The Crime Control Effects of Prosecuting Intimate Partner Violence in Hamilton County, Ohio:

Reproducing and Extending the Analyses of Wooldredge and Thistlethwaite

Executive Summary

Joel Garner, Principal Investigator
Joint Centers for Justice Studies, Inc.

Christopher Maxwell, Co-Principal Investigator
University of Michigan and
Michigan State University

January 2008

This project was supported by Award # 2006-IJ-CX-0005 from the National Institute of Justice, Office of Justice Program, U. S. Department of Justice. Points of view in this report are those of the authors and do not necessarily reflect the official position of the Joint Centers, Michigan State University, the University of Michigan, the National Institute of Justice, the Office of Justice Programs or the U.S. Department of Justice. Additional financial assistance for this research was provided by an award from the Harry Frank Guggenheim Foundation.

The Crime Control Effects of Prosecuting Intimate Partner Violence in Hamilton County, Ohio:

Reproducing and Extending the Analyses of Wooldredge and Thistlethwaite

Executive Summary

This research seeks to enhance our understanding of the effects of prosecution, conviction and sentence severity on subsequent offending against intimate partners. Our approach is to conduct in-depth analyses of the data (Wooldredge, 2000) used in four recent publications conducted by John Wooldredge and Amy Thistlethwaite (Wooldredge and Thistlethwaite, 1999; 2002; 2005; Wooldredge, 2002). These publications were selected because of the breath, depth, and quality of their design, implementation, and analyses, the likely impact of their reported findings on the scientific knowledge and future policy making, and the availability of the data for re-analysis. Our secondary analyses of these data are designed to more clearly explicate the specific nature of the published analyses and to identify the extent to which the published analyses can be reproduced from the available data.

This research goes beyond reproducing Wooldredge and Thistlethwaite's published analyses and develops alternative tests of the relationship between prosecution, conviction, and sentence severity and the pattern of repeat violence among intimate partners.

This report is organized into three parts. Part 1 reviews the prior research on the amount of prosecution and conviction for intimate partner violence and the reported effectiveness of these criminal sanctions on repeat offending. The primary focus of this report is the contribution of four publications by Wooldredge and Thistlethwaite on our understanding of the effectiveness of criminal sanctions in reducing repeat offending in Hamilton County, Ohio.

The second part of this report uses the publicly archived data produced by Wooldredge and Thistlethwaite to determine the extent to which their multivariate and multi-level analyses of the crime control effects of criminal sanctions can be reproduced by independent analysts. Using three explicit criteria for reproducibility, we determine that, while the vast majority of their findings can be reproduced, we could not confirm their findings about the lack of a crime control effect for offenders sentenced to probation–our analyses show probation to be consistently associated with reduced repeat offending.

In Part 3, we extend the analyses of Wooldredge and Thistlethwaite. Using their archived data, we conducted new analyses which identify a crime control effect associated with the filing of charges, with obtaining a conviction and with a sentence to probation. Among all 3,662 arrestees, a jail sentence has no effect on repeat offending; among only convicted offenders, a jail sentence is associated with increased repeat offending.

Findings About Prior Research on Intimate Partner Violence

The Amount of Prosecution

* A detailed review of the prior research reveals great variability in the rate at which intimate partner violence results in a criminal prosecution and in a conviction.

* Based on data from 52 English language reports, we determine that, on average, more than one third of the offenses reported to the police and more than 60% of all arrests for intimate partner violence resulted in the filing of criminal charges.

* Based on data from 94 English language reports, we determine that, on average, more than half of all prosecutions of intimate partner violence results in a conviction on one or more charges.

The Crime Control Effects of Prosecution, Conviction and Sentence Severity

* In thirty prior studies of the effects of intimate partner violence prosecution, conviction, and sentence severity, the predominate finding reported is no effect on repeat offending.

* Among those studies reporting findings that do show a statistically significant effect for prosecuted cases, twice as many show a reduction than show an increase in re-offending. For conviction and sentence severity, the statistically significant findings are about equally common between showing an increase or a decrease in repeat offending..

Wooldredge and Thistlethwaite's Findings

In four publications reporting a series of rigorous multivariate and multi-level analyses based on large but varying samples, multivariate and multi-level statistical models, and measures of repeat offending, Wooldredge and Thistlethwaite report:

* There are consistent crime increasing effects associated with not prosecuting intimate partner violence.

* There are inconsistent crime control effects for cases where the offender is convicted or sentenced to a treatment program, probation or jail sanctions.

* There are no consistent direct crime control effects for numerous measures of an individual's stakes in conformity and the social context in which the offender resides.

* There are no consistent relationships between repeat offending the interaction between sanction types and an individual's stakes in conformity or the social context in which they reside.

Findings About the Reproducibility of Wooldredge and Thistlethwaite

Data Completeness and Documentation

* The archived data are generally well documented but incomplete.

* Data on sentences to fines and to a short treatment program are missing. Data on census tract numbers and on two of fourteen census based demographic variables are also missing in the archived data; we are able to reconstructed the missing census tract numbers and census data and construct tests of the multivariate and multi-level models.

* Only summary information on repeat offending was archived; dates of criminal justice processing events or the dates of new arrests were not provided. This was sufficient to reproduce the published findings but it preclude additional analyses of the role of new arrests that might occur before the completion of an offender's sentence.

Reproducing Descriptive Statistics

* We reproduce most of the reported frequency counts and variable means exactly.

* In a small number of instances, the descriptive statistics in the published reports includes errors generated by the authors or as part of the publication process.

Criteria for Reproducing Findings Reported in Multivariate and Multi-Level Analyses

We applied three criteria for testing whether published findings have been reproduced:

1) whether the differences between the reported and reproduced coefficients were within less than .1,

2) whether the reported and reproduced findings were in the same direction and satisfied the same criteria for statistical significance, and

3) whether the differences between the reported and reproduced effects exceeded a test of statistical significance.

* We interpret success at reproduction as confirming the published findings.

* We interpret lack of success at reproduction as not confirming the published or the reproduced findings. Lack of success at reproduction is co-produced by the nature of the originally reported analyses, the quality of the archived data and data documentation and the abilities of the secondary analysts.

Differences in Reported and Reproduced Multivariate and Multi-Level Analyses

The Effects of Criminal Sanctions

* In 78 of 86 tests (81.4%) the reproduced findings are within .1 of the reported findings.

* In 10 of 43 tests (72.1%) the reproduced findings have the same direction and statistical significance as the reported findings.

* In 39 of 43 tests (93.0%) the differences between the reproduced and reported findings are not statistically significant.

* The primary substantive difference is that the reproduced findings provide more support for the crime control effects of probation than the reported findings.

The Effects of Stakes in Conformity

* In 11 of 12 tests (91.7%) the reproduced findings are within .1 of the reported findings.

* In 5 of 6 tests (83.3%) the reproduced findings have the same direction and statistical significance as the reported findings.

* In 6 of 6 tests (100%) the differences between the reproduced and reported findings are not statistically significant.

* The primary substantive difference is that the reproduced findings provide more support for economic factors and less support for a high school degree.

The Interaction of Sanction and Stakes in Conformity

* In 36 of 54 tests (83.3%) the reproduced findings are within .1 of the reported findings.

* In 18 of 27 tests (66.7%) the reproduced findings have the same direction and statistical significance as the reported findings.

* In 27 of 27 tests (100%) the differences between the reproduced and reported findings are not statistically significant.

* The primary substantive difference is that the reproduced findings provide less support for the interaction of between sanctions and stakes than the reported findings.

The Effects of Social Context

* In 27 of 36 (75.5%) the reproduced findings are within .1 of the reported findings.

* In 15 of 18 tests (66.7%) the reproduced findings have the same direction and statistical significance as the reported findings.

* In 18 of 18 tests (100%) the differences between the reproduced and reported findings are not statistically significant.

* The primary differences are that the reproduced findings provide less support for the effects of social context than the reported findings.

The Interaction of Sanctions and Context

* In 39 of 48 (81.3%) the reproduced findings are within .1 of the reported findings.

* In 13 of 24 tests (66.7%) the reproduced findings have the same direction and statistical significance as the reported findings.

* In 24 of 24 tests (100%) the differences between the reproduced and reported findings are not statistically significant.

* The primary differences are that the reproduced findings provide less support for the interaction of sanctions and social context than the reported findings.

Findings from the Re-Analysis and Extension of Wooldredge and Thistlethwaite

The Effects of Sanctions and Stakes in Conformity

In a multivariate model of the time to first re-arrests:

* The prosecution of arrestees is associated with less repeat offending.

* The conviction of arrestees is associated with less repeat offending.

* The sentencing of arrestees to probation is associated with less repeat offending.

* The sentencing of arrestees to a treatment program is not associated with less repeat offending.

* The sentencing of arrestees to jail is not associated with less repeat offending.

* Among convicted offenders, being sentenced to jail is associated with more repeat offending; this effect persists when controls for the propensity to jailed are included.

* Employment of the offender is consistently associated with less repeat offending; however, the effectiveness of prosecution, conviction, or sanction severity did not vary by whether the offender was employed or not.

* Marriage has no direct effect on repeat offending; however, the effectiveness of the conviction sanction was enhanced if the offender was married. No other sanction type was affected by the marital status of the offender.

Implications for Policy

* Our findings demonstrate the importance of verifying the published results of criminological research. Although our analyses confirm most of the findings published by Wooldredge and Thistlethwaite, for a sizeable and important minority of findings, we did not confirm their results.

* Our analyses suggest that the evidence for a crime control effect for the prosecution of intimate partner violence offenders, their conviction, and sentencing them to probation is stronger than the inconsistent findings reported by Wooldredge and Thistlethwaite.

* This research is only one of more than two dozen studies about the effectiveness of post arrest criminal justice sanctions for intimate partner violence. The predominate finding in that literature is one of no effect; even with the findings from our reproduction and our extension of Wooldredge and Thistlethwaite, the weigh of the available evidence about the effectiveness of criminal sanctions shifts only slightly toward a more positive assessment.

* Our research found that some of the findings of one prior study varied depending on how the data were analyzed, even when the new analyses sought to match the original analyses exactly. As far as we know, none of the other 30 studies have been subjected to the type of scrutiny applied here. Our assessment of that body of research and other criminological research would be stronger if more reported findings had been confirmed by independent analyses.

Implications for Research

* Reproducibility is an often stated but infrequently tested tenet of scientific research. This research identified three criteria for determining when scientific findings have and have not been confirmed and applied them to multiple analyses by Wooldredge and Thistlethwaite.

* Future research seeking to conduct secondary analyses to reproduce the findings of prior research would be stronger if it built on the multiple criteria set out in this report.

* Our ability to confirm most of the Wooldredge and Thistlethwaite's findings speaks well to the strengths of their efforts, the clear descriptions of their measures and methods, and the quality of their data and data documentation. Our inability to confirm some of their findings, especially some finding important for the crime control effects of criminal sanctions, provides a basis to revise somewhat our assessment of the research literature on this issue.

* Future analyses will be enhanced when archived data include detailed information about the dates of criminal justice processing events as well as the dates of repeat offenses that occur during as well as after the completion of a sentence.

* Our efforts at reproducing Wooldredge and Thistlethwaite's findings suggest the value of similar examinations of other studies about criminal sanctions prior to making any definitive policy judgments about their effectiveness in reducing repeat offending.

* Future efforts to synthesize findings on this and other criminological issues would be stronger if they were based on confirmed findings, instead of relying solely on the originally published findings.

Bibliography

Wooldredge, John D. *(2000) Reconsidering Domestic Violence Recidivism: Individual and Contextual Effects of Court Dispositions and Stake in Conformity in Hamilton County, Ohio, 1993-1998.* (Machine Readable Data File). Interuniversity Consortium for Political and Social Research Study # 3013. Ann Arbor, Michigan

Wooldredge, John D. 2002. "Examining the (Ir)Relevant of Aggregation Bias for Multilevel Studies of Neighborhood and Crime with an Example of Comparing Census Tracts to Official Neighborhoods in Cincinnati." *Criminology* 40(3):681-709.

Wooldredge, John D., and Amy Thistlethwaite. 1999. *Reconsidering Domestic Violence Recidivism: Individual and Contextual Effects of Court Dispositions and Stake in Conformity*. Final Report to the National Institute of Justice. Cincinnati, OH: University of Cincinnati.

Wooldredge, John D. and Amy Thistlethwaite. 2002. "Reconsidering Domestic Violence Recidivism: Conditioned Effects of Legal Controls by Individual and Aggregate Levels of Stake in Conformity." *Journal of Quantitative Criminology* 18(1, Mar):45-70.

Wooldredge, John D., and Amy Thistlethwaite. 2005."Court Dispositions and Rearrest for Intimate Assault." *Crime and Delinquency.* 51(18 Jan):75-102.

The Crime Control Effects of Prosecuting Intimate Partner Violence in Hamilton County, Ohio:

Reproducing and Extending the Analyses of Wooldredge and Thistlethwaite

A Final Report Submitted to the National Institute of Justice

Joel Garner, Principal Investigator
Joint Centers for Justice Studies, Inc

Christopher Maxwell, Co-Principal Investigator
University of Michigan and
Michigan State University

January 2008

This project was supported by Award # 2006-IJ-CX-0005 from the National Institute of Justice, Office of Justice Program, U. S. Department of Justice. Points of view in this report are those of the authors and do not necessarily reflect the official position of the Joint Centers, Michigan State University, the University of Michigan, the National Institute of Justice, the Office of Justice Programs or the U.S. Department of Justice. Additional financial assistance for this research was provided by an award from the Harry Frank Guggenheim Foundation.

The Crime Control Effects of Prosecuting Intimate Partner Violence in Hamilton County, Ohio: Table of Contents

Part One: Research Issues

The Crime Control Effects of Prosecuting Intimate Partner Violence in Hamilton County, Ohio: Table of Contents

The Crime Control Effects of Prosecuting Intimate Partner Violence in Hamilton County, Ohio: Table of Contents

The Crime Control Effects of Prosecuting Intimate Partner Violence in Hamilton County, Ohio

Table of Tables

The Crime Control Effects of Prosecuting Intimate Partner Violence in Hamilton County, Ohio

Table of Tables

The Crime Control Effects of Prosecuting Intimate Partner Violence in Hamilton County, Ohio

Part One: Research Issues

Introduction

This research seeks to enhance our understanding of the effects of prosecution, conviction and sentence severity on subsequent offending against intimate partners. Our approach is to conduct in-depth analyses of four recent publications conducted by John Wooldredge and Amy Thistlethwaite (Wooldredge and Thistlethwaite, 1999; 2002; 2005; Wooldredge, 2002). These publications were selected because of the breath, depth, and quality of their design, implementation, and analyses, the likely impact of their reported findings on scientific knowledge and future policy making, and the availability of the data for re-analysis. Our secondary analyses of these data are designed to more clearly explicate the specific nature of the published analyses and to identify the extent to which the published analyses can be reproduced from the available data.

This research goes beyond reproducing Wooldredge and Thistlethwaite's published analyses and develops alternative tests of the relationship between prosecution, conviction, sentence severity, and the patterns of repeat violence among intimate partners.

Organization of This Report

This report is organized into three parts. The three chapters of Part 1 review the prior research and the design of this research. Chapter 1 reviews the findings and the methodological strengths and weaknesses of the current research literature on the effectiveness of criminal sanctions for intimate partner violence. Chapter 2 reviews in detail the analyses and findings reported in the four publications by Wooldredge and his colleagues and places their publications

within the context of the larger body of research. Chapter 3 describes our approach to the reproduction and extension of the analyses by Wooldredge and Thistlethwaite.

The second part of this report presents the results of our efforts to reproduce the published findings of Wooldredge and Thistlethwaite. In Chapter 4, we present the findings of our efforts to reproduce the descriptive statistics reported by Wooldredge and Thistlethwaite. In Chapter 5, we report the results of our effort to reproduce their findings about the direct effects of criminal sanctions on subsequent rearrest. Chapter 6 presents our results about the direct effects of an offender's stakes in conformity and the extent to which the direct effects of sanctions are conditioned on an offender's stakes in conformity. The focus of Chapter 7 is the social context in which an offender lives, the effect of that context on rearrest, and the effect of social context on the relationship between sanctions and rearrest.

In Part 3, we extend the analyses of Wooldredge and Thistlethwaite. In Chapter 8, we report the results of our reformulated tests of the crime control effects of criminal sanctions and stakes in conformity. In addition, we use the available data to illustrate an analytical approach to separating the selection effects of criminal sanctions from the selection processes that determine which offenders are given more severe sanctions.

In Chapter 9, we summarize the substantive findings of this research. We identify the methodological strengths and weaknesses of the archived data and the substantive implications of our ability to reproduce Wooldredge and Thistlethwaite's analyses. Based on the findings from reproducing and extending the analyses of Wooldredge and Thistlethwaite, we provide what we think is a more policy relevant assessment of the crime control effects of prosecuting intimate partner violence, convicting offenders and sentencing them to a treatment program, to probation, or to jail, based on the data from Hamilton County, Ohio.

Importance of Crime Control Effects for Prosecution and Conviction

Since the early 1990s, the public debate about the appropriate response of the criminal justice system to intimate partner violence has emphasized the use of arrest, prosecution, conviction, and criminal sanctions to protect victims (Fagan, 1996; Ford, *et al.*, 1996). A recent National Academy of Sciences assessment of the effects of arrest concludes that "legal sanctions do have deterrent effects, although modest in magnitude" (Kruttschnitt, *et al.*, 2004: 7). The Academy also asserts that research on legal sanctions is generally poor in quality and that there was a need to increase support for more rigorous research.

According to some scholars (see Hirschel, *et al.* 1992; Tolman & Weisz, 1995; Mills, 1998; Zorza, 1992;), the lack of large and consistent effects of arrest on subsequent re-offending (Sherman, 1992; Maxwell, *et al.*, 2002) stems, in great part, from inadequate follow through by prosecutors and courts. According to this argument, when arrests are not prosecuted, offenders do not fear the consequences of their behavior and victims are not empowered by the criminal justice system (Cahn & Lerman, 1991; Cahn, 1992; Ellis, 1984; Lerman, 1986; Waits, 1985). Since the late 1960s, summaries of the research literature have consistently reported that both the prosecution and conviction for domestic violence occur rarely (Buzawa, *et al.* 1999; Dobash and Dobash, 1979; Hartman and Belknap, 2003; Jordan, 2004; Lerman, 1981; Martin, 1976; Miller, 1970; Parnas, 1970; Sherman; 1992; 2000; Tolman and Weisz, 1995; Walker, 1979; Worden, 2001). This assessment is also reflected in two reports from the National Academy of Science. Crowell and Burgess (1996, p. 118) report that "prosecution rates of battering cases typically have been low." Two years later, Chalk and King (1998, p. 279) found that

> the criminal justice system has traditionally been reluctant to impose fines, sentences, and other punitive sanctions on individuals charged with child maltreatment, domestic violence or elder abuse.

The available evidence suggests that the prosecution and conviction for intimate partner violence is more frequent than most of this research has assumed. Our review of 135 studies with sufficient information to produce a prosecution or conviction rate finds great variability in these rates across jurisdiction[1]. On average, one third of all reported offenses and more than three fifths of all arrests for intimate partner violence result in a prosecution; moreover, more than half of all prosecutions for intimate partner violence results in a conviction on one or more charges. Given that prosecution and conviction occurs more frequently than the prior research had asserted, questions about the effectiveness of criminal sanctions--prosecution, conviction and sentence severity–to reduce repeat offending become even more important.

While the effectiveness of arrest and prosecution has been questioned in the academic and reform literature, the Violence Against Women Act of 1994 and its subsequent re-authorizations continue to emphasize and promote the use of both arrest and prosecution through training grants and through direct support. The disconnect between the limited support for crime control effects for arrest and the continuing strong policy support by the Federal government raises an important question: what is known about the prevalence and frequency of repeat offending following the initiation of charges, the determination of a guilty verdict, or the imposition of more severe sanctions?

[1]A listing of these 135 studies and the number of offenses, arrests, prosecutions and convictions reported in provided in Appendix 1

Chapter 1: Research on the Crime Control Effects of Prosecuting Intimate Partner Violence

Hypotheses About Crime Control Effects

Our review of the research on intimate partner violence has identified seven central hypotheses about the crime control effects of prosecuting intimate partner violence offenders: 1) prosecution, 2) conviction, 3) sanction severity, 4) stakes in conformity, 5) social context, 6) sanctions / stakes interaction, and 7) sanctions / social context interaction.

The prosecution hypothesis is that the filing of criminal charges alone reduces repeat offending. The conviction hypothesis states that the effectiveness of criminal sanctions stems not from the mere filing of charges but from a formal conviction in criminal court. The sanction severity hypothesis asserts that it is the severity of the sentence, not the formal prosecution or conviction, that reduces subsequent aggression against intimate partners. While the prosecution and conviction hypotheses are relatively straightforward, the sanction severity hypothesis can involve the comparison of types and combinations of case dispositions where one or more dispositions are considered more severe than another disposition or group of dispositions.

There are two hypotheses related to an offender's stakes-in-conformity. The first stakes hypothesis is that stakes-in-conformity (marriage, employment, etc.) have a direct effect on repeat offending. The second stakes hypothesis contends that the direct effect of prosecution, conviction and sanctions are conditioned by the nature and extent of an offender's stakes-in-conformity; the expectation is that certain criminal sanctions will be more effective with offenders who have more stakes-in-conformity and less effective with offenders who have fewer stakes-in-conformity.

The social context hypothesis is a grand theme in sociology in general and criminology

in particular. This hypothesis asserts that individuals behave differently in different contexts. In this research literature, the concrete nature of this grand theme is that repeat offending will vary based on the characteristics of the neighborhood in which the offenders live. There is a second social context hypothesis that asserts that the effectiveness of sanctions will vary depending upon the characteristics of the offender's neighborhood. The relevance of these broad sociological theories to policy is frequently expressed as a rationale for emphasizing different policies in different neighborhoods and with different types of offenders.

The conceptualizing of the crime control effects of criminal sanctions as seven distinct hypotheses helps distinguish the effects of the policy choices available to the criminal justice system–the filing of charges, the willingness not to dismiss or drop charges, the sentences imposed upon the convicted, and the possibility of selecting alternative sanctions based on offender and neighborhood characteristics. Among the choices available to criminal justice officials are the policy choices of 1) emphasizing prosecution without emphasizing convictions or sentences or 2) prosecuting fewer offenders but emphasizing the conviction and sentencing of offender who are prosecuted (Jacoby, 1975). There are also sentencing options of fines, mandated treatment, probation and jail. These options available to prosecutors as part of plea bargains or to judges following conviction at trial. By distinguishing these policy components–prosecution, conviction, and sanction severity–we seek to improve the connection between research methods, analytical findings, and future policy decisions.

Substantive Findings: Crime Control Effects of Criminal Sanctions

The three sanction, two stakes-in-conformity and two social context hypotheses provide a basis for assessing the published findings about the crime control effects of crime sanctions. We have identified 30 reports that provide at least one explicit test of the relationship between

criminal sanctions and repeat offending for intimate partner violence. Table 1 - 1 summarizes the findings reported in these thirty studies for the three sanction hypotheses. For each of these studies, Table 1 - 1 displays the number of tests whose findings show statistically significant less repeat offending, more repeat offending, or no difference in repeat offending. Many studies report more than one test[2] of a particular hypothesis and studies with multiple tests often report inconsistent findings for each hypothesis.

The 30 studies reviewed here measure criminal sanctions in diverse ways and the associations they report of a particular sanction (e. g., diversion, probation, etc.) with repeat offending is frequently not interpreted by the original authors as a test of a broader hypothesis about prosecution, conviction or sentence severity. Thus, the summary of results we report below are based on our definition of prosecution as any time charges are filed and conviction as any form of a guilty verdict.

Based on the reported findings in Table 1 - 1, all three hypotheses about the crime control effects of sanctions for intimate partner violence find some support but none of them finds consistent support. The predominant finding in this literature is that criminal sanctions have no effect on subsequent re-offending. Out of the 164 tests, 107 (65.2%) show no statistically significant differences. In those studies where statistically significant effects were reported, the predominant finding favors the prosecution and conviction hypotheses. In 18 studies, 25 of the 61 tests (41.0%) of the prosecution hypothesis show less repeat offending following prosecution; only four (6.6%) of the 61 tests show more repeat offending following prosecution.

Fourteen of the 68 tests (20.6%) of the conviction hypothesis show reduced repeat

---

[2]Multiple tests can result from many sources: using offenses or arrests as a measure of repeat offending, using prevalence, frequency or time to failure parameters of the same measure, using different measures of criminal sanctions or using official records or victim interviews.

offending; only five (7.3%) of the tests show increase repeat offending following a conviction for intimate partner violence. The evidence in Table 1- 1 for the sentence severity hypothesis is less promising. Comparing more severe to less severe sentences, four out 35 tests (11.4%) show less repeat offending but five tests (14.2%) show more repeat offending.

Substantive Findings: Stakes in Conformity and Social Contexts

As displayed in Table 1 - 2, the stakes-in- conformity hypothesis was tested in 12 published reports with 16 of 63 statistical tests (25.4%) supporting the hypothesis, 47 (74.5%) showing no effect and none showing contrary results. These findings tend to support the underlying argument of the stakes hypothesis that repeat offending can vary depending upon the extent to which an offender has a stake-in-conformity.

Tests for an interaction between an offender's stakes-in-conformity and criminal sanctions following prosecution for intimate partner violence have been reported in only five reports (See Table 2 - 1) and four of those reports are based on the analysis of data from Hamilton County, Ohio. Like the previous hypotheses, these results are dominated by findings of no effect. Twenty-five out of 34 tests (73.5%) show no effect. Eight tests (23.5%) support this hypothesis and there is one reported test that is contrary to this hypothesis.

The social context hypothesis has been tested in four publications that were produced by the same team of researchers and based on analyses that were derived from the same sample of cases in Hamilton County, Ohio[3] (See Table 1 - 3). In these four publications, the social context hypothesis was tested using different subsamples, different statistical models and different

[3]In two of these publications, the social context variables are conceived of as aggregate level stakes-in-conformity. In order not to confound individual level and aggregate level measures of stakes-in-conformity and to not double count tests as stakes-in-conformity and social context, we record as aggregate level tests as measures of social context only.

outcome measures. Of the 19 tests reported in these four publications, 10 (52.6%) found that social characteristics of neighborhoods were associated with reduced repeat offending. In the other nine tests, social context was associated with no statistically significant differences in repeat offending.

Two of the studies using data from Hamilton County, Ohio also report tests of the social context-sanctions hypothesis. Based on these tests, support for this hypothesis is not very strong. Only three out of 29 tests (10.3%) confirm this hypothesis and six test results (20.7%) are contrary to this hypothesis--sanctions are associated with increased rates of rearrest in neighborhoods with more positive social contexts. As with the other six hypotheses, the predominant finding in 20 tests (68.9%) is no effect.

Summary of Crime Control Effects

We have identified thirty publications that report the extent to which the prosecution, conviction, or imposition of a more severe sentence is associated with lower levels of repeat violence between intimates. Consistent with the assessments from the National Academy of Sciences about the effects of sanctions generally (Blumstein, *et al.*, 1978; Kruttschnitt, *et al.*, 2004), the evidence from these 30 published reports is that the predominant finding is one of no effect; however, when statistically significant effects are reported, the evidence tends to favor the hypothesis that sanctions are associated with less subsequent offending more than it favors the hypothesis that sanctions are associated with more subsequent offending.

The evidence in the available research literature provides some support for the idea that the effectiveness of sanctions can vary by an offender's stakes-in-conformity and by the characteristics of the neighborhood where the offender lives. There is less support for the hypothesis that an offender's stakes-in-conformity mediates the effects of sanction and the

reported findings about whether social context mediates the effects of sanctions are mostly contrary, not supportive of this hypothesis.

Methodological Issues

In addition to the diversity of reported findings, we have identified several methodological limitations in this research literature that further limits our ability to reach definitive assessments of our seven hypotheses. We have identified five methodological issues--1) definition and operationalization of prosecution, conviction and severity of sentences, 2) measurement of repeat offending, 3) statistical power 4) selection biases, and 5) missing data--that appear widespread and problematic.

Definition of Prosecution and the Disposition of Criminal Charges

The research literature is often unclear about what constitutes a "prosecution". Given the extent of the research on the prosecution and conviction for intimate partner violence, there is considerable diversity about what constitutes a prosecution. Some studies count the filing of charges as a prosecution (Wooldredge & Thistlethwaite, 2002), but other studies report the effects of cases that are "fully" prosecuted (Kingsnorth, *et al.,* 2001). In the later understanding, cases that are dropped, dismissed or "*nolle prosed*" are not counted as prosecutions. Moreover, prosecutor records may show no charges filed or a dismissal of charges for a particular offense because the prosecutor was able to put the offender in jail by the revocation of probation or parole (Feeney, *et al.*, 1983; 1988; Kingsnorth, *et al.*, 2002. Ford and Regoli (1992) analyze cases randomly assigned to two distinct policies: allowing victims to drop prosecution or not. However, their analyses do not compare the outcomes of cases that are prosecuted with those that are not prosecuted. Similarly disparate definitions are used in describing how a case is disposed. Among the more problematic categories include "sentencing" offenders to diversion

programs or batterer treatment programs, sometimes with and sometimes without a formal conviction. Wilson and Klein's (2006) emphasize that many case dispositions are subsequently changed depending on an offender's behavior or participation in a treatment program after the initial disposition of the case. Thus, there is little consistency in what is meant by a prosecution or conviction and this limits our ability to summarize findings from one study to the next.

The research literature on criminal sanctions for intimate partner violence reflects the diversity in measures of sentence severity that has long vexed general court researchers. Some use a single measure ranging from dismissal to a jail sentence (Murphy, *et al.*, 1998; Peterson, 2004). Other studies (Davis, *et al.*, 1998) use the imposition of jail time as a dichotomous indicator of sentence severity. Of course, arrest can be conceived of as more severe than not being arrested and the same with prosecution versus not prosecution and conviction and not conviction. When the very meaning of prosecution, conviction and severity is so variable, tests of the effects of prosecution, conviction and sentence severity become difficult to interpret and to summarize across studies.

The meaning of the various sanction categories vary in another important way. The statistical tests about each of these hypotheses are derived from a diverse set of statistical comparisons. Many of the studies listed in Table 1 - 1 report multiple categories of criminal sanctions from the filing of charges, dismissal of charges, conviction with no sentence, fines, probation, treatment programs, and jail sentences. Each study selects a particular sanction category as the reference category against which the effectiveness of the other categories are compared. For instance, in some analyses, all prosecuted cases are compared to all cases not prosecuted; in other analyses, cases prosecuted are compared to all cases dismissed. Both of these options for statistical comparisons can create a test of the prosecution hypothesis.

Newmark, *et al.* (2001) and Hartley and Frohman (2003) add more complications; they report comparisons between cases processed in a specialized domestic violence court–where the prosecution of cases is more vigorously pursued--with domestic violence cases in other courts in the same jurisdiction.

There is similar diversity in how conviction cases are defined and whether the behavior of convicted offenders is compared to all offenders, all arrestees, or all prosecuted offenders. In addition, analyses can compare the relative severity of sentences to probation versus sentences to jail or they can compare offenders who are never prosecuted with offenders who go to jail. The lack of consistent definitions of criminal sanctions and of the alternatives to those sanctions limits our ability to synthesize the findings in this diverse research literature.

Measuring Repeat Offending

Among the thirty studies of the prosecution of intimate partner violence, we have identified the measures of repeat offending following prosecution (See Table 1 - 4). These studies vary greatly in the nature and size of the sample, the jurisdiction and time period of the study, and how repeat offending was measured. Some studies used official records to measure repeat offending; others used victim interviews; some used both. These studies also varied in what constitutes repeat offending. The study samples range from 74 victim interviews in Abbortsford, Canada (Marsland, *et al.*, 2001) to 6,489 police records in New York City (Peterson, 2003). Some studies measure any re-arrest; some counted any reported offenses; some required the repeat incident be against the same victim and some did not. Jolin, *et al.* (1998) report 14 different measures of repeat offending whose base rates vary from 5.8% to 61.3%.

These thirty reports measure repeat offending using times at risk that vary from 6 to 108

months. In most of these measures, the actual period at risk for measuring repeat offending begins immediately after the precipitating incident; other measures are not initiated until the formal disposition of the charges or at the time the criminal sanction ends, either with dismissal, acquittal, conviction, or the completion of any sentence. Not surprisingly, the base rates for repeat offending in these studies vary from 4.6% percent for an official record of a conviction for domestic assault within nine months of the disposition of charges in Eckberg & Podcopcaz (2002) to Wilson and Klein's (2006) report that 71.6% of the prosecutions in Quincy, Massachusetts resulted in a new arrest within 108 months of the initial incident.

There is an additional variant in the ways in which repeat offending is measured in prosecution research. Unlike arrest, prosecution takes time--sometimes several months from the initiation of charges to case disposition. Some, but not all offenders, are free on pretrial release and at risk to commit new offenses prior to the completion of the prosecution, the determination of guilty, or the imposition and completion of a sentence. To assess the crime control effects of criminal justice interventions, it is preferable to know when these interventions begin and end, when offenders are at risk, and when the repeat offending occurs. Most of the thirty publications on the crime control effects of criminal sanctions are silent on these timing issues. When repeat offending can occur before a prosecution or a conviction or a sentence begins, it is difficult to link changes in repeat offending to criminal sanctions that have not yet occurred.

Statistical Power

None of the studies listed in Table 1 - 1 reports the statistical power of their tests (Lipsey, 1990). Many of these studies have small sample sizes, especially for the analysis of convictions and sentences. The apparent lack of statistical power due to small sample sizes may explain why the predominant finding in this literature is "no effect." When statistical power is low, a finding

of "no effect" can just as easily mean that the research design was not rigorous enough to detect an effect as that no effect exists. This issue is particularly salient given the fact that the predominant finding about all seven hypotheses is that there are no statistically significant differences.

Selection Effects

Two types of selection bias are not well addressed in this research literature. Both types of bias stem from the processes that result in the selection of arrests to be prosecuted, of prosecuted cases resulting in a conviction, and convictions resulting in jail time. Sentencing often involves identifying the characteristics that determine jail time among a sample of convicted offenders. One concern is that this sample is not equivalent to the sample of offenders who were prosecuted or the sample of offenders who were arrested and that the relationships found in the convicted sample may not apply to the sample of prosecutions or arrests. Thus, the relationship between being sent to jail among a sample of convicted offenders may not be the same as the relationship between being sent to jail among the prosecuted sample. This issue is usually referred to as sample selection bias. Although the issue of sample selection bias was raised initially by Rauma (1984) and discussed extensively in the general literature on prosecution (Jacoby, 1975) and sentencing (Crutchfield*, et al.*, 1994; Zatz & Hagan, 1985), this issue is typically ignored in the published analyses of repeat offending following prosecution for intimate partner violence.

The second type of selection effect is well known to research on treatment effects (Sechrest, *et al.* 1979). This effect stems from the common presumption that, at each stage of criminal processing, offenders thought to be more likely to commit new offenses or more serious offenses are selected for more punitive treatments–arrested, prosecuted, convicted, and

incarcerated. To the extent that higher risk offenders are given more severe sanctions, research that does not separate out this selection effect cannot provide rigorous tests that distinguish the effect of the selection into the sanctioned group from the effect of the sanction treatment on subsequent offending. This type of selection effect can occur because of the normal operation of the criminal justice system or by the sampling methods of researchers: either way, it is important for analyses to recognize these limitations and to try to separate selection effects from the treatment effects of prosecution, conviction or sentencing severity. Only Wooldredge and Thistlethwaite (1999; 2002; 2005) and Wooldredge (2002) identify this limitation to their findings but they do not identify or implement any statistical controls to separate selection from treatment effects.

Missing Cases and Missing Data

There is also a consistent problem in this research literature with how missing data are addressed. Most of these studies report that they drop cases completely from their analyses where data from one or more variable in their model are missing. In other instances, the research imputes the value of missing cases. Among the 30 studies of the crime control effects of sanctions for intimate partner violence, the extent of missing data is frequently not reported. In none of these studies is the effect of imputing missing data considered in the substantive results or in the computation of statistical tests. While case-wise deletion is one possible method for addressing missing data, several other methods may be more appropriate and do not result in the loss of so many cases (Allison, 2001; Little and Rubin, 1987).

Summary of Methodological Issues

Individually and in combination, these methodological problems seriously threaten the validity and the reliability of the published findings about the crime control effects of

prosecuting intimate partner violence. If all these issues had been addressed, the existing studies may still have produced a diverse set of findings, maybe even the same diverse set of findings; however, because they were not addressed, the full value of the existing research for testing theories and for evaluating policies has not been realized.

These are not new methodological concerns. All of them have been raised by the National Academy of Sciences in their reviews of the research on deterrence and incapacitation (Blumstein, *et al.* (1978), rehabilitation, (Seschrest, *et al.*, 1979), Criminal Careers (Blumstein, *et al.*, 1986), sentencing (Blumstein, *et al.*, 1982), and domestic violence (Crowell and Burgess, 1996; Chalk and King, 1998; Kruttschnitt, *et al.*, 2004).

Table 1 - 1 Summary of Research on Sanction Hypotheses

		Prosecution			Conviction			Sentence Severity		
Study	Jurisdictions	Less	None	More	Less	None	More	Less	None	More
Belknap & Sullivan, 2003	Three Counties	4	1	0						
Buzawa, et al., 1999	Quincy	0	0	1	0	0	1	0	0	1
Davis, et al., 1998	Milwaukee	0	1	0	0	1	0	0	1	0
Dunford, 1990	Omaha				0	1	0			
Dunford, et al., 1990	Omaha				1	0	0			
Eckberg & Podcopcaz, 2002	Minneapolis				1	0	0			
Fagan, et al. 1984	Five U.S. Sites	1	0	0						
Fagan, 1989	Five U.S. Sites	1	1	0	1	1	0			
Ford and Regoli, 1993	Marion Co.	1	0	0						
Friday, 2006	Charlotte	0	1	0	0	1	0	0	1	0
Frisch, 2001	Six U.S. Sites				0	0	1			
Gross, et al., 2000	Chesterfield Co.				0	8	0	0	4	0
Hartley & Frohmann, 2003	Chicago				0	3	0	0	3	0
Jaffe, et al, 1993	London	5	0	0						
Jolin, et al., 1998	Portland	2	7	3						
Kingsnorth, 2006	Sacramento	0	1	0	0	1	0	0	1	0
Marsland, et al., 2001	Abbotsford	2	0	0						
Murphy, et al., 1998	Baltimore				2	0	0	2	0	0
Newmark, et al., 2001	Brooklyn				0	4	0			
Orchowsky, 1999	Alexandria	0	1	0				0	0	1
Peterson, 2003i	Brooklyn; Bronx				0	2	0	0	2	0
Peterson, 2004	Manhattan				1	2	1	1	2	1
Steinman, 1988	Lancaster Co.	0	1	0	0	1	0	0	3	0
Steinman, 1991	Lancaster Co.	0	1	0	0	1	0			
Tolman & Weisz, 1995	Dupage Co.	0	2	0						
Ventura & Davis, 2005	Toledo				1	0	0	1	0	0
Wooldredge & Thistlethwaite, 1999	Hamilton Co.	3	15	0	3	12		0	3	0
Wooldredge, 2002	Cincinnati	2	0	0	0	2	0			
Wooldredge & Thistlethwaite, 2002	Hamilton Co.	1	0	0	0	3	0	0	2	0
Wooldredge & Thistlethwaite, 2005	Hamilton Co.	3	0	0	4	6	2	0	4	2
Direction Reported		Less	None	More	Less	None	More	Less	None	More
Summary of Effects		25	32	4	14	49	5	4	26	5
Number of Studies		18			20			14		

Table 1 - 2: Summary of Research on Stakes in Conformity

		Individual Level Stakes in Conformity			Individual Level Stakes & Sanctions		
Study	Jurisdictions	Less	None	More	Confirm	None	Contrary
Friday, 2005	Charlotte	0	1	0			
Gross, et al., 2000	Chesterfield Co.	1	3	0			
Kingsnorth, 2006	Sacramento	0	4	0	0	1	0
Orchowsky, 1999	Alexandria	0	2	0			
Peterson, 2003i	Brooklyn; Bronx	2	3	0			
Peterson, 2004	Manhattan	3	16	0			
Steinman, 1988	Lancaster Co.	1	8	0			
Ventura & Davis, 2005	Toledo	1	7	0			
Wooldredge & Thistlethwaite, 1999	Hamilton County	3	0	0	5	13	0
Wooldredge, 2002	Cincinnatti	0	0	0	0	0	0
Wooldredge & Thistlethwaite, 2002	Hamilton County	3	2	0	3	8	1
Wooldredge & Thistlethwaite, 2005	Hamilton County	0	0	0	0	0	0

	Less	None	More	Confirm	None	Contrary
Tests	14	46	0	8	22	1
Studies		12			5	

Table 1 - 3: Summary of Research on Social Context

		Aggregate Level Social Context			Aggregate Level Context & Sanctions		
Study	**Jurisdiction**	**Confirm**	**None**	**Contrary**	**Confirm**	**None**	**Contrary**
Wooldredge & Thistlethwaite, 1999	Hamilton Co.	2	1	0	0	12	6
Wooldredge & Thistlethwaite, 2002	Hamilton Co.	2	0	0	3	5	0
Wooldredge, 2002	Cincinnatti	2	6	0			
Wooldredge & Thistlethwaite, 2005	Hamilton Co.	4	2	0			
		Confirm	**None**	**Contrary**	**Confirm**	**None**	**Contrary**
	Tests	10	9	0	3	17	6

Table 1 - 4: Outcome Measures Used in Intimate Violence Prosecution Research (Page 1 of 5)

Report Author/Date	Data Source	Time at Risk Starts at	Type of Behavior Repeat Incident	Recidivism Sample Size	Reported Base Rate	Months At Risk	6 Month Rate
Belknap & Sullivan, 2003	Victim Interviews	Disposition	Any CTS Item	160	38.1%	6	38.1%
				148	34.9%	6	34.9%
			Psychological Abuse	160	m = 1.11	6	N.A.
				148	m = .99	6	N.A.
Buzawa, et al., 1999	Police Records	Arraignment	Any Arrest	353	47.9%	12	24.0%
			Arrest for violence; same victim		22.1%	12	11.1%
			Arrest for violence; not same victim		10.8%	12	5.4%
			Arrest for nonpersonal offense		15.0%	12	7.5%
	Victim Interviews		Violence or Violation of Restraining Order	118	49.2%	12	24.6%
Davis, et al., 1998	Police Records	Disposition	Any arrest	1,133	N.R.	6	N.R.
Dunford, 1990	Police Records	Incident	Arrest; Same Victim	247	8.9%	6	8.9%
					16.2%	12	8.1%
			Reports; Same Victim		18.6%	6	18.6%
					28.7%	12	14.4%
	Victim Interviews		Same Victim Pushed/Hit	196	58.2%	6	58.2%
					62.2%	12	31.1%
Dunford, et al, 1990	Police Reports	Incident	Arrest; Same Victim	330	10.6%	6	10.6%
			Reports; Same Victim		16.7%	6	16.7%
	Victim Inteviews		Same Victim Pushed/Hit	242	40.5%	6	40.5%

Table 1 - 4: Outcome Measures Used in Intimate Violence Prosecution Research (Page 2 of 5)

Report Author/Date	Data Source	Time at Risk Starts at	Type of Behavior Repeat Incident	Recidivism Sample Size	Base Rate	Months At Risk	6 Month Rate
Eckberg & Podcopcaz, 2002	Police Records	Disposition	Arrest	6,187	30.7%	9	20.5%
			Arrest for Domestic Violence		14.1%	9	9.4%
			Conviction		14.8%	9	9.9%
			Conviction for Domestic Assault		4.6%	9	3.0%
Fagan, et al.,1984	Victim Interviews	Dispostion	Violence	149	32.2%	6	32.2%
Fagan, 1989		Incident	CTS plus sexual assault	270	28.5%	6	28.5%
Ford & Regoli, 1992b	Police Records	Disposition	Violence	642	3.1%	6	3.1%
	Victim Inteviews	Disposition	Violence	430	30.9%	6	30.9%
			CTS Violence		34.2%	6	34.2%
			CTS Severe Violence		20.5%	6	20.5%
Friday, et al. (2006)	Police Records	Incident	Any Domestic Violence Offense	766	34.1%	24	8.5%
Frisch (2001)	Police Records	Incident	Any Offense	6,803	36.6%	18	12.2%
			Domestic Violence Offense		31.6%		10.5%
			Aggrevated Offense		19.6%		6.5%
			Aggrevated DV Offense		15.7%		5.2%
Gross, et al., 2000	Police Records	Incident	Any Arrest	177	24.3%	18	8.1%
			Any Conviction		19.8%	18	6.6%
Hartley & Frohmann, 2003	Police Records	Incident	Any arrest	706	28.8%	6	28.8%
			Arrest for Domestic Violence		12.0%	6	12.0%
	Victim Interviews	Disposition	Kicked, bit or hit with fist	47	10.6%	6	10.6%
Jaffe, et al, 1993	Victim Interviews	Incident	Pushed, grabbed or shoved	90	50.9%	12	25.5%
			Slapped		28.3%	12	14.1%
			Kicked, hit, or bit		26.7%	12	13.4%

Table 1 - 4: Outcome Measures Used in Intimate Violence Prosecution Research (Page 3 of 5)

Report Author/Date	Data Source	Time at Risk Starts at	Type of Behavior Repeat Incident	Recidivism Sample Size	Base Rate	Months At Risk	6 Month Rate
Jolin, et al., 1998	Police Records	Incident	Prevalence of Re-victimization	883	14.0%	6	14.0%
			Prevalence of Arrest from Revictimization		7.8%	6	7.8%
			Prevalence of Re-offense		13.6%	6	13.6%
			Prevalence of Arrest from Re-Offense		8.0%	6	8.0%
			Frequency of Re-vcitimization		m = .20	6	m = .4
			Frequency of Arrest from Revictimization		m = .08	6	m = .16
			Frequency of Re-offending		m = .22	6	m = .44
			Frequency of Arrest for Re-offending		m = .08	6	m = .16
			Any Repeat Calls to same address		m = .46	6	m = .92
			Repeat Call for DV at same address		m = .14	6	m = .28
	Victim Interviews		Prevalence of victimization same offender	395	60.8%	6	60.8%
			Prevalence of victimization any offender		61.3%	6	61.3%
			Frequency of revictimization same offender		m = 3.28	6	m = 6.56
			Frequency of revictimization any offender		m = 3.41	6	m = 6.82
Kingsnorth, 2006b	Police Records	Incident	Arrest for Intimate Partner Violence	872	15.3%	18	5.1%

Table 1 - 4: Outcome Measures Used in Intimate Violence Prosecution Research (Page 4 of 5)

Report Author/Date	Data Source	Time at Risk Starts at	Type of Behavior Repeat Incident	Recidivism Sample Size	Base Rate	Months At Risk	6 Month Rate
Marsland, 2001	Victim Interviews	Incident	Assault	74	43.0%	27	9.6%
Murphy, et al., 1998	Police Records	Incident	Battery or Violation of Protection Order	235	15.7%		
			Violent Offense		25.5%	12 to 18	N.A.
Newmark, et al., 2001	Police Records	Disposition	Any Arrest	304	31.3%	12	15.7%
			Arrest for Violent Felony		4.9%	12	2.5%
			Arrest for Criminal Contempt		11.1%	12	5.6%
			Any Arrest		39.9%	18	13.3%
			Arrest for Violent Felony		7.2%	18	2.4%
			Arrest for Criminal Contempt		15.2%	18	5.1%
Orchowsky, 1999	Police Records	Incident	Domestic Violence Offense	1910	21.0%	variable	N.A.
Peterson, et al., 2003i	Police Records	Disposition	Any Arrest for Domestic Violence	6489	17.0%	18	5.7%
Peterson, 2004	Police Records	Disposition	Any Arrest for Domestic Violence	2134	14.2%	18	4.7%
Steinman, 1988	Prosecutor Records	Incident	Charged with Physical Violence or Threats	183	19.7%	12	9.8%
Steinman, 1991	Police Records or Victim Interview	Incident	Domestic Violence Offense	338	61.5%	Not Reported	N.A.
	Victim Interviews				59.5%		

Table 1 - 4: Outcome Measures Used in Intimate Violence Prosecution Research (Page 5 of 5)

Report Author/Date	Data Source	Time at Risk Starts at	Type of Behavior Repeat Incident	Recidivism Sample Size	Base Rate	Months At Risk	6 Month Rate
Tolman and Weisz, 1995	Police Records	Incident	Domestic Violence	341	29.9%	16	11.2%
Venture & Davis, 2005	Police Records	Disposition	Arrest for Domestic Violence	519	32.6%	12	16.3%
Wooldredge & Thistlethwaite, 1999; 2005	Police Records	Completion	Prevalence of Arrest for Domestic Violence	3110	16.0%	24	4.0%
		Completion	Frequency of Arrest for Domestic Violence	3110	m = .19	24	N.A.
		Completion	Time to First Arrest for Domestic Violence	3662	m = 10.0	Variable	N. A.
Wooldredge, 2002	Police Records	Completion	Arrest for Domestic Violence	1855	14.0%	24	3.5%
Wooldredge & Thistlethwaite, 2002	Police Records	Completion	Arrest for Domestic Violence	3110	14.0%	24	3.5%

Chapter 2: Wooldredge and Thistlethwaite's Contributions to our Understanding of the Effectiveness of Sanctions for Intimate Partner Violence

In Chapter 1, we reviewed thirty studies that report statistical tests of the effectiveness of prosecution, conviction, and sentence severity on subsequent offending for intimate partner violence. We identified the diversity of findings reported as well as a number of methodological issues which threaten the validity and reliability of the published findings.

In this chapter, we review in depth four publications produced by Wooldredge and Thistlethwaite (Wooldredge and Thistlethwaite, 1999; 2002; 2005; Wooldredge, 2002). We focus on these four publications because of the generally high quality of this research, the frequent citation to these studies in the research literature, and the range of substantive issues that are addressed. The first section of this chapter provides a general overview of the characteristics of these four publications. The second section compares and contrasts the design elements of the statistical analyses in these publications. The last section of this chapter compares and contrasts the substantive findings reported in each of these publications.

Section 1: Overview of Four Reports

Between 1999 and 2005, Wooldredge and his colleagues produced four publications (Wooldredge and Thistlethwaite, 1999; Wooldredge, 2002; Wooldredge and Thistlethwaite, 2002; and Wooldredge and Thistlethwaite, 2005) that use data from Hamilton County, Ohio to assess various hypotheses about the effectiveness of sanctions for intimate partner violence. These publications include a variety of multivariate and multi-level tests of all seven of the crime control hypotheses we identified in Chapter 1. All of these publications are based on official record data about repeat offending and the extent of criminal sanctions imposed for domestic violence arrests in Hamilton County, Ohio during the mid-1990s. The data from this study was

submitted to the National Institute of Justice and released in 2000 by the National Archive of Criminal Justice Data (Wooldredge, 2000). The four published analyses reviewed here use a variety of samples, statistical procedures, and measures of criminal sanctions, stakes-in-conformity, neighborhood context and repeat offending. In addition, the reported findings about the effectiveness of criminal sanctions vary between publications and, sometimes, within publications.

Final Report to the National Institute of Justice, 1999

This 111 page technical report (Wooldredge and Thistlethwaite, 1999) addresses a number of potential reasons for the inconsistent results from the prior research on the effectiveness of arrest by testing a variety of individual and aggregate level models of the impact of criminal prosecution and sentencing on rearrest. This study assesses the effectiveness of various court dispositions for preventing, reducing, and delaying domestic violence across a sample of 3,954 suspects arrested for misdemeanor domestic violence between August 1993 and May 1996 in Hamilton County (Cincinnati), Ohio. During this time, Hamilton County had a mandatory arrest policy for misdemeanor domestic violence suspects when police officers determine that there was an immediate threat to the victim. According to Wooldredge because they left the jurisdiction and could not be traced (p. 37), 292 arrestees were dropped from this sample, leaving a total sample of 3,662 at risk arrestees. Data on case suspect demographics were obtained from an intake interview. Information on court dispositions were obtained from court records. Follow-up data on subsequent arrests were captured from police records through the end of May 1998.

In the NIJ Final Report, Wooldredge and Thistlethwaite present three analytical models. The first model includes five control variables: offender sex, age, number of prior misdemeanor

convictions, prior incarcerations for non-domestic violence offenses, and whether they were living with the victim at the time of the offense. Model 1 also includes seven dichotomous measures of criminal case processing–no charges filed, charges dropped, defendant acquitted, sentenced to treatment program, sentenced to probation, sentenced to jail, and sentenced to both probation and jail. The excluded group in this analysis of case disposition is "no charges filed."

Model 1 also includes two composite measures. The first represents the offender's personal stakes-in-conformity and was created by a factor analysis of six components–a high school degree, a college degree, employment, employment in a skilled job, not receiving public assistance, and living at the current residence for at least five years. The second composite measure was also created by a factor analysis but this measure was created from six characteristics of the census tract where the arrest occurred–the proportion of residents with a high school degree, the proportion with a college degree, the proportion employed, the proportion employed in skilled occupations, the proportion not receiving public assistance, and the proportion at the same residence for five years or more. This second composite measure captures the social context of the arrestee's household[4].

Model 2 builds on Model 1 by testing whether particular sanctions are more or less effective with individuals with more or fewer stakes-in-conformity. Model 3 builds on model 2 by testing whether particular sanctions are more or less effective depending upon six measures of the social context of the census tract. Models 1 and 2 were tested using HLM version 4.04 (Bryk, Raudenbush, and Congdon 1992). Model 3 was examined using SPSS's Cox regression because HLM 4.04 does not have a mechanism for addressing censored data. For each variable in each

[4]In this publication, the social context is conceived of as an aggregate level measure of stakes-in-conformity.

analysis, this article reports unstandardized regression coefficients, standard errors and an * indicating if the test meets the $p < .05$ test.

In addition to these three models, this research uses three measures of repeat offending: 1) the prevalence of re-arrest within 24 months of the sentence completion, 2) the frequency of re-arrest within 24 months of sentence completion, and 3) the number of months from the original arrest to the first re-arrest, if any. Each of the three models were tested using each of the three outcome measures. Because 552 offenders had not completed their sentences before May 31, 1996 (24 months before the end of collecting data on re-arrests), they were dropped from the analyses of models 1 and 2. Thus, models 1and 2 are based on the sample 3,110 arrests. 448 (14,4%) of these 3,110 offender were re-arrested within 24 months.. 576 (15.7%) of the 3,662 offenders in Model 3 were re-arrested at least once.

Journal of Quantitative Criminology, 2002

In this article (Wooldredge and Thistlethwaite, 2002), Wooldredge and Thistletwaite emphasize the extent to which criminal sanctions vary by an offender's stakes-in-conformity. This study uses the sample of 3,110 arrests and much the same models as the NIJ Final report. This article tests three models using one outcome measure–the prevalence of re-arrest within 24 months of sentence completion. Each of these analyses include four of the five control variables used in the NIJ final report–offender sex, age, number of prior misdemeanor convictions and living with the victim at the time of the arrest. The measure of prior incarceration for non-domestic violence offenses, used in the 1999 NIJ Final Report, is dropped and the offender's race is added. In this analysis, the existence of pending charges is categorized as a control variable and not as a type of sanction.

This study formulates criminal sanctions as four dichotomous measures--no charges filed,

offender program, probation or jail with a program, and probation or jail without a program. In these analysis, these four types of case disposition are compared to cases that were originally charged but subsequently dismissed or acquitted.

The *JQC* article retains the basic approach to testing individual and aggregate level stakes-in-conformity. The analyses begins with the same six individual level components of stakes-in-conformity; however, instead of a single factor, this article uses an offender's residential stability as an independent measure and constructs an education and an economic status factor out of the five remaining components. At the aggregate level, this analysis uses the proportion of the census tract living at the same residence for more than five years and a single factor representing the other five aggregate level measures of social context.

In the JQC analysis, Model 2 includes the interaction between the two aggregate level measures of social context and the four sanction variables for eight tests of the sanctions / context hypothesis[5]. Model 3 involves the 12 individual interactions between four sanction variables and three individual level stakes variables. All the direct and indirect tests in the *JQC* article were produced using logistic regression using the HLM 5.0 (Raudenbush, *et al.*, 2003), a newer version of the statistical package used in the NIJ Final Report. For each variable in each analysis, this article reports unstandardized regression coefficients, standard errors and an * indicating if the test meets the $p < .05$ test.

Criminology, *2002*

This research (Wooldredge, 2002) focuses on aggregate effects that are measured at the census tract or at the neighborhood level. Wooldredge reports two multivariate, multi-level

[5]In the NIJ Final Report, model 2 included the individual level interactions; thus, the results from model 2 in the JQC article are not directly comparable to the results in model 2 in the NIJ Final Report.

models of the effects of twelve individual level statistical controls, two sanction variables, and four measures of community context. Both of these models are limited to 1,855 arrests within the city of Cincinnati. The first model structures the second level analysis using 126 census tracts; the second model structures the second level analysis using 48 neighborhoods. The comparisons between models 1 and 2 in this analysis are designed to assess the affect of using alternative geographic units of aggregation with the same statistical analysis.

The twelve individual level statistical controls are the same six used in the *JQC* analysis plus the six variables used to construct the individual level stake in conformity factor scores in the NIJ Final Report. At the aggregate level, six census measures are used to create two Social Class Factors, one when the census data is aggregated to 126 census tracts and one when the census data is aggregated to the 48 neighborhoods. Five of the census measures are the same as the measures used in the NIJ Final Report and in the *JQC* analyses. The residential stability variable is dropped from the factor analyses. The proportion of the population that is nonwhite replaces residential stability as the sixth component of the social factor score. The analysis of social context uses these factor scores as well as the measure of residential stability and two new aggregate measures–the proportion male and average age of the population.

The *Criminology* article tests the effects of sanctions by using two variables--no charges filed and convicted. As with the *JQC* article, the comparison group includes offenders who were arrested and charged but the cases were dismissed or they were acquitted. Despite the fact that this analysis uses all the individual level and aggregate level measures conceptualized in the NIJ Final Report and the *JQC* article as stakes-in-conformity, in this analysis they are presented as either individual level statistical controls or as measures of social context. The analyses in this article provide no tests that effectiveness of sanctions vary by an individual's characteristics or by

the social characteristics of the census tract or the neighborhood where the offender lives.

The analyses of the prevalence of re-arrest were conducted using HLM 5.0 and states that the analyses used with restricted maximum likelihood estimation procedures (Raudenbush, *et al.*, 2000). For each variable in each analysis, this article reports unstandardized regression coefficients, standard errors and an * indicating if the test meets the $p < .05$ test.

Crime and Delinquency, 2005

This article (Wooldredge and Thistlethwaite, 2005) highlights which types of criminal sanctions are associated with increased or decreased levels of repeat offending. The analyses reported here use multivariate, multi-level models to test for the effectiveness of criminal sanctions on the prevalence of re-arrest, the frequency of re-arrest and the time to first re-arrest. The model add a new individual level statistical control--offender has a substance abuse problem at arrest-- as well as the six other statistical controls used in the *JQC* and *Criminology* articles. Two aggregate level measures are used–proportion of population at same residence five plus years and proportion of population with college degrees. In this article, none of these seven statistical controls are conceptualized as stakes-in-conformity.

The three multivariate analyses presented in this article use another variation in the formulation of the sanctions variable. The comparison group is just dismissed cases. The six dichotomous sanctions variables are 1) no charges filed, 2) acquitted at trial, 3) intervention program, 4) probation, 5) jail and 6) probation and jail. The primary multivariate analysis provided is the direct effect of seven statistical controls and six sanctions variables for each of the three outcome measures. The prevalence and frequency models are created using HLM.5.0 statistical package (Raudenbush, *et al.* 2000). The time to failure model is created using Cox regression.

In a separate analysis of the prevalence of re-arrest, this article also reports tests for the interaction of criminal sanctions with six of the seven individual level characteristics and with the two aggregate level characteristics (Table 3, pp. 88-89). In this analysis, three continuous variables are reformulated as dichotomies--number of prior arrests for misdemeanor violence (0 and 1 versus more), the proportion in the same residence five years plus (bottom third versus upper third), and the proportion college graduates (bottom third versus upper third). The tests for interactions are not the traditional unstandardized regression coefficients with standard errors. The tests used are comparisons between the observed and predicted odds of re-arrest for 112 possible combinations–two groups for the intersection of cases for each of the seven possible court dispositions, a total of eight individual level and aggregate level characteristics.

Substantive Findings

Each of these four reports are based on large numbers of arrests for intimate partner violence. These reports test a variety of hypotheses about the effects of sanctions, stakes-in-conformity and social context on subsequent repeat offending between intimate partners. They use multivariate and multi-level models to test three criminal career parameters--the prevalence, frequency and time to first re-arrest. These reports utilize rigorous multivariate and multi-level statistical methods to test criminological theories and evaluate contemporary criminal justice policies. While this research would have been stronger if it had included information from victim interviews, it's methods and measures--large sample size, multiple outcome measures, tests of multiple hypotheses, and the use of rigorous statistical procedures--make it one of the pre-eminent assessments of the effectiveness of criminal sanctions on repeat violence between intimate partners. For these reasons, we think that these study's measures and methods are sufficiently strong that their substantive findings warrant serious attention.

We have structured our assessment of Wooldredge and Thistlethwaite findings according to the seven hypotheses we set out in Chapter 1. For each these hypothesis, we display whether the published findings show a statistically significant decrease in repeat offending (Less), a statistically significant increase in repeat offending (More), or no difference (No Effect).

Effects of Sanctions

The four Wooldredge and Thistlethwaite articles reviewed above employ a variety of sanction types in their published analyses. Table 2 - 1displays how the frequency of cases varies depending on whether the analyses is based on 3,662 arrests, 3,110 arrests, or 1,855 arrest and how sanction types are combined[6]. In all the analyses, the predominate sanction type is the dropping of charges after they have been filed. In the analyses using 3,110 arrests, 53.0% of the cases are dropped; in the analyses using 3,662 arrests, 45.0% are dropped.

Table 2 - 1 reveals that most of the cases dropped from the prevalence and frequency analyses were sentenced to probation. In analyses with 3,110 arrests, 14.8% of the arrests were sentenced to probation; in the analyses of 3,662 arrests, 25.0% of the arrests are sentenced to probation. Thus, the analyses reported here are not only based on samples of varying sizes but the proportion of arrests sentenced to probation can vary by nearly 100% from one analysis to another.

Table 2 - 2 displays Wooldredge and Thistlethwaite's published findings about the effects of sanctions[7]. Although these analyses are drawn from the same sample of arrests, each publication has a distinct set of analytical comparisons. In the NIJ Final Report, all other

[6]In the NIJ final report, pending cases are treated as a type of sanction. In the other three articles and in this review, this consideration is treated as a statistical control.

[7]All the findings reported in this section are the direct effects of sanctions and are derived from models which do not include interaction terms for individual or contextual variables.

sanctions are compared to the arrests where no charges were filed. In this formulation, Wooldredge and Thistlethwaite report that the only statistically significant effects were for arrestees sentenced to the diversion program and these effects were consistent for the analyses of the prevalence, frequency and time to first re-arrest. Arrestees sentenced to this program had rates of repeat offending that were lower than arrestees who were never charged.

In the analyses reported in the *JQC,* all other sanctions are compared to a reference group of arrestees whose charges were either dismissed or who were acquitted at trial. In this analysis, arrestees who were never charged had statistically significant higher rates of repeat offending than the reference group of dismissed and acquitted cases. They report no differences in repeat offending for arrestees sentenced to probation or jail. Similar findings are reported in the *Criminology* article which also uses dismissed and acquitted cases as the reference group. In the *Criminology* article, arrestees who were never charged had higher rates of repeat offending than the reference group. Again, arrestees sentenced to probation or to jail have rates of repeat offending that were similar to arrestees who charges were dropped or who were acquitted.

In their article in *Crime and Delinquency*, Wooldredge and Thistlethwaite report that arrestees sentenced to probation had statistically significant lower rates of repeat arrest than the reference group of dismissed cases and that this was found in the analyses of prevalence, frequency and time to first re-arrest. In addition, they report that, in all three analyses, arrestees with no charges filed and, in two out of three analyses, arrestees sentenced to both probation and to jail had higher rates of repeat offending.

Stakes-in-conformity

In the 1999 NIJ Final Report, Wooldredge and Thistlethwaite use factor analysis to combine six characteristics of arrestee – residential stability, high school graduation, college

graduation, employment, skilled employment and not receiving public assistance–into a single measure of stakes-in-conformity. In the *JQC* article, stakes-in-conformity is measured by residential stability, a factor for education derived from the two education measures and a factor for economic status derived from the economic measures. These same six measures are used as statistical controls in the *Criminology* article and two of them are used as statistical controls in the *Crime and Delinquency* article.

Table 2 -3 displays all of these results. As a single factor in the NIJ Final Report, they are a consistent predictor of repeat offending. As three measures, in the *JQC* article, only residential stability is associated with statistically significant reductions in repeat offending. In the analyses of Cincinnati only in *Criminology,* only the completion of high school and being employed are associated with reduce violence between intimates. In all three analyses in the *Crime and Delinquency* article, residential stability is once again associated with reduced re-offending but having a college education is not.

As displayed in Table 2 - 3, Wooldredge and Thistlethwaite report 24 tests of the relationship between various measures of an offender's stakes-in-conformity. There appears to be a consistent effect for residential stability in the entire Hamilton County but not for arrests made within the City of Cincinnati. In only 11 of the 24 tests reported by Wooldredge and Thistlethwaite, is there a statistically significant reduction in repeat offending for offenders with more stakes-in-conformity. The presence of some statistically significant effects provides some support for the stakes hypothesis but the lack of consistent support raises questions about whether all stakes-in-conformity will affect repeat offending. In addition, prior tests of the stakes-in-conformity hypothesis have emphasized the use of employment and marriage (Sherman, *et al.* 1992; Berk, *et al.*, 1992; Pate and Hamilton, 1992). Wooldredge and Thistlethwaite' analyses

provide mixed support for employment but do not use marriage as a measure of stakes-in-conformity.

Social Context

The analyses by Wooldredge and Thistlethwaite incorporate aggregate level measures of social context and their reported findings are summarized in Table 2 - 4. In the NIJ Final Report they combine six items extracted from the 1990 census into a single factor[8]. In that report, they find that this social context variable is associated with less repeat offending in two out of their three multivariate, multi-level analyses. In their article in *JQC,* they test two measures: 1) the proportion of the census tract resident there for greater than 5 years and 2) a social and economic status factor derived from five economic and educational measures of the census tract where the arrestee lived. Both of these measures are associated with statistically significant reductions in repeat offending.

Their analyses of arrests within the city limits of Cincinnati test four social context variables. In these analyses, residential stability and proportion male show no effect on repeat offending but mean age and a social class factor are associated with reduced offending when neighborhood is defined using census tracts but not when neighborhood is defined using official neighborhoods. In their most recent article, Wooldredge and Thistlethwaite once again test the effects of residential stability along with the proportion of the census tract that has a college degree. In this analysis, three out of three tests of the aggregate measures of proportion of residents with a college education are associated with statistically significant lower rates of repeat

[8]In the NIJ Final Report and the article in *JQC*, Wooldredge and Thistlethwaite conceptualize their social context measures as aggregate level stakes-in-conformity but do not do so in the *Criminology* or *Crime and Delinquency* articles. In order to avoid confusion about individual level and aggregate level measures, we report all aggregate level variables as measures of social context.

offending but that is true for only one out of three tests of the aggregate measures of residential stability.

In summary, Wooldredge and Thistlethwaite report 19 tests of the social context hypothesis; in nine of those tests, they find that social context is associated with reduced repeat offending following prosecution for intimate partner violence. In ten of these tests, the measures of social context were reported to have no effect on subsequent offending. Residential stability, either as part of a factor score or as an independent measure, was associated with less repeat offending in seven out of eight tests using samples from all of Hamilton County but was not a statistically significant effect in the two tests limited to Cincinnati proper.

Sanctions / Stakes Hypothesis

The argument that the effects of sanctions were conditioned upon an offender's stakes-in-conformity was initially raised by Sherman, *et al.* (1992) as an explanation for why arrest was not consistently associated with reductions in subsequent violence between intimates. This hypothesis has been tested more extensively in three companion pieces published in the *American Sociological Review* (Sherman, *et al.* 1992; Berk, et al, 1992; Pate and Hamilton, 1992). These tests involved the direction and statistical significance of the interaction term between a dichotomous sanction variable (arrest or not arrest) and dichotomous measures of personal stakes. The measures of stakes-in-conformity used in these analyses were prior violence, race, education and employment (See, Garner, *et al.* 1995). The tests reported compared an offender arrested and having stakes with all other offenders and a statistically significant negative coefficient was the criterion for determining support for this hypothesis.

Wooldredge and Thistlethwaite report statistical tests of individual level stakes-in-conformity in two publications–the NIJ Final Report and the *JQC* article (See Table 2 - 5). In the

NIJ Final Report, they use a single interval level measure of stakes-in-conformity and six dichotomous measures of sanctions. Each of these sanctions are compared to the group of arrestees for whom no charges were filed so each test can be interpreted as comparing a lesser sanction with a higher sanction. They report a statistically significant and negative relationship between stakes and being sanctioned by a sentence to probation and jail sanctions for all three models–prevalence, frequency and time to failure. They also report that the interaction between the stakes variable and being sentenced to a diversion program was also negative and statistically significant for two out of three models. For all other tests of the sanctions / stakes hypothesis, Wooldredge and Thistlethwaite report no statistically significant effects.

In the article in the *JQC*, Wooldredge and Thistlethwaite test the sanctions / stakes hypothesis using four sanction categories and three stakes variables. In this analysis, the program / stakes interaction shows less repeat offending in two out of three tests. The other nine tests of this hypothesis show no effect except for the interaction of the economic stakes factor and being sentenced to probation or jail without a program. This interaction was statistically significant but positive. Of the thirty tests of the sanctions / stakes hypothesis, seven supported it and twenty-three did not, including one which contradicted the hypothesis.

Sanctions / Social Context Hypothesis

The NIJ Final Report and the *JQC* article test the sanctions / social context hypothesis (See Table 2 - 6). When this hypothesis is conceptualized as aggregate stakes-in-conformity, the hypothesis is that the interaction of more severe sanctions and neighborhoods with greater stakes-in-conformity will result in lower rates of repeat offending. In the NIJ Final Report, the sanctions / social context hypothesis was tested 18 times. In thirteen of those tests there were no statistically significant effects. In five of those tests the statistically significant effects are

positive, that is, contrary to the sanctions / stakes version of this hypothesis. In the *JQC* article, the reported results for the sanctions / social context hypothesis are only a little more supportive. When the social context is residential stability, two of four tests show that more severe sanctions are associated with lower rates of rearrest. A third test of residential stability with the less severe sanction--no charges filed–also has statistically significant lower rates of repeat offending than the more severe sanction of being dismissed or acquitted. This finding contradicts the social context hypothesis when conceived of as an aggregate stake in conformity. None of the four tests of the Social and Economic Status measure of social context show statistically significant effects.

In the more general version of the sanctions / social context, any statistically significant effect confirms the hypothesis that social context affects the rate of repeat offending. In this conceptualization, these two publications provide support in eight out of twenty-six tests.

Statistical Controls

In these four publications, Wooldredge and Thistlethwaite utilize eight different variables as statistical controls. We display the findings from the models testing the direct effect of sanctions in Table 2 - 7. The results show rather consistent effects. In nine separate analyses, the 1) sex of the offender, 2) prior convictions for violence misdemeanors, and 3) the existence of pending charges at the time of arrest are consistent predictors of higher rates of repeat offending. The age of the offender is a consistent predictor of lower rates of repeat offending. The race of the offender is a consistent non-predictor in six tests and substance abuse at the time of the arrest is a consistent non-predictor in three tests. Living with your partner at the time of arrest was a statistically significant predictor of increased repeat offending in eight of nine statistical models.

The only statistical control with inconsistent effects was prior incarceration for a non-domestic violence offense–two tests of this measure of prior criminal record show no effects and

two show more repeat offending.

Summary of Findings Reported by Wooldredge and Thistlethwaite

In four publications, Wooldredge and Thistlethwaite provide complex, multivariate and multi-level analyses that bear directly on seven hypotheses about the effects of sanctions, about the effects of stakes-in-conformity, and about the effects of social contexts on repeat violence between intimate partners. The predominant finding for all seven of these hypotheses is that there are no statistically significant effects--29 out of 44 tests of three sanction hypotheses show no effect, 11 out of 22 tests of stakes-in-conformity show no effect, and 12 out of 19 tests of social context show no effect. Similarly, 9 out of 12 tests of the hypothesis that the effects of sanctions are conditioned by an offender's stakes-in-conformity show no statistically significant effect. Thirteen out of 22 tests about whether sanctions are conditioned by social context also generated no statistically significant effects. Each of the seven hypotheses found some support from at least some statistical tests but some of the statistically significant effects reported are contrary to the predicted direction.

These analyses are all derived from the same data collected from Hamilton County, Ohio during the mid-1990s, but the findings they report in these four publications vary for the effects of sanctions, vary for the effects of stakes-in-conformity and vary on the effects of social context[9]. With no clear result for any of these hypotheses, the published findings of this research cannot provide a sound basis for testing theories of crime control or for evaluating policies about the appropriate level of criminal sanctions for intimate partner violence.

[9]The only consistent findings in these four publications are the effects of the statistical controls for the age, race, and sex of the offender, the offender's record of violence misdemeanors and the existence of pending charges at the time of arrest (See Table 2 - 7).

Methodological Issues

Even if the findings from the four Wooldredge and Thistlethwaite reports were entirely consistent on one or all of the sanction, stakes, and social context hypotheses, the utility of these analyses for improving our understanding of theories and policies will be limited by several measurement and methodological issues. These are the same five issues that we identified in Chapter 1–the measurement of repeat offending, missing data, statistical power, the measurement of treatment effects, and separating treatment from selection effects.

Measurement of Repeat Offending

All of the Wooldredge and Thistlethwaite analyses are based on official records of arrests. These records are unable to capture offenses that are not reported to the police and they do not capture offenses that are reported but do not result in an arrest and an arrest record. Despite these limitations, arrest records are a common measure used in domestic violence research (and criminological research in general) because they can be a valid indicator or subsequent offending and because they are often more consistently available for all research subjects. For instance, some of the SARP arrest experiments used arrest records as a measures of repeat offending along with offense records and interviews with victims (Garner, *et al.* 1995).

Beyond this familiar limitation for arrest records, there are two other salient aspects of the outcome measures used by Wooldredge and Thistlethwaite. First, their measures of repeat offending capture events that occur after the completion of the criminal sanction process and this varies for cases with different types of dispositions. For instance, the time at risk for repeat offending begins after an offender completes his sentence to jail, probation or a treatment program, but the time at risk for repeat offending starts at arrest for cases where no charges are filed. For those arrestees who were acquitted at trial or for whom charges were dismissed or

never filed, the time at risk for repeat offending begins the day the charges were dropped or the trial ends.

The second salient aspect of Wooldredge and Thistlethwaite's measure of repeat offending is that seven out of nine statistical models used in these publications are based on a reduced sample of 3,110 cases. In testing the effects of sanctions, stakes, and social contexts on the prevalence and frequency of repeat offending, Wooldredge and Thistlethwaite restrict their analyses to cases where the time at risk following completion of the criminal sanction is at least 24 months.

These two aspects of their measures of repeat offending have strengths and weaknesses. Beginning the time at risk at the termination of the criminal sanction is an appropriate approach when the focus of the analysis is on studying the effects of prosecution and criminal court processing and controlling for the time at risk has long been recommended for the analysis repeat offending (Sechrest, *et al.*, 1979; Maltz. 1984). On the other hand, limiting the measurement of repeat offending ignores the more immediate effects of earlier stages of the criminal case processing (e.g. arrest, prosecution, conviction) on cases which result in often lengthy sentences. In addition, it is a common finding that repeat offending prior to case disposition can affect the actual disposition of the pending case and the likelihood of subsequent repeat offending. Wooldredge and Thistlethwaite's analyses do not address these issues. Similarly, excluding cases that do not have 24 months time at risk at the end of the data collection period causes Wooldredge and Thistlethwaite to drop 552 cases from most of their analyses. Wooldredge and Thistlethwaite do not report any differences in the samples of 3,110 and 3,662, but it is more likely that convicted cases with longer sentences would be excluded than cases with no charges filed or charges dropped.

These two aspects of measuring repeat offending--the beginning of the time at risk and the dropping of 552 cases--combine to weaken the statistical analyses reported in Wooldredge and Thistlethwaite's analyses of the prevalence and frequency of repeat offending.

Missing Data

One of the common aspects of social science research is the presence of missing data. One of the limitations of the reports by Wooldredge and Thistlethwaite is that they do not report any missing data for any individual level or aggregate level measures. Given the nature of the official reports used in this data collection, the presence of no missing data is unlikely. Wooldredge and Thistlethwaite appear to have addressed this issue but they do not report the extent of missing data or how they addressed this issue in their analyses.

Measurement of Sanction Effects

Wooldredge and Thistlethwaite report seven possible dispositions for the 3,662 domestic violence arrests in their data: 1) no charges filed, 2) charges filed but dismissed, 3) acquitted, 4) treatment program, 5) probation, 6) jail, and 7) probation and jail. Research on court dispositions often report multiple possible categories and often include categories for deferred prosecution and unknown case dispositions. There does not seem to be any set categories for disposition or how those categories are measured and reported in court research.

As displayed in table 2 - 2, each of their four publications combines these seven categories in different ways to produce statistical tests of the effects of sanctions. The NIJ Final Report uses all seven categories with no charges filed as the reference group or omitted category[10]. The *JQC*

[10]Wooldredge and Thistlethwaite use dichotomous variables for each mutually exclusive sanction category and each of these variables is compared to the intercept of the statistical model being tested. The cases in the reference or omitted category are included in the intercept.

article combines cases in the dismissed and acquitted categories and uses this combine group as the omitted category. The *Criminology* article uses the same reference group but combines cases sentenced to a program, probation or jail into a single category of convicted cases. The *Crime and Delinquency* article uses the same seven categories as the NIJ Final Report but uses the dismissed cases as the reference group.

Each of these four approaches are legitimate approaches to testing sanction effects and each of these reports provides appropriate justification for the approach taken in that report. That these four publications employ four different approaches using the same data highlights a common problem with the analysis and interpretation of sanction effects. For instance, in the NIJ Report the effect of the treatment program is actually the difference between the treatment program and the reference group for that model, which is the group of cases for which no charges were filed. However, the effect of the treatment program in the *JQC* article is the difference between the treatment program and the new reference group of dismissed and acquitted cases. In the *Criminology* article, the treatment group is combined with all other convicted cases and in the *Crime and Delinquency* article the effect of the treatment program is the contrast between the treatment program and just the dismissed cases.

Table 2 - 2 helps to clarify that the lack of a consistent comparison group in these four publications and the difficulty in interpreting sanction effects in these studies and in all studies of sanction effects. It is inappropriate to interpret any of these statistical findings as the effect of the treatment program or probation or jail. It is more appropriate to interpret these findings as the effects of the treatment program compared to no charges filed or the effects of the treatment program and case dismissal. We should speak of the effects of "treatment compared to no charges" or the effects of "treatment compared to dismissal." This issue is compounded by the

use of dichotomous variable for each sanction, instead of a single variable with multiple categories because the reference group is not just the omitted sanction category but all other cases included in the intercept term in the particular statistical model being used. Thus, the comparison group is not just the omitted sanction category but depends on the other variables in the statistical model being tested. We have seen that the number and nature of the variables included in the models used in the four Wooldredge and Thistlethwaite publications vary and this adds to the difficulties in interpreting the nature of sanction effects reported here and in all other studies of criminal sanctions.

Selection Effects Versus Treatment Effects

Wooldredge and Thistlethwaite address the difficulty of attributing cause and effect in their non-experimental research design (Wooldredge and Thistlethwaite, 1999: p. 92; 2002: p. 51; 2005: p. 82) and reference the partial solution of eliminating spuriousness obtained from using multivariate analyses. Their use of multivariate and multi-level analyses strengthens their claims that the reported results are, in fact, not easily attributed to spurious correlations. There is a related difficulty which may not be well addressed by traditional multivariate analyses–the selection of high risk offenders for more serious sanctions.

One of the reasons that an experimental design was selected in testing the crime control effects of arrest for domestic violence was the concern that suspects arrested by the police were more likely to be a higher risk for repeat offending than suspects who were not arrested (Sherman and Berk, 1984; Garner and Maxwell, 2000). Non experimental multivariate analyses address but do not eliminate this concern. The concern for this issue is heighten by the more deliberative process and the relative infrequency of prosecution, conviction and sentencing to severe sanctions. If prosecutors and courts make at least some part of their decisions to proceed with

criminal sanctions based on an assessment of the offender's potential for future repeat offending, it is possible that the association between sanctions and repeat offending is, at least in part, a selection effect. Under these conditions, higher risk offenders are given more severe sanctions. Unless this heightened risk is addressed by experimental or mutlivariate research designs, the associations between sanction categories and repeat offending reported by Wooldredge and Thistlethwaite (and other researchers) will be some unknown combination of the effect of selecting higher risk offenders and the effect of sanctions on offenders. For instance, in the *Crime and Delinquency* article, both the reduced repeat offending for the probation category and the increased repeat offending for the combined jail--probation category could be reasonably interpreted as either a selection effect or a treatment effect. For this reason, selection effects have not been eliminated as an alternative explanation for Wooldredge and Thistletwaite's reported findings, or for any of the findings reported in prior research on the crime control effects of sanctions for intimate partner violence.

Summary of Wooldredge and Thistlethwaite's Contributions

Wooldredge and Thistlethwaite have produce four diverse and rigorous analyses of the association between criminal sanctions and repeat offending. These analyses have provided detailed tests of seven hypotheses about the effects of sanctions, stakes-in-conformity and social context testing multivariate and multi-level models using a large sample of arrests. The predominate findings from these publications show no statistically significant effects for most of the tests of criminal sanctions, stakes-in-conformity or social contexts. While there is some empirical support for each of the seven hypotheses, some statistically significant effects are contrary to the predicted direction of the effects. The measures and methods used by Wooldredge and Thistlethwaite meet and exceed many contemporary standards for scientific research. These

four documents warrant continued attention as major contributors to our understanding of the effects of prosecuting intimate partner violence; however, the diverse findings they report and the methodological limitations we have identified limit the value of this research for testing theories or evaluating public policy.

Table 2 - 1: Frequency of Criminal Sanctions In Wooldredge and Thistlethwaite's Analyses

	Prevalence & Frequency Models		Time to Failure Model	
Case Dispostions	N = 3,110		N= 3662	
	#	%	#	%
No Charges Filed	224	7.2%	224	6.1%
Charges Dropped	1,649	53.0%	1,649	45.0%
Acquitted at Trial	235	7.6%	235	6.4%
Offender Program Only	246	7.9%	246	6.7%
Probation	461	14.8%	914	25.0%
Jail	229	7.4%	283	7.7%
Probation and Jail	66	2.1%	111	3.0%
Dispostions Used in JQC Article	N = 3,110			
Probation/Jail without Program	404	13.0%		
Probation/Jail with Program	373	12.0%		
Dispostions Used in Criminology Article	N= 1,855			
No Charges Filed	130	4.2%		
Convicted	575	18.5%		

Table 2 - 2: Findings about Criminal Sanctions

Publication		Types of Sanctions Imposed						
							Severe Sentence	
					Convicted			
NIJ Final Report			Prosecuted					
Samples	**Statistical Model**	**No Charges Filed**	**Dismissed**	**Acquitted**	**Program**	**Probation**	**Jail**	**Probation/Jail**
3,110	Prevalence	**Reference Group**	No Effect	No Effect	**Less**	No Effect	No Effect	No Effect
3,110	Frequency	**Reference Group**	No Effect	No Effect	**Less**	No Effect	No Effect	No Effect
3,662	Time to Failure	**Reference Group**	No Effect	No Effect	**Less**	No Effect	No Effect	No Effect

Journal of Quantitative Criminology, 2002

						Probation and/or Jail	
Samples	**Statistical Model**	**No Charges Filed**	**Dismissed**	**Acquitted**	**Program**	**Program**	**No Program**
3,110	Prevalence	**More**	**Reference Group**		No Effect	No Effect	No Effect

Criminology, 2002

Samples	**Statistical Model**	**No Charges Filed**	**Dismissed**	**Acquitted**	**Program**	**Probation**	**Jail**	**Probation/Jail**
1855 / 126	Prevalence-Tracts	**More**	**Reference Group**		No Effect			
1855 / 48	Prevalence-Neighborhoods	**More**	**Reference Group**		No Effect			

Crime and Delinquency, 2005

Samples	**Statistical Model**	**No Charges Filed**	**Dismissed**	**Acquitted**	**Program**	**Probation**	**Jail**	**Probation/Jail**
3,110	Prevalence	**More**	**Reference Group**	No Effect	No Effect	**Less**	No Effect	**More**
3,110	Frequency	**More**	**Reference Group**	No Effect	**Less**	**Less**	No Effect	**More**
3,662	Time to Failure	**More**	**Reference Group**	No Effect	No Effect	**Less**	No Effect	No Effect

Key to Cell Entries	**More - Statistically Significant Increases in Repeat Offending Reported**
	Less - Statistically Signficant Decreases in Repeat Offending Reported
	No Effect - No Statistically Significant Differences in Repeat Offending Reported
	Not Tested - This variable not included in this analysis

Table 2 - 3: Findings About Stakes in Conformity

Publication		General Factor Score					
NIJ Final Report		Five Year Resident	Education Factor		Economic Factor		
Samples	**Statistical Model**		High School	College	Employed	Skilled Employment	Not Receiving Public Assistance
3,110	Prevalence	**Less**					
3,110	Frequency	**Less**					
3,662	Time to Failure	**Less**					
Journal of Quantitative Criminology, 2002							
Samples	**Statistical Model**	Five Year Resident	High School	College	Employed	Skilled Employment	Not Receiving Public Assistance
3,110	Prevalence	**Less**	No Effect		No Effect		
Criminology, 2002		**Measures Not Conceptualized as Stakes in Conformity**					
Samples	**Statistical Model**	Five Year Resident	High School	College	Employed	Skilled Employment	Not Receiving Public Assistance
1855 / 126	Prevalence-Tracts	No Effect	**Less**	No Effect	**Less**	No Effect	No Effect
1855 / 48	Prevalence-Neighborhoods	No Effect	**Less**	No Effect	**Less**	No Effect	No Effect
Crime and Delinquency, 2002		**Measures Not Conceptualized as Stakes in Conformity**					
Samples	**Statistical Model**	Five Year Resident	High School	College	Employed	Skilled Employment	Not Receiving Public Assistance
3,110	Prevalence	**Less**	Not Tested	No Effect	Not Tested	Not Tested	Not Tested
3,110	Frequency	**Less**	Not Tested	No Effect	Not Tested	Not Tested	Not Tested
3,662	Time to Failure	**Less**	Not Tested	No Effect	Not Tested	Not Tested	Not Tested

Key to Cell Entries	
	More - Statistically Significant Increases in Repeat Offending Reported
	Less - Statistically Signficant Decreases in Repeat Offending Reported
	No Effect - No Statistically Significant Differences in Repeat Offending Reported
	Not Tested - This variable not included in this analysis

Table 2 - 4: Findings About Social Contexts

Publication					
NIJ Final Report		**Social Contexts**			
Samples	**Statistical Model**	General Factor Score			
3,110	Prevalence	**Less**			
3,110	Frequency	**Less**			
3,662	Time to Failure	No Effect			
Journal of Quantitative Criminology, 2002					
		Social Contexts			
Samples	**Statistical Model**	Same Residence	Social/Economic		
3,110	Prevalence	**Less**	**Less**		
Criminology, 2002		**Social Contexts**			
Samples	**Statistical Model**	Same Residence	Social Class	Proportion Males	Mean Age
1855 / 126	Prevalence-Tracts	No Effect	**Less**	No Effect	**Less**
1855 / 48	Prevalence-Neighborhoods	No Effect	No Effect	No Effect	No Effect
Crime and Delinquency, 2002		**Social Contexts**			
Samples	**Statistical Model**	Same Residence	College Education		
3,110	Prevalence	No Effect	**Less**		
3,110	Frequency	No Effect	**Less**		
3,662	Time to Failure	**Less**	**Less**		

Key to Cell Entries	**More - Statistically Significant Increases in Repeat Offending Reported**
	Less - Statistically Signficant Decreases in Repeat Offending Reported
	No Effect - No Statistically Significant Differences in Repeat Offending Reported
	Not Tested - This variable not included in this analysis

Table 2 - 5: Findings About Stakes / Sanction Hypothesis

Publication

NIJ Final Report			Stakes = General Factor Score					
			Sanctions					
Samples	Statistical Model	No Charges Filed	Dismissed	Acquitted	Program	Probation	Jail	Probation/Jail
3,110	Prevalence	Reference	No Effect	No Effect	**Less**	No Effect	No Effect	**Less**
3,110	Frequency	Reference	No Effect	No Effect	**Less**	No Effect	No Effect	**Less**
3,662	Time to Failure	Reference	No Effect	No Effect	No Effect	No Effect	No Effect	**Less**

Journal of Quantitative Criminology, 2002						
Sample	Statistical Model	No Charges Filed	Dismissed or Acquitted	Program	Probation / Jail without Program	Probation / Jail with Program
3,110	Prevalence					
Stakes	Same Residence	No Effect	Reference	**Less**	No Effect	No Effect
	Education Factor	No Effect	Reference	**Less**	No Effect	No Effect
	Economic Factor	No Effect	Reference	No Effect	**More**	No Effect

Key to Cell Entries	
	More - Statistically Significant Increases in Repeat Offending Reported
	Less - Statistically Signficant Decreases in Repeat Offending Reported
	No Effect - No Statistically Significant Differences in Repeat Offending Reported
	Not Tested - This variable not included in this analysis

Table 2 - 6: Findings about Sanctions / Social Context Hypothesis

NIJ Final Report, 1999							
		Sanctions					
		No Charges Filed	Dismissed	Acquitted	Program	Probation	Jail	Probation/Jail
Prevalence		Reference	No Effect	No Effect	No Effect	No Effect	**More**	**More**
Frequency		Reference	**More**	No Effect	No Effect	No Effect	**More**	**More**
Time to Failure		Reference	No Effect	No Effect	No Effect	No Effect	No Effect	**More**

Journal of Quantitative Criminology, 2002							
Prevalence		Sanctions					
	Social Context	No Charges Filed	Dismissed	Acquitted	Program	Probation/Jail	Probation/Jal/Program
	Same Residence	**Less**	Referrence		No Effect	**Less**	**Less**
Social Economic Status		No Effect	Referrence		No Effect	No Effect	No Effect

Key to Cell Entries	**More - Statistically Significant Increases in Repeat Offending Reported**
	Less - Statistically Signficant Decreases in Repeat Offending Reported
	No Effect - No Statistically Significant Differences in Repeat Offending Reported
	Not Tested - This variable not included in this analysis

Table 2 - 7: Findings About Statistical Controls

Publication		Statistical Controls Used							
NIJ Final Report				Convictions for Violent Misdemeanors	Incarcerated for Non-D.V. Offense	Not Living With Partner at Arrest	Pending Charges	African-American	Substance Abuse
Samples	**Statistical Model**	**Male**	**Age**						
3,110	Prevalence	**More**	**Less**	**More**	No Effect	No Effect	**More**	Not Tested	Not Tested
3,110	Frequency	**More**	**Less**	**More**	**More**	**Less**	**More**	Not Tested	Not Tested
3,662	Time to Failure	**More**	**Less**	**More**	No Effect	**Less**	**More**	Not Tested	Not Tested
Journal of Quantitative Criminology, 2002				Convictions for Violent Misdemeanors	Incarcerated for Non-D.V. Offense	Not Living With Partner at Arrest	Pending Charges	African-American	Substance Abuse
Samples	**Statistical Model**	**Male**	**Age**						
3,110	Prevalence	**More**	**Less**	**More**	**More**	**Less**	**More**	No Effect	Not Tested
Criminology, 2002				Convictions for Violent Misdemeanors	Incarcerated for Non-D.V. Offense	Not Living With Partner at Arrest	Pending Charges	African-American	Substance Abuse
Samples	**Statistical Model**	**Male**	**Age**						
1855 / 126	Prevalence-Tracts	**More**	**Less**	**More**	Not Tested	**Less**	**More**	No Effect	Not Tested
1855 / 48	Prevalence-Neighborhoods	**More**	**Less**	**More**	Not Tested	**Less**	**More**	No Effect	Not Tested
Crime and Delinquency, 2002				Convictions for Violent Misdemeanors	Incarcerated for Non-D.V. Offense	Living with Partner	Pending Charges	African-American	Substance Abuse
Samples	**Statistical Model**	**Male**	**Age**						
3,110	Prevalence	**More**	**Less**	**More**	Not Tested	**More**	**More**	No Effect	No Effect
3,110	Frequency	**More**	**Less**	**More**	Not Tested	**More**	**More**	No Effect	No Effect
3,662	Time to Failure	**More**	**Less**	**More**	Not Tested	**More**	**More**	No Effect	No Effect

Key to Cell Entries	
	More - Statistically Significant Increases in Repeat Offending Reported
	Less - Statistically Signficant Decreases in Repeat Offending Reported
	No Effect - No Statistically Significant Differences in Repeat Offending Reported
	Not Tested - This variable not included in this analysis

Chapter 3: The Design of this Research

We have reviewed the prior research on the crime control effects of prosecution intimate partner violence and identified the contribution of four publications by Wooldredge and Thistlethwaite in that body of research. The basic design of this research is to use the archived data from this project to build on the contributions of Wooldredge and Thistlethwaite. We use the available data to evaluate their research samples, to determine the extent to which we can reproduce their published findings about the effects of sanctions, stakes and social context, and to extend their analyses by addressing some of the methodological limitations identified in Chapters 1 and 2.

The Rationale for Reproducing Wooldredge and Thisltethwaite

The core of this project is the reproduction of the published descriptions and analyses of Wooldredge and Thistlethwaite using the archived data. Reproduction of published findings by independent researchers is one of the expectations of the solicitation that funded this project (NIJ, 2005), the establishment of the NIJ Data Resources Program in 1976 (Garner, 1981), and contemporary standards for research quality recognized by the National Academy of Sciences (Feinberg, *et al.* 1985). Moreover, in contemporary social research, the cost of data collection far exceeds the cost of analyzing data and disseminating research findings and one of the goals of the National Institute of Justice Data Resources Program is to increase the number and quality of analyses that can be produced with the limited financial resources available.

Secondary data analysis (Hyman, 1972; Bryant and Wortman, 1978; Boruch, *et al.*, 1981; Cordray and Orwin, 1983) is a method that uses some or all of the raw quantitative data from one or more prior studies to reproduce and perhaps build upon the originally reported analyses. Secondary data analysis is commonplace in the field of criminology. For instance, of the 20

articles in the last three issues of the premier criminological journal *Criminology* (November 2006 through May 2007), 18 involved quantitative data analysis. Only three of those articles involved new data collection. The other 11 data analysis articles were secondary analyses of previously collected and previously analyzed data.

Research that explicitly involves the reproduction of prior analyses is less common, especially in scientific journals. In the field of criminology, published examples of reproductions include Blumstein, *et al.*'s (1983) critique of Carlson, *et al.* (1980) assertions about the effect of prison capacity on prison population and Visher's (1986) re-analysis of Chaiken and Chaiken's (1982) and Greenwood's (1982) inmate surveys identified significant limitations that challenged the validity of the original estimates of offender crime commission rates and the incapacitative effects of imprisonment. More recently, errors identified by McCrary (2002) demonstrated that Levitt's (1997) assessment that increases in the number of police officers substantially reduced crime disappeared when those errors were corrected. On the other hand, Vandaele's (1978) reproduction of Erhlich's (1973) analysis of the deterrent effects of the criminal sanctions confirmed the original author's calculations. Sampson and Laub's (1993) multivariate analysis of the data collected by the Gluecks (1950) upheld many of the findings the Gluecks obtained through bivariate analyses. In each case, these published reproductions contributed to our detailed understanding and appreciation of the original analyses as well as to our general understanding of the underlying theoretical and policy issues and these are our goals with this research.

In this chapter, we describe the procedures we used 1) to obtain the archived data, 2) to reproduce the reported tests of the sanction, stakes, and social context hypotheses, and 3) to design and conduct revised tests of these hypotheses.

Accessing Available Data Files

The data and data documentation for Wooldredge and Thistlethwaite's research was released by the National Archive of Criminal Justice Data in 2000 (Wooldredge, 2000) and can be accessed on the NACJD web page following completion of a 'Terms of Use" agreement. Although these data include no direct identifiers of research subjects or dates of criminal justice processing events, they do include information on the research site, the dates the data collection began and ended and some potential personal identifiers, such as age, race and sex of victims and offenders. For this reason, the Joint Centers adopted a set of procedures to store and use these data that precluded unauthorized individuals from using the downloaded data or any research product to identify research subjects. These procedures were reviewed and approved by the Institutional Review Boards at the University of Michigan and the Joint Centers for Justice Studies, Inc. (See Appendix 2: IRB Approvals)

Detailed Comparison of Methods and Measures

Our original plan was to reproduce only the most recent of the Wooldredge and Thistlethwaite product but the diversity in these four publications helped illuminate the specific characteristics of each of these analyses in ways that might not have been possible had Wooldredge and Thistletwaite produced only one or two reports. This review involved repeated reading and close inspection of each document and ultimately, the production of the tables in Chapter 2 comparing and contrasting the multivariate analyses reported by Wooldredge and Thistlethwaite. Reading the data documentation and working with the archived data files also revealed aspects of the sampling, measurement and analyses that were not immediately obvious in reading the published reports alone. An important interim product was the articulation of the seven distinct hypotheses set out in Chapter 1 and the use of those hypotheses to structure the

tables in Chapters 1 & 2.

Reproducing Wooldredge and Thistlethwaite

While examples of secondary analyses and reproductions abound, we could find little in the way of textbooks or descriptions of what such efforts would and would not entail. While secondary analyses that involve the investigation of entirely new hypotheses would have their own internal logic, we determined that a design for reproducing published analyses requires, at a bare minimum, a detailed understanding of the published analyses, a detailed understanding of the archived data, and a matching of the archived data with the methods and measures used to generate the published findings. Since we are reproducing several related and overlapping analyses of the same data, the first part of our design is accomplished by the detailed literature review of the four Wooldredge and Thistlethwaite publications reported in Chapter 2.

The second element of our design required us to gain a detailed understanding of the archived data using the documentation provided by Wooldredge (2000) and disseminated by the National Archive of Criminal Justice Data. Our design called for identifying in the data and the data documentation the specific variables used in each of the four Wooldredge and Thistlethwaite publications. Because neither the reports nor the documentation involved the programs used to translate the raw data into published descriptions and analyses, the initial effort to determine the variables used involved 1) matching, as close as possible, the names used in publications and in the documentation and 2) matching the frequency counts of those variables in the publications and the raw data. In addition, Wooldredge and Thistlethwaite construct a variety of composite variables based on combining data from several variables in the archived data. None of the composite variables were included in the archived data and they needed to be reconstructed using the descriptions provided in the four publications.

The third element of our secondary analysis research design involved using the identified variables to reproduce the multivariate empirical findings about the effects of sanctions, stakes and social context on repeat offending. These findings were presented in a series of tables (Wooldredge & Thistlethwaite, 1999: Tables 6 - 8, pp. 71 - 75; Wooldredge and Thistlethwaite, 2002: Table III, pp. 58 - 59; Wooldredge, 2002: Table 5, p. 689; Wooldredge & Thistlethwaite, 2005: Table 2, p. 86 and Table 3, p. 88.). These findings are reported consistently as unstandardized regression coefficients each with associated standard errors and the use of an asterisk as an indicator of statistical significance. This tabular format simplifies creating a table by table, variable by variable comparison of our findings with their findings.

This was an iterative process. These three elements needed to be repeated several times. Details found in the data documentation and preliminary efforts to reproduce the descriptive or analytical tables often led to a closer reading of the published documents which led to new data analyses. The closer our re-analyses were able to match the descriptive and analytical finding, the more confidence we gained that we had, in fact, used the same variables and the same statistical methods as the original authors. After numerous iterations of reading reports and documentation and exploring alternative measures and methods, we produced a report detailing our ability to reproduce their descriptive measures. A copy of this report was sent to John Wooldredge and our effort was greatly and generously assisted by personal, phone and written communications with him. Collaboration with the original investigators[11] is a common element reported by previously published efforts reproducing criminological research (e.g., Blumstein, *et al.*, 1983; Vandaele,

[11]One legitimate complaint from producers of data is that the would-be secondary analysts make repeated requests for personal help and assistance on trivial matters and without investing the time and effort to gain a solid understanding of the archived data or the original analyses (Elliott, 1990).

1978).

We present the results of our efforts in a series of tables that display the extent to which we were able to reproduce the multivariate and multi-level analyses presented by Wooldredge and Thistlethwaite. While their consistent use of unstandardized regression coefficients and standard errors simplifies this presentation, our design called for using explicit criteria for determining the extent to which Wooldredge and Thistlethwaite's findings could be reproduced. We developed and applied three criteria for making that determination. The first is a simple comparison of the regression coefficients and standard errors. The second criterion is a determination of whether the reproduced results conform to the direction and statistical significance levels of the original analyses. The third criterion is to apply a statistical test to assess the significance of any differences in the sizes of original and reproduced coefficients.

There are strengths and weaknesses to using each of these three criteria for reproducibility. The rationale for comparing raw coefficients is based on the understanding that reproducibility is a mechanical process of applying exactly the same data using exactly the same statistical procedures, as if the original investigator had merely run the analyses twice. Wooldredge and Thistlethwaite report their findings in thousands and this level of precision may be artificial if the standard for reproducibility is exactness to the third decimal point. On the other hand, it is commonplace in social research to accept as consistent multivariate findings that are in the same direction and meet or exceed the traditional $p < .05$ level of statistical significance. Thus, these criteria seem appropriate in judging whether findings from a reproduction warrant changing our assessment about the direction and statistical significance of the original findings. Given the arbitrary nature of the .05 standard and the minor differences in coefficients or standard errors which could change the original findings, these criteria retain and perhaps amplifies the

limitations of the arbitrary cut point of $p < .05$ in frequentist (non-Bayesian) statistics.

The strength of our third criteria is that it brings statistical theory to bear on a judgment about whether the reported findings by the original investigators and the secondary analyst are in fact different from zero. We adopted a test proposed by Clogg, *et al.* (1995) to determine if the introduction of a new statistical control affects the reported relationship between two variables in a multivariate analysis. Paternoster, *et al.* (2002) use the same test to determine whether the relationship between two variables does or does not vary in separate analyses from independent samples. These criteria for reproducibility assume that the reproduction process is more stochastic than mechanical and that the important variable of interest is the unstandardized regression coefficient.

Our approach to this issue is to use the archived data and descriptions of their analyses provided by Wooldredge and Thistlethwaite to reproduce their multivariate analyses of the effects of criminal sanctions, stakes-in-conformity and social context on repeat offending. We report unstandardized regression coefficients and standard errors for both the original and secondary analyses and report for each variable how well they fare on all four measures of reproducibility. We use the consistency with which we are able to reproduce Wooldredge and Thistlethwaite's findings and the substantive consistency among the secondary analyses we produced to determine the impact of sanctions, stakes and social context on repeat violence between intimate partners.

Revised Tests of Sanction Effects

The data archived by Wooldredge (2000) provide seven dichotomous measures of criminal sanctions. Each of the four publications by Wooldredge and Thistlethwaite structure the test of criminal sanctions in unique ways (See Table 2 - 2). For instance, in the 1999 NIJ Final Report, they compare the group of cases for which no charges were filed with each of the six

other possible sanction outcomes–dismissed, acquitted, a treatment program, probation only, jail only, and a combination of probation and jail. The 2002 *JQC* article uses the dismissed and acquitted cases as the reference group. The 2002 *Criminology* article also uses the dismissed and acquitted cases as the reference group but combines the program, probation, and jail categories into a single conviction category. The 2005 *Crime and Delinquency* article uses only the dismissed cases as the reference group which is compared with each of the six alternative sanctions.

Wooldredge and Thistlethwaite provide reasonable rationales for each of these approaches but do not directly address the difficulties created when their different approaches generate different findings. In addition, the sanction alternatives in Hamilton County, Ohio are not necessarily the options available in other jurisdictions and this limits the generalizability of using the specific measures in their statistical models. Moreover, none of their approaches provide an unambiguous test of any of our three sanction hypotheses. For these reasons, part of the design of this research is to go beyond reproducing their approaches and to reformulate the available measures of criminal sanctions to more directly test the prosecution, conviction and sentence severity hypotheses. We produce these tests by constructing three new measures of criminal sanctions–prosecution, conviction, and sanction severity--and testing each of them in separate multivariate models. This approach conceives of criminal sanctions as broad policy options that are common to many jurisdictions. This approach provides a more direct test of these three hypotheses and it facilitates the synthesis of research findings from one jurisdiction to another.

Revised Measure of Re-arrest

The archived data include summary measures of the prevalence and frequency of re-arrest up to 24 months and the months to first re-arrest up to 35 months. Because the first two measures

require dropping 15 percent of the sample of arrests, we emphasize the use of the time to first rearrest measure.

Revised Tests of Stake Effects

Wooldredge and Thistlethwaite provide more extensive tests of the interaction between criminal sanctions and stakes-in-conformity than any publication since Sherman and his colleagues examined their role in four of the five spouse assault arrest experiments (Sherman, *et al.* (1992; Berk, et al, 1992; and Pate and Hamilton, 1992). The original analyses involved dichotomous measures of employment and marriage which provided a relatively unambiguous test of the presence of a sanction and a stake. Wooldredge and Thisltethwaite measure stakes with interval level factor scores for education and employment, as well as a dichotomous measure of residential stability. Only the dichotomous measure of stakes provides the same unambiguous tests of the sanction - stakes hypothesis. In addition, Wooldredge and Thistlethwaite do not report the direct effects of marriage or the interaction of marriage with criminal sanctions and combine offender employment with other economic measures. The data archived by Wooldredge and Thistlethwaite include measures of marriage and employment and our design calls for the use of these measures of stakes-in-conformity to assess the extent to which the diverse findings are due to the statistical formulation or to differences in the sample and the sanctions used. These differences in formulating the sanctions / stakes hypothesis make it difficult to determine why the tests reported by Wooldredge and Thistlethwaite do not support and in one instance directly contradict the findings reported by Sherman, *et al.* (1992), Berk, *et al.*, (1992), and Pate and Hamilton, (1992). In addition, Wooldredge and Thistlethwaite do not report independent tests of each stake in conformity and do not include independent tests of individual level stakes variables

and aggregate level social context measures. We determined that reporting the direct and indirect effects of marriage and employment in separate models precludes potential interactions among distinct and potentially correlated measures of stakes-in-conformity and social context.

Separating Treatment Effects from Selection Effects

As part of our proposal to NIJ, we emphasized the problems of interpreting the findings of non-experimental research as causal effects. Wooldredge and Thistlethwaite also identify this issue as a caveat to interpreting their findings. This is a particularly problematic issue in assessing the effects of criminal court sanctions where it is commonplace to fully prosecute and severely sanction offenders who are presumed to be at higher risk for re-offending.

Our design for addressing this issue is to use the emerging statistical technique of propensity scores as a way to illustrate one approach to separating selection effects from treatment effects. Employing the available data archived by Wooldredge and Thistlethwaite, we construct a multivariate model predicting whether a convicted offender is sentenced to jail. We save the value of what this model predicts as a measure of the offender's propensity for being selected for a jail sanction. We interpret this measure as the jail selection effect. We employ a second test of treatment effects using propensity scores obtained by stratifying cases into five equally sized groups using the propensity score.

We recognize that these two methods to separating selection and treatment effects are relatively innovative approaches for the literature on sanctions for intimate partner violence and that the data archived by Wooldredge and Thistlethwaite were not designed for this purpose. However, we prefer these relatively uncharted waters to the unsatisfactory tradition of ignoring this problem or doing nothing about it.

Summary of Our Research Design

The purpose of this research is to improve our understanding of the conditions under which criminal sanctions do and do not reduce repeat violence between intimate partners. Using archived data from Wooldredge and Thistlethwaite study of Hamilton County, Ohio, we determine the extent to which the original analyses and findings can be reproduced and the extent to which the published findings hold up in statistical models using alternative measures of criminal sanctions and of stakes-in-conformity. In addition, we use alternative statistical methods to address the extent to which the published findings entail the effects of selecting higher risk .individuals for more severe sanctions or the effects of treatment on subsequent offending.

The Crime Control Effects of Prosecuting Intimate Partner Violence in Hamilton County, Ohio: Part Two: Reproducing Wooldredge and Thistlethwaite

In the second part of this report, we include four chapters which report the findings of our efforts to reproduce the published findings of Wooldredge and Thistlethwaite. Chapter 4 describes the results of reproducing the descriptive statistics presented in their four publications. Chapter 5 reports the results of our efforts to reproduce the multivariate and multi-level analyses of the effects of criminal sanctions on re-arrest.

Chapter 6 provides a similar report for our reproduction of their analyses about the effects of an offender's stakes-in-conformity, including interactions between an offender's stakes and the effectiveness of criminal sanctions. Chapter 7 is limited to reproducing the Wooldredge and Thistlethwaite's analyses about the role of social context including the interactions of social context and criminal sanctions.

At the end of Chapter 7 we summarize the results of our efforts to reproduce the published analyses of Wooldredge and Thistlethwaite and discuss the implications of our findings about the reproducibility of their findings.

Chapter 4: Reproducing Descriptive Statistics

Introduction

This chapter presents the results of our effort to reproduce both the descriptive statistics and the analytical findings about the effects of criminal sanctions, stakes-in-conformity and social context on repeat violence between intimate partners. These descriptive statistics and analytical findings were originally reported in four publications by Wooldredge (2002) and Wooldredge and Thistlethwaite (1999; 2002; 2005). While our substantive interest is in the reproducibility of the analytical findings, reproducing the descriptive statistics is, we think, an essential first step in this process. The extent to which we can and cannot reproduce the basic descriptive statistics provide the basis for understanding whether any imperfections in reproducing the analytical findings can be attributed to the archived data or to the analytical procedures used[12].

Reproducing Descriptive Statistics

We report the reproducibility of the descriptive statistics provided by Wooldredge and Thistlethwaite for their measures of repeat offending, criminal case processing, statistical controls, individual level stakes-in-conformity, and social context. In this section, we report the sample sizes used, simple frequency counts, and means listed in the published reports and our reproduction of them. In the following discussion, we identify those instances where the published statistics and the reproduced statistics do not match exactly.

The single data file archived by Wooldredge and Thistlethwaite in 2000 includes 60 variables about 3,662 arrests from Hamilton County. 3,662 is the largest sample used in their analyses. We were able to identify the primary subsample of 3,110 arrests but we were unable to identify precisely the sample of 1,855 arrests used in their analyses of neighborhood effects

[12]We would like to thank John Wooldredge for his assistance in reviewing these descriptive statistics. Any errors that may remain in this report are those of the authors.

within the city of Cincinnati. The archived data do not include identifiers for census tracts but they do include 12 variables from the 1990 U.S. census. From the values of these 12 variables, we generated 206 unique combinations that conform to 205 identifiable census tracts. From the values of the 12 census variables, we determined that 686 of the arrests did not come from any particular tract but were represented by the average values of the other 205 tracts.

From the 205 census tracts we identified, we could determine which arrests occurred within the city of Cincinnati and were included in Wooldredge's 2002 analyses of neighborhood effects. We determined that 2,177 of the arrests occurred in Cincinnati and that 1,891 of those arrests had re-arrest data for 24 months. Part of the difference between the 1,855 arrests used by Wooldredge (2002) and the 1,891 arrests we identified may stem from the fact that the data archived in 2000 did not include indicators for those arrests dropped from the 2002 analyses because the offender was known to have moved from one census tract to another (Wooldredge, 2002). We used a crosswalk between Cincinnati census tracts and neighborhoods provided in an appendix of Maloney and Buelow (1997) to link the arrest data to Cincinnati neighborhoods. In addition, two census tract variables used in the *Criminology* article--proportion male and mean age--were not included in archived data. Thus, in order to construct the census tracts and neighborhoods and obtain these two census variables, it was necessary to supplement the information provided in the data and documentation provided by the National Archive of Criminal Justice Data.

Table 4 - 1 displays the descriptive statistics for the prevalence, frequency and months to first repeat arrest for each of the four publications. The last three columns report the same measures generated from the archived data for each of the three individual level samples used in their various analyses. This table shows that sizes for various samples used as well as the

prevalence and frequency or re-arrest and the mean number of months to first re-arrest. We were able to reproduce the prevalence and frequency measures reported by the four Wooldredge and Thistlethwaite publications within rounding error. However, the archived data do not match exactly either the reported number of cases for which there was information about time to first re-arrest or the reported mean number of months to first re-arrest.

Wooldredge and Thistlethwaite's measures of repeat offending capture events that occur after the completion of the criminal sanction process and this varies for cases with different types of dispositions. For instance, the time at risk for repeat offending begins after an offender completes his sentence to jail, probation or a treatment program, but the time at risk for repeat offending starts at arrest for cases where no charges are filed. For those arrestees who were acquitted at trial or for whom charges were dismissed or never filed, the time at risk for repeat offending begins the day the charges were dropped or the trial ends.

Table 4 - 2 displays the descriptive statistics for measures of criminal case processing reported in the four publications by Wooldredge and Thistlethwaite. We were able to reproduce exactly all of these measures except for the measure of a short 5 day program reported in the 2002 *JQC* article. The archived data do not include information about this program. Table 4 - 2 also displays one of the major differences between the entire sample of 3,662 arrests and the sample of 3,110 arrests for which data on 24 months of repeat offending was available. Among the complete sample of 3,662 arrests, 25.0% were sentenced to probation. Among the 3,110 arrests with 24 months follow-up, only 15% were sentenced to probation[13]. In addition, the data archived by Wooldredge and Thistlethwaite only include one type of case disposition for each arrestee;

[13]Wooldredge and Thistlethwaite (1999: 42) report that 549 out of 3,662 offenders were sentenced to probation but this appears to have been reported in error.

however, their discussion of the treatment program indicates that some of those arrestees where prosecuted and convicted and some were not. Thus, the seven mutually exclusive categories of case disposition used by Wooldredge and Thistlethwaite[14] do not capture the multiple dimensions of the sanctions imposed on an arrestee in Cincinnati and is frequently the case in other jurisdictions

Table 4 - 3 reports the eleven variables used by the four Wooldredge and Thistlethwaite publications to represent an offender's stakes-in-conformity. For most of the variables, we were able to reproduce within rounding error the frequency counts and means reported; however, the archived data and the published findings in the NIJ Final report vary by 8 for high school degree, by 12 for employed, and for residential stability. Comparing the reproducibility of the three factor scores is a little more uncertain since, by definition, these scores have a mean of zero and a fixed standard deviation. Wooldredge and Thistlethwaite report the minimum and maximum of these factor scores, which are not constrained but we were unable to reproduce those values exactly. We are uncertain as to the import of these differences but the differences do emphasize the value of archiving not only the raw data collected but also all computed variables used in published analyses.

Table 4 - 4 displays the extent to which we can reproduce the reported frequencies for eight variables used by Wooldredge and Thistlethwaite as statistical controls. We found two instances where the differences between the reported data and the archived data varied beyond

[14]Because Wooldredge and Thistlethwaite report some offenders in the treatment program as having been convicted, in some analyses, we group the treatment program as a post-conviction sanction. The direction and statistical significance of our findings do not change whether the offenders in the treatment program are grouped with convicted or not convicted offenders.

rounding error. In the 2002 *Criminology* article, Wooldredge reports that 29% of the offenders were not living with their spouses but the data show that for the sample of 1,891 arrests that figure is 35.4%. In addition, Wooldredge and Thistlethwaite report that the percent of offenders with pending charges is 13% but their 2005 *Crime and Delinquency* article reports and the archived data show that the actual frequency for the 3,662 and the 3,110 samples is 7.2%.

Table 4 - 5 displays the twelve social context measures used by Wooldredge and Thistlethwaite. We were able to reproduce all six direct census measures within rounding error and to come close to the minimums and maximums of the three factors scores. However, Wooldredge did not include a variable for the census tract and we found that there were fewer census tracts and neighborhoods in the data than there were reported in the *Criminology* article. Although there are a total of 207 census tracts in Hamilton County, only 204 contributed at least one arrest to this study. Similarly, only 119 of the 126 tracts within the city of Cincinnati contributed arrests and 47 of the 48 neighborhoods. These differences appear to stem from their use of the total number of possible tracts and neighborhoods and our use of the number of tracts and neighborhoods actually contributing arrests.

Except for the data on the short five day program, the archived data include all the direct measures used by Wooldredge and Thistlethwaite and, in most instances, we could reproduce the reported descriptive statistics exactly or within rounding error. In a few instances, the archived data varied somewhat from the published statistics and there are one or two instances of what appears to be typographical errors in the published reports. In addition, we were able to identify census tracts and produce what appear to be reasonable approximations of various composite scores used by Wooldredge and Thistlethwaite.

Summary of Findings

The data archived by Wooldredge include most but not all of the variables used by in the four publications by Wooldredge and Thistlethwaite. Data were missing for one type of case disposition, three factor scores, census tract numbers and two census variables. However, we were able to construct close approximations of all but the case disposition variable and we feel that the archived data provided a sound basis for attempting to reproduce the multivariate analyses of Wooldredge and Thistlethwaite.

Measuring Re-arrest

The decision to limit many analyses to 3,110 arrests with 24 month follow - up data has at least one previously unidentified implication for the analysis of criminal sanctions. This methodological approach, designed to provide consistent times at risk, has the effect of changing the proportion of cases sentenced to probation from 25% to 15%. Of the 552 cases dropped in the sample of 3,110 arrests, 453 (82.1%) were sentenced to probation. In addition, Wooldredge (2000) archives only summary outcome variables and does not include any information on when the case disposition occurred or on any re-arrests between the initial arrest and when the case disposition was completed. Prior research has reported substantial proportions of pre-disposition re-offending and we would have preferred to control for those arrests in our analyses. Lastly, the time to disposition variable archived by Wooldredge (2000) is measured in months instead of days and this can create technical difficulties in the analyses of time to first rearrest (Maltz, 1984). For these reasons, we believe that it would have been preferable if the archived data had included the raw data on the dates of the initial arrest, the case disposition, and any subsequent

arrests.[15]

Missing Data

After identifying the 205 census tracts, we determined that a substantial proportion of aggregate level data is missing. In 698 (19.17%) arrests Wooldredge and Thisltethwaite's data do not identify the census tract of the arrest. In these cases, they replace the missing data for the 12 census variables with the average of all other census tracts. This is not an unreasonable approach to handling missing data but none of their four publications or the data documentation identify this aspect of their data. The issue of missing aggregate level data raises the issue of the lack of missing data in any of the individual level data. It is unlikely that any official criminal justice records are without missing data. For this reason, it is likely that Wooldredge and Thistlethwaite revised the raw data for at least some of the variables used in these analyses or they excluded cases with missing data from their samples. Both approaches can be appropriate ways to address missing data but the four Wooldredge and Thistlethwaite publications do not report the extent of missing data or how they addressed this problem. Since there are alternative procedures for handling missing data (Little and Rubin, 1987), it would have been better had they archived the variables with missing data as well as those variable where they imputed the values when the data were missing.

[15]The National Archive of Criminal Justice Data has established procedures to protect the confidentiality of research subjects and still allow researchers access to data on the dates of criminal justice events.

Table 4 - 1: Reproducing Outcome Measures

Outcome Measure	Publication				Findings from Archived Data		
	NIJ Final Report, 1999	Journal of Quantitative Criminology, 2002	Criminology, 2002	Crime & Delinquency, 2005			
Sample Size	3,110	3,110	1,855	3,662	3,662	3,110	1,891
Re-arrest for initimate assault (within 24 months)							
No	2,662				3,086	2,662	1625
Yes	448				576	448	266
Percent Yes	14.4%	14.0%	14.0%	16.0%	15.7%	14.4%	14.1%
Number of Re-arrests for Intimate Assault (within 24 months)							
None	2,662				2,662	2,662	
One	353				353	353	
Two	71				71	71	
Three or More	24				19	19	
Four					3	3	
Five					1	1	
Six					1	1	
Unknown					552	0	
Mean Number of Re-Arrests for Intimate Assault				0.19	0.185	0.185	
Mean Number of Months to First Re-Arrest							
Sample	589				567	504	
Mean	9.29			10.02	9.55	10.42	

Table 4 - 2: Reproducing Case Processing Variables

Case Processing Measures		Publication				Findings from Archived Data		
		NIJ, 1999	JQC, 2002	Crim., 2002	C & D, 2005			
Sample Size		3,662	3,110	1,855	3,662	3,662	3,110	1,891
Prosecuted & Convicted								
	No					2,108	2,108	1,304
	Yes			31.0%		1,554	1,002	587
						42.4%	32.2%	31.0%
No Charges Filed								
	No	3438				3,438	2,886	1,763
	Yes	224				224	224	128
		6.1%	7.0%	7.0%	6.0%	6.1%	7.2%	6.8%
Charges Dropped								
	No	2013				2,013	1,461	835
	Yes	1649				1,649	1,649	1,056
		45.0%			45.0%	45.0%	53.0%	55.8%
Acquitted at Trial								
	No	3427				3,427	2,875	1,771
	Yes	235				235	235	120
		6.4%			6.0%	6.4%	7.6%	6.3%
Offender Program Only								
	No	3415				3,415	2,864	1,754
	Yes	246				246	246	137
		6.7%	8.0%		7.0%	6.7%	7.9%	7.2%
Probation								
	No	3113				2,748	2,649	1,621
	Yes	549				914	461	270
		15.0%			25.0%	25.0%	14.8%	14.3%
Jail								
	No	3379				3,379	2,881	1,751
	Yes	283				283	229	140
		7.7%			8.0%	7.7%	7.4%	7.4%
Probation and Jail								
	No	3551				3,551	3,044	1,851
	Yes	111				111	66	40
		3.0%			3.0%	3.0%	2.1%	2.1%
Probation or Jail/Five Day Program	No		13.0%			No Data		
	Yes		12.0%					

Table 4 - 3: Measures of Stakes in Conformity

Stakes In Conformity		Publications				Findings from Archived Data		
		NIJ, 1999	JQC, 2002	Crim., 2002	C & D, 2005			
Sample Size		3,662	3,110	1,855	3,662	3,662	3,110	1,891
Married	No					2,453	2,113	
	Yes					1209	997	
						33.0%	32.1%	
High School Degree	No	502				494	466	321
	Yes	3,160				3,168	2,644	1570
		86.3%	85.0%	83.0%		86.5%	85.0%	83.0%
Bachelor's Degree	No	3,512				3,512	2,981	1825
	Yes	150				150	129	66
		4.1%	4.0%	4.0%	0.04	4.1%	4.1%	3.5%
Employed	No	1,574				1,586	1,353	727
	Yes	2,088				2,076	1,757	1164
		57.0%		62.0%		56.7%	56.5%	61.6%
Employed at Skilled Job	No	3,001				3,001	2,547	1541
	Yes	661				661	563	350
		18.1%	18.0%	19.0%		18.1%	18.1%	18.5%
Employed Fulltime	No					1,693	1,439	
	Yes					1,969	1,671	
			65.0%			53.8%	53.7%	
Not Receiving Public Assistance	No	654				654	549	392
	Yes	3,008				3,008	2,561	1499
		82.1%	82.0%	79.0%		82.1%	82.3%	79.3%
Same Residence 5 + Years	No	3,104				3,216	2,732	1669
	Yes	558				446	378	222
		15.2%	15.0%	10.0%	12.0%	12.2%	12.2%	11.7%
Stakes in Conformity: Individual Composite								
	mean	0				0	-0.039	
	S.D.	3				3.02	3.05	
	Range	-7.3 to 8.3					-6.87 to 11.39	
Education Factor								
	mean		0				0	
	S.D.		1				1	
	Range		-1.85 to 1.34				-1.97 to 4.01	
Economic Status Factor								
	mean		0				0	
	S.D.		1				1	
	Range		-1.9 to 1.7				-1.85 to 2.14	

Table 4 - 4: Reproducing Statistical Control Measures

Statistical Controls	Publication				Findings from Archived Data		
	NIJ Final Report, 1999	Journal of Quantitative Criminology, 2002	Criminology, 2002	Crime & Delinquency, 2005			
Sample Size	3,662	3,110	1,855	3,662	3,662	3,110	1,891
Male							
No	585				585	505	330
Yes	3,077				3,077	2,605	1,256
	84.0%	84.0%	83.0%	84.0%	84.0%	83.8%	83.0%
Average Age	32.2	32.1	31.9	32.2	32	31.9	31.8
African American							
No					1,489	1,250	635
Yes					2,173	1,860	1,256
	60%	60.0%	67.0%	59.0%	59.3%	59.8%	66.4%
Number of Convictions for Violent Crimes							
Mean				0.77	0.79	0.78	
Number of Convictions for Violent Misdemeanors							
Mean	0.56	0.55	0.51		0.57	0.57	0.54
Prior Incarcerations (Not Domestic Violence)							
No	2,395				2,395	2,015	
Yes	1,267				1,267	1,095	
	34.6%				34.6%	35.2%	
Not Living with Spouse or Childred at Arrest							
No	2,692				2,613	2,194	1222
Yes	970				1,049	916	669
	26.5%	29.0%	29.0%	29.0%	28.6%	29.5%	35.4%
Charges Pending at Arrest							
No	3,179				3,398	2,877	1,776
Yes	483				264	233	115
	13.2%	13.0%	6.0%	7.0%	7.2%	7.5%	6.1%

Table 4 - 5: Reproducing Social Context Measures

Measures of Social Context	Publications					Findings from Archived* Data			
	NIJ, 1999	JQC, 2002	Criminology, 2002		C & D, 2005				
Level 2 Sample Size	204	204	126	48	3,662	3,662	206	119	47
Social Context Composite Factor									
Mean	0.0						0		
Standard Deviation	4.6						4.6		
Minimum	-14.6						-14.8		
Maximum	9.1						7.8		
Proportion in Census Tract									
High School Graduate		72%	68%	67%			72.0%	67.6%	66.8%
College Graduate		20%	19%	20%	14.0%	14.0%	19.3%	18.1%	19.1%
Employed		93%	90%	90%			92.5%	90.1%	89.8%
Skilled Occupation		24%	22%	23%			24.0%	21.9%	22.5%
No Public Assistance		89%	85%	84%			88.8%	84.1%	83.8%
Same Residence 5 Years +		54%	50%	49%	52.0%	52.0%	54.1%	50.1%	49.1%
Proportion Male			47%	48%				47.1%	47.9%
Mean Age			34.9	34				34.8	34.1
Socio-Economic Factor									
Mean		0.0					0		
Standard Deviation		1					1		
Minimum		-3.42					-2.50		
Maximum		1.84					2.60		
Social Class Factor									
Mean			0	0				0	0
Standard Deviation			1	1				1	1
Minimum			-2.81	-2.03				-2.77	-2.05
Maximum			1.97	1.99				1.92	1.77

* Data for the proportion male and mean age generated from data from the 1990 Census at the NACJD.

Chapter 5: Reproducing the Direct Effects of Sanctions

Introduction

In Chapter 5, we report the findings from our efforts to reproduce the Wooldredge and Thistlethwaite's multivariate and multilevel analyses of the effects of sanctions. Wooldredge and Thistlethwaite present their direct effects of sanctions in nine multivariate analyses. Three of these are provided in the 1999 NIJ Final Report, one in the 2002 *JQC* article, two in the 2002 *Criminology* article, and three in the 2005 article in *Crime and Delinquency*. These nine analyses are presented in a series of tables which list the regression coefficients and standard errors published by Wooldredge and Thistlethwaite alongside the regression coefficients and standard errors produced in our re-analyses. For the analyses of the prevalence and frequency of rearrest, Wooldredge and Thistletwaite report that they used a fixed effects model using the statistical analysis software HLM 3 (Bryk and Raudenbush, 1992). We used the same statistical model in the current version, HLM 5.0, of the same statistical software. For the analysis of time to first rearrest, both our analyses and Wooldredge and Thistlethwaite employ SPSS's Cox regression procedures.

Each table also includes three measures of reproducibility. The first measure is the simple difference between the reproduced coefficients and the published coefficients. Where this figure is greater than .1, we highlight the difference by using a bold font. Our second measure is a determination whether the reproduced analyses generate a consistent assessment of the direction and statistical significance of each regression coefficient. In these tables, this measure is listed as the "Same" or "Not Same". The third measure is a statistical comparison of the published and the reproduced coefficients and standard errors and, when this statistic exceeds the $p > .05$ level, it is

presented in bold font.

1999 NIJ Final Report - Prevalence Model

Table 5 - 1A presents the published findings of the direct effects of criminal sanctions, statistical controls, stakes-in-conformity and social context from Wooldredge and Thistlethwaite's 1999 Final Report to the National Institute of Justice. This table also includes our reproduction of those findings. We were unable to reproduce exactly any of the reported coefficients or standard errors published by Wooldredge and Thistlethwaite; however, all but five of the 28 coefficients we produced were within .1 of the published findings. We report such differences for three statistical controls--male offender, number of prior misdemeanor convictions, and the extent of family contact--and two sanction variables--probation and jail.

The differences in two of the three statistical control variables were not sufficient to change the direction and statistical significance of the reported findings; however, the effect of the family contact variable is statistically significant in the reproduction but not the published findings. Moreover, Wooldredge and Thistlethwaite report that the effect of dropping charges was associated with reduced re-arrest; however, that effect was not statistically significant. We also find that the effect of dropping charges is associated with reduced re-arrest but, in our reproduction, that effect is statistically significant. Wooldredge and Thistlethwaite also report that the sanction of probation is associated with increased re-arrest but that effect is not statistically significant. We find that the effect of probation is associated with reduced, not increased, re-arrest and that the effect is statistically significant.

In our third test of reproducibility, we find that the differences in published and reproduced coefficients for probation are statistically significant but that those for dropped charges are not. In addition, the difference between the published and reproduced findings for the

number of prior misdemeanor convictions is statistically significant even though the direction and statistical significance of the findings do not change in the reproduced findings from the published findings.

In summary, Table 5 - 1A shows that 1) none of the regression coefficients were reproduced exactly, 2) 23 out of 28 (82.2%) of the differences in coefficients were greater than .1, 3) the direction and statistical significance of 12 out of 14 effects (85.7%) were consistent between the published and reproduced findings, and 4) there were no statistically significant differences in the size of 12 out of 14 regression coefficients. Wooldredge and Thistlethwaite report that only one sanction variable--participation in a treatment program--was associated with reduced repeat offending; our analyses confirm that finding also found that arrests where charges are dropped and arrests where the offender is sentenced to probation only also have statistically significant reductions in repeat arrests. The change in charges dropped reflects a small and not statistically significant change in the reported coefficients but the change in the size of the effect for probation is large and statistically significant.

1999 NIJ Final Report - Frequency Model

The findings for the effects of criminal sanctions on the frequency of new arrests displayed in Table 5 - 1B show similar patterns to those reported in Table 5 - 1A. None of the coefficients match exactly, 20 of the 28 (71.4%) coefficients are similar to within .1, 10 of the 14 (71.4%) effects are in the same direction with the same statistical significance, and the size of the differences in 12 of the 14 (85.7%) effects are not statistically significant. In these analyses, the reproduced findings show that four of the sanction variables--dropped charges, acquitted, treatment program, and probation--are associated with statistically significant reductions in the frequency of re-arrest. In the original analyses, only the treatment program showed such effects.

The size of the differences in the sanction effects between the published and the reproduced analyses is statistically significant only for the probation variable.

1999 NIJ Final Report - Time to First Re-Arrest

Table 5 - 1C compares the published and the reproduced findings for the analyses of time to first re-arrest. These analyses are based on the sample of 3,662 arrests. Again, none of the coefficients were reproduced exactly and a sizeable majority (22 out of 28) of the raw differences in coefficients are less than .1. Eleven of the 14 effects are in the same direction and have the same level of statistical significance. In this model, the published and reproduced findings vary on two sanction measures--treatment program and probation. The published findings show that the treatment program is associated with statistically significant longer time to first re-arrest but that arrestees sentenced to probation do not. The reproduced analyses show probation associated with statistically significant longer time to first re-arrest but that arrestees sentenced to the treatment program do not. None of the differences in the size of the effects in the time to first re-arrest model are statistically significant.

Implication of these Findings

Our reproduction of the multivariate, multi-level analyses published by Wooldredge and Thistlethwaite in their 1999 Final Report to NIJ has found several substantive differences in the effects of criminal sanctions on subsequent offending. In our analyses of the prevalence, frequency and time to first re-arrest, we have found statistically significant lower re-arrest rates among arrestees sentenced to probation while Wooldredge and Thistlethwaite did not. In all three analyses, the differences in the raw coefficients for probation exceeded .1 and the statistical significance of the effect exceeded the traditional level of $p < .05$. In two of the three analyses, the differences in the size of the effects for probation were statistically significant.

We interpret these findings as meaning that, on eight out of nine tests for reproducibility, we could not reproduce the findings about the effects of probation reported by Wooldredge and Thistlethwaite in their 1999 Final Report to NIJ. In contrast, on most of the other variables in their statistical models, we were able to consistently reproduce their published findings. The success and failure to reproduce a prior analysis is co-produced by the quality of the description in the original publication, the accuracy of the available data documentation, the completeness of the archived data, and the capabilities of the researchers generating the reproduction. It is often difficult, as it is in this instance, to determine why were able to reproduce most of their findings but not their findings for the effects of probation.

Our failure to reproduce the Wooldredge and Thistletwaite's published findings about the direct effects of probation does not necessarily mean that their analyses are wrong and that ours are correct: our failure to reproduce means that we cannot provide a confirmation of their findings on this variable in this report. Like any body of research with inconsistent findings, it is appropriate to incorporate the strengths and weaknesses of both sets of findings into our understanding the effects of criminal sanctions on subsequent offending. Unlike most other efforts to synthesize inconsistent findings, our differences cannot be attributed to different samples or to alternative analytical approaches. Before coming to a judgment about the effects of sanctions generally or the effects of probation in particular, it is useful to complete our review of the six additional analyses of the effects of sanctions published by Wooldredge and Thistlethwaite.

JQC Prevalence Model

In their 2002 *JQC* article, Wooldredge and Thistlethwaite assess the direct effects of criminal sanctions in multivariate, multi-level model that includes six control variables, four

sanction variables, three stakes-in-conformity variable and two social context variables. In this analysis, arrests that result in dropped charges or in an acquittal are the excluded reference group of criminal sanctions[16]. Because the archived data do not include information about the 5-day program, we combined the two variables--probation and jail with the program and probation and jail without the program--into one category of all cases sanctioned with either probation or jail. Table 5 - 2 displays their published findings and our reproduction of their findings. Three of the published and reproduced coefficients are exactly the same and the differences between 23 of the 28 comparisons are less than .1. For two of the three sanction regression coefficients--No Charges Filed and Probation or Jail--the differences in the regression coefficients exceeded .1. Wooldredge and Thistlethwaite's published findings show that the effect for No Charges Filed is positive and statistically significant; in the reproduced analyses, the effect is still positive but no longer statistically significant. The substantial differences in the coefficients for Probation or Jail change the direction of the effect from positive to negative but the effect is not statistically significant at the $p < .05$ level.

In Table 5 - 2, the reproduced findings for one sanction variable (No Charges Filed), one stakes-in-conformity variable (Economic Scale) and one social context variable (residential stability) do not have the same level of statistical significance as the published findings. However, none of the differences in the size of the effects for any of the 14 variables are statistically significant using Clogg's Z.

Criminology Census Tract and Neighborhood Prevalence Models

In his 2002 *Criminology* article, Wooldredge examined the direct effects of sanctions

[16]In the NIJ Final report, the excluded category was those arrests for which no charges were filed.

when social context is measured at census tract level and at the neighborhood level. Both of these analyses are limited to arrests which occurred within the city limits of Cincinnati and for which data exist for at least 24 months of re-arrest. In our analyses, we were not able to reproduce the exact sample used by Wooldredge; he reports 1,855 arrests and we identified 1,891 arrests from census tracts within the city limits (See section above on descriptive statistics). We were able to identify all 12 control variables, the two sanction variables, and the four aggregate level measures of social context. In this section, we first report our comparison of published and reproduced findings for the analyses using census tracts. We then report the comparisons using smaller number of neighborhoods.

Census Tract Level of Analysis

Table 5 - 3A presents our comparison of the published and reproduced findings of the effects of criminal sanctions on repeat arrest when the social context variables are measured at the census tract level.[17] We could reproduce 27 of the 36 coefficients (75%) in his analyses within .1. In our reproduction, one control variable--High School Graduation--one sanction variable--No Charges Filed--and one measure of social context--Social Class Factor--are not in the same direction or have the same level of statistical significance as those published by Wooldredge. In none of these 36 measures are the differences in the size of the effects large enough to be statistically significant. As with our earlier analyses of the *JQC* article, Wooldredge reports statistically significant differences for No Charges Filed and we do not.

Neighborhood Level of Analysis

As with the NIJ Final Report, we were able to reproduce none of the regression

[17]In this article, Wooldredge reports his findings to two decimal points and we followed his format.

coefficients or standard errors exactly. However, in this effort, the difference between the published and the reproduced coefficients exceed .1 in 25 out of 36 (69.4%) comparisons (See Table 5 - 3B). In addition, the size of the differences between the published and reproduced effects was statistically significant for more than a third of the variables. Despite these many differences in the size of published and reproduced coefficients, for only one of the 18 variables did the reproduced effect not conform to the statistical significance of the published effect. The one variable that did not pass any of our tests of reproducibility was the sanction variable, no charges filed.

Summary of Sanction Effects in Criminology Article

Our reproductions of Wooldredge's two analyses of the direct effects of criminal sanctions finds consistent findings--neither sanction variable is statistically significant in both analyses. We were unable to confirm one of two findings concerning sanction effects reported by Wooldredge. At both level of aggregation, he reports that the comparison between No Charges Filed and the comparison group of Dropped and Acquitted cases is positive and statistically significant. In analyses at both levels, we find that the differences in regression coefficients are greater than .1 and the effect of No Charges Filed is not statistically significant. In their analyses at the census tract level, the size of the differences in the effects is not statistically significant but in their analyses at the neighborhood level it is significant. Thus, on five of six tests of reproducibility, we were unable to confirm Wooldredge's finding for No Charge Filed. For the second sanction variable, Conviction, we were able to confirm Wooldredge's findings of no effect in all six tests.

Comparison of Census Tract and Neighborhood Models

The central point of Wooldredge's article was to compare the effects of statistical controls,

criminal sanctions, and social context using larger (neighborhood) and smaller (census tracts) aggregations of social context. He reports (p 695) that the individual level zero order coefficients using difference levels of aggregation are "similar" and that there is "complete consistency" between these models in the significance of individual level relationships. In the first order model (See Table 5 - 3C), Wooldredge reports (p. 698) that all four social context variables are in the direction of lower re-arrest rates in both models and that, when census tracts are used, two of these measures--mean age and their social factor score--are statistically significant predictors. No social context factors are statistically significant when they are aggregated to the neighborhood level.

Wooldredge reports that the magnitude of the differences in coefficients for mean age and the social factor score across the two aggregation models is not statistically significant and we reproduce that finding. When we compare social context measures in the reproduced models at the tract and neighborhood level, we get results that are dissimilar to Wooldredge's results (See Table 5 - 3D). We find that none of the differences in the four social context coefficients are statistically significant. We also find that statistically significant reductions in re-arrest associated with the mean age of the census tract is not reproduced when the neighborhood level of aggregation is used. Using our first criteria of a raw difference of .1 or greater in the raw coefficients, we find that the raw differences in four out of the eight social context coefficients are greater than .1. Wooldredge relies exclusively on Clogg's statistical test of the magnitude of differences in coefficients and determines that there are no differences in using tracts or neighborhoods. Using our three measures of reproducibility to compare the tract and neighborhood models in our analyses, we are able to reproduce the four out of eight raw coefficients for the social context measures within .1, three out of four tests of statistical

significance, and four out of four statistical tests for differences in magnitude. In addition, Wooldredge found that the effects for all 14 individual level statistical controls were in the same direction and statistical significance; we find that the differences in 24 out of 28 coefficients exceed .1, that one measure--High School Graduate--changes from negative and non significant to negative and significant, and that the magnitude of differences in three variables--Prior Violent Misdemeanor, Not Cohabitating, and Charges Pending--are all larger in the tract level analysis than in the neighborhood level analysis. Thus, the main effects for comparing tract and neighborhood models which Wooldredge emphasizes--a statistical test of the magnitude of differences in the social context measures--are consistent in his and our findings; however, our other measures of reproducibility suggest less consistency than Wooldredge's analyses report.

Crime and Delinquency Prevalence, Frequency and Time Models

In their 2005 article in *Crime and Delinquency* Wooldredge and Thistlethwaite structure the criminal case disposition into six dichotomous variables, with cases dropped being the reference category. Their statistical models include nine individual level and two aggregate level variables under the heading of "Extra-Legal and Legal Characteristics." Tables 5 - 4A, 5 - 4B and 5 - 4C display the extent to which we were able to reproduce the effects of these 11 variables on the prevalence, frequency and time to first new arrest respectively. In each of these analyses, we performed 68 tests of reproducibility and, in each case, we were able to obtain far higher rates of reproducibility than in the three prior publications.

In the analysis of the prevalence of re-arrest in Table 5 - 4A, the differences between raw coefficients exceeded .1 only three out of 34 opportunities. None of the tests of significance changed and none of the changes in the magnitude of the effects was statistically significant. In the analysis of the frequency of re-arrest in Table 5 - 4B, the raw coefficients for one of the

aggregate level variables--Proportion of the Population at Same Residence Five or More Year--could not be reproduced within .1 and one of the sanction variables--No Charges Filed--changed from statistically significant to not statistically significant. None of the differences were of sufficient magnitude to be statistically significant. Finally, our analyses of time to first new arrest passed all but two tests of reproducibility--the standard errors for the offender having a college degree varied by .15 and for Pending Charges by .24. In fact, the published and reproduced coefficients in Table 5 - 4C matched exactly to two decimal points in 20 out of 24 opportunities.

The extent to which we are able to reproduce Wooldredge and Thistletwaite *Crime and Delinquency* analyses conform more to the notion that the reproduction of published findings is a mechanical process akin to an original investigator re-running a statistical program.[18] However, we have evidence that this is not necessarily the case from our other effort to reproduce the results from other publications using these same data. The findings from our reproduction of Wooldredge and Thistlethwaite analysis of time to first new arrest provide a real world example of the often hypothesized limitation inherent in relying solely on a fixed p value for determining the existence or absence of an effect.

It is useful to note that the minor variations in the regression coefficients and standard errors for No Charges Filed changed whether these analyses met or failed the traditional criteria in narrative reviews for reporting the effects of quantitative analyses. The high degree of reproducibility of most individual level and aggregate level variables in these three analyses contrast sharply with our failure to reproduce the same level of statistical significance for the No Charges Filed variable in the analyses of frequency of new arrests and time to first new arrest.

[18]The *Crime and Delinquency* analyses were based on a copy of the archived data supplied to Wooldredge by the National Archive of Criminal Justice Data (Wooldredge, 2006).

The magnitudes of the differences between published and reproduced coefficients are small and not statistically significant and the effect for No Charges Filed was reproduced in all three tests of reproducibility applied to the prevalence analyses.

The published and reproduced analyses in the *Crime and Delinquency* article generate consistent findings in all three analyses of the effects of probation. Probation is associated with statistically significant lower prevalence and frequency of re-arrest and increased length to the first new arrest. In addition, the published and reproduced analyses are also consistent in showing that arrestees who are acquitted, sentenced to a treatment program or sentenced to jail alone are re-arrested at rates similar to arrestees whose criminal charges are dropped by the prosecutor. Another consistent finding in the published and reproduced analyses is the effect of being sentenced to both jail and probation. In the prevalence and frequency analyses, these effects are positive and statistically significant. In the analysis of time to first new arrests, both the published and reproduced analyses find nearly identical results--the effects of being sentenced to both probation and jail are positive but not statistically significant.

Table 5 - 1A: Effects of Sanctions Final Report Prevalence Model

Predicting Prevalence of Re-Arrest (Direct Effects)			Original	Secondary	Measures of Reproducibility: $(b^1 - b^2)$	Direction & Statistical Significance	Clogg's Z
	Months at Risk		24	24			
Samples	Individuals		3,110	3,110	N.A.	N.A.	N.A.
	Census Tracts		N.R.	197			
Constant		b	**-2.00**	-2.018	**-0.41**	**Same**	**N.A.**
		se	**N.R.**	0.069			
Control Variables							
Male		b	**0.688**	**0.834**	**-0.146**	**Same**	-0.65
		se	**0.121**	**0.191**	-0.070		
Age		b	**-0.017**	**-0.023**	0.006	**Same**	0.81
		se	**0.004**	**0.007**	-0.003		
# Prior Misdemeanor Convictions		b	**0.219**	**0.033**	**0.186**	**Same**	**5.15**
		se	**0.035**	**0.009**	0.026		
Incarceration (Not DV)		b	0.083	0.125	-0.042	**Same**	-0.32
		se	0.069	0.114	-0.045		
No Family Contact		b	-0.051	**-0.371**	**0.320**	**Same**	1.72
		se	0.139	**0.123**	0.016		
Case Processing (Not Filed)							
Charges Dropped		b	-0.285	**-0.377**	0.092	**Not Same**	0.39
		se	0.150	**0.179**	-0.029		
Acquitted		b	-0.311	-0.410	0.099	**Same**	0.31
		se	0.192	0.257	-0.065		
Treatment Program		b	**-0.493**	**-0.567**	0.074	**Same**	0.22
		se	**0.216**	**0.265**	-0.049		
Probation		b	0.153	**-1.145**	**1.298**	**Not Same**	**3.70**
		se	0.250	**0.246**	0.004		
Jail		b	-0.044	-0.300	**0.256**	**Same**	0.81
		se	0.213	0.236	-0.023		
Probation + Jail		b	0.372	0.348	0.024	**Same**	0.06
		se	0.278	0.326	-0.048		
Charges Pending		b	**0.800**	**0.822**	-0.022	**Same**	-0.10
		se	**0.161**	**0.161**	0.000		
Individual Level Stakes in Conformity		b	**-0.038**	**-0.058**	0.020	**Same**	0.84
		se	**0.013**	**0.019**	-0.006		
Aggregate Level Stakes-in-Conformity		b	**-0.031**	**-0.027**	-0.004	**Same**	-0.26
		se	**0.007**	**0.013**	-0.006		

Table 5 - 1B: Effects of Sanctions Final Report Frequency Model

					Measures of Reproducibility		
Predicting Frequency of Re-Arrest (Direct Effects)			**Original**	**Secondary**	**(b^1 - b^2)**	**Direction & Statistical Significance**	**Clogg's Z**
	Months at Risk		**24**	**24**			
Samples	**Individuals**		**3110**	**3,110**	**N.A.**	**N.A.**	**N.A.**
	Census Tracts		**216**	216	**N.A.**		
Constant		b	-1.582	**-1.957**	**0.375**	**Same**	**N.A.**
		se	**N.R.**	**0.064**	**N.A.**		
Control Variables							
Male		b	**0.527**	**0.614**	-0.087	**Same**	-0.39
		se	**0.161**	**0.154**	0.007		
Age		b	**-0.025**	**-0.021**	-0.004	**Same**	-0.61
		se	**0.004**	**0.005**	-0.001		
# Prior Misdemeanor Convictions		b	**0.229**	**0.027**	**0.202**	**Same**	**8.47**
		se	**0.023**	**0.006**	0.017		
Incarceration (Not DV)		b	**0.169**	0.139	0.030	**Not Same**	0.26
		se	**0.076**	0.090	-0.014		
No Family Contact		b	**-0.909**	**-0.391**	**-0.518**	**Same**	-1.72
		se	**0.283**	**0.103**	**0.180**		
Case Processing (Not Filed)							
Charges Dropped		b	-0.190	**-0.266**	0.076	**Not Same**	0.42
		se	0.121	**0.135**	-0.014		
Acquitted		b	-0.255	**-0.506**	**0.251**	**Not Same**	0.89
		se	0.181	**0.214**	-0.033		
Treatment Program		b	**-0.506**	**-0.662**	**0.156**	**Same**	0.53
		se	**0.188**	**0.227**	-0.039		
Probation		b	0.289	**-0.966**	**1.255**	**Not Same**	**4.20**
		se	0.224	**0.198**	0.026		
Jail		b	-0.107	-0.236	**0.129**	**Same**	0.51
		se	0.182	0.180	0.002		
Probation + Jail		b	0.364	0.417	-0.053	**Same**	-0.17
		se	0.211	0.226	-0.015		
Charges Pending		b	**0.888**	**0.733**	**0.155**	**Same**	0.85
		se	**0.143**	**0.113**	0.030		
Individual Level Stakes in Conformity		b	**-0.024**	**-0.046**	0.022	**Same**	1.13
		se	**0.012**	**0.016**	-0.004		
Aggregate Level Stakes-in-Conformity		b	**-0.025**	**-0.026**	0.001	**Same**	0.07
		se	**0.007**	**0.012**	-0.005		

Table 5 - 1C: Effects of Sanctions Final Report Time to Failure Model

Predicting Time to First Re-Arrest (Direct Effects)			Original	Secondary	Measures of Reproducibility $(b^1 - b^2)$	Direction & Statistical Significance	Clogg's Z
Months at Risk			24	24			
Samples	Individuals		3,662	3,662	N.A.	N.A.	N.A.
	Census Tracts		207	197			
Control Variables							
Male		b	**0.63**	0.72	-0.09	**Same**	-0.38
		se	**0.17**	**0.16**	0.01		
Age		b	**-0.02**	**-0.02**	0.00	**Same**	0.33
		se	**0.01**	**0.01**	0.00		
# Prior Misdemeanor Convictions		b	**0.18**	**0.17**	0.01	**Same**	0.27
		se	**0.03**	**0.03**	0.00		
Incarceration (Not DV)		b	0.16	0.16	0.00	**Same**	0.02
		se	0.09	0.09	0.00		
No Family Contact		b	**-0.79**	**-0.23**	**-0.56**	**Same**	-1.52
		se	**0.36**	**0.10**	**0.26**		
Case Processing (Not Filed)							
Charges Dropped		b	-0.24	-0.24	0.00	**Same**	0.00
		se	0.15	0.15	0.00		
Acquitted		b	-0.04	-0.34	**0.30**	**Same**	0.97
		se	0.22	0.22	0.00		
Treatment Program		b	**-0.44**	-0.40	-0.04	**Not Same**	-0.13
		se	**0.22**	0.22	0.00		
Probation		b	-0.26	**-0.88**	**0.62**	**Not Same**	1.94
		se	0.27	**0.17**	**0.10**		
Jail		b	-0.16	-0.24	0.08	**Same**	0.30
		se	0.19	0.19	0.00		
Probation + Jail		b	0.34	0.09	**0.25**	**Same**	0.70
		se	0.27	0.23	0.04		
Charges Pending		b	**0.69**	**0.73**	-0.04	**Same**	-0.22
		se	**0.13**	**0.12**	0.01		
Individual Level Stakes in Conformity		b	**-0.04**	**-0.05**	0.01	**Same**	0.35
		se	**0.02**	**0.02**	0.00		
Aggregate Level Stakes-in-Conformity		b	-0.010	**-0.03**	0.02	**Not Same**	1.22
		se	0.010	**0.01**	0.00		

Table 5 - 2: Effects of Sanctions *JQC* Prevalence Model

					Measures of Reproducibility		
			Original	Secondary	$(b^1 - b^2)$	Direction & Statistical Significance	Clogg's Z
Months at Risk			24	24			
Samples	Individuals		3,110	3,110	N.A.	N.A.	N.A.
	Census Tracts		204	197			
Constant		b	-2.082	-2.013	-0.069	N.A.	N.A.
		se	N.R.	0.070	N.A.		
Control Variables							
Male		b	0.808	0.910	-0.102	Same	-0.39
		se	0.183	0.189	-0.006		
Age		b	-0.016	-0.019	0.003	Same	0.39
		se	0.006	0.006	0.000		
African - American		b	0.074	0.114	-0.040	Same	-0.24
		se	0.121	0.115	0.006		
# Prior Misdemeanor Convictions		b	0.216	0.215	0.001	Same	0.01
		se	0.050	0.040	0.010		
No Family Contact		b	-0.413	-0.373	-0.040	Same	-0.23
		se	0.127	0.123	0.004		
Pending Charges		b	0.867	0.893	-0.026	Same	-0.11
		se	0.169	0.159	0.010		
Case Processing (Dropped & Acquitted)							
No Charges		b	0.475	0.317	0.158	Not Same	0.63
		se	0.177	0.179	-0.002		
Offender Program		b	-0.086	-0.179	0.093	Same	0.31
		se	0.206	0.220	-0.014		
Probation or Jail Without Program		b	0.141	-0.253	0.394	Same	1.60
		se	0.208	0.131	0.077		
Probation or Jail With Program		b	0.537	N.A.	N.A.	N.A.	N.A.
		se	0.387	N.A.	N.A.	N.A.	N.A.
Offender Stakes in Conformity							
Residential Stability		b	-0.424	-0.534	0.110	Same	0.41
		se	0.176	0.204	-0.028		
Education Scale		b	-0.098	-0.071	-0.027	Same	-0.33
		se	0.058	0.058	0.000		
Economic Scale		b	-0.045	-0.138	0.093	Not Same	1.15
		se	0.057	0.057	0.000		
Neighborhood Stakes in Conformity							
Residency		b	-1.013	-0.668	-0.345	Not Same	-0.45
		se	0.504	0.584	-0.080		
SES		b	-0.182	-0.126	-0.056	Same	-0.67
		se	0.050	0.067	-0.017		

Table 5 - 3A: Sanctions *Criminology* Census Tract Prevalence

		Original	Secondary	Measures of Reproducibility		
Individual Level Sample		**1,855**	**1891**			
Aggregate Level Sample		**207**	**197**	$(b^1 - b^2)$	**Direction & Statistical Significance**	**Clogg's Z**
Individual Level						
Constant	**b**	**-2.02**	-2.04	0.00	**N.A.**	**N.A.**
	s.e.	**N.R.**	0.09	**N.A.**	**N.A.**	**N.A.**
Male	**b**	**1.08**	**1.12**	-0.04	**Same**	-0.11
	s.e.	**0.24**	**0.25**	-0.01		
Age	**b**	**-0.02**	**-0.03**	0.01	**Same**	0.47
	s.e.	**0.01**	**0.01**	0.00		
Same Residence 5+ Years	**b**	-0.30	-0.36	0.06	**Same**	0.17
	s.e.	0.24	0.25	-0.01		
African-American	**b**	0.18	0.22	-0.04	**Same**	-0.18
	s.e.	0.16	0.17	-0.01		
High School Graduate	**b**	**-0.36**	-0.32	-0.04	**Not Same**	-0.19
	s.e.	**0.16**	0.17	-0.01		
College Degree	**b**	0.09	0.57	**-0.48**	**Same**	-1.12
	s.e.	0.20	0.38	**-0.18**		
Employed	**b**	**-0.33**	**-0.38**	0.05	**Same**	0.23
	s.e.	**0.16**	**0.17**	-0.01		
Employed in Skilled Occupation	**b**	-0.12	-0.12	0.00	**Same**	-0.01
	s.e.	0.22	0.20	0.02		
Not Receiving Public Assistance	**b**	0.26	0.31	-0.05	**Same**	-0.19
	s.e.	0.17	0.20	-0.03		
Prior Violent Misdemeanor Convictions	**b**	**0.27**	**0.27**	0.00	**Same**	0.02
	s.e.	**0.05**	**0.06**	-0.01		
Not Cohabitating at Arrest	**b**	**-1.29**	**-0.35**	**-0.94**	**Same**	-1.55
	s.e.	**0.59**	**0.15**	**0.44**		
Old Charges Pending at Arrest	**b**	**0.84**	**0.89**	-0.05	**Same**	-0.15
	s.e.	**0.25**	**0.23**	0.02		
No Charges Filed in this Case	**b**	**0.64**	0.38	**0.26**	**Not Same**	0.81
	s.e.	**0.22**	0.24	-0.02		
Convicted in this Case	**b**	-0.02	-0.05	0.03	**Same**	0.16
	s.e.	0.14	0.15	-0.01		
Aggregate Level						
Proportion Males	**b**	-0.40	-0.36	-0.04	**Same**	-0.01
	s.e.	2.28	1.97	**0.31**		
Mean Age	**b**	**-0.03**	**-0.04**	0.01	**Same**	0.54
	s.e.	**0.01**	**0.02**	-0.01		
Proportion Same Address	**b**	-0.16	0.43	**-0.59**	**Same**	-0.49
	s.e.	0.83	0.86	-0.03		
Social Class Factor	**b**	**-0.13**	0.03	**-0.16**	**Not Same**	-1.40
	s.e.	**0.06**	0.10	-0.04		

Table 5 - 3B: Sanctions *Criminology* Neighborhood Prevalence

		Original	Secondary	Measures of Reproducibility		
Individual Level Sample		**1,855**	**1891**			
Aggregate Level Sample		**48**	**47**	**(b^1 - b^2)**	**Direction & Statistical Significance**	**Clogg's Z**
Individual Level Relationships						
Constant	**b**	**-2.02**	-2.04	0.00	**N.A.**	**N.A.**
	s.e.		0.09	**N.A.**	**N.A.**	**N.A.**
Male	**b**	**1.03**	**0.10**	**0.93**	**Same**	**5.45**
	s.e.	**0.17**	**0.02**	**0.15**		
Age	**b**	**-0.03**	**0.00**	-0.03	**Same**	**-2.72**
	s.e.	**0.01**	**0.00**	0.01		
Same Residence 5+ Years	**b**	-0.31	-0.04	**-0.27**	**Same**	-0.94
	s.e.	0.29	0.02	**0.27**		
African-American	**b**	0.25	0.03	**0.22**	**Same**	1.39
	s.e.	0.16	0.02	**0.14**		
High School Graduate	**b**	**-0.36**	**-0.04**	**-0.32**	**Same**	**-2.09**
	s.e.	**0.15**	**0.02**	**0.13**		
College Degree	**b**	0.08	0.05	0.03	**Same**	0.15
	s.e.	0.19	0.04	**0.15**		
Employed	**b**	**-0.29**	**-0.05**	**-0.24**	**Same**	-1.69
	s.e.	**0.14**	**0.02**	**0.12**		
Employed in Skilled Occupation	**b**	-0.12	-0.02	**-0.10**	**Same**	-0.61
	s.e.	0.17	0.02	**0.15**		
Not Receiving Public Assistance	**b**	0.21	0.04	**0.17**	**Same**	1.06
	s.e.	0.16	0.02	**0.14**		
Prior Violent Misdemeanor Convictions	**b**	**0.27**	**0.04**	**0.23**	**Same**	**5.64**
	s.e.	**0.04**	**0.01**	0.03		
Not Cohabitating at Arrest	**b**	**-1.31**	**-0.04**	**-1.27**	**Same**	**-2.70**
	s.e.	**0.47**	**0.02**	**0.45**		
Old Charges Pending at Arrest	**b**	**0.81**	**0.14**	**0.67**	**Same**	**2.46**
	s.e.	**0.27**	**0.03**	**0.24**		
No Charges Filed in this Case	**b**	**0.61**	0.05	**0.56**	**Not Same**	**2.39**
	s.e.	**0.23**	0.03	**0.20**		
Convicted in this Case	**b**	-0.04	0.00	-0.04	**Same**	-0.24
	s.e.	0.15	0.02	**0.13**		
Aggregate Level Relationships						
Proportion Males	**b**	1.31	-0.25	**1.56**	**Same**	-0.01
	s.e.	2.20	0.35	**1.85**		
Mean Age	**b**	-0.02	0.00	-0.02	**Same**	0.54
	s.e.	0.02	0.00	0.02		
Proportion Same Address	**b**	-0.03	-0.06	0.03	**Same**	-0.49
	s.e.	0.69	0.15	**0.54**		
Social Class Factor	**b**	-0.06	0.01	-0.07	**Same**	-1.40
	s.e.	0.07	0.01	0.06		

Table 5 - 3C: Published Census Tract and Neighborhood Effects

		Tracts	Neighborhoods	Measures of Reproducibility		
Individual Level Sample		**1,855**	**1,855**			
Aggregate Level Sample		**207**	**48**	$(b^1 - b^2)$	**Direction & Statistical Significance**	**Clogg's Z**
Individual Level						
Constant	**b**	**-2.02**	**-2.02**	0.00	**N.A.**	**N.A.**
	s.e.	**N.R.**	**N.R.**	**N.A.**	**N.A.**	**N.A.**
Male	**b**	**1.08**	**1.03**	0.05	**Same**	0.17
	s.e.	**0.24**	**0.17**	0.07		
Age	**b**	**-0.02**	**-0.03**	0.01	**Same**	0.71
	s.e.	**0.01**	**0.01**	0.00		
Same Residence 5+ Years	**b**	-0.30	-0.31	0.01	**Same**	0.03
	s.e.	0.24	0.29	-0.05		
African-American	**b**	0.18	0.25	-0.07	**Same**	-0.31
	s.e.	0.16	0.16	0.00		
High School Graduate	**b**	**-0.36**	**-0.36**	0.00	**Same**	0.00
	s.e.	**0.16**	**0.15**	0.01		
College Degree	**b**	0.09	0.08	0.01	**Same**	0.04
	s.e.	0.20	0.19	0.01		
Employed	**b**	**-0.33**	**-0.29**	-0.04	**Same**	-0.19
	s.e.	**0.16**	**0.14**	0.02		
Employed in Skilled Occupation	**b**	-0.12	-0.12	0.00	**Same**	0.00
	s.e.	0.22	0.17	0.05		
Not Receiving Public Assistance	**b**	0.26	0.21	0.05	**Same**	0.21
	s.e.	0.17	0.16	0.01		
Prior Violent Misdemeanor Convictions	**b**	**0.27**	**0.27**	0.00	**Same**	0.00
	s.e.	**0.05**	**0.04**	0.01		
Not Cohabitating at Arrest	**b**	**-1.29**	**-1.31**	0.02	**Same**	0.03
	s.e.	**0.59**	**0.47**	0.12		
Old Charges Pending at Arrest	**b**	**0.84**	**0.81**	0.03	**Same**	0.08
	s.e.	**0.25**	**0.27**	-0.02		
No Charges Filed in this Case	**b**	**0.64**	**0.61**	0.03	**Same**	0.09
	s.e.	**0.22**	**0.23**	-0.01		
Convicted in this Case	**b**	-0.02	-0.04	0.02	**Same**	0.10
	s.e.	0.14	0.15	-0.01		
Aggregate Level						
Proportion Males	**b**	-0.40	-1.31	**0.91**	**Same**	0.29
	s.e.	2.28	2.20	0.08		
Mean Age	**b**	**-0.03**	-0.02	-0.01	**Not Same**	-0.45
	s.e.	**0.01**	0.02	-0.01		
Proportion Same Address	**b**	-0.16	-0.03	**-0.13**	**Same**	-0.12
	s.e.	0.83	0.69	**0.14**		
Social Class Factor	**b**	**-0.13**	-0.06	-0.07	**Not Same**	-0.76
	s.e.	**0.06**	0.07	-0.01		

Table 5 - 3D: Reproduced Census Tract & Neighborhood Effects

		Tract	Neighborhood	Measures of Reproducibility		
Individual Level Sample		**1,855**	**1,855**			
Aggregate Level Sample		**197**	**47**	**(b^1 - b^2)**	**Direction & Statistical Significance**	**Clogg's Z**
Individual Level						
Constant	**b**	-2.04	-2.04	N.A.	N.A.	N.A.
	s.e.	0.09	0.09	N.A.	N.A.	N.A.
Male	**b**	**1.12**	**0.10**	**1.02**	**Same**	4.00
	s.e.	**0.25**	**0.02**	**0.23**		
Age	**b**	**-0.03**	**0.00**	-0.02	**Same**	-2.74
	s.e.	**0.01**	**0.00**	0.01		
Same Residence 5+ Years	**b**	-0.36	-0.04	**-0.32**	**Same**	-1.30
	s.e.	0.25	0.02	**0.22**		
African-American	**b**	0.22	0.03	**0.20**	**Same**	1.17
	s.e.	0.17	0.02	**0.15**		
High School Graduate	**b**	-0.32	**-0.04**	**-0.27**	**Not Same**	-1.55
	s.e.	0.17	**0.02**	**0.15**		
College Degree	**b**	0.57	0.05	**0.52**	**Same**	1.35
	s.e.	0.38	0.04	**0.34**		
Employed	**b**	**-0.38**	**-0.05**	**-0.33**	**Same**	-1.92
	s.e.	**0.17**	**0.02**	**0.15**		
Employed in Skilled Occupation	**b**	-0.12	-0.02	**-0.10**	**Same**	-0.49
	s.e.	0.20	0.02	**0.18**		
Not Receiving Public Assistance	**b**	0.31	0.04	**0.27**	**Same**	1.35
	s.e.	0.20	0.02	**0.18**		
Prior Violent Misdemeanor Convictions	**b**	**0.27**	**0.04**	**0.23**	**Same**	**4.06**
	s.e.	**0.06**	**0.01**	0.05		
Not Cohabitating at Arrest	**b**	**-0.35**	**-0.04**	**-0.31**	**Same**	**-2.03**
	s.e.	**0.15**	**0.02**	**0.13**		
Old Charges Pending at Arrest	**b**	**0.89**	**0.14**	**0.75**	**Same**	**3.21**
	s.e.	**0.23**	**0.03**	**0.20**		
No Charges Filed in this Case	**b**	0.38	0.05	**0.32**	**Same**	1.33
	s.e.	0.24	0.03	**0.21**		
Convicted in this Case	**b**	-0.05	0.00	-0.05	**Same**	-0.32
	s.e.	0.15	0.02	**0.14**		
Aggregate Level						
Proportion Males	**b**	-0.36	-0.25	**-0.11**	**Same**	-0.01
	s.e.	1.97	0.35	**1.61**		
Mean Age	**b**	**-0.04**	0.00	-0.04	**Not Same**	0.54
	s.e.	**0.02**	0.00	0.02		
Proportion Same Address	**b**	0.43	-0.06	**0.49**	**Same**	-0.49
	s.e.	0.86	0.15	**0.70**		
Social Class Factor	**b**	0.03	0.01	0.02	**Same**	-1.40
	s.e.	0.10	0.01	0.09		

Table 5 - 4A: Effects of Sanctions *Crime and Delinquency* Prevalence Model

		Original	Secondary	Measures of Reproducibility		
Individual Level Sample		**3,110**	**3,110**			
Aggregate Level Sample		**207**	**204**	**(b^1 - b^2)**	**Direction & Statistical Significance**	**Clogg's Z**
Extra-Legal and Legal Characteristics						
Constant	**b**	**-1.74**	**-2.02**	0.00	**N.A.**	**N.A.**
	s.e.		**0.07**	**N.A.**	**N.A.**	**N.A.**
Male	**b**	**0.76**	**0.78**	-0.02	**Same**	-0.06
	s.e.	**0.19**	**0.19**	0.00		
Age	**b**	**-0.02**	**-0.02**	0.00	**Same**	0.33
	s.e.	**0.01**	**0.01**	0.00		
African-American	**b**	0.14	0.13	0.01	**Same**	0.08
	s.e.	0.11	0.11	0.00		
Same Residence 5+ Years	**b**	**-0.52**	**-0.52**	0.00	**Same**	0.01
	s.e.	**0.20**	**0.20**	0.00		
College Degree	**b**	-0.17	0.14	**-0.31**	**Same**	-0.93
	s.e.	0.15	0.30	**-0.15**		
Proportiion of Population at Same Residence 5+ Years	**b**	-0.63	-0.39	-0.24	**Same**	-0.27
	s.e.	0.63	0.60	0.03		
Proportion of Population with College Degree	**b**	**-1.38**	**-1.36**	**-0.02**	**Same**	-0.02
	s.e.	**0.59**	**0.56**	0.03		
Substance Abuse Problem at Arrest	**b**	-0.16	-0.15	-0.01	**Same**	-0.01
	s.e.	0.29	0.29	0.00		
Number of Prior Violent Convictions	**b**	**0.19**	**0.19**	0.00	**Same**	0.06
	s.e.	**0.03**	**0.03**	0.00		
Living with Parnter at Arrest	**b**	**0.38**	**0.37**	0.01	**Same**	0.08
	s.e.	**0.12**	**0.12**	0.00		
Old Charges Pending at Arrest	**b**	**0.87**	**0.88**	-0.01	**Same**	-0.03
	s.e.	**0.16**	**0.16**	0.00		
Court Dispositions (Dropped)						
No Charges Filed in this Case	**b**	**0.35**	**0.36**	-0.01	**Same**	-0.04
	s.e.	**0.17**	**0.18**	-0.01		
Acquitted	**b**	-0.03	-0.03	0.00	**Same**	0.02
	s.e.	0.21	0.21	0.00		
Program	**b**	-0.19	-0.20	0.01	**Same**	0.03
	s.e.	0.22	0.22	0.00		
Probation	**b**	**-0.79**	**-0.81**	0.02	**Same**	0.09
	s.e.	**0.20**	**0.20**	0.00		
Jail	**b**	0.11	0.11	0.00	**Same**	-0.01
	s.e.	0.19	0.19	0.00		
Probation and Jail	**b**	**0.69**	**0.72**	-0.03	**Same**	-0.08
	s.e.	**0.29**	**0.29**	0.00		

Table 5 - 4B: Effects of Sanctions *Crime and Delinquency* Incidence Model

		Original	Secondary	Measures of Reproducibility		
Individual Level Sample		**3,110**	**3,110**			
Aggregate Level Sample		**207**	**197**	**$(b^1 - b^2)$**	**Direction & Statistical Significance**	**Clogg's Z**
Extra-Legal and Legal Characteristics						
Constant	**b**	-1.39	**-1.96**	0.00	**N.A.**	**N.A.**
	s.e.	**N.R.**	**0.07**	**N.A.**	**N.A.**	**N.A.**
Male	**b**	**0.55**	**0.57**	0.02	**Same**	0.07
	s.e.	**0.16**	**0.15**	-0.01		
Age	**b**	**-0.02**	**-0.02**	0.00	**Same**	-0.21
	s.e.	**0.01**	**0.01**	0.00		
African-American	**b**	0.10	0.05	-0.05	**Same**	-0.41
	s.e.	0.10	0.09	-0.01		
Same Residence 5+ Years	**b**	**-0.43**	**-0.42**	0.01	**Same**	0.02
	s.e.	**0.19**	**0.17**	-0.02		
College Degree	**b**	-0.23	0.06	**0.29**	**Same**	0.37
	s.e.	0.13	0.26	**0.13**		
Proportiion of Population at Same	**b**	-0.85	-0.98	**-0.13**	**Same**	-0.52
	s.e.	0.57	0.55	-0.02		
Proportion of Population with College Degree	**b**	**-1.20**	**-1.13**	0.07	**Same**	0.19
	s.e.	**0.52**	**0.49**	-0.03		
Substance Abuse Problem at Arrest	**b**	-0.24	-0.23	0.01	**Same**	0.02
	s.e.	0.28	0.25	-0.03		
Number of Prior Violent Convictions	**b**	**0.18**	**0.16**	-0.02	**Same**	-0.79
	s.e.	**0.03**	**0.02**	-0.01		
Living with Parnter at Arrest	**b**	**0.38**	**0.39**	0.01	**Same**	0.04
	s.e.	**0.11**	**0.10**	-0.01		
Old Charges Pending at Arrest	**b**	**0.79**	**0.76**	-0.03	**Same**	-0.19
	s.e.	**0.14**	**0.11**	-0.03		
Court Dispositions (Dropped)						
No Charges Filed in this Case	**b**	**0.33**	0.26	-0.07	**Not Same**	-0.38
	s.e.	**0.16**	0.14	-0.02		
Acquitted	**b**	-0.22	-0.24	-0.02	**Same**	-0.08
	s.e.	0.20	0.18	-0.02		
Program	**b**	**-0.40**	**-0.39**	0.01	**Same**	0.02
	s.e.	**0.20**	**0.20**	0.00		
Probation	**b**	**-0.70**	**-0.73**	-0.03	**Same**	-0.12
	s.e.	**0.18**	**0.17**	-0.01		
Jail	**b**	0.07	0.07	0.00	**Same**	-0.02
	s.e.	0.17	0.15	-0.02		
Probation and Jail	**b**	**0.65**	**0.64**	-0.01	**Same**	-0.03
	s.e.	**0.25**	**0.20**	-0.05		

Table 5 - 4C: Effects of Sanctions *Crime and Delinquency* Time Model

		Original	Secondary	Measures of Reproducibility		
Individual Level Sample		**3,110**	**3,110**	$(b^1 - b^2)$	**Direction & Statistical Significance**	**Clogg's Z**
Aggregate Level Sample		**207**	**197**			
Extra-Legal and Legal Characteristics						
Male	**b**	**0.67**	**0.68**	-0.01	**Same**	-0.03
	s.e.	**0.15**	**0.15**	0.00		
Age	**b**	**-0.02**	**-0.03**	0.01	**Same**	0.45
	s.e.	**0.01**	**0.01**	0.01		
African-American	**b**	0.15	0.15	0.00	**Same**	0.01
	s.e.	0.10	0.09	0.01		
Same Residence 5+ Years	**b**	**-0.43**	**-0.43**	0.00	**Same**	-0.01
	s.e.	**0.17**	**0.17**	0.00		
College Degree	**b**	-0.08	-0.10	0.02	**Same**	0.08
	s.e.	0.12	0.27	**-0.15**		
Proportiion of Population at Same Residence 5+	**b**	**-1.04**	**-1.04**	0.00	**Same**	0.00
	s.e.	**0.49**	**0.50**	-0.01		
Proportion of Population with College Degree	**b**	**-1.06**	**-1.10**	0.04	**Same**	0.05
	s.e.	**0.48**	**0.48**	0.01		
Substance Abuse Problem at Arrest	**b**	-0.10	-0.10	0.00	**Same**	0.00
	s.e.	0.22	0.22	0.00		
Number of Prior Violent Convictions	**b**	**0.16**	**0.16**	0.00	**Same**	0.13
	s.e.	**0.02**	**0.02**	0.00		
Living with Parnter at Arrest	**b**	**0.24**	**0.24**	0.00	**Same**	0.00
	s.e.	**0.10**	**0.10**	0.00		
Old Charges Pending at Arrest	**b**	**0.72**	**0.72**	0.00	**Same**	-0.02
	s.e.	**-0.12**	**0.12**	**-0.24**		
Court Dispositions						
No Charges Filed in this Case	**b**	**0.34**	0.24	0.10	**Not Same**	0.47
	s.e.	**0.15**	0.15	0.01		
Acquitted	**b**	-0.10	-0.10	-0.01	**Same**	-0.02
	s.e.	0.18	0.18	0.00		
Program	**b**	-0.18	-0.18	0.00	**Same**	-0.01
	s.e.	0.19	0.19	0.00		
Probation	**b**	**-0.64**	**-0.64**	0.00	**Same**	0.01
	s.e.	**0.13**	**0.13**	0.00		
Jail	**b**	0.01	0.01	0.00	**Same**	0.00
	s.e.	0.15	0.15	0.00		
Probation and Jail	**b**	0.33	0.34	-0.01	**Same**	-0.02
	s.e.	0.20	0.20	0.00		

Chapter 6: Reproducing the Effects of Stakes-in-Conformity

This chapter compares the published findings reported by Wooldredge and Thistlethwaite concerning the relationship between repeat arrests and an offender's stakes-in-conformity with the findings we generated as part of our reproduction. We examine both the direct effects of stakes-in-conformity as well as the extent to which an offender's stakes-in-conformity conditions the direct effects of criminal sanctions.

Measuring Stakes-in-conformity[19]

In the 1999 NIJ Final Report and in the 2002 *JQC* article, Wooldredge and Thistlethwaite factor analyze dichotomous measures of six offender characteristics--residential stability, high school graduation, college degree, employment, skilled employment, and receiving public assistance--to create a general stakes-in-conformity factor, an education factor, and an economic factor. In the 1999 NIJ Final Report, they use the general factor as the sole measure in the three tests (prevalence, frequency and time to first re-arrest) of the effects of stakes-in-conformity on re-arrest. In the 2002 *JQC* article, they use three measures--the education factor, the economic factor, and the dichotomous measure of residential stability-- to test the effects of stakes-in-conformity and the interaction of these measures with criminal sanctions in a model of the prevalence of re-arrest.[20]

---

[19]Chapter 7 of this report includes all aggregate level measures, some of which Wooldredge and Thistlethwaite sometimes conceptualize as measures of stakes-in-conformity.

[20]In the 2002 *Criminology* article, Wooldredge uses these same six dichotomous variables in his analyses but does not conceptualize them as stakes-in-conformity and does not report their interactions with any sanction variables. Similarly, the multivariate analyses in the 2005 *Crime and Delinquency* article uses two of the six dichotomous measures but they are not conceptualized there as stakes-in-conformity.

The Direct Effects of Stakes-in-Conformity

Table 6 - 1 displays the published findings of the direct effects of these six tests of stakes-in-conformity and the findings from our reproduction of those analyses. In both the published and the reproduced analyses, the six variable factor score used in the 1999 NIJ Final Report is consistently associated with statistically significant decreases in the rates of re-arrest. In addition, the raw differences between coefficients and between effect sizes are small and not statistically significant. In the *JQC* article, there is similar consistently only for the residential stability measures. In both published and reproduced analyses, this variable is associated with statistically significant decreases in re-arrest. The relationship of the education and the economic subscales with re-arrest are both negative but not statistically significant in the published analyses; in our analyses, the education subscale is not statistically significant but the economic subscale is. Only one of the raw coefficients exceed .1 and none of the differences in the magnitude of the effect sizes are statistically significant. Thus, our analyses confirm Wooldredge and Thistlethwaite's finding about the effects of stakes-in-conformity in nine out of nine tests of reproducibility in the NIJ Final Report and in seven out of nine tests of reproducibility in the *JQC* article.

The substantive findings of the published and reproduced analyses tend to support the argument that an offender's stakes-in-conformity can be associated directly with lower rates of repeat offending. However, the strength of that effect depends on the measure of stakes-in-conformity used. The offender's residential stability, either as a direct measure, or as part of a general factor score, appears to have the most consistent relationship to decreases in re-arrest. Wooldredge and Thisltehwaite's measures of stakes-in-conformity are associated with lower rates of re-arrest in four out of six tests; in our re-analyses, this is the case for five out of six tests.

Sanctions / Stakes Interaction

In the NIJ Final Report, Wooldredge and Thistlethwaite presented 18 tests of the sanctions / stakes hypothesis. (See Table 6 - 2). In five out of 18 tests, they report there is no statistically significant effect for the interaction of stakes and criminal sanctions. In all three models, they find a consistent statistically significant negative relationship between stakes-in-conformity and being sentenced to both probation and jail. In the prevalence and frequency model, they find a similar negative and statistically significant effect for being sentenced to a treatment program. In the Time to Re-Arrest model, the interaction of sanctions and treatment program is negative but not statistically significant. Based on their findings, it appears that while most tests show no effects, some effects support the hypothesis that the effects of sanctions are stronger with offenders with more stakes.

Our reproduction of the interaction effects in the NIJ Final Report finds none of these negative associations between stakes and sanctions. Only one of our tests is statistically significant and that interaction is positive, i.e. offenders with more stakes sentenced to probation are more likely to be re-arrested than those with fewer stakes. The raw difference in 8 of the 36 coefficients (22.2%) in Table 6 - 2 are greater than .1 and level of statistical significance in six of the published effects are not consistent with the reproduced effects. In none of the 18 tests is the magnitude of the differences in the effects statistically significant. Thus, while their analyses passed most of the tests of reproducibility, in no instance can we confirm the limited support they find for the sanctions / stakes hypothesis.

In their 2002 *JQC* article, Wooldredge and Thistlethwaite provide another 12 tests of the sanctions / stakes hypothesis (See Table 6 - 3). In nine of these tests, they report no statistically significant association for the interaction of sanctions and stakes-in-conformity. They do find

support for the sanctions / stakes hypothesis in two tests–the relationship between the treatment program and residential stability and the education subscale. They also report statistically significant evidence that contradicts the hypothesis in one test–the association between the being sentenced to probation or to jail and the economic subscale.

Because the information about the short five day treatment program was not included in the archived data, we were not able to test three of the reported interaction between sanctions and stakes in this article. Among the 9 tests we could generate, we did not reproduce any of the statistically significant effects Wooldredge and Thisltethwaite report that support or contradict the sanctions / stakes hypothesis in the *JQC* article. In six of the 18 coefficients in Table 6 - 3 exceed .1 and three of the nine effects we could test did not meet the $p > .05$ level of statistical significance. In no instance were the differences in the effect magnitude statistically significant. Again, our reproduction was unable to confirm any of the associations between sanctions, stakes and re-arrest reported by Wooldredge and Thistlethwaite. However, none of the tests of differences in the size of the reported effects show statistically significant differences between their published results and our reproduced results.

Table 6 - 1: Effects of Stakes in Conformity

NIJ Final Report		Original	Secondary	Measures of Reproducibility: $(b^1 - b^2)$	Measures of Reproducibility: Direction & Statistical Significance	Measures of Reproducibility: Clogg's Z
Stakes Factor Score						
Prevalence	b	**-0.038**	**-0.058**	0.020	**Same**	0.87
	se	**0.013**	**0.019**	-0.006		
Frequency	b	**-0.024**	**-0.046**	0.022	**Same**	1.10
	se	**0.012**	**0.016**	-0.004		
Time to First Re-Arrest	b	**-0.040**	**-0.050**	0.010	**Same**	0.35
	se	**0.020**	**0.020**	0.000		
JQC Article						
Residential Stability	b	**-0.424**	**-0.534**	**0.110**	**Same**	0.41
	se	**0.176**	**0.204**	-0.028		
Education Scale	b	-0.098	-0.071	-0.027	**Same**	-0.33
	se	0.058	0.058	0.000		
Economic Scale	b	-0.045	**-0.138**	0.093	**Not Same**	1.15
	se	0.057	**0.057**	0.000		

Table 6 - 2: Final Report Sanctions / Stakes Interaction

Prevalence Model				Measures of Reproducibility		
Stakes /Sanction Interactions		Original	Secondary	$(b^1 - b^2)$	Direction & Statistical Significance	Clogg's Z
Stakes-Dropped	b	-0.039	0.056	-0.095	**Same**	-1.16
	se	0.043	0.069	-0.026		
Stakes-Acquitted	b	-0.071	0.060	**-0.131**	**Same**	-1.10
	se	0.074	0.094	-0.020		
Stakes-Program	b	**-0.144**	-0.026	**-0.118**	**Not Same**	-0.99
	se	**0.070**	0.096	-0.026		
Stakes-Probation	b	0.141	0.150	-0.009	**Same**	-0.07
	se	0.093	0.086	0.007		
Stakes-Jail	b	0.044	0.111	-0.067	**Same**	-0.59
	se	0.068	0.092	-0.024		
Stakes-Probation + Jail	b	**-0.240**	-0.156	-0.084	**Not Same**	-0.46
	se	**0.118**	0.140	-0.022		
Frequency Model						
Stakes-Dropped	b	-0.047	0.047	-0.094	**Same**	-1.44
	se	0.037	0.054	-0.017		
Stakes-Acquitted	b	-0.076	0.046	**-0.122**	**Same**	-1.20
	se	0.065	0.079	-0.014		
Stakes-Program	b	**-0.150**	-0.038	**-0.112**	**Not Same**	-1.15
	se	**0.055**	0.081	-0.026		
Stakes-Probation	b	0.071	**0.157**	-0.086	**Not Same**	-1.08
	se	0.042	**0.068**	-0.026		
Stakes-Jail	b	-0.021	0.068	-0.089	**Same**	-0.92
	se	0.064	0.073	-0.009		
Stakes-Probation + Jail	b	**-0.151**	-0.040	**-0.111**	**Not Same**	-0.94
	se	**0.075**	0.090	-0.015		
Time to Re-Arrest Model						
Stakes-Dropped	b	-0.01	0.03	-0.04	**Same**	-0.51
	se	0.05	0.06	-0.01		
Stakes-Acquitted	b	-0.03	0.03	**-0.06**	**Same**	-0.50
	se	0.08	0.08	0.00		
Stakes-Program	b	-0.07	-0.03	**-0.04**	**Same**	-0.35
	se	0.08	0.08	0.00		
Stakes-Probation	b	0.12	0.03	0.09	**Same**	0.81
	se	0.09	0.06	0.03		
Stakes-Jail	b	0.05	0.07	-0.02	**Same**	-0.17
	se	0.07	0.07	0.00		
Stakes-Probation + Jail	b	**-0.21**	-0.04	**-0.17**	**Not Same**	-1.23
	se	**0.10**	0.09	0.01		

Table 6 - 3: *JQC* Sanctions / Stakes Interaction

Prevalence Model				Measures of Reproducibility		
Stakes /Sanction Interactions		Original	Secondary	$(b^1 - b^2)$	Direction & Statistical Significance	Clogg's Z
Offender Stakes in Conformity						
Residential Stability	b	**-0.412**	**-0.507**	0.095	**Same**	0.30
	se	**0.196**	**0.246**	-0.050		
Education Scale	b	**-0.100**	-0.003	-0.097	**Not Same**	-1.10
	se	**0.050**	0.073	-0.023		
Economic Scale	b	-0.040	**-0.145**	**0.105**	**Not Same**	1.20
	se	0.050	**0.072**	-0.022		
Offender Stakes/Sanction Interactions						
Residency/No Charges	b	0.583	0.699	**-0.116**	**Same**	-0.12
	se	0.650	0.730	-0.080		
Education/No charges	b	0.008	0.000	0.008	**Same**	0.03
	se	0.182	0.240	-0.058		
Economic/No charges	b	-0.182	-0.260	0.078	**Same**	0.30
	se	0.166	0.205	-0.039		
Residency/Program	b	**-0.955**	-41.900	**40.945**	**Not Same**	0.00
	se	**0.333**	16112202.99	**-16112202.65**		
Education/Program	b	**-0.421**	-0.396	-0.025	**Not Same**	-0.07
	se	**0.213**	0.273	-0.060		
Economic/Program	b	0.171	0.051	**0.120**	**Same**	0.44
	se	0.152	0.230	-0.078		
Residency / Probation or Jail	b	0.216	0.123	0.093	**Same**	0.13
	se	0.497	0.516	-0.019		
Education / Probatioon or Jail	b	0.140	-0.205	**0.345**	**Same**	1.52
	se	0.184	0.133	0.051		
Economic / Probation or Jail	b	**0.344**	0.177	**0.167**	**Not Same**	0.74
	se	**0.176**	0.142	0.034		
Residency/Prob. Or Jail	b	-0.090	**N.A.**	**N.A.**	**N.A.**	**N.A.**
Without Program	se	0.565	**N.A.**	**N.A.**	**N.A.**	**N.A.**
Education/Prob. Or Jail	b	0.035	**N.A.**	**N.A.**	**N.A.**	**N.A.**
Without Program	se	0.176	**N.A.**	**N.A.**	**N.A.**	**N.A.**
Economic/Prob. Or Jail	b	0.122	**N.A.**	**N.A.**	**N.A.**	**N.A.**
Without Program	se	0.175	**N.A.**	**N.A.**	**N.A.**	**N.A.**

Chapter 7: Reproducing the Effects of Social Context

Direct Effects of Social Context

In this chapter, we report the extent to which we were able to reproduce the published findings in Wooldredge and Thistlethwaite about the effects of social context on re-arrest and the extent to which the effects of criminal sanctions are conditioned by the social context in which the offenders reside. In their four publications, Wooldredge and Thistlethwaite employ six different measures of social context to generate 19 tests of their direct effect on rates of re-arrest (See Table 7 - 1). In a substantial majority of tests, the direct effect of social context past all three criteria for reproducibility. The differences in 28 of the 38 raw coefficients were less than .1 and 16 of 19 reproduced effects had the same level of statistical significance as the published findings. In no instances did the differences in the size of the reported effects reach statistical significance.

In ten of the 19 tests, the published findings provide statistically significant support for the social context hypothesis. We were able to reproduce eight of those ten supportive findings. In our re-analysis of the time to first re-arrest in the NIJ Final Report, we found statistically significant effects where the published finding did not. In 9 of the 19 tests, their measure of social context was not associated with statistically significant increases or decreases in re-arrest. These mixed findings provide some limited support for the social context hypothesis but these effects depend on the type of social context measure used and the nature of the statistical model involved.

Sanctions / Social Context Hypothesis

Wooldredge and Thistlethwaite test the hypothesis that the effects of sanctions vary

depending on an offender's social context in the NIJ Final Report (See Table 7 - 2) and in the *JQC* article (See Table 7 - 3). In the NIJ Final Report, the association of the aggregate level factor score derived from six census variables with the six sanction variables is statistically significant in seven out of 18 tests; however, all of these statistically significant associations are positive, showing that locations with more favorable characteristics are associated with increased, not decreased, rates of re-arrest. In fact, all 18 effects reported in the NIJ Final report testing the interaction of sanctions and social context generate positive coefficients. These results are contrary to sanctions / social context hypothesis. All but two of the 18 effects in the reproduction analyses are also positive but none of those effects are statistically significant. Thus, we were unable to reproduce any of the statistically significant effects reported by Wooldredge and Thistlethwaite. However, the magnitude of the differences in these effects was not statistically significant in any of the 18 tests reported in Table 7 - 2.

In the *JQC* article, Wooldredge and Thistlethwaite test the interaction of two social context measures–residential stability and a social and economic factor score with four sanction variables–No Charges Filed, Treatment Program, Probation or Jail with a Short Program, and Probation or Jail without a short program (See Table 7 - 3). In these eight tests, they report that three of the four interactions with residential stability show statistically significant reductions in the prevalence of re-arrest but that none of the four interactions with the social and economic factor are statistically significant. Because of missing data, we were only able to test six of the tests and we were able to reproduce only one of the statistically significant interactions reported by Wooldredge and Thisltethwaite– residential stability and probation or jail. Of the twelve coefficients we could produce, the differences in coefficients exceed .1 eight times. On our third criteria for reproducibility–differences in the size of effects–we found no statistically significant

differences.

Our secondary analyses tends to confirm Wooldredge and Thistlethwaite's finding of mixed support for the direct effects of positive social context on reductions in re-arrest. On the other hand, we were unable to confirm their findings about either the positive or negative interactions between sanctions and social context. In only one out of 26 tests displayed in Tables 7 - 2 and 7 - 3, could we confirm that re-arrests rates are lower for offenders who live in more positive social contexts and who receive more severe sanctions.

Summary of Part Two - Reproducing Wooldredge and Thisltethwaite

In this section, we review the extent to which we have been able to reproduce Wooldredge and Thislethwaite's findings. In addition, we use the results of our reproductions to provide a new assessment about the effectiveness of sanctions, stakes-in-conformity and social context.

Reproducing Descriptive Statistics

The data archived by Wooldredge include most but not all of the variables used by in the four publications by Wooldredge and Thistlethwaite. Data were missing for one type of case disposition, three factor scores, census tract numbers and two census variables. However, we were able to construct exact or close approximations of all but the case disposition variable and we feel that the archived data provided a sound basis for attempting to reproduce the multivariate analyses of Wooldredge and Thistlethwaite. The use of summary data measuring re-arrests and the absence of dates for the initial arrest, case disposition and all re-arrests weakens the value of these data for our analyses.

Quantitative Measures of Reproducibility

We have employed data archived by Wooldredge to determine the extent to which we can reproduce the multivariate and multilevel analyses of the effects of criminal sanctions, stakes-in-conformity, and social context on repeat arrests. We identified and implemented three alternative operational criteria for determining if we can reproduce Wooldredge and Thistlethwaite's research findings. In Table 7 - 4, we report the number and percent of their statistical tests of sanction effects that passed our criteria for reproducibility. These tables reveals that we have frequently but not completely reproduced their findings. In testing the effects of criminal sanctions, we have reproduced their findings in 81.4% of the tests of differences in regression coefficients greater than .1. In 72.1% of the tests of the direction and statistical significance, we were able to reproduce their results. In 95.8% of their tests, we were able to meet our third criteria for reproducibility–differences in effects sizes that do not exceed a $p > .5$ statistical test. There are comparable percentages ranging from 66.7% to 100.0% for our efforts to reproduce tests about the stakes-in-conformity hypotheses and from 75% to 100.0% for the social context hypotheses.

The results in these three tables reveal that we met the criteria for reproducibility less frequently (66.7% to 75.0%) using the traditional measure of a consistent direction and level of statistical significance. We were able to meet the criteria for differences in raw coefficients in 81.3% to 83.3% of the tests. In 119 out 122 tests (97.5%) of the third measure of reproducibility–Clogg's Z–we were able to reproduce Wooldredge and Thistlethwaite's findings about the effects of sanctions, stakes-in-conformity and social context.

The variability in these results suggest that our three measures of reproducibility are setting different standards or taping separate aspects of reproducibility. Interestingly, in our

assessment of Wooldredge and Thistlethwaite's publications the measure with the lowest percentage of reproducibility across studies and hypotheses is the traditional method for judging consistency in findings across studies–the direction and statistical significance of findings. Thus, our innovative measures capture greater reproducibility than traditional qualitative measures. While we recommend additional work developing and testing these and other quantitative measures of reproducibility, we recommend that future efforts to measure reproducibility use one or more of these measures as a way to provide some consistency across studies of reproducibility.

Because Wooldredge and Thistlethwaite produced four separate publications, each with multiple statistical tests of each hypothesis, we have the luxury of counting the reproducibility of hundreds of individual statistical tests and to obtain an estimate of the proportion of tests which met three criteria for reproducibility. These results suggest that we have established a degree of reproducibility in the work of Wooldredge and Thistlethwaite greater than Blumstein, *et al.* (1983) found in Carlson (1980) but not as great as Vandaele (1978) found in Erhlich, (1973). However, without a larger number of formal reproductions of prior research in *Criminology* or other fields of research, there is a lack of solid empirical basis for assessing the relative success of our efforts at reproduction here. If future efforts build on our attempt to systematically define and measure reproducibility, it might be possible to better assess the extent to which percentage of reproduced findings reported here exceeds or falls below other research[21].

The Effectiveness of Criminal Sanctions

Although we think the systematic quantitative assessment of how frequently we could reproduce Wooldredge and Thistlethwaite's findings is important, it does not directly address the

[21]. Reproducibility is co-produced by three main elements-the nature of the original publication, the data documentation, and the skill and competence of the researchers producing the reproduction.

substantive nature of the differences in their findings and our findings about the effectiveness of criminal sanctions. This is particularly true with regard to the effect of the prosecution in general and the probation only sanction in particular. Wooldredge and Thistlethwaite never directly compare prosecuted against not prosecuted cases but they do use not prosecuted cases (no charges filed) as the reference group in the NIJ Final Report and as a distinct sanction category in their other analyses. In the NIJ Final Report, the not prosecuted cases are consistently associated with statistically significant increases in re-arrest compared to the treatment program and we confirmed those findings in two out three tests. In the other three publications, they report that offenders who were not charged had higher rates of re-arrest compared to various reference groups. Although the not prosecuted cases in this study constitute only 6.1 percent of the arrests, these findings seem to suggest a crime control effect for prosecution.

There also appears to be considerable evidence for a crime control effect for probation. In the 1999 NIJ Final Report, they found consistent reductions in re-arrest associated with the treatment program in the prevalence, frequency and time to first arrest models and no effect for all other sanctions. We confirm two out the three findings for the treatment program but also find consistent reductions associated with being sentenced to probation. In their *JQC,* they report that coefficient for the combine probation and jail sanction is positive but not statistically significant; our reproduction shows that the probation and jail sanction is negative and close to being statistically significant ($t = 1.93$). In the *Criminology* article, they combine all conviction sanctions together and report a negative but not statistically significant effect for these sanctions; our results are similar to theirs. In their *Crime and Delinquency* article, they report that the probation only sanction is consistently associated with reduced re-arrest and we confirm those findings. Thus, one of the major substantive differences between their findings and our findings

is that we find more consistent support for a crime control effect for the criminal sanction of probation.

In this and in other efforts at reproduction, not only is the percent of findings that can be reproduced important but also which particular findings can and cannot be confirmed. Wooldredge and Thistlethwaite report inconsistent findings for probation but our findings show probation to be more consistently associated with statistically significant reductions in re-arrest.

The Role of Stakes-in-conformity and Social Context

Wooldredge and Thistlethwaite report the direct and indirect effects of a variety of stakes-in-conformity and social context variables and we confirmed most of their findings. With one exception, none of the stakes or context variables was consistently associated with re-arrest either in the published or the reproduced findings. They reported and we confirmed that the composite factor score generated from six census tract demographic variables in the 1999 NIJ Final Report was consistently associated with statistically significant reductions in re-arrest. These findings suggest the potential value of a generalize measure of stakes-in-conformity for future research and, perhaps, that the support for these hypotheses using individual variables is so limited as not to warrant additional research. The lack of empirical support for the role of stakes-in-conformity may stem from the lack of a clear articulation of what is and is not an offender's stake in conformity and which sanctions are more or less severe.

The evidence presented by Wooldredge and Thistlethwaite and which we confirm is about evenly divided between supporting and not supporting the hypothesis that social context has a direct effect on re-arrest; however, we could not confirm their limited support for an interaction between social context and the effectiveness of sanctions. Again, the general factor score used in the 1999 NIJ Final Report had a fairly consistent association with re-arrest in their publications

and our reproduction but no single measure of social context has a consistent statistically significant association with re-arrest. This limited support is further weakened given that Wooldredge and Thistlethwaite collected 12 aggregate level measures but never use more than six of them in any particular analyses. Again, we suspect that future progress in understanding the role of social context will stem from improvements in conceptual developments from which improved empirical associations may result.

Table 7 - 1: Effects of Social Context (Page 1 of 2)

NIJ Final Report: General Factor Score		Original	Secondary	(b^1 - b^2)	Direction & Statistical Significance	Clogg's Z
Prevalence Model	b	**-0.031**	**-0.027**	-0.004	**Same**	-0.26
	se	**0.007**	**0.013**	-0.006		
Frequency Model	b	**-0.025**	**-0.026**	0.001	**Same**	0.07
	se	**0.007**	**0.012**	-0.005		
Time to First Re-Arest	b	-0.010	**-0.03**	0.02	**Not Same**	1.22
	se	0.010	**0.01**	0.00		
JQC Article Social Context Measures						
Proportion Same Address	b	**-1.013**	-0.668	**-0.345**	**Not Same**	-0.45
	se	**0.504**	0.584	-0.080		
Social Economic Factor	b	**-0.182**	**-0.126**	-0.056	**Same**	-0.67
	se	**0.050**	**0.067**	-0.017		
Criminology Census Tract Measures of Social Context						
Proportion Males	b	-0.40	-0.36	-0.04	**Same**	-0.01
	s.e.	2.28	1.97	**0.31**		
Mean Age	b	**-0.03**	**-0.04**	0.01	**Same**	0.54
	s.e.	**0.01**	**0.02**	-0.01		
Proportion Same Address	b	-0.16	0.43	**-0.59**	**Same**	-0.49
	s.e.	0.83	0.86	-0.03		
Social Class Factor	b	**-0.13**	0.03	**-0.16**	**Not Same**	-1.40
	s.e.	**0.06**	0.10	-0.04		
Criminology Neighborhood Measures of Social Context						
Proportion Males	b	1.31	-0.25	**1.56**	**Same**	-0.01
	s.e.	2.20	0.35	**1.85**		
Mean Age	b	-0.02	0.00	-0.02	**Same**	0.54
	s.e.	0.02	0.00	0.02		
Proportion Same Address	b	-0.03	-0.06	0.03	**Same**	-0.49
	s.e.	0.69	0.15	**0.54**		
Social Class Factor	b	-0.06	0.01	-0.07	**Same**	-1.40
	s.e.	0.07	0.01	0.06		

Table 7 - 1 : Direct Effects of Social Context (Page 2 of 2)

Crime and Delinquency Measures of Social Context		Original	Secondary	$(b^1 - b^2)$	Direction & Statistical Significance	Clogg's Z
Prevelance Model						
Proportion Same Address	b	-0.63	-0.67	0.04	**Same**	0.05
	s.e.	0.63	0.63	0.00		
Proportion with College Degree	b	**-1.38**	**-1.51**	**0.13**	**Same**	0.15
	s.e.	**0.59**	**0.59**	0.00		
Frequency Model						
Proportion Same Address	b	**-0.43**	**-0.42**	-0.01	**Same**	-0.05
	s.e.	**0.19**	**0.17**	0.02		
Proportion with College Degree	b	-0.23	0.06	**-0.29**	**Same**	-1.00
	s.e.	0.13	0.26	-0.13		
Time to First Re-Arrest Model						
Proportion Same Address	b	**-1.04**	**-1.04**	0.00	**Same**	0.00
	s.e.	**0.49**	**0.50**	-0.01		
Proportion with College Degree	b	**-1.06**	**-1.10**	0.04	**Same**	0.05
	s.e.	**0.48**	**0.48**	0.01		

Table 7 - 2: Final Report Sanctions / Social Context Interactions

Final Report Sanction & Social Context Models		Original	Secondary	$(b^1 - b^2)$	Direction & Statistical Significance	Clogg's Z
Prevalence Model						
Context-Dropped	b	0.026	0.019	0.007	**Same**	0.14
	se	0.024	0.046	-0.022		
Context-Acquitted	b	0.031	0.012	0.019	**Same**	0.25
	se	0.036	0.065	-0.029		
Context-Program	b	0.041	0.026	0.015	**Same**	0.19
	se	0.042	0.068	-0.026		
Context-Probation	b	0.042	-0.049	0.091	**Same**	1.19
	se	0.049	0.059	-0.010		
Context-Jail	b	**0.080**	0.015	0.065	**Not Same**	0.94
	se	**0.038**	0.057	-0.019		
Context-Probation + Jail	b	**0.145**	0.136	0.009	**Not Same**	0.09
	se	**0.051**	0.083	-0.032		
Frequency Model						
Context-Dropped	b	**0.043**	0.022	0.021	**Not Same**	0.55
	se	**0.018**	0.034	-0.016		
Context-Acquitted	b	**0.050**	0.013	0.037	**Not Same**	0.62
	se	**0.026**	0.053	-0.027		
Context-Program	b	0.050	0.029	0.021	**Same**	0.31
	se	0.031	0.059	-0.028		
Context-Probation	b	0.027	-0.039	0.066	**Same**	1.07
	se	0.040	0.046	-0.006		
Context-Jail	b	**0.085**	0.046	0.039	**Not Same**	0.73
	se	**0.032**	0.044	-0.012		
Context-Probation + Jail	b	**0.125**	0.100	0.025	**Not Same**	0.38
	se	**0.039**	0.054	-0.015		
Time to First Re-Arrest Model						
Context-Dropped	b	0.02	0.02	0.00	**Same**	-0.10
	se	0.03	0.03	0.00		
Context-Acquitted	b	0.00	0.00	0.00	**Same**	0.01
	se	0.04	0.05	-0.01		
Context-Program	b	0.03	0.01	0.02	**Same**	0.26
	se	0.04	0.05	-0.01		
Context-Probation	b	0.03	0.01	0.02	**Same**	0.37
	se	0.05	0.04	0.01		
Context-Jail	b	0.04	0.03	0.01	**Same**	0.13
	se	0.03	0.04	-0.01		
Context-Probation + Jail	b	**0.130**	0.05	0.08	**Not Same**	1.09
	se	**0.050**	0.05	0.00		

Table 7 - 3: *JQC* Sanctions / Social Context Interactions

JQC Pervalence Model		Original	Secondary	$(b^1 - b^2)$	Direction & Statistical Significance	Clogg's Z
Residency/No Charges	b	**-3.575**	-2.190	**-1.385**	**Not Same**	-0.50
	se	**1.767**	2.168	**-0.401**		
SES/No Charges	b	0.056	-0.044	**0.100**	**Same**	0.36
	se	0.164	0.225	-0.061		
Residency/Program	b	1.670	1.257	**0.413**	**Same**	0.12
	se	2.185	2.546	**-0.361**		
SES/Program	b	-0.080	-0.133	0.053	**Same**	0.16
	se	0.180	0.278	-0.098		
Residency/Probation or Jail with Program	b	**-5.409**	**-3.946**	**-1.463**	**Same**	-0.60
	se	**1.984**	**1.447**	**0.537**		
SES/Probation or Jai with Programl	b	0.245	0.047	**0.198**	**Same**	1.00
	se	0.128	0.151	-0.023		
Residency/Probation or Jail without Program	b	**-4.700**	**N.A.**	**N.A.**	**N.A.**	**N.A.**
	se	**1.952**	**N.A.**	**N.A.**	**N.A.**	**N.A.**
SES/Probation or Jail without Program	b	-7.000	**N.A.**	**N.A.**	**N.A.**	**N.A.**
	se	0.149	**N.A.**	**N.A.**	**N.A.**	**N.A.**

Part Three: A More Policy - Oriented Analysis

While the reproduction of Wooldredge and Thistlethwaite's analyses has merit in its own right, it also provides the empirical basis for the construction of alternative analyses of the crime control effects of criminal sanctions and stakes-in-conformity. Our design is intended to be more directly relevant to the sanction policies advocated in the Violence Against Women Act of 1994. Our design uses a revised measure of time to first re-arrest as part of an effort to provide more direct tests of the prosecution, conviction and sentence severity hypotheses as well as more traditional tests of the stakes-in-conformity hypotheses. Our approach restructures their detailed measures of criminal sanctions into more generalizable measures of prosecution, conviction and sentence severity and incorporates a larger number and variety of measures of an arrestee's prior criminal record. In addition, we emphasize two traditional measures of an offender's stakes-in-conformity--marriage and employment.

Our use of broader categories of criminal sanctions is designed to be more relevant to the pro-prosecution and pro-conviction policies encouraged as part of the Violence Against Women Act of 1994. We make direct comparisons with cases that are prosecuted with those that are not prosecuted and with cases that are convicted and those that are not convicted. In addition, we create a variety of measures of sanction severity and generate multivariate tests of the extent to which those measures are associated with increased, decreased or no change in the time to first re-arrest.

The final aspect of our enhanced analyses is the use of propensity scores to address the confounding of the effects of being selected for more severe sanctions and the treatment effects of the sanctions on the offender's subsequent behavior.

Chapter 8: A Revised Test of Sanctions and Stakes-in-conformity

We began the design of our analyses by reviewing in detail all the variables included in the archived data as well as those variables we constructed as part of our effort to reproduce their published findings. A listing of those variables is provided in the appendix to this chapter.

Measuring Repeat Offending

Measures of repeat offending in the data archived by Wooldredge and Thistlethwaite are limited to summary measures of official criminal history records of the number and timing of arrests that occurred after the completion of the disposition of the charges against the arrestee. We have identified the strengths and weaknesses of these data and the analytical approaches used in the Wooldredge and Thistlethwaite publications (See Chapter 2). We were particularly concerned with the limitations that stem from the fact that the 24 month prevalence and frequency measures were unavailable for 15% of the total sample and for half of the cases sentenced to probation. For this reason, we decided to use the available information on months to first new arrest for our enhanced analyses of re-arrest following prosecution for intimate partner violence[22]. For 10 cases for which the prevalence measure indicated that there was a re-arrests, the data on months to re-arrest were missing. Following the recommendations of (Little and Rubin, 1987), we replaced these missing data with the mean number of months (15) to first re-arrest derived from the 686 cases with data on time to first re-arrest.

Domains

We organized all the available information archived by Wooldredge and Thistlethwaite into four domains–personal characteristics, prior record, criminal sanctions, and social context.

[22]Because the archived data are summary measures and do not include the actual dates of arrest, case disposition or when the criminal history data were obtained, we are unable to control for the variability in time at risk in these measures.

Within each domain, we then selected individual or groups of variables based on their empirical association with our measure of repeat offending, theoretical or policy interests and the extent to which similar variables overlapped. In each of the three domains, we provide a rationale for the selection of the individual variables, a description of their bivariate relationship with the prevalence of re-arrest and the results of a domain specific multivariate Cox regression. Based on these data reduction efforts from these domain-specific findings, we construct a basic multivariate model from which we can conduct enhanced tests of the prosecution, conviction and sanction severity hypotheses as well as the hypotheses about the direct and indirect effects of stakes-in-conformity.

Personal Characteristics

In Wooldredge and Thistlethwaite's archived data, we identified 25 measures of the personal characteristics of arrestees, 10 measures of prior criminal record, 15 measures of criminal sanctions, and 10 measures of social context (See Table 8 - 1). From the 25 personal characteristics, we identified eight personal characteristics of the arrestees that warranted inclusion in our model (See Table 8 - 2). Six of the eight characteristics selected are dichotomous measures of whether the arrestee was male, African American, employed, married, had a high school degree and had lived at the same residence for five years or longer. Two characteristics–the age of the arrestee and the number of children–are measured as interval level variables. As displayed in the domain specific multivariate Cox regressions reported in Table 8 - 2, the relationship of seven of these eight personal characteristics with time to first new arrest is statistically significant. Three are associated with increases in re-arrest--being male, African American, and with more children. Four are associated with decreases in re-arrest–older arrestees, those with a high school degree, those with the same residence for five or more years,

and those employed. Several measures of employment were available but we selected a composite measure which used the several available existing variables on the length and nature of employment to determine simply if the arrestee was employed or not. One variable included in our model--marriage--was selected based on its conceptual relevance in domestic violence research and its prior use in tests of stakes-in-conformity.

Criminal Record

The archived data include nine measures of prior criminal conduct–two measures of pending charges, three measures of misdemeanor convictions, two measures of felony convictions, and two measures of prior incarcerations. We constructed a tenth measure capturing all prior convictions for violence (See Table 8 - 2). All of these measures, except the minor misdemeanor convictions, were strongly and positively associated with decreased rates of survival. We selected one measure for pending charges, one measure of prior convictions, and one measure for prior incarcerations. As displayed in the three univariate Cox regressions, all three of these measures are associated with statistically significant increases in re-arrest[23].

Case Disposition

Wooldredge and Thistlethwaite's data include seven, mutually exclusive dichotomous measures of the disposition of the current arrest[24]. In Table 8 - 3, we list those seven measures along with seven new measures we constructed from the archived data. Because of our interest in the policy and practice of prosecuting charges of intimate partner violence and obtaining

[23]Statistically significant results in Chapter 8 Tables are indicated by color–**red** for statistically significant positive effects, **blue** for statistically significant negative effects.

[24]Because Wooldredge and Thistlethwaite report some offenders in the treatment program as having been convicted, in some analyses, we group the treatment program as a post-conviction sanction. The direction and statistical significance of our findings do not change whether the offenders in the treatment program are grouped with convicted or not convicted offenders.

convictions, we constructed two dichotomous measures of criminal sanctions, which capture whether arrests were prosecuted or not and whether they resulted in convictions or not. With these measures, we expect to obtain more direct tests of the prosecution and conviction hypotheses.

We used the archived data to construct two additional dichotomous measures of sanction severity: 1) any jail time or not, and 2) sentenced to either probation or jail or not. These nonexclusive measures are intended to provide alternative tests of those case dispositions traditionally considered at the high end of severity among misdemeanor offenses. The archived data includes two interval level measures of the length of probation and the length of jail time which we employ as an additional way to test the sanction severity hypothesis[25]. While Wooldredge and Thistlethwaite's measures are mutually exclusive categories, our sanction categories overlap each other and, in some instances, Wooldredge and Thistlethwaite's categories.

The univariate Cox regression findings presented in Table 8 - 3 display the extent to which these 14 sanction measures are associated with the time to first re-arrest. Two sanction categories–acquittal and the treatment program–are not associated with re-arrest. Of the 12 statistically significant sanction variables, six are associated with lower re-arrest and six are associated with increased re-arrest. We use the prosecution and conviction variables because they provide a direct test of the prosecution and conviction hypotheses. Structuring the appropriate tests of the sanction severity hypothesis is more complicated. We chose to test four dichotomous measures of sanction severity–probation or jail, probation and jail, any jail, and jail only. Each of

[25] The length of the probation term was missing for 386 cases and the length of the jail term was missing for 26 cases. For cases with missing data, we substituted the mean of probation length (15 months) and the mean of jail length (2.1).

these measures capture important and distinct aspects of sanction severity. Dispositions to probation or to jail, regardless of the shortness or length of the sentences, are traditionally accepted as the most severe dispositions imposed for misdemeanor offenses and comparing those cases with all other dispositions is a clear test of more and less severity.

Testing the Prosecution and Conviction Hypotheses

We use the 8 variables in the personal characteristics domain and three variables in the prior record domain to construct a basic multivariate model explaining time to first re-arrest. As displayed in Table 8 - 4, for only two out of 11 variables does the direction and statistical significance of the effects reported in the domain specific models change when the arrestees personal characteristics and prior record are considered in a single model. In the combined model, the offender's race and the presence of any prior incarcerations are no longer statistically significance predictors of time to first re-arrest.

As displayed in Table 8 - 4, prosecution of arrestees is associated with longer time to re-arrest and this effect is statistically significant. In this analysis, the comparison group are those arrestees for which no charges were filed. Similarly, there are statistically significant increases in the time to re-arrest when a variable for the conviction of arrestees is added to the basic model. The comparison group for this analysis are those arrestees who were not convicted–no charges filed, charges dismissed, or acquitted. These findings provide consistent support for the prosecution and conviction hypotheses and for policies of holding intimate partner offenders accountable for their violent behavior by the filing of criminal charges and for obtaining criminal convictions.

Testing the Sanction Severity Hypothesis

Our first measure of sanction severity (See Table 8 - 5) includes all three conviction

sanctions–treatment program, probation only, and any jail time–in an analysis of all 3,662 arrests. In this analysis, neither the treatment program nor jail time is statistically significant but the probation only sanction is and is associated with reduced time-to-first arrest. The comparison group in this analysis is all arrests that did not result in a conviction.

Our second tests of the sanction severity hypothesis includes the same three sanction types but this analysis is among the 3,438 prosecuted cases. The results are nearly identical to the initial test, showing that probation is associated with statistically significant reductions in re-arrest and that jail and the treatment program are not. The comparison group in this analysis is all prosecuted cases that did not result in a conviction.

The third test combines sentences to probation and to jail as a single dichotomous variables. This analysis includes all arrested cases and shows that compared to not being convicted or sentenced to the treatment program, the group of arrestees sentenced to either probation or jail have statistically significant increased rate of survival following their sanction. The fourth test using this sample shows that survival rates for jail time are no different than the survival rates when all other dispositions, including probation, are combined. These analyses among all arrestees and among prosecuted samples show probation to be associated with increased survival and treatment program and jail to be associated with no differences in survival rates.

Our fifth and sixth tests of the sentence severity hypothesis are conducted among just the convicted offenders. These tests are designed to determine if there are statistically significant differences between the three types of sanctions–probation, jail time, or treatment program. The model used in the fifth analysis includes measures for the treatment program and jail time. The comparison group in this analysis is the probation group. The results show that, among the 1,554

convicted offenders, the time-to-first arrest is shorter for those sentenced to the treatment program or to jail compared to those sentenced to probation and that these effects are both statistically significant. In a similar analysis, our sixth test compares jail time against a combination of offenders sentenced to probation or to the treatment program. Thus, among convicted offenders, jail time is consistently associated with shorter survival times.

Separating Selection Effects from Treatment Effects

Wooldredge and Thistlethwaite (1999: p. 92; 2002: p. 51; 2005: p. 82) acknowledge an important and well-known constraint on using associations in observational data to make assertions about causal effects of sanctions on re-arrest. While multivariate and multiple level analyses help to isolate the extent to which the sanctions are associated with increases or decreases in re-arrest, they are limited in their ability to determine the extent to which the empirical association between sanctions and re-arrests stems from the process by which offenders get selected for prosecution, conviction and more severe sentences or from the treatment effects of those sanctions. This issue is particularly (but not exclusively) salient for convicted offenders who are sentenced to jail, as opposed to a treatment program or to probation.

In our earlier analyses (See Table 8 - 5, page 2) we determined that, among convicted offenders, being sentenced to jail is associated with shorter survival times; however, those multivariate analyses are insufficient to determine how much of that association stems from a selection effect–the processes by which it was decided to sentence an offender to jail--or from a treatment effect of the sanction itself. In this section, we apply established statistical procedures (Luellen, et al, 2005; D'Agostino, 1998) to the data archived by Wooldredge to estimate a convicted offenders' propensities to be sentenced to jail. We use this approach to construct an interval level measure of a convicted offender's propensity to be sentenced to jail. We interpret

this measure as a selection effect. Most of the other 29 studies examining the effectiveness of criminal sanctions reviewed in Chapter 1 do not even acknowledge the possibility that selection effects confound their results and none of those studies, including Wooldredge and Thistlethwaite, attempt any analyses to disentangle selection and treatment effects.

We considered and rejected the idea of using three propensity scores--one for the process that determines whether an arrest is prosecuted, one for the process that determines whether a prosecution results in a conviction, and one for the process that determines which convicted offenders are sentenced to jail time. We decided not to consider controlling for the propensity to be prosecuted or the propensity to be convicted because we found that both of those sanctions were associated with statistically significant increases in the time to repeat offending. Thus, in those analyses, the hypothesized effect of the sanction treatment has been established without the need to control for the possibility of a contrary effect of the selection process. Moreover, the success of this approach is dependent upon the availability of data and statistical models that explain criminal justice processing. Many of the variables of interest in explaining case processing--seriousness of the offense, nature of the evidence, victim preferences, prosecutor resources, etc.--were not collected as part of the original effort by Wooldredge and Thistlethwaite. Because of these limitations and because of the failure of prior research in this subfield to address this issue, our analyses are intended primarily as an example for future research on the effectiveness of criminal sanctions for intimate partner violence and only secondarily as a basis for an assessment of the effectiveness of criminal sanctions as a response to intimate partner violence.

Our approach to illustrating a method to separate selection and treatment effects involves developing a logistic model that used 35 variables that predict which of 1,554 convicted cases

were sentenced to jail. The available data were collected primarily to predict re-arrest and do not include many variables, such as strength of evidence or injuries to the victim, that might help us explain sentence severity. However, our model explains 16% of the variance in being sentenced to jail. From this logistic regression, we retained the value of the predicted probability of being sentenced to jail. We use this "propensity" score in two ways, to estimate the treatment effect of being sent to jail and to assess its relationship with subsequent re-arrest. Table 8 - 6 reports the results of our use of the propensity score in four Cox regression models. First, we show the simple univariate regression models of re-arrest for comparing sentenced to any jail time and a second two-variable model that displays the reduction of the effect size for any jail time in a model that also includes the propensity score for those sentenced to jail. Both variables show a positive and statistically significant association with re-arrest but the coefficient for the sentenced to jail model is reduced from .829 to .472 when the propensity measure is added to the analyses. This difference is not statistically significant.

In our third Cox regression reported in Table 8 - 6, we show the results of a twelve variable Cox regression originally using the conviction sample and including a measure of any jail time as a sanction variable[26]. In the fourth Cox regression in Table 8 - 6, we compare the results of this analysis with a model that includes the propensity score for being sentenced to jail. In this model, the jail measure is still positive and statistically significant and only slightly reduced from the third model (from .488 to .421). The propensity score measure is positive but not statistically significant. In addition, two variables that are statistically significant predictors of re-arrest in the third model–high school and prior incarceration--are not statistically significant in the fourth model in Table 8 - 6.

[26]This model was originally reported in Table 8 - 5.

We interpret these models as showing that both the treatment effect for jail and the selection effect for jail are both positive and statistically significant; when additional statistical controls are introduced, only the treatment effect is statistically significant. In this instance, the treatment effect size was reduced but remained statistically significant and positive, suggesting that jail time reduces the survival rate independent of an offender's propensity to be jailed.

Our second approach is also recommended by D'Agostino (1998). This technique emphasizes the role of propensity scores as a way to approximate the conditions of a controlled experiment. In a controlled experiment, measured and unmeasured offender characteristics are, within certain probability limits, expected to be comparable between treatment and control groups. Like most studies that rely on found data and do not have random assignment of treatment, treatment and control groups in Cincinnati data are not similar. In Table 8 - 7A, we report the extent to which there are statistically significant differences between prosecuted and not prosecuted cases, between convicted and not convicted cases and between jailed and not jailed cases on the 51 offender characteristics for which data are available. For 24 out of 50 characteristics (48.0%) there are statistically significant differences between the prosecuted and not prosecuted cases. For conviction, it is 16 out of 50 (32%) and for jail it is 37 out of 50 (74%).

We separated the 1,554 arrests that resulted in a conviction into five groups of equal size based on the propensity score developed above. Table 8 - 7B displays the number of cases for each quintile and the proportion of cases in each quintile that were actually sentenced to jail. In the last column in Table 8 - 7B, we report the percent of characteristics within each quintile for which there were statistically significant differences between the jailed and non-jailed groups.

These differences range from 4% to 10%, far less than the 74% for the entire sample.[27] In Table 8 - 7C, we report the percentage of jailed and not jailed offenders re-arrested within each quintile, for the average of the five quintiles and for the sample of convicted offenders. For four of the five quintiles, there are no statistically significant differences between the rates of re-arrest for jailed and not jailed offenders. In the fifth quintile (with the highest propensity to be sentenced to jail), the 158 offenders not jailed had an average re-arrest rate of 16.5% and the 152 offenders that had been jailed had an average re-arrest rate of 30.9%. These differences are statistically significant, as are the differences for the complete sample of convicted offenders and for the average rates for the five strata. These results suggest two conclusions. First, there is only a slight reduction in the size of the jail treatment effect when the propensity to be sentenced to jail is taken into consideration. Second, the treatment effect of jail is concentrated among those one fifth of the convicted cases with the highest propensity to be sentenced to jail.

We had anticipated that the use of propensity scores would find a greater proportion of the jail effect attributable to the selection factor but in two separate tests--the multivariate Cox regressions and in the comparison of the means of re-arrest within five strata based on the propensity score–the selection effect was not significant and the treatment effect, though reduced, remained significant. We tested for a sample selection effect and did not find one that persisted in any multivariate model. This provides some additional support for the conclusion that jail time is, in fact, a causal factor changing the pattern of repeat offending.

There are two caveats that weaken the strength of this finding. First, the available data do not include many factors which might explain a jail sentence and this limits the strength of the

[27]D'Agostino (1998) recommends conducting this diagnostic as an indication of the reduction of bias stemming from the use of the propensity score.

tests for selection effects using a propensity score. Second, the concentration of treatment effect in the fifth quintile suggests that the effect of jail is not uniform. Future research would be stronger if it could identify those types of offenders for whom jail does and does not increase repeat offending.

This and other approaches to separating selection and treatment effects need more development and testing and should be applied to research studies whose data collection strategies have a greater emphasis on explaining which offenses result in arrests, prosecutions, convictions and incarceration as well as explaining which of these aspects of criminal sanctions are associated with increased or reduced rates of re-arrest.

Stakes-in-conformity

Sherman, *et al.*, 1992; Berk, *et al.* 1992 and Pate and Hamilton, 1992 used an offender's employment and marriage status to test the stakes-in-conformity hypothesis and the sanctions / stakes hypothesis. Using these same two measures of stakes-in-conformity here, the results of our basic model (See Table 8 - 4) show that for our sample, employment is associated with increased survival rates but that marriage is not. In table 8 - 8A, we test the hypothesis that the effects of prosecution are moderated by an arrestee's stakes-in-conformity. The findings show that among employed arrestees sanctioned with prosecution there is no statistically significant association with re-arrest. The overall statistically significant association between prosecution and re-arrest persists among arrestees that are prosecuted but not employed. These findings do not support the sanctions / stakes hypothesis; in fact, the employed and prosecuted arrestees have shorter, not longer survival periods than the unemployed and prosecuted arrestees. The last panel of Table 8 - 8A shows the results of testing the sanctions / stakes hypothesis using prosecution and marriage. In this test, the association between re-arrest and prosecution is negative among both the married

and unmarried arrestees but neither of the effects is statistically significant.

As displayed in Table 8 - 8B, the effects of conviction persist when interactions for employment and marriage are added to the model; as with the prosecution model, the association of sanctioned and employed arrestees with re-arrest is positive but not statistically significant. However, in this test of the sanctions / stakes hypothesis, the association between re-arrest and convicted and married arrestees and convicted and unmarried arrestees are both negative and both statistically significant. The effectiveness of conviction is enhanced among married arrestees and these empirical findings support the sanctions / stakes hypothesis.

In Table 8 - 8C, the statistically significant and negative association between re-arrest and being sentenced to either probation or jail persists in the models testing for the interaction of sanctions and the stakes of employment and marriage. In these analyses, both the marriage and the employment interactions are not statistically significant, which do not support the argument that sanctioned arrestees with stakes will have lower levels of re-arrest. Similarly, the lack of a statistically significant effect for any jail time persists in models with marriage and employment interaction terms and both of these terms are not statistically significant. Table 8 - 8C includes two more tests of the sanctions / stakes hypothesis based on the 1,554 arrests that result in a conviction. Within this sample, neither of the sanction and stakes interaction terms is statistically significant and this provides additional evidence that do not support the sanctions / stakes hypothesis. Overall, our findings provide little support for the hypothesis that the effects of criminal sanctions are enhanced for offenders with stakes-in-conformity.

Summary of Our Revised Analyses

We used the data archived by Wooldredge (2000) to assess the three sanction hypotheses, the two hypotheses about stakes-in-conformity and the two hypotheses about the role of social

context. We structured the sanction variables to distinguish arrests that were and were not prosecuted and prosecutions that did and did not result in convictions. In separate analyses, we found consistent support for the prosecution hypothesis and for the conviction hypothesis--both of these criminal sanctions are associated with statistically significant increases in time to first re-arrest. These findings support the policies encouraging the use of prosecution and conviction promoted by the Violence Against Women Act

Compared to all other arrestees, the arrestees sentenced to probation only had significantly longer survival periods. Similarly, among only convicted offenders, survival periods were longer for offenders sentenced to probation than those sentenced to a treatment program or to jail. In separate analyses among those arrested and among those prosecuted, survival rates for arrestees sentenced to jail were no different than arrestees that were not convicted. However, among convicted offenders, when jailed offenders are compared with those sentenced to probation, the jailed offenders have significantly smaller survival rates. This finding of reduced time-to-failure for jailed offenders persisted in two analyses that controlled for an offender's propensity to be sentenced to jail.

Arrestees sentenced to treatment programs had survival rates no different than arrestees who were not convicted. Among convicted offenders, arrestees in the treatment program had survival rates higher than arrestees sentenced to probation and this effect was statistically significant.

We found that an offender's employment was associated with statistically significant increase in the survival rates but that marriage was not. Moreover, offenders with stakes-in-conformity–married or employed–were not more likely to be more responsive to criminal sanctions than offender's without stakes-in-conformity–unmarried or unemployed offenders.

These findings provide little support for the stakes in conformity or the sanctions / stakes hypotheses.

Table 8 - 1: Measures Available in Wooldredge Data (page 1 of 2)

					Standard
Personal Characteristics	N	Minimum	Maximum	Mean	Deviation
Male Arrestee	3662	0	1	0.840	0.366
Age of Arrestee	3662	18	60	32.043	9.044
African American Arrestee	3662	0	1	0.593	0.491
Arrestee Has High School Diploma	3662	0	1	0.865	0.342
Arrestee Has Some College	3662	0	1	0.209	0.407
Arrestee Has Bachelor Degree	3662	0	1	0.041	0.198
Arrestee Employed Full Time	3662	0	1	0.538	0.499
Arrestee Employed Part Time	3662	0	1	0.164	0.370
Arresteee Employed 1 Year or More	3662	0	1	0.291	0.454
Arrestee Employed 6 Months or More	3662	0	1	0.377	0.485
Arrestee Employed 3 Months or More	3662	0	1	0.438	0.496
Arrestee Employed in Skilled Labor	3662	0	1	0.181	0.385
Arrestee Received Public Assistance	3662	0	1	0.179	0.383
Arrestee Any Current Employed	3662	0	1	0.567	0.496
Arrestee Married	3662	0	1	0.330	0.470
Arrestee Never Married	3662	0	1	0.466	0.499
Arrestee Married or Widowed	3662	0	1	0.336	0.473
Arrestee Divorsed or Separated	3662	0	1	0.197	0.398
Arrest Lives with Intimate Partner	3662	0	1	0.714	0.452
Number of Children	3662	0	14	1.952	1.695
Any Children	3662	0	1	0.803	0.398
Arrestee County Resident 5+ Years	3662	0	1	0.702	0.458
Arrestee at Same Residence 5+ Years	3662	0	1	0.122	0.327
Arrestee Addicted to Alcohol or Drugs	3662	0	1	0.040	0.195
Composite Stakes Measure	3662	-6.869	11.388	0.000	3.032
					Standard
Criminal Sanctions	N	Minimum	Maximum	Mean	Deviation
No Charges Filed	3662	0	1	0.061	0.240
Charges Dropped	3662	0	1	0.450	0.498
Acquitted at Trial	3662	0	1	0.064	0.245
Treatment Program Only	3662	0	1	0.067	0.250
Probation Only	3662	0	1	0.250	0.433
Jail Only	3662	0	1	0.077	0.267
Probation and Jail	3662	0	1	0.030	0.171
Any Probation Time	3662	0	1	0.280	0.449
Any Jail Time	3662	0	1	0.108	0.310
Probation or Jail	3662	0	1	0.357	0.479
Revised Length of Probation Term	3662	0	72	4.199	7.636
Revised Length of Jail Term	3662	0	12	0.226	0.982
Prosecuted	3662	0	1	0.939	0.240
Convicted	3662	0	1	0.424	0.494

Table 8 - 2: Personal Characteristics and Prior Record

Arrestee Characteristics		Descriptive Statistics: Rearrest No	Yes	%
Male	No	538	47	8.0%
	Yes	2,548	529	17.2%
African American	No	1,294	195	13.1%
	Yes	1,792	381	17.5%
Employed	No	1,285	301	19.0%
	Yes	1,801	275	13.2%
Married	No	2,046	407	16.6%
	Yes	1,040	169	14.0%
# of Children	0	621	100	13.9%
	1	803	150	15.7%
	2	743	128	14.7%
	3	471	103	17.9%
	4	231	47	16.9%
	5	96	21	17.9%
	6	61	12	16.4%
	7	33	6	15.4%
	8	14	7	33.3%
	9	5	1	16.7%
	10	3	1	25.0%
	11	2	0	0.0%
	12	2	0	0.0%
	14	1	0	0.0%
High School Degree	No	393	101	20.4%
	Yes	2,693	475	15.0%
Same Residence for 5 Years	No	2,679	537	16.7%
	Yes	407	39	8.7%
All Arrests		3,086	576	15.7%
Mean Age of Arrestee		32.4	30.4	

Cox Regression with Personal Characteristics Only	B	SE	Sig.	Exp(B)
Male	0.889	0.153	0.000	2.433
Age	-0.024	0.005	0.000	0.976
African American	0.203	0.092	0.027	1.225
Employed	-0.359	0.085	0.000	0.698
Married	-0.040	0.096	0.679	0.961
# of Children	0.077	0.024	0.002	1.080
High School	-0.305	0.110	0.006	0.737
Resident 5 years	-0.497	0.168	0.003	0.609

-2 Log Likelihood	X^2	df	Sig.
9241.12	114.82	8	0.000

Criminal Record		Descriptive Statistics: Rearrest No	Yes	%
Any Charges Pending	No	2,909	489	14.4%
	Yes	177	87	33.0%
Any Prior Conviction for Violence	No	1,951	249	11.3%
	Yes	1,135	327	22.4%
Any Prior Incarceration	No	2,027	300	12.9%
	Yes	1,059	276	20.7%

Cox Regression with Prior Record Measures Only	B	SE	Sig.	Exp(B)
Any Charges Pending	0.856	0.117	0.000	2.353
Any Conviction for Violence	0.623	0.090	0.000	1.865
Any Incarceration	0.256	0.089	0.004	1.291

-2 Log Likelihood	X^2	df	Sig.
9230.94	155.06	3	0.000

Table 8 - 3A: Criminal Sanctions

Descriptive Statistics

Criminal Sanctions		Rearrest		
		No	Yes	%
Charges Filed	No	2921	517	15.0%
	Yes	165	59	26.3%
Charges Dropped	No	1723	290	14.4%
	Yes	1363	286	17.3%
Acquitted	No	2885	542	15.8%
	Yes	201	34	14.5%
Treatment Program Only	No	2871	545	16.0%
	Yes	215	31	12.6%
Probation Only	No	2254	494	18.0%
	Yes	832	82	9.0%
Jail Only	No	2860	519	15.4%
	Yes	226	57	20.1%
Jail and Probation	No	3002	549	15.5%
	Yes	84	27	24.3%
Prosecuted	No	165	59	26.3%
	Yes	2921	517	15.0%
Convicted	No	1729	379	18.0%
	Yes	1357	197	12.7%
Any Probation Time	No	2170	467	17.7%
	Yes	916	109	10.6%
Any Jail Time	No	2776	492	15.1%
	Yes	310	84	21.3%
Probation or Jail	No	1944	410	17.4%
	Yes	1142	166	12.7%
All Arrests		3,086	576	15.7%
		Mean	Mean	
Length of Probation Term		4.45	2.87	
Length of Jail Term		0.21	0.33	

Table 8 - 3B: Criminal Sanctions

15 Cox Regressions with One Sanction Variable Each

Sanction Measure	B	SE	Sig.	Exp(B)	-2 LL	X^2	df
No Charged	0.612	0.137	0.000	1.844	9347.24	20.44	1
Chrages Dropped	0.176	0.083	0.035	1.192	9359.67	4.47	1
Acquitted	-0.106	0.177	0.549	0.899	9363.75	0.36	1
Program Only	-0.253	0.185	0.171	0.776	9362.10	1.89	1
Probation Only	-0.717	0.119	0.000	0.488	9321.49	37.72	1
Jail Only	0.306	0.140	0.028	1.359	9359.68	4.86	1
Probation and Jail	0.570	0.197	0.004	1.768	9357.05	8.59	1
Prosecution	-0.612	0.137	0.000	0.542	9347.24	20.44	1
Conviction	-0.347	0.088	0.000	0.707	9347.98	15.79	1
Probation or Jail	-0.310	0.092	0.001	0.734	9352.34	11.41	1
Any Probation	-0.514	0.106	0.000	0.598	9338.24	23.90	1
Any Jail	0.409	0.118	0.001	1.506	9353.23	12.18	1
Probation Length	-0.028	0.007	0.000	0.972	9344.01	17.54	1
Jail Length	0.099	0.034	0.003	1.104	9356.90	8.79	1

Table 8 - 4: Prosecution and Conviction Models

Predictors	Complete Sample N = 3,662 Personal Characteristics				Complete Sample N = 3,662 Personal Characteristics and Criminal Record			
	B	SE	Sig.	Exp(B)	B	SE	Sig.	Exp(B)
Male	0.889	0.153	0.000	2.433	0.668	0.156	0.000	1.951
Age	-0.024	0.005	0.000	0.976	-0.027	0.006	0.000	0.973
African American	0.203	0.092	0.027	1.225	0.152	0.092	0.096	1.164
Employed	-0.359	0.085	0.000	0.698	-0.231	0.087	0.008	0.794
Married	-0.040	0.096	0.679	0.961	-0.004	0.096	0.970	0.996
# of Children	0.077	0.024	0.002	1.080	0.061	0.025	0.013	1.063
High School	-0.305	0.110	0.006	0.737	-0.293	0.111	0.008	0.746
Residence 5 years	-0.497	0.168	0.003	0.609	-0.374	0.169	0.027	0.688
Charges Pending					0.742	0.118	0.000	2.101
Conviction for Violence					0.572	0.092	0.000	1.772
Any Prior Incarceration					0.117	0.090	0.194	1.125
	-2 LL	X^2	df	Sig.	-2 LL	X^2	df	Sig.
Overall	9241.12	123.00	8	0.000	9153.10	223.79	11	0.000
Change						88.02	3	0.000

Predictors	Complete Sample N = 3,662 Personal Characteristics, Criminal Record and Criminal Prosecution				Complete Sample N = 3,662 Personal Characteristics, Criminal Record and Criminal Conviction			
	B	SE	Sig.	Exp(B)	B	SE	Sig.	Exp(B)
Male	0.657	0.156	0.000	1.929	0.672	0.156	0.000	1.958
Age	-0.027	0.006	0.000	0.974	-0.027	0.006	0.000	0.973
African American	0.149	0.092	0.104	1.161	0.144	0.091	0.116	1.155
Employed	-0.224	0.087	0.010	0.800	-0.232	0.087	0.007	0.793
Married	-0.001	0.096	0.988	0.999	0.016	0.096	0.867	1.016
# of Children	0.059	0.025	0.017	1.060	0.058	0.025	0.018	1.060
High School	-0.301	0.111	0.007	0.740	-0.272	0.111	0.014	0.762
Residence 5 years	-0.368	0.169	0.030	0.692	-0.382	0.169	0.024	0.683
Charges Pending	0.744	0.118	0.000	2.104	0.728	0.118	0.000	2.071
Conviction for Violence	0.544	0.093	0.000	1.723	0.586	0.092	0.000	1.796
Any Prior Incarceration	0.124	0.090	0.169	1.132	0.111	0.091	0.222	1.117
Prosecution	-0.287	0.141	0.041	0.750				
Convicton					-0.334	0.088	0.000	0.716
	-2 LL	X^2	df	Sig.	-2 LL	X^2	df	Sig.
Overall	9149.21	229.80	12	0.000	9138.34	237.79	12	0.000
Change		3.89	1	0.049		14.76	1	0.000

Table 8 - 5: Sanction Severity Models (Page 1 of 2)

Predictors	Complete Sample N = 3,662 Sanction Severity Among Arrested Offenders				Prosecuted Sample N = 3,438 Sanction Severity Among Prosecuted Offenders			
	B	SE	Sig.	Exp(B)	B	SE	Sig.	Exp(B)
Male	0.647	0.157	0.000	1.909	0.579	0.159	0.000	1.785
Age	-0.027	0.006	0.000	0.973	-0.028	0.006	0.000	0.972
African American	0.129	0.092	0.158	1.138	0.157	0.096	0.102	1.170
Employed	-0.233	0.086	0.007	0.792	-0.161	0.091	0.077	0.851
Married	0.018	0.096	0.850	1.018	0.004	0.101	0.970	1.004
# of Children	0.057	0.025	0.021	1.059	0.058	0.026	0.026	1.060
High School	-0.236	0.112	0.034	0.790	-0.249	0.116	0.032	0.779
Residence 5 years	-0.387	0.169	0.022	0.679	-0.427	0.178	0.017	0.653
Charges Pending	0.713	0.118	0.000	2.041	0.753	0.123	0.000	2.123
Conviction for Violence	0.570	0.092	0.000	1.767	0.559	0.097	0.000	1.749
Prior Incarceration	0.103	0.091	0.256	1.108	0.156	0.097	0.107	1.168
Treatment Program	-0.174	0.188	0.354	0.840	-0.142	0.189	0.454	0.868
Probation Only	-0.652	0.122	0.000	0.521	-0.625	0.125	0.000	0.535
Any Jail Time	0.032	0.122	0.791	1.033	0.053	0.124	0.669	1.054

	-2 LL	X^2	df	Sig.	-2 LL	X^2	df	Sig.
Overall	9118.6	253.2	14	0.000	8120.0	235.3	14	0.000
Change		34.5	3	0.000		31.1	3	0.000

Predictors	Complete Sample N = 3,662 Sanction Severity Probation or Jail				Complete Sample N = 3,662 Sanction Severity Any Jail			
	B	SE	Sig.	Exp(B)	B	SE	Sig.	Exp(B)
Male	0.669	0.156	0.000	1.953	0.666	0.156	0.000	1.946
Age	-0.027	0.006	0.000	0.973	-0.027	0.006	0.000	0.973
African American	0.146	0.091	0.110	1.157	0.151	0.092	0.099	1.163
Employed	-0.234	0.087	0.007	0.791	-0.230	0.087	0.008	0.794
Married	0.011	0.096	0.911	1.011	-0.005	0.096	0.958	0.995
# of Children	0.060	0.025	0.016	1.061	0.061	0.025	0.013	1.063
High School	-0.283	0.111	0.011	0.753	-0.292	0.111	0.008	0.747
Residence 5 years	-0.385	0.169	0.023	0.680	-0.374	0.169	0.027	0.688
Charges Pending	0.734	0.118	0.000	2.084	0.742	0.118	0.000	2.099
Conviction for Violence	0.591	0.092	0.000	1.806	0.569	0.092	0.000	1.766
Prior Incarceration	0.117	0.091	0.198	1.124	0.116	0.090	0.200	1.123
Probation or Jail	-0.349	0.092	0.000	0.705				
Jail Only					0.073	0.141	0.604	1.076

	-2 LL	X^2	df	Sig.	-2 LL	X^2	df	Sig.
Overall	9138.15	238.48	12	0.000	9152.84	224.07	12	0.000
Change		14.95	1	0.000		0.26	1	0.607

Table 8 - 5: Sanction Severity Models (Page 2 of 2)

Predictors	Conviction Sample N = 1,554 Treatment and Jail Among Conviction Sample				Conviction Sample N = 1,554 Jail Among Conviction Sample			
	B	SE	Sig.	Exp(B)	B	SE	Sig.	Exp(B)
Male	0.961	0.330	0.004	2.615	0.977	0.330	0.003	2.656
Age	-0.036	0.010	0.000	0.964	-0.036	0.010	0.000	0.965
African American	0.125	0.157	0.426	1.133	0.125	0.157	0.426	1.133
Employed	0.006	0.148	0.965	1.006	0.011	0.148	0.942	1.011
Married	-0.230	0.170	0.175	0.794	-0.222	0.170	0.192	0.801
# of Children	0.039	0.044	0.379	1.039	0.036	0.044	0.411	1.037
High School	-0.472	0.183	0.010	0.624	-0.458	0.182	0.012	0.633
Residence 5 years	-0.472	0.316	0.136	0.624	-0.462	0.316	0.144	0.630
Charges Pending	0.675	0.208	0.001	1.965	0.663	0.208	0.001	1.940
Conviction for Violence	0.483	0.157	0.002	1.622	0.470	0.156	0.003	1.600
Prior Incarceration	0.400	0.153	0.009	1.491	0.380	0.153	0.013	1.463
Treatment Program	0.491	0.212	0.021	1.633				
Any Jail Time	0.590	0.160	0.000	1.805	0.486	0.151	0.001	1.626
	-2 LL	X^2	df	Sig.	-2 LL	X^2	df	Sig.
Overall	2754.8	123.9	13	0.000	2759.7	119.5	12	0.000
Change		15.1	2	0.001		10.1	1	0.001

Table 8 - 6: Effects of Any Jail Time & Propensity to be Jailed

Predictors	Conviction Sample N = 1,554 Any Jail Time				Conviction Sample N = 1,554 Any Jail Time & Propensity to Be Jailed			
	B	SE	Sig.	Exp(B)	B	SE	Sig.	Exp(B)
Any Jail Time	0.829	0.144	0.000	2.290	0.472	0.155	0.002	1.603
Propensity to be Jailed					2.831	0.410	0.000	16.963
	-2 LL	X^2	df	Sig.	-2 LL	X^2	df	Sig.
	2843.8	35.0	1	0.000	2801.2	88.9	2	0.000

Predictors	Conviction Sample N = 1,554 Any Jail Time				Conviction Sample N = 1,554 Any Jail Time & Propensity to Be Jailed			
	B	SE	Sig.	Exp(B)	B	SE	Sig.	Exp(B)
Male	0.977	0.330	0.003	2.656	0.839	0.341	0.014	2.315
Age	-0.036	0.010	0.000	0.965	-0.038	0.010	0.000	0.963
African American	0.125	0.157	0.426	1.133	0.066	0.162	0.684	1.068
Employed	0.011	0.148	0.942	1.011	0.029	0.148	0.847	1.029
Married	-0.222	0.170	0.192	0.801	-0.174	0.172	0.313	0.841
# of Children	0.036	0.044	0.411	1.037	0.019	0.045	0.673	1.019
High School	-0.458	0.182	0.012	0.633	-0.243	0.226	0.282	0.785
Residence 5 years	-0.462	0.316	0.144	0.630	-0.477	0.316	0.131	0.621
Charges Pending	0.663	0.208	0.001	1.940	0.514	0.227	0.023	1.672
Conviction for Violence	0.470	0.156	0.003	1.600	0.394	0.164	0.016	1.482
Prior Incarceration	0.380	0.153	0.013	1.463	0.273	0.167	0.102	1.314
Any Jail Time	0.486	0.151	0.001	1.626	0.441	0.154	0.004	1.554
Propensity to be Jailed					1.166	0.711	0.101	3.208
	-2 LL	X^2	df	Sig.	-2 LL	X^2	df	Sig.
	2759.7	119.5	12	0.000	2757.1	123.2	13	0.000

Table 8 - 7A: Treatment Group Differences without Propensity Scores

			Characteristics with	
Sample	Treatment	Control	Stat. Sign. Differences	
Total	Not Prosecuted	Prosecuted	Number	Percent
3,662	224	3,438	24	48.0%

Total	Not Convicted	Convicted	Number	Percent
3,662	2,108	1,554	16	32.0%

Total	Not Jailed	Jailed	Number	Percent
3,662	3,268	394	37	74.0%

Conviction	Not Jailed	Jailed	Number	Percent
1,554	1,160	394	35	70.0%

Table 8 - 7B: Treatment Group Differences with Propensity Scores

				Characteristics with	
Propensity	Sample			Stat. Sign. Differences	
Quintlies	Size	Not Jailed	Jailed	Number	Percent
Lowest	311	288	23	5	10.0%
Second	311	262	49	2	4.0%
Third	311	231	80	5	10.0%
Fourth	311	221	90	3	6.0%
Highest	310	158	152	5	10.0%

Table 8 - 7C Re-arrest by Jail Sanction and Propensity Score

Propensity	Sample				Statistical
Quintiles	Size	Not Jailed	Jailed	F Test	Significance
Lowest	311	3.1%	4.3%	0.102	0.750
Second	311	6.5%	14.3%	3.541	0.061
Third	311	11.3%	16.3%	1.349	0.246
Fourth	311	15.8%	17.8%	0.175	0.676
Highest	310	16.5%	30.9%	9.214	0.003
All Cases	1.554	9.7%	21.3%	36.406	0.000

Propensity	Sample			Chi-square	Statistical
Quintiles	Size	Not Jailed	Jailed	F Test	Significance
Strata Average	1,544	10.6%	16.7%	10.405	0.001

Table 8 - 8A: Prosecution Sanction and Stakes in Conformity

Predictors	Complete Sample N = 3,662 Prosecution Model				Complete Sample N = 3,662 Prosecution and Employment Test of Stakes in Conformity			
	B	SE	Sig.	Exp(B)	B	SE	Sig.	Exp(B)
Male	0.657	0.156	0.000	1.929	0.645	0.157	0.000	1.905
Age	-0.027	0.006	0.000	0.974	-0.027	0.006	0.000	0.973
African American	0.149	0.092	0.104	1.161	0.152	0.092	0.097	1.164
Employed	-0.224	0.087	0.010	0.800	-0.426	0.212	0.045	0.653
Married	-0.001	0.096	0.988	0.999	0.000	0.096	0.999	1.000
# of Children	0.059	0.025	0.017	1.060	0.058	0.025	0.019	1.060
High School	-0.301	0.111	0.007	0.740	-0.303	0.111	0.006	0.739
Residence 5 years	-0.368	0.169	0.030	0.692	-0.366	0.169	0.030	0.693
Charges Pending	0.744	0.118	0.000	2.104	0.747	0.118	0.000	2.110
Conviction for Violence	0.544	0.093	0.000	1.723	0.544	0.093	0.000	1.724
Any Prior Incarceration	0.124	0.090	0.169	1.132	0.126	0.090	0.162	1.135
Prosecution	-0.287	0.141	0.041	0.750	-0.369	0.159	0.020	0.691
Prosecuted & Employed					0.235	0.223	0.292	1.265

Predictors	Prosecution and Marriage Test of Stakes in Conformity			
	B	SE	Sig.	Exp(B)
Male	0.661	0.157	0.000	1.936
Age	-0.027	0.006	0.000	0.974
African American	0.149	0.092	0.104	1.161
Employed	-0.226	0.087	0.009	0.798
Married	0.191	0.285	0.502	1.211
# of Children	0.059	0.025	0.018	1.060
High School	-0.300	0.111	0.007	0.741
Residence 5 years	-0.366	0.169	0.030	0.693
Charges Pending	0.746	0.118	0.000	2.108
Conviction for Violence	0.545	0.093	0.000	1.724
Any Prior Incarceration	0.120	0.091	0.184	1.128
Prosecution	-0.225	0.168	0.179	0.798
Prosecuted & Married	-0.214	0.300	0.475	0.807

Table 8 - 8B: Conviction Sanction and Stakes in Conformity

Predictors	Complete Sample N = 3,662 Conviction Model				Complete Sample N = 3,662 Conviction and Employment Test of Stakes in Conformity			
	B	SE	Sig.	Exp(B)	B	SE	Sig.	Exp(B)
Male	0.672	0.156	0.000	1.958	0.672	0.156	0.000	1.959
Age	-0.027	0.006	0.000	0.973	-0.027	0.006	0.000	0.973
African American	0.144	0.091	0.116	1.155	0.145	0.092	0.113	1.156
Employed	-0.232	0.087	0.007	0.793	-0.294	0.103	0.004	0.745
Married	0.016	0.096	0.867	1.016	0.013	0.096	0.896	1.013
# of Children	0.058	0.025	0.018	1.060	0.058	0.025	0.019	1.060
High School	-0.272	0.111	0.014	0.762	-0.269	0.111	0.015	0.764
Residence 5 years	-0.382	0.169	0.024	0.683	-0.385	0.169	0.023	0.680
Charges Pending	0.728	0.118	0.000	2.071	0.729	0.118	0.000	2.073
Conviction for Violence	0.586	0.092	0.000	1.796	0.585	0.092	0.000	1.795
Any Prior Incarceration	0.111	0.091	0.222	1.117	0.113	0.091	0.211	1.120
Conviction	-0.334	0.088	0.000	0.716	-0.424	0.120	0.000	0.655
Convicted & Employed					0.193	0.172	0.263	1.213

Predictors	Complete Sample N = 3,662 Conviction and Marriage Test of Stakes in Conformity			
	B	SE	Sig.	Exp(B)
Male	0.669	0.156	0.000	1.953
Age	-0.027	0.006	0.000	0.973
African American	0.137	0.091	0.135	1.146
Employed	-0.229	0.086	0.008	0.795
Married	0.160	0.115	0.164	1.173
# of Children	0.059	0.025	0.017	1.061
High School	-0.272	0.111	0.014	0.762
Residence 5 years	-0.377	0.169	0.026	0.686
Charges Pending	0.735	0.118	0.000	2.086
Conviction for Violence	0.588	0.092	0.000	1.800
Any Prior Incarceration	0.109	0.091	0.228	1.115
Conviction	-0.210	0.104	0.044	0.811
Convicted & Married	-0.421	0.196	0.032	0.656

Table 8 - 8C: Sanction Severity & Stakes in Conformity (p.1 of 2)

Predictors	Complete Sample N = 3,662 Probation/Jail & Employment Test of Stakes in Conformity				Complete Sample N = 3,662 Probation/Jail & Marriage Test of Stakes in Conformity			
	B	SE	Sig.	Exp(B)	B	SE	Sig.	Exp(B)
Male	0.671	0.156	0.000	1.957	0.667	0.156	0.000	1.949
Age	-0.027	0.006	0.000	0.973	-0.028	0.006	0.000	0.973
African American	0.147	0.092	0.109	1.158	0.139	0.092	0.129	1.149
Employed	-0.310	0.102	0.002	0.734	-0.233	0.087	0.007	0.792
Married	0.009	0.096	0.927	1.009	0.113	0.111	0.308	1.120
# of Children	0.059	0.025	0.017	1.061	0.060	0.025	0.015	1.062
High School	-0.278	0.111	0.012	0.757	-0.283	0.111	0.011	0.754
Residence 5 years	-0.387	0.169	0.022	0.679	-0.387	0.169	0.022	0.679
Charges Pending	0.734	0.118	0.000	2.082	0.740	0.118	0.000	2.096
Conviction for Violence	0.588	0.092	0.000	1.801	0.592	0.092	0.000	1.807
Any Prior Incarceration	0.120	0.091	0.187	1.127	0.115	0.091	0.204	1.122
Probation or Jail	-0.476	0.131	0.000	0.621	-0.244	0.108	0.024	0.783
Probation/Jail & Employed	0.260	0.185	0.158	1.298				
Probation/Jail & Married					-0.359	0.206	0.082	0.698

Predictors	Any Jail and Employment Test of Stakes in Conformity				Any Jail and Marriage Test of Stakes in Conformity			
	B	SE	Sig.	Exp(B)	B	SE	Sig.	Exp(B)
Male	0.663	0.156	0.000	1.940	0.660	0.156	0.000	1.935
Age	-0.027	0.006	0.000	0.974	-0.027	0.006	0.000	0.973
African American	0.146	0.092	0.111	1.157	0.149	0.092	0.105	1.160
Employed	-0.286	0.093	0.002	0.751	-0.231	0.086	0.008	0.794
Married	-0.006	0.096	0.952	0.994	-0.002	0.103	0.981	0.998
# of Children	0.060	0.025	0.014	1.062	0.061	0.025	0.013	1.063
High School	-0.276	0.111	0.013	0.759	-0.281	0.111	0.011	0.755
Residence 5 years	-0.374	0.169	0.027	0.688	-0.373	0.169	0.027	0.689
Charges Pending	0.734	0.118	0.000	2.083	0.739	0.118	0.000	2.094
Conviction for Violence	0.556	0.092	0.000	1.743	0.562	0.092	0.000	1.754
Any Prior Incarceration	0.118	0.090	0.193	1.125	0.114	0.090	0.206	1.121
Any Jail	0.001	0.168	0.997	1.001	0.185	0.142	0.192	1.203
Any Jail & Employment	0.383	0.235	0.104	1.466				
Any Jail & Marriage					-0.019	0.259	0.940	0.981

Table 8 - 8C Sanction Severity & Stakes in Conformity (p 2 of 2)

Predictors	Conviction Sample N = 1,554 — Jail and Employment Test of Stakes in Conformity				Conviction Sample N = 1,554 — Jail and Marriage Test of Stakes in Conformity			
	B	SE	Sig.	Exp(B)	B	SE	Sig.	Exp(B)
Male	0.978	0.330	0.003	2.658	0.979	0.330	0.003	2.662
Age	-0.036	0.010	0.000	0.965	-0.036	0.010	0.000	0.964
African American	0.123	0.157	0.434	1.131	0.122	0.157	0.438	1.130
Employed	-0.073	0.188	0.697	0.929	0.015	0.148	0.921	1.015
Married	-0.221	0.170	0.194	0.802	-0.381	0.226	0.092	0.683
# of Children	0.034	0.044	0.436	1.035	0.037	0.044	0.395	1.038
High School	-0.455	0.182	0.013	0.635	-0.456	0.182	0.012	0.634
Residence 5 years	-0.459	0.316	0.147	0.632	-0.450	0.316	0.155	0.638
Charges Pending	0.661	0.208	0.001	1.936	0.662	0.208	0.001	1.939
Conviction for Violence	0.463	0.156	0.003	1.590	0.469	0.157	0.003	1.598
Prior Incarceration	0.382	0.153	0.012	1.465	0.374	0.153	0.014	1.454
Any Jail	0.386	0.206	0.060	1.471	0.389	0.174	0.026	1.476
Any Jail & Employment	0.203	0.281	0.470	1.225				
Any Jail & Marriage					0.363	0.324	0.262	1.438

Chapter 9: Summary of Findings and Conclusions

In this chapter, we summarize three sets of findings. First, we summarize our ability to reproduce the published findings of Wooldredge and Thistlethwaite. Second, we summarize the contribution of our reproduction findings to our understanding of the effects of sanctions, stakes-in-conformity, and social context. Finally, we summarize the contribution of our revised analyses of the effects of sanctions and stakes-in-conformity on re-arrest and the implications of three results to our understanding of criminological theory and public policy. We conclude this chapter with a discussion of the implications of our findings for public policy and future research.

Data Completeness

The data archived by this project include 56 of 61 variables needed to construct all the multivariate analyses published by Wooldredge and Thistlethwaite. Two sanction measures, two demographic characteristics and the census tract number were not included in the archived data. Five factor scores were not provided in the archived data but it was possible to compute approximate measures from the raw data.

The archived data do not include information on the dates of 1) the original arrest, 2) case disposition, 3) the end of case disposition or sanction, and 4) any subsequent re-arrest. These data would have permitted more precise analyses of the time sequencing of beginning and end of criminal sanctions and subsequent repeat offending. None of Wooldredge and Thistlethwaite's published findings indicate the nature and extent of missing data in either the individual level or the aggregate level variables. The archived data would have been a more valuable resource for confirming their analyses and for conducting additional analyses if the archived data included complete information on criminal sanctions and raw data on the dates of important criminal

processing events and of re-arrests.

Wooldredge and Thistlethwaite's studies were selected for re-analysis because of the importance of the many hypotheses they address about the role of criminal sanctions, stakes-in-conformity, and social context and because of the generally high quality of the research design and statistical analyses they employed. Using three separate measures of reproducibility, we determined that most but not all of the published findings could be reproduced within certain well-specified criteria; however, several substantively important findings could not be reproduced according to one or more criteria for reproducibility.

Quantitative Measures of Reproducibility

In Table 9 - 1, we summarize these results by reporting the number and percent of their statistical tests of sanction effects that passed our criteria for reproducibility. In testing the effects of criminal sanctions, we have reproduced their findings in 81.4% of the tests of differences in regression coefficients greater than .1. In 72.1% of the tests of the direction and statistical significance, we were able to reproduce their results. In 95.8% of their tests, we were able to meet our third criteria for reproducibility–differences in effects sizes that do not exceed a $p > .5$ statistical test. There are comparable percentages ranging from 66.7% to 100.0% for our efforts to reproduce tests about the stakes-in-conformity hypotheses (See Tables 9 - 2 and 9 - 3) and from 75% to 100.0% for the social context hypotheses (See Tables 9 - 4 and 9 - 5).

The results in these three tables reveal that we met the criteria for reproducibility less frequently (66.7% to 75.0%) using the traditional measure of a consistent direction and level of statistical significance. We were able to meet the criteria for differences in raw coefficients in 81.3% to 83.3% of the tests. In 119 out 122 tests (97.5%) of the third measure of reproducibility–Clogg's Z–we were able to reproduce Wooldredge and Thistlethwaite's findings

about the effects of sanctions, stakes-in-conformity and social context.

The variability in these results suggests that our three measures of reproducibility are setting different standards or capturing separate aspects of reproducibility. In our assessment of Wooldredge and Thistlethwaite's publications, the measure with the lowest percentage of reproducibility across studies and hypotheses is the method traditionally used in literature reviews for judging consistency in findings across studies–the direction and statistical significance of findings. Thus, our innovative measures capture greater reproducibility than traditional qualitative measures. While we recommend additional work developing and testing these and other quantitative measures of reproducibility, we also recommend that future efforts to measure reproducibility use one or more of these measures as a way to provide some consistency across studies of reproducibility.

Because Wooldredge and Thistlethwaite produced four separate publications each with multiple statistical tests of each hypothesis, we have the luxury of counting the reproducibility of hundreds of individual statistical tests and to obtain an estimate of the proportion of tests which met three criteria for reproducibility. These results suggest that we have established a degree of reproducibility in the work of Wooldredge and Thistlethwaite greater than Blumstein, *et al.* (1983) found in Carlson, *et al.* (1980) but not as great as Vandaele (1978) found in Erhlich, (1973). However, without a larger number of formal reproductions of prior research in *Criminology* or other fields of research, there is a lack of solid empirical basis for assessing the relative success of our efforts at reproduction here. If future efforts build on our attempt to systematically define and measure reproducibility, it might be possible to better assess the extent to which the percentage of reproduced findings reported here exceeds or falls below other research.

The Effectiveness of Criminal Sanctions in the Published and Reproduced Analyses

The use of seven sanction types and the existence of inconsistent findings both within and between the published and the reproduced analyses precludes a simple summary of the effects of criminal sanctions. Using the traditional criterion of reproducibility, Table 9 - 6 shows that the predominant finding in both the published and reproduced analyses is that most sanction types have no effect on repeat offending. However, in six tests in three publications, the published findings show a consistent crime increasing effect for the 6% of the offenders not prosecuted. Moreover, both the published and reproduced analyses find some evidence for crime reducing effects for the 6% of offenders sentenced to the treatment program and, more consistently, for the 25% of the offenders sentenced to probation. The published findings show a consistent crime control effect for probation in the *Crime and Delinquency* article but not the NIJ Final Report; the reproduction analyses find a consistent crime control effect for probation in both analyses. Thus, one of the major substantive differences between their published findings and our findings is that we find more consistent support for a crime control effect for the criminal sanction of probation.

The reproduced analyses confirm the published findings about the rate of re-arrest for 3% of offenders sentenced to both probation and jail. In the NIJ Report, there is no effect; in the Crime and Delinquency analyses, these offenders are associated with increased rates of re-arrest.

In general, the reproduced analyses are consistent with the four published analyses–both provide some evidence of crime control effects for certain types of criminal sanctions. The published analyses find consistent support for the filing of charges and inconsistent support of the use of probation. The reproduced analyses find consistent support for the use of probation and inconsistent support for the filing of charges.

The Effectiveness of Criminal Sanctions in the Revised Analyses

Our revised analyses sought to build on the strengths of Wooldredge and Thisltethwaite's archived data to identify the extent to which there is empirical support for the three sanction hypotheses. Using Cox regression to assess the time-to-first failure for all 3,662 arrests, the revised analyses find consistent crime control effects for prosecuting arrestees, for obtaining convictions, and for sentencing arrestees to probation. Given the large proportion of offenders in this study who are prosecuted (93.9%), convicted (48.9 %), and sentenced to probation (28.0%), these consistent crime control effects are the predominant findings from this re-analysis.

Among all arrestees, the revised analyses find no effect for the most severe sanction of jail; however, among the 1,554 convicted offenders, the sanction of jail is associated with reduced time-to failure and this negative effect persists when controls are provided for the effects of selecting certain types of offenders to be jailed. Thus, among the 3% of offenders sentenced to jail, we found no support for the sanction severity hypothesis and some partial support for the contrary effect–the most severe sanction decreases the survival rate.

We interpret these findings as providing consistent support for the prosecution and convictions hypotheses–both of these less serious sanctions are associated with longer survival periods. Beyond the severity of prosecution and conviction, we found that crime rates are lower for probation than for treatment programs or for jail. Among all arrests, jail has no effect on repeat offending but, among convicted offenders, the evidence here is contrary to the sentence severity hypothesis. We emphasize the crime control effects of prosecution and conviction and probation because the numbers of offenders prosecuted, convicted and sentenced to probation are large (3,438 vs. 1,554 vs. 914). We de-emphasize the crime increasing effects of jail because the effect only exists among convicted offenders and the number of jailed offenders is relatively small (394).

When the 394 offenders sentenced to jail and 914 offenders sentenced to probation are considered as one treatment, the combined effect is still a statistically significant increase in the time-to-failure.

The Role of Stakes-in-Conformity and Social Context

Wooldredge and Thistlethwaite report the direct and indirect effects of a variety of stakes-in-conformity and social context variables and we confirmed most of their findings. With one exception--the composite factor score-- none of the stakes or context variables was consistently associated with re-arrest either in the published or the reproduced findings. They reported and we confirmed that the composite factor score generated from six census tract demographic variables in the 1999 NIJ Final Report were consistently associated with statistically significant reductions in re-arrest. These findings provide support for the stakes-in-conformity hypothesis and suggest the potential value of a general measure of stakes-in-conformity for future research. However, the published analyses and our reproduction analyses were unable to identify a single individual measure of stakes-in-conformity that was consistently associated with re-arrest rates. The lack of empirical support for specific measures of an offender's stakes-in-conformity may stem from the lack of a clear articulation of what is and is not an offender's stake in conformity as well as which sanction types are to be considered more or less severe.

The evidence presented by Wooldredge and Thistlethwaite (and which our re-analysis confirms) is about evenly divided between supporting and not supporting the hypothesis that social context has a direct effect on re-arrest. Again, the general factor score used in the 1999 NIJ Final Report had a fairly consistent association with re-arrest in their publications and our reproduction but no single measure of social context has a consistent statistically significant association with re-arrest. This limited support is further weakened given that Wooldredge and Thistlethwaite

collected 12 aggregate level measures but never use more than six of them in any particular analyses.

Our reproduction could not confirm even their limited support for the hypothesis that the effectiveness of criminal sanctions varies by the nature of the social context. Moreover, some of the effects identified by Wooldredge and Thistlethwaite and by our reproduction of their analyses are contrary to the expected direction of the sanctions / social context interaction. The predominant finding from their analyses and our reproduction is that social context, as conceived and measured in this research, does not affect repeat offending or the effectiveness of sanctions[28]. We suspect that future progress in understanding the role of social context will stem from improvements in conceptual developments from which improved empirical associations may result.

Implications for Policy

Our analyses of 3,662 arrests from Hamilton County, Ohio provides additional evidence that some criminal sanctions can slow the timing of repeat offending against intimate partners. In our analysis, the prosecution, conviction, and sentencing of offenders to probation are associated with statistically significant improvements in the timing of subsequent offending. Sentencing arrestees to treatment programs or to jail does not, on average, change the time to first re-arrest. While our findings are based on multivariate analyses of a large sample of arrests, convictions and sentences to probation, these findings stem from one analysis of arrests from one jurisdiction at one point in time. The larger body of published research on the effectiveness of criminal sanctions tends to show no crime control or crime increasing effect for criminal sanctions. Our reproduction

[28]Because our revised analyses are limited to the use of all 3,662 arrests and HLM 5.0 does not incorporate hierarchical survival models, we have not included aggregate level hypotheses in our revised analyses.

and our re-analyses move the preponderance of the evidence toward support for the effectiveness of prosecution and conviction, but our contribution is only one out of thirty published studies.

Our research findings suggest that, on average, treatment programs and jail sentences do not reduce subsequent offending. These findings conform to the general findings of Wooldredge and Thistlethwaite and to the larger research literature.

Since the larger research literature, Wooldredge and Thistlethwaite's analyses, our reproduction of their analyses, and our re-analyses provide little support for the stakes-in-conformity, the social context hypotheses, or for the hypotheses that the effects of criminal sanctions are dependent upon an offender's stakes-in-conformity or social context, the weight of the available evidence does not support developing sanctioning policies that vary according to an offender's personal stakes-in-conformity or social conditions.

Implications for Research

Future research on the effects of criminal sanctions needs to build on the strengths of the analyses presented here and the strengths of Wooldredge and Thistlethwaite's data collection and analyses. The large sample size and variety of individual and aggregate level measures provide the basis for more rigorous analyses of both direct as well as indirect effects of sanctions. We believe that the value and generalizability of future research would be strengthened by the use of designs that tested the impact of broader and more common sanctions categories as well as more particularlistic categories. Both approaches have strengths and weaknesses and ought to be used in combination.

We recommend Wooldredge and Thistlethwaite's sensitivity to the fact that most sanction types are not implemented immediately and the value of examining repeat offending after a sanction has been completely implemented. Given the variability in length of time it takes to

implement various sanctions and the likelihood of repeat offending during as well as after sanctions have been implemented, future research would be enhanced if it collected, used and archived data on the timing of criminal justice processing events and of repeat offending[29].

A major weakness of the Wooldredge and Thistlethwaite design is the absence of alternative measures of repeat offending. Future research would be stronger if it was conducted in jurisdictions with official criminal history records of sufficient detail to identify repeat offenses as well as repeat arrests, as well as distinguishing repeat offenses between intimate partners and other parties.

This research is one of many studies that demonstrate the potential of secondary analyses to improve our understanding of previously published findings and to produce new analyses testing specific hypotheses and addressing explicit policy options. The analyses produced here were possible only because of encouragement and incentives provided to the original data collectors to archive their data. Those encouragements and incentives need to be maintained and strengthened to ensure that archived data are complete, fully documented, and in sufficient detail to support exact reproductions of published analyses.

Given our inability to reproduce some of the important substantive findings of Wooldredge and Thistlethwaite, future syntheses of research findings, be they literature reviews or meta-analyses, might incorporate into their methodologies the extent to which data from prior research have been publicly archived and whether the published findings can be or have been reproduced by independent analyses.

[29]Provisions for archiving such data and conforming to legal and ethical standards for maintaining the confidentiality of research subjects have been established at the National Archive of Criminal Justice Data.

Table 9 - 1: Percent of Findings Reproduced - Effects of Sanctions

		Measure of Reproducibility					
		Coefficient Differences		Direction & Statistical Significance		Clogg's Z	
NIJ Final Report		Statistical Tests	Percent Reproduced	Statistical Tests	Percent Reproduced	Statistical Tests	Percent Reproduced
	Prevalence	12	83.3%	6	66.7%	6	83.3%
	Frequency	12	66.7%	6	50.0%	6	83.3%
	Time to First Arrest	12	66.7%	6	66.7%	6	100.0%
Journal of Quantiative Criminology							
	Prevalence	6	66.7%	3	66.7%	3	100.0%
Criminology							
	Tract Level	4	75.0%	2	50.0%	2	100.0%
	Neighborhood Level	4	25.0%	2	50.0%	2	50.0%
Crime and Delinquency							
	Prevalence	12	100.0%	6	100.0%	6	100.0%
	Frequency	12	100.0%	6	83.3%	6	100.0%
	Time to First Arrest	12	100.0%	6	83.3%	6	100.0%
Totals for Four Publications		86	81.4%	43	72.1%	43	93.0%

Table 9 - 2: Percent of Findings Reproduced - Stakes in Conformity

		Measure of Reproducibility					
		Coefficient Differences		Direction & Statistical Significance		Clogg's Z	
NIJ Final Report		Statistical Tests	Percent Reproduced	Statistical Tests	Percent Reproduced	Statistical Tests	Percent Reproduced
	Prevalence	2	100.0%	1	100.0%	1	100.0%
	Frequency	2	100.0%	1	100.0%	1	100.0%
	Time to First Arrest	2	100.0%	1	100.0%	1	100.0%
Journal of Quantiative Criminology							
	Prevalence	6	83.3%	2	66.7%	3	100.0%
Totals for Two Publications		12	91.7%	6	83.3%	6	100.0%

Table 9 - 3: Percent of Findings Reproduced - Sanction / Stakes Interaction

		Measure of Reproducibility					
		Coefficient Differences		Direction & Statistical Significance		Clogg's Z	
NIJ Final Report		Statistical Tests	Percent Reproduced	Statistical Tests	Percent Reproduced	Statistical Tests	Percent Reproduced
	Prevalence	12	83.3%	6	66.7%	6	100.0%
	Frequency	12	75.0%	6	50.0%	6	100.0%
	Time to First Arrest	12	75.0%	6	83.3%	6	100.0%
Journal of Quantiative Criminology							
	Prevalence	18	66.7%	9	66.7%	9	100.0%
Totals for Two Publications		54	83.3%	27	66.7%	27	100.0%

Table 9 - 4: Percent of Findings Reproduced - Social Context Effects

NIJ Final Report		Measure of Reproducibility: Coefficient Differences – Statistical Tests	Coefficient Differences – Percent Reproduced	Direction & Statistical Significance – Statistical Tests	Direction & Statistical Significance – Percent Reproduced	Clogg's Z – Statistical Tests	Clogg's Z – Percent Reproduced
	Prevalence	2	100.0%	1	100.0%	1	100.0%
	Frequency	2	100.0%	1	100.0%	1	100.0%
	Time to First Arrest	2	100.0%	1	0.0%	1	100.0%
Journal of Quantiative Criminology							
	Prevalence	4	75.0%	2	50.0%	2	100.0%
Criminology							
	Tract Level	8	62.5%	4	75.0%	4	100.0%
	Neighborhood Level	8	62.5%	4	100.0%	4	100.0%
Crime and Delinquency							
	Prevalence	4	75.0%	2	100.0%	2	100.0%
	Frequency	4	75.0%	2	100.0%	2	100.0%
	Time to First Arrest	4	100.0%	2	100.0%	2	100.0%
Total for Four Publications		**36**	**75.0%**	**18**	**83.3%**	**18**	**100.0%**

Table 9 - 5: Percent of Findings Reproduced - Sanctions / Context Interactions

NIJ Final Report		Measure of Reproducibility: Coefficient Differences – Statistical Tests	Coefficient Differences – Percent Reproduced	Direction & Statistical Significance – Statistical Tests	Direction & Statistical Significance – Percent Reproduced	Clogg's Z – Statistical Tests	Clogg's Z – Percent Reproduced
	Prevalence	12	100.0%	6	66.7%	6	100.0%
	Frequency	12	100.0%	6	33.3%	6	100.0%
	Time to First Arrest	12	100.0%	6	83.3%	6	100.0%
Journal of Quantiative Criminology							
	Prevalence	12	33.3%	6	83.3%	6	100.0%
Total for Two Publications		**48**	**91.7%**	**24**	**66.7%**	**24**	**100.0%**

Table 9 - 6: Published and Reproduced Findings about Sanctions (page 1 of 2)

Publication		Types of Sanctions Imposed						
							Severe Sentence	
					Convicted			
NIJ Final Report - Published			Prosecuted					
Samples	**Statistical Model**	**No Charges Filed**	**Dismissed**	**Acquitted**	**Program**	**Probation**	**Jail**	**Probation/Jail**
3,110	Prevalence	**Reference Group**	No Effect	No Effect	**Less**	No Effect	No Effect	No Effect
3,110	Frequency	**Reference Group**	No Effect	No Effect	**Less**	No Effect	No Effect	No Effect
3,662	Time to Failure	**Reference Group**	No Effect	No Effect	**Less**	No Effect	No Effect	No Effect
NIJ Final Report - Reproduced								
Samples	**Statistical Model**	**No Charges Filed**	**Dismissed**	**Acquitted**	**Program**	**Probation**	**Jail**	**Probation/Jail**
3,110	Prevalence	**Reference Group**	**Less**	No Effect	**Less**	**Less**	No Effect	No Effect
3,110	Frequency	**Reference Group**	**Less**	**Less**	**Less**	**Less**	No Effect	No Effect
3,662	Time to Failure	**Reference Group**	No Effect	No Effect	No Effect	**Less**	No Effect	No Effect
Journal of Quantitative Criminology, 2002 - Published								
						Probation and/or Jail		
Samples	**Statistical Model**	**No Charges Filed**	**Dismissed**	**Acquitted**	**Program**	**Program**	**No Program**	
3,110	Prevalence	**More**	**Reference Group**		No Effect	No Effect	No Effect	
Journal of Quantitative Criminology, 2002 - Reproduced								
						Probation and/or Jail		
Samples	**Statistical Model**	**No Charges Filed**	**Dismissed**	**Acquitted**	**Program**	**Program**	**No Program**	
3,110	Prevalence	No Effect	**Reference Group**		No Effect	No Effect	No Effect	

Key to Cell Entries	
	More - Statistically Significant Increases in Repeat Offending Reported
	Less - Statistically Signficant Decreases in Repeat Offending Reported
	No Effect - No Statistically Significant Differences in Repeat Offending Reported
	Not Tested - This variable not included in this analysis

Table 9 - 6: Published and Reproduced Findings about Sanctions (page 2 of 2)

Publication		Types of Sanctions Imposed						
							Severe Sentence	
					Convicted			
Criminology, 2002 - Published			Prosecuted					
Samples	**Statistical Model**	**No Charges Filed**	**Dismissed**	**Acquitted**	**Program**	**Probation**	**Jail**	**Probation/Jail**
1855 / 126	Prevalence-Tracts	**More**	**Reference Group**		No Effect			
1855 / 48	evalence-Neighborhoods	**More**	**Reference Group**		No Effect			
Criminology, 2002 - Reproduced								
Samples	**Statistical Model**	**No Charges Filed**	**Dismissed**	**Acquitted**	**Program**	**Probation**	**Jail**	**Probation/Jail**
1855 / 126	Prevalence-Tracts	No Effect	**Reference Group**		No Effect			
1855 / 48	evalence-Neighborhoods	No Effect	**Reference Group**		No Effect			
Crime and Delinquency, 2005 - Published								
Samples	**Statistical Model**	**No Charges Filed**	**Dismissed**	**Acquitted**	**Program**	**Probation**	**Jail**	**Probation/Jail**
3,110	Prevalence	**More**	**Reference Group**	No Effect	No Effect	**Less**	No Effect	**More**
3,110	Frequency	**More**	**Reference Group**	No Effect	**Less**	**Less**	No Effect	**More**
3,662	Time to Failure	**More**	**Reference Group**	No Effect	No Effect	**Less**	No Effect	No Effect
Crime and Delinquency, 2005 - Reproduced								
Samples	**Statistical Model**	**No Charges Filed**	**Dismissed**	**Acquitted**	**Program**	**Probation**	**Jail**	**Probation/Jail**
3,110	Prevalence	**More**	**Reference Group**	No Effect	No Effect	**Less**	No Effect	**More**
3,110	Frequency	No Effect	**Reference Group**	No Effect	**Less**	**Less**	No Effect	**More**
3,662	Time to Failure	No Effect	**Reference Group**	No Effect	No Effect	**Less**	No Effect	No Effect

Key to Cell Entries	
	More - Statistically Significant Increases in Repeat Offending Reported
	Less - Statistically Signficant Decreases in Repeat Offending Reported
	No Effect - No Statistically Significant Differences in Repeat Offending Reported
	Not Tested - This variable not included in this analysis

Table 9 - 7: Published and Reproduced Findings about Stakes (Page 1 of 2)

Publication		General Factor Score					
NIJ Final Report - Published		Five Year Resident	**Education Factor**		**Economic Factor**		
Samples	**Statistical Model**		High School	College	Employed	Skilled Employment	Not Receiving Public Assistance
3,110	Prevalence	**Less**					
3,110	Frequency	**Less**					
3,662	Time to Failure	**Less**					
NIJ Final Report - Reproduced		Five Year Resident	**Education Factor**		**Economic Factor**		
Samples	**Statistical Model**		High School	College	Employed	Skilled Employment	Not Receiving Public Assistance
3,110	Prevalence	**Less**					
3,110	Frequency	**Less**					
3,662	Time to Failure	**Less**					
JQC, 2002 - Published			**Education Factor**		**Economic Factor**		
Samples	**Statistical Model**	Five Year Resident	High School	College	Employed	Skilled Employment	Not Receiving Public Assistance
3,110	Prevalence	**Less**	No Effect		No Effect		
JQC, 2002 - Reproduced		Five Year Resident	High School	College	Employed	Skilled Employment	Not Receiving Public Assistance
Samples	**Statistical Model**						
3,110	Prevalence	**Less**	No Effect		**Less**		

Table 9 - 7: Published and Reproduced Findings about Stakes (Page 2 of 2)

***Criminology,* 2002 - Published**		**Measures Not Conceptualized as Stakes in Conformity**					
Samples	**Statistical Model**	Five Year Resident	High School	College	Employed	Skilled Employment	Not Receiving Public Assistance
1855 / 126	Prevalence-Tracts	No Effect	**Less**	No Effect	**Less**	No Effect	No Effect
1855 / 48	Prevalence-Neighborhoods	No Effect	**Less**	No Effect	**Less**	No Effect	No Effect

***Criminology,* 2002 - Reproduced**		Five Year Resident	High School	College	Employed	Skilled Employment	Not Receiving Public Assistance
Samples	**Statistical Model**						
1855 / 126	Prevalence-Tracts	No Effect	**Less**	No Effect	**Less**	No Effect	No Effect
1855 / 48	Prevalence-Neighborhoods	No Effect	No Effect	No Effect	**Less**	No Effect	No Effect

Crime and Delinquency, 2002		**Measures Not Conceptualized as Stakes in Conformity**					
Samples	**Statistical Model**	Five Year Resident	High School	College	Employed	Skilled Employment	Not Receiving Public Assistance
3,110	Prevalence	**Less**	Not Tested	No Effect	Not Tested	Not Tested	Not Tested
3,110	Frequency	No Effect	Not Tested	**Less**	Not Tested	Not Tested	Not Tested
3,662	Time to Failure	**Less**	Not Tested	No Effect	Not Tested	Not Tested	Not Tested

Crime and Delinquency, 2002		Five Year Resident	High School	College	Employed	Skilled Employment	Not Receiving Public Assistance
Samples	**Statistical Model**						
3,110	Prevalence	**Less**	Not Tested	No Effect	Not Tested	Not Tested	Not Tested
3,110	Frequency	No Effect	Not Tested	**Less**	Not Tested	Not Tested	Not Tested
3,662	Time to Failure	**Less**	Not Tested	No Effect	Not Tested	Not Tested	Not Tested

Table 9 - 8: Published and Reproduced Findings about Social Contexts (p. 1 of 2)

NIJ Final Report - Published		Social Contexts
Samples	**Statistical Model**	General Factor Score
3,110	Prevalence	**Less**
3,110	Frequency	**Less**
3,662	Time to Failure	No Effect

NIJ Final Report - Reproduced		Social Contexts
Samples	**Statistical Model**	General Factor Score
3,110	Prevalence	**Less**
3,110	Frequency	**Less**
3,662	Time to Failure	**Less**

JQC, 2002 - Published		Social Contexts	
Samples	**Statistical Model**	Same Residence	Social/Economic
3,110	Prevalence	**Less**	**Less**

JQC, 2002 - Reproduced		Social Contexts	
Samples	**Statistical Model**	Same Residence	Social/Economic
3,110	Prevalence	No Effect	**Less**

Table 9 - 8: Published and Reproduced Findings about Social Contexts (p. 2 of 2)

Criminology, 2002 - Published		Social Contexts			
Samples	**Statistical Model**	Same Residence	Social Class	Proportion Males	Mean Age
1855 / 126	Prevalence-Tracts	No Effect	**Less**	No Effect	**Less**
1855 / 48	Prevalence-Neighborhoods	No Effect	No Effect	No Effect	No Effect

Criminology, 2002 - Reproduced		Social Contexts			
Samples	**Statistical Model**	Same Residence	Social Class	Proportion Males	Mean Age
1855 / 126	Prevalence-Tracts	No Effect	No Effect	No Effect	**Less**
1855 / 48	Prevalence-Neighborhoods	No Effect	No Effect	No Effect	No Effect

C & D, 2002 - Published		Social Contexts	
Samples	**Statistical Model**	Same Residence	College Degree
3,110	Prevalence	No Effect	**Less**
3,110	Frequency	**Less**	No Effect
3,662	Time to Failure	**Less**	**Less**

C & D, 2002 - Reproduced		Social Contexts	
Samples	**Statistical Model**	Same Residence	College Degree
3,110	Prevalence	No Effect	**Less**
3,110	Frequency	**Less**	No Effect
3,662	Time to Failure	**Less**	**Less**

Key to Cell Entries	
	More - Statistically Significant Increases in Repeat Offending Reported
	Less - Statistically Signficant Decreases in Repeat Offending Reported
	No Effect - No Statistically Significant Differences in Repeat Offending Reported
	Not Tested - This variable not included in this analysis

Table 9 - 9: Published & Reproduced Findings About Sanctions/ Context Interactions

NIJ Report, 1999 - Published		Sanctions						
	Statistical Model	No Charges Filed	Dismissed	Acquitted	Program	Probation	Jail	Probation/Jail
	Prevalence	Reference	No Effect	No Effect	No Effect	No Effect	**More**	**More**
	Frequency	Reference	**More**	**More**	No Effect	No Effect	**More**	**More**
	Time to Failure	Reference	No Effect	No Effect	No Effect	No Effect	No Effect	**More**

NIJ Report, 1999 - Reproduced		Sanctions						
	Statistical Model	No Charges Filed	Dismissed	Acquitted	Program	Probation	Jail	Probation/Jail
	Prevalence	Reference	No Effect	No Effect	No Effect	No Effect	No Effect	No Effect
	Frequency	Reference	No Effect	No Effect	No Effect	No Effect	No Effect	No Effect
	Time to Failure	Reference	No Effect	No Effect	No Effect	No Effect	No Effect	No Effect

Journal of Quantitative Criminology, 2002 - Published							
Prevalence		Sanctions					
	Social Context	No Charges Filed	Dismissed	Acquitted	Program	Probation/Jail	Probation/Jal/Program
	Same Residence	**Less**	Referrence		No Effect	**Less**	**Less**
	Social Economic Status	No Effect	Referrence		No Effect	No Effect	No Effect

Journal of Quantitative Criminology, 2002 - Reproduced							
Prevalence		Sanctions					
	Social Context	No Charges Filed	Dismissed	Acquitted	Program	Probation/Jail	Probation/Jal/Program
	Same Residence	No Effect	Referrence		No Effect	**No Data**	**Less**
	Social Economic Status	No Effect	Referrence		No Effect		No Effect

The Crime Control Effects of Prosecuting Intimate Partner Violence in Hamilton County, Ohio Bibliography

Allison, Paul D. 2001. *Missing Data*. Series on Quantitative Applications in the Social Sciences, vol. 136. Thousands Oaks, CA: Sage Publication.

Belknap, Joanne and Cris M. Sullivan. 2002. *Longitudinal Study of Battered Women in the System: The Victims' and Decision-Makers' Perceptions.* Final Report submitted to the National Institute of Justice. Boulder, CO: University of Colorado. 99.

Berk, Richard A., and Phyllis J. Newton. 1985. "Does Arrest Really Deter Wife Battery? An Effort to Replicate the Findings of the Minneapolis Spouse Abuse Experiment". *American Sociological Review* 50(2):253-262.

Berk, Richard A., Alec Campbell, Ruth Klap, and Bruce Western. "The Deterrent Effect of Arrest in Incidents of Domestic Violence: A Bayesian Analysis of Four Field Experiments." *American Journal of Sociology* 57 (October):698-708.

Blumstein, Albert J., Jacqueline Cohen, and William Gooding. 1983. "Influence of Capacity on Prison Population - A Critical Review of Some Recent Evidence." *Crime & Delinquency* 29(1):1-15.

Blumstein, Alfred, Jacqueline Cohen, Susan E. Martin, and Michael Tonry, Editors. 1982. *Research on Sentencing: The Search for Reform*. Panel on Sentencing Research, National Research Council. Washington, DC: National Academy Press.

Blumstein, Alfred, Jacqueline Cohen and Daniel Nagin. 1978. *Deterrence and Incapacitation: Estimating the Effects of Criminal Sanctions on Crime Rates*. Washington, D.C.: National Academy of Press.

Blumstein, Alfred, Jacqueline Cohen and Daniel Nagin. 1978. *Deterrence and Incapacitation: Estimating the Effects of Criminal Sanctions on Crime Rates*. Washington, D.C.: National Academy of Press

Blumstein, Alfred, Jacqueline Cohen, Jeffery Roth, and Christy Visher, Eds. 1986. *Criminal Careers and "Career Criminals.* Washington, D.C.: National Academy of Press. 1261--93.

Boruch, Robert F., Paul M. Wortman, David S. Cordray, and Associates. 1981. *Re-Analyzing Program Evaluations. San Francisco: Jossey - Bass Publishers.:*

Bryant, F. B. and Paul M. Wortman (1978), "Secondary Analysis: The Case for Data Archives," *American Psychologist*, 33:381-387.

Bryk, Anthony S. and Stephen W. Raudenbush. 1992. *Hierarchical Linear Models: Applications and Data Analysis Methods*. Advanced Quantitative Techniques in the Social Sciences, vol. 1. Newbury Park, CA: Sage Publications.

Buzawa, Eve, Gerald T. Hotaling, Andrew Klein, and James Byrne. 1999. *Response to Domestic Violence in a Pro-Active Court Setting*. Washington, D.C.: National Institute of Justice.

Cahn, Naomi R. 1992. "Innovative Approaches to the Prosecution of Domestic Violence Crimes: An Overview." Pp. 161-79 in *Domestic Violence: The Changing Criminal Justice Response*, edited by Buzawa, Eve S. and C. G. Buzawa. Wesport, CT: Greenwood Publishing.

Cahn, Naomi R. and Lisa G. Lerman. 1991. "Prosecuting Women Abuse." Pp. 95-112 in *Women Battering: Policy Response*. Cincinnati, OH: Anderson Publishing Co.

Carlson, Kenneth, Patricia Evans, John Flanagan, and Associates Abt. 1980. "American Prisons and Jails. Volume II: Population Trends and Projections." Washington, DC: U.S. National Institute of Justice.

Chalk, Rosemary A., and Patricia A. King, (Eds). 1998. *Violence in Families: Assessing `Prevention and Treatment Programs. Committee on the Assessment of Family Violence Intervention*, Board on Children, Youth, and Families, National Research Council and Institute of Medicine. Washington, DC: National Academy of Science.

Clogg, Clifford C., Eva Petkova, and Adamantios Haritou. 1995. "Statistical Methods for Comparing Regression Coefficients Between Models." *American Journal of Sociology* 100(5, March):

Cordray, David S. and R. C. Orwin (1983), "Improving the Quality of Evidence: Intersections Between Primary Evaluation, Secondary Analysis, and Quantitative Synthesis," in R. J. Light, *Evaluation Studies Review Annual*, 8:91-119.

Crowell, Nancy A. and Ann W. Burgess, Editors. 1996. *Understanding Violence Against Women*. Panel on Research on Violence Against Women, Committee on Law and Justice, Commission on Behavioral and Social Science and Education, National Research Council. Washington, DC: National Academy of Sciences.

Crutchfield, Robert D., George S. Bridges, and Susan R. Pitchford. 1994. "Analytical and Aggregation Biases in Analyses of Imprisonment: Reconciling Discrepancies in Studies of Racial Disparity." *Journal of Research in Crime and Delinquency* 31(2, May):166-82.

D'Agostino, Jr., Raph B. 1998. "Tutorial in Biostatistics: Propensity Score Methods for Bias Reduction in the Comparison of Treatment to a Non-Randomized Control Group." *Statistics in Medicine* 17:2265-81.

Davis, Robert C. and Christopher D. Maxwell. 2002. *Preventing Repeat Incidents of Family Violence: A Re-analysis of Data from Three Field Tests*. Final Report Submitted to National Institute of Justice. New York City: Vera Institute of Justice.

Davis, Robert C., Barbara E. Smith, and Laura B. Nickles. 1998. "The Deterrent Effect of Prosecuting Domestic Violence Misdemeanors." *Crime & Delinquency* 3(44, July):434--442.

Dunford, Franklyn W. 1990. "System-Initiated Warrants for Suspects of Misdemeanor Domestic Assault: A Pilot Study." *Justice Quarterly* 7(4, December):631-53.

Dunford, Franklyn W., David Huizinga, and Delbert S. Elliott. 1990. "The Role of Arrest in Domestic Assault: The Omaha Police Experiment." *Criminology* 28(2):183-206.

Dutton, Donald G. 1987. "The Criminal Justice Response to Wife Assault." *Law and Human Behavior* 11(3):189-206.

Eckberg, Deborah A. and Marcy R. Podkopacz. 2002. *Domestic Violence Court: Case Processing Update and Recidivism Analysis*. Fourth Judicial District of the State of Minnesota. Hennepin County, MN: Hennepin County District Court Research Division.

Ehrlich, Isaac. 1973. "Participation in Illegitimate Activities: A Theoretical and Empirical Investigation." *Journal of Political Economy*, May/June:521-64.

Elliott, Delbert. S. 1990. Personal Communication with the first author.

Fagan, Jeffrey A. 1988. "Contributions of Family Violence Research to Criminal Justice Policy on Wife Assault: Paradigms of Science and Social Control." *Violence and Victims* 3(3):159-86.

———. 1996. "The Criminalization of Domestic Violence: Promises and Limits." Presented at

the Conference on Criminal Justice Research and Evaluation, January. Washington, D.C.: National Institute of Justice.

Fagan, Jeffrey, E. Friedman, S. Wexler, and V. L Lewis. 1984. *National Family Violence Evaluation*. Final Report submitted to the National Institute of Justice, San Francisco: Urban and Rural Systems Associates. 315.

Feeney, Floyd and Forrest Dill, Adrienne Weir. 1983. *Arrests Without Conviction: How Many and Why They Occur*. U.S. Government Printing Office.

Feinberg, Stephen E., Margaret E. Martin, and Miron L. Straf, Editors. 1985. *Sharing Research Data*. Washington, D.C.: National Research Council.

Ford, David A. and Mary Jean Regoli. 1992. "The Preventive Impact of Policies for Prosecuting Wife Batterers." Pp. 181-208 in *Domestic Violence: The Changing Criminal Justice Response*, edited by E. Buzawa and C. Buzawa. Westport, Conn.: Auburn House.

Ford, David A. and M. J. Regoli. 1993. *Indianapolis Domestic Violence Prosecution Experiment*. Final Report submitted to the National Institute of Justice, Indianapolis: Indiana University. 88.

Ford, David A., Ruth Richard, Stephen Goldsmith, and Mary Jean Regoli. 1996. "Future Directions for Criminal Justice Policy on Domestic Violence." Pp. 243-65 in *Do Arrests and Restraining Orders Work?* edited by E. S. Buzawa and C. G. Buzawa. Thousand Oaks, CA: Sage Publications.

Ford, David A. And Susan Breall. 2000. *Violence Against Women: Synthesis of Research for Prosecutors*. Washington, D.C.: National Institute of Justice.

Friday, Paul C., Vivian B. Lord, M. Lyn Exum, and Jennifer L. Hartman. 2006. *Evaluating the Impact of a Specialized Domestic Violence Police Unit*. Final report submitted to the National Institute of Justice. Charlotte, NC: University of North Carolina at Charlotte.

Frisch, Lisa A., Melissa I. Mackey, Donna Hall, and Alissa Polllitz Worden. 2001. *Family Protection and Domestic Violence: Intervention Act of 1994: Evaluation of the Mandatory Arrest Provisions*. Albany, NY: Division of Criminal Justice Services, New York State. 109.

Garner, Joel H. 1981. "National Institute of Justice: Access and Secondary Analysis." Pp. 43-49 in *Reanalyzing Program Evaluation*, edited by R. F. Boruch, P. M. Wortman and D. Cordray. San Francisco: Jossey-Bass Inc.

Garner, Joel, Jeffrey A. Fagan, and Christopher D. Maxwell. 1995. "Published Findings from the Spouse Assault Replication Program: A Critical Review." *Journal of Quantitative Criminology* 11(1, Mar):3-28.

Garner, Joel H. and Christopher D. Maxwell. 2000. "What Are the Lessons of the Police Arrest Studies?" *Journal of Aggression, Maltreatment & Trauma* 4(1):83-114.

Garner, Joel H., Christopher Maxwell, and Cedrick Heraux. 2002. "Characteristics Associated with the Prevalence and Severity of Force Used by the Police." *Justice Quarterly*, 19:4, pp. 705-746.

Garner, Joel H. and Christopher D. Maxwell. 2005. The Effects of Prosecution on Intimate Partner Violence. Proposal submitted to the National Institute of Justice. Shepherdstown, WV: Joint Centers for Justice Studies, Inc.

Glueck, Sheldon and Eleanor Glueck. 1950. *Unraveling Juvenile Delinquency*. Cambridge, Mass.: Harvard University Press.

Gross, Melissa, Elizabeth P. Gramer, Janett Forte, Tara Kunkel, and Laura J. Moriarty. 2000.

"The Impact of Sentencing Options on Recidivism Among Domestic Violence Offenders: A Case Study." *American Journal of Criminal Justice* 24(2):301-12.

Hartley, Carolyn and Lisa Frohmann. 2003. *Cook County Target Abuser Call (TAC): An Evaluation of a Specialized Domestic Violence Court*. Final Report submitted to the National Institute of Justice. Iowa City: University of Iowa.

Hirschel, David and Ira Hutchison. 2001. "The Relative Effects of Offense, Offender and Victim Variables on the Decision to Prosecute Domestic Violence Cases." *Violence Against Women* 7(1):46-59.

Hyman, Harold, 1972. *Secondary Analysis of Sample Surveys*. New York: Wiley.

Jaffe, Peter G., Blaine Hastings, Deborah Reitzel, and Gary W. Austin. 1993. "The Impact of Police Laying Charges." Pp. 62-95 in *Legal Responses to Wife Assault: Current Trends and Evaluation*, edited by N. Zoe Hilton. Newbury Park, CA: Sage Publications.

Jacoby, Joan E. 1975. *Issues in Pre-Trial Screening*. Report submitted to the National Institute of Justice, Bureau of Social Science Research, Inc. 48.

Jolin, Annette, Feyerherm, Robert Fountain, and Sharon Friedman. 1998. *Beyond Arrest: The Portland Oregon Domestic Violence Experiment*. Final Report submitted to the National Institute of Justice, Portland, OR: Portland State University. 194.

Kingsnorth, Rodney. 2006. "Intimate Partner Violence: Predictors of Recidivism in a Sample of Arrestess." *Violence Against Women* 12(10):917-35.

Kingsnorth, Rodney F., Randall C. MacIntosh, Tereceira Berdahl, Carrie Blades, and Steve Rossi. 2001. "Domestic Violence: The Role of Interracial Dyads in Criminal Court Processing." *Journal of Contemporary Criminal Justice* 17(2):123-41.

Kingsnorth, Rodney F., Randall C. MacIntosh, and Sandra Sutherland. 2002. "Criminal Charge or Probation Violation? Prosecutorial Discretion and Implications for Research in Criminal Court Processing." *Criminology* 40(3): 553-577.

Kruttschnitt, Candace, Brenda L. McLaughlin, and Carol Petrie (eds). 2004. *Advancing the Federal Research Agenda on Violence Against Women*, Washington, D.C.: National Academy Press.

Lerman, Lisa G. 1986. "Prosecution of Wife Beaters: Institutional Obstacles and Innovations." In *Violence in the Home: Interdisciplinary Perspectives*, edited by M. Lystad. New York: Brunner-Mazel.

Levitt, Steven D. 1997. "Using Electoral Cycles in Police Hiring to Estimate the Effect of Police on Crime." *American Economic Review* 87:1230 - 1250.

Lipsey, Mark W. 1990. *Design Sensitivity: Statistical Power for Experimental Research*. Newbury Park, CA: Sage Publications.

Little, Roderick. J. and Donald B. Rubin.1987. *Statistical Analysis with Missing Data*. New York: John Wiley.

Luellen, Jason, K., William R. Shadish, and M. H. Clark. 2005. "Propensity Scores: An Introduction and Experimental Test." *Evaluation Review* 29(6):530-58.

Maloney, Micheal E. and Janet R. Buelow. 1997. *The Social Areas of Cincinnati: An Analysis of Social Needs (3rd Edition) Patterns for Three Census Decades*. Cincinnati: School of Planning, University of Cincinnati. 149.

Maltz, Michael. 1984. *Recidivism*. Orlando, Florida: Academic Press, Inc.

Marsland, Lauren, Darryl Plecas, and Tim Segger. 2001. *Reticence and Re-Assault Among Victims of Domestic Violence in a Pro-Charge Jurisdiction*. 36.

Maxwell, Christopher D. 1998. "The Specific Deterrent Effect of Arrest on Aggression Between Intimates and Spouses." Ph. D. Diss., Newark, New Jersey, Rutgers, the State University of New Jersey.

Maxwell, Christopher D., Joel H. Garner, and Jeffrey A. Fagan. 2002. "Research, Policy and Theory: The Preventive Effects of Arrest on Intimate Partner Violence." *Criminology and Public Policy* 2(1):51-80.

McCrary, Justin. 2002. "Using Electoral Cycles in Police Hiring to Estimate the Effect of Police on Crime: Comment." *American Economic Review* 92:1236-43.

McLeod, Maurren. 1983. "Victim Noncooperation in the Prosecution of Domestic Assault." *Criminology* 21(3):395-416.

Mills, Linda. 1998. "Mandatory Arrest and Prosecution Policies for Domestic Violence." *Criminal Justice and Behavior* 25(3, September):306-18.

Murphy, Christopher M., Peter H. Masser, and Kenneth I. Maton. 1998. "Coordinated Community Intervention for Domestic Abusers: Intervention System Involvement and Criminal Recidivism." *Journal of Family Violence* 13(3):263-84.

Newmark, Lisa, Mike Rempel, Kelly Diffily, and Kamala Mallik Kane. 2001. *Specialized Felony Domestic Violence Courts: Lessons on Implementation and Impacts from the Kings County Experience*. Report submitted to the Center for Court Innovations and the National Institute of Justice. Washington, D.C.: The Urban Institute. 145.

Orchowsky, Stan J. 1999. *Evaluation of a Coordinated Community Response to Domestic Violence: The Alexandria Domestic Violence Intervention Project*. Final report submitted to the National Institute of Justice. Richmond, VA: Applied Research Associates.

Parnas, Raymond L. 1970. "Judicial Response to Intra-Family Violence." *Minnesota Law Review* 54:585-645.

Pate, Anthony and Edwin E. Hamilton. 1992. "Formal and Informal Deterrents to Domestic Violence." *American Sociological Review* 57 (October):691-97.

Paternoster, Raymond, Robert Brame, Paul Mazerolle, and Alex Piquero. 1998. "Using the Correct Statistical Test for the Equality of Regression Coefficients." *Criminology* 36(4):859-66.

Peterson, Richard R., Barbara Geller Diaz, Elizabeth Walton, Raymond Caligiure, Taehyon Kim, Bernice Linden-Reed. 2003i. *The Impact of Case Processing on Re-Arrest Among Domestic Violence Offenders in New York City*. Final Report. New York City: New York City Criminal Justice Agency. 76.

--------. 2003. *Combating Domestic Violence in New York City, 2001.* New York: New York Criminal Justice Agency, Inc.

Peterson, Richard R. 2004. *The Impact of Manhattan's Specialized Domestic Violence Court.* New York: New York Criminal Justice Agency.

Rauma, David. 1984. "Going for the Gold: Prosecutorial Decision-Making in Cases of Wife Assault." *Social Science Research* 13(4):321-251.

Raundenbush, Stephen W. and Anthony S. Bryk. 2003. *Hierarchical Linear Models: Application and Data Analysis Methods*. 2d ed. Edited by J. DeLeeuw and R. A. Berk. Advanced Quantitative Techniques in the Social Science, vol. 1, no. 2nd. Thousand Oaks, CA: Sage Publications.

Rebovich, Donald J. 1996. "Prosecution Response to Domestic Violence: Results of a Survey of Large Jurisdictions." Pp. 176-91 in *Do Arrests and Restraining Orders Work?* edited by

E. S. Buzawa and C. G. Buzawa. Thousand Oaks, CA: Sage Publications.

Sampson, Robert J. and John H. Laub. 1993. *Crime in the Making: Pathways and Turning Points Through Life*. Cambridge, MA: Harvard University Press.

Sechrest, Lee, Susan O. White and Elizabeth Brown. 1979. *The Rehabilitation of Criminal Offenders: Problems and Prospects*. Washington, D.C. National Academy Press.

Sherman, Lawrence W. and Richard A. Berk. 1984. "The Specific Deterrent Effects of Arrest for Domestic Assault." *American Sociological Review* 49:261-72.

Sherman, Lawrence W. 1992. *Policing Domestic Violence: Experiments and Dilemmas*. New York: Free Press.

Sherman, Lawrence W., Douglas A. Smith, Janell D. Schmidt, and Dennis P. Rogan. 1992. "Crime, Punishment, and Stake in Conformity: Legal and Informal Control of Domestic Violence." *American Sociological Review* 57 (October):680-90.

Spohn, Cassia, and Jeffrey Spears. 1996. "The Effect of Offender and Victim Characteristics on Sexual Assault Case Processing Decisions." *Justice Quarterly.* 13(4):649-679.

Steinman, Michael. 1988. "Evaluating a System-Wide Response to Domestic Abuse: Some Initial Findings." *Journal of Contemporary Criminal Justice* 4(3):172-86.

———. 1991. "Coordinated Criminal Justice Interventions and Recidivism Among Batterers." Pp. 221-36 in *Woman Battering: Policy Responses*, edited by M. Steinman. Cincinnati, OH: Anderson Publishing.

Taylor, Bruce and Christopher Maxwell. 2003. *The Sacramento Batterer Treatment Program Evaluation*, Final Report Submitted to the National Institute of Justice.

Tjaden, Patricia, and Nacy Thoennes. 2000. "Prevalence and Consequences of Male-to-Female and Female-to-Male Partner Violence as Measured by the National Violence Against Women Survey". *Violence Against Women* 6(2):142-161.

Tolman, Richard and Arlene Weisz. 1995. "Coordinated Community Intervention for Domestic Violence: The Effects of Arrest and Prosecution on Recidivism of Woman Abuse Perpetrators." *Crime & Delinquency* 41(4, October):481-95.

Vandaele, Walter. 1978. "Participation in Illegitimate Activities: Ehrlich Revisited." Pp. 270-335 in *Deterrence and Incapacitation: Estimating Effects of Criminal Sanctions on Crime Rates*, edited by A. Blumstein and J. Cohen. Washington, D.C.: National Academy of Sciences.

Visher. Christy A. 1986. "The Rand Inmate Survey: A Reanalysis". Pp. 161 - 211 in Blumstein, Albert, Jacqueline Cohen, Jeffrey A. Ross, and Christy A. Visher. Eds. *Criminal Careers and "Career Criminal* . Volume I. Washington, D.C. National Academy Press.

Ventura, Lois A. and Gabrielle Davis. 2005. "Domestic Violence: Court Case Convictions and Recidivism. Violence Against Women. 11(2): 255-277.

Weisz, Arlene, David Canales-Portaltin, and Neva Nahan. 2001. *An Evaluation of Victim Advocacy Within a Team Approach*. Final Report submitted to the National Institute of Justice. 363.

Wilson, Douglas and Andrew Klein. 2006. *A Longitudinal Study of a Cohort of Batterers Arraigned in a Massachusetts District Court 1995-2004*. Final report submitted to the National Institute of Justice. Cambridge, MA: BOTEC Analysis Corporation.

Wooldredge, John D., and Amy Thistlethwaite. 1999. *Reconsidering Domestic Violence Recidivism: Individual and Contextual Effects of Court Dispositions and Stake in Conformity*. Final Report submitted to the National Institute of Justice. Cincinnati, OH: University of Cincinnati.

Wooldredge, John D. and Amy Thistlethwaite. 2002. "Reconsidering Domestic Violence Recidivism: Conditioned Effects of Legal Controls by Individual and Aggregate Levels of Stake in Conformity." *Journal of Quantitative Criminology* 18(1, Mar):45-70.

Wooldredge, John D. and Amy Thistlethwaite. 2004. Bilevel Disparities in Court Dispositions for Intimate Assault. Criminology. 42(2):417-456.

Wooldredge, John D., and Amy Thistlethwaite. 2005."Court Dispositions and Rearrest for Intimate Assault." *Crime and Delinquency.* 51(18 Jan):75-102.

Wooldredge, John D. *(2000) Reconsidering Domestic Violence Recidivism: Individual and Contextual Effects of Court Dispositions and Stake in Conformity in Hamilton County, Ohio, 1993-1998*. (Machine Readable Data File). Inter-university Consortium for Political and Social Research Study # 3013. Ann Arbor, Michigan

Zatz, Marjorie S. and John Hagan. 1985. "Crime, Time, and Punishment: An Exploration of Selection Bias in Sentencing Research." *Journal of Quantitative Criminology* 1(1):103-26.

Zimring, Franklin E. and Gordon J. Hawkins. 1973. *Deterrence: The Legal Threat in Crime Control*. Chicago and London: The University of Chicago Press.

Zorza, Joan. 1992. "The Criminal Law of Misdemeanor Domestic Violence." *Journal of Criminal Law & Criminology* 83(1, Spring):46-72.

Appendix 1:

Studies with Data on

Intimate Partner Violence

Offenses,

Arrests,

Prosecutions,

and Convictions

Studies with Data on Intimate Partner Violence Offenses, Arrests, Prosecutions and Convictions (Page 1 of 4)

Authors	Jurisdiction	Start	Stop	State	Country	Offenses	Arrests	Prosecutions	Convictions
Ames & Dunham 2002	Clinton Co.	1999	1999	NY	U.S.			124	63
Ames 2001	Clinton Co.	1998	2000	NY	U.S.	1559	475	353	138
Archer, et al. 2002	Six U.S. Sites	1999	1999		U.S.			2470	1396
Belknap & Graham 2000	Cincinnati	1997	1997	OH	U.S.		2670	2241	969
Belknap & Sullivan 2002	Three U.S. Counties	1999	2000		U.S.			178	132
Blowers & Hartman 2004	Charlotte	2003	2003	NC	U.S.			1207	464
Brown 2004	Edmonton	1999	2000	AB	CAN	2934		2317	
Brown 2004	Edmonton	2001	2001	AB	CAN			366	189
Burris & Jaffe 1983	London	1979	1981	ONT	CAN			79	33
Buzawa, et al. 1999	Quincy	1995	1996	MA	U.S.		353	333	121
Carlson & Nidey 1995	Unnamed county	1990	1993	IA	U.S.			548	363
Cook, et al. 2004	Five British Sites	2003	2003		U.K.			216	69
Cramer 1999	Chesterfield	1997	1997	VA	U.S.			140	35
Cretney & Davis 1997	Bristol	1993	1993		U.K.			203	99
Crocker 2005	Ontario	1970	2000	ONT	CAN			252	113
Davis 1983	Brooklyn	1978	1978	NY	U.S.			103	45
Davis, et al. 1998	Milwaukee	1994	1995	WI	U.S.		2508	1580	836
Dawson 2004	Toronto	1974	1996	ONT	CAN			210	167
Dinoviter & Dawson 2007	Toronto	1997	1998	ONT	CAN			474	299
Douglas 2007	Queensland	2005	2005		AUS			646	539
Dunford 1990	Omaha	1986	1987	NE	U.S.		108	43	
Dunford, et al. 1990	Omaha	1986	1987	NE	U.S.		109	71	
Durose, et al. 2005	11 Unnamed Counties	2000	2000		U.S.			457	326
Eckberg & Podkopacz 2002	Minneapolis & Suburbs	1998	2001	MN	U.S.			6902	3302
Fagan 1989	Five Jurisdictions	1978	1979		U.S.			74	18
Felson & Pare 2007	U.S.	1970	1993		U.S.	1963	948		357
Ferraro & Boychuk 1992	Phoenix	1987	1988	AZ	U.S.			104	83
Field & Field 1973	Washington, D. C.	1966	1966	DC	U.S.	7500		199	
Finn 2003	Two Georgia Counties	2002	2002	GA	U.S.			170	107
Fleury 2002	Three U.S. Counties	1999	2000		U.S.			178	123
Ford & Regoli 1993	Marion Co.	1986	1987	IN	U.S.			642	310
Ford 1983	Marion Co.	1978	1978	IN	U.S.	325	123	30	26
Forst, et al. 1977	Washington, D. C.	1974	1974	DC	U.S.		345		59
Friday, et al. 2006	Charlotte	2003	2003	NC	U.S.	891	448	439	169

Studies with Data on Intimate Partner Violence Offenses, Arrests, Prosecutions and Convictions (Page 2 of 4)

Authors	Jurisdiction	Start	Stop	State	Country	Offenses	Arrests	Prosecutions	Convictions
Frisch, et al. 2001	Six New York Sites	1997	1997	NY	U.S.	5332	1743		715
Gamache, et al. 1988	Three Unnamed Cities	1982	1984	MN	U.S.		96		53
Gavin & Puffett 2005	New York City	2002	2002	NY	U.S.			26530	9601
Gewitz, et al. 2006	Ramsey Co.	1996	2002	MN	U.S.			892	690
Grace 1995	Five British Sites	1992	1992		U.K.		388	295	
Greenwood, et al. 1973	Los Angeles	1971	1971	CA	U.S.	524		62	
Gross, et al. 2000	Chesterfield Co.	1997	1997	VA	U.S.			177	130
Hanmer, et al. 1999	West Yorkshire	1996	1997		U.K.	1870	502	139	
Hartley & Frohmann 2003	Chicago	2000	2001	IL	U.S.			706	203
Henning & Feder 2005	Shelby Co.	2000	2000	TN	U.S.		4178	3317	
Hester & Westmarland 2005	Cheshire; Tauton	2001	2002		U.K.	1240		399	
Hester & Westmarland 2005	Northampton	2001	2001		U.K.			286	108
Hester & Westmarland 2005	Six British Jurisdictions	2001	2001		U.K.			432	252
Hester, et al. 2003	Three British Cities	2004	2004		U.K.	291	218		31
Hilton, et al. 2004	Ontario	1996	1996	ONT	CAN		589	312	
Hirschel & Hutchison 1991	Charlotte	1987	1989	NC	U.S.	686	452	156	125
Hirschel, et al. 2007	19 Jurisdictions	2000	2000		U.S.		1327	1198	
HMCPSI & HMIC 2004	Six British Jurisdictions	2002	2003		U.K.	118	71	25	13
HMCPSI & HMIC 2004	Six British Jurisdictions	2002	2003		U.K.			418	210
Jaffe & Burris 1981	London	1979	1981	ONT	CAN	444		104	
Jaffe, et al. 1986	London	1979	1979	ONT	CAN	443		320	
Jaffe, et al. 1993	London	1987	1990	ONT	CAN	1296		1007	
Jaffe, et al. 1993	London	1988	1989	ONT	CAN	90		52	
Jolin, et al. 1998	Portland	1996	1996	OR	U.S.		927	325	189
Keilitz, et al. 1997	Three U.S. Jurisdictions	1994	1995		U.S.	285		82	
Keilitz, et al. 1999	Seven U.S. Jurisdictions	1998	1998		U.S.			8378	3119
Kelley & O'Brien 1994	Boston	1993	1994	MA	U.S.	2152	775	107	
Kelly & Levy, 2002	Baltimore	2001	2001	MD	U.S.		19166	3178	1244
Kelly, et al. 1999	Islington	1993	1995		U.K.		149	34	20
Kingsnorth & MacIntoch 2007	Sacramento	2000	2000	CA	U.S.		8461	6502	4995
Kingsnorth 2006	Sacramento	1999	2001	CA	U.S.		872	679	516

Studies with Data on Intimate Partner Violence Offenses, Arrests, Prosecutions and Convictions (Page 3 of 4)

Authors	Jurisdiction	Start	Stop	State	Country	Offenses	Arrests	Prosecutions	Convictions
Kingsnorth, et al. 2001	Sacramento	1995	1996	CA	U.S.		455	369	259
Kingsnorth, et al. 2002	Sacramento	1999	1999	CA	U.S.		1427	1095	753
Lerman 1981	Seattle	1978	1980	WA	U.S.	2630		1116	638
Lerman 1981	Westchester Co.	1979	1980	NY	U.S.	1218		416	177
Marsland, et al. 2001	Abbotsford	1997	1998	BC	CAN		74		24
Martin 1994	Connecticut	1988	1988	CN	U.S.		4138	578	458
Maryland Network 2003	Maryland	1993	2000	MD	U.S.			123507	25594
McDermott, et al. 2003	Jackson Co.	1996	1999	IL	U.S.		1136	699	200
McLeod 1983	Detroit	1978	1979	MI	U.S.	5480		515	
Miller 2000	Sacramento	1997	1999	CA	U.S.			3979	2893
Mouzos & Makkai 2004	Australia	2002	2003		AUS	310		57	37
Muller & Dutton 1982	Vancouver	1982	1982	BC	CAN			175	64
Newmark, et al. 2001	Brooklyn	1997	1997	NY	U.S.			229	208
Ogrodnik 2006	64 urban areas	1995	2004		CAN	211791		177904	
Orchowsky 1999	Alexandria	1996	1997	VA	U.S.			2623	1100
Palmer 1999	Vancouver	1997	1997	BC	CAN			354	256
Patterson 2003	154 police agencies	2001	2001		CAN	34609		27566	
Pennell & Burke 2002	San Diego Co.	1996	1999	CA	U.S.	1571	611	350	310
Pennell, et al. 2000	San Diego Co.	1996	1996	CA	U.S.	2756	905	536	477
Peterson 2002	Three NY Boroughs	1998	1998	NY	U.S.			5139	1604
Peterson 2004	Manhattan	1998	2001	NY	U.S.			2239	643
Peterson, 2003c2	New York City	2001	2001	NY	U.S.			6707	2280
Peterson, et al 2003c	New York City	1998	1998	NY	U.S.			6818	2386
Phillips, et al.1998	Ten British Sites	1993	1994		U.K.		217	124	
Quarm & Schwartz 1985	Hamilton Co.	1980	1980	OH	U.S.			1235	133
Ransbottom & Libertun 2006	Boulder Co.	2003	2005	CO	U.S.			3783	2689
Rauma 1984	Santa Barbara	1978	1979	CA	U.S.	199		75	
Robinson 2003	Cardiff	2002	2002		U.K.			77	21
Ryan & Petrzelka 2003	Woodbury Co.	1995	1999	IA	U.S.			1236	689
Salazar, et al. 2007	Two Georgia Counties	1993	1999	GA	U.S.		2322	852	842
Schmidt & Steury 1989	Milwaukee	1984	1984	WI	U.S.	2212		209	
Schulman 1979	Kentucky	1979	1979	KY	U.S.	79	43	35	
Seith 2005	Fribourg	1996	1996		SW		126	31	
Sherman 1992	Milwaukee	1988	1989	WI	U.S.		802	37	3

Studies with Data on Intimate Partner Violence Offenses, Arrests, Prosecutions and Convictions (Page 4 of 4)

Authors	Jurisdiction	Start	Stop	State	Country	Offenses	Arrests	Prosecutions	Convictions
Smith, et al. 2001	Four U.S. Jurisdictions	1997	1998		U.S.			1329	882
Smithey, et al. 2002	Unnamed City	1998	1999	TX	U.S.		291	234	23
Steinman 1988	Lancaster Co.	1985	1986	NE	U.S.		183	139	68
Steinman 1991	Lancaster Co.	1986	1986	NE	U.S.	338	266	140	73
Steketee, et al. 2000	Washington	1998	1998	DC	U.S.	247		54	10
Stroshine & Robinson 2003	Midwest U.S. City	1996	1998		U.S.	219	99	26	
Taylor 2006	ACT	2003	2004	ACT	AUS		478	450	
Thoennes 2007	Two Oregon Counties	1999	2005	OR	U.S.			69	49
Tjaden & Thoennes 2000E	U.S	1995	1996		U.S.	713	235	176	75
Tolman & Weisz 1995	Dupage Co.	1992	1992	IL	U.S.	341	182	157	106
Toon, et al. 2005	Arizona	2003	2005	AZ	U.S.		16723		7586
Trainor, et al. 2002	166 Canadian Agencies	2000	2000		CAN	30806		25192	
Turley & Haas 2004	11 WV. Counties	2002	2002	WV	U.S.			1831	828
Uchida. et al. 2001	Colorado Springs	1999	1999	CO	U.S.		132	50	14
Uekert, et al. 2001	Manchester	1996	1996	NH	U.S.			204	107
Urbis Keys Young 2001	ACT	1998	2000	ACT	AUS			349	182
Urbis Keys Young 2001	ACT	2000	2001	ACT	AUS	332	288	206	176
Ursel 1994	Winnipeg	1990	1992	SK	CAN			3316	1940
Ursel 2003	Winnipeg	1992	1999	SK	CAN			9488	7785
U.S. Commission on Civil Rights 1982	Maricopa Co.	1979	1979	AZ	U.S.	23		11	
Valley, et al. 2005	Two British Jurisdictions	2003	2004		U.K.			291	168
Ventura & Davis 2005	Toledo	2000	2001	OH	U.S.			1982	472
Vera 1977	New York City	1971	1971	NY	U.S.		46		21
Walby & Allen 2004	Britain	2001	2001		U.K.	148	31	16	
Wasoff 1982	Three Scottish Cities	1980	1980		U.K.			58	46
Weisz, et al. 2001	Detroit	1998	1998	MI	U.S.	1057	313	149	63
Williams 1976	Washington	1973	1973	WA	U.S.		682	403	
Wilson & Klein 2006	Quincy	1995	1996	MA	U.S.		342	279	89
Wooldredge & Thistlethwaite 2002	Hamilton County	1993	1996	OH	U.S.		3110	2892	478
Wooldredge & Thistlethwaite 2005	Hamilton County	1993	1996	OH	U.S.		3662	3443	1318
Wooldredge 2002	Cincinnati	1993	1996	OH	U.S.		1855	1725	575
Woolery 2004	Wisconsin	2001	2001	WI	U.S.	27454	23190	17021	
Worden 2001	Five New York Towns	1996	1996	NY	U.S.	2129	783	631	370
Wordes 2000	Berkeley	1996	1997	CA	U.S.	138	89	66	20
Worrall, et al. 2006	Southern California City	2003	2003	CA	U.S.		245	96	
Yearwood & Lubitz 1999	Nine N.C. Jurisdictions	1997	1997	NC	U.S.			3419	1329

Appendix 2:

Copies of Approvals for this Research by Institutional Review Boards for the Joint Centers for Justice Studies, Inc. and the University of Michigan.

May 2, 2006

Joel Garner, PhD and Chris Maxwell, Ph.D.
Principal Investigators
Joint Centers for Justice Studies, Inc.

RE: Initial IRB review of The Crime Control Effects on Prosecuting Intimate Partner Violence, #2006-91447-WV-IJ

Dear Dr. Garner and Dr. Maxwell,

This will confirm that the Social Solutions International, Inc. Institutional Review Board (IRB) met on May 1, 2006, on an expedited basis, to review the above-captioned research project's Initial Review Memo and Proposal Statement of Work (SOW). Assisting me with this review was Shakira Daugherty.

The IRB members present and voting unanimously approved this project, with no reservations or concerns expressed. It is your responsibility to notify your Federal Government project and contract officers that your project has been reviewed and approved. Please note that you are required to notify the IRB immediately if you make changes to the protocol that affect the participants.

This initial approval is effective for 12 months from the date the IRB met and approved the project, after which time your project must again be presented to the IRB for an annual review.

If you have any questions, please feel free to contact me (202.336.6097 or kchen@apa.org).

Sincerely yours,

Karen Chen, PhD
Chair
Institutional Review Board
Social Solutions International, Inc.

UNIVERSITY OF MICHIGAN
eResearch.umich.edu
Behavioral Sciences Institutional Review Board (IRB) • 540 East Liberty Street, Suite 202, Ann Arbor, MI 48104-2210 • phone (734) 936-0933 • fax (734) 998-9171 • irbhsbs@umich.edu

Date: 3/28/2006

To: Dr. Christopher Maxwell

Cc: DRDA, IRB Behavioral Sciences

Subject: Initial Study Approval

The Behavioral Sciences Institutional Review Board (IRB) has reviewed and approved the research proposal referenced below. The IRB determined that the research is compliant with applicable guidelines, state and federal regulations, and the University of Michigan's Federalwide Assurance with the Department of Health and Human Services (HHS).

Any proposed changes/amendments in the research (e.g., personnel, procedures, or documents), no matter how minor, must be approved in advance by the IRB unless necessary to eliminate apparent immediate hazards to research subjects.

The approval period for this project is listed below. *Please note your expiration date.* If the project is scheduled to continue beyond this date, submit a Scheduled Continuing Review application **at least two months prior** to the expiration date to allow the IRB sufficient time to review and approve the project. **If the approval lapses, no work may be conducted on this project until appropriate approval has been obtained, except as necessary to eliminate apparent immediate hazards to research subjects.**

The IRB must be informed of all unanticipated or adverse events (i.e., physical, social, or emotional) or any new information that may affect the risk/benefit assessment of this research.

The online forms for amendments, adverse event reporting, and scheduled continuing review can be obtained by accessing the eResearch workspace for this approved study at https://eresearch.umich.edu.

It is expected that only the current IRB-approved version of the informed consent document(s) will be used in conjunction with this research. To obtain and download a copy of the current IRB-approved informed consent document(s), PIs and Study Staff should access the eResearch workspace for this approved study and view the “Documents” tab.

Submission Information:
Title: The Effects of Prosecution on Violence Between Intimate Partners
Full Title:
IRB File Number: HUM00004331
Initial IRB Approval Date: **3/27/2006**
Current IRB Approval Period: **3/27/2006** - **3/26/2007**
Expiration Date: **3/26/2007**
eResearch workspace: The Effects of Prosecution on Violence Between Intimate Partners
UM Federalwide Assurance: FWA00004969 Expiration 6/12/06
Sincerely,

James Sayer
Chair, IRB Behavioral Sciences

Appendix 3:

Statistical Programs

and

Data Files

Created as Part of this Research

Joint Centers for Justice Studies, Inc

526 Willowdale Drive
Shepherdstown, WV 25443
(204) 876-3460

January 8, 2008

Bernie Auchter
National Institute of Justice
812 7th Street, N.W.
Washington, D.C.

Dear Bernie;

I have attached to this letter a CD-ROM which includes the various SPSS and HLM statistical programs that were used to create the analyses reported in Final Report for NIJ Award # 2006-IJ-CX-0005, *The Crime Control Effects of Prosecuting Intimate Partner Violence in Hamilton County, Ohio: Reproducing and Extending the Analyses of Wooldredge and Thistlethwaite.*

I have also included on that CD-ROM copies of the data files generated by these programs. These data include census data and census tract and neighborhood identifiers not included in the originally archived data.

The files included are:

HLM Programs
Final Report Prevalence Model 1.txt
Final Report Prevalence Model 2.txt
Final Report Prevalence Model 3.txt
Final Report Frequency Model 1.txt
Final Report Frequency Model 2.txt
Final Report Frequency Model 3.txt

Cd Prevalence Model.txt
Cd Frequency Model.txt
crim02.hood.txt
crim02tract.txt

JQC.Table3 Model1.txt
JQC.Table3 Model2.txt
JQC.Table3 Model3.txt

SPSS Programs
Final Report Time to Failure Model.sps
CD Time to Failure Model.sps

Data Files
Hamilton County Census Tracts.sav
Crim02 Neighborhood.sav

Sincerely,

Joel H. Garner