

JURY SIZE AND VERDICT CONSISTENCY: “A LINE HAS TO BE DRAWN SOMEWHERE”?

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This research tests the simulated impact of Supreme Court decisions which allow for smaller than twelve-member juries. It identifies variation in judicial output that results from competing operating structures of jury decision making. The research employed a quasi-experimental design to address important problems of simulation, such as structural and functional verisimilitude. The sample consisted of 110 juries composed of nearly 1000 jurors. The findings indicate that a jury's size affects its behavior. Larger juries hang more often than smaller ones do. The degree to which this avoids the committing of a Type I or Type II judicial error remains to be seen; nevertheless, the Court was wrong in assuming that there are no differences in the behavior of twelve- and six-member juries.

I. INTRODUCTION

The Supreme Court of the United States has frequently been called upon to assess the validity of changes in the size of juries. Although the Constitution does not specify jury size, over the years the number twelve has become widely accepted as the “proper” size. The Supreme Court first dealt with the issue of jury size in 1898, when it held in *Thompson v. Utah* that a twelve-member jury was constitutionally required in federal criminal cases. A year later, it held in *Capital Traction Co. v. Hof* (1899) that a twelve-member jury was also required in federal civil trials. At the same time, in *Maxwell v. Dow* (1900), the Court rejected a challenge to the constitutionality of an eight-member jury in a state criminal case. The Court held that the Sixth Amendment's right to trial by jury was not applicable to the states by virtue of the Fourteenth Amendment, and in dictum it indicated its approval of the smaller jury in state cases.

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In 1968 the Court ruled, for the first time, that the Sixth Amendment's guarantee of a right to trial by jury *was* applicable to the states in all nonpetty criminal cases (*Duncan v. Louisiana*). This ruling opened the way for a direct challenge to smaller juries in state courts, and such a challenge occurred almost immediately: the case was *Williams v. Florida* (1970). At issue was a Florida statute permitting six-member juries to try all noncapital criminal cases. The Court found that although the size of a jury at common law was twelve, the Constitution should not be presumed to have incorporated that norm. If the framers had intended to stipulate the proper size of a jury, they would have done so. By a seven-to-one vote, the Court held that six-member juries are constitutional in noncapital state prosecutions. Writing for the majority, Justice White observed:

[T]he fact that the jury at common law was composed of precisely 12 is a historical accident, unnecessary to effect the purposes of the jury system and wholly without significance "except to mystics."

A few, largely descriptive, studies of the effect of jury size (Wiehl, 1968; Tamm, 1962; Cronin, 1958; Note, 1958) were cited to support the Court's conclusion that ". . . there is no discernible difference between the results reached by the two different sized juries."

The *Williams* case stimulated a veritable industry of jury studies (Mills, 1973; Stoeber, 1972; Kessler, 1973; Bermant and Coppock, 1973). This new research was in turn cited in a 1973 case, *Colgrove v. Battin*, which upheld the constitutionality of six-member juries in federal civil cases. By a five-to-four vote, the Supreme Court held that 12-member juries were not required by the Seventh Amendment's guarantee of trial by jury in civil cases.

Three states—Georgia, Louisiana, and Virginia—then enacted statutes reducing the size of some criminal court juries to five, and the constitutionality of five-member juries was challenged in *Ballew v. Georgia* (1978). The justices were unanimous in holding that juries of less than six persons in state criminal trials involving nonpetty offenses were unconstitutional. But there was no agreement on a common opinion. Only one justice, Stevens, joined Justice Blackmun in the main opinion; three justices—Powell, Burger, and Rehnquist—explicitly disdained Blackmun's "heavy reliance on numerology derived from statistical studies," noting that it had not been tested by the mechanism of the adversary process

and that “the studies relied on merely represent unexamined findings of persons interested in the jury system.”

Blackmun’s opinion was unusual in its synthesis of, and reliance upon, social science studies of jury size. It was, in fact, largely accurate in its portrayal of these studies, which showed important differences between six- and twelve-member juries. The opinion, and certainly the studies, thoroughly undermine the factual basis of the Court’s judgments in *Williams* and *Colgrove*. But, perhaps because of the pull of *stare decisis*, Blackmun concluded perversely that only juries of *less than six* members were invalid in state criminal cases.

While we adhere to, and reaffirm our holdings in *Williams v. Florida*, . . . studies, most of which have been made since *Williams* was decided in 1970, lead us to conclude that the purpose and functioning of the jury in a criminal trial is seriously impaired, and to a constitutional degree, by a reduction in size to below six members (*Ballew*, 1978: 239).

Thus, research showing the difference between six- and twelve-member juries became the basis for supporting the constitutionality of six-member juries. No research was cited—none is available—which distinguished between five- and six-member juries. Powell’s concurring opinion made no pretense at logic: “. . . the line between five- and six-member juries is difficult to justify, but a line has to be drawn somewhere if the substance of the jury trial is to be preserved.” The research reported here makes a modest attempt to examine the accuracy of the Court’s “line drawing” by studying the effect of a jury’s size on its verdict, and on the process through which that verdict is reached.

II. MEASURING THE EFFECTS OF JURY SIZE

Jury research faces numerous practical and legal obstacles. Negative responses to efforts by the Chicago Jury Project to tape actual jury deliberations suggest that maintaining the confidentiality of jury deliberations is deeply ingrained in our culture. It is also protected by federal law¹ and Supreme Court doctrine.² With direct observation effectively foreclosed, jury research has proceeded along other paths. The “real world” approach,³ mathematical modeling,⁴ survey research,⁵ and simulation constitute the main approaches and techniques.

¹ 18 USC § 1508. One phase of the Chicago Jury Project involved the taping of actual jury deliberations in Wichita, Kansas. This raised such an uproar about secrecy violations that a federal law prohibiting such direct observation was soon passed.

² See *Sinclair v. U.S.* (1929), where the Court frowned upon the attempts to discover what transpired during a jury’s deliberations.

³ See Beiser and Varrin (1975) and Mills (1973) as examples of jury research employing the “real world” approach.

Each has its advantages and limitations. The research reported in this article seeks to improve upon one of the most maligned yet potentially useful approaches—the quasi-experimental design of jury simulation.

The concept of simulation often evokes disquiet among social scientists, even though it is a commonly used technique in the biological sciences (Zeisel, 1973: 118). The basic drawback of simulation, according to some critics, is that some simulations lack realism and what Campbell and Stanley (1963) have called “external validity”: the ability to generalize beyond the constraints of the limited simulation (Lempert, 1966). Is it ever really possible, they ask, to learn about the operation of a real institution or process merely by observing and measuring the behavior of a contrived substitute? There is much that is valid in this critique, but it is also true that problems inherent in operationalizing simulation techniques can be minimized with more rigorous simulation designs (Bermant *et al.*, 1974).

Jury simulations need to control three procedures especially vulnerable to bias: selection of a suitable case; maintenance of structural verisimilitude; and resolution of the reality critique through functional verisimilitude. Without adequate controls, data of questionable validity and reliability is likely to be produced.

Selection of a Suitable Case

Almost without exception, jury simulations have used cases in which the defendant’s guilt or innocence was clear. But this is unrealistic. If a large majority of a project’s participants initially evaluate the defendant as either guilty or innocent (Davis *et al.*, 1975), little variance can be found in group verdicts. Given the predominance of guilty pleas in criminal cases, and settlement rates in civil cases, jury trials are relatively rare events. They are not likely to occur where there is absolutely no doubt about the outcome. Simulations, to be meaningful, must utilize cases in which the defendant’s guilt or innocence (or level of responsibility in a civil case) is at least uncertain. In this research, the search for a “balanced” verdict was fulfilled by using the transcript from a real murder trial in which the jury hung and its vote was divided among

⁴ See Grofman (1979) and Penrod and Hastie (1979) for excellent reviews of mathematical modeling and jury decision making.

⁵ See Reed (1965) as an example of survey jury research.

various alternative verdicts. A pretest supported these expectations.⁶

There is always a question of whether to use a criminal or civil case. Since another aspect of this research deals with the effects of possibly prejudicial pretrial publicity on juries, and since this phenomenon is predominantly associated with criminal cases, a criminal case was chosen.

Finally, many jury simulations fail to present sufficient evidence to allow variation in juror recall of evidence. Most real trials take longer than the conventional 20 to 30-minute simulation. In this study participants were given a 90-minute presentation—time enough, I thought, for them to become distracted and subsequently miss evidence as jurors do in real trials.

Maintenance of Structural Verisimilitude

Structural verisimilitude can be defined as the product of efforts to tailor “methods and means of subject selection more closely to the realities of courtroom practice” (Bermant *et al.*, 1974: 224). In some research this is referred to as experimental realism. Most jury simulations, however, employ (or coerce?) students as subjects. Coerced samples rarely behave naturally. Students may be less concerned with the effectiveness and outcome of the project than adults would be, and consequently they may take their role as “jurors” either too lightly or with undue seriousness of purpose. Students do not, for the most part, pay the taxes that support courts and juries, and they are probably more shielded than most adults are from the fear and effects of crime. They are less likely to become “involved” in the simulated trial, and they may be too casual in trying to interpret the evidence. If students are also impatient and inclined to end the experiment as soon as possible, the number of hung juries that results may be lower than might otherwise occur. Obviously, to the extent that any of these suppositions about student jurors is correct, the dynamics of a simulation experiment will be adversely affected.

A number of studies reveal that the behavioral orientation of students differs from that of the general population (Kessler, 1975; Simon and Mahan, 1971; Forston, 1972). Students tend to be more liberal in their political views, and their education may train them to be more attentive during presentations. This training may result in more efficient recall of evidence and better management of that evidence during deliberations.

⁶ There were 77 guilty votes, 67 not guilty votes, and 39 jurors who refused to commit themselves.

Management of evidence may be critical to the outcome of deliberations, and the distribution of the values of the dependent variable may differ between students and real jurors. Any substantial fluctuation in the dependent variable between samples may mask or exaggerate the effect of the independent variable. For example, if there is no variance in the ability of students to reach a verdict or hang (e.g., if they always reached a verdict), then determining the impact of jury size is impossible.

For these reasons, and to offer contrast with most jury simulations, the participants in this project were drawn from the jury rolls in Fayette County, Kentucky. Lawyers, judges, individuals currently involved in police work, a member of Congress, doctors, and those who had served on a real jury during the preceding year were excluded from participation in the project, since all would have been excluded by statute from actual jury service.

Since there was an element of self-selection in choosing the participants in the project, the problem of generalizing from a possibly unrepresentative sample is clearly present. Jury research generally does not deal with this problem which is, of course, a common one in all survey research. Jurors who agreed to participate were compared to those who did not. Controlling for sex, race, age, and political party affiliations, no significant differences between the two groups were found.⁷ The final sample totaled 928 jurors.

A second problem for jury simulations is to provide a setting which, as much as possible, duplicates the courtroom milieu. This simulation took place in a courtroom, contrary to common practice. For example, Bray (1976) found that only nine out of 45 studies took place in a real courtroom.

A related issue is the mode of presentation. The choice was between audiovisual presentation and a "live" (dramatically re-enacted) trial. Studies show few differences in the degree of motivation or interest of jurors, and in their retention of trial-related information, between a live trial and a videotaped presentation (Bermant and Jacobovitch, 1975; Miller and Fontes, 1977). Needless to say, audiovisual techniques are more successful in simulating reality than either written summaries or use of audio equipment only (Juhnke *et al.*, 1979; Bermant *et al.*, 1975; Kessler, 1975;

⁷ Research suggests that some demographics do affect juror behavior (e.g., Marston, 1924; Strodtbeck and Mann, 1956; James, 1959; Reed, 1965; Kalven and Zeisel, 1966; McGuire and Bermant, 1977).

Bermant *et al.*, 1974). A live trial is still preferable. But live trials have an additional and serious problem of exact presentation reproducibility. In this study, 110 performances would have been required. Consequently, audiovisual presentations were employed.

A third problem involves the decision-making process. Bray (1976) found that only half of the jury studies permitted actual juror deliberation. Clearly, asking a group of individuals to pass judgment on a simulated case without group interaction belies the notion of simulation. If one intends to study the jury as an institution, mock jurors must be allowed to act as a group. Indeed, simulating a deliberative setting and process is *the* most important element in maintaining structural verisimilitude (McGuire and Bermant, 1977; Myers and Kaplan, 1976; Hans and Doob, 1976; Izzett and Leginski, 1974; Davis *et al.*, 1975). Juries in this study were permitted to deliberate for an unlimited amount of time.

Functional Verisimilitude: A Need for Efficacious Decision Making

The most frequent complaint about jury simulations is that participants know their decision will not affect the defendant, and therefore has no consequence. If functional verisimilitude is to be maintained, a researcher must substitute a set of consequences for the one that is lost. In this case the substitute object of efficacy was the judicial system as a whole. Kentucky had recently adopted a judicial reform amendment to the State Constitution. Participants were informed that the results of this project would be forwarded to the Kentucky Administrative Office of the Courts for its consideration in implementing that amendment. They were also told that the U.S. Department of Justice would receive a copy of this research.

Several additional steps were taken to help insure conscientious participation. First, the taping of the sessions made participants more accountable for their behavior. Second, jurors were told their verdict would be compared to that of the real jury. This might have encouraged them to strive for what they thought was the "correct decision." Third, an initial vote was taken at the outset of the deliberations. As in real deliberations, a juror's self-esteem is at stake in defending a personal preference—a defense which may reduce the impact of the artificial setting. Finally, a juror's investment of time in the experiment is probably the most important factor

encouraging conscientious participation. The evidence suggests that our jurors did, in fact, undertake their task conscientiously.

III. OPERATIONALIZING JURY SIZE AND THE DIRECTION OF A VERDICT

Since the project was originally designed to test the simulated impact of *Williams v. Florida*, jurors were alternately assigned to either six- or twelve-member juries. However, some jurors never showed up, and numerous juries had odd numbers of jurors, ranging from four to twelve members. The Supreme Court's coincidental but judicious timing of the *Ballew v. Georgia* decision made it realistic to consider the behavior of juries with fewer than six members. Juries approximating twelve members were considered in the same category as twelve-member juries, partly for the sake of convenience and partly because the Court itself implied that there was little or no difference among sizes of six and above. The study proceeded, therefore, with 14 four- and five-member juries, 42 six-member juries, and 46 ten-, eleven- and twelve-member juries (of which 35 had twelve members).⁸

A jury's verdict can take three forms: guilty, not guilty, or hung. Juries that initially reported out deadlocked were sent back twice to try and reach a decision. However, if they returned a third time without a verdict, then a hung jury was declared.

Prior research suggests that variation in decision alternatives affects juror behavior (Vidmar, 1972). Therefore, jurors were provided an opportunity to vote "undecided," for two reasons. First, in a real setting jurors have a right to abstain. In fact, several jurors in the real case simulated by this project abstained on the final ballot. Second, we wanted to avoid situations where participants felt obligated to reach a verdict because they were participating in an experiment. Providing jurors with an "undecided" option may have mitigated an expectation that their duty was to reach a definitive verdict (i.e., guilty or not guilty)—a verdict that might not have been made in a realistic setting.

The first ballot was taken immediately prior to deliberation. Real juries often take a ballot before deliberating in order to

⁸ Eight nine-member juries were excluded from the analysis because of the small sample size, as well as the fact that the Court never dealt with the issue of nine-member juries.

ascertain exactly where the group stands.⁹ Deliberations were initiated if a definitive verdict was not reached on the first ballot. Future ballots were taken whenever the jury foreperson thought it appropriate.

IV. EFFECT OF JURY SIZE ON THE DIRECTION OF THE VERDICT

Controversy about the effect of a jury's size on the direction of its verdict is grounded less in theory than in speculation. Many objections to *Williams v. Florida* were premised on the assumption that smaller juries would convict at a greater rate. In *Williams* the Court rejected this thinking.¹⁰

It might be suggested that the 12-man jury gives a defendant a greater advantage since he has more chance of finding a juror who will insist on acquittal and thus prevent conviction. But the advantage might just as easily belong to the State, which also needs only one juror out of twelve insisting on guilt to prevent acquittal.

In fact there is no *a priori* reason to suspect that the advantage or disadvantage of size will fall either to the state or to the defendant. That there is no inherent advantage or disadvantage of jury size is supported by the results reported in Table 1. In this study, there was no difference whatsoever between six- and twelve-member juries. It is true that other studies have found size to be associated with a propensity to convict. The most frequently cited are those employed by the Court to justify its *Colgrove* decision (Bermant and Coppock,

Table 1. Jury's Final Definitive Verdict by Jury Size

Jury's Final Definitive Verdict	Jury's Size Category	
	Six-Member	Twelve-Member
Guilty	41% (n.16)	41% (n.13)
Not Guilty	59% (n.23)	59% (n.19)

n. 71*

Corrected Chi. Sq. = 0.0 (1 df)

Chi. Sq. p. < 1.0

Kendall's Tau B = .004

* This Table includes only six- and twelve-member jury categories which had definitive verdicts.

⁹ See Valenti and Downing (1975: 659 n. 1) for further substantiation of this point.

¹⁰ This particular example must be restricted to juries working under a unanimous rule.

1973; Kessler, 1973; Mills, 1973; Stoeber, 1972). However, these studies are plagued by methodological inadequacies already discussed at length in the literature and substantiated by other critiques (Penrod and Hastie, 1979; Gerbasi *et al.*, 1977; Saks, 1977; Lempert, 1975; Zeisel and Diamond, 1974; Diamond, 1974).

Most research to date relates jury size only to the final vote. Low correlation coefficients are usually interpreted to mean that size has no effect on jury behavior. But this procedure tells us nothing about the effect of size on the deliberations of the jury. This can only be done with reference to some measured predisposition at Time₁, when compared to the verdict at Time₂.

For example, let us assume that in a particular case there is no relationship between jury size and verdict (i.e., opinion at Time₂), based on correlation techniques. This would seem to support the assumptions of *Williams* and *Colgrove*. However, further analysis may reveal a strong relationship between jury size and its vote prior to deliberations (i.e., opinion at Time₁). If we want to identify causality between size and verdict we would now have evidence to suggest that something transpired between Time₁ (where a strong relationship existed) and Time₂ (where no relationship existed)—an event attributed to some difference between the two times. This difference would be the group dynamics associated with different group sizes. Therefore, an important potential effect of size would be masked if we only examined its impact on Time₂ (i.e., its verdict). Simulations provide us the opportunity to study “change” by looking at “panel data.”

Given the goal of attributing change in opinion to jury size, Valenti and Downing (1975) provide us with an innovative perspective on the jury size debate. They argue that the critical factor is not the overall size of the jury which is probably related to the relative size of the minority, but rather the absolute size of the minority in each jury. Their argument is based on Asch's (1952) research which suggests it is the minority's *absolute* (i.e., a fixed number), not its *relative* size (i.e., a function of the jury's size) which reinforces its resistance to conforming. Once the size of the minority reaches a certain level (i.e., three), increasing its size by increasing the jury's size will not improve its resistance to majority pressures. It is merely “[p]roviding the minority member with an ally [that] greatly increase[s] his resistance to persuasion by the majority” (Valenti and Downing, 1975: 657). Other research also supports this opinion (Hare, 1976; Davis *et al.*, 1975;

Rosenblatt and Rosenblatt, 1973; Zeisel, 1972; Thomas and Fink, 1963).

If we adopt Valenti and Downing's (1975) operational definition of a viable minority (i.e., at least two jurors), how can such a minority affect jury behavior? A viable minority can behave in a variety of ways: (1) it may refuse to conform to the majority's opinion; (2) it may convert the majority to its own point of view; or (3) it may eventually be persuaded to join the majority. This research examines the first two modes. Things left unexplained may be attributed to the third mode (i.e., a majority which prevailed on the final ballot).

The first of these behavior modes refers to a minority's ability to remain cohesive and resist pressures to conform. In a jury setting, such behavior would result in a hung jury.

Hypothesis 1: Juries with viable minorities hang more often than do juries without viable minorities.

However, in resisting conformity, viable minorities may yield a second outcome. The viable minority's refusal to conform stimulates discussion, and through this discussion the majority may eventually be persuaded of its "error." One indication of a minority's success in altering the majority's opinion is the finding of a defendant guilty or innocent as opposed to hanging. In short, how consistent is a jury's first ballot with its final verdict? Consistency is operationalized by comparing the majority vote prior to deliberations (i.e., the original verdict propensity) with the group's final verdict. Those whose original propensities matched their definitive verdicts were said to be consistent. Juries were labeled inconsistent where such matching failed to materialize. Since the jury is the level of analysis, there is no description of changes in the opinions of individual jurors. The article only deals with changes in the opinions of the group. This can be converted into the following hypothesis.

Hypothesis 2: Juries with viable minorities will have more inconsistencies between their original verdict propensities and their final verdicts than will juries who do not have viable minorities.

Table 2 documents Valenti and Downing's contention (1975: 657-658) that:

[I]f having at least two supporters is the critical number that makes a minority viable, then it can be shown that viable minorities would occur more frequently in 12- than in 6-member juries. . . .

Hypotheses 1 and 2, and Valenti and Downing's contention, suggest how a jury's size affects both its propensity to hang and

Table 2. Size of the Jury's Minority by Jury Size

Size of Minority	Combined Jury Size Categories		
	Five-Member	Six-Member	Twelve-Member
No Minority	57% (n.8)	29% (n.12)	9% (n.4)
1 Member	29% (n.4)	38% (n.16)	11% (n.5)
2+ Members (Viable Minority)	14% (n.2)	33% (n.14)	80% (n.37)

n. 102*

Chi. Sq. = 32.03 (4 df)

Chi. Sq. p. < .00001

Gamma = .702

* This table excludes 8 nine-member juries.

consistency between its predisposition (i.e., the preliminary vote) and final verdict. Since viable minorities occur with greater frequency in larger juries, and since viable minorities probably produce more hung juries than nonviable minorities do (i.e., Hypothesis 1); then:

Hypothesis 3: Larger juries will hang significantly more often than smaller juries.

Finally, since viable minorities occur more frequently in larger juries, and since juries that have viable minorities will have more inconsistencies between their first ballot and their final verdicts than juries without viable minorities will (i.e., Hypothesis 2); then:

Hypothesis 4: The final verdicts of larger juries will differ from their first-ballot dispositions more often than is the case with smaller juries.

V. FINDINGS AND IMPLICATIONS

Table 3 presents data relevant to the behavior of viable minorities. Viable minorities (as defined by Ballot 1) are more likely to cause juries to hang than are nonviable minorities.¹¹ Therefore, it seems that viable minorities are more successful at resisting conformity pressures. Asch (1952), Zeisel (1972), and Valenti and Downing (1975) appear safe in their assumption that providing the dissenter with an ally increases the ability of the dissenter to resist pressures to conform.

¹¹ This result is further substantiated when the dependent variable is dichotomized into those juries that hung and those which reached a definitive verdict.

n. 68
Chi.Sq. 5.65 (2 df)

Chi.Sq.p. .05
Cramer's V. .29

Table 3. Jury's Consistency Between Its Verdict and Predisposition, by Size of the Jury's Minority

Consistency Between Jury's Predisposition and Verdict	Size of Minority		
	0 No Minority	1 Nonviable	2+ Viable
Final Verdict Consistent with Predisposition	100% (n.14)	88% (n.15)	57% (n.21)
Final Verdict Inconsistent with Predisposition	0% (n.0)	0% (n.0)	16% (n.6)
Hung Jury	0% (n.0)	12% (n.2)	27% (n.10)

n. 68*

Chi. Sq. = 12.93 (4 df)

Chi Sq. p. < .01

Cramer's V. = .31

* Since this analysis deals only with those juries who had a definitive predisposition, the forty-two juries who did not have a predisposition that was definitive were excluded from the analysis.

However, it is much easier to resist the temptations of peer pressure than it is to convince a majority to abandon its position. Can intense minority preference facilitate the breakdown of majority consensus? Group deliberation is an important intervening variable. The discussion process allows for a complete examination of the issues—an examination which may bring to light issues that might change the majority's opinion.

The small chance of this occurrence affords special significance to the six cases, shown in Table 3, in which viable minorities prevailed over initial majorities. Only juries with viable minorities produced verdicts that were inconsistent with the group's predisposition.¹² This finding would probably have even greater import as the size of the minority increases; however, the number of viable minorities with more than two members in this data set is too small for further testing. In any event, juries with viable minorities tend to both hang and/or change the opinion of a majority more often than do juries without viable minorities. Since we have already demonstrated a strong correlation between jury size and the presence of a viable minority, the stage is now set for testing the syllogism's

¹² This finding maintains its legitimacy even when hung juries are excluded from the statistical analysis.

n. 56

Chi.Sq. .02

Chi.Sq.p. .02

Cramer's V. .36

final extension: do larger juries hang, and deviate from their predisposition, more often than smaller juries do?

Although Table 4 indicates that twelve-member juries hang with greater frequency than their smaller counterparts do, the small sample size (especially that of the unplanned-for, but tested, less-than-six-member juries) may account for the lack of statistical significance in this relationship.¹³ If the 42 six- and twelve-member juries which had no definitive predispositions are included in the analysis, twelve-member juries hang significantly more often than do smaller juries.¹⁴ This is primarily a function of the resistance ability of viable minorities. Table 4 further suggests that six-member juries show less consistency between their predisposition and verdict

Table 4. Jury's Consistency between Verdict and Predisposition by Size Category

Jury's Consistency Between Predisposition and Verdict	Jury's Size Category		
	Five-Member	Six-Member	Twelve-Member
Final Verdict Consistent With Predisposition	78% (n.7)	81% (n.17)	73% (n.24)
Final Verdict Inconsistent With Predisposition	0% (n.0)	14% (n.3)	3% (n.1)
Hung Jury	22% (n.2)	5% (n.1)	24% (n.8)

n. 63*

Chi. Sq. = 6.27 (4 df)

Chi. Sq. p. < .18

Cramer's V = .22

* Since this table only deals with six- and twelve-member juries which have definitive predispositions, 5 nine-member juries which had a predisposition and 42 other juries with no definitive predisposition were excluded from the analysis.

¹³ If one looks at only the six and larger juries, the hypothesized relationship becomes more apparent.

n. 54

Chi.Sq. 5.23 (2 df)

Chi.Sq.p. .07

Cramer's V. .31

¹⁴

n. 102

Chi.Sq. 6.84 (2 df)

Chi.Sq.p. .03

Cramer's V. .26

¹⁵ It is not significant even when hung juries are excluded from the statistical analysis.

n.52

Corrected Chi.Sq. .19 (1 df)

Phi .13

than do the larger ones. Although the relationship is not statistically significant,¹⁵ the fact that it conflicts with the direction of Hypothesis 4 poses a real dilemma.

An explanation resides in the relationship between jury size and the presence of viable minorities. Since only juries with viable minorities had verdicts inconsistent with their predispositions (see Table 3), given the fact that 14 six-member juries had viable minorities (see Table 2), it is not surprising to find viable minorities with inconsistent opinions even in six-member juries.

In fact, we have an interesting phenomenon at work. Table 3 illustrates that almost all of the juries which either hung, or rendered verdicts at odds with their predispositions, had viable minorities. Therefore, most of the juries in Table 4 that either hung or rendered inconsistent verdicts also had viable minorities. What we need to examine is the behavior of viable minorities in groups of different sizes.

Table 5. Consistency between Verdict and Definitive Predisposition of Juries with Viable Minorities, by Jury's Size Category

Jury's Consistency between Predisposition and Verdict	Jury's Size Category	
	Six-Member	Twelve-Member
Final Verdict Consistent With Predisposition	40% (n.2)	95% (n.18)
Final Verdict Inconsistent With Predisposition	60% (n.3)	5% (n.1)

n.24*

Corrected Chi. Sq. = 5.05 (1 df)

Chi. Sq. p. < .02

Kendall's Tau B = -.60

* This Table 5 includes only those six- and twelve-member juries which had both viable minorities, and definitive final verdicts.

Although the sample of juries with viable minorities is relatively small, we do find a significant relationship worthy of comment. Viable minorities in six-member juries are more successful in converting the majority than are viable minorities in larger groups. The smaller the majority, the fewer the number of people that need to be converted. The smaller the jury, the easier it becomes to change the majority's opinion. This is not inconsistent with Hypothesis 3; the larger the jury, the larger the absolute size of the majority and the greater the

chances the conversion will not take place—subsequently the jury hangs.

If our judicial system is to be consistent and avoid both convictions in the presence of reasonable doubt, and the committing of either a Type I or Type II judicial error (i.e., conviction of an innocent person, or release of a guilty person), then the findings of this research have some modest policy implications. First, jury structures which promote resistance to majority persuasion are preferable to those which facilitate a quick and easy decision. Such resistance may not result in a reversal of the initial majority position, but it certainly encourages full discussion of the issues.

If we assume that hung juries are an indication of reasonable doubt, and accept some research findings which suggest that hung juries favor conviction significantly more often than acquittal (Flynn, 1977; Kalven and Zeisel, 1966), then policy makers who prefer to avoid a Type I rather than a Type II error should opt for the structure which produces more hung juries—for them the larger jury is the obvious answer. It may be preferable to release 100 guilty defendants rather than convict one innocent suspect, but we must also be cognizant of society's growing intolerance with the number of released "guilty" defendants. Thus, there is now increasing pressure to avoid Type II errors. This research takes neither position, but merely provides policy makers with a rationale for choosing between jury sizes.

Second, inconsistent behavior may assume different values depending on the environment (i.e., the size). In larger juries, inconsistent behavior may result from extended discussion, itself a function of the greater number of ideas produced by a larger number of people. Therefore, increased discussion and ideas may avoid judicial errors.

On the other hand, inconsistent behavior in smaller juries may result more from the smaller number of people who must be convinced than from increased discussion. In the environment of smaller groups, nothing indicates whether inconsistencies point toward a correct or incorrect decision. The changing of verdicts may be attributed to an authoritarian personality who finds it easier to gain control of a smaller group. All we can assume is that the stifling of discussion is not conducive to fact finding, a critical element in high-quality deliberations.

Since this research suggests that smaller juries have a greater propensity to be inconsistent (i.e., change their

opinions—changes more likely caused by factors other than those which would indicate high-quality deliberations) and that larger juries have a greater likelihood of hanging in preference to conviction, those policy makers preferring to avoid a Type I judicial error should clearly opt for the larger jury.

Generally speaking, there are some behavioral consequences when reverting to smaller juries—consequences which the Court later identified in *Ballew* (1978: 1035):

[R]ecent empirical data suggest that progressively smaller juries are less likely to foster effective group deliberation. At some point, this decline leads to inaccurate fact finding and incorrect application of the common sense of the community to the facts. Generally, a positive correlation exists between group size and both the quality of the group performance and group productivity.

However, we need empirical research with tasks more comparative to that which juries perform than was cited in *Ballew*. The work of Saks (1977) provides a good starting point. Let us look at the effects of jury structure on such things as the accuracy of evidence recall and the quality of deliberations. If we can identify and operationalize some of these surrogate measures of “correctness,” perhaps then we can correlate these measures with verdict consistency. After all, propensities to acquit or convict are relatively meaningless when compared to the goal of reaching a correct decision. We need to be more conscientious about “drawing the line” than simply suggesting it needs to be drawn *somewhere*.

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