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

**Reproducing and Extending the Analyses of
Wooldredge and Thistlethwaite**

Executive Summary

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January 2008

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**The Crime Control Effects of Prosecuting
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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.

- * 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.

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A Final Report Submitted to the National Institute of Justice

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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 jurisdiction¹. 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?

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

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 test² 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

²Multiple 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, Ohio³ (See Table 1 - 3). In these four publications, the social context hypothesis was tested using different subsamples, different statistical models and different

³In 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 prosequi*” 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 Abbotsford, 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 & Podcopcz (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

Study	Jurisdictions	Prosecution			Conviction			Sentence Severity		
		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			Individual Level		
		Stakes in Conformity			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	Cincinnati	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	Cincinnati	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	Reported		6 Month Rate
				Sample Size	Base Rate	Months At Risk	
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	Arrestment	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
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	Disposition	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 Interviews	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%
			Aggravated Offense		19.6%		6.5%
			Aggravated 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	Incident	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%	12 to 18	N.A.
			Violent Offense		25.5%		
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%

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 household⁴.

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

⁴In 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 1 and 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 Thistlethwaite 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 hypothesis⁵. 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

⁵In 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 - 1 displays 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 combined⁶. 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 sanctions⁷. 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

⁶In 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.

⁷All 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 factor⁸. 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

⁸In 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 context⁹. 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.

⁹The 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).

