Targeted Interventions for High-Risk Domestic Violence Victims*

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Domestic violence accounts for 50% of female homicides in the U.S. The criminal justice system – with which the majority of victims initiate contact in the years leading up to their deaths – may be uniquely suited to prevent these tragedies. There remains considerable debate, however, on whether victims at high risk are *identifiable*, and whether criminal justice system responses targeted towards them can be *effective*. We study this approach in Chicago, where victims gauged to be at highest risk are selected for additional outreach, prosecutorial, and advocacy resources to increase the likelihood of successful criminal prosecution. Leveraging variation in prosecutors' tendencies to classify cases as high risk, we show that this approach rapidly and persistently lowers the likelihood of homicide for victims on the margin of inclusion. Additionally, prosecutors are proficient at identifying high-risk victims, considerably outperforming standard machine learning algorithms.

Keywords: Domestic violence, recidivism, prosecutors, incarceration

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1 Introduction

How should the criminal justice system respond to domestic violence (DV)? Violent incidents committed by intimate partners, immediate family members, and other relatives account for over a fifth of all violent crime in the U.S. (Truman & Morgan, 2014). However, rates of victim disengagement in DV cases can be as high as 90%, which can limit the effectiveness of traditional responses like criminal prosecution.[1]

Much of the public debate focuses on preventing the most severe form of DV: domestic homicide. Domestic homicide accounts for 50% of female homicides and 20% of all homicides in the U.S. (CDC, 2018). Following high-profile domestic homicides, it is not unusual for state legislatures to pass laws named after the victim that would have potentially prevented the death in question; examples include Marsy's Law (California, 2008), Diane's Law (Illinois, 2014), Laura's Law (Arkansas, 2015), Jennifer's Law (Connecticut, 2021), and Aisha's Law (Ohio, 2021).[2] Whether these tragedies are preventable via a criminal justice system response, however, depends in part on the degree to which these responses are *effective*, and in part on the degree to which victims at high risk can be *identified* in advance of these crimes.[3]

[1]Prior research has documented victim disengagement and case dismissal rates in domestic cases that range from 25 to 90%, even for cases initiated by victims (Field & Field 1973, Parnas 1973, Ford 1983, Ford & Regoli 1992, Fisher 2004), driven by the fact that participation in the legal process can threaten victims' relationships with their intimate partners, the safety of themselves and their children, and the stability of shared responsibilities such as income generation, childcare, and housing (Strube 1988, Dunford *et al.* 1990, Sagot 2000, WHO 2002).

[2]These laws typically increase legal protections for victims and their families or enhance the surveillance of defendants.

[3]A related concern is that some homicide victims may not engage with the criminal justice system at all prior to their deaths, which would limit the ability of the criminal justice system to reduce domestic homicide rates. Prior research shows, however, that the share of domestic homicide victims with prior law enforcement contact is fairly high, ranging from 50% to 90% depending on the jurisdiction (McFarlane *et al.* 2001; Koppa & Messing 2019). Our own calculations using Chicago Police victimization records between 2009-18 indicate that 53% of female intimate partner homicide victims had prior victimization histories that were reported to the police.

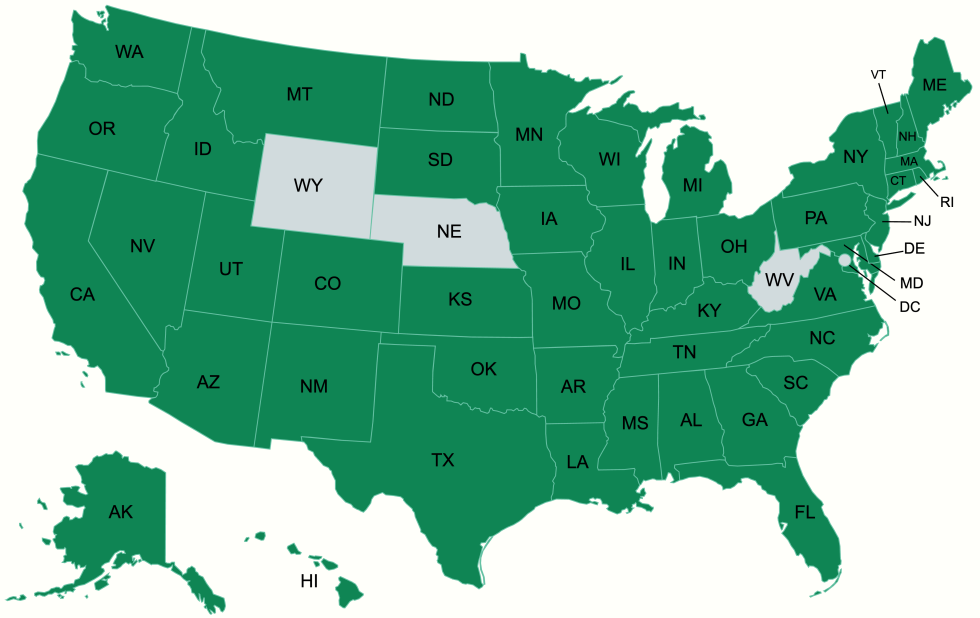
A priori, it is not obvious whether interventions led by the criminal justice system can meaningfully enhance victim safety. Rates of victim disengagement and case dismissal in DV cases are very high – even for court cases originally initiated by victims. Anticipating this disengagement, states in the 1980s and 1990s enacted several laws to minimize the role of victim autonomy and circumvent their non-cooperation, mandating arrest and prosecution in *all* DV cases; research shows, however, that these policies are not effective at reducing homicide risk for victims (Aizer & Bo 2009, Chin & Cunningham 2019).[4] At the same time, research shows that specialized police units and courtrooms that focus exclusively on DV cases can increase cooperation with police officers and prosecutors, and reduce the incidence of non-fatal DV (Jolin *et al.* 1998; Golestani *et al.* 2021).

Prior research is encouraging in that it shows that it is possible to increase victim engagement with the criminal justice system, but is silent on whether this approach can deter domestic homicide. This gap is not for lack of policy experimentation – federal funding under the 1994 Violence Against Women Act (VAWA) has made targeted interventions for high-risk DV victims ubiquitous across the U.S. (see Figure 1 and Table A2).[5] These interventions encourage collaboration across local law enforcement, prosecutors, courtrooms, victim advocates, and service providers, and are aimed at addressing barriers that high-risk DV survivors may face while pursuing a legal response against their current or former domestic partners. These efforts typically include an

[4]Policies like restricting firearm access for individuals with DV histories, increasing the number of female police officers, and reducing the threat of deportation have been shown to reduce the incidence of domestic homicide (Raissian 2016; Miller & Segal 2019; Amuedo-Dorantes & Deza 2022). However, these policies are not informative about how to increase victim safety following the filing of a criminal complaint in a high-risk case.

[5]Some of these interventions have been studied but the research designs are not rigorous – e.g., they do not include contemporaneous comparison groups. As such, we do not discuss the findings of those studies here.

Figure 1: States with Targeted Interventions for High-Risk Domestic Violence Cases



Notes: This map highlights states with interventions that are (1) run by the criminal justice system in collaboration with agencies that provide services such as advocacy, counseling, and civil legal representation; (2) targeted towards high-risk DV cases. For more details on each state's interventions, see Table A2.

assessment of victim risk, and the channelling of additional resources towards those identified as high-risk;[6] these resources usually help the victim develop a safety plan, navigate the criminal justice system, and access appropriate victim services. To the best of our knowledge, only two evaluations of interventions for high-risk DV victims that use contemporaneous comparison groups exist, and both evaluate the impact of connecting high-risk victims with service providers. Koppa (2018) uses a difference-in-differences strategy to show that Maryland's Lethality Assessment Program (LAP) – under which officers determine lethality risk based on victim responses to a questionnaire-based as-

[6]Commonly used assessments include the Danger Assessment, the Spousal Assault Risk Assessment, the Ontario Domestic Assault Risk Assessment, and the Domestic Violence Screening Instrument These assessments are typically short checklists or questionnaires filled in by responding officers and/or services providers, and tend to capture risk factors such as the defendant's criminal history and the victim's barriers to support.

sessment, and provide the 60% of victims flagged as high-risk with a personalized safety plan and connection to a local DV hotline – reduced the county-level incidence of female homicides committed by males by 37-44%. Black *et al.* (2022) use inverse propensity weighting to study a similar intervention in England, where the 9% of cases flagged as high-risk by police officers are connected with services such as safety planning and housing support, but find no discernible effect on violent recidivism.[7]

While encouraging, these results are silent on at least two dimensions. First, what is the effect of an enhanced criminal justice system response *in addition to* connections with service providers for victims at elevated risk? Second, can homicide risk be assessed accurately enough to intervene in advance of these crimes? In this paper, we study both questions.

Using a rich panel dataset on all DV arrests between 2001 and 2013 in Chicago, we show that an enhanced criminal justice response targeted towards a small share (i.e., 6%) of victims gauged to be at highest risk for for future harm can improve victim safety. Run by the Domestic Violence and Sexual Assault Division within the Cook County State's Attorney's Office, the *Target Abuser Call* program aims to enhance victim cooperation in high-risk DV misdemeanor and class IV felony cases by channelling additional resources towards victims to ease their participation constraints and increase their likelihood of pursuing a legal response.[8] Descriptive research shows that victims included in the program are much more likely to receive outreach services, en-

[7]The authors do not estimate the impact on homicide. It is not straightforward to extrapolate the findings across these two outcomes – homicide is an outcome that is very likely to be reported, while the same is not true for violent recidivism. For instance, the 2021 National Crime Victimization Survey showed that only 46% of violent victimizations were reported to law enforcement in the U.S.

[8]Misdemeanor convictions carry sentences of up to 1 year, which are usually served in the county jail instead of prison. Class IV, III, II, I, and X felony convictions carry prison sentences of 1-3, 2-5, 3-7, 4-15, and 6-30 years respectively. Misdemeanor and class IV offenses account for 98% of DV arrests.

gage with prosecutors and advocates, and attend court hearings; they are less likely to drop charges; their cases are more likely to result in a conviction and jail time, and they are less likely to be living with the defendant after six months (Frohmann & Hartley, 2003).[9] While supportive of the direct intent of the program – increased victim cooperation and conviction rates – these associations are silent on whether victim safety is enhanced, a gap that this paper fills.

To estimate the causal impact of this approach on victim safety, we utilize quasi-experimental variation in the assignment of DV cases to prosecutors in Chicago, and track victim outcomes for nine years following the initial DV arrest.[10] These prosecutors differ systematically in the rate at which they select cases for the intervention, which we use as an instrument for whether a given case is selected. To credibly evaluate the impact on victim safety, we do not rely on police data, which reflects the recurrence of violence as well as the decision to contact law enforcement, and makes it difficult to interpret results.[11] Instead, we use individual-level death records, which are collected and maintained by the Cook County Medical Examiner's Office.[12] We find that the targeted intervention rapidly and persistently reduces the risk of re-victimization by homicide by 4 percentage points, or 82% of the complier mean. Most of this reduction

[9]Frohmann & Hartley (2003) compare 103 cases that were included in the targeted intervention with 219 cases that were not between December 2000 and August 2001; the victim appearance rate was 40 per cent for the former group and 73 per cent for the latter (see Figure 6 for more of their findings). Similar patterns have been documented in other settings – see https://www.ojp.gov/pdffiles1/nij/grants/222912.pdf

[10]This time period is the longest follow-up period permitted by the data. We discuss this in more detail in Section 3.

[11]This is especially likely to be true if the intervention – like the one we study here – is intended to increase victim cooperation with law enforcement. Analysis of the National Crime Victimization Surveys administered by the Bureau of Justice Statistics between 2006-10 found that 47% of serious intimate partner violence incidents were not reported to the police; in at least 17% of unreported cases, the most important reason for not reporting was the belief that police could not or would not help (Langton *et al.*, 2012).

[12]Our primary outcome is victim death by homicide, but we also use this data to conduct falsification tests using outcomes like death due to natural causes like cardiovascular disease.

is apparent within the first few months after a victim's induction into the program, and persists for nine years after the initial incident date (i.e., the end of our follow-up period). Conversations with the program providers (prosecutors and victim advocates) indicate that enhanced outreach by staff members from both organizations may explain at least part of the increase in engagement and decrease in homicide risk; this interpretation is supported by Frohmann & Hartley (2003), whose descriptive research shows that individuals included in the program are much more likely to have spoken with prosecutors, investigators and victim witness specialists hired by the prosecutor's office, advocates, and civil attorneys both before and during their court appearances.[13]

As our estimates are identified off of an instrumental variable, they speak directly to the expected benefits for cases on the margin of inclusion in the intervention, i.e., those that would be served if jurisdictions were to expand access to these kinds of programs. In Section 4.3, we use this insight to show that the expected benefits of expanding slots in this program in Chicago far exceed the expected costs. As a result, we argue that jurisdictions intent on reducing the prevalance of serious DV victimization should consider implementing or expanding targeted interventions of this kind.[14]

To answer the second research question – whether criminal justice system actors can accurately assess homicide risk for DV victims – we combine estimates of our complier mean (i.e., homicide risk for victims at the margin of inclusion in the program) with machine learning techniques. In our setting, prosecutors do not rely on a validated risk

[13]See Figure 6 for more of the study's descriptive findings.

[14]This paper also contributes to the empirical literature on prosecutorial discretion and decision-making (Glaeser & Piehl 2000, Bjerk 2005, Boylan 2005, Shermer & Johnson 2010, Fischman & Schanzenbach 2012, Rehavi & Starr 2014, Pfaff 2017, Nyhan & Rehavi 2017, Silveira 2017, Arora 2018, Krumholz 2019, Didwania 2020, Harrington & Shafer 2020, Jordan 2020, Ouss & Stevenson 2020, Sloan 2020, Tuttle 2021). This area of research is burgeoning due to growing policymaker interest in the outsized role that prosecutors play in the criminal justice system; in this paper, we shift focus away from the direct impact that prosecutors have on case outcomes, and instead examine the beneficial role that they may be able to play in enhancing victim safety.

assessment in their decision-making process, and we explore whether machine learning can improve upon their risk predictions.[15] To do this, we train a standard machine learning algorithm – gradient-boosted decision trees – on victim and defendant characteristics, and show that while it is predictive of homicide risk, it is not able to predict risk as well as prosecutors do.[16] To ensure that the outcome is not contaminated by the direct – and as shown above, beneficial – impact of the program, the algorithm is trained on all cases *except* those that are selected for the targeted program. We then compare the risk of homicide for the riskiest cases identified by the algorithm with the risk of homicide for those at the margin of inclusion into the program – i.e., the complier mean (0.0444). As the former set of cases face, on average, considerably *less* risk than the latter set of cases, we conclude that prosecutors are proficient at identifying high-risk victims and that machine learning algorithms are unlikely to improve beneficiary selection in our setting.

The rest of this paper is organized into five sections. Section 2 provides information on the institutional setting, and outlines the research design. Section 3 describes our data sources. Section 4 presents our estimates of the impact of the targeted approach on victim homicide and discusses mechanisms that may be driving the results. Section 5 shows that standard machine learning tools are unlikely to improve the identification of high-risk victims, indicating that prosecutors are proficient at predicting homicide risk. Section 6 concludes.

[15] In order for an improvement in risk predictions to automatically mean an improvement in program impact, individuals at highest risk of homicide would also need to be the most responsive to the intervention we study.

[16] In our setting, prosecutors can access both qualitative (e.g., case narratives, in-person exchanges with the victim on current and prior cases, etc) and quantitative (e.g., decades-long arrest and conviction histories for the defendant) information that is not easily observable by researchers and data engineers.

2 Research Design

This section begins by describing the targeted intervention for high-risk DV cases that has been run by the Cook County State's Attorney's Office since 1997. Next, we describe how DV case files are assigned to specialized prosecutors for screening, at which point they select a small fraction – 6% on average – of cases to include in the program. The empirical strategy uses the quasi-experimental assignment of cases to these prosecutors, and variation in the rate at which they include cases in the program, to estimate its effects on future victimization.

2.1 Cook County's Targeted Program for High Risk DV Cases

Since 1997, the Cook County State's Attorney's Office (SAO) has run a specialized program – Target Abuser Call (TAC) – to enhance victim cooperation in DV misdemeanor and class IV felony cases that are deemed to be at high risk for future victimization.[17] TAC prosecutors do not use a formal risk assessment tool, but prioritize cases that involve long DV histories, injuries, weapons, and serious threats against the victim and/or their family members. Once selected, TAC cases are offered multiple supports to increase the likelihood of successful criminal prosecution:

- **Specialized Prosecutors.** Two Assistant State's Attorneys (prosecutors) focus exclusively on TAC cases, and handle each case from onset to final disposition. TAC prosecutor caseloads are significantly lower than other prosecutors in the DV division. As a result, significantly more prosecutorial capacity is allocated

[17] Misdemeanor convictions carry sentences of up to 1 year, which are usually served in the county jail instead of prison. Class IV, III, II, I, and X felony convictions carry prison sentences of 1-3, 2-5, 3-7, 4-15, and 6-30 years respectively. Misdemeanor and class IV offenses account for 98% of DV arrests in our sample.

towards each TAC case by the SAO.[18]

- **TAC Investigators.** Four SAO investigators focus exclusively on following up with TAC victims as soon as their case is initiated; they serve subpoenas in person, distribute information on court dates and processes, assess victim safety, and ease apprehension about court appearances. Because these investigators focus exclusively on TAC cases, they are able to dedicate significantly more time and energy per case than other investigators in the DV division.

- **TAC Victim Witness Specialists.** Two victim witness specialists work directly with victims to answer questions about the legal process, and provide support around court processes and appearances. Like prosecutors and investigators in the TAC program, they are able to allocate more resources per case than those who work on non-TAC cases.

- **(Non-SAO) Advocates and Civil Attorneys.** Advocates provide counseling, attend court with victims, and make referrals to services; civil attorneys provide representation in civil legal proceedings (e.g., child custody cases). While these services may be accessed by individuals whose cases are not included in TAC, a case's inclusion in TAC guarantees a connection with these service providers.

These supports translate into meaningful, albeit descriptive, differences between TAC and non-TAC cases, as documented by Frohmann & Hartley (2003), and summarized in Figure 6. TAC victims are far more likely to be handed subpoenas in person instead of receiving them by mail, and report engaging with prosecutors, investigators, victim witness specialists, and advocates at higher rates both before and after court.

[18] In 2007, fifteen prosecutors in the DV division handled non-TAC cases – i.e. 94% of all cases, while the two TAC prosecutors handled the 6% selected for TAC (Landis, 2007).

2.2 Assignment of Cases to Specialized Prosecutors

TAC prosecutors screen DV cases on a daily basis to select, on average, 6% of cases for inclusion in the program, prioritizing those that involve a history of abuse, or the incidence or threat of serious injuries, weapons, or familial harm. Cases are assigned for screening based on incident location – one prosecutor screens cases corresponding to arrests made in 12 of Chicago's Police Districts, and the other screens cases corresponding to the remaining 10 Districts; Figure 2 displays the geographic allocation of districts to the first prosecutor in green, and the second in grey.

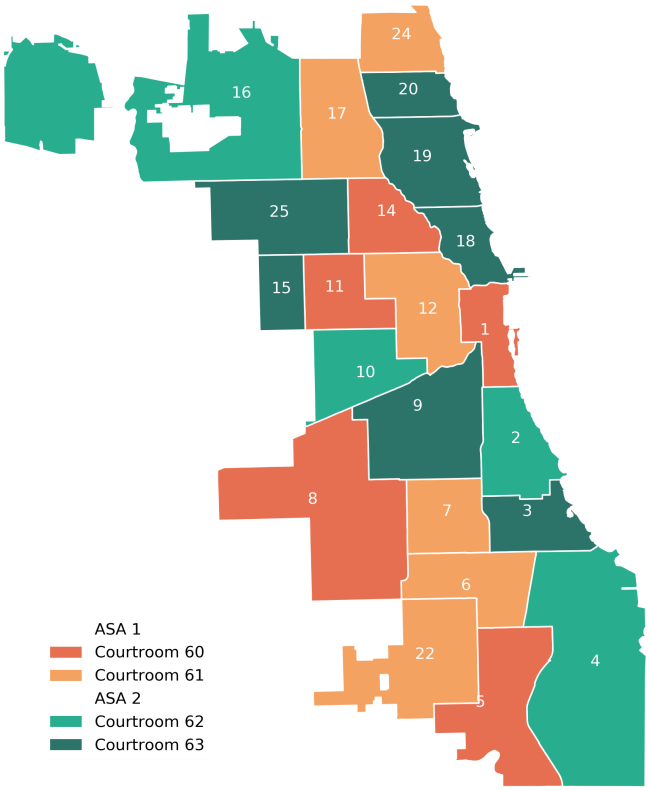
Prosecutors typically serve as part of the specialized program for about a year, before being rotated out onto another division within the Cook County State's Attorney's Office (SAO). Data on 34 distinct prosecutor stints between 1999-2013 indicates that the average length of service on the program is 11 months, with 68% serving on the program for an average of 14 months. Further, these rotations tend to *not* be prosecutor-specific, and happen in pairs – 65% of prosecutor stints began within a month of each other, despite serving separate geographies within Chicago.

2.3 Prosecutors' Inclusion Rates as an Instrumental Variable

The rotation of prosecutors in and out of the targeted program creates variation in which prosecutor makes inclusion decisions on each case, even when we look at cases occurring within the same district within the same calendar year.[19] Further, some prosecutors are much more likely to select cases for the targeted program than others, with the selection rate varying from 4% on the lower end to 10% on the higher end within our sample.

[19]We further restrict attention to cases occurring within 150 days of a prosecutor switch. In Figure 5, we show that our results are only strengthened when we restrict attention to incidents that occur within 30, 60, 90, or 120 days of each prosecutor switch; these samples are by definition smaller, but arguably expected to be more balanced along unobservable dimensions.

Figure 2: Jurisdictional Split Across Specialized Prosecutors



Notes: This map reflects how DV arrest cases in Chicago are assigned for screening to the two specialized prosecutors at the Cook County State's Attorney's Office. The numbers identify Chicago Police Districts; arrests made in the twelve districts shaded in green are screened by one prosecutor (Assistant State's Attorney / ASA 1), while the second prosecutor screens arrests made in the remaining ten districts (ASA 2). Data Source: Cook County State's Attorney's Office.

Combined with the conditional (on incident district and year) random assignment of cases to prosecutors, this rotation gives rise to exogenous variation in the probability that a case is included in the targeted program.

Our empirical specification uses the (leave-one-out-mean) rate at which a prosecutor selects cases as an instrument for whether a given case is included in the program. Our

empirical specification can be described by:

$$P_{i,0} = \gamma Z_{j(i)} + \alpha X_i + \varepsilon_{i,0} \quad (1)$$

$$Y_{i,t} = \beta_t P_{i,0} + \gamma X_i + \epsilon_{i,t} \quad (2)$$

We normalize the data so that period zero reflects the time period in which incident i took place. $P_{i,0}$ is an indicator variable equal to 1 if incident i is included in the targeted program in period 0, $Z_{j(i)}$ is the mean rate at which the prosecutor j assigned to incident i includes cases other than i in the program, X_i is a vector of control variables including victim characteristics and district-year fixed effects, and $Y_{i,t}$ is the dependent variable of interest measured at point t after the initial incident date. β_t reflects the impact of the targeted program on cases that were included because they were assigned to a more inclusive prosecutor. Standard errors are two-way clustered at the individual and district-year level.

2.4 Testing the Instrument

Before putting our instrument to use, we present evidence that it is a relevant predictor of case inclusion rates, is uncorrelated with victim and arrestee characteristics, and satisfies tests of the monotonicity condition.

Relevance. Table 1 displays first stage estimates, which indicate that the instrument is a relevant predictor of whether a given case is included in the targeted program. Column (1) shows that the prosecutor's case inclusion rate is a positive, statistically significant predictor of whether a given case is included; column (2) shows that the inclusion of district-year month fixed effects reduces the magnitude of the point estimate, but that

Table 1: First Stage Estimates: Full Sample

	(1)	(2)
	Dependent Variable: Pr(Targeted Program)	
Prosecutor's Inclusion Rate	1.0672***	0.9422***
	(0.0972)	(0.1387)
Observations	96,575	96,575
District-Year F.E.		Yes
F-Statistic (Instrument)	120.4609	46.1744

Notes: Sample consists of 96,575 arrests between 2001-13 flagged as domestic incidents by the Chicago Police Department that occur within 150 days of a prosecutor switch. Standard errors are two-way clustered at the individual (victim) and district-year level. Data Source: Chicago Police Department, Cook County State's Attorney's Office. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

it remains statistically significant at the 1 per cent level, and economically meaningful – the estimates indicate that being assigned to a prosecutor with a 1 percentage point higher inclusion rate increases the probability of being included in the targeted program by 0.94 percentage points.

Figure 3 depicts the distribution of the instrument after partialling out district-year fixed effects. Overlaid is a local linear regression of whether a case is selected for the targeted program on the instrument. The relationship between the instrument and the targeted program is positive and monotonically increasing in a prosecutor's inclusion rate.

Conditional Independence. For our instrument to be valid, the case inclusion rate of a prosecutor must be uncorrelated with case characteristics that could affect future outcomes, once we condition on incident district and year. Table 2 summarizes victim and arrestee characteristics in column 1, shows that they are predictive of whether a

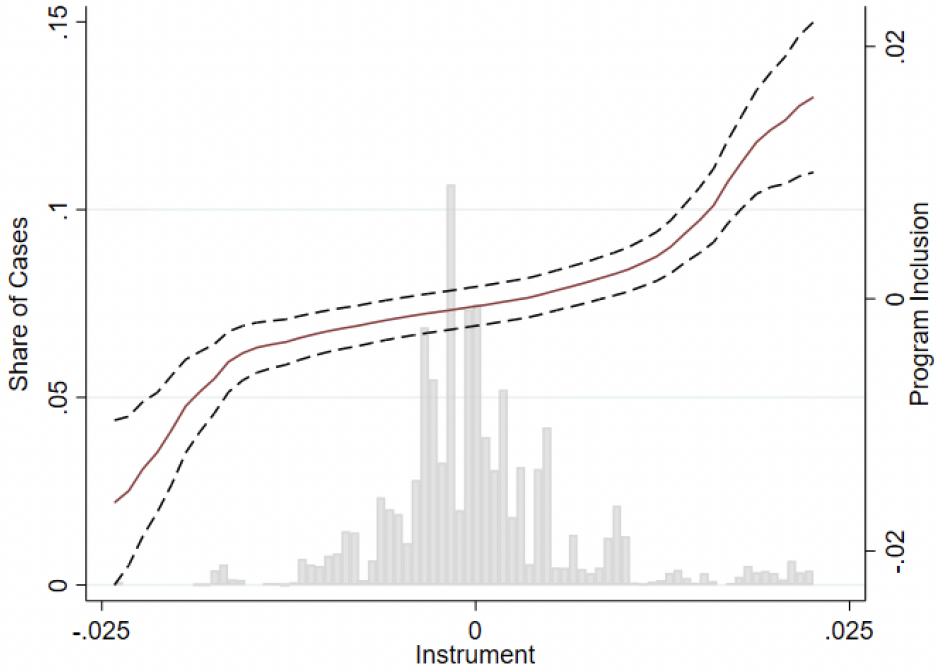


Figure 3: Instrument and Program Inclusion

Notes: This figure shows the distribution of the instrument, the leave-one-out-mean of a prosecutor / Assistant State's Attorney / ASA's rate of selecting cases for the targeted program, residualized by district-year. The solid line is a local linear regression of whether a case is selected for the targeted program (also residualized by district-year) on the residualized instrument, and the dashed lines depict 95% confidence intervals. A case assigned to a more inclusive ASA (computed using all cases except the current case) has a higher likelihood of being selected for the targeted program. Data Sources: Chicago Police Department, Cook County State's Attorney's Office.

given case is chosen for the targeted program in column 2 (joint F-statistic = 95.1745, p-value < 0.0001), but are not predictive of prosecutors' case inclusion rates in column 3 (joint F-statistic = 1.3847, p-value = 0.1489).

Table 2: Testing for the Random Assignment of Cases to Specialized Prosecutors

Characteristic	Mean	Pr(Included)	Prosecutor Inclusion Rate
	(1)	(2)	(3)
Victim			
Prior Victimization	0.1720	0.0508***	0.0000
	(0.3773)	(0.0035)	(0.0001)
Prior Aggravated Victimization	0.0210	0.0241**	-0.0002
	(0.1433)	(0.0098)	(0.0002)
Age	34.5167	-0.0001**	-0.0000
	(12.7721)	(0.0001)	(0.0000)
Male	0.2114	-0.0289***	-0.0001**
	(0.4083)	(0.0018)	(0.0001)
Black	0.6430	0.0088**	0.0000
	(0.4791)	(0.0040)	(0.0001)
White-Hispanic	0.2023	-0.0036	-0.0001
	(0.4017)	(0.0038)	(0.0001)
Black-Hispanic	0.0042	-0.0062	0.0000
	(0.0645)	(0.0134)	(0.0005)
Asian/Pacific Islander	0.0081	-0.0055	-0.0002
	(0.0896)	(0.0085)	(0.0004)
Arrestee			
Prior Domestic Arrest	0.2174	0.0940***	0.0001
	(0.4125)	(0.0032)	(0.0001)
Age	32.8850	0.0019***	-0.0000
	(10.7407)	(0.0001)	(0.0000)
Male	0.8365	0.0325***	-0.0002***
	(0.3698)	(0.0018)	(0.0001)
Black	0.6749	0.0058	-0.0001
	(0.4684)	(0.0042)	(0.0001)
White-Hispanic	0.2004	-0.0007	0.0001
	(0.4003)	(0.0046)	(0.0001)
Black-Hispanic	0.0072	0.0316***	-0.0001
	(0.0843)	(0.0115)	(0.0003)
Asian/Pacific Islander	0.0067	-0.0102	-0.0003
	(0.0818)	(0.0088)	(0.0003)
Victim, Arrestee Live Together	0.4612	-0.0010	0.0000
	(0.4985)	(0.0018)	(0.0001)
Observations	96,575	96,575	96,575
F-statistic		95.1745	1.3847
p-value		0.0000	0.1489
Dependent Variable Mean		0.0623	0.0559

Notes: Sample consists of 96,575 arrests between 2001-13 flagged as domestic incidents by the Chicago Police Department that occur within 150 days of a prosecutor switch. Standard deviation in parentheses in column (1); standard errors in parentheses in columns (2) and (3). Regressions include district-year fixed effects, and standard errors are two-way clustered at the individual and district-year level. Data Sources: Chicago Police Department, Cook County State's Attorney's Office.

Monotonicity. If the causal effect of the targeted program is heterogeneous across cases, we must assume that the instrument also satisfies the monotonicity condition – i.e. cases that are included in TAC by a more inclusive prosecutor would also be included in TAC by a less inclusive prosecutor. This assumption ensures that our estimates identify the average treatment effect among cases who may not have been included in TAC if they had been assigned to a different prosecutor.

One testable implication of the monotonicity assumption is that first stage estimates are non-negative within any sub-sample. Tables A3 and A4 show that our instrument satisfies these tests – we construct the prosecutor inclusion rate using our sample of 96,575 cases, and estimate the first stage on sixteen separate sub-samples based on victim and arrestee characteristics. Consistent with the monotonicity assumption, each of these first stage estimates is positive and statistically distinguishable from zero.

3 Data

We employ several administrative datasets in our analysis. Both the raw data as well as the probabilistic record linkage algorithm used to link these datasets are described in detail in the Data Appendix (Section A1).

Briefly, information on prosecutor stints as well as cases selected for the targeted program between 1999-2019 were collected from the Cook County State's Attorney's Office (SAO). These were linked to Chicago Police Department (CPD) arrest and victimization records using a unique identifier for each incident for the same time period. Individual death records between 2000-22 were obtained from the Cook County Medical Examiner's Office (CCMEO).

Our study sample focuses on arrests between 2001-13 flagged as domestic incidents

by the Chicago Police Department. We choose this time frame for two reasons:

1. Our sample begins in 2001 to allow for a consistent lookback period of 2 years – i.e., for each individual in our sample, we compute prior victimization and criminal history based on the two years preceding each incident in our sample. These variables are used to test the conditional independence assumption in Section 2.4 and to train the machine learning algorithm in Section 5.

2. Our sample ends in 2013 because our research design is unable to isolate the causal impact of the targeted program beyond this date. The federal grant that funded the program ended in early 2014, at which point it continued to operate but with fewer resources. In 2014, the DV Division within the SAO also set up a separate collaborative program – a DV Multi-Disciplinary Team consisting of police officers, prosecutors, probation officers, and victim service providers – which makes it impossible to separate the effects of these two interventions after this date.[20]

We further restrict attention to cases occurring within 150 days of a prosecutor switch.[21] These restrictions yield a final estimation sample of 96,575 arrests between 2001-13 that are flagged as domestic incidents by CPD. In Figure 5, we show that our results are only strengthened when we restrict attention to incidents that occur within 30, 60, 90, or 120 days of each prosecutor switch; these samples are by definition smaller, but arguably expected to be more balanced along unobservable dimensions.

[20]For more details about the DV Multi-Disciplinary Team in Cook County, see https://cook-county.legistar.com/LegislationDetail.aspx?ID=2129010&GUID=D77C48E9-35E0-4014-8E9F-99FF89854FB9&Options=&Search=.

[21]The sample is slightly unbalanced without this restriction, likely driven by switchover dates that are very early or late in the calendar year. These dates lead to far more cases after and before the switch respectively, and the samples end up being statistically dissimilar along observable dimensions.

4 Impact on Victim Safety

This section presents our findings on the impact of the targeted program on victim safety, and discusses mechanisms that may be driving these findings.

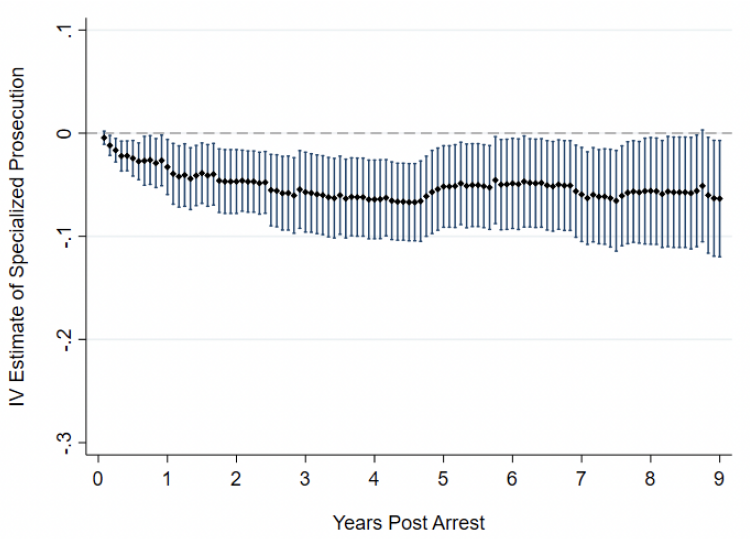
4.1 Homicide Victimization

Our outcome of interest is victim death by homicide. We focus on this outcome for three reasons – one, the focus of the program is to select and serve individuals that are gauged to be at highest risk of harm, and this outcome captures the most severe form of re-victimization; two, DV continues to drive a substantial proportion of homicides in the U.S. – 20% of homicides overall and 50% of female homicides are committed by a current or former intimate partner; three, under-reporting of DV is high, and focusing on an outcome that is accurately reported ensures that our estimates are not driven by changes in reporting behavior.

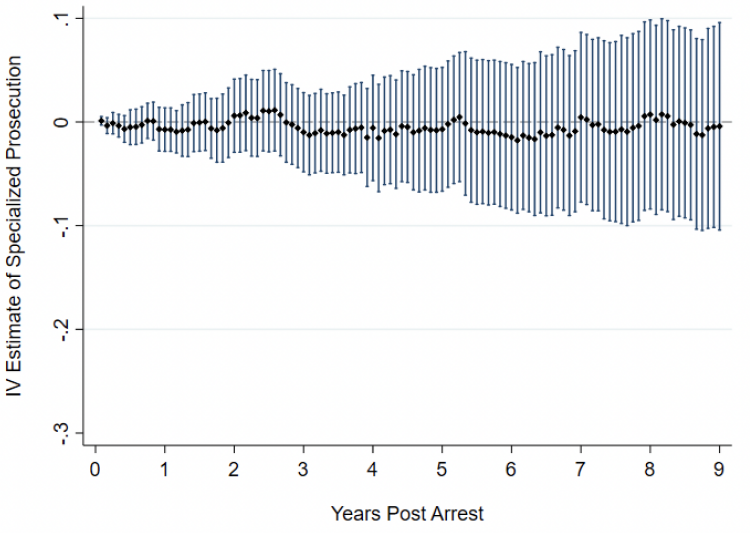
We first show that the targeted program reduces homicide victimization, and that this effect persists in the long run (9 years). The first panel of Figure 4 displays the estimated impact of the program on homicide victimization over a 9-year period in 30 day increments, with a noticeable decline in the first few months. The effect size continues to increase as we look at longer follow-up periods, and the estimate remains statistically distinguishable from zero even 9 years after the incident date. The second panel of Figure 4 presents the results of a falsification test; here, the outcome is victim death by natural causes such as cardiovascular disease, an outcome that we would not expect the targeted program to affect. Consistent with this expectation, we find no discernible impact on the likelihood of victim death due to natural causes – the point

Figure 4: Impact of the Targeted Intervention on Victim Death

(a) Primary Outcome: Victim Death by Homicide by Year t



(b) Falsification Test: Victim Death by Natural Causes by Year t



Notes: These figures display 2SLS point estimates and 95% confidence intervals of the impact of the targeted program on victim death by cause. Standard errors are two-way clustered at the individual and district-year level. Data Sources: Chicago Police Department, Cook County State's Attorney's Office, Cook County Medical Examiner's Office.

Table 3: Impact of the Targeted Program on Victim Death by Homicide

	1 year	5 years	9 years
	Reduced Form		
Prosecutor Inclusion Rate	–0.0384***	–0.0487***	–0.0634**
	(0.0137)	(0.0183)	(0.0281)
	2SLS Estimates		
Targeted Program	–0.0408***	–0.0517**	–0.0673**
	(0.0148)	(0.0201)	(0.0296)
	2SLS Estimates with Controls		
Targeted Program	–0.0401***	–0.0501**	–0.0658**
	(0.0144)	(0.0194)	(0.0289)
N	96,575	96,575	96,575
District-Year F.E.	Yes	Yes	Yes
Mean	0.0005	0.0018	0.0030
Non-TAC Compliers:			
Mean	0.0444	0.0532	0.0733
Share of All Homicides	0.5928	0.2046	0.1651

Notes: Sample consists of 96,575 arrests between 2001-13 flagged as domestic incidents by the Chicago Police Department that occur within 150 days of a TAC prosecutor switch. Standard errors are two-way clustered at the individual and district x year level. Controls in the third panel include victim age, sex, race, and prior victimization, arrestee age, sex, race, and prior domestic arrest, and whether the victim and arrestee share the same address. Share included in TAC = 6.23%, share of always-takers = 5.70%, share of compliers = 1.21%. Data Sources: Cook County Medical Examiner's Office, Chicago Police Department, Cook County State's Attorney's Office. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$

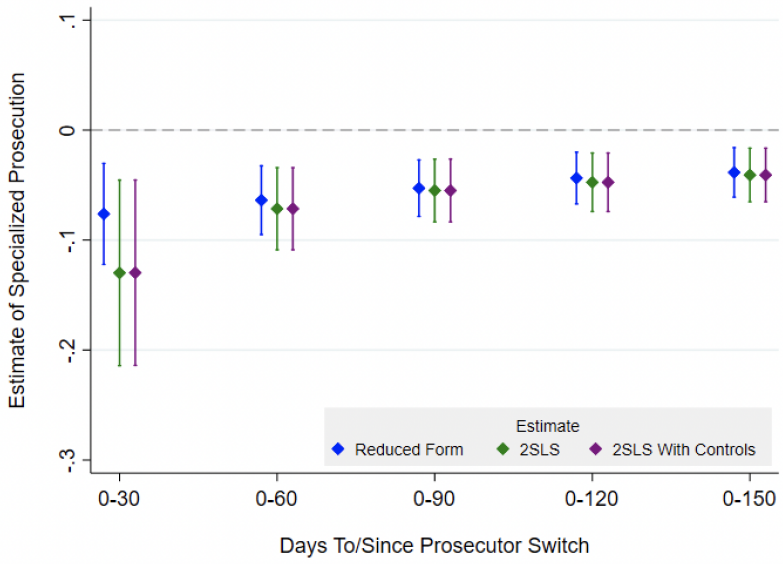
estimates are very small in magnitude, and statistically indistinguishable from zero.[22]

Table 3 displays point estimates and standard errors of the impact of the targeted intervention on victim death by homicide 1, 5, and 9 years after the initial incident date. For those included in the intervention, the chance of re-victimization by homicide is lowered by 4 percentage points within the first year; the point estimate increases in magnitude and statistical significance persists as we look at a 9-year follow up pe-

[22]Figure A3 extends this analysis to show the estimated impact on victim death due to any cause, by suicide, by accident, or due to undetermined causes.

riod. The last panel shows that these estimates are robust to controlling for victim characteristics (age, sex, race, prior victimization), defendant characteristics (age, sex, race, prior arrests), and shared characteristics (whether the victim and defendant live together). Finally, Figure 5 shows that these results persist when we look at incidents that occur within 30, 60, 90, or 120 days of a prosecutor switch date; these samples are by definition smaller, but arguably expected to be more balanced along unobservable dimensions. Point estimates are larger and remain statistically distinguishable from zero. Overall, these estimates indicate that the program has a large and persistent beneficial impact on victim safety.

Figure 5: Impact of the Targeted Program on Homicide Victimization by Days To/Since Prosecutor Switch



Notes: This figure plots estimates of the impact of the targeted program on homicide victimization within 1 year along with 90% confidence intervals for different samples of the data. Sample sizes are 25,605, 47,884, 67,869, 83,625, and 96,575 domestic incident arrests for the 0-30, 0-60, 0-90, 0-120, and 0-150 day samples respectively. Data Sources: Chicago Police Department, Cook County State's Attorney's Office, Cook County Medical Examiner's Office.

A notable feature of the estimated decrease in homicide risk in Table 3 is that

the point estimates are very large relative to the overall mean. To an extent, this is unsurprising – our estimates only speak to effects for individuals on the margin of being included in the program, i.e., those that are expected to be at much higher risk for homicide than the average individual. To show this explicitly, we estimate homicide risk for compliers that are *not* included in TAC. We follow Bhuller *et al.* (2020) and Agan *et al.* (2021), who perform similar calculations in judge-IV and prosecutor-IV settings respectively.[23]

First, we estimate the share of always-takers π_a – cases that would be included in TAC regardless of the prosecutor they are assigned to – using the share of cases included even by the least inclusive prosecutors:

$$\pi_a = Pr[Included \,|\, Z = Z_{min}]$$

Similarly, we estimate the share of never-takers π_n – cases that would not be included in TAC regardless of the prosecutor assigned to their case – using the share of cases excluded even by the most inclusive prosecutors:

$$\pi_n = 1 - Pr[Included \,|\, Z = Z_{max}]$$

We implement this by regressing whether a given case is included in the targeted program on the prosecutor's inclusion rate Z and district-year fixed effects γ_{dt}[24]

$$Included_i = \alpha Z + \gamma_{dt} + \varepsilon_{idt}$$

For each district-year cell, we recover π_a and π_n using Z_{min} and Z_{max} within that cell. The share of compliers π_c is estimated as $1 - \pi_a - \pi_n = Pr[Included \,|\, Z_{min} < Z < Z_{max}]$.

[23] These papers rely on the insights of Abadie (2003) and Dahl *et al.* (2014).

[24] We include district-year fixed effects as the assignment of cases to different prosecutors is treated as as-good-as-random *conditional* on the district and year in which the incident took place.

In our setting, 5.7% of cases are always-takers, 1.2% are compliers, and 93.1% are never-takers.

Next, we estimate homicide risk for complier cases that were not included in the program. We do this in three steps. First, we restrict attention just to cases excluded from the program, and regress homicide outcomes on the prosecutor's inclusion rate Z and district-year fixed effects ω_{dt}

$$Y_i = \beta Z + \omega_{dt} + \epsilon_{idt}$$

Second, we estimate homicide risk for never-takers based on the outcomes for cases assigned to the most inclusive prosecutors that were excluded from the program: $E(Y_i \,|\, Excluded, Z = Z_{max})$ for each district-year cell. Finally, we use the insight that homicide risk among excluded cases assigned to the least inclusive prosecutors is a weighted average of risk for compliers and never-takers:

$$\begin{aligned} E(Y_i \,|\, Excluded, Z = Z_{min}) \;\; = \;\; & \tfrac{\pi_c}{\pi_c+\pi_n} E(Y_i \,|\, Excluded, Z_{min} < Z < Z_{max}) \\ & + \tfrac{\pi_n}{\pi_c+\pi_n} E(Y_i \,|\, Excluded, Z = Z_{max}) \end{aligned}$$

Re-arranging, we can estimate homicide risk for compliers that were not included in the program as:

$$\begin{aligned} E(Y_i \,|\, Excluded, Z_{min} < Z < Z_{max}) \;\; = \;\; & \tfrac{\pi_c+\pi_n}{\pi_c} E(Y_i \,|\, Excluded, Z = Z_{min}) \\ & - \tfrac{\pi_n}{\pi_c} E(Y_i \,|\, Excluded, Z = Z_{max}) \end{aligned}$$

The last two rows of Table 3 display the resultant estimates. 1-year homicide risk for compliers excluded from the program is 0.0444, nearly ninety times higher than the sample mean. An alternative way to understand this risk is that complier cases *not* included in the targeted program make up 0.7% of all cases but account for 59% of all

homicides in the 1-year follow-up period.[25] Similarly, 9-year homicide risk for compliers excluded from the program is 0.0733, over twenty times higher than the sample mean; these cases account for 17% of all homicides in the nine-year follow-up period.

The reduced form estimate of –0.0384 in Column 1 of Table 3 indicates that being assigned to a prosecutor that includes all cases relative to one that includes none reduces 1-year homicide risk for complier cases by 86%. The 2SLS estimate of –0.0401 in Column 1 indicates that actually being included in the targeted program reduces 1-year homicide risk for complier cases by 90%. The estimates in Column 3 show that this is not a short-term effect, and that the targeted program also reduces long-term homicide risk by close to 90%.

Prior research shows that DV interventions may also improve defendant safety by providing victims with an alternative to the most extreme form of self-defense – murder Aizer & Bo (2009). To test this hypothesis in our setting, we use the same empirical strategy to estimate the impact of the targeted intervention on defendant death. Figure A4 displays the estimated impact on defendant death due to any cause, by homicide, by natural causes, by suicide, by accident, or due to undetermined reasons. All six panels show that we are unable to reject the null hypothesis of no effect on defendant death in our setting.

4.2 Mechanisms

In this section, we discuss several mechanisms that may be driving the program's impact on victim safety, relying extensively on Frohmann & Hartley (2003)'s descriptive findings (summarized in Figure 6).

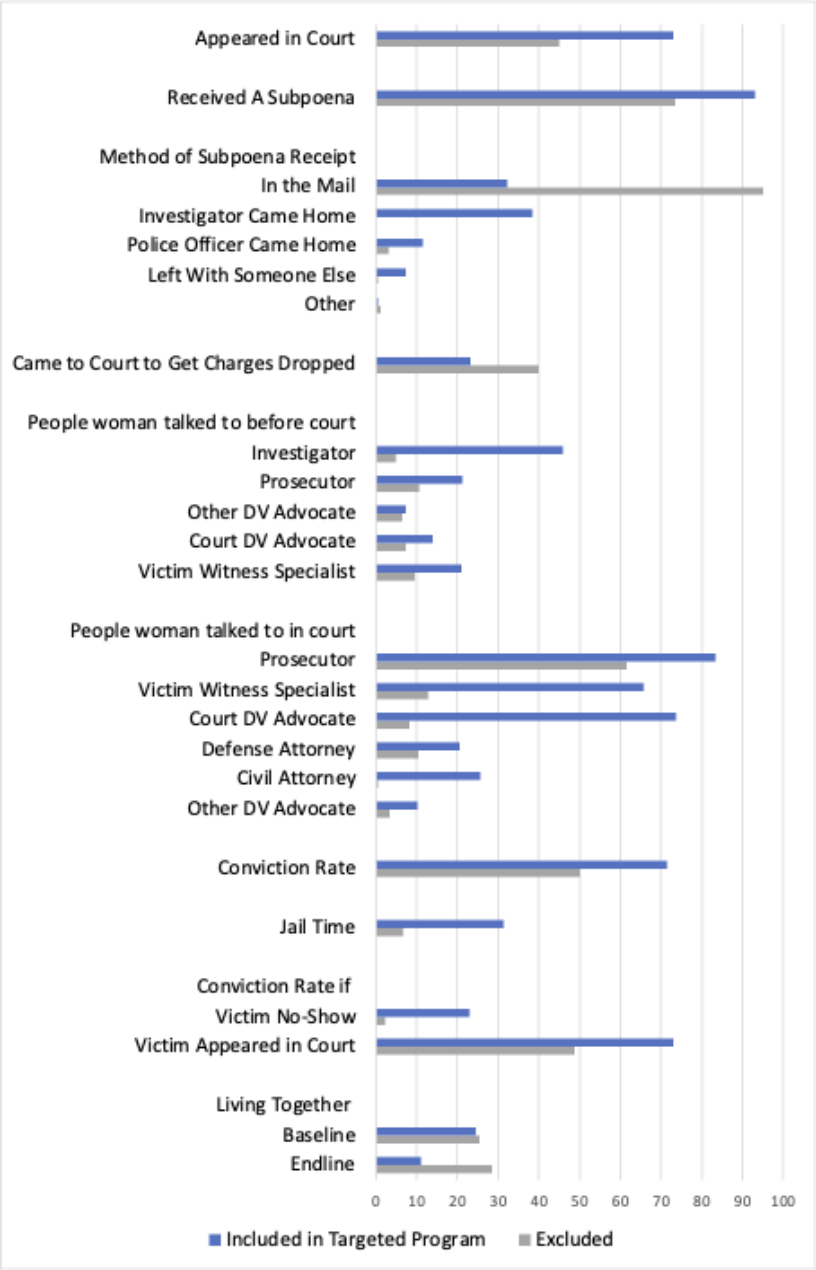
[25] We estimate this as $[\pi_a + \pi_c - Pr(Included)] * 0.0444/0.0005 = 0.5816$

Criminal Conviction. A central feature of the program is an increase in prosecutor capacity – unlike other DV cases, cases included in the targeted intervention are prosecuted vertically, with the same prosecutor handling the case from start to finish. In practice, this translates to lighter caseloads for TAC prosecutors, who have more time to dedicate to each case than prosecutors handling non-TAC cases. In 2007, for instance, fifteen prosecutors in the DV division handled non-TAC cases – i.e. 94% of all cases, or 6% each, while the two TAC prosecutors handled the 6% selected for the targeted program, or 3% each (Landis, 2007). This mechanism is consistent with Frohmann & Hartley (2003)'s findings of low drop rates and higher conviction rates for TAC cases.

Incapacitation. One implication of this increase in prosecutor capacity may be an increase in the probability of a carceral sentence for the offender in question – i.e. cases included in TAC may be more likely to end in jail or prison sentences, and this long-term incapacitation effect may be driving the observed reduction in homicide risk.

In order to test this theory directly, we would need access to jail *and* prison admission data for the period 2001-22. This is because the vast majority of DV cases in our sample are misdemeanors, and carry sentences of at most one year in jail. Unfortunately, we do not have access to jail data. However, we argue that while short-term incapacitation may spur the improvement in victim safety, long-term incapacitation is unlikely to be driving the documented long-run results for two reasons. First, we know that the targeted program can increase the likelihood of a carceral sentence, but that this incapacitation effect ends within 12 months. Even if an increase in the probability of a jail sentence for DV defendants drives the observed improvement in victim safety, the persistence of these effects well beyond the one-year mark in Figure 4 indicates that incapacitation in jail can explain only part of the program's impact. Second, we have

Figure 6: Comparing Cases Included in and Excluded from the Targeted Intervention



Notes: This figure displays some of Frohmann & Hartley (2003)'s descriptive findings, based on 103 cases included in and 219 cases excluded from the targeted intervention between December 2000 and August 2001.

access to prison admissions data between 2006-22, which we can use to estimate the program's impact on prison sentences (which usually last longer than a year). Figure A5 displays the estimated impact on defendant prison incarceration for any offense, homicide, battery, or any domestic offense; all four panels show that we are unable to reject the null hypothesis of no effect on defendant prison incarceration for nine years following the index arrest in our setting.

Information on Court Processes. To capture the perspectives of SAO's non-profit program partners, we interviewed representatives from Life Span, whose advocates assist and accompany victims to court, and provide support throughout the prosecution process. Their observation was that the program has been especially impactful in reducing the rate at which DV court cases are dropped from further prosecution, and that the main mechanism driving this difference is the high-touch subpoena process and timely communication about court dates by SAO investigators on TAC cases. Specifically, SAO investigators will reach out to victims once they have been inducted into the program, and distribute subpoenas that specify when, where, and why victims need to come to court. This in-person process enables investigators to increase the salience of the legal process, answer victim questions, and problem-solve to address victim needs like transportation to and from court. This mechanism is supported by Frohmann & Hartley (2003); Figure 6 shows a striking difference in how TAC victims receive subpoenas as compared to non-TAC victims. Over 90% of non-TAC victims receive subpoenas in the mail, while this is only true for a third of TAC victims; TAC victims are much more likely to receive a subpoena from an SAO investigator, police officer, or other individual than in the mail. They are also more likely to report receiving a subpoena than non-TAC victims.

Victim Beliefs. The targeted program is focused on a small proportion of victims that prosecutors believe to be at high risk for future harm. Some victims may update their beliefs regarding their own safety risk as they continue to engage with prosecutors, investigators, and advocates; this may make them more likely to take actions to protect themselves, including but not necessarily limited to participating in the prosecution process. This mechanism is supported by previous research, which finds that risk assessments are an effective way of communicating risk to victims (Heilbrun *et al.*, 2000). This mechanism is also consistent with Frohmann & Hartley (2003)'s finding that TAC victims were just as likely to report living with the defendant as non-TAC victims at baseline, but far less likely at endline (six months later). While this physical separation may be attributed to the victim's participation in the criminal prosecution process, it is plausible that this is a protective action that is taken by victims independent of their decision to pursue a legal response.

Reporting. Individuals may become more willing to report incidents of violence once they understand that their cases are being taken seriously, increasing the potential cost of re-offending for their partners. The ability of DV interventions to improve victims' faith in law enforcement has been previously documented by research (Jolin *et al.* 1998, Lockwood & Prohaska 2015, Messing *et al.* 2015), and is consistent with the positive coefficients for some, albeit not all, types of reported victimization in the long run, plotted in Figure A6.

4.3 Cost-Benefit Analysis.

Using our most conservative estimate – a reduction in the 1-year homicide rate by 3.8 percentage points for individuals at the margin of inclusion – expanding slots in the program by 50% to include 826 cases (or 9%) instead of 551 cases (or 6%) per year could prevent $0.038 * 276 = 10$ homicides per year.[26] As a baseline, CPD reported 49 domestic homicides in 2007 (the middle of our study period), and 55 domestic homicides in 2020. Recent estimates of the social cost of a single homicide range in the millions, which easily outweighs increasing annual expenditure on the program by 50%, or $460,000 (McCollister *et al.*, 2010).[27]

5 Can Machine Learning Improve Targeting?

Recent work has demonstrated the ability of machine learning algorithms to provide more accurate forecasts of future criminal justice outcomes when compared to selections made by humans (Chalfin *et al.*, 2016; Kleinberg *et al.*, 2018; Bhatt *et al.*, 2023). This evidence highlights the potential utility of combining machine learning-derived forecasts with human selection decisions when targeting interventions under capacity constraints. When the number of program slots are limited, the impact of an intervention can potentially be improved by selecting those for inclusion that have the highest predicted baseline risk of the outcomes the program aims to reduce (Bhatt *et al.*, 2023).[28]

In this section we test whether a machine learning algorithm trained on all DV

[26] Between 2000-18, TAC selected 10,463 cases, an average of 551 cases per year.

[27] The Chicago Tribune reported that the annual cost of the program in 2003 was $650,000, or $923,000 in 2020 USD.

[28] A related parameter to target is the treatment response. In percentage point terms, treatment responsivity is a function of baseline risk, as in, the maximum value of treatment responsivity will be higher the higher the baseline risk.

cases *not* selected for the targeted intervention can produce more accurate assessments of homicide risk than those of specialized prosecutors. If the algorithm can improve upon prosecutor forecasts, then it might be possible to increase the impact of TAC by serving individuals with higher baseline risk of future homicide than the status quo practice. However, we present evidence that prosecutors are *better* at selecting high-risk cases for the targeted program. Although the algorithm is able to pass standard performance checks, the riskiest cases identified by our algorithm face, on average across follow-up periods of 1, 5, and 9 years, 83% less risk than cases at the margin of inclusion in the the targeted intervention. Put differently, if the number of cases that could be included in the targeted intervention was increased, prosecutors would allocate those slots to individuals at substantially higher risk than those that the algorithm would select.

We start with a sample of 150,174 unique victim-arrestee pairs involved in all DV arrests between 2000 and 2013.[29] This sample excludes those that are included in the targeted intervention to ensure that our estimates of homicide risk are not confounded by programmatic effects. Next, we generate out-of-sample predictions for all victim-arrestee pairs using a gradient boosting model (Ke *et al.*, 2017) to predict future homicide victimization. We split the 114,844 unique arrestees into five mutually exclusive folds to limit the chance that the same person is included in both the training and evaluation set (Chouldechova *et al.*, 2018). We then iteratively hold out each of these folds and train a separate model for 1-, 5-, and 9-year follow-up windows.

To predict the outcome, we create 199 predictive features that fall into five categories: counts of prior victimizations for the victim, counts of prior arrests for the ar-

[29]This sample is larger than the one used in Section 4 because it does not restrict attention to cases that occur within 150 days of a prosecutor switch.

restee, demographic attributes for the victim, demographic attributes for the arrestee, and facts about the current case.[30] As ML algorithms can have difficulty in predicting outcomes with very low base rates, we also predict two related outcomes that are more prevalent proxies of the true outcome of interest. To that end, we also train model to predict any violent revictimization (base rate = 43%) and violent felony revictimization (base rate = 14%).

Figure 7 shows the performance of the three separate outcome models when evaluated on future homicide as measured by the Area Under the Curve (AUC) (Fawcett, 2006) — the probability that an algorithm correctly ranks a randomly homicide victim higher than a randomly chosen case which does not result in future homicide. All models have AUCs higher than 0.5 (meaning they are better than random chance), though we see that the violent felony predictor generally tends to perform better than the homicide predictor across all followup years.

Figure 7 shows that homicide can be predicted from police data with some accuracy, but does not tell us whether this accuracy justifies the use of an ML tool to augment prosecutor decisions. One way we can answer this is by comparing the risk profile of those at highest risk as determined by our models with that for compliers who were not included in the the targeted intervention. This is an appropriate comparison because these individuals were excluded from the program simply because of the prosecutor that they were assigned to, not because of specifics of the case, victim, or arrestee. Additionally, their outcomes are unadulterated by the direct effects of the program, which Section 4 shows are large and statistically distinguishable from zero. We use their risk of homicide as a benchmark that an ML tool must exceed in order to improve how the program is targeted.

[30]The counts are computed for the last 30, 60, 90, 180, 365, 730, and 1,460 days.

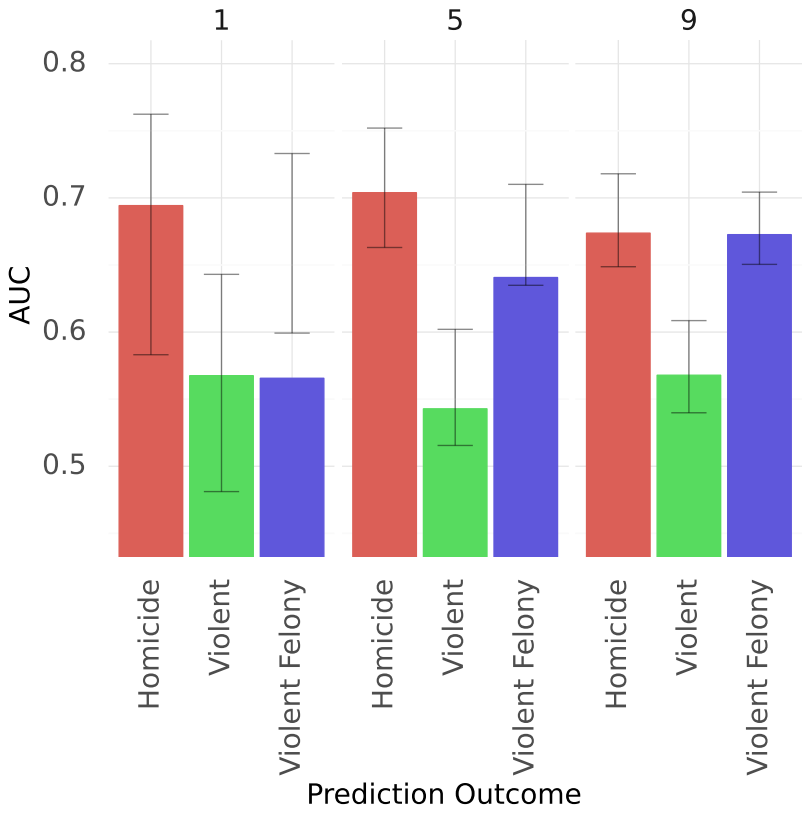


Figure 7: Predictive Performance by Training Outcome

Notes: This figure shows the predictability of future homicide victimization, as measured by the Area Under the Curve (AUC), for three training outcomes (homicide, violent, and violent felony) and three different followup periods (1, 5, and 9 years). Each panel shows the AUCs for different followup periods. Within each panel is the AUC for the different outcomes. For example, the third panel shows the AUC for predicting future homicide within the next nine years when cases are ranked by future 9-year homicide, violent, and violent felony risk, respectively. Bootstrapped 95% confidence intervals were generated using 1,000 replicates.

To make the comparison meaningful, we restrict attention to the 1.0% of individuals that each model identifies as the riskiest; 1.0% is also the size of the complier group that is not included in the targeted intervention relative to all cases that are excluded from the program (which is the subset that the ML tools are trained on and make predictions for). Table 4 shows that similarly sized groups of beneficiaries identified by the ML tool have meaningfully lower homicide rates. Column (2) of Table 4 shows

Table 4: Comparing the Selections of ML and Prosecutors

Follow-up (1)	Outcome Rate (2)	CCM (3)	Realized Risk (4)	Predicted Risk (5)
1	0.0005	0.0444	0.0000	0.0013
5	0.0018	0.0532	0.0083	0.0195
9	0.0030	0.0733	0.0207	0.0202

Notes: Table compares the risk level of cases selected by specialized prosecutors and machine learning forecasts. Column (1) shows the follow-up period (in years) over which the values in the other columns are computed; Column (2) shows the overall homicide victimization rate; Column (3) shows the non-TAC complier control mean; Column (4) shows the actual homicide victimization rate for those selected by the algorithm; Column (5) shows the predicted homicide victimization rate for those selected by the algorithm.

that the homicide victimization rate for cases not-included in the program as a result of being assigned to less inclusive prosecutors ranges from 4.44% (1-year followup) to 7.33% (9-year followup). On the other hand, columns (3) and (4) show the actual and predicted victimization rates, respectively, for similarly sized groups of individuals selected by the algorithm (and not selected by prosecutors). On average across the three followup years, the realized risk of ML-selected cases is a substantial 83% lower than the rate for prosecutor near-selections. This comparison provides evidence that prosecutors are proficient at identifying individuals at high-risk, and that ML tools that augment this decision-making process are unlikely to improve beneficiary selection. The gap between proficiency measures for prosecutors and ML tools suggests that prosecutors incorporate both quantitative and qualitative information beyond what is available in police data to make their selections (effectively).

6 Conclusion

In the United States, 20% of homicides are perpetrated by current or past intimate partners. Further, contact with law enforcement prior to homicide is high, indicating that criminal justice system actors may be uniquely positioned to identify individuals at heightened risk, and address constraints that prevent victims from ensuring their own safety. This understanding has led to decades of state and federal investment in DV reduction programs that encourage collaboration across criminal justice agencies and service providers and focus on high-risk survivors of DV. Yet, little is known about whether they are an effective way to enhance safety, especially those at elevated risk.

This paper studies this approach in Cook County, Illinois, and shows that identifying victims at high risk and addressing their constraints in participating in the legal process is a promising way to improve their safety in the long run. The intervention is extremely cost effective, a finding consistent with calculations for a law enforcement-service provider collaboration that focuses on high-risk victims in Maryland (Koppa, 2018). Additionally, our estimates speak directly to the beneficial effects expected for cases on the margin of inclusion, indicating that jurisdictions intent on reducing the escalation of DV should consider establishing or expanding these kinds of programs.

Our analysis also reveals that machine learning tools based on administrative criminal justice data may not always improve program implementation. This may be partly driven by the fact that criminal justice system actors often have access to both qualitative and quantitative information that is not observable by researchers and data engineers. As a result, we argue that deploying machine learning tools is unlikely to be universally beneficial, even in criminal justice system contexts where prior research has demonstrated benefits like improved targeting and reduced bias (Berk *et al.* 2016,

Kleinberg *et al.* 2018, Grogger *et al.* 2021, Heller *et al.* 2022).

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Online Appendix

A1 Data

This section provides additional details about the raw CPD, SAO, and CCMEO data, the identification of domestic incidents and arrests, and the record linkage algorithm used to locate unique people – victims and/or arrestees – across records.

A1.1 CPD Incident Records

We start with 8,102,603 incident records between January 1999 and August 2019, each of which include a unique case identifier (CPD Records Division or RD number). We drop a small number of duplicate records (n = 48) and records that are assigned to more than one geographical district in Chicago (n = 56).[31]

Domestic Incidents. We flag 14% (n = 1,118,178) of incidents as domestic using two fields:

1. If a binary field called Domestic is marked "Yes"
2. If the Illinois Uniform Crime Reporting (IUCR) Code is:
    - 486 (Domestic Battery Simple)
    - 488 (Aggravated Domestic Battery – Handgun)
    - 489 (Aggravated Domestic Battery – Other Firearm)
    - 496 (Aggravated Domestic Battery – Knife/Cutting Instrument)
    - 497 (Aggravated Domestic Battery – Other Dangerous Weapon)
    - 498 (Aggravated Domestic Battery – Hands/Fist/Feet Serious Injury)
    - 499 (Aggravated Domestic Battery)

[31] We drop the latter set of records because prosecutor assignment is based on the incident location.

- 584 (Violation of Stalking No Contact Order)
- 4386 (Violation of Civil No Contact Order)
- 4387 (Violate Order of Protection)
- 4388 (Violation of Bail Bond - Domestic Violence)
- 4750 (Disclose Domestic Violence Victim Location)

A1.2 CPD Victim Records

This dataset also spans January 1999 - August 2019, and contains 5,911,192 victim records. We drop a small number of duplicate records (n = 29). Victimization records include incident identifiers, but do not include unique person identifiers. To construct each person's victimization history, we use each victim's identifying information (including name, home address, date of birth, and age) in conjunction with the probabilistic record linkage algorithm described in section A1.9.

Domestic Victimizations. We flag 18% (n = 1,085,333) of all victimizations as domestic using two fields within CPD's victim records:

1. If a binary field called Domestic, filled out by police officers, is marked "Yes"
2. If the binary field Domestic is marked "No" but the charge description field contains any of the tokens "Dom", "Order", or "Protect". These include
    - Domestic Battery Simple
    - Violate Order of Protection
    - Aggravated Domestic Battery: Other Dangerous Weapon
    - Aggravated Domestic Battery: Knife/Cutting Instrument
    - Aggravated Domestic Battery: Hands/Fist/Feet Serious Injury

- Aggravated Domestic Battery
- Violate Bail Bond: Dom Violence
- Aggravated Domestic Battery: Handgun
- Violation OF Stalking No Contact Order
- Aggravated Domestic Battery: Other Firearm
- Notification: Order Protection

Aggravated Domestic Victimizations. To identify the most serious subset of domestic victimizations, we use a third field within CPD's victim records: Uniform Crime Reporting (UCR) codes. 1% of all victim records are flagged as aggravated domestic victimizations based on meeting two conditions:

1. Domestic victimization (based on the criteria above)
2. UCR code = 01A (Homicide), 01B (Involuntary Manslaughter), 02 (Criminal Sexual Assault), 03 (Robbery), 04A (Aggravated Assault), or 04B (Aggravated Battery)

A1.3 CPD Arrest Charge Records

This dataset contains 5,971,207 records, which includes all charges associated with each arrest made between January 1999 and August 2019. We flag 5% (n = 289,416) of charges as domestic based on whether a binary field called Domestic Violence is marked "Yes".

A1.4 CPD Arrest Records

We start with 2,626,933 adult arrest records between January 1999 and August 2019. CPD arrest records include a unique person identifier (an Illinois Record or IR number), based on a fingerprint scan, that allows us to construct each individual's arrest history. In addition to this identifier, CPD arrest records include incident identifiers that can be used to link

together arrestees and victims associated with each incident. The arrest data can also be linked to police-recorded information about arresting charges, charge descriptions, UCR codes, the location and time of the incident and the arrest, and identifying information about the arrestee (name, date of birth, demographic information).

Domestic Incident Arrests. We flag 262,770 – or 10% – of all arrests as those related to domestic incidents using four datasets that are linked to the arrest records using incident identifiers. An arrest is flagged as domestic if it meets any of the four conditions listed below.

1. Domestic Incident Record (see Section A1.1)
2. Domestic Victim Record (see Section A1.2)
3. Domestic Arrest Charges (see Section A1.3)
4. Dataset: Arrest Reports

   The charge description field contains any of the tokens "Dom", "Vio", "Order", or "Protection".[32] Table A1 lists all of the charge descriptions that meet this criterion. These tokens account for 138,681 – or 6% – of all arrests.

[32]We exclude arrests with the following charge descriptions that happen to contain the four tokens listed above but are unlikely to be relevant to our study sample: "Injure Domestic Animal", "Closure Order – Violation", "Juvenile Child Protection Warrant", "Odometer Fraud", "Firearms – Protection of Minors", and "Violation of Child Passenger Protection Act".

Table A1: Arrest Report Charge Descriptions Including "Dom", "Order", "Protection", "Vio"

Charge	Count	Share
Domestic Battery - Bodily Harm	83,620	0.603
Domestic Battery - Physical Contact	26,367	0.190
Domestic Battery	15,928	0.115
Domestic - Violation Of Order Of Protection	7,283	0.053
Violate Order Protection	2,857	0.021
Domestic Battery - Aggravated	952	0.007
Domestic Battery - Bodily Harm/Prior	353	0.003
Domestic - Vio Order Protection/Other Prior	262	0.002
Domestic Battery - Other Prior	225	0.002
Domestic - Vio Order Protect/Prior/Vio Of Order	191	0.001
Domestic - Violate Order Protect/Prior Dom Battery	136	0.001
Domestic Battery - Phys Contact/Prior	116	0.001
Vio Order After Served Notice	86	0.001
Domestic -Vio Order Protection/Notice/Prior Vio Ordr Protect	61	0.000
Domestic Battery - Bodily Harm/Vio Order Protect	58	0.000
Aggravated Domestic Batter/Strangle	44	0.000
Interfere Report Domestic Violence	26	0.000
Violate Order/Prior Domestic Battery	22	0.000
Domestic - Vio Ordr/Notice/Prior Battery	22	0.000
Domestic Battery - Phys Contact/Vio Ordr Protect	19	0.000
Domestic Battery - Aggravated	19	0.000
Domestic - Vio Ordr Protect/Other Prior	14	0.000
Stalking - Agg - Viol Restr Order/Viol Order Protect	12	0.000
Violation Of Order Of Protection	<10	0.000
Interf Rept Domestic Violence	<10	0.000
Violate Order Of Protection	<10	0.000

A1.5 Study Sample

We start with the 262,770 domestic arrests identified using at least one of the fields discussed in the previous section. Next, we drop

- 52,372 arrests after 2013; as discussed in Section 3, we are unable to isolate the impact of the intervention beyond this date
- 23,255 arrests prior to 2001; this allows for a lookback period of 2 years (based on which variables such as prior victimization and prior arrest are generated)
- 34,181 arrests where victim information is missing; this information is needed to construct re-victimization outcomes.
- 30,254 arrests in district-year pairs where only one prosecutor served as a specialized DV prosecutor; this is because our research design utilizes variation in prosecutors *within* district-year pairs.

We are left with a study sample of 96,575 domestic arrests. Summary statistics can be found in column (1) of Table 2.

A1.6 SAO Records

The Domestic Violence Division within the SAO maintains information on

- 34 prosecutors and the terms of their service on the targeted intervention program between 1999 and 2013, as well as which courtrooms[33] they were assigned to. This information was de-identified and shared with the research team, and is used as the basis of the research design.

[33]See Figure 2 for the mapping between CPD districts and courtrooms.

- Cases that were included in the targeted intervention, including a unique case identifier (CPD Records Division or RD number). As this is a field that is generated by CPD, it is used to link cases selected for the targeted intervention to our sample of domestic arrests. 7,329 – or 6% – of domestic arrests are included in the the targeted intervention.

A1.7 CCMEO Records

The Cook County Medical Examiner's Office maintains individual-level death records between 2000 and 2022. This data includes a unique record identifier, full name, date of birth, date of death, sex, race, ethnicity, and address. This data was merged with the CPD data using the probabilistic record linkage algorithm described in Section A1.9.

A1.8 IDOC Prison Population Data

Since mid-2005, the Illinois Department of Corrections (IDOC) has publicly released data on all individuals incarcerated in Illinois prisons 2-4 times a year.[34] For each incarcerated individual, these files include an IDOC ID, name, demographics like race and gender, and information about the type of offense and sentencing county.

A1.9 Probabilistic Record Linkage

While CPD arrest records and SAO case records include a unique person identifier, CPD victimization and CCMEO death records do not. As such, we use a probabilistic record

[34]The first dataset in this series lists all individuals in custody on June 30th 2005. The next file is dated June 30th 2006. From June 2006 to December 2017, IDOC released this data every 6 months; since 2018, this information has been released every 3 months. This data can be accessed at https://www2.illinois.gov/idoc/reportsandstatistics/Pages/PopulationDataSets.aspx.

linkage algorithm to identify unique individuals across the arrest, victimization, prosecution, and death record datasets. For details on the algorithm itself, see McNeill & Jelveh (2021). In this section, we describe the basics of the linking procedure.

To link CPD records that refer to same person, we take the post-2010 IR number (the fingerprint-based person identifier associated with arrest records) as ground truth.[35]

Our record linkage algorithm produces a collection of records referring to the same person which we call a *cluster*. In assigning records to clusters, the algorithm follows researcher-specified rules based on the context of the data. For our linkage, we specify the following constraints. First, a cluster can have at most one post-2010 IR number. Second, a homicide record cannot link to another record if the homicide record's event date came before the other record's event date. Third, percent of victimization records do not have date-of-birth information—an important predictor of true positive links—which can lead to a large number of false positive links. To reduce the chance of these false positives, we introduce a constraint that if at least one record in a record pair is missing date-of-birth information, enforce that the age field (if not missing) in the two records is within 3 years. We also enforce that if at least one record in a potential cluster is missing date-of-birth information, all other records in the cluster not missing date-of-birth information must have similar dates of birth.[36]

The record linkage procedure identifies 5,426,703 individuals across the three decades of our data. We filter the set of clusters to exclude two sets of people: those with no CPD records from the past 50 months relative to a given prediction date, and those who only had a single victimization in the past 50 months. We exclude these individuals for two reasons. First, they have much lower baseline risk: percent of these individuals were shot during the follow-up period, compared to percent of people in our cohort. Second, a large number of victimization records do not have date of birth information, making them more likely to incorrectly link to

[35]The consistency of IR numbers is somewhat spotty at the beginning of the sample but improved considerably over time. As such, we do not treat IR numbers prior to 2010 as ground truth.

[36]We operationalize this by enforcing that these dates of birth be within two character edits of each other.

another record. To address this challenge, our linking algorithm was conservative in creating links that involved a record with no date of birth information. That is, a large share of clusters with only one record are clusters where that one record is a victimization record with no date of birth. As a result, dropping single victimizations implicitly reduces the influence of record-linkage error caused by poor data quality. As such, our sample selection criteria reduce data integrity issues, while still capturing most of the identifiable population with elevated risk.

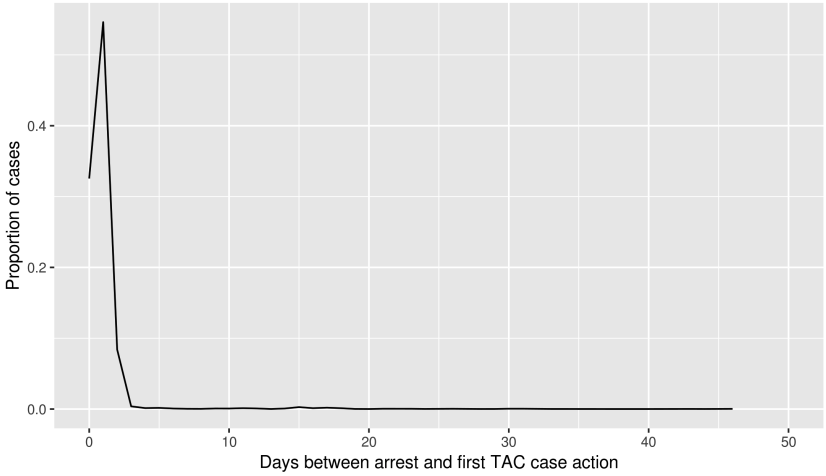
A2 Figures

Figure A1: Domestic Victimizations in Chicago 2000-2018



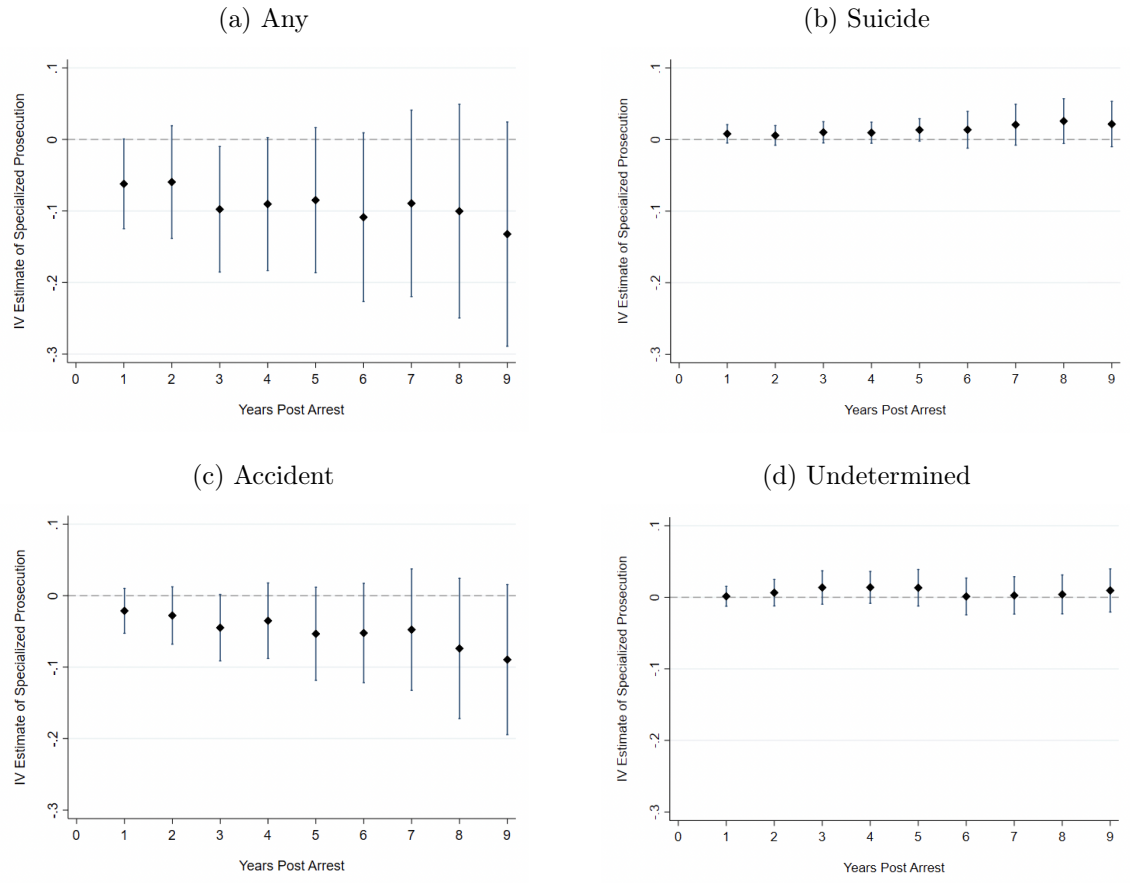
Notes: This figure plots the number of domestic victimizations reported to the Chicago Police Department between 2000 and 2018. Source: Chicago Police Department.

Figure A2: Days Between Arrest and First Prosecutorial Action



Notes: This figure plots the distribution of days between arrest and first prosecutorial action on cases selected for the targeted intervention. Sources: Chicago Police Department, Cook County State's Attorney's Office.

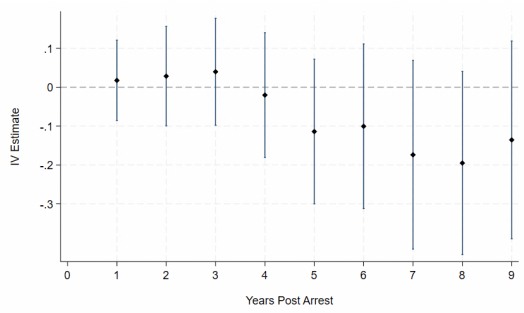
Figure A3: Impact on Victim Death by Cause



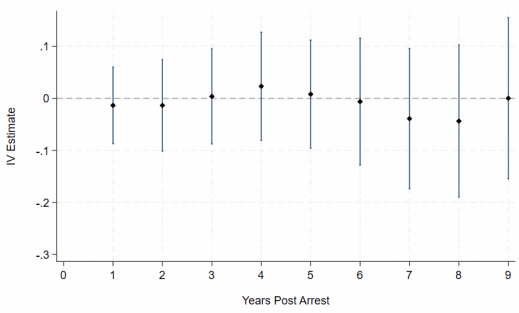
Notes: These figures display 2SLS point estimates and 95% confidence intervals of the impact of the targeted intervention on victim death by cause. Standard errors are two-way clustered at the individual and district-year level. Data Sources: Chicago Police Department, Cook County State's Attorney's Office, Cook County Medical Examiner's Office.

Figure A4: Impact on Defendant Death by Cause

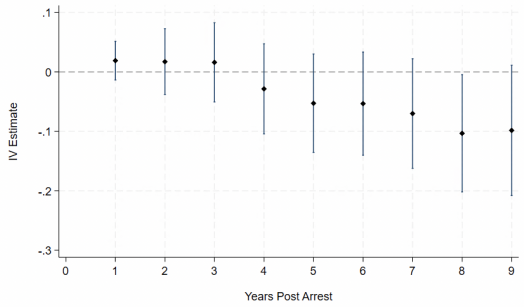
(a) Any



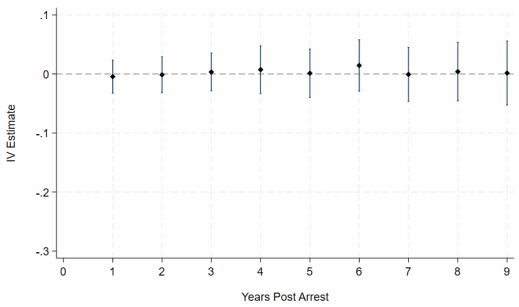
(b) Homicide



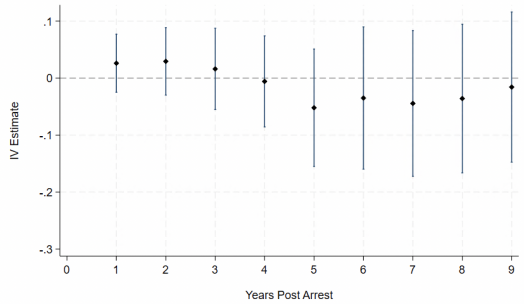
(c) Natural Causes



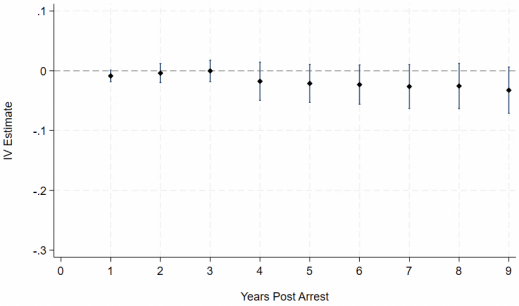
(d) Suicide



(e) Accident

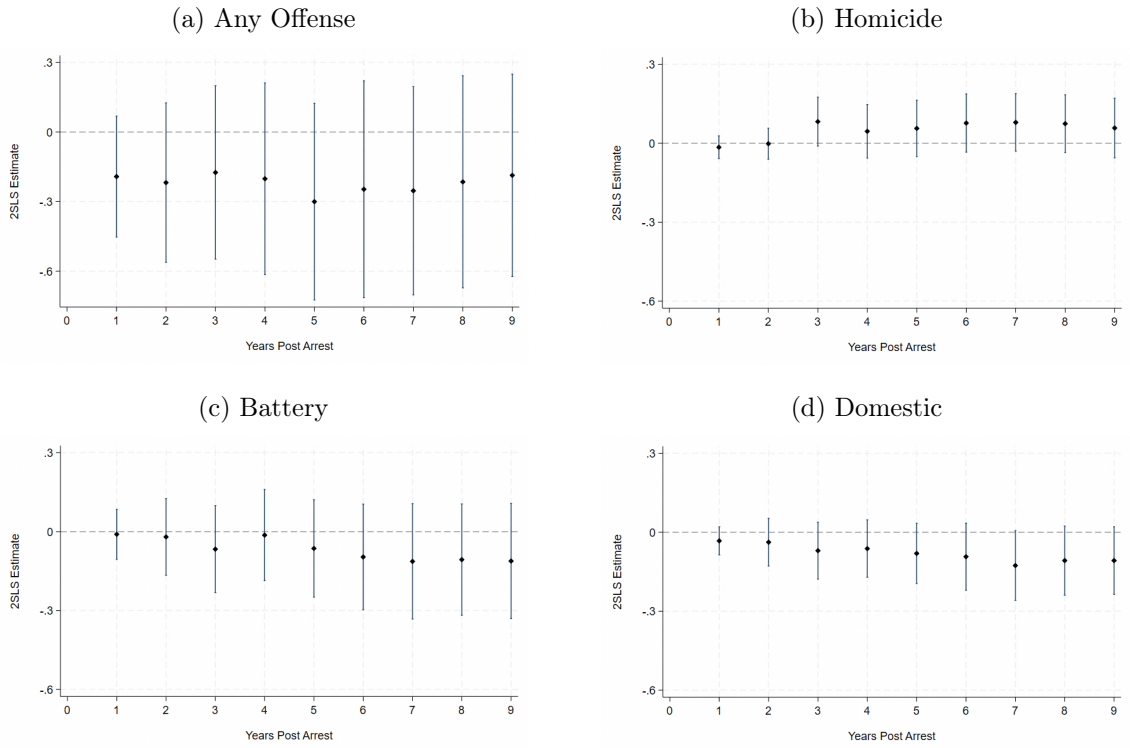


(f) Undetermined



Notes: These figures display 2SLS point estimates and 95% confidence intervals of the impact of the targeted intervention on defendant death by cause. Standard errors are two-way clustered at the individual and district-year level. Data Sources: Chicago Police Department, Cook County State's Attorney's Office, Cook County Medical Examiner's Office.

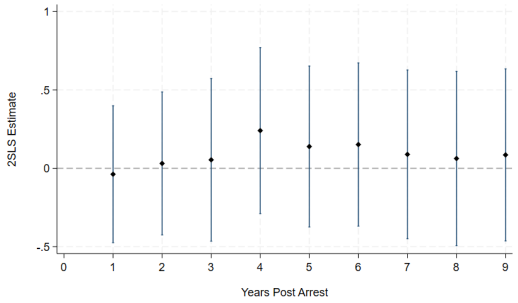
Figure A5: Impact on Defendant Prison Incarceration by Offense Type



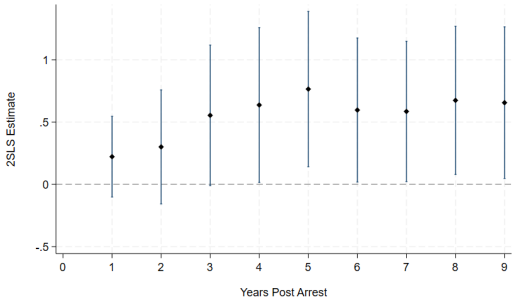
Notes: These figures display 2SLS point estimates and 95% confidence intervals of the impact of the targeted intervention on defendant incarceration by offense type. Standard errors are two-way clustered at the individual and district-year level. Data Sources: Chicago Police Department, Cook County State's Attorney's Office, Illinois Department of Corrections.

Figure A6: Impact on Victimization Reported to the Police

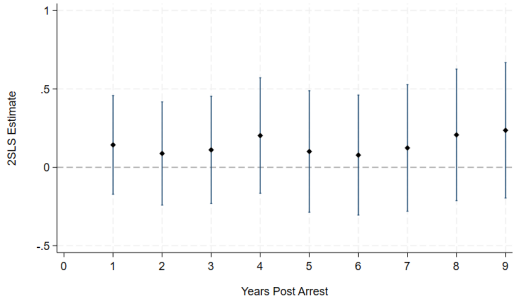
(a) Violent Offenses



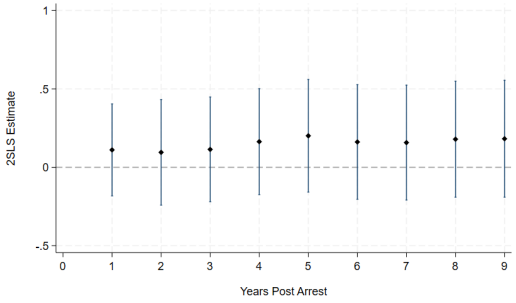
(b) Property Offenses



(c) Domestic Offenses



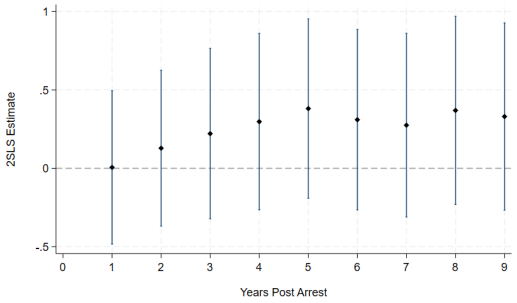
(d) Offenses by Same Defendant



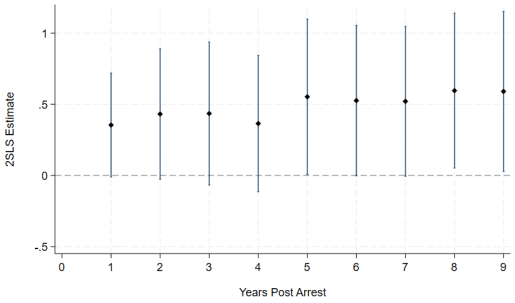
Notes: These figures display 2SLS point estimates and 95% confidence intervals of the impact of the targeted intervention on victimization reported to CPD by offense type. Standard errors are two-way clustered at the individual and district-year level. Data Sources: Chicago Police Department, Cook County State's Attorney's Office.

Figure A7: Impact on Defendant Re-Arrest

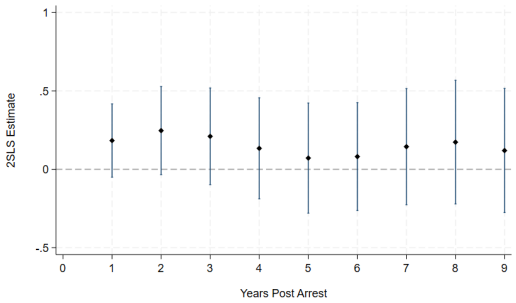
(a) Any Offenses



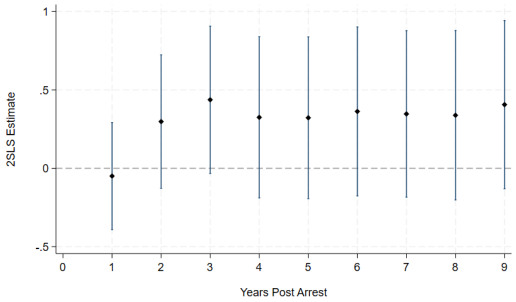
(b) Violent Offenses



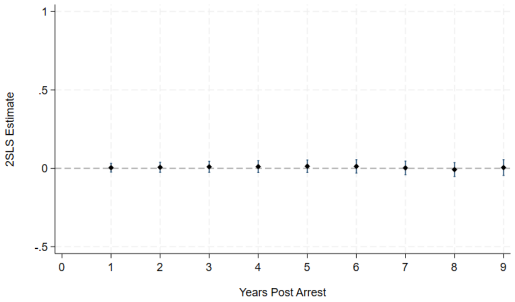
(c) Property Offenses



(d) Felony Offenses



(e) Homicide



Notes: These figures display 2SLS point estimates and 95% confidence intervals of the impact of the targeted intervention on defendant re-arrest by offense type. Standard errors are two-way clustered at the individual and district-year level. Data Sources: Chicago Police Department, Cook County State's Attorney's Office.

A3 Tables

DA-LE: Danger Assessment for Law Enforcement[37]

DVHRT: Domestic Violence High Risk Team

LAP: Lethality Assessment Program

MDT: Multi-Disciplinary Team

D-LAG: Dangerousness/Lethality Assessment Guide

Table A2: Targeted Interventions for High-Risk Domestic Violence Cases in the U.S.

State	Name	Start Year	Collaborating Agencies				Targeted	Jurisdiction
			Police	Prosecutor	Court	Services		
AL	LAP	2013	✓			✓	✓(high-risk)	State
AK	Fairbanks DV Probation Project	2011		✓		✓	✓(high-risk)	City
AZ	LAP	2010	✓			✓	✓(high-risk)	State
AR	Laura's Law	2015	✓	✓		✓	✓(high-risk)	State
CA	Contra Costa LAP	2015	✓			✓	✓(high-risk)	County
	Crim. Just. Advocacy Unit	1981	✓		✓	✓	✓(felony)	City
	Tulare County HRT	2017	✓	✓		✓	✓(high-risk)	County
CO	DV Enhanced Response Team	1996	✓		✓	✓	✓(high-risk)	City
	17^{th} Jud. Dist. DV HRT	2022	✓	✓		✓	✓(high-risk)	County
CT	LAP	2010	✓			✓	✓(high-risk)	State
DE	Kent County DV HRT	2019	✓			✓	✓(high-risk)	County
	LAP	2010	✓			✓	✓(high-risk)	State
FL	InVEST	2022	✓	✓	✓	✓	✓(high-risk)	Multi-County
GA	LAP	2009	✓			✓	✓(high-risk)	Metropolitan
HI	LAP	2016	✓			✓	✓(high-risk)	State
ID	Coalition Against DV		✓	✓		✓	✓(high-risk)	State
IL	Target Abuser Call	1997		✓		✓	✓(high-risk)	County
IN	South Bend DA-LE	2022	✓			✓	✓(high-risk)	City
IA	LAP	2016	✓		✓	✓	✓(high-risk)	
KS	DV Lethality Assessment	2011	✓			✓	✓(high-risk)	State
KY	LAP	2012	✓			✓	✓(high-risk)	Multi-city
ME	Enhanced Police Intervention Collab.	1977	✓			✓	✓(high-risk)	Cumberland County
MD	LAP	2005	✓			✓	✓(high-risk)	City, County
MA	LAP	2010	✓			✓	✓(high-risk)	County
	Greater Newburyport DVHRT	2005	✓	✓	✓	✓	✓(high-risk)	City
MI	Canton Township DV HRT	2015	✓	✓		✓	✓(high-risk)	Township
MN	LAP	2010	✓			✓	✓(high-risk)	County
MS	LAP	2009-13	✓			✓	✓(high-risk)	State
MO	LAP	2009	✓			✓	✓(high-risk)	County

[37]More information about the DA-LE and DVHRT programs can be found at https://geigerinstitute.org/

MT	LAP	2016	✓			✓	✓(high-risk)	
NV	Clark County DV HRT	2022	✓	✓		✓	✓(high-risk)	County
NH	LAP	2009	✓		✓	✓	✓(high-risk)	State
NJ	Essex C. Family Justice Center	2010	✓	✓	✓	✓	✓(high-risk)	County
NM	LAP	2015	✓			✓	✓(high-risk)	State
NY	Kings C. Felony DV Court	1996		✓	✓	✓	✓(felony)	County
	Oneida C. DV HRT	2021	✓	✓		✓	✓(high-risk)	County
	Suffolk C. Integrated DV Court	2002	✓	✓	✓	✓	✓[38]	County
NC	LAP	2013	✓			✓	✓(high-risk)	State
	Gaston C. DA-LE	2022	✓	✓		✓	✓(high-risk)	County
ND	LAP	2012	✓			✓	✓(high-risk)	Multi-county
OH	Cuyahoga C. DV HRT	2016	✓			✓	✓(high-risk)	County
OK	LAP	2010	✓			✓	✓(high-risk)	State
OR	LAP	2015	✓			✓	✓(high-risk)	Multi-county/city
PA	LAP	2012	✓			✓	✓(high-risk)	State
RI	Specialized DV Supervision Unit	1994			✓[39]		✓(high-risk)	State
SC	LAP	2009-13	✓			✓	✓(high-risk)	State
SD	LAP	2009-13	✓			✓	✓(high-risk)	State
TN	LAP	2012	✓			✓	✓(high-risk)	Multi-city/county
TX	Harris County DV HRT	2018	✓	✓	✓	✓	✓(high-risk)	County
	Pasadena DA-LE	2018	✓	✓	✓		✓(high-risk)	City
	LAP	2012	✓			✓	✓(high-risk)	State
UT	LAP	2015	✓			✓	✓(high-risk)	State
VT	LAP	2012	✓			✓	✓(high-risk)	County
VA	LAP	2012	✓			✓	✓(high-risk)	State
WA	LAP	2009-13	✓			✓	✓(high-risk)	State
WI	Milwaukee County DV HRT	2017	✓	✓		✓	✓(high-risk)	County
WV	D-LAG	2018	✓	✓	✓	✓	✓(high-risk)	State

[38] 2+ pending DV cases; 1 pending DV case and 1 pending matrimonial case

[39] Run by the Probation Unit within the RI Department of Corrections

Table A3: Monotonicity Tests by Victim Characteristic

	(1)	(2)
	Dependent Variable: Pr(Specialized Prosecution)	
	Prior Domestic Victimization	No Prior Domestic Victimization
Prosecutor's Inclusion Rate	2.3501***	0.6999***
	(0.3083)	(0.1340)
Observations	16,607	79,968
F-Statistic (Instrument)	58.0933	27.2653
	Age $\leq$ Median	Age $>$ Median
Prosecutor's Inclusion Rate	1.0628***	1.0189***
	(0.1251)	(0.2048)
Observations	50,346	46,229
F-Statistic (Instrument)	72.1291	24.7542
	Female	Other
Prosecutor's Inclusion Rate	1.2813***	0.5701***
	(0.1240)	(0.1740)
Observations	75,731	20,844
F-Statistic (Instrument)	106.6852	10.7348
	Black	Non-Black
Prosecutor's Inclusion Rate	1.1730***	0.8753***
	(0.1170)	(0.1988)
Observations	62,093	34,482
F-Statistic (Instrument)	100.5511	19.3939

Notes: Sample consists of 96,575 arrests between 2001-13 flagged as domestic incidents by the Chicago Police Department that occur within 150 days of a prosecutor switch. Median age is 32. Regressions control for district-year fixed effects, and standard errors are two-way clustered at the individual and district-year level. Data Source: Chicago Police Department, Cook County State's Attorney's Office. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$

Table A4: Monotonicity Tests by Arrestee Characteristic

	(1)	(2)
	Dependent Variable: Pr(Specialized Prosecution)	
	Prior Domestic Arrest	No Prior Domestic Arrest
Prosecutor's Inclusion Rate	2.4719***	0.5219***
	(0.2520)	(0.1305)
Observations	20,996	75,579
F-Statistic (Instrument)	96.2039	16.0045
	Age $\leq$ Median	Age $>$ Median
Prosecutor's Inclusion Rate	0.8435***	1.1238***
	(0.1101)	(0.2523)
Observations	50,629	45,946
F-Statistic (Instrument)	58.7296	19.8324
	Male	Other
Prosecutor's Inclusion Rate	1.2448***	0.5923***
	(0.1243)	(0.1681)
Observations	80,787	15,788
F-Statistic (Instrument)	100.2667	12.4097
	Black	Non-Black
Prosecutor's Inclusion Rate	1.1047***	1.0328***
	(0.1134)	(0.2190)
Observations	65,181	31,393
F-Statistic (Instrument)	94.8338	22.2485

Notes: Sample consists of 96,575 arrests between 2001-13 flagged as domestic incidents by the Chicago Police Department that occur within 150 days of a prosecutor switch. Median age is 30. Regressions control for district-year fixed effects, and standard errors are two-way clustered at the individual and district-year level. Data Source: Chicago Police Department, Cook County State's Attorney's Office. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$