

Estimating the negative and racialized consequences of the police-centric response to intimate  
partner violence

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## **Abstract**

Estimating the negative and racialized consequences of the police-centric response to intimate partner violence

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Intimate partner violence (IPV) is estimated to impact about one in four U.S. women in their lifetime and represents 15% of all violent crime. Total violent crime rates have steadily declined across the country, but rates of IPV victimization have fallen at far slower rates and the incidence of intimate partner homicide has been increasing in recent years. These alarming trends suggest that current strategies for IPV prevention are insufficient and may even be counterproductive. Since the 1970s, the U.S. has developed and maintained a police-centric response to IPV—a response that relies on arrest as its primary tool through practices and policies including mandatory arrest laws and other pro-arrest policing practices. This police-centric response to IPV persists despite increased recognition of the harms of mass criminalization and incarceration and growing calls for criminal legal reform, and despite a lack of empirical evidence that policing and arrest in fact prevent or reduce IPV.

In addition, there are strong theoretical reasons to believe, and emerging empirical evidence to suggest, that there are negative consequences of the police-centric response to IPV that extend beyond subsequent IPV victimization, including the increased risk of all-cause mortality among survivors of IPV and the increased risk of child protective services involvement for families. However, there is very limited quantitative research estimating these negative consequences of IPV policing. There are also likely profound racialized disparities in the

consequences of IPV policing because of the ways in which policing, and the criminal legal system more broadly, disproportionately harms Black communities and other communities of color. In this dissertation, I assessed the state- and county-level consequences of the police-centric response to IPV for the health and safety of IPV survivors and estimated the extent to which those consequences have differential impacts across racialized groups.

The first chapter presents a systematic scoping review synthesizing the existing evidence concerning the negative and racialized consequences of the police-centric response to IPV. The review included all empirical studies (quantitative and qualitative) focused on a U.S. population that assessed consequences of IPV policing. A total of 34 articles were included in the review. I found that survivor criminalization was the most studied negative consequence of IPV policing and the existing evidence suggests that IPV policing has increased the risk of survivor arrest. I also found that there have been numerous rigorous studies on the effects of mandatory arrest laws on population-level measures of IPV victimization, including IPV homicide rates. The evidence to date generally suggests there is no association between mandatory arrest rates and population-level IPV victimization rates. The review also identified gaps in the evidence base: specifically, there is a need for research on additional potential consequences of IPV policing such as police violence against survivors, child protective services involvement, and measures of the psychosocial and physical health of survivors.

The second chapter presents a difference-in-differences analysis estimating the effect of state-level changes in warrantless arrest legislation for IPV on the all-cause mortality of IPV survivors from 1980-2019 in the U.S. I analyzed panel data measured at the state-year level and included data for all U.S. states and the District of Columbia. I used women's all-cause mortality age 20-54 as a proxy for all-cause mortality among IPV survivors. I used quasi-Poisson

regression models with a population offset term with robust standard errors to model the association between state-year changes in warrantless arrest legislation and all-cause mortality. I also fit two models with race-specific rates of women's all-cause mortality (20-54 years) to assess if there are differences by racialized group. Overall, the findings suggested that there is no detectable effect of mandatory arrest laws on women's mortality (20-54 years) at the population level, however, there appears to be a harmful effect of preferred arrest laws. Despite no documentation of a harmful effect of mandatory arrest laws on women's mortality (20-54 years), this null effect should be weighed against known, documented harmful effects of mandatory arrest such as its role as a driver of female arrests and arrests of IPV survivors.

The third chapter presents a county-level analysis of the intersection between IPV arrest practice and family surveillance from 2000-2019 in large U.S. counties. I hypothesized that family surveillance and subsequent intervention by child protective services agencies would be a negative consequence of the police-centric response to IPV because of direct coordination between police and child welfare systems and the subsequent increased risk of child protective services involvement that may follow from the arrest of a parent. I evaluated family surveillance and child protective services involvement as a negative consequence because of the harmful and racialized ways in which the U.S. child welfare apparatus targets, punishes, and breaks apart Black families. The criminal legal system and child welfare system are both rooted in structural racism and, thus, their coordination is expected to cause generational harm for Black families. The outcome of interest was the rate of child maltreatment reports that received a response from child welfare agencies and the exposure of interest was the percentage of IPV incidents that were reported to police and resulted in arrest, measured at the county-year level. I used Poisson regression and modeled between- and within-county effects using three types of models: 1) a

multilevel county random intercept model, 2) a multilevel county random intercept model with state fixed effects, and 3) a county fixed effects model. I also fit two additional models with race-specific outcome data and conducted an interaction analysis by the percentage of Black residents in the county to assess if there were differences by racialized groups and within different racialized contexts.

The findings presented in the third chapter demonstrated no overall association between the percentage of police-reported IPV incidents that resulted in arrest and child maltreatment report rate at the county level. In addition, there was no county-level evidence to suggest that the association differed for the Black vs. non-Hispanic white child maltreatment report rates. These county-level findings are inconsistent with existing individual-level survey research and qualitative research that provide evidence that IPV policing is associated with child protective services involvement through direct coordination between police and the child welfare system. However, the interaction analysis demonstrated that the percentage of police-reported IPV incidents that resulted in arrest was positively associated with the Black child maltreatment report rate within counties with a low percentage of Black residents. Therefore, the association between IPV policing and family surveillance may vary by individual and contextual factors. This was the first study to examine the intersection between IPV policing and family surveillance at the county level, so additional research is warranted to assess whether the findings can be replicated. Individual family-level data would be useful to further interrogate the relationship between IPV policing and family surveillance.

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## Introduction

The crisis of intimate partner violence (IPV)—violence committed against a spouse or romantic partner—is age-old. What has changed over time is the public response to this crisis. Rooted in patriarchal structures that have dominated much of ancient and modern history, IPV was long considered a private matter that did not warrant outside intervention, and survivors of violence<sup>1</sup> had little recourse when seeking help (1,2). In the U.S., the 1970s brought on a paramount shift in the ways in which we respond to IPV via the Battered Women’s Movement and other national feminist movements (3). Specifically, IPV was redefined in the eyes of the law as a public safety concern and a criminal matter rather than a private matter (3).

At the same time, the U.S. was undergoing an unprecedented expansion in its criminal legal apparatus, resulting in what is now known as “mass incarceration.” Today, the U.S. incarcerates a higher rate and number of people than any other nation (4). At any given time, there are more than 2 million people incarcerated in a jail or prison, representing a 700% increase in the incarcerated population since 1970 (5). These two national shifts may seem unrelated, but they are in fact inextricably linked—the criminalization of IPV has played an undeniable yet often overlooked role in the U.S. jail and prison boom (6). This link is demonstrated clearly through federal policy and funding. For example, the Violence Against Women Act of 1994 (VAWA) was the first federal legislation in the U.S. enacted to address IPV and was a critical component of the 1994 Crime Bill, which is often cited as a driver of mass incarceration (7). It has consistently been among the most influential policies guiding the

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<sup>1</sup> In this dissertation, I use the term survivor to refer to people who have experienced IPV victimization. However, it is important to note that not all people who have experienced IPV victimization identify as survivors and in some cases the term may be inappropriate, such as in the case of IPV homicide.

nation's social service and criminal legal response to IPV (7). While the legislation has provided critical funding for social services that support the needs of survivors of violence, it has focused on criminal legal intervention as the primary response to IPV. When it was first passed in 1994, VAWA appropriated 62% of funding for criminal legal intervention and 38% for social services (7). By its 2013 reauthorization, the proportion of VAWA funds appropriated for criminal legal intervention increased to 85%, with just 15% earmarked for social services (7). The link between the criminalization of IPV and mass incarceration is also demonstrated through state and local policies, such as through the nationwide popularity of mandatory arrest laws for IPV, which require arrest for misdemeanor IPV incidents.

Today, we still maintain a police-centric response to IPV that uses policing as its primary tool for IPV prevention and response, including through mandatory arrest laws. Often, a survivor experiencing IPV has few options for government intervention beyond contacting the police. This response has remained relatively unchanged despite growing recognition of the racialized harms of mass criminalization and incarceration in the U.S. It is well documented that the U.S.'s system of policing and its broader carceral system disproportionately targets and punishes Black communities and other communities of color (8–12). Over the past decade, there has been increasing bipartisan support for criminal legal reform measures to reduce the number of people exposed to the criminal legal system. However, criminal legal reform efforts are often focused exclusively on misdemeanors and nonviolent felonies and have largely excluded IPV (3,13–15). This phenomenon has been referred to as “domestic violence exceptionalism” (16). Meanwhile, many jurisdictions are enhancing the implementation of existing policies such as mandatory arrest and mandatory minimum sentencing for IPV-related offenses, without consistent evidence of their effectiveness at reducing IPV. In fact, IPV is often invoked as the primary public safety

issue that requires increased policing, criminalization, and incarceration (17–20). IPV assaults are more likely to result in arrest, prosecution, and incarceration than non-IPV assaults (6). Additionally, in 2020, lawmakers used concerns over IPV to rollback bail reform in New York (21) and increase sentencing lengths in Oklahoma (22).

This persistent reliance on the police-centric response to IPV, despite it being rooted in a system of mass criminalization that disproportionately punishes people of color, may give the appearance that police keep IPV survivors safe. However, the body of empirical research concerning the preventive effect of IPV policing has produced no consistent or compelling evidence that policing and arrests reduce subsequent IPV (23–32). In addition, there are likely negative and racialized consequences of this police-centric response that disproportionately impact survivors who belong to socially marginalized groups. Such consequences have been documented by survivor advocacy groups and include the criminalization and/or arrest of survivors for acts directly tied to their own survival (e.g., self-defense), violence at the hands of police when they are called for help, and intervention by child protective service agencies for “failing to protect” children from IPV in the home. In the anti-domestic violence movement, there is growing recognition that a police-centric response to IPV is associated with an increased prevalence of these consequences, particularly among low-income survivors of color (33). However, these negative consequences are often not considered in criminal legal research and social science research when assessing the impacts of IPV-related policing/arrest practices.

Herein lies the problem: Policing remains the primary response to IPV despite a lack of evidence concerning the preventive effect of policing on IPV and despite the potential negative consequences of the police-centric response to IPV. While there is a substantial research literature focused on the preventive effect of policing for subsequent IPV victimization, yielding

no clear or consistent evidence, there has been little to no empirical research focused on the potential negative and racialized consequences of the police-centric response to IPV that extend beyond IPV victimization. I aim to fill this gap in the literature and inform national debate concerning the merits and harms of the police-centric response to IPV through the following research aims:

**Research Aim 1: Synthesize the existing evidence concerning the negative consequences of the police-centric response to IPV through a systematic scoping review**

- Research Aim 1a: Synthesize and build upon existing theoretical arguments for studying negative consequences of the police-centric response to IPV for survivor health and well-being (outcomes other than IPV victimization) and the importance of assessing racialized differences with respect to such consequences
- Research Aim 1b: Conduct a systematic literature search to identify empirical studies focused on negative consequences of the police-centric response to IPV on survivor health and well-being
- Research Aim 1c: Assess the extent to which the extant research has considered differences in the negative consequences of the police-centric response to IPV across racialized groups

**Research Aim 2: Estimate the effect of state-level changes in mandatory arrest legislation on mortality of IPV survivors from 1980-2019 in the U.S.**

- Research Aim 2a: Conduct a difference-in-differences analysis to estimate the effect of state-level changes in mandatory arrest legislation on all-cause mortality rates for women aged 20-54 (as a proxy for all-cause mortality among IPV survivors)

- Research Aim 2b: Assess if there are racialized differences in the relationship between state-level changes in mandatory arrest legislation and women's mortality (20-54 years) using separate models with race-specific women's mortality rates

**Research Aim 3: Estimate the effect of county-level IPV arrest practice on county-level rates of child maltreatment reports from 2000-2019 in large U.S. counties.**

- Research Aim 3a: Estimate the effect of county-level differences in the percentage of police-reported IPV incidents that result in arrest on rates of child maltreatment reports using multilevel random effects models and county fixed effects models
- Research Aim 3b: Assess if there are racialized differences in the relationship between IPV arrest percentages and rates of child maltreatment reports using separate models with race-specific child maltreatment report rates and an interaction analysis by county racial composition

# Chapter 1: Negative consequences of the policing response to IPV: A scoping review

## 1.1 Introduction

IPV impacts one in four women in the U.S. and constitutes a growing proportion of violent crime (34,35). IPV includes physical violence, sexual violence, psychological abuse, stalking, and other forms of coercion between current or former spouses or dating partners. Since the 1980s, the primary response to IPV in the U.S. has been policing, prosecution, and incarceration. This “carceral” response to IPV has persisted despite inconclusive evidence of its effectiveness, increased recognition of the harms of mass criminalization and incarceration, and growing calls for criminal legal reform (36,37). However, there are strong theoretical reasons to believe that the carceral response to IPV—especially policing—also has negative consequences for survivor health and safety. These consequences likely disproportionately harm survivors of color. In this scoping review, I conduct the first ever summary of the empirical evidence concerning such negative consequences of IPV policing.

### *Problematizing IPV and responses to IPV*

IPV is a highly prevalent problem with severe consequences for health and safety (38). While IPV can affect anyone in an intimate relationship, patriarchal power structures in the U.S. and globally make women (cisgender and transgender) particularly vulnerable to severe forms of IPV victimization compared to cisgender men (1,39). National estimates indicate that more than 20% of women have experienced severe physical violence from an intimate partner in their lifetime (34). There are also pronounced racialized disparities in the prevalence and severity of IPV. For example, in New York City, Black women represent 13% of the city’s population but 30% of intimate partner homicide victims in the city (40). The health and safety consequences of

IPV are manifold, including physical injury and death, as well as long-term chronic health consequences such as heart disease, reproductive disorders, gastrointestinal disorders, and psychiatric disorders (41–43). Additionally, violent crime rates have steadily declined across the country, but rates of IPV have fallen at far slower rates and have begun to stabilize (35). Moreover, rates of IPV homicide have been increasing in recent years (44). The need for effective strategies to reduce and prevent IPV is at a critical stage for public health and public safety.

Prior to the 1980s, IPV had historically been viewed as a private matter that did not require external intervention (45,46). However, through the confluence of the Battered Women’s Movement and the rise of the “tough-on-crime” era, IPV was redefined in the public eye as a criminal legal issue (45,47). Applying a criminal legal perspective to the problem of IPV has in turn shaped strategies to reduce and prevent IPV as well as the study of the effectiveness of those strategies. Namely, the U.S. employs a “carceral feminist” response to IPV, meaning that it relies on policing, prosecution, and incarceration as its primary tools to address gender-based violence (36,37). This carceral feminist response to IPV arose as part of a larger carceral evolution in the U.S., as racialized criminalization was applied to a growing number of social problems leading to unprecedented levels of policing and criminal legal punishment, particularly for Black Americans and other communities of color—what is now widely referred to as mass incarceration (48).

A primary example of the carceral feminist response to IPV is mandatory arrest laws. Mandatory arrest laws require police to make an arrest in response to calls for IPV, regardless of police discretion and survivor wishes (49). These laws grew in national popularity due in part to the findings from a 1984 randomized trial called the Minneapolis Domestic Violence Experiment

(MDVE). The MDVE was a six-month trial with 314 subjects in which police officers were randomly assigned to make an arrest, send the abusive partner away for 8 hours, or offer counseling for a sample of misdemeanor IPV cases (23). Results from the MDVE showed that men who were arrested had reduced likelihood of recidivism after six months (23). By 1989, over 75% of jurisdictions across the country changed their domestic violence laws to allow for warrantless arrests for domestic violence misdemeanors (45) and by 1992, 15 states and the District of Columbia adopted mandatory arrest laws for domestic violence (25). Mandatory arrest laws are one type of warrantless arrest law for IPV. Other types of warrantless arrest laws for IPV include discretionary arrest laws (officers may arrest without a warrant) and preferred arrest laws (arrest is the preferred response). Today, mandatory arrest and other pro-arrest policies remain a major component of the nation's police response to IPV. The policing response to IPV also includes policing practices, such as enforcement of no-contact orders, and staffing decisions, such as the funding of specialized police units for domestic violence.

The predominantly criminal legal approach to IPV has not only shaped responses to IPV, but also how the effectiveness of such responses is measured. Specifically, research evaluating the impacts of criminal legal responses to IPV are primarily focused on criminal legal outcomes, such as reported crime rates, arrest rates, and recidivism rates. Applying a critical public health perspective to the study of IPV and responses to IPV demands consideration of longer-term downstream population health consequences that reflect broader constructs of survivor health and well-being beyond criminal legal measures of victimization. It is also important for researchers to consider long-term outcomes when evaluating the effectiveness of an intervention. For example, in the case of arrest for an IPV incident, the violence may stop in the immediate

term if the abusive partner is incarcerated, but the violence may continue or worsen in the longer term if the root cause of the violence goes unaddressed.

*Theoretical frameworks supporting and challenging IPV policing*

Proponents of mandatory arrest policies and other police-centric responses to IPV argue that they protect survivors through deterrence. Deterrence theory purports that people make rational choices about whether to commit crimes and that crime can be deterred through certain and severe punishment because it creates an imbalance in which the costs of committing a crime outweigh the benefits (50,51). In the case of IPV, the ostensible goal of the policing response is to use punishment as a tool to deter future IPV incidents. Despite this theoretical foundation, there is limited empirical evidence in support of deterrence theory. For example, reviews of the empirical literature report that perceptions of punishment severity and certainty have weak effects, at best, on crime prevention (51,52). These reviews also report that other indicators of strong policing-based responses to criminalized behavior, such as police force size, arrest rates, and incarceration rates, have demonstrated little effect on crime rates over time (51–53). Greater theoretical understanding of the social conditions that place structural constraints on rational decision making and choice, such as poverty and social isolation, have challenged the assumptions of deterrence theory (53). Taken together, the theoretical and empirical evidence suggests that the drivers of criminalized behaviors are more complex than those described within deterrence theory, and because punitive crime control policies are not responsive to these complex drivers they often fail to have the desired effect on long-term behavior change and crime prevention (51).

Critiques of the policing response to IPV fall under a number of conceptual frameworks, but they share a common argument that IPV policing fails to address the root causes of IPV.

Here I will focus on the abolition feminism framework, which integrates feminist analysis with police and prison abolition. Abolition feminism stands in opposition to carceral feminism, which, as described above, is the belief that systems of policing and prisons can and should respond to and prevent IPV (37). Instead, abolition feminism posits that systems of policing and prisons are forms of racialized gendered violence themselves, for example through widespread violence committed by police or the violence of invasive strip searches in correctional facilities, and therefore are incompatible tools for violence prevention (54). IPV policing policies and practices extend the reach of carceral control, contributing to mass criminalization and incarceration, which is known to have harmful economic, psychosocial, and health consequences, particularly for Black Americans (54). IPV survivors may suffer serious consequences from engaging with the criminal legal system not only because of the potential arrest and incarceration of their abusive partner, with whom they may have strong emotional and financial ties, but also because the survivor may be criminalized themselves (e.g., being arrested in a case of self-defense or surveilled by the child welfare system) (54,55). Proponents of abolition feminism argue that strategies for police/prison abolition can simultaneously address the harms of mass criminalization and the root causes of IPV to better serve the interests of all survivors (54). An example of such a strategy is the redirection of public funding from police and prison infrastructure toward affordable housing infrastructure because housing is the most common need identified by IPV survivors (3,56).

The U.S. has developed and maintained a police-centric response to IPV despite increased recognition of the harms of mass criminalization and incarceration and growing calls for criminal legal reform. However, criminal legal reform efforts have excluded violent criminalized behavior generally, and IPV specifically (2,3,13–15). In fact, criminal legal reform

efforts often include specific exceptions for IPV or even include harsher criminal legal punishments for IPV (2,3,13–15). In maintaining and reinforcing a police-centric response to IPV, policymakers are ostensibly weighing the societal costs of increased criminalization against the public safety benefits of IPV policing and determining that the benefits outweigh the costs. However, what are the public safety benefits of IPV policing? There is a widespread assumption that IPV policing improves survivor safety and does not have negative consequences for survivors. What evidence exists to support this assumption?

*Evidence concerning the effect of IPV policing on subsequent victimization*

After nearly three decades of research on the topic, there is no consistent empirical evidence that IPV policing decreases subsequent IPV victimization at the individual level. Following the MDVE, the National Institute of Justice funded a series of replication studies in five U.S. cities to measure the effect of arrest vs. non-arrest alternatives such as mediation or separation for misdemeanor domestic violence assault causes on subsequent revictimization (24,28,29,57,58). Those trials resulted in inconsistent findings, with some showing evidence of a deterrent effect of arrest, some showing no difference between arrest and non-arrest, and one showing an increased risk of subsequent violence after arrest. Variation in study locations/populations, sample size, and alternate police responses tested may have driven some of these differences in findings. Since then, multiple systematic reviews and meta-analyses have synthesized the findings from these trials and more recent trials. The most recent meta-analysis, published in 2020, included a total of 11 trials evaluating the individual effects of arrest vs. non-arrest on subsequent IPV victimization (59). The summary estimate across the 11 studies was null (OR=0.946; 95% CI: 0.787, 1.138), demonstrating that arrest has no effect on subsequent IPV victimization at the individual level (59).

Even if the existing evidence shows no individual-level effect of arrest on subsequent IPV revictimization, proponents may still argue that a police-centric approach for IPV should be maintained because it may at least help a small number of survivors. However, there are likely negative consequences of the police-centric response to IPV that may cause generalized harm for survivors and that have received less attention in the extant scientific literature. Additionally, there are strong theoretical reasons and emerging empirical evidence to suggest that such consequences have disproportionate impacts on survivors of color.

#### *Negative consequences of IPV policing*

IPV policing can actually increase the frequency and severity of IPV in some cases by exacerbating underreporting due to fear of arrest or because an abusive partner may become more violent in a retaliatory response to having the police called on them (60). Additional potential negative consequences include the criminalization of survival, or the arrest, prosecution, and/or incarceration of survivors of violence for acts tied to their own survival, such as self-defense (61); police violence against survivors (62,63); and child protective services involvement (62,64). IPV policing can trigger investigations from child protective service agencies because there is typically strong coordination between government systems of policing and family surveillance (64–66). These potential consequences of IPV policing and others, are likely to impact the psychosocial and economic well-being of survivors in the short- and long-term and, as a result, may have generalized consequences for survivor health. For example, partner incarceration is longitudinally associated with negative health outcomes for women including increased risk of substance use, major depressive disorder, and AIDS (67–70).

These negative consequences and others, if systematically studied and verified at the population level, may suggest that the police-centric response to IPV in fact does more harm than

good for survivor safety, health, and well-being. Namely, if IPV policing is not only ineffective at decreasing subsequent IPV victimization at the individual level, but also leads to harmful consequences for survivor safety and well-being, then it is critical to reimagine our response to IPV.

### *Purpose of the present review*

The empirical evidence concerning the negative consequences of IPV policing beyond individual-level IPV revictimization has not yet been comprehensively reviewed and synthesized. Further, extant studies are published across journals in different fields, including criminology, law, economics, and women's studies, making them difficult to assess as a coherent body of research. The objective of the present scoping review is to summarize, for the first time, the existing research concerning the negative consequences of policing and arrest as a response to IPV, including population-level estimates of IPV revictimization, rates of reporting IPV to police, survivor criminalization, and other survivor-based measures of health and well-being. I also assess the extent to which prior research has considered racialized differences in those negative consequences.

## **1.2 Methods**

### *Literature search*

I conducted a search of Web of Science, ProQuest (which includes MedLine), and EBSCO Host (which includes the National Criminal Justice Reference Service and PsycInfo). I used the following search terms: ("mandatory arrest" OR "arrest policy" OR "arrest rate" OR "rate of arrest") AND ("domestic violence" OR "intimate partner violence" OR "domestic abuse" OR "spous\* assault" OR "wife abuse" OR "wife beating"). I additionally restricted the search to

articles published in English in scholarly journals in the year 1980 or later because the police-centric response to IPV arose in the 1980s. The last search was completed on September 7, 2021.

#### *Inclusion/exclusion criteria*

Eligible articles included all empirical studies (quantitative and qualitative) focused on a U.S. population that assessed consequences of the police-centric response to IPV. The study population could be local or national so long as the outcome was a survivor-focused outcome. The specific exposures of interest were policing policies and practices in response to IPV, such as mandatory arrest or other warrantless arrest legislation/policies, individual arrests for IPV, interactions with police, or the implementation of specialized IPV policing units or specialized training. The outcomes of interest were any survivor-based outcome, excluding individual-level IPV revictimization because of the many existing reviews and meta-analyses focused on individual-level IPV revictimization (59,71–77). Outcomes of interest included population-level measures of IPV victimization rates or IPV homicide rates and survivor-based outcomes other than risk of revictimization, such as risk of dual arrest, rates of reporting to police, and measures of survivor health and well-being.

#### *Screening and abstraction*

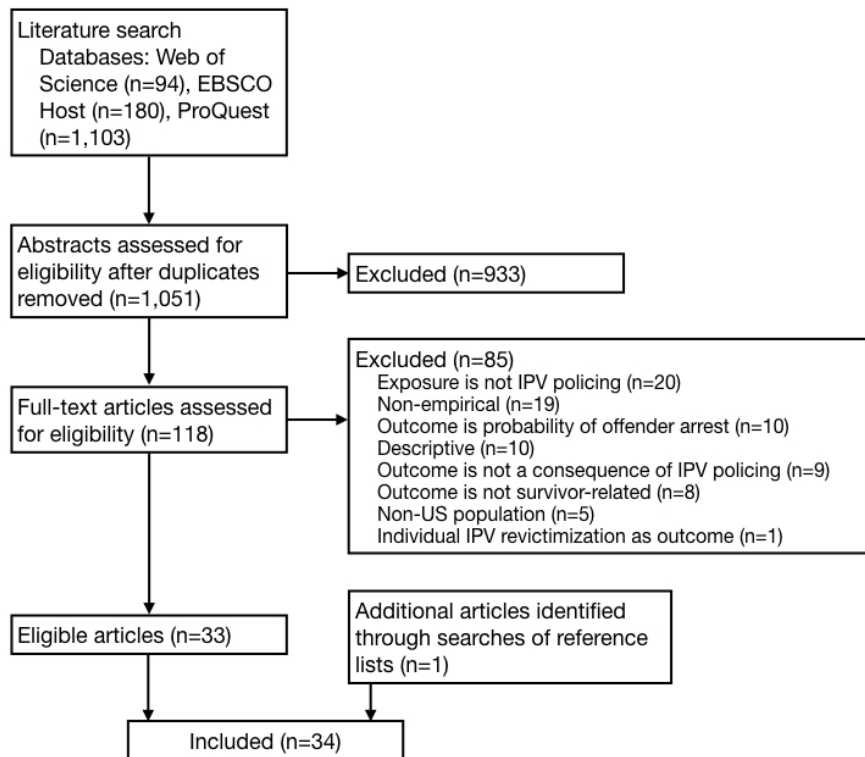
I used the systematic review management software Covidence to conduct the screening and abstraction process. After removing duplicates, I individually conducted the title and abstract screen. The full-text screen and data abstraction process were conducted in collaboration with two student colleagues. Two reviewers independently screened each full-text article for inclusion. Any disagreements between the two reviewers were resolved with input from the third reviewer. I also conducted a review of the reference lists of all included articles to identify additional articles that met the inclusion criteria. Using a standardized form, data were abstracted

independently by two reviewers on study characteristics such as study design, study population, unit of analysis, operationalization of exposure and outcome, main findings, and whether the study included a racial analysis. I pilot tested the form on three studies and edited as necessary before using it for all included studies. Data abstraction forms were compared, and any disagreements were resolved with input from the third reviewer. Methods were consistent with the Preferred Reporting Items for Systematic reviews and Meta-Analyses extension for Scoping Reviews (PRISMA-ScR) (checklist in Appendix A). The review was registered with Prospero, an international database of systematic review protocols, on March 12, 2021 (CRD42021288849).

### 1.3 Results

#### *Search results*

Figure 1 depicts a flowchart summarizing the search and screening process. The literature



**Figure 1: Flowchart summarizing article search and selection process**

search yielded 1,051 articles after duplicates were removed. Of those, 933 were excluded during the title and abstract screen, resulting in 118 articles for the full-text screen. An additional 85 articles were excluded during the full-text screen. The most common reasons for exclusion were: (1) exposure is not IPV policing (n=20), (2) non-empirical (e.g., systematic reviews or legal scholarship) (n=19), (3) outcome is probability of offender arrest (n=10), and (4) descriptive (n=10) (e.g., describing characteristics of a population with no analytic comparisons). Following the full-text screen, 33 eligible articles were identified. One additional article was identified through searches of reference lists of included articles, resulting in a total of 34 articles that met the inclusion criteria.

#### *Description of included articles*

This section provides an overview of the publication year, journal discipline, study design, study population, exposure(s), and outcome(s) of each study as well as a discussion of whether the study included an analysis of differential impacts of IPV policing by race. Characteristics of included studies are also summarized in Table 1.

*Publication year.* All of the included articles were published in 1994 or later, with the majority published after 2002.

*Journal discipline.* Criminology was the most common discipline of the journals in which the included articles were published: 35% (n=12) of the articles were published in a criminology journal. Other common journal disciplines were women's studies (n=5), violence (multidisciplinary) (n=5), and economics (n=4).

*Study design.* Of the 25 quantitative studies, nearly half (n=12) were longitudinal, population-level analyses using data from multiple jurisdictions over time. Of the remaining 13

quantitative studies, eight were cross-sectional, three were randomized trials, one was a cohort study, and one was a simulation. Nine studies were qualitative.

*Study setting.* About 24% (n=8) of the included studies used a national sample and another 21% (n=7) used a study sample that covers many states, metropolitan statistical areas, or cities. The remaining studies, excluding the simulation study, were focused on one to five jurisdictions, spanning various regions across the country.

*Exposures.* The majority of included articles (53%, n=18) assessed the impacts of warrantless arrest laws for IPV as the IPV policing exposure, typically operationalized as mandatory arrest laws alone or as a combination of mandatory, preferred, and discretionary arrest laws. For an additional four articles, the exposure of interest was the individual arrest of the abusive partner. Other common exposures included measures of general police contact or interactions for an IPV incident (n=3) and police force size/staffing (n=3).

*Outcomes.* The most common survivor-related outcomes of IPV policing analyzed in the included studies were criminalization of survivors (n=11), various measures of survivors' experience or satisfaction with police (n=10), population-level measures of IPV or other domestic violence homicide rates (n=7), willingness to or likelihood of reporting IPV to police (n=4), and population-level measures of IPV victimization other than homicide rates (n=3).

*Racial analysis.* The majority of studies (71%, n=24) did not include an analysis of differences in the consequences of IPV policing by racialized groups, despite evidence of pronounced racialized disparities in impacts of IPV victimization and general exposure to policing (40,78–80). Just ten studies included a racial analysis, which typically involved a stratified analysis or test for interaction in quantitative studies or a thematic focus on impacts of IPV policing for specific racialized groups in qualitative studies.

**Table 1: Characteristics of included studies**

First author	Year	Journal discipline	Study design	Study setting	Exposure(s)	Outcome(s)	Racial analysis	Findings summary
Yegidis	1994	Social work	Cross-sectional	4 IPV shelters in FL county	Police interactions under preferred arrest policy	Experience with police	Yes	Discrepancy between survivor wishes and police action; Black women less satisfied with police response than white women
Erez	1998	Violence (multidisciplinary)	Qualitative	2 OH counties	Police contact	Experience with police	No	Survivors wish to retain autonomy in attempts to stop abuse
Stephens	2000	Criminology	Qualitative	Semi-rural NY county	Abusive partner arrest under new mandatory arrest law	Experience with police	No	Survivors wish to retain autonomy in attempts to stop abuse
Dugan	2002	Criminology	Longitudinal, population-level	48 large US cities	Warrantless arrest laws	IPV homicide rates	Yes	Mandatory arrest associated with lower white femicide rate, though not robust to city-dependency test
Bui	2003	Women's studies	Qualitative	4 US cities	Police contact	Help seeking decisions	Yes	Experiences of survivors and help-seeking behaviors are complex
Dugan	2003a	Criminology; Public policy	Longitudinal, population-level	National	Warrantless arrest laws	Household violence; reporting to police	No	Mandatory arrest associated with lower odds of spousal violence and informing police; no effect on family or dating violence
Dugan	2003b	Law	Longitudinal, population-level	48 large US cities	Warrantless arrest laws; local pro-arrest policies; local police commitment index	IPV homicide rates	Yes	Pro-arrest law and policies associated with lower IPV homicide for unmarried subgroups; Local specialized unit and training associated with higher IPV homicide for Black unmarried women
Miller	2003	Criminology	Randomized trial	Miami-Dade County, FL	Abusive partner arrest	Willingness to call police; satisfaction with police; feelings of safety; feelings of agency	No	Arrest positively related to perception of safety, negatively related to perception of legal power, no effect on perception of personal power

Hickman	2003	Law	Cohort	Miami-Dade County, FL	Victim preferred arrest outcome	Reporting to police	No	Victim preference associated with increased reporting to police
Wilson	2004	Criminology	Cross-sectional	National	Abusive partner arrest; whether victim reported incident to police	Satisfaction with police	No	Arrest associated with satisfaction with police; victim reporting incident associated with lower satisfaction vs. someone else reporting
Shim	2005	Violence (multidisciplinary)	Qualitative	New York metropolitan area	Arrest policies; victim exposure to arrest	Any consequence of being arrested	Yes	Experiences of survivors and help-seeking behaviors are complex
Rajah	2006	Women's studies	Qualitative	New York City	Warrantless arrest laws	Victim identity challenges	Yes	Documented negative consequences of mandatory arrest policies for survivors
Salazar	2007	Violence (multidisciplinary)	Longitudinal, population-level	2 GA counties	Coordinated community response aiming to increase IPV legal sanctions	Female exposure to criminal legal system	No	Intervention associated with increased in female arrests
Hirschel	2007a	Criminology	Cross-sectional	19 states	Warrantless arrest laws	Dual arrests	No	Odds of dual arrest higher in mandatory vs. discretionary agencies
Frye	2007	Violence (multidisciplinary)	Cross-sectional	New York City	Warrantless arrest laws	Arrest outcome	Yes	Of cases reviewed, 5% were unwanted arrests, 9% were dual arrests, 24% were retaliatory arrests; Female arrest was more likely when partner was white
Hirschel	2007b	Criminology	Cross-sectional	National	Warrantless arrest laws	Dual arrests	No	Mandatory arrest laws increased likelihood of dual arrest for all relationship categories; Same sex couples more likely to experience dual arrest
Schwartz	2009	Sociology	Longitudinal, population-level	National	Triangulation of crime and violence data sources	Arrest gender gap	No	No evidence that underlying violence by women is increasing; Increases in women's arrests more likely

driven by increasing pro-arrest policies

Iyengar <sup>2</sup>	2009	Economics	Longitudinal, population-level	National	Warrantless arrest laws	IPV homicide rate; family homicide rate	Yes	Mandatory arrest laws associated with 60% increase in IPV homicide rate; Effect stronger among Black people
Hovmand	2009	Methods	Simulation	N/A	Warrantless arrest laws	Victim arrests	No	Mandatory arrest laws associated in increase in victim arrests
Zeoli	2010	Health	Longitudinal, population-level	46 large US cities	Warrantless arrest laws; laws reducing firearm access in IPV cases; police staffing levels	IPV homicide rate; IPV firearm homicide rate	No	Mandatory arrest laws not associated with IPV homicide rate or firearm homicide rate; Warrantless arrest laws associated with decrease in IPV homicide rate and firearm homicide rate; Police officers per capita negatively associated with both outcomes
Zeoli	2011	Public policy	Longitudinal, population-level	46 large US cities	Warrantless arrest laws	IPV homicide rate	No	Law classification schemas influence estimated effects; For one classification, mandatory arrest laws associated with decrease in total IPV homicide rate and male IPV homicide rate; Using 3 other classifications, no effects found
Leisenring	2011	Sociology	Qualitative	2 jurisdictions in Western state	Warrantless arrest laws	Victim identity challenges	No	Documented negative consequences of mandatory arrest policies for survivors
Xie	2012	Criminology	Longitudinal, population-level	40 large MSAs	Warrantless arrest laws; police force size; social service staff size	IPV victimization	Yes	No effect of mandatory arrest laws on IPV victimization; Police officers and social service workers per capita associated with lower IPV victimization

<sup>2</sup> Corrigendum to Iyengar 2009 issued in 2019 noting potential but unverified coding error

Durfee	2012	Women's studies	Cross-sectional	28 states and DC	Warrantless arrest laws	Arrest outcomes	No	Mandatory arrest positively associated with female arrest and dual arrest
Dichter	2013	Criminology	Qualitative	Large East Coast city	Victim arrest as result of IPV police call	Any consequence of being arrested	No	Documented negative consequences of mandatory arrest policies for survivors; Need for new IPV response involving alternatives to arrest
Brame	2015	Criminology	Randomized trial	Lexington County, SC	Proactive enforcement of no-contact orders	Victim experiences of aggression, knowledge, safety, and contact	No	Intervention associated with higher levels of stalking/threatening behaviors and experiences of physical aggression, not associated with survivor knowledge or safety
Novisky	2015	Women's studies	Cross-sectional	5 IPV shelters in Midwestern state	Victim support for mandatory arrest policy	Reporting to police	No	Victim support for mandatory arrest positively associated with likelihood of reporting to police
Li	2015	Criminology	Qualitative	IPV shelter in Northeastern state	Victim arrest for IPV charge	Any consequence of being arrested	No	Experiences of survivors and help-seeking behaviors are complex; Need for new IPV response involving alternatives to arrest
Sherman	2015	Criminology	Randomized trial	Milwaukee, WI	Abusive partner arrest	Victim mortality	Yes	Victim mortality rate higher among people whose partner was arrested; Effect concentrated among Black participants
Hirschel	2017	Women's studies	Cross-sectional	4 states	Primary aggressor laws	Single and dual arrests	No	Primary aggressor laws negatively associated with likelihood of dual arrest
Dasgupta	2018	Economics	Longitudinal, population-level	National	Warrantless arrest laws	Domestic violence homicide rates; youth mental health and behavioral outcomes	No	No effect of warrantless arrest laws on domestic violence homicide rates; No effect on 4 of 5 youth behavioral outcomes; Warrantless arrest laws negatively associated with youth suicidal ideation

Chin	2019	Economics	Longitudinal, population-level	National	Warrantless arrest laws	IPV homicide rates	No	No effect of mandatory and preferred arrest laws on homicide rates; Discretionary arrest laws associated with decrease in current spouse homicide and increase in common-law spouse homicide
Cook	2019	Economics	Longitudinal, population-level	National	Warrantless arrest laws	IPV intimidation and assault rate	No	Mandatory arrest laws negatively associated with intimidation rate only when unemployment rates between 4.75% and 8.5%; No effect on assault rate
Gezinski	2020	Violence (multidisciplinary)	Qualitative	Utah	Warrantless arrest laws	Experience with police	No	Documented negative consequences of mandatory arrest policies for survivors; Need for new IPV response involving alternatives to arrest

Exposures	Outcomes																
	Population-level IPV homicide rate	Other population-level IPV victimization rate	Reporting of IPV to police	Survivor criminalization	Survivor experience with police <sup>3</sup> or feelings of safety	Police violence	Child welfare involvement	Other public health outcomes									
Warrantless arrest laws	L	L	L	L	L	L	X	X	Q	Q							L
	L	L					X	S									
	L																
Individual abusive partner arrest						R			R	X	Q						R
Police force size/staffing	L	L															
Other																	

L = Longitudinal, R = Randomized trial, C = Cohort, X = Cross-sectional, Q = Qualitative, S = Simulation  
 population-level

Note: Studies with multiple exposures and/or outcomes may be reflected more than once in the figure

Figure 2: Graphical summary of included articles by study design, exposure, and outcome

<sup>3</sup> Survivor experience with police includes challenges with identity work

### *Graphical summary and evidence gaps*

Figure 2 provides a graphical summary of the studies by study design, exposure, and outcome, elucidating important patterns and gaps in the evidence base. First, I found that the studies evaluating population-level IPV victimization outcomes were focused on population-level exposures, such as warrantless arrest laws and police force size/staffing. These studies all used longitudinal data. As noted above, of the included articles, the most studied outcome of IPV policing was survivor criminalization, often dual arrest or survivor arrest specifically. However, many of these studies used cross-sectional data, limiting the ability for valid causal inference. There have been only two longitudinal studies focused on survivor criminalization as a negative consequence of IPV policing, and in those studies the IPV policing exposure of interest was (1) a specific coordinated community response involving increased IPV legal sanctions in two Georgia counties (81) and (2) the triangulation of various population-level crime and violence data sources (82). There have been no longitudinal studies to date on the impact of warrantless arrest laws on survivor criminalization.

An additional notable gap in the evidence base is the lack of studies on police violence and child welfare involvement as outcomes of IPV policing. In a 2015 national survey of anti-violence service providers, advocates, and attorneys, one-third of respondents reported that police sometimes or often used inappropriate force against survivors of sexual assault and domestic violence and 89% of respondents reported that contact with police sometimes or often resulted in involvement with child protective services (62). Additionally, in two of the qualitative studies included in this review, study participants reported that police threatened to remove children from their home or reported actual investigation or involvement with child protective services (83,84). These findings will be discussed further in the following section. Despite

existing evidence that police violence and child welfare involvement are common consequences of IPV policing, no research to date on IPV policing has explicitly focused on these outcomes.

Finally, Figure 2 shows that there have only been two studies published on the generalized health consequences of IPV policing beyond IPV revictimization. Those two studies focused on youth mental and behavioral health outcomes (85) and victim all-cause mortality (86). This lack of research concerning health-related consequences of IPV policing may be reflective of the disciplinary focus of past research on criminology with little to no research in the public health field. There is a large body of evidence demonstrating the public health consequences of systems of criminalization and incarceration (87–91). Therefore, it is likely that IPV policing practices and policies that lead to increased criminalization and incarceration would have generalized public health consequences, and such consequences should be explored further in the IPV literature.

### *Summary of findings*

#### Quantitative studies grouped by outcome type

*Survivor criminalization.* As mentioned above, the majority of research on survivor criminalization as a negative consequence of IPV policing consists of cross-sectional studies of mandatory arrest laws. There is also one simulation study of mandatory arrest laws. These studies all provided evidence that mandatory arrest laws are associated with an increased risk of survivor criminalization, measured as higher rates of female arrests, victim arrests, and dual arrests (49,92–95). A longitudinal study evaluating the effects of a coordinated community response to IPV with the goal of increasing legal sanctions against male abusive partners found that the intervention also increased the rate of female arrests (81). A study of the impact of primary aggressor laws, which require police to determine the primary aggressor in an IPV

dispute before making an arrest (96), on the risk of dual arrest in mandatory arrest jurisdictions found that dual arrest was less likely than single arrest in states with primary aggressor statutes (97). Finally, Schwartz et al. (82) triangulated multiple data sources (e.g., arrests, convictions, victim-based reports of crime) to explore whether a narrowing of the gap between men and women violent crime arrest rates was driven by women committing more violent offenses or by changes in policing practice/mobilization of law, such as through mandatory arrest laws, and findings strongly supported the latter theory of police widening the net of criminalization.

*IPV and domestic violence homicide rates.* There have been seven studies using large, longitudinal datasets to assess the effect of warrantless arrest laws on rates of IPV homicide. With respect to mandatory arrest laws, four of these seven longitudinal studies demonstrated no significant effect of mandatory arrest laws on IPV homicide rates in most analytic models (30,85,98,99). Two of those studies used an observational time-series approach and focused on large U.S. cities only (98,99) while the other two conducted national state-level difference-in-differences analyses (30,85). In a longitudinal time-series study, Dugan (100) found that mandatory arrest laws specifically for violations of orders of protection were associated with lower rates of white femicide but had no effect on Black femicide over the study period. The author additionally notes, however, that the effect on white femicide was not robust as it was dependent on the inclusion of specific cities in the dataset. In a difference-in-differences study, Iyengar (101) found that mandatory arrest laws were in fact associated with an increase in the IPV homicide rate, however Chin and Cunningham (30) identified potential coding errors in Iyengar's analysis, which could not be verified. The evidence base generally suggests there is no effect of mandatory arrest laws on IPV homicide rates and discrepancies in the classification and recorded timing of mandatory arrest statutes may explain disparate findings (99).

With respect to exposures other than mandatory arrest laws, Chin and Cunningham (30) found evidence that discretionary arrest laws are associated with a decrease in spousal IPV homicide rates. Dugan et al. (60) assessed associations between four relevant exposures (warrantless arrest laws, mandatory arrest laws, local pro-arrest policies, and specialized police training or staffing) and intimate partner homicide rates among 12 victim groups (defined by combinations of race, gender, and marital status). The authors found that state and local pro-arrest policies were associated with lower intimate partner homicide rates among multiple unmarried subgroups (60). They also found that local police agencies with a specialized domestic violence unit or specialized training were associated with higher homicide rates for unmarried Black women (60).

*Other IPV and domestic violence victimization rates.* Three longitudinal studies using national data focused on how mandatory arrest laws and other measures of IPV policing impact rates of violent victimization other than homicide (31,32,102) and the findings were somewhat inconsistent. Xie et al. (31) found that mandatory arrest laws had no effect on IPV victimization rates among women. Meanwhile, Dugan (32) found that mandatory arrest laws that specifically require arrest for violations of orders of protection were associated with lower odds of spousal violence but not family or dating violence. When assessing interaction by county unemployment rates, Cook et al. (102) found that mandatory arrest was associated with lower rates of intimidation at unemployment rates roughly between 4.75% and 8.5%, but not at lower or higher rates of unemployment. The authors found no effect of mandatory arrest laws on assault rates across any level of unemployment.

Xie et al.'s (31) analysis was a multilevel analysis using data from 40 metropolitan statistical areas over a 16-year period (1989-2004). Cook et al.'s (102) analysis was conducted at

the county level over a more recent 16-year period (2000-2015); however, the authors did not specify how many counties were included in the analysis. Dugan's (32) analysis was a national household-level analysis over a 6.5-year period (January 1992-June 1998) using data from over 500,000 households, however, the author did not account for clustering by household given the repeated measures over time. Importantly, Cook et al. (102) measured the outcome of IPV victimization using crime data collected and reported by the Federal Bureau of Investigation (FBI), thereby only representing victimization reported to police. This could bias the findings given that mandatory arrest laws may result in underreporting of IPV to police (as discussed below). Xie et al. (31) and Dugan (32) used data from the National Crime Victimization Survey, which is still subject to reporting bias but is expected to be a more valid measure of victimization than crime data (32). Given the predominantly null findings and limitations of the studies, there is insufficient evidence to support claims that mandatory arrest laws are protective for generalized IPV victimization prevalence. Xie et al. (31) additionally found that metropolitan statistical areas (MSAs) with more police officers per capita and more social service workers per capita had lower rates of IPV victimization.

*Survivors' experience with police.* The evidence base on IPV survivors' experience with police is heterogenous and lacks consistency across constructs and measures. The literature includes two randomized trials, one testing the effects of individual arrest of the abusive partner (103) and the other testing the effects of proactive enforcement of no-contact orders (104). The first randomized trial showed that abusive partner arrest was negatively associated with survivor perception of legal power (i.e., perceived agency in criminal legal decisions), positively associated with perception of safety, and not associated with perception of personal power (i.e., perceived control over economic and social resources) (103). These outcomes were all assessed

shortly after the police intervention (103). The second trial showed that proactive enforcement of no-contact orders—involving proactive contact between police and survivors to inform survivors of the requirements of the orders, advise them on how to collect evidence in case of a violation, and monitor compliance—was associated with higher levels of stalking/threatening behaviors and experiences of physical aggression (104). Proactive enforcement of no-contact orders was not associated with greater survivor knowledge about the no-contact order or survivor safety (104). Consistent and validated measures of survivor experiences with and attitudes toward IPV policing are needed.

*Reporting of IPV to police.* Four studies evaluated the rate of reporting of IPV to police as a consequence of IPV policing. One used nationally representative data to assess the association between mandatory arrest laws for violation of protection orders and informing police of IPV and demonstrated that mandatory arrest laws for protection order violations were associated with lower odds of informing police (32). Two of the four studies used victim interview data from the Spouse Assault Replication Program randomized trial in Miami-Dade County (103,105). One study demonstrated no association between the abusive partner being randomly assigned to arrest and the survivor’s willingness to call police in the future (103) and the other found that survivors receiving their preferred arrest outcome (arresting the abusive partner vs. not arresting) were more likely to report future victimization to police (105). Finally, a cross-sectional study of women in domestic violence shelters in a Midwestern state found that survivors who supported mandatory arrest policies had higher odds of reporting IPV to police, though this association may be the result of reverse causation given the use of cross-sectional data (106). Together, the limited evidence suggests that mandatory arrest laws at the population

level are associated with decreased reporting to police, while at the individual level, survivors having agency in the arrest decision may lead to increased reporting.

*Other outcomes.* Two studies evaluated other public health outcomes, namely victim all-cause mortality and youth mental and behavioral health outcomes (85,86). The first was a 23-year follow-up of the Spouse Assault Replication Program randomized trial in Milwaukee (86). The authors found the rate of all-cause mortality among IPV victims whose partners had been randomly assigned to arrest was 64% higher than the rate among victims whose partners were not assigned to arrest. The second study included a multi-level analysis of warrantless arrest laws and five youth mental and behavioral health outcomes (85). The authors found no effect of warrantless arrest laws on four of the five outcomes; warrantless arrest laws were associated with a 5.1% reduction in the probability of youth suicidal ideation. In gender-stratified analysis, this effect was present among girls but not boys.

### Qualitative studies

Four of the nine qualitative studies included in this review focused on the experience of survivors who were wrongfully arrested or the challenges survivors face in successfully presenting themselves to police as the person experiencing violence, referred to as “identity work,” under mandatory arrest policies (83,107–109). The studies detailed the psychological and emotional trauma of being wrongfully arrested as well as the material consequences of arrest such as incurred expenses, loss of employment, involvement from child protective services, and loss of child custody (83,107–109). In two of the studies, some participants reported that one positive consequence of the arrest was that it served as a catalyst for them to leave the abusive relationship (83,109). The other qualitative studies primarily focused on survivors’ experiences and interactions with police (84,110–113). Common themes included: 1) inadequate police

response involving victim-blaming and verbally abusive language; 2) hesitancy toward calling police because survivors relied on the abusive partner financially or feared retaliation from family or the abusive partner; 3) lack of autonomy and choice; and 4) survivors having difficulty navigating the criminal legal system and feeling that the abusive partner was better able to manipulate the system in their favor.

Together, the authors of these nine qualitative studies drew a number of shared conclusions based on their findings. First, the experiences of IPV survivors and their help-seeking behaviors are complex, shaped by both individual factors as well as structural and cultural forces (109,111,112). Second, authors confirmed that there are empirically verified negative consequences of mandatory arrest policies for survivors, including failed identity work, criminalization, and arrest (83,107,108,113). Third, there is an urgent need for new emergency intervention services for IPV reduction and prevention that include alternatives to arrest (83,109,110,113). Finally, authors concluded that survivors wish to retain some autonomy in their attempts to stop the experience of abuse and that IPV interventions must be informed by survivor voices (84,110).

#### Studies with a racial analysis

As mentioned above, ten studies included a racial analysis of the negative consequences of IPV policing. Three of these ten studies were qualitative, assessing how the experience of police contact or survivor arrest varied for specific racial/ethnic groups and how survivors of color faced unique challenges (107,111,112). The majority of the quantitative studies that included a racial analysis assessed if the effect of IPV policing on survivor outcomes differed among Black survivors compared with white survivors (31,60,86,100,101,114). Three studies found that the negative effects of IPV policing or mandatory arrest laws were more pronounced

among Black survivors than white survivors (86,101,114) and similarly, as discussed above, one found that mandatory arrest laws were associated with lower rates of white femicide but had no effect on Black femicide (100). Two studies found no significant differences in the effect of state mandatory arrest laws on homicide rates or victimization rates by race, respectively (31,60). Together, the evidence suggests that the negative consequences of IPV policing may be more pronounced among Black survivors than white survivors.

#### **1.4 Discussion**

In this scoping review, I summarized the evidence base concerning the negative consequences of IPV policing that extend beyond individual revictimization. I found that survivor criminalization is the most studied negative consequence of IPV policing and the existing evidence suggests that the police response to IPV has increased the risk of survivor arrest. However, the evidence is primarily limited to cross-sectional studies; additional longitudinal studies would strengthen understanding of the causal nature of this relationship. I also found that there have been numerous rigorous studies of the effects of mandatory or other warrantless arrest laws on population-level measures of IPV victimization, including IPV homicide rates. The evidence to date is inconclusive given that findings have been conflicting, likely due in part to a lack of consensus concerning the classification of warrantless arrest laws as discretionary, preferred, or mandatory and their effective dates. An additional key finding is that there is a dearth of research on police violence, child protective services involvement, and health outcomes as potential consequences of IPV policing. Finally, only 29% of studies included a racial analysis of the negative consequences of IPV policing. Those that did provided evidence to suggest that the harms of the police response to IPV may be more pronounced among Black survivors compared with white survivors. This finding is consistent with the

substantial body of evidence documenting the racially disparate harms of policing and criminalization generally (8,78,79,115).

This scoping review provides the first-ever summary of the negative consequences of IPV policing beyond individual IPV revictimization, which allows for a more informed and nuanced calculus concerning the costs and benefits of the police-centric response to IPV, and specifically mandatory arrest laws. The findings from this review should be considered alongside reviews and meta-analyses on the individual-level effect of arrest vs. non-arrest on IPV revictimization or recidivism. Taken together, there is no consistent or compelling evidence that IPV policing reduces IPV victimization at the individual or population levels. Therefore, the evidence base does not support deterrence theory in the context of IPV policing. In addition, the present review demonstrates that (1) there are some established negative consequences of IPV policing, such as increased risk of survivor arrest, and (2) the negative consequences of IPV policing may have disparately harmful effects on Black survivors. Policymakers should consider this evidence when evaluating the effectiveness of the current police-centric response to IPV.

The present review also highlights gaps in the evidence base. Specifically, additional research is needed on potential consequences of IPV policing such as police violence against survivors, child protective services involvement, and measures of the psychosocial and physical health of survivors. To date, the study of the police-centric response to IPV has primarily been the domain of criminology. Applying a critical public health perspective to the study of the police-centric response to IPV demands consideration of generalized negative consequences, expanding focus from narrow, short-term measures of recidivism and revictimization to broader measures of overall survivor health and well-being.

### *Limitations of the review and individual studies*

There are important limitations of the present scoping review. First, the existing research on the negative consequences of IPV policing is quite heterogenous making comprehensive synthesis of findings difficult. For example, numerous themes were interrogated in the qualitative research, all of which could not be summarized in depth. I discussed those themes that were common across multiple studies, however this process of identifying common themes across the qualitative studies was not systematic. Additionally, many of the quantitative studies used different constructs, measures, and classifications for common exposure and outcome variables, making clear synthesis of findings challenging. Most notably, efforts should be made to reach consensus on the classification of warrantless arrest statutes.

Second, this review only included peer-reviewed articles published in scholarly journals. There is certainly grey literature, including reports published by the U.S. Department of Justice and the American Civil Liberties Union, as well as case studies collected by IPV service provider organizations that offer valuable insight concerning the negative consequences of IPV policing. Those contributions to the evidence base are not covered in this scoping review.

Finally, while this scoping review did not include a formal appraisal of study limitations and risks of bias in accordance with scoping review guidelines, it is important to note some general limitations of the evidence base. Many studies used robust methods to test the effects of warrantless arrest laws, such as using a difference-in-differences approach to take advantage of the differential timing at which states passed laws and including a number of potential confounders that may have influenced both passage of the laws and violent victimization rates. However, many studies did not apply a causal inference framework to their analysis and either did not include important confounders that may result in a spurious relationship or included a

large number of predictors in their models without theoretical justification for their inclusion. Additionally, many studies used administrative data sources, such as the National Incident Based Reporting System (NIBRS) or the Supplementary Homicide Report (SHR), and did not evaluate the potential sources of selection bias and information bias that may result from the use of these data sources or discuss strategies to mitigate such sources of bias. These sources of administrative crime data generally only reflect IPV incidents that are reported to police and rely on good faith, consistent reporting from law enforcement agencies. The field of IPV research would benefit from increased scope and accessibility of administrative data sources beyond FBI data sources, such as the National Crime Victimization Survey, as well as investment in local primary data collection efforts from survivor cohorts. Lastly, it is important to note that a corrigendum was issued in reference to Iyengar's 2009 publication (101,116) due to a potential but unverified coding error.

### *Conclusion*

Given the growing prevalence of severe IPV in the U.S. and increasing calls to divest from policing and prisons as responses to social and public health problems, there is an urgent need to critically evaluate the current police-centric response to IPV, including the widespread use of mandatory arrest laws. Such carceral policies have established societal costs, given the racialized harms of mass criminalization and incarceration. To date, debate concerning the effectiveness of the policing response to IPV and mandatory arrest laws has primarily focused on its specific deterrent effect in the prevention of short-term individual-level revictimization. Not only do the data suggest no effect of arrest on individual IPV revictimization (59), but there is now increased understanding of the broader negative consequences of this response. The present review summarizes this emerging evidence base and highlights areas for further investigation.

Potential generalized harms of the police-centric response to IPV that may extend beyond the risk of revictimization, and the risk that such harms might disproportionately impact Black survivors, must be considered in critically evaluating our nation's response to IPV.

## **Chapter 2: The effect of warrantless arrest laws for intimate partner violence on women's mortality (20-54 years): A difference-in-differences analysis**

### **2.1 Introduction**

In this chapter, I consider the generalized and race-specific health effects of warrantless arrest laws for survivors of IPV. In its introductory section, I review the sociopolitical circumstances that gave rise to the increased popularity of warrantless arrest laws for IPV; discuss past and present debate concerning the effectiveness of warrantless arrest laws; and present a theoretical framework for how warrantless arrest laws, and mandatory arrest laws in particular, are expected to produce generalized negative health consequences for survivors of IPV, particularly Black survivors. I then discuss the approach of the present study to estimate such generalized health consequences.

#### *The rise of warrantless arrest laws for IPV*

IPV response efforts in the U.S. have shifted dramatically over the last several decades. Historically, IPV was considered a private matter and survivors of violence had little recourse, despite legislation criminalizing IPV (2). In the 1970s, the nation's response to IPV began to fundamentally shift. In response to coinciding grassroots women- and survivor-led movements in the 1970s, IPV was redefined in the eyes of the law as a public safety concern as opposed to a private matter (45). A primary focus of many grassroots movements was to secure funding to build and support shelters that offered housing for survivors escaping abusive relationships and provided counseling, advocacy, and childcare services (56,117). At the same time, there was a national bipartisan movement to use law enforcement to respond to a growing list of social

problems and to increase the use of aggressive policing practices. Many mainstream feminist organizations viewed this as an opportunity to fight for a stronger criminal legal response to IPV (47). Today, the U.S. response to IPV is predominantly criminal legal system-focused and one of its core components is mandatory arrest legislation, which allows the least police discretion of the three types of warrantless arrest laws for IPV.

Mandatory arrest laws can be implemented at the state or local levels and require police to make an arrest in response to calls for IPV regardless of survivor wishes. Not only does this allow police to make warrantless arrests in misdemeanor cases of IPV, but it also removes police discretion in any case where there is probable cause to believe that an IPV offense has been committed (49). Mandatory arrest laws are one of three types of warrantless arrest laws for IPV, the other two being preferred arrest and discretionary arrest. These three types of laws can be conceptualized on a spectrum of police discretion, with discretionary arrest allowing police officers full discretion regarding whether or not to make an arrest (“may arrest”) and mandatory arrest allowing officers little to no discretion (“shall arrest”) (99). Preferred arrest laws fall in the middle of the spectrum, allowing some police discretion but indicating that arrest is preferred by the state.

In addition to the sociopolitical environment of the 1980s that favored tough-on-crime responses to social and public health problems, there were two acute events in 1984 that contributed to the nationwide rise of mandatory arrest laws. The first was a multi-million-dollar lawsuit against the police department of Torrington, Connecticut due to police negligence in a case of IPV. Tracey Thurman suffered a violent assault by her ex-husband after repeated inaction by the local police (118). After this lawsuit, police departments and local governments across the country began viewing IPV cases as potential liabilities (119). The second event in 1984 was the

publication of results from the Minneapolis Domestic Violence Experiment (MDVE) (23). As mentioned in Chapter 1, the MDVE was a landmark randomized controlled trial (RCT) designed to estimate the deterrent effect of arrest for IPV on subsequent IPV perpetration, and was the first scientifically controlled test of the effect of arrest for any crime (120). The findings of this trial showed that men who were arrested for IPV had reduced likelihood of reoffending after six months. As a result of these findings, the 1984 U.S. Attorney General's Task Force on Family Violence recommended that arrest be the preferred response to family violence (24) and declared there was sufficient evidence for the national implementation of pro-arrest domestic violence policies (45). By 1989, over 75% of jurisdictions across the country changed their domestic violence laws to allow for warrantless arrests for domestic violence misdemeanors (45). Many of these jurisdictions enacted the strongest type of warrantless arrest laws, mandatory arrest laws: by 1992, 15 states and the District of Columbia adopted mandatory arrest laws for domestic violence (25).

*Ongoing debate concerning the effectiveness of warrantless arrest laws*

Since the rise of mandatory and other warrantless arrest laws, there has been ongoing debate concerning their effectiveness in protecting the safety of IPV survivors. Immediately following the publication of the findings of the MDVE and widespread adoption of mandatory arrest laws, considerable criticism was raised about the use of a single study to inform nationwide policy changes (121,122). The National Institute of Justice funded five additional replication studies known as the Spouse Assault Replication Program, which were completed in Charlotte, Colorado Springs, Miami-Dade, Milwaukee, and Omaha (24,28,29,57,58). The replication studies produced inconsistent findings and failed to corroborate the results from the original MDVE. What is more, the authors of the original MDVE argued against the enactment

of mandatory arrest laws, which removed police officer discretion (23). Additionally, beginning in the 1990s, researchers have published multiple qualitative studies documenting survivor experiences with police responses under mandatory arrest laws and elevating survivor voices. As reviewed in Chapter 1, this body of qualitative research demonstrates how, for decades, survivors have expressed their desire to retain choice and autonomy in their efforts to stop the experience of IPV (84,110).

More recently, mandatory arrest laws have played a central role in local elections and nationwide reckonings within domestic violence coalitions and advocacy organizations. For example, in the 2021 election for the Manhattan District Attorney, mandatory arrest laws were a topic of heated debate and framed as a women's safety issue. Candidates who opposed mandatory arrest laws and other tough-on-crime policies for IPV were depicted in campaign advertisements as being anti-women (123). In June 2020, during international uprisings in support of Black lives in light of numerous highly publicized police murders of Black people, 47 state and territorial domestic violence coalitions signed on to a "Moment of Truth Letter" (33). The letter named the ways in which the mainstream anti-domestic violence movement had failed to be responsive to the needs of Black, Indigenous, and other survivors of color. The letter also expressed support for "addressing mandatory arrest" and its role in criminalizing survival. Reactions to this letter were mixed and some local law enforcement groups withdrew support of domestic violence coalitions that signed on to the letter (124,125). Clearly, the effectiveness of warrantless arrest laws and their negative consequences remain a contentious issue today.

#### *Evidence of population health effects of warrantless arrest laws*

As discussed in Chapter 1, the findings from individual-level trials of the effect of arrest on subsequent IPV incidents have been synthesized in meta-analyses, the most recent of which

demonstrated no effect of arrest on subsequent IPV victimization (59). But warrantless arrest laws are population-based interventions (often enacted at the state level) that impact entire systems, not just individual couples. For example, they may influence reporting behaviors among survivors and bystanders, tactics used by abusive partners, and IPV resource and service landscapes. Results of individual-level randomized trials do not provide evidence related to the population effects of enacting statewide policies, such as warrantless arrest laws.

Mandatory arrest laws and other warrantless arrest laws are ostensibly designed to reduce population levels of IPV victimization through the deterrence of future IPV incidents. However, as summarized in Chapter 1, the existing population-level observational research evaluating the effect of mandatory arrest laws on IPV victimization rates and IPV homicide rates has provided no consistent evidence of an effect. With respect to IPV victimization rates, Xie et al. (31) and Cook et al. (102) found no association between mandatory arrest laws and IPV victimization/assault rates among women. Dugan (32) found that mandatory arrest laws specifically for protection order violations were associated with lower odds of spousal violence but not dating violence. In the same study, Dugan (32) also found that mandatory arrest laws for protection order violations were associated with lower odds of reporting IPV to police, which could be an indication of decreased help-seeking. With respect to IPV homicide rates, the majority of population-level studies using large, longitudinal datasets demonstrated no significant effect of mandatory arrest laws on IPV homicide rates (30,85,98,99). Thus, existing evidence does not suggest that mandatory arrest laws reduce population levels of IPV victimization and may also reduce reporting of (and therefore help-seeking for) IPV victimization.

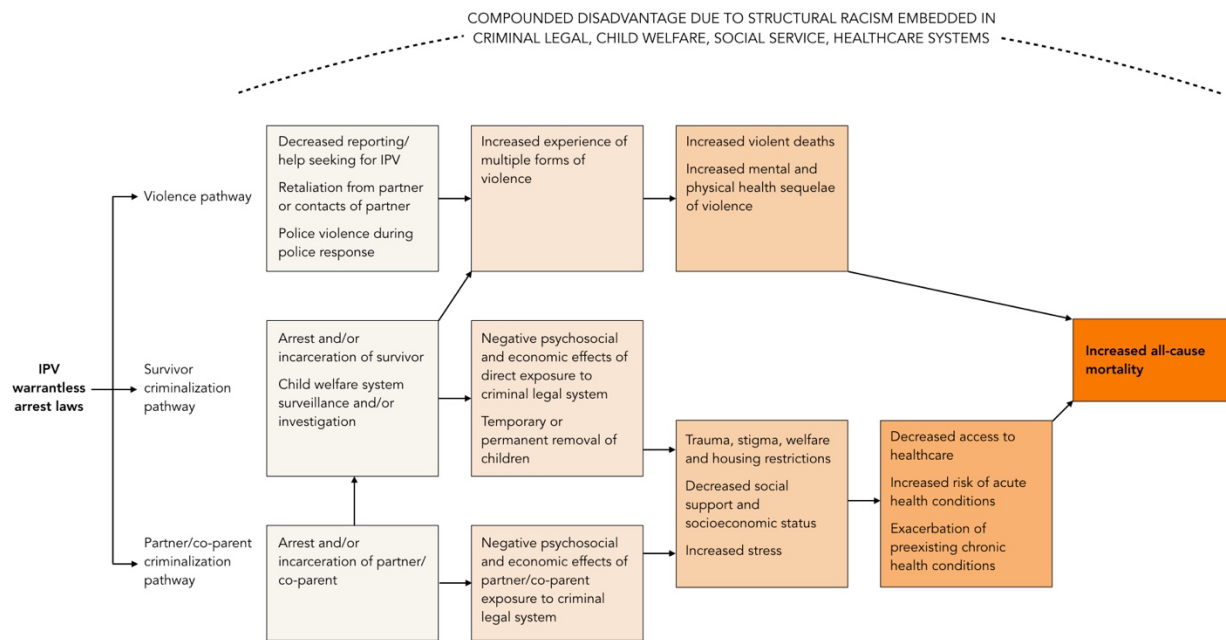
In addition, population-level research has identified negative consequences of mandatory arrest laws for survivors that extend beyond the risk of revictimization. For example, Chapter 1 presents a body of evidence focused on mandatory arrest laws as a driver of the criminalization of survival, as measured through female arrest rates, survivor arrest rates, and dual arrest rates (49,92–95). Moreover, there are theoretical reasons and limited evidence from survey research to suggest that there are additional negative consequences of mandatory arrest laws. Specifically, many survivor-led organizations (126–128) have called attention to the following additional consequences: police violence against survivors during police responses, surveillance and intervention by child protective services, and economic and psychosocial consequences of having a partner incarcerated.

These negative consequences may lead to worse generalized health and safety outcomes for survivors than if mandatory arrest laws were not in place. One of the most fundamental indicators of generalized health consequences in epidemiologic research is all-cause mortality. Mortality rates are also not subject to potential underreporting of IPV victimization caused by mandatory arrest laws. As such, the negative consequences of mandatory and other warrantless arrest laws may be detected as an increase in all-cause mortality for survivors. In addition, these consequences are expected to disproportionately impact socially marginalized groups, particularly Black women, because of the ways in which policing, and the criminal legal system more broadly, disproportionately harms Black communities and other communities of color (63,129–131). Notably, while not the focus of this research study, immigrant women and families also likely face disproportionate harms from mandatory arrest laws given racialized cooperation between the criminal legal and immigration systems (132). Studying the generalized health consequences of warrantless arrest laws and their potential role as drivers of racialized

health disparities would improve understanding of the impacts of these state-level interventions and, if generalized health consequences are empirically documented, strengthen movement demands for IPV interventions that are more responsive to the needs of survivors of color.

*Theoretical framework for generalized health consequences of warrantless arrest laws*

Figure 3 is a conceptual diagram depicting three theoretical pathways through which IPV warrantless arrest laws are expected to result in generalized health consequences for survivors and ultimately manifest in increased all-cause mortality. These pathways reflect mechanisms that are expected to be triggered shortly after enactment of warrantless arrest laws and/or exposure to an IPV police response, however the cascade of effects depicted in each pathway may take varying lengths of time to manifest. For example, impacts on chronic disease risk may have a longer latency than impacts on risk of violence.



**Figure 3: Conceptual diagram of potential mechanisms through which IPV warrantless arrest laws are expected to increase all-cause mortality for survivors**

## Violence pathway

First, the violence pathway focuses on the ways in which warrantless arrest laws could perversely lead to an increase in the frequency and/or severity of IPV and other forms of violence rather than a decrease. As demonstrated in Dugan's (32) research, mandatory arrest laws can exacerbate underreporting of IPV given that survivors may not want their partners to be arrested. This can lead to increased IPV as survivors are less likely to be connected with supportive services if their victimization goes unreported. There is also the risk that mandatory arrest laws increase violent behavior among abusive partners because policing and incarceration are often violent and/or traumatic exposures. The presence of police during an IPV incident can escalate the situation and the abusive partner can become more violent in a retaliatory response to the survivor having called the police on them (60). In cases of arrest, the abusive partner may be exposed to violence in jail or prison, which can increase mental health problems, drug use, and violence upon release, while the root cause of the violent behavior goes unaddressed (133). Even when abusive partners are incapacitated in jail or prison and physically unable to directly harm their intimate partners, they can use others to harass, threaten, or otherwise harm their intimate partners (134). Thus, it is plausible that warrantless arrest laws actually increase the frequency or severity of IPV for some survivors.

Another component of the violence pathway is the violence that survivors can experience at the hands of police who respond to calls for help. In a 2015 national survey of anti-violence service providers, advocates, and attorneys, one-third of respondents reported that police sometimes or often used inappropriate force against survivors of sexual assault and domestic violence (62,63). Additionally, IPV survivors are less likely than others to speak out about police violence because they may view police as their only option for assistance in the future and they

need police to remain willing to respond to future calls for help (63). Black survivors are likely at increased risk of police violence compared to white survivors given historical and ongoing state sponsored racialized and gendered violence committed against Black women at the hands of police (63,135). Together, these potential increased experiences of violence may contribute to increased violent deaths and other mental and physical health sequelae of violence, such as chronic pain, depression, post-traumatic stress disorder, and suicide (42,136).

#### Survivor criminalization pathway

The second pathway is the survivor criminalization pathway in which survivors are arrested, incarcerated, or otherwise criminalized as a result of warrantless arrest laws. As discussed above, research demonstrates that mandatory arrest laws lead to increased arrests of IPV survivors. This may occur for a number of reasons. Survivors often have the police called on them by their abusive partner as a form of retaliation or coercion (93) or by a bystander, and mandatory arrest laws lead to their arrest (63). Additionally, survivors may be arrested, prosecuted, and/or incarcerated for acts directly tied to their own survival. For example, survivors may be arrested in cases of self-defense, charged with “failure to protect” children if children are present when police are responding to an IPV incident, or arrested after being coerced into a criminal act by an abusive partner (61).

While it is not known how many incarcerated women have been incarcerated for acts of survival, the large majority of women in U.S. prisons have survived violence prior to incarceration: according to the ACLU, 79% of incarcerated women have reported past physical abuse and over 60% have reported past sexual abuse (137). Furthermore, reports suggest that as many as 90% of women incarcerated for killing a man previously experienced violence at the hands of that man (138). Women also receive harsher sentences on average for killing partners

who are men than men receive for killing partners who are women (138). Finally, evidence suggests that the criminalization of survival disproportionately impacts Black survivors. In a study of predictors of dual arrest for IPV incidents in the District of Columbia, researchers found that compared to “Black offender/White victim” couples, couples composed of two Black people had 2.4 times the odds of dual arrest (139).

Relatedly, survivors can also be criminalized through surveillance and/or investigation by the child welfare system. As previously mentioned, survivors can receive a criminal charge of “failure to protect” when police respond to cases of IPV and children are present in the household. Not only can a police response result in criminal charges for the survivor, but it can also result in a formal report to child protective services and the temporary or permanent removal of children from the home due to coordination between police agencies and child protective service agencies. Police are one of the primary reporting mechanisms to child protective services, producing about 20% of all child maltreatment reports (66). In the 2015 national survey of anti-violence advocates described previously, 89% of respondents reported that contact with police sometimes or often resulted in involvement with child protective services (62). Additionally, qualitative research among survivors has documented how police have involved child protective services or threatened to remove children when called to respond to IPV (83,84).

Again, surveillance and criminalization through the child welfare system is expected to disproportionately impact Black survivors. The racialized disparities within the child welfare system are stark: Not only are Black families substantially overrepresented in the child welfare system, but Black children are also more likely to be removed from their home and less likely to be adopted or reunified with their families compared with white children (65). A 1990s national study of the child welfare system found that children of color were more likely to be placed in

foster care and less likely to receive in-home services than white children, even when they shared the same problems and characteristics (140).

There is a robust body of evidence documenting the ways in which criminalization and incarceration are directly harmful to one's health. Incarceration is associated with lower life expectancy and higher rates of numerous acute and chronic health conditions among incarcerated people (141,142). Additionally, qualitative research has documented the profound psychological trauma that follows child removal for mothers, which is compounded by social costs (e.g., stigma) and civil disqualifications (e.g., housing, employment, and welfare restrictions) (143). These social costs and civil disqualifications can lead to premature mortality in numerous ways because they influence socioeconomic status and social support, which are considered root or "fundamental" causes of health (144,145). All of the systems driving the survivor criminalization pathway (the criminal legal system, the child welfare system, the social service system) are rooted in structural racism—targeting, regulating, and punishing Black families by design (8,65,146–149). Therefore, the racialized integration of these systems to drive survivor criminalization is expected to compound and exacerbate racialized disparities in criminal legal system involvement, poverty, social support, and health (149).

#### Partner/co-parent criminalization pathway

The final pathway describes the negative health effects that survivors may experience due to the arrest and/or incarceration of their abusive partner, who may play multiple roles in the survivor's life including romantic partner and/or co-parent. Mandatory arrest laws may increase mortality among survivors because survivors and their abusive partners often have strong emotional and financial ties, such that the arrest of the abusive partner may create a context of risk for the survivor (30,101,116,150). For example, partner incarceration results in a higher risk

of substance use, major depressive disorder, and AIDS for women (67–70). The survivor may also suffer from the social costs and civil disqualifications of their partner/co-parent’s incarceration, including stigma and restrictions on housing and employment opportunities. Again, these consequences may lead to premature mortality for the survivor because they would impact the survivor’s acute and chronic stress levels, socioeconomic status, and levels of social support. As described above, this pathway is also defined by integrated systems rooted in structural racism that will likely exacerbate existing racialized disparities in criminal legal system involvement, poverty, social support, and health.

Through these three theorized pathways, it is expected that IPV warrantless arrest laws have generalized negative health consequences for survivors that may manifest in increased mortality risk. Such generalized health consequences were documented in Sherman and Harris’ 2015 follow-up study of the Milwaukee Spouse Assault Replication Program RCT that was discussed in Chapter 1 (86). The researchers found that, 23 years after random assignment, people whose abusive partners were arrested had a risk of death due to all causes that was 64% higher than people whose abusive partners were warned and allowed to remain at home (86). Moreover, this finding disproportionately affected Black participants in the study: among the Black participants, arrest was associated with a 98% higher risk of death compared to a 9% higher risk among the white participants (86). Notably, the researchers found no differences between the treatment groups with respect to risk of homicide, the difference in mortality was reflected in differences in deaths due to heart disease and “other internal causes” (internal causes other than heart disease and cancer), suggesting that the hypothesized generalized health consequences of IPV policing may extend beyond risk of homicide (86). Beyond this study, there

is a pronounced lack of research examining the generalized public health consequences of warrantless arrest laws, particularly at the population level, as demonstrated in Chapter 1.

*Purpose of present study and approach*

The purpose of the present study is to fill this gap in the evidence base by: (1) estimating the negative health consequences of warrantless arrest laws for IPV survivors that may manifest in mortality for women aged 20-54 due to causes beyond intimate partner homicide alone; (2) exploring racialized differences in the negative health consequences of warrantless arrest laws; and (3) offering a population-level observational approach to further explore the findings from Sherman and Harris' individual-level randomized trial (86). The present study is limited to women aged 20-54 because early/mid-life mortality is a more proximate outcome than mortality in late life. There are specific causes of death that are likely to drive an association between warrantless arrest laws and mortality among women aged 20-54, such as homicide, suicide, and substance use. Therefore, I may primarily be testing the violence pathway in Figure 3 because death due to violence/injury is more likely to manifest in earlier life compared to death due to chronic disease. However, all pathways in Figure 3, including the survivor criminalization and partner/co-parent criminalization pathways, may involve causes of death that manifest in earlier life such as suicide and substance use-related death.

Despite, these specific causes of death being primary drivers of mortality among women aged 20-54, in this study I focus on all-cause mortality for two reasons, one practical and one substantive. First, the data completeness and accuracy for all-cause mortality is high and there is no risk of misclassification by cause of death. Second, the mechanisms between warrantless arrest laws and mortality are extremely complex, likely operating through pathways of compounded disadvantage and resource deprivation (Figure 3). Therefore, it is important not to

unnecessarily restrict the range of possible causes of death that may result from exposure to potential consequences such as police violence, arrest and incarceration, or child removal.

For example, warrantless arrest laws may lead to an increased risk of death due to unintentional injury among survivors if, as a result of the arrest of their partner, their own arrest, or engagement with child protective services, they experience a cascade of negative effects including working several jobs, spending more time driving in a car, high stress levels, lack of healthy sleep patterns, lack of consistent healthcare, etc. Similarly, this cascade of effects could lead to an increased risk of death due to diabetes among survivors, for example, if, due to financial consequences of having a partner arrested or being arrested themselves, they lack consistent access to insulin.

It is important to note that the present study is exploratory, and any detected effect is expected to be small because I am using all-cause mortality as a distal public health indicator that has myriad causes and I am including all women aged 20-54 as a proxy for IPV survivors. As an initial investigation, it is fruitful to determine if an association between warrantless arrest laws and all-cause mortality can be detected in this proxy population, before then aiming to tease out the specific mechanisms among a more precisely measured population of survivors, and using more proximate and precise outcomes. I hypothesize that there will be a small positive effect of mandatory arrest laws on all-cause mortality and that this effect will be stronger for Black women compared with white women.

## **2.2 Methods**

### *Design and setting*

The present study is a national retrospective analysis of state-level changes in IPV warrantless arrest legislation and women's mortality (20-54 years). I utilized a difference-in-

differences approach to take advantage of the differential timing at which various states adopted warrantless arrest legislation. A difference-in-differences approach controls for time-invariant baseline differences between states as well as secular trends in the outcome that do not correspond to the timing of legislation enactment. The aim is to thereby isolate the effect of warrantless arrest law enactment. To this end, I analyzed panel data measured at the state-year level and include data for all U.S. states and the District of Columbia. The analytic period covers 1980 through 2019. The analytic period begins in 1980 because the 1980s was the time period during which many states began enacting warrantless arrest laws and because covariate data are consistently available from 1980 onwards.

#### *Data sources and measures*

##### Women's mortality (20-54 years)

The outcome of interest was women's mortality rate (20-54 years). I obtained annual state-level data on women's all-cause mortality rates for women aged 20-54 from 1980-2019 from the National Vital Statistics System from the Centers for Disease Control and Prevention (CDC) Underlying Cause of Death database (151). I chose the outcome of mortality among women as a proxy for mortality among IPV survivors specifically because women experience a higher prevalence and severity of IPV compared to men and to be consistent with the existing research upon which I aim to build (86). I further restricted the outcome of women's all-cause mortality to women aged 20-54 because after reviewing the literature, I found that the majority of women who experience IPV and have a police response to their experience of IPV are roughly between the ages of 20 and 54 and because this exact age cutoff aligns with the data structure in the National Vital Statistics System across all years of interest (152–155). Based on National Crime Victimization Survey data, the average annual nonfatal IPV victimization rate for women

aged 20-24 is the highest among any reported age-gender group at roughly 11.3 per 1,000 and this incidence rate declines as age increases (156). Notably, this measure of mortality among women aged 20-54 will include some but not all people who have experienced IPV and will include women who have not experienced IPV. However, I posit that given the generally high prevalence of IPV among this age-gender group, an increase in mortality among people who have experienced IPV will be reflected in the mortality rate among this group. Additionally, this restriction to women aged 20-54 limits the analysis to mechanisms of premature/early mortality, which are more likely to be reflected in the violence pathway depicted in Figure 3. Longer acting mechanisms between exposure to warrantless arrest laws and late life mortality, which are reflected more in the remaining two pathways, are less likely to be captured in this analysis.

I also obtained race-stratified estimates of women's all-cause mortality for those aged 20-54, specifically for Black women and white women. This race-stratified outcome data allowed me to assess racialized differences in the relationship between IPV arrest legislation and women's mortality (20-54 years).

Finally, for a sensitivity analysis described below, I obtained measures of state-year homicide rates for women aged 20-54 from two different sources. The first source is the National Vital Statistics System from the CDC Underlying Cause of Death database. However, for state-years with fewer than 10 homicide counts among the population group of interest, the exact number of deaths due to homicide is suppressed in public National Vital Statistics System data. Therefore, the secondary source of homicide rates for women aged 20-54 was the FBI Supplementary Homicide Report, which includes data on all homicides as reported by law enforcement agencies across the country (157). These homicide data cover the period of 1980-2016. Notably, some jurisdictions do not report to the FBI Supplementary Homicide Report for

some years so certain state-years (4%) are excluded in this analysis. For state-years with complete homicide data from the National Vital Statistics System and the FBI Supplementary Homicide Report, the data from both sources were highly correlated (Pearson’s correlation coefficient = 0.98).

Enactment of warrantless arrest legislation

The exposure of interest was the type of IPV warrantless arrest law in a given state-year. All states in the U.S. currently have existing legislation concerning warrantless arrests for misdemeanor IPV incidents (or for a broader category of misdemeanor offenses) and historical information about these statutes is public record. However, there is some disagreement among legal scholars and researchers about classifying the statutes as discretionary, preferred, or mandatory arrest laws and about the effective year of the statutes. For this study, I reviewed four leading classification schemas (Miller, 2004 (158); Hirschel et al., 2007 (159); American Bar Association, 2011 (160); and Chin and Cunningham, 2019 (30)) and two leading schemas focused on the effective dates of the statutes (Zeoli et al., 2011 (161) and Chin and Cunningham, 2019 (30)). I compared these schemas against each other and identified any discrepancies. When discrepancies arose, I conducted my own archival review of the statute using WestLaw Edge (Thomson Reuters; Toronto, Canada) to make a determination.

Table 2 displays my final classification schema and the effective year for each statute. My classification schema and effective dates are equivalent to the recently published Chin and Cunningham schema (30) except in four cases. Those four cases are discussed in Appendix B.

**Table 2: Classification and effective year of IPV warrantless arrest statutes by state**

<b>State</b>	<b>Statute</b>	<b>Classification</b>	<b>Effective year</b>
AL	Code of Ala. §15-10-3	Discretionary	1989
AK	Alaska Stat. §18.65.530	Mandatory	1996
AZ	Ariz. Rev. Stat. §13-3601	Mandatory	1991

AR	Ark. Code Ann. §16-81-113	Preferred	1991
CA	Cal. Pen. Code §13701, Cal. Pen. Code §836	Preferred	1996
CO	Colo. Rev. Stat. §18-6-803.6	Mandatory	1994
CT	Conn. Gen. Stat. §46b-38b	Mandatory	1986
DC	D.C. Code §16-1031	Mandatory	1991
DE	11 Del C. §1904	Discretionary	1984
FL	Fla. Stat. §741.29, Fla. Stat. §901.15	Discretionary	1992
GA	Ga. Code Ann. §17-4-20	Discretionary	1981
HI	Haw. Rev. Stat. §709-906	Discretionary	1973
ID	Idaho Code §19-603	Discretionary	1979
IL	725 ILCS 5/112A-30	Discretionary	1993
IN	Ind. Code Ann. §35-33-1-1	Discretionary	2000
IA	Iowa Code §236.12	Mandatory	1986
KS	Kan. Stat. Ann. §22.2401; Kan. Stat. Ann. §22.2307	Discretionary; Mandatory	1984; 1992
KY	Ky. Rev. Stat. §431.005	Discretionary	1980
LA	La. Rev. Stat. §46:2140	Mandatory	1985
ME	19-A Me. Rev. Stat. §4012	Mandatory	1980
MD	MD. Crim. Proc. Code §2-204	Discretionary	1986
MA	Mass. Gen. Laws Ann. ch. 209A §6	Mandatory; Preferred	1978; 1991
MI	MLCS §764.15a; MCLA §776.22	Discretionary; Preferred	1978; 1995
MN	Minn. Stat. §629.341	Discretionary	1978
MS	Miss. Code Ann. §99-3-7	Mandatory	1995
MO	Mo. Rev. Stat. §455.085	Mandatory	1989
MT	Mont. Code Ann. §46-6-311	Preferred	1985
NE	Neb. Rev. Stat. §29-404.02	Discretionary	1989
NV	Nev. Rev. Stat. Ann. §171.137	Mandatory	1985
NH	N.H.Rev.Stat. Ann. §594:10	Discretionary	1979
NJ	N.J. Stat. §2C:25-21	Mandatory	1991
NM	N.M. Stat. Ann. §31-1-7, N.M. Stat. Ann. §40-13-7	Discretionary	1979
NY	N.Y. Crim. Proc. Law §140.10	Mandatory	1996
NC	N.C. Gen. Stat. §15A-401	Discretionary	1991
ND	N.D. Cent. Code §14-07.1-11; N.D. Cent. Code §14-07.1-10	Discretionary; Preferred	1989; 1995
OH	O.R.C. Ann. §2935.032	Mandatory	1994
OK	22 Okl. St. §40.3	Discretionary	1986
OR	Or. Rev. Stat. §133.055	Discretionary; Mandatory	1977; 1981
PA	18 Pa. Cons. Stat. §2711	Discretionary	1986
RI	R.I. Gen. Laws §12-29-3	Mandatory	1988
SC	S.C. Code Ann. §16-25-70	Mandatory	1995
SD	S.D. Codified Laws §23A-3-2.1	Mandatory	1989
TN	Ten. Code Ann. §36-3-619	Preferred	1995
TX	Tex. Code Crime. Proc. Art. 14.03	Discretionary	1989
UT	Utah Code Ann. §77-36-2.2	Mandatory	1991
VT	Vt. R. Cr. P. Rule 3	Discretionary	1985
VA	Va. Code Ann. §19.2-81.3	Mandatory	1997
WA	Rev. Code Wash. §10.31.100	Mandatory	1984

WV	W.Va. Code §48-27-1002	Discretionary	1994
WI	Wis. Stat. §968.075	Mandatory	1989
WY	Wyo. Stat. §7-20-102, §35-21-107	Discretionary	1982

I operationalized the warrantless arrest law variable in two ways. First, as a 4-level nominal categorical variable: discretionary arrest law, preferred arrest law, mandatory arrest law, none (referent). This operationalization is presented in the model statement below. Second, as a dichotomous variable: mandatory arrest law vs. no mandatory arrest law (combining discretionary arrest law, preferred arrest law, and none as the referent).

#### Time-varying covariates

The timing of IPV arrest legislation is believed to be fairly exogenous and not driven by factors that may also drive women’s all-cause mortality (30,101) and the difference-in-differences approach that I used is expected to control for any stable differences between states and any national trends over time. However, in an effort to eliminate potential sources of residual confounding, I controlled for time-varying state-level covariates that are associated with women’s mortality and may have had an influence on IPV arrest legislation. Specifically, I included state violent crime rate, state poverty rate, state overall unemployment rate, state women-to-men employment ratio, state percentage of Black residents, and political party in control of the state legislature. This list of potential confounders aligns with the existing literature assessing the impact of mandatory arrest legislation on intimate partner homicide using a difference-in-differences approach (30,101). Together, these covariates are expected to generally reflect the sociopolitical conditions in a state that may influence the enactment and timing of policy changes such as IPV warrantless arrest legislation. However, the potential for residual confounding by some unmeasured factor remains.

Annual state violent crime rate was obtained from the FBI Summary Reporting System (162). Annual state poverty rate was obtained from the Census Bureau Current Population Survey, Annual Social and Economic Supplements (163). Annual state unemployment rate was obtained from the Bureau of Labor Statistics Local Area Unemployment Statistics Program (164). Annual state women-to-men employment ratio was obtained from the Current Population Survey Expanded State Employment Status Demographic Data (165). Annual percentage of Black residents was obtained from the Census Bureau State Intercensal Population Estimates (166). Political party in control of the state legislature was obtained from the National Conference of State Legislatures (167).

Lastly, the fully adjusted models also included a group-specific linear trend variable, where a group represents the set of states that enacted the same type of IPV warrantless arrest legislation in the same year, as recommended by Chin and Cunningham (30) and Goodman-Bacon (168). This is a common robustness check that aims to control for common unobserved within-group pre-intervention trends. The variable was defined as a group-specific dummy variable interacted with a linear trend variable—in this case, year, operationalized as a continuous variable.

### *Statistical analyses*

#### Main analysis

The most common approach to estimate a difference-in-differences effect in a setting with multiple units and time periods is a two-way fixed effects model that includes unit and time fixed effects. In the present study, I used a two-way fixed effects quasi-Poisson model with state ( $s$ ) and year ( $t$ ) fixed effects, specified as follows:

$$\ln(E(y_{st}|x)) = \beta_0 + \beta_1 D_{st} + \beta_2 P_{st} + \beta_3 M_{st} + \beta_4 X_{st} + \beta_5 t_g + \sigma_s + \tau_t + \ln(p) \text{ where:}$$

$y_{st}$  represents the state-year mortality count among women aged 20-54

$D_{st}$  represents a dummy variable indicating whether state  $s$  had a discretionary arrest law in year  $t$

$P_{st}$  represents a dummy variable indicating whether state  $s$  had a preferred arrest law in year  $t$

$M_{st}$  represents a dummy variable indicating whether state  $s$  had a mandatory arrest law in year  $t$

$X_{st}$  is a vector of time-varying state-level covariates

$t_g$  represents the group-specific linear trend variable

$\sigma_s$  represents state fixed effects (dummy variable for each state)

$\tau_t$  represents year fixed effects (dummy variable for each year)

$\ln(p)$  represents the population offset term

Because my outcome of interest was a rate (mortality rate), I used quasi-Poisson regression models with a population offset term. I used quasi-Poisson regression instead of Poisson regression because in initial Poisson models, there was evidence of overdispersion (a dispersion parameter meaningfully greater than 1). Quasi-Poisson models maintain the same distributional assumptions as Poisson models, but allow for a flexible dispersion parameter (169). Lastly, for all models, I calculated robust standard errors to account for non-independence between panel observations from the same state (170).

#### Analysis of racialized disparities

Additionally, I specified two race-specific models in the same way as the main model, except the outcomes of interest were the state-year mortality rate among white women aged 20-54 and the state-year mortality rate among Black women aged 20-54. Because, as described above, mortality counts in state-years with fewer than 10 deaths in the specified population group are suppressed in the public National Vital Statistics System, there was substantial missing data on Black women's mortality for states with small Black populations. As a result, for all race-specific models, I excluded 12 states with 10,000 or fewer Black women aged 20-54 for the majority of the study period. I calculated two separate sets of effect estimates for white women

and Black women and compared the estimates qualitatively to assess any notable differential effects by racialized group.

### Sensitivity analyses

As a sensitivity analysis, I tested the relationship between the IPV warrantless arrest laws and women's mortality (20-54 years) due specifically to homicide. Prior research has shown no association between mandatory arrest laws and intimate partner homicide (30,101,116). However, this analytic check is important to determine to what extent, if any, an observed association with all-cause mortality is explained by homicide. I operationalized the homicide rate data in three ways: 1) using data from the FBI Supplementary Homicide Report from 1980-2016, 2) using data from the National Vital Statistics System from 1980-2019 and excluding state-years with suppressed homicide counts, and 3) using data from the National Vital Statistics System from 1980-2019 and replacing suppressed homicide counts with 4.5 (the midpoint of potential values 0 to 9).

A key assumption of the analytic models is that the states with mandatory arrest laws would have followed the same outcome trends as states without mandatory arrest laws if not for the implementation of the law. This is described as the parallel trends assumption, or the assumption that states with and without the intervention have parallel trends in the pre-intervention period. An additional assumption of difference-in-differences models with multiple groups and time periods is that the treatment effect is consistent over time, also known as non-dynamic treatment effects (168,171). Recently, researchers have noted the ways in which a violation of either of these assumptions may produce a biased result when using two-way fixed effects models (168,171). Therefore, as a sensitivity analysis, I conducted an alternative approach to estimate a difference-in-differences intervention effect with multiple groups and

time periods, known as the Callaway Sant'Anna approach (171). The approach entails estimating individual group-time treatment effects using doubly robust estimands and robust standard errors clustered at the state level. In the present specification, I assumed unconditional parallel trends (no covariates included) and modeled the log of women's mortality rate (20-54 years). I then calculated the aggregate summary treatment effect recommended by Callaway and Sant'Anna and also present an event study plot of the average treatment effects by number of years the law has been in place (171).

## **2.3 Results**

### Main analysis

The main analysis includes 2,040 state-year observations, representing all 50 states and the District of Columbia over the 40-year study period. Table 3 presents the results from the main analysis, demonstrating null findings for all fully adjusted associations, except for preferred arrest laws compared to no warrantless arrest law. In initial models, mandatory arrest laws appeared to have a small protective effect for women's mortality (20-54 years) when compared to no warrantless arrest law (RR=0.975; 95% CI: 0.951, 0.999) and compared to no mandatory arrest law (RR=0.978; 95% CI: 0.958, 0.997). However, this effect was not robust to inclusion of a group-specific linear trend, suggesting that this observed association may be due to confounding. In the fully adjusted model, after inclusion of a group-specific linear trend, preferred arrest laws appeared to have a harmful effect for women's mortality (20-54 years) compared to no warrantless arrest law. Specifically, preferred arrest laws were associated with a 4.9% increase in women's mortality rate (20-54 years) (RR=1.049; 95% CI: 1.027, 1.072). In the study sample, the average women's mortality rate (20-54 years) was 156 per 100,000, therefore a 4.9% increase represents an increase of approximately 8 deaths per 100,000.

**Table 3: Effect of warrantless arrest laws on total all-cause mortality rate for women aged 20-54 (N=2,040)**

Statute type	Model 1 (unadjusted)		Model 2		Model 3 (fully adjusted)	
	RR	95% CI	RR	95% CI	RR	95% CI
<b>Categorical<sup>1</sup></b>						
Discretionary	0.958	0.938, 0.979	0.993	0.975, 1.012	0.990	0.973, 1.008
Preferred	0.893	0.856, 0.931	0.996	0.972, 1.020	1.049	1.027, 1.072
Mandatory	0.911	0.880, 0.943	0.975	0.951, 0.999	0.997	0.968, 1.027
<b>Dichotomous<sup>2</sup></b>						
Mandatory	0.950	0.919, 0.982	0.978	0.958, 0.997	0.992	0.964, 1.021

Model 1: includes state and year fixed effects only

Model 2: includes state and year fixed effects and all time-varying state-level covariates but no group-specific linear trend

Model 3: includes state and year fixed effects and all time-varying state-level covariates and group-specific linear trend variable

<sup>1</sup>Referent group is no warrantless arrest law

<sup>2</sup>Referent group is no mandatory arrest law

#### Analysis of racialized disparities

The analyses with race-specific outcome data include 1,560 state-year observations, representing the 39 states with a sufficient population of Black women aged 20-54 over the 40-year study period. Results from these analyses are presented in Tables 4 and 5. The findings were generally consistent with the main analysis. For Black women, there appeared to be a small protective effect of mandatory arrest laws, but this effect appeared to be due to confounding, after accounting for group-specific linear trend in the fully adjusted model. Additionally, in the fully adjusted model, preferred arrest laws appeared to have a harmful effect for white women's mortality (20-54 years). Specifically, preferred arrest laws were associated with a 5.2% increase in white women's mortality rate (20-54 years) (RR=1.052; 95% CI: 1.024-1.081). In the study sample, the average mortality rate for white women aged 20-54 is 146 per 100,000, therefore a 5.2% increase represents an increase of approximately 8 deaths per 100,000. The results for the effect of preferred arrest laws on Black women's mortality (20-54 years) were in the same direction, however the risk ratio was smaller and the 95% confidence interval included the null

(RR=1.022, 95% CI: 0.984-1.062). There is no strong evidence to support racialized differences in the effect of mandatory arrest laws on women’s mortality (20-54 years) for Black women compared with white women because the effects are null for both groups.

**Table 4: Effect of warrantless arrest laws on total all-cause mortality rate for white women aged 20-54 (N=1,560)**

Statute type	Model 1 (unadjusted)		Model 2		Model 3 (fully adjusted)	
	RR	95% CI	RR	95% CI	RR	95% CI
<b>Categorical<sup>1</sup></b>						
Discretionary	0.963	0.942, 0.985	0.998	0.980, 1.017	0.982	0.963, 1.000
Preferred	0.888	0.847, 0.930	1.004	0.978, 1.031	1.052	1.024, 1.081
Mandatory	0.922	0.894, 0.951	0.987	0.965, 1.009	0.993	0.968, 1.019
<b>Dichotomous<sup>2</sup></b>						
Mandatory	0.965	0.937, 0.993	0.986	0.969, 1.003	0.995	0.971, 1.020

Model 1: includes state and year fixed effects only

Model 2: includes state and year fixed effects and all time-varying state-level covariates but no group-specific linear trend

Model 3: includes state and year fixed effects and all time-varying state-level covariates and group-specific linear trend variable

<sup>1</sup>Referent group is no warrantless arrest law

<sup>2</sup>Referent group is no mandatory arrest law

**Table 5: Effect of warrantless arrest laws on total all-cause mortality rate for Black women aged 20-54 (N=1,560)**

Statute type	Model 1 (unadjusted)		Model 2		Model 3 (fully adjusted)	
	RR	95% CI	RR	95% CI	RR	95% CI
<b>Categorical<sup>1</sup></b>						
Discretionary	0.984	0.955, 1.014	0.985	0.957, 1.014	0.990	0.962, 1.018
Preferred	1.020	0.985, 1.057	1.021	0.984, 1.058	1.022	0.984, 1.062
Mandatory	0.938	0.895, 0.984	0.959	0.921, 0.998	0.977	0.934, 1.022
<b>Dichotomous<sup>2</sup></b>						
Mandatory	0.939	0.901, 0.978	0.958	0.929, 0.989	0.980	0.937, 1.025

Model 1: includes state and year fixed effects only

Model 2: includes state and year fixed effects and all time-varying state-level covariates but no group-specific linear trend

Model 3: includes state and year fixed effects and all time-varying state-level covariates and group-specific linear trend variable

<sup>1</sup>Referent group is no warrantless arrest law

<sup>2</sup>Referent group is no mandatory arrest law

## Sensitivity analyses

Table B1 in Appendix B presents the results from the sensitivity analysis focused on deaths for women aged 20-54 due to homicide only. The findings were null in all model specifications, suggesting no association between warrantless arrest laws and homicide rates for women aged 20-54.

Finally, Table B2 in Appendix B presents the aggregate estimate of group-time average treatment effects using the Callaway Sant'Anna approach that aims to address potential bias in two-way fixed effects models with dynamic treatment effects. The summary estimate for mandatory arrest laws compared to no mandatory arrest was also null, consistent with the main findings. Figure B1 presents an event study plot of the average treatment effects by years since effective date of the legislation. The plot suggests no clear violation of the parallel trends assumption given that pre-intervention estimates are close to 0. Additionally, the plot suggests the time-specific average treatment effects were also null. The average treatment effect estimates for 15 or more years after the legislation was enacted appeared to trend toward negative values, however the confidence intervals were very wide because only a small number of states contributed to these estimates so these estimates should be interpreted with caution. All of the confidence intervals for post-intervention treatment effects included the null, except the last estimate at 38 years after intervention, which was only informed by one exposed state-year observation.

## **2.4 Discussion**

### *Summary of findings*

Overall, the findings from the fully adjusted models suggest that there is no detectable effect of mandatory arrest laws on women's mortality (20-54 years) at the population level;

however, there appears to be a harmful effect of preferred arrest laws. In addition, the findings do not provide evidence of racialized differences in the effect of warrantless arrest laws on women's mortality (20-54 years) and there are no detectable effects of warrantless arrest laws on homicide rates among women aged 20-54. There is very limited research on the generalized health consequences of warrantless arrest laws – this is one of the first studies to attempt to measure such consequences – therefore, it is challenging to situate these findings within a larger evidence base. The finding of no effect of mandatory arrest laws on homicide rates is generally consistent with recent research demonstrating no effect of mandatory arrest laws on IPV homicide rates (30). The findings of an association between preferred arrest laws and higher all-cause mortality may align with findings from the one existing study that assessed all-cause mortality as an outcome of arrest for IPV, though at the individual level: In their individual-level RCT follow-up, Sherman and Harris (86) found an increased risk of all-cause mortality for people whose partners were randomized to arrest compared to people whose partners were not arrested 23 years after the intervention. However, one would expect to see a similar relationship between mandatory arrest laws and higher all-cause mortality, which was not detected.

#### *Potential interpretations of findings*

There is no obvious explanation for why preferred arrest laws appear to have a harmful effect on women's mortality (20-54 years) while discretionary and mandatory arrest laws do not. This finding may be reflective of within-state variation in actual implementation of warrantless arrest laws where the state law is not an accurate reflection of on-the-ground practice at the local level within the state (172). Additional research using actual rates of arrest for reported IPV incidents as the exposure of interest is needed to further interrogate this finding.

Regarding mandatory arrest laws specifically, there are a number of potential interpretations of the null findings from this study. First, there may truly be no effect of mandatory arrest laws on women's mortality (20-54 years) at the population level. This may be a plausible conclusion if any effect of mandatory arrest laws on 20-54-year-old all-cause mortality only exists for women who have experienced IPV, and if any such effect is small. Because I used all women aged 20-54 as a proxy for survivors of IPV, a small effect of mandatory arrest laws among only women who had experienced IPV might be washed out. This limitation may explain why Sherman and Harris' randomized trial (86) detected effects on all-cause mortality among a sample of IPV survivors but no effects were detected in this analysis. However, if mandatory arrest laws are in fact effective tools for IPV prevention, as they are intended to be, one might expect some protective effect on women's mortality (20-54 years) given the high burden of IPV among this group.

A second interpretation of the null findings is that the data or methods employed in this exploratory analysis were not sufficient to detect generalized health effects of mandatory arrest laws. This analysis focused on effects of law enactment that may be detected through changes in 20-54-year-old mortality and is not focused on detecting causal mechanisms that may manifest in increased mortality in later life. It may be the case that any effect of mandatory arrest laws on women's mortality only manifests in later life (55 years and later), as would be expected for mechanisms involving chronic disease. In general, mortality is a distal, downstream public health indicator. Other more proximate indicators of health may be more appropriate outcomes. For example, based on the hypothesized mechanisms for how mandatory arrest laws may result in generalized health consequences that are depicted in Figure 3, more proximate health outcomes

of interest may include rates of substance use, depression, anxiety, emergency room visits, and diagnosis of chronic disease.

Lastly, a third potential interpretation of the findings is that there are heterogeneous effects for certain subgroups that were not analyzed and these differential effects may be washing each other out to produce null findings. For example, mandatory arrest laws may have different effects in rural vs. urban areas within states where the landscape of criminalized behavior is likely very different in addition to differences in the funding, structure, and size of policing and other criminal legal responses.

The general conclusion that I draw from this study is that it does not provide evidence of harmful public health consequences of mandatory arrest laws and it also does not provide evidence that mandatory arrest laws work to improve public health. The study provides evidence to suggest that preferred arrest laws may have generalized negative public health consequences for women. Additional studies to interrogate or replicate this finding are warranted.

#### *Strengths and limitations*

The present study has strengths and limitations. With respect to strengths, first, the study takes advantage of the variation in the timing of the implementation of warrantless arrest laws across states. By employing a difference-in-differences approach, I controlled for baseline differences between states and national trends in women's mortality. As a result, the risk of residual confounding is low. It is possible that there are factors that influenced the timing at which states passed warrantless arrest legislation, such that the timing of the laws is not truly random. I am confident, however, that I have accounted for such potential confounders through the inclusion of multiple time-varying covariates. This selection of time-varying covariates is consistent with existing studies that have assessed the effect of warrantless arrest laws. Risk of

confounding stems primarily from the appropriate measurement of these time-varying confounders. I used national administrative data sources, such as the Census and Bureau of Labor Statistics, which are not without limitations, such as variation in reporting by state. However, these are widely considered the best data available for state-specific poverty, employment, and other demographic data.

An additional strength of the proposed study is the use of longitudinal state-level administrative data for the outcome of interest and covariates. The administrative data sources used, for example, the National Vital Statistics System, are considered accurate and reliable particularly at the state level. Thus, the risk of information bias for the main outcome is low. The exposure of interest was based on state statutes concerning warrantless arrest legislation. There is some risk of misclassification because researchers have disagreed on the classification of some state statutes. I believe I minimized this risk by reviewing numerous existing classification schemas for inconsistencies and conducting my own independent archival review of the statutes.

Finally, a strength of the proposed study is that the methods used are consistent with prior research concerning the population-level effects of warrantless arrest laws, while adding an important contribution to the literature by focusing on a more expansive outcome than intimate partner homicide— all-cause mortality. The present study builds directly upon an existing individual-level randomized trial showing that people whose abusive partners were randomized to arrest for misdemeanor IPV incidents had higher odds of all-cause mortality than people whose abusive partners were not randomized to arrest (86). The present study extends this research question to assess if warrantless arrest laws have impacts on women's mortality (20-54 years) at the state level. This state-level extension is important because warrantless arrest laws are implemented and operate at the state level, impacting how survivors interact with systems

and seek help. Thus, its consequences should be evaluated at higher levels of organization. The present study also builds on past literature by employing rigorous sensitivity analyses, including application of the new Callaway Sant'Anna approach to address potential bias from two-way fixed effects models.

The present study is not without limitations. First, the study estimates the effect of warrantless arrest laws and not the effect of actualized changes in policing practice or rates of arrest. The advantage of this approach is that it tests the effects of these widely implemented laws through multiple mechanisms, including normative declaration (the societal signaling of actions that will not be tolerated) (173), changes in IPV reporting rates, and changes in actual policing practices/arrest rates. However, as a result, the findings cannot be interpreted as the consequences of actualized changes in policing practice. Rather they can only be interpreted as the effect of enacting this legislative policy.

Second, a state is a large and heterogenous geographic area. There is likely some variation at the local level in the effect of warrantless arrest laws on women's mortality and variation in the local implementation of warrantless arrest laws, not only due to policies such as Dillon's Rule and Home Rule, which allow municipalities some discretion to enact local legislation that differs from state legislation (174), but also because police departments implement warrantless arrest laws to varying degrees (172). The proposed analysis lacks the precision to detect such localized effects of warrantless arrest laws on women's mortality.

Third, as discussed above, mortality is a distal outcome that does not reflect the wide range of more proximate health consequences that may result from exposure to warrantless arrest laws.

Fourth, as discussed above, I restricted the outcome to women's mortality because an estimated one in four women in the U.S. have experienced IPV and women suffer from more severe incidents of IPV compared to men (34,175). However, as a result, this study does not reflect the consequences of warrantless arrest laws for IPV survivors of other genders and does not reflect potential consequences for women who may not have directly experienced IPV, thereby making an effect among IPV survivors more difficult to detect.

Finally, as previously discussed, there is an emerging understanding among research scholars of the limitations of the two-way fixed effects approach for difference-in-differences estimation with multiple groups and time periods, namely potential biases that arise when the treatment effect varies across units or across time periods within units (168,171,176). Alternative estimation methods, such as the Callaway Sant'Anna approach used in the sensitivity analysis, have been introduced. However, these methods are still new and there is not yet consensus on how to determine the best approach and model specifications for each research question. While I believe the Callaway Sant'Anna approach was the clearest and most accessible option for estimating group-time treatment effects in this study, it is possible that increased use of these novel methods over time will improve our understanding of which method and model specifications are best suited to specific research questions.

### *Conclusions and next steps*

In conclusion, this exploratory analysis provides new evidence that preferred arrest laws may be harmful for survivor health and does not provide evidence that mandatory arrest laws are harmful or protective for survivor health. Because this was an exploratory analysis, assessing the generalized public health consequences of warrantless arrest laws for the first time, there are important next steps that should be explored in future research. First, future research on this topic

should consider analyzing longitudinal measures of public health consequences among survivors specifically and consider more proximate health indicators than mortality alone. Furthermore, there may be value in more subgroup analyses in addition to race-stratified analyses, such as examining rural vs. urban differences. Finally, additional research that measures the impact of actual local IPV arrest practice would be a valuable contribution to the literature.

Despite no documentation of a harmful effect of mandatory arrest laws on women's mortality (20-54 years), this null effect should be weighed against known, documented harmful effects of mandatory arrest such as its role as a driver of women's arrests and arrests of survivors of IPV (49,92-95). In this larger context, the evidence collectively suggests that mandatory arrest laws and other warrantless arrest laws should be critically reexamined because there have been no consistently demonstrable benefits of these laws.

## **Chapter 3: The relationship between intimate partner violence policing and family surveillance**

### **3.1 Introduction**

In this chapter, I focus on one potential negative and racialized consequence of IPV policing: family surveillance. In its introductory section, I present the concept of family surveillance and place it within the larger context of the U.S.'s carceral infrastructure. I then discuss the theoretical and empirical foundation for why there may be a relationship between IPV policing and family surveillance, followed by a presentation of the gaps in the research literature and the approach of the present study to estimate the county-level racialized impact of IPV policing practice on family surveillance.

#### *Family surveillance as an extension of the carceral state*

The U.S. child welfare system is ostensibly designed to promote the safety of children and strengthen families, primarily through investigating and intervening after reports of suspected child maltreatment, defined as child abuse (physical, sexual, or emotional) and/or neglect (177). However, anti-violence experts and advocates are increasingly recognizing it as a “family regulation system” or “family policing system” that surveils and punishes predominantly poor mothers of color, separating families while failing to address the root causes of child maltreatment (64,147). The family policing system and the criminal legal system are long-standing, mutually-reinforcing U.S. institutions rooted in structural racism, which target, regulate, and punish Black families by design, rather than ameliorating the underlying social problems they purport to address (e.g., child maltreatment, violence, theft, substance use) (8,65,146,147,149). Therefore, exposure to the family policing system can be conceptualized as a

negative consequence for families, especially Black families, much like exposure to the criminal legal system has cascading harmful effects for Black families.

Similar to the racialized criminalization of poverty evidenced in the criminal legal system, poverty is also a strong driver of family policing intervention, particularly for Black families (178). The vast majority of child removals occur due to neglect not abuse, and child neglect is often conflated with poverty (147). Many of the common reasons children are removed from their homes are rooted in a lack of material resources, such as lack of childcare while parents are at work, insufficiently stocked refrigerators, or multiple family members sharing a room (65). Instead of prioritizing the provision of resources to ameliorate the conditions that give rise to child maltreatment, the state surveils at-risk families, enforces mandates through the threat of child removal and/or criminalization, and may carry out such threats as punishment for non-compliance.

Exposure to and intervention by the family policing system can have devastating consequences for families. The trauma of family separation, even temporarily, can have harmful psychosocial and behavioral health effects for both children and parents (65,143,179,180). For children, separation and attachment disorders, post-traumatic stress disorder, substance use, and anxiety are all common outcomes stemming from the trauma of family separation (180). Similar to how parents can be criminalized through the family policing system, children—especially Black children—are more likely to be pushed into the criminal legal system once exposed to family policing (147). Foster care serves as a pipeline to the juvenile justice system and adult prison system (147). In addition, foster care settings generally have a high prevalence of abuse and neglect, so children may be at a higher risk of maltreatment after being removed from their

families (65,180). Research has also documented the intense trauma, grief, social stigma, and behavioral health challenges that parents experience after child removal (143,181).

The pronounced racialized disparities within the family policing system result in Black families and other families of color being disproportionately burdened by the harms of family surveillance (65,180). In 2019, Black children represented 23% of the foster care population while representing just 14% of the general child population (182,183). The family policing system involves several stages of discretionary decision-making, and research has documented significant racialized bias in each of these decisions (180). Black families are more likely to be surveilled by the system and less likely to receive in-home services than white families (180). Additionally, Black children experience higher rates of removal and lower rates of adoption and reunification compared to white children (65).

Not only are policing and family policing indirectly mutually reinforcing systems given how they both systematically punish and criminalize Black people in the U.S., but they are also directly integrated. Policing is one of the primary systems used to surveil families, leading to their entry into the family policing system. Given the substantial body of research demonstrating racialized inequities in police contact and stops (80,184–187), where police disproportionately target Black people and other people of color, family surveillance is also patterned by racialized policing practice. Police officers are mandated to report suspected child abuse and/or neglect to local child protection agencies and they produce about 20% of all child maltreatment reports nationwide (66). These reports may then lead to investigation and intervention by child protection agencies, including removal of children into foster care. In a 2019 national study, Edwards (66) found that policing practice was closely linked with family surveillance at the county level: between-county and within-county differences in arrest rates were significantly

positively associated with the number of police-initiated reports of child maltreatment. In this context, family surveillance and the broader family policing system can be seen as an extension of the racialized U.S. carceral state, in addition to systems of policing, prosecution, and incarceration (80,184–187).

*Framework for relationship between IPV policing and family surveillance*

I hypothesize that family surveillance is a negative and racialized consequence of IPV policing. When police respond to an IPV incident and children are in the home, police may submit a maltreatment report to local child protection agencies and may also criminally charge the survivor with “failure to protect” the children (179,188). Survey research and qualitative research examining survivors’ experience with police has documented how police may file or threaten to file child maltreatment reports when responding to IPV cases. In a 2015 national survey of anti-violence service providers, advocates, and attorneys, 89% of respondents reported that contact with police sometimes or often results in child protective services involvement (62). In two of the qualitative studies identified in the scoping review presented in Chapter 1, survivors reported that police threatened to remove their children or reported that police contact resulted in actual involvement with child protective services (83,84). Therefore, one mechanism through which IPV policing is expected to influence family surveillance is through the direct coordination between police and child welfare agencies where police officers submit a child maltreatment report after responding to an IPV incident.

A second, more indirect mechanism through which IPV policing is expected to result in increased family surveillance is that when police arrest one parent (whether the abusive partner or the survivor), the subsequent risk of child protective services involvement may be heightened. There are a number of reasons why the arrest and/or incarceration of one parent would increase

risk of child protective services involvement. For example, the arrest and/or incarceration of a parent can lead to job loss resulting in loss of an income source and/or a lack of childcare while one parent is working and the other is incarcerated. If a parent is placed on community supervision (e.g., pretrial supervision or probation), a law enforcement officer may frequently visit the home leading to heightened family surveillance and an increased risk of child protective services involvement. Through these pathways, and others, it is expected that IPV policing would indirectly increase the downstream risk of family surveillance and child protective service involvement through the arrest of a parent.

To summarize, the theoretical and empirical framework guiding the present study is that systems of policing and family surveillance are intersecting and mutually reinforcing systems rooted in structural racism and IPV is a context in which this intersection is heightened, especially for Black families. Therefore, there is an expectation that areas in which police make more arrests for IPV will have higher rates of family surveillance via direct coordination between police and child welfare systems and the subsequent increased risk of child protective services involvement that may follow from the arrest of a parent.

#### *Gaps in the research literature*

While limited survey and qualitative research has documented anti-violence advocates' perspectives and individual survivor perspectives on how child protective services involvement can be a negative consequence of IPV policing, this relationship has not been quantitatively estimated. As identified in the scoping review presented in Chapter 1, no empirical research on the outcomes of IPV policing has focused on child protective services involvement. If an association between IPV policing and family surveillance is detected, it would provide population-level empirical evidence suggesting that IPV survivors are suffering downstream

consequences of IPV policing, while the root cause of their experience of violence may remain unaddressed. Additionally, an increased risk of child protective services involvement following IPV policing could discourage survivors from reporting their experience of IPV and seeking help out of fear of losing child custody. Given that policing and arrest remain the primary response to IPV, it is necessary to understand any potential negative consequences of this response for survivors and their families.

Further, it is critical to examine whether any relationship between IPV policing and family surveillance disproportionately impacts Black families given how systems of policing and family policing disproportionately target Black families. There are multiple ways to interrogate how this relationship may differ by race. It is of interest to understand both how IPV policing is associated with Black families' exposure to the family policing system compared to white families' exposure as well as how the relationship between IPV policing and family surveillance may differ in predominantly white counties compared to more racially diverse counties. Prior research has demonstrated that as the percentage of Black residents in a county increases, the number of police-initiated child maltreatment reports for Black children decreases (66). This suggests that Black families living in predominantly white counties may be particularly vulnerable to surveillance and targeting by police and the family policing system.

#### *Purpose of present study and approach*

In the present study, I aim to examine the association between IPV policing practice and family surveillance at the county level, including an analysis of racialized disparities in this association. I hypothesize that counties in which arrests are more frequent for IPV incidents will also have higher rates of child maltreatment reporting and that this association will be stronger for Black child maltreatment report rates compared to white child maltreatment report rates. I

also hypothesize that, consistent with prior research, the association between IPV policing practice and family surveillance will be stronger for Black families living in predominantly white counties compared with Black families living in more diverse counties. The present study is the first to use longitudinal, administrative data to estimate an association between IPV arrest practice and rates of child maltreatment reports. As such, the study involves the creation of an unprecedented multi-source, multilevel dataset linking county-level IPV incident and arrest data with county-level child maltreatment data.

It is important to note that an observed association between IPV policing and family surveillance could indicate that families experiencing IPV are at higher risk of perpetrating child maltreatment and therefore arrest is an effective tool for identifying child maltreatment. The present analysis is focused on the outcome of child maltreatment *reporting*, which leads to investigation by child protective service agencies. I am unable to distinguish between very severe cases of child maltreatment that warrant intervention and less severe or unsubstantiated cases. However, while the family policing system is ostensibly designed to respond to and address cases of child maltreatment, there is no evidence to suggest that the system effectively identifies and addresses severe cases of child maltreatment, especially given that the majority of child maltreatment cases are indistinguishable from poverty (147). I assess child maltreatment reporting as a negative outcome because of the downstream harms that can be triggered from a child protective service investigation, such as temporary or permanent removal of children, as discussed above. While the main analysis cannot rule out the effective detection of high-risk child maltreatment cases as the underlying driver of an observed association, the analyses by race may provide some insight. If the association between IPV policing and family surveillance differs by race or by county racial composition, that may suggest that the detection of high-risk

child maltreatment cases is not the only driver of child maltreatment reporting because there is no reason to expect such an association to be patterned by race.

## **3.2 Methods**

### *Design and setting*

The present study is a county-level retrospective longitudinal analysis assessing whether counties with a higher percentage of police-reported IPV incidents resulting in arrest have a higher rate of child maltreatment reporting to child welfare agencies. The unit of analysis was county-year. The analysis was restricted to large urban and suburban U.S. counties only and covers the 20-year period of 2000 through 2019 (rationale described below).

### *Data sources and measures*

#### Child maltreatment report rate

The outcome of interest was the rate of investigated child maltreatment reports in a given county and year. I obtained annual county-level counts of child maltreatment reports that were investigated by child welfare agencies from the National Child Abuse and Neglect Data System (NCANDS) (189), managed by the Office of the Administration for Children and Families in the U.S. Department of Health and Human Services. NCANDS is the most comprehensive source of national data on child maltreatment reports, containing several million records annually through reporting from state and local child welfare agencies (66). Reports of child maltreatment that are screened out by the child welfare agency as not requiring an investigation are not included in NCANDS. NCANDS data are provided at the case level, including demographic information about the child and the jurisdiction in which the case occurred. NCANDS transitioned to a new case reporting system in 2000, therefore I restricted the study period to 2000-2019.

County code is only provided for counties with 1,000 or more reports to protect the confidentiality of the children represented in the dataset. I used this county code information to obtain estimates of the number of child maltreatment reports per county and year. I also used the case-level demographic information to obtain race-specific estimates of the number of maltreatment reports for Black children and non-Hispanic white children per county and year. Annual county-level rates of maltreatment reports were calculated by dividing the number of reports by the under-20 county population. Race-specific annual rates were calculated using race-specific numerators and denominators. Total under-20 county population and race-specific under-20 county population estimates were obtained from the U.S. Census Bureau (190). Under-20 population estimates were used rather than under-18 population estimates because race-specific under-18 population estimates by county are not publicly available through the U.S. Census Bureau.

Given the suppression of data for counties with fewer than 1,000 cases, there is a risk of selection bias, in which the population of a county and its rate of maltreatment reports may influence selection into the study. To minimize the risk of the outcome measure influencing selection into the study and producing a bias, I restricted the potential sample to the 280 largest U.S. counties (based on 2019 population estimates). The 280 largest counties are those counties with a population of about 240,000 or larger. By restricting the target population to large counties, I minimize the risk that the child maltreatment report rate is influencing selection (e.g., regardless of whether the county has a low or high rate of maltreatment reports, their large population makes it unlikely that there will be fewer than 1,000 annual cases in the county).

Data submitted to NCANDS undergo several rounds of validation checks to improve consistency and validity of the data across states for use in research (191). However, it is highly

likely that state-level variation in NCANDS reporting remains, thus I included a state-level fixed effect in one set of models, as is best practice (66,191). The inclusion of a state fixed effect should also adjust for any potential differential effect in counties in states with mandatory arrest laws compared with counties in states without mandatory arrest laws. In addition, there are gaps in reporting with missing data in some counties; the extent of missing data and approaches to address it are discussed below. For counties that report to NCANDS, race/ethnicity data were missing in about 5% of cases. Cases with missing race/ethnicity data were included in the total counts of child maltreatment reports but excluded from race-specific counts of child maltreatment reports. Despite these potential limitations, NCANDS is the best available source for national data on child maltreatment reporting.

#### Percentage of police-reported IPV incidents that result in arrest

The exposure of interest was the percentage of IPV incidents that were reported to police and resulted in arrest, reflecting county-level IPV arrest practice. I obtained estimates of the number of IPV incidents and arrests by county by year from the FBI National Incident-Based Reporting System (NIBRS) (192). NIBRS provides incident-level data on all criminal offenses that are reported to police and logged as an incident report from reporting agencies for 24 Group A crimes, including assault, sex offenses, and homicide. Nearly 9,000 law enforcement agencies report data to NIBRS, representing about half of the U.S. population.

For this analysis, I used the victim segments and administrative segments of Jacob Kaplan's concatenated NIBRS files (193). These files provide each year of unaltered NIBRS data in an RDS file format (193). The victim segment of the data includes data on the victim's "relationship to the offender." These relationship data were used to identify IPV-specific incidents and arrests: those for which the relationship between the two parties was a current or

former dating or spousal relationship. The administrative segment of the data includes an identifying code for the law enforcement agency and an indicator for whether the incident resulted in an arrest. I used Law Enforcement Agency Identifiers Crosswalk (LEAIC) files (194) to link law enforcement agencies to their respective counties and aggregate counts of IPV incidents and arrests to the county level. I calculated annual county-year estimates of the percentage of police-reported IPV incidents that resulted in arrest by dividing the number of IPV incidents that resulted in arrest by the total number of IPV incidents.

While NIBRS is the only national source of detailed crime and arrest data, there are important risks concerning the validity and reliability of the data. As mentioned above, not all law enforcement agencies report to NIBRS, so there is a high level of missing data due to non-reporting. The extent of missing data and approaches to address it are discussed below. The FBI Uniform Crime Reporting (UCR) Program conducts quality control checks by monitoring deviations in monthly reports from each law enforcement agency (195). However, the accuracy of NIBRS data relies on voluntary, good faith reporting by each agency. As a result, there is no way to verify that all reported crimes and arrests are captured in the data and there may be variation across agencies regarding what is ultimately reported to NIBRS.

#### Time-varying covariates

Given that this is an observational study, there is a risk of confounding due to common causes of IPV arrest practice and reports of child maltreatment. At the most proximal level, a county's violent crime rate and police force size are expected to influence the percentage of IPV incidents that result in arrest and the rate of child maltreatment reports. I adjusted for both of these variables, obtaining county-level annual data on violent crime rate from the FBI UCR County-Level Detailed Offense and Arrest Data (196) and on number of police employees per

capita from Jacob Kaplan's concatenated UCR Law Enforcement Officers Killed and Assaulted (LEOKA) file (197). Both of these measures suffer from the same validity concerns as the NIBRS data because, while they undergo quality control checks by the UCR program, they rely on good faith reporting by law enforcement agencies.

There are additional county- and state-level factors that are likely to create a strong criminal legal response in the jurisdiction that will ultimately lead to a higher percentage of IPV incidents that result in arrest and a higher rate of reports to child welfare agencies: the population density of the county and the political party in control of the state legislature. Population density data were obtained using county land area from the Vera Institute of Justice's Incarceration Trends file (198) and population data from the U.S. Census Bureau (190). Political party in control of the state legislature data were obtained from the National Conference of State Legislatures (167).

Finally, at a more distal level, processes that drive whether a county has a stronger criminal legal response are rooted in place-based dynamics of racial capitalism, shaping which communities are surveilled, criminalized, and punished and which communities receive non-punitive social services (199,200). While racial capitalism is a broad construct that cannot be directly measured, the concentrated disadvantage and inequity that results from racial capitalism can be measured through proxies of county-level demographics. Specifically, I controlled for these processes by adjusting for the percentage of Black residents in a county, county poverty rate, and county unemployment rate. While these demographic factors are crude proxies for the complex processes that shape criminalization and punishment, they reflect the inequitable spatial distribution that results from these processes. Percentage of Black residents was obtained from Census county population data (190). Annual county poverty rate was obtained from the Census

Bureau Small Area Income and Poverty Estimates (SAIPE) Program (201). Annual county unemployment rate was obtained from the Bureau of Labor Statistics Local Area Unemployment Statistics (LAUS) Program (164).

Risk of confounding stems primarily from the appropriate measurement of these time-varying confounders. I am using national administrative data sources, such as data from the FBI, Census, and Bureau of Labor Statistics, which are not without limitations, as discussed. However, these are widely considered the best national data available for crime, poverty, employment, and other demographic data.

Notably, it is plausible that the underlying county prevalence of IPV affects IPV policing practice and child maltreatment reporting and could thereby be a potential confounder. There is no accurate measure of the true prevalence of IPV because IPV is notoriously underreported (113). However, I am making the assumption that incidents of IPV that are not reported to police are unlikely to have an impact on IPV policing practice because, by definition, police are unaware of these incidents. The exposure of interest is the percentage of IPV incidents reported to police that result in arrest rather than the absolute rate of IPV arrests because the absolute rate of IPV arrests is influenced by both the rate of police-reported IPV incidents and the police response to those incidents (arrest vs. no arrest). By focusing on the percentage of police-reported IPV incidents that result in arrest, I aim to isolate the police response. For example, counties in states with mandatory or preferred arrest laws are expected to have a high percentage of IPV incidents resulting in arrest even if the prevalence of police-reported IPV is low. (In this sample, the mean IPV arrest percentage is 49% in counties in states with discretionary arrest laws, 52% in counties in states with preferred arrest laws, and 60% in counties in states with mandatory arrest laws.) It is still possible that the rate of police-reported IPV incidents impacts

the percentage of incidents that result in arrest. For example, law enforcement agencies may be encouraged to make arrests in a higher percentage of IPV incidents if the prevalence of reported incidents is increasing. As such, I conduct a sensitivity analysis using the IPV arrest rate as the exposure and control for the rate of police-reported IPV incidents. This sensitivity analysis is described further below.

### Missing data

Because not all jurisdictions report administrative data to NIBRS and NCANDS, there was substantial missingness for both the exposure and outcome variables that was unlikely to be randomly patterned. First, if a county was missing all exposure data or all outcome data over the 20-year analytic period, then the county was dropped from the analysis. Of the 280 largest U.S. counties that were eligible for inclusion, 120 counties were completely excluded leaving 160 counties (57%) remaining for inclusion that had exposure and outcome data available for at least one year. This resulted in a total sample of 3,200 county-years.

For the remaining 3,200 county-years, I compared the characteristics of county-years with complete data to county-years with missing data for the exposure and outcome variables to assess the extent to which county-years with complete data were systematically different from those with missing data. Tables C1 and C2 in Appendix C present this comparison. The tables demonstrate statistically significant differences between county-years with complete vs. missing data for almost all covariates. Some of the largest differences were that county-years with missing exposure or outcome data had lower population densities and lower violent crime rates than county-years with complete data. Additionally, the Black child maltreatment report rate was significantly higher for county-years with complete exposure data than county-years with missing exposure data. These differences demonstrate that missingness is not completely at

random and a complete case analysis is expected to be biased. Therefore, I addressed missingness using multiple imputation using chained equations (MICE) for the main analysis and present the complete case analysis as a sensitivity analysis.

I ran 5 imputations using the “mice” package in R and imputed missing data using a two-level normal model (2l.norm), which accounted for repeated measures by county (202). All variables with any missing data were imputed, including IPV arrest percentage, count of child maltreatment reports (total and race-specific), violent crime rate (missing years 2015 and 2017-2019), and police force size (missing one county-year). All other variables described above were included in the imputation model except the state fixed effect because it is collinear with the county fixed effect. I restricted the range of possible values for IPV arrest percentage to 0 and 100 and the range of possible values for count of child maltreatment reports to 0 and 80,000. To assess the quality of the imputation process, I plotted the distribution of the exposure and outcome variables for complete cases vs. imputed datasets (see Appendix C). After imputation, I rounded imputed values for count of child maltreatment reports to whole integers to be compatible with the statistical models used.

### *Statistical analyses*

#### Main analysis

Because my outcome of interest is a rate (rate of child maltreatment reports), I used Poisson regression models where the offset term was the under-20 population. The main analysis involved three types of models, assessing between- and within-county effects. Model 1 was a multilevel random intercept model accounting for non-independence at the county level. Model 2 was equivalent to Model 1 with the addition of state fixed effects to additionally control for baseline differences by state, including administrative data reporting differences. Model 3 was a

county fixed effects model that assessed within-county effects only. There were three outcomes of interest for all models: 1) total child maltreatment report rate, 2) Black child maltreatment report rate, and 3) non-Hispanic white child maltreatment report rate. The exposure of interest, IPV arrest percentage, was standardized such that all models reflect the change in outcome associated with a standard deviation increase in IPV arrest percentage. I first conducted a series of crude analyses, including year fixed effects as the only covariate. I then conducted a series of adjusted analyses including all time-varying covariates described above.

### Interaction analysis

In addition, I conducted an interaction analysis to gain insight into how this association may vary within different racialized contexts, measured by the county-year's percentage of Black residents. I created a binary indicator variable representing low vs. high percentage of Black residents in the county-year where low is the sample mean of 14.5% or less and high is above the mean. I replicated the series of adjusted analyses (Models 1-3) with all three outcomes of interest including an interaction between the exposure (IPV arrest percentage) and this binary indicator of county-year percentage of Black residents. I report estimates for the associations of interest within county-years with low vs. high percentages of Black residents.

### Sensitivity analyses

I conducted four sensitivity analyses. First, I conducted a complete case analysis in which any county-years with missing exposure or outcome data were excluded from the analysis, leaving 2,033 complete cases for analysis. In the complete case analysis, I replaced any missing covariate values (violent crime rate and police force size) using linear interpolation.

The second sensitivity analysis used the multiply imputed datasets and introduced a lag such that the exposure data preceded the outcome data by 1 year and the covariate data preceded

the exposure data by 1 year (preceding the outcome data by 2 years). The purpose of this lagged sensitivity analysis was to assess whether the results would be consistent when the temporal ordering of confounder, exposure, and outcome was more clearly defined. The main analysis did not include this lag because it is more likely that referrals from police to the family policing system would occur within the same year as the IPV incident. This sensitivity analysis simply served as a robustness check.

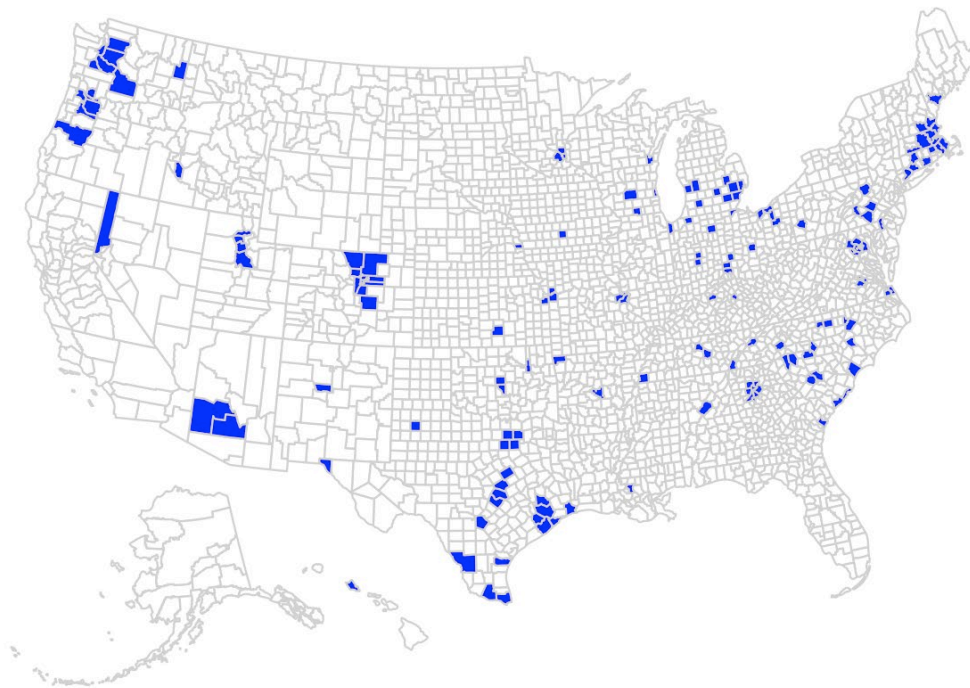
In the third sensitivity analysis, I replaced the exposure of interest (percentage of police-reported IPV incidents that result in arrest) with the absolute rate of IPV arrests. I additionally controlled for the rate of police-reported IPV incidents. I used the same multiple imputation approach as in the main analysis to address missing data and I standardized the exposure so the results reflect the change associated with a standard deviation increase in IPV arrest rate per 100,000. The purpose of this sensitivity analysis was to assess if IPV arrest rate is a more informative measure of IPV policing practice than the percentage of IPV incidents that result in arrest, especially given that there is more variation in the absolute rate compared with a percentage measure that is restricted to 0 through 100.

In the final sensitivity analysis, I replicated the interaction analysis but used percentage of non-Hispanic white residents to measure county racial composition rather than percentage of Black residents. I used the sample mean of 65.7% to create the cutoff for low vs. high percentage of non-Hispanic white residents. The purpose of this sensitivity analysis was to determine if the findings were consistent when using this alternate measure of county racial composition.

### **3.3 Results**

Figure 4 displays a map of the 160 counties that are included in the study sample. Table 6 presents the characteristics of the county-years included in the analysis using complete data as

well as the percentage of missing data for each variable. The exposure, IPV arrest percentage, was missing for 31.2% of county-years and the outcomes, total and race-specific child maltreatment report rate, were missing for 12.3% of county-years. Table C3 in Appendix C presents the characteristics of county-years and percentage missing for each variable by the binary variable of low vs. high percentage of Black residents used in the interaction analysis. Notably, county-years with a low percentage of Black residents have much lower population densities on average (769.6 vs. 1,689.4 people per mi<sup>2</sup>) due to having larger average land area. Therefore, the percentage of Black residents can also be considered a proxy for urbanicity.



**Figure 4: Map of 160 counties included in study sample**

**Table 6: Characteristics of county-years and missingness for each variable**

<b>Variable</b>	<b>Mean (SD) or n (%)</b>	<b>Percent missing</b>
IPV arrest percentage	55.4% (19.7%)	31.2%
Total child maltreatment report rate (per 10,000)	418.5 (223.4)	12.3%
Black child maltreatment report rate (per 10,000)	809.0 (497.0)	12.3%
Non-Hispanic white child maltreatment report rate (per 10,000)	298.3 (200.2)	12.3%
Population density (people per mi <sup>2</sup> )	1,108.7 (1,420.3)	0%
Percentage of resident population Black	14.5% (13.7%)	0%
Poverty rate	12.7% (5.2%)	0%
Unemployment rate	5.5% (2.1%)	0%
Violent crime rate (per 10,000)	45.9 (28.4)	20.0%
Police employees per capita (per 1,000)	2.9 (1.1)	0.03%
Political party of state legislature		0%
<i>Democrat</i>	887 (27.7%)	
<i>Republican</i>	1,731 (54.1%)	
<i>Split</i>	582 (18.2%)	

Main analysis

Results from the crude analyses including only year as a covariate are presented in Table 7. All associations of interest were close to the null and the 95% confidence intervals all included the null suggesting no association between county IPV arrest percentage and child maltreatment report rate as well as no associations for race-specific child maltreatment report rates. Results from the adjusted analyses are presented in Table 8. These results were consistent with the crude analysis, suggesting no association between county IPV arrest percentage and child maltreatment report rate for total child maltreatment reports and race-specific child maltreatment reports.

**Table 7: Crude associations between county IPV arrest percentage and child maltreatment report rate (N=3,200)**

<b>Outcome</b>	<b>Model 1</b>	<b>Model 2</b>	<b>Model 3</b>
	<b>RR (95% CI)</b>	<b>RR (95% CI)</b>	<b>RR (95% CI)</b>
Total child maltreatment report rate	1.005 (0.965, 1.048)	1.005 (0.965, 1.048)	1.006 (0.964, 1.048)
Black child maltreatment report rate	1.003 (0.956, 1.053)	1.003 (0.955, 1.053)	1.003 (0.955, 1.053)
Non-Hispanic white child maltreatment report rate	1.006 (0.963, 1.051)	1.006 (0.963, 1.051)	1.006 (0.962, 1.051)

Model 1: county random intercept model (crude model includes year only)

Model 2: Model 1 + state fixed effects

Model 3: county fixed effects model

Multiple imputation used to address missing data

**Table 8: Adjusted associations between county IPV arrest percentage and child maltreatment report (N=3,200)**

<b>Outcome</b>	<b>Model 1</b>	<b>Model 2</b>	<b>Model 3</b>
	<b>RR (95% CI)</b>	<b>RR (95% CI)</b>	<b>RR (95% CI)</b>
Total child maltreatment report rate	1.004 (0.965, 1.044)	1.004 (0.965, 1.044)	1.004 (0.965, 1.045)
Black child maltreatment report rate	1.003 (0.956, 1.053)	1.003 (0.956, 1.053)	1.003 (0.955, 1.054)
Non-Hispanic white child maltreatment report rate	1.006 (0.964, 1.050)	1.006 (0.964, 1.050)	1.006 (0.964, 1.050)

Model 1: county random intercept model (adjusted model includes year, population density, percentage of Black residents, poverty rate, unemployment rate, violent crime rate, police force size per capita, and political party in control of state legislature)

Model 2: Model 1 + state fixed effects

Model 3: county fixed effects model

Multiple imputation used to address missing data

**Table 9: Adjusted associations between county IPV arrest percentage and child maltreatment report by percentage of Black residents in the county-year (N=3,200)**

<b>Outcome</b>	<b>Group by % of Black residents</b>	<b>Model 1</b>	<b>Model 2</b>	<b>Model 3</b>
		<b>RR (95% CI)</b>	<b>RR (95% CI)</b>	<b>RR (95% CI)</b>
Total child maltreatment report rate	Low	1.013 (0.995, 1.032)	1.013 (0.995, 1.032)	1.013 (0.995, 1.032)
	High	0.992 (0.906, 1.087)	0.992 (0.906, 1.087)	0.992 (0.904, 1.089)
Black child maltreatment report rate	Low	1.013 (1.006, 1.021)	1.013 (1.006, 1.021)	1.014 (1.006, 1.021)
	High	1.000 (0.902, 1.107)	1.000 (0.902, 1.108)	1.000 (0.900, 1.110)
Non-Hispanic white child maltreatment report rate	Low	1.036 (0.999, 1.074)	1.035 (0.999, 1.074)	1.036 (0.999, 1.074)
	High	0.952 (0.869, 1.044)	0.952 (0.869, 1.044)	0.952 (0.868, 1.045)

Model 1: county random intercept model (adjusted model includes year, population density, percentage of Black residents, poverty rate, unemployment rate, violent crime rate, police force size per capita, and political party in control of state legislature)

Model 2: Model 1 + state fixed effects

Model 3: county fixed effects model

Multiple imputation used to address missing data

Low percentage of Black residents is 14.5% or less

### Interaction analysis

The majority of the results from the interaction analysis had confidence intervals that included the null, similar to the main analysis (Table 9). However, a notable pattern emerged in this analysis. The effect estimates suggest that the association between county IPV arrest percentage and child maltreatment report rate trends in the positive direction in counties with a lower percentage of Black residents and trends in the negative direction in counties with a higher percentage of Black residents. These divergent trends may explain why the main analysis produced effect estimates very close to the null. The one finding from the interaction analysis with high precision and a confidence interval that did not include the null was that in counties with a low percentage of Black residents, a standard deviation increase in the county's IPV arrest

percentage was associated with a 1.3-1.4% increase in the county's Black child maltreatment report rate (Models 1 and 2: RR=1.013 (95% CI: 1.006, 1.021); Model 3: RR=1.014 (95% CI: 1.006, 1.021). In counties with a low percentage of Black residents, the average Black child maltreatment report rate was 877 per 10,000, so a 1.3-1.4% increase would result in 11-12 additional reports per 10,000. The association between IPV arrest percentage and the non-Hispanic white child maltreatment report rate in counties with a low percentage of Black residents also trended positive but the confidence interval included the null.

### Sensitivity analyses

Table C4 in Appendix C presents the results from the complete case sensitivity analysis (N=2,033). The results from the complete case analysis demonstrated a very small negative association between county IPV arrest percentage and child maltreatment report rate in all models for total child maltreatment report rate (RR=0.998, 95% CI: 0.997, 0.999) and non-Hispanic white child maltreatment report rate (RR=0.993, 95% CI: 0.991, 0.995), but no association with Black child maltreatment report rate. These associations should be interpreted with caution given the very small magnitude and the high likelihood of bias due to the missing data not being missing completely at random.

Table C5 in Appendix C presents the results from the sensitivity analysis in which exposure data are lagged to precede the outcome by 1 year and covariates are lagged to precede the exposure by 1 year. The results were all null, consistent with the main analysis.

Table C6 in Appendix C presents the results from the sensitivity analysis with the absolute rate of IPV arrests as the exposure and controlling for the police-reported IPV incident rate. The results were all null, consistent with the main analysis.

Table C7 in Appendix C presents the results from the sensitivity analysis replicating the interaction analysis using percentage of non-Hispanic white residents. The results are consistent with the main results, trending toward a negative association in counties with a low percentage of non-Hispanic white residents and trending toward a positive association in counties with a high percentage of non-Hispanic white residents. The one association with high precision and a confidence interval that did not include the null was that in counties with a low percentage of non-Hispanic white residents, the IPV arrest percentage is associated with a lower non-Hispanic white child maltreatment report rate (RR=0.943, 95% CI: 0.930, 0.957). This finding together with the finding from the main interaction analysis suggest that the association between IPV policing and family surveillance trends negative for Black and white children living in more diverse counties.

### **3.4 Discussion**

#### *Summary of findings*

The findings from this study demonstrate no detectable overall association between the percentage of police-reported IPV incidents that result in arrest and child maltreatment report rate at the county level. In addition, there was no significant evidence to suggest that this overall association differs for the Black child maltreatment report rate compared to the non-Hispanic white child maltreatment report rate. Associations were null in between-county and within-county models. These findings are inconsistent with existing survey research and qualitative research that provide evidence that IPV policing is associated with child protective services involvement through direct coordination between police and the family policing system.

The interaction analysis provided additional context to interpret the null findings, suggesting that the association between IPV policing practice and family surveillance may be

positive in counties with a lower Black population and negative in counties with a higher Black population. Specifically, the results demonstrated that in counties with a low Black population, the percentage of IPV incidents that result in arrest is positively associated with the Black child maltreatment report rate. This may suggest that the intersection between systems of IPV policing and family surveillance is heightened in jurisdictions with fewer Black residents and/or in jurisdictions with lower population density and that this intersection of carceral systems results in more targeted surveillance of the Black families living in these jurisdictions. This is consistent with prior research demonstrating that for Black families, a higher white county population composition predicts higher police-initiated child maltreatment reporting (66).

There is a growing body of research documenting that certain racialized disparities in police surveillance and arrests are stronger for Black people living in predominantly white neighborhoods. For example, studies have shown that Black drug arrests increase as a neighborhood's white population increases (36), Black adolescents report higher rates of police discrimination in predominantly white neighborhoods (204), and racialized disparities in school discipline are worse for Black students in more integrated schools (205). It is hypothesized that Black people may be at increased risk of surveillance and punishment in predominantly white contexts because these contexts may have a stronger culture of white supremacy, and white community members and government agencies may be more emboldened to profile and target Black community members because they are "hypervisible" (206,207) and pose a "racial threat" to the community (204).

In this study, findings suggest that the link between IPV policing and family surveillance is stronger for Black families in counties with a lower percentage of Black residents, which may indicate that coordination between police and family policing systems is stronger when it comes

to surveilling Black families in these counties or that there are other stronger drivers of family surveillance for Black families in counties with a higher percentage of Black residents. These findings are inconsistent with the alternative explanation that an association between IPV policing and family surveillance is due to IPV arrest being an effective tool for identifying families engaging in child maltreatment because there is no reason to believe that this association would differ by county racial composition. Overall, this is the first study to assess these relationships quantitatively at the county level, so additional research is needed to verify these results. In particular, the lack of an overall association between IPV policing and family surveillance requires further investigation given the survey and qualitative research documenting the presence of an association. The null association may be due to a cancelling of effects between predominantly white vs. more diverse counties, however more research is needed to explore this, ideally using individual family-level data.

### *Strengths and limitations*

The present study has strengths and limitations. First, with respect to strengths, this study involves the creation of a novel county-level dataset linking IPV arrest data and child welfare data for the first time. The NCANDS dataset has a wealth of information about each child maltreatment report and the linkage of NIBRS and NCANDS data could allow for further exploration of relationships between IPV arrest practice and child protective service responses. For example, future research could explore intersections between IPV, child protective services, and substance use given the growing trend of drug testing women in hospitals without consent and reporting to child welfare agencies (208).

The present study also has important limitations. First, as with any observational study, there is a risk of unmeasured confounding. I believe I identified a set of covariates that, after

adjustment, sufficiently blocked all confounding pathways between the exposure and outcome. However, given that the processes by which criminalization and policing are disproportionately distributed across counties are complex, there remains a risk of residual confounding by some unmeasured factor.

A second limitation that I discussed above is the risk of selection bias due to data suppression in the NCANDS data for counties with fewer than 1,000 reports of child maltreatment. In order to minimize the risk of selection bias, I restricted the target population to a relatively small number of urban and suburban U.S. counties. As a result, I minimized the risk that the outcome of interest will influence selection into the study, however I thereby restricted the generalizability of the study to urban and suburban counties only. An advantage of restricting the study population to large urban and suburban U.S. counties is that I minimized the risk of measurement error in the exposure and outcome data. Given that both NCANDS and NIBRS are national administrative datasets, there is likely variation in reporting across jurisdictions. Specifically, smaller jurisdictions with fewer resources are likely to have lower quality data reporting (209). By restricting this analysis to large counties that typically have more resources for data monitoring and reporting, I minimized the risk of poor data reporting. However, the interaction analysis demonstrated that the association between IPV policing and family surveillance may be stronger and more positive in counties with a lower percentage of Black residents, which also reflected counties with a lower population density. By excluding small, rural counties from the analysis, I may have excluded counties where the association of interest is strongest.

Furthermore, as stated in the Methods section, the NCANDS dataset includes all child maltreatment reports that resulted in an investigation by a child welfare agency. The null

findings from this study could be due to the fact that this is a broad outcome, which is expected to include child maltreatment reports that ultimately did not result in further intervention by the child welfare agency (such as mandated family counseling or child removal). Future researchers may consider replicating this analysis with a more severe outcome such as child removal rates. Similarly, the null findings could be due to the use of IPV arrest as the exposure of interest rather than any police encounter. Data on IPV incidents that do not result in any police interaction were not available for this analysis. If such data were available, future research could consider the effect of any police encounter on family surveillance rather than the effect of arrest vs. no arrest.

An additional limitation related to the use of federal administrative datasets is that there was substantial missingness, particularly in the exposure of interest. While I believe that the use of MICE to deal with missing data that were not missing completely at random was the best available solution to this problem, there is no way to validate that the multiple imputation approach appropriately mimicked an accurate, complete dataset. Future research using a more complete data source for local IPV policing practice, perhaps through primary data collection, would be a valuable contribution to the literature.

Finally, the present study is limited to a recent 20-year period (2000-2019) due to a lack of consistent data prior to 2000. This restricted time period is advantageous in that it reflects the most recent data available and thus has more relevance for current policy. However, the study period does not reflect the time period over which there were substantial changes in IPV policing policy across the country: the 1980s and 1990s. There is expected to be less variation in IPV arrest percentages in the 2000s and 2010s compared with these prior decades, and as a result it may have been more difficult to detect small effects.

### *Conclusions and next steps*

This is the first study to evaluate the relationship between the police-centric response to IPV and child welfare. Given growing nationwide recognition of the role of the family policing system as an auxiliary of mass incarceration and mass criminalization, and as yet another institutional product of structural racism in our nation's response to social problems, this research question is of particular topical relevance (64–66). National survey data suggest that anti-violence service providers perceive a relationship between how police respond to IPV incidents and the triggering of child protective services involvement (62), however this relationship has never been empirically evaluated at the population level. This study is also one of the first to consider racialized differences in the potential consequences of the police-centric response to IPV. Given the harmful impacts of both policing and family policing systems on the health of Black families in the U.S., it is critical to estimate whether Black families are more strongly impacted by IPV policing than white families and how the racialized context of the areas in which they live further shapes their risks.

Given that this is the first study to examine the intersection between IPV policing and family surveillance at the county level, additional research is warranted to assess whether the null findings can be replicated. While this study demonstrated no statistically significant overall associations at the county level, this does not mean that IPV policing is not in fact linked with family surveillance. The interaction analysis suggests that family surveillance is a negative consequence of IPV policing specifically for Black families living in counties with a low percentage of Black residents. In addition, survey research and qualitative research have documented the risk of child welfare intervention and child removal as a result of police intervention for IPV (62,83,84). Novel data sources, beyond federal administrative data, may be

better suited to the study of this relationship, including primary data collection of families' experience with the family policing system after exposure to IPV policing.

## Conclusion

In this dissertation, I synthesized the existing evidence concerning the negative and racialized consequences of the police-centric response to IPV through a systematic scoping review. I then used a difference-in-differences approach to estimate the effect of state-level changes in mandatory arrest and other warrantless arrest legislation on mortality of IPV survivors from 1980-2019 using women's mortality (age 20-54) as a proxy. I also assessed if there were racialized differences in this effect. Finally, I used between-county and within-county models to estimate the effect of county-level IPV arrest practice on county-level rates of child maltreatment reports from 2000-2019 in large U.S. counties. Again, I assessed if there were racialized differences in this effect.

### *Summary of findings*

In the scoping review, I found that there is a dearth of population-level longitudinal research on the negative and racialized consequences of the police-centric response to IPV. The evidence base to date has primarily consisted of individual-level randomized trials, which have collectively provided no evidence of a protective effect of arrest on IPV incidence at the individual level and are ill-suited to measure population-level consequences of a state-level policy like mandatory arrest laws.

The population-level research that exists on the potential negative consequences of IPV policing have generally produced mixed findings. These mixed findings are likely due in part to ongoing disagreement among researchers and legal scholars concerning the classification of IPV warrantless arrest laws. Mixed findings may also be due to a lack of sub-group analyses to identify and measure potential nuance in differential effects of IPV policing for different social groups. In particular, the scoping review illuminated the lack of research assessing racialized

differences in the consequences of IPV policing, despite the increased risk of IPV and police intervention for Black communities and other socially marginalized groups (40,63,129–131).

While the overall evidence base concerning the population-level negative consequences of the police-centric response to IPV is limited and mixed, the literature demonstrates strong evidence that one negative consequence of mandatory arrest laws is the increased risk of arrest for women and survivors (49,92–95).

After establishing this gap in the literature concerning empirical, population-level studies of the negative and racialized consequences of IPV policing, I aimed to begin filling this gap with the two empirical aims of this dissertation (Chapters 2 and 3). Similar to the limited available literature, the empirical aims of this dissertation also provide somewhat mixed evidence regarding the negative consequences of the police-centric response to IPV. In Chapter 2, mandatory arrest laws were not found to be appreciably associated with an increased rate of women's all-cause mortality (age 20-54). However, preferred arrest laws were associated with an increased rate of women's all-cause mortality (age 20-54). There is no clear explanation for why preferred arrest laws were found to have a harmful effect on women's mortality while discretionary and mandatory arrest laws were not. This finding could reflect within-state variation in actual implementation of warrantless arrest laws; however, additional research is needed to verify this finding.

In Chapter 3, county-level IPV policing practice was not statistically significantly associated with increased family surveillance overall, however, it was associated with increased family surveillance specifically for Black families in counties with a low percentage of Black residents. This finding is consistent with existing research demonstrating that the rate of police-initiated child maltreatment reporting increases for Black families as the percentage of white

residents in a county increases. This finding may indicate that coordination between police and family regulation systems is stronger when it comes to surveilling Black families in predominantly white counties or that there are other stronger drivers of family surveillance for Black families in counties with a higher percentage of Black residents. Together, the novel empirical findings presented in Chapters 2 and 3 suggest that there is some evidence of harmful negative and racialized consequences of the police-centric response to IPV and those consequences may depend on the type of policing response and the context in which it occurs.

### *Strengths and limitations*

In addition to the study-specific strengths and limitations discussed in each chapter, this dissertation has general strengths and limitations that are important to consider. First, a strength of this dissertation is the use of sophisticated methodological techniques in the two empirical chapters that aim to address potential pitfalls of observational research with administrative data sources. For example, I used transparent, theoretical frameworks to identify an appropriate set of potential confounders to include in each model in an effort to block sources of confounding bias. I also used analytic modeling techniques that further addressed the risk of confounding and bias due to spatial clustering (e.g., the use of a difference-in-differences analysis in Chapter 2 to control for baseline differences between states; the use of random intercept and fixed effects models in Chapter 3 to account for repeated measures by county and control for baseline differences between counties, respectively; and the inclusion of robust standard errors). Lastly, in Chapter 3, I used multiple imputation using chained equations to address patterned missing data in the administrative data sources.

The state- and county-level analyses employed in this dissertation have the benefit of measuring population-level negative consequences of IPV policing, reflecting how IPV policing

impacts residents of entire counties and states. However, these analyses are also less precise than analyses focused specifically on IPV survivors and their families. This analytic choice was primarily a product of data availability. This lack of precision could in part explain some of the null findings. Future research using primary data collection to precisely measure survivor health and well-being outcomes would be an important contribution to the literature.

Finally, an important limitation of this dissertation is the use of national administrative datasets. While these datasets provide the most comprehensive, publicly available data on the topics studied, they are often incomplete and may be inaccurate in some cases. For example, for confidentiality reasons, the National Vital Statistics System suppresses data for jurisdictions with fewer than 10 deaths and NCANDS suppresses data for counties with fewer than 1,000 child maltreatment reports. Due to this data suppression, I restricted the study samples: in the race-stratified analysis in Chapter 2, states with a small number of Black women aged 20-54 were excluded and the analysis in Chapter 3 was restricted to a small number of urban and suburban counties. The ability to access complete data from the National Vital Statistics System and NCANDS would expand the generalizability of the analysis and ensure that there is no risk of selection bias. Additionally, the use of NIBRS data to measure county-level IPV policing practice is a notable limitation of the dissertation because not all police agencies report to NIBRS and because there are numerous reasons to question the accuracy of NIBRS data given that there are delays in reporting, agencies may have different decision-making processes for reporting, and local agencies and the federal government have mixed incentives concerning how to report crime and arrest rates (210,211). The accuracy of NIBRS data cannot be empirically validated and there is no obvious alternate option to obtain comparable county-level measures of police arrest practice.

### *Implications and next steps*

Ever since the first mandatory arrest laws were established in response to the results of the Minneapolis Domestic Violence Experiment and growing federal support for mandatory arrest, a debate has ensued concerning the effectiveness of these laws, and IPV policing generally, to prevent or reduce IPV. Even today, heated debate continues as in the 2021 election for Manhattan District Attorney during which pro- and anti-mandatory arrest law arguments played a central role in campaigns with leading candidates taking opposing positions (123). There also continues to be tension between local law enforcement and domestic violence advocacy coalitions over the police-centric response to IPV where many local domestic violence coalitions are calling for divestment from the criminal legal system and experiencing backlash from their local police agencies, such as losing partnerships and referrals (33,124,125).

Yet after nearly three decades of research on the topic, there is still no compelling empirical evidence to suggest that mandatory arrest laws and IPV policing are effectively serving survivors by reducing or preventing IPV. It is also likely that IPV policing is driving local incarceration in many jurisdictions, particularly those enforcing mandatory arrest policies. This dissertation now contributes to the growing body of evidence documenting negative and racialized consequences of IPV policing for survivors. Together, the research literature suggests that the police-centric response to IPV does not have measurable benefits and is causing additional harms for survivors, families, and society at large. Therefore, it is unclear who this response is serving and there are many potential interventions for IPV that we are failing to test and invest in due to our reliance on this police-centric response.

This dissertation is among the first empirical, population-level studies of the negative and racialized consequences of the police-centric response to IPV for survivor health and well-being

that extend beyond IPV revictimization. It is novel in its application of a critical public health analysis to this topic, considering the long-term population health consequences of IPV policing that reflect broader constructs of survivor health and well-being other than short-term criminal legal measures of victimization. While this study is among the first of its kind, it is situated within a broader research literature, including randomized trials and qualitative research, documenting the limitations of the police-centric response to IPV and its potential negative consequences. This dissertation did not test the effectiveness of alternative interventions or policy responses, but the existing evidence base on the ineffectiveness of the police-centric response to IPV together with the demands of a growing coalition of survivor organizations are enough to warrant altering course. There are a number of immediate policy changes that could be enacted at the federal, state, and local levels to limit our overreliance on the police-centric response to IPV and build a more survivor-centered, holistic response.

In March 2022, President Biden reauthorized the Violence Against Women Act (VAWA) (212). The law includes provisions to provide financial incentives to states with pro-arrest IPV laws, including mandatory arrest laws, despite a lack of evidence to support their effectiveness (213). Prior to its next reauthorization, Congress has the opportunity to eliminate these provisions from VAWA and redirect that federal funding toward social service interventions to address the root causes of IPV and be more responsive to survivors needs, such as through investments in housing for survivors and abusive partners. VAWA is the primary federal tool that shapes the funding landscape of IPV services in the nation, so it serves as an important lever to shift funding away from policing and criminal legal intervention and toward social service and public health infrastructure.

At the state level, state legislatures can move to eliminate mandatory and preferred arrest laws, given the lack of evidence of their effectiveness and documented negative consequences. States also manage crime victim compensation programs, and many states require cooperation with law enforcement (e.g., through a police report or statement from the prosecutor's office) to be eligible for compensation. An additional policy change that states can enact is removing cooperation with law enforcement as an eligibility requirement for crime victim compensation.

Lastly, there are many steps local jurisdictions can take while waiting for legislative change at the state and federal levels. Local jurisdictions should consider investing in IPV interventions that are responsive to survivors' primary needs such as emergency and long-term housing and survivor funds/grant programs. Cities and counties may also consider piloting unarmed community responses to promote early intervention in IPV cases. While additional research documenting and quantifying the negative and racialized consequences of the police-centric response to IPV will be valuable, the current evidence base suggests that we can serve survivors more effectively by divesting from IPV policing and investing in survivor-driven, non-violent strategies for IPV prevention.

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## Appendix A

**Table A1: Preferred Reporting Items for Systematic reviews and Meta-Analyses extension for Scoping Reviews (PRISMA-ScR) Checklist**

SECTION	ITEM	PRISMA-ScR CHECKLIST ITEM	REPORTED ON PAGE #
<b>TITLE</b>			
Title	1	Identify the report as a scoping review.	6
<b>ABSTRACT</b>			
Structured summary	2	Provide a structured summary that includes (as applicable): background, objectives, eligibility criteria, sources of evidence, charting methods, results, and conclusions that relate to the review questions and objectives.	N/A
<b>INTRODUCTION</b>			
Rationale	3	Describe the rationale for the review in the context of what is already known. Explain why the review questions/objectives lend themselves to a scoping review approach.	6-13
Objectives	4	Provide an explicit statement of the questions and objectives being addressed with reference to their key elements (e.g., population or participants, concepts, and context) or other relevant key elements used to conceptualize the review questions and/or objectives.	13
<b>METHODS</b>			
Protocol and registration	5	Indicate whether a review protocol exists; state if and where it can be accessed (e.g., a Web address); and if available, provide registration information, including the registration number.	15
Eligibility criteria	6	Specify characteristics of the sources of evidence used as eligibility criteria (e.g., years considered, language, and publication status), and provide a rationale.	14
Information sources*	7	Describe all information sources in the search (e.g., databases with dates of coverage and contact with authors to identify additional sources), as well as the date the most recent search was executed.	13
Search	8	Present the full electronic search strategy for at least 1 database, including any limits used, such that it could be repeated.	13
Selection of sources of evidence†	9	State the process for selecting sources of evidence (i.e., screening and eligibility) included in the scoping review.	14
Data charting process‡	10	Describe the methods of charting data from the included sources of evidence (e.g., calibrated forms or forms that have been tested by the team before their use, and whether data charting was done independently or in duplicate) and any processes for obtaining and confirming data from investigators.	14-15
Data items	11	List and define all variables for which data were sought and any assumptions and simplifications made.	15
Critical appraisal of individual sources of evidence§	12	If done, provide a rationale for conducting a critical appraisal of included sources of evidence; describe the methods used and how this information was used in any data synthesis (if appropriate).	N/A
Synthesis of results	13	Describe the methods of handling and summarizing the data that were charted.	15

SECTION	ITEM	PRISMA-ScR CHECKLIST ITEM	REPORTED ON PAGE #
<b>RESULTS</b>			
Selection of sources of evidence	14	Give numbers of sources of evidence screened, assessed for eligibility, and included in the review, with reasons for exclusions at each stage, ideally using a flow diagram.	15-16
Characteristics of sources of evidence	15	For each source of evidence, present characteristics for which data were charted and provide the citations.	Table 1
Critical appraisal within sources of evidence	16	If done, present data on critical appraisal of included sources of evidence (see item 12).	N/A
Results of individual sources of evidence	17	For each included source of evidence, present the relevant data that were charted that relate to the review questions and objectives.	Table 1
Synthesis of results	18	Summarize and/or present the charting results as they relate to the review questions and objectives.	15-32
<b>DISCUSSION</b>			
Summary of evidence	19	Summarize the main results (including an overview of concepts, themes, and types of evidence available), link to the review questions and objectives, and consider the relevance to key groups.	32-33
Limitations	20	Discuss the limitations of the scoping review process.	34-35
Conclusions	21	Provide a general interpretation of the results with respect to the review questions and objectives, as well as potential implications and/or next steps.	35-36
<b>FUNDING</b>			
Funding	22	Describe sources of funding for the included sources of evidence, as well as sources of funding for the scoping review. Describe the role of the funders of the scoping review.	vi

JBIG = Joanna Briggs Institute; PRISMA-ScR = Preferred Reporting Items for Systematic reviews and Meta-Analyses extension for Scoping Reviews.

\* Where *sources of evidence* (see second footnote) are compiled from, such as bibliographic databases, social media platforms, and Web sites.

† A more inclusive/heterogeneous term used to account for the different types of evidence or data sources (e.g., quantitative and/or qualitative research, expert opinion, and policy documents) that may be eligible in a scoping review as opposed to only studies. This is not to be confused with *information sources* (see first footnote).

‡ The frameworks by Arksey and O'Malley (6) and Levac and colleagues (7) and the JBI guidance (4, 5) refer to the process of data extraction in a scoping review as data charting.

§ The process of systematically examining research evidence to assess its validity, results, and relevance before using it to inform a decision. This term is used for items 12 and 19 instead of "risk of bias" (which is more applicable to systematic reviews of interventions) to include and acknowledge the various sources of evidence that may be used in a scoping review (e.g., quantitative and/or qualitative research, expert opinion, and policy document).

From: Tricco AC, Lillie E, Zarin W, O'Brien KK, Colquhoun H, Levac D, et al. PRISMA Extension for Scoping Reviews (PRISMA-ScR): Checklist and Explanation. *Ann Intern Med.* 2018;169:467–473. doi: [10.7326/M18-0850](https://doi.org/10.7326/M18-0850).

## **Appendix B**

### **Discrepancies between my classification of warrantless arrest laws and classification schema from Chin and Cunningham (2019)**

- 1) For Kansas, Chin and Cunningham, 2019 (30) reported that the state enacted a mandatory arrest law in 1991 while Zeoli et al., 2011 (161) reported that the law was enacted in 1992 and that prior to the enactment of the mandatory arrest law, Kansas had a previous general warrantless arrest law for a number of offenses including IPV. This earlier discretionary arrest law was enacted in 1984. I reviewed the statutes in WestLaw and determined that the prior discretionary arrest law is relevant for IPV and that the mandatory arrest law was approved April 25, 1991 but went into effect on January 1, 1992.
- 2) For Michigan, the American Bar Association, 2011 (160) schema identified a more recent preferred arrest law that the state enacted in 1995 after it had enacted a discretionary arrest law in 1978.
- 3) For New Mexico, I selected the 1979 effective date in accordance with Zeoli et al., 2011 (161) because I confirmed via WestLaw that this 1979 discretionary arrest law went into effect prior to the 1987 discretionary arrest law.
- 4) For West Virginia, I reviewed the statutes in WestLaw and confirmed that the effective date was 1994 in accordance with Zeoli et al., 2011 (161) and not 1993 as reported by Chin and Cunningham, 2019 (30).

**Table B1: Effect of warrantless arrest laws on homicide rate for women aged 20-54**

<b>Statute type</b>	<b>Model 1</b>		<b>Model 2</b>		<b>Model 3</b>	
<b>Categorical<sup>1</sup></b>	<b>RR</b>	<b>95% CI</b>	<b>RR</b>	<b>95% CI</b>	<b>RR</b>	<b>95% CI</b>
Discretionary	0.990	0.927, 1.058	0.969	0.923, 1.018	0.967	0.921, 1.015
Preferred	0.991	0.887, 1.107	0.927	0.860, 1.000	0.943	0.876, 1.015
Mandatory	0.990	0.875, 1.121	0.962	0.885, 1.047	0.969	0.892, 1.054
<b>Dichotomous<sup>2</sup></b>						
Mandatory	0.993	0.871, 1.132	0.977	0.895, 1.067	0.983	0.901, 1.072

Model 1: Homicide rate from FBI Supplementary Homicide Report (1980-2016)

Model 2: Homicide rate from National Vital Statistics System where suppressed data for state-year homicide counts smaller than 10 are excluded from model (1980-2019)

Model 3: Homicide rate from National Vital Statistics System where suppressed data for state-year homicide counts smaller than 10 are replaced with 4.5 (1980-2019)

All models are fully adjusted

<sup>1</sup>Referent group is no warrantless arrest law

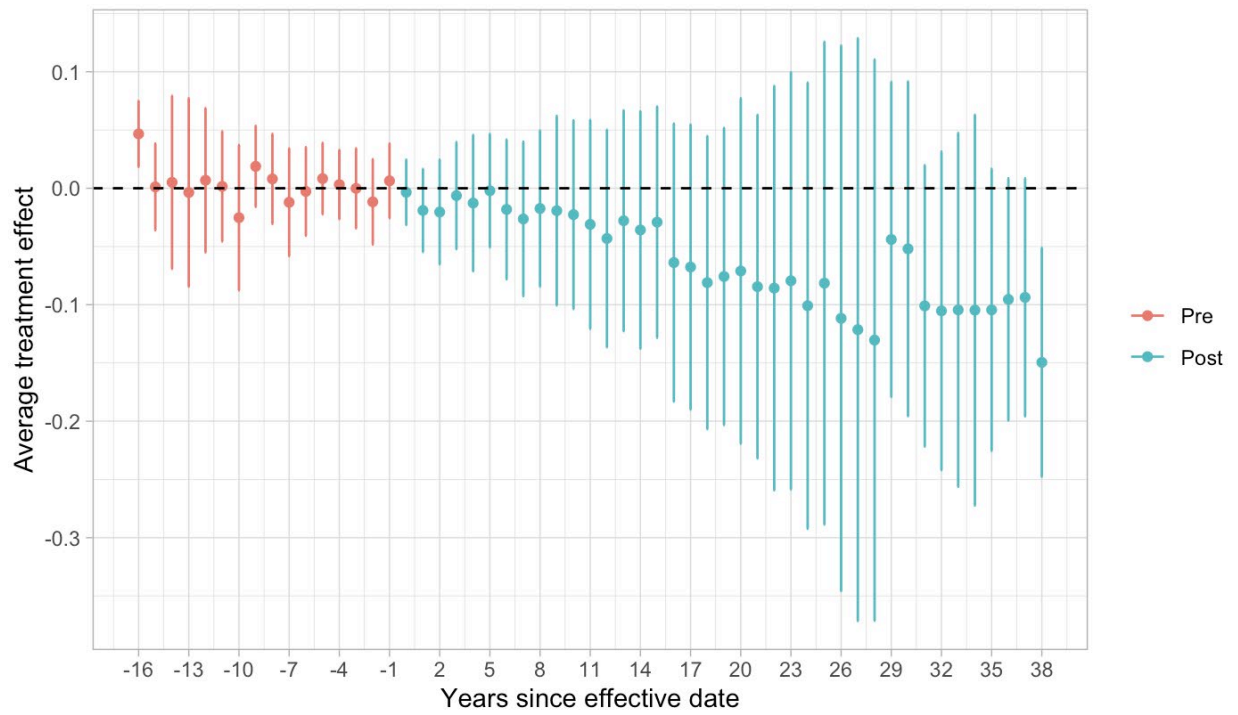
<sup>2</sup>Referent group is no mandatory arrest law

**Table B2: Aggregate of group-time average treatment effects using Callaway Sant'Anna approach**

<b>Statute type</b>		
<b>Dichotomous</b>	<b>RR</b>	<b>95% CI</b>
Mandatory	0.949	0.893, 1.008

Assuming unconditional parallel trends: no time-varying covariates included  
Referent group is no mandatory arrest law

Figure B1: Average treatment effect by years since effective date of law using Callaway Sant'Anna approach



## Appendix C

**Table C1: Characteristics of county-years with complete IPV arrest percentage compared to county-years missing IPV arrest percentage**

	<b>Complete (n=2,203)</b>	<b>Missing (n=997)</b>	
<b>Variable</b>	<b>Mean (SD) or n (%)</b>	<b>Mean (SD) or n (%)</b>	<b>p-value</b>
Total child maltreatment report rate (per 10,000)	438.3 (231.1)	366.5 (192.4)	p<.01
Black child maltreatment report rate (per 10,000)	866.0 (517.1)	659.3 (403.4)	p<.01
Non-Hispanic white child maltreatment report rate (per 10,000)	299.9 (196.8)	294.0 (209.0)	p=.49
Population density (people per mi <sup>2</sup> )	1,168.7 (1,623.1)	976.3 (793.4)	p<.01
Percentage of resident population Black	13.8 (12.0)	15.9 (16.9)	p<.01
Poverty rate	12.4 (4.6)	13.4 (6.1)	p<.01
Unemployment rate	5.4 (2.1)	5.8 (2.1)	p<.01
Violent crime rate (per 10,000)	3,072.5 (4,395.7)	2,749.0 (2,746.0)	p=.02
Police employees per capita (per 1,000)	2.9 (1.0)	3.1 (1.2)	p<.01
Political party of state legislature			p<.01
<i>Democrat</i>	600 (27.2%)	287 (28.8%)	
<i>Republican</i>	1,264 (57.4%)	467 (46.8%)	
<i>Split</i>	339 (15.4%)	243 (24.4%)	

**Table C2: Characteristics of county-years with complete child maltreatment report rate compared with county-years with missing child maltreatment report rate**

	<b>Complete (n=2,808)</b>	<b>Missing (n=392)</b>	
<b>Variable</b>	<b>Mean (SD) or n (%)</b>	<b>Mean (SD) or n (%)</b>	<b>p-value</b>
IPV arrest percentage	55.4% (19.6%)	54.5% (21.5%)	p=.59
Population density (people per mi <sup>2</sup> )	1,147.0 (1,467.3)	834.7 (981.6)	p<.01
Percentage of resident population Black	14.7 (13.5)	13.0 (15.1)	p=.04
Poverty rate	13.1 (5.2)	10.2 (4.0)	p<.01
Unemployment rate	5.6 (2.1)	4.9 (2.0)	p<.01
Violent crime rate (per 10,000)	3,083.8 (3,983.9)	2,248.7 (3,249.8)	p<.01
Police employees per capita (per 1,000)	2.9 (1.1)	2.9 (1.3)	p=.48
Political party of state legislature			p<.01
<i>Democrat</i>	781 (27.8%)	106 (27.0%)	
<i>Republican</i>	1,578 (56.2%)	153 (39.0%)	
<i>Split</i>	449 (16.0%)	133 (33.9%)	

**Table C3: Characteristics of county-years and missingness for each variable by percentage of Black residents**

Variable	Low % of Black residents (n=2,020)		High % of Black residents (n=1,180)	
	Mean (SD) or n (%)	Percent missing	Mean (SD) or n (%)	Percent missing
IPV arrest percentage	59.9% (20.0%)	30.6%	47.4% (16.5%)	32.0%
Total child maltreatment report rate (per 10,000)	394.0 (210.5)	13.0%	459.4 (237.9)	11.0%
Black child maltreatment report rate (per 10,000)	876.7 (552.9)	13.0%	695.5 (358.1)	11.0%
Non-Hispanic white child maltreatment report rate (per 10,000)	290.6 (178.8)	13.0%	311.1 (231.2)	11.0%
Population density (people per mi <sup>2</sup> )	769.6 (660.4)	0%	1,689.4 (2,047.5)	0%
Poverty rate	11.7% (5.4%)	0%	14.4% (4.3%)	0%
Unemployment rate	5.4% (2.1%)	0%	5.7% (2.1%)	0%
Violent crime rate (per 10,000)	35.1 (19.5)	19.4%	64.8 (31.5)	21.0%
Police employees per capita (per 1,000)	2.5 (0.7)	0%	3.6 (1.3)	0%
Political party of state legislature		0%		0%
<i>Democrat</i>	656 (32.5%)		231 (19.6%)	
<i>Republican</i>	974 (48.2%)		757 (64.2%)	
<i>Split</i>	390 (19.3%)		192 (16.3%)	

Low percentage of Black residents is 14.5% or less

**Table C4: Complete case analysis of adjusted associations between county IPV arrest percentage and child maltreatment report (N=2,033)**

<b>Outcome</b>	<b>Model 1</b>	<b>Model 2</b>	<b>Model 3</b>
	<b>RR (95% CI)</b>	<b>RR (95% CI)</b>	<b>RR (95% CI)</b>
Total child maltreatment report rate	0.998 (0.997, 0.999)	0.998 (0.997, 0.999)	0.998 (0.997, 0.999)
Black child maltreatment report rate	1.000 (0.998, 1.002)	1.000 (0.998, 1.002)	1.000 (0.998, 1.002)
Non-Hispanic white child maltreatment report rate	0.993 (0.991, 0.995)	0.993 (0.991, 0.995)	0.993 (0.991, 0.995)

Model 1: county random intercept model (adjusted model includes year, population density, percentage of Black residents, poverty rate, unemployment rate, violent crime rate, police force size per capita, and political party in control of state legislature)

Model 2: Model 1 + state fixed effects

Model 3: county fixed effects model

**Table C5: Sensitivity analysis of adjusted associations between county IPV arrest percentage and child maltreatment report with a lag (N=3,200)**

<b>Outcome</b>	<b>Model 1</b>	<b>Model 2</b>	<b>Model 3</b>
	<b>RR (95% CI)</b>	<b>RR (95% CI)</b>	<b>RR (95% CI)</b>
Total child maltreatment report rate	1.004 (0.975, 1.034)	1.004 (0.975, 1.034)	1.004 (0.975, 1.034)
Black child maltreatment report rate	1.000 (0.964, 1.037)	1.000 (0.964, 1.037)	1.000 (0.963, 1.037)
Non-Hispanic white child maltreatment report rate	1.010 (0.959, 1.064)	1.010 (0.959, 1.064)	1.010 (0.959, 1.064)

Model 1: county random intercept model (adjusted model includes year, population density, percentage of Black residents, poverty rate, unemployment rate, violent crime rate, police force size per capita, and political party in control of state legislature)

Model 2: Model 1 + state fixed effects

Model 3: county fixed effects model

Multiple imputation used to address missing data

County IPV arrest percentage is lagged by 1 year prior to child maltreatment report and covariates are lagged 2 years prior to child maltreatment report

**Table C6: Sensitivity analysis of adjusted associations between county IPV arrest rate and child maltreatment report (N=3,200)**

<b>Outcome</b>	<b>Model 1</b>	<b>Model 2</b>	<b>Model 3</b>
	<b>RR (95% CI)</b>	<b>RR (95% CI)</b>	<b>RR (95% CI)</b>
Total child maltreatment report rate	0.989 (0.968, 1.010)	0.989 (0.968, 1.010)	0.989 (0.968, 1.010)
Black child maltreatment report rate	0.976 (0.908, 1.049)	0.976 (0.908, 1.049)	0.976 (0.907, 1.050)
Non-Hispanic white child maltreatment report rate	0.974 (0.908, 1.044)	0.974 (0.908, 1.045)	0.974 (0.907, 1.045)

Model 1: county random intercept model (adjusted model includes year, population density, percentage of Black residents, poverty rate, unemployment rate, violent crime rate, police force size per capita, and political party in control of state legislature)

Model 2: Model 1 + state fixed effects

Model 3: county fixed effects model

Multiple imputation used to address missing data

**Table C7: Sensitivity analysis of adjusted associations between county IPV arrest percentage and child maltreatment report by percentage of non-Hispanic white residents in the county-year (N=3,200)**

<b>Outcome</b>	<b>Group by % of non-Hispanic white residents</b>	<b>Model 1</b>	<b>Model 2</b>	<b>Model 3</b>
		<b>RR (95% CI)</b>	<b>RR (95% CI)</b>	<b>RR (95% CI)</b>
Total child maltreatment report rate	Low	0.985 (0.945, 1.027)	0.985 (0.945, 1.027)	0.985 (0.945, 1.028)
	High	1.029 (0.942, 1.124)	1.029 (0.941, 1.125)	1.029 (0.939, 1.128)
Black child maltreatment report rate	Low	0.988 (0.925, 1.054)	0.988 (0.925, 1.054)	0.988 (0.924, 1.055)
	High	1.032 (0.944, 1.128)	1.032 (0.944, 1.128)	1.032 (0.943, 1.130)
Non-Hispanic white child maltreatment report rate	Low	0.943 (0.930, 0.957)	0.943 (0.930, 0.957)	0.943 (0.930, 0.957)
	High	1.049 (0.978, 1.126)	1.049 (0.978, 1.126)	1.049 (0.977, 1.127)

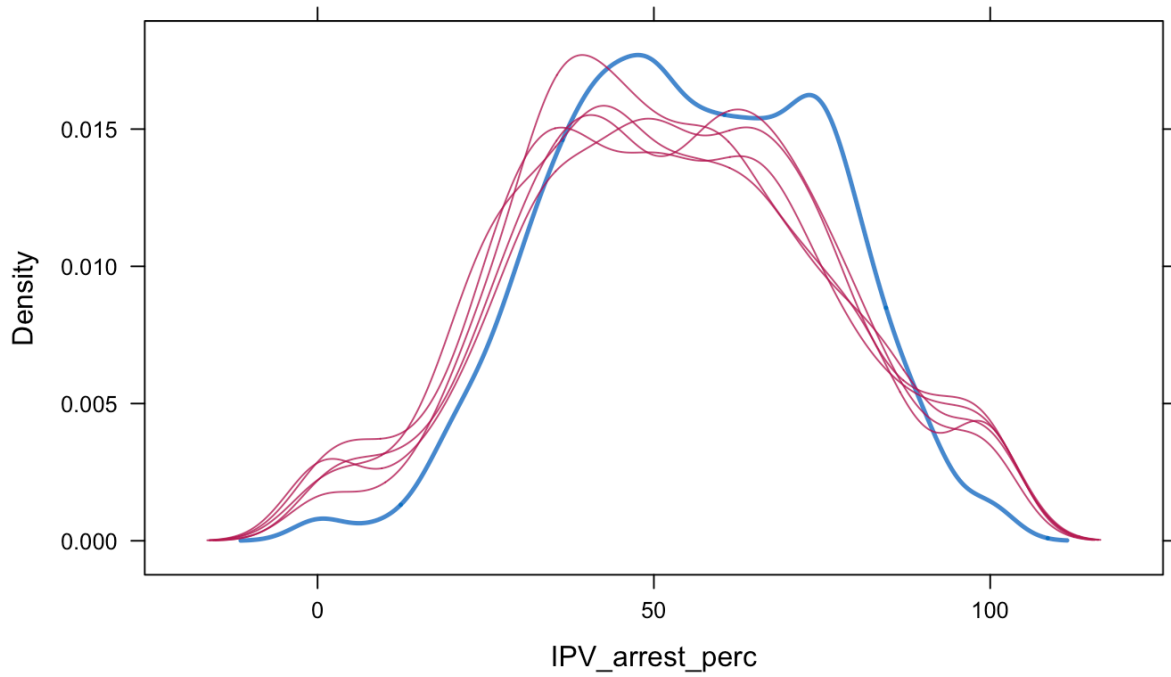
Model 1: county random intercept model (adjusted model includes year, population density, percentage of Black residents, poverty rate, unemployment rate, violent crime rate, police force size per capita, and political party in control of state legislature)

Model 2: Model 1 + state fixed effects

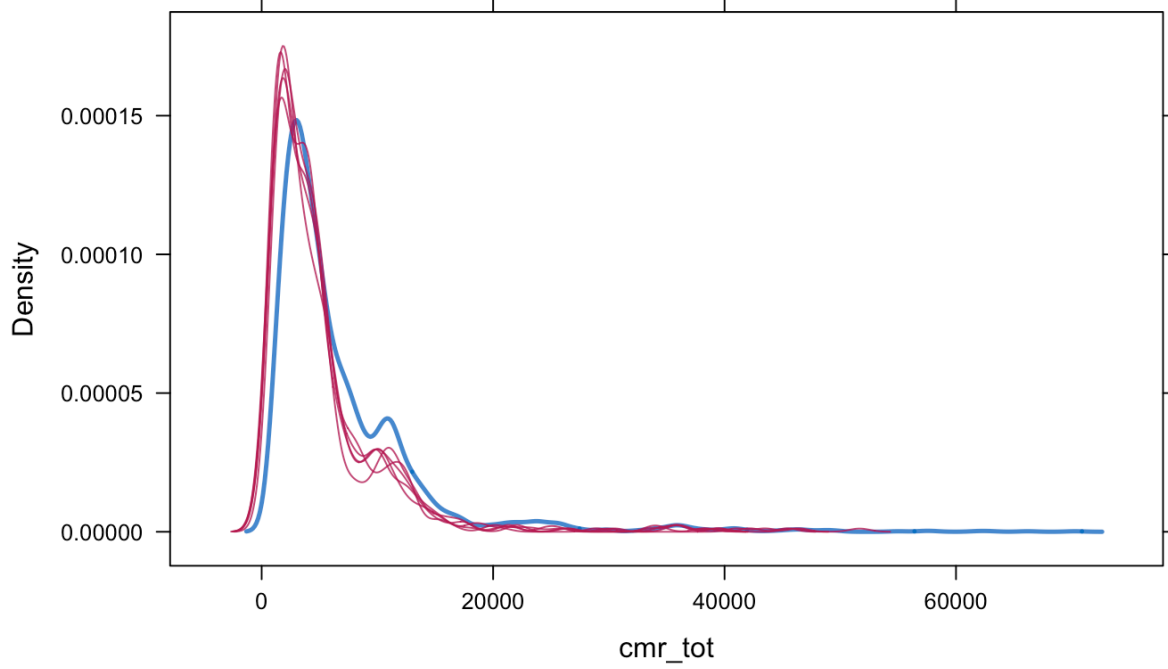
Model 3: county fixed effects model

Multiple imputation used to address missing data

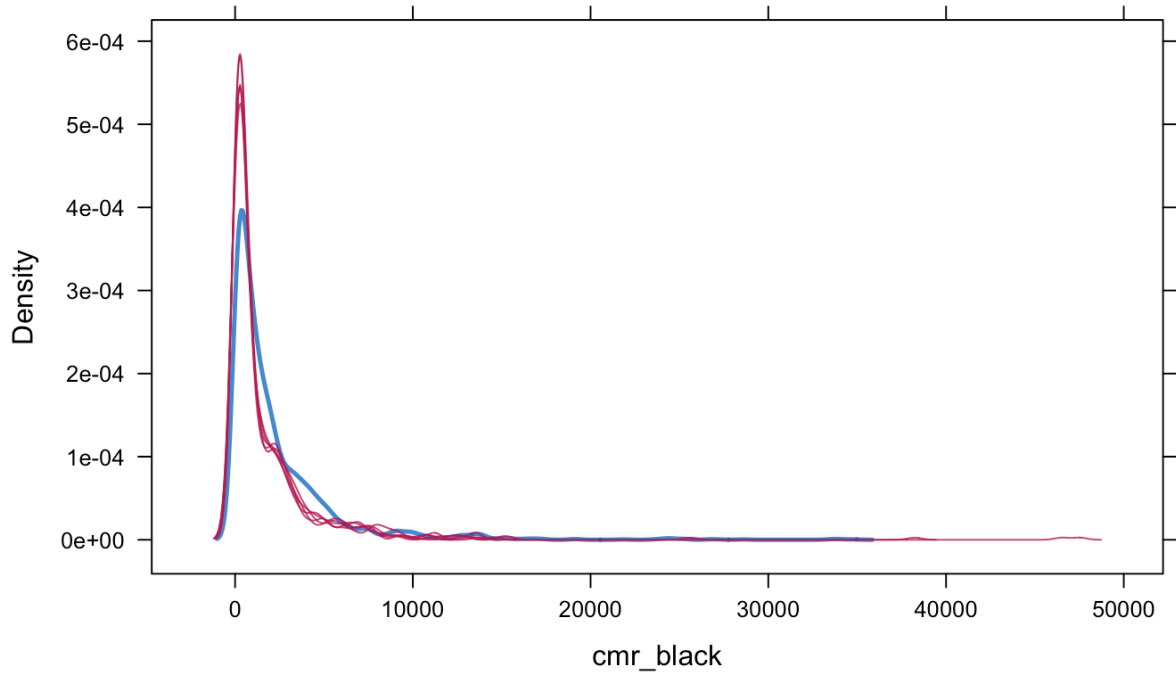
Low percentage of non-Hispanic white residents is 65.7% or less



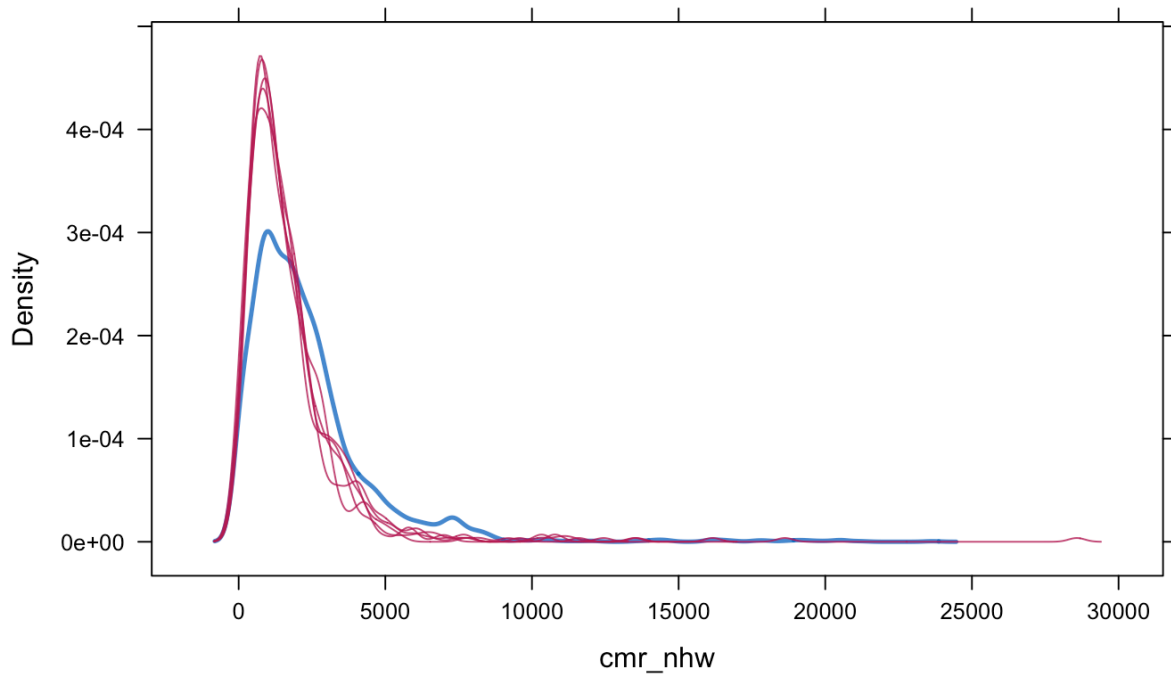
**Figure C1: Distribution of exposure variable (IPV arrest percentage) where blue line represents complete cases and red lines represent five imputed datasets**



**Figure C2: Distribution of main outcome variable (total count of child maltreatment reports) where blue line represents complete cases and red lines represent five imputed datasets**



**Figure C3: Distribution of secondary outcome variable (count of Black child maltreatment reports) where blue line represents complete cases and red lines represent five imputed datasets**



**Figure C4: Distribution of secondary outcome variable (count of non-Hispanic white child maltreatment reports) where blue line represents complete cases and red lines represent five imputed datasets**