financial reward contingent on completion of the study may be perceived as positive, invoking the rule of reciprocation. Furthermore, because the participants' choice about whether to initially link to the Web site is most likely quick and not extensively processed, it is driven largely by heuristics (Groves, Cialdini, & Cooper, 1992). As such, the rule of reciprocation may be sufficient to invoke a participant's full participation or may combine with other heuristic rules, such as authority, scarcity, liking or present emotional state. A choice based on heuristics also allows methodological variables to have a larger effect (see Crawford, Couper & Lamias, 2001).

Offering financial incentive may also affect the distribution of attitudes or

personality traits of the sample. For example, Rush, Phillips, and Panek (1978) showed that paid participants differed significantly from non-paid volunteers in that paid participants were more social and had stronger interpersonal relationships, along with personality differences. Also, in a situation where no financial incentive is offered or where the offered incentive is not perceived as very strong (i.e., a low expected value), the strongest determinant of participation may be the relevance or import of the subject matter to the participant (Groves et al., 1992, Groves et al., 2000). As such, a study without a financial incentive or with only a small incentive may attract only those with strong attitudes toward the subject matter of the study.

Furthermore, the type and amount of the financial incentive may be related to motivation and attitudes. Direct payment to each participant may be a stronger extrinsic incentive than a chance to win money through a lottery, and offering payment to a charity introduces additional motivations (e.g., altruism). In addition, one particular concept that may be related to financial incentives is participants' risk taking and decision making. It is frequently found that choosing between a small but certain payment (e.g., $5) and a larger but uncertain chance at a larger payment (1-in-a-100 chance at $500), most people are risk-aversive and choose the guaranteed payment. This risk aversion increases as the guaranteed payment increases in value and as the alternative has a higher probability but lower value (Kahnemann & Tversky, 2000; Kuehberger, 1998; Kuehberger, Schulte-Mecklenbeck & Perner, 1999). Thus, people should be more likely to participate in research if they are offered direct payment, and those who participate in a study that offers payment through a lottery should be less risk-averse. Birnbaum (1999; 2000a) offers some support for this; his two samples from the Internet, paid by a lottery, were

less risk-averse than a laboratory sample of undergraduates. Finally, risk aversion may be particularly relevant to research in an area such as jury decision-making studies involving negligence decisions, where jurors are asked to second-guess whether the defendant's behavior was reasonable in light of the risks known to the defendant.

Most previous research on the effect of financial incentives comes from research from economists or those studying employees' behavior, where performance-based financial incentives are hypothesized to increase quantity and quality of performed tasks. Research has generally shown that incentives increase the quantity of performance but only affects the quality of certain tasks, such as clerical tasks or judgment and decision tasks like recall of items, predictions based on formulas, and easily-solved problems (e.g., Camerer & Hogarth, 1999; Jenkins, Mitra, Gupta, & Shaw, 1998). In studies like the present research, incentives often are contingent on completion of the entire study, in essence just one (aibeit large) task. When the financial incentive is a lottery, where participants have only a chance at winning money, and thus low value expectancy. Both these differences should mitigate the effects of incentives. Further, there is no "correct" performance, so even if effort is increased through financial incentives differences may not be found.

Rush et al. (1978) found that participants offered a financial incentive committed more errors of omission on a dichotic listening task and were less field-dependent in a perceptual task. Singer et al. (1998) do not report any findings of effects of financial incentives on survey results, though one online source notes that offering incentives may increase liking of a product (see http://www.virtualsurveys.com/news/oct2000news/incentives.htm). In O'Neil and Penrod's (2001) path model to explain capital jury

decision making there was a direct effect of offering payment on sentencing verdicts such that participants offered a financial incentive were less likely to vote for death, but there was not a total effect. There were no interactions involving the financial incentives variable; Frick et al. (1999) also did not find any main effects or interactions.

Privacy and Anonymity

Cho and LaRose (1999) identify four types of privacy that are involved in Internet-based research. First, physical privacy is an individual's sense of freedom from unwanted intrusions of sight, sound, or contact. Web-based research projects may invade a person's physical privacy if unsolicited emails are sent in the effort to solicit participants, which may be more intrusive than receiving unsolicited postal mail (Sorkin, 2001). The "interactional privacy" of groups may also be invaded if emails are trolled from online groups like newsgroups. The ethical question of whether this is permissible or desirable has not been answered, though it has received some attention (Cho & LaRose, 1999; Thomas, 1996). The exact legal situation of unsolicited emails that are not commercial is also unclear (Kelin, 2001). Next, individuals want to maintain control over both their personal information ("informational privacy") and the psychological or emotional states that may be indicated by such information ("psychological privacy") (Cho & LaRose, 1999). Keeping personal information such as name, address, social security number or email address safe is an important concern for many Internet users (Cho & LaRose, 1999; D. O'Neil, 2001).

Infringement upon participants' privacy may deter some from beginning or completing the experiment for several reasons. Participants may become angry, may fear that personal information could be intercepted, or may be concerned that their data could

be associated with them, thus reducing anonymity (Joinson, 1999). However, email

solicitations may be needed to reach targeted populations or to collect data quickly. Also,

collecting personal information may be essential. For example, name, address and social

security number may be required in order to pay participants by a State-issued check.

Also, collecting participants' email addresses helps to control against multiple responses.

Provoking participants' privacy concerns in a Web-based study may alter the

demographic distribution of the sample. Generally speaking, Caucasians are less

concerned about privacy on the Internet than non-Whites, men less concerned than

women, and those with higher incomes less concerned than those with low incomes (D.

O'Neil, 2001). If concerns about privacy for mail surveys hold as well for Web-based

studies, then older people and more-educated individuals may also be less concerned

about privacy (see Singer, Mathiowetz & Couper, 1993). Whether these demographic

differences translate into motivational, attitude, or personality differences has rarely been

investigated. While administering a personality survey using paper and the Web, Joinson

(1999) manipulated whether participants were instructed to put their name at the

beginning of the survey. The author found that anonymous undergraduate participants

reported lower social anxiety and social desirability and higher self-esteem. Other

research in computer-mediated communication has found that Web users may be less

publicly self-aware (i.e., concerned about others' opinions) and more privately self-aware

(reflective of own thoughts) (Matheson & Zanna, 1988).

It has been shown that anonymity can influence behaviors such as aggression and

helping, and group behavior. Also, Hazelwood and Brigham (1999) showed that

anonymous juries were more likely to convict and were more punitive than non-

anonymous juries. Anonymity (or participants' perceptions of it) and assurances of confidentiality also influence responses to research questions, but most findings that participants are more likely to respond or respond more honestly when anonymous and/or with promised confidentiality come on responses to sensitive, personal information (see, e.g.. Ong & Weiss, 2000; Rasinski, Willis, Baldwin, Baldwin, Yeh, & Lee, 2000; Singer, von Thurn & Miller, 1995; Wildman, 1977).

Two studies have investigated effects of an anonymity or privacy manipulation in Web-based studies. Frick et al. (1999) manipulated whether participants were asked to provide personal information (demographics of gender, age, and nationality and their email address or telephone number) either before beginning the study or after answering the dependent variables. The authors found that dropout was higher when the personal information was requested at the end. However, it was not reported whether there was an initial difference in the number of participants who began the study. There were also not a main effect or interactions involving the personal information manipulation. O'Neil and Penrod (2001) manipulated whether participants were asked to provide their email address before beginning the study was manipulated. Those asked to submit their email addresses were less likely to move on to the second page of the study, but then those who were not asked to submit their addresses were less likely to move on to the third page containing the research materials. In other words, there was an initial difference in percentage that went to the second page but then the difference "evened out." It is possible that asking participants to enter an email address simply hurried the departure of those who were not motivated to participate and the rest did not continue beyond the second page. There was no main effect or any interactions involving this variable on

sentencing verdicts.

Web site and page design

There are numerous technologies and software programs that can be used to serve a Web site and design its Web pages. In addition, any Web page can be designed in a great number of ways; researchers must choose how many pages there will be, in what order they will be presented, what will be on each page, and how each page will be organized. Participants will use a wide variety of computers (memory, processing speed and screens), browsers in different versions, operating systems, and methods of connecting to the Internet. All browsers, systems or connections handle or present Web pages differently, if they are able to do so at all. Each choice that a Web-based researcher makes in designing the Web site and pages could affect the dropout rate and/or the responses given.

The present research focuses on how the tables on pages of stimulus materials or dependent measures are designed because it can widely differ between studies. Tables, if used at all, can vary in terms of size, location, order, use of graphics or colors, and which form elements (radio buttons, check boxes, drop-down menus) are used. Text and form elements may also appear differently depending on the participant's screen resolution and browser. If the screen is too small for text or tables that do not wrap to the next line, part may appear off the side of the screen, requiring the participant to scroll in order to read or answer the question.

Different organization or presentation of stimulus materials and questions may have an effect on response rate and ultimately on responses. Survey researchers have extensively investigated questionnaire design (Dillman, 2000; Krosnick, 1999), and

similar concerns apply to Web-based research. Dillman, Tortora, Conradt and Bowker

(1998) found that a survey with a "fancy" design (that used HTML tables to organize

questions, alternating colors and graphics within those tables, with answer boxes on the

extreme right side of the table; 959kB in size and 682 seconds to transmit over a 14.4k

modem) was completed by significantly fewer respondents who took more time to do so

than those who received a "plain" design (no tables nor color, with the answer boxes on

the left side; 317kB and 225 seconds). Research has also demonstrated how table design

can influence responses in paper surveys; table design can be related to mean values and

measurement error (e.g., Dillman, 2000; Krosnick, 1999). However, similar variables

have not been included in previous Web-based research. In Web-based experiments, table

design may affect responses but should not interact with other manipulations.

<p align="center">Informed Consent over the Web</p>

The unique ethical aspects of conducting research over the Internet have received

some attention, but no decisive protocols have emerged (Azar, 2000; Nosek, Banaji, &

Greenwald, 2002; Thomas, 1996). Ethical concerns include the representativeness of

Web samples, whether valid data are collected, protecting participants' privacy, and

ensuring an adequate debriefing. Researchers must also be concerned with telling

participants the necessary information regarding the benefits and risks associated with the

study, ensuring that participants understand that information, and obtaining voluntary

consent in light of that information (Frankel & Siang, 1999). The present research

focuses on these latter concerns and different methods of obtaining informed consent. Of

course, there is no question that informed consent should be obtained for all studies.

Web-based researchers have suggested and used several different methods of

obtaining informed consent. Generally, participants "sign" an informed consent form by clicking on the button at the bottom of the page to continue on to the first page of the experiment, thereby giving implied consent. A few other methods have been suggested, but not utilized by Web-based researchers. Smith and Leigh (1997) proposed that participants read the consent form and then click a button, but then the Web site would provide them with a password or ID number either on a new page or in a new pop-up window. Participants would then have to enter this password under a statement informing them that they are giving consent by typing it in. Alternatively, the authors suggested having the participants submit their email address and the server would then automatically email the participant the password or ID number. There has been no previous research regarding different methods of obtaining informed consent, in terms of dropout rates, sample characteristics, or results.

Other Methodological Variables

The present research did not manipulate any other methodological variables, but did measure some variables that could differ across participants. As noted by several authors (Reips, 2000; Nosek et al., 2002), participants participate in Web-based research under a wide variety of conditions. It is possible to measure some of these variables, such as day of the week, what Web page referred participants to the study, and participants' browser type and operating system. O'Neil and Penrod (2001) found that participants who participated on the weekend were less likely to drop out than participants on weekdays and were sensitive to a manipulation of the defendant's future dangerousness while participants on weekdays were not. Participants on the weekend may have more time and thereby give more effort toward completing the study and understanding the

stimulus materials. The time of the day at which participants participate may also be relevant, but is more difficult to measure. The concern about different solicitation methods producing different samples applies as well to different referring pages. Finally, browser type and operating system may affect how Web-based studies appear to participants, if the study works at all.

Substantive Variables

Although the present research was primarily directed at investigating the effects of methodological variables, each study also manipulated several substantive variables. Each study addressed a different area of research in psychology and law, enabling some comparisons across substantive domains.

Opening Statements

Previous research has debated whether jurors reach a verdict in a case as early as after the parties' opening statements, if not before. Although jurors may state in interviews that they were not influenced by opening statements (e.g., Hans & Sweigart, 1993), some empirical research has shown that the content and structure of opening statements can affect verdicts (Pyszczynski, Greenberg, Mack, & Wrightsman, 1981; Pyszczynski & Wrightsman, 1981). However, Linz, Penrod and McDonald (1986) analyzed opening statements in actual trials and did not find a relationship between the content or structure of opening statements and verdicts.

Pennington and Hastie's (1986; 1992) research into their "Story Model" of jury decision-making suggests how opening statements could influence processing of evidence and ultimately verdicts. The model proposes that jurors, as they hear the evidence that is presented to them during a trial, actively construct a whole story out of

the parts by imposing a narrative structure. A juror next combines the judge's instructions and prior knowledge about crime categories to form conceptual units (concepts) about their verdict choices. Then, the juror categorizes the story by comparing the features of the story to those of the verdict category just formed, and reaches a verdict. A clearly structured opening statement may lead jurors to construct a story in line with the opening statement, thus leading to favorable outcomes for that side. Similarly, a poorly organized opening statement could lead the juror to construct a story favoring the other side.

Death Penalty Jury Decision-Making

Several programs of research, including one at the University of Nebraska, have directed much attention to jury decision-making in the sentencing phase of capital cases. Some of this research has investigated attorneys' and jurors' use of aggravating and mitigating factors, including what evidence is considered mitigating by jurors, 'mitigating' being defined as reducing the likelihood that a defendant is sentenced to death (for a review see Brank, Studebaker, Garven, Patry & Penrod, 2001). Research has shown that some but not all evidence proffered as mitigating in fact has a mitigating effect on jurors' decisions.

Similarly, courts do not consider all proffered mitigating evidence admissible and relevant. Under *Lockett v. Ohio* (1978), jurors cannot "be precluded from considering as a mitigating factor any aspect of a defendant's character or record and any of the circumstances of the offense that the defendant proffers as a basis for a sentence less than death." (p. 604) limited only by the requirement that the evidence be relevant to the above aspects. Nonetheless, some jurisdictions (e.g., Texas and Arizona state courts and the federal Fifth Circuit Court of Appeals) require that the proffered evidence "excuse or

explain the commission of the offense" (*Mines v. State*, 1992, p. 951). Further, there must

be explicit evidence in the record linking the defendant's background with the murder.

Without this "nexus," these courts hold that evidence cannot be mitigating (for a review,

see Havens-Cortes, 1993). However, jurors may perceive mitigating evidence as

explaining or excusing the defendant's actions or may just feel a sympathetic reaction to

the defendant's situation, or possibly any combination of the two (Crocker, 1999). It is

not clear that jurors require a link between the evidence and the murder before allowing

evidence to influence them to not sentence a defendant to death.

<center>*Repressed Memory Cases*</center>

There is a very strong debate over repressed and recovered memories, both over

the existence of repression and over the ability of lost memories to be accurately

recovered through therapeutic techniques (e.g., Brown, Scheflin, & Hammond, 1998;

Loftus, 1993). Relatively less research has investigated how jurors react to cases in which

some party or witness has repressed and/or recovered a memory. Some research has

found that jurors are skeptical of repressed and recovered memories in terms of lower

witness credibility and/or fewer favorable verdicts (Key, Warren, & Ross, 1996; Loftus,

1993) but other studies have not (Golding, Sanchez, & Sego, 1999; Tetford & Schuller,

1996). Previous studies have not addressed how jurors react to spontaneously recovered

memories (i.e., memories that were repressed but not recovered in therapy). Also, the

simulations have consistently focused on child sexual abuse cases but no research has

explored whether the type of abuse moderates mock jurors' reactions to repressed and

recovered memories.

Jurors and Cost-Benefit Analyses

The weighing of costs and risks against benefits and utility, generally called cost-benefit analyses (CBAs), pervades tort law. The Restatement's recent formulation of the negligence standard explicitly measures reasonableness and negligence of an act in terms of "the foreseeable likelihood that it will result in harm, the foreseeable severity of the harm that may ensue, and the burden that would be borne by the actor and others if the actor takes precautions that eliminate or reduce the possibility of harm" (Proposed § 4). Product liability law also had developed to now incorporate CBAs. The Third Restatement of Products Liability now states "a product is defective in design when the foreseeable risk of harm posed by the product could have been reduced by the adoption of an reasonably alternative design and the omission of the alternative design renders the product not reasonably safe" (§ 2b). As the standard incorporates reasonableness it also incorporates a CBA.

Jurors, when making a decision about whether an action is unreasonable and thus negligent or whether a product is "not reasonably safe" and thus defective, are also performing a CBA, consciously or not. One manner of examining what variables are considered by jurors in their CBAs is to examine how jurors react to other people's CBAs. The present research investigates how jurors react to companies' CBAs in the context of a product liability case. In such cases, companies can perform CBAs to determine whether to change the design of a product before it is manufactured or sold and whether to change or recall a product that had been found to be defective after it has been sold. In such cases, if the cost to change or recall the product is more than the cost to settle lawsuits arising from injuries caused by the defective product, the company will not

change or recall the product.

Previous research (Viscusi, 2000; 2001) suggests that jurors react negatively, with outrage, when companies perform CBAs. Viscusi (2000) found that if a company performed a CBA there was an increased probability of punitive damages as well as a marginally significant increase in the magnitude of the awards. Jurors may be reacting with outrage to the company putting a price on life and/or using that value in a CBA (see MacCoun, 2000; Fiske & Tetlock, 1997; Tetlock, Kristel, Elson, Green, & Lerner, 2000).

Study 1

The first study sought to replicate the effects of sample type and financial incentive found in previous studies (Frick et al., 1999; O'Neil & Penrod, 2001) in a different context and to explore two new methodological variables---method of obtaining consent and design of the Web pages. All manipulated methodological variables were hypothesized to affect dropout rates, such that non-students, those offered a financial incentive, those who only had to click a button to give consent, and those who viewed simple tables would be less likely to drop out. Day of the week was also predicted to affect dropout rates but browser type and operating system were not. Only sample type and table design were hypothesized to be related to attitudes, such that non-students would show more variance with no mean difference, but simple tables would be related to higher means and decreased error variance. Methodological variables were not hypothesized to be related to verdict preferences. The substantive variables involved in this study involved opening statements in civil trials. It was hypothesized that opening statements organized according to Pennington and Hastie's (1986) Story Model would be more influential and poorly-organized opening statements would be less influential than a control condition. Negative attitudes toward the civil jury system and toward attorneys were hypothesized to be favorable for the defendant. Interactions between variables on dropout rates and results were not hypothesized but were examined in exploratory analyses because little prior research has addressed such interactions.

Method

Web Study

There were two samples of participants for this Web-based study---University of

Nebraska-Lincoln undergraduates and non-students (or at least non-Nebraska students). Non-students were solicited through links on four Web pages that collect such links, maintained by the Social Psychology Network (http://www.socialpsychology.org/expts.htm), American Psychological Society (http://psych.hanover.edu/APS/exponnetlist.html), the Web Experimental Psychology Lab (http://www.genpsy.unizh.ch/Ulf/ Lab/WebExpPsyLab.html), and the University of Nebraska-Lincoln's Psychology Department (http://psych.unl.edu/psychlaw/research.asp). Data were collected through these links from January 25 to June 9, 2001. Undergraduates participated from February 1 to April 13, 2001.

A mockup to this Web study is available at http://psych.unl.edu/psychlaw/oldstudies/arguments/study.asp. The first page seen by participants introduced the study and gave general instructions. For non-students, the study manipulated whether participants were offered a chance at payment through a lottery for prizes of $50, $25, and $10 or not offered any financial incentive. Participants offered a lottery were not told their odds of winning. Since undergraduates participated for course extra credit, undergraduates were not shown the first Web page.

The second Web page presented to participants contained the text of the consent form and seven questions about the participants' demographic information. This study manipulated how participants gave informed consent to participate in the study. Half the participants were required to (and given instructions to) click one button below the consent form text to receive a passcode, which they then entered in a box at the bottom of the page before clicking another button to begin the study. The other half only had to click the button at the very bottom of the page to imply consent and begin the study.

Also, this study manipulated the format and appearance of the tables that contained the demographic questions. The "complex" design had one table for each question, with alternating color backgrounds and specified to be a size 120% of the width of participants' monitor screens (which would extend off all participants' screens), and for items asking for a Likert-type rating there was a number from 1 to 9 above each radio button and the response designation (e.g., "strongly agree") appeared at the far ends of the radio buttons. The "simple" design had several items within each table (grouped according to a shared response metric), used only a gray background for the head of the table where the response choices appeared, and had a fixed width of 600 pixels, which should fit on all participants' monitors. This manipulation appeared on this page in the design of the tables containing the questions about demographics.

The third Web page contained 15 questions that asked participants to rate on nine-point Likert-type scales whether they agreed with certain statements measuring attitudes about attorneys' and judges' behavior and general problems with the civil jury system (e.g., that jury awards are too large). The manipulation of the format of the tables was repeated here with these tables.

The fourth Web page gave participants a one-paragraph description of the case: the teenaged plaintiff died when a tree that he was cutting down outside the defendant's house unexpectedly fell on him and killed him. Next, there appeared the text of the plaintiff's opening statements in the case, in which the plaintiff argued that the defendant was negligent because he did not adequately supervise and protect the younger plaintiff as he watched the plaintiff cut down the tree. The study manipulated the style in which the statement was organized, as participants received either a "bad" version that was

poorly organized and made unsupported, extraordinary claims; a "good" version that was organized according to Pennington and Hastie's (1986) Story Model to make it easier to understand; or a control version that was neither bad nor good. Participants then answered questions, again on nine-point Likert-type scales, evaluating the attorney on several dimensions (e.g., credibility, persuasiveness, intelligence) and a few items about how well the participant understood the opening statement. Participants were then asked if they had to reach a verdict at that point for whom would it be, to rate how certain they were of that verdict, and to rate in percent (in ten-point intervals) how responsible each party was for the "injuries and damages." The manipulation of the format of the tables was repeated here with these tables.

The fifth Web page presented the defense attorney's opening statement, which asserted that the plaintiff was responsible for his own injuries. The style of the defense's statement was manipulated in the same manner as the plaintiff's: participants received either a "bad," "good," or control version. Participants answered the same questions as they had after the plaintiff's opening statement. The manipulation of the format of the tables was repeated here with these tables.

The final Web page of the study gave participants who were offered a chance at payment through a lottery the opportunity to enter their personal information and gave all participants debriefing information.

Other studies

Another researcher (Robert Ray) conducted two other studies on the same topic previous to the Web-based study. Data were collected from 135 University of Nebraska-Lincoln undergraduates on a paper-and-pencil version and from 28 undergraduates on a

Web-based version. However, both these versions were much longer than the Web-based study described above. That is, participants were presented opening arguments from both sides in three cases, one of which was the same used in the second Web-based study, and evaluated the attorneys and gave verdicts after each argument. There were also a series of additional questions about participants' knowledge of the legal system that were presented with the initial attitudinal questions and several manipulation checks after all opening statements had been presented, all of which was omitted from the second Web-based study.

The data for the case scenario shared with the Web-based study were extracted from the previous data sets in order to investigate whether the studies reached similar conclusions about the substantive variables. The data from the previous studies were used only in the analyses of verdict preferences, described below, because the several differences between versions introduced confounds into any potential comparison between data collected using paper-and-pencil and over the Web.

Results

The Database

There were 870 entries in the database for the non-students. 200 entries identifiable as search engines, robots, or spiders were deleted. The database was also examined for duplicate entries from the same individual, as judged by looking for the same IP address (at least the first three numbers) and same browser information over the course of the study, with exceptions for entries for AOL users, who share dynamically assigned IP addresses. 81 entries were identified as duplicate entries of the same individual and were removed, leaving only one entry per individual. Judging from the

timing of the entries most of these entries came from participants who for some reason reloaded the first page or who returned to the beginning of the study after completing it. There were only 4 instances identifiable as a participant attempting to complete the study multiple times. This left 589 unique entries in the database.

There were 301 entries in the database for undergraduate participants. Twenty-three duplicate entries were removed, leaving 278 unique undergraduate participants who visited the first page of the study.

Preliminary Analyses

The attitude items concerning the civil jury system and attorneys, which appeared on the third Web page, were factor analyzed. Specifying a principal components extraction of factors with eigenvalues greater than one and Varimax rotation, four factors were revealed. However, because certain items had high loadings on two factors (one encompassing attitudes about whether lawyers treat other parties with respect and the other attitudes about whether lawyers treat other people fairly) and computed factors scores were highly correlated ($r = .59$), those two factors were combined into one factor, measuring attitudes favorable beliefs about attorneys (four items were retained, $\alpha = .87$). Another factor measured negative impressions of certain parts of the civil jury system (e.g., "Attorneys help their clients sue for false claims" and "Americans are too willing to pursue lawsuits"; 4 items, $\alpha = .79$). The final factor contained only one item measuring beliefs specifically about juries: "Jury awards are too large."

The rating items that appeared after the plaintiff and defendant's opening statement were also factor analyzed, separately. Specifying a principal components extraction of factors with eigenvalues greater than one and Varimax rotation, four factors

were revealed. Two multi-item factors measured participants' evaluation of the attorney (6 items, including those measuring trustworthiness, credibility and likeability; α = .91 for plaintiff's attorney ratings; α = .93 for defendant's attorney ratings) and participants' evaluation of the comprehensibility of the opening statement (3 items; α = .81 for plaintiff's statement; α = .77 for defendant's statement). The other two factors were had only one item and measured sympathy toward the plaintiff and sympathy toward the defendant.

Demographic Analyses

Analyses examined whether the demographic characteristics of the non-student sample that finished the study (n = 114) differed across level of the methodological variables. The relevant demographic characteristics were age, gender, ethnicity, educational background, and location (categorized as either East, South, West, Midwest, or a foreign country). Overall, the completed sample of non-students were mostly female (64.0%), Caucasian (82.5%), had not yet graduated college (64.1% [probably only half of these were students, as measured by having some college and being under 24 years old]), were roughly evenly distributed across locations, and with a mean age of 30.4 (range from 18 to 67 years old). Chi-square analyses showed no pattern of relationship between gender, ethnicity, and educational background and the methodological variables. There was a higher percentage of participants from foreign countries in the condition not offered payment through a lottery (42.5% vs. 19.4%, $\chi^2(4)$ = 9.89, p = .045). There was no mean difference in age across levels of the methodological variables.

Attitudinal Analyses

Analyses investigated whether the methodological variables in the Web-based

study were related to participants' attitudes toward the civil jury system and attorneys. One-way ANOVAs were conducted to explore mean differences on participant's satisfaction with the judicial system as measured on the second Web page, the individual attitude items from the third Web page, and the two multi-item factors derived from those items. Differences in the amount of variance were also tested using Levene's test for homogeneity of variance. Non-students were more satisfied with the judicial system ($M = 5.28$ vs. 4.49, $F(1,350) = 19.58$, $p < .001$) but also had stronger negative impressions of the civil jury system ($M = 21.21$ vs. 19.55, $F(1,350) = 4.34$, $p = .038$). There was a significant difference in the amount of variance between non-students and undergraduates on three items but there was no pattern, as there was more variance among undergraduates on two items but more among non-students on one item.

Analyses showed no mean difference on the items or factors between whether participants participated on the weekend or a weekday, but there was significantly more variance on three items among participants on the weekend. There were no mean differences nor differences in variance related to whether non-students participants were offered payment or the method of obtaining informed consent. There were also no mean differences on items or factors related to the design of the tables on the Web pages (large, colorful tables vs. simple, colorless tables) but there was significantly more variance in 5 items and one factor (the one measuring negative impressions) when the items were presented in the large, colorful tables.

Scale structure analyses.

Multiple-group confirmatory factor analyses were conducted to test measurement invariance for the attitude items on the third Web page of the study (2 factors with 4

items each) between undergraduates and non-students and between participants who viewed each version of the tables in the study. Such analyses expand beyond tests of homogeneity of variance to investigate whether items measure the same latent variable in different groups. Differences in factor structure or factor loadings indicate measurement variance. Multiple-group analyses can also test for differences in the amount of variance in the latent variable, similar to other tests of variance equality, and also for differences in the amount of unexplained variance (or measurement error) for each individual item. Generally, if factor loadings and factor variances are invariant but there are differences in items' total variance between groups, this will be reflected in differences in measurement error (see Kline, 1998).

A preliminary confirmatory factor analysis of the scale structure was performed to identify an appropriate model specification. The fit indices of χ^2, root mean square error of approximation (RMSEA), standardized root mean square residual (SRMR), CFI and NNFI were used to evaluate the fit of the model. The χ^2 index measures the degree of lack of fit (as measured by the difference between the population and the fitted covariance matrices) multiplied by (n-1) and is tested for significance. The RMSEA is a standardized measure of the lack of fit between the reproduced and the population's covariance matrix per degree of freedom: a value of .05 indicates a close fit but a value under .08 shows a reasonable fit. The standardized RMR is the square root of the average standardized residual from the reproduced matrix and a value less than .05 indicates a close fit. The CFI compares the lack of fit of the tested model to the lack of fit of a baseline, null model, accounting for degrees of freedom of each model, and a value over .95 indicates a close fit. The NNFI is the ratio of the difference between the tested model

and the baseline, null model to the difference between a perfectly-fitting model and the null model that takes degrees of freedom for each model into account, and a value over .95 indicates a close fit.

The analysis revealed that a model specified with each item loading only on the latent factor associated with it from the exploratory factor analysis produced a poor fit to the data ($\chi^2(19) = 355.78$, $p < .001$; RMSEA = .20; NNFI = .67; CFI = .77; SRMR = .10). Modification indices suggested adding an error covariance between two items in the factor measuring favorable beliefs toward lawyers ("Attorneys desire to treat people in a fair and reasonable way" and "Attorneys make an effort to treat people fairly"). Such a modification resulted in a closer fit to the data ($\chi^2(18) = 82.72$, $p < .001$, RMSEA = .098, NNFI = .93; CFI = .96; SRMR = .065). As such, this modified model was used in the multiple-group analyses. The multiple-group analysis was performed by starting with a model in which all parameters were held invariant across groups and relaxing parameter types one at a time. Items loadings were allowed to vary across groups first, then factor variances, then factor covariance, then the item error variances, and finally the item error covariance. A significant change in χ^2 indicates that the parameters are not invariant across groups. In such a case, the computed parameter values and the modification indices were examined to locate the specific parameters that were not invariant across groups.

Analyses revealed that there were no differences between undergraduates and non-students on factor loadings or factor variances. There was a difference between groups on the factor covariance ($\Delta \chi^2(1) = 10.77$, $p < .05$) in that the covariance among non-students tended to be negative (-.71, $z = -1.27$) but the covariance among

undergraduates was positive (.98, z = 3.49) . There was also a significant difference on

the item error variances ($\Delta \chi^2(8)$ = 19.83, $p < .01$). Three items, all part of the factor

measuring favorable beliefs toward lawyers, had significantly more measurement error in

the non-student sample as compared to the undergraduate sample. Finally, there was a

significant difference between groups on the item error covariance ($\Delta \chi^2(1)$ = 6.56, $p <$

.01) such that the error covariance was stronger among non-students than undergraduates.

A separate multiple-group analysis revealed no differences in factor loadings,

factor covariance, or item error covariance between the scale structures from items on the

two different table versions. There was a significant difference between groups on the

factor variances ($\Delta \chi^2(2)$ = 6.23, $p < .05$) in that there was more variance in the factor

measuring negative impressions of the civil jury system among participants with the

colorful, large tables. There was no difference in variance in the other factor, however.

Also, there was a significant difference between groups in the item error variances (Δ

$\chi^2(8)$ = 51.82, $p < .01$). Three items contained significantly more unexplained variance

when presented in the colorful, large table, while one had more unexplained variance

when presented in the simple, smaller table. However, this latter effect was much smaller

than the other three.

Additional multiple-group confirmatory factor analyses were conducted to test

measurement invariance of the items after the plaintiff's opening statement differed

between undergraduates and non-students and between participants who viewed each

version of the tables in the study. Preliminary confirmatory factor analyses of the scale

structure revealed that a model specified with each item loading only on the latent factor

associated with it from the exploratory factor analysis. This model produced a poor fit to

the data ($\chi^2(26)$ = 206.70. p < .001; RMSEA = .15; NNFI = .85; CFI = .89; SRMR = .069). However, because no modification index was substantially greater than others, the model was not revised for the multiple-group analyses.

Analyses revealed no differences between undergraduates and non-students on the factor variances, the factor variance, or the item error variances for the scales after the plaintiff's statement. There was a significant difference between groups on the item loadings on the factors ($\Delta \chi^2(7)$ = 14.45, p < .05). This difference was attributable to the item "The attorney's presentation got at the truth" having a higher loading on the factor evaluating the attorney among non-students than undergraduates.

Separate analyses revealed no differences between undergraduates and non-students on the item loadings, the factor variances, or the factor variance for the scales after the defendant's statement. There was a significant difference between groups on the item error variances ($\Delta \chi^2(9)$ = 25.54, p < .01). There was more measurement error in the non-student sample on five items as compared to the undergraduate sample.

Also, analyses revealed no differences in the factor covariance or item error variances between the scale structures from items after the plaintiff's opening statement on the two different table versions. There was a significant difference between groups on the item loadings ($\Delta \chi^2(7)$ = 15.52, p < .05) such that the loadings of two items (ratings of the lawyer's likeability and whether the statement was "difficult to understand") were higher among participants who received the simple, colorless tables. There was also a difference between groups on the factor variances ($\Delta \chi^2(2)$ = 7.04, p < .05) in that the was more variance in the factor evaluating the attorney among participants who viewed the simple, colorless tables.

Separate analyses revealed no difference between table versions in the item loadings. factor variances or factor covariance of the scales after the defendant's opening statement. There was a significant difference on the item error variances ($\Delta \chi^2(9) = 22.35$, $p < .01$). There was more unexplained variance in two items when they appeared in the simple. colorless tables and more variance in one item when it appeared on the large. color tables.

Dropout Analyses

As noted above. there were 589 unique non-student participants who reached the first page of the study. 384 (65.2%) participants continued beyond the first page. Of those who reached the consent form page. 184 (47.9%) began the study and reached the page with the attitudinal questions. Of those. 159 (86.4%) reached the plaintiff's opening statement. Of those. 126 (79.2%) continued to the defendant's statement. Of those. 114 (90.5%) finished the study. Overall. only 19.4% of non-students who visited the first page of the study finished it.

There were 278 unique undergraduates who visited the first page of the study. which for them was the page with the consent form text. Of those who reached the consent form page. 200 (71.9%) began the study. Of those. 193 (96.5%) finished the attitude questions. Of those. 188 (97.4%) reached the defendant's opening statement. Of those. 186 (98.9%) finished the study. Overall. 66.9% of undergraduate participants who visited the first page finished the study.

Server-side scripting (see Schmidt. 2000) was used to detect and record the browser that the participant used to view the study and the operating system of the participant's. Participants' browsers were categorized as: Internet Explorer, Netscape.

AOL, or "other/unknown". Operating system categories were: Windows 95 or earlier; Windows 98; Windows ME, 2000 or XP; Windows NT; Macintosh; or other/unknown. Preliminary chi-square analyses were conducted to investigate whether each dependent variable described more fully below was related to participants' browser type or operating system. For this study, there was no significant pattern of relationship between browser type or operating system and dropout rates at any point in the study. These variables were not included in the subsequent multivariate analyses.

Since payment was not manipulated for the undergraduate participants, the analysis of whether those who reached the first page of the study began the study included only the non-student sample. Whether the participant was offered payment (offered payment coded as 1) and a dichotomous variable representing whether the participant visited the site on a weekend (on a weekend coded as 1) were used as predictors. Logistic regression analyses revealed no effect of payment (65.8% of those offered payment began vs. 64.5%, $B = .06$, $p = .736$) or of day of the week (62.4% of those on the weekend began vs. 66.0%, $B = -.16$, $p = .439$). The interaction also was not significant.

Now using the entire sample, the two other manipulated methodological variables (appearance of tables (simple, colorless version coded as 1) and method of obtaining consent (using a password coded as 1)) and a variable indicating sample type (non-students coded as 1) were added as predictors of whether the participants who reached the Web page with the consent form continued to the study's third page. The only significant main effect was that non-students were much less likely to continue (47.9% vs. 71.9%, $B = -1.20$, Wald $= 35.35$, $p < .001$). Analyses also tested the nine two-way interactions

among sample type and methodological variables, and three approached significance.

First, more non-students continued on when presented with simplified tables (51.3% vs. 44.4%) but, unexpectedly, fewer undergraduates continued on with the simplified tables (66.4% vs. 77.3%) (B = .80, Wald = 3.75, p = .053). Second, participants on the weekend were more likely to continue with simplified tables (70.7% vs. 54.8%) but there was no such relationship on weekdays (54.7% vs. 59.3%) (B = .84, Wald = 3.40, p = .065). Third, non-students not offered payment were less likely to continue if a password had been required to give consent (40.5% vs. 47.4%) but those offered payment were, unexpectedly, more likely to continue if a password was required (57.1% vs. 46.2%) (B = .77, Wald = 3.26, p = .071).

The demographic information collected on the consent form page (age, gender, educational background, region or country, and satisfaction with the civil jury system) was added as predictors of whether those who gave consent and began the study completed the page of attitudinal questions (n = 384). Again at this point, adults were marginally less likely to continue (86.4% vs. 96.5%, B = -1.24, Wald = 3.81, p = .051). The only other significant main effect was that participants who viewed the simplified tables were more likely to continue (94.8% vs. 88.6%, B = .87, Wald = 4.24, p = .039). The same interactions as above were tested, and offering payment again interacted with the method of giving consent, but the pattern of the interaction here was the opposite of before---non-students not offered payment were more likely to continue if a password had been required (91.2% vs. 80.4%) and there was little difference among those offered payment (85.7% vs. 89.6%) (B = -3.01, Wald = 5.98, p = .014).

There were two more Web pages in the study. Analyses did not focus on dropout

at each point separately because of the 52 participants who reached the plaintiff's

opening statement but dropped out before finishing most (38) did so before finishing the

page with the plaintiff's statement, thus not allowing analyses of dropout during the

defendant's statement. Thus, the next analyses focused on whether those who reached the

plaintiff's opening statement finished the study.

The three attitude factors derived from the items on the third Web page plus

dummy codes for the manipulated substantive variables (using the control version of the

statements as the comparison condition) were added as predictors of whether those who

reached the plaintiff's opening statement finished the study. Of the 22 predictors, 5 main

effects were significant. Again, non-students were less likely to finish the study (62.0%

vs. 93.0%, $B = -1.87$, Wald $= 5.54$, $p = .019$). Older participants were more likely to

finish the study ($B = .06$, Wald $= 4.21$, $p = .040$). Compared to college graduates without

graduate training ($n = 26$), participants who had not completed high school ($n = 9$) were

less likely to finish (22.2% vs. 80.8%, $B = -4.63$, Wald $= 9.94$, $p = .002$). Compared to

residents of Midwestern states (as categorized by the present author $n = 29$ non-students),

participants from foreign countries ($n = 51$; 56.9% vs. 86.2% of non-students, $B = -1.76$,

Wald $= 6.03$, $p = .014$) and from Western states ($n = 23$; 69.6% vs. 86.2%, $B = -1.76$,

Wald $= 4.33$, $p = .037$) were less likely to finish. The method of giving consent interacted

with the design of the tables, in that among those for whom a password was not required

those receiving the simplified tables were more likely to continue (89.0% vs. 81.3%) but

for those for whom a password had been required those receiving the simplified tables

were less likely to finish (80.0% vs. 91.3%) ($B = -2.23$, Wald $= 6.47$, $p = .011$).

A final analysis focused on the variable of whether those who reached the consent

form (n = 662) finished the study (that is, completed the next three pages). Results of this

analysis showed that, although neither variable significantly influenced dropout at any

one point in the study, overall participants on the weekend were more likely to finish the

study than those on a weekday (52.5% vs. 43.7%, B = .61, Wald = 7.61, p = .006) and

non-students who were offered payment were more likely to finish than those not (34.7%

vs. 24.3%, B = .49, Wald = 4.51, p = .034). Non-students were less likely to finish than

undergraduates (29.7% vs. 66.9%, B = -1.90, Wald = 75.55, p < .001). The interaction

term between whether payment was offered and the method of giving consent was

significant, in that non-students not offered payment were less likely to continue if a

password had been required to give consent (20.2% vs. 27.8%) but those offered payment

were more likely to continue if a password had been required (40.8% vs. 28.8%) (B = .96,

Wald = 3.98, p = .046). Also, the interaction between sample type and table design

approached significance (B = .73, Wald = 2.64, p = .104). Similar to the effect above,

non-students were more likely to continue when viewing simplified tables (34.4% vs.

24.9%), but undergraduates were unexpectedly less likely (62.8% vs. 70.9%).

Effects on Verdict Preference

Data from all three studies on this topic were combined to analyze verdict

preferences. Data from 12 participants in the Web-based study (9 undergraduates and 3

non-students) were excluded because the amount of time they spend reading the

arguments was less than 90 seconds, which indicated that the statements were not read at

all. Also, data from 29 participants who indicated that they resided in a foreign country

were excluded because these participants would not be jury-eligible. This resulted in a

final sample size of 422.

After the plaintiff's opening statement and then again after the defendant's opening, participants were asked to indicate for which party they would vote as a juror if they had to make a decision and to rate their confidence in that verdict on a Likert-type scale from one to nine. These items were combined to create a continuous scale from –9 (very confident in a verdict for the defendant) to 9 (very confident in a verdict for the plaintiff). This variable, the manipulated methodological and substantive variables, measures of participants' attitudes, and participants' evaluation of the attorneys were entered into a seven-level fully saturated path model.

The first level contained dummy codes representing seven methodological variables: whether the participant was an undergraduate or non-student; whether the data were collected from a Web-based or paper-and-pencil version; whether the data were collected from the long or short version of the study; and whether the participant participated on a weekend or weekday; and the three manipulated methodological variables from the Web-based study. The first level also contained four attitude items or factors shared among all versions of the study: a rating of general satisfaction with the courts; the factor measuring favorable attitudes toward lawyers; the factor measuring negative impressions of parts of the civil jury system; and a single item measuring a belief whether jury damage awards are too high. There were also two dummy codes representing the version of the plaintiff's opening statement received, with the control version as the comparison condition.

The second level contained the four variables constructed from the items after the plaintiff's opening statement: the factor evaluating the lawyer, the factor measuring the comprehensibility of the statement, and the two items measuring sympathy for the

plaintiff and the defendant, respectively. The third level contained only the composite variable measuring verdict preference after the plaintiff's opening statement. The fourth level contained two dummy codes representing the version of the defendant's opening statement received, with the control version as the comparison condition. The fifth level contained variables similar to those in the second level but measured after the defendant's opening statement: the factor evaluating the lawyer, the factor measuring the comprehensibility of the statement, and the two items measuring sympathy for the plaintiff and the defendant. The sixth level was the final dependent variable, the composite score measuring verdict preference after the defendant's opening statement.

The path analysis was conducted using structural equation modeling techniques in LISREL 8.3, using maximum likelihood estimation and analyzing the covariance matrix. Preliminary analyses were conducted to identify significant two-way interactions involving methodological variables that could be included in the model. Analyses focused only on interactions involving methodological variables, but included interactions with the manipulated variables and the four factor scores after the plaintiff and defendant's openings. First, residualized interaction terms were computed (Cohen, 1978; Lance, 1988). Next, the main effect variables and interaction terms were entered into a hierarchical multiple regression. Such analyses showed that at no point were there more significant interactions than would be expected by chance at the .05 level, thus no interactions were retained in the model.

Methodological Variables.

There were no main effects of any methodological variable on mock jurors' verdict preference, either after the plaintiff's or the defense's opening statement. In fact,

there were only four significant total effects of any methodological variable on any

endogenous variable. Non-students gave harsher evaluations of the plaintiff's attorney (M

= 32.1 out of 45 vs. 35.1 for undergraduates across all versions of the study; total effect =

-.22, z = -2.70). Since evaluations of the attorney were related to verdict preference (see

below), this translated into an indirect effect of sample type on verdict preference after

the plaintiff's opening statement (indirect effect = -.11, z = -2.24). Also, participants on

the short, Web-based version of the study gave lower ratings of sympathy toward the

plaintiff after the plaintiff's opening (M = 5.11 out of 9 vs. 6.08; total effect = -.23, z = -

2.11) and lower ratings of the comprehensibility of the defendant's opening statement

(total effect = -.26, z = -2.40). Participants who were presented with the simple, colorless

tables gave more positive evaluations of the defense attorney (total effect = .13; z = 2.39).

Substantive Variables.

The manipulation of the style of the opening statements was somewhat successful

in terms of its influence on mock jurors' evaluations of the attorney and the statement.

Participants who received the "bad" version of the plaintiff's opening statement evaluated

the plaintiff's attorney less favorably (total effect = -.16, z = -2.98) and indicated that the

statement was harder to understand than the control condition (total effect = -.29, z = -

5.49). Participants who received the "good" version based on the Story Model found the

opening statement to be easier to understand (total effect = .17, z = 3.13) and tended to

rate the plaintiff's attorney more favorably (total effect = .10, z = 1.75). Similarly,

participants who received the "good" version of the defendant's opening statement rated

the defense's attorney more favorably (total effect = .11, z = 2.13). However, there were

no differences from the control condition in evaluations of the attorney or the opening

statement when participants received the "bad" version of the defense's opening statement.

These differences in evaluations of the attorneys and the statements did not translate into significant effects on verdict preferences, however. There was a marginally significant effect in that mock jurors' given the "bad" version of the plaintiff's opening statement were less supportive of the plaintiff (total effect = -.10, z = 1.81), but no other effect of any manipulation approached significance. Participants' evaluations of the plaintiff and defense's attorneys were significantly related to verdict preference in the expected directions (total effects = .31 and -.28, respectively), but as participants found the plaintiff's opening statement easier to understand they were unexpectedly *less* supportive of the plaintiff (total effect = -.17, z = -3.42). There was no relationship between ratings of the comprehensibility of the defense's opening statement and verdict preference.

Sympathy toward the plaintiff and defendant also was related to verdict preference. Both ratings of sympathy after the plaintiff's opening statement were related to verdict preference at that point, but only the rating of sympathy for the defendant had a significant total effect on verdict preference after the defendant's opening statement. Both ratings of sympathy after the defendant's opening statement were significantly related to verdict preference at the end (on final verdict preferences, total effects = .13 for plaintiff sympathy and -.22 for defendant sympathy).

Also, participants' verdict preference after the plaintiff's opening statement was strongly related to verdict preference after the defendant's opening statement (total effect = .63) (ratings of sympathy at the two points were also strongly related). Participants'

verdict preference after the plaintiff's opening statement was also significantly related to subsequent evaluations of the defense attorney and the defense's opening statement, in that mock jurors who were more likely to vote for the plaintiff initially gave less favorable evaluations to the attorney and found the defense's statement harder to understand (total effects = -.28 and -.16, respectively). Similarly, mock jurors who were higher in sympathy for the defendant after the plaintiff's opening statement gave more favorable evaluations of the attorney (total effect = .08).

Finally, participants' favorable attitudes toward attorneys were related to more favorable evaluations of the plaintiff and defendant's attorneys (total effect = .11 and .12, respectively) and to higher sympathy for the plaintiff after each opening statement (total effect = .13 at each point). Participants' negative attitudes about the civil jury system were also related to increased sympathy for the plaintiff (total effect = .09 after plaintiff's opening and .14 after defendant's).

Study 2

The second study investigated differences between non-student samples by recruiting participants from two sources. The study also sought to replicate effects of different methods of obtaining consent (from Study 1) and of asking for personal information at different times (Frick et al. 1999; O'Neil & Penrod, 2001). All manipulated methodological variables and day of the week were hypothesized to be related to dropout, such that participants solicited from a pre-developed pool, those who only had to click a button to give consent, those not asked for personal information until the end of the study, and those participating on the weekend would be less likely to drop out. No methodological variables were hypothesized to be related to sample characteristics or to affect the study's results. The substantive variables in this study involved capital jury decision making and jurors' consideration of purported mitigating evidence. The case facts were modeled off the facts of *Hoskins v. State* (2000), in which the Arizona Supreme Court held that evidence of mental illness and child abuse were not mitigating because there was no link established between the illness or abuse and the defendant's state of mind during the murder. It was hypothesized that evidence of child abuse and mental illness would each be mitigating without an explicit link, and that expert testimony providing such a link would have an additional mitigating effect.

Method

All participants completed this study over the Web; a mockup is available at: http://psych.unl.edu/psychlaw/oldstudies/web2/study.asp. Participants were solicited in two ways. First, the link to the study was posted on the four Web sites that collect such links. The study collected data through these links from April 12, 2000 to January 9,

2002. Second, the study recruited other non-student participants through

studyresponse.com, a Web site that has developed a pool of participants (about 1,100)

interested in participating in Web-based experiments. The developers of the Web site

contacted randomly selected participants by email and directed those who were interested

to this study. The study collected data from these participants from December 11, 2001 to

January 5, 2002. 100 solicitations were sent on December 11, 100 on December 13, 100

on December 17, and 150 on December 22, 2001. 14 solicitations were returned as

undeliverable.

The first Web page presented to participants included basic instructions and the

text of the consent form. All participants were offered a chance at payment through a

lottery with prizes of $150, $75, and $25. All participants answered four questions about

their attitudes toward the death penalty. Participants rated their support for the death

penalty on a Likert-type scale from one to nine and answered three death-qualification

questions: "Is your attitude toward the death penalty so strong that it would seriously

affect you as a juror and interfere with your ability to perform your duties?", "Do you

believe that the death penalty should be imposed in EVERY first-degree murder case?"

and "Do you feel that the death penalty should NEVER be imposed?". Three variables

were manipulated (fully crossed) on this page. First, for half the participants there was an

additional line at the end of the fifth paragraph in the consent form that told participants a

password to enter at the bottom of the page to begin the study, while the other half only

had to click the button at the bottom of the page to begin. Participants who entered the

wrong password in the box were given a message to re-read the consent form and try

again. Second, half the participants were given an additional, 15-item scale measuring

attitudes toward the death penalty (see O'Neil, Patry & Penrod, 2002) and the other half

did not fill out this scale until the fourth Web page of the study. Third, for the non-pool

participants, the study manipulated whether participants were given at this point the

option (it was not required) to enter a name, address and social security number if the

participant wanted to be entered into the lottery. However, because the participants

solicited from the StudyResponse.com pool were guaranteed anonymity when they

signed up, the study did not ask these participants to enter their names. Instead, for the

pool participants, the study manipulated whether participants were required at this point

to enter personal demographic information (including ethnicity and annual family

income).

The second Web page of the study presented a summary of the case facts, the

prosecution's evidence of aggravating factors, and the defense's evidence of mitigating

factors. The case facts described how the defendant was arrested and convicted for capital

murder after he killed the victim in order to steal her car. The prosecution argued the

aggravating factors of pecuniary gain (murder to steal the car) and heinousness (multiple

gunshot wounds). The substantive manipulations of this study appeared in the defense's

mitigation evidence. All participants received evidence about the defendant's good

behavior in prison and the defendant's age (20 years) at the time of the crime. One

manipulation was whether participants received either: (1) no evidence that the defendant

was emotionally and physically abused as a child: (2) just that evidence: or (3) that

evidence plus additional expert testimony from a psychiatrist that there is a link between

the abuse that the defendant suffered and the crime. A similar three-level variable

involved the mitigating evidence of the defendant's paranoid schizophrenia and its

relationship to the crime. The condition that presented just evidence of mental illness stated that the mental illness developed after the defendant's arrest. The expert testimony added testimony about delusions during the murder in order to link the mental illness and the crime.

The third Web page presented instructions outlining Arizona's sentencing scheme (as it is performed by judges in that state), taking the instructions as cited in the *Hoskins* case where possible. There were three multiple-choice questions about the instructions on this page. These questions are not sufficient to measure comprehension and were intended only to make participants think a little about the instructions. The fourth Web page contained the rest of the dependent and other measures. Participants first rated the presence of aggravating and mitigating factors on nine-point Likert-type scales and then gave a verdict of either death or life without parole. All participants then answered on Likert-type scales 13 questions about their beliefs about the case. Participants who had not completed the 15-item scale measuring attitudes toward the death penalty on the first Web page did so at this point. Non-pool participants solicited through links who had not been given on the first page the option of entering personal information for the lottery were given the option here, while pool participants who had not entered demographic information did so here. After submitting this information participants were presented a final page with debriefing information.

<div align="center">

Results

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The Database

There were 1,894 entries in the database for participants solicited through links. 200 entries identified as search engines, robots, or spiders were eliminated. The database

was also examined for duplicate entries from the same individual, as judged by looking

for the same IP address (at least the first three numbers) and same browser information

over the course of the study, with exceptions for entries for AOL users, who share

dynamically assigned IP addresses. 162 duplicate entries were excluded. Based on timing

of the entries most of these entries came from participants who for some reason reloaded

the first page or who returned to the beginning of the study after completing it. There

were only 2 instances identifiable as a participant attempting to complete the study

multiple times. This left 1,432 unique entries in this database.

There were 372 entries in the database for participants solicited from

studyresponse.com. There were no search engine entries; 48 duplicate entries were

removed. 324 unique entries remained in this database. This is an initial response rate of

74.3% (324/436 successful solicitations).

Preliminary Analyses

As constructed and validated by O'Neil, Patry and Penrod (2001), the scale

measuring attitudes toward the death penalty had five factors: general support, retributive

sentiments, whether the death penalty is a deterrent, whether the death penalty is cheaper

than life imprisonment, and whether a defendant sentenced to LWOP nonetheless gets

out on parole. Using data from several studies, O'Neil et al. (2001) performed a

confirmatory factor analysis and suggested a structural model with each item loading on

the factor on which it had the highest loading in the confirmatory factor analysis, plus an

additional loading of one item ("The desire for revenge is a legitimate reason for favoring

the death penalty") on the general support factor. Also, the previous analysis suggested

adding two item error covariances between items making up the general support factor.

Using the modified model suggested by O'Neil et al. (2001), another confirmatory factor

analysis was conducted on the data collected in this study (N = 464). The model

described above produced a close fit to the data as measured by common fit indices

($\chi^2(77)$ = 154.96, p < .001, RMSEA = 0.045, NNFI = 0.97, CFI = 0.97, SRMR = 0.036).

Demographic Comparison

The demographic characteristics of those participants solicited from the links on

Web sites and the studyresponse.com pool were compared. Since the demographic

questions always appeared on the verdict form page for the non-pool participants.

analyses compared the characteristics of only those participants who completed the study.

Analyses revealed that, while the entire sample was mostly female (76.1% overall), the

pool participants who completed the study had an even higher proportion of females

(83.1% vs. 70.3% of non-pool participants; $\chi^2(1)$ = 9.83, p = .002). The distribution of

ages was also significantly different between the groups, with the pool participants

tending to be older ($\chi^2(5)$ = 59.98, p < .001). For instance, there were more pool

participants between 41 and 50 years of age (23.4% vs. 7.5%) and between 51 and 60

(9.5% vs. 2.9%), and fewer pool participants were 21 years old or younger (6.5% vs.

25.1%). Also, pool participants were more likely to be married or divorced (47.3% vs.

29.3% and 13.4% vs. 4.6%, respectively [and thereby less likely to be single]; $\chi^2(3)$ =

38.23, p < .001).

Attitudinal Analyses

Analyses explored whether participants' attitudes toward the death penalty were

related to the methodological variables. Analyzing only participants who participated

after September 11, one-way ANOVAs showed that participants recruited from

studyresponse.com were more supportive of the death penalty and had stronger pro-death penalty beliefs, as indicated by significantly higher means on 12 items and 4 factor scores. Differences in the amount of variance were also tested using Levene's test for homogeneity of variance. There was more variance among participants who were solicited through links on 3 items and one factor (beliefs about relative cost of the death penalty). There were no mean differences on items or factor scores related to when the attitude scale was completed. but there was significantly more variance on six items and one attitude factor (about concerns about parole) when the items appeared at the end of the study. There was less support for the death penalty among participants on the weekend, as reflected by lower means on six items and two factor scores. and more variance on three items and the general support factor score on the weekend. There were no mean differences or differences in variance related to how informed consent was obtained or where personal information for payment was asked for (for those solicited through links) or where demographic information was asked for (for those from studyresponse.com). Finally. after September 11. support for the death penalty among those solicited through links increased (M = 21.92 vs. 18.90, $F(1,261)$ = 3.98, p = .048) as did retributive attitudes concerning the death penalty (M = 15.42 vs. 13.36, $F(1,261)$ = 5.87, p = .016). effects that were primarily driven by responses to two items---after September 11 participants agreed less that "No matter what crime a person has committed executing them is a cruel punishment" and more that "There are some murderers whose death would give me a sense of personal satisfaction."

Scale Structure Analysis.

Multiple-group confirmatory factor analyses were then conducted to test

measurement invariance in the measurement model specified by O'Neil et al. (2001)

between when the scale appeared on the first Web page and when it appeared on the last

Web page, between different participant recruitment sources (pool vs. links), and between

participants who participated before and after September 11. Since all pool participants

participated after September 11, a three-group analysis was conducted to address the

latter two issues together. That analysis found no differences in the loadings, factor

variances, or factor covariances between groups. There were significant differences in the

item error variances ($\Delta \chi^2(30) = 110.35$, $p < .001$) and the item error covariances ($\Delta \chi^2(4)$

$= 12.62$, $p < .05$). This was attributable to an increase in unexplained variance on four

items after September 11 and on two items among those solicited by links. The difference

in item error covariances was seen as a stronger negative covariance among participants

after September 11.

The multiple-group CFA that investigated differences between when the scale

appeared on the first Web page and when it appeared on the last Web page found no

differences between groups on the factor loadings, factor variances, factor covariances, or

the item error covariances. There was a significant difference between groups on the item

error variances ($\Delta \chi^2(15) = 66.90$, $p < .001$). Further analyses showed that the error

variances for six items were higher when the scale appeared at the end of the study.

Dropout Analyses

There were 1,756 unique participants who reached the first page of the study.

Only 550 participants (31.3%) answered the questions on the first page and began the

study. 520 participants (94.5% of those who began the study) went past the page with the

case facts, 485 (88.2% of those who began the study) went past the jury instructions, and

458 (83.3% of those who began the study; 26.1% of those who reached the first page)

finished the study. 219 of those who finished the study came from the studyresponse.com

pool, for a completion rate of 50.2% (219 out of 436 unreturned solicitations)

Again, server-side scripting was used to detect and record the browser that the

participant used to view the study, the operating system of the participant's computer,

and the referring page to the study (i.e., the page that contained the link to the study).

Participants' browsers were categorized as: Internet Explorer, Netscape, AOL, or

"other/unknown". Operating system categories were: Windows 95 or earlier; Windows

98; Windows ME, 2000 or XP; Windows NT; Macintosh; or other/unknown. In addition

to the four Web sites noted above that posted the link, referring pages included search

engines (mostly Google (www.google.com)), a few university pages that posted a link to

the study, and other unidentifiable referrers. The referring source of participants from the

studyresponse.com pool was generally the participants' email program. Preliminary chi-

square analyses were conducted to investigate whether each dependent variable described

more fully below was related to participants' browser type, operating system, or referrer.

Analyses of referrer did not include pool participants. Unless otherwise noted below,

there was no significant pattern of relationship between browser type, operating system,

or referrer and dropout rates at any point in the study.

Also, because the variable is of interest regarding capital sentencing verdicts,

preliminary analyses of dropout also investigated the effects whether the participant

participated before or after September 11, 2000. Chi-square analyses of the non-pool

participants revealed no pattern of relationship between whether participants participated

after September 11 and dropout rates at any point in the study. Thus this variable was not

included in any multivariate analyses.

The manipulated methodological variables were first tested as predictors of whether participants who reached the first page began the study. In addition, analyses included a dichotomous variable representing whether the participant reached the first page of the study on a weekday or weekend (coded 1 if was on a weekend). Also, because preliminary analyses indicated a significant difference, dummy codes were created to represent what page referred the participant to the study. Using participants who were referred by one of the three Web sites outside of Nebraska as the comparison condition, other codes represented those participants who were referred by Nebraska's Web site, some "other or unknown" Web site (which includes participants who typed the URL directly into their browser), or a search engine. Interactions among the methodological variables and between the methodological variables and the day of the week variable (a total of 12) were also tested.

Logistic regression analyses showed that participants from the studyresponse.com pool were much more likely to begin the study (74.1% vs. 21.6% of non-pool participants; $B = 2.70$, Wald = 147.45, $p < .001$). Participants who were required to enter a password that was given to them in the consent form were less likely to begin the study (28.4% vs. 34.4% of participants who just had to click a button; $B = -.293$, Wald = 6.25, $p = .012$). Participants who were referred to the study from Nebraska's Web site were more likely to begin the study than participants who were referred from one of the other three sites (of the non-pool participants, 33.1% vs. 19.1% from the other three sites; $B = .75$, Wald = 16.42, $p < .001$), as were participants who came from some "other or unknown" Web site (of whom 28.2% began; $B = .50$, Wald = 10.45, $p = .001$). Participants who

reached the site through a search engine were less likely to begin the study (1.2% [1 out of 82]; B = -2.89, Wald = 8.27, p = .004). Analyses also showed that participants over the weekend were more likely to begin than participants on weekdays, but this main effect is qualified by a significant interaction between participant source and day of the week, such that there was no effect of day of the week among pool participants (75.0% on the weekend vs. 74.0% on weekdays) but non-pool participants on the weekend were much more likely to begin than those on weekdays (29.8% on the weekend vs. 19.6%; B = 1.37, Wald = 4.24, p = .039).

Analyses next examined the dependent variable of whether those participants who began the study (n = 550) went beyond the second Web page, which presented the case facts of the sentencing hearing. Predictors entered into the analyses were the manipulated methodological and substantive variables, the variable whether the participant participated on a weekend, and four variables measured on the study's first page: the participants' attitude toward the death penalty, whether the participant indicated that they would impose the death penalty in "every" murder case, whether the participant would "never" impose the death penalty, and the count of how many times the participant unsuccessfully clicked the button at the bottom of the consent form to begin the study. Logistic regression analyses revealed that non-pool participants who had not been given the option of entering their personal information on the first page of the study were less likely to continue beyond the case facts (90.3% vs. 96.6% of those who had had the option of entering their personal information: B = -1.13, Wald = 4.36, p = .037). Also, participants who indicated that they would impose the death penalty in every murder case were marginally less likely to continue than other participants (91.8% vs. 95.1%, B = -

1.09, Wald = 3.83, p = .0501). Because of the small number of participants who dropped out at this point, only a limited number of interactions could be tested. None of the interaction terms among the methodological variables and between the methodological variables and the day of the week variable were significant.

The same predictors were used in analyses of whether those who reached the Web page with the jury instructions continued beyond that page (n = 520). Logistic regression analyses showed that there were no significant predictors of dropout at this point in the study. Also, no interactions were significant.

The same predictors were also used in analyses of whether those participants who reached the Web page to record their verdict (n = 485) finished that page. Logistic regression analyses revealed that participants who received the case facts that did not include any evidence about the defendant being abused as a child were more likely to continue than participants who received such evidence (98.2% vs. 92.0% of those who received just that evidence; B = 1.54, Wald = 5.34, p = .021). No other variables, and no interactions, were significant predictors.

A final analysis investigated predictors of whether those participants who began the study (i.e., reached the case facts; n = 550) finished the study. Logistic regression analyses revealed that, over the course of the study, non-pool participants who had not been given the option of entering their personal information on the first page of the study were less likely to finish the study (72.7% vs. 82.0% of those who could enter their personal information; B = -.57, Wald = 4.02, p = .045). Also, although this variable was not significant at any one point in the study, over the course of the study participants who were presented the case facts without any mention of the defendant's mental illness were

less likely to finish than those who received just that evidence (79.3% vs. 89.5%: $B = -.79$, Wald $= 4.40$, $p = .036$). In addition to the interactions tested in previous analyses, this analysis also tested interactions between the manipulated substantive variables and participants' attitude toward the death penalty and the methodological variables. No interactions were significant.

Effects on Sentencing Verdicts

Of the 458 participants who finished the study, data from 30 were excluded as invalid because the time spent reading the case facts was less than 90 seconds and/or participants answered the questions in a discernible pattern (e.g., answering all ones). Thirteen of the 30 participants were from studyresponse.com's pool. This left a final sample size of 428. 222 solicited from links and 206 from studyresponse.com.

The substantive dependent variable of interest in this study was the dichotomous choice of sentence between death and life imprisonment without parole. Because the measured variable was dichotomous but can be conceptualized as an indicator of an underlying latent continuous variable (i.e., mock jurors' likelihood of imposing death), a probit regression is appropriate. Using the techniques outlined by Winship and Mare (1983), multiple probit regressions were conducted in order to structure analyses into a path analysis. Winship and Mare (1983) demonstrate how path analysis can be employed with endogenous and/or dependent variables that are measured discretely by rescaling the ordinary probit regression coefficients, which are computed under the assumption that the error variance of the latent variable equals one, to what the regression coefficients would be if computed under the assumption that the total variance of the latent variable equals one. Ordinary probit regression coefficients are not suggested because with the addition

of additional predictors the total variance of the latent variable increases, changing the distribution of the latent variable. Original coefficients (indicated as B_1 in the description of results and in Table 1) are rescaled by multiplying each by the square root of the inverse of the total variance of the latent variable in the ordinary probit regression (which is reported in PRELIS's probit regression output). Once rescaled, the regression coefficients (indicated as B_2 below) are accurate measures of the total and direct effects of each variable in the path analysis. Standardized regression coefficients (beta weights; indicated as β below) were then computed by multiplying the rescaled value by the standard deviation of the variable.

The first level of the path analysis included both methodological and substantive variables. The methodological variables were those manipulated (source, method of obtaining consent, location of attitude scale, location of personal information questions (for non-pool participants), and location of demographic information (for pool participants)), and variables indicating whether the participant participated on a weekend or weekday and either before or after September 11. The substantive variables were the manipulated variables (with the condition where participants received just the mitigating evidence as the comparison condition), factor scores for the five factors of the attitudes toward the death penalty scale, two variables for death qualification measuring whether participants indicated that they would "always" or "never" impose the death penalty in murder cases, and two single items measuring participants' beliefs whether evidence of mental illness or child abuse "never makes a defendant less responsible for murder."

The second level of the path analysis included participants' ratings whether they agreed that the listed aggravating and mitigating factors were present in the case (two and

six factors, respectively), plus two factor scores computed from items on the final page

measuring participants' sympathy and empathy toward the defendant. The final level was

the dichotomous sentencing choice.

Analyses also tested all two-way interactions between the variables on the first

level except those involving the death qualification variables or between attitude factors.

There was a total of 109 interaction terms, computed as residualized cross-products

(Cohen, 1978; Lance, 1988). Thirteen interaction terms were significant, more than

would be expected by chance alone at the .05 level. The strongest six interactions are

described below, but the interaction terms were not included in the path analysis.

Keeping all 109 interaction terms in the model that included mediators resulted in a

model that explained all the variance in the dependent variable. However, when the non-

significant interaction terms were removed from the model, the interaction terms that had

been significant were no longer significant (because of the change in the dependent

variable's total variance). Also, when interaction terms were added to the model after the

first-level variables the coefficients of those variables, to which the interaction terms

were orthogonal, changed, thus obscuring the effect of each variable.

Methodological Variables.

Analyses showed that no methodological variable had a direct effect, either total

or direct, on sentencing verdicts. However, four interactions involved methodological

variables. Two interactions involved whether participants participated on the weekend or

weekday and the substantive manipulation of child abuse evidence. The percentage

voting for death on the weekend in the condition without any evidence of child abuse

(15.3%) was close to the overall weekend average (13.0%), but participants on the

weekend who received just evidence about child abuse were unexpectedly more likely to vote for death (25.9%). In contrast, participants on weekdays showed a slight effect in the opposite, expected direction (33.6% in the no-abuse condition vs. 25.7%) (interaction B_1 = -4.79, t = -2.96). This may be attributable to the fact that participants on the weekend in the only-abuse condition agreed more that child abuse never makes a defendant less responsible (mean = 6.48 vs. nothing higher than 5.96 in other conditions). Participants on the weekend also appeared more sensitive to the additional expert testimony about the effect of child abuse, as only 4.5% voted for death in that condition, while participants on weekdays showed a smaller effect (19.6% in the link condition vs. 25.7%) (interaction B_1 = -6.65, t = -2.75). Yet again, participants on the weekend in the link condition also agreed less that child abuse never makes a defendant less responsible (M = 4.31 vs. 5.77 on a weekday).

In addition, whether participants participated before or after September 11 moderated the effects of one substantive manipulation and one attitude factor. As noted below, mental illness had a significant mitigating effect, but this effect was stronger among participants after September 11 (B_1 = 3.78, t = 2.63). This is attributable to the fact that a greater percentage of participants in the condition with no evidence of mental illness voted for life after September 11 (44.1% vs. 25.0% before September 11), but the increase was not as great in the condition with just evidence of mental illness (24.3% vs. 12.5%). Also, before September 11 participants' with stronger retributive sentiments were more likely to vote for death (B_1 = .075, t = 2.17), but after September 11 participants with stronger retributive sentiments were marginally less likely to vote for death (B_1 = -.026, t = -1.68) (interaction B_1 = -.34, t = -2.62).

Methodological variables had only sporadic effects on the mediating aggravating

and mitigating factors. Participants solicited by links on Web sites found the murder to be

less heinous (M = 6.25 vs. 6.82, B_1 = -.82, β = -.19, t = -2.27, p = .024). Non-pool

participants who were not given the opportunity to enter their personal information on the

first page found the defendant to be less mentally ill (M = 5.02 vs. 5.82, B_1 = -.74, β = -

.13, t = -2.26, p = .025) and marginally agreed more that the murder was for pecuniary

gain (M = 6.03 vs. 5.37, B_1 = 0.62, β = .11, t = 1.96, p = .051). Participants who had to

give a password in order to imply consent agreed more that the defendant would be a

well-behaved inmate (M = 6.23 vs. 5.77, B_1 = .41, β = .10, t = 2.00, p = .046). These were

the only four effects of methodological variables on mediating variables: out of the 70

effects tested 3 to 4 would be expected by chance alone.

Substantive variables.

Only three first-level variables had total effects on sentencing verdicts. Consistent

with prior studies, general support for the death penalty was the strongest predictor of

verdicts (B_1 = .10, B_2 = .07, β = .13, t = 6.56). Participants who indicated that they would

always impose the death penalty were more likely to do so (B_1 = .72, B_2 = .49, t = 2.41).

The only significant effect of a manipulated variable showed a mitigating effect of mental

illness---participants who did not receive evidence of mental illness were more likely to

impose the death penalty (37.5% vs. 20.8% with the evidence: B_1 = .60, B_2 = .40, β = .17,

t = 2.67).

There was a significant direct but not total effect of the additional expert

testimony about the link between child abuse and the murder in that those given the

expert testimony were less likely to impose death (direct effect B_1 = -.62, B_2 = -.31, β = -

.14, t = -2.45). This direct effect was apparently counteracted by the effect of the expert

testimony on mock jurors' ratings of the purported mitigating factor that the "defendant

was emotionally immature and impulsive." Participants in the link condition agreed more

that the defendant was emotionally immature (M = 6.72 vs. 5.92 in the abuse-only

condition, B_1= .65, β = .13, t = 2.43), which, as noted below, had an unexpected

aggravating effect on verdicts.

Two significant interactions involved the manipulation of the evidence pertaining

to child abuse, which interacted with participants' beliefs whether child abuse could ever

make a defendant less responsible. To explain the interaction participants were divided

according to whether they agreed or disagreed that child abuse "never makes a defendant

less responsible for murder" (with those at the midpoint categorized as disagreeing

because they were below the mean). Among participants who disagreed, and therefore

believed more that child abuse could make a defendant less responsible, evidence of child

abuse had a mitigating effect (11.3% imposing death in the evidence-only condition vs.

28.4% in the no-evidence condition), while there was no such effect among people who

believed more that child abuse could not make a defendant less responsible (32.2% vs.

37.2%) (interaction B_1 = -.81, t = -3.43). However, participants who believed more that

child abuse could not make a defendant less responsible were sensitive to the additional

expert testimony (24.6% imposing death in the link condition vs. 37.2%), while

participants who believed more that child abuse could make a defendant less responsible

showed no additional effect of the added testimony (10.1% vs. 11.3%) (interaction B_1= -

.64, t = -2.58).

Some, but not all, aggravating and mitigating factors also were related to verdicts.

Participants who rated the crime as more heinous were more likely to impose death (B_1 = .23, B_2 = .11, β = .25, t = 4.36), but there was no effect of ratings of whether the crime was committed for pecuniary gain (B_1 = .07, B_2 = .04, β = .09, t = 1.74). The significant mitigating factors were ratings that the defendant would be a well-behaved inmate (B_1 = -.11, B_2 = -.06, β = -.12, t = -2.13) and that he was mentally ill (as measured by the defendant's "capacity to appreciate the wrongfulness of his conduct or to conform to the requirements of the law"; B_1 = -.09, B_2 = -.04, β = -.11, t = -2.13), and the defendant's young age at the time of the murder (B_1 = -.18, B_2 = -.09, β = -.23, t = -3.87). The listed mitigating factor that the defendant was "emotionally immature and impulsive" in fact had an aggravating effect (B_1 = .10, B_2 = .05, β = .12, t = 2.11). Sympathy for the defendant also had a mitigating effect (B_1 = -.05, B_2 = -.03, β = -.16, t = -2.45) but empathy did not (B_1 = .02, B_2 = .01, β = .04, t = 0.76). There were marginal but non-significant effects of the defendant's remorse (B_1 = -.10, B_2 = -.05, β = -.10, t = -1.80) or the defendant's "dysfunctional childhood" (B_1 = .09, B_2 = 05, β = .11, t = 1.76).

Indirect effects (i.e., mediation) were investigated using Baron and Kenny's (1986) approach, which finds mediation when there is a total effect of an exogenous variable, a direct effect of an endogenous variable, and a direct effect of the exogenous variable on the endogenous variable. Complete mediation exists when after adding the endogenous variable to the model the direct effect of the exogenous variable is not significant. In this model, the effect of general support was partially mediated by the mitigating factors of the defendant's age and the defendant's impulsivity, as well as sympathy for the defendant.

Study 3

The third study further investigated the effect of offering financial incentives in different distributions of lottery prizes and again manipulated how informed consent is obtained. Also, in order to resolve a confound in Study 1, this study separately manipulated whether the tables on the Web pages contain color and whether or not they extend off the participants' computer screen. Again, each methodological variable and day of the week was hypothesized to affect dropout, such that participants offered a lottery with many smaller prizes, those who just had to click a button to give consent, those with colorless tables, those with tables that did not extend off the screen, and those on the weekend would be less likely to drop out. It was hypothesized that too-wide tables would be related to lower means on attitude items, but no other effect of methodological variables on sample characteristics was predicted. In addition, data was collected from a paper-and-pencil version of the study by another researcher (Faimon, Bornstein, & DiLillo, 2002). Attitudes and results were hypothesized to not differ between versions of the study. The substantive variables in this study addressed jurors' reactions to repressed memories. It was hypothesized that jurors would be more skeptical (in terms of verdicts and victim believability) of repressed memories than non-repressed memories, and that allegations of abuse involving intercourse would be more believable than other types of abuse but only when the memory was therapeutically recovered (Faimon et al., 2002).

Method

Both undergraduate and non-student participants were recruited for this study. Undergraduate participants completed a paper-and-pencil version of the study for course extra credit. Non-student participants completed this study over the Web; a mockup is

available at: http://psych.unl.edu/psychlaw/oldstudies/memory/study.asp. Non-student

participants were solicited in two ways. First, the link to the study was posted on three

Web sites that collect such links (not the Web Experimental Psychology Lab). The study

collected data through these links from November 14, 2001 to January 30, 2002.

The first page of the study displayed general instructions, the text of the consent

form, and seven questions asking participants to rate on a Likert-type scale from 1 to 9

their level of agreement with statements about their motivation for participating. The

items asked about whether participants were interested in the subject matter of the study,

whether they were participating "only for the chance to win money," whether it is

important to help researchers conducting psychological experiments, concerns about

privacy, understanding of the study's risks and benefits, enjoyment of participating in

Web-based studies, and whether Web-based studies can provide valuable information

about human behavior. The manipulations of table design first appeared for the table

containing these questions. The table appeared either in black and white or in color and

was specified as a width either 80% or 110% of the participants' browser screen

resolution width. Also, the study intended to manipulate and fully cross whether non-

students solicited through links were offered a lottery either for 1 prize of $250 or 1 prize

of $100, 4 of $25, and 10 of $5. Odds of winning were not presented to participants.

However, due to a programming error this manipulation only appeared among one-eighth

of participants and was not fully crossed with other variables. The majority of

participants were offered the lottery with multiple, smaller prizes.

The manipulation of how informed consent was obtained determined what

participants saw next. Half of participants were directed to a page that contained six

yes/no questions about participants understanding of the study's risks and benefits and other information in the consent form. Participants' answers to these questions were checked and if all did not indicate that participants understood the information participants were directed to a page that asked them to again read the consent form. This process continued until participants answered all six questions demonstrating understanding. The other half of participants proceeded directly to the next Web page. containing the case facts and jury instructions.

Two substantive variables were manipulated within the case facts, which involved a 20-year-old female college student in therapy for problems associated with possible sexual assault by here older uncle. First. there were four types of abuse: fondling. oral sex. intercourse. and satanic ritual abuse. Second, the victim's memories either had been remembered throughout her life. had surfaced during therapy. or had come back spontaneously upon smelling a cologne similar to her uncle's.

The next Web page repeated the jury instructions and asked participants to indicate their verdict on the charge of sexual assault of a child. Participants also rated their confidence in the verdict. the believability of the victim and the defendant. and the heinousness of the crime. all on nine-point Likert-type scales. There were three manipulation check questions, three demographic information questions (age. gender. and ethnicity). and five questions measuring participants' beliefs about repressed memories, trauma. and therapy. Finally, participants were asked eight completely optional yes/no questions about personal previous sexual abuse. Upon submitting that information. participants reached a page where they could enter their personal information for payment through the lottery (for non-students) and read debriefing information.

Results

The Database

There were 1,140 entries in the database. 41 entries identified as search engines, robots, or spiders were excluded. Also excluded were 111 entries identified as repeated entries from the same individual, as judged by looking for the same IP address (at least the first three numbers) and same browser information over the course of the study, with exceptions for entries for AOL users, who share dynamically assigned IP addresses. There were only 2 identified instances of one person attempting to complete the study multiple times. This left a total of 988 unique participants who visited the first page of the study.

Preliminary Analyses

The items measuring attitudes toward participating appearing on the first Web page were factor analyzed. Specifying a principal components extraction of factors with eigenvalues greater than one and Varimax rotation, six items loaded strongest on one factor. Three items with univocal loadings (whether it is important to help researchers, whether the participant enjoys participating, and whether Web-based studies provide valuable information) were summed to form a measure of favorable attitudes toward participating in Web-based studies in general ($\alpha = .84$).

Two separate exploratory factor analyses were conducted on the items about participants' beliefs about repressed memory and trauma were factor analyzed and the items about participants' perceptions of the case. Specifying a principal components extraction of factors with eigenvalues greater than one and Varimax rotation, two factors emerged from the first analysis--beliefs that repressed memories happen and can be

recovered (3 items, α = .78) and favorable beliefs about therapy (2 items, α = .45). The second factor analysis showed three factors—believability of the defendant (2 items, α = .63), believability of the victim (2 items, α = .64), and seriousness of the crime (3 items, α = .68). Since several items had different response scales, scores were standardized before they were summed to produce factor scores.

Demographic Analyses

On the last page of this study participants were asked to enter their gender, age and ethnicity. Overall, the completed sample (n = 176) was mostly female (77.8%), somewhat young (48.9% was 24 years old or younger, only 18.2% over 42 years old), and Caucasian (83.5%). Chi-square analyses examined the pattern of relationship between participants' demographic characteristics (in this study limited to gender, age and ethnicity) and the methodological variables, including referrer. The analysis of referrers included only participants who came from the UNL site and those referred from other Web sites because the other two categories had a small number of participants. Analyses showed that, compared to participants referred from other Web sites, participants referred from UNL's site were more likely to be female (90.6% vs. 75.8%, $\chi^2(1)$ = 4.78, p = .029) and were older (32.1% vs. 14.3% were over 42 years old, 22.6% vs. 53.8% under 24 years old, $\chi^2(4)$ = 16.13, p = .003). Also, participants who completed the study after having to answer additional questions about informed consent were younger (54.1% vs. 44.0% under 24 years old, 11.8% vs. 24.2% over 42 years old, $\chi^2(4)$ = 10.28, p = .036). No other patterns of relationship were significant.

Attitudinal Analyses

Using only the data from the Web version of the study, one-way ANOVAs tested

whether there were mean differences in responses to the items on the first Web page across levels of the manipulated variables or different referring pages. Differences in the amount of variance were also tested using Levene's test for homogeneity of variance. The analysis of differences between referrers focused only on the differences between participants who came from the UNL site and those referred from other Web sites because the other two categories had a small number of participants. Compared to participants referred from other Web sites, participants referred from the UNL site had more favorable attitudes toward the study in that they felt that it was more important to help researchers (7.96 vs. 7.34, $F(1, 253) = 5.22$, $p = .023$), more enjoyed participating in Web-based experiments (7.93 vs. 6.48, $F(1, 253) = 21.40$, $p < .001$), and felt more that Web-based studies provided valuable information (7.97 vs. 7.10, $F(1, 253) = 11.30$, $p = .001$). For each of these items there was also significantly more variance among participants referred from sites other than UNL's. Also, participants referred from the UNL site agreed more that they were participating only for money (3.16 vs. 1.87, $F(1, 253) = 20.29$, $p < .001$) and agreed marginally more that they understood the risks and benefits of the study (7.72 vs. 7.17, $F(1, 253) = 3.80$, $p = .052$).

Of the 21 ANOVA tests involving the three methodological variables manipulated on the first page, only one was significant—participants viewing colorful tables had lower scores than participants viewing grayscale tables on the item measuring whether it was important to help researchers (7.20 vs. 7.75, $F(1, 253) = 5.39$, $p = .021$). However, on all seven items the scores were lower among participants viewing colorful tables (an average mean difference of .38), and on six items scores were lower among participants viewing the too-wide tables (an average mean difference on all seven items of .16). There

was also more variance on all seven items when presented in colorful tables, the difference being significant on three items. Multiple regression analyses were then conducted to test whether there was an interaction between the manipulations of table color and width; none of the seven interaction tests were significant. Finally, participants on the weekend were more concerned about keeping personal information safe on the Web (M = 7.57 vs. 6.66, $F(1,253)$ = 39.28, p = .011), and there was more variance in three items among participants on weekdays.

Analyses also examined whether there were attitudinal differences between, of those who began the study, those who finished and those who did not. One-way ANOVAs showed that participants who finished the study felt it was more important to help researchers (M = 7.74 vs. 6.85, $F(1,253)$ = 12.37, p = .001), were more concerned about privacy (M = 7.18 vs. 6.24, $F(1,253)$ = 8.02, p = .005), understood the study's risks and benefits more (M = 7.61 vs. 6.54, $F(1,253)$ = 16.27, p < .001), enjoyed participating in Web-based studies more (M = 7.10 vs. 6.05, $F(1,253)$ = 11.55, p = .001), and felt more that Web-based studies provide valuable information (M = 7.52 vs. 6.53, $F(1,253)$ = 10.03, p = .002). There was also more variance among participants who began the study on all seven items, a difference that achieved significance on five items.

Similar analyses were conducted on the items and factor scores measuring participants' beliefs about repressed memories and therapy. Participants who were presented with colorful tables appeared to be more skeptical of repressed memories, as indicated by lower means on two items and the factor score measuring beliefs about repressed memories (e.g., factor mean = -.44 vs. .87, $F(1,166)$ = 9.07, p = .003). There was also more variance on these two items and the factor score. Means were also

significantly lower for two items and both factors were lower when the tables extended off the participants' screens (e.g., same factor mean = -.34 vs. 72, $F(1,166) = 6.28$, $p = .013$). There were no mean differences or differences in the amount of variance related to the method of obtaining informed consent, day of the week, or site from which non-student participants were referred.

Finally, ANOVAs were conducted to examine differences in attitudes between the paper, undergraduate sample and the Web-based, non-student sample. There were no mean differences detected, but there was significantly more variance among the Web-based, non-student sample on three items and both factor scores.

Scale Structure Analyses

Because several factors had only two items, which may cause the model to not be identified and reduces the number of parameters that may differ between groups, no multiple-group confirmatory factor analyses were performed for this study.

Dropout Analyses

Of the 988 participants who visited the first page of the study, 255 (25.8%) clicked the button at the bottom of the page to begin the study. Not all these participants reached the page with the case scenario; of the 131 who were randomly assigned to receive the additional questions about informed consent 32 (24.4%) did not continue beyond that page. As such, 223 (22.6% of those who reached the first page) reached the page with the case facts. Of those, 197 (88.3%) proceeded to the final page. Of those, 176 (89.3%; 17.8% of those who reached the first page) completed the study.

Server-side scripting was used to detect and record the browser that the participant used to view the study, the operating system of the participant's computer,

and the referring page to the study (i.e.. the page that contained the link to the study). Participants' browsers were categorized as: Internet Explorer, Netscape. AOL, or "other/unknown". Operating system categories were: Windows 95 or earlier; Windows 98; Windows ME, 2000 or XP; Windows NT; Macintosh; or other/unknown. In addition to the three Web sites noted above that posted the link, referring pages included search engines (though only 7 participants were referred in this way), a few university pages that posted a link to the study (41 participants came from the University of Wisconsin at Milwaukee through a link posted on a class's Blackboard account), and other unidentifiable referrers. Preliminary chi-square analyses were conducted to investigate whether each dependent variable described more fully below was related to participants' browser type, operating system. or referrer. Unless otherwise noted below, there was no significant pattern of relationship between browser type, operating system, or referrer and dropout rates at any point in the study.

Logistic regression analyses investigated whether the methodological variables of table design and payment. manipulated on the study's first Web page, were related to whether participants clicked the button to continue beyond the first page. Since the manipulation of method of obtaining informed consent was not seen before participants clicked the button. this variable was not included. Preliminary analyses did indicate a significant pattern of relationship between referring page and dropout, so this analysis included three dummy code variables representing the UNL referring sites, the other school referring sites. and the "other or unknown" sites (which for this analysis included search engines, none of which clicked to continue), with the non-UNL sites as the comparison condition. Analyses revealed no significant effects of the manipulated

variables, but participants referred from UNL sites and other schools were each more likely to begin the study than participants referred from other Web sites (38.0% vs. 20.1%, B = .95, Wald = 29.06, p < .001; and 62.2% vs. 20.1%, B = 1.96, Wald = 36.52, p < .001, respectively). Also, the increase in percentage of those continuing on over the weekend approached significance (28.8% vs. 25.0%, B = .34, Wald = 3.63, p = .057). Participants presented with colorful tables were also marginally more likely to begin the study (27.6% vs. 24.1%, B = .26, Wald = 3.02, p = .082). In this and all subsequent analyses, the main effect of the lottery manipulation was tested but interactions involving this variable were not because it was not fully crossed with all variable; there were no significant interactions in this analysis (out of the 11 tested).

Next, because the manipulation of how informed consent was given made half the participants answer additional questions about information presented in the consent form, a separate analysis investigated whether participants who reached the first page of the study began the study <u>and</u> reached the page displaying the case facts. This analysis included the dummy code representing the manipulation of manner of obtaining consent and interactions involving that variable. Again, participants referred from UNL sites and other schools were each more likely to begin the study than participants referred from other Web sites (34.0% vs. 17.1%, B = .96, Wald = 27.13, p < .001; and 55.6% vs. 17.1%, B = 1.88, Wald = 34.02, p < .001, respectively). Participants on the weekend were more likely to begin the study (27.9% vs. 21.1%, B = .52, Wald = 7.98, p = .005). Participants who were given the additional questions were less likely to continue to the case facts (18.6% vs. 27.2%, B = -.44, Wald = 7.52, p = .006), but this effect was qualified by an interaction with the day of the week variable, such that participants were

less likely to continue when given additional questions only on weekdays (16.1% vs.

26.9% compared to 27.6% vs. 28.2% on weekends; B = .79, Wald = 4.27, p = .039).

Analyses next explored the variable whether participants who reached the case

facts clicked to continue beyond that page. Dummy codes representing the substantive

manipulated variables, with the fondling abuse condition and non-repressed memory

condition as the comparison condition, were added as predictors of dropout. Also,

analyses added as predictors attitudes toward participating as measured on the first page,

consisting of the computed factor score and the other four items. The logistic regression

analysis showed no significant main effects of any variable. Interactions among the

manipulated methodological variables and day of the week (a total of 6) were also tested

and none were significant.

The analysis of whether those who reached the final page containing the repeated

instructions, dependent variables and other questions finished the study also revealed no

significant main effects. However, opposite from what was observed before, the effect of

method of obtaining informed consent approached significance, in that participants who

had answered the additional questions about information in the consent form were more

likely to finish the study (94.4% vs. 85.0%, B = .96, Wald = 2.49, p = .114). Similar

effects were seen in a final analysis that focused on predictors of whether those who

reached the case facts finished the study (i.e., cumulative dropout over the last two pages

of the study). Participants who had answered the additional questions about information

in the consent form were marginally more likely to finish the study (85.9% vs. 73.4%, B

= .73, Wald = 3.61, p = .057). Also, participants who indicated that they understood the

risks and benefits of the study more were marginally more likely to finish (B = .23, Wald

= 2.96, p = .085). No interactions were significant in either analysis.

Effects on Verdicts

Data from the Web-based and paper-and-pencil version of the study were combined for substantive analyses, and data collection method was included as a variable. Of the 176 participants who finished the study, data from 8 were excluded as invalid because the time spent reading the case facts was less than 45 seconds and/or participants answered the questions in a discernible pattern (e.g., answering all ones). This left a final sample size of 413, including the 245 undergraduate participants. In order to conform with how the substantive results will be presented elsewhere (Faimon et al., 2002), verdicts were coded -1 for not guilty and 1 for guilty and that was multiplied by participants' ratings (from 1 to 7) in the confidence of their verdict to form an aggregate, continuous measure from -7 (very confident in not guilty verdict) to 7 (very confident in a guilty verdict). Analyses were structured as a three-level path analysis using SEM techniques in LISREL 8.3.

The first level contained the manipulated methodological variables (paper vs. Web, lottery, table color, table width, and method of obtaining consent), dummy codes for the manipulated substantive variables (with the fondling condition and the non-repressed conditions as the comparison condition), and the two factor scores measuring beliefs about repressed memory and therapy. In addition, the first level included a variable indicating the participants' gender and a variable indicating whether the participant indicated in an answer to the last eight questions that they had experienced any form of sexual abuse as a child.

The second level consisted of the three factor scores measuring participants'

perceptions of the seriousness of the crime and the believability of the defendant and victim. The third level was the continuous verdict variable.

Preliminary analyses were conducted to identify significant two-way that could be included in the model. First, residualized interaction terms were computed (Cohen, 1978; Lance, 1988). Next, the main effect variables and interaction terms were entered into a hierarchical multiple regression. All interactions involving methodological variables (except the lottery manipulation), substantive variables, and attitudes entered on the first level (a total of 82) were tested. Only two were significant: since at least three would be expected by chance at the .05 level, the interactions were not interpreted. Similarly, analyses tested interactions between the manipulated variables on the first level and those on the second level (a total of 27); none were significant.

Methodological Variables.

There were no main effects, either total or direct, of methodological variables on mock jurors' verdicts. There were no mean differences or interactions involving the method of data collection (Web vs. paper) variable. Indeed, the only effect of methodological variables was that non-student participants on the Web-based version of the study rated the victim as more believable (M = .48 vs. -.33, β = .46, z = 2.22).

Substantive Variables.

Greater details and discussion of the substantive analyses are provided elsewhere (Faimon et al., 2002). In the LISREL analysis, none of the manipulated variables had a significant effect on verdicts, though the effect of a scenario describing sexual abuse involving oral sex (compared to abuse involving fondling) approached significance (total effect β = .11, z = 1.90). Compared to the fondling condition, all three other types of

abuse were perceived as more severe, but perceptions of severity of the abuse were not related to verdicts. Believability of the plaintiff was related to verdicts ($\beta = 0.13$, $z = 2.61$) but neither memory type nor abuse type was related to believability.

Study 4

The fourth study investigated methodological variables relevant to participants'

risk-taking behavior in a context where jurors decide the reasonableness of other people's

risky behavior. Participants chose between two different distributions of lottery prizes

with an equal expected value but a bigger payoff in one condition. Also, the study

manipulated whether demographic information was required and when personal

information (name, address and SSN) was asked for. All methodological variables and

day of the week were hypothesized to affect dropout, such that participants choosing a

lottery with many smaller prizes, those who were not required to enter demographic

information, those who were not asked for personal information until the end of the

study, and those on a weekend would be less likely to dropout. Participants' choice of

lottery distribution was hypothesized to be related to measures of risk-seeking and risk-

aversiveness, but no other methodological variable was hypothesized to be related to

attitudes or risk-seeking. The substantive context was jurors' reactions to defendant

companies' performing a cost-benefit analysis when deciding whether to change or recall

a defective product. It was hypothesized that a company's performance of a CBA, a post-

sale scenario, a higher level of known risk, and positive attitudes toward the civil jury

system would be related to pro-plaintiff outcomes, both verdicts and damage awards.

Method

All participants completed this study over the Web: a mockup is available at:

http://psych.unl.edu/psychlaw/oldstudies/wta/study.asp. Both undergraduate and non-

students were recruited for this study. Non-student participants were solicited through a

link to the study posted on the four Web sites that collect such links. The study collected

data through these links from October 22, 2001 to February 25, 2002. Undergraduate participants were solicited through the psychology department's participant pool and undergraduates participated for course credit.

The first Web page introduced the study, gave general instructions, and provided the text of the consent form. Non-students were offered a financial incentive of being entered into a lottery upon completion of the study. Non-students were asked to choose whether they would prefer to be entered in a lottery either with one prize of $500 or with one prize of $100, three prizes of $50, six prizes of $25, and ten prizes of $10. Odds of winning any prize in either lottery were specified as 1-in-100. Participants were told that upon completion of data collection the researchers would randomly choose one of those lotteries and award the prizes. For undergraduates this question asked if he or she were not participating for course credit which lottery he or she would prefer. Also, this study manipulated whether personal information (name, address and SSN for non-students; just name and SSN for undergraduates) was asked for at this point or not until the end of the study. Entering the information was optional. Then, all participants rated their agreement on nine-point Likert-type scales with five statements about their motivation for participating. Four items asked about whether participants were interested in the subject matter of the study, whether it is important to help researchers conducting psychological experiments, concerns about privacy, and understanding of the study's risks and benefits. The other item asked non-students whether they were participating "only for the chance to win money," but for undergraduates was phrased as participating only for course credit.

The second Web page contained four questions about participants' preferences for

risk and concerns about safety. One questions asked participants whether, if they had the choice, they would prefer to sell a product with a rare chance of a severe injury (1-in-100,000 chance of $500,000 injury) or with a more common chance of a less-severe injury (1-in-1000 chance of $5000 injury). Another question asked participants if they would prefer to receive $5 or be entered in a lottery with a 1-in-100 chance of receiving $500. Another question asked participants how much more, in addition to the base cost of $10,000 for building a deck, they would pay to assure that it would never collapse; this is a measure of participants' "willingness to pay" for additional safety (Geistfeld, 2001). The final question attempted to measure how much of a discount on a car there would have to be before a participant would buy a car with a potentially defective gas tank (a measure of participants' "willingness to accept" for increased risk), but the question was poorly worded and was not included in analyses. Also, participants rated their agreement with eleven items on nine-point Likert-type scales, measuring attitudes toward the civil jury system and corporations.

The third Web page presented the case scenario, in which the victim was killed when gas fumes ignited in the engine compartment of his boat, killing him. Expert testimony attributed the explosion to a crack in copper tubing that was part of the boat's main fuel line, a crack that developed because of a stress point created when the tubing was "flared" during manufacturing. The plaintiff also presented evidence of other similar explosions attributed to the same cause and evidence from company documents that the defendant knew of the potential risk. The defense's expert attributed the explosion to another cause and produced evidence of extensive safety testing before sale of the boat. There were three substantive manipulations of the case facts. First, the company's

documents showed that the defendant knew of the potential defect either before sale (during manufacturing) or after sale (based on a series of similar accidents). Second, the level of risk was either low (5 accidents with 1 death) or high (25 accidents with 5 deaths). Third, the company either performed a cost-benefit analysis after learning of the risk or did not. When the CBA was performed, it would cost the company four times as much to change or recall the product than it would to expect to pay in lawsuit settlements. After reading the case facts participants rated their agreement with 13 items on nine-point Likert-type scales, about perceptions of the case, the defendant's knowledge of risk, outrage toward the defendant, and sympathy for the plaintiff and defendant.

The fourth Web page provided participants with jury instructions. There were three causes of action: negligence in the manufacturing of the product, negligence premised on the defendant's post-sale duty to warn of known risks, and strict liability. Instructions also addressed the award of compensatory and punitive damages. Participants answered three multiple-choice questions about the instructions.

The fifth Web page asked participants to record their verdict on each cause of action, and award compensatory and punitive damages if appropriate. Participants then provided demographic information and submitted their data. The study manipulated whether participants were required to enter all demographic information or not. For half of participants a JavaScript validation script was used to require completion of all questions: if participants did not answer all questions they received a brief text box message to answer the question that they missed.

The final Web page thanked participants and provided debriefing information. If participants had not entered their personal information (name, address, and SSN) on the

first page, they did so on this page.

Results

The Database

There were 575 entries in the database for the non-students. 62 entries identifiable

as search engines, robots, or spiders were deleted. The database was also examined for

duplicate entries from the same individual, as judged by looking for the same IP address

(at least the first three numbers) and same browser information over the course of the

study, with exceptions for entries for AOL users, who share dynamically assigned IP

addresses. 43 entries were identified as duplicate entries of the same individual and were

removed, leaving only one entry per individual. There were not any instances identifiable

as a participant attempting to complete the study multiple times. This left 468 unique

entries in the database.

There were 333 entries in the database for undergraduate participants. 60

duplicate entries were removed, leaving 273 unique undergraduate participants who

visited the first page of the study.

Preliminary Analyses

The items measuring participants' attitudes relevant to the civil jury system and

products liability cases were factor analyzed, revealing four factors. One factor included

participants' negative opinions toward plaintiffs and juries (e.g., "There are too many

frivolous lawsuits today" and "Jury awards are too large"; 4 items; $\alpha = .78$). The second

factor measured participants' belief that companies should be held to a higher standard

than individuals (2 items; $\alpha = .74$). The third factor measured participants' beliefs about

the extreme to which companies should be concerned about safety (e.g., "Corporations

should market a product only after it has been tested for every possible risk"; 2 items; α =

.77). The final factor contained only one item, measuring beliefs that "companies must

often make difficult tradeoffs between safety and cost."

The items measuring participants' reactions to the case that appeared after the

case facts were factor analyzed, showing three factors. One factor incorporated all of

participants' negative opinions toward the defendant and its actions (e.g., "I am upset by

the defendant's actions"; 5 items, α = .92). The other two factors measured sympathy

toward the plaintiff (2 items; α = .71) and sympathy toward the defendant (2 items; α =

.74).

Demographic Analyses

Overall, the completed sample of non-students (n = 89) was mostly female

(77.5%), Caucasian (86.5%), had had at least some college classes (88.7%; 48.9% had

graduated college), were single or married (44.9% and 39.3%, respectively), and were

roughly evenly distributed across age and family income categories (12.5% made less

than $20,000 per year). There was a significant difference in the pattern of distribution of

age related to when participants entered personal information--participants who had had

the option of entering personal information for payment on the first page were older (e.g.,

33.3% vs. 12.2% were between 40 and 49, 12.5% vs. 43.9% were between 22 and 29;

$\chi^2(5)$ = 16.08, p = .007). Also, participants referred from sites other than UNL's were

older, in that a higher percentage fell between the ages of 30 to 39 (24.2% vs. 4.5%) and

fewer were under 21 (9.7% vs. 36.4%; $\chi^2(5)$ = 12.85, p = .025). Finally, analyzing only

participants with Windows operating systems (except NT), a greater percentage of

participants using Windows 95 were female than those using Windows 2000, ME or XP

(84.0% vs. 60.9%, $\chi^2(2) = 8.45, p = .015$). There were no other patterns of relationship between demographic categories and methodological variables.

Also, of those participants who were required to enter all demographic information, only six who did submit their data tried to do so without answering all demographic questions. Three participants had not entered their gender, and three (two of the same) had not entered their annual family income. Of those for whom entering demographic information was optional, only two did not answer all questions---one omitted annual family income and one educational background.

Attitudinal Analyses

One-way ANOVAs tested whether there were mean differences in responses to the items on the first Web page measuring attitudes toward and motivation for participating and the distribution of participants lottery choice across levels of the methodological variables or different referring pages. Non-students indicated greater interest in the subject matter of the study ($M = 7.07$ vs. 5.89, $F(1,336) = 30.81, p < .001$), agreed less that they were participating "for the chance to win money" than undergraduates agreed that they were participating "only for course credit" ($M = 3.43$ vs. 7.17, $F(1,336) = 227.11, p < .001$), and were marginally more concerned about keeping personal information safe on the Web ($M = 7.41$ vs. 6.93, $F(1,336) = 3.48, p = .063$). There were no significant mean differences between when the participant participated (weekend vs. weekday) or whether personal information was asked for on the first page, and there were no differences in the amount of variance related to any methodological variable. Of the 338 participants who indicated a preference for a lottery condition on the first page, 106 (31.4%) chose the lottery with one prize of $500. There were no

differences in the pattern of distribution of lottery choices related to any methodological variables.

The analysis of differences between referring pages for non-students compared only those referred from the UNL site and those referred from other Web sites. Compared to participants referred from other Web sites, participants referred from the UNL site felt that it was more important to help researchers (M = 7.94 vs. 7.18, $F(1,127)$ = 6.70, p = .011) and agreed more that they understood the risks and benefits of the study (M = 8.15 vs. 7.56, $F(1,127)$ = 4.02, p = .047). For these items there was significantly more variance among participants referred from sites other than UNL's. Also, participants referred from the UNL site were more concerned about keeping personal information safe (M = 7.73 vs. 7.61, $F(1,127)$ = 6.54, p = .012) and agreed more that they were participating only for money (M = 3.86 vs. 2.47, $F(1,127)$ = 10.41, p = .002). There was more variance among participants referred from UNL's site on the latter item. There was no difference in the pattern of distribution of lottery choices between referring sites.

Analyses also compared those who began the study but did not finish and those who did finish. Those who finished the study felt marginally more that it was important to help researchers (M = 7.66 vs. 7.23, $F(1,336)$ = 3.53, p = .061). A similar trend was seen when only data for non-students was analyzed (M = 7.88 vs. 7.35, p = .064), and non-students who finished agreed more that they were participating for the chance to win money (M = 3.83 vs. 2.69, $F(1,136)$ = 7.02, p = .009). No other mean differences were significant, and the only difference in amount of variance was that there was more variance among those who did not finish on the item asking whether it was important to help researchers.

Analyses also investigated whether methodological variables were related to participants' responses to the questions about risk preferences, concerns for safety, and attitude items and factors on the study's second Web page. More non-students indicated that they would opt to receive $5 rather than have a 1-in-100 chance of receiving $500 (60.5% vs. 47.4%, $\chi^2(1) = 4.94, p = .026$). Also, participants referred from a UNL Web site were more likely to choose to risk a more rare, less severe injury (54.5% vs. 31.5%, $\chi^2(1) = 5.09, p = .024$). Further, there was a significant relationship between participants' lottery choices on the first page and second pages such that participants chose the risky or less risky option at both points ($\chi^2(1) = 31.99, p < .001$). In responses to the attitude questions, non-students had higher means on six items, reflected in higher factor scores for the factor measuring negative opinions about the civil jury system ($M = 27.46$ vs. 24.97, $F(1,306) = 13.49, p < .001$) and that a corporation should be held to a higher standard than individuals ($M = 12.24$ vs. 11.27, $F(1,306) = 4.91, p = .028$). There was also significantly more variance in six items and all factor scores among non-students. Participants referred from UNL's Web site had higher means on several items, a difference that was significant on three items. These differences translated into mean differences on two factor scores. Participants from UNL's Web site tended to have stronger negative opinions about the civil jury system ($M = 28.40$ vs. 25.79, $F(1,104) = 3.17, p = .078$) and felt more that companies should test products for all possible risks ($M = 15.32$ vs. 13.82, $F(1,104) = 3.66, p = .058$). For three items there was more variance among participants referred from sites other than UNL's.

Scale Structure Analyses.

Because several factors had only two items, which may cause the model to not be

identified and reduces the number of parameters that may differ between groups, no

multiple-group confirmatory factor analyses were performed for this study.

Dropout Analyses

Of the 468 non-students that reached the first page of the study, 138 (29.5%)

clicked the button to begin the study. Of those, 114 (82.6%) finished the preliminary

attitude questionnaire. Of those, 103 (90.4%) continued beyond the case facts. Of those,

93 (90.3%) went beyond the jury instructions and of those 89 (95.7%) finished the study.

Overall, 19.9% of those participants who visited the first page of the study finished it.

Of the 273 undergraduates who visited the first page of the study, 200 (73.3%)

began the study. Of those, 194 (97.9%) finished the attitude questionnaire. Of those, 190

(97.9%) continued beyond the case facts. Of those, 188 (98.9%) finished the jury

instructions. Of those, 187 (99.5%) finished the study. Overall, 68.5% of undergraduates

who visited the first page of the study finished it.

Again, server-side scripting was used to detect and record the browser that the

participant used to view the study, the operating system of the participant's computer,

and the referring page to the study (i.e., the page that contained the link to the study).

Participants' browsers were categorized as: Internet Explorer, Netscape, AOL, or

"other/unknown". Operating system categories were: Windows 95 or earlier; Windows

98; Windows ME, 2000 or XP; Windows NT; Macintosh; or other/unknown. In addition

to the three Web sites noted above that posted the link, referring pages included search

engines (27 participants in this study), a few university pages that posted a link to the

study (only 5 participants for this study), and other unidentifiable referrers. Preliminary

chi-square analyses were conducted to investigate whether each dependent variable

described more fully below was related to participants' browser type, operating system, or referrer. Unless otherwise noted below, there was no significant pattern of relationship between browser type, operating system, or referrer and dropout rates at any point in the study.

The first logistic regression analysis investigated the effect of sample type (undergraduate vs. non-student) and whether participants were asked to enter their personal information for payment on whether participants who reached the study clicked the button to imply consent and begin the study. In addition, preliminary analyses revealed a bivariate relationship between referring page and participants' operating system and thus these variables were dummy coded and included as predictors. For referring page the comparison condition was participants referred from a UNL site and for operating system those using Windows ME, 2000, or XP. The logistic regression analysis showed that non-students were much less likely to begin the study than undergraduates (29.5% vs. 73.3%, $B = -1.70$, Wald $= 71.85$, $p < .001$). Participants having Windows 95 or earlier ($n = 49$), who as noted above had a larger percentage of females, were more likely to begin the study than those using Windows ME, 2000, or XP ($n = 167$; 52.3% vs. 44.2%, $B = .70$, Wald $= 5.11$, $p = .024$). Although not significantly different from either other group, the percentage of participants using Windows 98 who began the study was 49.1%, implying a trend that participants with older operating systems were more likely to begin the study. Also, participants referred from other Web sites ($n = 177$) were less likely to begin than participants referred from a UNL site ($n = 240$; 25.4% vs. 35.0%, $B = -.47$, Wald $= 4.39$, $p = .036$). Although no participants referred from a search engine began the study, this difference was not significant because

of the small number of participants referred that way ($n = 27, p = .35$). 21 interaction terms were also tested (all interactions except those between operating system and referring page, because those interactions did not always have participants in each of the four cells of the interaction) and 2 were significant. Since one would be expected by chance alone, only the strongest interaction was interpreted. That interaction showed that among participants who were not asked to enter their personal information for payment those on the weekend were slightly more likely to begin the study than those on weekdays (50.8% vs. 43.7%) but among participants who were asked those on the weekend were less likely to begin the study (39.3% vs. 47.5%; interaction $B = -1.17$, Wald = 5.47, $p = .019$).

Next, analyses explored the variable of whether participants who began the study clicked to continue beyond the second Web page, which contained the preliminary attitude questions. Five variables regarding attitudes toward participating recorded on the first Web page (the question that differed between undergraduates and non-students was not included) were added as predictors. Again, non-students were less likely to continue than undergraduates (82.6% vs. 97.0%, $B = -1.80$, Wald = 7.46, $p = .006$). Participants who had had the option of entering their personal information on the first Web page were more likely to continue than those who had not been given the option (94.3% vs. 87.7%, $B = 1.05$, Wald = 5.33, $p = .021$). Also, participants who expressed a preference for a lottery with multiple, smaller prizes were more likely to continue than those with a preference for a lottery with a single, large prize (93.5% vs. 85.8%, $B = .89$, Wald = 4.19, $p = .041$). Further, participants who indicated that they understood the study's risks and benefits more were also more likely to continue ($B = .34$, Wald = 4.96, $p = .026$). Finally,

and unexpectedly, participants who were more interested in the subject matter of the study were marginally *less* likely to continue (B = -.26, Wald = 3.65. p = .056). Interactions between the methodological variables and participants' attitudes were also tested (but interactions within attitudes were not), and none were significant.

Because of the small number of participants (15 and 12) who dropped out over the course of the next two pages, which contained the case facts and the jury instructions, analyses of dropout at each point focused on main effects and a limited number of interactions—those between sample type and the manipulated methodological and substantive variables and participants' lottery preference. The analysis of whether participants who reached the case facts clicked to continue added as predictors the dummy codes for the manipulated substantive variables, the five factor scores for participants' attitudes as measured on the second Web page, and participants' answers to the three risk questions. Analyses again showed that non-students were less likely to continue than undergraduates (90.4% vs. 97.9%, B = -1.97. Wald = 6.38. p = .012). Also. participants who indicated a preference for the lottery with multiple, smaller prizes were again marginally more likely to continue (96.3% vs. 92.3%. B = 1.38. Wald = 3.82. p = .051). This latter effect may be qualified by an interaction with sample type, in that the difference in percentage that continued was visible among non-students (93.9% vs. 81.3%) but there was little difference among undergraduates (97.8% vs. 98.3%). The test of this interaction approached significance (B = 3.15. Wald = 3.10, p = .078). No other main effects or interactions were significant.

The analysis of whether participants who reached the Web page with the jury instructions continued on to the next page used the same predictors as the last analysis. At

this point, non-students were again less likely to continue (90.3% vs. 98.9%, B = -2.19, Wald = 5.18, p = .023). Also, participants who felt to a greater degree that it was important to help researchers conducting experiments were more likely to continue (B = .90, Wald = 5.50, p = .019) and participants with stronger negative opinions about the civil jury system were less likely to continue (B = -.17, Wald = 3.86, p = .049). In contrast to the effect above, participants who indicated that they understood to a greater degree the risks and benefits of the study were here *less* likely to finish the study (B = -2.10, Wald = 4.34, p = .037). Also in contrast to above, participants who indicated a preference for the lottery with multiple, smaller prizes were here marginally *less* likely to continue (B = -2.80, Wald = 3.71, p = .054).

No multivariate analyses were conducted on whether participants who reached the page of dependent measures finished the study because only five participants dropped out at this point. As described above, the final manipulation, whether participants' demographic information was required or not, appeared on this page. All five participants who dropped out were in the condition requiring demographic information, a significant bivariate relationship ($\chi^2(1)$ = 4.71, p = .03). Four of these participants were non-students.

A final analysis was conducted to examine the variable of whether participants who reached the page with the case facts finished the rest of the study (i.e., the next three pages). Overall, non-students were less likely to finish the study than undergraduates (78.1% vs. 96.4%, B = -2.10, Wald = 15.42, p < .001). Also, although the effect was not significant at any one point, overall participants who received the case scenario when a CBA was performed were less likely to finish the final three pages of the study (B = -1.00, Wald = 4.74, p = .029). This effect may be qualified in that it was present only

among non-students; 69.1% of non-students finished in the condition where the CBA was performed vs. 86.4% when it was not, while there was no difference among undergraduates (96.5% vs. 96.3%, respectively). This interaction term approached significance ($B = -1.93$, Wald $= 3.37$, $p = .067$). No other main effect or interaction was significant. The effect that participants who indicated that they understood to a greater degree the risks and benefits of the study were less likely to finish approached significance ($B = -.36$, Wald $= 3.20$, $p = .074$).

Effects on Dependent Variables

Of the 89 non-students who finished the study, data from 9 were excluded as invalid because the time spent reading the case facts was less than 120 seconds and/or participants answered the questions in a discernible pattern (e.g., answering all ones). Data from 17 of the 187 undergraduates were excluded for the same reasons. This left a final sample size of 250.

There were six substantive dependent variables of interest in this study: liability decisions on three causes of action, compensatory damage awards, whether punitive damages were imposed, and the amount of punitive damage awards. Because four of the measured variables (the three liability verdicts and whether punitive damages were awarded) were dichotomous but can be conceptualized as an indicator of an underlying latent continuous variable (i.e., mock jurors' perceptions of the reasonableness and recklessness of the defendant's actions, respectively), a probit regression is appropriate. Using the techniques outlined by Winship and Mare (1983) and discussed above, multiple probit regressions were conducted in order to structure analyses into a path analysis.

The damage award variables were analyzed in several ways. After Windsorizing outliers (Dixon & Tukey, 1968), the first analyses analyzed the entire sample including those who did not find the defendant liable. Since those who did not find the defendant liable did not award compensatory damages or punitive damages, these variable were treated as "censored-below" in PRELIS 2.3. As such, this analysis can be characterized as focusing on a latent variable underlying damage awards with a cutoff below which all values are zero. This latent variable may be described as mock jurors' evaluations of the overall responsibility of the defendant, similar to those underlying the dichotomous judgments. A path analysis was conducted using LISREL 8.3 analyzing the covariance matrix computed by PRELIS with the awards variable defined as "censored-below." Separate analyses also analyzed just the awards of those who found the defendant liable on at least one cause of action. These analyses used the raw awards and the natural log of awards as dependent variables (compare Chapman & Bornstein, 1996).

The same variables appeared in the path analyses for each dependent variable. The first level contained the methodological variables (weekend vs. weekday, student vs. non-student, location of asking for personal information, and whether demographics were required), the manipulated substantive variables (timing of scenario, whether CBA was performed, and level of risk), the four attitude factor scores, the natural log of the measure of participants' willingness to pay for additional safety, and a dichotomous variable indicating participants' preference to sell a product with a rare chance of a severe or with a more common chance of a less-severe injury (from the second Web page). Also, because of a significant relationship between responses to the two lottery questions (one on the first Web page and one on the second Web page: $\chi^2(1) = 31.99, p < .001$) the two

questions were combined to create a classification of risk seeking. Participants who indicated a preference for a lottery with a smaller chance at a larger prize on both questions were classified as risk seeking. Participants who preferred a lottery with better odds of winning smaller prizes on both questions were classified as risk averse. As such, this created two dummy codes for a three-level variable (either risk seeking, risk averse, or neither).

The second level contained five potential mediating variables. Three were the factor scores from the factors noted above: outrage toward the defendant and sympathy toward the plaintiff and defendant. The other two variables were single-item measures of mock jurors' perceptions of the defendant's knowledge of a "severe risk" before and after the sale of the defective product. The third level was the dependent variable for the analysis.

Analyses also tested certain two-way interactions between variables on the first level and between variables on the first and second levels. Analyses did not test all possible two-way interactions: no interaction terms involving the attitude factors other than the factor measuring negative opinions toward the civil jury system or involving whether the participant participated on the weekend (because of the small number who did) were included. This left 44 interaction terms, computed as residualized cross-products (Cohen, 1978; Lance, 1988), among variables on the first level to be tested. The interactions between the first and second-level variables were limited to those between sample type, timing of the scenario, and whether a CBA was performed and the second-level variables. 15 such interaction terms were tested. When the number of significant interactions exceeded the number that would be expected by chance at the .05 level, the

strongest interactions were interpreted. Also, since many interactions noted below

involved sample type, further analyses tested three-way interactions involving sample

type and the strongest two-way interactions. Again, however, the interaction terms were

not included in the path analysis, for the same reasons noted in Study 2.

Pre-sale Negligence Claim.

There were two main effects on verdicts as to the pre-sale negligence claim. Non-

students found for the plaintiff more often than undergraduates (46.9% vs. 22.9%, B_1 =

.71, B_2 = .63, β = .29, z = 3.67). Also, participants classified as risk-seeking were more

likely to find liability (B_1 = .62, B_2 = .55, β = .23, z = 2.61). However, this latter effect

was qualified by 2 of 14 significant two-way interactions (out of 44 tested), of which only

the strongest 7 will be discussed below. The effect of participants' belief that companies

should be held to a higher standard than individuals, such that those holding companies to

a higher standard found for the plaintiff more often, approached significance (B_1 = .05 B_2

= .04, β = .16, z = 1.92).

The risk-seeking variable was one of four that interacted with sample type.

Among undergraduates, those classified as risk-seeking were more likely to find liability

than those not classified neither as risk-seeking nor risk-averse (38.1% vs. 12.7%), but

there was no such difference among non-students (56.3% vs. 54.5%; interaction B_1 = -

2.56, t = -2.59). However, undergraduates classified as risk-averse also tended to be more

likely to find for the plaintiff (23.1% vs. 12.7%), but non-students classified as risk-

averse were less likely to find for the plaintiff (39.5% vs. 54.5%; interaction B_1 = -2.96, t

= -2.99).

Also, it appeared that only non-students were sensitive to the manipulation of

timing of the scenario (pre-sale vs. post-sale), as the post-sale scenario led to fewer verdicts for the plaintiff on the pre-sale negligence claim (as expected) among non-students (26.8% vs. 67.5%) but there was no difference among undergraduates (21.2% vs. 24.7%; interaction B_1 = -2.43, t = -3.33). Further, non-students who would opt to risk a more common, less severe injury found for the plaintiff less often than non-students who would opt for the rare, severe injury risk (40.8% vs. 56.3%), but among undergraduates the opposite effect was observed (28.6% vs. 13.8%, respectively; interaction B_1 = -2.13, t = -2.84).

The manipulation of the timing of the scenario also moderated the effect of the amount participants would be willing to pay for additional safety (interaction B_1 = -.51, t = -3.09). In the pre-sale scenario, participants who were willing to pay more for safety were somewhat more likely to vote for the plaintiff (B_1 = .11, t = 1.75), while in the post-sale scenario participants who were willing to pay more were somewhat less likely to find for the plaintiff (B_1 = -.07, t = -1.30).

Two interactions involved participants' preference to risk either a rare, severe injury or a more common, less severe injury. First, regarding the manipulation of level of risk known to the defendant, in the low-risk condition participants who opted to risk a rare, severe injury were less likely to find liability than those who opted to risk a more-common, less severe injury (21.2% vs. 36.1%), but there was no such difference in the high-risk condition (35.6% vs. 28.2%; interaction B_1 = -2.46, t = -3.33). However, this interaction is qualified by a three-way interaction involving sample type. Among undergraduates, participants' risk preference in the low-risk condition was related to verdicts such that those who opted to risk a rare, severe injury were less likely to find

liability (3.1% vs. 33.9%) but there was no relationship in the high-risk condition (24.2% vs. 22.4%). Among non-students, those who opted to risk a more common, less severe injury were less likely to find liability in both the low- and high-risk conditions, but the difference was greater in the high-risk condition (50.0% vs. 40.7% in the low-risk condition, 66.7% vs. 40.9% in the high-risk condition; interaction $t = 2.28$).

Also, the interaction between participants' preference and their negative opinions toward the civil jury system appeared in analyses of each of the three liability verdicts and thus will be noted. Among participants who chose to risk the common, less severe injury, participants with stronger negative attitudes were unexpectedly somewhat more likely to find for the plaintiff ($B_1 = .03$, $t = 1.45$), while among participants who chose to risk the rare, severe injury participants with stronger attitudes were less likely to find for the plaintiff ($B_1 = -.07$, $t = -1.94$; interaction $B_1 = .17$, $t = 2.65$). This interaction was also qualified by a three-way interaction involving sample type. The unexpected effect of negative attitudes being related to a favorable outcome for the plaintiff was seen only among undergraduates, while the expected effect of negative attitudes leading to a pro-defendant outcome was seen only among non-students (interaction t = 2.83).

Analyses showed no effects of mediating variables. However, there were two significant two-way interactions involving these variables and the manipulation of the timing of the scenario. In the pre-sale scenario, jurors' perception of the degree of the defendant's knowledge about the risk pre-sale was positively related to verdicts for the plaintiff ($B_1 = .19$, $t = 2.01$), but, somewhat unexpectedly, perception of the degree of the defendant's knowledge about the risk post-sale was negatively related to verdicts ($B_1 = -.19$, $t = -1.78$). In the post-sale scenario, neither perception was related to verdicts (for

pre-sale knowledge, $B_1 = .04$, $t = 0.56$; for post-sale knowledge, $B_1 = .03$, $t = 0.36$)

(interaction for pre-sale knowledge $B_1 = -.60$, $t = -2.77$; interaction for post-sale

knowledge $B_1 = .81$, $t = 3.10$).

Post-sale Negligence Claim.

The only main effect on verdicts as to the post-sale negligence claim was that

non-students found for the plaintiff more often than undergraduates (74.1% vs. 39.4%, B_1

$= .93$, $B_2 = .80$, $\beta = .37$, $z = 4.77$). Two effects approached significance. Participants who

were classified as risk-averse were marginally more likely to find for the plaintiff (56.5%

vs. 41.2%: $B_1 = .38$, $B_2 = .33$, $\beta = .16$, $z = 1.92$) and participants who agreed more that

"companies must often make difficult tradeoffs between safety and cost" were less likely

to find for the plaintiff ($B_1 = -.09$, $B_2 = -.08$, $\beta = -.15$, $z = -1.93$).

For this variable 2 of 44 two-way interactions were significant, the number that

would be expected by chance. However, the strongest interaction was common to all

liability verdicts and thus will be noted—among participants who chose to risk a common,

less severe injury, participants with stronger negative attitudes were unexpectedly more

likely to find for the plaintiff ($B_1 = .06$, $t = 2.55$), while among participants who chose to

risk the rare, severe injury participants with stronger attitudes were less likely to find for

the plaintiff ($B_1 = -.03$, $t = -1.01$; interaction $B_1 = .16$, $t = 2.68$). The three-way interaction

among these variables and sample type was not significant ($t = 1.37$). However, another

three-way interaction was significant, such that higher risk was related to more verdicts

for the plaintiff only in the post-sale scenario and only among non-students (82.4% vs.

62.5% in that condition vs. no significant differences in other conditions: interaction $B_1 = $

3.11, $t = 2.39$).

There was one significant main effect of the mediating variables. Participants' perceptions of the defendant's knowledge of a severe risk post-sale was positively related to verdicts for the plaintiff ($B_1 = .20$, $B_2 =$, $\beta =$, $z = 3.09$). There were also three significant interactions (of the 15 tested). Even for the post-sale cause of action, in the pre-sale scenario jurors' perceptions of the degree of the defendant's knowledge about the risk pre-sale was positively related to verdicts for the plaintiff ($B_1 = .18$, $t = 2.21$), and perceptions of the degree of the defendant's knowledge about the risk post-sale was not related to verdicts ($B_1 = -.00$, $t = -0.03$). In the post-sale scenario, perceptions of the degree of the defendant's knowledge about the risk post-sale was related to verdicts ($B_1 = .46$, $t = 4.19$), but, in contrast, perceptions of knowledge of risk pre-sale was negatively related to verdicts ($B_1 = -.20$, $t = -2.41$) (interaction for pre-sale knowledge $B_1 = -.63$, $t = -3.81$; interaction for post-sale knowledge $B_1 = .71$, $t = 3.23$). In addition, participants' outrage toward the defendant was moderated by sample type, in that among non-students outrage had its predicted pro-plaintiff effect ($B_1 = .05$, $t = 1.60$) but among undergraduates an opposite effect was seen ($B_1 = -.02$, $t = -1.40$).

Strict Liability Claim.

Here again, non-students found for the plaintiff more often than undergraduates (46.9% vs. 27.1%, $B_1 = .52$, $B_2 = .46$, $\beta = .21$, $t = 2.69$). Participants who believed more that corporations should be held to a higher standard than individuals were also more likely to find for the plaintiff ($B_1 = .08$, $B_2 = .07$, $\beta = .28$, $t = 3.27$). Those classified as risk-seeking voted for the plaintiff more often than those neither risk-seeking nor risk-averse (44.8% vs. 25.9%, $B_1 = .61$, $B_2 = .53$, $\beta = .23$, $t = 2.56$). However, this main effect is qualified by one of the six (out of 44; three will be interpreted) significant interactions,

in that the effect held only for those who would pay less for additional safety (for those below the mean, 58.8% vs. 12.5%, compared to 39.0% vs. 31.1% for those above the mean; interaction $B_1 = -.42$, $t = -2.06$).

The other two interactions were those common to other liability verdicts. In the pre-sale scenario, participants who were willing to pay more for safety were somewhat more likely to vote for the plaintiff ($B_1 = .11$, $t = 1.77$), while in the post-sale scenario participants there was no such relationship ($B_1 = .01$, $t = 0.22$; interaction $B_1 = -.30$, $t = -2.10$). Also, among participants who chose to risk the common, less severe injury participants with stronger negative attitudes were unexpectedly more likely to find for the plaintiff ($B_1 = .04$, $t = 1.43$), while among participants who chose to risk the rare, severe injury participants with stronger attitudes were less likely to find for the plaintiff ($B_1 = -.09$, $t = -2.69$; interaction $B_1 = .18$, $t = 3.23$). The three-way interaction present above was not significant here ($t = 1.42$).

There were no significant main effects of mediating variables. The interaction between sample type and outrage toward the defendant was significant in this analysis too, as outrage was positively related to verdicts for the plaintiff among non-students ($B_1 = .08$, $t = 2.78$) but there was no relationship among undergraduates ($B_1 = -.01$, $t = -0.64$).

Compensatory Damage Awards.

As noted above, analyses of compensatory damage awards included the entire sample and used PRELIS and LISREL to conduct a path analysis. Similar to above, analyses showed an effect of sample type (total effect $\beta = .33$, $z = 5.55$) and being classified as risk-seeking (total effect $\beta = .22$, $z = 3.40$). Only 1 of the 44 two-way interactions between first-level variables was significant. However, this interaction was

also present in analyses of liability verdicts. In the pre-sale scenario, participants who

were willing to pay more for safety were somewhat more likely to vote for the plaintiff

(.09, z = 1.65), while in the post-sale scenario participants there was no such relationship

(-.04, z = -0.67; interaction total effect β = -.12, z = -2.16). None of the four three-way

interactions were significant.

Mock jurors' perception of the defendant's knowledge of a post-sale risk was

related to awards (β = .24, z = 3.99). However, this effect was qualified by an interaction

with the timing of the scenario, such that the effect appeared only in the post-sale

scenario (β = .49, z = 0.57 vs. .03, z = 8.08, in the pre-sale scenario; interaction effect = -

.17, z = -2.84). Also, in the post-sale scenario the relationship between perceptions of pre-

sale knowledge of risk was negative (β = -.22, z = 3.85), while it was positive in the pre-

sale scenario (β = .10, z = 1.82; interaction β = .13, z = 2.19). Further, among non-

students outrage toward the defendant was positively related to awards (β = .16, z = 1.30)

but, unexpectedly, among undergraduates outrage was negatively related to awards (β = -

.16, z = -2.01; interaction β = .12, z = 2.11).

Hierarchical regression analyses of the damage awards for only those who found

the defendant liable on at least one cause of action (n = 146) showed a different pattern of

results. Non-students (β = .23, p = .007) and those classified as risk-seeking (β = .23, p =

.021) gave higher damage awards, but there were seven significant interactions, of which

five will be interpreted. The effect of being risk-seeking was qualified such that the effect

was stronger among non-students (M = $3.44M vs. $1.15M; undergraduates M = $738K

vs. $497K; interaction β = .26, p = .026). Sample type also moderated the effect of the

manipulation of whether the defendant performed a CBA. Among adults, performing a

CBA led to increased damage awards (M = $2.08M vs. $1.27M) but there was no effect among undergraduates ($571K vs. $670K; interaction β = .23, p = .017).

The timing of the scenario also moderated the effect of performing a CBA. There was no effect of performing a CBA in the post-sale scenario (M = $893K vs. $818K), but there was an effect in the pre-sale scenario (M = $1.23M vs. $900K; interaction beta = -.23, p = .019). There were two interactions that involved the manipulation of when participants could enter personal information. The strongest interaction overall was that mock jurors' negative attitudes toward the civil jury system were related to lower compensatory damage awards when participants had had the option of entering their personal information on the first Web page (β = -.24, p = .037), but there was an opposite trend among participants who had not yet entered their personal information (β = .22, p = .113; interaction β = -.26, p = .006). Similarly, among participants who had had the option of entering their personal information on the first Web page, those who would prefer a rare, more severe risk gave lower damage awards ($1.26M vs. $1.73M) but there was no difference among participants who had not yet had the option ($1.69M vs. $1.73M; interaction β = .21, p = .022).

The two-way interactions between sample type, timing of the scenario, and whether a CBA was performed were not qualified by a three-way interaction among these variables. However, the interaction between timing of the scenario and whether a CBA was performed was qualified by a three-way interaction with the manipulation of the level of risk, such that performing a CBA had a pro-plaintiff effect only in the high-risk condition (interaction β = .14, p = .064). The only second-level variable related to compensatory damage awards was the perception of the defendant's knowledge of risk

post-sale (β = .24, p = .014). There were no significant interactions involving second-level variables.

Finally, similar analyses were also conducted using the natural log of award amounts as the dependant variable. Such analyses produced similar results, with a few exceptions. First, the main effect of being classified as risk-seeking was not significant nor was the interaction that qualified it. Also, the interaction between when participants were asked for personal information and negative attitudes toward the civil jury system was not significant.

Awarding Punitive Damages.

The probit regression analysis of whether participants felt that punitive damages were appropriate included all participants, even those who did not find the defendant liable. As such, the latent variable analyzed by the probit regression was similar to the variable underlying liability verdicts, but the threshold value would be higher because recklessness is a higher standard than negligence and unreasonableness. Non-students were more likely to impose punitive damages than undergraduates (48.8% vs. 31.2%, B_1 = .50, B_2 = .45, β = .21, z = 2.64). Participants classified as risk-seeking were more likely to impose punitive damages (48.3% vs. 23.5%, B_1 = .75, B_2 = .68, β = .29, z = 3.23), and so were participants classified as risk-averse (41.1% vs. 23.5%, B_1 = .42, B_2 = .38, β = .19, z = 2.07).

There were 5 significant interactions of 44 among first-level variables, of which 3 will be interpreted. Sample type moderated the influence of negative attitudes toward the civil jury system, in that there was no relationship among undergraduates (B_1 = .01, t = .54) but there was a negative relationship among non-students (B_1 = -.07, t = -2.50)

(interaction B_1 = -.13, t = -2.53). Also, among participants who chose to risk the common, less severe injury participants there was no effect of attitudes (B_1 = .001, t = 0.06), while among participants who chose to risk the rare, severe injury participants with stronger attitudes were less likely to find for the plaintiff (B_1 = -.06, t = -2.12; interaction B_1 = .18, t = 3.23). Finally, among participants who had had the option of entering personal information on the first Web page, those who read the low-risk scenario condition were unexpectedly more likely to impose punitive damages (48.4% vs. 28.0%), while there was no such relationship among participants who had not yet had the option (32.9% vs. 36.9%; interaction B_1 = 1.03, t = 2.30).

Participants who more strongly perceived that the defendant knew of a severe risk post-sale (B_1 = .16, t = 2.39) and who indicated more outrage (B_1 = .02, t = 1.97) were more likely to impose punitive damages. However, there were interactions involving these variables and the manipulation of the timing of the scenario. In the pre-sale scenario, jurors' perception of the degree of the defendant's knowledge about the risk pre-sale was positively related to verdicts for the plaintiff (B_1 = .17, t = 2.15), and there was no relationship of defendant's knowledge about the risk post-sale to verdicts (B_1 = -.05, t = -0.51). In the post-sale scenario, the opposite pattern emerged, as perception of defendant's knowledge about the risk post-sale was positively related to verdicts (B_1 = .46, t = 3.73) but pre-sale knowledge was not (B_1 = -.04, t = -.51) (interaction for pre-sale knowledge B1 = -.50, t = -2.88; interaction for post-sale knowledge B1 = .84, t = 3.37).

Punitive Damage Award Amounts.

Analyses of punitive damage awards in LISREL 8.3 with the dependent variable defined as "censored-below" showed a significant main effect of sample type (total effect

β = .18, z = 2.97) and of being classified as risk-seeking (total effect β = .26, z = 3.91).

The effect of participants' preference for a more common, less severe risk was also

significant (total effect β = .12, z = 1.97).

Both main effects were qualified by significant interactions; overall four

interactions were significant and the strongest two will be interpreted. First, the effect of

sample type appeared only in the pre-sale scenario (adult path β = .33, z = 3.38) and not

in the post-sale scenario (adult path β = .01, z = 0.11; interaction total effect β = -.16, z =

-2.81). Second, the effect of being classified as risk-seeking appeared only among

participants who preferred a more common, less severe risk (β = .36, z = 4.08 vs. β = -

.05, z = -0.37; interaction total effect β = -.16, z = 2.79). Also, participants' perception of

the defendant's knowledge of a severe risk post-sale (total effect β = .22, z = 2.41) and

outrage toward the defendant (total effect β = .18, z = 1.97) were both related to punitive

damage awards.

The only main effect that appeared when the data for only those who awarded

punitive damages was analyzed was the effect of participants' preference for a more

common, less severe risk (β = .34, p = .001). There were four significant interactions, two

of which will be noted. Again, the effect of being classified as risk-seeking appeared only

among participants who preferred a more common, less severe risk, though this was

because participants who were both risk-seeking and who preferred a more common, less

severe injury gave particularly high punitive damage awards (M = $24.8M vs. no other

condition over $10M; interaction β = .12, p = .035). Similarly, the strongest interaction

was that being classified as risk-seeking had a significant relationship among only non-

students, because non-students so classified gave particularly high punitive damage

awards (M = $32.5M vs. no other condition over $12M; interaction β = .16, p = .008).

When the natural log of punitive damage awards was analyzed, similar results were obtained. The effect of risk preference was still significant, but the effect of when participants were asked for personal information was significant also, in that participants who had had the option of entering personal information gave lower punitive damage awards (β = -.32, p = .003). The significant interactions were the same.

Mediation of Effects.

Overall, very few first-level variables were significantly related to the potentially mediating variables on the second level. Non-students gave higher mean ratings to their perception of the defendant's knowledge of risk both pre- and post-sale, and tended to feel more sympathy toward the plaintiff, and less sympathy and more outrage to the defendant. However, as noted above, the variables on the second level were generally not significantly related to the dependent variables except in an interaction.

Discussion

Methodological Variables

Table 2 presents a summary of results regarding the manipulated methodological

variables in all four studies. The effects of methodological variables on dropout can be

grouped into two types and explained in terms of timing and participants' motivation to

participate. First. some variables had consistent effects. as certain conditions led to

increased dropout at several points in the study and there was a cumulative effect over the

entire study. Variables such as sample type. financial incentives. and table design

influenced dropout throughout the study because they applied to the study as a whole—

data was collected from two samples. financial incentives were offered for completion of

the entire study, and tables were present on every Web page. Thus one condition may

have provoked or the opposite condition may have reduced dropout among those low in

motivation.

Second. other variables had effects on dropout at one point in the study but then

effects in the opposite direction appeared at subsequent stages of the study. That is. any

initial difference in dropout was "evened out" by a later effect so that there was no

cumulative effect. Variables for which this pattern emerged included method of obtaining

informed consent and where personal information was asked for—variables that appeared

on only one Web page in each study. Among those participants who do not finish the

study, conditions with increases in dropout at one point in the study leave fewer

participants (and thus a smaller percentage of the entire sample) in the study after that

point. Thus. a later effect of increased dropout in the opposite condition appears. O'Neil

and Penrod (2001) interpreted these effects in terms of provoking dropout among those

low in motivation to participate. After dropout is provoked, the greater percentage of those low in motivation in the opposite condition drop out in future stages of the study.

Some methodological variables were also related to sample characteristics, both demographics and attitudes, and substantive results. Demographic differences were primarily seen between solicitation methods, including sample type and referring Web page. Attitudinal differences were also observed across solicitation methods but also were related to table designs and when participants participated; differences in the amount of variance were more common than mean differences. Sample type was also the methodological variable that showed the most effects on substantive results.

Sample Type

Each of the four studies collected data from at least two different samples. Studies 1 and 4 compared undergraduates from the University of Nebraska to non-students (or at least not students at Nebraska) solicited through links on Web sites. Study 2 collected data from only non-students, solicited from two different sources--links on Web sites and studyresponse.com's participant pool. Study 3 did not allow a direct comparison between undergraduate and non-student data because the undergraduates completed a paper-and-pencil version and non-students participated over the Web. Overall, there were significant differences in dropout rates, attitudes, and substantive results between undergraduates and non-students, but fewer differences between non-students recruited through different means.

As expected, non-students were much more likely to drop out of a study than undergraduates. For instance, only 19.4% of non-students who visited the first page of Study 1 finished it compared to 66.9% of undergraduate participants. This main effect

was occasionally qualified by an interaction with other variables but usually was not. This reinforces the desire to use undergraduates as participants as long as comparable results are obtained. Similarly, in Study 3 non-students solicited from links on Web sites were more likely to drop out than non-students solicited from studyresponse.com. That results between the groups did not differ supports the use of pre-constructed participant pool, of which more need to be developed.

However, analyses consistently showed differences in attitudes, both in terms of mean differences and differences in the amount of variance, between undergraduates and non-students. Studies 1 and 4 both found that non-students had stronger negative attitudes toward the civil jury system and were more satisfied with the judicial system. Both Levene's test of homogeneity of variance showed greater variance in responses to individual items and factor scores among non-students. Multiple-group confirmatory factor analyses also showed greater error variance among non-students. Similarly, Study 2 found a difference in attitudes between non-students solicited from different sources, though the difference in means on items and factor scores was more pronounced than the difference in amount of variance. Participants solicited by links on Web sites were generally less supportive of the death penalty and showed greater variance on three items and one factor score.

The mean differences between non-students and undergraduates were not hypothesized, but little prior research has addressed undergraduates' attitudes toward the civil jury system and the scales used in the present studies had not been used before. Non-students' experience with the legal system, primarily through media reports, may have caused their opinion about the civil jury system to sour. Further, as expected,

undergraduates' less well-formed attitudes (Sears, 1986) had less variance than non-students' attitudes, which were more extreme. The difference between non-students solicited from different sources was also not expected. The attitudes of those solicited by links on Web sites were comparable to prior research (O'Neil et al., 2001) and those solicited from studyresponse.com were higher in support for the death penalty.

There were many differences between undergraduates and non-students in substantive analyses. Study 1 did not find a direct effect of sample type on verdict preferences but did find an indirect effect mediated through evaluations of the plaintiff's attorney. In Study 4 non-students consistently reached more favorable outcomes for the plaintiff, in terms of liability verdicts and damage awards. In addition, Study 4 found two- and three-way interactions between sample type and substantive manipulations, risk preferences, and attitudes. Some manipulations (e.g., timing of the scenario, whether a CBA was performed) had their hypothesized effects only among non-students. However, similar effects were not found in comparisons of non-student samples recruited from different sources (links on Web sites vs. a pre-constructed participant pool). Participants in Study 2 solicited by links on Web sites found the murder to be less heinous but otherwise there were no main effects or any interactions involving sample type.

Prior research in psychology and law has generally found equivalent results between undergraduates and non-students (Bornstein, 1999). When mean differences do exist, undergraduates are usually more lenient, as they were in Studies 1 and 4. Interactions involving sample type, which threaten the external and ecological validity of studies, are rare in previous psycholegal research. O'Neil and Penrod (2001) found an interaction involving sample type and attitudes toward the death penalty. However, in

that study undergraduates' attitudes were different than attitudes in previous studies, suggesting a peculiarity about the undergraduates in that study but conforming to previous research that showed different effects of attitudes between sample types. Still, the interactions in the present research involved attitudes, which could be expected, but also risk preferences and manipulations. The latter two types may depend on the subject matter of the study. Only two previous studies have addressed civil jury decision making in the context of jurors' reactions to CBAs and both had a non-student sample; something about such cases may cause different behavior between sample types. Results did show that non-students were more interested in the subject matter of the study, suggesting that non-students may have been particularly sensitive or undergraduates particularly disinterested. Future research is needed to determine whether research into CBAs should be conducted using both sample types.

Financial Incentives

Three studies included some variable relevant to offering participants (only non-students) some form of financial incentive. Study 1 manipulated whether participants were offered no incentive or the chance to be entered into a lottery. The design of Study 3 intended to manipulate the distribution of prizes in a lottery (one large prize or several smaller prizes) but a programming error led to this manipulation not being fully crossed with other variables. Study 4 did not manipulate the variable, but let participants choose whether they wanted to be entered into a lottery with one large prize or several smaller prizes. Overall, giving participants an increased chance of winning something by either offering them a lottery (Study 1) or their choice of a lottery with several smaller prizes (Study 4) were related to decreased dropout rates, but financial incentive variables

occasionally affected substantive analyses.

Both Study 1 and Study 4 found a relationship between offering a financial incentive and dropout rates. Of those who began the study, participants in Study 1 were more likely to finish the study if they were offered a chance at payment through a lottery (an increase of 10.4%). Frick et al. (1999) found a similar effect, but O'Neil and Penrod (2001) did not. The latter authors suggested that length of the study and subject matter might moderate the effects of financial incentives. Study 1 was similar in length to O'Neil and Penrod's study but had a different subject matter, suggesting the primacy of the effect of subject matter. Further, this effect was qualified by an interaction with the manipulation of method of obtaining consent, such that the effect of offering a financial incentive appeared more strongly among participants who had to go through additional procedures to give informed consent. The interaction between method of obtaining informed consent and offering a financial incentive should be studied in future research.

In Study 4, participants, especially non-students, who expressed a preference for a lottery with multiple, smaller prizes were more likely to continue than those with a preference for a lottery with a single, large prize. This effect was seen in two analyses, such that participants preferring multiple, smaller prizes were more likely to continue all the way through the case facts. However, the opposite effect was then seen in analyses of whether the participant continued beyond the jury instructions. Still, participants expressing either preference may participate regardless of the prize distribution offered. Unfortunately Study 3 was unable to investigate whether a manipulation of prize distributions influenced dropout rates. This should be addressed in future research.

Offering a financial incentive did not have a main effect on verdict preference in

Study 1 nor were there any interactions. O'Neil and Penrod (2001) did find a marginal effect of offering a financial incentive in their study of capital jury decision making, suggesting that effects of financial incentives may depend on subject matter. In Study 4 participants' lottery preference was not included by itself in substantive analyses; instead, the variable was combined with another variable indicating lottery preference to categorize participants as risk-seeking, risk-averse, or neither. As discussed below, participants classified as such as risk-seeking were more likely to reach outcomes favorable for the plaintiff (e.g., more liability verdicts, higher compensatory damage awards), as were those classified as risk-averse on some measures. Whether a manipulation of financial incentive would be similarly related to outcomes in product liability cases remains to be investigated in future research. As discussed above, participants who participate in expectation of being entered in a lottery with a single large prize may be more risk-seeking (see Kahnemann & Tversky, 2000) and thus reach pro-plaintiff outcomes in negligence cases.

Personal Information

Two studies manipulated the location when or manner in which personal information was collected from participants. Study 2 manipulated when participants solicited through links on Web sites had the option of entering their name, address and social security number for the lottery and manipulated when participants solicited through studyresponse.com's pool were asked to complete demographic questions. Study 4 again manipulated when participants had the option of entering name, address, and social security number to be entered in a lottery. Study 4 also manipulated whether participants were required through a JavaScript validation script to answer all

demographic questions or not. Overall, asking participants to enter personal information was related to dropout rates and effects on substantive analyses were found in one study.

Neither study found a significant main effect of asking for personal information on dropout at the point at which participants had the option of entering personal information. Study 4 did find an interaction such that there was an initial effect on dropout but just among participants who participated on the weekend. However, both studies found significant main effects such that there was increased dropout on the page after personal information was asked for among those who had not been asked for the information. The failure to find an increase in dropout contrasts with prior research (Frick et al. 1999; O'Neil & Penrod, 2001). One difference is that entering name, address, and SSN was optional in this study. Still, some participants may not have realized that, producing only a marginal increase in dropout.

Requiring participants to answer demographic questions produced a small effect on dropout. Although only five participants dropped out of the study at this point in Study 4, all five were in the condition where answers were required, a significant bivariate relationship. The effect on dropout in this study was minimal, however, because demographic information was not asked for until the last Web page of the study.

In Study 4, but not in Study 2, the variable of when personal information was asked for was related to several dependent measures. Participants who had not yet had the option of entering personal information gave higher punitive damage awards (when the natural log of awards was analyzed). Also, among these participants the relationship between negative attitudes toward the civil jury system and compensatory damage awards was opposite from what was expected---stronger negative attitudes were related

to higher compensatory damage awards. Similarly, participants' preference for a rare, more severe risk, which was expected to be related to lower damage awards, among these participants had no such relationship. These results may have been caused by something about the younger participants who completed the study only in the condition where personal information had not been collected. The lack of similar effects in Study 2 and in previous research suggests that a study's subject matter may moderate the effects of asking for personal information.

Table Design

Two studies manipulated the design of the tables that contained the dependent and other measures. Study 1 manipulated whether the tables appeared simple (several items per table, grayscale, and 600 pixels in width) or complex (multiple tables with alternating colors and extending off the participants' browser screen). Study 3 removed some of these confounds, separately manipulating whether the tables appeared with color and whether the table was fixed to 80% or 110% of the participants' browser screen resolution. Overall, table design had a small effect on dropout rates and a consistent effect on the scale data collected but ultimately did not affect the dependent variable or moderate other relationships with the dependent variable.

Study 1 found that simplified tables led to decreased dropout at several points in the study, though this relationship was moderated at one point by day of the week (i.e., reduced dropout only among participants on the weekend) and sample type (i.e., reduced dropout among non-students but increased dropout among undergraduates). The cumulative effect of simplified tables on those who began the study was significant but only among non-students, whereas for part of the study simplified tables led to decreased

dropout among undergraduates. Thus, among non-students simplified tables consistently led to increased dropout, but among undergraduates the complex tables provoked early dropout.

In comparison, Study 3 did not find a cumulative effect of table design on dropout or any effect at any one point. Unexpectedly, colorful tables were related to an initial decrease in dropout (i.e., more participants began the study when they saw colorful tables), followed by subsequent increases in dropout. Although not significant at any point or cumulatively, the manipulation of table width showed an opposite trend—an initial increase in dropout with wider tables followed by decreases. There was no significant interaction between these variables. As such, the dropout caused in Study 1 cannot be clearly attributed to the differences in table color or width. Instead, the important variable may be the organization of the tables (i.e., several items grouped together or location of response anchors for Likert-type scales) or some interaction between organization, size, and color. Similarly, differences in subject matter and number of questions on each page may have moderated the effects of table design. These variables should be addressed in future research. Nonetheless, these results reinforce the applicability to Web-based research of the importance of questionnaire design (Dillman, 2000).

The most consistent effect of table design was more variance on items and factor scores when the tables used colors and were too wide. For instance, in Study 1 Levene's test for homogeneity of variance found more variance in five items and one factor score when the item appeared in the large, colored tables. Multiple-group confirmatory factor analyses supported this finding, showing more unexplained variance in three items and

more variance in one latent factor when presented in the colorful, large table.

Results from Study 3 showed that the use of color and the table's width each had small, independent effects on means and amount of variance and that the effect of the use of color was slightly stronger. There was an average mean decrease of .38 (on a nine-point scale) when color was used and an average mean decrease .16 when the tables were too wide. The differences in Study 3 usually did not achieve significance but were consistent as lower means and more variance appeared in the colorful and too-wide tables. There was no such pattern in Study 1, where no mean difference was significant, suggesting some other difference between the tables (e.g., organization, number of questions, location of labels) as a moderator.

Although the colors of the table probably appeared similar across all participants, the manipulation of table width was confounded with participants' screen resolution. Since table with was manipulated as a percentage of the screen resolution, participants with larger resolutions (e.g., 1280x1024) would have wider tables, in terms of pixel size. Alternatively, and manipulation of table width according to width in pixels would produce tables of different relative width depending on screen resolution, with only some participants having too-wide tables. As such, the effect of the manipulation of relative width may have been tempered by the differences in absolute width. Screen resolution is a variable that should be accounted for in future research.

The effects of table design on dropout and the measurement of certain items were not translated into effects on the dependent variable or into moderation of other effects. Neither Study 1 nor Study 3 showed any main effect of or interaction involving table design variables.

Method of Obtaining Informed Consent

Three studies manipulated the manner in which participants indicated their informed consent to participate. The control condition simply had participants click on a button at the bottom of the consent form text to imply consent and begin the study. Study 1 had participants in the experimental condition click one button to imply consent and receive a passcode that they then entered in a text box and then click another button to begin the study. Study 2 embedded a passcode in the text of the consent form and participants had to read the consent form and then enter the passcode in a text box and click a button to begin the study. Study 3 manipulated whether participants were asked six additional questions about the information in the consent form on a page between the consent form and the first substantive page of the study. Overall, each method influenced dropout rates in the study at the point of the manipulation but then opposite effects were observed on latter pages, and there were no effects on the dependent variable.

Studies 2 and 3 showed an increase in the percentage of participants who dropped out of the study when something other than simply clicking a button was required of them (an increase of 16.0% in Study 2, 8.6% in Study 3). Although not significant, an opposite trend toward decreased dropout appeared on subsequent pages of the in these two studies. In Study 1 the initial effect and subsequent effects were moderated by whether participants were offered a financial incentive. Among non-students who were not offered a financial incentive, there was an initial increase in dropout (6.9%), followed by a decrease in dropout. However, over the rest of the study there again was increased dropout when a passcode had been required. Among non-students who were offered a financial incentive, there was a generally consistent but unexpected increase in dropout

when a passcode had been required. This suggests that participants who were offered a financial incentive *and* who had to go through additional procedures to give consent were particularly less likely to drop out of the study. Again, the interaction between method of obtaining informed consent and offering a financial incentive should be studied in future research.

Still, no study showed an effect of method of obtaining consent on any substantive analyses, neither a main effect nor any interaction. Thus initial differences caused by requiring more effort to give informed consent, if any, were evened out by the end of the study and were not related to results.

Day of the Week

Each of the four studies collected data from participants on weekdays and weekends. Consistent with prior research (O'Neil & Penrod, 2001), each study found that participants on the weekend were more likely to continue on at some stage of the study, usually the beginning, though this effect was moderated in Study 2 by sample type and in Study 4 by whether participants had had the option of entering personal information.

Results showed occasional, but not consistent, mean differences and differences in the amount of variance in items measuring attitudes and beliefs. Participants' attitudes toward participating in Web-based studies showed one mean difference and more variance on three items among participants on weekdays in Study 3 but there were no differences in means or variance in Study 4. Study 2 was the only study to show any mean difference in participants' study-relevant attitudes, and only Studies 1 and 2 found more variance in responses to certain items among participants on the weekend. This suggests that the subject matter of each study moderated the effect of when participants

participated.

Likewise, three studies investigated whether day of the week moderated the effect of any substantive variable but only Study 2 found any effects. Study 2 found that participants on the weekend were particularly skeptical of a defendant's mitigating evidence of child abuse but also were particularly influenced by expert testimony explaining a link between child abuse and the murder. Studies 1 and 3 did not find any similar effects. O'Neil and Penrod (2001) also found an interaction between day of the week and a manipulation of evidence in the context of a capital sentencing hearing. Again, this suggests the moderating effect of subject matter on methodological variables.

Location of Scales

Study 2 manipulated whether a 15-item scale measuring attitudes toward the death penalty appeared on the first or last Web page. The presence of the scale was not related to dropout at either point in that study. There were no mean differences on items or factor scores related to when the attitude scale was completed, but there was significantly more variance on six items and one attitude factor when the items appeared at the end of the study. Analyses of another study of capital jury decision making presented elsewhere (O'Neil & Claussen-Schulz, 2002) manipulated the location of the same scale, and found more variance on two items when it appeared at the end of the study. However, those analyses also showed more variance in one item when it appeared at the front of the study. One possible explanation of the differences is that the other study collected data only from undergraduates.

Manipulating scale location investigated whether asking about participants' attitudes toward the death penalty would prime those attitudes, increasing their effect on

sentencing verdicts. Scale location did not interact with any variables, including the attitude factor scores, thus not supporting the priming hypothesis. A similar Web-based study (O'Neil & Claussen-Schulz, 2002) found one interaction between location and participants' belief that a defendant sentenced to life without parole would nonetheless be released on parole, such that that belief had a stronger relationship to verdicts when the scale appeared at the front of the study. The case facts from the previous study may have made concerns about parole more applicable. Future researchers, in all subject matters, should be concerned about where attitude scales are located.

September 11

Study 2 was the only study that collected data both before and after September 11. This variable was hypothesized to be relevant to capital jury decision-making. There were no differences in dropout rate related to this variable. Analyzing on those participants solicited through links on Web sites (because all participants solicited from the pool participated after September 11), support for the death penalty and retributive attitudes increased after September 11. Also, more participants sentenced the defendant to death after September 11 but only in the condition where the defendant was not mentally ill, and before September 11 participants' with stronger retributive sentiments were more likely to vote for death, but an opposite trend was observed after September 11. Although such variables are not within the control of researchers, these results show how uncontrollable events can influence ongoing research.

Browsers, Operating Systems, and Referring Pages

Using server-side scripting, all four studies recorded what browser and operating system participants were using and three studies recorded (for non-students) the page that

referred the participant to the study. No study found any relationship between browser type and dropout rates, attitudes, or verdicts. Study 4 was the only study to find any effect of operating system, as participants using older operating systems (e.g., Windows 95) were more likely to begin the study and also had a greater percentage of females.

Three studies found that participants referred by either UNL's collection of links to studies (http://psych.unl.edu/psychlaw/research.asp) or from another study (as some studies that were done collecting data referred participants directly to other studies) were more likely to begin the study than participants referred either from other Web sites that collect links. Analyses also showed that participants referred from some UNL site had a greater percentage of females and were older. These participants also had more favorable attitudes toward participating in the study and toward Web-based research in Studies 3 and 4, but also were more likely to say that they were participating for money. The UNL site may have attracted a group of mostly female participants who are interested in regularly participating in studies for money. Finally, participants who reached a study through a search engine very rarely began the study and in no study did any such participant finish the study. Thus submitting links to search engines may not have any effect on sample size.

Attitudes toward Participating

Studies 3 and 4 asked participants about their attitudes toward Web-based research and their motivation for participating. Non-students showed a greater interest in the subject matter of Study 4 and were slightly more concerned about keeping personal information safe over the Internet. Also, consistent differences in attitudes appeared between referring pages. Univariate analyses revealed differences in attitudes and

motivation between those who finished the study and those who did not but the only

variable related to dropout rates in multivariate analyses was whether participants

understood the risks and benefits associated with the study, where both patterns of

dropout discussed above were observed. The items asked differed conceptually but

covaried strongly, thus all tapping into some general measure of attitudes or motivation.

In addition. the effects on dropout observed in Studies 3 and 4 were independent of the

measured attitudes and motivation. As explained by Rogelberg, Fisher. Maynard. Hakel

and Horvath (2001), attitudes toward research are important determinants of who

participates. Thus. future research should add additional measures of these attitudes and

motivations to further explain causes of dropout.

Length of Study and Substantive Variables

Each study also manipulated substantive variables. Such manipulations changed

both the content and the length of the stimulus materials. Studies 2 and 4 found different

dropout rates between conditions of manipulated substantive variables. Study 2 showed

that participants who received case facts describing child abuse and those who received

evidence of mental illness were each less likely to finish the study. Study 4 showed that

participants who read a case scenario when a CBA was performed were less likely to

finish. Although the increase was not a lot (only one paragraph), in these studies

increased length was related to increased dropout.

Study length has been suggested as a moderator of the effect of methodological

variables (O'Neil & Penrod, 2001). Longer studies will have fewer cumulative effects of

methodological variables because of the "evening out" effect discussed above with

variables that are present at one point of the study. Thus, on average the final sample in

longer studies will be more motivated to participate than the final sample in short studies. Short studies wanting a highly motivated sample may employ Reips's (1999. cited in Reips, 2000) "high hurdle technique," in which variables that provoke dropout are grouped together at the beginning of the study. The present research would suggest adding variables associated with financial incentive. table design, and method of obtaining informed consent to that technique.

Web vs. Paper-and-Pencil Results

No study allowed a direct comparison between data collected over the Web versus with paper and pencil. Study 1 collected data both ways but the Web study was much shorter and contained only one case scenario. Still. the substantive results between the two versions were compared and no main effects on verdicts or interactions with other variables were found. Study 3 also collected data both ways. but the Web-based study had non-student participants while undergraduates completed the paper-and-pencil version. The only difference found was that there was significantly more variance among the Web-based. non-student sample on three items and two factor scores measuring attitudes toward repressed memories and therapy. The difference in variance is consistent with prior research that found increased variance in a Web-based. non-student sample (see Krantz & Dalal. 2000). Although no direct comparison was made, the lack of differences in Study 3 supports the validity of Web-based research as the replacement for the paper-and-pencil study using undergraduates.

Substantive Variables

Opening Statements

Different versions of the plaintiff's and defendant's opening statements were

related to differences in ability to understand the statements and in evaluations of the

respective attorneys, but did not influence mock jurors' verdict preferences. The lack of

effects may be attributable to the specific case scenario. The majority of mock jurors

voted for the defendant after the plaintiff's opening statement (65.6%), and even more

did so after the defendant's opening statement (82.7%). As such, producing a clearer,

more understandable statement of the facts may have only succeeded in showing that the

defendant was not responsible for the accident. For other cases where the defendant is

more at fault a "good" opening statement may produce different effects.

Results did show, however, that evaluations of the attorneys and sympathy toward

the plaintiff and defendant had significant effects on verdict preference. Attitudes about

attorneys and the civil jury system did not. Whether attitudes influence verdicts should be

investigated in other research with different case scenarios. Finally, mock jurors who

favored the plaintiff after the plaintiff's opening statement gave harsher evaluations of the

defense attorney and found his opening statement to be harder to understand, showing

how pre-formed opinions can influence subsequent information processing.

Capital Jury Decision Making

Mock jurors in Study 2 did not require expert testimony describing a link or

"nexus" between proffered evidence and the murder in order to give that testimony a

mitigating effect, at least for evidence of the defendant's mental illness. In fact, mock

jurors sentenced the defendant to death less often even though the defendant's mental

illness developed and was diagnosed only *after* the murder. Additional expert testimony

about delusions at the time of the murder did not have any additional mitigating effect.

Also, the mitigating effect became stronger after September 11 as jurors sentenced the

mentally ill defendant to death at the same rate before and after, but after September 11 sentenced the not mentally ill defendant to death more often.

There was no similar pattern of main effects regarding the evidence of child abuse. However, among participants who believed that child abuse could make a murderer less responsible evidence of child abuse had a mitigating effect, and there was no additional effect of the added expert testimony. Participants who believed more that child abuse could not make a murderer less responsible did not give evidence of child abuse a mitigating effect, but were sensitive to the additional expert testimony. As such, some participants did give evidence of child abuse a mitigating effect without an explicit link between the evidence and the murder.

Consistent with prior research, some but not all purported aggravating and mitigating factor had their hypothesized effect. Heinousness of the murder, the defendant's young age, the defendant's mental illness, and that the defendant would be a well-behaved inmate had significant relationships to sentencing verdicts. That the defendant was "emotionally immature and impulsive," which was listed as a mitigating factor, in fact had a significant aggravating effect. Also consistent with prior research, participants' general support for the death penalty had the strongest effect on verdicts. The effect was partially mediated by the mitigating factors of the defendant's age and the defendant's impulsivity as well as sympathy for the defendant, but the effect was primarily direct.

Jurors and Cost-Benefit Analyses

While the manipulated variables generally did not have significant main effects, there were numerous interactions involving these variables and measures of participants'

risk preference, willingness to pay for additional safety, and attitudes. Results generally did not support the hypothesis that performing a CBA would be related to pro-plaintiff outcomes. Such an effect was only seen in analyses of compensatory damage awards, and only among those who found the defendant liable, only among non-students, and only in the pre-sale scenario and high-risk condition. A recent conference presentation (O'Neil. Page. Bornstein. & Penrod. 2002) suggests that the comparison between a company doing nothing and a company performing a CBA will not show significant effects. Instead. O'Neil et al. (2002) found a significant pro-plaintiff effect of the company performing a CBA only when compared to the condition where a company puts a price on life (and thus states how much it will cost to not fix the defect). In such a condition the company may be perceived as being punished enough through the costs of settling lawsuits. and performing a CBA shows jurors that the company will in fact be saving money and thus should be punished through liability and damage awards.

Although verdicts on each cause of action were supportable in both the pre- and post-sale scenarios. it was expected that the pre-sale scenario would produce fewer verdicts on the post-sale negligence cause of action and vice versa. This pattern was seen in that the post-sale scenario led to fewer verdicts for the plaintiff on the pre-sale negligence claim. but only among non-students. However, there was no such pattern on the other two liability verdicts. Also, certain variables had effects only in the pre-sale scenario. As noted above. performing a CBA had a pro-plaintiff effect on compensatory damage awards only in the pre-sale scenario: the effect of sample type on punitive damage awards also appeared only in the pre-sale scenario. These effects demonstrate how jurors treat pre- and post-sale scenarios differently. Future research into civil jury

decision making needs to take these differences into account.

The third substantive manipulation addressed the severity of the risk known to the defendant, and, according to the law and the hypothesis, increased knowledge of risk should be related to increased findings of liability. This manipulation appeared in an occasional interaction but generally had no effect on liability verdicts or damage awards, probably because the manipulation did not influence participants' perception of defendant's knowledge of risk. However, participants' perception of knowledge of risk was related to verdicts, such that increased pre-sale knowledge led to more pro-plaintiff outcomes in the pre-sale scenario, and that increased post-sale knowledge led to more pro-plaintiff outcomes in the post-sale scenario. Future research should include a stronger manipulation of severity of risk to investigate whether jurors are sensitive to different risk levels.

In addition to the manipulated variables, Study 4 investigated the relationship between participants' risk-taking, concerns about safety, and attitudes, all of which had effects on verdicts and damage awards. First, it was hypothesized that participants classified as risk-averse would be favorable to the plaintiff because the defendant had exposed the plaintiff to more than the minimal risk possible, but that those classified as risk-seeking would approve more of the defendant's actions because the defendant had taken a risk. Results supported the former hypothesis, but effects relevant to the latter showed opposite effects. Although not significant in every analysis of liability verdicts, there was a consistent pattern that both participants classified as risk-seeking *and* participants classified as risk-averse found for the plaintiff more often than participants classified neither way. One possible explanation is that participants classified as risk-

seeking may be expecting big payoffs (reflected in the larger but less common lottery prize) and therefore may be favorable to a suing plaintiff. Alternatively, participants classified as risk-seeking may view the defendant's risky actions more favorably but nonetheless expect the defendant to compensate consumers when they are injured by a product. Future research should use different measures of participants' risk seeking behavior to clarify whether the observed effects come from risk preferences or some other attribute.

Participants' willingness to pay for additional safety was measured by one question that asked how much more, in addition to the base cost of $10,000 for building a deck, they would pay to guarantee that it would never collapse. It was hypothesized that participants willing to pay more for additional safety would disapprove more of the defendant not paying to assure additional safety. There was no main effect of this variable, but the interaction between how much participants were willing to pay for additional safety and the scenario timing manipulation appeared in several analyses. In the pre-sale scenario, participants who were willing to pay more for safety were more likely to vote for the plaintiff while in the post-sale scenario participants there was no such relationship. This interaction should have been expected because consumers do not have to pay anything for changes or recalls of a product after it has been sold, and the question did not ask if participants would pay anything in a post-sale scenario. This finding also supports the hypothesis, addressable in future research, that some people will accept a potentially dangerous product but expect a company to compensate consumers when they are injured. Future research should continue to investigate this variable, as well as how much participants would be willing to accept in exchange for using a more

dangerous product (see Geistfeld, 2001).

Participants were asked whether they would prefer to make a product that had a rare chance of a severe injury (1-in-100,000 chance of $500,000 injury) or a more common chance of a less severe injury (1-in-1000 chance of $5000 injury). It was hypothesized that those preferring the rare, severe injury would favor the defendant because that is what occurred in the case, but that hypothesis was not supported. On all three liability verdicts and whether jurors would impose punitive damages this variable interacted with jurors' attitudes toward the civil jury system and there was one three-way interaction between those variables and sample type. Among those who preferred the rare, severe injury, stronger negative attitudes were related to fewer pro-plaintiff outcomes, but the opposite effect was seen among those who preferred the more common, less severe injury. Alternatively, preference for the common, less severe risk was related to fewer pro-plaintiff outcomes among those with favorable attitudes toward the civil jury system, but among those with negative attitudes, those who preferred the rare, severe risk were less likely to reach pro-plaintiff outcomes.

This interaction may reflect two types of jurors that are favorable toward plaintiffs. First, some jurors who have more respect for the civil jury system may prefer products that rarely injure consumers but also expect that when products are defective and injure consumers the manufacturer or seller will compensate the injured consumer. Thus if there is a lawsuit involved, these jurors are more likely to find for the plaintiff in order to compensate him or her. Second, other jurors who have negative opinions about the civil jury system are averse to severe injuries and also expect that the flawed system will compensate the injured consumer, possibly more than necessary. These jurors may

be projecting what they expect will happen instead of what they actually want to happen. The three-way interaction suggests that non-students are more likely to be the former and undergraduates the latter. Future research should investigate whether such jurors can be identified.

Conclusions

Web-based researchers should consider the methodological variables associated with doing research over the Web. Methodological variables had consistent effects on dropout rates. Although variables other than sample type were generally not related to differences in sample characteristics in terms of demographic, attitudinal, and motivational variables and had infrequent effects on results, differential dropout may cause differences in some variable not measured in these studies that could affect results of other studies. The effect of sample type observed in Study 4 should also be investigated in future research to see if there is a problem with validity of research conducted on undergraduates. Finally, future research should address the effects of methodological variables across many substantive domains before any firm conclusions about methodological variables can be made.

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Table 1

Total and Direct Effects for Path Analysis for Study 2

Variable	Total Effects				Direct Effects			
	B_1	B_2	β	z	B_1	B_2	β	z
Level 1 Methodological Variables								
Weekend	-.34	-.28	-.09	-1.39	-.56	-.28	-.10	-1.84
Post-9/11	-.27	-.18	-.08	-1.03	-.11	-.06	-.02	-0.37
From pool	-.36	-.24	-.12	-1.26	-.10	-.05	-.02	-0.28
Extra consent	.12	.08	.04	0.76	-.01	-.01	-.00	-0.06
Scale at front	-.08	-.06	-.03	-0.52	-.35	-.18	-.09	-1.84
PI at front	.05	.04	.02	0.22	.00	.00	.00	0.00
Demog. at front	-.01	-.01	-.00	-0.04	.10	.05	.02	0.39
Level 1 Substantive Variables								
DQ-Always	.73	.49	.13	2.42	.36	.18	.05	1.01
DQ-Never	-.17	-.11	-.04	-0.32	-.02	-.01	-.00	-0.03
No MI	.60	.40	.17	2.67	.61	.30	.13	2.33
MI + Link	.25	.17	.08	1.14	.11	.05	.03	0.43
No CA	.23	.18	.08	1.25	.37	.18	.09	1.38
CA + Link	-.30	-.20	-.09	-1.45	-.62	-.31	-.14	-2.45
Support for DP	.10	.07	.64	6.56	.09	.05	.45	5.10

Variable	Total Effects				Direct Effects			
	B_1	B_2	β	z	B_1	B_2	β	z
Retribution	-.01	-.01	-.04	-0.67	.00	.00	.00	0.08
Deterrence	-.01	-.01	-.05	-0.82	-.01	-.01	-.04	-0.74
Cost	-.02	-.01	-.07	-1.05	-.02	-.01	-.06	-1.03
Parole	.00	.00	.01	0.23	.00	.00	.00	0.09
MI can mitigate	.01	.00	.01	0.18	.00	.00	.00	0.02
CA can mitigate	.06	.04	.11	1.87	.03	.01	.04	0.77
Level 2 Variables								
Pecuniary gain	.07	.04	.09	1.74				
Heinousness	.23	.11	.25	4.36				
Well-behaved	-.11	-.06	-.12	-2.13				
Remorse	-.10	-.05	-.10	-1.80				
Young age	-.18	-.09	-.23	-3.87				
Impulsive	.10	.05	.12	2.11				
Mentally ill	-.09	-.04	-.11	-2.13				
Diff. childhood	.09	.05	.11	1.76				
Sympathy for Δ	-.05	-.03	-.16	-2.45				
Empathy for Δ	.02	.01	.04	0.76				

Note. Variables with z-values greater than 1.96 are significant, $p < .05$.

Table 2

Summary of Results Regarding Manipulated Methodological Variables.

	Dropout	Attitudes	Results
Sample Type	1 & 4: NS more likely to drop out than UG 3: NS from links more likely to drop out than NS from pool	1 & 4: Mean differences and more variance among NS 3: Mean differences and more variance among NS from links	1: One indirect ME. no interactions 3: One ME on mediator. no interactions 4: Several main effects and interactions between NS and UG
Financial Incentives (FI)	1: Ps offered FI less likely to drop out than Ps not offered FI 4: Ps preferring multiple lottery prizes less likely to drop out	1: No differences 4: Ps preferring multiple prizes chose risky lottery choice less often.	1: No ME or interactions 4: ME of risk-seeking Ps more likely to have pro-plaintiff outcome.
Personal Information (PI)	2 & 4: ME not sig. at point where PI asked for. but increase in dropout after that point.	2 & 4: No differences	2: No ME or interactions 4: ME and interaction with attitudes on punitive awards

	Dropout	Attitudes	Results
Personal Information (PI)	**4:** Requiring demographic info had minimal ME		
Table Design	**1:** Simple tables led to less dropout, especially among NS **3:** Trend that color tables related to initial decreased dropout & too-wide tables to increased dropout, then opposite effects	**1:** No ME but more variance from complex tables **3:** Slight differences & more variance with color & too-wide	**1 & 3:** No ME or interactions
Method of Consent	**1:** Interaction with FI: increase in dropout if had to do more than click button among Ps not offered FI **2 & 3:** Increase in dropout if had to do more than click button	**1, 2 & 3:** No differences	**1, 2 & 3:** No ME or interactions

Note: Numbers in bold type indicate the study number. Abbreviations used: ME = main effect; NS = non-students; Ps = participants; UG = undergraduates.