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## Warrantless arrest laws for domestic violence: How are youth affected?

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**Warrantless arrest laws for domestic violence:  
How are youth affected?**

Kabir Dasgupta & Gail Pacheco

2016/07

# **Warrantless arrest laws for domestic violence: How are youth affected?**

Kabir Dasgupta\* and Gail Pacheco<sup>✓</sup>  
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## **Abstract**

This study empirically examines the impact of warrantless arrest laws (designed to deter domestic violence) on multiple youth outcomes. Utilizing variation in the timing of implementation of the laws, and employing a difference in differences framework, we examine both the direct and indirect impacts on youth in the United States. There appears to be no significant direct link between warrantless arrest laws and domestic violence-related homicides. However, on the indirect front, we do find strong evidence that implementation of the arrest laws result in a drop in the probability of youth experiencing suicidal ideation, and some evidence pointing to a drop in their likelihood of engaging in substance use behaviour. This analysis also accounts for important heterogeneities in laws across states, and our findings are robust to multiple sensitivity checks, aimed at addressing key threats to identification.

**JEL classification:** I12; K36

**Keywords:** Domestic violence; Warrantless arrest laws; Homicide; Youth; Mental health; Difference-in-differences

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This paper is an upgraded version of an earlier empirical study related to youth outcomes of domestic violence-related warrantless arrest laws. We are grateful to Catherine Maclean, Michael Leeds, Douglas Webber, Andrew Sfekas, and other colleagues at Temple University, Economics Department for their valuable guidance and suggestions. We would also like to thank participants of APPAM, 2015, Southern Economic Association, 2015, and ASHEcon, 2016 conferences for providing helpful comments. We are solely responsible for all errors committed in this study.

## 1. Introduction

Domestic violence is a major social concern, which has attracted substantial policy intervention in the last three decades. It includes all forms of physical, emotional, sexual, psychological, and economic abuse (Department of Justice, 2015)<sup>1</sup>. It is usually described as violent crimes committed by intimate partners (current and former spouses, boyfriends, and girlfriends), immediate family members (such as parents, children and siblings), and other relatives. The most common form is that committed by intimate partners. The scale of this issue is evidenced by the fact that domestic violence accounted for 21 percent of an annual average of over 6.6 million non-fatal violent crimes committed in the United States over the period of 2003 to 2012 (Truman & Morgan, 2014).

One of the main policy responses to this issue of concern has been the enactment of warrantless arrest laws at the state-level, which promotes the role of law enforcement agencies in attempting to reduce the prevalence rates of domestic violence. The literature examining the impact of these laws is sparse, and there has been, to our knowledge, no empirical investigation of the impact on youth outcomes. It is estimated that around 15.5 million children witness domestic violence every year in the United States (McDonald & Grych, 2006). Additionally, youth may also be at risk of being directly abused by other family members and/or their partner. There are numerous studies that illustrate the transitory and persistent youth outcomes associated with domestic violence exposure during childhood. These outcomes include poor physical and mental health conditions, various psychological disorders, and emotional problems (Beitchman et al., 1992; Briere & Elliott, 1994; Mullen et al., 1996; Edleson, 1999; Margolin, 1998; Holden, 2003; Kitzmann et al., 2003; Wolfe et al., 2003; Holt et al., 2008; Safe Horizon, 2015)<sup>2</sup>.

Warrantless arrest legislation permits police to arrest a suspected abuser without a warrant if they have probable cause to believe that the crime was committed, even if the event took place out of their presence. The strictness or severity of these laws is heavily dependent on the amount of discretionary power assigned to police (Hirschel et al., 2007; Hirschel, 2008). The extent of this power results in state laws being categorised into mandatory, preferred or discretionary; with the expectation that mandatory equates to the strictest form of legislation. Zeoli et al. (2011a) also discuss other important heterogeneities in the arrest laws characterized by inclusion of additional factors that are required to make arrests and specification of time limits within which a domestic violence arrest should be made. Considering these important variations in state arrest laws provides useful insights into our policy evaluation analysis.

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<sup>1</sup> Economic abuse refers to acts to make an individual financially dependent on another individual by maintaining control over his or her financial resources. Retrieved from <https://www.justice.gov.ovw/areas-focus> on December 29, 2014.

<sup>2</sup> Safe Horizon is a large non-profit organization that works with governmental agencies to provide support to victims of violence and abuse and prevent violent crimes; Retrieved from <http://staging.safehorizon.org/page/domestic-violence-statistics--facts-52.html> on December 29, 2014.

Both the variation in the type of arrest law, as well as state-level variation in the timing of implementation of these laws are utilized in this study to empirically investigate the effect of these laws on multiple relevant youth outcomes. The timing differences in particular, provides an exogenous source of variation.

We first make use of state-level data on domestic violence-related homicide rates, to ascertain evidence of the direct impact of the arrest laws on fatal cases of domestic violence. We find no evidence of a significant link, regardless of whether we focus on youth as the victims or offenders. We then extend our analysis to investigate a range of indirect effects on youth in terms of their mental wellbeing and other behavioural outcomes. To do so, we make use of data from a large scale nationally representative youth survey (the Youth Risk Behavior Surveys – YRBS) over the period 1991 through to 2013, and employ differences-in-differences models to estimate the causal impact of the arrest laws on youth outcomes.

Our findings with the YRBS analysis point to the passage of arrest laws resulting in a significant drop in suicidal tendency for youth, especially for females. There is also some evidence that there is a drop in substance use behaviour, although this is not as strong as the impacts found for suicidal tendency. These results potentially indicate that the arrest laws resulted in youth feeling more protected and thus improved their mental wellbeing.

The paper is organized as follows: Section 2 provides a background on the domestic violence warrantless arrest laws in the United States; Section 3 reviews related literature on youth issues commonly associated with domestic violence, as well as current evidence on the impacts of warrantless arrest laws; Section 4 outlines the data and variables utilized in this study; Section 5 discusses the empirical approach; Section 6 presents a discussion of the core results and concludes with potential policy implications of the analysis; and Section 7 provides our concluding remarks.

## **2. Background**

### **2.1 Emergence of warrantless arrest laws for domestic violence**

Prior to the late 1970s, domestic violence was considered to be a personal family matter. The primary role of law enforcement agencies in domestic violence incidents was limited to employing conflict resolution tactics (advising and counselling services). These measures were mostly non-punitive and therapeutic in nature (Buzawa & Buzawa, 1993; Maxwell et al., 2001; Iyengar, 2009; Zeoli et al., 2011a). An offender could only be arrested without a warrant if a domestic violence crime was witnessed by the law enforcement officer (Miller, 2005; Zeoli et al., 2011a). Additionally, there was a noticeable absence of proper training programs and relevant institutional support for police and this impeded development of the required skills required by law enforcement officials when handling domestic violence cases. It was further observed, that the attitude of police towards dealing with domestic violence varied with

socio-economic status of victims and offenders. For example, police were less likely to show compassion towards members of lower socio-economic status and other traditionally marginalized populations (Black, 1980; Buzawa & Buzawa, 1993).

In the 1980s, there was an upsurge in the number of domestic violence lawsuits, and greater momentum in women's rights movements. The latter of which demanded increased participation of law enforcement agencies in domestic violence cases (Buzawa & Buzawa, 1993; Hirschel, 2008; Zeoli et al., 2011a). As a consequence, individual states began implementing domestic violence reform statutes, with the first state to take this action being Pennsylvania. These reforms allowed for more punitive measures against domestic violence offenders (Buzawa & Buzawa, 1993; Zeoli et al., 2011a).

There also appears to be unanimous agreement that the Minneapolis Domestic Violence Experiment (MDVE) in 1982 was pivotal in triggering a passage of arrest laws (Sherman & Berk, 1984; Buzawa & Buzawa, 1993; Maxwell et al., 2001; Iyengar, 2009; Zeoli et al., 2011a). MDVE was the first controlled field experiment performed to test the effectiveness of police actions in domestic violence. Funded by a grant from the National Institute of Justice, the experiment was jointly implemented by Lawrence W. Sherman, Director of Research at the Police Foundation and the Minneapolis Police Department. The experimental design involved random assignment of one of three police responses upon receiving a domestic violence call. Police could either - i) arrest the suspect, ii) separate the parties involved in domestic violence, or iii) advise the parties (Sherman & Berk 1984; Wanless, 1996). Victims were interviewed about subsequent violence at two-week intervals for six months after the initial incident occurred. Results showed that arresting the abuser was much more effective in reducing domestic violence compared to the other two responses (Sherman & Berk 1984; Buzawa & Buzawa, 1993; Schmidt & Sherman, 1996; Wanless, 1996). Evidence of deterrent effects of arrests to counter domestic violence incidents obtained from MDVE was used to justify the passage of pro-arrest laws across the U.S. (Iyengar, 2009).

Also in the early 1980s, Congress implemented the Family Violence Prevention and Services Act (FVPSA) to provide funding for domestic violence programs, which included training for law enforcement officers, provision of shelters and counselling services to victims, as well as funds for research on domestic violence cases (Iyengar, 2009).

## 2.2 Types of warrantless arrest laws

Table 1 provides a full list of the state-specific warrantless arrest statutes along with their effective years of implementation. Previous studies have classified warrantless arrest laws based on the amount of discretion an investigating police official has with regard to arresting a suspected offender (Zeoli et al., 2011b). In particular, arrest laws for domestic violence can be classified as discretionary, preferred, or mandatory (Hirschel et al., 2007; Hirschel, 2008; Zeoli et al., 2011b). At present, 22 states have a discretionary arrest law, 6 states have a

preferred arrest law, and the remaining 23 states have a mandatory arrest law (Hirschel et al., 2007; Hirschel, 2008).

State laws that allow police officers to decide whether to arrest the suspected offender are categorized as discretionary laws. Under these laws, police have full discretion in making an arrest. Preferred arrest law allows police to decide whether an arrest should be made, however it also indicates a state's preference for arresting a suspect. Cases where an arrest law requires police to arrest a suspect is termed mandatory. Based on these definitions, it is likely that mandatory arrest laws will result in a higher arrest rate, when compared to preferred and discretionary laws.

At this point it is important to note that Zeoli et al. (2011b) recently discussed the lack of consensus among researchers on the classification of domestic violence arrest laws. They argue that the conflicting opinions among researchers are mainly due to interpretive differences caused by complexity of the laws. In general, we follow the premise that statutes that use the phrase 'may arrest' are termed as discretionary laws; statutes that express state's preference for arrest and recommend arrest as a preferred action in domestic violence incidents are called preferred laws; and statutes that use phrases such as 'shall arrest' or 'must arrest' are identified as mandatory laws. These definitions are based on the work by Hirschel et al. (2007), Hirschel (2008), and Zeoli et al. (2011b).

One other area that lacks consensus is the passage year. As shown in Table 1, the passage years denoted are based on information primarily obtained from Iyengar (2009), and Zeoli et al. (2011a; 2011b). The dates were also further verified in HeinOnline and Lexis Nexis databases. However, alternative passage dates are provided by Zeoli et al (2011b) and these are provided in parenthesis in Table 1. We later test the robustness of our empirical findings by replacing the standard passage dates, with the alternative dates.

A final aspect of arrest laws to consider for the purposes of this research is whether there are other heterogeneous elements to state laws, in addition to timing and classification into mandatory, preferred or discretionary. For instance, while the majority of states allow arrests for misdemeanor domestic violence, 22 states allow warrantless arrests for domestic violence only if an incident involves a severe offence or a felony causing serious injury to the victim (Zeoli et al., 2011a). These requirements are identified as additional factors in states' warrantless arrest provisions under which law enforcement agencies can classify whether a particular domestic violence case requires arrest. States' arrest laws can also vary with respect to time limits available to police for making an arrest after the incident has occurred (Zeoli et al., 2011a). In particular, 21 states specify time limits for law enforcement officers to make an arrest upon being reported of a domestic violence incident. While 6 states require the police to make an arrest immediately upon being reported of a domestic violence incident (California, Idaho, Illinois, Louisiana, Maine, and South Carolina). For others the time limit to make a warrantless arrest can vary between 4 hours and 72 hours (after an incident has been reported) depending on nature of states' requirements. However, for Wisconsin, domestic

violence victims are required to report within 28 days after a domestic violence incident for a warrantless arrest to occur. Information on both of these heterogeneities are provided in Table 1, and are utilized in the forthcoming empirical analysis (for further details, see Zeoli et al., 2011a).

**Table 1**  
**Warrantless Arrest Laws for Domestic Violence**

State	Statute	Passage Year	Additional factors required to make an arrest?	Any time limit to make an arrest?
Discretionary law				
Alabama	Ala. Code § 15-10-3 (a)(8)	1989		
Delaware	Del. Code Ann. tit.11§1904(a)(4)	1984	✓	
Florida	Fla. Stat. ch. 741.29 (3)	1992*		
Georgia	Ga. Code Ann. § 17-4-20 (a)	1981		
Hawaii	Haw. Rev. Stat §709-906 (2)	1980		
Idaho	Idaho Code §19-603 (6)	1979		✓
Illinois	725 ill. Comp. Stat. 5/112A-30	1993*		✓
Indiana	Ind. Code Ann. §35-33-1-1 (a)(5)(C )	1980		
Kentucky	Ky. Rev. Stat. Ann. § 431.005(2)(a)	1996*		
Maryland	Md. Code Ann. § 2-204	1986	✓	✓
Michigan	Mich. Comp. Laws § 764.15a	1978		
Minnesota	Minn. Stat. § 629.341 (1)	1978		✓
Nebraska	Neb. Rev. Stat. § 29-404.02(3)	1989	✓	
New Hampshire	N.H. Rev. Stat. Ann. § 173-B:9 & N.H. Rev. Stat. Ann. § 594:10 (l)(b)	1979		✓
New Mexico	N.M. Stat. Ann§ 40-13-7 (B)(5)	1987		
North Carolina	NC Gen. Stat. § 15A-401 (b)(2)	1991*		
Oklahoma	Okla. Stat. tit. 22, § 40.3 (B)	1987	✓	✓
Pennsylvania	18 Pa. Cons. Stat. § 2711(A)	1986	✓	
Texas	Tex. Code Crim. P. Ann. art. 14.03 (a)(4)	1989		
Vermont	Vt .R. Cr. P. 3(a)(C )	1985*		
West Virginia	W.Va. Code § 48-27-1002(a)	1994*	✓	
Wyoming	Wyo. Stat. Ann. § 7-20-102 (a)	1987		✓
Preferred Arrest				
Arkansas	Ark. Code Ann.§ 16-81-113	1991*	✓	✓
California	Cal. Penal Code § 13701 (b)	1993* (1996)		✓
Massachusetts	Mass. Gen. Laws Ann. ch. 209A § 6 (7)	1988		
Montana	Mont. Code Ann.§ 46-6-311(2)(a)	1991*	✓	
North Dakota	N.D. Cent. Code § 14-07.1-10(1)	1995*	✓	✓
Tennessee	Tenn. Code Ann.§ 36-3-619	1995*		



**Table 1 (continued)**  
**Warrantless Arrest Laws for Domestic Violence**

State	Statute	Passage Year	Additional factors required to make an arrest?	Any time limit to make an arrest?
<b>Mandatory Arrest</b>				
Alaska	Alaska Stat. § 18.65.530(a)	1996*		✓
Arizona	Ariz. Rev. Stat. Ann. § 13-3601(B)	1991*	✓	
Colorado	Colo. Rev. Stat. § 18-6-803.6(1)	1994*		✓
Connecticut	Conn. Gen. Stat. § 46b-38b(a)	1987 (1986)		
District of Columbia	D.C. Code Ann. § 16-1031	1991*	✓	
Iowa	Iowa Code § 236.12(2)(3)	1990 (1986)	✓	
Kansas	Kan. Stat. Ann. § 22-2307(b)(1)	2000* (1992)	✓	
Louisiana	La. Rev. Stat. Ann. § 46:2140 ; Ch. C. Art. 1573(1)	1985	✓	✓
Maine	Me. Rev. Stat. Ann. tit. 19-A, § 4012(5)	1995* (1979)		✓
Mississippi	Miss. Code Ann. § 99-3-7(3)	1995*		✓
Missouri	Mo. Rev. Stat. § 455.085	1989	✓	
Nevada	Nev. Rev. Stat. Ann. § 171.137(1)	1989 (1985)		✓
New Jersey	N.J. Stat. Ann. § 2C:25-21(a)	1991*	✓	
New York	N.Y. Crim. Proc. Law § 140.10(4)(a)	1996*		
Ohio	Ohio Rev. Code Ann. § 2935.032(A)(1)(a)(i)	1995*	✓	
Oregon	Or. Rev. Stat. § 133.055(2)(a)	2001* (1977)		
Rhode Island	R.I. Gen. Laws § 12-29-3(c)(1)	2000* (1988)	✓	✓
South Carolina	S.C. Code Ann. § 16-25-70(B)	2002* (1995)	✓	✓
South Dakota	S.D. Codified Laws § 23A-3-2.1	1998* (1989)	✓	✓
Utah	Utah Code Ann. § 77-36-2.2(2)(a)	2000* (1995)		
Virginia	Va. Code Ann. § 19.2-81.3(B)	2002* (1996)		
Washington	Wash. Rev. Code § 10.31.100(2)(c)	1999* (1984)	✓	✓
Wisconsin	Wis. Stat. § 968.075 (2)(a)	1996* (1987)	✓	✓

\*States that implemented warrantless arrest laws for domestic violence incidents during the YRBS study period (1991-2013).

Note: Information on passage years of the arrest laws have been primarily obtained from Iyengar (2009), Zeoli et al. (2011a; 2011b). The information is further verified in HeinOnline and Lexis Nexis databases. Hirschel et al. (2007), Hirschel (2008), and Zeoli et al. (2011b) provide classification of the arrest laws in terms of mandatory, preferred, and discretionary arrests. Information related to alternative passage years (provided in parentheses) and other classifications of the arrest laws (mentioned above) are obtained from Zeoli et al. (2011a).

### **3 Literature summary**

#### **3.1 Domestic violence and youth outcomes**

Child and youth outcomes associated with domestic violence exposure have been widely studied across various disciplines (Beitchman et al., 1992; Briere & Elliott, 1994; Cicchetti & Toth, 1995; Kolbo et al., 1996; Mullen et al., 1996; Margolin, 1998; Brown et al., 1999; Edleson, 1999; Widom & Hiller-Sturmhofel, 2001; Holden, 2003; Kitzmann et al., 2003; McDonald et al., 2006; McDonald & Grych, 2006; Holt et al., 2008; Child Welfare Information Gateway, 2013; Izaguirre & Calvete, 2015). Children and adolescents exposed to domestic violence are victimized, either by directly experiencing abusive behavior from their family members and intimate partners or by witnessing domestic violence in their homes.

Studies focusing on young victims who are directly abused by their family members and intimate partners show a strong association between domestic violence and various physical as well as mental health-related disorders (Beitchman, 1992; Briere & Elliott, 1994; Cicchetti & Toth, 1995; Mullen et al., 1996; Brown et al., 1999; Widom & Hiller-Sturmhofel, 2001; Child Welfare Information Gateway, 2013). In particular, common physical health issues include cardio-vascular problems, diabetes, asthma, obesity, and hypertension and the usual mental health problems cited are poor development of cognitive and non-cognitive abilities, mental stress, and anxiety. Furthermore, young victims may also suffer from emotional and psychological complications such as depression, low self-esteem, and incidence of suicidal thoughts. Long-term consequences of childhood experience of domestic violence can include problems related to substance misuse (smoking, alcohol, and illicit drugs), abusive behavior, and delinquency among adolescents.

Research on child outcomes associated with witnessing of domestic violence also shows similar evidence of child health and behavioral problems (Margolin, 1998; Edleson 1999; Kitzmann et al., 2003; McDonald et al., 2006).

In terms of the expected size of affected youth population (via being a victim of direct abuse, or witnessing the domestic violence acts), it is estimated that annually, approximately 7 million children (on an annual basis) reside in households associated with severe domestic violence in the U.S. (McDonald et al., 2006; DeBoard-Lucas, 2011). Further, 686,000 children were identified as direct victims of maltreatment (or some form of domestic violence) in 2012 (US Department of Health and Human Services, 2013). This information highlights the importance of both these groups, and the necessity of empirical analysis to encompass a wide range of potential outcomes (both direct and indirect) resulting from the passage of arrest laws.

It should be noted at this point, that while there are a myriad of studies investigating a range of youth outcomes associated with experiencing or witnessing domestic violence, there have been no studies to date examining what the impact is of tougher domestic violence legislation on said youth.

### 3.2 Potential mechanisms

Based on the extant literature available, there is evidence pointing towards the potential of both negative and positive outcomes with respect to the passage of warrantless arrest laws. Early evidence from Sherman and Berk (1984) using the MDVE pointed in the positive direction. Their results indicated that arresting a domestic violence offender deterred subsequent violence against the victim during the six-month follow-up period (after the initial incident was reported to police). In an attempt to replicate the findings of MDVE, five more field experiments were performed as a part of the Spouse Assault Replication Program (SARP) funded by National Institute of Justice between 1981 and 1991 (Maxwell et al., 2001).<sup>3</sup> These experiments produced inconsistent findings on the deterrent effects of arrests, and this may have been partially attributable to the lack of consistent information used for analysis in each replication study.

Improving on the statistical analysis of the earlier field experiments, Maxwell et al. (2001) used comparable dependent and independent variables, and found evidence consistent with MDVE. Specifically, their results indicated that arresting domestic violence abusers was related to a reduction in subsequent aggression against their intimate partners. In particular, an abuser was less likely to repeat the crime after being arrested.

More recent research has focussed on investigating a wider range of outcomes. For instance, Iyengar (2009) found evidence indicating intimate partner homicide rates increase in states that mandate arrests for domestic violence relative to other states. She explains her results based on two theories, 'reporting' and 'reprisal'. The 'reporting' theory argues that in the presence of strict arrest laws, domestic violence victims might be less willing to report an incident to police due to fear of facing subsequent violence from their abusers and other psychological and emotional reasons. The reprisal hypothesis is related to the abuser's response to their arrests. It is possible that abusers may react to their arrests with greater aggression and punish their victims more severely after returning home. Under both these theories, warrantless arrest laws may result in a negative outcome for domestic violence victims.

Further, Hirschel et al. (2007) argues that the passage of mandatory and preferred arrest laws leads to an increase in misdemeanor arrests and dual arrests. In dual arrests, both the parties involved in domestic violence get arrested. This usually happens when an investigating officer is unable to distinctly identify the primary offender and the victim. Iyengar (2009) uses the rising trend of dual arrests as one explanation why reporting might decline in the presence of mandatory arrest laws.

Given the possibility of differing mechanisms at play that result from the passage of warrantless arrest laws, it is not clear what the effect on youth may be. For example, if more punitive measures for domestic violence perpetrators leads to a reduced risk of domestic violence, then this will lead to better youth outcomes by improving family relationships and home environments. However, in case the arrest triggers reprisal, this could lead to worse

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<sup>3</sup> The experiments were conducted in Charlotte, North Carolina; Colorado Springs, Colorado; Dade County, Florida; Milwaukee, Wisconsin; and Omaha, Nebraska.

outcomes for youth, as they are more likely to directly experience domestic violence, or witness it. Additionally, if the passage of laws leads to an increase in dual arrests (as found in Hirschel et al. (2007)), and both parents are removed from a household, their children are likely to be exposed to lack of financial support, parental care, and parental monitoring. This can cause further behavioral and health-related problems. Also, in the event the arrested parties are the primary wage earners in the family, then lack of access to financial resources caused by the arrests may induce older teenagers and young adults to quit their education and seek employment to support their families.

## 4 Data

As will be highlighted in the methods section, the empirical analysis in this study is divided into two components. At first, we focus on state-level measures of domestic violence-related homicides, while focussing on youth, both as victims and as offenders in this context. This analysis aims to capture the direct impact of warrantless arrest laws, in terms of whether they have hindered the propensity for domestic violence-related events (homicide rates in particular). The second component of the empirical strategy in this study is to investigate youth outcomes at the individual-level, and look at more indirect or potential spillover effects of the legislation, such as impacts on substance use behaviour, or suicidal ideation, etc.

In this section, we therefore separately identify the state-level data for the initial models, before outlining the individual-level YRBS data for the latter analysis. We also present a summary of how the main explanatory variable has been created (warrantless arrest laws), as well as brief details of the additional controls used in this study.

### 4.1 State-level domestic violence-related measures

The state-level data on domestic violence-related crimes are obtained from the Federal Bureau of Investigation's (FBI) Uniform Crime Reports (UCR)<sup>4</sup>. Table 2 provides detailed description of the state-level dependent variables for the period that coincides with the YRBS study period (1991-2012)<sup>5</sup>. In particular, we use UCR's Supplementary Homicide Reports (available for the period 1976-2012) for annual state-level information on homicides<sup>6</sup>. We utilize victim-offender relationship information from the homicide data to construct a measure of domestic violence-related crimes. We include homicides involving family members (father, mother, daughter, son, stepfather, stepmother, stepdaughter, stepson,

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<sup>4</sup> We access FBI's UCR data from the National Archive of Criminal Justice Data (NACJD). NACJD is a part of the Inter-University Consortium for Political and Social Research (ICPSR) at the University of Michigan, that provides access to researchers to the agency-level, incident-level, and county-level data of UCR. Data retrieved on December 20, 2016 from [https://www.icpsr.umich.edu/icpsrweb/content/NACJD/guides/ucr.html#desc\\_al](https://www.icpsr.umich.edu/icpsrweb/content/NACJD/guides/ucr.html#desc_al).

<sup>5</sup> To maintain consistency with the YRBS study period (1991-2013), we restrict our study period to 1991-2012 for domestic violence-related homicides. However, we also perform regressions using domestic violence-related measures of homicides for 1980-2012. We do not find any significant effects of the laws on the homicide rates. Results are available upon request.

<sup>6</sup> We utilise information on fatal crimes, as it is often difficult to obtain accurate measures of non-fatal violent crimes, as a large proportion of violent crimes are not reported to the police (Langton et al., 2012). Secondly, an alternative data source for state-level measures of domestic violence incidence (non-fatal crimes) is the National Crime Victimization Survey (NCVS). However, due to strict administrative restrictions on data usage, we were not able to access the NCVS data.

husband, wife, in-law, and other family member) and other intimate as well as ex-intimate partners (boyfriend, girlfriend, ex-husband, and ex-wife).

We construct three state-level dependent variables based on homicides. These are overall victimization rate (annual number of homicide victims per 100,000 inhabitants), youth victimization rate (annual number of homicide victims aged under 20 per 100,000 inhabitants), and youth offence rate (annual number of homicide offenders aged under 20 per 100,000 inhabitants). The annual state estimates of inhabitants are obtained from the Supplementary Homicide Reports.<sup>7</sup> The majority of the homicide incidents in the Supplementary Homicide Reports involve a single victim and a single offender. However, some homicides have multiple victims and/or multiple offenders. In our dependent variables, we include additional counts of victims and offenders for incidents that involve multiple parties. We do not consider overall offence rates as states' annual number of offenders are approximately equivalent to the annual number of victims.

The domestic violence-related homicide variables used in our analysis are a broader version of dependent variables used in past studies that specifically examine how warrantless arrest laws affect intimate partner violence (Iyengar 2009; Zeoli et al. 2011b). In comparison, we account for all possible domestic violence relationships in our study. In Table 2, we find that the overall homicide victimization rate is 2.7 in 100,000 inhabitants during the study period, while youth victimization is 0.6 per 100,000 inhabitants, and the youth offence rate is 0.3 per 100,000 inhabitants.

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<sup>7</sup> The Supplementary Homicide Reports' annual state population estimates (inhabitants) are aggregated at the agency level (jurisdiction covered by each law enforcement agency) and are less than population estimates reported in the US. Census Bureau.

**Table 2**  
**Description of state-level measures of domestic violence-related homicide rates**  
**(1991-2012)**

Dependent Variable	Description	Measure	Mean (SD)
Overall victimization	Measure of incidence of domestic violence-related homicides with respect to overall population.	Number of homicide victims/100,000 inhabitants.	2.729 (1.931)
Youth victimization	Measure of incidence of domestic violence-related homicides with respect to youth.	Number of young homicide victims/100,000 inhabitants.	0.605 (0.511)
Youth offence	Measure of incidence of domestic violence-related homicides committed by young offenders.	Number of young homicide offenders/100,000 inhabitants	0.281 (0.363)

Note: Criminal information and relevant population estimates are sourced from Uniform Crime Reporting Data. Domestic violence related homicides include offences committed by boyfriend, brother, husband, common-law wife, daughter, father, girlfriend, husband, in-law, mother, other family, stepdaughter, stepfather, sister, stepmother, son, stepson, wife, ex-husband, ex-wife. Youth is defined as age below 20.

#### 4.2 Individual-level youth outcomes

In Table 3, we provide detailed information on the five dependent variables that are used as measures of youth mental health and behavioral outcomes. We utilize the repeated cross-sectional data from the YRBS to construct our measures. The national YRBS commenced in 1991. The CDC conducts the national YRBS biennially in order to monitor various types of health-related behaviors of young individuals across the US. The individual-level data is based on a nationally representative sample of high-school students, ages 12 through 18 years primarily. Administered by the state education and health agencies, the state YRBS are coordinated by the Centers for Disease Control and Prevention (CDC). Using state identifiers (FIPS code), we combine the state and national YRBS to accommodate as many arrest law implementation as possible for identification. This is similar to the approach followed by Sabia & Anderson (2014), Sabia et al. (2014), Anderson et al. (2015), and Sabia & Bass (2016).

Our selection of the relevant YRBS variables follow three specific criteria. First, to ensure maximum variation in the implementation of the arrest laws, we consider only those variables that are present across all surveys (1991-2013). The survey information in YRBS is modified over time to include detailed information on youth health and risky behaviors. We

exclude some relevant variables as they were introduced in the YRBS during later years.<sup>8</sup> Second, we exclude health-related events and behaviors that specifically occur on school campus, since YRBS provides broader measures of the same youth outcomes that are more relevant to our study. Finally, we exclude life events that do not provide any specific information regarding the time of occurrence. In order to ensure accuracy of our estimates, we restrict our analysis to survey year-specific outcomes. In particular, we consider events that occurred in periods just preceding each survey year (ranging between 30 days and 12 months).

The above selection process results in thirteen YRBS variables that are used to construct our five dependent variables of interest. Specific details regarding the thirteen variables are provided in Appendix Table A.1. Using the selected variables, we construct two binary indicators for youth violence, one for suicidal tendencies, and two to capture youth's substance use behavior. Details of these are provided in Table 3, along with descriptive information of these variables, by gender subgroups.

The youth violence indicators measure whether a person was involved in physical fights that required medical attention (*serious fight*) and whether a person was involved in any physical fights or carried weapon such as gun, knife, or club (*violence index*). Since people who were involved in serious fights are a subset of people who were involved in physical fights, we treat serious fights as a separate measure of youth violence. The *Suicidal tendency index* equals 1 if an individual reported to have considered, planned, or attempted suicide. The two substance use variables capture information on individuals' smoking and tobacco use (chewing tobacco), drinking, and illicit drug use (marijuana and cocaine) behavior. As shown in Table 3, *Substance use index 2* captures more frequent spells of smoking and drinking activities compared to *Substance use index 1*.

As shown in Table 3, we observe that 3.8 percent of the sample was involved in physical fights that required medical attention. In comparison, 38.9 percent of the sample was involved in either physical fights or carried weapon. 21.8 percent of the high-school students in these surveys reported to have had suicidal thoughts; and 48.2 percent reported to have used some form of substance. However, the proportion of substance users drops to 39.5 percent, if we restrict the substance users to more frequent smokers and binge-drinkers (people who reported to have five or more drinks in succession at least once). Further, we find that the prevalence of violence and substance use is significantly higher among boys compared to girls. The reverse is true for suicidal ideation, and all gender differences are statistically significant at the 1 percent level.

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<sup>8</sup> Some examples include academic grades (introduced in 2001), feeling of sadness/hopelessness (introduced in 1999). As most states implemented the warrantless arrest laws before the period the excluded variables were introduced, there is therefore not much variation in these potential explanatory variables.

**Table 3**  
**Description of YRBS dependent variables (1991-2013)**

Dependent Variable	Description	Binary Measure	Youth	Girls	Boys	p-value
			Mean (SD)	Mean (SD)	Mean (SD)	
			$(\mu_f)$	$(\mu_m)$	$(\mu_f - \mu_m)$	
Serious Fights	Indicator for a person's involvement in physical fights that lead to serious injury.	0: Never 1: At least once	0.038 (0.192)	0.025 (0.156)	0.052 (0.222)	0.000
Violence Index	Indicator for a person's involvement in physical fights or whether a person carried weapon (gun, knife, or club).	0: Never 1: At least once	0.389 (0.487)	0.272 (0.444)	0.512 (0.499)	0.000
Suicidal Tendency Index	Indicator for considering, planning, or attempting suicide.	0: Never 1: At least one/once	0.218 (0.413)	0.261 (0.439)	0.173 (0.379)	0.000
Substance use index1	Indicator for number of days a person smoked cigarette, had at least one drink, chewed tobacco, used marijuana, and used other drugs.	0: Never 1: At least one/once	0.482 (0.499)	0.459 (0.498)	0.506 (0.499)	0.000
Substance use index2	Indicator for number of cigarettes a person smoked per day, number of days a person had five or more drinks in quick succession, chewed tobacco, used marijuana, and used other drugs.	0: Never 1: At least one/once	0.395 (0.488)	0.356 (0.479)	0.435 (0.495)	0.000

Note: Data sourced from YRBS 1991-2013. Details of subcomponents used to create the youth outcome indicators are provided in Appendix, Table A.1.

### 4.3 Warrantless arrest laws

We use the passage years of the warrantless arrest laws reported in Table 1 to construct our key explanatory variable in the upcoming empirical specifications. More specifically, we create a variable  $Law_{st}$ , which is a binary indicator for whether state  $s$  has a warrantless arrest law for domestic violence at time  $t$ .  $Law_{st}$  equals zero for years prior to the passage date (including the passage year itself), and then equals one thereafter. This manner of coding is based on the timing of implementation of the laws, and data collection of our key youth outcome information. We rely on the Youth Risk Behaviour Surveys (YRBS) for youth information, which are conducted during spring (February-March). Given that arrest laws



were usually enacted after spring in their passage years, it makes sense to assume the law would not have an impact at the time of data collection for YRBS.

To account for heterogeneity in states' warrantless arrest laws, we employ the above method to construct binary indicators for individual components of an arrest law (see Table 1). To capture variation in the discretionary powers afforded to police, we make use of two dummy variables, one for states with mandatory arrests and the other for states with discretionary and preferred arrests. For arrests' time-limit requirements, we construct separate indicators for states that require police to make an arrest within a certain time limit and states that do not. Finally, with respect to additional factors for arrests, we create binary indicators for states that restrict police to make an arrest only in serious domestic violence incidents and states that allow police to make an arrest for a misdemeanour offence. Further, using similar methods, we also construct an additional variable using alternative dates (provided in parentheses in Table 1 to study if researchers' interpretive differences with respect to the passage years of the laws affect causal estimates obtained in the main analysis.

Descriptive information of warrantless arrest laws and the heterogeneities relevant for the regression analysis are provided in Table 4, as well as details of the source of the information.

**Table 4**  
**Summary statistics of characteristics of arrest laws (1991-2013)**

Variables	Mean (St. Dev)	Source of definition
<b>Warrantless Arrest law</b>	0.866 (0.341)	Zeoli et al. (2011a; 2011b), Iyengar (2009)
Mandatory law	0.347 (0.476)	Zeoli et al. (2011a; 2011b), Iyengar (2009), Hirschel et al. (2007), Hirschel (2008)
Non-mandatory law (i.e. Discretionary or Preferred)	0.519 (0.499)	Zeoli et al. (2011a; 2011b), Iyengar (2009), Hirschel et al. (2007), Hirschel (2008)
Additional factors required	0.369 (0.483)	Zeoli et al. (2011a)
Additional factors not required	0.497 (0.500)	Zeoli et al. (2011a)
Time limit to make an arrest	0.334 (0.472)	Zeoli et al. (2011a)
No time limit to make an arrest	0.532 (0.499)	Zeoli et al. (2011a)

Note: Using years of passage of warrantless arrest laws and their specific provisions, we create 0-1 binary indicators for constructing our explanatory variables. The above descriptive information are estimated based on the binary indicators.

#### 4.4 State-specific economic and policy controls

Table 5 reports summary statistics of all the right-hand side variables (bar  $Law_{st}$ ) included in our regressions. As shown in the table, there are a number of state-level controls included in the upcoming analysis. As implementation of warrantless arrest laws for domestic violence may be correlated with state-specific characteristics (and public policies), exclusion of such state-level information may bias the true estimates of the effects of the laws. For example, states with important public welfare policies that can affect domestic violence are likely to implement warrantless arrest laws for domestic violence. Further, with respect to the YRBS outcomes, the analysis is based on a sample of youth who are enrolled in high-school. Since due to data limitations, we are unable to track young people who are out of school, our regression estimates may suffer from sample selection bias. Controlling for relevant state-level characteristics that may affect enrollment status of young people shall address such empirical concerns. In this regard, we control for annual high-school dropout rates and arrest rates for offences against family and children. We refer to National Center for Education Statistics' definition of states' dropout rate and use Current Population Survey data to estimate states' annual high-school dropout rates (for people aged 16 to 24).

Next, we utilize UCR's yearly arrest data (available in NACJD) for information on arrests made by the police for offences against family and children. We use state population estimates (US Census Bureau) to calculate arrest rates (number of arrests per 100,000 people). Further, we collect data on criminal sanctions related to child witness of domestic

violence (Child Welfare Information Gateway, 2013), school anti-bullying laws (US Department of Education, 2011), beer taxes (Beer Institute), and cigarette taxes (Tax Burden on Tobacco).

As shown in Table 5, we also control for state-level economic conditions measured by seasonally adjusted unemployment rates (Bureau of Labor Statistics) and per-capita personal income in 2005 dollar terms (Bureau of Economic Analysis). Finally, for our analysis on state-level measures of homicide rates, we use demographic information of states' population from the US Census Bureau to construct demographic indicators. In a similar fashion, the individual-level demographic controls for the YRBS analysis include age, sex, race, and ethnicity.

**Table 5**  
**Summary statistics of state and individual-level controls (1991-2013)**

Variables	Mean (St. Dev)	Source
<b>State-level variables included in all models</b>		
Child witness to domestic violence law	0.252 (0.434)	HeinOnline and Lexis Nexis
School anti-bullying law	0.297 (0.457)	US Department of Education, www.bullypolice.org
Cigarette Tax (in dollars)	0.711 (0.696)	Tax Burden on Tobacco
Beer Tax (in dollars)	0.263 (0.229)	Beer Institute
Per capita personal income (in 2005 dollars)	31572.520 (9582.421)	Bureau of Economic Analysis
Unemployment rate	5.774 (1.930)	Bureau of Labor Statistics
Overall arrests for offences against family and children (per 100,000 people)	39.447 (38.443)	Uniform Crime Reports (Yearly Arrest data)
High-school dropout rate	0.099 (0.038)	Current Population Survey
<b>Demographic information included in state-level models</b>		
Male population	0.491 (0.009)	US Census Bureau
White population	0.820 (0.137)	US Census Bureau
Hispanic population	0.083 (0.091)	US Census Bureau
Adult population	0.721 (0.024)	US Census Bureau
<b>Demographic information included in individual-level models</b>		
Males	0.489 (0.499)	National and state YRBS
White	0.612 (0.487)	National and state YRBS
Hispanic	0.125 (0.331)	National and state YRBS
Age less than equal to 14	0.107 (0.309)	National and state YRBS
15 years	0.252 (0.434)	National and state YRBS
16 years	0.266 (0.442)	National and state YRBS
17 years	0.237 (0.425)	National and state YRBS
Age more than 17	0.136 (0.342)	National and state YRBS

Note: YRBS variables are binary indicators and the mean values represent the proportion of each category in the data.

## 5 Empirical Approach

The analysis uses variation across states and time in the implementation of warrantless arrest laws for domestic violence for identification of the effects of the arrest laws. We estimate five regression models, ranging from a baseline model (model 1) to a more saturated model (model 5).

With respect to our **state-level analysis** using homicide rates, we begin with our baseline model (Model 1, shown by Equation 1), where we regress the state-level dependent variables on warrantless arrest laws by controlling for state fixed effects and year fixed effects only.

Our baseline model (Model 1) is:

$$DV_{st} = \alpha_0 + \alpha_1 Law_{st} + \gamma_s + \lambda_t + \varepsilon_{st} \quad (1)$$

where  $DV_{st}$  is a state-level measure of domestic violence-related crime rates (homicide victimization and offence rates; see section 4.1). The variable  $Law_{st}$  is a binary indicator for whether state  $s$  has a warrantless arrest law at time  $t$ .  $\gamma_s$  captures time-invariant state fixed effects and  $\lambda_t$  represents year fixed effects that controls for factors affecting the nation as a whole.

In Model 2, we incorporate demographic characteristics in the right hand side. In Model 3, we add in controls for state-specific characteristics and economic and policy controls. In model 4, we additionally control for state-specific linear time trends. Finally, in Model 5 (shown by Equation 2), we perform a parameterized event study by controlling for pre-treatment trends in the implementation of the arrest laws.

Our most saturated model (Model 5) is:

$$DV_{st} = \beta_0 + \beta_1 Law_{st} + \beta_2' P_{st} + \beta_3' Z_{st} + \Omega_s t + \theta_1 \delta_{st} + \theta_2 (\delta_{st} * Law_{st}) + \gamma_s + \lambda_t + v_{st} \quad (2)$$

where, in addition to the variables present in equation (1),  $P_{st}$  is a vector of states' demographic controls and  $Z_{st}$  is a vector of state-level controls. The parameter of interest,  $\beta_1$  measures the effect of warrantless arrest laws on the domestic violence-related crime rates.

Identification of  $\beta_1$  is contingent upon arrest laws being uncorrelated with unobserved variables that affect youth mental health and behavioral outcomes. One of the possible threats to identification of  $\beta_1$  is policy endogeneity which may exist if the unobserved factors are correlated with both arrest laws and the dependent variables. To address this empirical issue, we include state-specific linear time trends ( $\Omega_s t$ ) to control for state-level omitted variables evolving over time at a constant rate.

Further concerns related to identification of  $\beta_1$  may arise if states respond to varying trends in domestic violence-related homicides by implementing domestic violence arrest laws. To address this concern, we control for pre-treatment trends in implementation of the arrest laws (Angrist & Pischke, 2009). To conduct a parameterized event study, the variable,  $\delta_{st}$  in equation (2), is constructed in a way such that it takes negative values for periods prior to implementation of arrest law in a state and positive values in the post-implementation period. For example, if a state  $s$  implemented a warrantless arrest law in the year 2005,  $\delta_{st}$  equals -2 in the study year 2003 for that state, -1 in 2004, 0 in 2005, 1 in 2006, 2 in 2007, and so on. Including the interaction term ( $\delta_{st} * Law_{st}$ ) allows the linear trend in outcomes to vary before and after implementation of arrest laws. Hence,  $\theta_1$  estimates the pre-implementation trend in the youth outcomes, while  $\theta_2$  identifies the difference in the outcomes before and after warrantless arrest laws were passed. If  $\theta_1$  is significantly different from zero, policy endogeneity may be present. Hence, by controlling for pre-treatment trends and their interaction with the arrest laws, we account for potential sources of bias affecting our causal estimates. We perform ordinary least squares (OLS) regression for all five models, and in results not reported here (for brevity sake), we check to ensure our findings are consistent with negative binomial and Poisson regression models.

With respect to the individual-level analysis of youth outcomes, we employ a similar empirical strategy as above. In particular, for the YRBS outcomes, Model 5 is:

$$Y_{ist} = \rho_0 + \rho_1 Law_{st} + \rho_2' X_{ist} + \rho_3' Z_{st} + \Omega_s t + \sigma_1 \delta_{st} + \sigma_2 (\delta_{st} * Law_{st}) + \gamma_s + \lambda_t + u_{ist} \quad (3)$$

where  $Y_{ist}$  is a binary indicator of youth outcomes (violence, suicidal tendency, and substance use) of individual- $i$  from state  $s$  at time  $t$ .  $X_{ist}$  is the vector of individual demographic controls (age, sex, race, and grade) and  $u_{ist}$  is the error term. The remaining control variables have similar interpretation as in equation (2). We perform probit regressions for the binary dependent variables.

In all our regressions, standard errors are corrected for clustering at the state-level.

## 6 Results

### 6.1 Domestic violence rated homicide rates

We report point estimates of the effects of the warrantless arrest laws on domestic violence-related homicide rates in Table 6 (These are the results of Models 1 to 5 described in equations 1 and 2). In columns (1) to (3), OLS regression coefficients measure the laws' effects on overall homicide victimization rates, youth victimization rates, and youth offence rates. Although, warrantless arrest laws have a negative relationship with domestic violence homicide rates across all regression specifications (Models 1 to 5), the regression coefficients are not statistically significant.<sup>9</sup>

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<sup>9</sup> Regression results from non-linear models (negative binomial and Poisson) were also conducted and provide qualitatively similar findings. These results are available upon request.

There is however, evidence of the importance of controlling for pre-treatment trends, which has not been undertaken in past studies that investigate the impact of warrantless arrest laws. In the case of overall victimization (column 1) and youth offence rates (column 3),  $\delta_{st}$  is negative and statistically significant (at the 5 percent and 1 percent level, respectively). This is a signal of policy endogeneity, whereby states that implement arrest laws for domestic violence experience declining trends in the two domestic violence-related outcomes leading up to the passage of the laws.

**Table 6**  
**Differences-in-differences estimates of the effects of domestic violence warrantless arrest laws on domestic violence-related homicide rates (1991-2012)**

	Overall victimization	Youth victimization	Youth offence
	(1)	(2)	(3)
<b>Model 1: Baseline specification</b>			
Arrest law	-0.202 (0.191)	-0.054 (0.050)	-0.040 (0.057)
<b>Model 2: Model 1 + state demographic controls</b>			
Arrest law	-0.193 (0.177)	-0.064 (0.044)	-0.033 (0.048)
<b>Model 3: Model 2 + state characteristics + economic / policy controls</b>			
Arrest law	-0.129 (0.138)	-0.052 (0.047)	-0.034 (0.070)
<b>Model 4: Model 3 + state-specific linear time trends</b>			
Arrest law	-0.124 (0.188)	-0.039 (0.062)	-0.032 (0.070)
<b>Model 5: Model 4 + pre treatment trends</b>			
Arrest law	-0.108 (0.185)	-0.039 (0.063)	-0.034 (0.070)
$\delta_{st}$	-5.019** (8.269)	-2.089 (2.001)	-5.527*** (1.242)
$\delta_{st} * \text{Arrest law}$	0.099 (0.080)	0.004 (0.023)	-0.018 (0.014)
State fixed effects	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes
Sample size	1069	1069	1069

Note: OLS regressions coefficients are reported. Standard errors are corrected for clustering at the state-level and are provided in parentheses. State demographic controls include proportion of males, White, Hispanic, and adult population. State characteristics and economic & policy controls include high-school dropout rates, arrest rate for offences against family and children, unemployment rates, per capita personal income, cigarette tax, beer tax, anti-bullying policies, and criminal sanctions for child witness of domestic violence laws. Overall homicide rate is number of victims per 100,000 people. Youth homicide/offence rate is counts of victims/offenders aged under 20 per 100,000 people aged under 20.

\*, \*\*, and \*\*\* denote significance at the 10%, 5% and 1% level.

We next consider important heterogeneities in the arrest laws and report estimated regression coefficients in Table 7. The regressions are performed using Model 5 specifications (the most saturated model). In Panel 1, we consider heterogeneity with respect to police's discretionary powers in making a domestic violence arrest. States with discretionary and preferred arrest laws are termed as non-mandatory. Our results suggest that extent of police's discretionary power does not have a significant effect on the homicide rates, regardless of whether we view aggregate results or youth-specific findings, i.e. column (1) versus columns (2) and (3).

While the youth victimization results (column 2) appear to be in agreement to the most comparable findings<sup>10</sup> by Iyengar (2009), the overall victimization results (column 1) are in contrast. Iyengar (2009) finds a significant negative impact of mandatory warrantless laws for family homicide rates. There are several potential reasons for this difference in findings. Iyengar's (2009) study covers a different time period (1976 to 2003 compared to our timeframe of 1991 to 2012) and has less coverage (28 states versus the full complement of 51 states in this analysis). The other sources of difference may stem from minor differences in specification (Iyengar (2009) does not incorporate controls for pre-treatment trends), as well as her use of a narrower definition for domestic violence homicides<sup>11</sup>.

The next heterogeneity under consideration in Table 7 is differences across states in terms of whether the arrest laws require police to make an arrest only under serious circumstances (defined as additional factors). We find a significant negative effect of implementation of these types of arrest laws, with a decline in the youth victimization rate of 0.15 per 100,000 individuals (see Panel 2, column 2). This effect is statistically significant at the 10 percent level.

Further, implementation of arrest laws that specify time limits for making an arrest appears to lead to an increase in the overall victimization rate by 0.39 per 100,000 individuals (see Panel 3, column 1). This effect is statistically significant at the 5 percent level.

Finally, we do not find any significant relationship between arrest laws and domestic violence-related homicide rates when we construct our explanatory variable using alternative passage years of law (Panel 4, columns 1 to 3).

In summary, given the lack of statistical significance across the majority of the estimated coefficients in Table 7, there is no evidence of a particularly strong link between warrantless arrest laws and domestic violence-related homicides, particularly in terms of

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<sup>10</sup> The most comparable outcome variable in Iyengar (2009) is "Family homicides, school age child of offender victims". This covers homicides committed by fathers, mothers, step-fathers, or step-mothers, against sons, daughters, step-sons or step-daughters, where the victim is aged 6 to 18. As shown in Section 4.1, our definition is broader in terms of what encompasses domestic violence, and our youth age group is aged under 20.

<sup>11</sup> Iyengar's (2009) definition includes only homicides committed by fathers, mothers, daughters, sons, step-fathers, or step-mothers, step-daughters or step-sons. In contrast, our broader definition includes all family members, and other intimate as well as ex-intimate partners, such as boyfriend, girl-friend, ex-husband, and ex-wife.

youth, which are the focus of this study. If the reporting and reprisal theory was dominating, we would have expected to find a positive and significant link between arrest laws and domestic violence-related homicide rates; whereas if the arrest laws acted as a deterrent (as hypothesized in the MDVE), we would expect to see a negative and significant association. As we are finding no strong or consistent evidence of either outcome, this could mean that neither mechanisms are occurring in a significant fashion, or that they are both occurring and cancelling the impacts of each other out.

**Table 7**  
**Heterogeneity in domestic violence warrantless arrest laws with respect to domestic violence-related homicide rates (1991-2012)**

	Overall victimization (1)	Youth victimization (2)	Youth offence (3)
<b>Panel 1: Extent of police discretion</b>			
Mandatory	-0.086 (0.176)	-0.033 (0.049)	-0.036 (0.093)
Non-Mandatory	-0.148 (0.342)	-0.050 (0.126)	-0.032 (0.071)
<b>Panel 2: Arrest requires additional factors to domestic violence</b>			
Required	-0.090 (0.340)	-0.154* (0.086)	-0.056 (0.071)
Not required	-0.123 (0.134)	0.053 (0.068)	-0.017 (0.107)
<b>Panel 3: Time limit for arrest to occur</b>			
Yes	0.394*** (0.143)	-0.000 (0.095)	0.009 (0.131)
No	-0.111 (0.127)	-0.070 (0.077)	-0.052 (0.036)
<b>Panel 4: Alternative implementation date</b>			
Arrest law	0.011 (0.236)	0.044 (0.063)	-0.004 (0.100)
State fixed effects	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes
State demographic controls	Yes	Yes	Yes
State-level controls	Yes	Yes	Yes
State-specific linear time trends	Yes	Yes	Yes
Pre-treatment trends	Yes	Yes	Yes
Sample size	1069	1069	1069

Note: OLS regressions coefficients are reported in the table above. Standard errors are corrected for clustering at the state-level and are provided in parentheses. State demographic controls include proportions of Hispanic, Whites, female, and adult population. State-level controls include high-school dropout rates, arrest rate for offences against family and children, unemployment rates, per capita personal income, cigarette tax, beer tax, anti-bullying policies, and criminal sanctions for child witness of domestic violence laws.

\*, \*\*, and \*\*\* denote significance at the 10%, 5% and 1% level.



## 6.2 YRBS outcomes

The regression results in the previous section may not be indicative of the potential spillover or indirect effects of warrantless arrest laws for domestic violence on youth. In this section, we utilise a nationally representative sample of youth, to investigate a number of possible outcomes of the warrantless arrest laws, ranging from greater exhibitions of violence (via serious fights and a violence index), to suicidal tendency, and different forms of substance use. These outcomes can be considered as potential negative externalities of the warrantless arrest laws.

Table 8 reports the marginal effects of probit regression estimates of the relationship between the warrantless arrest laws and youth mental and behavioral outcomes. Norton (2012) observes, that unlike in linear models, variables that affect dependent variables but are uncorrelated with explanatory variables may impact point estimates of non-linear models such as logit or probit, conditional on their inclusion in the estimated model. However, marginal effects in linear and non-linear regression models do not vary with independent unobserved heterogeneity.

In Table 8, similar to our approach in the previous section, we track changes in the relationship between the warrantless arrest laws and various youth outcomes from Model 1 to Model 5 by adding relevant sets of controls that are likely to be correlated with both the laws and the dependent variables.

Viewing the results for the most saturated model (i.e. Model 5), we find that implementation of warrantless arrest laws for domestic violence leads to a 10.5 percent ( $0.004/0.038 = \text{marginal effect/mean for serious fights}$ ) increase in the probability of youth's involvement in fights that lead to serious injury that had to be treated by a doctor or a nurse (serious fight). With respect to the other measure of youth violence (violence index), we find that the arrest laws do not have any significant effect on the probability of carrying weapons or being involved in physical fights in general (Model 5; column 2).

In column 3 of Table 8, regression results suggest that implementation of warrantless arrest laws is negatively related to the probability of having suicidal ideation (or attempting suicides). The estimated marginal effect is also statistically significant across all model specifications. In particular, in Model 5, we find that passage of domestic violence warrantless arrest laws leads to a 7.3 percent ( $0.016/0.218$ ) reduction in the probability of exhibiting suicidal tendency. The effect is significantly different from zero at the 1 percent level. We also find in column 3 that there is evidence of a declining trend in suicidal tendency leading up to the passage of the laws – the pre-treatment impact is negative and significant at the 1 percent level.

**Table 8**  
**Effects of domestic violence warrantless arrest laws on YRBS youth outcomes**

	Serious fight	Violence Index	Suicidal tendency Index	Substance Use Index1	Substance Use Index2
	(1)	(2)	(3)	(4)	(5)
<b>Model 1: Baseline specification</b>					
Arrest law	-0.000 (0.002)	0.001 (0.009)	-0.010* (0.005)	-0.006 (0.013)	0.006 (0.011)
<b>Model 2: Model 1 + Individual demographic controls</b>					
Arrest law	-0.002 (0.002)	-0.003 (0.009)	-0.011** (0.005)	-0.001 (0.012)	0.012 (0.010)
<b>Model 3: Model 2 + State characteristics and economic &amp; policy controls</b>					
Arrest law	-0.002 (0.003)	-0.004 (0.009)	-0.011** (0.005)	-0.005 (0.011)	0.008 (0.010)
<b>Model 4: Model 3 + State-specific linear time trends</b>					
Arrest law	0.004* (0.003)	0.009 (0.010)	-0.018*** (0.006)	-0.016 (0.010)	-0.004 (0.017)
<b>Model 5: Model 4 + Pre-treatment trends</b>					
Arrest law	0.004* (0.002)	0.008 (0.009)	-0.016*** (0.005)	-0.017* (0.011)	-0.006 (0.017)
$\delta_{st}$	-0.001 (0.001)	-0.007 (0.006)	-0.010*** (0.004)	-0.003 (0.004)	0.004 (0.005)
$\delta_{st} \times$ Arrest law	-0.000 (0.001)	-0.000 (0.003)	0.005** (0.002)	-0.000 (0.004)	-0.003 (0.005)
State fixed effects	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes
Sample Size	686676	842081	843429	859453	859453

Note: Marginal effects from probit regressions using YRBS data are reported above. Standard errors are corrected for clustering on the states and are provided in parentheses. Individual demographic controls include age, sex, race, and ethnicity. State characteristics and economic & policy controls include high-school dropout rates, arrest rate for offences against family and children, unemployment rates, per capita personal income, cigarette tax, beer tax, anti-bullying policies, and criminal sanctions for child witness of domestic violence laws. \*, \*\*, and \*\*\* denote significance at the 10%, 5% and 1% level.

In the final two columns in Table 8, we report marginal effects of the arrest laws on two indicators of youth's substance use behavior. For Substance use Index 1, passage of arrest laws leads to a 3.5 percent reduction in the probability of using substance (smoking, drinking, marijuana, and cocaine) (Model 5). The effect is statistically significant at the 10 percent level. Further, although in Model 5, the arrest laws appear to have a negative relationship with youth's substance use measure that includes more frequent spells of smoking and drinking (Substance use index 2), the marginal effect is not statistically significant.<sup>12</sup>

<sup>12</sup> For consistency, we perform separate regressions using the individual measures of youth violence, suicidal ideation, suicidal attempts, and substance use behavior that are used to construct the broader measures of youth outcomes in our study. The direction of impact of the arrest laws on the individual youth outcomes are similar to nature of relationship observed for the indices that are used in our primary analysis. Results are available upon request.

In Table 9, we examine how important elements of warrantless arrest laws affect youth outcomes. This is a repeat of the analysis performed within Table 7, but with the YRBS outcomes as dependent variables rather than state-level domestic violence homicide rates.

The key finding in Table 9 is that in the majority of specifications, arrest laws result in a reduction in suicidal tendency. This was the case of both mandatory and non-mandatory; as well as where additional factors are required, and not required; and also when there is a time limit for the arrest to occur. For example, in cases where there is a time limit for arrest to occur, the passage of the arrest law results in a reduction, on average, of 10.5 percent (0.023/0.218) in the probability of suicidal tendency.

In terms of serious fights, there are two conditions under which arrest laws result in an increase in this outcome for youth. These are when the arrest laws do not require additional factors, and when there is no time limit for the arrest to occur. Both marginal effects equate to a 13.2 percent (0.005/0.038) increase in probability of serious fights. Additionally, as was found in Table 8, we again find in Table 9, no significant impacts on the violence index, regardless of the heterogeneity in the arrest laws.

Finally, there is some evidence of a negative association between arrest laws and substance use (especially index 1). The significant negative effects are for the cases of non-mandatory arrest laws, and arrest does not require additional factors, alternative implementation dates, and no time limit for arrest to occur – with the first three cases being relevant for substance use index 1, and the last case being of relevance to substance use index 2.

The full range of results in Table 9 signal that heterogeneities in arrest laws are important to understand and investigate further, with potentially differing impacts on youth outcomes depending on the nature of the laws. The only outcome where this was not particularly evident was suicidal tendency, where there is strong and consistent evidence of a decline in tendency with the passage of arrest laws, regardless of the form they take. This may serve as a signal that youth view arrest laws as having a deterrent impact and those vulnerable to experiencing or witnessing domestic violence are less likely to consider suicide.

**Table 9**  
**Heterogeneity in domestic violence warrantless arrest laws with respect to YRBS outcomes**

	Serious fight	Violence Index	Suicidal tendency Index	Substance use Index1	Substance use Index2
	(1)	(2)	(3)	(4)	(5)
<b>Panel 1: Extent of police discretion</b>					
Mandatory	0.004 (0.004)	0.009 (0.011)	-0.019*** (0.007)	-0.004 (0.012)	-0.006 (0.018)
Non-Mandatory	0.004 (0.003)	0.008 (0.015)	-0.011* (0.007)	-0.034** (0.016)	-0.007 (0.030)
<b>Panel 2: Arrest requires additional factors to domestic violence</b>					
Required	0.003 (0.003)	0.003 (0.012)	-0.017** (0.008)	-0.002 (0.012)	0.017 (0.026)
Not required	0.005* (0.003)	0.012 (0.011)	-0.015** (0.006)	-0.027* (0.015)	-0.023 (0.020)
<b>Panel 3: Time limit for arrest to occur</b>					
Yes	0.004 (0.005)	0.018 (0.011)	-0.023*** (0.007)	-0.014 (0.009)	-0.007 (0.018)
No	0.005* (0.003)	-0.001 (0.014)	-0.008 (0.005)	-0.020 (0.017)	-0.006* (0.026)
<b>Panel 4: Alternative implementation date</b>					
Arrest law	0.004 (0.003)	-0.005 (0.010)	-0.014*** (0.006)	-0.022* (0.013)	-0.009 (0.019)
State fixed effects	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes
Individual demographic controls	Yes	Yes	Yes	Yes	Yes
Economic and policy controls	Yes	Yes	Yes	Yes	Yes
State-specific linear time trends	Yes	Yes	Yes	Yes	Yes
Pre-treatment trends	Yes	Yes	Yes	Yes	Yes
Sample size	686676	842081	843429	859453	859453

Note: Marginal effects from probit regressions using YRBS data are reported above. Standard errors are corrected for clustering on the states and are provided in parentheses. Individual demographic controls include age, sex, race, and ethnicity. State characteristics and economic & policy controls include high-school dropout rates, arrest rate for offences against family and children, unemployment rates, per capita personal income, cigarette tax, beer tax, anti-bullying policies, and criminal sanctions for child witness of domestic violence laws.

\*, \*\*, and \*\*\* denote significance at the 10%, 5% and 1% level.

As a final step in our empirical endeavour to investigate the link between warrantless arrest laws and youth outcomes, we analyze sex-specific impacts of the laws on the relevant YRBS outcomes. In Table 10, we report the marginal effects obtained from the probit regressions using the YRBS sample disaggregated by sex.

For girls, we observe that implementation of warrantless arrest laws leads to an 8.1 percent decrease in the probability of having suicidal tendencies (column 3) and a 5.2 percent

decrease in the probability of using substance (Substance use Index 1; column 4). The effects are statistically significant at the conventional levels.

For boys, arrest laws lead to an 11.5 percent increase in the probability of being involved in serious fights (significant at the 10 percent level) and a 1.9 percent decrease in the probability of using substance (substance use index 1).

**Table 10**  
**Sex-specific effects of domestic violence warrantless arrest laws**

	Serious fight	Violence Index	Suicidal tendency Index	Substance use Index1	Substance use Index2
	(1)	(2)	(3)	(4)	(5)
High-school girls	0.003 (0.002)	-0.004 (0.011)	-0.021** (0.008)	-0.024** (0.009)	-0.013 (0.016)
Sample size	349898	429871	430197	437732	437732
High-school boys	0.006* (0.004)	0.025 (0.017)	-0.010* (0.005)	-0.009 (0.014)	0.001 (0.020)
Sample size	336778	412210	413232	421721	421721
State fixed effects	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes
Individual demographic controls	Yes	Yes	Yes	Yes	Yes
Economic and policy controls	Yes	Yes	Yes	Yes	Yes
State-specific linear time trends	Yes	Yes	Yes	Yes	Yes
Pre-treatment trends	Yes	Yes	Yes	Yes	Yes

Note: Marginal effects from probit regressions using YRBS data are reported above. Standard errors are corrected for clustering at the state-level and provided in parentheses. Individual demographic controls include age, sex, race, and ethnicity. State characteristics and economic & policy controls include high-school dropout rates, arrest rate for offences against family and children, unemployment rates, per capita personal income, cigarette tax, beer tax, anti-bullying policies, and criminal sanctions for child witness of domestic violence laws.

\*, \*\*, and \*\*\* denote significance at the 10%, 5% and 1% level

## 7 Conclusions

This paper's aim was to empirically investigate the impact of warrantless arrest laws on youth. The impact of such laws has received some attention in the literature (see Iyengar (2009), and Zeoli et al. (2011a and 2011b) as examples), but the impact on youth specifically has not been conducted, to the best of our knowledge. We utilize variation in the timing of implementation of the laws, and employ a difference in differences framework to examine both the direct and indirect impacts on youth in the United States. Our population sample covered all 51 states and accounted for a range of possible heterogeneities in arrest laws, such as mandatory versus non-mandatory, whether additional factors were required for an arrest, if there was a specified time limit for arrest to occur, and also potential alternative implementation dates, depending on the interpretation of the legal statutes.

With the passage of the warrantless arrest laws, we expect two possible mechanisms to be at play. Firstly, these laws may act in a deterrent fashion and reduce the incidence of domestic violence cases. Secondly, due to the stricter nature of these laws, this may result in a reduction in reporting and a fear of reprisal. If the latter rings true and there is an increase in reprisal, then we may see an increase in the incidence of domestic violence cases. It is ofcourse also possible that both mechanisms are operating and if they are equal in weight, then they may cancel each other out. This may be the potential reason behind why we find no significant link between the arrest laws and domestic violence-related homicide rates. This is the case for both youth as victims and as offenders, for the period of 1991 to 2012. This indicates no observed direct impact on fatal cases of domestic violence of the passage of laws.

We next looked at a range of indirect impacts of the arrest laws on the mental wellbeing and behavioural outcomes for youth, using YRBS data for a similar timeframe, 1991 to 2013. Our findings with the YRBS analysis point to the passage of arrest laws resulting in a significant drop in suicidal tendency for youth, especially for females. There is also some evidence that there is a drop in substance use behaviour. These results potentially indicate that the warrantless arrest laws resulted in youth feeling safer and more protected and thus improved their mental wellbeing, and resulting activities.

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Appendix

Table A.1

Summary information of YRBS variables (sub-components) used to construct individual-level dependent variables

Variable	Survey Questions	Binary Coding	Used to construct	Youth Mean (SD)	Girls Mean (SD)	Boys Mean (SD)	p-value ( $\mu_f - \mu_m$ )
Serious Fight	<i>During the past 12 months, how many times were you in a physical fight in which you were injured and had to be treated by a doctor or nurse?</i>	0: Never 1: At least once	-	0.038 (0.192)	0.025 (0.156)	0.052 (0.222)	0.000
Fight	<i>During the past 12 months, how many times were you in a physical fight?</i>	0: Never 1: At least once	Violence index	0.319 (0.466)	0.243 (0.429)	0.400 (0.489)	0.000
Carry weapon	<i>During the past 30 days, on how many days did you carry a weapon such as a gun, knife, or club?</i>	0: Never 1: At least once	Violence index	0.196 (0.397)	0.081 (0.273)	0.318 (0.465)	0.000
Suicide consideration	<i>During the past 12 months, did you ever seriously consider attempting suicide?</i>	0: No 1: Yes	Suicidal tendency index	0.180 (0.384)	0.225 (0.417)	0.133 (0.339)	0.000
Suicide plan	<i>During the past 12 months, did you make a plan about how you would attempt suicide?</i>	0: No 1: Yes	Suicidal tendency index	0.142 (0.348)	0.171 (0.377)	0.110 (0.313)	0.000
Suicide attempt	<i>During the past 12 months, how many times did you actually attempt suicide?</i>	0: Never 1: At least once	Suicidal tendency index	0.085 (0.279)	0.106 (0.308)	0.061 (0.240)	0.000
Smoking days	<i>During the past 30 days, on how many days did you smoke cigarettes?</i>	0: Never 1: At least once	Substance use Index1	0.229 (0.420)	0.218 (0.413)	0.240 (0.427)	0.000

Cigarettes per Day	<i>During the past 30 days, on the days you smoked, how many cigarettes did you smoke per day?</i>	0: Never 1: At least one/day	Substance use Index2	0.233 (0.423)	0.222 (0.415)	0.245 (0.430)	0.000
Drinking	<i>During the past 30 days, on how many days did you have at least one drink of alcohol?</i>	0: Never 1: At least once	Substance use Index1	0.416 (0.492)	0.405 (0.490)	0.427 (0.495)	0.000
Binge-drinking	<i>During the past 30 days, on how many days did you have 5 or more drinks of alcohol in a row, that is, within a couple of hours?</i>	0: Never 1: At least once	Substance use Index2	0.253 (0.435)	0.226 (0.418)	0.281 (0.449)	0.000
Chew tobacco	<i>During the past 30 days, on how many days did you use chewing tobacco, snuff, or dip, such as Redman, Levi Garrett, Beechnut, Skoal, Skoal Bandits, or Copenhagen?</i>	0: Never 1: At least once	Substance use Index1/ Index2	0.071 (0.258)	0.021 (0.145)	0.124 (0.330)	0.000
Marijuana	<i>During the past 30 days, how many times did you use marijuana?</i>	0: Never 1: At least once	Substance use Index1/ Index2	0.200 (0.399)	0.172 (0.378)	0.229 (0.420)	0.000
Other drugs	<i>During the past 30 days, how many times did you use any form of cocaine, including powder, crack, or freebase?</i>	0: Never 1: At least once	Substance use Index1/ Index2	0.035 (0.184)	0.026 (0.160)	0.044 (0.205)	0.000

Note: Data sourced from YRBS 1991 - 2013