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]

A priori, it is not obvious whether interventions led by the criminal justice system

[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.

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 & Dal 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).

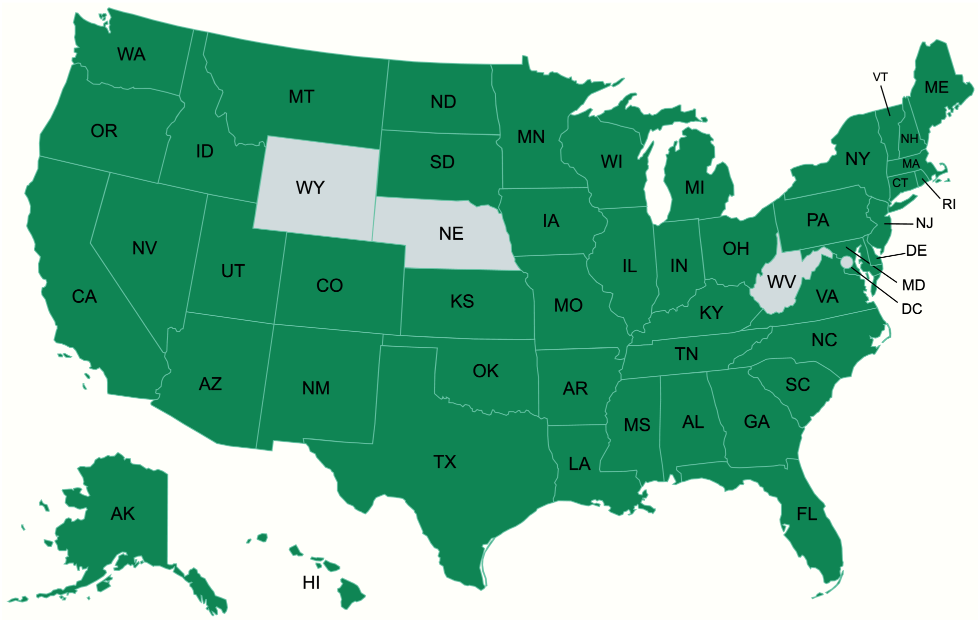
Prior research is encouraging in that it shows that it is possible to increase victim engagement with the criminal justice system, but is silent on whether this approach can deter domestic homicide. This gap is not for lack of policy experimentation – federal funding under the 1994 Violence Against Women 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 among local law enforcement officers, 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 assessment of victim risk, and the channelling of additional resources towards those identified as high-risk;[6] these resources help the victim develop a safety

[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). It is unclear, however, whether these approaches can be used 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.

[6]Commonly used assessments include the Danger Assessment, the Spousal Assault Risk As-

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.

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 assessment, 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 Eng-

sessment, the Ontario Domestic Assault Risk Assessment, and the Domestic Violence Screening Instrument These assessments are typically short checklists or questionnaires filled in by police officers and/or service providers, and tend to capture risk factors such as the defendant's criminal history and the victim's barriers to support.

land, 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] Besides the fact that the studies find mixed results, they also leave two questions unanswered. 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, how accurate are the criminal justice system's assessments of victim risk, and can they be aided by data-driven forecasting tools?

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 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, engage 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

[7] The authors do not estimate the intervention's impact on homicide. It is not straightforward to extrapolate 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.

[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

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 intervention's 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 90% of the complier mean. Most of this reduction 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

was 73 per cent for the former group and 40 per cent for the latter (see Figure 6 for more of their findings). Similar patterns of increased victim satisfaction with specialized DV responses have been documented in other settings – for instance, 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 a falsification test using victim death due to natural causes (e.g., cardiovascular disease).

(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 4.4, we use this insight to show that the expected benefits of expanding slots in this program in Chicago far exceed the expected costs.[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 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. To ensure that the outcome is not contaminated by the direct – and as shown above, beneficial – impact of the program,

[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.

[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 responsive to the intervention we study.

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.[16]

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.

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.

[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.

2.1 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. The caseloads of TAC prosecutors are significantly lower than other prosecutors in the DV division. As a result, significantly more prosecutorial capacity is allocated towards each TAC case than other DV cases.[18]

- **TAC Investigators.** Four investigators hired by the SAO 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 hired by

[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.

[18] In 2007, fifteen prosecutors in the DV division handled non-TAC cases – i.e. 94% of all cases (or 6.3% each), while the two TAC prosecutors handled the 6% selected for TAC (or 3% each) (Landis, 2007).

the SAO 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), 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 appearances. TAC victims are less likely to request that criminal charges be dropped, and their cases are more likely to end in a conviction and jail time. TAC victims are also less likely to be living with the defendant six months after case initiation than non-TAC victims.

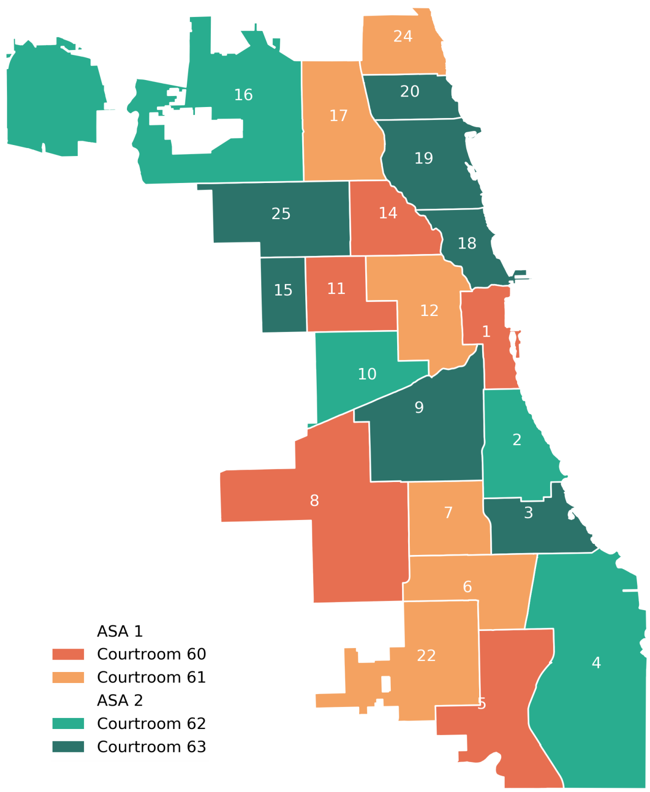
2.2 Assignment of Cases to Specialized Prosecutors

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.[19] Cases are assigned

[19]Figure A1 shows that for 99% of cases included in TAC, the first prosecutorial action takes place within 2 days of the arrest.

for screening based on incident location – one prosecutor screens cases corresponding to arrests made in eleven of Chicago's Police Districts, and the other screens cases corresponding to the remaining eleven Districts; Figure 2 displays the geographic allocation of districts to the first prosecutor in orange, and the second in green.

Figure 2: Jurisdictional Split Across Prosecutors



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

Prosecutors typically serve as part of TAC for a year before being rotated out onto another division within the 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.[20] 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. 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.[21] Our empirical specification can be described by:

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

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

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

[20] 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.

[21] We compute this propensity based on *all* cases that a prosecutor selects from except the case at hand, even cases that do not occur within 150 days of a prosecutor switch or those that belong to different district-year cells.

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, which measures whether the outcome occurred between the original incident date and the follow-up period t. 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.[22]

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.

Table 1: First Stage Estimates

	(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. See Appendix A1 for a precise definition of domestic incidents. 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. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

[22] While the need to cluster at the victim level is unambiguous, the choice of unit for the second level of clustering is more subjective. We cluster at the district-year level to account for correlation in potential outcomes because of common shocks. We also show that clustering at the victim and prosecutor level does not meaningfully impact the precision of our results (compare Tables 3 and A6).

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 fixed effects reduces the magnitude of the point estimate slightly, but also that 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.

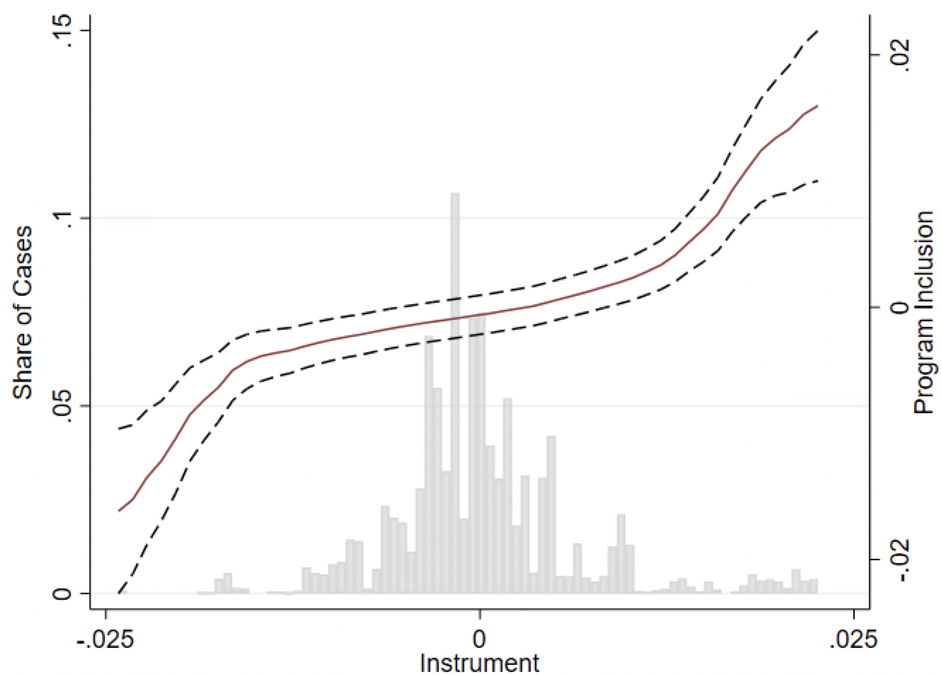


Figure 3: Instrument and Program Inclusion

Notes: This figure shows the distribution of the instrument (the leave-one-out-mean rate at which the prosecutor selects 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.

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 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 Prosecutors

Characteristic	Mean (1)	Pr(Included) (2)	Prosecutor Inclusion Rate (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. See Appendix A1 for precise definitions of domestic and aggravated domestic arrest/victimization. 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 arrest 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.[23]

We further restrict attention to cases occurring within 150 days of a prosecutor switch.[24] 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.

[23] For more details about the DV Multi-Disciplinary Team in Cook County, see https://cookcounty.legistar.com/LegislationDetail.

[24] 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 intervention on victim safety, and discusses mechanisms that may be driving these findings.

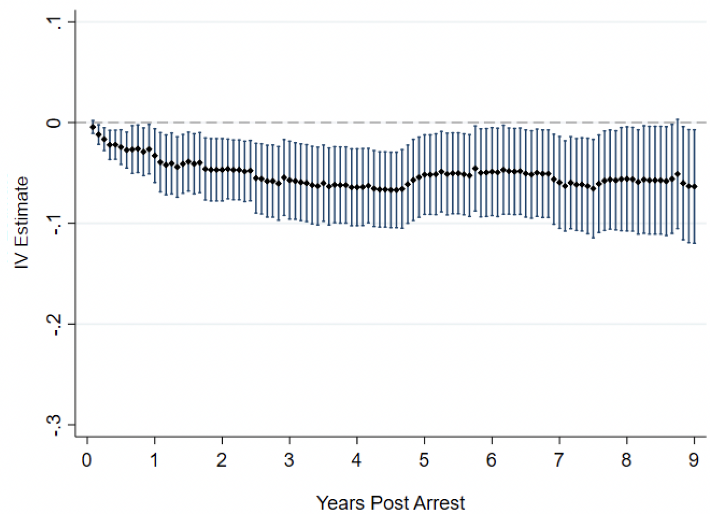
4.1 Homicide Victimization

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.

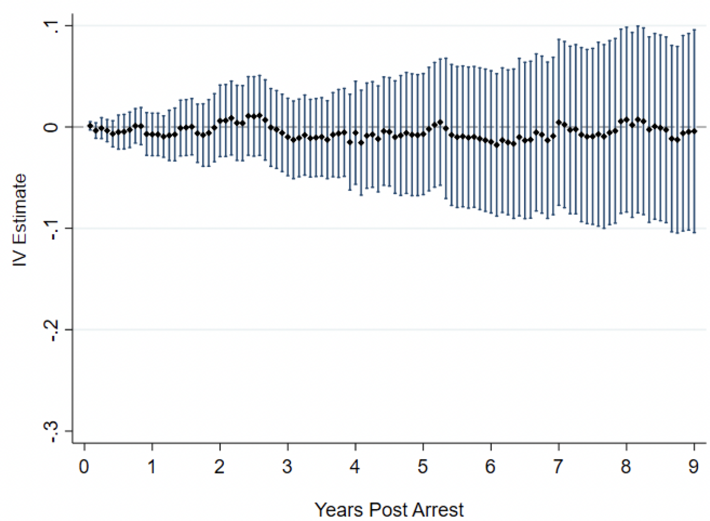
We first show that the targeted program reduces homicide victimization, and that this effect persists in the long run (9 years). The first panel of Figure 4 displays the estimated impact of the program on homicide victimization over a 9-year period in 30 day increments, with a noticeable decline in the 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 estimates are very small in magnitude, and statistically indistinguishable from

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.

zero.[25]

[25] Figure A2 extends this analysis to show the estimated impact on victim death due to any cause, by suicide, by accident, or due to undetermined causes.

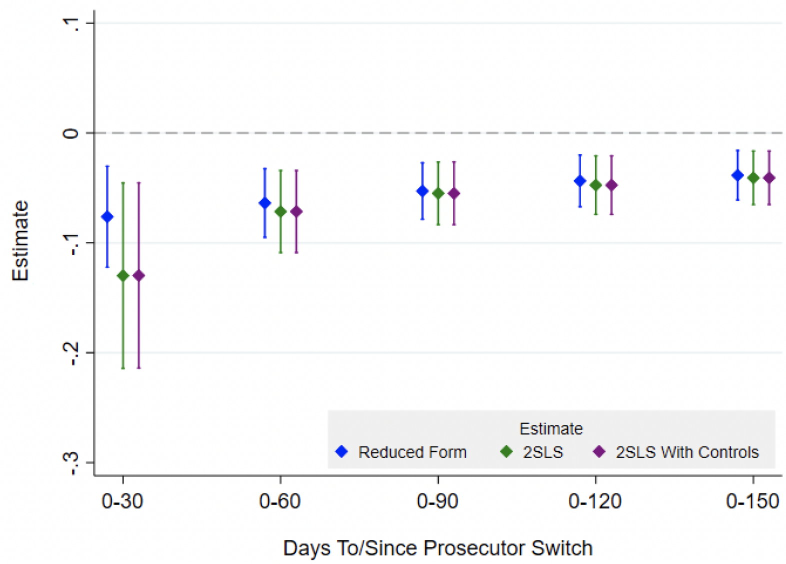
Table 3: Impact of the Targeted Program on Victim Death by Homicide

	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)
Observations	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. See Appendix A1 for a precise definition of domestic arrests. 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. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$

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 arrest 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 period. 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 2SLS 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.[26]

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}[27]

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

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 $1 - \pi_a - \pi_n = Pr[Included \,|\, Z_{min} < Z < Z_{max}]$. 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 TAC. We do this in three steps. First, we restrict attention just to cases excluded from TAC,

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

[27] 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.

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 TAC: $E(Y_i \mid Excluded, Z = Z_{max})$. 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:

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

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

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

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.[28] 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

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

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 & Dal 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 A3 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 Additional Checks

In this section, we describe three additional checks beyond the falsification test (Panel (b) of Figure 4) and estimates based on narrowing in on the prosecutor switch date (Figure 5).

Inference. In a recent paper, Lee *et al.* (2022) propose using tF critical values instead of t-ratio tests for IV, as the latter significantly over-reject in situations when instruments are not sufficiently strong (i.e., the first-stage F-statistic < 104.67). Table A5 shows that our conclusions are robust to using tF critical values, generated using the Stata package *tf* provided by the authors. Given the strength of our first stage (F-statistic ≈ 46), the 5% tF critical value is 2.17.

Clustering. Heretofore, standard errors have been two-way clustered at the victim and district-year level. While the need to cluster at the victim level is unambiguous – individuals can be victimized several times – the choice of unit for the second level of clustering is more subjective. In Table A6, we show that clustering standard errors at the victim and prosecutor level instead of the victim and district-year level does not meaningfully impact the precision of our results.

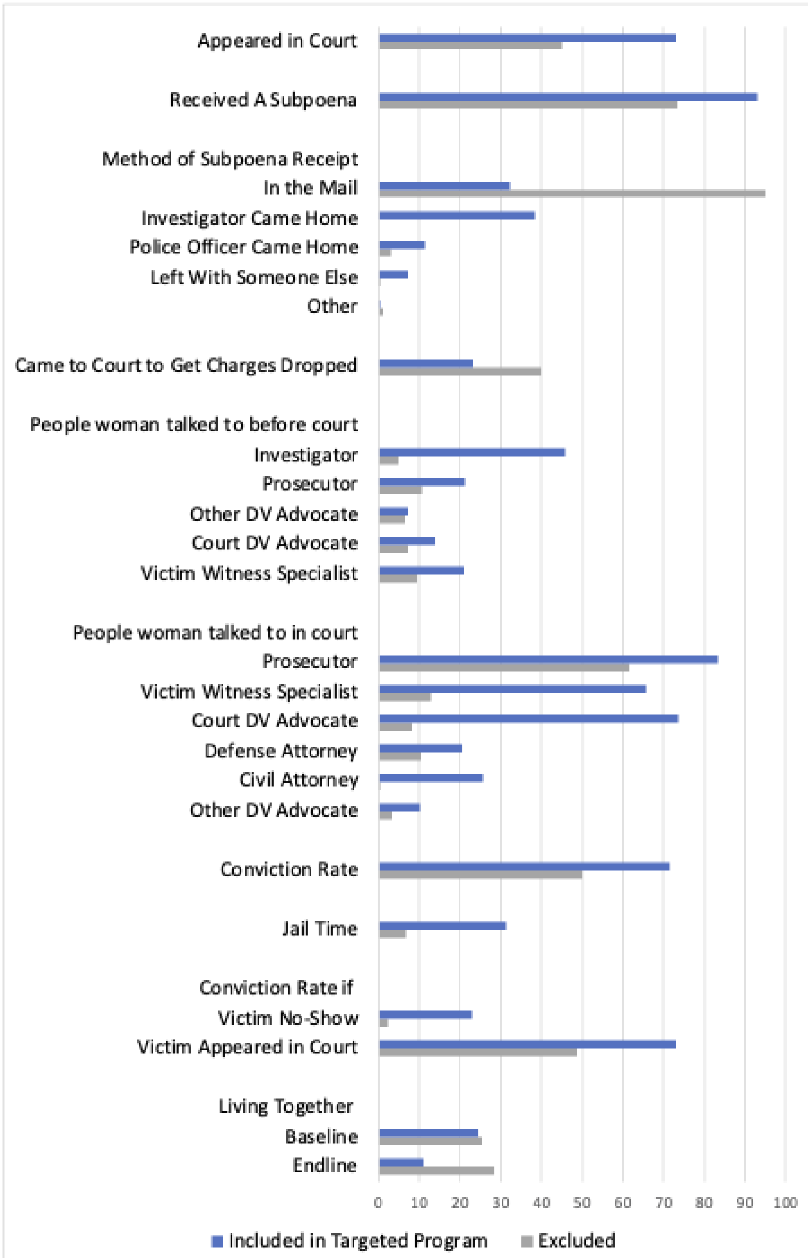
Linkage Errors. Our outcome of interest (homicide victimization) is linked to CPD victimization records using probabilistic record linkage based on fields such as victim name and date of birth, described in detail in Section A1.9. One concern given the rarity of homicide is that our results may be particularly sensitive to linkage errors. While this concern is partly assuaged by the falsification test presented in Figure 4, we show in Figure A4 that our estimates are actually closest to zero in situations where the error rate of the linking algorithm (= false positive rate + false negative rate) is highest. We relegate the details of the construction of this figure to Appendix Sections A1.10 and A1.11.

4.3 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).

Salience of and Engagement in Court Processes. To capture the perspectives of SAO's non-profit program partners, we interviewed representatives from Life

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.

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 cases are dropped (i.e., criminal charges are dismissed), 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. TAC victims are more likely to appear in court, and less likely to come to court to get criminal charges dropped. These differences indicate that, on average, defendants in TAC cases are exposed to longer criminal court proceedings, which may increase their perceived cost of re-offending.

Criminal Prosecution. A central feature of TAC 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 – irrespective of whether the victim shows up for scheduled court appearances – for TAC cases.

Incapacitation. One implication of the 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 for this period. 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.[29] 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 access to prison admissions data between

[29]Recent research shows that short-term incapacitation can reduce the risk of re-victimization in DV cases – Amaral *et al.* (2023) show that arrests in DV cases can virtually eliminate the elevated risk that victims face in the 48 hours following a DV incident. Crucially, this short-term reduction is not offset by an increase in long-term violence, and is actually accompanied by a *decrease* in offending over the year following the initial incident.

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.

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.

4.4 Cost-Benefit Analysis.

Using our most conservative estimate – a reduction in the 1-year homicide rate by 4 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 an additional 0.04 * 276 = 11 homicides per year.[30] 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). [31]

5 Identification of High-Risk Victims

In this section, we evaluate whether criminal justice system actors can accurately assess homicide risk for DV victims, and whether these assessments can be improved upon by modern data-driven methods. To do this, we compare the implicit risk predictions made by prosecutors with those made by a machine learning (ML) algorithm. This is a meaningful test because ML has been shown to provide *more* accurate forecasts of future criminal justice outcomes than humans in several contexts (Chalfin *et al.*, 2016; Kleinberg *et al.*, 2018; Grogger *et al.*, 2021).

We present evidence that prosecutors are *better* at selecting high-risk cases for the targeted program than the algorithm. We do this by training an ML algorithm

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

[31] See https://www.chicagotribune.com/ for reporting by the Chicago Tribune about the $650,000 annual cost of the program in 2003, equivalent to $923,000 in 2020 USD.

on all DV cases *not* selected for the targeted intervention; this ensures that the intervention's (beneficial) impact does not contaminate the estimate of homicide risk. The ideal comparison to the ML selections would be cases identified by prosecutors for inclusion, but who do not end up in the targeted program. As this is not available to us, we use estimates of our complier mean (i.e., homicide risk for victims at the margin of inclusion into TAC). This group represents individuals who would have been selected for the targeted program but happened to be assigned to a prosecutor with a relatively higher threshold for inclusion.

We find that although the algorithm has some ability to forecast homicide victimization, the riskiest cases identified by the algorithm face – on average across follow-up periods of 1, 5, and 9 years – 83% less risk than cases at the margin of inclusion into TAC. Put differently, if the number of cases that could be included in the targeted intervention was increased, our results suggest that prosecutors would allocate those slots to individuals at substantially higher risk than those that the algorithm would select. One explanation for the proficiency gap between prosecutors and ML tools is that TAC prosecutors operate in an information-rich environment and are able to incorporate both quantitative (e.g., decades-long arrest and conviction histories for the defendant) and qualitative (e.g., case narratives, in-person exchanges with the victim on current and prior cases, etc) information beyond what is available in police data to make their selections (effectively).

Implementation. We start with a sample of 150,174 unique victim-arrestee pairs involved in all DV arrests between 2000 and 2013.[32] This sample excludes those that are included in the targeted intervention to ensure that our estimates of homicide

[32]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.

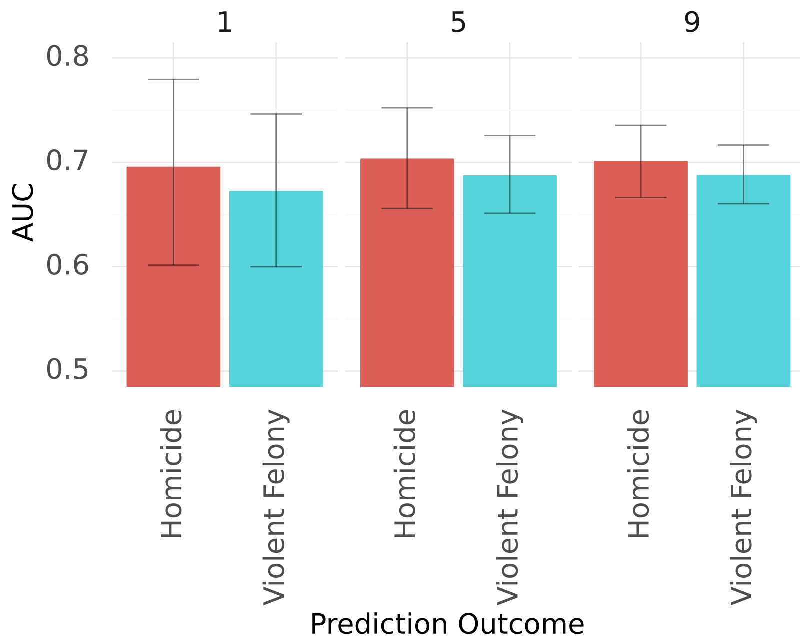
risk are not confounded by programmatic effects. Next, we generate out-of-sample predictions for all victims using a gradient boosting model (Ke *et al.*, 2017) to predict future homicide victimization. We split the 114,844 unique arrestees into five mutually exclusive folds to limit the chance that 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 homicide risk, we create 199 predictive features based on counts of prior victimizations for the victim, counts of prior arrests for the arrestee, demographic attributes for the victim, demographic attributes for the arrestee, and facts about the current case.[33] As ML algorithms can have difficulty in predicting outcomes with very low base rates King & Zeng (2001); He & Garcia (2009), we also train a proxy predictor for homicide using a related outcome – violent felony victimization (base rate = 14%) – that is more prevalent than the true outcome of interest (homicide victimization – base rate = 0.3%).

Performance. Figure 7 shows the performance of the two 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 chosen case which results in future homicide higher than a randomly chosen case which does not result in future homicide. All models have significantly higher AUCs higher than 0.5 (meaning they are better than random chance), though we see that the homicide predictor tends to perform better than the violent felony predictor across all follow-up years.[34]

[33]The counts are computed for the last 30, 60, 90, 180, 365, 730, and 1,460 days.
[34]Bootstrapped 95% confidence intervals were computed using 1,000 samples.

Figure 7: Predictive Performance by Training Outcome



Notes: This figure depicts a measure of the predictability of homicide victimization – Area Under the Curve – for two training outcomes (homicide, violent felony victimization) and three follow-up periods (1, 5, 9 years). Bootstrapped 95% confidence intervals were generated using 1,000 replicates. Data Sources: Chicago Police Department, Cook County State's Attorney's Office, Cook County Medical Examiner's Office.

Figure 7 shows that homicide can be predicted from police data with some accuracy, but does not tell us whether this accuracy exceeds that of prosecutors. One way we can answer this question 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

Table 4: Comparing Prosecutor and ML Selections

Follow-Up (1)	Outcome Rate (2)	Complier Mean (3)	Realized Risk (4)	Predicted Risk (5)
1	0.0005	0.0444	0.0006	0.0011
5	0.0018	0.0532	0.0130	0.0041
9	0.0030	0.0733	0.0213	0.0060

Notes: This table compares homicide risk for cases selected by specialized prosecutors and a machine learning algorithm. 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 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.

program is targeted.

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 gradient boosting models are trained on and make out-of-sample 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 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 follow-up) to 7.33% (9 year follow-up). Columns (3) and (4) reflect actual and predicted homicide victimization rates 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 trained on police data are unlikely to improve beneficiary selection.

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 programs that focus on high-risk survivors of DV and encourage collaboration across criminal justice agencies and service providers. Yet, little is known about whether they are an effective way to enhance safety, especially for 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 criminal prosecution 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 into the program, indicating that jurisdictions intent on reducing the escalation of DV should consider establishing or expanding these kinds of interventions.

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 and Methods

This section provides additional details about the raw CPD, SAO, CCMEO, and IDOC data, the identification of domestic incidents and arrests, and the record linkage algorithm used to infer 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).[35]

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

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)
    - 584 (Violation of Stalking No Contact Order)
    - 4386 (Violation of Civil No Contact Order)

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

- 4387 (Violate Order of Protection)
- 4388 (Violation of Bail Bond - Domestic Violence)
- 4750 (Disclose Domestic Violence Victim Location)

A1.2 CPD Victimization Records

This dataset also spans January 1999 - August 2019, and contains 5,911,192 victimization 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 victimization history, we use each victim's identifying information (name, home address, date of birth, and age) and 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. Uniform Crime Reporting (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 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 following conditions:

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. Domestic Arrest Reports: The charge description field contains any of the tokens "Dom", "Vio", "Order", or "Protection".[36] Table A1 lists all of the charge descriptions that meet this criterion.

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.
- 26,133 arrests that did not occur within 150 days of a prosecutor switch

[36]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".

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

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

A1.6 SAO Records

The Domestic Violence Division within the Cook County State's Attorney's Office shared information on

- 34 prosecutors and their tenure on the targeted intervention program between 1999 and 2013, as well as which courtrooms[37] they were assigned to. This information

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

was de-identified and shared with the research team, and is used as the basis of the research design.

- 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 were 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.[38] For each incarcerated individual, these files include an IDOC ID, name, date of birth, 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 unique record and person identifiers, CPD victimization, CCMEO death records, and IDOC incarceration records do not. As such, we use a probabilistic record linkage algorithm called *Name Match* to identify unique

[38] The first dataset in this series lists all individuals in custody on June 30$^{\text{th}}$ 2005. The next file is dated June 30$^{\text{th}}$ 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.

individuals across the arrest, victimization, prosecution, death record, and incarceration datasets. For full details on the algorithm itself, see McNeill & Jelveh (2021).

In our setting, a large share of victimization records include age but not precise date of birth information; the latter is an important determinant of whether two records are predicted to be associated with the same person. We first describe the key aspects of the linking algorithm and then describe changes we made to reduce the chance of false positive links:

1. **Blocking**: Since it is computationally infeasible to compare every record to each other record to estimate link probability, Name Match employs a filtering step, commonly referred to as blocking, to remove pairs of records that are unlikely to be matches. *Name Match*'s blocking step relies on information about a person's first name, last name, date of birth, and age.

2. **Pair-Level Prediction**: For the set of record pairs that passed the blocking step, a supervised machine learning algorithm is run to estimate the probability the two records in a pair refer to the same person. In this step, we take the post-2010 IR numbers (the fingerprint-based person identifier associated with arrest records) as ground truth for training a random forest algorithm.[39] The features that the algorithm is trained on include various distance measures computed between a record pair's first name, last name, birth date, record date, age, address, sex, race, and ethnicity fields. In order to avoid imputing date-of-birth related distances for record pairs where at least one record has missing date of birth, we run two separate random forest models: One for record pairs with date-of-birth information, and one for record pairs with missing date of birth for at least one record in a pair.[40] After the random forest models are trained, they then generate predictions for all record pairs. A holdout

[39]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.

[40]Since all arrest records with post-2010 IR numbers include date of birth information, we censor date of birth in those record pairs when we train the no-date-of-birth model.

sample of CPD arrest records are used to identify the probability threshold above which a record pair is considered a predicted link. A separate threshold is determined for the date-of-birth and missing date-of-birth models.[41]

3. **Clustering**: To identify all records that refer to the same person, the output of the previous step is treated as a network graph where the nodes are records and the edges are predicted links, i.e., those record pairs with match probability above the threshold calculated in the previous step.[42] A clustering algorithm, which is a modified version of depth-first search, is run on the network graph which outputs *clusters*, or the set of records that have been predicted to refer to the same person. This step works as follows: consider two record pairs that are predicted matches: $\{A, B\}$ with match probability 0.99 and $\{A, C\}$ with match probability 0.90. The record pairs are first sorted from highest to lowest match probability. The clustering algorithm first considers $\{A, B\}$ and determines that the two records refer to the same person. The algorithm then considers $\{A, C\}$ and determines that C belongs to the cluster that already contains A and B. In other words, the set of records $\{A, B, C\}$ are determined to refer to the same person.

We allow domain knowledge to influence cluster formation, particularly in order to avoid false positive links that may be generated from records with no date-of-birth information. To do so, when *Name Match* considers whether two nodes belong to the same cluster, the algorithm first ensures the link does not violate any user-defined edge-level constraints. This paper employs two edge-level constraints:

1. If one of the records in the pair is a death record, the death date on that record must come after the event date on the other record. This prevents links from forming where, for example, a person has an arrest after they have died.

[41] The chosen thresholds maximize the F_1 score, or the harmonic mean of precision and recall calculated on heldout record pairs. For the model with date-of-birth information, the chosen threshold was 0.70 while for the missing date-of-birth model the threshold was 0.69.

[42] We use IR numbers rather than predictions when deciding to link two post-2010 arrest records.

2. If one of the records does not have date-of-birth information, then the difference in recorded ages between the two records must be less than three years.

Next, the clustering algorithm considers cluster-level constraints. This paper employs three cluster-level constraints:

1. Only one death record is allowed per cluster. For example, if a cluster already contains a death record, and a currently considered edge also contains a new death record, the cluster-level constraint would prevent the new death record from joining the cluster
2. A cluster is constrained to have at most one post-2010 IR number
3. 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.[43] The purpose of this constraint is to prevent clusters from forming with wide variability in known dates of birth, which is a side effect of including records without date of birth in the matching process.

A1.10 The Impact of Linkage Errors

Since our outcome is measured via linking to death records, we devote this section to the impact of linkage errors on our IV estimates.[44] We assume that linking error is independent of the instrument; Appendix Table A7 shows support for this assumption.[45] The IV estimator in our setting with a continuous treatment is:

$$\tau_{IV} = \frac{\sum_i (y_i^* - \bar{y})(z_i - \bar{z})}{\sum_i (T_i - \bar{T})(z_i - \bar{z})}$$

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

[44] See Tahamont *et al.* (2021) for a discussion of the impact of linking errors on estimates generated in randomized controlled trials with perfect compliance.

[45] Specifically, we reproduce the balance table (Table 2) but include an indicator for whether a victim cluster contains any records which do not contain date-of-birth information. The joint F-statistic is 1.47 (p-value = 0.11).

where y^* is the true value of the outcome, T is whether a case was selected for TAC, and z is a prosecutor's selection rate. Since linkage error only impacts measurement of the outcome, we focus on the numerator, which simplifies to $\sum_i y_i^*(z_i - \bar{z})$. Under matching error, we observe $y_i = 1$ when $y^* = 0$ (a false positive link) or $y_i = 0$ when $y^* = 1$ (a false negative link). For simplicity, we assume that the instrument has been demeaned so that $\bar{z} = 0$ and refer to the demeaned instrument as $\tilde{z}$. With matching error, the estimated value of the numerator is:

$$\sum_i y_i \tilde{z}_i = \sum_{\{j|y_j^*=1\}} P(y_j = 1|y_j^* = 1)\ \tilde{z}_j + \sum_{\{k|y_k^*=0\}} P(y_k = 1|y_k^* = 0)\ \tilde{z}_k$$

where $P(y_j = 1|y_j^* = 1)$ is the probability of a true positive link for record j and $P(y_k = 1|y_k^* = 0)$ is the probability of a false positive link for record k.

$$\sum_i y_i \tilde{z}_i = P(y_j = 1|y_j^* = 1)\ \tilde{Z}_1 + P(y_k = 1|y_k^* = 0)\ \tilde{Z}_0$$

where $\tilde{Z}_m = \sum_{\{n|y_n^*=m\}}\ \tilde{z}_n$. Using $\tilde{Z}_0 + \tilde{Z}_1 = 0$

$$\sum_i y_i \tilde{z}_i = \left[P(y_j = 1|y_j^* = 1) - P(y_k = 1|y_k^* = 0)\right]\ \tilde{Z}_1$$

Using $P(y_j = 1|y_j^* = 1) + P(y_j = 0|y_j^* = 1) = 1$

$$\sum_i y_i \tilde{z}_i = \left[1 - P(y_j = 0|y_j^* = 1) - P(y_k = 1|y_k^* = 0)\right]\ \tilde{Z}_1 = [1 - FNR - FPR]\ \tilde{Z}_1$$

i.e., the numerator under linking error is related to the true value of the numerator through the attenuation term $\Big[1 - P(y_j = 0|y_j^* = 1) - P(y_k = 1|y_k^* = 0)\Big]$, or [1 - the False Negative Rate (TPR) - False Positive Rate (FPR)] of the linking algorithm.

A1.11 Robustness to Linkage Error

To understand the impact of errors on our estimates, we vary the linking constraints and create multiple measures of the FNR and FPR. Figure A4 shows how the IV estimate, FNR, and FPR vary under 24 distinct scenarios. The main takeaway is that impact estimates are closest to zero when the total error rate (= FNR + FPR) is highest.

We vary two parameters that influence whether a record pair is predicted to be a link:

1. In the prediction step, we use four distinct thresholds used to classify a record pair a probable match – 0.70, 0.80, 0.90, and 0.99.[46] The higher the threshold, the higher the likelihood of false negative links and the lower the likelihood of false positive links (see panels (b) and (c) of Figure A4).
2. In the clustering step, when one of the records does not have date-of-birth information, we vary the maximum difference in recorded ages between two records from 0 to 5. The lower the maximum age difference, the higher the chance of a false negative link and the lower the chance of a false positive link (see panels (b) and (c) of Figure A4).

This creates 4 * 6 = 24 different datasets, each of which generates a distinct IV estimate.

To estimate error rates, we run a related experiment on CPD arrest data (which contains "ground-truth" information – i.e., fingerprint-based identifiers or IR numbers – unlike the death or victimization records). We start with 884,115 post-2010 arrest records with non-null IR numbers. We create a dataset of pseudo-death records by sampling 100,000 arrest IDs and then dropping duplicate IRs, leaving us with 78,217 pseudo-death records. With the 784,115 records remaining, we create pseudo-victimization records by censoring the date of birth information for 600,000 of these records. We additionally drop 152,641 pseudo-victimization records where the victimization occurred after the associated person's pseudo-death record. We then run *Name Match* on this data, varying the threshold and maximum age difference

[46] We use the same thresholds for records with and without date-of-birth information.

as above. We estimate false positive and false negative errors rates for each specification, broken out by record pairs where both records have date-of-birth information and by record pairs where at least one record is missing date-of-birth information. Specifically,

- The number of true positive matches is computed as the count of pseudo-victimization records whose IR numbers are also in the pseudo-death dataset *and* where the associated records in the two datasets have been assigned to the same cluster.
- The number of false negative matches is computed as the count of pseudo-victimization records whose IR numbers are also in the pseudo-death dataset *but* where the associated records were not assigned to the same cluster.
- The number of true negative matches is computed as the count of pseudo-victimization records whose IR numbers are not in the pseudo-death dataset *and* where the cluster assigned to the psuedo-victimization record is not in the psuedo-death dataset.
- The number of false positive matches is computed as the count of pseudo-victimization records whose IR numbers are not in the pseudo-death dataset *but* the associated cluster for the pseudo-victimization records also exists in the pseudo-death dataset.

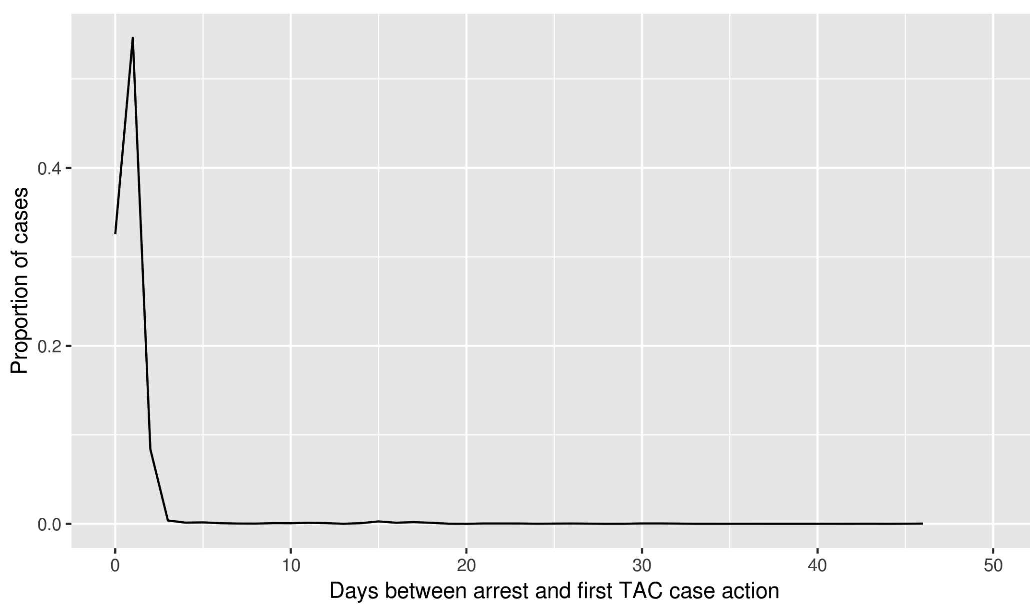
Note that these metrics are computed out of sample since *Name Match* is blinded to the IR number for the pseudo-death records.

The results of these related experiments are shown in Figure A4. Panel (a) shows the impact on one-year homicide victimization for the 24 datasets created by the first experiment. We see that varying the maximum age difference parameter does not have much impact on the estimates, however, the strictest threshold of 0.99 results in an estimate that is noticeably smaller than (though not statistically distinguishable from) that at other thresholds.

Panels (b) and (c) in Figure A4 plot error rates computed from the second experiment. Panel (b) shows the FPR, computed as the number of false positives divided by the number of pseudo-victimization records that do not have an IR number in the psuedo-homicide records. While we see that the FPR increases as we loosen the threshold for considering a record pair to be a predicted match, the range of values is very small – from slightly less than 0.0005 to 0.0015. Panel (c) shows the FNR, computed as the number of pseudo-victimization records with an IR number in the pseudo-death records, but where the records were not assigned to the same cluster. We see that the FNRs are higher than the FPRs, especially for the strictest threshold of 0.99. We additionally see that the FNR is higher for records without date-of-birth information than for records with date-of-birth information. This difference is due to the conservative rules discussed above for preventing false positive links for records without date-of-birth information. Overall we see that the estimated treatment effect is smallest when the FNR is highest, which is consistent with matching error that is uncorrelated with the instrument attenuating the size of the treatment effect.

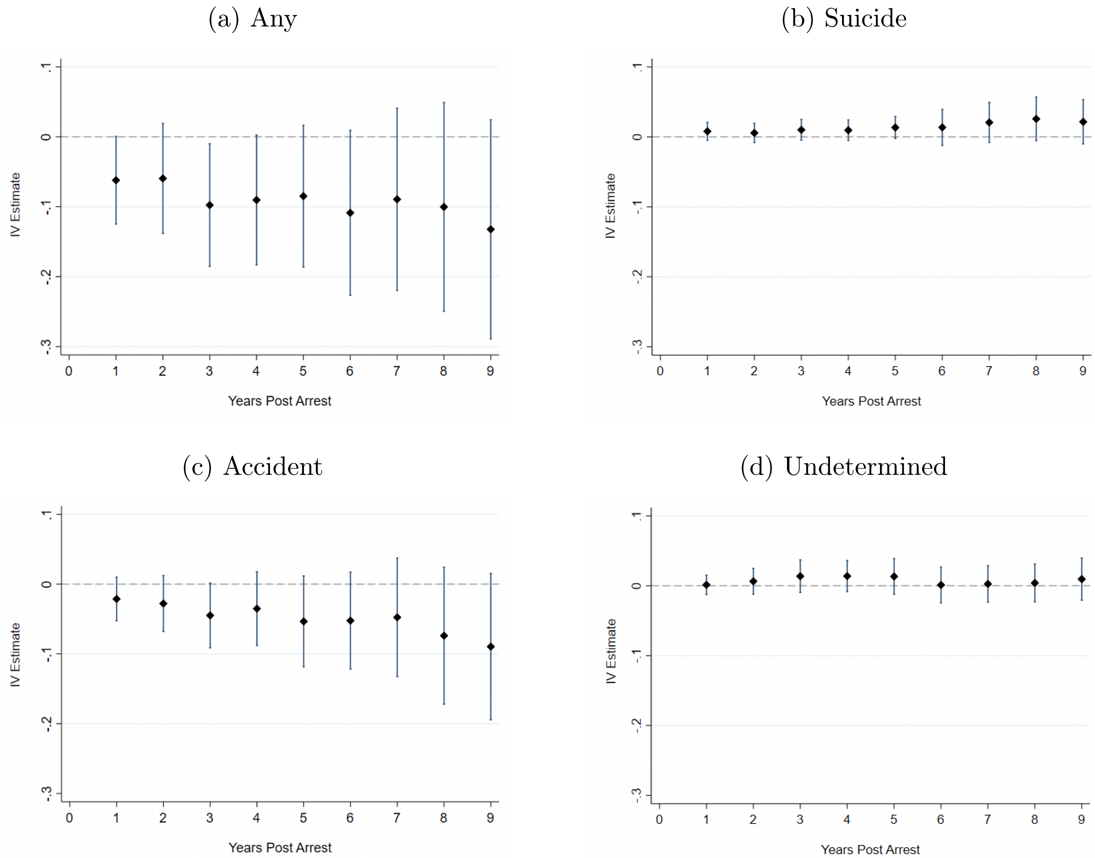
A2 Figures

Figure A1: 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.

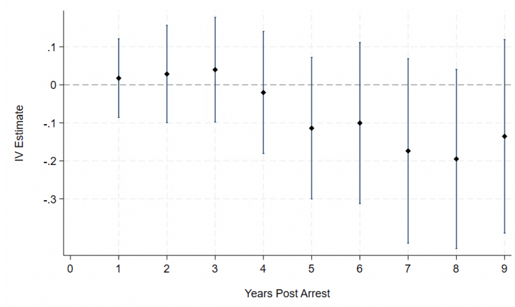
Figure A2: Impact on Victim Death by Cause



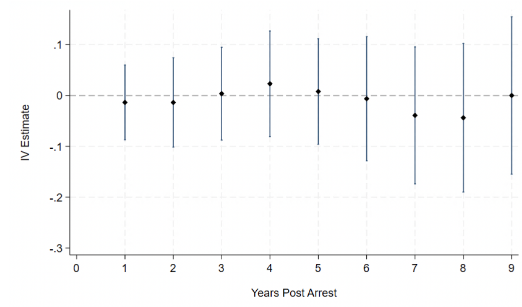
Notes: These figures display 2SLS point estimates and 95% confidence intervals of the impact of the targeted intervention on victim death by cause. Standard errors are two-way clustered at the individual and district-year level. Data Sources: Chicago Police Department, Cook County State's Attorney's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

Figure A3: Impact on Defendant Death by Cause

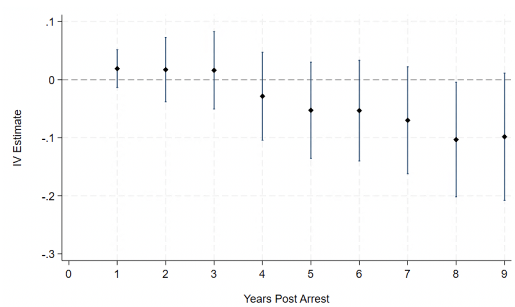
(a) Any



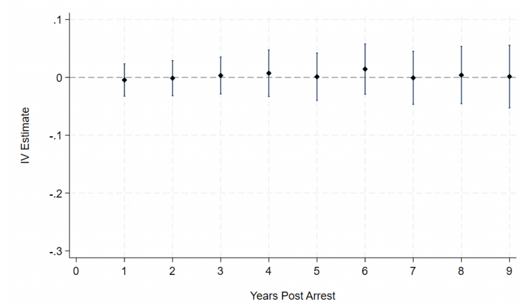
(b) Homicide



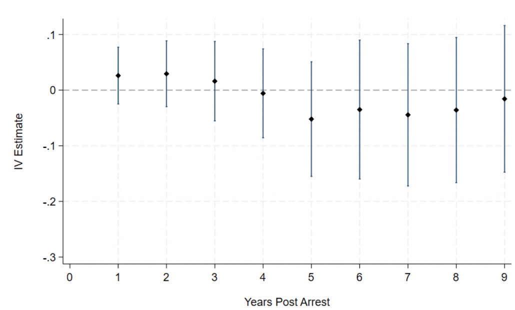
(c) Natural Causes



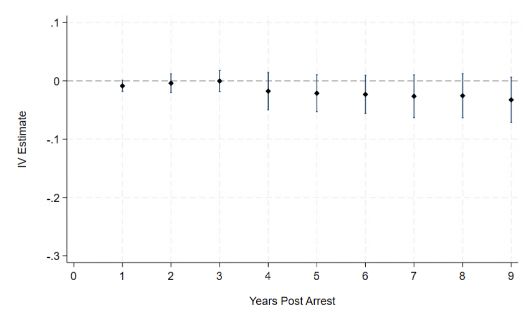
(d) Suicide



(e) Accident



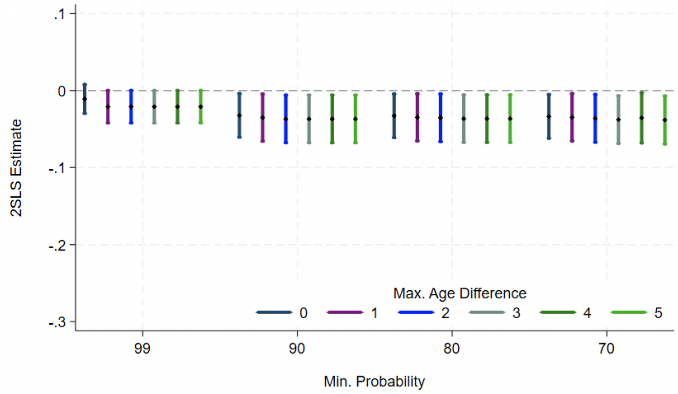
(f) Undetermined



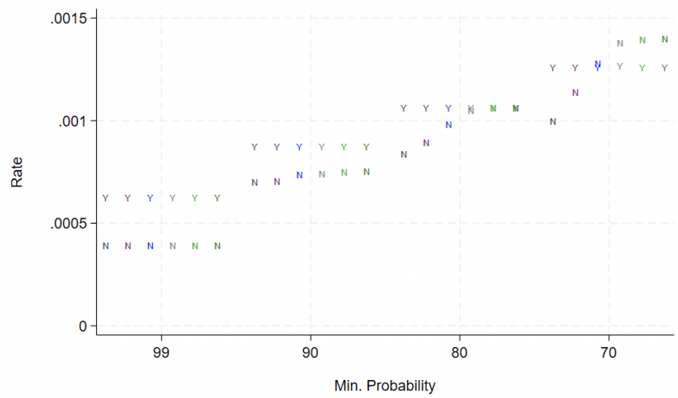
Notes: These figures display 2SLS point estimates and 95% confidence intervals of the impact of the targeted intervention on defendant death by cause. Standard errors are two-way clustered at the individual and district-year level. Data Sources: Chicago Police Department, Cook County State's Attorney's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

Figure A4: Varying Match Quality

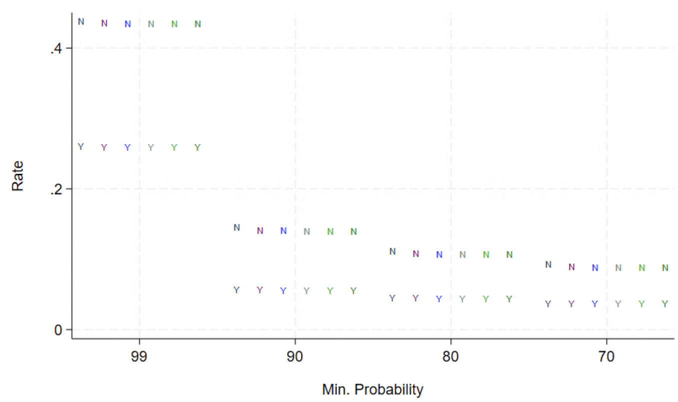
(a) Impact on Homicide Victimization within 1 Year



(b) False Positive Rates



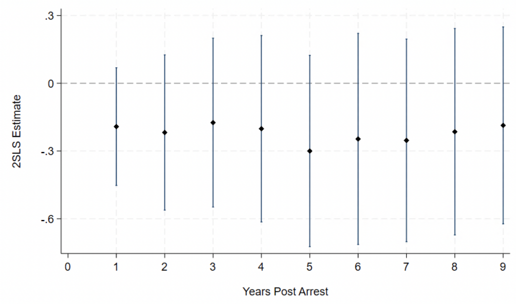
(c) False Negative Rates



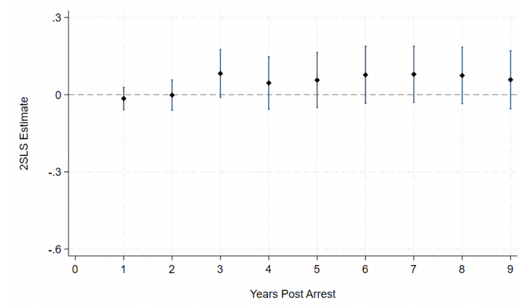
Notes: These figures show how impact estimates and error rates vary as we move from stringent matching criteria (maximum age difference in years across records = 0 and the probability that two records are the same ≥ 99%) to more lax criteria (0 and 70% respectively). We plot error rates separately for records with date of birth ("Y") and without ("N"). The situations in which impact estimates are closest to zero are those where the error rate (= false positive rate + false negative rate) is highest. 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

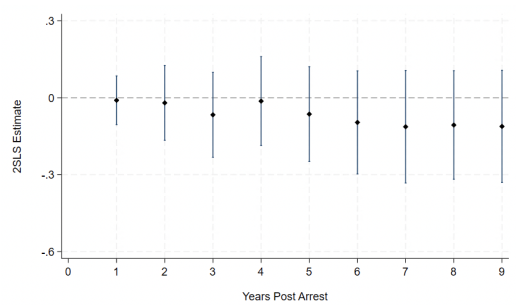
(a) Any Offense



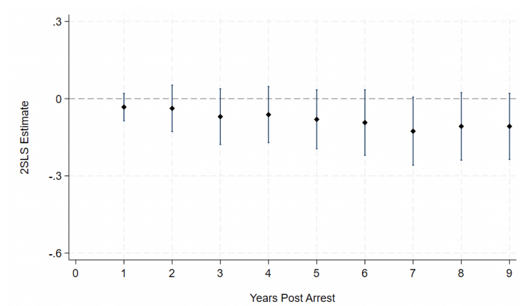
(b) Homicide



(c) Battery



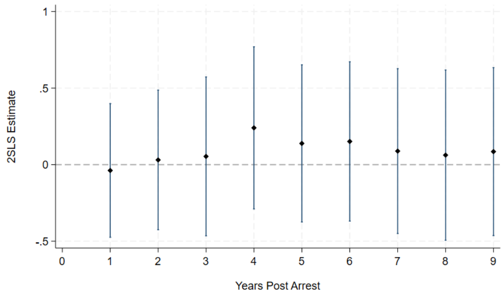
(d) Domestic



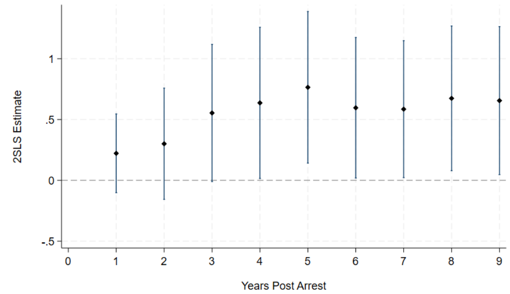
Notes: These figures display 2SLS point estimates and 95% confidence intervals of the impact of the targeted intervention on defendant incarceration by offense type. Standard errors are two-way clustered at the individual and district-year level. Data Sources: Chicago Police Department, Cook County State's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

Figure A6: Impact on Victimization Reported to the Police

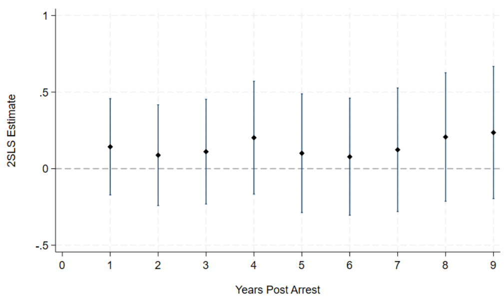
(a) Violent Offenses



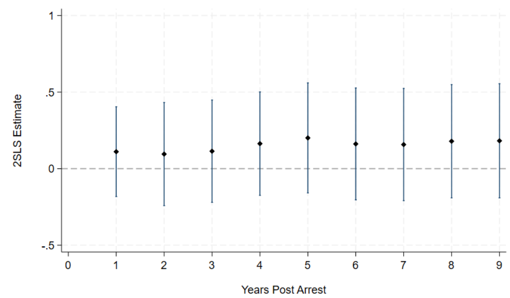
(b) Property Offenses



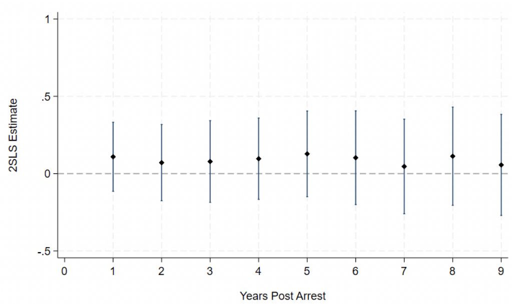
(c) Domestic Offenses



(d) Offenses by Same Defendant



(e) Protection Order Violations



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.

A3 Tables

DA-LE: Danger Assessment for Law Enforcement[47]

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
	17th 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
MT	LAP	2016	✓			✓	✓(high-risk)	
NV	Clark County DV HRT	2022	✓	✓		✓	✓(high-risk)	County

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

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	✓	✓	✓	✓	✓[48]	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			✓[49]		✓(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

[48] 2+ pending DV cases; 1 pending DV case and 1 pending matrimonial case

[49] 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	2.1653***	0.6999***
	(0.4397)	(0.1340)
Observations	16,607	79,968
F-Statistic (Instrument)	24.2474	27.2653
	Age $\leq$ Median	Age $>$ Median
Prosecutor's Inclusion Rate	0.8898***	1.0189***
	(0.1849)	(0.2048)
Observations	50,346	46,229
F-Statistic (Instrument)	23.1534	24.7542
	Female	Other
Prosecutor's Inclusion Rate	1.0414***	0.5701***
	(0.1641)	(0.1740)
Observations	75,731	20,844
F-Statistic (Instrument)	40.2666	10.7348
	Black	Non-Black
Prosecutor's Inclusion Rate	0.9807***	0.8753***
	(0.1712)	(0.1988)
Observations	62,093	34,482
F-Statistic (Instrument)	32.8187	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. See Appendix A1 for a precise definition of domestic victimization. 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	2.4102***	0.5219***
	(0.3975)	(0.1305)
Observations	20,996	75,579
F-Statistic (Instrument)	36.7563	16.0045
	Age $\leq$ Median	Age $>$ Median
Prosecutor's Inclusion Rate	0.8649***	1.1238***
	(0.2026)	(0.2523)
Observations	50,629	45,946
F-Statistic (Instrument)	18.2215	19.8324
	Male	Other
Prosecutor's Inclusion Rate	1.0723***	0.5923***
	(0.1588)	(0.1681)
Observations	80,787	15,788
F-Statistic (Instrument)	45.5957	12.4097
	Black	Non-Black
Prosecutor's Inclusion Rate	0.8918***	1.0328***
	(0.1779)	(0.2190)
Observations	65,181	31,393
F-Statistic (Instrument)	25.1279	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. See Appendix A1 for a precise definition of domestic arrests. 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. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$

Table A5: Impact of the Targeted Program on Victim Death by Homicide
Lee *et al.* (2022) 5% tF Standard Errors

	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**
	(0.0163)	(0.0201)	(0.0296)
	2SLS Estimates with Controls		
Targeted Program	–0.0401**	–0.0501**	–0.0658**
	(0.0157)	(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. See Appendix A1 for a precise definition of domestic incidents. 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. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$

Table A6: Impact of the Targeted Program on Victim Death by Homicide
Standard Errors Clustered at the Victim and Prosecutor Level

	1 year	5 years	9 years
	Reduced Form		
Prosecutor Inclusion Rate	–0.0384**	–0.0487**	–0.0634**
95% C.I.	[−0.0707,−0.0146]	[−0.0865,−0.0164]	[−0.1202,−0.0027]
p-value	0.0100	0.0141	0.0401
	2SLS Estimates		
Targeted Program	–0.0408**	–0.0517**	–0.0673**
95% C.I.	[−0.0815,−0.0111]	[−0.1090,−0.0078]	[−0.1343,−0.0070]
p-value	0.0170	0.0290	0.0320
	2SLS Estimates with Controls		
Targeted Program	–0.0401**	–0.0501**	–0.0658**
95% C.I.	[−0.0795,−0.0119]	[−0.1017,−0.0063]	[−0.1279,−0.0092]
p-value	0.0150	0.0290	0.0300
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. See Appendix A1 for a precise definition of domestic incidents. Standard errors are two-way clustered at the individual and prosecutor level. Since the number of prosecutors is small (n = 26), wild bootstrap confidence intervals and p-values are generated via the Stata command *boottest*. 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. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$

Table A7: Testing for the Random Assignment of Cases to Prosecutors

Characteristic	Mean (1)	Pr(Included) (2)	Prosecutor Inclusion Rate (3)
Victim			
Prior Victimization	0.1720	0.0492***	0.0000
	(0.3773)	(0.0035)	(0.0001)
Prior Aggravated Victimization	0.0210	0.0240**	-0.0002
	(0.1433)	(0.0098)	(0.0002)
Age	34.5167	-0.0001	-0.0000
	(12.7721)	(0.0001)	(0.0000)
Male	0.2114	-0.0291***	-0.0001**
	(0.4083)	(0.0018)	(0.0001)
Black	0.6430	0.0076*	0.0000
	(0.4791)	(0.0040)	(0.0001)
White-Hispanic	0.2023	-0.0040	-0.0000
	(0.4017)	(0.0038)	(0.0001)
Black-Hispanic	0.0042	-0.0066	0.0000
	(0.0645)	(0.0127)	(0.0005)
Asian/Pacific Islander	0.0081	-0.0048	-0.0002
	(0.0896)	(0.0085)	(0.0004)
Missing Date of Birth	0.1759	-0.0121***	0.0001
	(0.3807)	(0.0019)	(0.0001)
Arrestee			
Prior Domestic Arrest	0.2174	0.0942***	0.0000
	(0.4125)	(0.0031)	(0.0001)
Age	32.8850	0.0019***	-0.0000
	(10.7407)	(0.0001)	(0.0000)
Male	0.8365	0.0327***	-0.0002***
	(0.3698)	(0.0017)	(0.0001)
Black	0.6749	0.0056	-0.0001
	(0.4684)	(0.0041)	(0.0001)
White-Hispanic	0.2004	-0.0007	0.0001
	(0.4003)	(0.0044)	(0.0001)
Black-Hispanic	0.0072	0.0312***	-0.0001
	(0.0843)	(0.0113)	(0.0003)
Asian/Pacific Islander	0.0067	-0.0099	-0.0004
	(0.0818)	(0.0088)	(0.0003)
Victim, Arrestee Live Together	0.4612	-0.0008	0.0000
	(0.4985)	(0.0018)	(0.0001)
Observations	96,575	96,575	96,575
F-statistic		97.1677	1.4677
p-value		0.0000	0.1065
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. See Appendix A1 for precise definitions of domestic and aggravated domestic arrest/victimization. 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.