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Does the certainty of arrest reduce domestic violence? Evidence from mandatory and recommended arrest laws☆

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ABSTRACT

Domestic violence remains a major public policy concern despite two decades of policy intervention. To eliminate police inaction in response to domestic violence, many states have passed mandatory arrest laws, which require the police to arrest abusers when a domestic violence incident is reported. Using the FBI Supplementary Homicide Reports, I find that mandatory arrest laws actually increased intimate partner homicides. I discuss two potential mechanisms for this increase in homicides: decreased reporting by victims and increased reprisal by abusers. I investigate validity of these hypotheses by examining the effect of mandatory arrest laws on different sub-groups and by analyzing family homicides where the victim is less often responsible for reporting. There appears to be consistent evidence for the reporting mechanisms. For family homicides, mandatory arrest laws appear to reduce the number of homicides. This study therefore provides evidence that these laws may have perverse effects on intimate partner violence, harming the very people they seek to help.

1. Introduction

Women are more likely to be beaten, raped, or killed by a current or former male partner than by anyone else (Epstein, 1999). Despite two decades of increased public awareness, domestic violence remains a serious public policy issue in the United States. States, faced with increased liability for police inaction in the mid-eighties to late-nineties, passed laws requiring the warrentless arrests of individuals police believed to be responsible for misdemeanor assault of an intimate partner. Many of these policies were justified by results from a randomized experiment that demonstrated that arrests were effective at deterring future violence. This experiment was extended to support mandating arrest in all cases of domestic violence. However, the experiment provided no evidence on the effectiveness of a public policy requiring arrest. Policies which mandate arrest (i.e. make arrests certain, conditional on reporting) may have a different result from experiments which probabilistically apply arrest. Indeed the empirical analysis presented in this paper demonstrates that mandatory arrest laws increase intimate partner homicides.

Using a difference-in-difference framework, I tested to see if mandatory arrest laws affected the level of domestic violence. I found that intimate partner homicides increased by about 60% in states with mandatory arrest laws. These results may be due to changes in the reporting behavior of victims or increased reprisal by abusers after arrest. Because police intervention may decrease the risk of escalation and thus the risk of homicides, this rise in homicide rates is consistent with a decline in reporting for intimate

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partner homicides. These results are also consistent with a reprisal explanation, in which abusers respond to arrest with increased future violence against their partners. It is possible that both effects are occurring, which may increase the overall rate of homicides. To test for evidence of either hypothesis, I compare homicide rates among sub-populations. I find evidence of a differential response by race, which is consistent with the reporting explanation to the extent that different sub-populations have different levels of distaste for contacting the police. I also test the effect on family violence to try to distinguish the potential explanations. In most cases of child abuse, the reporting of abuse comes from a third party (such as a teacher or doctor). In such cases, the certainty of arrest does not shift the incentives of the third party to report, and as such we would expect to see a deterrence effect from arrest. In contrast, if it is simply arrest that angers the abuser, we might expect to find a similar rise in child abuse. I find evidence of declines in familial homicides consistent with the theory that a decline in reporting by victims may be responsible for the perverse effects of mandatory arrest laws.

This study attempts to provide a careful evaluation of a public policy that currently enjoys both popular and financial support. This enhanced understanding of the full range effects generated by arrest laws for domestic abuse will hopefully be useful in constructing a more effective criminal justice response to domestic violence. This paper also highlights an important consideration for policy makers; criminal justice policies aimed at deterring violence must concern themselves with both the probability of detection and the actual penalty enhancement. Laws like mandatory arrest laws may fail if victims respond as much or more than offenders to policy changes. Careful consideration of the incentives for victims as well as criminals is required to avoid government programs that become counterproductive, harming the very people they seek to help.

2. The emergence of mandatory arrest laws for intimate partner violence

Policies that encourage or require arrest of domestic abusers play a prominent role in the government's attempt to combat domestic violence. This is in part because the criminalization of domestic violence represented the major shift in the acceptance and treatment of domestic violence and in part because arrest laws represent a transparent mechanism by which the government can enforce anti-violence statutes.

The criminal justice response to intimate partner violence has been codified in many jurisdictions through policies which encourage or mandate the arrest of individuals who commit domestic abuse. Currently, fourteen states and the District of Columbia have passed mandatory arrest laws. These laws require police to arrest a suspect without a warrant, if there is probable cause to suspect that an individual has committed some form of assault (either misdemeanor or felonious) against an intimate partner or family member. An additional eight states have recommended arrest laws, which specify arrest as a recommended but not required when confronted with probable cause that an intimidate partner or familial assault has occurred. States in both of these groups are reported in Table 1.

To begin an evaluation of arrest laws for domestic abuse, it is first necessary to establish why these laws were initially passed. Mandatory arrest laws appear to have been passed largely in response to a court decision in Connecticut as well as what many took

Table 1
Mandatory arrest laws by state

	State	Year passed	Code/statute
Recommended arrest states	AZ	1991	Ariz. Rev. Stat. Ann. §13-3601(B)
	CA	1993	Cal. Penal Code §836(c)(1)
	KS	2000	Kan. Stat. Ann. §22-2401(c)(2)
	MS	1995	Miss. Code Ann. §99-3-7(3)(a)
	MO	1989	Mo. Ann. Stat. §455.085(1)
	NY	1994	N.Y. Crim. Proc. Law §140.10(4)
	OH	1994	Ohio Rev. Code Ann. §2935.032(A)(1)(a)
	SC	2002	S.C. Code Ann. §16-25-70(B)
Mandatory arrest states	AK	1996	Alaska Stat. §18.65.530(a)
	CO	1994	Colo. Rev. Stat. Ann. §18-6-803.6(1)
	CT	1987	Conn. Gen. Stat. §46b- 38b(a)
	DC	1991	D.C. Code Ann. §16-1031(a)
	IA	1990	Iowa Code §236.12(3)
	ME	1995	Me. Rev. Stat. Ann. Tit. 19-A, §4012(6)(D)
	NV	1989	Nev. Rev. Stat. Ann. §171.137(1)
	NJ	1991	N.J. Stat. Ann. §2C:25-21(a)
	OR	2001	Or. Rev. Stat. §133.055(2)(a)
	RI	2000	R.I. Gen. Laws §12-29-3(c)(1)
	SD	1998	S.D. Codified Laws §23A-3-2.1
	UT	2000	Utah Code Ann. §7-36-2.2(2)(a)
	VA	2002	Va. Code Ann. §19.2-81.3(B)
	WI	1996	Wis. Stat. Ann. §968.075(2)(a)
	WA	1999	Wash. Rev. Code Ann. §10.31.100(2)

Source: West (2003). Mandatory arrest states are defined as states where officers have no discretion as to whether or not to make a warrentless arrest when an intimate partner offense is reported. Recommended arrest states are defined as states where officers are instructed, but not required, to make a warrentless arrest when an intimate partner offense is reported. For specific information on coverage see data appendix.

to be empirical support for arrest from a randomized experiment. Nevertheless, we might be skeptical of any subsequent analysis attempting to establish the causal effect of arrest laws on domestic violence if these laws were passed in response to a rising trend in domestic violence levels. This does not appear to be the case.

Historically, law enforcement has been reluctant to arrest or even intervene in cases of domestic violence. In fact in the 1970s, the American Bar Association (1973) urged police to use conflict resolution, not arrests, when intervening in "conflicts… which occur between husband and wife." In the early eighties this began to change as attitudes towards domestic violence changed. Increasingly, there was political pressure for states to offer more protection for victims of domestic violence.[1] Moreover, there was a shift in the medical opinion regarding the dangers posed to victims of domestic violence. For instance, the American Medical Association began to advise its members that counseling was dangerous and increased its effects to educate its member about the harms of domestic violence.

The emergence of mandatory arrest laws occurred within this environment, largely prompted by two events. First, Thurman v. City of Torrington (1984), a lawsuit in Connecticut, established the right to police protection from domestic violence.[2] Threats of future lawsuits served as motivation for municipalities to protect themselves from liability issues by implementing more aggressive arrest policies.[3] The establishment of police liability in domestic violence cases created the desire to monitor and regulate police enforcement of restraining orders and intervention in violent incidents. A natural means to do this was the removal of police discretion through a policy which mandated arrest. However, arrest policies might not have been so widely adopted had it not been for the second event—the Minnesota Domestic Violence Experiment (MDVE). The results of this experiment were used by US Department of Justice, academics, legislators, and criminal justice spokespersons to justify and support mandatory arrest policies (Mignon et al., 1995).[4] For example, the Violence Against Women Act (1994) used the results from this experiment to justify grants and funds to support pro-arrest policies in various states.[5] This change in funding and availability of training encouraged several more states to pass mandatory arrest laws.

The use of mandatory arrest laws was thus in many respects predicated on the results of MDVE. This experiment, funded by the Minnesota Police Department, the Police Foundation, and the Department of Justice, was run by randomly assigning a police response to domestic violence calls (Wanless, 1996). Police applied one of three possible treatments: (1) advising and counseling the couple, (2) separating the individuals, or (3) arresting the suspect. Researchers then interviewed the victims shortly after police involvement and then followed up every two weeks for six months. The original results found that arresting the suspect resulted in substantially less future violence than did either advising or counseling (Sherman, 1992).[6] An in-depth evaluation of the results by Tauchen and Witte (1995) found that arrest resulted in significantly more deterrence than either advising or separating the couple, consistent with the original findings of the experiment. However, unlike the original findings, Tauchen and Witte used a dynamic setting and found that most of the deterrent effect of arrest occurs within two weeks of the initial arrest. Thus, any deterrent effect that exists is highly transitory.

While this experiment provided support for the contention that arrest deters abuse, the applicability of the findings is uncertain. The public in general and battered women in particular were not informed of this experiment. Thus, the experiment actually tested the effect of a probabilistic arrest rather than a deterministic policy which requires arrest. This difference is significant because of behavioral differences that may arise in an ongoing nature of the relationship between the battered women and their abusers. A noteworthy difference between a mandatory arrest policy and the MDVE is the potential response by battered women to the certainty of an arrest of their abuser. The response by batterers relative to the response by victims to the increased costs of abuse may be relevant when determining the efficacy of an intervention such as mandatory arrest laws.

Thus, while a full discussion of the political economy of the emergence of domestic violence laws is beyond the scope of this paper, the motivation of most of these arrest laws does not appear to have been the level of intimate partner violence. In a detailed analysis of the motivations for mandatory arrest laws, Stark (1993) states that the most important reason for passing mandatory arrest laws was controlling police behavior in response to political pressure and liability exposure. Reducing the level of violence was actually only of distant concern after the desire to immediately stop violence and avoid liability from inaction. Additionally, in

[1] In 1984, the U.S. Attorney General established domestic violence as a crime by ordering all criminal justice agencies to treat it as such. In the same year, Congress passed the Family Violence Prevention and Services Act, which funded some domestic violence programs (for shelters, counseling, research, and training of law enforcement agencies). Also in 1984, the Victims of Crime Act allocated $150 million for grant aid to survivors of crimes. Priority for this grant aid was given to victims of domestic violence, sexual assault, and child abuse (Brooks, 1997).

[2] In 1984, Tracey Thurman, a battered woman was awarded 2.3 million dollars in damages from the city of Torrington, Connecticut. The Torrington Police failed to intervene in Thurman's domestic dispute despite her frequent calls for help resulting in severe injuries and permanent paralysis for Ms. Thurman. This decision signaled a shift in legal liability where not only individual officers but also police departments could be held liable for failure to protect battered women. Twenty-four police officers, as well as the City of Torrington were held accountable for their failure to intervene (Sparks, 1997).

[3] Although *Thurman v. Torrington* is certainly the most famous case establishing the right to police protection from domestic violence, several other states also found police departments liable for failing to protect battered women (Heise and Chapman, 1992). The first class action lawsuit, *Scott v. Hart*, No. C-76-2395, was filed against the Oakland, California city police in 1976. Two months later, in *Bruno v. Codd*, 396 N.Y.S.2d 974 (Sup. Ct. Special Term 1977), found police liable for failure to comply with state laws regarding domestic violence. In 1985, *Sorichetti v. City of New York* held police liable for failing to investigate and enforce violations of orders of protection. See Bracher (1996) for a discussion of these cases. For a more detailed discussion on the legislative history of mandatory arrest laws, see Buzawa and Buzawa (1996) and Stark (1993).

[4] While the results from the experiment formed the justification for many pro-arrest policies, the authors and other experts cautioned against generalizing from these findings.

[5] Currently, the Violence Against Women Office at the Department of Justice spends between $30 to 50 million each year on grants to encourage these mandatory arrest laws.

[6] Further replications of the MDVE in Milwaukee, Omaha, Colorado Springs and Charlotte have produced mixed results. For a comparison of these experiments, see Symposium on Domestic Violence, 1992.

as much as public pressure existed regarding the passage of these laws, this pressure was not due to the level of violence but rather perceived government treatment of offenders. For example, Buzawa and Buzawa (1996) argue that alternative reforms (such as mediation) were dismissed as inappropriate or sexist as American society became more conservative and punitive towards offenders. Thus, while these laws were not passed as parts of omnibus "tough on crime" legislation, they represent the desire to adopt a more punitive approach to crime. Finally, the timing of arrest law passages appears to be tied to both the publication of the MDVE results, the promotion of these results by the Justice department in subsequent years, and finally federal funding of these policies after 1994.[7] In this vein, pre-law passage crime rates do not significantly predict the timing of the law passage (results not reported). Thus, these laws appear to be at least plausibly exogenous and are a useful means of identifying the effects of the certainty of arrest on intimate partner violence.

3. Behavioral responses and the unintended consequences of arrest law

The quasi-experimental results presented in this study appear to conflict with the findings of the MDVE. The conflict can be explained by recognizing that the MDVE estimated the effect of an actual arrest conditional on reporting while the estimates presented in this study estimate the unconditional effect of the certainty of arrest. The MDVE held constant the probability of reporting given violence because all cases in the experiment required an initial report of domestic abuse to the police. Thus, the MDVE estimated the effect of a decrease in batter's utility, when they abuse and are reported, on their probability of choosing violence in the future. Unfortunately, if the victim also faces an increased cost from the increased penalty, then the overall effect of these laws on abuse is theoretically ambiguous (and empirically these laws appear to increase levels of abuse).

To illustrate how changes in the level of homicides can be linked to the total number of abusive incidents, consider a model where with some small probability, p, domestic abuse escalates to murder. For n intimate partner incidents, the number of homicides in a jurisdiction is then pn. There are two main theories on potential responses which may result in increased homicides after arrest laws: reporting (victim response) and reprisal (abuser response). I will consider each in turn.

The reporting hypothesis suggests that victims are less willing to report an incident if their abuser will be arrested. Suppose that the probability of reporting given violence decreases after the passage of mandatory arrest laws. Because police presence, regardless of the police response, can disrupt a violent incident and prevent escalation to homicide, this failure to report to the police can increase the rate of intimate partner homicides. Thus, the victim's decision not to notify the police may increase p. This is not the overall effect of the law since the threat of arrest deters (as it did in the MDVE), then n, may decrease. In this case, the effect of the law on homicides is ambiguous. While arrest, conditional on reporting, deters violence, the unconditional effect of arrest on violence may be small or zero if victims substantially reduce their reporting.

In the case of domestic violence, victims may decide not to report for several reasons. First, there is a psychological and emotional component of intimate partner abuse that often generates victims who remain committed to their abuser and do not wish to send him to prison. Thus, the victim's guilt may increase her/his own costs of reporting as well as the abusers.[8] Second, if abusers are arrested but no further legal action is taken, they may return home within a day of their arrest and further terrorize their victim. In a non-experimental evaluation of mandatory arrest as a policy, Lyon (1999) used a logistic model to compare the likelihood of arrest under mandatory arrest laws versus pro-arrest laws in two cities in Michigan. She found that once a victim calls the police to report an incident, she is significantly less likely to call again. She posits this was likely because police intervention in the form of an arrest resulted in retribution by the abuser deterring future reporting.[9] Third, in many cases, arrest laws resulted in the victim also being arrested if there was evidence that she (or he) physically assaulted her (or his) partner. In many areas, women constitute nearly 20% of domestic violence arrests, a far higher percentage than the estimated proportion of female abusers.[10] Over half of these female arrestees can be identified as previous victims of intimate partner violence (Martin, 1997). Anecdotal evidence from some battered women advocates suggests that these "dual arrests" are the most serious problem with mandatory arrest.[11] Dual arrests have serious implications for victims who are immigrants and may be deported if convicted of assault. In addition, those who have children face potential loss of custody during the arrest period. All of these costs may result in an increased unwillingness to report abuse to the police.

A different explanation for the response could be that abusers respond to arrest by punishing victims and this increases intimate partner homicides. Suppose p increases because violence becomes more severe. This could occur if abusers are very angry

[7] For further discussion of this, see Gelles (1996) which describes the rapidity of the law changes and the timing of various law and policy changes at the city and state level.

[8] Recent research finds that many women do not perceive any benefit from mandatory arrest laws, no-drop policies (requiring prosecution conditional on reporting) and mandatory medical reporting and these laws may make them less willing to report in the future (See Smith, 2000).

[9] Rennison (2001) found that fear of reprisal from abuser was the most commonly cited cause for not reporting a domestic violence incident. This is hotly contested claim. Mills (1998), based on research by Sherman and Berk (1984), claims that arrests actually increase re-assaults. More recent work by Maxwell et al. (2001) find that there is no significant change in the risk of assault.

[10] For example, in Phoenix, AZ, 18% of domestic violence arrests are women (AZCASA). Women are thought to be abusers in less than 5% of intimate partner violence cases (Dobash et al., 1992). Though some work suggests there is a surprisingly high rate of female on male abuse (see Strauss and Gelles, 1980) this work is problematic and, for the most part, ignores the severity and context of the violence (see Blau, 1998). This is particularly relevant in the case of intimate partner abuse. For example, suppose a husband spent years beating his wife severely. At the time of the survey, the husband shoved his wife and she immediately threatened him with a knife. The conflict tactics scale (CTS) treats the wife's behavior as aggressive when it is, in context, clearly defensive. Moreover, the CTS fails to properly differentiate acts of violence that constitute severe abuse. When severity of abuse is considered, men typically have the higher rates of the most dangerous behaviors, such as firing a gun, repeated their violence more often, and do more physical harm. For a greater discussion see DeKeseredy and Schwartz (1998).

[11] This statement is based on conversations with individuals at battered women's coalitions in NJ, AZ, NY, CA, CT and IL.

when returning home after arrest and so more frequently commit violence against their partners. Thus for a given n intimate partner incidents, the number of homicides pn increases. Note that if there is no deterrence effect, *i.e.* n is constant, then, once again, the effect on the law is to increase violence. However, if there is a decrease in incidents (n declines) then the overall effect of mandating arrest is ambiguous. This response is consistent with evidence on victim fears. As discussed above, fear of reprisal is the most commonly cited reason for not reporting. To the extent that the fear is rational, this is consistent with the reprisal hypothesis. Moreover, when a victim leaves her relationship that she is at the greatest risk from her partner.[12] If arrest allows women to leave, then reprisal rates may increase. Evidence against this hypothesis comes from the MDVE, which found no significant increase in reprisal, though this may be because the abusers did not blame their victims for their subsequent arrest (instead blaming police officers). Subsequent studies find no significant deterrence effects, but a significant increase among some sub-groups of offenders.

4. Estimating the effectiveness of mandating arrest

To test the effectiveness of mandatory arrest laws, I consider the effect of these laws on intimate partner abuse. This requires special attention to the total number of incidents of domestic violence, not simply the number of reported incidents because the fraction of incidents that are reported to the police is potentially affected by this policy.[13] If I cannot observe unreported incidents, changes in the number of reported incidents and the total number of incidents (both reported and unreported) are observationally equivalent.[14] In part because I can observe victim–offender relationship and in part because these crimes are almost perfectly reported, I use measure of intimate partner homicides as a way to measure intimate partner abuse. Assuming that police intervention can reduce the probability of violence changes in the intimate partner homicide measure may provide insight into the impact of mandatory arrest laws on intimate partner violence.[15]

To construct a dataset of intimate partner homicides, I use the FBI Uniform Crime Reports, Supplementary Homicide Reports which provide data for all homicides that took place from 1976 to 2003 in all 50 states and the District of Columbia with additional descriptive variables about the victim, offender, and the nature of the crime (FBI, 1976–1999, 1965–1999, 1990–1999). For each state, I define intimate partner violence as the set of relationships covered by the law. In most cases, intimate partner homicides include any homicide committed against a husband, wife, common-law husband, common-law wife, ex-husband, or ex-wife. While the specific coverage of arrest laws varies by state, the general categories and their proportion of the overall number of homicides are listed in Table 2.[16] The data are constructed at the incident level with about 6.5% of the sample (36,442 observations) being intimate partner homicides.[17] I constructed a count of the number of relevant homicides by aggregating the incidents of intimate partner homicide, as defined above, in a given state for each year from 1976 until 2003. I also aggregated the number of intimate partner homicides by the race of the victim and offender and by sex of the victim and offender. Estimates are then scaled using census estimates for state population.[18]

A plot of the trend in various types of homicides before and after a mandatory arrest law is passed suggests that these laws may have had a significant impact on intimate partner abuse. Fig. 1 shows the rate of intimate partner and family homicide rates as a function of time since the arrest law change. There appears to be a discrete increase of about 0.4 intimate partner homicides per 100,000. There is only a small decline in the number of family violence homicides. In contrast, Fig. 2 shows that recommended arrest laws have relatively little effect on intimate partner or familial homicides.

Comparing intimate partner homicides in states with and without arrest laws before and after the passage of these laws, I estimate a linear regression of the impact of mandatory arrest laws on the number of intimate partner homicides per 100,000 inhabitants. Column (1) of Table 3 reports some coefficients from this regression. The mandatory arrest effect variable is defined as 1 in states that passed mandatory arrest laws in the years after the law was passed. Similarly, recommended arrest effect variable equals 1 in states that passed recommended arrest laws in the years after the law was passed. The results suggest that mandatory arrest laws are responsible for an additional 0.8 murders per 100,000 people. This corresponds to a 54% increase in intimate partner homicides.

There does not appear to be a significant effect in recommended arrest law states. Although the coefficient is negative, it is measured relatively imprecisely. Estimates in columns (2) and (3) of Table 3 include controls for some other state characteristics and crime rates. Because these laws are between the previous discretionary arrest system and the mandatory arrest, we might expect a

[12] Ronech Bachman and Linda Salzman (1995).

[13] The National Incident Based Reporting System, which does provide identification of the victim–offender relationships, is therefore ill-suited to the purposes of this study. Because the NIBRS is comprised solely of reported incidents, analysis of this data is not useful for measuring the true incidence of domestic violence.

[14] An ideal data source for this type of analysis would be the National Crime Victimization Survey (NCVS) with state level identifiers. Although previous researchers were able to access geo-coded versions of this data (see for example Farmer and Tiefenthaler, 2003), recent changes in the administration and management of the NCVS make such access no longer possible. Some analysis using this data previously obtained suggests that mandatory arrest laws may reduce intimate partner violence but also reduce the number of cases which are reported in the system (Dugan, 2003). Additional information about reasons why NCVS access is no longer possible is available upon request.

[15] The linkage between misdemeanor assault prevalence and intimate partner homicide is well established. See for example Gwinn and O'Dell (1993). Moreover the underlying causes are linked; see Mercy and Saltzman (1989).

[16] The specific coverage of each law is reported in the legal appendix.

[17] There is some measurement error in the victim–offender relationship variable. About 1.25% of female victims reported as having a relationship to their offender that would imply she's a man and about 0.43% of male victims reported as having a relationship to their offender that would imply he's a woman. Together, these account for about 200 observations and less than 1% of the total sample. This is due to the classification of multiple homicides. In multiple victim homicides, the first victim–offender relationship is recorded for all of the victims. Because the selection of the "first" victim tends to be arbitrary and this constitutes a very small fraction of the overall sample, these cases are excluded from analysis.

[18] This scaling by population seems the appropriate deflater as arrest laws often apply to unmarried couples. As a robustness check, the subsequent analysis has been repeated with the number of married couples with little qualitative effect on the coefficients.

Table 2
Definition and summary statistics for homicide categories

		Intimate partner homicide	Familial homicide	"Other" homicide	Excluded groups
N		36,422	24,664	299,961	26,700
Total percent of sample		6.45	4.37	53.10	4.73
	Husband	0.28	–	–	–
	Wife	0.52	–	–	–
	Common-law husband	0.07	–	–	–
	Common-law wife	0.07	–	–	–
	Ex-husband	0.02	–	–	–
	Ex-wife	0.04	–	–	–
		–		–	–
Fraction of category homicides	Mother/step-mother	–	0.11	–	–
committed against	Father/step-father	–	0.15	–	–
	Son	–	0.27	–	–
	Daughter	–	0.19	–	–
	Brother	–	0.19	–	–
	Sister	–	0.04	–	–
	Stepson	–	0.04	–	–
	Stepdaughter	–	0.02	–	–
	In–law	–	–	0.01	–
	Neighbor	–	–	0.02	–
	Acquaintance	–	–	0.44	–
	Employee	–	–	0.00	–
	Employer	–	–	0.00	–
	Friend	–	–	0.07	–
	Other known	–	–	0.09	–
	Stranger	–	–	0.35	–
	Other family			0.02	
	Homosexual relation	–	–	–	0.06
	Boyfriend	–	–	–	0.33
	Girlfriend	–	–	–	0.61

Notes: Fractions based on FBI Supplementary Homicide Reports, 1976–2003. Numbers in sub-categories may not sum to one due to rounding errors. 177,138 observations, or 31.36% of the sample was committed by an unknown assailant. The statistical implications of these missing observations are discussed in the data appendix.

smaller but positive effect on homicide rates. There are several reasons why this might not happen: First, if the arrest is perceived by abusers as discretionary, then they may not blame the victim for being arrested, reducing the reprisal rate. Second, because officers have discretion, victims may be more willing to call the police hoping to get an intermediate response. Finally, police themselves may not have changed their behavior much, opting to retain discretion and fill out paperwork rather than simply arresting.

There are several potential state-year factors that may be associated with both increased arrests and increased domestic violence. One important factor is the state crime rate, which may indicate how crime-prone society is as well as the other crimes police must deal

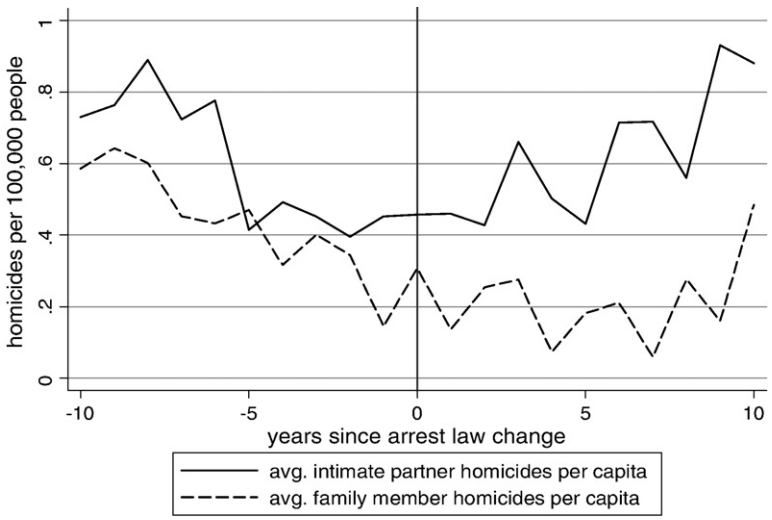

Fig. 1. Intimate partner and familial homicide rates in mandatory arrest law states. Notes: Means based on author's own calculations using Supplementary Homicide Reports 1976–2003. Intimate partner homicides include homicides of husbands, wives, ex-husbands, ex-wives, common-law husbands and common-law wives. Mandatory arrest states are defined as states where officers have no discretion as to whether or not to make a warrentless arrest when an intimate partner offense is reported.

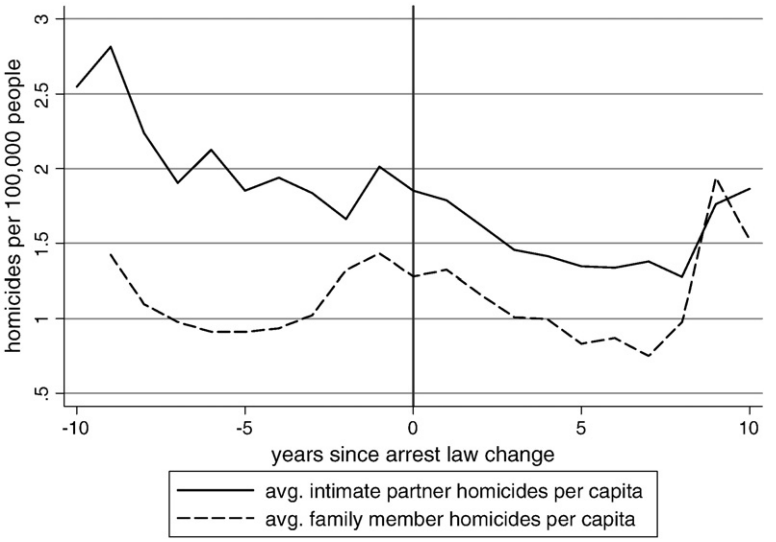

Fig. 2. Intimate partner and familial homicide rates in recommended arrest law states. Notes: Means based on author's own calculations using Supplementary Homicide Reports 1976–2003. Intimate partner homicides include homicides of husbands, wives, ex-husbands, ex-wives, common-law husbands and common-law wives. Recommended arrest states are defined as states where officers are instructed, but not required, to make a warrentless arrest when an intimate partner offense is reported.

with. To measure the violent crime rate, I used the number of rape, robbery, and assault crime reports per 100,000 people from the FBI's Uniform Crime Reports. Column (2) of Table 3 reports these results. Another concern might be state economic conditions which may increase domestic violence. I use average annual state unemployment rate derived from the Current Population Survey to control for this effect. There appears to be little effect of these limited covariates on the mandatory arrest law effect.

Column (3) of Table 3 includes a more rich set of covariates including state-year level variables on demographics, economic conditions and social policies. Because of racial differences in crime rates, I include some demographic controls (such as fraction of population that is black or white). I also include share of prison population in the state that is aged under 20, 20–35, 36–49 and 50 or older, which may be indicative of police behavior and crime enforcement levels in a given state. In addition to the unemployment rate used in the previous specification, I include economic covariates of crime such as state-year average log personal income and male–female employment ratio. Finally, the state social policy controls which are related to crime generally include whether the state has the death penalty and the AFDC/TANF max for a family of 3. I also included a control for when the state passed unilateral divorce laws based on Stevenson and Wolfers (2006). After including these covariates, the coefficient on the effect of mandatory arrest laws on intimate partner homicides shrinks to about 0.76, which is slightly smaller but similar in magnitude to the estimates from previous specification.

Because there was a significant secular trend in the domestic violence homicide rates, I estimated several specifications with trend variables. Column (4) reports the results when including a linear trend and column (5) includes the results when including a quadratic trend. The inclusion of trend controls appears to increase the coefficient, suggesting that declining rates of intimate partner violence were inducing an underestimate of the full effect of the law. Column (6) reports the results from including state-specific linear trends. The coefficient is still larger than the estimates without a trend, consistent with the downward trend biasing the OLS estimates.

To estimate the effect of the adoption of these laws over time, the specifications reported in column (7) include a time since law change interaction effect. Combined with the year fixed effects this both controls for any differences at a given point in time (year fixed effect) as well as differences generated from the duration of the law (years since law change). The main effect of mandatory arrest laws corresponds to the effect of mandatory arrest laws in the initial year of passage. This effect is about half the size of previously estimated effect and insignificant. However, while the effect in the initial year is not significantly different than zero, the effect in the second year (the mandatory arrest law main effect plus the 1 year post-law change effect) is about 0.7 and is significant with a p-value of 0.02 (joint test statistics not reported in the table). The total law effect in later years is similarly significant (although the 2 years post-law effect is significant only at the 10% level) and there does not appear to be a significantly different effect of these laws over time. The effect does not appear to grow significantly over time (although there does appear to be a slight lag in the effect, which is to be expected). It is somewhat surprising that the effect of the law does not grow over time. There are several potential reasons why this may be the case. First is the annual nature of the data which means that monthly growth over the first and second years may be missed and is aggregated into a single point estimate. Second, because the outcome variable is homicides, it may be less sensitive to the more subtle changes over time and, thus, is a relatively blunt outcome to measure the temporal diffusion of behavior. Finally, this is consistent with a reprisal story where the behavioral response is a one-time adoption immediately after the law change. If that is the case, and most police agencies adopted the law relatively rapidly, then we would not expect to see the effect grow over time.

Table 3
Difference-in-Difference Estimates of Mandatory and Recommended Arrest Laws

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	All intimate partner homicides per 100,000 inhabitants								
Dependant variable mean	1.48								
Mandatory arrest law effect	0.83**	0.81**	0.76**	1.15***	1.15***	1.10***	0.47	0.53***	0.41
(=1 in MA law states after law change)	(0.33)	(0.33)	(0.34)	(0.40)	(0.41)	(0.39)	(0.30)	(0.17)	(0.33)
Recommended arrest law effect	−0.61	−0.66	−0.62	−0.31	−0.30	−0.36	−0.96*	−0.03	−0.14
(=1 in RA law states after law change)	(0.61)	(0.59)	(0.47)	(0.64)	(0.63)	(0.63)	(0.54)	(0.18)	(0.32)
Unemployment rate	–	0.01	0.04	0.019	0.019	0.02	0.02	−0.035	−0.03**
		(0.07)	(0.09)	(0.07)	(0.07)	(0.07)	(0.07)	(0.014)	(0.01)
1 year post-law change	–	–	–				0.23	–	−0.03
							(0.27)		(0.38)
2 year post-law change	–	–	–				0.14	–	0.03
							(0.31)		(0.39)
3 or more years post-law change	–	–	–				0.41	–	0.15
							(0.42)		(0.34)
Estimation method	OLS	OLS	OLS	OLS	OLS	OLS	OLS	Poisson	Poisson
Controls for other violent crime rates [a]	N	Y	Y	Y	Y	Y	Y	Y	Y
Controls for unemployment rate [b]	N	Y	Y	Y	Y	Y	Y	Y	Y
State-year demographic variables [c]	N	N	Y	Y	Y	Y	Y	Y	Y
State-year economic and social controls [d]	N	N	Y	Y	Y	Y	Y	Y	Y
Linear trend	N	N	N	Y	N	N	N	N	Y
Quadratic trend					Y	N	N		N
State-specific trend controls	N	N	N	Y	Y	Y	N	N	N
Post-law interaction effects	N	N	N	N	N	N	Y	N	N
State fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y
R-squared	0.6125	0.6127	0.6214	0.6275	0.6284	0.6252	0.6746	–	–

Notes: All regressions include 992 observations. The dependant variable for each column is the column title per 100,000 inhabitants. Robust standard errors, clustered by state, are reported in parentheses. Coefficients that are significant at the .05 (.01, .1) percent level are marked with ** (***, *). Intimate partner homicides include homicides of husbands, wives, ex-husbands, ex-wives, common-law husbands and common-law wives. Mandatory arrest (MA) states are states which require an arrest conditional on a report of domestic violence. Recommended arrest (RA) states are states where officers are instructed but not required to make a warrentless arrest when an intimate partner offense is reported.

[a] Crime rate controls use FBI Uniform Crime reports for the number of crimes per 100,000 inhabitants. Indexed crimes included in the violent crime variable are murder, robbery, assault, and rape. Indexed crimes included in the nonviolent crime count are burglary, larceny, motor vehicle theft and drug crimes.

[b] Unemployment estimates are based on the March Current Population Survey.

[c] State demographic controls are based on the March Current Population Survey and include variables for the fraction of the population that is black, white, and other race, as well as age composition indicating share of prison population that is aged 14–19, 20–49, 50 or older.

[d] State economic control variables are based on the March Current Population Survey and include the variables log state personal income per capita, and female-to-male employment ratio. State social policy controls include max AFDC/TANF for a family of 3, unilateral divorce laws indicators (based on classification in Stevenson and Wolfers, 2006) and indicators for whether the state has the death penalty.

These results appear robust to a count model, the results of which are reported in columns (8) and (9). The count model estimates use a Poisson likelihood function with robust standard errors. The results in column (8) for the mandatory arrest law effect are nearly half the size of the corresponding OLS estimates. The coefficient in the model, which includes the full set of controls (column 9) is about two-thirds the size of the similar OLS estimate. Nevertheless, the coefficient remains positive and significant, suggesting an effect of about 35%.

Thus far, little attention has been paid to the bias that unknown homicides might introduce into evidence. Underlying this is the assumption that it is less likely that family homicides are not likely to be unsolved as the offender would be a known individual (as opposed to a stranger-on-stranger crime in which the offender may be entirely unknown to the police). This assumption is not entirely accurate and indeed may produce some bias in measuring homicide rates (see Riedel, 1998). However, a broad range of studies (e.g. Williams and Flewelling, 1987; Pampel and Williams, 2000) have suggested that family homicides need substantially less adjustment than intimate partner homicides and as such this assumption may not be too harmful.

Of particular concern is if the types of homicides that remain unsolved are changing over time and perhaps even if such changes are correlated with changes in mandatory arrest laws. For example, if more intimate partner homicides remain unsolved in non-mandatory arrest law states than in mandatory arrest law states, then the count constructed in this paper could reflect this rather than a true effect from the mandatory arrest law policy. Missing offender data might affect the identification if the probability of an unknown victim–offender relationship is significantly related to the whether a state passed a mandatory arrest law or not. In particular, I am concerned if the measurement error in the victim–offender variable is correlated with the law change, thus generating bias in the estimates.

I address this concern in several ways. First, I do a simple correlation between the fraction of cases with known offenders and an indicator for states that passed mandatory and recommended arrest laws after the passed the law. I find that mandatory arrest laws are not significantly correlated and recommended arrest laws are mildly positively correlated. However, such correlations may be misleading because it conflates year, state, and year-state variation. Second, I test to see if the fraction of cases with known offenders increased before and after arrest law passage. Note that I am not testing if states that have mandatory or recommended

arrest laws are more likely to have more unknown victim–offender relationships because the identification problem occurs only if the *adoption* of the law is associated with a *change* in the fraction of homicides with missing relationships. I estimate a difference-in-differences specification for the fraction of homicides with unknown relationships in states that passed arrest laws relative to those that did not before and after the law change. I find no significant relationship between law change and the fraction of unknown cases suggesting that simply omitting the unknown offenders may not be important. Finally, to test the sensitivity of the estimated mandatory arrest programmatic effect, I use three imputation procedures and re-estimate the results from Table 3. The first method used is a within-state measure (Williams and Flewelling, 1987). This procedure assumes that the distribution of homicides with an unknown distribution equals the distribution of homicides with a known distribution in a given state-year. The second measure is a weighted-sum approach which predicts the probability that a homicide was committed by an intimate partner (based on Pampel and Williams, 2000). This model uses the data on victim and circumstance of crime characteristics of homicides incidents to predict the relationship between victim and offenders at the incident level. The third measure (Fox, 2004) is a weighted-sum approach that predicts the fraction of unsolved cases which may be intimate partner homicides at the aggregate level.[19] There is nearly no difference in the point estimates from the imputed samples, although the larger standard errors make inference more difficult.[20] Thus, although the coefficients are only marginally significant, there appears to be little evidence of bias introduced from the unknown offender cases.

Because mandatory arrest laws were an important means by which domestic violence became represented and treated as a criminal justice issue (as compared to a family or community problem), we might be concerned that these laws will have a disparate impact on communities which have greater mistrust of the criminal justice system. In particular, some studies have shown that African-American women may be especially reluctant to report crimes to the police, preferring instead to handle instances within their own communities.[21] To evaluate the effect on different sub-groups of interest, columns (2), (3), and (4) of Table 4 compare the effect of mandatory arrest laws on intimate partner homicides committed between white couples, African-American couples and Asian couples, respectively. The point estimate for white and blacks are similar although blacks have a larger increase in percentage terms (based on mean homicides rates reported in the first row of each column, whites have a base rate of 0.81 intimate partner homicides per 100,000, and blacks have a rate of 0.51 per 100,000). This provides some evidence that the negative effect of mandatory arrest laws is disproportionately strong in certain communities. If we believe that certain communities may be less willing to report to police, the reporting effect might be stronger in those communities. In this case, I find some evidence of this, which may suggest that reporting by victims could explain the rise in homicides. The larger (in percent terms) effect among blacks and Asians provides support for the reporting effect over the reprisal effect if aversion to the police in general makes the response of minority communities stronger than the response in white communities.

Thus far, I have given little attention to the question of fault when constructing these counts. This is relevant because the intimate partner homicide count used thus far likely includes some homicides which are eventually (but not initially) classified as self-defense or justifiable. While I cannot identify "self-defense" killings from murders, homicides of males by their female intimate partner may more closely approximate the subset of cases for which self-defense is a plausible future classification. Column (5) of Table 4 presents estimate of intimate partner homicides with only female victims killed by male intimate partners. Column (6) presents estimates of intimate partner homicides committed against male by their female intimate partners. Intimate partner homicides of females increase about 50%, a similar percent increase to the main, unrestricted estimate (presented in column 1 of Table 4). Similarly, homicides of males by their female intimate partners are significantly affected by mandatory arrest laws—in fact, the effect is larger in percent terms. Overall, these results are consistent with either a reporting or reprisal effect in response to the law change. Indeed to the extent that police intervention facilitate some flight or escape by victims, murder of the abuser may be a substitute for other improved outside options. This evidence is consistent with studies that suggest that battered women who kill their husbands do so more often when they have fewer extra-legal opportunities.[22]

In an effort to verify the difference-in-difference framework, I test the effect of mandatory arrest laws on various sets of uncovered homicides. If the difference-in-difference estimates find a significant effect of mandatory arrest laws on homicides between individuals who should be unaffected by domestic and family violence laws, then it is likely the differences identified above may be unrelated to the passage of these laws.

For the purposes of these falsification tests, I define a class of homicides called "other homicides" which includes homicides committed against employees, employers, friends, other known individuals, and strangers.[23] These homicides should be unaffected by mandatory arrest laws. I estimate two specifications, one with only state and year fixed effects, and one with the full set of covariates described above. The results from these regressions are reported in Table 5, columns (1) and (2). In both specifications, neither mandatory arrest laws nor recommended arrest laws have a significant effect on the homicide level of

[19] A detailed description of each procedure is available in the data appendix.

[20] Results from this analysis area available in Data Appendix Table A1.

[21] This point is highly contested. Evidence from the National Crime Victimization Survey suggests that African American women report intimate partner violence at higher rates than do their white or Asian counterparts (see for example Rennison, 2001). However, surveys and outreach workers cite general mistrust of the police, mistreatment of the police and concerns that reporting will send partners with criminal records back to prison as reasons why under-reporting may be more prevalent in African-American communities (see Hamptonet al., 2003).

[22] See for example O'Keefe (1997). This is also consistent with evidence that finds female perpetrated abuse is affected not by criminal justice options but by outside extra-legal resources (e.g. shelters) (Browne and Williams, 1989).

[23] I have excluded homicides committed by individuals of "unknown relationship." While it is likely that these homicides were not committed by immediate family members or intimate partners, it is was not possible to estimate the subset of these homicides that would be covered and thus all are excluded. For additional information on this point see the data appendix.

Table 4
Estimates of the effect of mandatory and recommended arrest laws on intimate partner homicides for various sub-groups

	(1)	(2)	(3)	(4)	(5)	(6)
	All intimate partner homicides per 100,000 inhabitants	Intimate partner homicides with white victims and perpetrator	Intimate partner homicides with black victims and perpetrators	Intimate partner homicides with Asian victims and perpetrators	Homicides of females by male intimate partners	Homicides of males by female intimate partners
Dependant variable mean	1.48	0.81	0.59	0.01	0.89	0.59
Mandatory arrest law effect	1.1525***	0.5080**	0.6208***	0.0144*	0.6023**	0.5502***
(=1 in MA law states after law change)	(0.4067)	(0.2339)	(0.1981)	(0.0077)	(0.2459)	(0.1863)
Recommended arrest law effect	−0.2960	−0.1225	−0.1342	0.0118	−0.0738	−0.2222
(=1 in RA law states after law change)	(0.6348)	(0.2785)	(0.3435)	(0.0124)	(0.3251)	(0.3270)
Unemployment rate	0.0196	0.0181	0.0066	−0.0000	0.0037	0.0159
	(0.0741)	(0.0339)	(0.0403)	(0.0005)	(0.0366)	(0.0383)
Controls for other crime rates [a]	Y	Y	Y	Y	Y	Y
Controls for unemployment rate [b]	Y	Y	Y	Y	Y	Y
State-year demographic variables [c]	Y	Y	Y	Y	Y	Y
State-year economic and social controls [d]	Y	Y	Y	Y	Y	Y
State fixed effects	Y	Y	Y	Y	Y	Y
Linear trend	Y	Y	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y	Y	Y
R-squared	0.6284	0.6733	0.5643	0.4536	0.6448	0.5910

Notes: All regressions include 994 observations. The dependant variable for each column is the column title per 100,000 inhabitants. Robust standard errors, clustered by state, are reported in parentheses. Coefficients that are significant at the .05 (.01, .1) percent level are marked with ** (***, *). Intimate partner homicides include homicides of husbands, wives, ex-husbands, ex-wives, common-law husbands and common-law wives. Mandatory arrest (MA) states are states which require an arrest conditional on a report of domestic violence. Recommended arrest (RA) states are states where officers are instructed but not required to make a warrentless arrest when an intimate partner offense is reported.

[a] Crime rate controls use FBI Uniform Crime reports for the number of crimes per 100,000 inhabitants. Indexed crimes included in the violent crime variable are murder, robbery, assault, and rape. Indexed crimes included in the nonviolent crime count are burglary, larceny, motor vehicle theft and drug crimes.

[b] Unemployment estimates are based on the March Current Population Survey.

[c] State demographic controls are based on the March Current Population Survey and include variables for the fraction of the population that is black, white, and other race, as well as age composition indicating share of prison population that is aged 14–19, 20–49, 50 or older.

[d] State economic control variables are based on the March Current Population Survey and include the variables log state personal income per capita, and female-to-male employment ratio. State social policy controls include max AFDC/TANF for a family of 3, unilateral divorce laws indicators (based on classification in Stevenson and Wolfers, 2006) and indicators for whether the state has the death penalty.

uncovered homicides. To more closely approximate the homicides of females, I also estimate these two specifications on a count of "other homicides" which have female victims. The results from these two regressions are reported in columns (3) and (4) of Table 5 and again, there appears to be no significant effect of these laws on homicide rates. Finally, I test the effect of arrest laws on intimate and familial homicides which are uncovered by arrest laws. These include homicides committed by boyfriends, girlfriends, homosexual partners, and non-nuclear family relatives. Because the SHR data is from police reports, the distinction between cohabiting or common-law married partners is somewhat blurred. While some states do treat cohabiting and common-law married partners differently, the question of whether mandatory arrest laws are enforced in cases of cohabiting intimate partner violence is unclear and to date there does not appear any systematic evidence to answer the question. In the case of non-cohabiting intimate partners, the law will only be enforced if these groups are specifically covered.[24] The results are reported in columns (5) and (6) of Table 5. These results suggest that there is no significant effect of these laws on uncovered homicides and the estimated effects are significantly smaller.[25]

5. Evidence of a "reporting" effect from mandatory arrest laws

As discussed above, there are two main mechanisms which could explain the increase in homicides after the enactment of mandatory arrest laws: the effect on victims (reporting) and the effect on abusers (reprisal). While it is likely that both effects are operating, the former is of particular concern from a policy design perspective. To the extent that the effect is driven by abuser reprisal, an enhanced criminal justice approach which would simply incapacitate abusers might prove an effective means of modifying mandatory arrest laws. However, if the laws deter victims from reporting, then an enhanced criminal justice approach

[24] Including boyfriends and girlfriends in the intimate partner counts for states in which these groups are not explicitly covered does not significantly change the results. They remain a relatively small fraction of all intimate partner homicides and while there does not seem to be a significant effect on this group, the results for common-law married, married, and formerly married couples are robust to their inclusion. Coverage varies by state and is available in the legal appendix.

[25] Fisher test for equality between mandatory and recommended arrest law coefficients is rejected at the 0.02 level. The comparison is between specifications reported in column (6) of Table 3 and column (6) of Table 5. For details on the test statistic and distribution see Fisher (1970).

Table 5
Falsification tests of difference-in-difference estimates of the effect of mandatory and recommended arrest laws

	(1)	(2)	(3)	(4)	(5)	(6)
	"Other homicides" per 100,000 inhabitants		"Other homicides" with female victims		Intimate partner homicides uncovered by arrest laws	
Dependant variable mean	10.37		1.41		0.73	
Mandatory arrest	4.2531	3.9985	0.5064	0.4631	0.2929	0.2656
Law effect	(2.7014)	(2.6749)	(0.4168)	(0.4036)	(0.2103)	(0.2071)
Recommended	0.5980	0.2595	−0.2492	−0.3104	0.1237	0.0911
Arrest law effect	(3.8689)	(3.8360)	(0.5675)	(0.5531)	(0.2128)	(0.2050)
		−0.1151		−0.0167		−0.0153
		(0.4438)		(0.0569)		(0.0233)
Controls for other crime rates [a]	N	Y	N	Y	N	Y
Controls for unemployment rate [b]	N	Y	N	Y	N	Y
State-year demographic variables [c]	N	Y	N	Y	N	Y
State-year economic and social controls [d]	N	Y	N	Y	N	Y
Linear trend	Y	Y	Y	Y	Y	Y
State fixed effects	Y	Y	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y	Y	Y
R-squared	0.6434	0.6454	0.6011	0.6050	0.6669	0.6710

Notes: All regressions include 994 observations. The dependant variable for each column is the column title per 100,000 inhabitants. Robust standard errors, clustered by state, are reported in parentheses. Coefficients that are significant at the .05 (.01, .1) percent level are marked with ** (***, *). "Other homicides" include homicides committed against employees, employers, other (non-immediate) family, friends, other known individuals, and strangers. Intimate partner homicides uncovered by law refers to relationships that are classified as intimate partner but were not specified in the state's arrest law statute. See Legal Appendix for detailed coverage by state. Mandatory arrest (MA) states are states which require an arrest conditional on a report of domestic violence. Recommended arrest (RA) states are states where officers are instructed but not required to make a warrentless arrest when an intimate partner offense is reported.

[a] Crime rate controls use FBI Uniform Crime reports for the number of crimes per 100,000 inhabitants. Indexed crimes included in the violent crime variable are murder, robbery, assault, and rape. Indexed crimes included in the nonviolent crime count are burglary, larceny, motor vehicle theft and drug crimes.

[b] Unemployment estimates are based on the March Current Population Survey.

[c] State demographic controls are based on the March Current Population Survey and include variables for the fraction of the population that is black, white, and other race, as well as age composition indicating share of prison population that is aged 14–19, 20–49, 50 or older.

[d] State economic control variables are based on the March Current Population Survey and include the variables log state personal income per capita, and female-to-male employment ratio. State social policy controls include max AFDC/TANF for a family of 3, unilateral divorce laws indicators (based on classification in Stevenson and Wolfers, 2006) and indicators for whether the state has the death penalty.

could exacerbate the problem. Moreover, the elasticity of victim's reporting response is of particular concern when designing any criminal justice response to report-driven crimes (such as interpersonal and, more specifically, intimate partner violence).

I begin to explore the reporting hypothesis by using the National Crime Victimization Survey at the Metropolitan Statistical Area (MSA) level. I test whether reporting and arresting patterns changed around the law change times. I also used the NCVS to look at trends in reasons to report or not report. An important caveat is that this version of the NCVS is not a nationally representative sample and contains only 40 urban areas. While a full discussion of the correlates of reporting behavior is beyond the scope of this paper, the NCVS provides some insight into how reporting varies by race, income, education level, and crime type. Higher income whites are less likely to report a domestic violence incident to the police than lower income whites. In contrast, low income African-American victims are less likely to report intimate partner violence to any source, while high income African-American victims are more likely to report to the police. This may explain the differential effect between different sub-groups in Table 4, as different sub-populations of the racial/ethnic groups adjust their reporting behavior differently. There does not seem to be a significantly different reporting rate for intimate partner violence in the presence of a gun or threat of weapon use while there is for non-intimate partner violence. Finally, the probability of having no injury is significantly lower among victims of intimate partner violence than victims of any other type of violence crime.

The results from a regression of the probability of reporting on a mandatory arrest law indicator variable indicates that reporting declined by 4.5 percentage points in mandatory arrest states (about 12%) and 2.8% in recommended arrest law states (about 7%).[26] Reporting to non-police increased dramatically, by 6 percentage points or 22% (though only marginally significant at the 0.1 level). A parallel analysis on arrest rates provides evidence that suggests that arrests increased, although the coefficients are too imprecisely estimated to provide any more definitive evidence.

To further test the plausibility of the reporting hypothesis, I considered the effect of mandatory arrest laws on homicides committed against members of the immediate family. This test relies on the assumption that there are differences in how intimate partner abuse and child abuse are reported to the police. Unlike for adults, children typically do not report their own physical abuse

[26] Specification includes fixed effects for year and MSA. MSA's classified as having mandatory arrest laws: Denver, Minneapolis, Norfolk, Portland, Washington DC. MSA's classified as having recommended arrest laws: Anaheim, Charlotte, Cincinnati, Cleveland, Columbus, Los Angeles, Nassau-Suffolk, New York, Newark, Oakland, Phoenix, Riverside, Sacramento, St. Louis, San Diego, San Francisco, and San Jose. Regression results not reported. Specification includes year and MSA fixed effects. Results become insignificant when the sample is reduced to acknowledged intimate partner violence, although coefficients remain similar. This may be in part due to loss of power from the more restricted sample and in part due to the NCVS's difficulty in getting respondents to admit violence was due to intimate partner abuse. For detailed description see Coker and Stasny (1996).

Table 6
Difference-in-difference estimates of familial homicide rates

	(1)	(2)	(3)	(4)	(5)	(6)
	Family homicides per 100,000 inhabitants		Family homicides: child of offender victims		Family homicides, school-age child of offender victims	
Dependant variable mean	0.88		0.42		0.33	
Mandatory arrest	−0.4765**	−0.4497**	−0.2431*	−0.2326*	−0.1717	−0.1578
Law effect	(0.2202)	(0.2156)	(0.1271)	(0.1267)	(0.1165)	(0.1159)
Recommended	−0.0344	−0.0003	−0.0334	−0.0236	−0.0326	−0.0201
Arrest law effect	(0.3400)	(0.3324)	(0.1688)	(0.1723)	(0.1346)	(0.1378)
Unemployment rate		−0.0128		−0.0141		−0.0125
		(0.0304)		(0.0153)		(0.0112)
Controls for other crime rates [a]	N	Y	N	Y	N	Y
Controls for unemployment rate [b]	N	Y	N	Y	N	Y
State-year demographic variables [c]	N	Y	N	Y	N	Y
Linear trend	Y	Y	Y	Y	Y	Y
State-year economic and social controls [d]	N	Y	N	Y	N	Y
State fixed effects	Y	Y	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y	Y	Y
R-squared	0.7098	0.7171				

Notes: All regressions include 994 observations. The dependant variable for each column is the column title per 100,000 inhabitants. Robust standard errors, clustered by state, are reported in parentheses. Coefficients that are significant at the .05 (.01, .1) percent level are marked with ** (***, *). "Family homicides" include homicides committed by mothers, fathers, daughters, sons, step-mother, step-father, step-daughter, step-son, and other family members. "Family homicides: child of offender victims" include only homicides committed by fathers, mothers, step-fathers, or step-mothers, against sons, daughters, step-sons or step-daughters. "Family homicides: school-age child of offender victims" includes homicides committed by fathers, mothers, step-fathers, or step-mothers, against sons, daughters, step-sons or step-daughters in which the victim is aged 6–18.

[a] Crime rate controls use FBI Uniform Crime reports for the number of crimes per 100,000 inhabitants. Indexed crimes included in the violent crime variable are murder, robbery, assault, and rape. Indexed crimes included in the nonviolent crime count are burglary, larceny, motor vehicle theft and drug crimes.

[b] Unemployment estimates are based on the March Current Population Survey.

[c] State demographic controls are based on the March Current Population Survey and include variables for the fraction of the population that is black, white, and other race, as well as age composition indicating share of prison population that is aged 14–19, 20–49, 50 or older.

[d] State economic control variables are based on the March Current Population Survey and include the variables log state personal income per capita, and female-to-male employment ratio. State social policy controls include max AFDC/TANF for a family of 3, unilateral divorce laws indicators (based on classification in Stevenson and Wolfers, 2006) and indicators for whether the state has the death penalty.

to police. Instead, abuse is usually detected by an outside adult.[27] While domestic violence is rarely reported by neighbors and other non-family members and most often by the victim or family members, child abuse is rarely reported by the non-abusing spouse and much more frequently reported by a concerned outsider such as a teacher or a doctor.[28] Finally, reporting of abuse can frequently result in loss of custodial rights which can reduce the physical contact between abuser and child—something which cannot be mandated in the case of adult abuse.

Because mandatory arrest laws require the arrest of an abuser in a domestic situation, familial abuse was also covered by these laws. Given this situation, mandatory arrest laws should also affect the probability of severe violence to children by family members. I therefore defined "family homicides" as homicides committed against a father, mother, step-father, step-mother, son, daughter, step-son, step-daughter, brother, or sister.[29] I constructed a count of these homicides by state by year and defined a count of family homicides per 100,000 inhabitants. Columns (1) and (2) of Table 6 report the results from a regression of family homicides per 100,000 inhabitants on an indicator for the mandatory arrest laws, an indicator for recommended arrest laws controlling for state and year fixed effects. Column (2) adds the full set of demographic, economic and social policy controls. The results indicate that family homicides decreased by about 0.4 per 100,000, corresponding to a 42% decline.[30] Because the reporting hypothesis suggests that arrest laws should more strongly affect the abuse of children rather than the abuse of any residential individual, I estimate two sets of regressions on a more restricted dependent variable. The first restriction I impose is that the victim was a child (either by blood or marriage) of the perpetrator. These results are reported in columns (3) and (4) of Table 6 and are quite similar in percent change terms in homicides rates to the unrestricted family homicides levels. The second restriction I impose is that the children are of school age (i.e. age 6–17). It is certainly possible that abuse of quite small or quite old children may rely more on the reporting by an individual within the household and thus be subject to the same transference of costs as victims of intimate partner violence. In contrast, school-age children are likely to see teachers, doctors, and nurses on a regular basis. As such, heightened

[27] More specifically, of the nearly 2.8 million child abuse cases reported to child protective services agencies in 2000, 56.1% of all reports were from law enforcement, educators, medical and mental health professionals, social services personnel, child care providers and other mandated reporters. U.S. Department of Health and Human Services, Administration on Children, Youth and Families, Child Maltreatment 2000 (Washington, DC: U.S. Government Printing Office, 2002).

[28] Actually, many professionals have legal requirements to report suspected abuse which can compensate for any potential costs they might incur from reporting abusers in their community.

[29] For specific coverage by state law, see legal appendix.

[30] This substantial decline in familial homicides has been the subject of much discussion. See Durose et al. (2005).

abuse of these children is most likely to generate an increased likelihood of third party reporting. The results reported in columns (5) and (6) of Table 6 suggest a reduction in homicides of these children. Although the results are insignificant, the coefficient estimates are consistent with the larger sample estimates. These results are consistent with the model suggesting that once arrest laws do not rely on reporting by the abused, these laws appear to function as predicted, reducing harm to the protected individuals.

6. Conclusions

Many states use laws requiring the warrentless arrest of individuals believed to be responsible for intimate partner abuse as a major policy tool in their effort to end domestic violence. The results presented in this study suggest that this may in fact be counterproductive. Using data from the FBI Supplementary Homicide Reports from 1976–2003, I find that the level of intimate partner homicide increased in states with these mandatory arrest laws. I propose two potential mechanisms through which this may operate: the first is through increased reprisals by abusers and the second is through reduced reporting by victims. This latter mechanism operates because the failure to contact the police may result in fewer interventions risking an increased probability of escalating violence. To support this interpretation, I present evidence from the National Crime Victimization Survey on reporting behavior as well as evidence on the effect of these laws on different types of homicides. I estimated the change in familial homicides in response to mandatory arrest laws. These crimes are covered by arrest laws but the reporting of abuse typically is not performed by the victim. Thus the reporting effect should be reduced or eliminated. A difference-in-difference analysis reveals that familial homicides declined in response to mandatory arrest laws. In contrast, there was no significant effect of arrest laws on uncovered homicides. This suggests that mandatory arrest laws may deter reporting nullifying the potential deterrence intended by the required arrests.

The results presented in this paper should be interpreted with several caveats. First, while there has been a rise in intimate partner homicides, there could be a decline in the total number of incidents of domestic violence. As such, the effect of mandatory arrest laws on misdemeanor level violence remains an open question. Second, this paper addresses the empirical justification for these laws. To the extent that support for these laws is based on philosophical support for arresting abusers, the results of this paper do not speak to the justification for these laws.

The analysis in this study leaves open several issues. First, while intimate partner homicides may have increased, it is not certain that this corresponds to increased levels of intimate partner abuse. If intimate partner homicides and intimate partner abuse are negatively correlated, then arrest laws may decrease abuse while increasing homicides. The effect of mandatory arrest laws on less severe abuse therefore remains an open question. Second, the reasons why mandatory arrest laws produce perverse outcomes for victims of domestic violence are also uncertain. If abusers penalize victims with harsher abuse after arrests, then arrests are an insufficient response to domestic violence. In this scenario, stronger sentences and aggressive prosecution policies, which will incapacitate abusers, are necessary to ensure the safety of victims. On the other hand, if mandatory arrest laws fail because of the psychological component of abuse that is based on the emotional bonds between the abuser and the victim making victims unwilling to inflict harsh penalties on their abusers then an alternative approach which does not depend on victims reporting is needed. If the problem is a misapplication of the law (for example, through dual arrests) then preceding the enforcement of arrest laws, comprehensive police training is required. Finally, it is well known in the sociological and psychological literature that arrests are not sufficient to induce victims to leave their abusers.[31] If the objective of arrest laws is to promote a decline in the prevalence of intimate partner violence then policy efforts focused on providing victims the opportunities and resources to leave abusive situations are also required.

The irony that a mandatory arrest law intended to deter abuse actually increases intimate partner homicides is not lost on this author. Given the dangerous and pervasive nature of domestic violence, there is little doubt that state intervention, in some form, is required. Determining what shape that intervention takes is of vital importance. The results from this study suggest that mandating arrest is insufficient to deter abusers from killing their victims. Finding that arrests deter victim reporting rather than perpetrator abuse provides valuable insight into the intricacies facing governmental attempts to decrease intimate partner violence. While it appears that mandatory arrest laws are not sufficient to deter abuse, the set of policies that can effectively prevent abuse and protect victims remains an issue for future research.

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Corrigendum

Corrigendum to "Does the certainty of arrest reduce domestic violence? Evidence from mandatory and recommended arrest laws" [JPubEc 93(1-2), pp. 85-89]

Radha Iyengar

Domestic Violence remains a significant policy issue with policies to address it spanning social, economic, health, and criminal justice policies. In addressing these issues, an important use to deter future violence is imposing penalties on abusers, beginning with the arrest of these individuals when police are called to the scene of an incident. Such arrests were not common until states began passing warrantless arrest laws – laws that required or encouraged police to arrest individuals from a domestic violence incident. As discussed in Iyengar (2009), the theory of how increased arrests reduced future violence was largely premised on a theory of deterrence for abusers. That is, as arrests increased penalties for abuse, these abusers were less likely to commit violence in the future. However, there is also a risk in the context of intimate partner violence settings, where victims and abusers have greater emotional and financial ties, that such arrests also impose a cost on victims (for example, see victim interview discussions in Leisenring 2012). In a setting where the victim also faces an increased cost from the increased penalty, then the overall effect of arrest laws on abuse is theoretically ambiguous. This theoretic insight motivated the tests for the Iyengar (2009) paper.

Chin and Cunningham (2019) focus on the classification of laws (as had several previous papers, see for example Zeoli et al., 2011; Zelcer, 2014), providing important contributions in both how best to understand and classify statutes in a more robust manner. They also explore and attempt to replicate the original findings in Iyengar, 2009 to understand the role that the paper's statutory classification and estimation approach impacted the underlying findings. Unfortunately, the data and coding used for the final analysis of Iyengar, 2009 was over a decade old, stored on a combination of external hard drives and old computers, and in many cases no longer accessible. Given the time delay between the original publication and request for replication data, it was simply not possible (despite considerable effort) to provide a complete dataset, final coding files, or complete information needed for a fulsome replication effort by Chin and Cunningham. Thus, while both the effort to supply information and the effort to conduct the replication were conducted in good faith and to the best of the abilities of all involved, the results should be interpreted as suggestive of potential issues rather than dispositive of the findings.

Nevertheless, the replication results granted me sufficient p over the original findings to write an erratum and suggest explicit tion should be taken when interpreting the original findings fron 2009 paper. Alongside this caution, I view much of the eviden Chin and Cunningham as consistent with the initial theoretic and pirical insights of the original paper – namely that warrantless a laws that lack discretion for policy impose costs and both victims abusers. These costs can mediate the deterrent effect of the law abusers potentially by impacting reporting by victims. These cost be reduced by allowing policy discretion to respond to victim de not to arrest abusers in some cases. This short note summarizes s of the key findings and issues discussed in Chin and Cunningha well as what I view as the implications based on these analyses and lication attempts.

1. Summary of Findings and Issues

Chin and Cunningham (2019) set out to explore several primary ferences with Iyengar (2009). First, they seek to build on a growing literature that seeks a more refined classification to warrantless a laws. This builds on work in the legal community (Hirschel e 2007; Zeoli et al., 2011), Chin and Cunningham test a classifica scheme to determine the extent to which a more nuanced classifica might account for differences in estimated effects. Second, Chin Cunningham also narrow the scope of arrest implications, focusin current and former spouses rather than the broader set of intimate ners. This allows consistency across different measures (e.g. sup mentary homicides, crime reports) as well as different studies limits the direct comparison to Iyengar, 2009. Third, Chin and Cunn ham attempt to replicate Iyengar (2009) using the data and orig Stata code and log files I shared. I will focus my initial discussio the differences generated in this third area, but in interpreting the ings in the context of the failed replication will reference the find from Chin and Cunningham in the first two, as these speak to the in cations of the results in context.

The primary replication results from Chin and Cunningham are sented in Online Appendix Table II.3 of their paper. Panel A repli the original findings with my original data and Panel B applies my derlying code to new data to try to reconstruct the data from raw As noted above, while the table presents some findings from my in coding, it does not represent a full replication of the findings bec

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both the final code and data presented were simply not available for Chin and Cunningham to use. Nonetheless, the coding they included appears to be a reasonable interpretation of the research. Their findings indicate that there is no significant effect of mandatory arrest laws on intimate partner homicides and that there is a small but not robust effect of recommended arrest laws on intimate partner homicides. These findings are in contrast to Iyengar, 2009 which finds mandatory arrest laws increase intimate partner homicides. Both studies find a small, but not robust effect on recommended arrest laws (though breaking these out into preferred and discretionary arrest changes this finding and I discuss the implication of this coding difference below). Chin and Cunningham, in a very careful review of my code, suggest this difference may be generated by a benign ambiguity in merge syntax. While I cannot confirm or refute this because the final code is not available, this seems like a plausible result and one that I find credible given the careful and collaborative approach taken by Chin and Cunningham in understanding and approaching the replication of the original findings.

The findings of Chin and Cunningham certainly indicate that the significant effect of mandatory arrest laws on intimate partner homicides is not robust to specification. While as they note it is present in many of the findings it is both smaller in magnitude and insignificant in all specifications. As an additional note, the negative effect of recommended/discretionary arrest laws on intimate partner violence is in fact consistent with findings from Iyengar (2009) and this is relevant for interpretation of these findings.

2. Implications

Based on the thorough work in Chin and Cunningham, I think there are two important implications. One relates to the specific findings on mandatory arrest laws and the other to the broader interpretations of policies aimed at reducing intimate partner violence.

In terms of interpreting the results of Iyengar, 2009 and more broadly how warrantless arrests relate to mandatory arrests, I think it is worthwhile to return to the important distinction between failing to identify a statistically significant positive result and the continued lack of negative (predicted) effect from mandatory arrest laws. This has several implications for interpreting the impact of warrantless arrests. First, absent other channels of impact, arrests should reduce violence. This was the finding of the original set of domestic violence experiments (Sherman and Berk, 1984). Chin and Cunningham (2019) do not find a consistent deterrent effect of mandatory arrests.

Further support of the underlying trade-off between victim and offender costs is that both Iyengar (2009) and China and Cunningham (2019) find more predictable deterrent effects from preferred and discretionary arrest laws. An important insight from Iyengar (2009) is that while arrest policies were intended to deter future violence, they relied on victims reporting the underlying offenses. The original paper finds some negative impacts from 'recommended arrest' laws. These effects appear stronger when coding is adjusted to differentiate between discretionary and even preferred arrest. These laws allow greater balancing of victim needs and preferences with offender deterrence. Victims who might find arrest particularly costly have scope to prevent this outcome and thus you would anticipate that this could counterbalance the reporting effect, creating a more dominant deterrence approach.

In short, while the point estimate findings from Iyengar (2009) appear most likely due to a syntax error in coding or at best, extremely sensitive to definitions and covariates not available for replication, the underlying intuition is that arrest policies which rely on reporting to trade-off victim and offender costs do achieve effective reduction in intimate partner violence. This core insight is one worth continuing to apply in the context of other warrantless arrest policies on how various forms of discretion (as well as additional penalties to abusers such as mandating prosecution, or additional support for victims which m lower costs) may improve the efficacy of these laws.

These discussions feed a long-standing policy debate and discussi on efficacy and mechanisms to reduce violence against women (see f instance Dugan, 2001 or Niolon, 2017). A critical part of these debates both identifying potential policy mechanisms and sizing these effects. this context, research evaluations and replications of these findings ar then translation of the findings for policymakers is a critical area of in plications for any published paper. It is in this context that I would li to highlight the value of replication efforts like the one from Chin an Cunningham, the collaborative approach taken by the researchers, an the respectful forum provided by Journal of Public Economics. To often, replication efforts are treated as personal attacks on the origin researcher or undertaken to disprove initial findings. Such an effo does discredit to the policy analysis required to truly improve outcom When Cunningham and Chin initially reached out to replicate my fin ings, they were transparent about their concerns but also understandi that the long time (and many moves and computers) between the in tial analysis (which began as my third-year graduate student pape and this replication posed serious limitations. At the same time, I view their effort as a good faith desire to understand the impact of a compl issue with controversial policies. The ultimate findings that there w likely a coding error was then treated generously in their paper and r spectfully by the editors of the Journal of Public Economics.

In this context, the replication effort served to advance the scienti understanding of the original question at hand, which was ultimate the objective of the 2009 paper itself. I highlight these findings becau too often in discussing the implication of replications the focus is on t individual analysis of papers themselves and not the scientific process seeking imperfect answers to important questions. I believe a key imp cation of the analysis in Chin and Cunningham is that such work serve critical probative value that in and of itself is a valuable contribution the field.

3. Conclusion

Overall, the analysis from Chin and Cunningham appears sound an provides the plausible explanation that increase in homicide rates d to mandatory arrest laws found in Iyengar (2009) may have been t result of a syntax error in coding rather than a true causal effect. How ever, their subsequent findings and coding are consistent with the th ory and other evidence presented in Iyengar (2009) that arrest polici related to intimate partner violence may not have the strong deterre effect anticipated while other arrest policies, most notably those th preserve discretion, do have a negative effect. These outcomes cou be generated by the more complex relationship between victim and o fender in intimate partner relationships in which arrest policies impo costs on victims as well. Such an insight is critical for considering ho different policy options to penalize abusers can also support victi and it is valuable to have new and additional evidence regarding discr tionary arrest policies to continue to improve policing policy aimed reducing intimate partner violence.

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Further reading

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