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RECONSIDERING DOMESTIC VIOLENCE RECIDIVISM: INDIVIDUAL AND CONTEXTUAL EFFECTS OF COURT DISPOSITIONS AND STAKE IN CONFORMITY

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Executive Summary

Mandatory arrest policies in cases of misdemeanor domestic violence were implemented in many jurisdictions across the U.S. during the 1980s. Chapter 3 under title IV of the Crime Bill clearly states that mandatory arrest policies should be encouraged for domestic violence offenders (1994). However, the "true" success of arrest alone in preventing or delaying recidivism remains unknown (Sherman 1992, 1993a). Furthermore, any effectiveness of arrest may actually hinge on whether arrestees are actually prosecuted and convicted, and whether they are placed on probation and/or serve a jail sentence. Unfortunately, nothing is known about the effectiveness of particular court dispositions for preventing further domestic violence. This knowledge is important for more effective policies related to the control of domestic violence.

This report presents the results from a study of the effectiveness of various court dispositions for preventing, reducing, and delaying domestic violence across a sample of 3,662 suspects arrested for misdemeanor domestic violence in Hamilton County (Cincinnati), Ohio between August 1993 and May 1996. The main effects of court dispositions as well as how these effects

may be conditioned by informal social controls (stake in conformity) is examined. Like many other urban jurisdictions in the U.S., Hamilton County has a mandatory arrest policy for misdemeanor domestic violence suspects when police officers determine that there is an immediate threat to the victim. Although there is always some discretion involved in the arrest decision, this policy results in an 80 percent arrest rate for all such calls to the police.

All arrestees in the sample were tracked until June 1998. Both varied (up to 57 months) and fixed (2 year) follow-up periods are examined. Information collected from non-anonymous arrest reports, intake interview forms, court records, and the U.S. decennial census led to the construction of a rich data set including numerous statistical control variables as well as measures stemming from individual- and aggregate-level theories of stake in conformity (i.e., informal social control). The unique micro- and macro-level data permitted the following:

1. Empirical tests of the effectiveness of court dispositions for reducing the prevalence and incidence of recidivism as well as for delaying recidivism among misdemeanor arrestees, controlling for legal and social dimensions of risk.
2. An examination of relationships between recidivism and individual- and aggregate-level measures of stake in

conformity.

3. Analyses of the conditioned effects of court dispositions by stake in conformity on recidivism.

4. The construction of maps depicting the geographic distribution of domestic violence across Cincinnati.

5. A descriptive analysis of the time until recidivism (in months) for suspects falling into each disposition group.

Of the suspects who remained in the jurisdiction for the duration of the study, 17 percent were re-arrested for domestic violence during the study period. As much as one-fourth of the suspects were not even living with their partner at the time of arrest, 40 percent were unemployed, and 18 percent were receiving no financial support (including welfare). Two-thirds of the sample had not held a job for at least a year, 96 percent did not have college degrees, and nearly two-thirds had previous convictions (35 percent having been incarcerated previously for an offense other than domestic violence). Roughly 16 percent were female, over one-third were 35 years or older (versus only 23 percent under age 25), and 60% are African-American.

Regarding the analyses of court dispositions, support is found for the importance of filing charges against suspects in order to delay re-arrest, even if the cases ultimately result in subsequently dropped charges or trial acquittals. Also, offender

programs appear to have a suppression effect on the prevalence and incidence of re-arrest across the sample. Finally, persons arrested while old charges are still pending are significantly more likely to recidivate, suggesting that a failure to process cases with greater speed may also increase recidivism.

Regarding the individual- and aggregate-level measures of stake in conformity, only neighborhood-level stake maintains its statistical significance in the multivariate analysis. When including both levels of stake in conformity in the same model, the aggregate-level measure not only prevails in strength but also renders a non-significant relationship involving offender stake.

The significance of aggregate-level stake in conformity is consistent with studies of other types of predatory crimes revealing higher violent crime rates as well as higher individual-level likelihoods of violent crime in more socially "disorganized" neighborhoods. Further inspection of geographic maps of the distribution of residences for domestic violence arrestees reveals a preponderance of arrests closer to the central business district of Cincinnati. The heavy concentration in census tracts closest to the city' major thoroughfares (I-71, I-74 and I-75) is also consistent with the idea that poorer neighborhoods with more transient populations often correspond

with higher rates of predatory crime.

The analysis of the conditioned effects of court dispositions by offender and neighborhood stake in conformity yields support for the idea that some dispositions are more effective for persons with higher levels of individual stake, but the same is not true for persons residing in neighborhoods with larger proportions of higher stake residents. Specific findings for conditioned effects include the following:

1. Offender programs are more effective for reducing the prevalence and incidence of re-arrest among offenders with higher individual-levels of stake in conformity.
2. Split sentences (probation and jail) are more effective for reducing the prevalence and incidence of re-arrest as well as for delaying re-arrest among offenders with higher individual-level stake.
3. Prosecuting offenders from lower-stake neighborhoods significantly reduces the incidence of re-arrest for domestic violence.
4. The prevalence and incidence of re-arrest are lower for offenders from lower-stake neighborhoods serving split sentences.
5. The prevalence and incidence of re-arrest are actually higher for offenders from higher-stake neighborhoods serving jail or

split sentences, perhaps a consequence of a greater willingness of residents in these neighborhoods to call the police regarding domestic disputes, or possible labeling effects occurring among offenders less familiar with legal intervention.

The analysis of time to re-arrest reveals that, regardless of the follow-up period examined, sentences of jail alone have a greater incapacitative effect compared to probation alone and probation combined with jail. In other words, even though suspects who go to jail are typically worse risks compared to those receiving probation, sentences of jail serve to at least delay recidivism longer through its incapacitative effect.

Although suspects whose cases are ignored end up with some of the highest recidivism likelihoods in the sample overall, recidivists falling in this group actually have longer delays to re-arrest compared to other disposition groups (offender programs, probation, jail, and probation combined with jail). While it is possible that the factors leading to inaction on the part of prosecutors are related to this delay, the fact remains that a very high proportion of the group ultimately recidivate for domestic violence.

Similar patterns of delay can be observed for suspects whose cases are dismissed, acquitted, and who undergo the offender

program. However, unlike those whose cases are ignored, these suspects have among the lowest recidivism likelihoods in the sample. In short, the risk evaluations of these individuals made by prosecutors and judges seem to be fairly well-informed.

The results provide several consistencies with previous research on the effectiveness of arrest for reducing domestic violence recidivism. First, just as the main effects of arrest were not statistically significant in most of the arrest studies, most of the main effects of court dispositions are not significant as well. Second, the significant individual-level interactions tapping offender program and split sentences support previous findings related to formal and informal social controls. Finally, the findings for the conditioned effects of court dispositions suggest that formal and informal controls should be considered together when understanding domestic violence recidivism.

The contradictions with the arrest literature include higher likelihoods and rates of re-arrest for offenders in particular disposition groups with higher (aggregate-level) stake in conformity, the lack of an escalation effect for lower-stake offenders serving the same sentences that appear to "work" for higher-stake offenders at the individual-level, and a suppression effect for offenders from lower-stake neighborhoods who are

subjected to particular dispositions.

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Introduction

Domestic assault is the most common form of violence encountered by the police (Sherman 1992). More women are injured by the hands of a spouse or partner than any other source (Zorza 1992). Despite such trends, domestic assault historically has become an issue of public concern during periods when crime has been rampant. Efforts to criminalize domestic violence and subsequent efforts to enforce these laws thus have tended to occur in times of social turmoil when criminal behavior has been attributed to a breakdown in law and order (Pleck 1989).

Police response to the problem of domestic violence has traditionally been one of non-intervention. Help for domestic violence victims was left up to private organizations and charities. Beginning in 1970 and lasting until 1983, however, several jurisdictions established legislation intended to modify official and societal responses to the problem of domestic violence (Buzawa and Buzawa 1985). Despite such legislative trends, evidence suggests that during this time the police remained largely apathetic toward domestic violence (Zorza 1992).

In the early 1980s a research study was undertaken that would help to dramatically alter traditional police responses to domestic violence. In 1983 Lawrence Sherman and Richard Berk

(1984) conducted a controlled experiment in Minneapolis to determine which police response--arrest, separation, or mediation--would yield the largest decrease in domestic violence recidivism. The experiment found that arrested offenders were less likely to recidivate compared to offenders who were either separated or counseled by the police (Sherman and Berk 1984). Drawing from the experiment's conclusion, mandatory arrest policies were widely adopted across jurisdictions in the United States, and arrest became the predominant response to domestic violence (Sherman et al. 1992).

It was not until the original experiment was replicated five years later that questions were raised as to the effectiveness of mandatory arrest policies. Experiments conducted in Milwaukee, Omaha, and Charlotte revealed that arrest actually escalated the occurrence of domestic violence (Dunford, Huizinga, and Elliott 1990; Hirschel et al. 1990; Sherman et al. 1992). Researchers began examining differences across jurisdictions and found that extra-legal factors such as employment and marital status either increased or decreased the likelihood that arrest would deter this behavior. In addition, the social structure of a community (proportion of unemployed males or unmarried couples) was hypothesized to determine how residents react to arrest (Sherman et al. 1992).

Whether arrest deters domestic violence offenders remains unknown (Sherman et al. 1992). Further, the effectiveness of arrest could depend upon what happens to the offender after arrest. Offenders who are arrested are not necessarily prosecuted, convicted, and sentenced. While the relationships between time spent in custody upon arrest, prosecution, and domestic violence recidivism have been examined (Berk et al. 1992; Hirschel et al. 1992; Sherman et al. 1992), no studies exist that explore the possible relationship between other (and more punitive) sanctions and recidivism.

This study is not a replication of prior studies examining the effectiveness of arrest on domestic violence recidivism. Rather, the study is intended to expand upon prior research examining the separate and combined effects of formal (legal) and informal social control on domestic violence recidivism. The study examines both the main effects of different types of formal social control (prosecution, conviction, and various sentences) and informal social control processes on domestic violence recidivism. In addition, interaction effects between formal and informal social control processes are examined as well. Interactions have been examined in previous research (e.g., Berk et al. 1992; Dunford, Huizinga, and Elliott 1990; Sherman et al. 1992), but have been limited to analyzing the interactions

between arrest and employment or martial status. This study builds upon the existing body of research by exploring additional informal social control variables such as education, length of employment, and family ties. The present study further builds upon past research by examining the effects of these social control processes at the individual level as well as the contextual effects of community characteristics on individual behavior. Aggregate-level measures (proportion of college educated individuals, proportion of employed individuals, proportion of financially independent residents, etc.) are included to determine the extent that these neighborhood characteristics influence an individual's propensity to recidivate and/or increase or decrease the effectiveness of formal social control.

Background

Results from the Minneapolis Domestic Violence Experiment revealed that arrest, compared to separation and counseling, produced a significantly lower likelihood of recidivism for domestic violence (Sherman and Berk 1984). The experiment was replicated six times in five different cities: Omaha, Charlotte, Milwaukee, Colorado Springs, and Miami-Dade County. Yet only the study of Dade County yielded statistically significant main

effects of arrest on re-arrest for domestic violence involving the same victim (Garner, Fagan, and Maxwell 1995). Findings from the arrest studies are summarized in table 1.

Table 1. Summary of Findings of the Minneapolis Domestic Violence Experiment and Subsequent Replications: Same Victim

Findings	City					
	Minneapolis	Omaha	Charlotte	Milwaukee	Colorado Springs	Miami-Dade
6 month deterrent effect official measures	yes	no	no	no	no	1 of 2
6 month deterrent effect victim interviews	yes	slight	no	no	yes	yes
6-12 month escalation effect official measures	no	yes	yes	yes	no	no
6-12 month escalation effect victim interviews	*	no	no	no	no	no
escalation effect for unemployed	*	yes	*	yes	yes	*
deterrent effect for employed	*	yes	*	yes	yes	*

* no relationship reported

These studies provide mixed support for the applicability of the additive deterrence hypothesis (Grasmick and McLaughlin 1978) to an understanding of specific deterrence among domestic violence offenders (Sherman and Smith 1992). Although the additive hypothesis was originally derived under general deterrence theory, Sherman and Smith (1992) recognized that legal sanctions could maintain significant effects on recidivism independent of informal social controls.

The conflicting results across studies prompted speculation as to why arrest appears to "work" for some offenders and in some cities. One possible explanation is that the mixed findings could be a result of variation in formal social controls implemented by these courts after arrest (Garner, Fagan, and Maxwell 1995; Sherman and Smith 1992; Zorza 1992). Formal social control here refers to actions taken by the criminal justice system in response to crime (e.g., arrest, prosecution, and sentencing). More punitive dispositions may result in lower recidivism if individuals are deterred by such punishments. Any type of disposition that forces a person to do something that (s)he otherwise would not do can be considered a punishment (Packer 1968), but different dispositions provide varying degrees of inconvenience (Black 1976). For arrestees, not filing charges would be the mildest inconvenience to a suspect, followed by

subsequently dropped charges (due to some time spent in custody), prosecution and conviction (due to time spent in court), and sentencing (due to time spent serving a sentence). Therefore, the studies of jurisdictions providing less punitive legal sanctions after arrest may have produced no significant findings for arrest effects. This suggests that analyses of court dispositions may yield support for the additive (specific) deterrence hypothesis. Considering court sanctions is particularly important now that many jurisdictions (including Cincinnati) operate under a mandatory arrest policy for cases of domestic violence, so variation in the decision to arrest has been reduced considerably in some jurisdictions.

Another possible explanation for the inconsistent findings of the arrest studies involves the variation in how offenders were assigned to the treatment groups across those studies (Sherman, Schmidt, and Rogan 1992; Sherman and Smith 1992). Questions concerning whether arrest decisions had been consistently randomized within the experiments prompted concerns that the results could have varied if arrested suspects differed significantly in their levels of risk across studies. This led to interest in sub-group analyses of data from some of the studies in order to examine whether arrest was more effective for groups varying in social dimensions of risk (e.g., employed versus

unemployed persons). An analysis of whether the effects of legal sanctions are conditioned by levels of informal social controls constitutes a test of two other deterrence hypotheses applied to an understanding of specific deterrence: conditional deterrence, predicting that legal sanctions are only effective for deterring individuals who are committed to more conventional societal values (Sherman and Smith 1992; Tittle and Logan 1973; Williams and Hawkins 1986), and replacement deterrence, predicting that legal controls only deter persons who lack effective informal social controls (Grasmick and McLaughlin 1978; Sherman and Smith 1992; Silberman 1976).

Underlying the sub-group analyses of the data from Minneapolis, Omaha, Milwaukee, and Colorado Springs was the idea that individuals with a higher "stake in conformity" may be more likely deterred from committing additional acts of domestic violence subsequent to an arrest for domestic violence. This framework was borrowed from Jackson Toby (1957). According to Toby, persons differ with respect to their stake in conformity in that some have little to lose by committing crimes. As such, persons with lower stake in conformity are less likely to be deterred by formal sanctions. Applying Toby's idea to arrest effects on recidivism, investigators speculated that employed and married individuals each have a higher stake in conformity

(compared to unemployed and unmarried persons) and, as a consequence, have more to lose through formal criminal justice sanctions. Therefore, these individuals may be more likely deterred through arrest.

Findings emerged from the sub-group analyses that support the stake in conformity hypothesis. Using the data from Colorado Springs, Omaha, and Milwaukee, Berk and his colleagues (1992) created risk categories for offenders who were either employed or serving in the military. As hypothesized, the authors found a conditional deterrent effect of arrest for "good risk" individuals; that is, for individuals who were either employed or serving in the military at the time of arrest (Berk et al. 1992). Interestingly, an escalation effect was found for unemployed offenders, introducing the possibility of a labeling effect of legal sanctions for this sub-group (Berk et al. 1992). A re-analysis of the Milwaukee data also revealed that employed, married, and high school graduate offenders were all less likely to be involved in repeat offenses as measured by "hotline" reports made by the police to a battered women's shelter (Sherman and Smith 1992). It should be noted, however, that the findings for marital status in other re-analyses were not always in the hypothesized direction (Garner, Fagan, and Maxwell 1995). Nonetheless, these observations suggest that the stake in

conformity hypothesis offers a plausible explanation for the inconsistent results stemming from the arrest studies. They also support the conditional hypothesis of specific deterrence while refuting the replacement hypothesis (Sherman and Smith 1992).

Differences in the findings for the main effects of arrest on recidivism could have also resulted from variation in social structure across the cities examined. Similar to the argument involving individual-level stake in conformity, the social structure of a community could influence the deterrent effects of criminal justice sanctions for particular offenders (Sherman, Schmidt, and Rogan 1992; Sherman 1993b). According to Sampson and Wilson (1995), explanations of criminal behavior tend to rely solely on individual-level characteristics. However, several structural characteristics of neighborhoods (e.g., economic levels, family disorganization) have been found to correlate significantly with individual-level delinquency or adult criminality (Elliott et al. 1996; Gottfredson, McNeil, and Gottfredson 1991; Gunn et al. 1993; Sampson and Wilson 1995; Simcha-Fagan and Schwartz 1986). Therefore, not only may individuals with a higher stake in conformity feel that they have more to lose by committing crime, but also individuals residing in communities comprised of a significant proportion of higher-stake individuals may be deterred because of the possible stigma

faced if caught committing a crime. This would be consistent with the conditional hypothesis of specific deterrence.

From a theoretical perspective, the application of Toby's thesis and the speculation regarding contextual effects of community-level stake in conformity represent interesting extensions of sociological theory to an important criminal justice issue. This perspective provided the framework for the current investigation.

Informal Social Controls and Stake in Conformity

Since Durkheim (1933) first mentioned the concept of informal social control in his theory of anomie, the multiple dimensions of informal control have been called different terms by different criminologists. However, these terms all have in common an emphasis on two elements: direct and indirect social control (Kornhauser 1978). A breakdown in either may be sufficient to explain deviant behavior. Direct control involves attempts made by such social institutions as the family, school, and work to restrain an individual's behavior through supervision, whether through restrictions placed directly on an individual's behavior by others, or involvement in conventional activities and pastimes that limit opportunities to engage in deviance (Hagan, Simpson, and Gillis 1987; Hirschi 1969; Nye

1958; Reckless 1961; Reiss 1951; Wells and Rankin 1988). In contrast, indirect social control involves an internalization of conventional values emerging out of socialization and interdependence (Durkheim 1933; Hirschi 1969; Reckless 1958; Reiss 1951; Shaw and McKay 1942). The strength of social relationships may provide an explanation of criminal behavior. Further, persons who are strongly connected to informal social control institutions (i.e., the family, work, or school) are provided with the necessary social and psychological resources, such as conformist self-identity and emotional support, which to draw upon when faced with the temptation of criminal behavior (Hagan, Simpson, and Gillis 1987; Braithwaite 1989; Sampson and Laub 1993).

Control theory per se, grew out of the work of Shaw and McKay from the Chicago school. According to their theory of social disorganization, one of the consequences of disorganization is a breakdown in informal social control (Shaw and McKay 1942). Institutions such as the family, school, and church lose their ability to exercise informal social control over the individual members of a neighborhood (Shaw and McKay 1942). Parents are unable to meet their children's needs and lose their ability to effectively socialize their children.

The first sociologist after Shaw and McKay to pick up on the

idea of crime resulting from a breakdown in control was Albert Reiss in 1951. Reiss (1951) argued there are two types of control: personal and social. Personal or internal control refers to the ability of an individual to resist meeting his or her needs in ways that conflict with society's norms and values. Social or external control refers to the ability of social institutions and groups to make rules and norms binding on the individual members. Delinquency is a product of a breakdown in either type of control or both (Reiss 1951).

Jackson Toby (1957) introduced his stake in conformity hypothesis relatively soon after Reiss' control theory was proposed. Toby's hypothesis was in response to claims made by social disorganization theorists that neighborhood factors such as high rates of poverty, transience, and heterogeneity explain rates of delinquency (Shaw and McKay 1942). According to Toby, such factors are insufficient in their explanation because they fail to explain why some individuals residing in socially disorganized areas do not become delinquent. Persons differ with respect to their stake in conformity. Some have more to lose by committing crimes. As such, individuals with a higher stake in conformity are more likely to be deterred by formal sanctions.

Other control theories were introduced after Toby proposed his stake in conformity thesis. Hirschi's (1969) social bond

theory is perhaps one of the most well known, and a particular aspect of this theory maintains a close conceptual linkage to Toby's thesis. Hirschi's social bond theory asserts that conformity can be explained by an individual's bond to society. This bond is comprised of four related elements, but if an individual is weak on one element the bond will be weak overall (Hirschi 1969). Moreover, a weakness of any one element is sufficient to explain delinquency.

The elements of social bond include attachment, commitment, involvement and belief. Attachment refers to the extent an individual is emotionally invested in his or her family and friends. Commitment refers to the extent an individual is invested in conventional activities. Involvement refers to the amount of time and resources an individual invests in conventional activities and with his or her family and friends. Finally, belief is the element that refers to the extent to which an individual subscribes to and adopts the rules and norms of society (Hirschi 1969).

Hirschi's concept of commitment fits well with Toby's concept of stake in conformity. Hirschi's observation that a weak link in the bond contributes to a weak bond overall implies that individuals with lower stakes in conformity (lower commitment)

should maintain weak social bonds. Although Hirschi's social bond theory was originally offered as an explanation for delinquency, Sampson and Laub (1993) put forth a control theory to explain adult criminal behavior that integrates Hirschi's social bond theory with factors emergent over an individual's life course. Whereas the research on Hirschi's social bond theory as a whole tends to suggest only moderate to weak relationships between social control factors and delinquent involvement, Sampson and Laub (1993) found that adult social bonds in the form of job stability and marital attachment are strong predictors of criminal behavior. This bond can even be significant enough to negate the effects of prior delinquent involvement.

Aside from the specific parallel to Hirschi's concept of commitment, Toby's stake in conformity hypothesis emulates other control theories as well with the idea that persons with greater stake in conformity are insulated from the enticements of illegal behavior. Applying his thesis to delinquency, Toby noted that a juvenile's stake in conformity depends in part on a family's socio-economic status as well as the juvenile's success in education. This is why education (whether a high school graduate) and employment (whether or not) were previously examined by the arrest researchers in the context of Toby's hypothesis (Berk et al. 1992; Sherman and Smith 1992). However, additional dimensions

of these factors such as college degrees and type of employment (skilled versus unskilled) are also relevant to the broader concepts of educational achievement and socio-economic status applied to adults. Other individual-level attributes such as financial independence (not receiving public assistance) and residential stability are pertinent as well. Regarding the latter, Sampson (1992) observed that stability in residence increases an individual's sense of attachment to their community which increases an individual's friendships. This may, in turn, contribute to an individual's investment in conformity.

The dimensions of informal social control were originally applied to an understanding of primary delinquency and adult criminality. Yet Toby's stake in conformity thesis may also apply to an understanding of recidivism in the population of offenders [as discussed by Berk et al. (1992), Dunford, Huizinga, and Elliott (1990), Pate, Hamilton, and Annan (1991), and Sherman and Smith (1992)]. In short, offenders with higher stakes in conformity may be less likely to recidivate. As noted by the arrest researchers, an offender's stake may also mediate the effects of legal sanctions on recidivism. Punishments may either decrease, increase, or have no effect on crime depending upon the context in which the sanction is administered. Toby (1957) argues that persons with a significant stake in conformity may feel that

they have more to lose through formal sanctioning and are thereby more likely to be deterred. Therefore, more punitive sanctions may be more effective for reducing recidivism among offenders with higher stakes.

Considering Contextual Effects of Stake in Conformity

Informal social controls may operate at both the individual- and aggregate-level. The idea that informal social control processes operate on an aggregate level and that variations in these processes can explain rates of crime and delinquency forms the foundation of social disorganization theory (Shaw and McKay 1942). Poverty and transience can result in a neighborhood becoming "disorganized" (Shaw and McKay 1942). A consequence of disorganization is that the neighborhood -- an institution of social control -- can no longer exercise control over residents either directly through supervision or indirectly by transmitting conventional values (Kornhauser 1978). Crime and delinquency may flourish in such a neighborhood. Shaw and McKay also noted that disorganized neighborhoods contain high rates of repeat offenders.

Social disorganization theory as well as subsequent ecological theories of crime and delinquency were put forth as aggregate-level theories to explain rates of crime and

delinquency. Structural characteristics of a community such as family disruption, residential mobility, and neighborhood socio-economic status were variables found to be related to the crime rate. However, it has been hypothesized that the structural characteristics of a neighborhood can produce contextual effects on the behavior of individuals (Sampson 1991; Sherman 1992). A recent body of research renders support for examining the structural characteristics of a neighborhood as influences on the behavior of individuals (Elliott et al. 1996; Gottfredson, McNeil, and Gottfredson 1991; Gunn et al. 1993; Simcha-Fagan and Schwartz 1986).

Sampson (1991) examined the contextual effects of community characteristics on individual behavior. He found that an individual's length of residence as well as community residential stability increase an individual's attachment to a community by increasing an individual's friendships. This, in turn, is correlated with self-reported victimization. Pertinent to this study, Sampson (1991) observed that neighborhood characteristics may shape how parents relate to their children (i.e., through "family management practices"). An extension of this idea is that these characteristics may also influence how parents/partners relate to each other.

Toby (1957) himself observed that stake in conformity may

also operate at the aggregate-level to influence crime. Despite his criticism of social disorganization theory, Toby argued that his thesis might also explain differences in crime rates between neighborhoods. Specifically, communities with larger proportions of "higher stake" residents should have lower crime rates, representing an effect beyond simply adding individual-levels of stake in conformity among residents of a community. Larger proportions of higher stake individuals may effectively insulate potential offenders from engaging in deviance because of the negative stigma associated with legal sanctions by larger numbers of residents. Although Toby's observation relates to aggregate crime rates, Sherman (1992) extended this idea to possible contextual effects on individual-level likelihoods of recidivism.

Consistent with Toby's idea, neighborhood levels of stake in conformity should have a main (negative) effect on recidivism among offenders. Moreover, consistent with Sherman's (1992) idea, the effects of legal sanctions should also be mediated by aggregate-level stake in conformity. Existing research on domestic violence recidivism lends some support for this idea. Colorado Springs had a higher employment rate compared to Milwaukee, and a greater deterrent effect of arrest was found in Colorado Springs (Sherman 1992). Upon reviewing the inconsistent findings from the replication studies on arrest, Sherman (1992)

hypothesized that informal social control processes, such as employment, could also operate at the aggregate-level. That is, the higher the proportion of employed individuals residing in a community, the greater an individual's stake in conformity might be. These individuals may be more likely deterred through formal sanctioning (Sherman 1992). Sherman's hypothesis ties into Sampson's finding regarding community attachment. The argument can be made that the structural characteristics of a neighborhood influence an individual's attachment to their community and that the stronger an individual's attachment is to their community, the greater will be that individual's stake in conformity. The present study tests the ecological hypothesis by examining variation in census tract characteristics that reflect variation in aggregate-level dimensions of stake in conformity.

Some of the dimensions of aggregate-level stake in conformity focused on here can also be tied to various aspects of more recently developed ecological theories of crime. For example, the structural characteristics of a community may also influence individual criminal behavior by having an impact on "social capital," or the capabilities and efforts of individuals, families, and groups to solve commonly experienced problems (Coleman 1990; Hagan 1994). Social inequality coupled with a declining economy generates a process of "capital inequality"

caused by residential segregation, race-linked inequality, and concentrations of poverty (Hagan 1994). These processes of capital disinvestment discourage the creation of social capital where persons are less likely to invest in other individuals and the larger community (Coleman 1990; Hagan 1994).

The interactions of persons participating in social groups are a source of social capital. The consideration of residential stability in the present study fits well with this idea. Hagan, MacMillan, and Wheaton (1996) argue that residential stability offers (indirectly) an indication of the amount of social capital within a community because people need time to establish personal and social networks. According to Coleman (1990), social organization creates social capital and allows individuals to achieve goals that they would not be able to attain on their own. Applying this idea to the other dimensions of stake in conformity considered here, the facilitation of goal attainment may be further enhanced if social networks involve persons with higher socio-economic status.

The aggregate-level dimensions of stake in conformity related to employment and financial independence also overlap somewhat with Sampson, Raudenbush, and Earls' (1997) concept of concentrated disadvantage and the hypothesized effects of poverty and unemployment (in addition to other aspects of disadvantage)

on collective efficacy, or the willingness of individuals to help other residents of their neighborhoods. Although not a study of crime per se, Sampson and his colleagues speculated that victimization likelihoods may be lower for persons in less disadvantaged neighborhoods because of greater collective efficacy.

Methods

Sample and Data

The sample consists of all persons arrested for misdemeanor domestic violence in Hamilton County, Ohio during two time periods: August 1, 1993 to October 31, 1993 (the pilot study), and January 1, 1995 to December 31, 1996 (the grant study). Those arrested between November 1993 and December 1994 are not included because the county adopted a data base system for record keeping beginning in 1995. The original paper documents (including the information necessary to construct all of the independent variables for the model) for persons arrested prior to 1995 were unavailable by the time the grant project began. However, this did not prevent following up all of the cases included in the pilot study for every month after their original arrest. This is because a different data base was used to access the re-arrest data. It is a limited data base that does not include information

aside from the types and dates of re-arrests.

Selecting all arrests for these periods provides 3,954 unique individuals (due to repeat offending). For persons with multiple arrests, the earliest ones were coded as the initial arrests and all subsequent ones were coded as re-arrests for domestic violence. Available for analysis are 3,662 cases since 292 persons left the jurisdiction and could not be tracked.

The individual-level data were compiled from arrest reports, intake interview forms, and court records. The census tract data were obtained from the 1990 U.S. Census of Population and Housing. Intake interview data provided the demographic and legal characteristics of suspects and their initial arrests, court data provided the dispositions, and arrest data provided the dates and types of all re-arrests until May 31, 1998. This permitted an analysis of a fixed two year follow-up period for 3,110 suspects in the sample. The data allow for a survival analysis of varied follow-up periods (from 17 to 58 months) for all 3,662 suspects.

Hamilton County has 217 census tracts, one of which is completely commercial (i.e., no personal residences exist within the tract). Census tracts are created by local committees appointed by the Census Bureau to approximate what local residents perceive their neighborhood to be (Gunn et al. 1993). They are also created to be as homogeneous as possible in terms

of socio-demographic characteristics.

Census tracts have been criticized as not being theoretically meaningful in terms of providing data on those factors believed to mediate the effects of community structural characteristics (Sampson and Groves 1989). However, they have been shown to provide valid measures of the types of community structural characteristics examined here (Gottfredson, McNeil, and Gottfredson 1991). Further, a study conducted by Simcha-Fagan and Schwartz (1986), which compared measures derived from census data with data collected from interviews with community residents, revealed that the two measures were highly correlated in the expected direction. For these reasons, census tract data were used to construct the aggregate-level measure of stake in conformity. Also, aggregating across census tracts to create neighborhoods would result in a loss of internal homogeneity that is provided by the census tracts.

It must be noted that cases were not randomly assigned to various disposition groups, bringing the applicability of statistical inference into question (Berk and Freedman 1995). Short of obtaining a sample where every individual has the same selection probability, Berk and Freedman (1995) call for a more careful focus on the research questions that can be addressed by such data. For example, the results of the present study can be

used to forecast findings of future studies of the topic. If future studies yield consistency with some or all of the findings presented here, this would enhance the external validity of particular findings. This is useful for specifically defined populations from which the sample is drawn, as is the case for the present study (i.e., the population of persons arrested for misdemeanor domestic violence in an urban jurisdiction).

A form of cross-validation may also help to reduce concerns over these types of data (Berk and Freedman 1995). By dividing a large sample into two or more groups drawn randomly from the larger sample, findings can be compared across the sub-samples for consistency (although this does not provide a complete solution since the sub-samples still come from the same study). This was done for the present study by ordering the cases by social security numbers and systematically selecting every other case in order to create two sub-samples. The same analyses were conducted for each sub-sample, confidence intervals were constructed for parameter estimates, and an analysis was performed on whether the estimates from one sub-sample fell into the intervals from the other sub-sample. Only two percent of the estimates differed significantly between the two sub-samples, a figure that could have been expected by chance alone. This

supports the idea that, at a minimum, the findings presented here can be used to forecast those from future studies of the topic.

Inferences to the population of interest may also be more sound with longitudinal data, statistical controls for risk, and predictors that do not create collinearity problems such as unstable parameter estimates and large standard errors. All of these elements characterize the study presented here.

Variables

The measures and univariate descriptives are presented in table 2. Three dependent variables were examined based on recommendations by Blumstein, Cohen, and Nagin (1978) for sentencing research: whether a person was re-arrested for any domestic violence during a fixed, two year follow-up period beginning after the sentence (if any) had been completed (N=3,110), the number of re-arrests for domestic violence during the same follow-up, and the number of months between the end of an offender's sentence and re-arrest for domestic violence during a varied follow-up spanning 17 to 58 months (N=3,662). [These types of measures were examined in a re-analysis of the arrest data conducted by Garner, Fagan, and Maxwell (1995)]. The incidence measure was collapsed into four categories due to the heavily skewed distribution of re-arrests. The descriptives in

table 2 reflect all 3,662 suspects.

Table 2. Variables and Frequencies (Means and Standard Deviations Reported for Interval/Ratio Scales)

Variable	Category Categories	Frequencies
Dependent		
Re-arrested for d.v. during the 2 year follow-up	0 = no 1 = yes	2,662 448
Number of re-arrests for d.v. during 2 year follow-up	0 = none 1 = one 2 = two 3 = three or more	2,662 353 71 24
Months to re-arrest for d.v. during varied follow-up*	M = 9.77 s = 9.29 range: 1-35	n = 589
Case Processing and Outcome		
No charges filed	0 = no 1 = yes	3,438 224
Charges dropped	0 = no 1 = yes	2,013 1,649
Acquitted at trial	0 = no 1 = yes	3,427 235
Offender program	0 = no 1 = yes	3,416 246
Probation with or without a fine	0 = no 1 = yes	3,113 549
Jail with or without a fine	0 = no 1 = yes	3,379 283
Probation + jail with or without a fine	0 = no 1 = yes	3,551 111
Charges pending	0 = no	3,179

at arrest	1 = yes	483

continued...

* Analysis of this dependent variable included all cases in the sample, but the univariate descriptives reflect recidivists only.

Table 2. (Continued)

Variable	Category Categories	Frequencies
Stake in Conformity		
Individual-level s.i.c. (sum of z-scores for...)	M = 0.00 s = 3.00 range: -7.3-8.4	n = 3,662
High school degree	0 = no 1 = yes	502 3,160
Bachelors degree	0 = no 1 = yes	3,512 150
Employed	0 = no 1 = yes	1,574 2,088
Employed in skilled job	0 = no 1 = yes	3,001 661
Not receiving public assistance	0 = no 1 = yes	654 3,008
Same residence at least five years	0 = no 1 = yes	3,104 558
Aggregate-level s.i.c. (sum of z-scores for...)	M = 0.00 s = 4.60 range: -14.6-9.1	n = 216
Proportion high school graduates	M = 0.72 range: 0.22-0.97	s = 0.15

Proportion college graduates	M = 0.20 range: 0.00-0.69	s = 0.16
Proportion pop. employed	M = 0.93 range: 0.52-1.00	s = 0.07
Proportion in skilled occupation	M = 0.24 range: 0.04-0.49	s = 0.10
Proportion not receiving public assistance	M = 0.89 range: 0.35-1.00	s = 0.13
Proportion same residence past 5 years	M = 0.54 range: 0.13-0.75	s = 0.11

continued...

Table 2. (Continued)

Variable	Category Categories	Frequencies
Control Variables		
Male	0 = no 1 = yes	585 3,077
Age (in years)	M = 32.2 range: 18-90	s = 9.5
# prior convictions for violent misdemeanors	M = 0.56 range: 0-13	s = 1.11
Ever incarcerated for other than d.v.	0 = no 1 = yes	2,395 1,267
Not living with spouse and/or children at arrest	0 = no 1 = yes	2,692 970

Some of the published reports on arrest focused only on recidivism involving the same victim (i.e., Omaha, Charlotte, Minneapolis, and Miami), and many were limited to six month follow-up periods. Sherman and Smith (1992) subsequently criticized the use of the same victim measure due to its exclusion of domestic violence against other persons, and Reiss (1985) noted that focusing only on the same victim ignores possible displacement effects. This led to the construction of measures tapping re-arrest for domestic violence against any victim.

There is a potential problem with using arrests as measures of offense behavior and recidivism in that arrest data do not tap undetected crime. Sherman, Schmidt, and Rogan (1992) observed that the use of official measures yielded the same conclusions as victim interviews in 5 of the 7 arrest studies (5 of the 6 study sites) regarding the deterrent effects of arrest. While this lends greater credence to the use of arrest data, the two arrest studies yielding conflicting results reflect some potential for bias.

From a different perspective, using arrest data might be problematic if some of the arrests do not meet the standard of legal guilt. However, focusing only on convictions fails to account for an even greater number of cogent cases that are dismissed for any given reason (especially domestic violence

cases). Research comparing arrests and convictions suggests that arrests are a preferable measure of recidivism (Maltz 1984).

There are seven dummy variables tapping the entire range of court dispositions for the sample (see table 2), and "no charges filed" is excluded from the complete model for purposes of estimation and interpretation. Results for the other six disposition measures should then be interpreted relative to not filing charges (or "doing nothing" with those arrested). Differences between the coefficients for the dispositions included in the model can also be examined to determine whether some dispositions are "better" or "worse" than others. This can be done by constructing 95 percent confidence intervals around the coefficient estimates and seeing whether the estimates for other coefficients fall outside these intervals. Those falling outside can be treated as significantly different.

A dichotomous measure of whether suspects had old charges pending at arrest is also included in the complete model as a somewhat crude measure of the speed of case processing although this variable is not a "disposition" per se.

The variable labeled "offender program" refers to a two day counseling session related to domestic assault. Some of these offenders may have also served time on probation and/or in jail, but the number of those who did were too limited to permit a

separate (reliable) examination of such sentences.

A measure of sentence length (in months) was originally examined but excluded from the model because of its high correlation with the dummy variable tapping split sentences and its highly skewed distribution. Moreover, results for such a measure do not offer much insight into effective policies designed to reduce domestic violence recidivism because of its limited distribution for convicted misdemeanants. For example, whether offenders should be sentenced to one versus two months seems trivial.

The individual- and aggregate-level measures of stake in conformity each consist of the same six items measured at different levels. These items tap education, employment, public assistance, and residential stability (see table 2). This was done in order to determine the relative importance of the same dimensions measured at different levels. Measures tapping different dimensions at each level would lead to the question of whether the results for one level versus another were due to the greater importance of a particular level or simply the difference in measures between the two levels (Langbein and Lichtman 1978). The choice of items was determined in part by available census data (e.g., the U.S. Census Bureau defined residential stability in 1990 as residing at the same residence for 5 or more years),

and available individual-level data (e.g., public assistance recipients were known although actual income was unavailable). Therefore, it should be noted that the data set includes a number of additional individual- and aggregate-level variables that can be manipulated to create other measures of informal social control not included in the complete model examined for the analysis.

The redundancy in the education measures (i.e., including two variables at each level tapping high school and college) is justified on three grounds: Sherman and his colleagues found that arrestees with high school degrees were less likely to be re-arrested compared to those without high school degrees; a bachelors degree may provide an effect beyond having a high school degree; and only 4 percent of the sample had bachelors degrees (further justifying the inclusion of "high school degree").

A composite measure including all six items was constructed at each level. A measure including aspects of education, employment, public assistance, and residential stability seems to be a better reflection of the relatively broad concept of stake in conformity. Also, the addition of six measures to the complete individual-level model creates problems of multicollinearity within some of the census tracts, and the six aggregate-level measures are all strongly correlated ($R > 0.60$). These problems

are avoided with the composite measures. Finally, tests for interactions between court dispositions and the separate measures of stake in conformity would not be independent of each other due to the significant correlations between these items.

Equal weight was given to each element of the composite measure by summing the standard scores for these items within each level. The zero-order correlation between the individual and aggregate factors is 0.52 (R), indicating that each factor accounts for 27 percent of the variation in the other. The items included in these factors are offered only as proxy measures for Toby's thesis since they are really structural antecedents to the concept of stake in conformity rather than stake in conformity processes in and of themselves.

The individual-level measure of stake in conformity reflects offenders only whereas the aggregate-level measure reflects the population of offenders and non-offenders. This may explain the smaller range for the individual measure relative to the aggregate measure. Yet the significant variation that does exist in the individual measure (see table 2) suggests that the concept of stake in conformity may help to distinguish non-recidivists from recidivists. This is consistent with the applications of the thesis by Berk et al. (1992), Dunford, Huizinga, and Elliott (1990), Pate, Hamilton, and Annan (1991), and Sherman, Schmidt,

and Rogan (1992).

Some of the stake in conformity measures examined, such as education and employment, can be considered structural antecedents of informal social control, not informal social control processes in and of themselves. These variables are only offered as proxy measures for Toby's stake in conformity thesis. Hindelang (1973) noted that these types of variables do not tap elements of social control theory directly due to the social psychological nature of these elements. For example, to say that a person who has lived longer in one neighborhood is more "invested" in that neighborhood is not necessarily true, and the degree of this investment cannot be known without understanding the individual's perception of a neighborhood. Therefore, these measures are not perfect measures of the theoretical concepts. It is assumed, however, that there is overlap between the two (i.e., persons residing longer in particular neighborhoods are more likely to be invested in those communities).

Two measures of criminal history are included in the models as controls for risk of re-offending. For length of prior record, seven ratio measures were examined in order to identify the one that absorbs the most variation in re-arrests. These measures included the number of prior convictions for all misdemeanors, number of prior convictions for violent misdemeanors, number of

prior convictions for all felonies, number of prior convictions for violent felonies, number of prior convictions for all violent crimes, number of prior convictions for all felonies and misdemeanors, and number of prior convictions for all felonies, misdemeanors, and petty misdemeanors. Of these measures, prior convictions for violent misdemeanors is the best predictor. Prior incarceration for offenses other than domestic violence is also included because of the idea that the risk of domestic violence is somewhat higher for persons with histories of other criminal offending [see Bartol (1996) for a review of this literature].

Whether an individual was living with his/her spouse or partner at the time of the arrest that brought him/her into the sample is included as a control variable even though some might consider it to be an aspect of stake in conformity. This measure is used instead of marital status due to its stronger zero-order correlations with the outcome measures. While living arrangements may be an important aspect of stake in conformity in studies of other types of crime, in the context of understanding domestic violence it is treated here as a control variable because it taps variation in the opportunity to victimize one's partner.

A suspect's race was considered as a control variable but was dropped because of its non-significant relationships with the outcome measures in the complete model (despite significant zero-

order correlations at the 0.05 probability level).

Analytical Procedures for Examining the Prevalence and Incidence of Re-arrest

Hierarchical non-linear modeling was used for the analysis of the prevalence and incidence of re-arrest during the two year fixed follow-up period. This procedure is appropriate due to the inclusion of variables measured at two different levels of analysis. The statistical software used was HLM 4.04 (Bryk, Raudenbush, and Congdon 1996). The dichotomous dependent variable was examined using a non-linear model with a Bernoulli distribution whereas the limited interval scale was examined using a non-linear model with a Poisson distribution.

Hierarchical non-linear modeling is capable of estimating these types of models because of its ability to (a) center the individual- and aggregate-level stake in conformity measures (and interactions including these measures) to provide unique estimates of their separate effects, (b) weight the individual-level analysis to correct possible problems associated with differences in selection probabilities for the individual-level cases stemming from differences in the census tract of residence, (c) weight the aggregate-level analysis to correct heteroscedasticity resulting from unequal numbers of individual-level cases within each aggregate, and (d) provide two-stage

estimation to generate a reliable test of the aggregate-level null hypothesis (i.e., basing the test on the number of aggregates rather than the number of individual-level cases). For a discussion of alternative methods of estimation and their relative weaknesses, see Kreft and DeLeeuw (1998).

For the analysis of interactions between stake in conformity and the legal variables, there is an option in the HLM 4.04 software to estimate interaction effects between variables measured at level 2 (the aggregate-level) and those measured at level 1 (the individual-level). Such tests are not an option for interactions consisting solely of level 1 measures, so product terms for interactions between the individual-level stake in conformity measure and the legal variables were added to the complete models. At either level, a significant interaction coefficient would indicate a stronger relationship between a particular disposition and recidivism for higher or lower levels of stake in conformity compared to "doing nothing" with those particular offender groups. For example, split sentences may correspond with significantly less recidivism for higher stake persons compared to not filing charges against those particular individuals.

HLM 4.04 provides estimates of sigma-square (variance in an outcome measure existing within the aggregates at level 1) and

tau00 (variance existing between the aggregates at level 2). These two statistics should provide the information necessary to calculate improvements in explained within-group variance and explained between-group variance as one moves from an unconditional model (with no predictors) to more complex models. However, unlike the printout for linear analyses, the printout for non-linear analyses reports the change in tau but not the change in sigma-square. The latter value presented in the printout is always the within-group variance from the unconditional model. This makes it difficult to calculate a statistic reflecting the fit of the whole model. Therefore, the only model statistic presented is the proportion of the between-group variance explained by each model. A Science article by Sampson, Raudenbush, and Earls (1997) includes non-linear models estimated with the software. They also present only this model statistic.

Analytical Procedure for Examining Time to Re-arrest

The third outcome measure of re-arrest is the number of months that elapsed between the end of an offender's sentence and a re-arrest for domestic violence during the varied follow-up period including all suspects (see table 2). Unfortunately, there is no statistical technique available that provides a two-stage

estimation technique (such as hierarchical modeling) for right-censored data. Therefore, a pooled event history analysis with Cox regression was used to estimate the relative effects of the predictors in the complete model on time to re-arrest. Interaction terms for both levels of analysis were created by constructing product terms for court dispositions by stake in conformity. The results for the aggregate-level main and interaction effects from this analysis should therefore be interpreted with caution. However, we believe that the results for the individual-level effects of court dispositions and stake in conformity are valid.

Life table analysis was also used to describe the time to recidivism for each disposition group. Both event history analysis and life tables are appropriate for right-censored data and unequal follow-up periods (Allison 1984). The "life table" is generally used to follow the experiences of a closed cohort of persons born in the same year over time (Keyfitz 1977). It is used in mortality research, where the death of a cohort member constitutes a "decrement" to the life table. For this study, recidivists constitute the decrements to the life table and the cohorts being followed over time include persons receiving identical dispositions.

Two life table measures were compared across disposition groups: the cumulative survival (non-recidivism) rate, and the

hazard rate of recidivism. In demography, a hazard rate is an age-specific decrement rate defined as the probability that an individual who has survived to a given age will "exit" the cohort during that specific age interval (Kalbfleisch and Prentice 1980). The hazard rate examined here is a time-specific recidivism rate, describing the probability of recidivism during month b for an individual who has not yet recidivated before reaching month b. Although the follow-up spanned 58 months for some offenders, everyone re-arrested for domestic violence was re-arrested by the thirty-fifth month.

Results and Discussion

Figure 1 displays a map of the geographic distribution of persons arrested for domestic violence within the city limits of Cincinnati. A map of the distribution of domestic violence for Hamilton County is also displayed in figure 2. These maps are presented for two reasons. First, they help to describe the distribution of domestic violence in a major metropolitan area. Second, these maps provide an appreciation for the variation that exists between census tracts in the distribution of domestic violence (particularly figure 1). Yet despite the relatively broad distribution of domestic violence in figure 1, note the heavier pockets of arrests closer to the central business

district of Cincinnati as well as areas closest to the city's major thoroughfares (I-71, I-74 and I-75). These pockets are consistent with the ecological hypothesis that poorer areas with more transient populations coincide with higher rates of predatory crime.

Regarding recidivism for domestic violence, roughly 14 percent of the 3,110 suspects followed for at least two years were re-arrested for domestic violence. Of the 3,662 suspects in the larger sample, 16.1 percent were re-arrested at least once during the varied follow-up.

Other univariate descriptives for the sample are also worth reviewing, some of which are not displayed in table 2. Roughly one-quarter of the sample were not even living with the victim at the time of arrest. Over 40 percent of the sample were also unemployed, and nearly 18 percent were receiving no financial support upon arrest. The vast majority of the sample (96 percent) did not have college degrees and only one-third held jobs for at least a year.

A review of the other criminal history measures explored for the study indicated that a majority of the suspects (63 percent) have prior records. A significant percentage of the sample have been incarcerated previously for an offense other than domestic violence (35 percent), suggesting that many of these offenders

are somewhat eclectic in their criminal activities.

While a clear majority of the suspects are male, 16 percent are female. Over one-third of those arrested were 35 years or older at the time of arrest whereas 23 percent were under age 25. Finally, African-Americans constitute 60 percent of the sample.

Approximately 35 percent of the variance in the prevalence of re-arrest exists between census tracts whereas 65 percent exists within these tracts (at the individual-level). Similarly, for the incidence of re-arrest, the figures are 38 percent (between-group variance) and 62 percent (within-group).

Zero-order Relationships

Table 3 presents zero-order main effects of the predictors on the incidence of (number of re-arrests) and time to re-arrest for domestic violence. Relationships involving the prevalence of re-arrest are not presented because they are not significantly different from those for incidence. Results for the measure of no filed charges are presented because it is only necessary to leave this variable out of the complete model for estimation purposes.

Table 3. Zero-order Main Effects of Predictors on the Incidence of and Time to Re-arrest for Domestic Violence (Unstandardized Fixed Effects Reported)

	Dependent Variables	
Predictor	# rearrests (Poisson)	Months to re-arrest (Cox Regression)
Individual-level		
Control Variables		
Male	0.593**	0.760**
Age	-0.022**	-0.023**
# priors	0.307**	0.228**
Other than dv inc.	0.434**	0.428**
No family contact	-0.817**	-0.788*
Case processing and Outcome		
No charges filed	0.336*	0.476**
Charges dropped	-0.104	-0.122
Acquitted	-0.402*	-0.264
Offender program	-0.553**	-0.414*
Probation	0.595*	-0.010
Jail	0.396**	0.229
Probation+jail	0.879**	0.633**
Pending charges	1.038**	0.889**
Stake in Conformity	-0.056**	-0.055**

Aggregate-level		
Stake in Conformity	-0.036**	-0.032**
N	3,110	3,662

* $p < 0.05$; ** $p < 0.01$

Results for the court disposition variables reveal that re-arrests are fewer and time until re-arrest is longer for suspects who (a) have charges filed against them (versus those who do not), (b) undergo the domestic violence offender program, and (c) do not have old charges pending at the time of arrest. However, suspects convicted and sentenced to probation and jail are re-arrested more often and more quickly compared to others. Also, suspects who are not acquitted at trial, serve probation, and serve jail time were re-arrested more often during the follow-up period.

The relationships involving trial acquittals, the offender program, probation, jail, and split sentences may simply reflect a lack of controls for risk (where offenders who are more likely to recidivate are also more likely to be convicted at trial and receive harsher sentences whereas the "better risks" are more likely to be acquitted or forced to undergo the offender program). However, the relationship involving filed charges is consistent with the idea that "doing something" may be better

than "doing nothing."

Both levels of stake in conformity are significant predictors in the hypothesized directions of the incidence and delay of re-arrest ($p < 0.01$). Moreover, the magnitudes of these effects are comparable between the two levels of analysis. This suggests that re-arrests are fewer and time until re-arrest is longer for suspects who (a) have higher levels of education, (b) live longer at the same residence, (c) are employed, (d) work in skilled occupations, and (e) are financially independent. These results suggest that controlling for these social dimensions of risk may be important in order to obtain valid estimates of the main effects of court dispositions and recidivism likelihoods.

All of the statistical control variables are significantly related to the outcome measures (most at the 0.01 level of statistical significance). These results reinforce the importance of recognizing the possible relevance of these variables in related studies.

The life tables of time to re-arrest for different disposition groups provide an interesting complement to the results for the zero-order relationships involving court dispositions. For this reason, and because the life tables do not control for the other dimensions of risk included in the multivariate analyses, these tables are presented and discussed

first before turning to the findings for the complete models.

Life Tables of Time to Re-arrest for Domestic Violence

Tables 4 and 5 present the cumulative survival rates and hazard rates (respectively) of re-arrest during the follow-up period for each disposition group and the pooled sample. The follow-up period spanned 58 months for some offenders even though everyone re-arrested for domestic violence during the follow-up was re-arrested by month 35. In short, the survival rate for month 35 was the same as for month 58. This is interesting in and of itself because it suggests that three year follow-ups of these offenders may include 100 percent of all "failures."

Table 4a. Survival Analysis of Time to Re-arrest After Sentence Completion: Cumulative Proportions Not Recidivating (Cumulative Survival Rates; Pooled N = 3,662)

		Court Disposition						
Month	Pooled Sample	Ignore	Dismiss	Acquit	Off. Prog.	Proba-tion	Prob. +Jail	Chrgs Pend.
0	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000
1	.965	.983	.985	.979	.986	.918	.944	.836	.902
2	.954	.954	.973	.963	.975	.913	.937	.836	.871
3	.938	.917	.962	.959	.955	.909	.904	.801	.839
4	.928	.892	.950	.947	.951	.908	.891	.801	.812
5	.919	.888	.936	.939	.944	.903	.884	.801	.793
6	.911	.875	.927	.935	.930	.901	.871	.801	.789
7	.906	.863	.921	.931	.927	.899	.868	.801	.785
8	.903	.858	.918	.923	.927	.899	.868	.784	.781
9	.898	.858	.912	.919	.927	.898	.855	.784	.769
10	.896	.854	.909	.919	.923	.896	.852	.784	.765
11	.892	.850	.903	.919	.923	.896	.848	.784	.757
12	.889	.842	.899	.919	.923	.896	.842	.784	.750
13	.886	.829	.894	.919	.923	.896	.838	.784	.738
14	.883	.829	.890	.915	.920	.896	.835	.784	.730
15	.879	.809	.887	.911	.920	.889	.835	.784	.726
16	.876	.800	.884	.911	.920	.888	.828	.784	.722
17	.874	.800	.882	.907	.917	.888	.828	.784	.718
18	.870	.796	.877	.891	.917	.888	.828	.784	.710
19	.868	.792	.874	.887	.913	.888	.828	.784	.707
20	.866	.792	.871	.883	.913	.888	.828	.784	.703
21	.864	.784	.869	.883	.913	.884	.828	.784	.703
22	.862	.780	.867	.879	.913	.884	.825	.775	.703
23	.860	.775	.864	.875	.913	.884	.815	.775	.703
24	.856	.771	.860	.871	.906	.883	.815	.775	.691
25	.851	.763	.854	.867	.899	.883	.812	.775	.683
26	.849	.755	.850	.863	.899	.883	.812	.775	.679
27	.845	.751	.846	.859	.896	.883	.805	.775	.675
28	.843	.746	.843	.859	.896	.883	.802	.775	.671
29	.841	.746	.840	.855	.892	.883	.802	.775	.668
30	.839	.746	.837	.851	.892	.883	.799	.775	.664
31	.837	.738	.835	.851	.892	.883	.799	.775	.664
32	.837	.738	.834	.851	.892	.883	.799	.775	.660
33	.836	.738	.833	.851	.892	.883	.799	.775	.660

34	.836	.738	.833	.851	.892	.883	.799	.775	.660
35	.836	.738	.833	.851	.892	.883	.799	.775	.660

Table 4b. Survival Analysis of Time to Re-arrest After Sentence Completion: Standard Errors of Cumulative Survival Rates

		Court Disposition							
Month	Pooled Sample	Ignore	Off. Dismiss	Proba- Acquit	Prob. Chrgs Prog.	tion	Jail	+Jail	Pend.
0	.000	.000	.000	.000	.000	.000	.000	.000	.000
1	.002	.008	.002	.009	.008	.011	.012	.035	.018
2	.003	.014	.004	.011	.010	.012	.013	.035	.020
3	.003	.019	.004	.011	.014	.012	.017	.038	.022
4	.004	.021	.005	.013	.014	.012	.018	.038	.023
5	.004	.021	.006	.014	.015	.012	.018	.038	.024
6	.004	.022	.006	.015	.017	.012	.019	.038	.024
7	.004	.023	.006	.015	.017	.012	.020	.038	.024
8	.004	.023	.006	.016	.017	.012	.020	.039	.025
9	.004	.023	.007	.017	.017	.012	.020	.039	.025
10	.004	.024	.007	.017	.017	.013	.021	.039	.025
11	.005	.024	.007	.017	.017	.013	.021	.039	.025
12	.005	.024	.007	.017	.017	.013	.021	.039	.026
13	.005	.025	.007	.017	.017	.013	.021	.039	.026
14	.005	.025	.007	.017	.018	.013	.021	.039	.026
15	.005	.026	.007	.018	.018	.013	.021	.039	.027
16	.005	.027	.008	.018	.018	.013	.022	.039	.027
17	.005	.027	.008	.018	.018	.013	.022	.039	.027
18	.005	.027	.008	.020	.018	.013	.022	.039	.027
19	.005	.027	.008	.020	.018	.013	.022	.039	.027
20	.005	.027	.008	.020	.018	.013	.022	.039	.027
21	.005	.028	.008	.020	.018	.013	.022	.039	.027
22	.005	.028	.008	.021	.018	.013	.022	.040	.027
23	.005	.028	.008	.021	.018	.013	.023	.040	.027
24	.005	.028	.008	.021	.019	.013	.023	.040	.028
25	.005	.028	.008	.021	.020	.013	.023	.040	.028
26	.005	.029	.008	.022	.020	.013	.023	.040	.028
27	.005	.029	.008	.022	.020	.013	.023	.040	.028
28	.005	.029	.009	.022	.020	.013	.023	.040	.028

29	.005	.029	.009	.022	.020	.013	.023	.040	.028
30	.005	.029	.009	.022	.020	.013	.023	.040	.028
31	.006	.029	.009	.022	.020	.013	.023	.040	.028
32	.006	.029	.009	.022	.020	.013	.023	.040	.028
33	.006	.029	.009	.022	.020	.013	.023	.040	.028
34	.006	.029	.009	.022	.020	.013	.023	.040	.028
35	.006	.029	.009	.022	.020	.013	.023	.040	.028

Table 5a. Survival Analysis of Time to Re-arrest After Sentence Completion: Proportions Exposed to Risk Recidivating During Specific Month (Hazard Rates; Pooled N = 3,662)

		Court Disposition							
Month	Pooled Sample	Ignore	Dismiss	Acquit	Off. Prog.	Proba-tion	Jail	Prob. Chrgs +Jail	Pend.
0	.000	.000	.000	.000	.000	.000	.000	.000	.000
1	.034	.016	.014	.020	.013	.085	.057	.178	.102
2	.012	.030	.012	.016	.010	.005	.007	.000	.035
3	.016	.039	.011	.004	.021	.003	.035	.042	.036
4	.011	.027	.012	.012	.003	.001	.014	.000	.033
5	.010	.004	.014	.008	.007	.005	.007	.000	.024
6	.008	.014	.010	.004	.014	.001	.015	.000	.004
7	.004	.014	.005	.004	.003	.001	.003	.000	.005
8	.003	.004	.003	.008	.000	.000	.000	.021	.005
9	.005	.000	.006	.004	.000	.001	.015	.000	.015
10	.002	.004	.002	.000	.003	.001	.003	.000	.005
11	.004	.004	.006	.000	.000	.000	.003	.000	.010
12	.003	.009	.004	.000	.000	.000	.007	.000	.010
13	.004	.014	.005	.000	.000	.000	.003	.000	.015
14	.003	.000	.004	.004	.003	.000	.003	.000	.010
15	.004	.025	.003	.004	.000	.007	.000	.000	.005
16	.003	.010	.003	.000	.000	.001	.007	.000	.005
17	.002	.000	.003	.004	.003	.000	.000	.000	.005
18	.004	.005	.005	.017	.000	.000	.000	.000	.010
19	.002	.005	.003	.004	.003	.000	.000	.000	.005
20	.001	.000	.003	.004	.000	.000	.000	.000	.005
21	.002	.010	.002	.000	.000	.003	.000	.000	.000
22	.002	.005	.002	.004	.000	.000	.004	.011	.000
23	.003	.005	.003	.004	.000	.000	.012	.000	.000

24	.004	.005	.005	.004	.007	.001	.000	.000	.016
25	.005	.010	.006	.004	.007	.000	.004	.000	.011
26	.003	.010	.004	.004	.000	.000	.000	.000	.005
27	.003	.005	.004	.004	.003	.000	.008	.000	.005
28	.002	.005	.004	.000	.000	.000	.004	.000	.005
29	.002	.000	.002	.005	.003	.000	.000	.000	.005
30	.002	.000	.004	.005	.000	.000	.004	.000	.005
31	.001	.011	.002	.000	.000	.000	.000	.000	.000
32	.001	.000	.002	.000	.000	.000	.000	.000	.005
33	.000	.000	.000	.000	.000	.000	.000	.000	.000
34	.000	.000	.000	.000	.000	.000	.000	.000	.000
35	.000	.000	.000	.000	.000	.000	.000	.000	.000

Table 5b. Survival Analysis of Time to Re-arrest After Sentence Completion: Standard Errors of Hazard Rates

		Court Disposition						
Month	Pooled Sample	Ignore	Dismiss	Acquit	Off. Prog.	Proba-tion	Prob. Jail	Chrgs +Jail Pend.
0	.000	.000	.000	.000	.000	.000	.000	.000	.000
1	.003	.009	.003	.009	.008	.012	.013	.042	.019
2	.001	.012	.002	.007	.007	.003	.005	.000	.012
3	.002	.014	.002	.000	.010	.002	.012	.022	.012
4	.001	.011	.002	.007	.004	.000	.007	.000	.011
5	.001	.005	.003	.006	.006	.003	.005	.000	.010
6	.001	.008	.002	.004	.007	.002	.008	.000	.004
7	.001	.009	.002	.004	.004	.002	.004	.000	.004
8	.001	.005	.001	.006	.000	.000	.000	.016	.004
9	.001	.000	.002	.004	.000	.002	.008	.000	.008
10	.000	.005	.001	.000	.004	.002	.004	.000	.004
11	.001	.005	.002	.000	.000	.000	.004	.000	.006
12	.001	.007	.001	.000	.000	.000	.005	.000	.007
13	.001	.009	.001	.000	.000	.000	.000	.000	.008
14	.001	.000	.001	.004	.004	.000	.004	.000	.007
15	.001	.012	.001	.004	.000	.004	.000	.000	.005
16	.001	.007	.001	.000	.000	.002	.006	.000	.005
17	.000	.000	.001	.004	.004	.000	.000	.000	.005
18	.001	.005	.002	.009	.000	.000	.000	.000	.007
19	.000	.005	.001	.004	.004	.000	.000	.000	.005
20	.000	.000	.001	.004	.000	.000	.000	.000	.005

21	.000	.008	.001	.000	.000	.002	.000	.000	.000
22	.000	.005	.001	.004	.000	.000	.004	.011	.000
23	.000	.005	.001	.004	.000	.000	.007	.000	.000
24	.001	.005	.001	.004	.006	.002	.000	.000	.009
25	.001	.008	.002	.000	.006	.000	.004	.000	.007
26	.001	.006	.001	.004	.000	.000	.000	.000	.005
27	.001	.000	.001	.000	.004	.000	.006	.000	.005
28	.001	.006	.001	.000	.000	.000	.004	.000	.005
29	.001	.000	.001	.004	.004	.000	.000	.000	.005
30	.001	.000	.001	.004	.000	.000	.004	.000	.005
31	.001	.006	.001	.000	.000	.000	.000	.000	.000
32	.001	.000	.001	.000	.000	.000	.000	.000	.005
33	.000	.000	.001	.000	.000	.000	.000	.000	.000
34	.000	.000	.000	.000	.000	.000	.000	.000	.000
35	.000	.000	.001	.000	.000	.000	.000	.000	.000

Table 4a presents the cumulative survival rates (i.e., cumulative proportions not recidivating throughout the follow-up) for each group, and table 4b displays the corresponding standard errors of those rates. Similarly, tables 5a and 5b present the hazard rates (i.e., proportions at risk recidivating during specific months) and standard errors, respectively. The number of persons falling into each group is presented at the bottom of each table. If a person recidivated before a sentence was served completely, (s)he was included in the percentage of recidivists falling into the first month of the follow-up period. This permits an examination of the incapacitative effects of some dispositions versus others. The discussion that follows stems from careful examination of these rates in conjunction with their standard errors in order to determine significantly different

estimates of recidivism across disposition groups.

The cumulative survival rates presented in the last row of table 4a in conjunction with their standard errors indicate that offenders with pending charges had the highest recidivism rate (the lowest cumulative survival rate) of any group, with 34 percent recidivating by the last time period. This is consistent with the significant zero-order main effect for pending charges discussed previously. The next lowest cumulative survival rate corresponds with the group whose cases were ignored (no charges filed). The survival rate for the group is 74 percent, a figure that is significantly lower than the survival rates for all remaining groups except offenders serving jail and split sentences. This is also consistent with the zero-order relationship involving no filed charges. Finally, the survival rates for offenders undergoing the offender program (89 percent) and probation (88 percent) are significantly lower than for offenders sent to jail (80 percent) and serving split sentences (78 percent). For the groups aside from offenders with pending charges, anywhere from 11 percent (offender program) to 26 percent (no filed charges) recidivated by the end of the follow-up.

Offenders whose charges were pending and those receiving split sentences of probation and jail were the quickest to

recidivate, with 20 percent or more being re-arrested by the end of month 7. The hazard rates in table 5a also indicate much higher risks of recidivism occurring during the early months of follow-up for these groups. Although suspects sentenced to jail alone were typically worse risks compared to most others in the sample, sentences of jail alone served to at least delay recidivism longer through its incapacitative effect. Table 5a reveals a ten percent difference in risk during the first month between those sentenced to jail versus probation and jail.

Suspects whose cases were ignored ended up having some of the highest recidivism likelihoods in the sample, but recidivists from this group actually had longer delays to re-arrest compared to those receiving probation, jail, and split sentences. The hazard rates for this group are significantly higher for later periods (e.g., months 15, 21, 26, and 31) compared to all other groups. It is possible that the factors leading to inaction on the part of prosecutors were related to this delay, but the fact remains that a very high proportion of the group ultimately recidivated for domestic violence (26 percent), and risk was significantly greater in later periods for this group alone.

Similar patterns of delay can be observed for suspects whose cases were dismissed, acquitted, and sentenced to either the offender program or probation. However, unlike those whose cases

were ignored, the offender program and probation groups had among the lowest recidivism likelihoods in the sample. In short, the risk evaluations of these individuals made by prosecutors and judges seemed to be fairly well-informed. Note the significantly higher hazard rates for the more severe disposition groups during the early periods of follow-up. Month by month, these more severe disposition groups have consistently lower cumulative survival rates compared to the "better risks." Note the 11 percent difference in survival rates by the end of month 24 for probationers versus those sentenced to probation and jail. This difference was established after only two years and was maintained throughout the remainder of the follow-up.

Multivariate Findings for Unconditional Effects

Table 6 presents the results for the models predicting whether a suspect was re-arrested for domestic violence during the fixed, two year follow-up period. Table 7 displays the findings for the number of re-arrests for domestic violence during the same period. Table 8 presents the findings for the event history analysis of months to re-arrest for all offenders in the sample followed over varying periods of time. Each table includes three models: Model 1 presents the results for main effects only, model 2 presents both main effects and individual-

level interactions only, and model 3 presents main effects and interaction effects at both levels. All three models are presented so the similarities and differences between coefficients can be compared from one stage of the analysis to the next. A review of the estimates for main effects (in conjunction with their standard errors) reveals that they are relatively stable across the models even with the addition of the interaction terms.

Table 6. Hierarchical Bernoulli Models Predicting Prevalence of Re-arrest for Domestic Violence (Fixed Effects Reported)

	Model 1		Model 2		Model 3	
Predictor	Coeff.	SE	Coeff.	SE	Coeff.	SE
Constant	-1.997		-1.993		-2.020	
Individual-level						
Control Variables						
Male	0.688**	0.121	0.730**	0.172	0.735**	0.170
Age	-0.017**	0.004	-0.022**	0.007	-0.021**	0.007
# priors	0.219**	0.035	0.239**	0.046	0.235**	0.047
Other than dv inc.	0.083	0.069	0.120	0.085	0.123	0.085
No family contact	-0.051	0.139	-0.970*	0.383	-0.929*	0.385
Case Processing						
Charges dropped	-0.285	0.150	-0.290	0.158	-0.172	0.257
Acquitted	-0.311	0.192	-0.385	0.233	-0.271	0.354
Offender program	-0.493*	0.216	-0.592*	0.266	-0.655*	0.365
Probation	0.153	0.250	-0.298	0.355	-0.040	0.419
Jail	-0.044	0.213	-0.185	0.270	0.177	0.359
Probation+jail	0.372	0.278	-0.035	0.406	0.571	0.444
Pending charges	0.800**	0.161	0.842**	0.162	0.921**	0.284

Stake in Conformity	-0.038**	0.013	-0.005	0.046	-0.004	0.047
Interactions						
Stake (x) Dropped	-0.039	0.043	-0.037	0.045		
Stake (x) Acquitted	-0.071	0.074	-0.071	0.074		
Stake (x) Program	-0.144*	0.070	-0.145*	0.071		
Stake (x) Probation	0.141	0.093	0.136	0.093		
Stake (x) Jail	0.044	0.068	0.036	0.068		
Stake (x) Prob.+jail	-0.240*	0.118	-0.256*	0.133		
Stake (x) Pending	0.017	0.054	0.018	0.054		
Aggregate-level						
Stake in Conformity	-0.031**	0.007	-0.027**	0.008	-0.028**	0.008
Interactions						
Stake (x) Dropped			0.026	0.024		
Stake (x) Acquitted			0.031	0.036		
Stake (x) Program			0.041	0.042		
Stake (x) Probation			0.042	0.049		
Stake (x) Jail			0.080*	0.038		
Stake (x) Prob.+jail			0.145**	0.051		
Stake (x) Pending			0.014	0.026		
% between-group variance explained	51.592		64.648		82.313	

* $p < 0.05$; ** $p < 0.01$

Table 7. Hierarchical Poisson Models Predicting Incidence of Re-arrest for Domestic Violence (Fixed Effects Reported)

	Model 1		Model 2		Model 3	
Predictor	Coeff.	SE	Coeff.	SE	Coeff.	SE
Constant	-1.582		-1.513		-1.928	
Individual-level						
Control Variables						
Male	0.527**	0.161	0.513**	0.179	0.505**	0.172
Age	-0.025**	0.004	-0.022**	0.006	-0.019**	0.006
# priors	0.229**	0.023	0.221**	0.025	0.226**	0.027

Other than dv inc.	0.169* 0.076	0.107 0.072	0.114 0.074
No family contact	-0.909** 0.283	-1.051** 0.359	-1.039** 0.371
Case Processing			
Charges dropped	-0.190 0.121	-0.257 0.138	0.057 0.203
Acquitted	-0.255 0.181	-0.418 0.211	-0.161 0.281
Offender program	-0.506** 0.188	-0.654** 0.223	-0.611* 0.294
Probation	0.289 0.224	0.162 0.219	0.238 0.361
Jail	-0.107 0.182	-0.166 0.229	0.330 0.283
Probation+jail	0.364 0.211	0.166 0.362	0.630 0.346
Pending charges	0.888** 0.143	0.739** 0.126	0.880** 0.202
Stake in Conformity	-0.024* 0.012	0.020 0.037	0.024 0.039
Interactions			
Stake (x) Dropped		-0.047 0.037	-0.048 0.040
Stake (x) Acquitted		-0.076 0.065	-0.084 0.064
Stake (x) Program		-0.150** 0.055	-0.155** 0.056
Stake (x) Probation		0.071 0.042	0.111 0.047
Stake (x) Jail		-0.021 0.064	-0.034 0.061
Stake (x) Prob.+jail		-0.151* 0.075	-0.153* 0.073
Stake (x) Pending		-0.028 0.050	-0.022 0.048
Aggregate-level			
Stake in Conformity	-0.025** 0.007	-0.025** 0.007	-0.071** 0.020
Interactions			
Stake (x) Dropped			0.043* 0.018
Stake (x) Acquitted			0.050 0.026
Stake (x) Program			0.050 0.031
Stake (x) Probation			0.027 0.040
Stake (x) Jail			0.085** 0.032
Stake (x) Prob.+jail			0.125** 0.039
Stake (x) Pending			0.023 0.018
% between-group variance explained	73.789	84.156	94.702

* $p < 0.05$; ** $p < 0.01$

Table 8. Pooled Survival Models Predicting Months to Re-arrest for Domestic Violence (Cox Regression Coefficients Reported)

	Model 1		Model 2		Model 3	
Predictor	Coeff.	SE	Coeff.	SE	Coeff.	SE
Individual-level						
Control Variables						
Male	0.63**	0.17	0.64**	0.17	0.65**	0.17
Age	-0.02**	0.01	-0.02**	0.01	-0.02**	0.01
# priors	0.18**	0.03	0.19**	0.03	0.18**	0.03
Other than dv inc.	0.16	0.09	0.14	0.10	0.15	0.10
No family contact	-0.79*	0.36	-0.77*	0.36	-0.72*	0.36
Case Processing						
Charges dropped	-0.24	0.15	-0.25	0.15	-0.12	0.22
Acquitted	-0.37	0.22	-0.37	0.22	-0.32	0.31
Offender program	-0.44*	0.22	-0.45*	0.22	-0.29	0.29
Probation	-0.26	0.27	-0.23	0.28	-0.05	0.38
Jail	-0.16	0.19	-0.14	0.20	0.09	0.28
Probation+jail	0.34	0.27	-0.08	0.38	0.40	0.42
Pending charges	0.69**	0.13	0.69**	0.13	0.82**	0.18
Stake in Conformity	-0.04*	0.02	-0.03	0.05	-0.03	0.05
Interactions						
Stake (x) Dropped			-0.01	0.05	-0.01	0.06
Stake (x) Acquitted			-0.03	0.08	-0.02	0.08
Stake (x) Program			-0.07	0.08	-0.07	0.08
Stake (x) Probation			0.12	0.09	0.12	0.09
Stake (x) Jail			0.05	0.07	0.05	0.08
Stake (x) Prob.+jail			-0.21*	0.10	-0.23*	0.11
Stake (x) Pending			0.03	0.05	0.02	0.05
Aggregate-level						
Stake in Conformity	-0.01	0.01	-0.01	0.01	-0.04	0.02
Interactions						
Stake (x) Dropped					0.02	0.03
Stake (x) Acquitted					0.001	0.04

Stake (x) Program		0.03	0.04
Stake (x) Probation		0.03	0.05
Stake (x) Jail		0.04	0.03
Stake (x) Prob.+jail		0.13*	0.05
Stake (x) Pending		0.02	0.02
Model Chi-square	179.956 (14 df)	195.600 (21 df)	206.274 (28 df)

* $p < 0.05$; ** $p < 0.01$

Some observations related to the control variables are worth making before turning to the findings for court dispositions. All of the statistical control variables except prior incarceration for other crimes are significantly related to the two outcome measures in at least eight of the nine models. In conjunction with the relationships involving the composite measures of stake in conformity, the results reinforce the notion that controlling for these types of variables is important for obtaining valid estimates of relationships between court dispositions and recidivism.

Aside from a suspect's sex and age, the number of prior convictions for violent misdemeanors yields one of the strongest relationships in the model. A separate analysis revealed that this control variable alone renders a non-significant relationship between the prevalence of re-arrest and whether a suspect had ever been incarcerated previously for offenses other than domestic violence. This suggests that a history of less

serious criminal behavior may be a better predictor of re-arrest for domestic violence than a history of more serious criminal behavior. This was also suggested in a separate analysis of the bivariate relationships involving the different measures of prior record, where the relationships involving misdemeanors are stronger than those for felonies.

Re-arrests were also less likely, less frequent, and were delayed longer for persons not living with their original victim at the time of the original arrest. From a substantive perspective, this is consistent with the earlier contention that marital status and living arrangements should be treated separately from the concept of stake in conformity in the context of domestic violence. Given the unique offense examined, persons living with their partners have greater opportunities to engage in domestic violence due to their physical proximity. This may have implications for the "escalation" effects for married offenders found in some of the arrest studies (summarized by Garner, Fagan, and Maxwell 1995).

Regarding the main effects of court dispositions on the outcome measures, recall that the dummy disposition variable left out of the model is the measure of no filed charges. The results for the remaining disposition variables in each model, aside from pending charges, therefore reflect differences from "doing

nothing" with arrested suspects.

Results for the unconditional effects of the legal variables reveal that only one of the six disposition variables maintains a statistically significant difference from "doing nothing" for two of the outcome measures. That is, the prevalence and incidence of re-arrest are lower among persons who went through the offender program. Recall that the cumulative failure rate for persons with no charges filed was 24 percent versus only 11 percent for offenders who went through the program. Yet aside from this significant difference in the likelihood of re-arrest, even after controlling for risk, no other disposition appears to be any better than not filing charges for significantly reducing re-arrests. Moreover, comparisons of differences between the coefficient estimates (in conjunction with their standard errors) for the dummy disposition measures included in the models reveal no other significant differences for either outcome measure. The majority of main effects for the dispositions are therefore consistent with five of the seven arrest studies where the main effect of arrest was not statistically significant for predicting the prevalence and/or incidence of re-arrest (Garner, Fagan, and Maxwell 1995). While doing something may be better than doing nothing with these offenders, the gains of the offender program over other (more restrictive) dispositions appear to be modest.

This set of findings provide no support for the additive deterrence hypothesis. Although the offender program might have an effect on recidivism, this particular disposition should not be treated as a punishment intended to "deter" future offending. Rather, it is the only disposition examined that falls under a rehabilitative ideology. The program is intended to prevent future violence through counseling, not punishment.

In contrast to the dummy variables tapping case dispositions, the measure of pending charges is a statistically significant (and relatively strong) predictor of all three dependent variables ($p < 0.01$). Given that this variable is statistically significant even when controlling for prior record of violent misdemeanors and prior incarceration, this reinforces the earlier contention that the speed of case processing may be an important factor in reducing domestic violence recidivism. This is also consistent with the finding from the National Pre-trial Reporting Program (1992) that persons released after arrest for other types of crimes were significantly more likely to be re-arrested while waiting for trial compared to those whose cases were processed more expediently (Walker 1998). In short, faster processing may help to reduce the likelihood and frequency of re-arrest. Whether this is because the celerity of case processing contributes to a deterrent effect or because faster processing

reduces opportunities for victimization by separating offenders from their victims more quickly cannot be answered with the available data.

Results for the main effects of the stake in conformity measures presented in the first models of tables 6 and 7 indicate that both factors are significant predictors (in the predicted directions) of both outcome measures ($p < 0.05$). The magnitude of each estimate is also virtually identical to the other in model 1. However, the addition of the interactions involving stake in conformity at the first level serve to render non-significant main effects for the individual measure on both dependent variables, whereas the addition of the interactions at the second level does not have the same effect on the aggregate measure in either model. These results suggest that neighborhood levels of stake in conformity maintain significant unconditional effects on domestic violence recidivism whereas offender levels of stake do not. This observation supports the relevance of ecological theories, particularly a contextual theory of stake in conformity, for an understanding of repeat offending among adults. The applicability of an individual-level theory of stake in conformity is more limited in the context of main effects on recidivism only.

The results for aggregate-level stake in conformity

presented in table 8 are quite different from those in tables 6 and 7. However, recall that the pooled survival analysis may not provide valid estimates of the aggregate-level relationships examined. Yet the results for offender stake are consistent with those in the other two tables, suggesting that the main effect of offender stake is not important for predicting domestic violence recidivism.

The significance of the aggregate-level measure in tables 6 and 7 is consistent with studies of other types of predatory crimes revealing higher violent crime rates as well as higher individual-level likelihoods of violent crime in lower socio-economic neighborhoods (e.g., Elliott et al. 1996; Gottfredson, McNeil, and Gottfredon 1991; Gunn et al. 1993; Sampson 1991; Simcha-Fagan and Schwartz 1986). The findings for model 3 in both tables are also consistent with the criminological literature on the relative importance of aggregate- versus individual-level predictors of criminal behavior. When including both levels of predictors in the same model with the interactions, the aggregate-level measure of stake in conformity prevails in significance and magnitude whereas the individual-level measure becomes weaker (in table 7) and non-significant (in tables 6 and 7).

A qualification to the contextual main effects is that the causality from neighborhood characteristics to domestic violence

cannot be tested directly. This leaves open the possibility that persons more likely to engage in domestic violence may possess characteristics that lead them to reside in areas with particular characteristics. For example, Sampson, Raudenbush, and Earls (1997) observed that single persons and African-Americans may gravitate towards neighborhoods with lower socioeconomic status, contributing to higher concentrations of poverty at the aggregate-level. Nonetheless, these aggregate-level relationships provide incentive for research that can test the causal relationships directly.

As previously discussed, several aspects of the specific dimensions of aggregate-level stake considered here overlap conceptually with various aspects of social capital and concentrated disadvantage applied to an understanding of crime. While it is up to future research to delineate finer operational distinctions between the three sets of theoretical concepts, the significant contextual results provide incentive to do so.

Individual-level Conditional Effects

Results for the prevalence and incidence of re-arrest reveal significant interactions involving individual-level stake in conformity with the offender program and split sentences. The same is true for split sentences only in the analysis of time to

re-arrest. Persons with higher stake in conformity who undergo the offender program are significantly less likely to be re-arrested and were re-arrested less often compared to higher-stake persons whose cases are ignored. (This represents an effect beyond the significant main effect for all offenders in the sample). The same holds true for higher-stake persons serving split sentences in the analyses of all three outcome measures.

Comparing the interaction for offender program with those for the other individual-level interactions, the program corresponds with a significantly lower incidence of re-arrest for higher-stake persons when compared with higher-stake individuals serving probation only. On the other hand, the coefficient for split sentences in each of the three tables is not significantly different from any other interaction.

Further analysis reveals that higher-stake persons serving split sentences have a lower prevalence of re-arrest (compared to doing nothing with these types of offenders), whereas lower stake persons serving these sentences are neither more or less likely to be re-arrested. The finding of "no effect" for lower-stake individuals does not contradict the negative coefficient for the interaction given that a non-linear analysis was performed.

The findings for the individual-level interactions support the hypothesis that formal controls may be more effective for

individuals with higher stake in conformity. However, the observations offered in the arrest literature that formal sanctions may actually make matters worse for lower-stake offenders is not supported with the analysis of court dispositions. Only the results for higher-stake individuals are consistent with the previous arrest studies which included interactions between arrest and measures of stake in conformity. Garner, Fagan, and Maxwell (1995) observed that when there was a significant interaction between stake in conformity and arrest, with the exception of marital status, the results generally favored higher-stake individuals in that arrest appeared to have a greater suppression effect on re-arrest for this group. Researchers who found this significant link observed that arrest might have a conditional effect for higher-stake persons. Similarly, the offender program and split sentences may be more effective for persons with higher (individual) stake in conformity.

Aggregate-level Conditional Effects

For the discussion of the aggregate-level interactions involving court dispositions and stake in conformity, only the findings for the prevalence and incidence of re-arrest are interpreted due to the methodological reasons aforementioned.

The interactions involving the aggregate-level measure of stake in conformity with jail and split sentences are statistically significant predictors of both outcome measures. However, unlike the individual-level interactions, the coefficients are opposite in sign to those hypothesized. Jail and split sentences correspond with higher likelihoods of re-arrest as well as more re-arrests for persons living in census tracts with larger proportions of higher-stake residents (compared to the same types of individuals who have no charges filed against them). Also, prosecuting suspects and subsequently dropping charges corresponds with a higher incidence of re-arrest for suspects residing in "higher-stake" areas compared to not filing any charges against these types of individuals.

The finding for split sentences is particularly interesting in light of the significant negative relationship involving the same interaction measured at the individual-level. Further analysis revealed that split sentences have no appreciable effect on re-arrests for persons in lower-stake census tracts, yet a significant positive effect for those residing in higher-stake tracts. For jail sentences, the additional analysis revealed that offenders residing in higher-stake census tracts who serve jail sentences are re-arrested significantly more often than those who have no charges filed against them, whereas offenders in

lower-stake tracts serving these sentences are re-arrested significantly less often. Similar relationships exist for prosecution with subsequently dropped charges.

Sherman and Smith (1992) found support for a labeling (escalation) effect of arrest on re-offending for persons with lower individual levels of stake in conformity. However, the results presented here for the conditioned effects of court dispositions by both individual and aggregate levels of stake in conformity refute the applicability of a labeling (escalation) effect for lower-stake offenders when formal intervention occurs and sanctions are applied. On the contrary, prosecution and jail may actually have a suppression effect on arrest for individuals residing in lower-stake tracts. This provides some support for the replacement hypothesis applied to specific deterrence. That is, prosecution and jail sentences only appear to work effectively for offenders in low stake communities.

The findings presented here suggest that both individual-level and contextual theories of stake in conformity are applicable to an understanding of domestic violence recidivism. However, these applications are quite different from each other. The absence of significant main effects for individual-level stake in conformity on the prevalence and incidence of domestic violence recidivism suggest that the applicability of the

individual-level theory is limited to the role of offender stake in conditioning the effects of legal sanctions (in the direction of greater effectiveness for offenders with greater stake). On the other hand, the significant main effects of aggregate-level stake in conformity on recidivism in conjunction with conditioned effects that favor offenders in lower stake neighborhoods while generating escalation effects for those in higher stake neighborhoods suggest a broader yet more complicated application of the contextual theory. Although it would be inappropriate to provide substantive meaning to ex-post facto hypotheses related to the contextual findings for conditioned effects, such hypothesizing is useful in the context of specific directions for future research.

Collective Efficacy and Calls to the Police

Given the non-experimental nature of the data for the present study, it is logical to expect that higher risk offenders are more likely to receive the more severe dispositions including jail and split sentences, the sanctions that correspond with higher recidivism for offenders in higher stake neighborhoods. As noted previously, some aspects of the aggregate-level measure of neighborhood stake in conformity overlap with the concept of concentrated disadvantage introduced by Sampson, Raudensbush, and

Earls (1997). Sampson et al. found that less disadvantaged neighborhoods maintain higher levels of collective efficacy, or the willingness of individuals to help other residents of their neighborhoods. If offenders receiving more severe sanctions are more likely to recidivate, it is possible that neighbors in higher stake communities are more likely to get involved in these disputes (including calling the police) if these communities maintain higher levels of collective efficacy. Surveys of offenders asking who notified the police about their particular incidents would provide more insight into this possibility.

Labeling Effects in Higher Stake Communities

The results for the significant aggregate-level interactions also do not refute the possibility of a labeling effect of more severe sanctions for offenders living in higher-stake tracts, especially since no such effect appears for offenders in lower-stake tracts. The literature by Sherman, Berk, and their colleagues focuses heavily on the possible role of labeling theory in some of the individual-level conditioned effects they found related to arrest for lower stake offenders. That is, lower stake offenders who were arrested also recidivated more than those not arrested. However, a provocative comment from Manski and Nagin (1998) provides a possible explanation for why some of

the court sanctions examined here might correspond with more recidivism among offenders in higher-stake communities. Discussing the applicability of labeling theory to an understanding of juvenile court sanctions and the risk of re-offending, they noted how labeling theory predicts that any effects of stigmatization for juveniles tend to occur during the first few encounters with police and juvenile court, after which a deterrent effect may actually occur with additional interactions. A similar situation may exist with offenders in higher-stake areas who have less experience with the police and the criminal court. Such offenders who are arrested and sanctioned by the court may initially undergo a labeling process and, possibly, a short-term escalation in their criminal behavior. Offenders residing in lower-stake areas, on the other hand, may not be as susceptible to the labeling process due to their lack of relative deprivation in social status before and after formal intervention, and because they may have more experience with the system.

Pursuing this idea in future research would require survey data on offenders' perceptions of formal treatment. This ties in with Sherman's (1992) observation that offenders' perceptions of justice may influence the effects of formal sanctions on future behavior. Those perceiving a sanction as fair are more likely to

believe in the legitimacy of the law and may be less likely to break it (Paternoster et al. 1997; Tyler 1990). However, perceptions of fairness may be influenced by stake in conformity (Scheff and Retzinger 1991), but not necessarily in the direction of favoring offenders in higher-stake communities. Perceptions of injustice may be enhanced among offenders residing in higher-stake communities who receive more punitive dispositions if these individuals are more resentful of formal intervention (due to more privileged lifestyles compared to those living in lower-stake communities). This could ultimately damage their respect for the system and initially increase the likelihood of re-offending.

Implications for Current Knowledge and Future Research

Despite the focus on contradictions with previous research findings related to arrest and stake in conformity, the present study of court dispositions actually yields several consistencies. First, just as the main effects of arrest were not statistically significant in five of the seven arrest studies, most of the main effects of court dispositions were not significant as well. Second, the significant individual-level interactions tapping offender program and split sentences support previous findings related to formal and informal social controls.

Finally, the findings for the interactions suggest that formal and informal controls should be considered together when understanding domestic violence recidivism.

The contradictions with previous findings include higher likelihoods and rates of re-arrest for offenders in particular disposition groups with higher (aggregate-level) stake in conformity, the lack of an escalation effect for lower-stake offenders serving the same sentences that appear to "work" for higher-stake offenders at the individual-level, and a suppression effect for offenders from lower-stake neighborhoods who are subjected to particular dispositions. Aside from differences in research design, these contradictions could have resulted from differences between arrest and the formal sanctions examined here. Also, in contrast to the arrest studies, the focus here is on a population of persons already arrested. Finally, previous research did not include the contextual analyses which generated most of the inconsistencies found here.

The present study also provides a couple of new insights related to the main effects of formal interventions on domestic violence recidivism. First, in contrast to the Minneapolis and Dade County findings supporting the additive (specific) deterrence hypothesis related to arrest, this study finds support for a treatment effect of an offender program on recidivism.

Second, the finding that suspects were more likely to recidivate for domestic violence if charges from previous arrests were still pending also suggests that the speed of case processing may be important for reducing the likelihood of domestic violence recidivism. Perhaps a pre-occupation with the quality and length of sentences has taken attention away from the importance of efficiency in case processing.

The significant main effect of living arrangements is also important since the idea of "opportunity" is central to many criminological theories. In the context of understanding domestic violence recidivism, an offender's living arrangements may not adequately reflect his or her attachment to immediate family members. Nonetheless, this operational concept applied to domestic violence offenders may tap into an interaction between impulsivity and opportunity that is more reflective of Gottfredson and Hirschi's concept of low self-control (1990). Of course, further research is needed to test this idea.

The dramatic differences in how individual- and aggregate-level theories of stake in conformity apply to domestic violence recidivism suggest that each should be considered to provide a more comprehensive understanding of the topic. These differences also indicate that findings at one level cannot be inferred to possible results at the other level. The main effects of offender

stake are non-significant while support is found for the conditioned effects of legal sanctions by offender stake in the predicted directions. By contrast, the main effects of neighborhood stake are significant in the predicted directions although some of the conditioned effects are significant in the opposite hypothesized directions. Moreover, at the individual-level, the conditioned effects neither decrease or increase recidivism for lower stake offenders whereas some of the conditional effects at the aggregate-level actually appear to benefit offenders in lower stake neighborhoods. This provides an interesting (although somewhat complicated) picture of the role of neighborhood stake in conformity in predicting recidivism. Future research examining the relevance of collective efficacy and labeling may provide additional insight into the contextual findings presented here. Support for either of these theories would further extend the application of sociological theory to the topic.

The predominant focus on conditional effects is not intended to trivialize the significant main effect found for aggregate-level stake in conformity. This finding extends previous criminological research yielding support for the main effects of neighborhood characteristics on crime in general to an understanding of recidivism among a fairly large population of

offenders. The significance of the aggregate dimension of stake in conformity is also consistent with studies of other predatory crimes revealing higher violent crime rates as well as higher individual-level likelihoods of violent crime in more socially "disorganized" neighborhoods. A map of the geographic distribution of persons arrested for domestic violence in Cincinnati (see figure 1) reveals a preponderance of arrests closer to the central business district and areas closer to the city's major thoroughfares. This is consistent with the idea that poorer areas with more transient populations coincide with higher rates of predatory crime.

The methodological contributions of the the present study include more rigorous measures of stake in conformity compared to those offered previously, and a contextual analysis of the conditioned effects of court dispositions by aggregate measures of stake in conformity. Despite the limitations of non-random assignments to the disposition groups examined, cross-validation checks and statistical controls for risk and opportunity served to enhance the validity of the findings. At a minimum, this study provides a methodology that can be replicated in other jurisdictions in addition to results that can be used as forecasts in future research.

The role of collective efficacy in higher stake

neighborhoods, labeling effects of formal intervention, and perceptions of justice are all worthwhile pursuits in future studies of the topic. At a minimum, the results presented here provide a forecast for future research and a context in which these hypotheses can be tested.

Conclusions and Policy Implications

The findings from the study presented here reinforce the importance of filing charges against suspects in order to delay re-arrest, even if the cases ultimately result in subsequently dropped charges or trial acquittals. Also, offender programs appear to have a suppression effect on the prevalence and incidence of re-arrest regardless of individual- or aggregate-levels of stake in conformity. Finally, persons arrested while old charges are still pending are significantly more likely to be re-arrested, are re-arrested more often, and are re-arrested faster. These results suggest that a failure to process cases with greater speed may also increase recidivism.

The findings for conditional effects of court dispositions suggest that certain dispositions may contribute to the prevention of further domestic violence among certain types of offenders arrested for misdemeanor domestic violence. Specifically, offender programs appear to work even better

(beyond the significant main effects) for reducing the prevalence and incidence of re-arrest among offenders with higher individual-levels of stake in conformity. Split sentences also work for these types of offenders to reduce the prevalence and incidence of re-arrest as well as for delaying re-arrest.

Considering aggregate-level conditional effects of court dispositions, prosecuting suspects from lower-stake neighborhoods as well as sending convicted offenders from these areas to jail appear to have suppression effects on re-arrest. However, jail and split sentences also appear to escalate recidivism for offenders from higher-stake neighborhoods. This last finding could be the result of a greater willingness of residents in these neighborhoods to call the police regarding domestic disputes, or possible labeling effects occurring among offenders less familiar with legal intervention.

The analysis of time to re-arrest reveals that, regardless of the follow-up period examined, sentences of jail alone have a greater incapacitative effect compared to probation alone and probation combined with jail. In other words, even though suspects who go to jail are typically worse risks compared to those receiving probation, sentences of jail serve to at least delay recidivism longer through its incapacitative effect.

Although suspects whose cases are ignored end up with some

of the highest recidivism likelihoods in the sample overall, recidivists falling in this group actually have longer delays to re-arrest compared to other disposition groups (offender program, probation, jail, and probation combined with jail). While it is possible that the factors leading to inaction on the part of prosecutors are related to this delay, the fact remains that a very high proportion of the group ultimately recidivate for domestic violence.

Similar patterns of delay can be observed for suspects whose cases are dismissed, acquitted, and who undergo the offender program. However, unlike those whose cases are ignored, these suspects have among the lowest recidivism likelihoods in the sample. In short, the risk evaluations of these individuals made by prosecutors and judges seem to be fairly well-informed.

The results presented here indicate that sentencing policies designed to reduce domestic violence recidivism need not place unrealistic demands on probation officers and jail resources. For example, the significance of the main and conditional effects of the offender program over other (more severe) dispositions suggest that many offenders can be successfully dealt with in a relatively expedient fashion. Even the results for a suspect's living arrangements at the time of arrest underscore the importance of restraining orders for reducing opportunities for

violence.

Another recommendation follows from the results for the contextual analysis. The idea that the types of sentences for higher-risk offenders do not matter is not supported here. Pursuing charges against these individuals in addition to sentences of jail appear to be effective for reducing recidivism among these offenders. On the other hand, reducing violence among offenders living in higher-stake neighborhoods needs more careful consideration. More punitive dispositions may not be successful for these individuals. In some cases, more severe sentences may escalate the problem. This does not mean that such offenders should necessarily be treated more leniently, but it does underscore the need for additional research on the lifestyle characteristics of these offenders that may mediate the effects of legal controls. For example, the lower divorce rates in higher socio-economic neighborhoods could lead to greater opportunities for these offenders to repeat their assaults. Perhaps a greater use of restraining orders and victim counseling would be effective, but more research is needed on what works for these types of offenders.

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