
CRIME, PUNISHMENT, AND STAKE IN CONFORMITY: LEGAL AND INFORMAL CONTROL OF DOMESTIC VIOLENCE*

LAWRENCE W. SHERMAN
University of Maryland

DOUGLAS A. SMITH
University of Maryland

With

JANELL D. SCHMIDT AND DENNIS P. ROGAN
Crime Control Institute

Deterrence theories and labeling theories offer inconsistent predictions about the relative impact of legal and informal controls on the subsequent criminal activities of arrested persons. In a controlled experiment using police contacts for domestic violence offenses in Milwaukee, we test whether the effect of arrest on recidivism is conditional on key individual characteristics indicating a "stake in conformity." Contrary to deterrence theories, arrest had no overall crime reduction effect in either the official or victim interview measures of repeat domestic violence. Consistent with labeling theories, arrest increased recidivism among those with a low stake in conformity: the unemployed and the unmarried. Neither race nor a record of prior offenses conditioned the effect of arrest on subsequent domestic violence. The results are consistent with findings from similar experiments in Omaha, Dade County (Miami), and Colorado Springs.

In 1981 and 1982, the Minneapolis Police Department collaborated in an experiment to determine the effect on subsequent behavior of arresting domestic violence suspects (Sherman and Berk 1984). Suspects were randomly assigned to one of three police responses: (1) arrest, (2) threat of arrest (suspect told to leave the home), or (3) a "talking to" by police (suspect was left at the scene) (Berk and Sherman 1988). Results supported a specific deterrent effect of arrest. Although the authors cautioned against passage of mandatory arrest laws for domestic violence until further research could be conducted (Sherman and Berk 1984, p. 270), by 1991 the results contributed to the passage of such laws in 15 states (Zorza forthcoming).

The Minneapolis Experiment led the National Institute of Justice to fund replications in six additional cities. Findings from five of the six cities are now available: Omaha, Milwaukee, Charlotte (NC), Colorado Springs, and Dade County (Miami). Replications in Omaha, Charlotte, and Milwaukee found no evidence for a long-term deterrent effect of arrest on recidivism. Instead, they found significant long-term *increases* in subsequent incidents (Dunford, Huizinga, and Elliott 1990; Dunford forthcoming; Hirschel et al. 1990; Sherman et al. 1991). However, the Colorado Springs and Dade County replications found evidence of long-term deterrent effects, but no evidence of escalation effects (Berk, Campbell, Klap, and Western 1992; Pate, Hamilton, and Annan 1991).

One possible explanation for these diverse findings is that the samples selected in the six cities

* Direct all correspondence to Lawrence W. Sherman, Institute of Criminal Justice and Criminology, University of Maryland, 2220 LeFrak Hall, College Park, MD 20742. An early version of this paper was presented at the annual meeting of the American Society of Criminology in San Francisco, November 1991. We gratefully acknowledge the extensive field work of Deputy Inspector Dean J. Collins, Captain Anthony R. Bacich, and 35 officers of the Milwaukee Police Department. We are also indebted to Patrick R. Gartin and Ellen G. Cohn, and to the cooperation of Milwaukee Police Chiefs Robert Ziarnik and Philip Arreola, the Milwaukee Common Council, Kathleen Stolpmann, and the hotline volunteers of the So-

journer Truth House in making the experiment possible. We thank Joel Garner, Albert J. Reiss, Jr., Robert Boruch, Kinley Larntz, Lucy Friedman, and Alan Andrews, Robert J. Sampson, and anonymous *ASR* reviewers for their comments on earlier versions of this analysis. This work was supported in part by Grant #86JCXKO43 to the Crime Control Institute from the National Institute of Justice. Points of view or opinions are those of the authors and do not necessarily represent the official views of the U.S. Department of Justice.

differed in important ways. In the original Minneapolis experiment, Sherman and Berk (1984) noted that "there is a good chance that arrest works far better for some kinds of offenders than for others" (p. 270). Sherman (1984, p. 78) hypothesized from control theory that "more socially bonded people are more deterrable" and suggested that unemployed and unmarried persons would be least likely to be deterred by arrest. This hypothesis may account for the conflicting results across cities and between different measures of repeat violence within cities. Because socially bonded couples are more likely to grant interviews, arrest will show stronger deterrent effects when based on interview data rather than on official measures that capture all subsequent incidents reported to police. For example, the six-month victim interviews showed the strongest evidence of deterrence in Colorado Springs and Dade County, but the response rate was only 42 percent and 58 percent respectively.

We focus on Milwaukee and Omaha. The Colorado Springs and Dade County experiments are examined in companion articles in this *ASR* issue (see Pate and Hamilton 1992 and Berk et al. 1992). Data for Charlotte and Atlanta are not yet available, and the Minneapolis data suffered from a very low response rate (Gartin 1991).

A comparison of the characteristics of the sample in each city shows no consistent demographic differences related to deterrence results (Sherman 1992, Figure 6.2, p. 141), although the average proportion of black suspects is much lower in the three cities with some deterrent effects (36 percent) than in the three cities with some escalation effects (63 percent). Because differences other than demographics or informal controls may have caused the different results across cities, analysis of *individual* differences within cities should provide a clearer test of the hypothesis by holding other differences constant within each experiment.

We reanalyze recidivism data from the Milwaukee and Omaha experiments and consider the role of what Toby (1957) has called the individual's "stake in conformity" in predicting the effect of arrest on subsequent domestic violence. We hypothesize that individuals subjected to social control in jobs and marriages are more likely to be deterred by legal sanctions than are those without such stakes in conformity.

LEGAL AND INFORMAL CONTROL

The relationship between legal and informal social control is a central concern of social theory,

political ideology, and social policy. The debate over which mode of control is more effective has deep roots. Classical social theorists have assumed that only state-imposed sanctions deterred crime (Beccaria 1963; see also Hirschi 1986, p. 106), while Durkheim (Lukes and Scull 1983, p. 68) and other early sociologists believed that state sanctions had little effect in preventing crime compared to the informal bonds of social solidarity. Modern theorists of social control (Gottfredson and Hirschi 1990, p. 272) and social policy (Currie 1985) have failed to explore the interconnections between law enforcement and the "root causes" of crime.

The interactions between legal and informal controls may be far more powerful predictors of subsequent crime than either viewed in isolation. Although some theory and research has addressed these interactions in *general* deterrence, little systematic work is available on the role of informal control in the *specific* deterrence hypothesis that punishing criminals reduces their subsequent criminality. Arrest is by far the most frequent form of legal control — 14 million arrests are made each year, 5 arrests for every 100 Americans (Federal Bureau of Investigation 1989, p. 167). Many of these arrests are for crimes committed by repeat offenders (Blumstein, Cohen, Roth, and Visher 1986). Clearly, the effect of informal control on the deterrent effect of arrest is a critical consideration.

General Deterrence Hypotheses

The general deterrence literature includes three hypotheses about the interaction between legal and informal threats of punishment: the *conditional* hypothesis, the *replacement* hypothesis, and the *additive* hypothesis.

The conditional hypothesis. The conditional hypothesis claims that legal threats only deter potential offenders who are sufficiently tied to conventional society to suffer from its stigmatization of arrest. Durkheim noted that "it is shame which doubles most punishments, and which increases with them" (Lukes and Scull 1983, p. 62). Andenaes (1966, p. 964) claimed that the "dread of public scandal and social ruin" may be feared far more than the prospect of legal punishment. Zimring and Hawkins (1973) observed that "official *actions* can set off societal *reactions* that may provide potential offenders with more reason to avoid conviction than the officially imposed unpleasantness of punishment" (p. 174). In the strongest version of this idea, Tittle and

Logan (1973) suggested that "formal sanctions can be effective only if reinforced by informal sanctions" (p. 386; see also Williams and Hawkins 1986).

The replacement hypothesis. This hypothesis assumes that the threat of legal control is effective only when informal control is absent. Matza (1964) argued that "drifters" with few ties to conventional society are deterred by the belief that lawbreakers are caught and punished. This hypothesis was elaborated by Silberman (1976) and Grasmick and McLaughlin (1978), who suggested that people under strong informal controls don't even consider committing crimes, and hence the threat of law is irrelevant. Law replaces informal control, they suggested, only when potential offenders have nothing to lose from social stigmatization.

The additive hypothesis. The additive hypothesis, derived from Wrong (1961) by Grasmick and McLaughlin (1978), claims that both informal and legal controls deter potential offenders — the more of either type of control, the greater the deterrence. The replacement notion that informal control renders law irrelevant is rejected as part of the "oversocialized conception of man."

These hypotheses have usually been tested through controversial self-report survey methods unvalidated by other data sources (Paternoster, Saltzman, Waldo, and Chiricos 1983; Grasmick and Bursick 1990) and have usually focused on white subjects. Within their limitations, these tests have fairly consistently rejected the conditional hypothesis (Jensen 1969; Silberman 1976; Grasmick and McLaughlin 1978; Grasmick and Green 1980, p. 329; Grasmick and Bursick 1990; Nagin and Paternoster 1991). While the first four studies found support for the replacement hypothesis, that evidence is superseded by later studies that find a deterrent effect of law independent of the level of informal control. If anything, the general deterrence survey literature is most consistent with the additive hypothesis.¹

The Labeling Perspective and Specific Deterrence Hypotheses

The effect of legal punishment has traditionally been the domain of labeling and specific deterrence theories. The specific deterrence perspec-

tive claims that legal sanctions suppress crime by making punished persons more sensitive to legal threats in the future (Zimring and Hawkins 1973, p. 73). The labeling perspective claims that legal sanctions escalate crime by assigning the role (or label) of "criminal" to offenders (Tannenbaum 1938, pp. 19–20; Lemert 1951, p. 75).

Labeling theorists, much like general deterrence theorists, disagree over the interaction effects of legal controls and informal controls. The *greater vulnerability* version of the labeling hypothesis contends that the social stigmatization following an arrest produces an escalation in crime because labeled individuals care more about the opinions of conventional society. Harris (1976), for example, suggested that many American blacks are less influenced by labeling because they have already been labeled as "outsiders" by a predominantly white society (see also Ageton and Elliott 1974; Paternoster and Iovanni 1989, p. 382). Whites and high status individuals compared to blacks and low status individuals may experience a greater negative identity change and hence an increase in criminality after punishment because the former purportedly care more about avoiding punishment. This argument has received the strongest empirical support to date (Klein 1986, p. 75).

The alternative *less vulnerability* version of the labeling perspective contends that informal controls may reduce the effect of legal punishment because the labeled individual has other social resources that overcome the impact of labeling. Lofland (1969, pp. 178, 187, 204), for example, hypothesized that a strong cognitive orientation to a nondeviant identity and strong emotional bonds with others who deny the deviant identity may block the escalation to a deviant identity that legal punishment often brings. He concluded (p. 302) that when informal controls are strong, legal punishment is likely to function as a specific deterrent to crime. This view is consistent with the "conditional" hypothesis that the deterrent effect of law depends on the strength of informal controls.

Other ideas in specific deterrence theory are consistent with the general labeling prediction that legal punishment has an escalating effect on crime. Repeated legal punishment, for example, may lead to the discovery that the legal threat is overstated and that punishment is quite tolerable (Zimring and Hawkins 1973, pp. 165, 227). Legal punishment may thus have a different effect on the initial occasion than on subsequent occasions and may eventually increase crime (Cam-

¹ Recent evidence suggests that shame rather than social disapproval functions as an informal deterrent to crime (Williams and Hawkins 1989; Grasmick and Bursick 1990).

eron 1964, pp. 162–63; Smith and Gartin 1989). The anger of punished offenders who see the punishment as unfair and seek vengeance against the victim, the law, or society may also produce an escalation in crime (Lemert 1972, p. 67).

The entire conception of informal controls may also commit the “individualistic fallacy” of assuming that all individual-level correlations have individual-level causes. A broader conception might recognize that community-level processes shaping informal controls of individuals may have much greater causal power (Sampson and Wilson forthcoming). Measures of informal control, for example, may be correlated with strong ecological influences, such as the tendency of persons with low controls to be surrounded by similar persons (Toby 1957; Sampson and Groves 1989). Informal controls may also be correlated, ecologically or individually, with prior punishment (Schwartz and Skolnick 1962), which may reduce both the shame of punishment and the opportunities to build conventional social bonds in the future.

DATA AND METHOD

The central theoretical question is how legal punishment interacts with an offender’s level of informal control. Many measures of informal controls have been examined in survey research on crime (Hirschi 1969; Silberman 1976; Grasmick and McLaughlin 1978; Paternoster, Saltzman, Waldo, and Chiricos 1983; Williams and Hawkins 1989). In field experiments where released suspects cannot be interviewed, some measures are highly problematic. However, a more basic idea underlying these efforts is the individual’s “stake in conformity.” Toby (1957) first used this term when describing the role of informal controls in juvenile delinquency:

Youngsters vary in the extent to which they feel a stake in American society. For those with social honor, disgrace is a powerful sanction. For a boy disapproved of already, there is less incentive to resist . . . temptation. . . . Usually, the higher the socio-economic status of the family, the more the youngster feels he has to lose by deviant behavior. (p. 16)

Two key indicators of stakes in conformity, particularly in poor neighborhoods, are work and marriage (Sherman 1984). Work is a particularly important indicator of informal control in neighborhoods where unemployment is high (Ander-

son 1978, p. 63). Marriage also indicates how much a suspect has to lose by misbehaving (Muir 1977, p. 83). For example, Sampson and Laub (1990) found that among adults who were anti-social as children, strong ties to spouses and to employment were associated with much lower crime rates. Their findings were not based on marriage or employment per se, but on the quality and strength of those social ties as measured longitudinally in repeated in-depth interviews. Because we cannot measure the quality or strength of social ties, we assume that, on average, individuals with these bonds have a greater stake in conformity than unmarried and unemployed individuals.

Control variables are race and a record of prior offending: Race has been an important variable in research on the deterrent effect of arrest (Klein 1985); a record of prior offending is probably the best overall predictor of future offending rates.

The Milwaukee Experiment

From April 7, 1987 to August 8, 1988, the Milwaukee police department participated in an experiment involving 1,200 cases of misdemeanor domestic battery (for details see Sherman et al. 1991). Four districts with the most domestic violence reports were selected. The four districts were all racially mixed, but were highly segregated within districts. Incidents in which the suspect could not be located or had outstanding arrest warrants or restraining orders were excluded from the experiment. Incidents involving serious injury or threat of violence were also excluded.

The Sample

Suspects varied by race and “stakes in conformity.” Almost all (91 percent) of the suspects were male. Blacks comprised 79 percent of the suspects. Over one-half (56 percent) of the suspects were unemployed at the time they entered the experiment; employed suspects typically held blue-collar service jobs. Data on education were available for only 69 percent of the sample, and 56 percent of these had not completed high school. The majority of the couples (70 percent) had never married each other, but 58 percent reported cohabiting for two years or more. About one-third of the suspects had a record of a prior incident of domestic violence.

For each eligible suspect, police officers contacted police headquarters. Civilian research staff

informed them which of three randomly chosen experimental treatments were to be assigned to the suspect. Police compliance with the random assignment protocol was high, especially compared to the Minneapolis experiment (Sherman and Berk 1984). Only 20 cases (2 percent) received a treatment other than that assigned. Therefore, all analyses are performed using the assigned treatments.

The three treatments were: (1) a standard arrest, in which suspects were held until the morning unless they could post bail (a mean of 11 hours in custody); (2) a short arrest (mean of 3 hours in custody), in which the suspects were released on recognizance; and (3) a police warning, but no arrest. All arrested persons were taken to police headquarters for booking.

A standard script was used to warn suspects who were not arrested: "We're not going to arrest anyone here tonight. If we have to return, someone will go to jail. This is a list of people that can help you, both of you, with your problem. The D.A.'s office is on that list, and you can contact them if you want to press charges."

The two arrest responses were combined into a single category because the police administrative procedures required for the "short arrest" treatment raised questions about potential artifactual effects (Sherman et al. 1991) and may have limited the generalizability of the results. A replication of the analyses reported below with the two arrest treatments separated did not alter our original results (table available from the authors on request).

Differences among cities in post-arrest punishment clearly complicate comparisons. Of the suspects who were arrested, only 1 percent received court-imposed punishment in Milwaukee, 2 percent in Minneapolis (Sherman and Berk 1984), but 64 percent in Omaha (Dunford et al., p. 193).

Outcome Measures

Although several outcome measures were available in Milwaukee (Sherman et al. 1991), the present analysis is restricted to an official measure that we and the participating police believe to be the most reliable. Beginning 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 by a given suspect against any victim before and after the ex-

Table 1. Bivariate Relationships Between Two Recidivism Measures and Independent Variables: Milwaukee Hotline Data, 1987 to 1988

Independent Variable	Proportion With a Subsequent Offense	Subsequent Offenses per Year	Number of Cases
All cases	.363	.612	1,133
<i>Arrest</i>			
No	.344	.540	372
Yes	.372	.648	761
<i>Race^a</i>			
Black	.384 [†]	.660 [†]	898
White	.281	.444	235
<i>Marital status</i>			
Not married	.382 [†]	.660 [†]	798
Married	.316	.492	335
<i>Educational status</i>			
High school graduate	.346	.564	439
Not high school graduate	.409	.684	345
No information	.338	.600	349
<i>Any prior violence</i>			
No	.292 [†]	.420 [†]	768
Yes	.512	1.032	365
<i>Employed</i>			
No	.385	.696 [†]	636
Yes	.334	.516	497

[†] Significantly different across categories of the independent variable ($p < .05$).

^a Sixty-seven (67) cases of other races are excluded.

perimental incident. Therefore, our analysis is based on the hotline data.

We do not analyze the victim-interview data because they suffer measurement error directly correlated with the informal control measures. Victim willingness to report repeated violence already detected by police is strongly related in the Milwaukee data to marriage and employment. While there were no differences in the proportion of victim-reported incidents that were also reported to the hotline, there were substantial differences in the proportion of hotline incidents reported by the victims to the interviewers. Among married victims, only 11 percent of the known incidents were not reported to the interviewers compared to 29 percent for unmarried victims. A similar pattern holds for employment status (28 percent withheld among victims of the unemployed compared to 21 percent among the employed). Differential reporting also affects the

Table 2. Coefficients from Negative Binomial Regression of Number of Subsequent Violent Incidents on Selected Independent Variables: Milwaukee, 1987 to 1988

Independent Variable	Model 1	Model 2	Model 3
Arrest	.198* (1.97)	.427** (3.16)	.428** (3.18)
Black	.313* (2.33)	.295* (2.20)	.267* (2.15)
Prior violence	6.293*** (13.29)	6.319*** (13.46)	6.288*** (13.44)
High school graduate	-.176 (-1.61)	-.171 (-1.57)	-.169 (-1.55)
No information on education	-.085 (-.73)	-.085 (-.73)	-.080 (-.70)
Employed	-.051 (-.51)	.261 (1.50)	—
Married	-.183 (-1.70)	.026 (.14)	—
Stake in conformity	—	—	.147 (1.25)
Arrest × employed	—	-.434* (-2.10)	—
Arrest × married	—	-.300 (-1.33)	—
Arrest × stake in conformity	—	—	-.368** (-2.63)
Constant	-3.497	-3.644	-3.633
Alpha	.993	.979	.979
Log-likelihood	-1222.29	-1218.77	-1219.28
Number of cases	1,133	1,133	1,133

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

Note: Numbers in parentheses are *t*-values. Coefficient alpha is defined in a technical appendix on model estimation that is available from the authors on request.

control variable of race, with 28 percent of hotline incidents withheld among black victims compared to 14 percent among whites. Thus the hotline data provide a more reliable test of the theoretical issues of interest here.

FINDINGS

Table 1 presents summary information for two measures based on the "hotline" records of violent incidents in Milwaukee. The first measure is a dummy variable, coded 1 if there were any reports of subsequent violence by the offender. In these data, 36.3 percent of the offenders were involved in at least one subsequent incident of violence in the period following the experimental

case. Of the 411 repeat offenders, 45 percent had more than two incidents during any of the variable follow-up periods (6 to 18 months). Our second measure is the annual rate of subsequent violent incidents, which averaged .612 incidents per suspect per year.

Repeat violence is significantly higher among unmarried, black, and unemployed subjects. Subsequent battering is also more likely if the suspect had engaged in domestic violence during the previous year. However, results indicate that whether the subject was arrested or simply warned had no significant association with the occurrence or number of subsequent violent incidents.

Our central concern is whether the association between arrest and subsequent violence varies with the offender's stake in conformity as measured by employment status and marital status. The competing predictions of the conditional, replacement, and additive hypotheses can be assessed by allowing the effect of arrest to interact with marital status and employment status.

Table 2 presents the results for three models that assess these predictions. The dependent variable is the number of subsequent violent incidents.² The equations also control for differences in exposure time. Because cases entered the experiment at different times, the eligibility window for recidivism ranged from 6 to 22 months. To control for this factor, the natural log of time (in months) is factored into the likelihood of a repeat incident (see Appendix, available on request from authors). In the main effects model (Model 1), arrest has a significant positive effect on subsequent violence after controlling for employment status, marital status, race, prior violence, educational attainment, and length of time after the experimental incident.³

² The number of violent incidents uses more of the sample information than analyzing whether or not a subsequent incident occurred. We initially estimated a Poisson regression model because it offers an alternative to the bias that results from using ordinary linear regression models with this type of event-count data (King 1988). Preliminary tests (available in the Appendix from the authors) revealed that assumptions required by the Poisson model were inconsistent with the observed data. The negative binomial regression model is more consistent with the underlying data and provides a better fit to the data than the Poisson regression model.

³ "No information on education" is a dummy variable coded 1 if there was no information on educational attainment. This procedure retains the 349 cases lacking data on this variable, but the coefficient has no substantive meaning.

This finding is at odds with the additive hypothesis, that arrest has a universal deterrent effect. The additive hypothesis also predicts that the stake-in-conformity variables should have a direct negative effect on recidivism. Although the coefficients for marital status and employment status are negative, they are not significant. Thus, the analysis lends little support to the claim that legal and informal controls act independently to deter subsequent offenses.

Model 2, which adds the interactions between arrest and employment status and arrest and marital status to the main effects model, assesses the conditional and replacement hypotheses. The conditional hypothesis, which asserts that arrest has a greater deterrent effect among those with a strong stake in conformity, predicts that the estimated coefficients for the interaction terms will be negative. The replacement hypothesis, which asserts that legal sanctions deter only individuals with weak bonds to society, predicts positive signs for these interaction terms. The results clearly support the conditional deterrence hypothesis: Both interaction terms are negative, and the employment term is significant. A test of their joint contribution to the model yields a chi-square of 7.04 with 2 degrees of freedom ($p = .03$).⁴

The final equation reported in Table 2 includes a combined measure of the stake in conformity (Model 3). The stake-in-conformity variable is a three-category ordinal measure coded 0 for suspects who are unmarried and unemployed, 1 for suspects who are married or employed, and 2 for suspects who are married and employed. Results again show a significant negative interaction between arrest and the stake-in-conformity variable.

Results from Model 3 can be used to estimate the number of subsequent violent incidents for different categories of stake in conformity and whether the offender is arrested or not. For example, among individuals with a high stake in conformity (married and employed), arrest reduces the annual rate of subsequent violence by 25.2 percent from .650 to .486 incidents per year per suspect. Among those with a low stake in conformity (unmarried and unemployed), arrest is associated with a 53.5 percent increase in the

annual rate of subsequent violence, from .486 to .746 incidents. Thus, the offender's stake in conformity as measured by employment status and marital status specifies the influence of arrest on recidivism: Legal sanctions are associated with more subsequent violent incidents among those with a low stake in conformity and fewer violent incidents among those who are bonded to conventional society.

Although race and a record of prior violence are not included in our theoretical perspective, their importance as predictors of recidivism led us to repeat the analyses reported in Table 2 using dichotomized race and prior violence variables as the interaction categories (results not shown). Contrary to prior research (Smith and Gartin 1989), the results show that neither race nor prior offenses for domestic violence condition the effect of arrest on recidivism.

Summary

If we ask whether arrest influences the subsequent violence of those arrested, the answer is that, in general, it depends on the arrested person's stake in conformity. Although arrest had a significant positive association with recidivism in the main effects equation, subsequent analyses that allowed the influence of arrest to vary with employment status and marital status revealed a different picture. Arrested persons who lacked a stake in conformity were significantly more likely to have a repeat offense than their counterparts who were not arrested. Conversely, among those who were married and employed, arrest deterred subsequent violence. Collectively, these findings are consistent with the conditional hypothesis and provide little support for the replacement or additive hypotheses.

Replications

To check the generalizability of our findings, we estimated Model 3 in Table 2 using the official data from the Omaha Domestic Violence Experiment (Dunford et al. 1990). Sample size in Omaha ($N = 330$) was substantially smaller than in Milwaukee, but the 73 percent response rate for the victim interviews provided minimally reliable estimates of the measures of stakes in conformity: 69 percent of suspects were employed and 42 percent of couples were married. Our replication is based on the 239 eligible cases with those measures. Recidivism in our re-analysis of the Omaha data is measured as subsequent arrests of the

⁴ Because the interaction terms include a common component (whether the offender is arrested or not), the interaction terms are positively correlated. To test their joint contribution to the model, a chi-square was calculated by multiplying the difference between the log-likelihoods of Model 1 and Model 2 equations minus 2.

suspect for any crime against the victim, combined with complaints by the victim of subsequent crimes by the suspect that did not result in arrest. As in Milwaukee, the follow-up period varied because cases entered the experiment at different times; the range was 13 to 31 months.

In the Omaha sample, the coefficient for the interaction term between arrest and the stake in conformity is $-.230$ with a t -value of $-.55$, compared to $-.368$ ($t = -2.63$) in Milwaukee. Although the coefficient is consistent with the hypothesis that the effect of arrest on recidivism varies with the stake in conformity, the effect is not significant.⁵ The magnitude of the differences in point estimates for arrest effects on employed and unemployed, however, is even greater in Omaha than in Milwaukee (Sherman 1992, pp. 376–77).

The interaction between arrest and employment status receives additional support from a third city, Colorado Springs (Berk et al. 1992). In Colorado Springs, the effect of arrest on official measures of recidivism differs significantly between employed and unemployed suspects: A slight escalation effect of arrest is found among the unemployed, while a significant deterrent effect is apparent among the employed. These results are consistent with apparent differences in employment opportunities in Milwaukee and Colorado Springs, which may alter the meaning of unemployment as an indicator of stakes in conformity. The majority (72 percent) of Milwaukee victims said their assailants had not been able to hold a job in the preceding year. If currently unemployed persons in the Colorado Springs sample have better prospects in the labor market because of a healthier post-industrial economy compared to Milwaukee, they may have more to lose from arrest than suspects in the Milwaukee sample. Similarly, given the marginal nature of the jobs held by most Milwaukee suspects (Sherman 1992) compared to the high level of military employment among Colorado Springs suspects, the latter group may have more to lose from arrest than do the Milwaukee employed. Thus, the somewhat different results in Colorado Springs are consistent with the stake-in-conformity hypothesis.

The difference in the unemployment interactions in Colorado Springs compared to Milwaukee and Omaha is also consistent with the much

higher prevalence of marriage in the Colorado Springs sample. In Colorado Springs, 69 percent of the couples were married compared to 30 percent in Milwaukee and 42 percent in Omaha (Sherman 1992, Figure 6.2). Thus the unemployed suspects in Colorado Springs are likely to have a greater stake in conformity, and so the results are consistent with our prediction. In Milwaukee, arrest had the greatest escalation effect among suspects who were unmarried and unemployed. Finally, the fact that most of the Colorado Springs cases involved harassment rather than physical violence makes the Colorado Springs experiment less comparable to the Omaha and Milwaukee experiments. Violence was a threshold requirement for all cases in Omaha and Milwaukee.

Further support for the “stake-in-conformity” hypothesis comes from the Dade County (Miami) experiment (Pate and Hamilton 1992), in which 79 percent of the couples were married and all cases involved physical violence. On the densest official measure (offense reports), the escalation effect for unemployed suspects was clearly documented. Thus, all four experiments that have examined this hypothesis report an interaction with unemployment consistent with the stake-in-conformity hypothesis, at least in the official data.

DISCUSSION AND CONCLUSION

These findings provide modest support for the conditional deterrence hypothesis and strong support for the “less vulnerability” version of the labeling hypothesis. They provide clear evidence against the replacement and additive hypotheses and the “greater vulnerability” labeling hypothesis.

The Milwaukee and Omaha findings tend to provide more support for labeling than for deterrence, given the lack of significant deterrent effects of arrest among those with a high stake in conformity. For Colorado Springs, where the average stake in conformity may be higher, the findings lean the other way. Evidence for findings in both directions is found in Dade County, with different results from different measures. The clear absence of labeling effects among employed persons in all four cities demonstrates the conditional applicability of labeling theory to persons with a low stake in conformity and the failure of specific deterrence among such suspects. Within the limits of that condition, however, the labeling prediction appears quite powerful.

⁵ Berk et al. (1992) found less substantial effects in Omaha and Milwaukee because their dependent variable is whether a subsequent incident occurred. The interaction effect is much stronger when measured by frequency rates.

Whether labeling theory actually explains these results is another matter. We have no direct evidence of change in self-concept associated with arrest. Given the high prevalence of prior arrest among suspects (50 percent in Milwaukee), an alternative theory seems equally plausible: anger at the victim, at other lovers (Black 1982), or at society in general (Lemert 1972, p. 67). With little to lose, offenders with a low stake in conformity may have no reason to suppress their anger and may even have much to gain. As Rainwater (1970, p. 229) noted, violence may be a last ditch strategy to ward off the shame of being cut "down to size" by anyone, including the victim or the police.

These findings raise troubling questions about a common application of the replacement hypothesis in sociological theory: that legal sanctions should be increased in direct proportion to a breakdown in informal controls. Durkheim (Lukes and Skull 1983) observed that "one may even say that society resorts to legal punishments only when others are inadequate" (p. 62). Black (1976, p. 107) cited a wide range of ethnographic evidence to support the proposition that "law varies inversely with other social control," although he does not claim functionality. From an additive perspective, Braithwaite (1989, p. 170) suggested that while communitarian societies can choose between formal and informal control, individualistic societies with low informal control are "forced" to rely more heavily on legal sanctions to regulate behavior.

Our experimental findings clearly reveal a flaw in that logic. No matter how accurate the description is historically or comparatively, the replacement hypothesis runs counter to our evidence. The effectiveness of legal sanctions rests on a foundation of informal control. If formal sanctions deter only those with a stake in conformity, it makes little sense to increase formal sanctions for suspects whose stakes are low. If exercise can only strengthen a well-fed body, more exercise would be a foolish prescription for a starving body. While a greater severity of sanctions might compensate for the lack of informal controls, it seems unlikely that cities and states will pay much more to punish most misdemeanor offenses, which constitute the majority of all arrests. However, there is no direct evidence that greater severity would make a difference. Applying legal sanctions to persons without a stake in conformity may encourage more crime.

We have no basis for estimating whether these findings generalize to other offenses. The fact

that the findings are consistent in four different cities suggests that they may be broadly applicable to arrests for domestic violence. For domestic violence offenses, the findings raise troubling policy implications. A policy of arresting employed persons but not unemployed persons would punish employment. A policy of not arresting at all may erode the *general* deterrent effect of arrest on potential spouse abusers. Yet a policy of arresting all offenders may simply produce more violence among suspects who have a low stake in conformity.

Whatever the policy implications (Sherman 1992), a theory of the *effects* of sanctions should not view legal control and informal control as additive variables available for easy substitution. If legal sanctions applied in the absence of informal controls increase crime, they can hardly compensate for the missing "stake in conformity." Both legal and informal social mechanisms are required to control domestic violence, and perhaps other kinds of crime. Viewing punishment and the stake in conformity as mutually exclusive choices produces bad theory and bad public policy.

LAWRENCE W. SHERMAN is an experimental criminologist who has designed and directed over fifteen randomized experiments in criminal sanctions, including the Minneapolis Domestic Violence Experiment. He is Professor in the Institute of Criminal Justice and Criminology, University of Maryland at College Park, and President of the Crime Control Institute, a national research organization based in Washington, D.C. He is the author of Policing Domestic Violence: Experiments and Dilemmas (Free Press, 1992), and is also the editor of the 1992 Symposium issue of the Journal of Criminal Law and Criminology on the domestic violence arrest experiments. He is currently completing several experiments with the Kansas City, Missouri Police Department evaluating the consequences of drug raids on the criminal careers of neighborhoods and persons.

DOUGLAS A. SMITH is Professor of Criminology in the Institute of Criminal Justice and Criminology at the University of Maryland. His current research involves assessing the impacts of juvenile and adult correctional system actions on future criminality, the application of sequential processing models to studies of bias in juvenile justice processing, and a study of persistence or desistance from crime among samples of first-time offenders.

JANELLE D. SCHMIDT is Milwaukee Director of the Crime Control Institute.

DENNIS P. ROGAN is Vice President for Research of the Crime Control Institute.

REFERENCES

- Ageton, Suzanne and Delbert S. Elliott. 1974. "The Effect of Legal Processing on Delinquent Orientation." *Social Problems* 22:87-100.
- Andenaes, Johannes. 1966. "The General Preventive Effects of Punishment." *University of Pennsylvania Law Review* 114:949-83.
- Anderson, Elijah. 1978. *A Place on the Corner*. Chicago: University of Chicago Press.
- Beccaria, Cesare. 1963. *On Crimes and Punishments*, Translated by David Young. 1986. Indianapolis: Hackett.
- Berk, Richard A. and Lawrence W. Sherman. 1988. "Police Responses to Domestic Violence Incidents: An Analysis of an Experimental Design With Incomplete Randomization." *Journal of the American Statistical Association* 83:70-76.
- Berk, Richard A., Alec Campbell, Ruth Klap, and Bruce Western. 1992. "The Deterrent Effect of Arrest in Incidents of Domestic Violence: A Bayesian Analysis of Four Field Experiments" *American Sociological Review* 57:698-708.
- Black, Donald. 1976. *The Behavior of Law*. New York: Academic.
- _____. 1982. "Crime as Social Control." *American Sociological Review* 48:34-45.
- Blumstein, Alfred, Jacqueline Cohen, Jeffrey Roth, and Christy Visser. 1986. *Criminal Careers and "Career Criminals"*. Washington, DC: National Academy of Sciences.
- Braithwaite, John. 1989. *Crime, Shame and Reintegration*. Cambridge, England: Cambridge University Press.
- Cameron, Mary Owen. 1964. *The Booster and the Snitch*. New York: Free Press.
- Currie, Elliott. 1985. "Crimes of Violence and Public Policy: Changing Directions." Pp. 41-62 in *American Violence and Public Policy*, edited by L. A. Curtis, New Haven: Yale University Press.
- Dunford, Franklyn W. Forthcoming. "The Measurement of Recidivism in Cases of Spouse Assault." *Journal of Criminal Law and Criminology*.
- Dunford, Franklyn, David Huizinga, and Delbert S. Elliott. 1990. "The Role of Arrest in Domestic Assault: The Omaha Experiment." *Criminology* 28:183-206.
- Federal Bureau of Investigation. 1989. *Crime in the United States*. Washington, DC: U.S. Government Printing Office.
- Gartin, Patrick R. 1991. "A Replication and Analytic Extension of the Minneapolis Domestic Violence Experiment." Draft of Ph.D. dissertation, Institute of Criminal Justice and Criminology, University of Maryland, College Park.
- Gottfredson, Michael R. and Travis Hirschi. 1990. *A General Theory of Crime*. Stanford, CA: Stanford University Press.
- Grasmick, Harold G. and Steven D. McLaughlin. 1978. "Deterrence and Social Control (Comment on Silberman, ASR June, 1976)." *American Sociological Review* 43:272-78.
- Grasmick, Harold G. and Donald E. Green. 1980. "Legal Punishment, Social Disapproval and Internalization as Inhibitors of Illegal Behavior." *Journal of Criminal Law and Criminology* 71:325-35.
- Grasmick, Harold G. and Robert J. Bursick Jr. 1990. "Conscience, Significant Others, and Rational Choice: Extending the Deterrence Model." *Law and Society Review* 24:837-61.
- Harris, Anthony R. 1976. "Race, Commitment to Deviance and Spoiled Identity." *American Sociological Review* 41:432-42.
- Hirschel, J. David. 1991. Interview with author, Charlotte, NC, August.
- Hirschel, J. David, Ira W. Hutchison III, Charles W. Dean, Joseph J. Kelley, and Carolyn E. Pesackis. 1990. "Charlotte Spouse Assault Replication Project: Final Report." National Institute of Justice, Washington DC. Unpublished manuscript.
- Hirschi, Travis. 1969. *Causes of Delinquency*. Berkeley, CA: University of California Press.
- _____. 1986. "On the Compatibility of Rational Choice and Social Control Theories of Crime." Pp. 106-18 in *The Reasoning Criminal: Rational Choice Perspectives on Offending*, edited by D. B. Cornish and R. V. Clarke. New York: Springer-Verlag.
- Jensen, Gary F. 1969. "Crime Doesn't Pay: Correlates of a Shared Misunderstanding." *Social Problems* 17:189-201.
- King, Gary. 1988. "Statistical Models for Political Science Event Counts." *American Journal of Political Science* 32:838-63.
- Klein, Malcolm W. 1986. "Labeling Theory and Delinquency Policy: An Experimental Test." *Criminal Justice and Behavior* 13:47-79.
- Lemert, Edwin M. 1951. *Social Pathology*. New York: McGraw-Hill.
- _____. 1972. *Human Deviance, Social Problems and Social Control*. Englewood Cliffs, NJ: Prentice-Hall.
- Lofland, John. 1969. *Deviance and Identity*. Englewood Cliffs, NJ: Prentice-Hall.
- Lukes, Steven and Andrew Scull, eds. 1983. *Durkheim and the Law*. New York: St. Martin's Press.
- Matza, David. 1964. *Delinquency and Drift*. New York: John Wiley.
- Muir, William K., Jr. 1977. *Police: Streetcorner Politicians*. Chicago: University of Chicago Press.
- Nagin, Daniel S. and Raymond Paternoster. 1991. "The Preventive Effects of the Perceived Risk of Arrest: Testing an Expanded Conception of Deterrence." *Criminology* 29:561-85.
- Pate, Antony M. and Edwin E. Hamilton. 1992. "Formal and Informal Deterrents to Domestic Violence: The Dade County Spouse Assault Experiment." *American Sociological Review* 57:691-97.
- Pate, Antony M., Edwin E. Hamilton, and Sampson Annan. 1991. "Metro-Dade Spouse Abuse Repli-

- cation Project: Draft Final Report." Washington, DC: National Institute of Justice. Unpublished manuscript.
- Paternoster, Raymond and Leeann Iovanni. 1989. "The Labeling Perspective and Delinquency: An Elaboration of the Theory and an Assessment of the Evidence." *Justice Quarterly* 6:359-94.
- Paternoster, Raymond, Linda E. Saltzman, Gordon P. Waldo, and Theodore E. Chiricos. 1983. "Perceived Risk and Social Control: Do Sanctions Really Deter?" *Law and Society Review* 17:457-79.
- Rainwater, Lee. 1970. *Behind Ghetto Walls*. Chicago: Aldine.
- Sampson, Robert J. and Byron W. Groves. 1989. "Community Structure and Crime: Testing Social-Disorganization Theory." *American Journal of Sociology* 94:774-802.
- Sampson, Robert J. and John Laub. 1990. "Crime and Deviance Over the Life Course: The Salience of Adult Social Bonds." *American Sociological Review* 55:609-27.
- Sampson, Robert J. and William Julius Wilson. Forthcoming. "Race, Crime and Urban Inequality." In *Crime and Inequality*, edited by J. Hagan. Chicago: University of Chicago Press.
- Schwartz, Richard D. and Jerome E. Skolnick. 1962. "Two Studies of Legal Stigma." *Social Problems* 10:133-38.
- Sherman, Lawrence W. 1984. "Experiments in Police Discretion: Scientific Boon or Dangerous Knowledge?" *Law and Contemporary Problems* 47:61-81.
- _____. 1992. *Policing Domestic Violence: Experiments and Dilemmas*. New York: Free Press.
- Sherman, Lawrence W. and Richard A. Berk. 1984. "The Specific Deterrent Effects of Arrest for Domestic Assault." *American Sociological Review* 49:261-72.
- Sherman, Lawrence W., Janell D. Schmidt, Dennis P. Rogan, Patrick R. Gartin, Ellen G. Cohn, Dean J. Collins, and Anthony R. Bacich. 1991. "From Initial Deterrence to Long-Term Escalation: Short-Custody Arrest For Poverty Ghetto Domestic Violence." *Criminology* 29:821-50.
- Silberman, Matthew. 1976. "Toward a Theory of Criminal Deterrence." *American Sociological Review* 41:442-61.
- Smith, Douglas A. and Patrick R. Gartin. 1989. "Specifying Specific Deterrence: The Influence of Arrest on Future Criminal Activity." *American Sociological Review* 54:94-106.
- Tannenbaum, Frank. 1938. *Crime and the Community*. Boston: Ginn and Company.
- Tittle, Charles and Charles H. Logan. 1973. "Sanctions and Deviance: Evidence and Remaining Questions." *Law and Society Review* 7:371-92.
- Toby, Jackson. 1957. "Social Disorganization and Stake in Conformity: Complementary Factors in the Predatory Behavior of Hoodlums." *Journal of Criminal Law, Criminology and Police Science* 48:12-17.
- Williams, Kirk R. and Richard Hawkins. 1986. "Perceptual Research on General Deterrence: A Critical Review." *Law and Society Review* 20:545-72.
- _____. 1989. "The Meaning of Arrest for Wife Assault." *Criminology* 27:163-81.
- Wrong, Dennis. 1961. "The Oversocialized Conception of Man in Modern Sociology." *American Sociological Review* 26:183-93.
- Zimring, Franklin E. and Gordon J. Hawkins. 1973. *Deterrence: The Legal Threat in Crime Control*. Chicago: University of Chicago Press.
- Zorza, Joan. Forthcoming. "The Criminal Law of Misdemeanor Domestic Violence, 1970-1990." *Journal of Criminal Law and Criminology*.