Do Fair Procedures Matter? The Effect of Procedural Justice on Spouse Assault

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In a reanalysis of the Milwaukee Domestic Violence Experiment, we examine whether the use of fair procedures on the part of police officers called to the scene of a domestic assault inhibits subsequent assault. Consistent with expectations, we found that procedural justice did suppress subsequent violence, even in the face of adverse outcomes. When police acted in a procedurally fair manner when arresting assault suspects, the rate of subsequent domestic violence was significantly lower than when they did not. Moreover, suspects who were arrested and perceived that they were treated in a procedurally fair manner had subsequent assault rates that were as low as those suspects given a more favorable outcome (warned and then released without arrest). The suppression effect of procedural justice did not depend on the personal characteristics of suspects.

While precise estimates vary (Straus, Gelles, & Smith 1990; Sherman 1992; Bachman & Saltzman 1995), there is little dispute that the frequency of spouse assault in the United States is distressingly high, and that it wreaks great havoc on the immediate parties involved, their children, and others.[1] Unfortunately, it is not clear what the police should do about instances of spousal violence when called to the scene. At one point, based in part on findings from the Minneapolis Domestic Violence Experiment, it was thought that arresting the offender would deter future instances of spouse assault (Sherman & Berk 1984a; Sherman & Cohn 1989). In fact, the principal investigators of the experiment (Sherman & Berk 1984b:1) concluded that the Minneapolis study "strongly suggests that the police should use arrest in

[1] "Spouse assault" is used throughout this article to refer to violence between intimate couples regardless of their marital status.

most domestic violence cases" and that "[o]ther experiments in other settings are needed to learn more."

As a result of the publicity attendant to the publication of the findings from the Minneapolis experiment three things occurred: (1) legislatures around the United States hastily enacted mandatory arrest statutes for spouse assault, (2) police departments increased the frequency of arrest in cases involving spouse assault, and (3) replications of the original Minneapolis study were funded by the National Institute of Justice (NIJ) and conducted in six other jurisdictions (Sherman & Cohn 1989).[2] The published findings from these replications, which became known as the Spouse Assault Replication Program (SARP), have not uniformly found that arrest is an effective deterrent in spouse assault cases. In fact, the results are equivocal at best—findings range from arrest having no effect, to having a deterrent effect, and even to having an escalation effect (Berk et al. 1992a, 1992b; Dunford 1990; Dunford, Huizinga, & Elliott 1990; Hirschel, Hutchison, & Dean 1992a, 1992b; Pate & Hamilton 1992; Sherman & Smith et al. 1992; Sherman et al. 1991, 1992).

In an attempt to disentangle the disparate operational definitions, methodologies, and analytical strategies from these experiments, Garner, Fagan, and Maxwell (1995) examined the original SARP analyses in detail. Unfortunately, they could come to no definitive conclusion about the deterrent effect of arrest for spouse assault and concluded (p. 26):

> Until the kinds of common data analysis originally anticipated for SARP are completed and thorough and rigorous reanalyses of the archived data by independent investigators have established the empirical soundness of SARP findings, the fragmentary evidence and incomplete records provide a less than perfect foundation for understanding alternative police responses to spouse assault.

Even at the end of some seven experiments and millions of dollars, then, there is a great deal of ambiguity surrounding the question of how arrest impacts future spouse assault. What we have learned from these experiments is that arresting in spouse assault cases has no discernable effect in some jurisdictions, deters some offenders in other jurisdictions, and escalates the violence of still other offenders in other jurisdictions (Sherman 1992). Even within the same jurisdiction, the effect of arrest may depend on the length of the follow-up period, the length of detention, and characteristics of the offender (Sherman 1992; Sherman et al. 1991, 1992; Sherman & Smith et al. 1992). As a result, other than questioning the wisdom of a mandatory arrest strategy for spouse assault, policymakers are currently provided little or

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    [2] NIJ-sponsored spouse assault experiments were conducted in Milwaukee, Colorado Springs, Miami, Charlotte, Atlanta, and Omaha.

no clear guidance from this line of research as to how they should respond to such cases.

I. Compliance, Sanction Outcomes, and Sanction Procedures

What the original Minneapolis experiment and all the SARP projects had in common was an interest in the social control effectiveness of arrest as a sanction *outcome*. That is, in response to an incident of spouse assault, the outcome of arresting the perpetrator was compared with some other, less punitive, outcome (e.g., warning the parties to the dispute and temporarily separating them) to determine which response inhibited subsequent spouse assault. This emphasis on the outcome of imposed sanctions reflects the anchoring of the experiments in deterrence theory. Guided by notions of specific deterrence, those conducting the experiment believed that arresting those suspected of assaulting their partner would constitute a more severe sanction than not arresting them. The greater severity of the sanction would, they thought, more effectively inhibit subsequent violence than a less severe sanction, such as a warning by police.

What was largely ignored in each of these experiments is the possibility that particular kinds of police *procedure* might inhibit the recurrence of spouse assault. It is entirely possible that the *manner* in which sanctions are imposed has an independent and more powerful effect on spouse assault than the sanction outcome itself. Though infrequently included in discussions of criminological theory, there is a rich tradition in social psychology, particularly that pertaining to the psychology of authority, which argues that conformity to group rules is as much or more due to *fair procedure* as it is to fair or favorable outcomes (Thibaut & Walker 1975, 1978; Lind & Tyler 1988; Tyler 1990; Tyler & Lind 1992).

In speculating as to why an adherence to fair procedures by authorities would lead to conformity by subjects, some, like Thibaut and Walker, adopt the instrumentalist position that fair procedures will facilitate fair outcomes. It is the securing of favorable outcomes which then secures compliance. Others, like Lind and Tyler, take the position that people want to be treated fairly by authorities independent of any effect on favorable outcomes. A key proposition of this group-value model of procedural justice is that adhering to fair procedures will cement persons' ties to the social order because it treats them with dignity and worth and certifies their full and valued membership in the group. In this view, being treated fairly by authorities, even while being sanctioned by them, influences both a person's view of the legitimacy of group authority and ultimately that person's obedience to group norms (Tyler 1990).

An important implication of the procedural justice literature, therefore, is that *compliance may depend as much or more on the procedural fairness of sanction delivery as it does on the characteristics of the sanction imposed (i.e., its certainty and severity)*. In the Lind and Tyler group-value model of procedural justice, this compliance effect is not simply due to the fact that fair procedures ensure favorable outcomes. Judgments of procedural fairness are thought to be related to compliance independent of the effect of fair procedures in securing favorable outcomes. For example, the simple opportunity to state one's case before authorities make a decision—what Hirschman (1970) and Folger (1977) call giving *voice* and Thibaut and Walker (1975) refer to as process control—enhances the legitimacy of such authorities and fosters compliance. In a particularly powerful demonstration of the voice effect, Lind, Kanfer, and Earley (1990) found that giving people the opportunity to speak *after* a decision had been made was related to perceptions of fair judgments. It would appear, then, that voice matters for perceptions of fair judgments, even if it has no direct influence on outcomes.

We report here on our tests of several hypotheses about the effect of fair procedures on the subsequent likelihood and frequency of spouse assault. Although they will be discussed in greater detail below, our general hypotheses, gleaned from the procedural justice literature, predict that the perception of fair treatment by the police will restrain those who commit spouse assault as effectively as the experience of a favorable outcome, and that when outcomes are not favorable (i.e., one is arrested), a suspect's perception of fair treatment by police will effectively inhibit subsequent assaultive conduct while unfair treatment will not. Data from the Milwaukee Domestic Violence Experiment, one of the SARP replication sites, were used to test these hypotheses (Sherman 1992). After a brief discussion of the critical components of procedural justice and why we think fair procedures may be important in securing compliance, we present a more detailed discussion of our methods and analysis strategy and report our findings.

A. The Elements of Procedural Justice

Deterrence theory adopts an instrumental view of why people obey the law—they do so because the benefits of compliance outweigh the costs. When the costs of an action are high, as, say, when someone is arrested rather than warned and released, people are predicted to be less likely to repeat that action. The important elements of an instrumental or deterrence notion of conformity include the certainty and severity of anticipated punishment, coupled with the expected gains of one's actions. According to this view, what is important is the outcome of

events. More specifically, what people care about when they have contact with legal authorities is securing a favorable outcome for themselves.

In contrast, the group-value model of procedural justice is a normative theory of compliance: People obey the law, at least in part, out of a sense of duty or morality rather than self-interest. According to this normative or group-value model, compliance with laws occurs because people assume the obligation to do so. The obligation to comply with rules is assumed both because legal rules are viewed as moral and because legal authorities are viewed as legitimate. Given this normative position about compliance, the question arises, If it is not the favorableness of outcomes, what leads people to attribute morality and legitimacy to legal authorities? The short answer is "fair treatment"—persons attribute legitimacy to legal authorities and voluntarily follow rules out of a sense of duty and obligation when legal authorities treat them fairly. Why would fair treatment secure compliance to authorities? We have suggested earlier that persons who are treated fairly feel attached to the social order, that is, they perceive that they are valued members of the group. But we may then ask, What are the constituent elements of fair treatment? If people are not only looking to authorities for fair outcomes, what do they want in the way of fair treatment? In canvassing the procedural justice literature, Leventhal (1976, 1980; see also Tyler 1990:118–23; Tyler & Lind 1992) identified six components of procedural justice: (1) representation, (2) consistency, (3) impartiality, (4) accuracy, (5) correctability, and (6) ethicality.

Representation refers to the extent to which the party or parties to a dispute with legal authorities believe they had the opportunity to take part in the decisionmaking process. While disputing parties may not feel that they have the right to determine the outcome, it is important to their sense of fairness that they be given the opportunity to present their case to authorities and that their opinions be given consideration. The opportunity to state one's case is valued not because it is linked to favorable outcomes but because of its "value expressive" function (Tyler, Rasinski, & Spodick 1985; Lind et al. 1980). In this sense, representation or voice allows a person to have a sense of being a full and valued member of the group. Having been treated in a way that indicates one's views are valuable, one is much more likely to view authorities positively and to support and comply with their decisions, even if the outcome is personally unfavorable.

Consistency in decisionmaking refers to similarity of treatment. There are several different ways persons can expect legal authorities to act in a consistent manner. The first occurs when persons compare the treatment they receive with the treatment given other people. This is consistency in treatment across persons. In addition, persons also compare their current treatment

with both their past experiences and how they expect to be treated. This is consistency in treatment over time. In the first type of consistency, people want to be treated like other persons; in the second type, people want the same rules to be applied to them at different times. To the extent that legal authorities provide equal and invariant treatment, citizens are more likely to view their experiences in a positive light, perceive authorities as moral and legitimate, and comply with rules.

Impartiality occurs when legal authorities suppress any biases they have about the parties or the outcome of the dispute. Authorities act with bias whenever their treatment is affected by the personal characteristics (race, gender, age) of one or both of the parties or when they show favoritism toward one party over the other. Impartiality is similar to Tyler and Lind's (1992) notion of the neutrality of authorities. Independent of the favorableness of the outcome, persons are more likely to impute fairness and legitimacy to legal authorities and behave in accordance with rules when they perceive that authorities have acted in an impartial and unbiased manner.

The *accuracy* of a procedure refers to the ability of authorities to make competent, high-quality decisions. The components of an accurate legal procedure include the public airing of the problem (Tyler 1990:136) and the utilization of reliable and valid information. Procedures wherein authorities are seen as actively and publicly bringing the problem to light and attempting to solve it and those based on factual information are more likely to be viewed as fair.

The *correctability* of a procedure consists of the existence of other, higher-level authorities to whom one can appeal the current decision. To be perceived as procedurally fair, authorities must supply some mechanism by which decisions thought to be unfair or incorrect can be made right. When such procedures are in place, and are themselves perceived to be fair, citizens are more likely to view the decisions of authorities as procedurally fair and comply with them.

The final component is *ethicality*. Authorities act ethically when they treat citizens with respect and dignity. By treating people politely and showing respect for their rights as both citizens and persons, authorities convey the impression that such persons are valuable members of the group. Ethicality in authorities' conduct is comparable to Tyler and Lind's (1992) concept of standing or status recognition. Respectful treatment by legal authorities is seen to be directly related to perceptions that authorities are moral, legitimate, and are deserving of compliance.

The group-value model of procedural justice seems to offer some important insights concerning the manner in which police ought to respond to interactions with criminal suspects. If legal authorities are concerned with perceptions of legitimacy, and se-

curing compliance among those whose claims to innocence they reject, they should be concerned with *how* they treat citizens. This is particularly relevant in the area of police response to spouse assault. While there are restraints on the kinds of outcomes that police have at their disposal from legislative enactments, organizational imperatives, and the compelling demands by victims and victim advocacy groups, they can nevertheless react to spouse assault in a manner that ensures fair treatment.

B. Why Should Fair Procedures Matter?

From the discussion above, it should be apparent that supporters of the group-value model of procedural justice appeal to a "respect for rights" position. In interactions with legal authorities, persons are more likely to feel that they are valued group members when they are allowed to have their say, perceive that their views are taken seriously, believe that authorities have not prejudged either them or their case, know that erroneous decisions can at some later time be reviewed and corrected, and are treated with politeness. As much as what legal authorities do, then, how they do it communicates to citizens their status within the group. As such, the use of fair procedures facilitates the development of perceptions that authorities are both legitimate and moral. Once the perception that legal authorities are legitimate has been shaped, compliance with the law is enhanced, even when it conflicts with one's immediate self-interest.

This theory of compliance is also compatible with a number of extant criminological theories. For example, being treated by legal authorities in a manner that conveys respect for self and gives one the opportunity to participate in the process through which decisions are made may minimize the feelings of "negative affect" that engender strain and rule breaking (Agnew 1992). Procedural fairness, in other words, may be important in reducing the level of anger that any contact with legal authorities may produce, particularly those where the outcome is unfavorable. Being treated fairly and with respect by legal authorities may also be crucial in strengthening one's bond to conventionality (Hirschi 1969), even when these bonds may initially be quite tenuous. Finally, the elements of procedural fairness may make it less likely that one's contact with authorities will be a marginalizing and stigmatizing experience. An important component of Braithwaite's (1989) shaming and Sherman's (1993) defiance theories is that sanctions imposed by authorities in a respectful manner that scrupulously honors the value and dignity of subjects are likely to lead to compliance with rules. Braithwaite and Sherman provide extensive evidence that sanctions, when imposed in such a manner as to insult the dignity of persons, can also function to increase rather than reduce future offending.

Procedural justice may, therefore, be an important component of a labeling process that is inclusive, reintegrative, and noncriminogenic, again, even in the face of unfavorable outcomes.

The empirical literature also supports various components of the group-value model of procedural justice. First, there is evidence to indicate that the actions of authorities affect persons' perceptions of procedural fairness. For example, there are small-group experiments which indicate that the opportunity to state one's case to authorities enhances perceptions of procedural fairness (Lind & Tyler 1988). In a clear demonstration of the noninstrumental voice effect, Lind et al. (1990) found that even postdecision voice led to judgments about the fairness of the decision made and acceptance of group goals. In an attempt to extend the generality of the procedural justice effect to more serious conflicts, Casper, Tyler, and Fisher (1988) examined the experiences of convicted felony defendants sentenced for criminal violations. They reported that when the length of prison or jail sentence received and the perceived fairness of the outcome were controlled for, felony defendants were found to be more likely to believe that they were treated in a procedurally fair manner if they were given time to speak with an attorney and if they were treated respectfully by the arresting officer. In a comprehensive study of the effects of citizens' experiences with the police and courts, Tyler (1990) found that persons were more likely to perceive procedural fairness when they were given the opportunity to state their case (representation/voice), when they thought authorities were trying to be fair (impartiality), and when they were treated with respect (ethicality). This body of empirical research seems to indicate that how people are treated by legal authorities does affect their perceptions of procedural fairness.

There is also supporting evidence for the second link, that perceptions of procedural fairness affect perceptions of satisfaction with and legitimacy of legal authorities. Small-group experiments and surveys of subjects in minor dealings with legal authorities have found that feelings of procedural fairness both increase persons' satisfaction with their experience and the perceived legitimacy of authorities (Lind 1982; Tyler 1984; Tyler et al. 1985; Tyler, Rasinski, & Griffin 1986). In their study of felony defendants, Casper et al. (1988) found that perceptions of procedural justice were significantly related to the level of satisfaction with case outcomes. Similarly, Tyler (1990) reported that those who believed they had been treated by the police or court officials in a procedurally fair manner had more favorable evaluations of their experience and were more likely to view the authority of legal officials as legitimate.

There is, however, much more modest evidence that perceptions of procedural fairness or legitimacy are related to compli-

ance. McEwen and Maiman (1984) found that litigants in small claims court were more likely to comply with even unfavorable judgments if they believed the process to be fair. MacCoun et al. (1988) found that disputants in civil cases involving automobile claims were more likely to accept an arbitrator's award if they thought that the procedures which led to the decision were fair. Lind et al. (1993) reported a similar effect. In their study of people involved in an arbitrated civil lawsuit in federal court, they found that judgments of procedural fairness were strongly related to the decision to accept the arbitration award. Finally, Tyler (1990) found that judgments of procedural fairness were positively related to perceptions of the legitimacy of authorities, which in turn were related to a composite measure of self-reported minor offending (making noise, littering, drunk driving, speeding, petty theft, parking violations).

In sum, there are two compelling reasons to believe that adherence to fair procedures on the part of authorities will be related to compliance on the part of subjects. First, the procedural justice hypothesis that fair treatment by legal authorities has an important effect in generating compliance with rules can be accommodated by numerous theories. Second, previous research is consistent with procedural justice effects. Research in a wide variety of contexts has indicated that perceptions of procedural justice influences one's perceptions of the legitimacy of authorities, commitment to the organization and organizational goals, views of political leaders, and trust in the government (see Tyler & Lind 1992 for a review of this literature). It is clear, then, that procedural justice issues are important in understanding person's *attitudes* toward authorities.

Unfortunately, we have much more limited knowledge about whether procedural justice will matter for persons' future behavior. In fact, in spite of a rather abundant literature concerning the elements of procedural justice, and the relationship between fair procedures and attitudes about the fairness and legitimacy of authorities, we have rather meager evidence that feelings of procedural justice among subjects translate into compliance. Preliminary evidence suggests that parties to a civil dispute are more likely to accept a court's arbitration award if they perceive that the procedures used to arrive at the award were fair. And there is evidence that perceptions of procedural fairness in one's mundane dealings with the police (calling them for service, being stopped) or court are related to self-reported involvement in minor offending.[3] However, we do not yet know if fairness judgments are important within a criminal context, where the outcomes imposed by legal authorities are particularly unfavorable

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[3] In Tyler's (1990) study, for the majority of people, experience with authorities consisted of making a call to the police. Much smaller proportions reported that their experience included being stopped by the police or going to court.

(i.e., someone is arrested), and whether fair procedures in this context will affect compliance with serious legal norms.

The research reported on here is one of only a handful of published studies that has examined the relationship between procedural justice judgments and compliant behavior. It is only one of two studies (of which we are aware) to examine the effect of procedural justice on criminal behavior. Moreover, unlike Tyler's (1990) work on compliance with minor offending (littering, speeding, petty theft, parking violations), our research involves serious criminal activity (spouse assault). To our knowledge, therefore, it is the first study to examine the effect of fairness judgments regarding a punitive criminal sanction (arrest) on serious criminal behavior (assaulting one's partner).

The purpose of the current research is to examine the extent to which the perception of procedural fairness by suspects arrested for spouse assault effectively inhibits their subsequent violence. The preceding discussion of procedural fairness leads to the following four hypotheses:

> *Hypothesis 1: The prevalence and frequency of subsequent spouse assault will be lower for those given a warning than for those arrested if arrested offenders perceived themselves as being treated in a procedurally unfair fashion. The prevalence and frequency rates of those who were not arrested will not be significantly different from those who were arrested if arrestees perceive they were treated in a procedurally fair manner.*[4]

The first hypothesis states that the experience of a favorable outcome (being warned) will inhibit spouse assault to a greater extent than the experience of an unfavorable outcome (arrest) that is also perceived to be procedurally unfair. It also states that the experience of a favorable outcome (being warned) will inhibit spouse assault to the same extent as the experience of an unfavorable outcome (arrest) that is perceived to be procedurally fair. In other words, when an unfavorable outcome is paired with procedural fairness it will affect compliance to the same degree as a more favorable outcome. In conceptualizing this hypothesis, imagine that we will be monitoring the conduct of three groups of criminal suspects: those who were (1) warned, (2) arrested with procedural justice, and (3) arrested without procedural justice. We expect that the recidivism rates for the

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[4] The original Milwaukee spouse assault experiment, on which the research reported here is based, was not designed as a study of procedural justice effects. As we note below (sec. II.A.), in the original research only those suspects who were arrested were interviewed. As a result, we know nothing about what those who were warned and released thought about the fairness of the process and, thus, can know nothing about the effect of the process on their later recidivism. Therefore, we have no basis for exploring the full interaction between favorableness of outcome (warn vs. arrest) and perceived fairness of procedures.

first two groups will not be significantly different from each other, while the first and third will differ, with significantly more recidivism in the last group.

> *Hypothesis 2: Among those persons arrested for spouse assault, those who perceive themselves as being treated in a procedurally unfair manner will be more likely to commit acts of spouse assault in the future than those arrested persons who perceive themselves as being treated in a procedurally fair manner, net of other determinants of violence.*

This hypothesis is generated by the well-documented long-term criminogenic effect of arrest reported in the Milwaukee spouse assault study (Sherman 1992; Sherman et al. 1991, 1992:154, 167). That is, rather than acting as a specific deterrent and inhibiting spouse assault, the arrest of suspects in Milwaukee accelerated subsequent violence after one year.[5] We hypothesize here whether the procedural fairness of an arrest can overcome this criminogenic effect.

> *Hypothesis 3: The perceived procedural fairness of the arrest will have as important an influence on subsequent acts of spouse assault as the outcome of arrest (short vs. long jail detention), net of other determinants of violence.*

This hypothesis states that among those arrested, the effect of procedural justice on inhibiting spouse assault will be comparable to the effect for the favorability of outcome (long vs. short detention). Simply put, for those criminal suspects who get arrested, procedural justice matters, and matters as much as a favorable detention outcome.

> *Hypothesis 4: Procedural fairness will inhibit subsequent acts of spouse assault under both favorable and unfavorable outcome conditions (short-term and long-term detention).*

Hypothesis 4 predicts that effects of procedural justice will not vary by outcome favorability. We predict that fair procedures will restrain those who have previously assaulted their spouse when the arrest outcome is both favorable (short detention) and unfavorable (long detention). In other words, fair procedures matter for postarrest recidivism, and this effect is not conditional on the favorability of the arrest outcome.

With respect to the conditional effect of arrest, it should be noted that recent research on police response to spouse assault has suggested that arrest may have entirely different effects on subsequent violence depending on the personal characteristics of the offender. For example, Sherman et al.'s research (1991,

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[5] The criminogenic effect for arrest over the long term was found in the full sample. As Sherman et al. report (1991, 1992; Sherman 1992), arrest had a criminogenic effect among the unemployed and unmarried and a deterrent effect among the employed and married (those with a "stake in conformity").

1992; Sherman 1992; Sherman & Smith et al. 1992) has indicated that arresting suspects in spouse assault cases has substantial crime reduction or deterrent effects for those with a stake in conventional society (those with strong conventional social bonds—the employed and married) and an equally substantial crime escalation effect for those without strong conventional bonds (see also Pate & Hamilton 1992; Berk et al. 1992a, 1992b). The reason for the inconstant effect of arrest may be that those with a stake in conventional society have something placed in jeopardy by their arrest. Not wishing to jeopardize job or marriage by further confrontations with the police, these suspects are more likely to be deterred by their arrest experience. For those with little to lose, however, arrest may simply further erode their already weak ties to conventionality. Such socially alienated persons are likely to respond to their arrest with further acts of defiant violence. If one finds these results convincing, the policy implications are disturbing. They would suggest that the decision by the police to arrest or not arrest a suspect in a case of spouse assault should, in part, be affected by the suspects' personal characteristics, their behavior notwithstanding.

As to the predicted effect of procedural justice on postarrest reoffending, however, there is no compelling a priori reason to presume that fair treatment has a different effect on different persons. Being treated with respect, having one's side of the story listened to, the absence of bias, and other dimensions of procedural justice are predicted to lead to trust in authorities, a sense of belonging to the group, and, ultimately, obedience, regardless of one's gender, class, race, social position, or other personal characteristics. This prediction is predicated on the simple assumption that all people like to experience fair treatment. It is supported by the fact that previous social-psychological research in the procedural justice tradition has failed to identify a consistent personal characteristic that is related to the effect of fair treatment.[6] This leads to our final hypothesis:

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[6] Recent work by Huo et al. (1996), Smith and Tyler (1996), and Sherman (1993) are exceptions to this general statement. Huo et al. and Smith and Tyler found that procedural justice considerations were more important than instrumental ones when persons subject to the decision of a superior considered themselves part of a larger community or group that included authorities. Those who felt alienated from the superordinate group were more strongly affected by the favorability of outcomes. Sherman's defiance theory predicts that poorly bonded persons are more likely to respond to procedural injustice by escalating their criminal involvement than are those with a stronger "stake in conformity." The Milwaukee experiment contains no direct information about the extent to which suspects did or did not consider themselves part of the superordinate group. While Sherman et al.'s measure of a conventional "stake in conformity" (marital and employment status) overlaps some with Huo et al.'s and Smith and Tyler's notions of superordinate group attachment, they are certainly not the same theoretical construct. In spite of the implications of this preliminary work, we were reluctant to predict that one's marital and employment status will condition the procedural justice effect.

> *Hypothesis 5: The effect of perceived procedural fairness on re-offending will not interact with a person's stake in conformity.*

II. Methodology

A. Data

The data for this study were collected for the Milwaukee Domestic Violence Experiment (Sherman & Smith et al. 1992:680; Sherman et al. 1992:144–49), which was conducted from April 1987 to August 1988. In this experiment, all cases of misdemeanor domestic battery where probable cause to arrest existed were randomly assigned to one of three conditions: (1) warning with no arrest; (2) arrest with a brief detention period (average of 3 hours), and (3) arrest with a longer detention period (average of 11 hours). Warned suspects were immediately released at the scene and were not interviewed by the research team. Each arrested suspect was taken to police headquarters for processing. Suspects who were arrested were interviewed while in police custody. The interviews were conducted by research staff immediately after suspects were booked. A special team of 36 Milwaukee police officers implemented the experiment. These officers were assigned to four police districts that had both high rates of spouse assault and a large proportion of poor, minority residents. A case was deemed ineligible for random assignment if the suspect could not be located or had an outstanding arrest warrant or restraining order, or if the case involved either serious injury or the threat of violence. A total of 1,200 cases were included in the experiment.[7]

B. Sample

Approximately 91% of the suspects in the experiment were male. Since females composed such a small proportion of the total suspect sample, and because of the possibility that spouse assault involving a female perpetrator is a fundamentally different event from that involving a male, we decided to eliminate the 110 female suspects from this analysis. An additional 23 cases were deleted because of missing demographic or employment information (Marciniak 1994:58). Finally, within the remaining

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[7] In the original Milwaukee experiment, the participating officers were instructed on the importance of being faithful to the random assignment of suspects to the treatment levels. This training effort was quite successful since 1,180 of the 1,200 cases (98%) received their random assignment. During the training session, participating officers were told that the purpose of the experiment was to implement and examine the effect of an arrest policy for spouse assault. Since, however, the research was guided by a specific deterrence and not a procedural justice model, officers were "blind" with respect to the procedural justice implications of their actions.

subsample of 721 male arrestees, only 479 answered four or more of the questions of interest on the suspect survey. We restricted our analysis of the arrested suspects to these individuals because we do not wish to generalize the results to individuals who did not complete the vast majority of items on the survey. Our final analysis sample, therefore, includes 479 arrested suspects and 346 suspects who were warned but not arrested.

While the dependent variable was observed for the entire sample, only those suspects who were arrested and agreed to be interviewed provided data on whether they believed they had been treated in a procedurally fair manner. The full sample of 825 suspects is only used for some of the analyses reported here because only suspects who were arrested were interviewed and had an opportunity to provide this information. In the remaining analyses, the subsample of 479 arrestees were the principal focus.

C. Measures

As discussed above, several variables used in the analysis were available for the sample of 825 warned and arrested male suspects while others were only available for the subsample of 479 arrestees. Descriptive statistics for the full sample and the subsample of arrestees are presented in Table 1. The number of cases with valid data on the various items collected for the subsample of arrestees are presented in Table 1 as well.

The dependent variable in all the analyses reported here was the number of spouse assault incidents reported to the Milwaukee domestic violence hotline for each individual suspect.[8] The array of independent variables available for all subjects included age at first arrest (in years),[9] race/ethnicity, the rate of prior spouse assault,[10] marital status (1 = married, 0 = otherwise), employment (1 = employed, 0 = otherwise), the sum of the marriage and employment variables which Sherman and Smith et al. (1992:683) described as a measure of "stake in conformity," and the amount of time (in months) each suspect was tracked for spouse assault recidivism.

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[8] As Sherman and Smith et al. (1992:684) note, "[b]eginning in 1986, police were required to report all domestic violence incidents to a local women's shelter and to have the victim speak to counselors about procedures for prosecution. These 'hotline' telephone reports tap all cases of domestic violence known to police by a given suspect against any victim before and after the experimental incident."

[9] Current age was available in the data but not included in the analysis because it was strongly correlated with age at first arrest ($r = .735$; $p < .05$). This level of correlation between the two variables made model estimation using both of them extremely difficult.

[10] To calculate the rate of prior domestic violence, the number of domestic violence incidents involving the suspect reported to the Milwaukee domestic violence hotline (see note 8 above) was divided by the current age. The quotient was then multiplied by 365.25 to yield an annual rate.

Table 1. Descriptive Statistics

Variable Description	Valid N	Mean	S.D.	Range
Analysis variables for all subjects:				
Age at first arrest (in years)	825	26.404	9.579	10–70
Race/ethnicity				
White	825	.197	.398	0–1
Black	825	.759	.428	0–1
Other	825	.044	.204	0–1
Subject arrested	825	.581	.494	0–1
Prior domestic violence incidents	825	.639	1.187	0–10
Time at risk (in months)	825	14.348	4.866	6–22
Marital status (1 = married)	825	.304	.460	0–1
Employment (1 = employed)	825	.438	.496	0–1
Stake in conformity (marriage + employment)	825	.742	.734	0–2
Analysis variables for arrestee subsample:				
Long detention period after arrest	479	.489	.500	0–1
Perceived procedural justice (PPJ):				
3-item measurse (including representation, consistency, and impartiality)	412	.551	.336	0–1
5-item measure (3-item measure + standing and use of physical force)	411	.528	.217	0–1
Indicator variables:				
Member of community organization	478	.215	.412	0–1
Member of church	479	.363	.481	0–1
Angry about being arrested	457	.114	.318	0–1
Wrong to harm partner	467	.805	.397	0–1
Formal sanctions	389	.704	.457	0–1
Informal sanctions	398	.394	.489	0–1
Suspect or victim using drugs/alcohol	456	.814	.390	0–1
Severity of offense variables:				
Police calmed things down	477	.229	.420	0–1
Victim transported to hospital	394	.074	.261	0–1
Time police spent at scene (minutes)	479	25.864	15.774	0–180

The remaining variables in Table 1 were only collected on males within the subsample of arrestees who agreed to participate in the interview. These variables included three items that tap elements of the group-value model of procedural justice described above. Individuals who answered "yes" to "Did the officers take the time to listen to your side of the story?" were given a score of 1 on the *representation* measure, while suspects who answered this question with "no" were given a score of 0. Respondents were also asked, "When the police came, did you expect to be arrested?" Suspects who gave a "yes" response to this question were given a score of 1 on the *consistency* measure, while those who responded "no" received a score of 0. Suspects were also asked whether the police took the time to listen to their story as well as the victim's story. Those who answered "yes" or "no" to both questions were coded 1 on the *impartiality* measure, while those who answered "yes" to one of the questions and "no" to the other were assigned a score of 0. A composite measure of perceived procedural justice was created by taking the average of these three items. Among the subsample of arrestees who an-

swered these items, the reliability of the composite was moderate (coefficient $\alpha = 0.7$).

The three-item composite described above captures much of what the literature has described as important elements of procedural justice. They are, however, completely subjective or perceptual indicators of procedural justice. In an effort to expand both the content validity and the scope of the perceived procedural justice construct, we added two additional items to create a second, five-item measure. To include what Lind and Tyler (1992) refer to as *standing* or *dignitary concerns* (Lind et al. 1990), suspects were asked whether they had been handcuffed at all or handcuffed in front of the victim.[11] We presume that being handcuffed is a piece of information about one's standing in a group (what Tyler & Lind 1990:141 call "status recognition").

Individuals who reported they had been handcuffed were assigned a score of 0 on the handcuff measure, while individuals who had not been handcuffed were coded 1. Reverse coding was used on this item because, *ceteris paribus*, individuals who were handcuffed should believe that they were treated with less respect and dignity. Also, for use of physical force, those who reported its use against them were coded 0 on that measure, while individuals who did not report its use against them were assigned a score of 1. These two items were then added to the arithmetic average calculation above to yield a second measure of perceived procedural justice. The reliability of this five-item measure (coefficient $\alpha = 0.6$) was somewhat lower than the reliability for the three-item measure described above.

Other measures available on the subsample of arrestees included indicator variables (coded 1 when the attribute was present and 0 when absent) for whether the suspect (1) was a member of a community organization; (2) was a member of a church; (3) indicated that he was "angry" when arrested by the police (anger); (4) believed it was "always wrong" to physically harm his partner (beliefs); (5) on being asked about the certainty of arrest for hurting someone he lived with in the future, thought that arrest was "very likely" *and* that such an arrest, if it were to occur, would bother him "a lot" (formal sanctions); (6) thought it was "possible" that the people on whom he depended (e.g., friends, children, parents, brothers, and sisters) would reject or refuse to help him because of this arrest *or* thought that "people will generally respect [him] less because of this arrest" (informal sanctions); and (7) he or his partner had been drinking or using drugs at the time of the offense (substance use).

Three additional variables were included to control for the severity of the offense, even though the design of the experiment

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[11] Being handcuffed by the police might also reflect Leventhal's (1976, 1980) notion of the ethicality of authorities' conduct.

limited the range of variation on this variable (see Sherman et al. 1992:146). The first measure asked the suspect whether the police "had to calm things down," and the second measure asked the suspect whether the victim had to be transported to the hospital. Individuals who answered "yes" were coded 1, and individuals who answered "no" were assigned a score of 0 on both of these measures. A third measure was based on a report from the police about the amount of time (in minutes) the police remained at the scene of the incident.[12]

D. Analysis Overview

The analysis of the five hypotheses described above with the current data presented us with three major issues. First, the distribution of the dependent variable—the frequency of spouse assault recidivism—takes the form of a rare event count. The problems associated with treating event count variables (which are discrete) as continuous realizations of a normal data generating process are well documented (see, e.g., King 1989:126). These problems require us to preclude the use of methods such as standard mean difference tests and ordinary least squares regression that assume population normality of the dependent variable (see Gardner, Mulvey, & Shaw 1995:393–94).

The possibility of an underlying Poisson process is more plausible but restrictive insofar as Poisson processes generate distributions where the variance exceeds the mean with only chance frequency (Lawless 1987:209; King 1989:137). In general, a sample event count distribution exhibiting such "overdispersion" is unlikely to have been generated by a Poisson process. The consequence of assuming a Poisson process when the true process generates overdispersed data is generally an understatement of the coefficient variances (but no bias in the coefficients themselves) and an overstatement of test statistics on the hypothesis that a coefficient is equal to zero in the population (Gardner et al. 1995:399). Overdispersed data-generating processes tend to be of two types: (1) those due to population heterogeneity in the latent variable that generates the observed event count distribution; and (2) those due to contagion that derives from positive correlation in the distribution of events over the observation period (Cameron & Trivedi 1986:31–32).[13]

In this article, we assumed that a Poisson process generated the data as a baseline model, but we then explored the appropri-

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[12] It may be imagined that the amount of time the police spent at the scene of the assault was confounded with the representation indicator of procedural justice, i.e., police who listened to both sides of the story were generally at the scene a longer time. This was not true, however. There was no real correlation between time at the scene and either the composite measure of procedural justice or the representation indicator.

[13] As Lindsey (1995:231) notes, in a data set containing only aggregate event counts, there is no way to identify which process is most likely to have generated the data.

ateness of the negative binomial generalization to the Poisson regression model. On the basis of diagnostic evidence (Appendix A) we concluded that a negative binomial process was significantly more likely than a Poisson process to have generated the observed event count distribution (see also Sherman & Smith et al. 1992).

A second issue is whether separate processes generate the onset event (the initial recidivism incident) on the one hand and the count of subsequent incidents given that onset has occurred on the other. The literature on event count threshold or so-called hurdle models (see, e.g., King 1989:130–36; Mullahy 1986:342–46) addresses this problem, and we exploited the properties of these specifications to evaluate participation and frequency outcomes in the current analysis. Appendix B presents a derivation of the hurdle estimators used in this analysis. The most general form of the hurdle specification allows predictor variables to exert separate effects on prevalence and frequency. More restrictive specifications constrain some or all variables to exert the same effects on both prevalence and frequency. Since restrictive specifications such as these are special cases of the more general model, we can use likelihood ratio tests to arrive at reduced models that are consistent with the data.

Third, as the descriptive statistics in Table 1 show, nontrivial proportions of data were missing among the variables based on information in the interviews. Unfortunately, the elimination of cases that have missing data on any of these items (after purging the warned-only cases) reduced the effective sample size to an intolerably small $N = 223$ (this is equivalent to the "listwise" deletion procedure used in many statistical computing packages). Because of the prevalence of missing data in this analysis, it was necessary to find an alternative method for confronting the problem.[14]

One option, imputation of the sample mean on all missing items for all individuals, is an unsatisfying choice. This is equivalent to unconditional mean imputation as discussed by Little and Rubin (1987:44). Although this approach is attractive insofar as it retains all cases and is simple to implement, it has undesirable properties. Among the most important drawbacks are: (1) the variance of the item is deflated when the sample mean is

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[14] All the methods we considered, however, make strong assumptions about the process generating the missing observations; in particular, they assume that individual missing data patterns are not dependent on the values of the variables with missing data (Little & Rubin 1987:24). In light of the nontrivial nonresponse in the survey, therefore, our results should be interpreted cautiously. We also note that we conducted the same analyses as those presented here for the full sample of 721 arrestees. The results of these analyses, particularly with respect to our conclusions about the effects of perceived procedural justice, were virtually identical. Finally, we estimated models that imputed sample means and included covariates indicating the missing data patterns for each case (Cohen & Cohen 1983:297–98). These results led us to conclusions that also were virtually identical to those reported in this article.

assigned to all missing cases; and (2) the covariance matrix is distorted because the mean is imputed without conditioning on other variables included in the analysis (see, e.g., Little 1992: 1230–31).

Another option involves regression imputation where the variable with missing data is regressed on all of the nonmissing predictors across the sample and the missing value is then replaced by the estimated value from the regression function. This approach is attractive insofar as it allows us to construct a "best guess" as to what the valid data value would have been if we had been able to observe it. As Little and Rubin (1987:45) indicate, this "method projects the incomplete cases to the regression line." Although the regression imputation approach partially corrects the problems associated with unconditional mean imputation, variances and covariances are still underestimated within this framework (pp. 45–46).

A useful alternative to the unconditional mean imputation approach is the multiple imputation methodology Little and Rubin discuss (pp. 255–57). This approach involves several steps which we broadly summarize here (details are presented in Appendix C). First, we condition a variable with missing data on the other variables with no missing data. Second, we draw the imputed value from a probability distribution whose mean and variance are estimated from the valid cases. Third, we conduct this process ten times. Fourth, we analyze the data using standard methods for each of the ten imputed data sets. Finally, we combine the results of each of the ten analyses to acquire a final set of parameter estimates and standard errors. The multiple imputation method is consistent with Little's principles of imputation which dictate that (1) imputations should be conditioned on observed variables and (2) the best prediction should be augmented by an appropriate stochastic component (Little & Schenker 1995:60).

III. Results

The first issue to be addressed concerned the distribution of the dependent variable. First, we wanted to assess whether the count of spouse assault incidents after the experimental intervention was most likely to have been generated by a Poisson process or whether a negative binomial process was a more likely candidate. As noted above, the analysis results presented in Appendix A suggested that a negative binomial specification was preferred over the Poisson.

Second, we performed an initial test of whether the process generating the data was similar for both the onset event and the frequency of events given that onset occurred. This was essentially a test of whether the hurdle specification described above

yielded a higher likelihood of having generated the data than a more simple nonhurdle process. Appendix B demonstrates that the hurdle Poisson process was more likely to have generated the observed recidivism data than the simple Poisson process. Appendix B also shows that the negative binomial process yielded a higher log-likelihood value than the hurdle Poisson process. Interestingly, though, the test provided no support for choosing the hurdle negative binomial specification over the more parsimonious constrained negative binomial model. With more fully specified models, this continued to be the case. On the basis of these results, we restricted our formal interpretations to those suggested by the constrained negative binomial model.

Test of Hypothesis 1

Recall from above that the first hypothesis asserted that if perceived procedural justice has implications for spouse assault recidivism, those individuals who were warned should have lower recidivism rates than those individuals who were arrested but perceived that they had been treated unfairly. Moreover, according to Hypothesis 1, recidivism rates should not differ between those who were warned and those who were arrested but perceived they had been treated in a procedurally fair manner. To test this hypothesis, we used the entire sample of 825 suspects.

We began by estimating a full negative binomial hurdle model with all variables allowed to affect both participation and frequency conditional on participation. This analysis was conducted for both versions of the procedural justice measure described above.[15] The results of the constrained negative binomial specifications for Hypothesis Test 1 are presented in Table 2. These results provide support for the prediction of Hypothesis 1 regardless of whether we examine the three-item (Model 1) or the five-item measure of perceived procedural justice (Model 2). Under both scenarios, an arrest significantly elevates the recidivism rate, while a score on the perceived procedural justice variable that approaches 1.0 returns the recidivism rate approximately to the level of the warned-only group. As Table 2 shows, these

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[15]   For the warned-only group, of course, there were no data on the perceived procedural justice measure, and as the above section indicates, no scores were imputed for this group. Individuals in this group were all coded 0 on the perceived procedural justice measure. This coding scheme has no impact on the substantive conclusions. If they had been coded at the sample mean or any other constant value, for example, the difference would simply have been absorbed by the intercept term and the dummy variable for arrest. The practical implication of this is that the final expected rate of recidivism is insensitive to the choice of a value for the procedural justice measure for the warn-only group. Our purpose in conducting this comparison is simply to compare the expected recidivism rate for an individual who was warned (ignoring procedural justice) with the expected recidivism rates for three individuals at varying levels of perceived procedural justice who were arrested.

Table 2. Negative Binomial Regression of Domestic Violence Recidivism for Warned and Arrested Suspects ($N = 825$)

Variable Description	Model 1		Model 2	
	Coefficient	$\|t\|$-Ratio	Coefficient	$\|t\|$-Ratio
Intercept	−3.224	13.33	−3.225	13.35
Suspect arrested	.600	3.28	.721	3.28
Age at first arrest (in years)	−.014	2.34	−.014	2.35
Race/ethnicity:				
Black	.258	1.63	.258	1.63
Other	.365	1.19	.368	1.20
Prior violence	.441	11.30	.444	11.28
Stake in conformity	.167	1.35	.168	1.36
Stake in conformity × arrest	−.366	2.31	−.368	2.33
Perceived procedural justice (3-item measure)	−.479	2.31		
Perceived procedural justice (5-item measure)			−.724	2.22
ϕ	1.046	7.01	1.047	7.04
Log-likelihood	−911.78		−911.77	

results were obtained after adjusting for other predictors of spouse assault recidivism that were observable on all 825 subjects.

To secure a more intuitive sense of what is implied by these differences, we constructed an approximate comparison based directly on the results of Models 1 and 2 for four "hypothetical" individuals: (1) an individual who was warned; (2) an individual who was arrested and had a low score on the procedural justice measure; (3) an individual who was arrested and had a moderate score on the procedural justice measure; and (4) an individual who was arrested and had a high score on the procedural justice measure.

To conduct these comparisons we constrained all the predictor variables to their means except the indicator variables for arrest and the perceived procedural justice measure. We then allowed the arrest and procedural justice measures to vary with the other predictor variables held constant. Next, we calculated the systematic part of the model for four constellations of arrest and perceived procedural justice outcomes:

$$\psi_1 = (x_s{}' \, \Theta_s + 0 * \Theta_{\text{Arrest}} + 0 * \Theta_{\text{Arrest} \times \text{Stake}} + 0.0 * \Theta_{\text{Justice}}), \quad (1a)$$
$$\psi_2 = (x_s{}' \, \Theta_s + 1 * \Theta_{\text{Arrest}} + 1 * \Theta_{\text{Arrest} \times \text{Stake}} + 0.0 * \Theta_{\text{Justice}}), \quad (1b)$$
$$\psi_3 = (x_s{}' \, \Theta_s + 1 * \Theta_{\text{Arrest}} + 1 * \Theta_{\text{Arrest} \times \text{Stake}} + 0.5 * \Theta_{\text{Justice}}), \quad (1c)$$
$$\psi_4 = (x_s{}' \, \Theta_s + 1 * \Theta_{\text{Arrest}} + 1 * \Theta_{\text{Arrest} \times \text{Stake}} + 1.0 * \Theta_{\text{Justice}}), \quad (1d)$$

where x_s and Θ_s are subvectors of variables and their associated coefficients that were held constant. If we concatenate x_s to the arrest, arrest × stake in conformity, and procedural justice indicators to get vector w_j, we can estimate the variance of each of the ψ_j as

$$\sigma^2{}_\psi = w_j{}^T \Omega w_j, \quad (2)$$

where Ω is the estimated covariance matrix. We have used this estimate to build approximate confidence intervals around the estimates of each of the ψ_j. The calculation for the $100(1-\alpha)\%$ approximate confidence interval on λ_j (from eq. (2)) is given by

$$g^{-1}(\psi_j \pm z_{1-\alpha/2}\sigma_\psi), \tag{3}$$

where $g(\cdot)$ is the link function which, as equation (2) indicates, is $\exp(\cdot)$.[16] If we assume that the ψ_j are independent normal random variables, the calculation of these confidence intervals provides the basis for a test of whether $\psi_j = \psi_k$ (where $j \neq k$) which is constructed by calculating

$$z = [\psi_j - \psi_k] / [\sigma^2\{\psi_j\} + \sigma^2\{\psi_k\}], \tag{4}$$

where z is a standard normal random variable in large samples (Lindgren, McElrath, & Berry 1978:99).

The results of pairwise comparisons of these four categories using both of the perceived procedural justice measures described above are presented in Table 3. Consistent with Hypothesis 1 and the results presented above, they show that the contrast between a warned individual and an individual who was arrested but perceived he had been treated unfairly was statistically significant. Moreover, within the arrest category, the estimated difference between individuals reporting high and low levels of perceived procedural justice appears to be substantively and statistically significant. Although these results are based on the assumption that the variables being compared are independent and are drawn from a standard normal distribution, the conclusions from them appear to square well with the results presented in Table 2.

A final method for evaluating the substantive importance of these results involves a computation of the expected reoffending

Table 3. Contrasts between Warned Suspects and Arrested Suspects at Varying Levels of Perceived Procedural Justice

Category Description	Model 1		Model 2	
	ψ_j	σ_ψ	ψ_j	σ_ψ
Warn only (ψ_1)	−3.075	.089	−3.075	.089
Arrest & procedural justice = 0.0 (ψ_2)	−2.746	.130	−2.628	.182
Arrest & procedural justice = 0.5 (ψ_3)	−2.985	.075	−2.989	.075
Arrest & procedural justice = 1.0 (ψ_4)	−3.225	.126	−3.351	.177
Pairwise comparisons	δ_{ij}	$\lvert t \rvert$-Ratio	δ_{ij}	$\lvert t \rvert$-Ratio
Compare ψ_1 and ψ_2	.329	2.08	.448	2.21
Compare ψ_1 and ψ_3	.090	0.77	.086	0.74
Compare ψ_1 and ψ_4	.150	0.97	.276	1.39
Compare ψ_2 and ψ_3	.239	1.59	.362	1.84
Compare ψ_2 and ψ_4	.479	2.65	.724	2.85
Compare ψ_3 and ψ_4	.239	1.64	.362	1.88

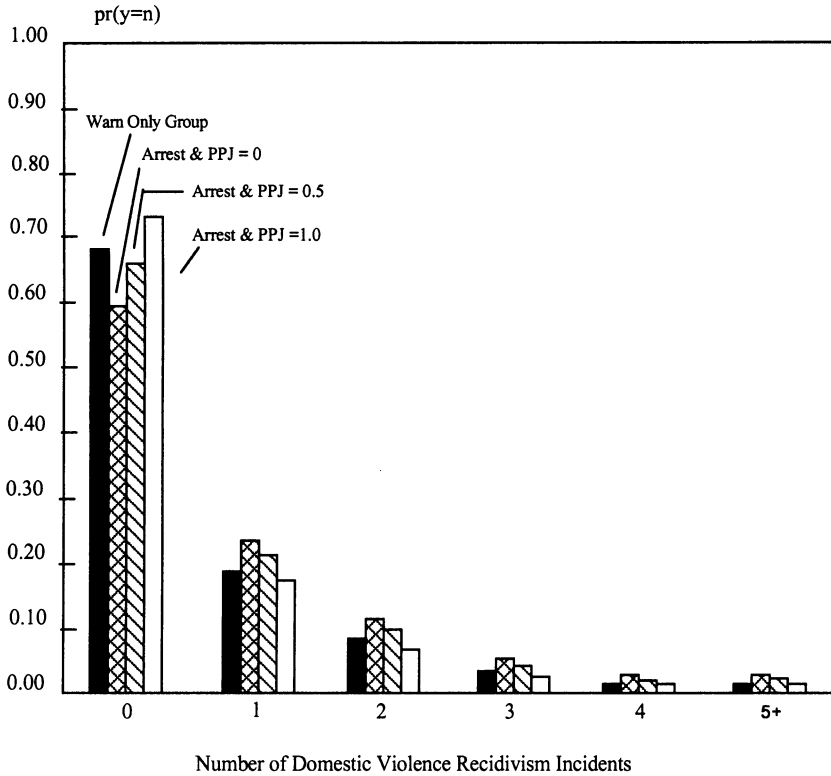[16] Details on this test are summarized in SAS Institute (1993:39).

Fig. 1. Probabilities of N domestic violence recidivism incidents during a hypothetical 12-month followup period using composites from Table 3 (Model 1).

rate, λ, for the various values of ψ reported in Table 3. By mapping these rates into a negative binomial density (see, e.g., Cameron & Trivedi 1986:33; King 1989:137), it is possible to compute the expected probability of $y = 0, 1, 2, \ldots, n$ incidents of subsequent spouse assault within a one-year period following the original spouse assault incident.

Figure 1 presents the respective probability distributions for each of the scenarios developed in Table 3.[17] Of particular interest in Figure 1 is (1) the similarity between the probabilities for the warned scenario and for the scenario where individuals were arrested but had high levels of perceived procedural justice; and (2) the difference between both of these scenarios and the arrest with low perceived procedural justice scenario. Under the current constraints for calculating λ, the probability that at least one recidivism event occurs is .094 higher for the arrest with no perceived procedural justice scenario than for the warn scenario. Moreover, the probability that at least one event occurs is .131

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[17] The three-item perceived procedural justice measures were used for these calculations; as Table 3 suggests, results were very similar using the five-item measure.

higher for the arrest with high levels of perceived procedural justice when compared with the arrest with no perceived procedural justice scenario. In sum, a comparison of these probability distributions reveals that greater levels of perceived procedural justice exert a significant recidivism inhibition effect, and one that is comparable in magnitude to that achieved by a favorable outcome (being warned).

Tests of Hypotheses 2–5

To evaluate whether the predictions of Hypotheses 2–5 were consistent with the data, we estimated several negative binomial regression models. We began with a negative binomial hurdle specification that included the core predictor variables used to test Hypothesis 1, a variable indicating whether individuals were detained for a long period,[18] measures of perceived procedural justice, all survey variables described above, and interaction terms between length of detention and procedural justice and stake in conformity and procedural justice.

Table 4 presents the constrained negative binomial regression coefficients for the two versions of the perceived procedural justice measure (Models 3 and 4 for the three- and five-item measures, respectively). Although the results suggested that a number of the predictor variables exerted statistically significant effects on spouse assault recidivism, many of the variables' effects were not significantly different from zero. On the basis of these results, we decided to estimate trimmed or reduced models with the objective of gaining more efficiency in the parameter estimates that approached statistical significance.

To estimate the trimmed models, effects with trivial *t*-values (where "trivial" was arbitrarily defined as an absolute value less than 1.2) were dropped from the analysis. Two of the terms dropped during this initial reduction were the interaction term between stake in conformity and perceived procedural justice and the main effect for stake in conformity. In none of the specifications that we attempted did these effects ever depart far from zero. The zero effect of the product term, of course, is exactly what Hypothesis 5 predicts, and we will consider it in more detail below.

In the first trimmed specification (see Table 5), along with the main effect for stake in conformity and the stake in conformity by perceived procedural justice product term, we purged age at first arrest, the time police spent at the scene, and the indicator variables for membership in a church, fear of formal and informal sanctions, whether the police had to calm things down,

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[18]   As noted above, the average length of detention for the long-term detainees was 11 hours; the average detention length for the short-term detainees was 3 hours. This dummy variable was coded as 1 if individuals were long-term detainees and 0 otherwise.

Table 4. Negative Binomial Regression of Domestic Violence Recidivism for Subsample of Arrested Suspects ($N = 479$)

Variable Description	Model 3		Model 4	
	Coefficient	$\lvert t\rvert$-Ratio	Coefficient	$\lvert t\rvert$-Ratio
Intercept	−3.196	6.36	−2.988	5.62
Arrest with long detention	−.437	1.60	−.600	1.56
Age at first arrest	−.004	0.46	−.004	0.47
Race/ethnicity:				
Black	.373	1.69	.370	1.68
Other	.048	0.11	.033	0.07
Prior violence	.455	7.13	.455	7.17
Stake in conformity	−.174	0.84	−.243	0.84
Perceived procedural justice:				
PPJ (3)[a]	−.771	2.08		
Long detention × PPJ (3)	.604	1.35		
Stake in conformity × PPJ (3)	.062	0.19		
PPJ (5)[a]			−1.229	2.24
Long detention × PPJ (5)			.931	1.33
Stake in conformity × PPJ (5)			.212	0.42
Member of community organization	−.242	1.29	−.236	1.24
Member of church	−.012	0.08	−.020	0.13
Angry about being arrested	−.262	1.20	.281	1.29
Wrong to harm partner	.257	1.32	.268	1.39
Formal sanctions	−.121	0.67	−.112	0.62
Informal sanctions	.038	0.25	.047	0.31
Suspect or victim using drugs/alcohol	.297	1.22	.291	1.21
Police calmed things down	.005	0.03	.010	0.06
Victim transported to hospital	.007	0.03	.031	0.12
Time police spent at scene (minutes)	.004	0.86	.004	0.91
ϕ	1.024	4.84	1.022	4.86
Log-likelihood	−530.56		−530.25	

[a] PPJ (3) = 3-item procedural justice measure; PPJ (5) = 5-item procedural justice measure. See Table 1.

and whether the victim had to be transported to the hospital. The terms representing membership in a community organization, anger over being arrested, a belief that spouse assault is wrong, substance use, and an interaction between perceived procedural justice and length of detention were retained.

Of particular interest in Models 3 and 4 is the magnitude and direction of the effect for the interaction between procedural justice and length of detention. Substantively, they suggest that the suppression effect observed for perceived procedural justice decayed for individuals who were detained for longer periods. It is possible that spouse assaulters who were detained for long periods (on average 11 hours) in the city jail with little or no outside contact, and with no due process to speak of, perceived their treatment as unfair. This decaying of the overall suppression effect may indicate that procedural justice provided at one point in the process (arrest) must be followed by fair procedures at later points of contact with authorities (in detention).[19] It is also possi-

[19] We cannot draw firm conclusions here because (1) those detained for long periods were interviewed at about the same point in the process as those detained for short

Table 5. Initial Trimmed Negative Binomial Regression of Domestic Violence Recidivism for Subsample of Arrested Suspects ($N = 479$)

Variable Description	Model 5		Model 6	
	Coefficient	$\lvert t\rvert$-Ratio	Coefficient	$\lvert t\rvert$-Ratio
Intercept	−3.414	8.70	−3.222	7.73
Arrest with long detention	−.435	1.68	−.612	1.67
Race/ethnicity:				
Black	.468	2.21	.462	2.18
Other	.132	0.30	.125	0.29
Prior violence	.465	7.31	.470	7.34
Perceived procedural justice:				
PPJ (3)	−.758	2.57		
Long detention × PPJ (3)	.577	1.34		
PPJ (5)			−1.160	2.59
Long detention × PPJ (5)			.939	1.39
Member of community organization	−.296	1.63	−.295	1.62
Angry about being arrested	−.252	1.17	−.263	1.22
Wrong to harm partner	.239	1.23	.252	1.31
Suspect or victim using drugs/alcohol	.298	1.28	.294	1.27
φ	1.049	5.25	1.049	5.30
Log-likelihood	−532.88		−532.67	

ble that procedural justice effects are more pronounced when outcomes are more favorable, although we and the literature would have predicted otherwise.

Because this interaction effect was inconsistent with the invariance hypothesis (Hypothesis 4) stated above, we were particularly interested in whether our conclusions about its significance would be susceptible to increased efficiency gained from eliminating statistically nonsignificant predictors. To assess the plausibility of imposing the above restrictions, the log-likelihood values for Models 5 and 6 were compared with those of Models 3 and 4. In neither case was there sufficient basis to reject the hypothesis that any of the parameters eliminated were different than zero in the population. Evaluation of the parameter estimates in this interim specification suggested the possibility of removing the main effects of anger, beliefs about whether assault was wrong, and substance use. Although these parameter estimates had exhibited nontrivial t-values in Models 3 and 4, the elimination of other nonsignificant predictors did not affect their t-values enough to reject the hypothesis that their true effect in the population was zero. The interaction between length of detention and perceived procedural justice (the three-item measure only) and the main effect of community organization membership were both statistically significant at a single-tailed 90% confidence level. Although our criterion for statistical significance would ordinarily require rejection of the null hypothesis at a 95% confi-

periods (immediately after booking), and (2) the mean levels of perceived procedural justice for the short- and long-term detention groups did not differ significantly.

dence level, the size of the interaction between length of detention and perceived procedural justice, in our view, suggested that additional scrutiny would be informative.

Due to the marginal significance of the interaction between detention length and perceived procedural justice, we elected to estimate another set of trimmed models that purged the clearly nonsignificant effects from Models 5 and 6. Models 7 and 8 (Table 6) present the results of this estimation. In neither the three-item nor the five-item specification of perceived procedural justice could we reject the hypothesis of no interaction at a conventional confidence level. Nevertheless, this interaction effect is close to being statistically significant, and we are hesitant to dismiss its importance completely. If the estimated values of this interaction are plausible, Table 6 shows that the recidivism suppression effect of perceived procedural justice is attenuated in the long-detention group of arrestees; the effect remains intact for the short-detention group. Given our missing data problems, we believe that the possibility of an interaction between outcome and process clearly cannot be ruled out by the results of this analysis.

Table 6. Second Trimmed Negative Binomial Regression of Domestic Violence Recidivism for Subsample of Arrested Suspects ($N = 479$)

Variable Description	Model 7		Model 8	
	Coefficient	$|t|$-Ratio	Coefficient	$|t|$-Ratio
Intercept	−2.943	11.17	−2.741	8.92
Arrest with long detention	−.468	1.80	−.645	1.76
Race/ethnicity:				
Black	.481	2.27	.474	2.24
Other	.190	0.44	.181	0.42
Prior violence	.450	7.62	.454	7.64
Perceived procedural justice:				
PPJ (3)	−.825	2.77		
Long detention × PPJ (3)	.595	1.38		
PPJ (5)			−1.235	2.78
Long detention × PPJ (5)			.957	1.42
Member of community organization	−.345	1.88	−.344	1.87
ϕ	1.081	5.35	1.083	5.41
Log-likelihood	−535.64		−535.59	

The final set of trimmed models are reported in Table 7 as Model 9 (three-item measure of procedural justice) and Model 10 (five-item measure of procedural justice). Formal likelihood ratio comparisons of these models with their constrained counterparts in Tables 4, 5, and 6 revealed no significant deterioration in the log-likelihood associated with the elimination of the nonsignificant predictor variables.

The results of the analyses reported in Table 7 using either of the two procedural justice measures led us to very similar conclu-

Table 7. Final Trimmed Negative Binomial Regression of Domestic Violence Recidivism for Subsample of Arrested Suspects ($N = 479$)

Variable Description	Model 9		Model 10	
	Coefficient	$|t|$-Ratio	Coefficient	$|t|$-Ratio
Intercept	−3.189	13.57	−3.072	11.50
Race/ethnicity:				
Black	.498	2.35	.496	2.34
Other	.237	0.54	.243	0.56
Prior violence	.428	8.37	.432	8.32
Perceived procedural justice:				
PPJ (3)	−.507	2.34		
PPJ (5)			−.751	2.20
Member of community organization	−.354	1.91	−.352	1.89
ϕ	1.084	5.42	1.104	5.56
Log-likelihood	−537.49		−537.58	

sions about the effects of each of the covariates. In addition to the recidivism inhibiting effect of perceived procedural justice, the analysis revealed that black arrestees were more likely to recidivate than their white counterparts. Moreover, as expected, those with higher rates of prior violence had higher rates of future spouse assault.

Hypothesis Test 2

As stated above, the second hypothesis anticipated that the incidence of repeat spouse assault would be higher for those arrestees who perceived that they had not been treated fairly, net of other determinants of violence. The trimmed negative binomial regression models in Table 6 indicate that perceptions of procedural justice have the anticipated recidivism-inhibiting effect predicted by Hypothesis 2. To examine this effect more closely, we constructed estimates of ψ (using the same procedure described above) for low (0.0), moderate (0.5), and high (1.0) levels of perceived procedural justice. Table 8 presents the estimates and their pairwise comparisons. The results of this assessment led to conclusions about the value of ψ under each condition that were virtually identical to those presented in Table 3 above. Moreover, these estimates yield probability distributions that closely coincide with those depicted in Figure 1.

Hypothesis Test 3

Consistent with the prediction of Hypothesis 3, the effect of perceived procedural justice was at least as strong as the effect of length of detention on recidivism. Under neither scenario of perceived procedural justice did the effect of detention length approach statistical significance and the effect of perceived procedural justice (as shown in Table 8) has substantively important

recidivism-inhibiting effects. When interpreting the results of this hypothesis test, however, one must bear in mind the possibility that detention length may condition the effect of perceived procedural justice (see below).

Table 8. Contrasts among Arrested Suspects at Varying Levels of Perceived Procedural Justice

Category Description	Model 9		Model 10	
	ψ_j	σ_ψ	ψ_j	σ_ψ
Arrest & procedural justice = 0.0 (ψ_1)	−2.709	.136	−2.591	.190
Arrest & procedural justice = 0.5 (ψ_2)	−2.962	.080	−2.966	.080
Arrest & procedural justice = 1.0 (ψ_3)	−3.216	.132	−3.342	.187
Pairwise comparisons	δ_{ij}	$\|t\|$-Ratio	δ_{ij}	$\|t\|$-Ratio
Compare ψ_1 and ψ_2	.253	1.61	.375	1.82
Compare ψ_1 and ψ_3	.507	2.67	.751	2.82
Compare ψ_2 and ψ_3	.253	1.64	.375	1.85

Hypothesis Test 4

As we have already noted, there was no conclusive evidence that the effect of perceived procedural justice was conditional on length of detention (as Hypothesis 4 predicted) or one's stake in conformity (as Hypothesis 5 predicted). Although the former interaction was explored in detail (based on initial estimates which suggested it might differ from zero), the results in the trimmed models in Table 6 provided weak support for the hypothesis that detention length conditioned the effect of perceived procedural justice. Nevertheless, the problem of missing data in this analysis precludes us from drawing an overly strong conclusion on this point; the possibility that substantive outcomes condition the effects of process is too plausible and substantively important to dismiss. Nevertheless, our best estimates from this analysis suggest that this possibility, while plausible, is not consistent with the data.

Hypothesis Test 5

The fifth hypothesis predicted that individuals' stake in conformity would not condition the effect of perceived procedural justice on spouse assault recidivism. Under this hypothesis, the effects of perceived procedural justice were expected to operate for individuals regardless of their marital and employment situations. Previous research (Sherman and Smith et al. 1992; Sherman et al. 1992) found that this variable did condition the effect of arrest, but the current analysis found no evidence that it conditioned the effect of perceived procedural justice. As shown in Models 3 and 4 (Table 4), the estimated effect of the interaction between perceived procedural justice and stake in conformity

was never more than trivially different from zero. On the basis of this evidence, we concluded that the effect of perceived procedural justice did not vary by one's stake in conformity.[20]

IV. Discussion and Conclusions

The results of these analyses provide at least moderate support for the prediction that spouse assaulters' perceptions of procedural justice and fair treatment by the police are important determinants of the propensity for future conduct. Although these analyses must be interpreted cautiously, the effect of perceived procedural justice on recidivism was consistent with each of our hypothesized expectations. Among the entire sample of arrested and nonarrested suspects, the recidivism-inhibiting effect of perceived procedural justice completely offset the criminogenic main effect of arrest. Furthermore, for the subsample of suspects who were arrested, the effect of perceived fair treatment by law enforcement authorities was negative and statistically significant. This result was obtained after restricting the sample to include only individuals who had been arrested. Finally, in that subsample of arrestees, there was no evidence that either the length of detention or suspects' stake in conformity (marriage and employment) conditioned the effect of perceived procedural justice. Moreover, our findings do not stand alone. Casper et al. (1988) found that the treatment a suspect receives by the police was the most important determinant of perceived procedural justice. With our evidence directly linking perceptions of procedural fairness to compliance with social norms, it would appear that being treated fairly does indeed matter.

It seems likely that both criminological theory and crime-control policy debates can be informed by these results. Theorists have long been concerned with the problem of secondary deviance (Paternoster & Iovanni 1989; Lemert 1951), while policymakers and researchers alike have long been concerned with the problem of recidivism (Maltz 1996:28–35). Several possibilities seem particularly plausible. For example, the array of collateral consequences of offending behavior may include a deterioration of one's ties to conventional persons and institutions; in short, arrest can attenuate informal social controls that restrain individuals from engaging in criminal behaviors or strengthen ties to networks of offenders or environments that encourage offending behavior (Paternoster & Iovanni 1989; Nagin & Paternoster 1991). As Tyler (1990:110) himself points out, "[i]f the proce-

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[20]   Of course, it is entirely possible that our measures of one's "stake in conformity" (marital and employment status) are poor proxies for one's commitment to the community or group. Future research should employ more subjective assessments of the extent to which individuals feel themselves to be integrated into and members of the group whose rules are being enforced.

dure is experienced by [criminal] defendants as unfair, . . . it may weaken their support for the legal system." The results presented herein suggest that if there are distinct causal processes that generate secondary deviance, the final outcome of criminal justice system reaction to primary deviance may not be the only reaction that is important. Although our results shed no additional light on what constitutes the most efficacious formal outcome, they do suggest that the *process* by which that outcome is delivered matters, even if the outcome is adverse.

A critical question is how this procedural treatment might be theoretically linked to decisions about whether to offend in the future. Perhaps the most straightforward hypothesis is that perceptions of unfair procedural due process weaken support for the legal system which, in turn, reduces inhibitions against or proclivities toward future illegal activity (Tyler 1990:108–12; Casper et al. 1988:503–4). Such obvious linkages notwithstanding, we believe that much existing criminological theory can accommodate the addition of a process-oriented concept. For example, social control theory (Hirschi 1969; Sampson & Laub 1993) would predict that bonds to conventional institutions and individuals restrain future offending behavior. A perception of just treatment (even if adverse) could reduce the likelihood that individuals will completely sever or further attenuate their ties to conventionality.

Another particularly interesting possibility, found in Agnew's (1992:53–54) recent statement of strain theory, suggests a similar linkage. According to Agnew, the gap between perceived just or fair outcomes and actual outcomes is a major source of strain. As discussed above, the social-psychological literature suggests that the process by which outcomes are achieved may well be as important as the outcomes themselves (see e.g., Casper et al. 1988:486–87, 503–4). It might be useful, therefore, to augment Agnew's theory with a normative component to complement the strong consequentialist emphasis of his current theory.[21] Such an augmentation would anticipate, *ceteris paribus*, a positive relationship between the anger or strain that is induced by perceived unfair or unjust processes and future offending behavior.[22] Most directly, these findings are in tune with both Braithwaite's and Sherman's theories of the conditional nature of formal sanctions by authorities. Both theories suggest that compliance is more likely when authorities impose sanctions while still honoring and respecting the dignity of offenders. When authorities sanction of-

[21]   Agnew has already suggested such an elaboration of his theory. In his 1992 article he states that an additional source of strain and anger among adolescents is the perceived violation of rules of fairness by authorities (parents and teachers).

[22]   When strain is checked by what Agnew (1992:72) calls "constraints to delinquent coping," then offending behavior is less likely to occur. Among those who have offended in the past, however, there is strong prima facie evidence that such constraints are relatively weak.

fenders without regard for procedural fairness, offenders are more likely to feel personal admonishment and to become angry and defiant (less compliant). In sum, the relatively simple notion of treating people in a fair and just manner easily harmonizes with major theoretical statements on crime and delinquency.

In addition to their relevance for etiological theories of crime, the empirical results of this analysis have important implications for police policies aimed at preventing spouse assault. In particular, the analysis shows that the effect of individuals' perceptions that they have been treated fairly on future offending behavior is statistically significant and substantively important. Analysis of the entire Milwaukee sample, composed of both arrested and warned suspects, suggested that the recidivism-reducing effect of perceived procedural justice is comparable in magnitude to the various effects of arrest and stakes in conformity—the two variables which have been the subject of much research and speculation in the SARP spouse assault experiments.

These intriguing results notwithstanding, there are three major weaknesses of this research which we hope will be more adequately addressed in future studies. First, we cannot claim to have controlled for all of the possible "third variables" that could possibly account for the relationship between perceptions of procedural justice and subsequent offending behavior. Second, it is not immediately obvious that perceptions of procedural justice are particularly amenable to manipulation by the police. For some individuals, fair treatment by the police may be a contradiction in terms while for others, perceived procedural justice may simply not be malleable. Although our five-item measure of perceived procedural justice included two items that are relatively objective (use of physical force and handcuffing), these two items exhibited quite lopsided empirical distributions. Our three-item measure was largely based, for example, on perceptions such as "how closely" the police listened to both the offender and the victim's sides of the story. It remains to be seen whether and to what extent perceptions such as these can be modified or affected by police behavior. Finally, as we have noted, the missing data shortcomings of this data set are substantial. Although we have attempted to confront this problem in a reasonable way, we cannot be sure that our treatment of the issue leads us to the correct conclusions. Ultimately, conclusions in either direction will only be attainable with more complete data.

Nevertheless, attempts to be attentive to suspects when they try to convey their side of the story, as well as efforts to treat individuals with basic dignity and respect, should not evoke a great deal of controversy. The results presented here suggest that suspects' perceptions that they have been so treated could be as important as the substantive outcomes they eventually experience.

A great deal of scholarly attention and public policy debate has been focused on the effect of arrest on spouse assault (Sherman 1992:1–24; Garner et al. 1995). Because research has not produced a consensus on the effect of arrest in spouse assault cases, most social scientists agree that more work needs to be done to better understand the potential deterrent and criminogenic effects of arrest (Sherman 1992:260–67). Until then, the less controversial proposition that conscientiously treating criminal suspects in a fair and impartial manner will have important crime-reduction effects is worth further consideration.

Appendix A: The Poisson and Negative Binomial Estimators

The likelihood on the data for the simple Poisson regression of the integer count of recidivism events (y_i) for the ith individual is given by

$$L\ (\Theta\,|\,y) = \Pi\ [\exp\ (-\lambda_i)\lambda^{y_i}]/y_i!\ , \tag{1}$$

where

$$\lambda_i = \exp\ [x_i{}'\Theta + \log\ (T_i)]\ , \tag{2}$$

and Θ is a vector of regression coefficients conformable for multiplication with a vector of variables for the ith individual, x_i. The term $\log\ (T_i)$ is the natural logarithm of the number of months of exposure time, and its regression coefficient was constrained to be equal to 1.0 (Maddala 1983:53). For this first assessment, we allowed a scalar 1.0 to enter the term x_i in order to fit an intercept-only model. Model 1 in Appendix Table A presents the results of this specification.

Appendix Table A. Comparison of the Poisson and Negative Binomial Specifications

Variable Description	Poisson Model		Negative Binomial Model	
	Coefficient	$|t|$-Ratio	Coefficient	$|t|$-Ratio
Intercept	−2.944	73.49	−2.944	47.73
ϕ			1.371	7.52
Log-likelihood	−1,104.63		−959.61	
No. of cases	825		825	

With the Poisson results in hand, we turned next to the possibility that a negative binomial process generated the data. The likelihood on the data for the negative binomial regression of the count of recidivism events is

$$L(\Theta,\ \phi\,|\,y) = \Pi\ [\Gamma(\lambda_i/(\phi-1)+y_i)]/[y_i!\Gamma(\lambda_i/(\phi-1))]\ \cdot$$
$$[((\phi-1)/\phi)^{y_i}\phi^{-\lambda_i/(\phi-1)}], \tag{3}$$

where λ_i retains its definition from equation (2), ϕ is a dispersion parameter estimated from the data, and $\Gamma(\cdot)$ is the gamma func-

tion which is easily evaluated using most statistical computer programs. When $\phi \to 0$ (the boundary of the parameter space), the negative binomial specification reduces to the Poisson model (Pohlmeier & Ulrich 1995:344; Lawless 1987:218–19). The results of estimating an intercept-only version of this model are presented as Model 2 in Appendix Table A. A likelihood ratio test of whether the negative binomial specification is more consistent with the data than the simpler Poisson model yielded strong evidence in favor of the former ($\chi^2_{(1)} = 290.04$; $p < .05$).

The differences between the Poisson and negative binomial specifications are perhaps best summarized with a graph. Figure A1 presents the distribution of observed event counts along with those expected by a Poisson model and a negative binomial model. The differences between the observed and Poisson expected frequencies yielded no support for the hypothesis that a Poisson process generated the data at conventional significance levels ($\chi^2_{(10)} = 4{,}698.52$; $p < .05$). The negative binomial model performed much better on this criterion yielding insufficient contrary evidence to reject the null hypothesis that the observed data were generated by a negative binomial process ($\chi^2_{(12)} = 7.44$; $p > .05$).
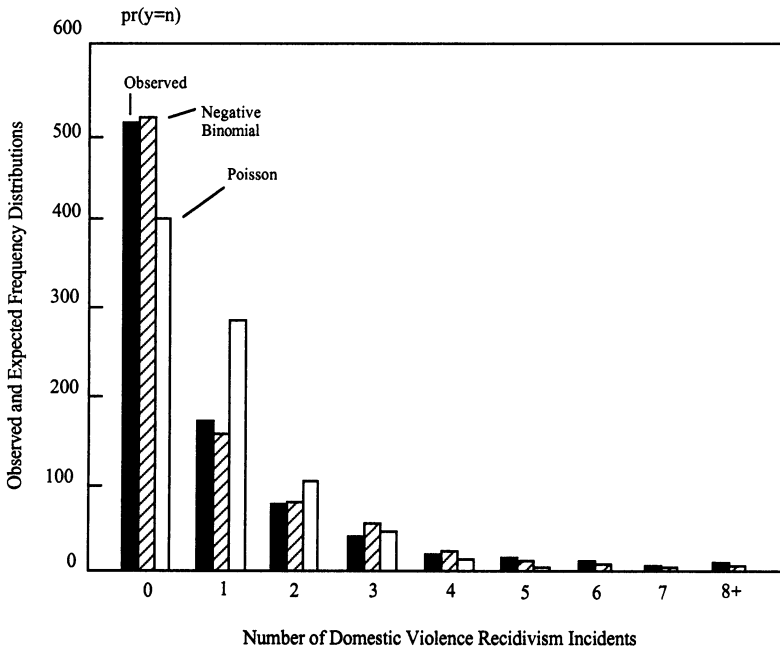

Fig. A1. Comparison of observed recidivism counts and counts expected under Poisson and negative binomial models.

Poisson fit: $\chi^2_{10} = 4{,}698.52$; $p < .05$
Negative binomial fit: $\chi^2_{12} = 7.44$; $p < .05$

NOTE: Top two categories are collapsed into the tenth category for the Poisson fit assessment because of very large cell contributions to the χ^2 test for these categories. This adjustment was unnecessary for assessing the negative binomial fit.

Appendix B: The Hurdle Poisson and Negative Binomial Estimators

The next task was to consider whether the hypothesis that the same underlying process generated both participation and frequency outcomes was consistent with the data. This is essentially a test of whether the hurdle specification was more likely to have generated the data than a constrained process. The general form of the likelihood function for hurdle or threshold models is given by

$$L (\Theta_0, \Theta_+, \phi \mid y) = \Pi \text{ prob } (y_i = 0 \mid \Theta_0) \cdot$$

$$\Pi \text{ prob } (y_i > 0 \mid \Theta_0) \cdot \text{prob } (y_i = n \mid \Theta_+, y_i > 0), \qquad (4)$$

where Θ_0 represents the estimated effects of predictor variables on the probability of crossing the threshold of having any events (i.e., the probability that y exceeds zero) and Θ_+ includes the estimated effects of predictor variables on the frequency of events given that at least one event occurs (King 1989:132; Mullahy 1986:346). The estimate of ϕ is, of course, zeroed out in the Poisson specification of this threshold model but may be nonzero in specifications that entertain the possibility of overdispersion such as the negative binomial model.

King (1989:132–33) has very carefully laid out the arguments for the Poisson version of the likelihood function in equation (4). For the first part of equation (4), we have

$$\text{prob } (y_i = 0 \mid \Theta_0) = [\exp (\lambda_{i0} \cdot \lambda_{i0}{}^0)] \, / \, 0! = \exp (-\lambda_{0i}), \qquad (5)$$

where

$$\lambda_{0i} = \exp [x_{0i}'\Theta_0 + \log (T_i)] \, . \qquad (6)$$

The second part of equation (4) is given by

$$\text{prob } (y_i > 0 \mid \Theta_0) = 1 - \exp (-\lambda_{0i}) \qquad (7)$$

and

$$\text{prob } (y_i = n \mid \Theta_+, y > 0) = \lambda_{+i}{}^{y_i} \, / \, [\exp (\lambda_{i+} - 1) \, y_i!] \, , \qquad (8)$$

where

$$\lambda_{+I} = \exp [x_{+i}'\Theta_+ + \log (T_i)] \, , \qquad (9)$$

and equation (8) is the truncated Poisson probability density function (pdf) for nonzero integers. As King (1989:133) notes, either $x_0 = x_+$ or $x_0 \neq x_+$ is acceptable because the unique contribution of the hurdle model is that it allows for different processes to affect onset and frequency conditional on onset. Still, the former condition and the terms in equations (5)–(9) indicate that the hurdle model reduces to what we call the constrained process (nonhurdle) model in the case where $\Theta_0 = \Theta_+$ (Pohlmeier & Ulrich 1995:346). The final step is to substitute the

terms in equations (5)–(9) into equation (4) which, for the Poisson specification, yields

$$L\ (\Theta_0, \Theta_+ \,|\, y) = \Pi \exp\ (-\lambda_{0i}) \cdot \Pi\ [1 - \exp\ (-\lambda_{;0i})] \cdot$$
$$[\lambda_{+i}{}^{y_i}\, /\, [\exp\ (\lambda_{i+} - 1)\ y_i!]]\ . \tag{10}$$

We maximized equation (10) to get maximum likelihood estimates of Θ_0 and Θ_+ for an intercept-only specification assuming that a Poisson process generated the data. The results of this analysis are presented as Model 1 in Appendix Table B. At the cost of one degree of freedom, the hurdle Poisson specification yielded a significantly higher likelihood of having generated the data ($\chi^2_{(1)} = 206.94$; $p < .05$). It is interesting to note, however, that the hurdle Poisson specification still had a lower log-likelihood value than the negative binomial model.[23] Consequently, we decided to pursue the estimation of a hurdle negative binomial model in order to compare its performance with that of the constrained process negative binomial model.

Appendix Table B. Comparison of the Hurdle Poisson and Negative Binomial Specifications

Variable Description	Poisson Model		Negative Binomial Model	
	Coefficient	$|t|$-Ratio	Coefficient	$|t|$-Ratio
θ_0	–3.402	59.36	–2.905	26.77
θ_+	–2.262	62.37	–3.084	9.08
ϕ			1.514	3.76
Log-likelihood	–1,001.16		–959.48	
No. of cases	825		825	

To motivate this test, we recall the general form of the likelihood function in equation (4). The relevant probabilities have been discussed by King (1989:138). The first part of the likelihood is given by the probability that a random variable is equal to zero:

$$\text{prob}\ (y_i = 0\,|\,\Theta_0, \phi) = \exp\ (-\lambda_0)\ , \tag{11}$$

where Θ_{0i} retains its definition from equation (6) and the second part is given by

$$\text{prob}\ (y_i > 0\,|\,\Theta_0, \phi) = 1 - \exp\ (-\lambda_0)\ ; \tag{12}$$

the negative binomial pdf for a random variable truncated at zero is

$$\text{prob}\ (y_i = n\,|\,\Theta_+, \phi, n > 0, y_i > 0) =$$
$$[\Gamma\ (\lambda_+/(\phi + y_i)]\ /\ [y_i! \Gamma\ (\lambda_+/\phi)] \cdot [1 - \exp\ (-\lambda_0)] \cdot$$
$$[\phi^{y_i}\ (1 + \phi)^{\,-\lambda_+/(\phi + y_i)}]\ , \tag{13}$$

which then substitutes into the likelihood function as

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[23] There is no test statistic available for directly comparing the hurdle Poisson and constrained process negative binomial models because they both contain exactly the same number of parameter estimates.

$$L\ (\Theta_0,\ \Theta_+,\ \phi\,|\,y)\ =\ \Pi\ \text{prob}\ (y_i = 0\,|\,\Theta_0,\ \phi)\ \cdot$$
$$\Pi\ \text{prob}\ (y_i > 0\,|\,\Theta_0,\ \phi)\ \cdot\ \text{prob}\ (y_i = n\,|\,\Theta_+,$$
$$\phi,\ n > 0,\ y_i > 0)\ . \qquad (14)$$

It is worth noting that other specifications for equations (11)–(12) are possible. Two obvious possibilities are the cumulative normal and cumulative logistic distribution functions. The use of these functions does not alter the results presented here.

The negative binomial model in Appendix Table B presents the results of maximizing equation (14) for an intercept-only model and shows that the hurdle negative binomial specification yields a significantly higher likelihood ($\chi^2_{(1)} = 83.36$; $p < .05$) of having generated the data than the hurdle Poisson model. But, as Appendix Table B also shows, the likelihood difference between the hurdle negative binomial specification in equation (14) and the constrained negative binomial specification in equation (3) is trivial ($\chi^2_{(1)} = 0.26$; $p > .05$). This result provides initial evidence that the hurdle specification's additional complexity provides little additional insight into the data generating process for recidivism event counts.

Appendix C: Treatment of Missing Data

Our approach to the missing data problem involved several steps. We illustrate these steps in the following discussion. First, consider the case of a dichotomous variable, z_i, whose values are observed for some individuals and are missing for others. Other variables (including the outcome variable), which we collect into a vector called x_i, are observed for all individuals. In order to fill in plausible values of z_i for those individuals whose values are unknown, we maximize

$$\log\ (L;\gamma) = \Sigma\ z_i \cdot \log\ [\text{pr}\ (z_i = 1)] + \Sigma\ (1 - z_i) \cdot 1 - \log\ [\text{pr}\ (z_i = 1)], \quad (15)$$

where pr $(z_i = 1) = \Phi(x_i'\gamma)$ and both summations are over all individuals with complete data on z_i. This allows us to compute pr $(z_i = 1 \mid x_i)$ for each individual in the data set. Among the subsample of individuals with missing data on z_i, we randomly generated the outcome of a Bernoulli trial with mean of $\pi = \text{pr}\ (z_i = 1|x_i) = \Phi\ (x_i'\ \gamma)$ (SAS Institute 1991:586). When the outcome of the Bernoulli trial was zero, we imputed the value of 0 to z_i; otherwise we imputed the value of 1 to z_i. In this procedure, then, imputed values of z_i are conditional on the observed values in x_i. Moreover, any given value imputed to z_i is not a best guess of the actual value; rather it is a random draw from an urn of Bernoulli trial outcomes whose mean is $\pi = \text{pr}(z_i = 1 \mid x_i) = \Phi\ (x_i'\gamma)$. Analysis of the full data set (including cases with imputed realizations of z_i), then, proceeds as usual (Little & Schenker 1995:60; Herzog & Rubin 1983:234).

Although the above procedure, as described, would be more appropriate than unconditional imputation of means or best guesses, there are two important drawbacks as well. First, the imputation of only one value for z_i does not adequately take into account the uncertainty that is inherent in any imputation procedure (because the actual data are unknown). A conventional remedy for the first problem is to repeat the imputation procedure $M \geq 2$ times, conduct a separate analysis of each resulting data set, and then combine the analyses to yield a set of parameter estimates whose variance reflects both the variability within and between the $M \geq 2$ separate analyses. This procedure is generally known as multiple imputation. To implement this procedure, one estimates the parameters (along with their covariance matrices) of interest in each of the $M \geq 2$ data sets and then combines them using the methods described by Little and Schenker (1995:65–66).

A second problem with the basic imputation procedure is that it overstates the certainty of the estimates of γ obtained from maximizing equation (15). One way to address this problem is to treat the set of parameter estimates (γ) as the center of a multivariate normal distribution whose variance is given by the covariance matrix of the parameter estimates. The set of parameters used for constructing the imputations can then be viewed as a vector randomly drawn from a population distribution of parameter vectors that is multivariate normal. Heitjan and Little (1991:18) observe that an approximation to this procedure which is "easier to implement" is to acquire M sets of parameter estimates from M different bootstrap samples of the complete cases and use a different set of parameter estimates for each of the M imputations (see also Efron 1994:469).

In sum, the analyses reported in this paper are based on $M = 10$ sets of imputations using the conditional random draw method described above. According to Rubin and Schenker (1986:371) and Heitjan and Little (1991:27), a small number of imputations ($2 \leq M \leq 10$) is satisfactory for most general purpose work, and we therefore adopted this convention. Following Heitjan and Little (1991:18) and Efron (1994:469), the conditioning for each of the $M = 10$ imputations was based on parameters estimated from a separate bootstrap sample. Finally, after analysis of each of the ten data sets using standard methods, we combined the $M = 10$ sets of parameter estimates and their covariance matrices to get final estimates of the parameters and their standard errors using the methods described in Little and Schenker (1995:65–66; see also Little & Rubin 1987:255–59).

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