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The Crime Control Effects of Criminal Sanctions for Intimate Partner Violence

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A prior review of published research established that once an intimate violence offense results in an arrest, the use of criminal prosecution and conviction is more commonplace than traditionally thought. The substantial use of criminal sanctions beyond arrest heightens the salience of whether criminal sanctions for intimate partner violence have a crime control effect or not. This research seeks to contribute to this discussion by providing a systematic review of 31 published studies that provide evidence regarding the crime control benefits from prosecution, conviction, and sentencing of intimate partner violence offenders. This review describes the characteristics of each of these studies, summarizes the substantive findings reported, and evaluates the research designs, measures, and methods used. Across these studies, we array 143 reported tests into three crime prevention hypotheses: the prosecution hypothesis, the conviction hypothesis, and the sanction severity hypothesis. Based on the analyses and conclusions produced by these studies, we find that the most frequent outcome is that sanctions that follow an arrest for intimate partner violence have no effect on the prevalence of subsequent offending. However, among the minority of reported analyses that do report a statistically significant effect, two thirds of the published findings show sanctions to be associated with reductions in repeat offending and one third show sanctions to be associated with

ONLINE TABLE: Detailed summaries of the 31 studies reviewed in this article can be found in the table available online at http://www.springerpub.com/pa. Click on the link to "The Partner Abuse State of Knowledge Project" and go to Topic 11 in the online document.

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increased repeat offending. Our examination of the methods used by these studies identified seven common issues that suggest that the research designs used are inadequate to assess the relevant public policies and criminological theories. Based on our systematic assessment of the published studies, we conclude that the preponderance of the reported findings show no effect for criminal sanctions; moreover, the quality of the research methods used in this research provide an insufficient basis to support a conclusion about the use of criminal prosecution and sentencing for intimate partner violence.

KEYWORDS: intimate partner violence; prosecution; conviction; sentencing; recidivism; deterrence

Beginning in the early 1970s, the perceived limitations in informal responses to the problem of spouse assault mobilized societal institutions to increase the range of formal social controls of offenders (Fagan & Browne, 1994). These controls emphasized the use of legal remedies such as the arrest, prosecution, and court-ordered treatment of offenders (Davis & Smith, 1995), with the central aim of preventing offenders from further abusing their intimate partners (Fagan, 1996). During this period, it was commonplace for domestic violence scholars to criticize the police for their failure to report domestic violence offenses, to arrest male offenders, to redress the victimization of women, or to treat violence among intimate partners as severely as violence against nonintimate partners (Blari, 1979; Fleming, 1979; Martin, 1976; Paterson, 1979; Roy, 1977). The first legal remedy that many argued for was to always arrest batterers at the scene of a domestic violence incident (Dobash & Dobash, 1979; Martin, 1976; Zorza, 1992). However, before the 1980s, in many U.S. jurisdictions police officers could only make an arrest for a misdemeanor assault when the offense was committed in their presence (Elliott, 1989). To address this problem, state legislatures in the late 1970s began revising arrest laws, and many police departments changed their arrest policies (Sherman & Cohn, 1989). By the mid-1990s, many states had passed legislation that authorized police officers to arrest offenders whenever they had probable cause to believe that a misdemeanor assault had occurred between intimate partners (Burt, Dyer, Newmark, Norris, & Harrell, 1996; Hirschel, Buzawa, Pattavira, Faggiani, & Reuland, 2007).

With the widespread adoption of proarrest laws and policies, the criticisms of the police abated and the policies and practices of prosecutors and judges toward intimate partner violence attracted heightened attention (Fagan, 1996; Ford, Richard, Goldsmith, & Regoli, 1996). Advocates argued that although

> mandatory arrest initiated the process of extending greater protections to victims of domestic violence, [the system] needed to carry through on that promise by building a system of prosecution that has sufficient information

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> and resources to effectively prosecute these difficult cases and provide necessary support and protection for victims. (Frisch, Mackey, Hall, Worden, & Polllitz, 2001, p. 8)

Some scholars likewise argued that the modest and inconsistent effects of arrest on subsequent reoffending were likely due in part to inadequate follow-through by prosecutors (Zorza, 1992).[1] Cahn and Lerman (1991) and Fagan (1996) maintained that the low rate of prosecution in domestic violence cases neutralized the actions of the police because deterrence ultimately results from the actions of prosecutorial and judicial actors whose actions lead to substantive punishment. Frisch et al. (2001) likewise asserted that

> [t]he efficacy of arrest may reside less in its immediate punitive effect on offenders than on the fact that it enables the criminal justice system to continue to operate on the case, taking actions that might deter offenders from future violence. (p. 17)

To advance the impact of the prosecution process, legislatures throughout the United States addressed several statutory shortcomings, for example, by enacting victim rights laws that required prosecutors to meet with victims about their decisions to charge defendants. Several states also require prosecutors to have written policies on domestic violence prosecution. Florida, for example, requires its state's attorney general and local prosecutors to adopt a "pro-prosecution policy" for domestic violence, encourages prosecutors to file even "over the objections of the victim if necessary," and calls for prosecutors to establish special units or assign prosecutors "to specialize in the prosecution of domestic violence cases . . . and receive training in domestic violence issues." By the mid-1990s, many other prosecutors representing larger jurisdictions in the United States have also adopted some form of a no-drop policy (Rebovich, 1996), specialized domestic violence prosecution or court programs, or they took other steps to increase the rate of or the success of prosecution (Klein, 2004; Maxwell, Robinson, & Kline, 2009).

Despite this increased attention and focus by prosecutors and courts, the use of criminal sanctions to deter and incapacitate intimate violence offenders remains controversial. This controversy is infused with ideological and political disputes about the complex dynamics of intimate partner violence and the strengths and weaknesses of mobilizing the criminal law to address that complexity (Mills, 1999, 2003; Stark, 2004). Furthermore, unlike what we now know about arrest, the use of more formal legal sanctions that go beyond arrest has never been seriously challenged by the scientific community (Maxwell, Garner, & Fagan, 2001). We do not believe that anyone or any group of scholars has established an empirical basis for the use of these sanctions to date. In this article, we begin this process by reporting results of our systematic review of published results that tested for an association between prosecution and subsequent patterns of intimate partner violence. We conclude this

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article with an assessment of the strengths and weaknesses of this body of research, and the extent to which our results support the deterrence rationale in instances of intimate partner violence.

THEORETICAL BASIS FOR THE CRIME CONTROL EFFECTS OF PROSECUTION

The implementation of formal responses to domestic violence departs from the traditional thinking that normative social processes regulate social behavior better than legal restraints (Gibbs, 1975; Tittle & Logan, 1973). Some scholars have argued that the use of legal restraints is typically a last resort option, used only when other informal social controls have failed to control behavior (Black, 1976; Miller & Simpson, 1991; Wilson, 1995). However, others have suggested that under no condition is the criminal law a particularly effective mechanism for controlling intimate partner violence (Straus, 1977; Zimring, 1986). Yet within the literature addressing how to control domestic violence, specific deterrence theory is often referenced as the justification for using more criminal justice interventions (e.g., Sherman & Berk, 1984). Although specific deterrence theorists argue that fear of future sanctions heightened by the application of the sanction is the causal mechanism by which sanction affect future criminal behavior (Zimring & Hawkins, 1972), Williams (2005) and Gibbs (1975) have suggested that sanctions might affect future criminal behavior through mechanisms other than fear of future sanctions. For instance, the imposition of a sanction may communicate that violence against partners violates social norms and the change in an offender's understanding of social norms leads to a change in his or her behavior. In addition, modern understanding of the etiology of violence between intimates argues that at least part of the effectiveness of criminal sanctions stems from the impact of sanctions on victim behavior. According to this line of reasoning, in jurisdictions where intimate partner violence is prosecuted and offenders convicted, victims and especially female victims may become empowered by the opportunity to mobilize or not mobilize the authority of the criminal law (Ford & Regoli, 1992; Jolin, Feyerherm, & Friedman, 1998). Unfortunately, the research on the effectiveness of criminal sanctions for intimate partner violence does not explicitly address the underlying causal mechanisms and typically does not collect information which would permit distinguishing the effects of fear, norms empowerment, or other potential mechanisms by which sanctions might affect future behavior. For this reason, we prefer to specify a generic crime control effect, which is purposefully neutral about the mechanism by which sanctions affect future behavior.

In addition, our assessment is that it may not be fruitful to conceptualize any sanction as part of a single generic "sanction effect." In theory and in policy discussions, there may not be a consistent effect for all sanctions or a single hypothesis about the direction and size of all types of sanctions. For this reason, we conceive of not just one but at least three distinct hypotheses about the effects of criminal sanctions on intimate partner violence. We label these as the prosecution hypothesis, the conviction

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hypothesis, and the sanction severity hypothesis. The first two hypotheses are relatively straightforward. The prosecution hypothesis asserts that repeat offending will become less frequent among offenders whom the prosecutor has filed at least one criminal charge. We conceive of prosecution as a simple dichotomy measure and do not consider other potentially important gradations of prosecutorial intensity, such as the number or severity of the charges. The conviction hypothesis asserts that repeat offending will become less frequent for those offenders who are convicted by a court, either by plea of guilty or by a trial or bench decision. We also conceive the conviction as dichotomous with a mixture of cases not arrested, not charged or acquitted cases included in the "no conviction" category. Our third hypothesis (the sanction severity hypothesis) states that there is a greater reduction in repeat offending the more severe the criminal sanction following a conviction. We distinguish tests of sanction severity from tests of prosecution and conviction, even though prosecution is a more severe sanction than no prosecution, and conviction is a more severe sanction than no conviction. For the analyses that involve a test using a measure created by rank ordering, all case dispositions from the lowest to highest severity are considered a test of the sanction severity hypothesis.

When specific types of sanctions imposed following conviction are compared with cases that are prosecuted but not convicted, we interpret these as tests of the conviction hypothesis only. When specific types of sanctions imposed following conviction are compared with cases that are not prosecuted, we interpret these tests as tests of the prosecution hypothesis only. This approach emphasizes the characteristics of the reference group in determining which hypothesis is tested, and it avoids double or triple counting individual tests as measures of prosecution, conviction, and sanction severity hypotheses.

This approach permits distinctions among three crucial policy choices in criminal justice processing—the decision to file charges, the decision to convict, and the type of sanction imposed on convicted offenders. If there were differential effects for prosecution, conviction, and severe sanctions, these distinctions would be missed if all tests of criminal sanctions are considered together. Second, the use of these three hypotheses structures how individual case outcomes are defined, and which case outcome comparisons should be included in any particular analysis. Given the diversity in how case outcomes are defined in different jurisdictions and, perhaps more importantly, how researchers decide which case outcomes will fall into the treatment group and which ones go into the control group, the use of these three hypotheses provides us a framework to synthesize findings across a diverse research literature.

This approach provide some differentiation among sanctions, which we think better conforms to both practical operations of the criminal justice system as well as identifying sanctions whose distinct natures may generate distinct effects. If there are differential effects for different types of sanctions, the use of the single sanction conceptualization may cause us to miss effects associated with some but not all sanctions.[2]

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METHODS

Sample

In 2009, Garner and Maxwell used a systematic review of the published literature to report about the amount of prosecution and conviction for intimate partner violence. They sought out English language reports of the prosecution and conviction for intimate partner violence regardless of the time or jurisdiction studied. They conducted extensive automated and hand searches of the prior research, examined references in known articles, and used web-based searches through the National Criminal Justice Reference Service, *Criminal Justice Abstracts*, and Google's search engine to identify, obtain, and review hundreds of documents, published articles, and books. When more than one report of the same analysis was available, they report choosing the largest, most inclusive, and most detailed of the reports to include in this review. Sometimes, a single document included separate analyses of different samples of offenses, arrests, or prosecutions, and they included these analyses as separate studies.

Garner and Maxwell's (2009) review determined that the rate of prosecution and conviction for this offense was not, as was commonly reported, a "rare" event. They report that, on average, one third of all reported offenses and three fifths of all arrests result in the filing of criminal charges. In addition, about a third of all arrests and half of all prosecutions for intimate partner violence result in a conviction. Among the 135 publications used to estimate the amount of prosecution and conviction by Garner and Maxwell (2009), we have identified 30 that report 31 distinct quantitative analyses that assess the relationship between the application of sanctions by the criminal courts and repeat offending.[3] This article reviews these published analysis to assess the effectiveness of using criminal sanctions to reduce violence between intimate partners.

The online table displays (in alphabetical order by the lead author's last name) some of the characteristics of the 31 studies that report the association of criminal sanctions for intimate partner violence with repeat offending. These studies were published over a 24-year period from 1984 to 2008 and were based on criminal justice selected samples from 41 primarily U.S. jurisdictions.[4] These studies were published in 17 government-sponsored reports, as 12 referred journal articles, and as chapters in 2 books. Nine of these studies sampled intimate partner violence incidents, eight sampled arrests, and 14 sampled prosecutions. The sample sizes used in these 31 studies vary from 74 offenses reported to the police in Abbotsford, British Columbia during 1997–1998 to 6,489 arrests in three New York City boroughs during 1998. Four of the 31 studies, all directed by the same first author, report eight unique results that draw cases from the same 3,662 Hamilton County, Ohio arrests; two of the eight analyses use all 3,662 arrests, five analyses use 3,110 of the 3,662 arrests, and one analysis selected just the 1,855 cases that occurred within the boundary of the city of Cincinnati, Ohio. Fourteen (44%) of the 31 studies contained only male offenders, nine studies included some female offenders but no study's sample was constituted by more than 20% female, and eight did not report information about the offender's sex. The

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distribution of the offenders' ethnicity across the studies varied widely, from one study with just 8% White to one with 95% White offenders. Combined, these 31 studies include 37,215 incidents of intimate partner violence that may have resulted in a prosecution, a conviction, or the imposition of criminal sanctions following a conviction.

ANALYSIS

Our assessment of this literature is presented in three parts. First, we provide a short narrative summary of the characteristics of each study (see online table); in each of the summaries, we specify which of the three sanction hypotheses were tested and the results of those tests (see Tables 1 and 2). Our second approach is to tabulate the reported findings from the 31 studies (see Table 3). For each study and by each hypothesis, we present the number of reported statistical tests that show that criminal sanctions were significantly associated with less repeat offending, more repeat offending, or do not significantly affect repeat offending.[5] Our last approach to reviewing this literature is to describe and critique the studies' measures, samples, and analytical approaches (see the online table). Following these three detailed reviews, we conclude this article with an assessment of the substantive findings reported, and the extent to which the methods used in these studies provide a solid basis for evaluating policy or testing theories about the effects of sanctions.

Narrative Summaries of 31 Published Analyses

With the three crime control hypotheses in mind, we review the 31 studies listed alphabetically in the online table (we array these summaries in Table 1). The narratives summarize the location and period of each study, the nature and size of the samples involved, the types of analyses employed, the ways in which recidivism was measured, and the direction and statistical significance of the reported findings. We also identify some major strengths and weaknesses of each study and how we interpret the author's reported findings in terms of our three hypotheses. This narrative review of each publication is designed to establish the diversity of samples, methods, and measures used to generate the findings of the 31 studies reviewed here.

Overall, we find that 20 of the 31 employed multivariate analyses to produce their key findings, including one use of an experimental design (see Ford & Regoli, 1992) and one use of hierarchical linear modeling (see Wooldredge, 2002). Nine of the studies used simple bivariate analyses to produce their findings, and two studies involved a before and after comparison within a single jurisdiction. Based on these types of analyses, seven of the papers concluded that their results support the use of more criminal sanctions, three papers concluded that their evidence is equivocal about the value of sanctions, and 11 papers concluded that sanctions had no or negative consequence on recidivism rates. The remaining 10 papers reached no conclusion that we felt fell within one of our three hypotheses, although all of them produce analyses that provided us relevant results.

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TABLE 1. Study Characteristics

Study No.	Citation	Research Sites	Sample Type	Sample Size	% Male Offenders	% White Offenders	Data Collection Start	Data Collection End
1	Belknap & Sullivan, 2002	Ingham Co., Michigan; Boulder Co., Colorado; and Denver, Co	Prosecution	178	100	55	1999	2000
2	Buzawa, et al., 1999	Quincy, Massachusetts	Prosecution	353	100	84	1995	1996
3	Davis, et al., 1998	Milwaukee, Wisconsin	Prosecution	1,133	86	45	1994	1995
4	Dunford, 1990	Omaha, Nebraska	Offense	247	99	45	1986	1987
5	Eckberg & Podkopacz, 2002	Hennepin Co., Minnesota	Prosecution	6,187	NR	NR	1998	2001
6	Fagan, et al. 1984	Five unspecified jurisdictions throughout the U.S.	Offense	270	95	57	1978	1979
7	Finn, 2003	Gwinnett & DeKalb Co., Georgia	Arrest	170	100	86	2002	2002
8	Ford & Regoli, 1992	Marion Co., Indiana	Prosecution	642	100	60 - OSA; 54 - VC	1986	1987
9	Friday, et al., 2006	Charlotte, North Carolina	Offense	826	84	22	2003	2003
10	Frisch, et al., 2001	Eight Communities (2 of 8 were in New York City), New York	Offense	6,813	80	NR	1997	1997
11	Gross, et al., 2000	Chesterfield Co., Virginia	Prosecution	177	100	63	1997	1997
12	Hartley & Frohmann, 2003	Chicago, Illinois	Prosecution	706	100	8	2000	2001

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13	Jaffe, et al., 1993	London, Ontario	Offense	90	100	95	1988	1989
14	Jolin, et al., 1998	Portland, Oregon	Arrest	927	100	62	1996	1996
15	Kingsnorth, 2006	Sacramento, California	Arrest	872	85	39	1999	2001
16	Klein & Tobin, 2008	Quincy, Massachusetts	Prosecute	342	100	85	1995	1996
17	Marsland, et al., 2001	Abbotsford. B.C.	Offense	74	100	73	1997	1998
18	Murphy, et al., 1998	Baltimore, Maryland	Offense	235	100	16	1994	1994
19	Newmark, et al., 2001	Brooklyn Co., New York	Prosecution	304	NA	NA	1997	1997
20	Orchowsky, 1999	Alexandria, Virginia	Offense	1,910	100	21	1996	1997
21	Peterson, 2003	New York City, New York	Prosecution	6,489	NR	NR	1998	1998
22	Peterson, 2003	Bronx Co., New York	Prosecution	1,435	NR	NR	1998	1998
23	Peterson, 2004	Manhattan Co., New York	Prosecution	2,134	NR	11	1998	2001
24	Steinman, 1988	Lancaster Co., Nebraska	Arrest	183	100	72	1985	1986
25	Steinman, 1991	Lancaster Co., Nebraska	Offense	338	NR	NR	1986	1986
26	Tolman & Weisz, 1995	Dupage Co., Illinois	Prosecution	341	100	79	1992	1992
27	Ventura & Davis, 2005	Toledo, Ohio	Prosecution	519	88	43	2000	2001
28	Wooldredge & Thistlethwaite, 1999	Hamilton Co., Ohio	Arrest	3,662	84	NR	1993	1996
29	Wooldredge, 2002	Cincinnati, Ohio	Arrest	1,855	83	NR	1993	1996
30	Wooldredge & Thistlethwaite, 2002	Hamilton Co., Ohio	Arrest	3,110	NR	NR	1993	1996
31	Wooldredge & Thistlethwaite, 2005	Hamilton Co., Ohio	Arrest	3,662	NR	NR	1993	1996

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TABLE 2. Outcome Measures Used in Intimate Violence Prosecution Research

Study No.	Record/Report Type	Nature of Repeat Behavior	Measurement Dimension	Target of Incident	Time at Risk Begin at	Recidivism Sample Size	Base Failure Rate	Months at Risk
1	Interview	Physical abuse	Prevalence	Same offender	Disposition	160	38.1%	6
	Interview					148	34.9%	12
	Interview	Psychological abuse	Frequency	Same offender		160	$M = 1.11$	6
	Interview					148	$M = 0.99$	12
2	Arrest	Any type	Prevalence	Same victim	Arraignment	353	47.9%	12
	Arrest	Violence	Prevalence	Same victim			22.1%	12
	Arrest	Violence	Prevalence	Other victim			10.8%	12
	Arrest	Nonpersonal offense	Prevalence	Other victim			15.0%	12
	Interview	Violence or violation of restraining order	Prevalence	Same offender		118	49.2%	12
3	Arrest	Any type	N. R.	Any victim	Disposition	1,133	N. R.	6
4	Arrest	Any type	Prevalence	Same victim	Incident	247	8.9%	6
	Arrest						16.2%	12
	Incident	(a) Feeling of being in danger, (b) pushed or hit, (c) physical injury	Prevalence	Same victim			18.6%	6
	Incident						28.7%	12
	Interview	Push or hit	Prevalence	Same victim		196	58.2%	6
	Interview						62.2%	12
5	Arrest	Nondomestic violence	Prevalence	Any victim	Disposition	6,187	30.7%	9
	Arrest	Domestic violence	Prevalence	Any victim			14.1%	9
	Conviction	Nondomestic assault	Prevalence	Any victim			14.8%	9
	Conviction	Domestic assault	Prevalence	Any victim			4.6%	9

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6	Interview	Violence	Prevalence	Same victim	Disposition	149	32.2%	6
	Interview	CTS plus abuse & sexual assault	Prevalence	Same victim	Incident	270	28.5%	6
7	Interview	Any abuse	Prevalence	Same victim	Disposition	170	38.8%	6
	Interview	Physical abuse	Prevalence	Same victim		170	16.5%	6
8	Victims call prosecutor or files complaint; police incident or arrest report, complaint to the court or other agencies	Violence	Prevalence	Same victim	Disposition	642	3.1%	6
	Interview	Violence	Prevalence	Same offender		430	30.9%	6
	Interview	CTS violence	Prevalence	Same offender			34.2%	6
	Interview	CTS severe violence	Prevalence	Same offender			20.5%	6
9	Incident	Any domestic violence	Prevalence	Same victim	Incident	766	34.1%	24
10	Incident	Any	Prevalence	Same victim	Incident	6,803	36.6%	18
	Incident	Domestic violence	Prevalence	Same victim			31.6%	
	Incident	Aggravated offense	Prevalence	Same victim			19.6%	
	Incident	Aggravated domestic violence	Prevalence	Same victim			15.7%	
11	Arrest	Any	Prevalence	Any victim	Incident	177	24.3%	18
	Conviction	Any	Prevalence	Any victim			19.8%	18
12	Arrest	Any	Prevalence	Any victim	Incident	706	28.8%	6
	Arrest	Domestic violence	Prevalence	Same victim			12.0%	6
	Interview	Kicked, bit, or hit with fist	Prevalence	Same offender	Disposition	47	10.6%	6

(continued)

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TABLE 2. (*continued*)

Study No.	Record/Report Type	Nature of Repeat Behavior	Measurement Dimension	Target of Incident	Time at Risk Begin at	Recidivism Sample Size	Base Failure Rate	Months at Risk
13	Interview	Pushed, grabbed, or shoved	Prevalence	Same victim	Incident	90	50.9%	12
	Interview	Slapped	Prevalence	Same victim			28.3%	12
	Interview	Kicked, hit, or bit	Prevalence	Same victim			26.7%	12
14	Incident	Offense	Prevalence	Same offender	Incident	883	14.0%	6
	Arrest	Offense	Prevalence	Same offender			7.8%	6
	Incident	Offense	Prevalence	Same victim			13.6%	6
	Arrest	Offense	Prevalence	Same victim			8.0%	6
	Incident	Offense	Frequency	Same offender			$M = 0.20$	6
	Arrest	Offense	Frequency	Same offender			$M = 0.08$	6
	Incident	Offense	Frequency	Same victim			$M = 0.22$	6
	Arrest	Offense	Frequency	Same victim			$M = 0.08$	6
	Calls to police	Any	Prevalence	Same address			46.0%	6
	Calls to police	Any	Prevalence	Same address			14.0%	6
	Interview	Victimization	Prevalence	Same offender		395	60.8%	6
	Interview	Victimization	Prevalence	Any offender			61.3%	6
	Interview	Victimization	Frequency	Same offender			$M = 3.28$	6
	Interview	Victimization	Frequency	Any offender			$M = 3.41$	6
15	Arrest	Intimate partner violence	Prevalence	Same victim	Incident	872	15.3%	18

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16	Arrest	Violent crime	Prevalence	Any victim	Incident	342	65.5%	108
	Arrest	Violent crime	Prevalence	Any intimate victim			50.9%	
	Arrest/restraining order	Violent crime/civil restraining order filed	Prevalence	Any victim			59.9%	
	Arrest/restraining order	Violent crime/civil restraining order filed	Frequency	Any victim			$M = 1.94$	
17	Interview	Assault	Prevalence	Same offender	Incident	74	43.0%	27
18	Charge	Battery or violation of protection order	Prevalence	Same victim	Incident	235	15.7%	12–18
	Charge	Violent offense	Prevalence	Any victim			25.5%	
19	Arrest	Any	Prevalence	Any victim	Disposition	304	31.3%	12
	Arrest	Violent felony	Prevalence	Any victim			4.9%	12
	Arrest	Criminal contempt	Prevalence	Any victim			11.1%	12
	Arrest	Any	Prevalence	Any victim			39.9%	18
	Arrest	Violent felony	Prevalence	Any victim			7.2%	18
	Arrest	Criminal contempt	Prevalence	Any victim			15.2%	18
20	Charge	Domestic violence	Prevalence	Any victim	Incident	1,910	21.0%	Variable
21	Arrest	Domestic violence	Prevalence	Any victim	Disposition	6,489	17.0%	18
	Arrest	Any offense					38.0%	18
22	Arrest	Domestic violence	Prevalence	Any victim	Disposition	1,435	18.0%	18
23	Arrest	Domestic violence	Prevalence	Any victim	Disposition	2,134	14.2%	18
24	Charge	Physical violence or threats	Prevalence	Same victim	Incident	183	19.7%	12

(continued)

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TABLE 2. (*continued*)

Study No.	Record/Report Type	Nature of Repeat Behavior	Measurement Dimension	Target of Incident	Time at Risk Begin at	Recidivism Sample Size	Base Failure Rate	Months at Risk
25	Arrest or citation	Domestic violence	Prevalence	Same victim	Incident	338	61.5%	Not reported
	Interview		Prevalence	Same offender			59.5%	
26	Incident	Domestic violence	Prevalence	Same victim	Disposition	341	29.9%	18
27	Arrest	Domestic violence	Prevalence	Same victim	Disposition	519	32.6%	12
28	Arrest	Domestic violence	Prevalence	Any victim	Completion	3,110	16.8%	24
	Arrest	Domestic violence	Frequency	Any victim		3,110	$M = 0.19$	24
	Arrest	Domestic violence	Survival	Any victim		3,662	$M = 9.77$	Variable
29	Arrest	Domestic violence	Prevalence	Same victim	Completion	1,855	14.0%	24
30	Arrest	Domestic violence	Prevalence	Same victim	Completion	3,110	14.0%	24
31	Arrest	Domestic violence	Prevalence	Any victim	Completion	3,110	16.0%	24
	Arrest	Domestic violence	Frequency	Any victim		3,110	$M = .19$	24
	Arrest	Domestic violence	Survival	Any victim		3,662	$M = 10.2$	Variable

Note. N. R. =Not Reported; CTS = Conflict Tactics Scale.

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TABLE 3. Number of Reported Statistical Tests About the Effectiveness of Sanctions for Intimate Partner Violence

Study No.	Authors/Data	Prosecution			Conviction			Sanction Severity		
		Less	None	More	Less	None	More	Less	None	More
1	Belknap & Sullivan, 2002	4	0	0						
2	Buzawa et al., 1999							0	0	1
3	Davis et al., 1998	0	1	0	0	2	0			
4	Dunford, 1990				0	8	0			
5	Eckberg & Podkopacz, 2002				1	2	0			
6	Fagan et al., 1984	1	1	0	1	1	0			
7	Finn, 2003							0	2	0
8	Ford & Regoli, 1992	2	0	0	0	2	0			
9	Friday et al., 2006				0	1	0			
10	Frisch et al., 2001				0	4	0			
11	Gross et al., 2000				0	8	0			
12	Hartley & Frohmann, 2003				0	0	5			
13	Jaffe et al., 1993	4	0	0						
14	Jolin et al., 1998	2	9	3						
15	Kingsnorth, 2006	0	1	0	0	1	0	0	1	0
16	Klein & Tobin, 2008	0	0	0	0	0	2	0	2	0
17	Marsland et al., 2001	2	0	0				0	1	0
18	Murphy et al., 1998				0	2	0	0	2	0
19	Newmark et al., 2001				0	2	0			
20	Orchowsky, 1999							0	0	1
21	Peterson, 2003				0	2	0			

(continued)

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TABLE 3. (*continued*)

Study No.	Authors/Data	Prosecution			Conviction			Sanction Severity		
		Less	None	More	Less	None	More	Less	None	More
22	Peterson, 2003	0	3	0						
23	Peterson, 2004				1	1	0	1	0	0
24	Steinman, 1988	0	1	0	0	1	0	0	3	0
25	Steinman, 1991	0	1	1						
26	Tolman & Weisz, 1995				0	2	0			
27	Ventura & Davis, 2005				1	0	0	1	0	0
	Subtotal	15	17	4	3	39	7	2	11	2
28	Wooldredge & Thistlethwaite, 1999	3	15	0						
29	Wooldredge, 2002	2	0	0	0	2	0			
30	Wooldredge & Thistlethwaite, 2002	1	2	0						
31	Wooldredge & Thistlethwaite, 2005	3	0	0	4	9	2			
	Direction reported	**Less**	**None**	**More**	**Less**	**None**	**More**	**Less**	**None**	**More**
	Summary of effects	24	34	4	7	50	9	2	11	2
	Number of studies		16 out of 31			20 out of 31			10 out of 31	

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The seven papers finding support for more sanctions concluded for example that "[b]oth social and legal sanctions contribute to desistance, though their effects appear to be greater for those with less severe histories of violence" (Fagan, 1989, p. 413), " . . . that coordinated interventions may have a cumulative effect on recidivism risk" (Murphy et al., 1998, p. 278), and that not charging someone following arrests " . . . detracts from any deterrent effect that a disposition and sentence may afford" (Marsland et al., 2001 p. 30). Among the 11 papers producing counterfactual findings, some concluded for instance that "[t]he presence of a penal sanction did not emerge as a deterrent to recidivism" (Frisch et al. 2001, p. 51), that "none of the sentencing options investigated here significantly curtailed domestic violence recidivism" (Gross et al., 2000, p. 309), and that "[t]he chance of six-month follow-up violence is unaffected by how a case is ultimately settled in court pursuant to tracking under a particular policy" (Ford & Regoli, 1993, p. 55).

Our independent assessment of these same analyses finds somewhat of a different distribution between support, equivocal, and no empirical support for criminal justice sanctions. Among all 31 studies, just four (13%) produced consistent evidence supporting one or more of hypothesis, eight (25%) found evidence both supporting and not supporting one or more hypotheses, and the remaining 19 (61%) reported no supporting evidence or evidence that pointed in a different direction (i.e., escalation of violence postsanction). Thus, our collective qualitative assessment, as well as that of most individual authors, is that there is less evidence supporting the use of more criminal sanctions to prevent further violence than there is evidence showing no crime prevention benefits because of sanctions beyond arrest. This conclusion, which follows our qualitative review of 31 studies, is similar to several earlier reviews of just a few of these studies produced by Davis et al. (1998) and Klein (2004).

To move from a broad qualitative summary of conclusions to a quantitative assessment of the benefits of criminal sanctions, we report in Table 3 the number of reported tests for each of 31 studies by whether those tests show that prosecution, conviction, or sanction severity is associated with less repeat offending, more repeat offending, or no difference in repeat offending. Based on these data, we then summarize the extent to which the published findings do and do not support our three hypotheses. Overall, we find that the most frequent outcome is that criminal justice sanctions that follow an arrest for intimate partner violence have no effect, in either a positive or a negative direction, on the pattern of subsequent offending. Sixty-six percent (95 out of 143) of the reported tests show no statistically significant differences in the quantity of repeat offending between different types of criminal sanctions. This relationship holds for all three hypotheses: 55% of the prosecution tests, 76% of the conviction tests, and 73% of the sentence severity tests show no effects on repeat offending. Besides, summarizing 142 significance tests, we also find that just three (10%) of the 31 studies produce unequivocal prevention effects following more sanctions, 10 (32%) studies find both significant preventive and nonpreventive effects, and 18 (58%) studies only find no preventive effects. Besides

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looking at the number of tests by hypothesis, we also assessed whether there were associations between the direction of the test results and the nature of the studies, such as whether a study's data came from victims or official sources, the type of data analysis (bivariate vs. multivariate), or the data collection period. Based on the findings produced by Jolin et al. (1998), we had, for example, hypothesized that findings based on victim interviews were more likely to find preventive effects. Unfortunately, this more detailed scrutiny of the findings also did not find a notable pattern between sanctions and repeat offending that would change our conclusions. Accordingly, these simple counts of significance tests and overall study findings favor our earlier conclusion that criminal sanctions for intimate partner violence have no effect on repeat offending.

Although the predominant finding reported in Table 3 favors the null hypothesis, there are 33 (23%) statistical tests that support one of the three crime control hypotheses and just 15 tests (10%) show contrary results—that prosecution, conviction, and severe sentences are associated with more, not less, repeat offending. This simple count of the published statistically significant test results provides some limited support for the idea that criminal justice sanctions can reduce violence against women. This comparison is more striking for the prosecution hypothesis, for which 24 tests support prosecution and only four tests show contrary results. However, the results for the conviction and the sentence severity hypotheses are more balanced with seven supporting and nine contrary findings for the conviction hypothesis, and two supporting tests and two contrary tests for the sentence severity hypothesis. The prosecution hypothesis finds support in 10 studies; the contrary results come from only three studies. Similarly, the conviction hypothesis finds some support in five studies and contrary evidence comes from only two studies.

In summary, based on our detailed review of each of these 31 studies and the summary of their reported findings displayed in Table 3, we conclude that the predominant finding reported in the research literature is that there is no crime control effect for the use of criminal sanctions against intimate partner violence. Support for these null findings and the limited support for the prosecution hypothesis, however, are dependent on the relative strengths and weaknesses of the research samples, measures, and methods used in this research. In a subsequent section, we use the narrative reviews and the study descriptions to establish the extent to which the samples, methods, and measures used in these studies vary and address the implications of that variation on the validity and reliability of those substantive findings.

Measures, Methods, and Samples Used to Test Crime Control Hypotheses

Our examination of the measures, samples, and methods used in the 31 studies of the effects of prosecution on repeat offending has identified seven methodological issues that appear to be sufficiently problematic that we recommend caution in using the published findings currently available to evaluate public policy or to test

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criminological theory. The seven methodological issues we have identified are (a) the measurement of repeat offending, (b) the measurement of criminal sanctions, (c) the analytical models tested, (d) the failure to report statistical power, (e) the lack of attention to missing data, (f) the inattention to the potential for sample selection bias, and (g) the lack of attention to broader theories of human behavior. Individually and in combination, these methodological issues raise serious concerns about whether the reported findings should be trusted, regardless of whether they show more repeat offending, less repeat offending, or no effects for criminal sanctions.

Measurement of Repeat Offending. We reviewed the 31 publications for any reported measures of new criminal behavior. Our review identified how these measures of recidivism vary along six dimensions: (a) the source of the data (e.g., police records, prosecution records, or victim interviews), (b) the type of offenses included (e.g., any offense, violence, or domestic violence), (c) the victim–offender relationship (e.g., intimate partners, other domestic relationships, any), (d) the point at which the time at risk for reoffending begins (e.g., following arrest, following case disposition, or following the completion of a sentence), (e) the time at which the time at risk ends (e.g., 6 months, 12 months, 18 months), and (f) whether the analyses addressed the prevalence of repeat offending, the frequency of repeat offending, or the time to first new offense.

Table 2 lists each measure for each study and displays how each of these studies varies on these six dimensions. Overall, the 31 studies collective report using 87 recidivism measures. Twenty-four (28%) of these measures stem from victim interviews and 41 (47%) measure recidivism using arrest reports. Twelve out of the 87 measures capture recidivism/failures as any new incident report. The remaining 10 measures use police call for service, prosecutor, or court records to capture recidivism. Some publications use only one measure of repeat offending but most use two or more measures. The Jolin et al. (1998) publication reports effects for 14 distinct recidivism measures. In 13 measures, any type of new offense counts as a repeat, whereas 30 measures only capture violence and another 19 measures are limited to domestic violence only. Just one measure counted psychological abuses. In 29 measures, the victim in the recidivism event could be any victim (i.e., does not need to be the same as the original offense); for 34 measures, only those events involving the same victim count as recidivism; and for two measures, the victims was someone not involved in the original incident.

In research on the effectiveness of arrest (see Garner & Maxwell, 2000; Sherman, 1992), the period of risk consistently begins on the first day after the original incident. That approach to defining recidivism is also used in 46 of the 87 measures in the literature on the effectiveness of criminal sanctions. Twenty-eight measures do not begin to measure repeat offending until after the case disposition has been decided; five measures begin the time at risk after the arraignment, and eight measures start at the time the offender is convicted (i.e., completion). In one study with four publications and five measures of recidivism, the researchers define each case's

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time at risk as when the disposition or sentence (including jail time) were completed (see Wooldredge & Thistlethwaite, 1999). They choose this start time because they assert that a proper test of a sanction cannot begin until the defendant was fully treated (e.g., served the entire sanction period).

In this literature, when the time at risk ends is even more variable. Thirty-one measures use the 6-month period common in the arrest studies (see Garner, Fagan, & Maxwell, 1995), but 18 measures use a 12-month time at risk, and 13 measures use an 18-month time at risk. In one study (see Klein & Tobin, 2008) with four outcome measures, the time at risk last 108 months. All measures are truncated at some point by the end of data collection and this can create variable times at risk in both official records as well as victim interviews. Seventy-three (84%) out of the 81 measures define recidivism as the prevalence of an event during a fixed period, 11 (13%) measure the frequency of new criminal events, and two of the measures capture the time to first new event. This substantial variability in how these measures of repeat offending are defined in turn produces a wide range in the rates of repeat offending. Table 2 shows that the recidivism rates ranged from a low of 3.1% (see Ford & Regoli, 1992) to the highest rate of 65.5% (see Klein & Tobin, 2008). This reported variability diminishes our ability to argue that the reported findings summarized in Tables 1 and 3 are derived from the same underlying behavior. In addition, the predominant source of information about repeat offending used in these studies is an official police record of subsequent criminal offenses and arrests of the offender. Few of the studies reviewed here provide details on the quality or completeness of those measures, whether those records include city, county, state, or national record checks, or even the date the record checks were completed. Even if common definitions or repeat offending were used, the failure of law enforcement and judicial agencies to promptly and accurately post case disposition information adds to the concerns about measuring repeat offending.

Measuring Case Dispositions. The research literature we reviewed here is often unclear or inconsistent about what constitutes a "prosecution," a "conviction," or what the nature of the sentence imposed is. Some studies count the filing of charges as a prosecution (e.g., Wooldredge & Thistlethwaite, 2002), but other studies report the effects of cases that are "fully" prosecuted (e.g., Kingsnorth, MacIntosh, Berdahl, Blades, & Rossi, 2001). In the later specification, cases that are dropped, dismissed, or "nolle prosed" by the court are not counted as prosecutions. Similarly, disparate definitions are used in describing how a case is disposed. Among the more problematic categories includes the one that captures offenders who are "sentenced" to diversion programs or batterer treatment programs, sometimes with and sometimes without a formal conviction. This issue is no more clearly found demonstrated than in the four publications that stem from the same data from Hamilton County, Ohio.

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Analytical Method. Each of these studies link criminal sanctions with repeat offending but the methodological rigor in the way they make those connections. Nineteen of these 31 studies tested for an association between criminal sanctions and repeat offending using multivariate statistical techniques. Some of those multivariate analyses include few control measures, whereas others consider an extensive array of factors including simultaneously specifying contextual and individual level effects. Two publications compare repeat offending rates before and after the introduction of specialized courts. In nine studies, the reported finding relied on simple bivariate comparisons and some of those analyses did not report tests of statistical significance. In combination with the various outcome measures and approaches to measuring case dispositions, the diversity in analytical methods used in this research literature does not provide a solid basis for the construction of standardized effect sizes with which to assess the average effect of sanctions for intimate partner violence.

Model Development and Testing. Not only do these studies vary in how they define prosecution, conviction, and sentencing severity, but the statistical findings about each of these hypotheses are derived from a diverse set of statistical comparisons. Four of the seven studies that address the prosecution hypothesis (see Davis et al., 1998; Fagan et al., 1984; Tolman & Weisz, 1995; Wooldredge & Thistlethwaite, 2002) conducted analyses comparing some cases where charges were filed with some cases where charges were not filed. Ford and Regoli (1993) compare alternative prosecutorial policies. Newmark et al. (2001) and Hartley and Frohmann (2003) 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 convictions are compared to all arrests or just to those arrests that result in prosecution. Some of this diversity is because some studies sample arrests and other studies only sample cases where at least one charge was filed.

Statistical Power. Not one of the 31 studies reports any statistical power computation for any of their statistical tests (Cohen, 1988). Eight of the 31 studies have sample sizes of less than 300; the median sample size is 581 cases. Moreover, most of the analyses reported involve less than the total number of cases because of missing data, or because of the particular comparisons reported only involve a fraction of the total cases. The lack of reporting about statistical power is a serious omission, especially because the predominant finding in this literature is "no effect." When statistical power is low (or in this case not reported at all), 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. Thus, interpreting the findings reported in Table 3 is even less straightforward, given that we cannot easily assess the validity of the 95 tests that show no effect.

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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 & Bomberg, 1975) and sentencing (Crutchfield, Bridges, & Pitchford, 1994; Hagan & Zatz, 1985), none of the published analyses of repeat offending following prosecution for intimate partner violence address this issue or acknowledge the problem as a possible caveat to their findings. The essence of this complex issue is that the process that determines whether an arrest leads to prosecution determines not only the size but also the extent to which the prosecution sample has a higher or lower risk for repeat offending. For instance, if only a small proportion of the low-risk offenders are prosecuted, then the prosecuted cases can be expected to be more likely to commit a new offense than the cases that are not prosecuted, just because of the selection process. Sample selection effects 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 try to separate selection effects from the treatment effects of prosecution, conviction, or sentencing severity. Given the assumption that criminal justice officials are more likely to impose more severe sanctions on offenders they consider more likely to reoffend, the reported findings are likely to be a combination of the effects of selection and the effects of criminal sanctions. What is reported in the studies summarized previously, is an unknown combination of selection and treatment effects, and this too is another impediment to understanding the reported findings from this body of research.

Missing Cases and Missing Data. There is also a consistent problem in this research literature with how missing data are addressed, particularly for studies that rely on victim interviews for their outcome data. Most of these studies report that they drop cases completely from their analyses where data from one or more variables in their model are missing. Although 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). Similarly, our review of the studies employing victim interviews as the source of outcome data found no consideration of the victim interview selection process and no attempts to address the nonrandom selection of victims who were interviewed (Brame, 2000; Maxwell, 1998).

The issues of selection bias and missing data are related. Dropping cases because of missing data can create selection bias in the sample of cases for which results are reported. In at least some analyses, the definition of the outcome measure creates problems with missing data and selection bias. For instance, Wooldredge and Thistlethwaite (1999) eliminated more than 500 cases from certain analyses. These cases were dropped for several reasons. First, one of the outcome measures Wooldredge and Thistlethwaite chose required 24 months at risk. Second, they chose to begin the time at risk when the case disposition was

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complete including any jail or probation time. Third, they stopped collecting data on repeat offending before the 24-month period was complete. Although each of these decisions are justified by the authors and in some cases applauded in their own right, the effect was to drop 500 cases from the analyses. Most of the 500 cases were sentenced to probation, and, in fact, they constituted half of the probation cases.

Lack of Theory. The last methodological issue that impedes our ability to make reliable judgments about the effectiveness of criminal sanctions for intimate partner violence is the failure to articulate a theoretical basis for how sanctions might affect future behavior. The existing research on intimate partner violence prosecution tends to be policy oriented and theoretical (Ford & Breall, 2003). This practical emphasis stems the prominent role of the modern domestic violence reform movement and the strong support for prosecution and other criminal justice responses in intimate partner violence incorporated into the Violence Against Women Act of 1994. The policy orientation can be seen in Murphy et al.'s (1998) focus on the effects of coordinated community responses. Similarly, Newmark et al. (2001) and Hartley and Frohmann (2003) concentrate on the differences between general and specialized domestic violence courts. Ford and Regoli (1993) compare a policy that allows victims to file charges independent of the police and a policy that denies victims the opportunity to drop charges.

Some studies have incorporated theoretical concerns. Ford and Regoli (1993) identify victim empowerment as the underlying mechanism to explain their results. In addition, the Minneapolis Domestic Violence Experiment (Sherman & Berk, 1984), the spouse assault replication studies (Garner & Maxwell, 2000), and several intimate partner violence prosecution studies (e.g., Davis et al., 1998; Thistlethwaite, Wooldredge, & Gibbs, 1998; Wooldredge & Thistlethwaite, 2002) use specific deterrence theory to articulate why the sanctions associated with criminal justice interventions might reduce repeat victimization.

Our approach in this review is not to attribute the reported treatment effects to a particular theoretical mechanism, such as specific deterrence or victim empowerment, because the available research literature does not provide sufficient information to distinguish the effects of one mechanism or another. Our approach is to label association between criminal sanctions and repeat offending as a generic "crime control" effect. This approach identifies the extent to which sanctions are associated with reductions in future offending, regardless of the underlying mechanisms involved. Using this approach, our review retains its relevance to understanding the effectiveness of the current policy of promoting criminal sanctions; moreover, it does not present itself as a test-specific deterrence, victim empowerment, or any other microtheories when the published reports do not provide the kind of information needed for such a test. Future research would be enhanced if it were designed to do what this review cannot

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generate more rigorous tests of the relative strength of these alternative microtheoretical mechanisms.

Summary of Methodological Issues. Individually and in combination, these methodological problems seriously weaken the validity and the reliability of the published findings about the crime control effects of prosecuting intimate partner violence. If 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. Yet because they were not addressed, the value of the existing research for testing theories and for evaluating policies is considerably less than it could be.

This assessment is based on a detailed review of each study, an explicit comparison of the reported findings about the effects of criminal sanctions, and an assessment of the extent to which the current research meets the basic expectations of contemporary standards for criminological research. Not only are there methodological limitations to each individual study, there are also limitations to the body of research. Our review of this body of research chronicled the lack of consistency on two crucial issues: how criminal sanctions are measured and how repeat offending is measured. If one or two studies diverted slightly from a common measure, it might be reasonable to employ some form of a standardized effect size but that is not the case here. Given that the prevalence of reoffending derived from these diverse measures varies from 3.1% to 71.3% and the unique conceptualizations and measurements of criminal sanctions used by these studies, it is asserted that this research, or any substantial subset of this research, provides a solid basis for accumulating knowledge, evaluating policy, or testing theories.

The inattention to issues of statistical power in this research severely undermines the validity of the predominant findings of no effect because many of the analytical designs may not have been rigorous enough to find an effect, even if there is one. The inattention to issues of selection bias undermines not only the findings that show more severe sanctions associated with more repeat offending but all other findings as well. The potential for selection bias exists in the allocation of all types of sanctions and the reported effects for all sanction comparisons are a combination of both selection and treatment effects. Interpreting the reported findings as solely treatment effects, without at least some controls for selection effects, invites error.

Finally, among these plausible alternative explanations for the effects of sanctions, the unique aspect of specific deterrence theory is that it posits changes in the level of fear within the offender as the crucial mechanism leading to reductions in repeat offending. However, none of the research on the effects of criminal sanctions measures the offender's fear of future sanctions and that consideration is not incorporated into any of the following analyses we reviewed. Some of the studies reviewed here (e.g., Ford & Regoli, 1992; Jolin et al., 1998) incorporate the idea of victim empowerment into their interpretation of their results, but they do not measure empowerment directly. To test these more specific theoretical mechanism, study designs need to

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include measures of offender fear or victim empowerment. Of course, both of these mechanisms—fear and empowerment—may be operational at the same time with the same victim–offender relationship; they may enhance each other or they may conflict, or even cancel each other out.

CONCLUSIONS

Our assessment of the published research on the crime control effects of criminal sanctions for intimate partner violence leads us to conclude that, although the preponderance of the reported findings show no effect for criminal sanctions, the diversity of reported findings and the relatively low quality of the research methods used in this research provide insufficient knowledge to support any conclusion about whether the widespread use of criminal prosecution and conviction for intimate partner violence leads to less, more, or has no difference in repeat offending. This assessment is more than a researcher's traditional lament for more and better research. Few among these 31 studies approximate the kind of research characteristics that were recommended more than 30 years ago by the National Academy of Sciences (see Blumstein, Cohen, Martin, & Tonry, 1982; Sechrest, White, & Brown, 1979), nor do they approach the methodological rigor used in the 1980s and 1990s to test the crime control effects of arrest for intimate partner violence (Sherman & Berk, 1984; Sherman, 1992). This negative assessment is about an entire body of research, not just one or two studies. Each of these 31 individual studies has strengths, and some of them have considerable strengths. Our assessment is that, as a body of research, it is difficult to assert that these studies provide a scientific basis to support or oppose a particular policy or to test a particular hypothesis about the crime control effects of criminal sanctions on intimate partner violence.

This assessment of the published research also leads to two recommendations for future research—one about future data collection and other about further data analyses. If our assessment about severe limitations with the current body of research is correct, the implementation of marginally improved but still poorly designed, single site, low powered, underfunded studies will not change our bleak assessment. One or even several more marginally improved studies will not likely to change our synthesis of this body of research. Perhaps with the production of 15 independent studies, all with some measurement or methodological improvements, it might become possible to draw stronger conclusions about the crime control effects of criminal sanctions. Such an approach would take resources and time. But given the inadequacies of the existing body of research, we not only question the likely benefits of such an approach but also the ethical propriety of conducting more similarly weak studies poorly measuring the crime control effects of criminal sanctions for intimate partner violence.

We recommend a structured program that can provide rigorous tests of the each crime control hypothesis. Any effort to make progress toward evidence-based policymaking on the effects of criminal sanctions must articulate the causal mechanisms

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under review, model when the effects will begin and dissipate, address the use of reliable and common outcome and sanction measures, distinguish selection effects from treatment effects, incorporate rigorous multivariate analyses, and meet contemporary standards for statistical power. These are minimum requirements for future research; we suspect that additional concerns such as the incorporation of community level characteristics and the identification of offender typologies are needed to advance policymaking and theory testing. If we want to know the crime control effects of criminal sanctions on intimate partner violence, what is also required is a commitment to a well-funded, well-designed long-term program of research. Such an approach would be a departure from the current structure of both the funding and the production of criminological research in general and intimate partner violence in particular. Such a structured approach does have a precedent in research on intimate partner violence. Although the methods and measures used in the program are no longer on the cutting edge of contemporary research standards, the National Institute of Justice sponsored Spouse Assault Replication Program, although not perfect, made progress toward providing the kind of rigorous, systematic evidence needed to assess the crime control effects of arrest for intimate partner violence (Garner & Maxwell, 2000).

There is some hope for the immediate future in the conduct of data analyses. Using the data that have been collected, it is possible to improve our understanding of this body of research and of the crime control effects of criminal sanctions through secondary data analyses. The raw data from many of the studies reviewed here have been publicly archived and can be used to generate more consistent tests of the crime control hypotheses. Depending on the nature of the original measure, secondary data analyses can produce more consistent measures of criminal sanctions and measures of recidivism. Bivariate and narrative analyses can be tested in multivariate models and diverse multivariate approaches can be made more consistent. If appropriate data are available to predict the imposition of sanctions, multivariate models can attempt to separate selection from treatment effects. The use of secondary data analyses provides the criminological community the opportunity to continue the critique of these 31 studies and to conceive and test alternative ways to more rigorously associate criminal sanctions with future violence between intimates. Further analyses of existing data may not only shed light on the effectiveness of criminal sanctions in these 31 studies but also provide the framework for the kinds of original analyses that could result from improved designs contemplated previously.

NOTES

1. Early reviews of the prosecution of intimate partner violence were critical of the lack of prosecutorial vigor (Cahn, 1992; Ellis, 1984; Lerman, 1986; Parnas, 1970; Waits, 1985), which seemed supported by McLeod's (1983) report that only 143 (2.4%)

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of 5,480 domestic violence arrests in Detroit resulted in a prosecution and by the report of Schmidt and Steury (1989) that 209 (9.4%) of 2,212 reported domestic violence offenses in Milwaukee resulted in a prosecution.

2. Our approach, however, may be too simplistic in disaggregating sanctions. For instance, there may be differential effects of prosecution, when the comparison groups include all known offenses or when the comparison group only includes cases where an arrest was made.
3. The Peterson (2003) study reported the results from two separate studies.
4. Two studies (see Jaffe et al., 1993; Marsland et al., 2001) use data from Canadian jurisdictions.
5. For this project, all references to statistical significance mean significant at the p value $< .05$ level.

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