Machine Learning Can Predict Shooting Victimization Well Enough to Help Prevent It

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Abstract

This paper shows that shootings are predictable enough to be preventable. Using arrest and victimization records for almost 644,000 people from the Chicago Police Department, we train a machine learning model to predict the risk of being shot in the next 18 months. Out-of-sample accuracy is strikingly high: of the 500 people with the highest predicted risk, almost 13 percent are shot within 18 months, a rate 128 times higher than the average Chicagoan. A central concern is that algorithms may "bake in" bias found in police data, overestimating risk for people likelier to interact with police conditional on their behavior. We show that Black male victims more often have enough police contact to generate predictions. But those predictions are not, on average, inflated; the demographic composition of predicted and actual shooting victims is almost identical. There are legal, ethical, and practical barriers to using these predictions to target law enforcement. But using them to target social services could have enormous preventive benefits: predictive accuracy among the top 500 people justifies spending up to $134,400 per person for an intervention that could cut the probability of being shot by half.

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1 Introduction

Gun violence in the U.S. causes widespread harm—to its direct victims and to the children, families, and communities surrounding them (Sharkey, 2018)—generating social costs on the order of $100 billion annually (Cook and Ludwig, 2000). Because addressing this problem with aggressive policing can generate its own significant social costs (e.g., Ang, 2021; Geller et al., 2014; Jones, 2014; Chalfin et al., 2022b), local policymakers are spending millions of dollars to prevent gun violence with social services rather than law enforcement.[1] How much these services can reduce shootings is shaped by how well program operators can anticipate participants' *ex ante* risk; even a very effective intervention will prevent few shootings if few participants would be victims or offenders in its absence. Unfortunately, we know little about whether a person's risk of future shooting involvement can be predicted accurately, a prerequisite for individually-targeted interventions to make a cost-effective difference.

In other settings, machine learning algorithms help solve this kind of prediction problem by forecasting future behavior accurately, consistently, and at scale (e.g., Obermeyer and Emanuel, 2016; Chouldechova et al., 2018; Kleinberg et al., 2018a; Hastings et al., 2020). But using algorithms to predict shootings, when most input data likely come from the criminal legal system, faces two key challenges. First, predicting outcomes as rare as shootings[2] has been a major challenge across many disciplines involving human behavior or other complex systems (Lo-Ciganic et al., 2019; Qi and Majda, 2020; Japkowicz, 2000; Martin et al., 2016; Salganik et al., 2020). Achieving adequate predictive performance may be particularly hard given the noise and distortions in crime data.[3]

[1] See, e.g., `https://www.chicago.gov/content/dam/city/sites/public-safety-and-violenc-reduction/pdfs/OurCityOurSafety.pdf`, `https://www.phila.gov/2021-04-14-how-the-city-is-addressing-gun-violence-2021-update-to-the-roadmap-to-safer-communities/`, `https://www.oaklandca.gov/topics/oaklands-ceasefire-strategy`, `https://monse.baltimorecity.gov/sites/default/files/MayorBMS_Draft_ViolenceReductionFrameworkPlan.pdf`, and `https://onse.dc.gov/service/people-promise`.

[2] Even in our setting of Chicago, where gun violence rates are high (though far from the highest among U.S. cities), shootings injure or kill about 0.1 percent of the population each year.

[3] Arrests or convictions of innocent people mean criminal legal records can overstate actual crim-

The second challenge is that algorithms trained on data from the criminal legal system may "bake in" that system's biases. For example, harsher treatment of non-White individuals, and especially of Black men, is well documented (e.g., Antonovics and Knight, 2009; Arnold et al., 2018; Eberhardt et al., 2004; Goncalves and Mello, 2021; Hoekstra and Sloan, 2022; Rehavi and Starr, 2014). If Black men are likelier to be arrested conditional on their behavior, then even "accurate" predictions of an outcome like shooting arrest may nevertheless overestimate their true risk of committing a shooting relative to individuals in other groups (Mayson, 2019; Starr, 2014). When such an algorithm is used to target legal interventions that curtail civil liberties, the burden of its false positive mistakes will be borne by the same groups who have historically been treated unfairly by the criminal legal system (e.g., Starr, 2014; Angwin et al., 2016; Lum and Isaac, 2016; Chouldechova, 2017; Kleinberg et al., 2017; Richardson et al., 2019; Mayson, 2019; Mehrabi et al., 2021). This concern recently led some mathematicians to abandon predictive policing because it is "simply too easy to create a 'scientific' veneer for racism."[4]

This paper demonstrates that even the biased information in police data can predict shootings with enough accuracy to save lives in the communities most affected by gun violence, and without distorting average risk across demographic groups. The key is to not predict shooting *arrest*, which may capture both shooting offending and potentially biased police decisions about whom to arrest. Training algorithms to predict an outcome that measures the behavior of interest differentially by group is one of the most consequential ways that bias can be "baked in" to predictions and lead to getting average group differences in risk dramatically wrong (Mullainathan and Obermeyer, 2021; Obermeyer et al., 2019). In the case of shootings, where we lack any unbiased measure of actual

inal behavior; low reporting rates for crimes like domestic violence mean not every offense is brought to the attention of law enforcement; and low clearance rates mean not everyone who commits a reported crime is arrested. And some cases are alleged to be outright data falsification; see, e.g., https://www.nydailynews.com/news/crime/fabricated-drug-charges-innocent-people-meet-arrest-quotas-detective-testifies-article-1.963021.

[4] https://www.popularmechanics.com/science/math/a32957375/mathematicians-boycott-predictive-policing/

offending, there is no way to compare predicted to actual offending, and so no way to assess either overall predictive performance or bias across groups.

Instead, we predict shooting *victimization*. Intervening with people at high risk of being victimized to keep them safe is a plausible alternative to intervening with potential offenders for reducing shootings (Cooper et al., 2006; Zun et al., 2006; Cheng et al., 2008; Green et al., 2017; Chalfin et al., 2022a). And as we argue below, shooting victimization is likely to be measured consistently across demographic groups in our setting. Theoretical work suggests that predicting this kind of well-measured outcome will recover accurate estimates of risk at the group level even if the predictors used to do so are measured differently across groups (Kleinberg et al., 2018b). If these predictions are accurate enough, then they could help to cost-effectively reduce gun violence, especially when paired with interventions that reduce victimization risk without imposing significant costs when mis-targeted (i.e., preventive services). But in practice, there is very little data about the predictability of shooting victimization, overall or by demographic group.

To fill this gap, we build a model to predict shooting victimization in Chicago over an 18-month period. The model uses arrest and victimization records for 643,914 people from the Chicago Police Department (CPD), including over 1,400 predictors that capture a person's demographic information, arrest and victimization histories, and the arrest and victimization histories of peers who were co-involved in prior criminal incidents.

We have two main sets of results. First, the model successfully identifies a small group of people at extraordinarily high risk of being shooting victims. Of the 500 people at highest predicted risk, almost 13 percent are actually shot during the following 18 months—a rate 18 times higher than everyone in our prediction sample of people with recent police contact (0.7 percent across 327,127 people) and 128 times higher than everyone in Chicago (0.1 percent). An intervention that could cut by half the risk of being shot for these 500 people would generate an estimated social cost savings of $67 million from the victimization reduction alone (Cook and Ludwig, 2000; Ludwig and Cook, 2001). If the intervention cost less than $134,400 per person, it would pay for itself. Our analysis unpacks what information the model uses to achieve this predictive performance.

Second, the predictions do not misrepresent average victimization risk across demographic groups. We show that Black male shooting victims are likelier to *have* a predicted risk, because they are likelier to have prior police contact.[5] This finding highlights how using police data limits an algorithm's ability to identify future victims with little or no prior police contact. But importantly, the algorithm accurately recovers group-level victimization risk regardless of race, age, or gender. As a result, the demographic composition of predicted shooting victims matches almost exactly that of actual shooting victims. In other words, the over-representation of Black men in police data does not yield predictions that overestimate the average victimization risk of Black men; the predictions are well-calibrated (right on average) within race and ethnicity and across the risk distribution.

Two points about these findings are important for interpretation. First, we are predicting shooting victimization risk under the status quo amount of intervention, incarceration, and mortality, or $Y(0)$ in a potential outcomes framework. To minimize shootings, interventions should ideally target the people for whom treatment effects, or $Y(1) - Y(0)$, are substantively largest.[6] However, in contexts with base rates as low as those for shootings, predicting $Y(0)$ is crucial to generating the evidence about $Y(1) - Y(0)$ that is required for such targeting to be possible. Choosing a study sample where $\bar{Y}(0)$ is too low leaves little room for an intervention to reduce shootings, making it difficult to detect a treatment effect. And even at high risk levels, statistical power is sensitive to small changes in $\bar{Y}(0)$. For example, the sample size required to detect a 50 percent reduction in shooting victimization increases by 50 percent when the sample is drawn from the $99.3 - 99.6^{\text{th}}$ percentiles of our predicted risk distribution, relative to when it is drawn from the $99.6 - 99.9^{\text{th}}$ percentiles,

5 Black men make up 69 percent of all shooting victims during the outcome period studied here. The model generates predictions for 74 percent of them, compared to half or fewer of the victims from other groups.

6 In practice, big *changes* in Y are sometimes (e.g., Heller, 2022), but by no means always (e.g., Ascarza, 2018; Haushofer et al., 2022), correlated with high *levels* of $Y(0)$.

where $\bar{Y}(0)$ is higher.[7] With an outcome as rare as being shot, accurately anticipating who will have a high $Y(0)$ is a necessary, if not sufficient, condition for testing preventive interventions and identifying optimal targeting strategies.

The second point important for interpretation is that getting the predictions right on average across demographic groups is not the same as the algorithm being "fair" or "unbiased." As we discuss in section 4.2, even algorithms that get group averages right across the risk distribution can still mis-rank individuals, both within and across demographic groups. How much mis-ranked predictions matter for "fairness" depends both on the decision rule one adopts to map the algorithm's predicted probabilities onto a serve/not serve decision (e.g., whether to use a global or group-specific threshold, whether to apply geographic or age restrictions as often done in practice, and so forth), as well as what kind of fairness one chooses to prioritize.[8] And importantly, whether an algorithmic decision rule helps or hurts any given definition of fairness depends heavily on what the counterfactual human decision-making process is. Because we have no systematic data on how violence prevention services are currently allocated, we cannot calculate whether any given decision rule would be more or less fair than the status quo in our setting.

As a result, we leave it as a central task for policymakers to map predictions onto service decisions in a way that satisfies their normative preferences about fairness in a given setting, and to weigh whether incorporating an algorithm increases or decreases bias relative to whatever decision-making process is the alternative. Our goal in this paper is not to evaluate particular, fictional use cases. Rather, it is to demonstrate that predicting the right outcome can get average demographic group risk right even when biased input data over-represent some groups, and that shootings are predictable enough with these data to make cost-effective individual interventions—and better research on those interventions—a plausible reality.

[7] See section 5 for additional details.

[8] It is well known that different definitions of fairness are in direct conflict with each other and not all can be simultaneously satisfied (e.g., Arnold et al., 2022; Berk et al., 2021; Chouldechova, 2017; Corbett-Davies et al., 2017; Cowgill and Tucker, 2020; Kleinberg et al., 2017).

To be clear, predicting shootings with police data is by no means a complete solution to gun violence, and predictions should be used with care. It is particularly important to attend to whom this kind of algorithm misses, and to the dangers and limitations of using these kinds of predictions to target law enforcement rather than social services (see section 5). Still, this paper establishes that shooting victimization is predictable enough for algorithmic screening to help ensure that preventive social services reach the people who need them, in the same way that algorithms have been proposed to screen for risk of depression, opioid abuse, and suicide in broader populations to prevent future harm (Garza et al., 2021; Eichstaedt et al., 2018; Hastings et al., 2020). A key remaining question is what kind of preventive services can reduce shootings for different parts of the risk distribution, which should be a priority for future research.

2 Related literature

The literature on using machine learning-based predictions to guide decision-making (e.g., Kleinberg et al., 2015, 2018a; Glaeser et al., 2016; Athey, 2017; Chouldechova et al., 2018; Hastings et al., 2020; Obermeyer and Emanuel, 2016; Obermeyer et al., 2019) does not engage with the risk of shootings. But it does provide two key priorities for evaluating predictive models (also see the discussion in Berk, 2008). First, predictions must be a true forecast, relying only on information available to the analyst at the time they are made. Second, a predictive model's performance must be assessed out-of-sample, i.e., using data separate from those with which the model is trained. Since in-sample predictive performance overstates how well observable features can predict future behavior due to over-fitting, it does not demonstrate the predictability of future violence. A third priority, especially relevant in our context, is generating risk predictions that capture true differences in risk across demographic groups, rather than differences reflecting discrimination by actors in the legal system embedded within the predicted outcome.

For these reasons, the large literatures in psychology and criminology on risk assessment instruments used to predict the likelihood of different types of violent offending (see reviews in Otto and Douglas, 2010; Hanson, 2005; Singh et al., 2011), as well as the risk factors correlated with violence more generally (Hawkins et al., 1998; Farrington et al., 2017), do not speak to the predictability of gun violence.[9] These studies typically either collect information from an interview to assess a known person's risk (e.g., a detainee or parolee) or examine in-sample correlations to identify potential risk factors. As such, they are not designed to establish how predictable shootings are, forecast and rank the risk of future shooting victimization across a large population, or assess whether those predictions distort true differences in risk due to underlying biases in the generation of criminal legal data.

We build on a handful of peer-reviewed papers and technical reports that have made important progress toward testing whether shootings are predictable. Berk et al. (2009) use machine learning to generate true forecasts and carefully explore predictive performance. But partly because the predictions were intended to target probation and parole services, they predict homicide charges rather than victimization. The use of an outcome partially determined by legal system actors makes it hard to assess whether the algorithm is predicting a person's risk of homicide offending or police and prosecutor decision-making about whom to arrest or charge, and relatively low clearance rates make it impossible to assess which offenders are being missed. Wernick (2018) and Wheeler et al. (2019) both study algorithms that predict a combination of shooting offending and victimization, and are therefore both subject to the same concern as Berk et al. (2009).[10] Chandler et al. (2011) predict the out-of-sample risk of being a shooting victim using ordinary least squares with Chicago Public Schools data. But their analysis is limited to high school students (a small minority of shooting victims) and does not report performance by group.

A large and influential body of work by Andrew Papachristos and coauthors (e.g.,

[9] For an overview of this literature, see Wheeler et al. (2019).

[10] Wheeler et al. (2019) also do not explore performance by demographic group.

Green et al., 2017; Papachristos et al., 2012; Papachristos and Wildeman, 2014; Papachristos et al., 2015a,b; Papachristos and Bastomski, 2018; Wood and Papachristos, 2019) documents the concentration of gun violence within social networks and explores the role these networks play in determining one's own risk of being shot. Green et al. (2017) provide a seminal insight about the role of social network measures in predicting the risk of shootings for prevention purposes, which directly influenced our feature selection below. But their prediction model relies on measures of co-arrest ties that do not appear in the data until after the time prevention would be delivered, making it infeasible for use as a pure forecasting method. They also fit and assess the performance of their model using the same data, making it difficult to determine how accurately the model predicts out-of-sample behavior.

This study improves upon and extends the prior literature by providing the three types of assessment needed to understand whether it is possible to predict who will be shot within a population without distorting risk across demographic groups. First, we perform a pure forecasting exercise using only data available at the time of prediction. Second, we assess performance on data not used in the model-building process, including information about shooting victims who are not included in the prediction data at all. And third, by predicting an outcome that is consistently measured across demographic groups, we document how many shootings a given use of data-driven predictions would capture or miss and for whom, what shapes those predictions, and how performance varies across race, gender, and age groups.

3 Method

We build a model that predicts a person's risk of being injured or killed by gunfire (shooting victimization) in the next 18 months using CPD data.[11] The key modeling decision we

[11] This excludes suicides and incidents where people are shot by police officers. We focus on an 18-month outcome period because we are interested in determining who is at high risk over a period of time, rather

make is to predict reported shooting victimization rather than arrest. Reported shooting victimization is much likelier to measure actual shooting victimization consistently across demographic groups than shooting arrest is to measure offending. Most shooting offenses do not result in an arrest in Chicago, and the likelihood of a shooting resulting in an arrest may vary across groups due to, among other factors, differences in police behavior.[12] In contrast, the Chicago data are consistent with most shooting victimizations, including non-fatal ones, being known to police, leaving little scope for reporting to vary significantly across groups.[13] This is likely due to the requirement that healthcare providers in Illinois are mandated to report firearm injuries to law enforcement,[14] combined with the very high likelihood that shooting victims obtain medical care.[15]

We start with CPD data on 12.7 million event-level records between August 1999 and October 2019, containing information on demographics, arrests, and reported victimizations in Chicago for both youth and adults. We then limit our sample and use a probabilistic record linkage algorithm (Tahamont et al., 2019) to uniquely identify 643,914 people for our modeling process, construct detailed predictive features about each person

than at one point in time. The former may be more appropriate for determining how to allocate preventive programming, which for a population at extremely high risk of gun violence can span months or years. For example, a related model was used to target a social service intervention in Chicago whose primary component is 18 months long (Bhatt et al., 2023).

[12] The best available data suggest that, in the first half of the 2010s, under half of homicides and fewer than ten percent of non-fatal shootings in Chicago resulted in an arrest (Kapustin et al., 2017).

[13] National estimates suggest that 22-26 percent of all non-police gun assaults are fatal. If non-fatal shooting victimizations are being under-reported in Chicago, we would expect that a higher proportion of reported shootings would be fatal (i.e., assuming shooters in Chicago had no worse aim than shooters elsewhere, fewer reported non-fatal shootings would result in a higher proportion of fatal shootings). In fact, Chicago actually has a lower proportion; only 16-18 percent of reported shootings in recent years are fatal. Note that this comparison relies the assumption that fatal victimizations from non-police gun assaults are mostly free of underreporting, a widely-held view among researchers (Loftin and McDowall, 2010; Carr and Doleac, 2016).

[14] 20 ILCS 2630/3.2

[15] This high likelihood is widely reported across violence prevention, medical, and law enforcement practitioners in Chicago. There may still be some selective under-reporting of non-fatal shooting injuries by victims who self-treat or live near the city border and seek care from providers outside CPD's jurisdiction, both of which in theory could vary by race. However, based on our conversations with practitioners, we think the magnitude of such selective under-reporting is likely to be quite small. Another potential source of measurement error that may be correlated with demographics, but also likely to be quite small in practice, is our procedure for linking records that lack a common identifier (see Appendix A).

in the sample, and train a model to predict their risk of shooting victimization in the following 18 months. We describe key aspects of this process below; for additional details, see Appendix A.

To predict a person's risk at a point in time, we require that they have at least one arrest or two reported victimizations in the 50 months prior (though importantly, we can observe all shooting victimizations even for people not in this prediction sample). People without recent arrests or reported victimizations, or with only one recent reported victimization, have a shooting victimization rate in the outcome period that is 70 times lower than that of people who meet the inclusion criteria. Furthermore, excluding people with a single reported victimization reduces the influence of record-linkage error caused by missing date-of-birth information in victimization records.[16] We then construct four categories of features for each person meeting this inclusion criterion. Demographic features include age, gender, race and ethnicity,[17] and police beats associated with home and incident addresses.[18] Arrest and victimization features include time-windowed counts of incidents, separately by incident type (e.g., robbery, shooting, vandalism).[19] For example, one feature counts the number of arrests for robberies involving a firearm within the past two years, while another counts the number of shooting victimizations within the past 90 days.

Finally, network features include time-windowed counts of arrests and victimizations among people to whom the focal person is connected through co-involvement in prior criminal incidents ("neighbors"), defined as either being arrested for the same incident or being a victim-offender pair in the same incident. For example, one feature counts

[16] For additional details, see Appendix A.2.

[17] It is worth noting that, in practice, there are many legal issues surrounding the inclusion of race and ethnicity in algorithms (Yang and Dobbie, 2020). We include it in our full model because it may help the algorithm make more accurate predictions by racial group when predictors are recorded differently by race (Kleinberg et al., 2018b). As we show in Appendix B.3.2, however, there is enough information in the non-race features that excluding it leaves predictive power and calibration by race basically unchanged.

[18] There are 277 total police beats in Chicago, compared to 866 census tracts and 77 community areas (neighborhoods).

[19] The time windows are within 30, 60, 90, 180, 365, 730, and 1825 days before the prediction date, as well as the time since August 1999.

the number of gun possession arrests in the past 180 days among neighbors to whom the focal person is connected directly ("first-degree neighbors"), while another counts the number of robbery victimizations in the past 90 days among neighbors one degree further removed ("second-degree neighbors"). Also included are features describing the local structure of the network graphs themselves, such as the focal person's centrality and number of neighbors. The full model includes 1,406 features in total (see Appendix A.3 for details on feature construction).

We train and test a gradient-boosted decision tree model (Friedman, 2002). We chose this approach because gradient-boosted tree-based models have been shown to generally outperform other machine learning approaches on tabular data (Caruana and Niculescu-Mizil, 2006; Caruana et al., 2008); see Appendix B.3.3, where we compare standard regression models with our main approach.[20]

One point of departure from traditional machine learning applications is the way we generate our hold-out test set. The typical approach is to subsample observations and hold out a set of individuals from the training process, so that tests of predictive power are performed on an entirely independent group. In our setting, however, removing person i from the training data will not necessarily remove all information about i from the model-building process. The reason is our inclusion of network features: the predictors we define for person j include arrest and victimization histories for those co-involved in incidents, which could include i. Information about i could therefore still appear in the training data through person j's network features, even if i is in a randomly subsampled hold-out test set. Typical subsampling would therefore not adequately address the risk that information about people in the test set could be leaked to the training set given our inclusion of network features—a situation which could lead to optimistic performance estimates.

[20] Deep learning methods are more accurate than tree-based methods when input data is "unstructured," i.e., audio, video, images, or text. However, deep learning has been shown to be outperformed by tree-based models, including gradient boosting, on structured data (Grinsztajn et al., 2199; Shwartz-Ziv and Armon, 2022).

To avoid this kind of information leakage between training and test data, we do not subsample observations. Instead, we divide the data into four calendar time cohorts (see Appendix Section A.4.1 and Appendix Figure A.1) and generate a hold-out test set via time separation. Each cohort has the same structure: a 50-month sample inclusion period followed by an 18-month outcome period. We use the first two cohorts to train the model and the third as a validation cohort for hyperparameter tuning. Note that we do not have a separate post-training calibration step, since gradient boosting already optimizes log loss. The final cohort is our test set, where we predict shooting risk for the 327,127 individuals in the out-of-sample 18-month outcome period starting April 1, 2018.[21] All results speak to the predictive performance for this group during this period.

4 Results

Our main results describe how effectively the algorithm can predict future gun violence to help target prevention services, with particular transparency surrounding two major concerns with using algorithms in practice: racial disparities and what influences predictions. We first assess the model's overall performance, focusing on its ability to identify the relatively small number of people at high predicted risk of being shot.[22] We then show how predictions vary by demographic group, unpacking who is identified and missed with this kind of approach. Finally, we describe how changing the type of information

[21] People can appear in more than one cohort, so observations are not entirely independent across cohorts. However, the time-windowed predictors and outcomes are defined relative to each cohort's prediction date. As a result, even when a person appears in multiple cohorts, their predictors and outcomes are defined over different time periods. Most importantly, the cohorts' outcome periods do not overlap, ensuring that the outcomes in the test set are never included during model training.

[22] Given how rare shootings are in the overall population, a common performance metric like accuracy, defined as the share of all predictions made correctly, will mostly be driven by correctly classifying people who are not shot. Another common metric, the area under the Receiver Operating Characteristic curve, or AUC, also characterizes performance across the entire risk distribution. In this paper, we are most interested in performance at the top of the risk distribution, since that will determine how effective any selection process for preventive services will be. As a result, we focus on performance measures at or above approximately the top 1 percent, recognizing that differences in AUC across models may not reflect differences in accuracy among the highest-ranked predictions.

available to the algorithm affects performance.

4.1 Performance overall

The top left panel of Figure 1 shows the overall distribution of the model's predictions and how they compare to realized rates of shooting victimization for the sample. The x-axis is the average predicted risk for each percentile of the risk distribution, with each point containing 1 percent of the test sample, or 3,271 people. The y-axis is the actual rate of shooting victimization in the 18-month outcome period for the 3,271 people in each bin.

Three features about the overall predictions are apparent. First, on average, the model's risk predictions are accurate (well-calibrated): their slope is close to the 45-degree line. Second, the vast majority of people in the sample are predicted to have a shooting victimization risk close to zero, as indicated by the mass of points in the bottom left of the graph. Finally, the predicted risk distribution is highly positively skewed, with points in the upper right of the graph corresponding to a small group of people in the long right tail whose predicted risk of being shot in the 18-month outcome period is very high. We discuss the other panels of Figure 1 in the next section.

Figure 2 reports two measures of model performance across the predicted risk distribution. Figure 2a shows Precision_k, or the share of people who are actually shot during the 18-month outcome period among the k people with the highest predicted risk:

$$\text{Precision}_k = \frac{\sum_{i=1}^{k} \mathbb{1}[\text{Shooting victim}_i = 1]}{k}$$

Figure 2b shows Recall_k, or the share of actual shooting victims during the 18-month outcome period who are among the k people with highest predicted risk:[23]

$$\text{Recall}_k = \frac{\sum_{i=1}^{k} \mathbb{1}[\text{Shooting victim}_i = 1]}{\text{Total shooting victims}}$$

[23] In the public health literature, precision is commonly referred to as positive predictive value, and recall is commonly referred to as sensitivity or the true positive rate.

We show two versions of recall in Figure 2b. The first, simply labeled recall, uses the total number of shooting victims in the prediction sample as the denominator, or 2,253. The second, labeled total recall, uses the total number of shooting victims in the entire city during the outcome period as the denominator, or 3,381. The difference between these two highlights a point we return to in the following section about whom predictions based on police data miss: one-third of eventual shooting victims are not in our prediction sample and therefore not assigned a predicted risk by the model. Though it is more common when evaluating the performance of a predictive algorithm to report recall, total recall helps to assess the ability of algorithmic prediction to identify shooting victims city-wide, regardless of whether they have enough prior police contact to be included in the prediction sample.

The share of people shot during the 18-month outcome period is startlingly high among those in the right tail of the distribution (Figure 2a). Among the $k = 500$ people with highest predicted risk, 12.8 percent, or 64 people, are shot. This is 18 times higher than the base victimization rate for the prediction sample (327,127 people) of 0.7 percent, and 128 times the city-wide victimization rate (2.7 million people) of 0.1 percent. Among the $k = 3,381$ people with highest predicted risk—corresponding to the actual number of shooting victims during the 18-month outcome period—9.5 percent are shot. Those at higher predicted risk for shooting victimization are also at significantly elevated risk for other adverse outcomes, like shooting arrest and other violent victimization (Appendix Table B.1).

The recall rates confirm that those in the right tail of the distribution account for an outsized share of all shooting victims (Figure 2b). Despite representing just under 0.02 percent of the city's population, the $k = 500$ people with highest predicted risk include 1.9 percent of the 3,381 total victims during the 18-month outcome period.[24] The $k = 3,381$ people with highest predicted risk—just over 0.1 percent of the city's population—include

[24] Considering only the 2,253 shooting victims in the prediction sample rather than all 3,381 victims, recall at this threshold is 2.8 percent.

9.5 percent of total victims.

Still, the recall rates make clear that not all shootings are easily predicted using observable factors derived from police data. Future victims are missed in two ways. First, by construction, the algorithm misses the 33.4 percent of victims who are not included in the prediction sample. This can be seen by the gap between the recall and total recall curves at $k = 327,127$ in Figure 2b. Second, some eventual victims are assigned a low predicted risk that leaves them outside the top $k = 500$ or even $k = 3,381$. This may be partly because being shot is inherently difficult to predict: it is the product of both a complex social phenomenon (i.e., engaging in high-risk behavior) and of randomness (being hit when fired at or being a bystander). But it may also be because the model can better distinguish risk among people about whom it has more, and more recent, information. For example, eventual victims among the $k = 3,381$ with highest predicted risk have seven times as many arrests in the prior year as eventual victims not among the $k = 3,381$ (2.8 vs. 0.4).

Importantly, using police data may still result in mis-predicting shooting victimization risk differentially by demographic group. A major concern about using police data for prediction is that racial differences in those data may not arise from true differences in behavioral risk, but rather from differences in police behavior toward racial groups. Taking advantage of the fact that our relatively well-measured outcome allows us to compare predicted and actual risk for the behavior of interest, we next turn to showing how much these differ by race, gender, and age.

4.2 Performance by group

Though the model's predictions match realized rates of shooting victimization overall (top left panel of Figure 1), it may still over- or under-predict risk—or fail to predict it altogether—more for members of some groups than others due to differences in how or whether they appear in police data. For example, Black individuals appear in the prediction sample four times as often as White individuals and almost three times as often as Hispanic individuals, despite each group making up roughly a third of the city's population. (Note that throughout the paper, we refer to individuals of any race as Hispanic if this is their indicated ethnicity; those to whom we refer as White or Black include only those who are non-Hispanic.[25]) A key concern is that if some of this over-representation is because Black residents are more likely to come into contact with the police due to over-policing of Black neighborhoods, or due to a greater propensity among officers to stop, search, or arrest them conditional on their behavior, then police data will systematically misrepresent Black individuals' behavior in a way that could generate inflated predictions of the shooting victimization risk they face. Similarly, under-policing of other groups may lead the model to under-predict their victimization risk.

The three remaining panels of Figure 1, which report calibration separately by race or ethnicity, show this is not the case on average, across the distribution of predicted risk. Each point is a bin containing one percent of people of the indicated race or ethnicity in the prediction sample. Relative to other groups, the distribution of predicted risk is wider—extends further to the right—for Black individuals, and their average predicted risk is 3 times higher. Yet importantly, the slope of the line shows that the higher predicted risks of shooting victimization for Black individuals are not inflated: they are, on average, accurate probability estimates, falling close to the 45-degree line all the way across the risk distribution. If anything, it is the White and Hispanic individuals predicted to be in the very highest percentile of shooting victimization risk for whom the predictions may slightly overestimate risk, as indicated by the points below the 45-degree line (see Appendix B.1 for further quantification and discussion). But on average, these data yield predictions that are accurate about the risk of shooting victimization across racial groups and across the risk distribution.

Figures 3 and 4 provide a fuller accounting of how the use of police data shapes the demographic composition of the predictions relative to the demographic composition of

[25] Race and ethnicity information contained in the data likely reflect the views of officers rather than the subjects themselves.

those who actually end up being shot.[26] As shown in Figure 2b, two-thirds of shooting victims have enough prior police contact to appear in our prediction sample. To show who is missing from the prediction sample, Figure 3 compares the race/ethnicity and gender composition of all 3,381 shooting victims to the 2,253 victims with enough data to receive a prediction. The blue bars show the number of all shooting victims in each group and the orange bars the number in our prediction sample, with the label reporting the share of all victims in that group who are in our sample. Three-fourths of all Black male victims are in our sample and therefore receive predictions, compared to roughly half or fewer of the victims from other demographic groups. This pattern is consistent with the over-representation of Black men in police data more generally, though in this case it may aid predictions about shootings since Black men comprise the largest share of all victims (69 percent). A key implication of Figure 3 is the need for other methods and data sources to help identify and prioritize for prevention people at high risk of victimization who would be missed by an algorithm trained solely using police data.

Figure 4 provides further evidence that the predictions are successfully matching the true demographic composition of shooting victims, as well as the demographic implications of one particular use of the predictions. The first two rows provide an additional breakdown of who is included or missed in the sample by showing the percentage of victims in each race, gender, and age group for all 3,381 shooting victims city-wide (first row) compared to the demographics of the 2,253 victims in the prediction sample (second row). Further dividing the data from Figure 3 by the median age of shooting victims, 23, does not change the picture of sample selection; comparing the second row to the first shows that Black male victims in both age categories are slightly over-represented in the data relative to the other groups. The third row shows the demographic composition of "predicted victims" in the sample, calculated by averaging across all 327,127 people in the prediction sample while weighting each person by his or her predicted risk of vic-

[26] Additional detail about the demographic compositions of the samples presented in these figures can be found in Appendix Table B.2.

timization (see Appendix A.5.1 for details). Comparing the second and third rows again demonstrates that the calibration of the model's predicted probabilities does not vary systematically by demographic group; the demographic shares of predicted victims are quite close to those of actual victims in the prediction sample, with predictions just barely under-stating the proportion of Black male victims in both age groups. Additional detail on predictive performance by demographics is in Appendix B.1.

If we predicted an outcome like arrest, we would be unable to determine whether differences in predictions across demographic groups are due to true differences in behavioral risk across them, or whether they are due to differences in police decision-making about whom to arrest from each group. In our case, however, we predict an outcome that captures the true behavior of interest (shooting victimization) with little differential error across groups, and the model is relatively well-calibrated by race. So we can conclude that even if our arrest predictors represent a distorted picture of differences in offending across groups, the resulting predictions of our outcome—whether someone is shot—are not, on average, systematically biased across the race, age, and gender groups in the data.

This is broadly consistent with theoretical work finding that an algorithm with access to information that allows it to reconstruct race can "learn" accurate race-specific rankings of risk (Kleinberg et al., 2018b). It is important to note, however, that calibration within demographic groups does not imply that the algorithm removes all potential influence of differential policing (across or within groups). For example, suppose Black neighborhoods are over-policed relative to non-Black neighborhoods. The resulting differential measurement error in the predictors can still affect how well the algorithm can rank across groups, even when getting group averages right (Corbett-Davies et al., 2017). And if differences in how the predictors are measured are driven by unobservables (e.g., if there is unobserved heterogeneity in over-policing within Black neighborhoods), then the algorithm may be unable to learn the differential relationship between the predictors and the outcome. In that case, there could still be mis-ranking within groups (Kleinberg et al., 2018b).

As discussed in the introduction, the implications of this kind of mis-ranking for measures of fairness or bias hinge on the decision rule that maps predictions onto service decisions, and how that compares to the counterfactual non-algorithmic decision rule. The costs of each decision rule also rest on whether the services provided are helpful or harmful to the individual and society.[27] These issues are crucial for using algorithms in practice. But in the absence of specific use cases, we do not have enough information to make broad claims about fairness or bias. Instead, we offer one simple example to highlight how decision rules shape who would be served.

As suggested by the dramatically different risk distributions by race/ethnicity in Figure 1, any decision rule that offers prevention services to everyone above some high threshold of predicted risk in this setting will end up serving a disproportionately Black population, as well as a small number of Hispanic and White individuals. The fourth row of Figure 4 shows the demographic implications of one such threshold rule as a stylized example: serving the 3,381 people at highest predicted risk.

Compared to actual and predicted victims, this group overwhelmingly comprises Black men, and particularly young Black men. It includes almost no women. And older Black men are under-represented despite making up the plurality of actual victims. Importantly, the concentration of young Black men at the top of the predicted risk distribution does not indicate falsely inflated risk. In fact, even within this above-threshold group, the model is most calibrated for young Black men, while (consistent with the right side of the Hispanic and White panels in Figure 1) over-predicting for most other groups (see Appendix Table B.4 for performance measures by group under this decision rule). This pattern likely reflects both the higher true risk of shooting victimization among some young Black men and the model's ability to distinguish those individuals. Further examination about why some groups of victims are more easily identified by the model, and whether this informs

[27] The costs may also be a function of the sources of bias. For example, all else equal, stakeholders may place a higher cost on imbalance in fairness measures if the cause is bias from policing as opposed to if the cause was from true underlying differences in risk.

what kind of services would be most useful to them, is warranted.[28]

There are, of course, normative fairness questions involved with any way of allocating a limited amount of services, including an algorithmic threshold rule (see section 5 for discussion). The descriptive result here, which may help inform those normative discussions, is that a threshold rule would allocate services disproportionately to young Black men in a case where the algorithm is, on average, getting the demographic distribution of shooting victims quite close to correct. And depending on where it is drawn, this kind of threshold allocation rule would likely miss almost all female victims. Such a rule violates measures of fairness that necessitate equal representation across groups, as well as a number of alternative fairness measures.[29] We therefore make no claim that successful group calibration, even across the risk distribution, means that a given use-case of the algorithm, such as the threshold allocation rule described here, would be "fair."

4.3 What matters for performance?

We are interested not only in whether the model can identify people at high risk of being shooting victims, but also what information allows it to do so. A common strategy for answering this question in machine learning applications is to report the "importance" of individual features. One way to do this is by assessing how much a given feature affects the predictions of a model that has already been built, such as by permuting the feature's values and measuring the impact on prediction errors using the same model (Breiman, 2001). However, this approach can be easily misinterpreted, especially when

[28] For example, if the risk of domestic violence shooting victimization is harder to predict using information contained in police data, and if a larger share of female shooting victims are injured or killed in such incidents, then this may help explain why the model assigns low predicted risk to these victims. In contrast, if young Black men are disproportionately victimized in shooting incidents that are easier to predict using information contained in police data, then this may help explain why the model assigns higher predicted risk to these victims. We cannot explore these issues directly since we have no information about the nature of the incident in which someone was shot, but this would be a useful avenue for future work.

[29] The very uneven distribution of shooting risk also complicates calculating some of these error rates: there are so few White and female individuals above this threshold, for example, that measures like false positive rates would have very large confidence intervals.

closely correlated features exist (Toloşi and Lengauer, 2011). For example, if a model loads heavily on one feature and not its correlated counterpart, then the former feature may be "important" in terms of affecting predictions within a given model, while at the same time not materially changing model performance when that feature is left out entirely. An alternative approach that better answers the importance question is to retrain the model leaving out the feature in question (Lei et al., 2018). By allowing the remaining features to substitute for the missing information, this approach determines which features capture information that is substantively important for predictive performance and cannot be found in other features. While ideal, it is often impractical to leave out one feature at a time and rerun the computationally expensive model-building process. Therefore, to implement this in practice and aid interpretation, we focus on removing *groups* of features by substantive type and retraining the model each time.

Figure 5 reports precision for the full model and three other models that each exclude certain feature sets.[30] The x-axis ranks everyone in the prediction sample by their predicted risk of victimization, with highest predicted risk on the left. For each rank k, the y-axis reports the precision, or share actually victimized during the outcome period, of the people with that predicted risk or higher. For example, for the full model, among the $k = 1,000$ people with the highest predicted risk, almost 12 percent are shot during the outcome period; among the top $k = 4,000$ people, approximately 9 percent are shot. Because noise in our precision measure increases as k, the number of people above a predicted risk threshold, decreases, we start the graph at $k = 500$. A bootstrap 95 percent confidence interval is plotted around each model.[31]

Two feature sets of particular interest are those containing information about a person's own arrest history and those containing information about the arrest and victimization histories of people in a person's "network." As others have noted (e.g., Richardson et al.,

[30] Performance measures for additional models excluding different feature sets are reported in Appendix B.3.

[31] For additional details, see Appendix A.5.2.

2019; Lum and Isaac, 2016; Luh, 2019), arrest data contain errors, may be subject to intentional manipulation, and are shaped in significant part by officer behavior. In the extreme, if arrest data provide little signal about individual behavior, then even if individual behavior plays a large role in a person's risk of shooting victimization, arrest data would provide little predictive power. Separately, Green et al. (2017) shows that network information may be useful in predicting shooting victimization, particularly if, as they argue, gun violence propagates through a social network as people co-engage in risky behavior with their peers.

As Figure 5 shows, features related to a person's own arrests matter substantially for performance, as excluding them reduces precision by about two percentage points relative to the full model. The story is less clear for network-related features: excluding them on their own does not affect performance, but excluding them *in addition to* own arrest features appears to lower precision.[32] This pattern suggests that both a person's own arrest history *and* the arrest and victimization histories of their network neighbors contain valuable signal for predicting their shooting victimization risk. But while the signal contained in the network features is likely also captured by a person's own arrest history, a person's own arrest history contains additional signal that is not captured by their network features.[33]

5 Discussion

This paper demonstrates that re-purposing police data allows us to identify small groups of people at outsized risk of being shot. The immense social cost of gun violence—to victims, their families, and their communities—justifies spending a lot to reduce this risk.

[32] Although we do not test for the statistical difference across curves at every point, the fact that the 95 percent confidence intervals of the "no own arrests" and the "no own arrests and no network information" models overlap suggests that we cannot statistically distinguish their performance.

[33] This may be, in part, because a person's network features are constructed using information from their own arrest history. For additional analyses exploring the sensitivity of the model's performance to the number of features and modeling complexity, see Appendix B.3.3.

For example, the 500 people with the highest predicted risk represent just 0.02 percent of Chicago's population but 1.9 percent of its shooting victims over an 18-month period. This amount of gun victimization generates an estimated social cost of just over $134 million (Cook and Ludwig, 2000; Ludwig and Cook, 2001).[34] If an intervention could cut this group's risk by half, it would save $134,400 in social costs for each of the 500. The algorithm could also help target larger interventions: the 3,400 people with highest predicted risk are 0.1 percent of Chicago's population but account for almost 10 percent of its shooting victims during the outcome period. At an estimated social cost of $678 million, reducing this risk by half would save $99,750 for each of the 3,400. Even with the uncertainty inherent in estimates of gun violence's social costs, the magnitudes involved are likely to be staggering. The fact that it is possible to anticipate who so many shooting victims will be, given the huge costs involved, is a strong argument for trying to prevent their victimization.

Predicting shooting victimization can also be important for research aimed at identifying effective interventions. While interventions should target the people whom treatment would most benefit—those with large (negative) values of $Y(1) - Y(0)$—reaching participants with lower $Y(0)$ may reduce statistical power in a given sample size (or conversely, require a larger sample size to detect a given effect). For example, suppose a study sample had the same average shooting victimization risk as the 1,000 people with highest predicted risk identified by our algorithm (average risk = 11.7 percent). It would require an experiment with a sample size of 734 to detect a 50 percent reduction in shootings.[35] In contrast, if the sample was drawn from the next 1,000 highest predicted riskiest people (average risk = 8.0 percent), then the sample size required to detect a 50 percent reduction in shootings would increase by 51 percent to 1,106. In a world of limited resources, better

[34] These studies estimate the social cost of a gunshot injury to be $1.2 million in 1998, or $2.1 million in inflation-adjusted 2022 dollars, using a nationally representative contingent valuation survey of adults in the U.S.

[35] The calculation uses a difference of proportions test with 80 percent power and 5 percent chance of Type I error.

prediction of $Y(0)$ can be an important input into identifying $Y(1) - Y(0)$.

Of course, identifying those in need of prevention is only the first step. Preventing shooting victimization also requires an intervention that addresses why a person is at high risk of it or changes something external to reduce that risk. Research about social service interventions' effectiveness at reducing gun violence for this population is relatively limited.[36] Generating evidence about who is responsive to which kinds of prevention efforts, and how that varies across the risk distribution, should be a high priority.

It is also crucial to acknowledge that even a model capable of identifying a group of people at very high risk of being involved in gun violence will get that prediction wrong for many—in our case, most—people in the group. And as discussed above, getting group averages correct still leaves room for differential error rates by demographic group, depending on how predictions are used. Given these realities, the costs of misdirecting an intervention can vary significantly. Providing a slot in a social program to someone whose actual risk is much lower than predicted, for example, incurs an important opportunity cost but is unlikely to harm the recipient.[37] Targeting proactive policing efforts that could infringe on someone's civil liberties or perpetuate racially-discriminatory police practices in their community, on the other hand, may impose unacceptably high costs on the recipient (Stevenson and Mayson, 2021) and those around them.

There are other reasons not to use predictions of shooting victimization risk to target proactive policing efforts. In addition to the potential legal barriers posed by using any algorithmic predictions for such targeting,[38] these proactive policing efforts are designed

[36] The most well-studied model, Cure Violence, has mixed evidence of success (Butts et al., 2015; Buggs et al., 2020). Other programs providing mentorship and life coaching to those at high risk of gun violence in the community (Corburn and Fukutome-Lopez, 2020) or who are hospitalized (Cheng et al., 2008; Cooper et al., 2006; Zun et al., 2006) are being studied non-experimentally or at small scale. A preventive intervention delivered by police in Chicago to men identified by a predictive model was not found to reduce victimization (Saunders et al., 2016).

[37] Even when a model's high predicted risk of victimization is correct, offers of preventive services made on the basis of algorithmic predictions need to be implemented carefully to avoid stigmatizing or even potentially further endangering the recipients.

[38] Predictions based partly on prior police behavior may not meet the requisite standards for performing certain police actions that can infringe on a person's civil liberties. For example, it is unclear whether

to intervene with (and prevent the actions of) future *offenders*, not the future *victims* we seek to predict. Our results provide no basis for concluding that the risks of shooting victimization and offending are interchangeable. Without a measure of true offending, we cannot assess how well predicting victimization does at predicting offending, nor whether it is more or less accurate in identifying future offenders than the status quo methods used by police. This uncertainty points to an ethical challenge: it is difficult to justify targeting policing efforts—which often create large negative externalities—on the basis of shooting victimization risk, given the unclear marginal benefits and high potential costs of doing so.

Importantly, however, the results in this paper suggest that ignoring the ability of police data to predict shooting victimization altogether is not a solution; the counterfactual of *not* using information that could improve the effectiveness of gun violence prevention efforts carries its own cost. Current resource allocation mechanisms often rely on the staff of community violence prevention organizations, sometimes in partnership with law enforcement or hospital staff. Such individuals' social networks and expert judgment likely capture risk factors that police data miss. But they also introduce their own potential for bias and may miss high-need people whom the relevant staff do not know. Additionally, local organizations have good reason to target those who are easiest to find and least costly to serve. If the people at highest risk are also the hardest to identify and serve, then algorithms may be an effective way to direct potentially life-saving services toward those who might not otherwise receive them.[39]

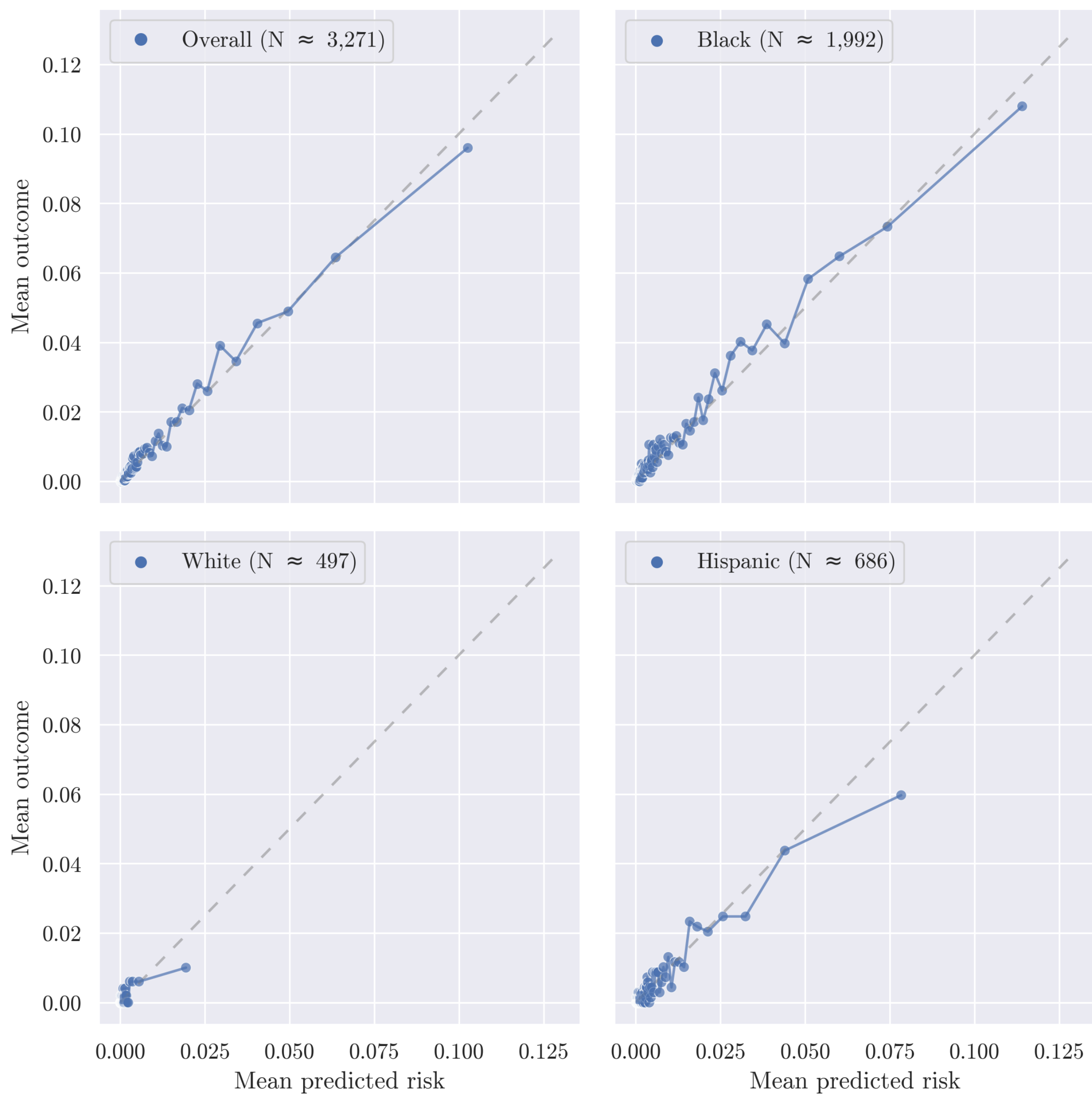
an algorithmic prediction would be sufficient to meet the "reasonable suspicion" standard necessary to effect a traffic stop or the "probable cause" standard necessary to effect an arrest, particularly if police could justify future action against someone by interacting more with them today in a way that raises their predicted risk.

[39] One example of how algorithmic prediction can be used to direct gun violence prevention services is READI Chicago (Bhatt et al., 2023). In that setting, a predictive model closely related to the one studied here identified men at very high risk of involvement in future gun violence. Publicly available information about them was provided to community violence prevention organizations, who offered the men a chance to voluntarily participate in an intervention designed to reduce their risk. No information about them was shared with law enforcement. Other men who could benefit from the intervention were identified by the community organizations themselves, or by jail, prison, and parole staff. In this way, the model was a complement to, rather than a substitute for, human expertise; it helped find people who could benefit

The key insight of this paper is that an algorithm using police data—which are readily available in most cities—to predict a well-measured outcome can be a useful tool for preventing morbidity and mortality from gun violence. Training the algorithm to predict shooting victimization rather than arrest ensures that it is predicting the outcome of interest, rather than whom police decide to arrest (Obermeyer et al., 2019; Mullainathan and Obermeyer, 2021). We note that the algorithm's ability to predict gun victimization occurs in a social context where law enforcement is often the primary state institution enmeshed in the lives of Black men at high risk of gun violence. In a different context, where other government agencies and non-profit organizations more extensively engage with people facing such risks, there will likely be other information available to help target preventive services. Shifting toward this context could have a number of benefits, including reducing the social costs of excessive police contact (e.g., Pager, 2003; Harris, 2016; Mello, 2021; Agan and Starr, 2017). Until then, a small group of people face an extraordinarily high risk of being shot, with few systematic ways to identify them available. We demonstrate that it is currently possible for an algorithm to predict shooting victimization well enough to direct services that can help save lives.

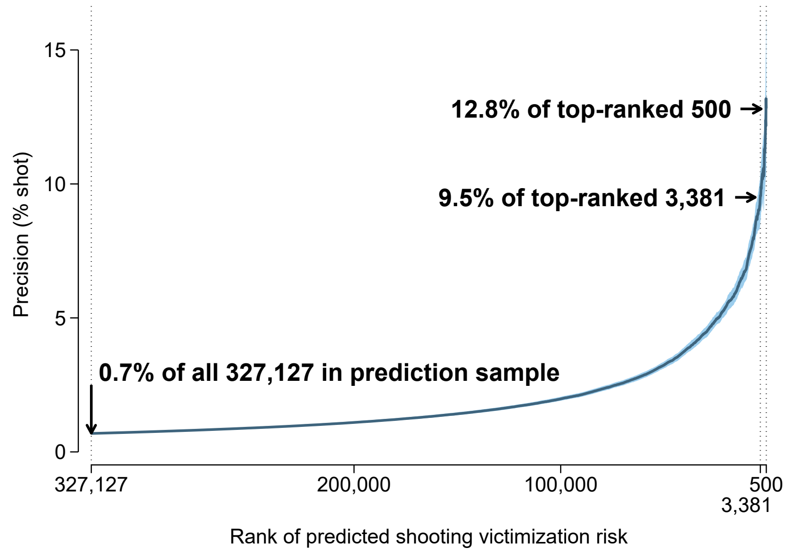
from programming but who might not otherwise be found. Crucially, this approach was developed in consultation with people who live in the affected communities.

Figure 1: Predicted versus actual risk of shooting victimization by bin (calibration), overall and by race/ethnicity

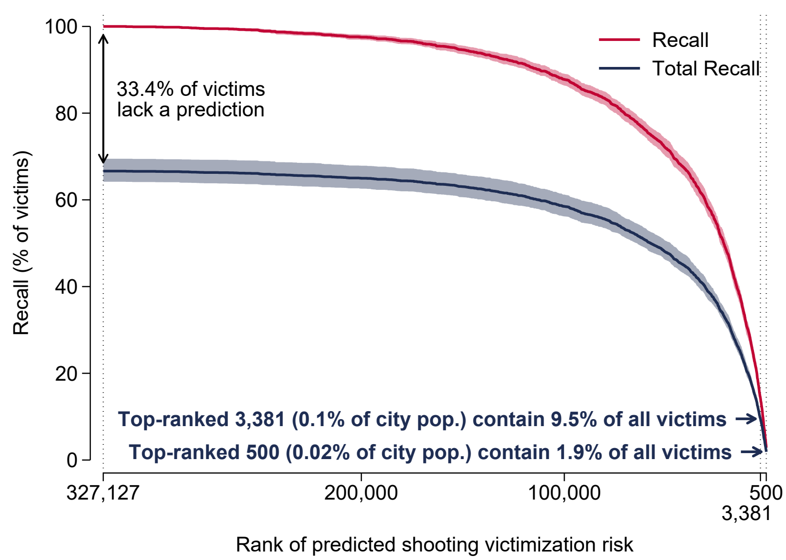


Note: Figure shows mean predicted risk and shooting victimization rate within each percentile of the overall (top left panel) and race/ethnicity-specific (remaining panels) predicted risk distributions. Race/ethnicity categories are mutually exclusive: non-Hispanic White, non-Hispanic Black, and Hispanic of any race.

Figure 2: Predictive performance for shooting victimization



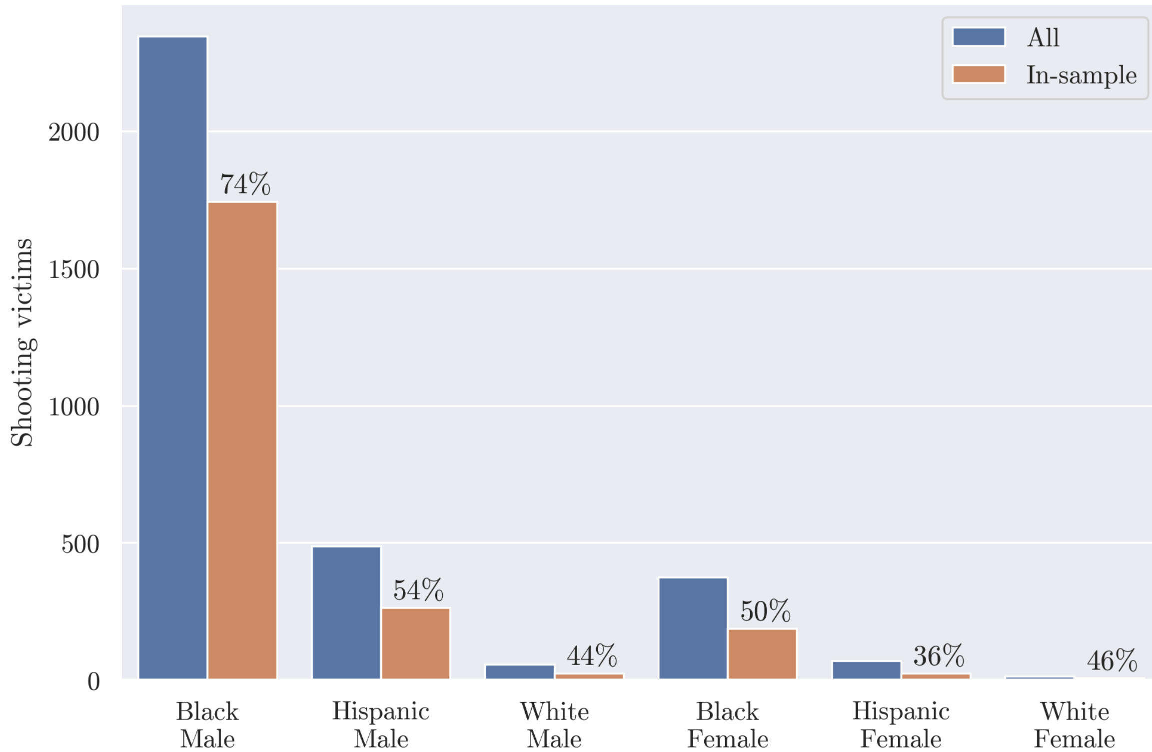
(a) Precision



(b) Recall

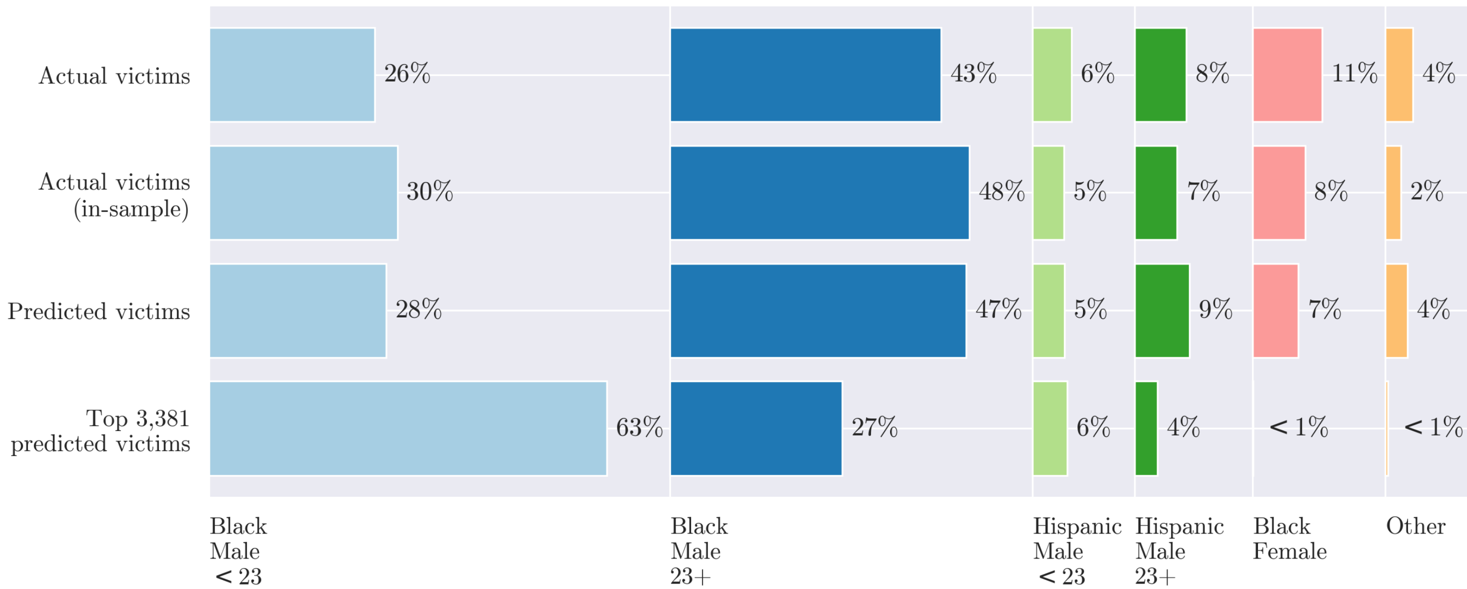
Note: Performance during the 18-month outcome period starting April 1, 2018 of the full model trained to predict shooting victimization. Precision shows share of the k people with the highest predicted risk who are actually shot during the 18-month outcome period. Recall shows the proportion of the 2,253 actual shooting victims in the prediction sample who are among the k people with highest predicted risk. Total recall shows the share of all 3,381 actual shooting victims in the city who are among the k people with highest predicted risk. Bootstrapped 95 percent confidence shown (see Appendix A.5.2 for details).

Figure 3: Demographic composition of all victims and those in prediction sample



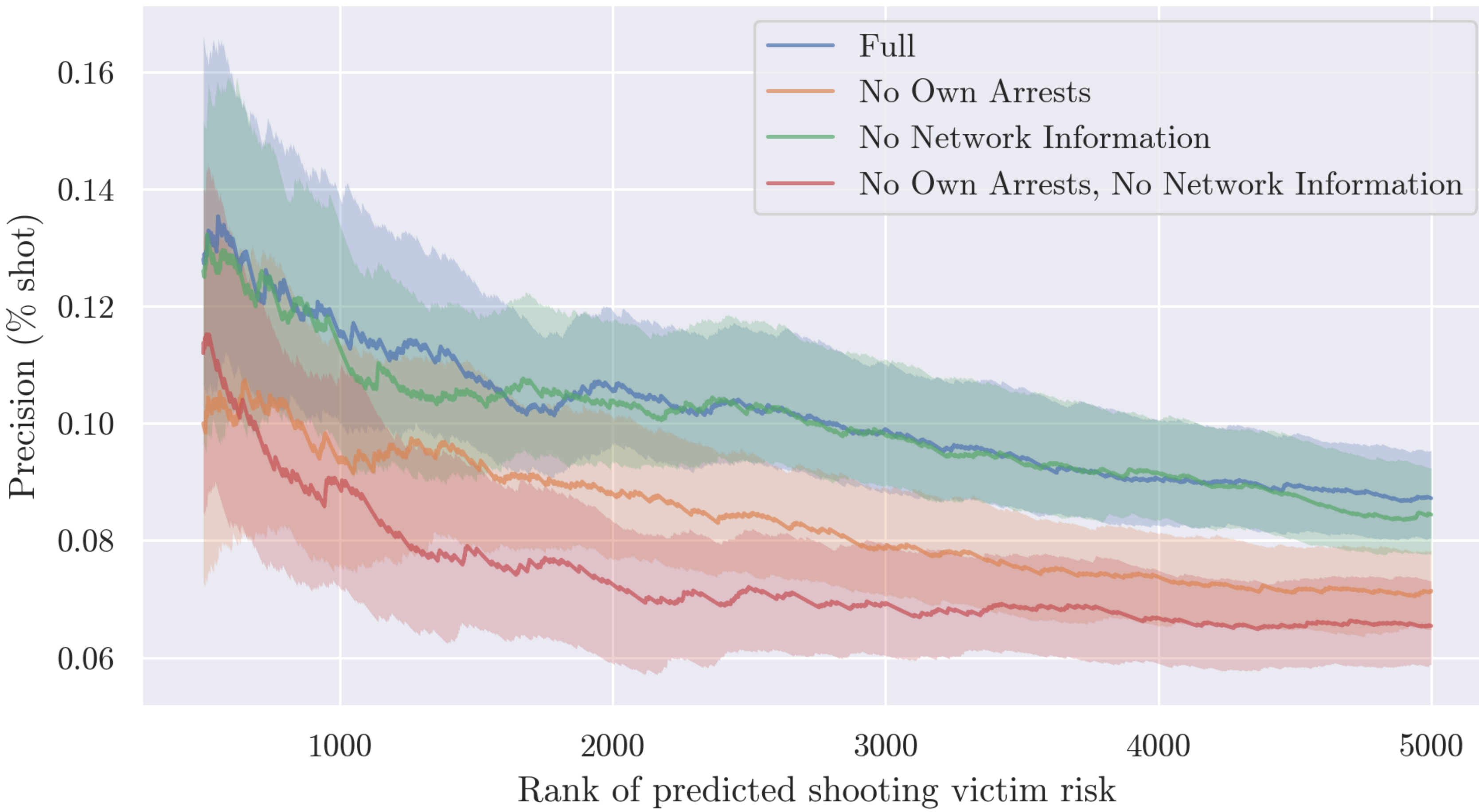
Note: Figure reports counts of shooting victims separately by race/ethnicity and gender, among all 3,381 shooting victims during the 18-month outcome period and the 2,253 victims in the prediction sample. Percentages above the in-sample bars report the share of all shooting victims in that demographic group (each blue bar) who appear in the prediction sample.

Figure 4: Demographic composition across victim groups



Note: Figure reports the proportion of each row in the indicated demographic category, with rows showing all actual shooting victims, those in the prediction sample, predicted shooting victims, and the 3,381 people with the highest predicted risk of victimization. To reduce visual clutter, demographic groups accounting for very small shares of actual and predicted victims—Hispanic women, White men, White women, individuals with missing race/ethnicity or gender information, and Black or Hispanic men with missing age information—are combined in the "Other" category. The demographic shares for predicted shooting victims (third horizontal bar) are based on the 327,127 people in the prediction sample reweighted by their predicted risk of victimization (see Appendix A.5.1 for details).

Figure 5: Precision across models with different feature sets



Note: Figure shows precision, or the share actually victimized during the outcome period, of the $k \leq 5,000$ people with the highest predicted risk of shooting victimization, for models trained with different feature sets. Due to noise in precision at low values of k, we start the graph at $k = 500$. Bootstrapped 95 percent confidence intervals shown (see Appendix A.5.2 for details).

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Online Appendix for "Machine Learning Can Predict Shooting Victimization Well Enough to Help Prevent It"

Sara B. Heller, Benjamin Jakubowski, Zubin Jelveh & Max Kapustin

A Methods

This section provides additional details regarding our modeling process. First, we describe the raw CPD data, as well as the record linkage algorithm used to identify unique people across records. Next, we discuss the features (predictors) generated from these records. Finally, we discuss how we construct a set of cohorts and use them for model training and evaluation.

A.1 Data

Our model predicts a person's probability of becoming a shooting victim in the 18 months following the date of prediction, using information from 12.7 million CPD records. Available records describe 3,783,724 arrest, 8,911,412 victimization, 23,859 shooting, and 11,598 homicide events that occurred between August 1999 and October 2019 in Chicago.[40] We proceed to describe the relevant attributes of each of type of event record.

A.1.1 Arrest records

CPD arrest records include a unique person identifier (an *Illinois Record (IR) number*), based on a fingerprint scan, that allows us to construct a person's entire CPD arrest history. In addition to this person identifier, CPD arrest records include an incident identifier that can be used to link together the arrestees and victims associated with a single incident. The arrest data also contain police-recorded information about: arresting charges,[41] charge descriptions, and UCR codes; the location and time of the incident and the arrest; demographics of the arrestee; and information about whether the arrest was gang related (and, if so, the arrestee's CPD-identified gang affiliation).

A.1.2 Victim records

While victimization records include an incident identifier, they do not include a unique person identifier. As such, the raw data do not allow us to construct a person's entire victimization history. However, the records provide each victim's identifying information (including name, home address, and date of birth), and therefore support probabilistic

[40] The shooting data start in 2010.

[41] Arrests are associated with one or more arresting charge, and our arrest features consider the full set of charges on the arrest.

record linkage, described below. The victimization records also contain information about the type of victimization incident, including a description and UCR code.

A.1.3 Shooting and homicide victimization records

Similar to victimization records, shooting and homicide victimization records include an incident identifier but no unique person identifier. These records also include demographic information, facilitating record linkage. Shooting and homicide records are used to construct our outcome.

A.2 Record linkage

While CPD arrest records include a unique person identifier, victimization, shooting, and homicide records do not. As such, we use a probabilistic record linkage algorithm to associate unique individuals with all of their records across the CPD data. For details on the algorithm itself, see McNeill and Jelveh (2021). In this section, we describe the basics of the linking procedure.

To link CPD records that refer to same person, we take the post-2010 IR number (the person identifier associated with arrest records) as ground truth, allowing us to identify the set of unique individuals arrested during the study period and to associate these individuals with their arrest records.[42] Since records are already linked within the arrest data, probabilistic record linkage primarily allows us to address two remaining data challenges: associating arrested individuals with their non-arrest records—the various victimization incidents—and identifying the unique individuals represented across the victimization, shooting, and homicide data who did not experience a CPD arrest during the study period.

Our record linkage algorithm produces a collection of records referring to the same person which we call a *cluster*. In assigning records to clusters, the algorithm follows researcher-specified rules based on the context of the data. For our linkage, we specify the following constraints. First, a cluster can have at most one post-2010 IR number. Second, a homicide record cannot link to another record if the homicide record's event date came before the other record's event date. Third, 73.2 percent of victimization records do not have date-of-birth information—an important predictor of true positive links—which can lead to a large number of false positive links. To reduce the chance of these false positives, we introduce a constraint that if at least one record in a record pair is missing date-of-birth information, enforce that the age field (if not missing) in the two records is within 3 years. We also enforce that if at least one record in a potential cluster is missing date-of-birth information, all other records in the cluster not missing date-of-birth information must have similar dates of birth.[43]

The record linkage procedure identifies 5,426,703 individuals across the three decades of our data. We filter the set of clusters to exclude two sets of people: those with no

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

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

CPD records from the past 50 months relative to a given prediction date, and those who only had a single victimization in the past 50 months. We exclude these individuals for two reasons. First, they have much lower baseline risk: 0.01 percent of these individuals were shot during the follow-up period, compared to 0.7 percent of people in our cohort. Second, a large number of victimization records do not have date of birth information, making them more likely to incorrectly link to another record. To address this challenge, our linking algorithm was conservative in creating links that involved a record with no date of birth information. That is, a large share of clusters with only one record are clusters where that one record is a victimization record with no date of birth. As a result, dropping single victimizations implicitly reduces the influence of record-linkage error caused by poor data quality. As such, our sample selection criteria reduce data integrity issues, while still capturing most of the identifiable population with elevated risk.

A.3 Feature generation

Record linkage identifies the set of unique individuals represented in the CPD data, and associates each individual with their CPD arrest, victimization, shooting, and homicide record set. To predict an individual's risk of being shot as of a given prediction date, we aggregate over these associated records to construct *(person, prediction date)*-level features.[44] We construct four broad types of features: demographic, arrest, victimization, and network features. Table A.1 provides a summary of this final feature set, described by type below.

When an individual has no data in either the arrest or the victimization records, we assign a count of 0 to each relevant set of features. For the time-since features, which are not counts, we assign a missing value to the relevant features rather than a 0, and program the LightGBM package to include those instances and count their features as missing. Similarly, when a categorical feature is missing (e.g., police beat or gender), we assign a special category which is treated as missing. If an individual is missing network features due to having no co-arrests or co-victimizations, we assign 0s for those features and include an indicator that the set of those features is missing (i.e., the person is not part of the network map).

[44] When generating features for a given *(person, prediction date)*, we restrict to records available prior to the prediction date.

Table A.1: Feature counts by type and subtype

Feature Type	Feature Subtype	Count
Demographics	Age	4
Demographics	Race	3
Demographics	Gender	3
Demographics	Police Beat	3
Arrest	Indexed	104
Arrest	Fine-Grained	365
Arrest	Gang	3
Victimization	Indexed	84
Victimization	Fine-Grained	233
Network	1st and 2nd Degree	598
Network	Centrality, degree	10
Total		1,406

A.3.1 Demographic features

We construct 13 demographic features from information on an individual's age, race, gender, and home address.[45] As with most administrative data, police records are often noisy, with different values of theoretically invariant characteristics appearing across multiple records for the same individual. We represent age and race using the modal value across an individual's record set. When exact date of birth is missing, we treat the age feature as missing and construct a missing indicator; this occurs only for 10,766 people who are only in the victimization records (i.e., have never been arrested). However, most of these records include an approximate age, which we use to construct an additional approximate age feature for each individual, as well as a similar missing indicator for approximate age information. [46] We represent gender using three separate features: an individual's (1) most recently recorded gender, (2) modal gender, and (3) the number of distinct genders with which they are associated. We summarize a person's home address and race using these same three types of features for their police beats.

A.3.2 Arrest features

We construct 472 features summarizing an individual's prior arrest history. These arrest features fall into three broad types: indexed arrest features, fine-grained arrest features, and gang features.

To compute indexed arrest features, we bucket the charges associated with an arrest into several broad, overlapping categories: domestic incidents, drug crime, drug dealing, gun assault or battery, gun battery, gun robbery, property crime, violent crime, Part I

[45] For discussion regarding the inclusion of race in the model, see Appendix B.3.2.

[46] We combine true and approximate age information to classify people as over- or under-23 when reporting performance metrics.

violent crime, and all types of crimes. Then, we summarize individual arrest histories within each index using three types of time-aware features:

1. Time since first indexed arrest;
2. Time since most recent indexed arrest;
3. Total number of indexed arrests within the following time windows: the previous 30, 60, 90. 180, 270, 365, or 730 days, and over the individual's entire CPD arrest history (beginning in August 1999).

While these indexed arrest features provide a rich summary of an individual's arrest history, they could still potentially mask heterogeneity in the predictive value of different sorts of incidents collapsed into each index. As such, we augment our representation of prior arrests with 365 fine-grained arrest features that count how many arrests an individual has, within each time window, by unique UCR code and charge.

Finally, in addition to indexed and fine-grained prior arrest features, we compute three measures of an individual's prior CPD-identified gang affiliation. These measures include (i) an indicator of whether the individual has any prior gang-affiliated arrests, (ii) the number of unique gangs with which an individuals has been associated, and (iii) the most recent gang with which an individual is associated.

A.3.3 Victimization features

We construct 317 features summarizing an individual's history of victimization. Paralleling our treatment of prior arrests, we compute both indexed and fine-grained measures of prior victimization, using the same time windows and indices.

A.3.4 Network features

Since CPD arrest, victimization, shooting, and homicide records all share an event identifier, we construct a network using information on events within five years of the prediction date that includes two types of links: (i) links between co-arrestees, and (ii) links between arrestees and victims.[47] After constructing this network, we generate two types of features summarizing an individual's position within it.

First, we compute aggregate statistics describing an individual's network connections (whom we also refer to as neighbors). We compute two types of aggregate statistics. The first counts incidents, while the second counts people. Specifically, the first type of aggregate counts the number of incidents involving an individual's neighbors, by incident type and time window. For example, we count the number of property crime incidents that occurred in the last 365 days and resulted in the arrest of a neighbor. The second type of aggregate counts the number of neighbors involved in incidents, again by incident type and time window. For example, we count the number of neighbors arrested for property crime incidents within the last 365 days. We compute these two types of aggregates separately for an individual's first- and second-degree network connections.

[47] Note this corresponds to the bipartite projection of the bipartite $person \leftrightarrow incident$ graph.

Second, we compute features describing the underlying network structure, including an individual's degree and eigenvector centrality, as well as the maximum degree and eigenvector centrality of their first- and second-degree neighbors.

A.4 Model training

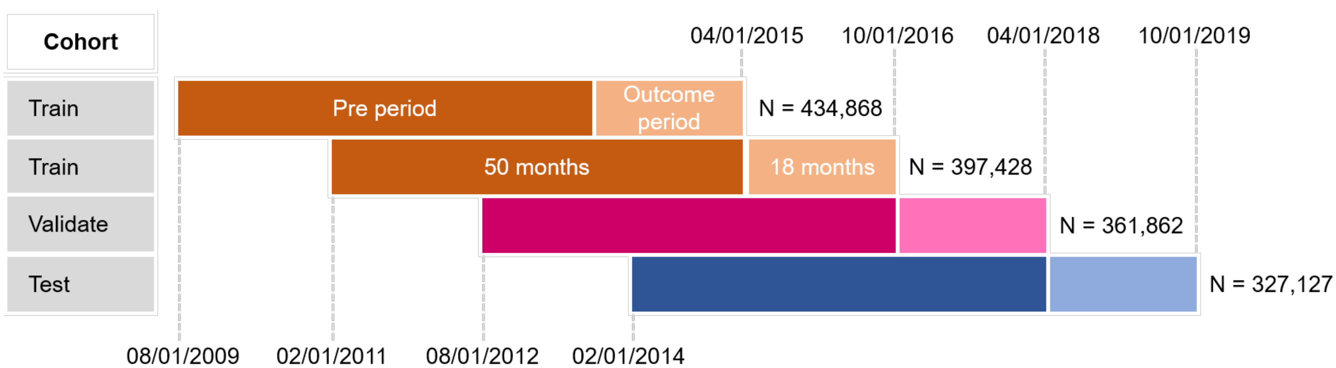
To maximize flexibility, especially in the top tail of the risk distribution, we train and test a gradient-boosted decision tree model (Friedman, 2002). Because we include network features in the model, we cannot use traditional sub-sampling to generate a hold-out test set. Even if individual i were part of a randomly sub-sampled test set, information about i's risk could still be used in model-building to the extent he has peers in the training data and appears in their features. To avoid this kind of overfitting, we divide the data into calendar time cohorts as follows.

A.4.1 Defining cohorts

To define cohorts, we first establish four non-overlapping 18-month outcome periods. Then, we identify cohorts of individuals who have had either an arrest or two victimizations during the 50 months preceding each 18-month outcome period (see Figure A.1).

We use the first two cohorts to train the model. We split the third cohort into a 50 percent validation set for hyperparameter tuning, a 25 percent set for calibrating the predictions from the model, and a 25 percent set to optimize the number of trees in the gradient boosting model via "early stopping" (Raskutti et al., 2011).[48] The final cohort is our test set, where we predict shooting risk ($\hat{p}$) for the out-of-sample 18-month outcome period starting on April 1, 2018.

Figure A.1: Model cohort structure



A.4.2 Hyperparameter tuning

We optimize the performance of our gradient-boosted decision tree model using random search over the following hyperparameters: number of leaves, minimum number of data

[48] We found that model calibration was not meaningfully improved when we applied our calibration procedure, therefore we only report the results for the raw predictions in this paper.

in each leaf, learning rate, and the fraction of data instances and features to use in building each tree. Our random search procedure is as follows:

1. We randomly sample $N = 100$ hyperparameter configurations from this search space.
2. For each hyperparameter configuration, we fit a gradient-boosted decision tree model over the two training cohorts, using early stopping (based on minimizing log loss on a partition of the validation cohort) to optimize the number of rounds of boosting (i.e., the number of decision trees in the ensemble).
3. From this set of $N = 100$ random hyperparameter configurations, we select the configuration that maximizes precision evaluated at the rank that equals the number of shooting victims in the validation set.
4. Finally, we refit the model, using the selected hyperparameters, over the combined training and validation cohorts.

A.5 Model evaluation

We evaluate the performance of our model on the test set (prediction sample). While our primary evaluation metrics are described in the main paper, this section provides additional detail on (i) construction and interpretation of the $\hat{p}$-weighted prediction sample (Figure 4) and (ii) construction of bootstrap confidence intervals for precision and recall at k (Figures 2 and 5, and Appendix Figure B.4).

A.5.1 $\hat{p}$-weighted prediction sample

Figure 4 includes a "Predicted victims" series that shows the demographic composition of a weighted sample, where individuals in the prediction sample are weighted based on their predicted shooting risk $\hat{p}_i$. Specifically, for a given demographic subgroup G, this series shows

$$\text{\% in demographic group } G = \frac{\sum_{i \in G} \hat{p}_i}{\sum_i \hat{p}_i}$$

where $\hat{p}_i$ is predicted risk for the i^{th} individual. If the model generated perfect predictions, then the demographic composition of predicted victims would be the same as the demographic composition of actual victims in the prediction sample. As such, differences between the second and third horizontal bars in Figure 4 indicate misprediction.

A.5.2 Bootstrap confidence intervals

Appendix Tables B.1 and B.5, as well as Figures 2, 5, and Appendix Figure B.4, include 95 percent bootstrap confidence intervals for several statistics at different k. These are constructed from 1,000 bootstrap samples, where each bootstrap sample is generated by:

1. Bootstrap resampling the prediction sample (i.e., drawing $N_{prediction} = 327,127$ instances from the test set, with replacement).

2. Within each bootstrap sample, computing Precision_k and Recall_k at different k (e.g., $k = 1, 2, \ldots, 5000$).

The 95 percent confidence intervals report the 2.5^{th} and 97.5^{th} percentiles from this bootstrap distribution.

While this bootstrap procedure characterizes prediction set sample variance, it does not account for other sources of variation in our procedure (e.g., training set sample variance, or explicit randomness in the gradient boosting algorithm).

B Additional Results

B.1 Prevalence of other outcomes among those with high predicted risk of shooting victimization

The main text reports predictive performance for the primary outcome of interest, shooting victimization, when ranking people by their predicted risk of that outcome. Because the risk of being shot is likely correlated with the risk of other socially costly outcomes, efforts to reduce the risk of shooting victimization among this group may reduce the risk of these other outcomes as well. We do not focus on quantifying the benefits of reducing the risk of these other outcomes, since they are less reliable measures of the underlying behavior of interest (i.e., the relationship between arrest for violent crime and true violent offending is likely to be noisier and to differ by racial group, relative to the relationship between shooting victimization in the police data and true shooting victimization).

Nonetheless, because efforts to prevent shooting victimization among this group may produce other large benefits, this section reports on the prevalence of other measures of violence among those predicted to be at high risk of shootings. Note that we are not training a model to predict these other outcomes, since that would likely confound police behavior or willingness to report violence to the police with true individual risk. Rather, we are reporting on the prevalence of different violence measures among groups defined by their ranking in the shooting victimization predictions.

Appendix Table B.1 below reports our standard measures of model performance, precision and recall, for the full shooting victimization model evaluated on four different outcomes: shooting victimization, shooting arrest, violent crime victimization, and violent crime arrest.

Table B.1: Predictive performance of shooting victimization predictions for other outcomes

k	Precision	Recall	Total Recall
Shooting Victim			
500	0.128 (0.100, 0.162)	0.028 (0.022, 0.036)	0.019 (0.015, 0.024)
3,381	0.095 (0.085, 0.106)	0.143 (0.128, 0.158)	0.095 (0.085, 0.106)
327,127	0.007 (0.007, 0.007)	1.000 (0.957, 1.043)	0.666 (0.638, 0.695)
Shooting Arrest			
500	0.036 (0.020, 0.054)	0.050 (0.028, 0.075)	0.039 (0.022, 0.058)
3,381	0.025 (0.020, 0.030)	0.234 (0.187, 0.284)	0.181 (0.144, 0.219)
327,127	0.001 (0.001, 0.001)	1.000 (0.897, 1.103)	0.772 (0.692, 0.852)
Violent Crime Victim			
500	0.208 (0.174, 0.246)	0.006 (0.005, 0.007)	0.003 (0.002, 0.003)
3,381	0.178 (0.166, 0.189)	0.037 (0.034, 0.039)	0.015 (0.014, 0.015)
327,127	0.050 (0.049, 0.051)	1.000 (0.984, 1.015)	0.397 (0.390, 0.403)
Violent Crime Arrest			
500	0.184 (0.150, 0.218)	0.019 (0.015, 0.022)	0.012 (0.010, 0.015)
3,381	0.157 (0.145, 0.170)	0.107 (0.099, 0.116)	0.071 (0.065, 0.076)
327,127	0.015 (0.015, 0.016)	1.000 (0.973, 1.028)	0.658 (0.641, 0.677)

Note: Performance and recall from the full model trained to predict shooting victimization during the 18-month outcome period starting April 1, 2018. Bootstrapped 95 percent confidence intervals are in parentheses. Model performance is evaluated on the four outcomes shown, for the k people with the highest predicted risk of shooting victimization. Violent crimes refer to the Part I violent index offenses: aggravated assault, aggravated battery, forcible rape, murder, and robbery. Prediction sample size is 327,127.

The people whom the model predicts to be at higher risk of shooting victimization are indeed at higher risk for these other adverse outcomes during the 18-month outcome period as well. For example, among the 500 people at highest predicted risk of shooting victimization, 3.6 percent are arrested on suspicion of carrying out a shooting (36 times the base rate in the whole test set of 0.1 percent); 20.8 percent are reported as victims of a violent offense (4.2 times the base rate); and 18.4 percent are arrested on suspicion of carrying out a violent offense (12.3 times the base rate).

B.2 Victim counts and performance by demographic group

Figures 3 and 4 in the main text show the proportion of shooting victims that fall into different demographic groups. This section adds some additional information to the summaries in the main text. To be transparent about the underlying size of each group, Appendix Table B.2 below reports the counts across demographic categories of four groups: all shooting victims, shooting victims in the prediction sample, predicted victims (see discussion above in Appendix A.5.1), and the $k = 3,381$ people with the highest predicted risk.

Table B.2: Demographic composition of actual and predicted shooting victims

Race	Gender	Age	Actual victims (N=3381)	Actual victims (in sample) (N=2253)	Predicted victims (rounded)	Top 3,381
Black	Male	<23	887	673	604	2134
		23+	1454	1071	1011	923
	Female	<23	159	65	48	3
		23+	214	123	108	0
Hispanic	Male	<23	210	112	110	186
		23+	276	152	187	121
	Female	<23	22	10	10	0
		23+	47	15	21	0
White	Male	<23	10	6	7	5
		23+	46	19	28	9
	Female		13	6	10	0
Other/Missing			43	1	8	0

Note: Counts for White females of all ages reported due to small cell sizes.

Figure 1 in the main text shows that the predictions are well-calibrated overall and by racial group, with some overestimation among those predicted to be at the very highest risk within the distributions for White and Hispanic individuals. Appendix Table B.3 sheds additional light on calibration by contrasting the base shooting victimization rate within the prediction sample and the average prediction, both by race/ethnicity (as in Figure 1) and further broken down by age and gender (as in Figure 4).

Consistent with the calibration plots in the main text (Figure 1), average predictions are generally quite similar to observed rates of shooting victimization, even within race/ethnicity-age-gender groups. The model slightly under-predicts risk for Black men and women (by anywhere from 0.001 for younger Black women to 0.003 for younger Black men), while it slightly over-predicts risk for older Hispanic men (by 0.001) and older White women.

Table B.3: Base rate and average predicted risk by race, gender, and age for prediction sample

Race	Gender	Age	N	Base Rate by Group	Mean predicted risk
Black	All	All	199192	0.01	0.009
	Female	All	77371	0.002	0.002
		<23	12187	0.005	0.004
		23+	65184	0.002	0.002
	Male	All	121782	0.014	0.013
		<23	18991	0.035	0.032
		23+	102790	0.01	0.01
Hispanic	All	All	68613	0.004	0.005
	Female	All	20333	0.001	0.001
		<23	3450	0.003	0.003
		23+	16882	0.001	0.001
	Male	All	48260	0.005	0.006
		<23	7661	0.015	0.014
		23+	40597	0.004	0.005
White	All	All	49710	0.001	0.001
	Female	All	18443	<0.001	0.001
		<23	1264	0.001	0.001
		23+	17177	<0.001	0.001
	Male	All	31235	0.001	0.001
		<23	2291	0.003	0.003
		23+	28933	0.001	0.001
Other Race/Gender		All	7287	<0.001	0.001
Missing Race/Gender/Age		All	2434	<0.001	0.001

Note: Table shows the base rate, or the proportion of each group that becomes a shooting victim during the outcome period, along with the average predicted risk within each group. Note that the "All" age rows include individuals of that race/ethnicity and gender who are missing age information; as a result, the number of observations in the under- and over-23 rows do not exactly total to N for the "All" row. The final row groups everyone with missing race/ethnicity, gender, and/or age information together.

Of course, average predictions being similar to base rates at a group level does not mean each individual's prediction is accurate. To assess accuracy at the individual level, one must establish a decision rule that translates predicted risk levels into classifications of "positive" (predicted to be shot) and "negative" (predicted not to be shot) for each person. There are many different classification rules one could use. Given the uneven demographic distribution of individuals across the risk distribution, different decision rules could have different implications for who is correctly and incorrectly classified.

Since a natural kind of decision rule is a threshold rule, where policymakers would consider everyone above some global risk threshold as a positive prediction and everyone below as a negative prediction, we show the implications of one such threshold (the same that is shown in Figure 4): serving the 3,381 individuals with the highest predicted risk (motivated by the fact that there are 3,381 actual victims in the outcome period). Appendix Table B.4 shows precision and average predicted risk within race/ethnicity and age groups for the subset of men among the top 3,381 highest predictions. We omit women and the age breakdown for White men in this table because there are so few of these individuals in this top-ranked group.

Table B.4: Precision and average predicted risk by race and age for men among the top 3,381

Race	Gender	Age	N	Precision	Mean predicted risk
Black	Male	All	3057	0.098	0.102
		<23	2134	0.105	0.106
		23+	923	0.083	0.093
Hispanic	Male	All	307	0.065	0.099
		<23	186	0.07	0.098
		23+	121	0.058	0.099
White	Male	All	14	0.0	0.15

Note: Table reports statistics for White males of all ages together and omits 6 individuals belonging to other demographic groups due to small cell sizes.

Comparing the two columns gives a sense for subgroup calibration for this subsample, and precision shows the proportion of true positives (such that $1 - Precision$ is the false discovery rate). Again we emphasize that this not reflective of performance across the whole sample, but rather provides additional information on the fairness implications of a "top 3,381" decision rule.

Comparing the mean predicted risk with the realized risk (precision) in Appendix Table B.4 shows several key patterns. First, consistent with the subgroup calibration panels in Figure 1, predicted risk among this top tail is quite close to the realized risk for Black men, but overstates the realized risk for Hispanic and White men on average. The age breakdown further shows that the predictions are best calibrated for younger minority men, who tend to have fewer but more recent police contacts. The older minority men chosen with this decision rule tend to have elevated predictions relative to their realized risk. We note that the reported numbers for White men have huge implicit confidence intervals since there are only 14 of them; while it is notable that none of these 14 is shot during the outcome period, we hesitate to over-interpret a pattern from so few individuals.

In terms of classification among the top 3,381, the model has the highest true positive rate (and thus lowest false discovery rate) for Black men, of whom 9.8 percent are correctly classified, i.e., become shooting victims in the outcome period. In contrast, among Hispanic men—a much smaller group of 307 compared to 3,057 Black men—only 6.5 percent are correctly classified. This is consistent with argument in the main text that the over-representation of Black men in the top tail of the risk distribution is not because estimates of their risk are distorted (inflated), but rather because the model does a better job at identifying Black men who face genuinely higher risk of victimization. These true positive rates are extremely high from a substantive standpoint, identifying 300 Black men and 20 Hispanic men for whom preventive services might have kept them from serious injury or death. Nonetheless, the fact that 90 percent of Black men and 94 percent of Hispanic men above this threshold are not shot during the outcome period again emphasizes how costly it would be target any intervention that reduced people's civil liberties based on these predictions.

B.3 Further detail on what matters for prediction

B.3.1 Performance by groups of features

The main text presents predictive performance leaving out 3 sets of features: own arrests, peer information (networks), and both (Figure 5). We perform a similar exercise, dropping sets of features and retraining a new model, for additional combinations of features. Appendix Figure B.1 reports precision for the full model and different models that each exclude certain feature sets. To ensure the lines are not all on top of each other, we limit the scale to the top 5,000 ranked individuals in each model. Past 5,000, most of the differences in performance tend to be quite small. We do not show confidence intervals to make the figure more readable, but note that there is a fair amount of noise from sampling variation at any given k. Appendix Table B.5 quantifies the precision differences and 95 percent bootstrapped confidence intervals at $k = 500$ and $k = 3,381$, as well as reporting recall and total recall.

Figure B.1: Precision across models with different feature sets

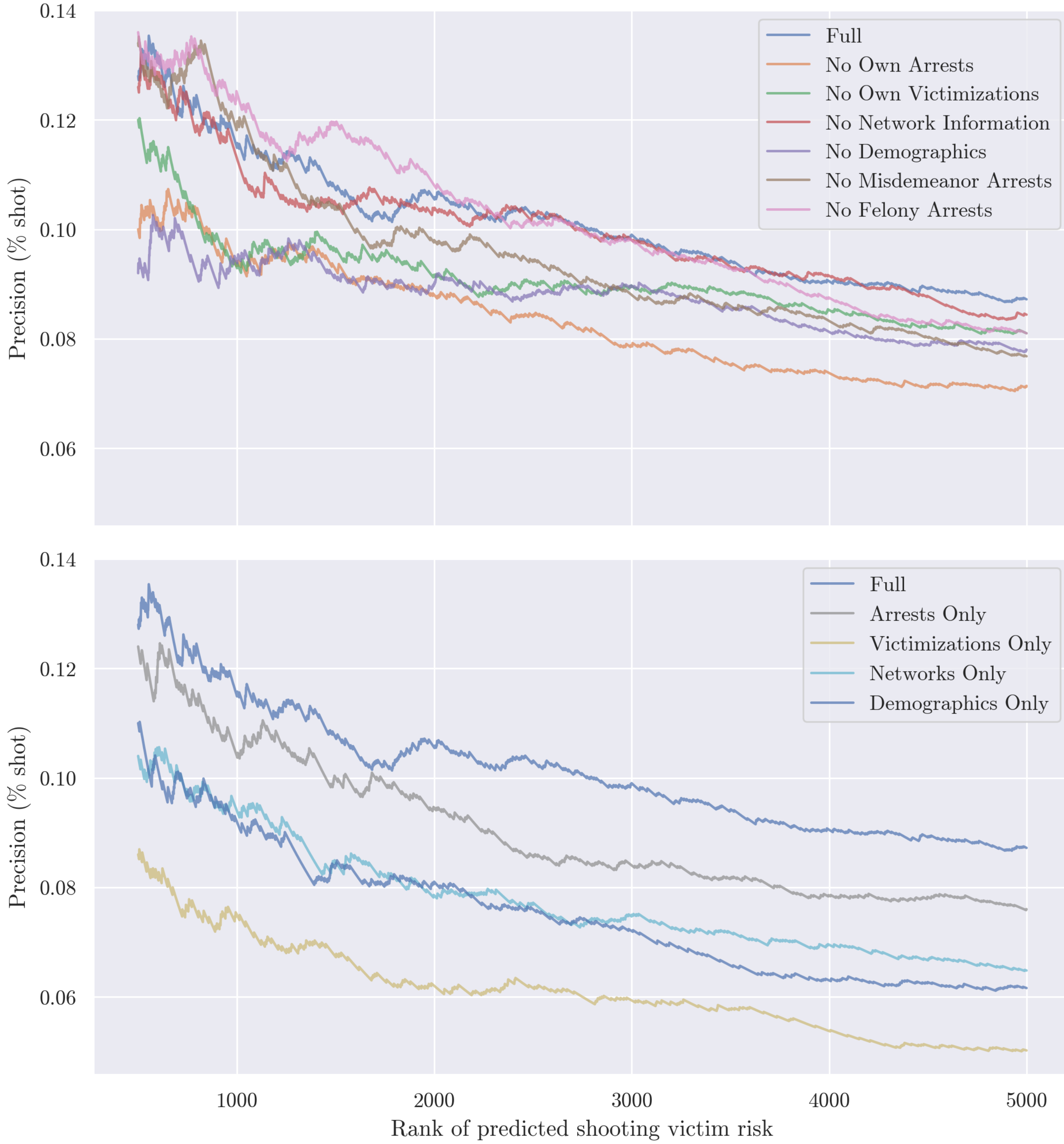


Table B.5: Predictive performance by feature set

Feature Set	Top 500			Top 3,381		
	Precision	Recall	Total Recall	Precision	Recall	Total Recall
Full	0.128 (0.107, 0.166)	0.028 (0.024, 0.037)	0.019 (0.016, 0.025)	0.095 (0.086, 0.108)	0.143 (0.129, 0.161)	0.095 (0.086, 0.108)
No Network Information	0.126 (0.097, 0.151)	0.028 (0.022, 0.034)	0.019 (0.014, 0.022)	0.095 (0.086, 0.107)	0.142 (0.130, 0.160)	0.095 (0.086, 0.107)
No Felony Arrests	0.136 (0.103, 0.159)	0.030 (0.023, 0.035)	0.020 (0.015, 0.024)	0.094 (0.086, 0.103)	0.142 (0.129, 0.154)	0.094 (0.086, 0.103)
No Own Victimizations	0.120 (0.092, 0.141)	0.027 (0.020, 0.031)	0.018 (0.014, 0.021)	0.088 (0.081, 0.098)	0.133 (0.121, 0.147)	0.088 (0.081, 0.098)
No Race	0.118 (0.092, 0.147)	0.026 (0.020, 0.033)	0.017 (0.014, 0.022)	0.088 (0.079, 0.099)	0.131 (0.119, 0.149)	0.088 (0.079, 0.099)
No Misdemeanor Arrests	0.134 (0.101, 0.163)	0.030 (0.022, 0.036)	0.020 (0.015, 0.024)	0.087 (0.081, 0.096)	0.130 (0.121, 0.143)	0.087 (0.081, 0.096)
No Own Arrests or Victimizations	0.120 (0.094, 0.150)	0.027 (0.021, 0.033)	0.018 (0.014, 0.022)	0.087 (0.078, 0.098)	0.130 (0.117, 0.147)	0.087 (0.078, 0.098)
No Demographics	0.092 (0.071, 0.123)	0.020 (0.016, 0.027)	0.014 (0.010, 0.018)	0.087 (0.078, 0.097)	0.130 (0.117, 0.146)	0.087 (0.078, 0.097)
Arrests Only	0.124 (0.097, 0.167)	0.028 (0.022, 0.037)	0.018 (0.014, 0.025)	0.082 (0.074, 0.092)	0.123 (0.111, 0.137)	0.082 (0.074, 0.092)
Arrests + Networks Only	0.100 (0.077, 0.127)	0.022 (0.017, 0.028)	0.015 (0.011, 0.019)	0.079 (0.073, 0.088)	0.119 (0.109, 0.133)	0.079 (0.073, 0.088)
No Own Arrests	0.100 (0.073, 0.132)	0.022 (0.016, 0.029)	0.015 (0.011, 0.020)	0.077 (0.068, 0.086)	0.115 (0.102, 0.129)	0.077 (0.068, 0.086)
Networks Only	0.104 (0.078, 0.132)	0.023 (0.017, 0.029)	0.015 (0.012, 0.020)	0.072 (0.065, 0.080)	0.107 (0.098, 0.120)	0.072 (0.065, 0.080)
No Own Arrests, No Network Information	0.112 (0.085, 0.140)	0.025 (0.019, 0.031)	0.017 (0.013, 0.021)	0.069 (0.061, 0.078)	0.103 (0.092, 0.117)	0.069 (0.061, 0.078)
Demographics Only	0.110 (0.085, 0.138)	0.024 (0.019, 0.031)	0.016 (0.013, 0.020)	0.067 (0.061, 0.077)	0.101 (0.092, 0.115)	0.067 (0.061, 0.077)
Victimizations Only	0.086 (0.064, 0.111)	0.019 (0.014, 0.025)	0.013 (0.009, 0.016)	0.058 (0.052, 0.065)	0.087 (0.078, 0.097)	0.058 (0.052, 0.065)

Note: Models differ based on the feature sets available to them during training (see text below). Model performance is evaluated on shooting victimization during the outcome period for the $k = 500$ and $k = 3,381$ people with the highest predicted risk of shooting victimization. Bootstrapped 95 percent confidence intervals are in parentheses. Models are sorted by Total Recall for the top 3,381.

The definitions of the models that leave out particular feature sets are as follows:

Top panel

1. Full: The main model reported in the text with all available features
2. No Own Arrests: Excludes all arrest features for the focal person (but includes them for first- and second-degree peers)
3. No Own Victimizations: Excludes all victimization features for the focal person (but includes them for first- and second-degree peers)
4. No Network Information: Excludes all features about the focal person's first- and second-degree peers
5. No Demographics: Excludes race, gender, age, and location information
6. No Misdemeanor Arrests: Excludes all misdemeanor arrests when constructing features, altering counts for focal person as well as first- and second-degree peers
7. No Felony Arrests: As above but excludes felony arrests and includes misdemeanor arrests

Bottom panel

1. Full: Same as above
2. Arrests Only: Uses only information on arrests, excluding victimization and demographic information
3. Victimizations Only: Uses only information on victimizations, excluding arrests and demographic information
4. Networks Only: Uses only information on first- and second-degree peers, excluding all information on the focal person
5. Demographics Only: Uses only information on demographics, excluding arrests and victimization information

As the top panel shows, the feature set that reduces precision the most when excluded is a person's own arrest history. Excluding a person's own victimization history or demographic information also appears to reduce precision substantially for the $k = 2,500$ people with the highest predicted risk, though we cannot distinguish these apparent performance differences relative to the full model from statistical noise.[49]

As the bottom panel shows, the biggest loss of information comes from using only victimization records when building the model. Using just demographics or just network features does slightly better than victimizations alone, but not as well as a model trained using just own arrest features. This pattern echoes the finding in the main text that the information contained in arrests records—including information a person's network neighbors derived from these records—is particularly valuable in predicting the risk of being a shooting victim.

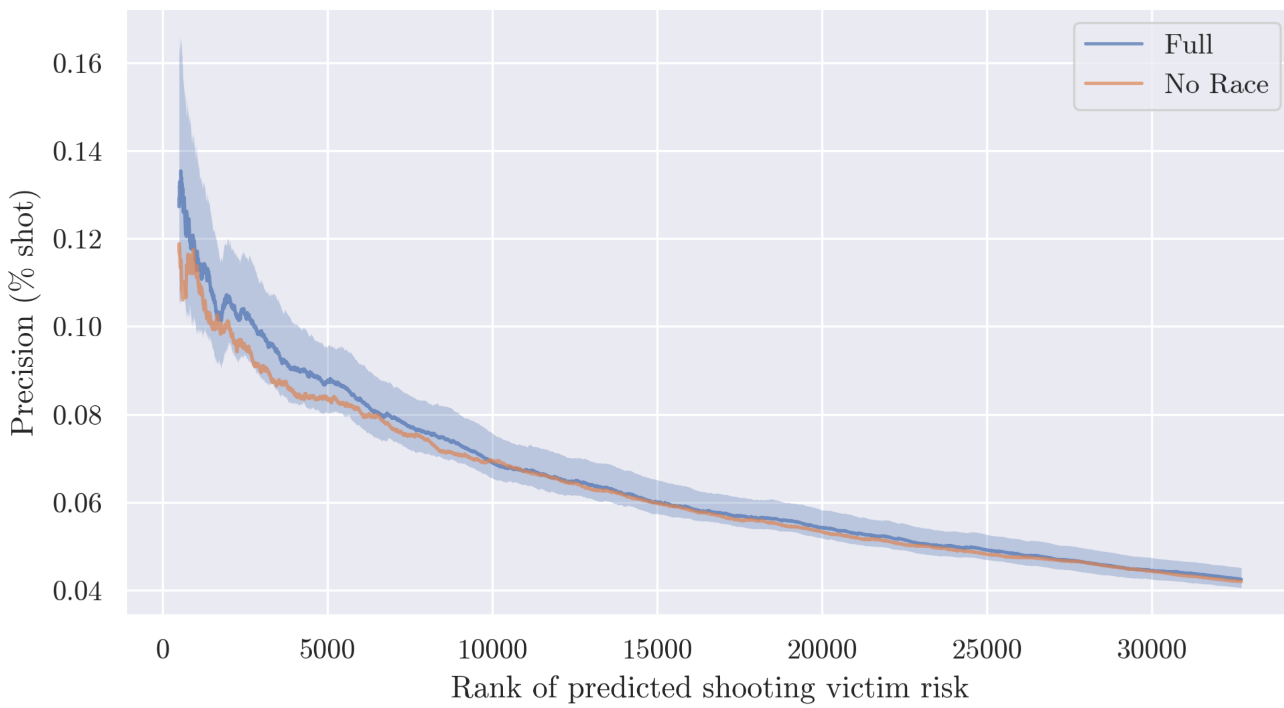
[49] Noise helps to explain why there is a small gap above $k = 1,500$ where the "no felony arrest" model appears to outperform the full model.

B.3.2 Prediction without race

Our main results come from a model that includes race in the model-building process. Many legal scholars believe that including race as an algorithmic input is likely unconstitutional, though the debate around this question is not completely settled (e.g., Yang and Dobbie, 2020).

Importantly, as shown in Appendix Figure B.2, the inclusion of race has a relatively trivial effect on predictive performance.[50] There is perhaps some small loss of precision at the extreme top of the risk distribution, but it is not statistically distinguishable from the full model that uses race. This is not unusual in settings where many other features in the prediction are correlated with race (Starr, 2014).

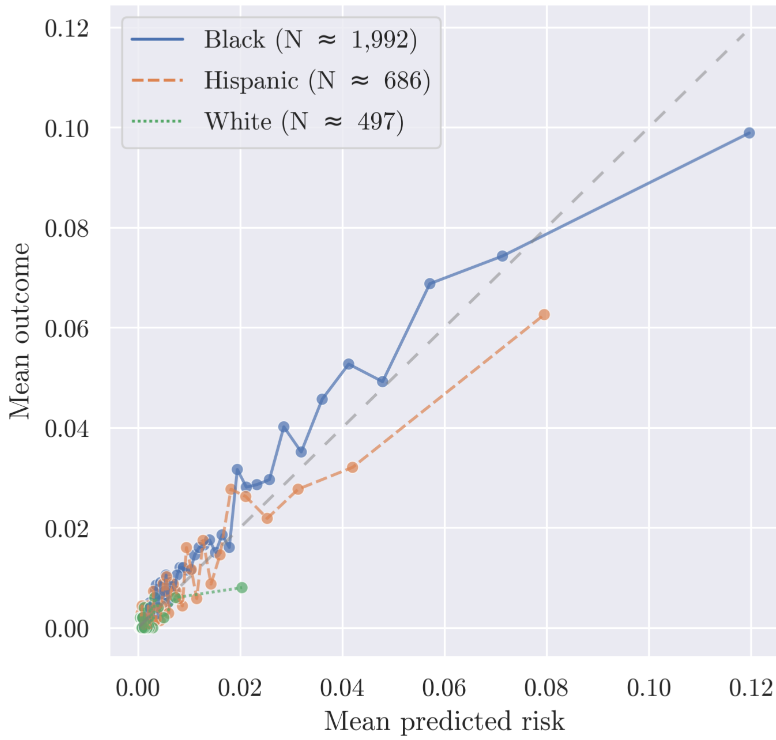
Figure B.2: Precision across models with and without race indicators



Note: Bootstrapped 95 percent confidence interval for full model shown. See Appendix A.5.2 for details.

[50] We show performance for the top 10 percent of the sample, since showing the full sample makes the scale too small to see the very small differences at the top.

Figure B.3: Calibration for model built without race indicators



Appendix Figure B.3 also shows little change in calibration within race/ethnicity groups relative to the full model shown in Figure 1. So although we show the main results from a model including race, the arguments contained in the paper are equally applicable for settings that require the model to exclude race.[51]

B.3.3 Performance by number of features & model complexity

A different way to ask what information matters is not to focus on sets of features grouped by theme, but on the number of features available and the complexity of the algorithm used to predict with them. Black box models may not be appropriate in all high-stakes settings (Rudin, 2019). A simpler model with only a few features may aid in interpretability, trust, and uptake (Ustun and Rudin, 2019). Multiple researchers have identified settings where complex models with more features provide minimal performance improvements over simple models with fewer features (e.g., Dressel and Farid, 2018; Jung et al., 2017; Angelino et al., 2018; Stevenson and Slobogin, 2018; Stevenson and Mayson, 2021). Thus, for use in these contexts, it is important to understand how much of the predictive accuracy of the full model can be captured by a drastically smaller set of features and simpler modeling techniques.

We explore these questions in our setting by first creating a rank-ordered set of 50 features from the full set of all 1,406 features. To generate this smaller set of 50 features,

[51] When a somewhat different version of this prediction model was used for social service referrals in practice, we excluded race; see, e.g., `https://osf.io/ap8fj/`.

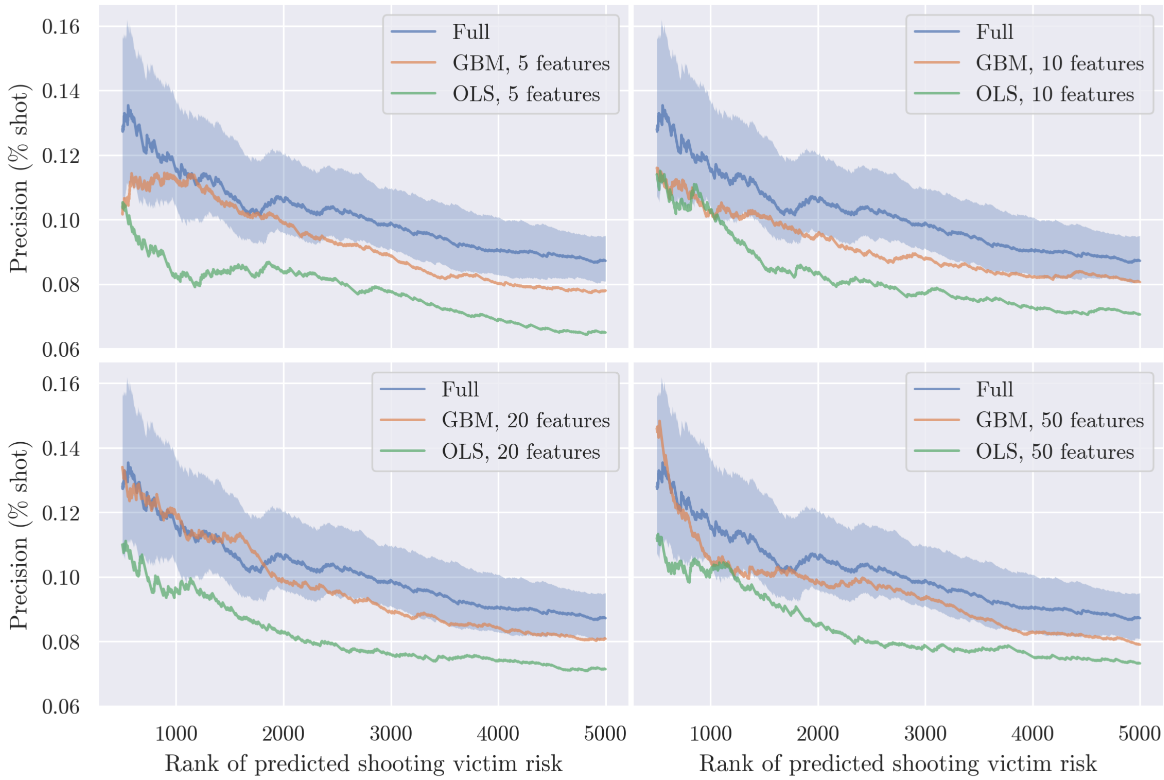
we use a simple stepwise residualization procedure. First, we select the single feature that is most highly correlated with shooting victimization in the first two cohorts. Then we remove the correlation between all other features and the selected feature. To do so, we replace the value of each unselected feature with the residual from a linear regression of each unselected feature onto the feature with the highest correlation. We then repeat the process, searching each time for the feature with the highest remaining correlation with the outcome after removing the correlation with already-selected features. Given a particular collection of features to start with, this approach produces a rank ordering of the features in that collection with the highest linearly independent relationship with the outcome.

Finally, we build models using both gradient-boosted decision trees (GBM) and ordinary least squares (OLS) using only the $n \in \{5, 10, 20, 50\}$ highest-ranked features, comparing their performance to that of the full model built using GBM with 1,406 features.

Appendix Table B.6 reports the set of 50 features chosen by this process. The first column shows the set to which each feature belongs; the second column provides a description of the feature, where text in parentheses indicate a subtype of the feature; the third column shows, where appropriate, the time window over which the feature was measured, where “Total” indicates features that look back to the beginning of the data (August 1999); the fourth column shows the correlation between the residualized version of the feature and the outcome; and the fifth column shows the correlation between the unresidualized version of the feature and the outcome.

Appendix Figure B.4 reports the same precision plot as Figure 4, with separate panels for different numbers of the top n features reported in Appendix Table B.6. Each panel shows the precision for the full model, and for models using GBM and OLS with only the indicated top n features. Across the panels, the parsimonious GBM models perform similarly to the full model, with their precision usually falling within the full model’s confidence interval. In contrast, the parsimonious OLS models, while appearing to improve slightly in performance with the number of features available to them, still perform more poorly: even with $n = 50$ features, the precision of the OLS model is approximately two percentage points lower than that of the full model, although these differences are not always statistically significant when accounting for sampling variation. This pattern of results suggests that it may be possible to achieve similar performance to the full model using a relatively small set of features, but only with a flexible modeling technique like GBM rather than OLS.

Figure B.4: Precision across models with different model types and number of features



Note: Figure shows precision, or the share actually victimized during the outcome period, of the $k \leq 5,000$ people with the highest predicted risk of shooting victimization, for the full model, a gradient-boosted model with a limited set of features, and an ordinary least squares model with the same limited set of features (Appendix Table B.6). Due to noise in precision at low values of k, we start the graphs at $k = 500$. Bootstrapped 95 percent confidence interval for full model shown (see Appendix A.5.2 for details).

To provide further insight into which predictors provide the most independent information, and to emphasize how the information in features can be substitutable, we repeat the stepwise residualization procedure described above for the other feature sets shown in Figure 4. Appendix Tables B.7, B.8, and B.9 respectively show the list of the top 50 features identified by this process for the following three feature sets: no network features, no own-arrest features, and the combination of no network and no own-arrest features. We then reran our modeling process, but only gave the algorithm access to the 50 most correlated variables for each feature set, identified in our stepwise residualization procedure.

As the feature lists show, the top 50 predictors change quite a bit as different feature sets are removed. For example, the feature with the second highest partial correlation with shooting victimization in the full model is the number of first degree neighbors who are shooting victims. But when we build a model without network features, overall performance is just as high; as Appendix Table B.10 shows, both precision and recall at

$k = 500$ and $k = 3,381$ are quite similar across all models that use 50 features. This emphasizes the point in the main text that standard "importance" measures within a single model do not capture which variables are uniquely important to prediction; other correlated variables can often capture similar information when the "important" variables are removed. To get a clearer understanding of which kinds of features truly matter, in the sense that their removal would harm predictive performance, we must compare predictive performance in models trained without particular variables, as in the main text.

Appendix Table B.10 and Appendix Figure B.4 highlight that it is generally possible to achieve comparable performance to the full model in the tail. The biggest loss in performance is from removing all arrest information on both focal individuals and their neighbors. Of course, it is typically impossible to know *a priori* which small set of features will achieve performance as close as possible to a model with access to the full set of features. The process of solving this constrained optimization problem is itself a machine learning challenge (Rudin, 2019). In practice, settings that require smaller numbers of features could engage in this process.[52]

[52] See Luminosity and York (2020) for a real-world example of developing a risk assessment for pretrial arraignment decisions in New York City.

Table B.6: Top 50 features from the stepwise residualization procedure when given access to all feature sets

Feature Set	Description	Time Window	Correlations Residualized	Original
arrests	# of own-arrests (any)	730 days	0.109	0.109
networks	# of 1st degree neighbors victimized (shootings)	Total	0.064	0.093
arrests	Ever gang-affiliated		0.041	0.095
demographics	Age (modal)		-0.027	-0.052
victims	# of victimizations (shootings)	Total	0.026	0.064
arrests	# of own-arrests (property crime)	730 days	-0.025	0.036
arrests	# of own-arrests (public alcohol consumption)	730 days	-0.022	0.013
networks	# of victimizations (shootings) by 1st degree neighbors	Total	-0.020	0.090
arrests	# of own-arrests (criminal trespass–vehicle)	270 days	0.020	0.036
arrests	# of own-arrests (robbery)	Total	0.020	0.056
arrests	# of own-arrests (warrant)	730 days	-0.020	0.045
arrests	# of own-arrests (gambling)	Total	0.020	0.068
arrests	# of own-arrests (simple battery)	730 days	-0.016	0.027
arrests	# of own-arrests (reckless conduct)	Total	0.016	0.072
arrests	# of own-arrests (chicago municipal code)	730 days	-0.015	0.041
demographics	Sex (most recent)		-0.015	-0.017
networks	# of arrests (property crime) by 1st degree neighbors	365 days	0.015	0.075
arrests	# of own-arrests (solicitation)	730 days	0.015	0.055
arrests	# of own-arrests (criminal trespass–land)	730 days	-0.014	0.039
demographics	Missing date of birth		-0.014	-0.014
demographics	# of unique police beats		-0.013	0.027
arrests	# of own-arrests (possession controlled substance)	730 days	-0.013	0.024
arrests	# of own-arrests (drug)	Total	0.013	0.049
victims	# of victimizations (shootings)	270 days	0.012	0.040
arrests	# of own-arrests (firearm possession)	Total	0.011	0.035
arrests	# of own-arrests (traffic)	730 days	-0.011	0.030
networks	# of 2nd degree neighbors victimized (property crime)	60 days	-0.011	0.046
networks	# of arrests (violent crime) by 1st degree neighbors	270 days	0.010	0.072
arrests	# of own-arrests (aggravated assault school employee)	Total	0.010	0.027
arrests	# of own-arrests (obstructing identification)	Total	0.010	0.033
networks	# of victimizations (gun robbery) by 2nd degree neighbors	30 days	0.009	0.031
networks	# of victimizations (shootings) by 1st degree neighbors	730 days	-0.009	0.079
arrests	# of own-arrests (drug paraphenelia possession)	730 days	-0.009	0.001
victims	# of victimizations (drug abuse)	Total	-0.009	-0.002
arrests	# of own-arrests (gun robbery)	730 days	-0.009	0.014
arrests	# of own-arrests (solicitation)	Total	0.009	0.060
arrests	# of own-arrests (criminal trespass–real property)	730 days	-0.009	0.020
arrests	# of own-arrests (burglary)	Total	0.009	0.036
networks	# of 1st degree neighbors victimized (gun assault or battery)	270 days	0.008	0.068
networks	# of 1st degree neighbors arrested (gun battery)	60 days	-0.008	0.006
arrests	# of own-arrests (unique gang names)		-0.008	0.095
arrests	# of own-arrests (aggravated battery w/ firearm)	Total	0.008	0.024
arrests	# of own-arrests (drug deal)		0.008	-0.016
arrests	# of own-arrests (replica firearm–pellet gun)	Total	0.008	0.030
arrests	# of own-arrests (gang name indicator)		-0.008	-0.014
networks	# of victimizations (domestic incident) by 2nd degree neighbors	90 days	-0.008	0.053
arrests	# of own-arrests (aggravated battery)	Total	0.008	0.019
arrests	# of own-arrests (resist/obstruct officer)	Total	0.007	0.022
arrests	# of own-arrests (traffic)	90 days	0.007	0.020
arrests	# of own-arrests (violent crime)	270 days	-0.007	0.027

Note: Features are listed in descending order of residualized correlation, except for the first feature. The first column shows the set to which each feature belongs. The second column provides a description of the feature. Text in parentheses indicate a subtype of the feature. The third column shows, where appropriate, the time window over which the feature was measured. Time windows listed as “Total” indicate features that look back to the beginning of our data (August 1999). The fourth and fifth columns show the correlation between the residualized and unresidualized version of the feature and the outcome, respectively.

Table B.7: Top 50 features from the stepwise residualization procedure when not given access to network features

Feature Set	Description	Time Window	Correlations Residualized	Correlations Original
arrests	# of own-arrests (any)	730 days	0.109	0.109
arrests	Ever gang-affiliated		0.056	0.095
victims	# of victimizations (shootings)	Total	0.041	0.064
demographics	Age (modal)		-0.031	-0.052
arrests	# of own-arrests (gambling)	Total	0.026	0.068
arrests	# of own-arrests (warrant)	730 days	-0.025	0.045
arrests	# of own-arrests (public alcohol consumption)	Total	-0.023	0.010
arrests	# of own-arrests (drug)	730 days	0.021	0.040
arrests	# of own-arrests (reckless conduct)	Total	0.021	0.072
arrests	# of own-arrests (property crime)	730 days	-0.019	0.036
arrests	# of own-arrests (robbery)	Total	0.018	0.056
arrests	# of own-arrests (simple battery)	730 days	-0.016	0.027
arrests	# of own-arrests (possession controlled substance)	730 days	-0.016	0.024
demographics	Sex (most recent)		-0.016	-0.017
arrests	# of own-arrests (panhandling)	Total	-0.014	-0.002
demographics	Missing date of birth		-0.013	-0.014
arrests	# of own-arrests (criminal trespass–vehicle)	270 days	0.013	0.036
arrests	# of own-arrests (traffic)	730 days	-0.012	0.030
arrests	# of own-arrests (gun assault or battery)	Total	0.011	0.034
arrests	# of own-arrests (unique gang names)		-0.011	0.095
victims	# of victimizations (shootings)		-0.011	-0.028
arrests	# of own-arrests (reckless conduct)	730 days	0.011	0.067
victims	# of victimizations (gun assault or battery)	270 days	0.011	0.033
arrests	# of own-arrests (solicitation)	Total	0.010	0.060
arrests	# of own-arrests (criminal trespass–land)	730 days	-0.010	0.039
arrests	# of own-arrests (firearm possession)	Total	0.010	0.035
arrests	# of own-arrests (burglary)	Total	0.010	0.036
arrests	# of own-arrests (solicitation)	365 days	0.010	0.044
arrests	# of own-arrests (drug paraphenelia possession)	730 days	-0.010	0.001
arrests	# of own-arrests (aggravated assault school employee)	Total	0.010	0.027
arrests	# of own-arrests (obstructing identification)	Total	0.009	0.033
arrests	# of own-arrests (larceny)	Total	-0.009	-0.001
arrests	# of own-arrests (replica firearm–pellet gun)	Total	0.008	0.030
arrests	# of own-arrests (gang loitering)	Total	0.008	0.043
arrests	# of own-arrests (gun battery)	730 days	-0.008	0.011
arrests	# of own-arrests (criminal trespass–real property)	730 days	-0.008	0.020
arrests	# of own-arrests (gang name indicator)		-0.008	-0.014
arrests	# of own-arrests (drug crime)	730 days	-0.008	0.073
arrests	# of own-arrests (manufacturing/dealing public school)	Total	0.008	0.027
arrests	# of own-arrests (traffic)	90 days	0.008	0.020
arrests	# of own-arrests (chicago municipal code)	730 days	-0.007	0.041
arrests	# of own-arrests (possession controlled substance)	Total	0.007	0.034
arrests	# of own-arrests (gun robbery)	730 days	-0.007	0.014
arrests	# of own-arrests (heroin possesion)	Total	-0.007	0.008
arrests	# of own-arrests (aggravated robbery)	Total	0.007	0.028
arrests	# of own-arrests (simple assault)	730 days	-0.007	0.021
arrests	# of own-arrests (resist/obstruct officer)	Total	0.007	0.022
arrests	# of own-arrests (aggravated battery)	Total	0.007	0.019
arrests	# of own-arrests (mob action/fail to withdraw)	Total	0.007	0.024
arrests	# of own-arrests (possession of paint marker–intent to deface)	Total	-0.007	0.004

Note: See bottom of Table B.6 for column definitions.

Table B.8: Top 50 features from the stepwise residualization procedure when not given access to own-arrest features

Feature Set	Description	Time Window	Correlations Residualized	Original
networks	# of 1st degree neighbors ever gang-affiliated	365 days	0.103	0.103
victims	# of victimizations (shootings)	Total	0.047	0.064
victims	# of victimizations (drug abuse)	Total	-0.039	-0.002
demographics	Age (modal)		-0.035	-0.052
demographics	Missing date of birth		-0.025	-0.014
networks	# of 1st degree neighbors victimized (any victimization)	365 days	0.024	0.081
victims	# of victimizations (any victimization)	730 days	-0.020	-0.014
demographics	Sex (most recent)		-0.016	-0.017
demographics	# of unique police beats		0.016	0.027
victims	# of victimizations (days since first victimization)		-0.015	-0.020
networks	# of 1st degree neighbors ever gang-affiliated		-0.015	0.096
victims	# of victimizations (shootings)		-0.013	-0.028
victims	# of victimizations (reckless conduct)	Total	-0.013	-0.001
networks	# of arrests (drug deal) by 2nd degree neighbors	730 days	0.012	0.066
victims	# of victimizations (weapons violation)	Total	0.012	-0.001
networks	# of victimizations (domestic incident) by 2nd degree neighbors	90 days	-0.011	0.053
networks	# of arrests (property crime) by 1st degree neighbors	365 days	0.011	0.075
victims	# of victimizations (aggravated–handgun)	Total	0.010	0.046
networks	# of 1st degree neighbors victimized (gun battery)	270 days	0.010	0.066
networks	# of 2nd degree neighbors victimized (gun robbery)	30 days	0.009	0.031
networks	# of 1st degree neighbors arrested (drug deal)	730 days	0.009	0.068
networks	# of arrests (violent crime) by 1st degree neighbors	270 days	0.009	0.072
networks	# of arrests (gun battery) by 1st degree neighbors	60 days	-0.008	0.006
victims	# of victimizations (gun battery)	Total	0.008	0.057
networks	# of 2nd degree neighbors victimized (property crime)	60 days	-0.008	0.046
networks	# of arrests (any) by 2nd degree neighbors		-0.007	0.073
networks	# of arrests (domestic incident) by 2nd degree neighbors	30 days	0.007	0.047
victims	# of victimizations (simple domestic battery)	Total	-0.007	-0.014
networks	# of 1st degree neighbors ever gang-affiliated	270 days	-0.007	0.101
networks	# of victimizations (shootings) by 2nd degree neighbors	Total	0.007	0.085
networks	# of 1st degree neighbors ever gang-affiliated	60 days	-0.007	0.083
demographics	Number of unique races recorded		-0.007	-0.006
networks	# of arrests (property crime) by 1st degree neighbors	90 days	0.006	0.052
networks	# of arrests (gun assault or battery) by 1st degree neighbors	730 days	0.006	0.052
networks	# of victimizations (any victimization) by 2nd degree neighbors		-0.006	0.072
networks	# of 2nd degree neighbors		-0.006	0.016
victims	# of victimizations (gun battery)	60 days	-0.006	0.013
victims	# of victimizations (shootings)		0.006	0.034
victims	# of victimizations (gun assault or battery)	90 days	0.006	0.022
networks	# of 1st degree neighbors victimized (domestic incident)	270 days	-0.006	0.025
networks	# of 2nd degree neighbors		0.006	0.075
victims	# of victimizations (shootings)	730 days	-0.006	0.051
victims	# of victimizations (violent crime)	730 days	0.005	0.025
networks	# of 1st degree neighbors victimized (property crime)	60 days	-0.005	0.012
networks	# of 2nd degree neighbors arrested (gun robbery)	60 days	-0.005	0.033
networks	# of 2nd degree neighbors victimized (shootings)	Total	0.005	0.085
networks	# of arrests (any) by 1st degree neighbors	30 days	0.005	0.075
networks	# of 1st degree neighbors ever gang-affiliated	730 days	-0.005	0.103
victims	# of victimizations (child abduction)	Total	0.005	0.007
networks	# of victimizations (shootings) by 1st degree neighbors	60 days	-0.005	0.027

Note: See bottom of Table B.6 for column definitions.

Table B.9: Top 50 features from the stepwise residualization procedure when not given access to network or own-arrest features

Feature Set	Description	Time Window	Correlations Residualized	Original
victims	# of victimizations (shootings)	Total	0.064	0.064
demographics	Age (modal)		-0.048	-0.052
demographics	Missing date of birth		-0.033	-0.014
demographics	# of unique police beats		0.025	0.027
victims	# of victimizations (days since first victimization)		-0.024	-0.020
demographics	Sex (most recent)		-0.017	-0.017
victims	# of victimizations (simple domestic battery)	Total	-0.016	-0.014
victims	# of victimizations (aggravated–handgun)	Total	0.014	0.046
victims	# of victimizations (shootings)		-0.013	-0.028
victims	# of victimizations (property crime)	Total	-0.012	-0.031
victims	# of victimizations (aggravated battery)	Total	0.012	0.035
victims	# of victimizations (days since last victimization)		0.011	0.006
victims	# of victimizations (gun assault or battery)	270 days	0.010	0.033
demographics	Approximate age (modal)		-0.009	-0.049
victims	# of victimizations (shootings)		0.006	0.034
victims	# of victimizations (criminal sexual assault)	Total	-0.005	-0.005
demographics	Number of unique races recorded		-0.005	-0.006
victims	# of victimizations (to property)	Total	0.005	-0.017
victims	# of victimizations (child abduction)	Total	0.005	0.007
victims	# of victimizations (shootings)		-0.005	-0.037
victims	# of victimizations (days since last domestic incident)		-0.005	-0.002
victims	# of victimizations (violent crime)		0.005	-0.011
victims	# of victimizations (any victimization)	730 days	-0.005	-0.014
victims	# of victimizations (gun battery)	60 days	-0.005	0.013
victims	# of victimizations (child endangerment)	Total	0.004	0.010
victims	# of victimizations (gun assault or battery)	90 days	0.004	0.022
victims	# of victimizations (telephone threat)	Total	-0.004	-0.011
victims	# of victimizations (violent crime)	730 days	0.004	0.025
demographics	Race (most recent)		-0.004	-0.003
victims	# of victimizations (gun battery)	30 days	-0.004	0.009
victims	# of victimizations (gun battery)	365 days	-0.004	0.039
victims	# of victimizations (gun robbery)	60 days	0.003	0.004
victims	# of victimizations (attempted strongram)	Total	-0.003	-0.003
victims	# of victimizations (violent crime)	Total	-0.003	0.023
victims	# of victimizations (shootings)	270 days	0.003	0.040
victims	# of victimizations (gun battery)	Total	0.003	0.057
victims	# of victimizations (aggravated–handgun)	270 days	-0.003	0.033
victims	# of victimizations (shootings)	730 days	-0.003	0.051
victims	# of victimizations (armed knife)	Total	-0.003	-0.003
victims	# of victimizations (aggravated)	Total	-0.003	-0.003
victims	# of victimizations (simple battery)	Total	0.003	-0.008
demographics	Police beat (modal)		0.002	0.001
victims	# of victimizations (aggravated–other dangerous weapon)	365 days	0.002	0.005
victims	# of victimizations (simple battery)		-0.002	-0.005
victims	# of victimizations (gun assault or battery)	730 days	0.002	0.044
victims	# of victimizations (aggravated assault)	Total	-0.002	0.007
victims	# of victimizations (aggravated domestic battery)	Total	0.002	0.007
victims	# of victimizations (aggravated battery)	90 days	-0.002	0.011
victims	# of victimizations (gun assault or battery)		0.002	-0.039
victims	# of victimizations (harassment by electronic means)	Total	-0.002	-0.005

Note: See bottom of Table B.6 for column definitions.

Table B.10: Predictive performance for limited feature sets chosen by the stepwise residualization procedure

	Top 500			Top 3,381		
Feature Set	Precision	Recall	Total Recall	Precision	Recall	Total Recall
Full	0.128	0.028	0.019	0.095	0.143	0.095
Full - Top 50	0.146	0.032	0.022	0.089	0.133	0.089
No Networks - Top 50	0.130	0.029	0.019	0.081	0.122	0.081
No Own Arrests - Top 50	0.110	0.024	0.016	0.080	0.120	0.080
No Own Arrests or Networks - Top 50	0.078	0.017	0.012	0.064	0.097	0.064

Note: Performance and recall from models trained to predict shooting victimization during the 18-month outcome period starting April 1, 2018. Models differ based on the feature sets available to them during training. Model performance is evaluated on shooting victimization during the outcome period, for the $k = 500$ and $k = 3,381$ people with the highest predicted risk of shooting victimization.