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The Specific Deterrent Effects of Criminal Sanctions for Intimate Partner Violence: A Meta-Analysis

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CRIMINOLOGY

THE SPECIFIC DETERRENT EFFECTS OF CRIMINAL SANCTIONS FOR INTIMATE PARTNER VIOLENCE: A META-ANALYSIS

JOEL H. GARNER, CHRISTOPHER D. MAXWELL & JINA
LEE*

A dozen systematic reviews published since 1978 have sought to clarify the complexities of deterrence theory. These reviews emphasize the general deterrent effects of police presence, arrest, and incarceration on rates of homicide and other serious crimes, such as assault, rape, and burglary. These reviews provide less attention to specific deterrence processes and to the deterrent impacts of intermediate sanctions, such as prosecution or conviction; none of these reviews incorporate any of the research on criminal sanctions for intimate partner violence. To address these limitations, this research uses meta-analytic methods to assess the specific deterrent effects of three post-arrest criminal sanctions—prosecution, conviction, and

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incarceration—for one offense type—intimate partner violence. Based upon 57 studies that reported 237 tests of specific deterrence theory, the effects of sanctions varied: there is a marginal deterrent effect for prosecution, no effect for conviction, and a large escalation effect among incarcerated offenders. In addition, deterrent effects in the available research are stronger in tests that use more rigorous research designs, that measure repeat offending using victim interviews instead of official records, and that use new offenses against the same victim—not new arrests or new convictions against any victim—as the criteria for repeat offending.

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INTRODUCTION

The relationship between criminal sanctions and criminal behavior is an enduring theme throughout classical and contemporary criminological thought.¹ Lawrence Sherman goes so far as to assert that the “conceptual core

¹ See, e.g., Johannes Andenaes, *The General Preventive Effects of Punishment*, 114 U. PA. L. REV. 949 (1966); CESARE BONESANA DI BECCARIA, AN ESSAY ON CRIMES AND PUNISHMENT (Albany, NY, W.C. Little & Co. 1872); JEREMY BENTHAM, AN INTRODUCTION TO THE PRINCIPLES OF MORALS AND LEGISLATION (New Ed., Clarendon Press 1907) (1780); NAT’L RSCH. COUNCIL, DETERRENCE AND INCAPACITATION: ESTIMATING THE EFFECTS OF CRIMINAL

of criminology is the science of sanction effects”² Scholars have interpreted the relationship between the application of criminal sanctions and subsequent criminal offending in terms of labeling theory,³ re-integrative shaming,⁴ victim empowerment,⁵ and defiance.⁶ Much of the contemporary scholarly literature on criminal sanctions, like much of the current policy attention, focuses on deterrence theory—the argument that potential offenders are dissuaded from future criminality by the threat of future penalties that are appropriately swift, certain, and severe.⁷

The deterrence framework, articulated by Cesare Beccaria as part of an effort to reduce the severity of the then-common penalties of execution, torture, and lengthy incarceration,⁸ is employed in the contemporary American context to support the use of capital punishment;⁹ longer prison sentences;¹⁰ mandatory prison terms for using a firearm in the commission of a felony;¹¹ the threat of more severe sanctions for youth violence and drug sales;¹² prosecution for tax evasion;¹³ increases in the number of sworn police

SANCTIONS ON CRIME RATES (Alfred Blumstein, Jacqueline Cohen & Daniel Nagin, eds., 1978); GERHARD O. W. MUELLER, SENTENCING: PROCESS AND PURPOSE § 12 (1977); Jack P. Gibbs, *Crime, Punishment, and Deterrence*, 48 SW. SOC. SCI. Q. 515 (1968); Daniel S. Nagin, *Deterrence in the Twenty-First Century*, 42 CRIME & JUST. 199 (2013).

² Lawrence W. Sherman, *Defiance, Deterrence, and Irrelevance: A Theory of the Criminal Sanction*, 30 J. RSCH. CRIME & DELINQ. 445, 446 (1993) (emphasis omitted).

³ See David P. Farrington & Joseph Murray, *Empirical Tests of Labeling Theory in Criminology*, in 18 LABELING THEORY: EMPIRICAL TESTS 1, 1–9 (David P. Farrington & Joseph Murray eds., 2014) (describing the history of testing labeling theory).

⁴ See generally JOHN BRAITHWAITE, CRIME, SHAME AND REINTEGRATION (1989) (presenting the seminal articulation of the logic and effects of reintegrative shaming).

⁵ See generally Lauren Bennett Cattaneo & Lisa A. Goodman, *What is Empowerment Anyway? A Model for Domestic Violence Practice, Research, and Evaluation*, 5 PSYCH. VIOLENCE 84 (2015) (identifying a common empowerment framework for researchers and practitioners).

⁶ See Sherman, *supra* note 2, at 459–66.

⁷ E.g., JACK P. GIBBS, CRIME, PUNISHMENT AND DETERRENCE (1975); FRANKLIN E. ZIMRING & GORDON J. HAWKINS, DETERRENCE: THE LEGAL THREAT IN CRIME CONTROL (1973) (explicating the elements of deterrence theory).

⁸ See BECCARIA, *supra* note 1, at 93–111.

⁹ See Issac Ehrlich, *The Deterrent Effect of Capital Punishment: A Question of Life and Death*, 65 AM. ECON. REV. 397 (1975) (providing an early economic analysis of the benefits of capital punishment).

¹⁰ Daniel Kessler & Steven D. Levitt, *Using Sentence Enhancements to Distinguish Between Deterrence and Incapacitation*, 42 J.L. & ECON. 343, 346–50 (1999).

¹¹ ATT’Y GEN.’S TASK FORCE ON VIOLENT CRIME, FINAL REPORT, 30–32 (1981).

¹² See DAVID M. KENNEDY, DETERRENCE AND CRIME PREVENTION: RECONSIDERING THE PROSPECT OF SANCTION 142–65 (Peter Reuter & Ernesto U. Savona eds., 2009).

¹³ Steven Klepper & Daniel Nagin, *Tax Compliance and Perceptions of the Risks of Detection and Criminal Prosecution*, 23 L. & SOC’Y REV. 209, 238–39 (1989).

officers;¹⁴ police foot patrols;¹⁵ hot-spots policing;¹⁶ stop-and-frisk police tactics;¹⁷ arrest for juvenile offenders;¹⁸ arrest of prostitution clients;¹⁹ and arrest,²⁰ prosecution,²¹ conviction,²² and incarceration²³ for intimate partner violence.

Amidst the violent upheavals of the English Civil War, Thomas Hobbes asserted that the state's use of criminal sanctions was necessary to preserve the commonwealth, without which life would be "solitary, poor, nasty, brutish, and short."²⁴ A century later in Italy, Cesare Beccaria argued that, while criminal sanctions are necessary, authorities should constrain their use because "[t]he end of punishment, therefore, is no other than to prevent the criminal from doing further injury to society, and to prevent others from committing the like offence."²⁵ Beccaria's twin goals for punishment provide the groundwork for modern distinctions between specific deterrence—to influence the sanctioned offender—and general deterrence—to influence the behavior of others. General deterrent effects of sanctions are seen as having an indirect impact on the behavior of individuals whether or not they have

¹⁴ Steven D. Levitt, *Using Electoral Cycles in Police Hiring to Estimate the Effect of Police on Crime*, 87 AM. ECON. REV. 270, 286 (1997).

¹⁵ Jerry H. Ratcliffe, Travis Taniguchi, Elizabeth R. Groff & Jennifer D. Wood, *The Philadelphia Foot Patrol Experiment: A Randomized Controlled Trial of Police Patrol Effectiveness in Violent Crime Hotspots*, 49 CRIMINOLOGY 795, 818 (2011).

¹⁶ Anthony A. Braga, Andrew V. Papachristos & David M. Hureau, *The Effects of Hot Spots Policing on Crime: An Updated Systematic Review and Meta-Analysis*, 31 JUST. Q. 633, 655–56 (2014).

¹⁷ See David Weisburd, Alese Wooditch, Sarit Weisburd & Sue-Ming Yang, *Do Stop, Question, and Frisk Practices Deter Crime?*, 15 CRIMINOLOGY & PUB. POL'Y 31, 46–47 (2016).

¹⁸ See Douglas A. Smith & Patrick R. Gartin, *Specifying Specific Deterrence: The Influence of Arrest on Future Criminal Activity*, 54 AM. SOC. REV. 94, 102–03 (1989).

¹⁹ See Devon. D. Brewer, John J. Potterat, Stephen Q. Muth & John M. Roberts, Jr., *A Large Specific Deterrent Effect of Arrest for Patronizing a Prostitute*, 1 PLOS ONE 1, 1–2 (2006).

²⁰ See Lawrence W. Sherman & Richard A. Berk, *The Specific Deterrent Effects of Arrest for Domestic Assault*, 49 AM. SOC. REV. 261, 268–70 (1984).

²¹ BARBARA J. HART & ANDREW R. KLINE, PRACTICAL IMPLICATIONS OF CURRENT INTIMATE PARTNER VIOLENCE RESEARCH FOR VICTIM ADVOCATES AND SERVICE PROVIDERS 156–59 (2013).

²² Edna Erez, *Domestic Violence and the Criminal Justice System: An Overview*, 7 ONLINE J. ISSUES NURSING 4, 7–8 (2002), <http://ojin.nursingworld.org/MainMenuCategories/ANAMarketplace/ANAPeriodicals/OJIN/TableofContents/Volume72002/No1Jan2002/DomesticViolenceandCriminalJustice.html> [https://perma.cc/633P-KKL5].

²³ Cheryl Hanna, *The Paradox of Hope: The Crime and Punishment of Domestic Violence*, 39 WM. & MARY L. REV. 1505, 1542–44 (1997).

²⁴ THOMAS HOBBS, *LEVIATHAN* 78 (Rod Hay ed., McMaster Univ. 1999) (1651).

²⁵ BECCARIA, *supra* note 1, at 47.

previously committed an offense or whether they have been sanctioned or not. Specific deterrent effects of sanctions have traditionally been seen as more direct and as only affecting offenders who have been sanctioned.²⁶ However, our understanding of specific deterrence has been broadened to consider the extent to which avoiding sanctions increases future offending as well as whether being sanctioned reduces future offending.²⁷

I. LITERATURE REVIEW

A. REVIEWS OF DETERRENCE RESEARCH

The extensive body of research that invokes deterrence theory to interpret the empirical relationships between criminal sanctions and offending has generated a dozen detailed reviews since 1978.²⁸ These reviews assess a large and diverse body of research that includes a variety of empirically tested hypotheses about whether, to what extent, and in what direction various aspects of contemporary criminal sanctions affect any or all aspects of subsequent criminal behavior.²⁹ While none of these reviews capture all the composite parts of deterrence theory, they are part of a vibrant, contemporary enthusiasm to clarify the complexities of deterrence theory and

²⁶ Johannes Andenaes, *Does Punishment Deter Crime?*, 11 CRIM. L.Q. 76, 78–79 (1968).

²⁷ Mark C. Stafford & Mark Warr, *A Reconceptualization of General and Specific Deterrence*, 30 J. RES. CRIME & DELINQ. 123, 127–29 (1993).

²⁸ See Robert Apel, *Sanctions, Perceptions, and Crime: Implications for Criminal Deterrence*, 29 J. QUANTITATIVE CRIMINOLOGY 67, 93 (2013); Robert Apel & Daniel S. Nagin, *General Deterrence*, in THE OXFORD HANDBOOK OF CRIME AND CRIMINAL JUSTICE 179, 179–206 (Michael Tonry, ed., 2011); Aaron Chalfin & Justin McCrary, *Criminal Deterrence: A Review of the Literature*, 55 J. ECON. LITERATURE 5, 5–6 (2017); Philip J. Cook, *Research in Criminal Deterrence: Laying the Groundwork for the Second Decade*, in 2 CRIME & JUST. 211, 211–268 (1980); Steven N. Durlauf & Daniel S. Nagin, *Imprisonment and Crime: Can Both Be Reduced?*, 10 CRIMINOLOGY & PUB. POL'Y 13, 13–54 (2011); Daniel S. Nagin, *Criminal Deterrence Research at the Outset of the Twenty-First Century*, 23 CRIME & JUST. 1, 1–42 (1998) [hereinafter Nagin, *Criminal Deterrence Research*]; Daniel S. Nagin, *General Deterrence: A Review of the Empirical Evidence*, in DETERRENCE AND INCAPACITATION: ESTIMATING THE EFFECTS OF CRIMINAL SANCTIONS ON CRIME RATES, *supra* note 1, at 95–139 [hereinafter Nagin, *General Deterrence: A Review of the Empirical Evidence*]; Daniel S. Nagin, Robert M. Solow & Cynthia Lum, *Deterrence, Criminal Opportunities, and Police*, 53 CRIMINOLOGY 74, 74–100 (2015); Raymond Paternoster, *The Deterrent Effect of the Perceived Certainty and Severity of Punishment: A Review of the Evidence and Issues*, 4 JUST. Q. 173 (1987); Raymond Paternoster, *How Much Do We Really Know About Criminal Deterrence?*, 100 J. CRIM. L. & CRIMINOLOGY 765, (2010) [hereinafter Paternoster, *How Much Do We Really Know*]; Travis C. Pratt & Francis T. Cullen, *Assessing Macro-Level Predictors and Theories of Crime: A Meta-Analysis*, 32 CRIME & JUST. 373, 415–17, 427–28 (2005); Kirk R. Williams & Richard Hawkins, *Perceptual Research on General Deterrence: A Critical Review*, 20 L. & SOC'Y REV. 545, 545–572 (1986).

²⁹ See *supra* note 28.

to determine the conditions under which deterrent effects can and cannot be found. One consistent theme in these reviews is that detecting the existence and estimating the size of deterrent effects is difficult and that every approach to studying deterrence effects has inconsistent findings, and methodological and measurement limitations.³⁰

Research on general deterrence has long relied on research designs that use official records of actual sanctions to compare the annual number of arrests, police officers, prison populations, or executions with data on offense types, such as homicide, assault, rape, robbery, and burglary, captured in the FBI's Uniform Crime Reporting Program.³¹ In this macro-level research approach, the number of individual-level sanctions and offenses are aggregated to the level of cities, counties, standard metropolitan statistical areas, or states, and compared between jurisdictions and over time. The recognition of measurement errors in the available data and the inability to resolve disputes over the appropriate identification criteria in macro-level statistical models encouraged evaluators of deterrence theory to study the effects of variation in official sanctions in single locations over time,³² between treatment and control locations in a particular jurisdiction,³³ among different individuals in a particular jurisdiction,³⁴ or for the same individuals over time.³⁵ In the 1970s, Franklin Zimring recommended the use of targeted policy interventions such as clinical trials, longitudinal surveys, matching, propensity scoring, quasi-experiments, bivariate and multivariate analyses, comparisons of nonequivalent treatment and control groups, and even qualitative studies as valuable approaches for testing deterrence theory.³⁶ Subsequently, evaluations of policy interventions became a major

³⁰ See *id.*

³¹ Nagin, *General Deterrence: A Review of the Empirical Evidence*, *supra* note 28, at 99–111.

³² See, e.g., Lan Shi, *The Limit of Oversight in Policing: Evidence from the 2001 Cincinnati Riot*, 93 J. PUB. ECON. 99 (2009).

³³ See, e.g., Lawrence W. Sherman & David Weisburd, *General Deterrent Effects of Police Patrol in Crime "Hot Spots": A Randomized, Controlled Trial*, 12 JUST. Q. 625 (1995).

³⁴ See, e.g., John D. Wooldredge & Amy Thistlethwaite, *Reconsidering Domestic Violence Recidivism: Conditioned Effects of Legal Controls by Individual and Aggregate Levels of Stake in Conformity*, 18 J. QUANTITATIVE CRIMINOLOGY 45 (2002).

³⁵ See, e.g., Kirk R. Williams & Richard Hawkins, *Wife Assault, Costs of Arrest, and the Deterrence Process*, 29 J. RSCH. & CRIME DELINQ. 292 (1992).

³⁶ Franklin E. Zimring, *Policy Experiments in General Deterrence: 1970–75*, in DETERRENCE AND INCAPACITATION: ESTIMATING THE EFFECTS OF CRIMINAL SANCTIONS ON CRIME RATES *supra* note 1, at 140, 140–86.

component of the contemporary literature on deterrence theory.³⁷ While macro-level analyses comparing crime rates between jurisdictions are limited to the study of general deterrence, focused policy interventions can test either general deterrence or specific deterrence, depending primarily on whether the link between sanctions and subsequent offending is analyzed at the individual level or at one or more aggregate levels. Focused policy interventions typically are limited to one or two jurisdictions and this weakens their ability to generalize study findings to other jurisdictions without independent replications in numerous and diverse locations.³⁸ Although there are additional methodological considerations in the study of focused policy interventions, Daniel Nagin's assessment is that "well-conducted experimental and quasi-experimental studies of deterrence provide the most convincing evidence of the circumstances under which deterrence is and is not effective."³⁹

In addition to macro-level studies and evaluations of focused policy interventions, existing reviews of deterrence research include a substantial body of research that does not rely on the objective properties of punishment (measured by official records) but more directly assesses the role that perceptions of criminal sanctions play in potential offenders' decision-making process (measured primarily through in-depth interviews with potential offenders).⁴⁰ Just as the macro-level comparisons of sanction policies cannot assess the specific deterrent impact of official sanctions on individuals, evaluations of focused policy interventions based only on official records of sanctions cannot determine how potential offenders make decisions. They can, however, determine whether the association between sanctions and subsequent behavior is or is not in the direction predicted by deterrence theory. While there is general agreement on the importance of understanding the link between official sanctioning behavior and the perceptions of potential offenders about those sanctions, researchers disagree sharply⁴¹ about the extent to which changes in the likelihood or severity of

³⁷ Steven N. Durlauf & Daniel S. Nagin, *The Deterrent Effects of Imprisonment*, 43, 48 in *CONTROLLING CRIME: STRATEGIES AND TRADEOFFS* (Philip J. Cook, Jens Ludwig & Justin McCrary eds., 2011); Nagin, *Criminal Deterrence Research*, *supra* note 28, at 4.

³⁸ William Alex Pridemore, Matthew C. Makel & Jonathan A. Plucker, *Replication in Criminology and the Social Sciences*, 1 ANN. REV. CRIMINOLOGY 19, 21–24 (2018).

³⁹ Nagin, *supra* note 1, at 215–16.

⁴⁰ Paternoster, *How Much Do We Really Know*, *supra* note 28, at 780–86.

⁴¹ Compare Nagin, Solow & Lum, *supra* note 28, at 95 ("We are optimistic that creative interviewing techniques can be devised to identify how police tactics influence offender perceptions of apprehension risk."), with Justin T. Pickett & Sean Patrick Roche, *Arrested Development: Misguided Directions in Deterrence Theory and Policy*, 15 CRIMINOLOGY &

official sanctions are accurately perceived by potential offenders as well as the importance of these perceptions, accurate or not, in offender decision-making. Research on offender decision-making, embedded within rational choice theory, includes factors other than criminal sanctions that might better explain the circumstances under which individuals will and will not get involved in one or more types of criminal behavior.⁴² Those factors may vary by an individual's personality traits and do not need to be rational assessments of the potential gain or loss involved in offending.⁴³ Moreover, these factors could include emotions and other irrational considerations that might be especially relevant to understanding violence between intimate partners.⁴⁴ Research from the rational choice perspective can also incorporate recent advances in behavioral economics that emphasize the use of heuristics and other mental shortcuts in a wide range of human decision-making.⁴⁵ The broader scope of explanatory factors considered within the rational choice perspective will likely enhance our understanding of the relative impact of sanctions in offender decision-making. However, none of the existing studies assessing the impact of prosecution, conviction, or imprisonment on intimate partner violence have collected this type of information from potential offenders. For this reason, our current understanding of the impact of post-arrest sanctions on intimate partner violence is derived from analyses on focused policy interventions.

B. LIMITATIONS IN DETERRENCE REVIEWS

The existing reviews of deterrence research have two substantial limitations. First, they focus almost exclusively on the use of arrest or imprisonment and ignore the possible impact of intermediate level sanctions. Steven Durlauf and Daniel Nagin's review of deterrence research argues that in future tests of deterrence theory, the magnitude of deterrent effects will depend critically on the specific form of the sanction policy being studied.

PUB. POL'Y 727, 729 (2016) ("All prior studies examining the correlation between objective and perceived arrest risk have yielded null results.").

⁴² See Thomas A. Loughran, Raymond Paternoster & Alex R. Piquero, *Individual Difference and Deterrence*, in DETERRENCE, CHOICE, AND CRIME 211, 215–19 (Daniel S. Nagin, Francis T. Cullen & Cheryl Lero Jonson eds., 2018).

⁴³ Justin T. Pickett & Shawn D. Bushway, *Dispositional Sources of Sanction Perceptions: Emotionality, Cognitive Style, Intolerance of Ambiguity, and Self-Efficacy*, 39 LAW & HUM. BEHAV. 624, 624 (2015).

⁴⁴ Jean-Louis van Gelder & Reinout E. de Vries, *Rational Misbehavior? Evaluating an Integrated Dual-Process Model of Criminal Decision Making*, 30 J. QUANTITATIVE CRIMINOLOGY 1, 4–5 (2014).

⁴⁵ See generally DANIEL KAHNEMAN, THINKING, FAST AND SLOW (2011) (demonstrating dual-track decisionmaking processes in a wide range of circumstances).

Using an analogy from medicine, they argue that just as specific drugs are evaluated based on their effectiveness at treating particular ailments, it would be of great value if future research paid more attention to evaluations of the effects of distinct types of sanctions on specific types of crimes.⁴⁶ The second limitation in the existing reviews of deterrence research is that none of them consider the use of criminal sanctions for violent offenses against an intimate partner, nor do they provide a rationale for excluding this substantial body of research.

The purpose of this article is to address both of these limitations. First, it provides a concise, qualitative review of the large number of existing studies on the specific deterrent effects of arrest for intimate partner violence. Second, it provides three systematic, quantitative meta-analyses of the existing research on the specific deterrent effects of prosecuting, convicting, and incarcerating intimate partner violence offenders.

C. DETERRENT EFFECTS OF ARREST

There is a large body of research that has addressed the specific deterrent effects of arrest for intimate partner violence. The seminal study in this area is a field experiment conducted in Minneapolis which randomly assigned intimate partner violence offenders to one of three treatment groups: an arrest, on-scene counseling, or physical separation. In the Minneapolis experiment, the prevalence of re-offending by arrested offenders was about half the rate of re-offending for offenders not arrested.⁴⁷ The results of the Minneapolis experiment received extensive media attention and contributed to the widespread adoption of policies that encouraged the use of arrest for intimate partner violence.⁴⁸ The visibility and impact of the Minneapolis experiment led to a coordinated program of five new experiments designed as close but not exact replications of the Minneapolis experiment. The results of these five experiments were published independently and the extent of their support for specific deterrent effects of arrest varied by the source of data on re-offending, by measures of re-offending, and by jurisdiction.⁴⁹

⁴⁶ Durlauf & Nagin, *supra* note 37, at 85–86.

⁴⁷ Sherman & Berk, *supra* note 20, at 267–68.

⁴⁸ EVE S. BUZAWA & CARL G. BUZAWA, DOMESTIC VIOLENCE: THE CRIMINAL JUSTICE RESPONSE 94–99 (2003).

⁴⁹ ANTHONY PATE, EDWIN E. HAMILTON & SAMPSON ANNAN, METRO-DADE SPOUSE ABUSE REPLICATION PROJECT: DRAFT FINAL REPORT 621–66 (1991) (showing deterrent effects from both official records and victim interviews but only the effects from victim interviews were statistically significant); Richard A. Berk, Alec Campbell, Ruth Klap & Bruce Western, *A Bayesian Analysis of the Colorado Springs Spouse Abuse Experiment*, 83 J. CRIM. L. &

None of the reported findings from these five studies found deterrent effects as strong as those reported for the Minneapolis study; however, several efforts to systematically assess the evidence from these experiments tend to support the existence of some deterrent effects for arrest. First, a qualitative assessment summarized the findings from the original Minneapolis study and the five replication studies by arguing that the general direction of the findings from three studies favored deterrent effects and that the general direction of the other three studies favored escalation effects. This review also argued that arrest had different effects on different types of offenders.⁵⁰ A detailed summary of the published findings from the Minneapolis experiment, the five replication experiments, and from a companion experiment on arrest warrants⁵¹ identified a total of thirty-five common tests of specific deterrence; twenty-five (71%) of those tests were in the direction of a deterrent effect and ten (29%) were in the direction of an escalation effect. Eight of the twenty-five deterrent effects and none of the escalation effects were statistically significant.⁵²

A meta-analysis of the findings from the six arrest experiments limited only to data derived from victim interviews found an overall deterrent effect for arrest.⁵³ An analysis of individual-level archived data for all five replications involving 4,032 incidents with adult male suspects reported that,

CRIMINOLOGY 170, 184–98 (1992) (reporting deterrent effect from both official records and from victim interviews, neither of which were statistically significant); Franklyn W. Dunford, David Huizinga & Delbert S. Elliot, *The Role of Arrest in Domestic Assault: The Omaha Police Experiment*, 28 CRIMINOLOGY 183, 195–202 (1990) (showing deterrent effects from victim interview and escalation effects from official records, neither of which were statistically significant); J. David Hirschel, Ira W. Hutchinson, III & Charles W. Dean, *The Failure of Arrest to Deter Spouse Abuse*, 29 J. RSCH. CRIME & DELINQ. 7, 19–28 (1992) (also showing deterrent effects from victim interview and escalation effects from official records, neither of which were statistically significant); Lawrence W. Sherman, Janelle D. Schmidt, Dennis P. Rogan, Douglas A. Smith, Patrick R. Gartin, Ellen G. Cohn, J. Collins & Anthony R. Bacich, *The Variable Effects of Arrest on Criminal Careers: The Milwaukee Domestic Violence Experiment*, 83 J. CRIM. L. & CRIMINOLOGY 137, 150–56 (1992) (showing escalation effects for both official records and victim interviews, neither of which were statistically significant).

⁵⁰ LAWRENCE W. SHERMAN, JANELLE D. SCHMDIT & DENNIS P. ROGAN, *POLICING DOMESTIC VIOLENCE: EXPERIMENTS AND DILEMMAS* 16–18 (1992).

⁵¹ Franklyn W. Dunford, *System-Initiated Warrants for Suspects of Misdemeanor Domestic Assault: A Pilot Study*, 7 JUST. Q. 631, 641–50 (1990) (reporting consistent deterrent effects for arrest warrants).

⁵² Joel Garner, Jeffrey Fagan & Christopher Maxwell, *Published Findings from the Spouse Assault Replication Program: A Critical Review*, 11 J. QUANTITATIVE CRIMINOLOGY 3, 11–20 (1995).

⁵³ David B. Sugarman & Sue Boney-McCoy, *Research Synthesis in Family Violence: The Art of Reviewing the Research*, 4 J. AGGRESSION, MALTREATMENT & TRAUMA, 55, 66–69 (2000).

based on victim interviews, arrest was associated with statistically significant reductions of 25% in the prevalence of new victimizations. The frequency of new victimizations was reduced by 30%. Based on official records, arrest was associated with an 8% reduction in the frequency of new victimizations, but those effects were not statistically significant.⁵⁴

Three teams of scholars have used non-experimental data extracted from the National Crime Victimization Survey (NCVS) to further examine the specific deterrent effect of arrest. Based upon 2,565 incidents from 1992 to 2002, Richard Felson and his colleagues found that arrest was correlated, but not significantly so, with a reduction in subsequent violence.⁵⁵ Hyunkag Cho and Dina Wilke used a larger sample of 3,495 incidents the NCVS from 1987 to 2003 and found a statistically significant specific deterrent effect for arresting intimate partner violence offenders.⁵⁶ Min Xie and James Lynch reported a statistically significant deterrent effect for arrest among a sample of 1,336 victims of intimate partner violence in the NCVS during the period from 1996 through 2012; however, among a propensity score matched subsample of 688 victims, they report a nonsignificant effect in the direction of deterrence.⁵⁷

A recent non-experimental study based entirely on official police reports concerning 5,466 couples in Seattle, Washington found that arrest was associated with statistically significant reductions in both the prevalence and frequency of future incidents of physical abuse.⁵⁸ Lastly, in the one study of the effects of arrest on subsequent intimate partner violence that measured individual perceptions of sanction costs, a national-level panel study found statistically significant negative associations between individual perceptions of the costs of arrest in 1986 with the prevalence of subsequent wife assault in 1987.⁵⁹ While these nonexperimental studies generally rely on smaller samples and use less rigorous methods than the SARP experiments, the

⁵⁴ Christopher D. Maxwell, Joel H. Garner & Jeffrey A. Fagan, *The Preventive Effects of Arrest on Intimate Partner Violence: Research, Policy and Theory*, 2 *CRIMINOLOGY & PUB. POL'Y* 51, 64–66 (2002).

⁵⁵ Richard B. Felson, Jeffrey M. Ackerman & Catherine A. Gallagher, *Police Intervention and the Repeat of Domestic Assault*, 43 *CRIMINOLOGY* 563, 576–78 (2005).

⁵⁶ Hyunkag Cho & Dina J. Wilke, *Does Police Intervention in Intimate Partner Violence Work? Estimating the Impact of Batterer Arrest in Reducing Revictimization*, 11 *ADVANCES SOC. WORK* 283, 291–92 (2010).

⁵⁷ Min Xie & James P. Lynch, *The Effects of Arrest, Reporting to the Police, and Victim Services on Intimate Partner Violence*, 54 *J. RSCH. CRIME & DELINQ.* 338, 353–55 (2017).

⁵⁸ Vivian H. Lyons, Mary A. Kernic, Ali Rowhani-Rahbar, Victoria L. Holt & Marco Carone, *Use of Multiple Failure Models in Injury Epidemiology: A Case Study of Arrest and Intimate Partner Violence Recidivism in Seattle, WA*, 6 *INJ. EPIDEMIOLOGY* 1, 5–6 (2019).

⁵⁹ Williams & Hawkins, *supra* note 35, at 301–05.

reported effects consistently show at least small reductions in repeat offending following arrest. However, none of the experimental and nonexperimental studies about the deterrent effects of arrest were included in prior reviews of deterrence research.⁶⁰

The existing research on the effects of arrest for intimate partner violence is large, diverse, methodologically strong, and finds numerous but not universal conditions under which some specific deterrent effects can be identified. The mixed findings about the specific deterrent effects of arrest suggest that it is plausible, but uncertain, that intimate partner violence offenders may be responsive to specific deterrent effects produced by more severe criminal sanctions, such as prosecution, conviction, and incarceration.

D DETERRENT EFFECTS OF POST-ARREST SANCTIONS

The increased attention to and use of arrest as a preferred response to intimate partner violence in the 1980s generated heightened attention about whether those arrests were followed up with the filing of charges, convictions or incarceration of offenders.⁶¹ While some scholars have questioned whether more severe post-arrest sanctions would, on their own, have any deterrent effects on repeat intimate partner violence offending,⁶² there is widespread support for the use of these sanctions.⁶³ Further evidence of support for the use of prosecution in domestic violence incidents is found in the Violence Against Women Act of 1994,⁶⁴ under which the federal government continues to provide financial support and training for the development of intimate partner violence law enforcement, prosecution, and victim services programs by state and local agencies.⁶⁵

⁶⁰ See *supra* note 28.

⁶¹ See Joan Zorza, *Criminal Law of Misdemeanor Domestic Violence, 1970–1990*, 83 J. CRIM. L. & CRIMINOLOGY 46, 71–72 (1992).

⁶² Naomi R. Cahn & Lisa G. Lerman, *Prosecuting Woman Abuse*, in WOMEN BATTERING: POLICY RESPONSES 95, 98–99 (Michael Steinman ed., 1991); Linda G. Mills, *Mandatory Arrest and Prosecution Policies for Domestic Violence: A Critical Literature Review and the Case for More Research to Test Victim Empowerment Approaches*, 25 CRIM. JUST. & BEHAV. 306, 311–13 (1998).

⁶³ See, e.g., Donna Wills, *Domestic Violence: The Case for Aggressive Prosecution*, 7 UCLA WOMEN'S L.J. 173, 174–76 (1997); Angela Corsilles, *No-Drop Policies in the Prosecution of Domestic Violence Cases: Guarantee to Action or Dangerous Solution?*, 63 FORDHAM L. REV. 853, 881 (1994); Cheryl Hanna, *No Right to Choose: Mandated Victim Participation in Domestic Violence Prosecutions*, 109 HARV. L. REV. 1849, 1909 (1996).

⁶⁴ Violent Crime Control and Law Enforcement Act of 1994, Pub. L. No. 103–322, 108 Stat. 1796 (1994).

⁶⁵ See 34 U.S.C. §§ 12511, 10441–50 (2018); Memorandum, U.S. Dep't of Just., Off. on Violence Against Women, Formula Grant Programs (on file with J. Crim. L. & Criminology) (including funding requirements for law enforcement and prosecution programs).

Some researchers and advocates for the use of more severe post-arrest sanctions argued that prosecution and conviction in intimate partner violence cases are so infrequent that they were incapable of having any overall effect on offender behavior. Advocates for stronger sanctions have claimed that prosecution “is rarely used.”⁶⁶ Leading researchers have asserted that there was “widespread underprosecution of domestic violence cases,”⁶⁷ and that there were “extremely infrequent prosecutions and adjudication” of intimate partner violence.⁶⁸ A report from the National Research Council stated that “prosecution rates of battering cases typically have been low.”⁶⁹ If these assertions were accurate, it would be less important and more difficult to study whether post-arrest sanctions reduced repeat offending; however, a comprehensive review of 137 U.S. and Canadian studies reported that, while rates varied greatly between jurisdictions, on average: (1) one-third of reported offenses and three-fifths of recorded arrests for intimate partner violence resulted in the filing of criminal charges; and (2) more than half of all prosecutions for intimate partner violence resulted in a criminal conviction.⁷⁰ While there are no comparable statistics on prosecution or conviction rates for other offenses, the use of post-arrest sanctions is sufficiently large and widespread to justify critical examination of the potential effects of those sanctions.

Some scholars criticized the use of post-arrest criminal sanctions for intimate partner violence for other reasons. Jeffrey Fagan objected to using criminal sanctions as the primary mechanism to address intimate partner violence, citing the complexity of domestic violence incidents, variety in batterer types, and the vagaries of the criminal justice system.⁷¹ Both Donald Dutton and Linda Mills view mandated judicial and prosecutorial interventions as unduly diminishing the victims’ role in determining their own life course.⁷² In addition, David Ford reported that some victims prefer to separate themselves from the criminal justice system after the police have

⁶⁶ LENORE E. WALKER, *THE BATTERED WOMAN* 212 (1979).

⁶⁷ SHERMAN, SCHMIDT & ROGAN, *supra* note 52, at 244.

⁶⁸ Alissa P. Worden, *The Changing Boundaries of the Criminal Justice System: Redefining the Problem and the Response in Domestic Violence*, in *CRIMINAL JUSTICE 2000*, at 215, 221 (2000).

⁶⁹ NAT’L RSCH. COUNCIL, *UNDERSTANDING VIOLENCE AGAINST WOMEN* 118 (Nancy A. Crowell & Ann W. Burgess eds., 1996).

⁷⁰ Joel H. Garner & Christopher D. Maxwell, *Prosecution and Conviction Rates for Intimate Partner Violence*, 34 *CRIM. JUST. REV.*, 44, 53–54 (2009).

⁷¹ JEFFREY FAGAN, *THE CRIMINALIZATION OF DOMESTIC VIOLENCE: PROMISES AND LIMITS* 28–40 (1996).

⁷² DONALD G. DUTTON, *RETHINKING DOMESTIC VIOLENCE* 271 (2006); LINDA G. MILLS, *INSULT TO INJURY: RETHINKING OUR RESPONSES TO INTIMATE ABUSE* 11 (2003).

helped them resolve a particular violent incident.⁷³ These criticisms recognize possible impediments connected with mobilizing the criminal law, but they do not directly question the existence of specific deterrent effects when sanctions are applied.

The evidence supporting specific deterrent effects for arrest and the widespread official support for post-arrest sanctions enhances the importance of understanding the extent to which post-arrest criminal sanctions are associated with changes in future incidents of intimate partner violence. The only published review of the research on the deterrent effects of post-arrest sanctions on intimate partner identified thirty-one reports published between 1984 and 2005 that reported 143 statistical comparisons of offending rates following prosecution, conviction, or incarceration for intimate partner violence.⁷⁴ Those statistical comparisons were analyzed by counting whether the reported findings were in the direction predicted by deterrence theory and whether those findings reached the commonly accepted level of statistical significance.⁷⁵ Using this approach, this study found 24% of the reported findings showed more severe sanctions associated with less repeat offending, providing support for the deterrence hypothesis. Only 10% of the tests showed that more severe sanctions were associated with more repeat offending, supporting the escalation hypothesis. 66% of 143 statistical comparisons of prosecution, conviction, and incarceration sanctions generated no differences in repeat offending by the type of sanction received. These proportions varied by sanction type. 39% of the prosecution tests were associated with less repeat offending and only 6% were associated with more repeat offending. About equal proportions of the statistical tests for conviction and incarceration were and were not supportive of deterrent effects. Thus, the one prior effort to assess the existing research found that there appeared to be limited support for the existence of deterrence effects associated with post-arrest sanctions and that the limited support for deterrent effects varied by the type of post-arrest sanction.

The one existing review of the research on post-arrest sanctions has several limitations. First, it was not based on a systematic search of the existing literature and it did not explicitly define differences between the three sanction types. Second, it treated all the studies the same regardless of

⁷³ David A. Ford, *Prosecution as a Victim Power Resource: A Note on Empowering Women in Violent Conjugal Relationships*, 25 *LAW & SOC'Y REV.* 313, 315–17 (1991).

⁷⁴ Christopher D. Maxwell & Joel H. Garner, *Crime Control Effects of Criminal Sanctions for Intimate Partner Violence*, 3 *PARTNER ABUSE* 469, 484–85 (2012).

⁷⁵ These tests compared re-offending rates between offenders who were prosecuted with those who were not prosecuted; between those who were convicted with those who were not convicted; and those who were incarcerated with those that were not incarcerated.

their methodological rigor or the number of cases involved. Third, it did not consider the lack of independence among the tests, since as many as fifteen tests were generated from the same sample of cases. Lastly, the analysis of these 143 statistical tests is a simple count of the direction and statistical significance of reported findings without consideration of the relative size of the effects reported; thus, a test producing just marginally statistically significant results counts the same as a test whose effect is much larger. This approach is considered methodologically weak and unlikely to produce a reliable assessment of empirical research findings.⁷⁶

II. DESIGN OF THIS RESEARCH

This research applies more rigorous contemporary meta-analytic methods to assess the existing body of research on the specific deterrent effects of post-arrest criminal sanctions imposed on individuals accused of intimate partner violence. Our substantive focus is on the conditions under which specific deterrent effects do and do not exist. Our methodological focus is on the appropriate quantitative methods to test for the direction, size and statistical significance of these effects in the available research literature. Our statistical analyses are designed to address two questions. First, to what extent are post-arrest criminal sanctions—prosecution, conviction, or jail—associated with lower rates of subsequent intimate partner violence? Second, do characteristics of the study designs used in this body of research moderate the association of sanctions with subsequent behavior? Our substantive conclusions on deterrence effects stem primarily from the answers to the first question; the answers to the second question are intended primarily to inform the designs of future research on the use of sanctions for intimate partner violence.

A. META-ANALYTIC METHODS

Since its development as an independent specialty in the statistical sciences in the 1980s, meta-analysis has developed into a variety of formal processes for explicitly incorporating multiple characteristics of individual studies within a common research framework and is now used widely within criminology to summarize existing research examining a public policy or a scientific hypothesis;⁷⁷ meta-analytic methods produce standardized effect sizes for reported results and summarize those results within and between the

⁷⁶ FRANK L. SCHMIDT & JOHN E. HUNTER, *METHODS OF META-ANALYSIS: CORRECTING ERROR AND BIAS IN RESEARCH FINDINGS* 453–57 (3d ed. 2015).

⁷⁷ See Edward Wells, *Uses of Meta-Analysis in Criminal Justice Research: A Quantitative Review*, 26 *JUST. Q.* 268, 275–87 (2009).

available studies. In criminology (and other applied fields), meta-analyses typically produce two types of findings: first, an assessment of the empirical support for a particular theory, policy, or program; and second, an assessment of the quality of the existing research literature. Meta-analytic methods involve a number of explicit choices about how to define and identify the relevant research literature, how to compute standardized effect sizes, how to select among multiple analyses reported within individual studies, how to weigh and combine the contribution of individual studies, and how to assess the likelihood that unidentified studies may influence the findings of the meta-analysis. These explicit methods are contrasted against the potential for more subjective and implicit judgments about these same issues in qualitative literature reviews and in quantitative reviews that do not clearly and formally articulate how they identify and analyze the available research. Despite these differences, both qualitative and quantitative methods of research synthesis are constrained by the nature, quality, and size of the available research on a particular topic.

B. SCOPE OF THIS RESEARCH

To be considered for inclusion in this meta-analysis, a study needed to report about individuals who were alleged to have committed an intimate partner violence offense, or who were charged with or convicted of a violent offense against an intimate partner. The second criterion for inclusion is that the study reported a quantitative estimate at the individual level about the relationship between actual post-arrest dispositions of criminal charges for intimate partner violence and rates of subsequent offending by the suspects in those cases. This meta-analysis excludes analyses that used summary rates of case disposition and repeat offending aggregated at the court or jurisdiction level.

These criteria are derived from our interest in testing hypotheses about specific deterrence. Our search for prior research had no restriction based on the sample size or the type of statistical analyses reported. This meta-analysis includes studies regardless of when their data were collected, when their results were published, or the format of the written report. The review considered English-language refereed journal articles, book chapters, books, working papers, case reports, dissertations, and government reports.

While the design of this research is structured to assess the specific deterrent effects of prosecution, conviction and incarceration, most of the studies in this body of research conceptualized and articulated their efforts not as a test of deterrence theory, but as atheoretical comparisons of the effectiveness of one or more disposition types versus other disposition types. For instance, Wooldredge and Thistlethwaite reported comparisons between

defendants whose charges were dismissed versus those who were acquitted at trial; they also compared defendants who were convicted and fined to those who were convicted and sentenced to probation.⁷⁸ While these differences might be important for other criminological purposes, within our conceptual approach comparisons between dismissals and acquittals and between fines and probation do not produce a clear contrast between a less severe sanction and a more severe sanction and, therefore, were not considered as tests of specific deterrence theory. In addition to excluding aggregate-level analyses, the review is limited to comparisons of sanctions actually imposed on an individual and does not include tests that compared two or more sanction *policies* unless the report also included details about the nature and severity of the sanctions actually imposed in each case.

Because the primary objective of this research is to test deterrence theory, the meta-analysis established some theory-specific rules to code the nature of post-arrest criminal sanctions. The taken approach placed all criminal dispositions reported in the research into one of four categories of increasing severity: not prosecuted, prosecuted, convicted, and incarcerated. This conceptualization distinguishes between changes in legal status based on concrete actions by prosecutors, juries, or judges: filing of criminal charges, conviction in court, and a sentence involving incarceration. In this understanding of criminal sanctions, the more severe sanctions subsume the less severe ones—imprisoned defendants had been convicted and convicted defendants had been charged. Each reported comparison between a more severe sanction and a less severe sanction is a test of deterrence theory. Prosecuted cases compared to cases that were not prosecuted constitute a test of the specific deterrent effects of prosecution. Convicted cases compared to cases not convicted constitute a test of the specific deterrent effects of conviction and incarcerated cases compared to cases not incarcerated constitute a test of the specific deterrent effects of incarceration.

This theoretical approach generates three sanction-specific null hypotheses:

1. There is no difference in the rate of repeat offending for prosecuted and not prosecuted intimate partner violence offenders.
2. There is no difference in the rate of repeat offending for convicted and not convicted intimate partner violence offenders.
3. There is no difference in the rate of repeat offending for incarcerated and not incarcerated intimate partner violence offenders.

⁷⁸ John Wooldredge & Amy Thistlethwaite, *Court Dispositions and Rearrest for Intimate Assault*, 51 *CRIME & DELINQ.* 75, 86 (2005).

1. Studies Identified

Using the selection criteria stated above and following established approaches for searching for prior research documents,⁷⁹ this research completed a multiple keyword search of numerous online science publication indexes, reviewed the abstracts of the identified studies to identify potentially eligible ones, and conducted detailed reviews of publications that might include eligible studies. Our search strategy used the following indexes: the National Criminal Justice Reference Service's (NCJRS) abstracts database, the National Archive of Criminal Justice Data's (NACJD) Online Bibliography of Data-related Literature, ProQuest, Academic OneFile, Elsevier Scopus, PubMed.gov, and the Web of Science (SSCI). Our searches relied on three sets of relevant terms that best represent our key, intertwined topics of intimate partner violence (fifteen terms), criminal court disposition (fifteen terms), and recidivism (eleven terms).⁸⁰ However, pilot searches revealed that searching a term independently produced a large number of hits that were irrelevant to our focus. For instance, searching the phrase "intimate partner violence" on its own generated 24,791 hits, and the term "recidivism" resulted in 42,459 hits, each in a single electronic resource. Thus, we used composite search commands with terms such as "and," "or," and "not." Additionally, our searches were also filtered by the following disciplines: criminology, criminal justice, victimology, corrections, law and society, criminal rehabilitation, social problems, women's studies, family welfare, sociology, and social science.

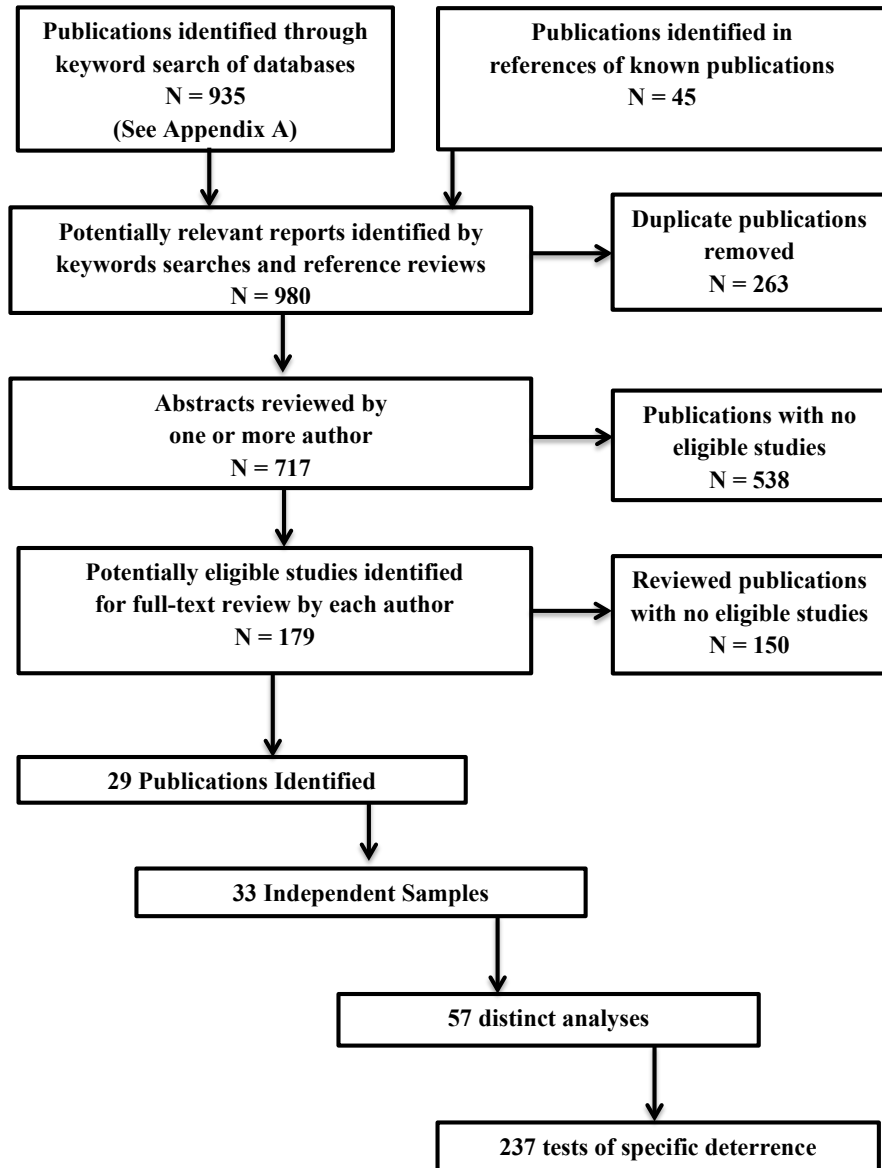
In addition to using these search terms, this review manually examined bibliographies of relevant articles, books, and reviews for additional studies. Once potential studies were identified, citations to those studies were searched for additional studies using Google scholar.⁸¹

⁷⁹ See generally David Moher, Alessandro Liberati, Jennifer Tetzlaff, Douglas G. Altman & The PRISMA Group, *Preferred Reporting Items for Systematic Reviews and Meta-Analyses: The PRISMA Statement*, 6 PLoS MED. 1 (2009) (describing guidelines for systematic reviews and meta-analyses).

⁸⁰ See *infra* Appendix 1 for lists of the search terms used for the systematic literature search.

⁸¹ See Jeffrey G. Reed & Pam M. Baxter, *Using Reference Databases*, in THE HANDBOOK OF RESEARCH SYNTHESIS AND META-ANALYSIS 73, 89–90 (Harris Cooper, Larry V. Hedges & Jeffrey C. Valentine eds., 2d ed. 2009) (identifying alternative ways to conduct citation searches and the assumptions underlying their use).

Figure 1: Identification of Research on Repeat Offending Following Post-Arrest Sanctions for Intimate Partner Violence



The results of searching the existing databases to identify reports about repeat offending following any post-arrest sanction for intimate partner violence are displayed in Figure 1. Our initial keyword search identified 935 indexed publications and an additional 45 publications through a review of bibliographic references and citations. After removing 263 duplicate publications, 717 publication abstracts were reviewed by one or more authors. The review of abstracts resulted in rejecting an additional 538 documents that did not include the type of research that met our inclusion criteria. The full-text review of the 179 potentially eligible studies required multiple reviews by all three co-authors to determine the exact nature of the reported findings, the sample sizes involved, and the direction of the reported effects. Moreover, the full-text review identified ten instances where the reported findings did not include all the information needed to produce a standardized effect—mostly standard errors or sample sizes. While meta-analyses would traditionally exclude these tests from their review, this approach was to contact the authors of those reports by email requesting the missing information about standard errors and sample sizes. Seven of the ten contacted authors provided the information needed to compute standardized effect sizes and these additional tests were included in this meta-analysis.

The selection criteria in this meta-analysis identified 237 tests of the specific deterrent effects of post-arrest sanctions for intimate partner violence in 29 documents (See Table 1). Published between 1989 and 2013, these documents used samples that ranged in size from seventy-four to 66,759 case dispositions. This literature includes analyses of more than 127,000 criminal incidents from thirty-three U.S. or Canadian jurisdictions. The full-text review of these publications also revealed that some publications included more than one independent sample of cases. For instance, in 2003, Peterson reported findings from a large sample of cases from all of New York City with three tests of conviction and three tests of incarceration.⁸² In the same publication, Peterson also reported findings limited to Bronx County with two tests of prosecution, two tests of conviction and two tests of incarceration.⁸³ In 2004, Peterson reported new findings from two samples—one from the last three months of 1998 and another from the first three months of 2001—in New York County (Manhattan). For each sample,

⁸² RICHARD R. PETERSON, N.Y.C. CRIM. JUST. AGENCY, *THE IMPACT OF CASE PROCESSING ON RE-ARRESTS AMONG DOMESTIC VIOLENCE OFFENDERS IN NEW YORK CITY* 43–49 (2003) (displaying the results of three bivariate tests and three multivariate tests in the New York City sample).

⁸³ *Id.* at 26–28; 30–34 (also displaying the results of three bi-variate and three multivariate tests for the Bronx sample but in a different configuration).

Peterson reported one test of the conviction and one test of incarceration.⁸⁴ Thus, in this example, our search process identified two documents, with four samples, five studies and twelve tests of specific deterrence. As displayed in Table 1, the complete results of our search process generated ten studies of the prosecution hypothesis, twenty-six studies of the conviction hypothesis, and twenty-one studies of the incarceration hypothesis.

Table 1. Publications, Sites, Sample Sizes and Sanctions Tested

Publication	Site	Sample Size	Sanctions Tested			
			Prosecution	Conviction	Incarceration	Any
Bell et al. (2013)	Unspecified Mid-Atlantic City	104		1	1	2
Buzawa et al. (1999)	Quincy, Massachusetts	353	10	4	4	18
Cattaneo & Goodman (2010)	Washington D.C.	142		6		6
Davis et al. (1998)	Milwaukee, Wisconsin	1,133	1	1	1	3
Fagan (1989)	Five Unspecified Jurisdictions	270	1	1		2
Finn (2013)	Two Counties in Georgia	170		2	2	4
Frantzen et al. (2011)	Bexar Co., Texas	415		1		1
Frisch et al. (2001)	Four Sites in New York State	3,816		1		1
George (2012)	Washington State	66,759			16	16
Gross et al. (2000)	Chesterfield Co., Virginia	177		8	4	12
Harrell et al. (2007: Mass)	Two Counties in Massachusetts	593		13		13
Harrell et al. (2007: Mich)	Two Counties in Michigan	441		13		13
Jaffe et al. (1993)	London, Ontario	90	8			8
Kingsnorth (2006)	Sacramento, California	872		1	1	2
Klein & Tobin (2008)	Quincy, Massachusetts	342		4	7	11
Macvaugh (2004)	Central Massachusetts	1,150		1	3	4
Marsland, et al. (2001)	Abbotsford, British Columbia	74		1	1	2
McFarlane, et al. (2000)	Houston, Texas	90	18			18
Murphy et al. (1998)	Baltimore, Maryland	235		2		2
Orchowsky (1999)	Alexandria, Virginia	1,910		1	2	3
Peterson (2003: NYCO)	New York Co., New York	6,849		3	3	6
Peterson (2003: Bronx)	Bronx Co., New York	1,435	2	2	2	6
Peterson (2004: 1998)	New York Co., NY, 1998	990		1	1	2
Peterson (2004: 2001)	New York Co., NY, 2001	1,249		1	1	2
Pinchevsky (2013: Rich)	Richland Co., South Carolina	1,068			3	3
Pinchevsky (2013: Lex)	Lexington Co., South Carolina	716			6	6
Quann (2006)	Ontario, Canada	1,000			4	4
Sloan et al. (2013)	North Carolina	29,700	2	2	2	6
Steinman (1988)	Lancaster Co., Nebraska	183	3	3	1	7
Tolman & Weisz (1995)	DuPage Co., Illinois	341	2	2		4
Ventura & Davis (2005)	Toledo, Ohio	519		1		1
Wooldredge (2007)	24 Counties in Ohio	353		1	3	4
Wooldredge & Thistlethwaite	Hamilton Co., Ohio	3,662	14	15	16	45
Number of Tests			61	92	84	237
Number of Studies			10	26	21	57

⁸⁴ RICHARD R. PETERSON, N.Y.C. CRIM. JUST. AGENCY, THE IMPACT OF MANHATTAN'S SPECIALIZED DOMESTIC VIOLENCE COURT 57–61 (2004) (reporting one multivariate test of conviction and one multivariate test of incarceration in each sample).

2. Multiple Tests within Studies

The reporting of multiple tests within a single study is typically encouraged within criminology and other scientific disciplines as a way for an individual study to demonstrate either the strength or generalizability of particular findings. However, multiple tests create problems for all approaches to research synthesis because it is not clear which tests, or combination of tests, to consider and how to weight the importance of one test over another. The particular nature of that problem for this meta-analysis is revealed in Table 1, which lists for each publication the number of individual tests of specific deterrence with each of our three sanction types—prosecution, conviction, and incarceration. Twelve of the thirty-three studies in Table 1 report tests for only one sanction type. However, fifteen report tests for two sanction types and six report tests for all three sanction types. Because some studies sometimes use some of the same incidents to test multiple sanction types, our tests across these three sanction types are not completely independent of each other. For this reason, we emphasize the separate results from each sanction type.

There is a second, and more important, concern about the independence of the 237 statistical tests reported in Table 1. Multiple tests of a particular hypothesis can occur in a number of different ways, such as when authors use data on repeat offending from victim interviews and official records, or when they report repeat offending for both three months after the sanction and separately for six months after the sanction. For instance, Peterson reports three tests of the conviction hypothesis in his New York City sample. One of those tests is produced by a bivariate analysis of any subsequent offenses against anyone. The second test is a bivariate analysis of any subsequent *violent* offense against any *domestic* partner and the third test is a multivariate analysis of the same offense and victim type as the second test. All three of these tests use the same set of individual case dispositions, and using all three tests would violate traditional statistical assumptions that each test is an independent estimate of the relationship under investigation.⁸⁵ When dependences exist between one or more tests, there is a greater likelihood of producing biased and imprecise estimates or falsely rejecting a true null hypothesis.⁸⁶ There are additional statistical concerns in this

⁸⁵ See NOEL A. CARD, *APPLIED META-ANALYSIS FOR SOCIAL SCIENCE RESEARCH* 191–94 (2012).

⁸⁶ See Mariola Moeyaert, Maaïke Ugille, S. Natasha Beretvas, John Ferron, Rommel Bunuan & Wim Van den Noortgate, *Methods for Dealing with Multiple Outcomes in Meta-Analysis: A Comparison Between Averaging Effect Sizes, Robust Variance Estimation and Multilevel Meta-Analysis*, 20 *INT'L J. SOC. RSCH. METHODOLOGY*, 559, 567–69 (2017); Betsy

research because some studies reported many tests and other studies reported only a few tests, or just a single one.

C. USING THE “BEST” TEST METHOD

The design of this research addresses the lack of independence among multiple tests of the same hypothesis within a single study by selecting the one “best” test based on the methodological rigor of the available tests within each study.⁸⁷ For instance, in Peterson’s test of the conviction hypothesis referenced earlier, the methodologically strongest test is the multivariate analysis using only violence offenses against an intimate partner. In addition to the merits of estimating the size of the deterrent effect based on standards of methodological rigor, this approach also benefits from directly estimating standard errors based on a single test. In selecting the “best” test, we assume that there are sufficiently clear methodological preferences for some types of analyses over others, that these preferences can be applied objectively to the existing research, and that these rules will result in the identification of the single “best” test within each study.

Based on the nature of the research found and the hypotheses tested in this research, the “best” test was identified using seven criteria for selecting from among duplicate tests within each of the fifty-seven studies listed in Table 1. These criteria are the type of analyses, the size of the treatment group, the length of the follow-up period, the source of data, the offense type, the victim type, and the criterion (e.g., re-offense, re-arrest, re-conviction) used to denote recidivism.

For analysis type, we selected multivariate over bivariate results, and then among multiple multivariate analyses, selected count regression models over survival analyses and survival analyses over logistic regression. Among bivariate models, counts and means were selected over correlations and correlations over simple prevalence measures. If there were still multiple tests after selecting from among these analysis types, we selected tests with the largest treatment group or the longest follow-up period as the preferred test. If these criteria were not sufficient to identify a “best” test, the use of victim interviews were preferred over official records, outcome measures based on new offenses were preferred over measures based on new arrests and use of new arrests was preferred over new convictions. If these criteria

J. Becker, *Multivariate Meta-Analysis*, in HANDBOOK OF APPLIED MULTIVARIATE STATISTICS AND MATHEMATICAL MODELING 499, 502–03 (Howard E. A. Tinsley & Steven D. Brown eds., 2000); MICHAEL BORENSTEIN, LARRY V. HEDGES, JULIAN P. T. HIGGINS & HANNAH R. ROTHSTEIN, *Multiple Outcomes or Time-Points within a Study*, in INTRODUCTION TO META-ANALYSIS 225, 226–27 (2009).

⁸⁷ MARK W. LIPSEY & DAVID B. WILSON, PRACTICAL META-ANALYSIS 113 (2001).

were not sufficient to identify a unique test for a particular hypothesis, outcome measures of violent offenses were preferred over non-violent offenses, and measures of offenses against the same victim were preferred over those against any victim.

While the statistical analysis, sample size, follow-up period, and the use of offense measures are more generic criteria for methodological rigor, the preference for tests involving violent offenses and the same or similar victims stem from our focus on deterrence of re-offending, not rearrest, and on intimate partner violence rather than violence in general. Arrest records are relatively easy to obtain, but often reflect police behavior as much as an offender's behavior towards their partner. Criminologists have long argued that arrest, conviction, and incarceration data are more appropriately viewed as measures of official response to criminal behavior rather than of offense rates or offenders themselves.⁸⁸ Moreover, the use of increased criminal sanctions for intimate partner violence is primarily designed to reduce future violence against intimate partners, not all types of offenses against all types of victims. In addition, a large proportion of intimate partner violence stems from multiple incidents with the same victim, which increases the preference for using measures of offense frequency over prevalence or time to failure measures in this research.⁸⁹

In twenty-two studies, only one test was reported; in those instances, we selected that test as the "best." Applying our criteria to the existing research on the specific deterrent effects of criminal sanctions, twelve of the "best" tests we selected were based on the use of multivariate analyses, the use of frequency measures, or the use of longer follow-up periods. In eleven tests, the sample size/follow-up period was the determining factor in selecting the "best" test. In five studies, we chose tests because they used re-offense as the criteria for recidivism. Six tests were picked because they defined repeat offending for violent offenses only, and we selected one test because it measured repeat offending of the same victim. The potential impact of using these selection criteria can be seen in Table 2, which reports the frequency of all 237 reported tests and the "best" tests on the seven selection criteria. Among all reported tests, only 55% were multivariate analyses; among the fifty-seven "best" tests, 72% of the tests were multivariate. In addition, forty-

⁸⁸ See John I. Kitsuse & Aaron V. Cicourel, *A Note on the Uses of Official Statistics*, 11 *SOC. PROBS.* 131, 132–34 (1963); DELBERT S. ELLIOTT, *CTR. FOR THE STUDY & PREVENTION OF VIOLENCE, LIES, DAMN LIES, AND ARREST STATISTICS* 3–5 (1995); Terence P. Thornberry & Marvin D. Krohn, *Comparison of Self-Report and Official Data for Measuring Crime*, in *MEASUREMENT PROBLEMS IN CRIMINAL JUSTICE RESEARCH* 43, 43–48 (John V. Pepper & Carol V. Petrie eds., 2003).

⁸⁹ Garner, Fagan & Maxwell, *supra* note 52, at 16–18.

four (77.2%) of the “best” tests count only violent repeat offending compared to 159 (67.1%) of all tests. Despite these improvements, the “best” tests use victim interviews only 22.8% of the time; for all tests they were used in 27.8%. Thus, this approach used tests that have some preferred methodological characteristics, but it can only select the “best” from among the available studies. Perhaps most importantly, this approach provides a strong basis for assuming independence between research observations within each of the three sanction hypotheses. The use of the “best” test approach was determined prior to the search for studies and the calculation of standard effect sizes.

To be consistent with prior deterrence research, the original design of this study specified testing three distinct hypotheses about the deterrent effects of sanctions and called for the use of a two-sided t test with the traditional criterion of $p < .05$ for the determination of statistical significance. The problems inherent in relying solely on this arbitrary standard recently led the American Statistical Association (ASA) to recommend that “[r]esearchers should bring many contextual factors into play to derive scientific inferences.”⁹⁰ Our use of a predetermined p value of 0.05 is consistent with the approach recommended by John Ioannidis.⁹¹ However, we provide some qualitative context for our reported findings and report the results of several sensitivity tests about our quantitative analyses, such as simple counts of statistical tests and revised meta-analytic findings when tests with the largest and smallest effect sizes are removed.

⁹⁰ Ronald L. Wasserstein & Nicole A. Lazar, *The ASA Statement on p-Values: Context, Process, and Purpose*, 70 *AM. STATISTICIAN* 129, 131 (2016).

⁹¹ John P.A. Ioannidis, *The Importance of Predefined Rules and Prespecified Statistical Analyses: Do Not Abandon Significance*, 321 *JAMA* 2067, 2067–68 (2019).

Table 2. Design Characteristics for 237 Effects and for 57 Best Effects

Design Characteristics	All Effects		Best Effect	
	N	%	N	%
Statistical Analyses				
Multivariate	130	54.9%	41	71.9%
Prevalence	89	37.6%	31	54.4%
Frequency	26	11.0%	9	15.8%
Time to Fail	15	6.3%	1	1.8%
Bivariate Analyses	107	45.1%	16	28.1%
Prevalence	75	31.6%	13	22.8%
Frequency	30	12.7%	3	5.3%
Time to Fail	2	0.8%	0	0.0%
Length of Follow-up				
6 months	33	13.9%	9	15.8%
1 Year	61	25.7%	9	15.8%
18 Months	38	16.0%	18	31.6%
2 Years	51	21.5%	12	21.1%
3 Years	20	8.4%	2	3.5%
5 Years or Longer	34	14.3%	7	12.3%
	237			
Data Source				
Victim Interviews	66	27.8%	13	22.8%
Official Records	171	72.2%	44	77.2%
Recidivism Measure				
New Offense	70	29.5%	15	26.3%
New Arrest	126	53.2%	34	59.6%
New Charge	28	11.8%	7	12.3%
New Conviction	13	5.5%	1	1.8%
Recidivism Offense				
Violent	159	67.1%	44	77.2%
Other	17	7.2%	0	0.0%
Any	61	25.7%	13	22.8%
Type of Victim				
Same	83	35.0%	21	36.8%
Intimate	34	14.3%	6	10.5%
Domestic	66	27.8%	19	33.3%
Any	54	22.8%	11	19.3%

D. ALTERNATIVE APPROACHES TO ADDRESSING INDEPENDENCE

A number of prominent researchers have recommended the use of alternative approaches for addressing the issue of multiple tests in prior research. The first alternative approach uses the average of all tests within each study to estimate both the size and the variance of each effect, even

though the use of the average of multiple variances has limited justification.⁹² The second alternative approach also uses the average effect size across all reported tests but uses statistical procedures to better estimate “robust” standard errors. “Robust” standard errors are often, but not always, larger than the actual standard errors.⁹³ A third alternative approach is to use hierarchical linear modeling (HLM), which simultaneously estimates effects sizes and standard deviations within and between studies.⁹⁴ All four of these approaches employ the traditional inverse-variance weighting method that gives greater weight to effects from larger studies whose smaller standard errors produce more precise effect size estimates.⁹⁵ These alternative approaches rely on different underlying assumptions about the nature of the existing research, the ability to rank order research methods, the amount and importance of statistical independence of reported tests, and the relative importance of different objectives of a meta-analysis. Despite the many similarities in the methods used by these four approaches, we think that the substantive arguments for using the “best” test approach are compelling and our conclusions rely heavily on the “best” test approach.

All of the approaches used to estimate deterrence effects use the same method to convert the results from different types of statistical tests published in the original reports into a common metric. Consistent with the recommendations made by several analysts, the logged odds ratio is used for this purpose.⁹⁶ To create logged odds ratios for all 237 tests, the bivariate proportions, mean differences, correlations, and regression coefficients, along with their associated measures of variance and relevant sample sizes, were entered into Comprehensive Meta-Analysis (version #3) (CMA) to produce a standardized odds ratio with associated standard errors for each of the 237 tests. The resulting standardized effects sizes and variance estimates

⁹² Fulgencio Marín-Martínez & Julio Sánchez-Meca, *Averaging Dependent Effect Sizes in Meta-Analysis: A Cautionary Note About Procedures*, 2 SPANISH J. PSYCH. 32, 37 (1999).

⁹³ Larry V. Hedges, Elizabeth Tipton & Matthew C. Johnson, *Robust Variance Estimation in Meta-Regression with Dependent Effect Size Estimates*, 1 RSCH. SYNTHESIS METHODS 39, 45 (2010).

⁹⁴ See generally STEPHEN W. RAUDENBUSH & ANTHONY S. BRYK, *HIERARCHICAL LINEAR MODELS: APPLICATIONS AND DATA ANALYSIS METHODS* (2d ed. 2002) (providing examples and explanations of the theory and use of hierarchical linear models).

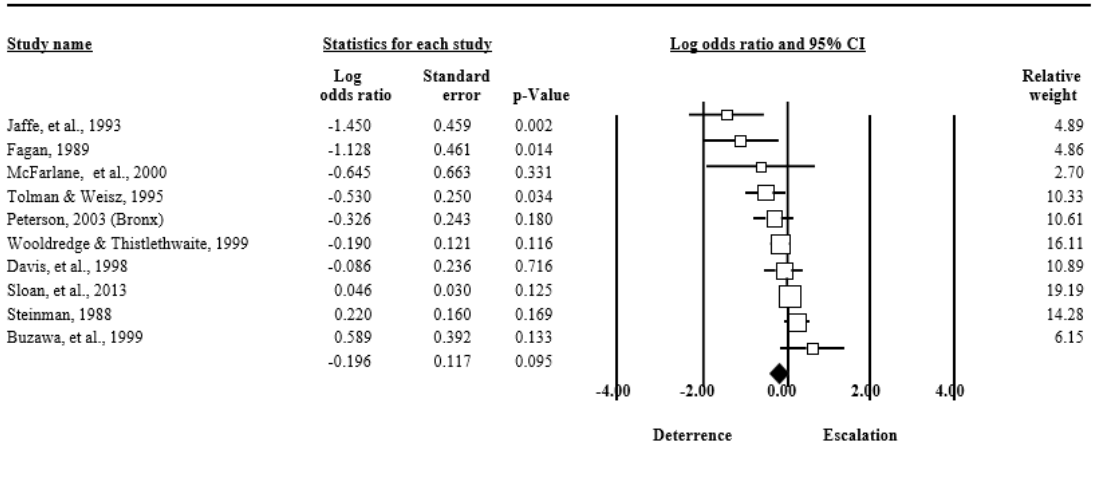
⁹⁵ Moeyaert, Ugille, Beretvas, Ferron, Bunuan & Van den Noortgate, *supra* note 86, at 561–62; Larry Hedges & Kimberly Maier, *Meta-Analysis*, in *THE SAGE HANDBOOK OF MULTILEVEL MODELING* 487, 497 (Mark A. Scott, Jeffrey S. Simonoff & Brian D. Marx eds., 2013).

⁹⁶ Joseph L. Fleiss & Jesse A. Berlin, *Effect sizes for dichotomous data*, in *THE HANDBOOK OF RESEARCH SYNTHESIS AND META-ANALYSIS*, 237 (Harris Cooper, Larry V. Hedges & Jeffrey C. Valentine, eds., 2d ed. 2009); TERRI D. PIGOTT, *ADVANCES IN META-ANALYSIS* 11–12 (2012); LIPSEY & WILSON, *supra* note 87, at 53.

produced by CMA are used in all four of the approaches we use to estimate specific deterrent effects. When using logged odds ratios, coefficients equal to zero represent no impact of the criminal sanction. In our comparison of more severe versus less severe sanctions, negative coefficients indicate a deterrent effect while positive coefficients indicate an escalation or criminogenic effect. Since the underlying population effect is likely to vary because of structural differences across studies, all four approaches use a mixed-effects model.

III. FINDINGS

Figure 2. Prosecution Effects



Note: Random effects meta-analysis model using best effects

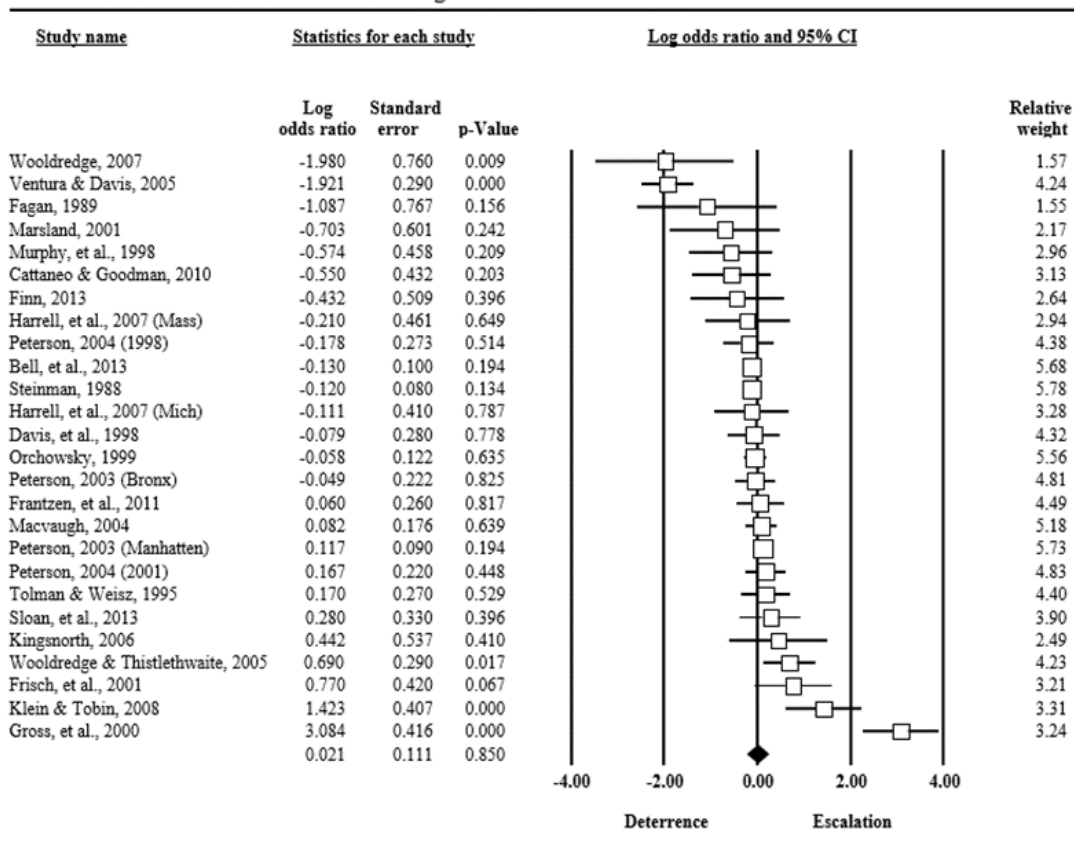
A. BEST EFFECTS

Figure 2 presents a forest plot of the results of each study’s “best” test sorted from the smallest estimated log odds ratio—the one most supportive of deterrence—to the highest estimated log odds ratio—the one least supportive of deterrence of the prosecution hypothesis. We also present the confidence interval for that estimate, the value of the two-sided *z* test, the *p* value for that test, and the relative weight of each study resulting from the use of inverse-variance weighting method. The relative weight among these studies varied from about three to almost twenty percent. Seven of the ten studies of the prosecution hypothesis produce an effect in the direction of a deterrent effect, and three of these effects exceed the 1.96 standard threshold for a statistically significant *t* test. These three studies combined relative weight is about twenty percent.

None of the three studies producing a log odds ratio in the direction of an escalation effect produced a statistically significant effect. However, the combined weight of the studies reporting escalation effects exceeds forty percent. A traditional literature review or vote-counting approach to synthesizing findings would likely find the seven-to-three ratio of results supportive of an overall deterrent effect; however, the relatively small number of studies and the substantial weight given to several studies not supportive of deterrent effects produce a meta-analytic finding in the direction of deterrence (-0.196) but with a nonsignificant p value of 0.095. We tested the sensitivity of this finding by removing the prosecution study most supportive of deterrence; this nine-study analysis produces a mean odds ratio to -.115, which is also not statistically significant. However, the prosecution results are sensitive to removing the study least supportive of deterrence; the analysis of the remaining nine studies produces a mean odds ratio of -0.246 with a statistically significant p value of .041.

In the same format as Figure 2, Figure 3 lists the twenty-six studies testing the specific deterrent effects of conviction. While fifteen (58%) of these studies produce effects predicted by deterrence theory, just two of those reported findings are statistically significant. Of the eleven studies showing an effect for the conviction sanction in the direction of escalation, three produce a statistically significant effect. The relative weights of these twenty-six studies range from 1.55 to 5.78, suggesting less dominance by any one study or group of studies than was present in testing the prosecution hypothesis. The meta-analytic summary of these twenty-six studies produces an effect size 0.021, which is not only small but is in the opposite direction from what one would expect to find if convictions produced a deterrent effect. Removing the study most supportive or least supportive of deterrence does not change the direction or statistical significance of the conviction effect.

Figure 3. Conviction Effects

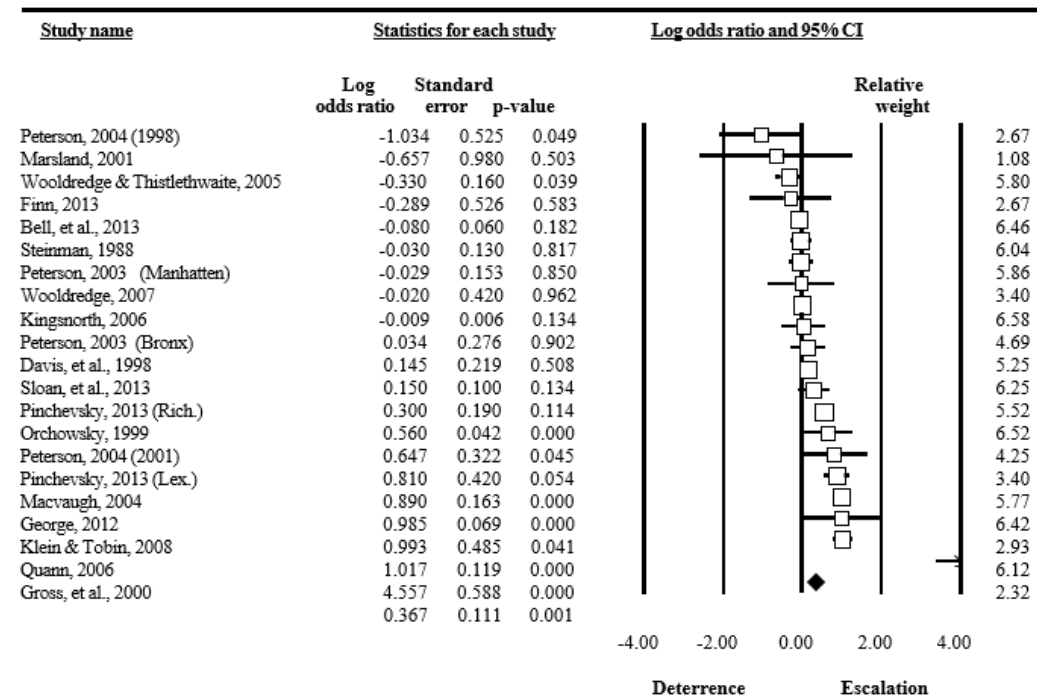


Note: Random effects meta-analysis model using best effects

Figure 4 lists the twenty-one studies which report on the specific deterrent effects of incarceration. Nine of these studies report an effect in the direction of deterrence, but only two of those are statistically significant. Of the twelve studies that reported an escalation effect, seven of them are statistically significant. The relative weights of these twenty-one studies range from a low of 1.08 to a high of 6.08, with no clear tendency for studies with a deterrent or escalation effect. With a seven-to-two ratio of statistically significant effects, a literature review or a vote-counting synthesis of these studies would likely reach a conclusion that incarceration is associated with more repeat offending. Our meta-analysis produced a statistically significant (p value = 0.001) log odds ratio of 0.367 which points towards an escalation of offending rather than a reduction in offending. Thus, in contrast to less

severe sanctions like prosecution or conviction, incarceration of intimate partner violence offenders is associated with a large, statistically significant increase in their rate of offending.

Figure 4. Incarceration Effects



Note: Random effects meta-analysis model using best effects

However, among these twenty-one studies, one effect size is five times larger than the next largest effect. Removing this one outlier reduces the average effect size to 0.269, but it is still substantial and statistically significant with a p value of 0.012. Removing the study that is most supportive of a deterrent effect for incarceration increases the log odds of escalation to 0.405 with a p value of less than .000.

Table 3: Summary Findings from Alternative Meta-Analytic Approaches

Effects of Prosecution							
Approach	Effect	Study	Log Odds		95% C. I.	z-value	p-value
	N	N	Ratio	s.e.			
Best	n.a.	10	-0.196	0.12	-0.43 - 0.03	-1.670	0.095
Average	n.a.	10	-0.255	0.13	-0.50 - -0.01	-2.015	0.044
HLM	61	10	-0.437	0.18	-0.80 - -0.07	-2.405	0.020
RSE	61	10	-0.312	0.15	-0.62 - -0.01	-2.042	0.046

Effects of Conviction							
Approach	Effect	Study	Log Odds		95% C. I.	z-value	p-value
	N	N	Ratio	s.e.			
Best	n.a.	26	0.021	0.11	-0.20 - 0.24	0.189	0.850
Average	n.a.	26	-0.099	0.09	-0.27 - 0.08	-1.119	0.263
HLM	92	26	-0.077	0.12	0.17 - -0.32	-0.631	0.530
RSE	92	26	0.191	0.19	0.56 - -0.18	1.025	0.310

Effects of Incarceraton							
Approach	Effect	Study	Log Odds		95% C. I.	z-value	p-value
	N	N	Ratio	s.e.			
Best	n.a.	21	0.367	0.11	0.15 - 0.59	3.293	0.001
Average	n.a.	21	0.359	0.09	0.18 - 0.54	3.970	0.000
HLM	84	21	0.443	0.12	0.68 - 0.20	3.716	0.000
RSE	84	21	0.690	0.18	1.06 - 0.32	3.753	0.000

As displayed in Table 3, using the “best” test approach generates distinct findings for each of the three hypotheses about post-arrest sanctions tested here. The ten prosecution studies produce a modest-sized effect in the direction of deterrence, but these findings do not meet the traditional two-sided *t* test for statistical significance. Our confidence in the prosecution findings, however, is limited by the small number of studies, the large variation in study impact due to the inverse-variance weighting procedure, and the sensitivity of these findings when removing the most and least favorable studies. Conversely, the existence of twenty-six more evenly weighted studies of the conviction hypothesis and the lack of an effect from removing outlier studies increases our confidence in the findings that there is no deterrent or escalation effect for the sanction of conviction. The twenty-one tests of the incarceration hypothesis produce a large effect in the direction of escalation effect exceeds the standards for statistical significance even when an extreme outlier is removed.

Some scholars are critical of the potential lack of objectivity in the selection of “best” tests. They and other scholars recommend the use of alternative meta-analytic methods for addressing the non-independence of research observations. This criticism of using the “best” test, if well founded, would undermine one of the main goals of meta-analysis, which is to remove the bias inherent in unstructured, qualitative methods of reviewing research literature. In order to address this concern, Table 3 reports the findings of our meta-analyses and the findings that are generated using three alternative meta-analytic methods. The first alternative approach averages effects within each study (Average). The second alternative approach uses all tests as independent effects but uses robust standard errors to estimate coefficients and compute statistical tests (RSE).⁹⁷ The third alternative approach uses hierarchical models incorporating other study level and effect level tests (HLM).⁹⁸ Table 3 reports the summary findings for each of the three sanction hypotheses for all four meta-analytic approaches.⁹⁹

While there are some differences in the size of the log odds ratios and standard errors between the four approaches, the substantive findings are strikingly similar. The findings of no effect for convictions and escalation effects for incarceration remain consistent across all four approaches. The findings for prosecution effects are all supportive of deterrence but, for all three of the alternative approaches, the *p* values exceed the traditional 0.05 standard, where the “best” test finding does not. However, the *p* values for two of the three alternative approaches to test the prosecution hypothesis exceed the 0.05 standard by just 0.004 and 0.006, producing the same dilemma of using a fixed standard, only this time on the side of finding a statistically significant effect. Similar to our conviction and incarceration findings, other criminological meta-analyses have reported consistent findings when implementing multiple meta-analytical approaches to the same body of research.¹⁰⁰

Our conclusion to accept the null hypothesis that prosecution does not affect repeat intimate partner violence is based on the findings produced by the “best” test approach to meta-analysis. Our preference for using these

⁹⁷ Hedges, Tipton & Johnson, *supra* note 93, at 41.

⁹⁸ RAUDENBUSH & BRYK, *supra* note 94, at 208–10.

⁹⁹ The results from the first two approaches—using the best and the average effect—were calculated using *Comprehensive Meta-Analysis Version 3*. The results from the “RSE” approach were calculated using *SPSS 25*. The HLM results were produced using *HLM 7: Hierarchical linear and nonlinear modeling*.

¹⁰⁰ Tammy Rinehart Kochel, David B. Wilson & Stephen D. Mastrofski, *Effect of Suspect Race on Officers' Arrest Decisions*, 49 *CRIMINOLOGY* 473, 489 (2011) (reporting, in Table 2, statistically significant odds-ratios between 1.53 and 1.74 for meta-analysis approaches using the average smallest, largest, and best effect sizes).

criteria over others is because our initial design included establishing explicit rules for selecting the best test among those reported in our 29 studies. We established these criteria and the use of the $p < .05$ standard with two-tailed test before we extracted data from publications to generate logged odds ratios and standard errors or compiled our forest plots or the formal statistical analyses reported in Table 3. As noted earlier, there is controversy in the research community about the value of retaining the .05 standard and there are multiple approaches to producing meta-analyses. Each approach has strengths and weaknesses and all have been used in articles appearing in refereed journals. Because there are advocates for each of these approaches, conclusions produced by only one approach could easily be regarded as suspect to some readers. While some scholars reject findings unless they are derived from an approach they prefer, we and others believe it is important to present results from multiple approaches so others can understand the extent to which findings vary by different meta-analysis approaches. We also believe that differences in outcome should not influence our preferences for one methodological approach over another.

In our meta-analyses, all four alternative approaches produced consistent findings for the conviction and incarceration hypotheses; therefore, those conclusions are not dependent on the approach we used. However, the three alternative approaches to testing the prosecution hypothesis do not produce findings consistent with those produced by the “best” test approach. While each approach produced a finding in the deterrence direction, only the “best” test approach did not meet the $p < .05$ level. Our reporting of findings from the three alternative approaches clarifies that the failure to find a statistically significant deterrent effect for prosecution depends on the approach we took. However, that does not mean that we give the findings from the alternative approaches equal weight in our conclusion about the effect of prosecution. Similarly, those who prefer alternative approaches need not give equal weight to our approach in their judgement about the effect of prosecution. Thus, while willing to report findings from alternative approaches, this paper relies on the findings from the statistical tests specified in our original design to determine the existence of statistically significant effects.

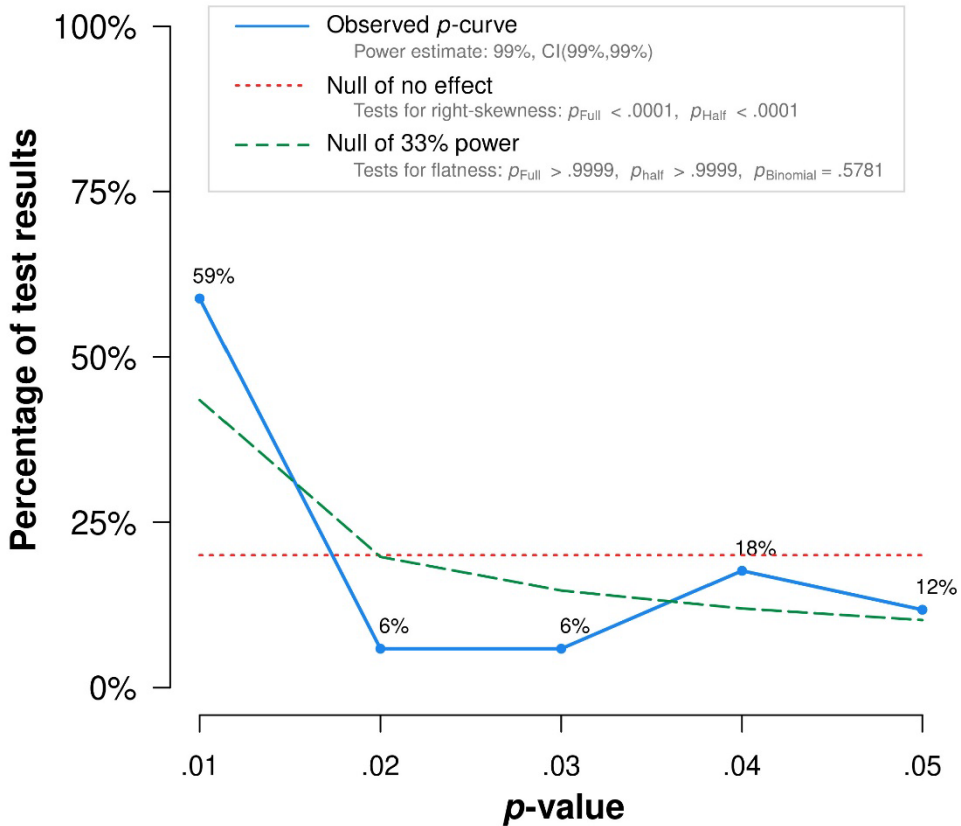
Traditionally, meta-analyses are concerned about the bias that might result from the existence of completed studies that are not reported and therefore cannot be included in a systematic review.¹⁰¹ The principal concerns have been the failure of authors to submit—or of journals to

¹⁰¹ See Robert Rosenthal, *The “File Drawer Problem” and Tolerance for Null Results*, 86 PSYCH. BULL. 638, 640 (1979).

publish—findings that either (1) find no statistically significant effects, or (2) are contrary to the accepted findings. We are less concerned with this issue because many available studies included here report no statistically significant differences, the ready availability of a large number of unpublished studies and the diverse findings in this field suggest that no particular finding is likely to be seen as the only acceptable finding. However, there is another concern that the available research may reflect a practice called *p*-hacking. *P*-hacking occurs when authors conduct many analyses but only report findings that exceed the *p* value < 0.05 standard. Given the great variety in the number of tests reported in our fifty-seven studies, the findings reported here might reflect *p* hacking. Using Uri Simonsohn, Leif Nelson, and Joseph Simmons' tests for *p*-hacking,¹⁰² we determined that most of our statistically significant results produce a *p* value < 0.01 and, therefore, they do not meet Simonsohn, et al.'s standards for *p*-hacking. Figure 5 displays this information and reports the statistical tests rejecting the hypotheses that (1) our studies show no evidentiary power and (2) our study's evidentiary power is inadequate.

¹⁰² Uri Simonsohn, Leif D. Nelson & Joseph P. Simmons, *P-Curve: A Key to the File-Drawer*, 143 J. EXPERIMENTAL PSYCH.: GEN. 534, 539–44 (2014).

Figure 5: P -Curve Tests



Note: The observed p -curve includes 17 statistically significant ($p < .05$) results, of which 12 are $p < .025$. There were 40 additional results entered but excluded from p -curve because they were $p > .05$.

B. STUDY DESIGN MODERATORS OF DETERRENT EFFECTS

Meta-analytic methods provide a basis to test which, if any, design characteristics of the available research are associated with stronger or weaker deterrent or escalation effects. This research uses all fifty-seven “best” effects to produce an analysis of nine moderators. Two moderators—whether studies were published in peer reviewed journals and whether the

studies were published before 2000—provide a test of selection effects of peer review and whether findings have changed over time. Six of these moderators were selected prior to data collection because of their presumed relationship to measuring repeat offending: (1) whether the analysis was multivariate or bivariate; (2) whether the analysis is based on the prevalence, frequency or time to first new failure; (3) whether the source of recidivism data is victim interviews or official records; (4) whether the recidivism is measured by a new offense, a new arrest, a new prosecution, or a new conviction; (5) whether recidivism involves only violent offenses or any offense; and (6) whether offenses against any victim or just the same victim

Table 4. Study Characteristics as Moderators of Specific Deterrent Effects

Study Characteristics	Log Odds					Q	p-value
	k	Ratio	s.e.	z-value	p-value		
Publication Type	57	0.06	0.052	1.081	0.280	3.655	0.056
Peer Reviewed	33	0.00	0.061	-0.049	0.961		
Reports and other	24	0.22	0.102	2.196	0.028		
Data Collection Year	57	0.09	0.062	1.486	0.137	0.128	0.720
Before 2000	42	0.08	0.066	1.241	0.215		
2000 and later	15	0.15	0.163	0.892	0.372		
Analysis Type	57	0.10	0.054	1.801	0.072	0.005	0.941
Bivariate	16	0.12	0.289	0.412	0.681		
Multivariate	41	0.10	0.055	1.755	0.079		
Recidivism Measure	57	0.06	0.058	1.018	0.308	5.913	0.052
Frequency	12	0.67	0.259	2.594	0.010		
Prevalence	44	0.03	0.061	0.413	0.680		
Survival	1	0.06	0.260	0.231	0.817		
Recidivism Data Source	57	0.07	0.055	1.268	0.205	18.066	0.000 *
Official Data	44	0.21	0.064	3.296	0.001		
Victim Interview	13	-0.31	0.105	-2.969	0.003		
Recidivism Criterion	57	0.21	0.049	4.214	0.000	73.710	0.000 *
Offense	15	-0.30	0.096	-3.065	0.002		
Arrest	34	0.20	0.066	3.059	0.002		
Charge	7	0.05	0.261	0.174	0.862		
Conviction	1	1.02	0.119	8.525	0.000		
Nature of Recidivism Offense	57	0.06	0.700	0.899	0.369	8.288	0.004 *
Any Offense	13	0.62	0.204	3.013	0.003		
Violent Offense	14	-0.01	0.074	-0.138	0.890		
Target of Recidivism Offense	57	0.00	0.046	0.003	0.998	10.165	0.001 *
Same Victim	21	-0.09	0.054	-1.621	0.105		
Other Victim	36	0.25	0.910	2.745	0.006		
Quality of Research Design	57	0.09	0.051	1.704	0.088	12.081	0.002 *
Weaker	14	0.34	0.230	1.477	0.140		
Modest	27	0.16	0.059	2.762	0.006		
Stronger	16	-0.26	0.115	-2.275	0.023		

are counted. In response to a suggestion from a reviewer, we created a 0 to 6 scale based on the standards for methodological rigor used to select the “best” tests and grouped each of the fifty-seven studies into three categories— weaker (less than 2), modest (2 to 4), and stronger (4 or higher)—to assess the extent to which overall research quality among the available studies moderates the effects of sanctions.

The findings displayed in Table 4 list the nine potential moderators along with the number of tests, the log odds ratio, and the z and p values for each category, as well as the results of the Q test for differences between the categories for each moderator.¹⁰³ For instance, the thirty-three peer reviewed studies had a log odds ratio of 0.00, but other types of documents had a log odds ratio of 0.22. Thus, studies producing an escalation effect are less likely found in peer review journals but, based on the Q statistic, these differences are not large enough to be statistically significant. Using these same criteria, there appears to be no trend in the direction of reported findings before and after 2000 or between bivariate and multivariate analyses. Similarly, differences in the size of the log odds ratios produced by tests using frequency measures (0.67) versus those using prevalence (0.03) or survival (0.06) measures are not large enough to meet the Q test for statistical significance. Thus, the effects of sanction are not moderated by publication type, data collection year, analysis type, or recidivism parameter.

Based on statistically significant Q tests, sanction effects are moderated by five other characteristics: the source of recidivism data, the recidivism criteria, the recidivism offense, the type of victim, and the overall quality of the research design. When recidivism is measured using victim interviews, there is a statistically significant deterrent effect; when measured using official records, there is a statistically significant escalation effect. When recidivism is based on new offenses, there is a statistically significant deterrent effect. When recidivism is based on new arrests or new convictions, there is a statistically significant escalation effect. Similarly, when recidivism is limited to violent offenses, the effect is essentially zero; when any type of offense is used to measure repeat offending, there is a statistically significant escalation effect. Analyses based on repeat offending against the same victim produce a nonsignificant deterrent effect; when the analyses include any victim, there is a statistically significant escalation effect. Lastly, the effect of sanctions is moderated by the summary measure of research quality with the stronger research designs producing a statistically significant deterrent effect and weaker designs generating escalation effects.

¹⁰³ See Tania B. Huedo-Medina, Julio Sánchez-Meca, Fulgencio Marín-Martínez & Juan Botella, *Assessing Heterogeneity in Meta-Analysis: Q Statistic or I² Index?*, 11 PSYCH. METHODS 193, 199–214 (2006).

Tammy Kochel and colleagues' bivariate tests of thirteen moderators of the relationship between race and arrest found no statistically significant moderator effects.¹⁰⁴ On the other hand, Travis Pratt and his colleagues' multivariate analyses found that deterrent theory effect sizes are "sensitive to a host of methodological variations."¹⁰⁵ Our bivariate moderator analyses found significant effects for five out of nine moderators; however, these five moderators are not independent of each other. The use of victim interviews, the offense criteria, and same-victim measures are highly correlated, so the independent impact of each one of these effects is uncertain.¹⁰⁶ These moderator analyses suggest that the methodological weaknesses of existing research constrain the ability of that research to accurately detect the extent to which alternative criminal sanctions reduce intimate partner violence.

Three of our fifty-seven studies are derived from Canadian samples. Removing the three Canadian studies changed the effect sizes slightly, but in no instance did their removal from the analyses change the direction or statistical significance of any of the tests of these three hypotheses.¹⁰⁷

IV. DISCUSSION

While our approach provides a rigorous review of the existing research, the methods and measures used in the available research are not always well-equipped to inform policy or to test theory. When possible, these weaker designs should be avoided in future efforts. More specifically, our meta-analysis identified several areas where investments and enhancements are needed to improve our understanding of deterrence theory. The first research priority should be to build on the small numbers of existing studies of prosecution effects with a few studies large and rigorous enough to resolve whether prosecution is or is not associated reduced amounts of intimate partner violence. For these studies, analyses of focused policy interventions

¹⁰⁴ Kochel, Wilson & Mastrofski, *supra* note 100, at 489–90.

¹⁰⁵ Travis C. Pratt, Francis T. Cullen, Kristie R. Blevins, Leah E. Daigle & Tamara D. Madensen, *The Empirical Status of Deterrence Theory: A Meta-Analysis*, in *TAKING STOCK: THE STATUS OF CRIMINOLOGICAL THEORY* 367, 384 (Francis T. Cullen, John Paul Wright & Kristie R. Blevins eds., 2008).

¹⁰⁶ See Mark W. Lipsey, *Those Confounded Moderators in Meta-Analysis: Good, Bad, and Ugly*, 587 *ANNALS AM. ACAD. POL. & SOC. SCI.* 69, 71–78 (2003) (illustrating the hazards and complexities of interpreting moderator analyses in meta-analyses).

¹⁰⁷ These sources were: Peter Jaffe, David A. Wolfe, Anne Telford & Gary Austin, *The Impact of Police Charges in Incidents of Wife Abuse*, 1 *J. FAM. VIOLENCE* 37 (1986); Lauren Marsland, Darryl Plecas & Tim Segger, *Reticence and Re-Assault Among Victims of Domestic Violence in a Pro-Charge Jurisdiction* (Jan. 2001) (unpublished report) (on file with J. Crim. L. & Criminology); NATHALIE QUANN, CANADA DEP'T OF JUST., *OFFENDER PROFILE AND RECIDIVISM AMONG DOMESTIC VIOLENCE OFFENDERS IN ONTARIO* (2006).

may be sufficient to address this basic question, especially if they can address the problem of selecting high-risk offenders for more severe sanctions and measure repeat offending using victim interviews as well as official records.

Even if successful, those prosecution studies cannot explain why potential offenders do or do not repeat their offenses. For that more important set of questions, future research on sanctions for intimate partner violence needs to study the decision-making of potential offenders and incorporate not just the characteristics of sanctions but the variety of factors—including emotions and fears—that can test alternative theories of the causes of intimate partner violence.¹⁰⁸ To be especially useful for understanding intimate partner violence, such panel studies should interview both partners and capture information relevant to other theories of intimate partner violence, such as victim empowerment.¹⁰⁹

None of the research included in this meta-analysis adequately addressed the extent to which more severe criminal justice sanctions were imposed on individuals because they were thought to have a higher likelihood of offending again. Some studies of imprisonment have used the random assignment of cases to judges or propensity scores to at least partially address the long-standing issue of selection bias in criminology;¹¹⁰ however, another promising approach is the development of accurate prediction models for future incidents of intimate partner violence and the comparison of predicted failures and actual failures for individuals arrested, prosecuted, convicted, or incarcerated.¹¹¹

In the broader field of deterrence research, theorists have yet to identify what is the appropriate minimum or maximum time at risk needed to assess properly the impact of criminal sanctions on subsequent offending. Our understanding of deterrence is insufficient to specify how quickly and for how long any particular sanction will or will not affect particular types of

¹⁰⁸ See Greg Pogarsky, Sean Patrick Roche & Justin T. Pickett, *Offender Decision-Making in Criminology: Contributions from Behavioral Economics*, 1 ANN. REV. CRIMINOLOGY 379, 389–91 (2018).

¹⁰⁹ See Cattaneo & Goodman, *supra* note 5, at 84; (arguing for the important place of victim empowerment as active causal agents in reducing intimate partner violence); Mills, *supra* note 62, at 313–16 (emphasizing the role of victims in the understanding the casual relationship between criminal justice policies and repeat offending).

¹¹⁰ See, e.g., Daniel P. Mears, Joshua C. Cochran, William D. Bales & Avinash S. Bhati, *Recidivism and Time Served in Prison*, 106 J. CRIM. L. & CRIMINOLOGY 81, 141–44 (2016) (reviewing the use of alternative methodological approaches to test the effects of imprisonment on future criminal behavior).

¹¹¹ See generally Kirk R. Williams & Richard Stansfield, *Disentangling the Risk Assessment and Intimate Partner Violence Relation: Estimating Mediating and Moderating Effects*, 41 L. & HUM. BEHAV. 344 (2017) (demonstrating how the use of domestic violence risk assessments was associated with reduced recidivism for high risk perpetrators).

criminal behavior. Should the specific deterrent effects of criminal sanctions last about as long as an aspirin cures a headache, as long as a flu vaccine prevents the flu, or as long as the polio vaccine prevents polio? Sanction theories needed to address that fundamental question before research can properly determine the appropriate time at risk for measuring repeat offending. Future research on sanctions for intimate partner violence may also benefit from what we learned from our moderator analysis. Although best viewed as more descriptive and exploratory than rigorous hypothesis testing, our moderator analysis suggests that future research on the deterrent effects of intimate partner violence would be stronger if it emphasized offense-based frequency measures of violent offending perpetrated against the same victim and if those measures of repeat offending were obtained from victim interviews.

This research was motivated by the inattention to intimate partner violence and to sanctions other than arrest or imprisonment in existing reviews of deterrence research. The findings presented here demonstrate that those reviews have ignored a large body of research that reveals real differences in the size and direction of the deterrent effects that different criminal sanctions have on intimate partner violence. Future research on deterrence might benefit from incorporating Durlauf and Nagin's suggestion that deterrent effects might vary by different types of sanctions and for a variety of offense types.¹¹²

At the present time, several federal programs promote the use of more prosecution, conviction, and incarceration for intimate partner violence. The evidence from this research is that there is more—not less—violence against intimate partners when prosecution and conviction are followed by incarceration. These findings provide systematic evidence against the use of incarceration for this offense; however, we appreciate the concerns that no one study, even a review of many studies like our meta-analyses, will likely produce sufficient knowledge to formulate new policies by itself.¹¹³ Nevertheless, the potential harm associated with incarceration cannot be ignored, and those who advocate for more frequent and more severe post-arrest sanctions must either develop alternatives to incarceration or identify other rationales that provide sufficiently large social benefits to outweigh the increased frequency of violence associated with the use of incarceration for intimate partner violence.

¹¹² Durlauf & Nagin, *supra* note 37, at 85–86.

¹¹³ Robert J. Sampson, Christopher Winship & Carly Knight, *Translating Causal Claims: Principles and Strategies for Policy-Relevant Criminology*, 12 *CRIMINOLOGY & PUB. POL'Y* 587, 594–96 (2013).

V. APPENDICES

Appendix 1. Keyword and Search Terms for Meta-Analysis

<u>Intimate Partner Violence</u>	<u>Court Disposition</u>	<u>Recidivism</u>
Domestic abuse	Charge	Deterrence
Domestic assault	Conviction	Reabuse
Domestic violence	Court	Rearrest
Family violence	Counseling	Reassault
Intimate partner violence	Criminal justice system	Recharge
Intimate partner abuse	Disposition	Recidivism
Intimate terrorism	Jail	Reconviction
Spousal abuse	Incarceration	Reoffense
Spousal violence	Legal Intervention	Repeat abuse
Wife assault	Legal system	Repeat offense
Wife abuse	Prison	Revictimization
Wife batter	Probation	
Violence against intimate partner	Prosecution	
Violence against spousal	Sanction	
Violence against women	Sentencing	

Appendix 2: Documents Used in the Meta-Analyses

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