The Value of Policing: Measuring the Tradeoff Between Crime Control and Use of Force

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Abstract

We provide estimates to inform the potential tradeoff between the crime reduction benefits and social costs associated with investments in policing. First, using stated-choice experiments in an original survey, we estimate willingness to pay for reductions in violent crime and police use of force. Our survey results indicate that citizens are willing to accept four additional violent crimes in return for one fewer force incident. Next, using data from Chicago and an identification strategy based on officers' predetermined work schedules, we estimate causal effects of marginal officers on crime and use of force. Combining these estimates with our survey findings, we show that while marginal officers provide a crime reduction benefit which exceeds their salary costs, incorporating their impacts on social costs associated with use of force reverses conclusions about cost-effectiveness.

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I. Introduction

A large literature has documented the efficacy of police in reducing crime and has argued that investments in policing are cost-effective by comparing the social value of the associated crime reduction with the fiscal costs of hiring officers (e.g., Levitt 1997; Di Tella and Schargrodsky 2004; Evans and Owens 2007; Chalfin and McCrary 2018; Mello 2019). In recent years, however, activists, pundits, and scholars have raised doubts about these conclusions, highlighting the other costs that police impose on society, most notably in the form of use of force against civilians (e.g., Crabtree 2020; Owens and Ba 2021).

Incorporating police use of force into a welfare analysis of investments in policing, such as hiring additional officers, requires answering two questions that remain largely overlooked in the empirical literature. First, how does society value police use of force relative to the crimes prevented by the police? And second, how much use of force is generated by police force expansions?

In this paper, we address these two questions and consider the associated implications for cost-benefit analyses of policing. Using an original survey, we estimate willingness to pay for reductions in violent crime and police use of force, finding that civilians place significantly more weight on decreases in force than reductions in violent crime. Across survey designs and populations, respondents are willing to accept roughly four additional violent crimes in exchange for one fewer use-of-force incident. We then estimate causal effects of marginal officers on violent crime and police use of force using administrative data from Chicago and a research design exploiting officers' predetermined work schedules, finding that marginally deployed officers reduce violent crime but increase use of force. Combining our survey and quasi-experimental estimates, we show that while marginal officers provide a crime reduction benefit which exceeds their salary costs, incorporating their impacts on social costs associated with use of force reverses conclusions about cost-effectiveness.

We estimate citizen valuations using conjoint analysis in surveys of over 1,000 U.S. residents (Green and Rao, 1971; Folke and Rickne, 2022; Maestas et al., 2023; Moshary et al., 2025). Respondents are asked to choose between housing options with different rental prices and characteristics, including neighborhood rates of violent crime and police use of force. We estimate a mixed multinomial logit model to recover the marginal utility each characteristic contributes to housing choice and calculate willingness to pay as the ratio of each characteristic's marginal utility to the marginal disutility of rent.

This conjoint approach addresses key limitations of prior methods for valuing public safety. Contingent valuation studies that directly ask respondents their willingness to pay for public safety programs are vulnerable to strategic and hypothetical bias (Chalfin, 2015; Ba et al., 2025). Hedonic regression approaches that infer valuations from observed housing prices face omitted variable bias, as crime rates and police activity correlate with many other neighborhood characteristics (Bishop and Murphy, 2011). Moreover, in standard hedonic settings where crime and police use of force do not vary independently, these models cannot identify the marginal rate of substitution between reductions in crime and use of force. Our design asks respondents to make explicit trade-offs between crime, force, and other neighborhood characteristics at specified prices. This approach both generates the independent variation to estimate how respondents trade off neighborhood characteristics and disciplines the stated values. To further address concerns about hypothetical bias, we conduct a second consequential survey in partnership with university housing services where students seeking off-campus housing complete the same task and receive personalized neighborhood recommendations based on their stated preferences (Kessler et al., 2019; Chan, 2021), creating direct incentives for truthful revelation.

Our primary survey results show that respondents are willing to pay \$9 in monthly rent to live in a neighborhood with one fewer violent crime per 10,000 residents per year. This implies that each household in that neighborhood has a willingness to pay of \$108 to avert one violent crime per 10,000 residents (or 4,348 households). We therefore estimate the social cost of a single violent crime to be $\$108 \cdot 4,348 \approx \$470,000$. Our estimates are in line with other designs that capture the full social willingness to pay, such as Cohen et al. (2004), and larger than hedonic methods that capture only the use-value of crime reduction.

Our survey results also show that respondents are willing to pay \$35 in monthly rent to reduce police use of force by one incident per 10,000 residents—approximately four times their willingness to pay for crime reduction. This implies a marginal rate of substitution of 0.252: respondents are indifferent between an intervention that prevents four violent crimes and an intervention that prevents a single use-of-force incident.

This four-to-one valuation ratio is remarkably stable. It holds across every survey variant and population we study. It holds when we add an explicit "quality of life" score to help respondents distinguish between the intrinsic costs of force versus its role as a signal of neighborhood quality, and it holds in our consequential survey where students receive actual housing recommendations. Confidence intervals around this marginal rate of substitution always exclude not only an equal valuation of force and crime (MRS = 1), but even a two-to-one ratio (MRS = 0.5). When we directly ask a subset of respondents why police use of force influenced their choices, 54 percent report concern about becoming a victim of unjustified force or broader harm to their community, suggesting that our estimates reflect genuine valuations rather than respondents' inferences about other unobservable amenities.

While there is no comparable estimate of willingness to pay to avoid police use of force (to our knowledge), our finding that force is substantially more costly than violent crime is consistent with several claims from the literature, including (i) larger settlements in lawsuits against officers than damages awarded in cases of civilian-on-civilian assault, (ii) a significant willingness to pay among civilians to file complaints against the police (Ba, 2020), and (iii) Ang (2021)'s finding that police killings have significantly larger impacts than civilian-on-civilian homicides on the educational outcomes of neighborhood youth.

The quasi-experimental literature on police manpower has reliably concluded that marginal officers are cost-effective, but these conclusions do not account for the potential costs from increasing use of force (Chalfin and McCrary, 2017). To further contextualize our survey estimates, we consider the implications for cost-benefit analyses from the literature by asking what impacts of officers on use of force would change conclusions about cost effectiveness. Our survey estimates imply that a marginal officer's social benefit in the form of crime reduction exceeds their social cost, including both their salary and costs associated with use of force, if their impact on violent crime is at least five times larger than their impact on use of force. In other words, our survey findings imply that marginal officers are not cost-effective if they use force at least once for every five violent crimes they prevent.

In the second part of the paper, we directly test whether marginal officers meet these standards using new quasi-experimental estimates from Chicago. Specifically, we address the gap in existing cost-benefit analyses, which consider impacts of officers on crime but not on use of force, by simultaneously estimating causal effects of officers on both outcomes using administrative data from the Chicago Police Department (CPD) covering 2010–2020.

Our identification strategy exploits institutional features of CPD's shift scheduling system. CPD officers are assigned to one of 22 patrol districts and one of six rotating day-off groups at the beginning of each year based on seniority, preferences, and staffing needs. Officers work four consecutive days followed by two days off, so exactly four of six groups are on duty each day. This assignment process generates variation in predicted daily staffing because officers are often divided unevenly across groups. For example, on some days a district may have 164 officers scheduled to work, while on others it may have only 156. This variation is predetermined by the operations calendar and is therefore independent of day-to-day crime fluctuations. We use the assigned number of officers as an instrument for realized staffing to isolate variation that is not driven by endogenous attendance or time-off decisions in response to contemporaneous crime conditions.

We find that marginal CPD patrol officers have undetectable effects on both force and crime, but tactical officers tasked with more high-stakes police work have relevant impacts on both margins. An additional tactical officer decreases violent crime by 0.0072 incidents per shift. Aggregated up over a year and scaled by our survey-based valuation of violent crime, this estimate implies roughly a $600,000 annual social benefit in the form of crime reduction associated with the marginal officer, well in excess of their salary costs of about $150,000. However, these officers also increase police use of force by 0.003 incidents per shift. The ratio of violent crimes prevented to force incidents generated is 2.3 – well below the valuation ratio of four or the break-even ratio of five, which accounts for officer salaries, required for cost effectiveness. Accounting for statistical uncertainty, we cannot reject the null that a marginal tactical officer provides zero net social value. Importantly, however, incorporating social costs associated with use of force shifts the police baseline from one where hiring more police is likely beneficial to one where it is likely harmful.

We make three contributions. First, we provide a unified welfare framework for evaluating policing which incorporates both the benefits of crime reduction and the social costs of police use of force. Traditional cost-benefit analyses compare the benefits of crime reduction to officer salaries (e.g., Levitt 1997; Di Tella and Schargrodsky 2004; Evans and Owens 2007; Chalfin and McCrary 2018; Mello 2019), but they abstract from enforcement-related harms. A separate literature documents the costs of aggressive policing, including harms to civil liberties (Owens et al., 2021), psychological costs in minority communities (Ang, 2021), and erosion of trust in law enforcement (Bobo and Thompson, 2006), yet these costs are rarely incorporated into formal welfare analysis.[1] By estimating both citizen valuations of police use of force and the causal effects of marginal officers on these two dimensions, we bridge

[1] Coury (2021) studies how exposure to crime affects demand for policing by measuring the effect of variation in criminal activity across neighborhood around election day. He finds that each additional violent crime leads to an increase in support for police union-endorsed ballot positions ranging from 2.9 percentage points for homicides to 0.4 percentage points for lesser crimes. He finds limited evidence that police shootings affect local electoral support for police. Ba (2020) uses variation in the costs of filing a complaint against police officers to estimate the WTP to pay to complete a complaint against an officer. Some of these complaints are for improper use of force.

these literatures and enable comprehensive cost-benefit analysis. Applying this framework to Chicago, we show that incorporating force costs can overturn standard conclusions.

Second, we provide the first quasi-experimental estimates of how police deployment affects use of force. While a large literature has examined the effects of policing on crime using a variety of quasi-experimental research designs, questions about the impacts of deployment or manpower changes on the social costs of enforcement remain unanswered. Using variation generated by predetermined officer scheduling, we estimate the impacts of marginal officers on both violent crime and use of force and illustrate the importance of the latter impact for cost-benefit analysis of policing investments. More broadly, our findings also highlight the importance of considering potential impacts on use of force for other policies affecting enforcement intensity, such as hot spots policing (Braga and Bond, 2008; Braga et al., 2019) and police militarization (Masera, 2021; Bove and Gavrilova, 2017; Harris et al., 2017).

Third, we provide new estimates of civilians' valuations of violent crime and police use of force based on a novel approach in this literature. Improving the estimation of willingness to pay for public safety is of significant importance for policy evaluation (Ludwig, 2010). Prior research on this topic has primarily relied on measures based on observed costs (Chalfin and McCrary, 2018), which do not necessarily capture the notion of willingness to pay. Survey-based estimates have used contingent valuation (Ludwig and Cook, 2001; Cohen et al., 2004) or hedonic regression (Bishop and Murphy, 2011) approaches, each of which faces well-known limitations. Our conjoint design addresses these challenges by requiring respondents to make explicit trade-offs between crime, police use of force, and other neighborhood characteristics at specified prices. This approach allows us to recover internally consistent welfare weights that can be integrated into cost-benefit analyses. We validate our design using a consequential survey in which respondents receive actual housing recommendations based on their stated preferences.

The remainder of the paper proceeds as follows. Section II presents our conjoint survey methodology and estimates of civilian valuations. Section III contextualizes our estimates and demonstrates their implications for existing cost-benefit analyses of policing investments. Section IV introduces our quasi-experimental design using the Chicago Police Department's scheduling calendar and presents estimated effects of marginal officers on both crime and force. Section V combines both empirical components to evaluate the cost-effectiveness of marginal CPD tactical officers. We conclude in section VI.

II. Survey Analysis

II.A. Survey Design

Our goal is to estimate the rate at which individuals are willing to trade crime reduction for the reduction of police use of force, holding other valued neighborhood attributes and prices fixed. Identifying this relative valuation requires exogenous variation that shifts crime and use of force independently over a rich support of different bundles. The variation available in observational settings typically fails this requirement. Policing strategies, local institutions, and neighborhood conditions jointly shape crime and use-of-force exposure, so cross-neighborhood differences and policy changes move these outcomes in correlated ways and alongside many other amenities.

Our survey experiment addresses this identification problem by independently randomizing the crime and use-of-force attributes and placing them on the same choice margin. This ensures that data from respondents contains the variation required to separately estimate their valuation of each attribute and recover the implied marginal rates of substitution between them.

We recruited 1,035 US renters on CloudResearch in August and September 2022. Figure I summarizes the survey flow. The survey began by collecting demographic information about each respondent. We then ask respondents to consider a hypothetical scenario where they must move to a new rental home.

We ask respondents how many bedrooms and what monthly rent they would be interested in paying for an apartment. We then present each respondent a choice set with five hypothetical homes with randomly generated characteristics and ask them to select their most preferred option. Respondents cannot move on to the next question until ten seconds have elapsed. This question is then repeated eleven more times, with new choice sets presenting homes with randomized amenities.

The hypothetical homes are given the following randomized characteristics: monthly rent, violent crime per 10,000 people, police use of force per 10,000 people, square footage, and quartile of school quality. Respondents are asked to assume that all other characteristics are identical across the choices. Prior to viewing the choice sets, respondents are shown detailed descriptions of our violent crime, police use of force, and school quality measures. They may also access reminder text while in the choice sets. We carefully introduce the definitions and distributions to ensure clarity, as respondents may interpret terms like violent crime and use of force differently. We also attempt to present these definitions in a neutral manner to avoid priming respondents to consider additional disutility associated with unjustified police use of force. These definitions, along with the distributions we provide, help guide respondents in making informed choices:

> **Violent Crime per 10,000 Residents**: The violent crime rate describes the number of violent crimes recorded in a neighborhood in 2019 per 10,000 residents. For example, a neighborhood with 100 violent crimes and 50,000 residents has 20 violent crimes per 10,000 residents. Violent crimes include murder (including negligent homicide), sexual assault, assault & battery, kidnapping, and robbery.
>
> Suppose 1/4 of neighborhoods have less than 53 violent crimes per 10,000 residents. About 1/4 of neighborhoods have more than 157 violent crimes per 10,000 residents.
>
> **Police Use of Force**: Police use of force per 10,000 residents describes the number of times police used force in a neighborhood in 2019 per 10,000 residents of that area. For example, a neighborhood with 48 uses of force and 40,000 residents has 12 uses of force per 10,000 residents.
>
> Police use of force refers to physical contact by a police officer, either directly or through the use of equipment, to compel a subject's compliance. These incidents include uses of firearms, other weapons, and unarmed force.
>
> Suppose 1/4 of neighborhoods have less than 4 uses of force per 10,000 residents. About 1/4 of neighborhoods have more than 12 uses of force per 10,000 residents.

To ensure we are providing plausible and familiar scenarios to respondents, we randomized home characteristics within a range that accords with the respondent's choice of bedrooms and rent. We code participants' location preferences as either a low-income, middle-income, or high-income neighborhood.[2] We create three ranges of crime and use of force possibilities that correspond to the three neighborhood income groups. We use data from Chicago and

[2]For each bedroom choice, we set two cutoffs – one that divides the price ranges between low-income and middle-income neighborhoods and the other between middle-income and high-income neighborhoods. We base these cutoffs on price ranges for each type of apartment in Chicago. These cutoffs are: for studios (900, 1400), for 1-bedrooms (1300, 2100), for 2-bedrooms (1600, 2500), and for 3-bedrooms (1700, 2600).

New York City to inform our choices of the end points in the ranges we create.[3]

In each of the twelve choice sets, we randomize monthly rent and allow only one other randomly selected characteristic to vary. For example, a choice set will present 5 homes with different rents and different square footage but identical violent crime, police use of force, and school quality measures. Then the next choice set presents 5 homes with different rents and different violent crime rates but identical square footage, police use of force, and school quality measures, etc.[4] A feature that does not vary is assigned a random number within a range informed by the participants' baseline information. While this design decision reduces statistical power, it helps simplify the subject's decision so that they can more easily provide us with higher quality data representing their preferences.

Attention Checks We implement multiple checks to ensure that survey respondents are paying attention and making meaningful selections. Our first attention check is implemented in the demographic portion of the survey and asks respondents to give an unusual answer.[5] Respondents who fail this attention check are dropped from our data.

We implement an additional attention check using two choice sets presented before the twelve rounds in the main survey. In each of these two choice sets, there is a dominating option. This apartment has the lowest rent and is identical in all other respects, except for its lowest violent crime rate. We screen out respondents who do not choose the dominating choice because they either do not understand the task or are not reading the questions. These subjects were not permitted to continue with the survey.

In the 6th of the remaining twelve choice sets, we implement a final attention check in which one option dominates all the others. The dominant option has the lowest crime rate and the lowest price, with all other features held fixed. This question is purely a quality control check. We do not use data from this choice set in our estimation, and respondents who do not choose the dominant option remain in our analysis sample. We find that 88 percent of subjects in the experiment choose the dominating option in this second quality check. This

[3]Specifically, the violent crime rate (per 10,000) ranges for low-income, middle-income, and high-income neighborhoods are (75, 250), (55, 100), and (25, 75), while use of force rates ranges are (9, 25), (5, 12) and (1, 9).

[4]Violent crime and police use of force vary in 30 percent of choice sets. Square footage and school quality vary in 20 percent of choice sets.

[5]We ask the following question: "In the past week, did you read any of the following newspapers, either online or in print? Please select all that apply. We also want to see whether people are reading the questions carefully. This question is a data quality check. Regardless of your true answer, please select "Daily News" and "None of the above."

passing rate is relatively high for a survey study, especially given that the attention check was substantially more complex than typical attention checks and was located towards the end of the survey, when survey fatigue was likely highest.

Summary Statistics Table A1 summarizes the characteristics of the 1,035 respondents to our primary survey. They are majority female and more likely to be Black than the average American. They tend to be middle-aged with 59 percent of the sample aged 30 to 60. One quarter have a Bachelor's degree or higher, and 70 percent are childless. Respondents tend to have lower incomes, with 71 percent making less than \$50,000 a year. Only 28 percent are seeking a one-bedroom or studio apartment, and the vast majority, 84 percent, choose a desired rent that places them in our low rent bucket.

II.B. Empirical Strategy

Our first goal is to recover the contributions of each experimentally varied characteristic to respondent utility. We model renters as having preferences for homes using a random utility model. Renter i receives utility U_{iat} from renting apartment a in choice set $t \in \{1, ..., 12\}$:

$$U_{iat} = X_a' \beta_i - \gamma p_{iat} + \delta W_i + \epsilon_{iat} \tag{1}$$

In Equation 1, X_a is the vector of non-price attributes considered in the survey. These attributes include square footage, school quality, neighborhood violent crime, and neighborhood police use of force. The monthly rent is p_{iat}, and W_i is a vector of covariates of the individuals making the decisions.[6] ϵ_{iat} is an idiosyncratic taste shock that we assume to be distributed Type 1 Extreme Value. Our data records 12 choices made by each individual, which we exploit by estimating a mixed multinomial logit model (McFadden and Train, 2000). We assume that individuals have the same disutility with respect to rent, γ, but may differ in their preferences for other neighborhood characteristics, which we model using normally distributed random coefficients, β_i. These coefficients capture renters' taste (or distaste) for apartment attributes. We will focus on the means of the estimated distributions of these random coefficients.[7]

[6]Individual characteristics are uncorrelated with X_a and p_{iat} due to their being randomly generated, but we include W_i in our model to improve statistical precision.

[7]Furthermore, our empirical strategy returns estimates of the *marginal* willingness to pay. The assumed linear utility function underlying it may poorly capture responses to large changes in neighborhood characteristics, especially those outside the range of random values generated in the survey. For example, some neighborhoods in Chicago have force rates well above the highest value considered in our study, so even a

We can then produce estimates of willingness to pay for each of the apartment characteristics considered in the survey by dividing the random coefficient's estimated mean by our estimate of γ. For example, if $\frac{\hat{\beta}_i^{sqft}}{\hat{\gamma}} = 2$, then increasing rent by \$2 and increasing square footage by 1 holds U_{iat} constant at the mean of $\hat{\beta}_i^{sqft}$. We would conclude that the typical respondent is willing to pay \$2 in monthly rent for one additional square foot of apartment space. When an estimated coefficient is negative, as they are for both violent crime and police use of force, this ratio can be thought of as a "price to accept." It captures the amount that monthly rent must *fall* to compensate for a unit increase in the neighborhood characteristic.

In addition to these ratios, we also calculate the marginal rate of substitution between violent crime and police use of force. This is $\frac{\hat{\beta}_i^{crime}}{\hat{\beta}_i^{force}}$, equivalent to the ratio of the associated willingnesses to pay. This captures the relative changes in violent crime and police use of force that leave neighborhood residents indifferent. For example, if $\frac{\hat{\beta}_i^{crime}}{\hat{\beta}_i^{force}} = 0.5$, then a policy that reduced violent crime by 100 incidents at the cost of 100 uses of force would reduce the utility of a typical neighborhood resident. However, utility would instead rise if the same reduction in violent crime could instead be achieved with fewer than 50 uses of force.

In the main survey, to examine whether trust in the police affects citizens' valuation of amenities, we randomized the provision of information that influenced participants' beliefs about police accountability. Additional details can be found in Appendix C.

II.C. Willingness to Pay for Violent Crime and Use of Force

Table I presents our estimates of willingness to pay. Prices, and therefore estimated WTPs, are scaled by \$100. Square footage is measured in 100-square-foot increments, school quality in 1 quartile increments, violent crime in 1 per 10,000 neighborhood residents, and police use of force in 1 per 10,000 neighborhood residents.[8]

We find that respondents are willing to pay roughly \$1 per square foot. They will also pay approximately \$130 in additional monthly rent to live near a school one quartile higher in quality. Meanwhile, each additional violent crime per 10,000 residents requires a \$9 reduction in monthly rent to keep respondents indifferent. For context, the 25th percentile of violent crime per 10,000 residents across Chicago police districts is 53, and the 75th percentile is 157.

respondent at the mean of the random coefficient distribution who moved to one of these neighborhoods from a lower-force neighborhood may not demand a reduction in rent that is exactly proportional to our estimated marginal willingness to accept

[8]Survey respondents were presented with actual prices, square footage, and school quartile. Their observed violent crime and use of force rates were both out of 10,000 residents.

Extrapolating linearly from our estimates of willingness to pay, respondents would demand a rent reduction of approximately $900 to move from a neighborhood at the 25th percentile in violent crime to one at the 75th percentile. Our estimate, therefore, implies substantial WTP when applied within the typical range of violent crime rates encountered in urban housing markets.

We estimate that one additional incident of police use of force per 10,000 residents in a neighborhood requires a $35 reduction in monthly rent to keep respondents indifferent. Hence, respondents are more sensitive to police use of force than they are to violent crime. However, there is also less variation in use of force across neighborhoods, with the interquartile range running from 3.4 to 12.1. Extrapolating linearly from our estimates of WTP, respondents would demand a rent reduction of approximately $300 to move from a neighborhood at the 25th percentile in use of force to one in the 75th percentile, all else equal.

The ratio of respondent willingness to pay to avoid violent crime and police use of force (both scaled per 10,000 residents) is informative about individuals' marginal rate of substitution between crime and police use of force. We find that respondents are willing to accept an increase in force of 0.252 in return for a decrease of one violent crime or, alternatively, would accept four additional violent crimes in return for one fewer force incident.

II.D. Alternative Explanations and External Validity

In this section, we assess the robustness and external validity of our willingness to pay estimates. We first examine whether respondents value crime and police use of force intrinsically or instead treat them as proxies for other, unobserved neighborhood characteristics. We then evaluate the external validity of our estimates along two dimensions. The first concern is sample representativeness: the population recruited for the study may have preferences that differ from those of the broader population (Hotz et al., 2005). Appendix Section B shows that while willingness to pay for crime and force abatement varies with race and income, the marginal rate of substitution between the two does not. The second concern is hypothetical bias: choices made in a stated-preference setting may differ from those made in consequential housing decisions (List et al., 2006).

Signaling Roles of Neighborhood Amenities To examine whether the WTP estimates are driven by intrinsic harms associated with police use of force or reflect underlying characteristics of the neighborhood that respondents perceive as correlated with police use of force, we conducted an additional survey experiment with a sample size of 256 participants in November 2024. In this experiment, we presented subjects with choices that had the same attributes as the original study. We additionally randomized half of the participants in this survey to consider an additional feature of the apartment: the Quality of Life score. For these participants, we explained that the Quality of Life score is a comprehensive measure of the block where the apartment is located provided by a local magazine. This score reflects various factors contributing to a high quality of life, including public order, crime rates, cleanliness and maintenance, the availability of parks and recreational areas, and community amenities. The score ranges from 60 to 100, with 60 being the lowest and 100 the highest.

The addition of the Quality of Life score reduces respondents' need to rely on the other neighborhood features as proxies for neighborhood quality and provides more comprehensive information about the neighborhood, enabling individuals to more directly consider their valuations of other amenities. With this additional context, participants are more likely to pay higher monthly rent when considering each amenity and also may alter their relative valuations of the amentities.

Panel A of Table II reports WTP estimates separately for respondents who were shown the additional Quality of Life (QOL) score and those who were not. We find that estimated WTP levels increase when respondents observe the QOL index. However, the implied MRS between violent crime and use of force is largely unaffected. The MRS is 0.21 for respondents shown the QOL index and is statistically indistinguishable from the estimate for respondents not shown the index. This finding suggests that, in the absence of signaling effects, the estimated WTPs in the main results reflect the intrinsic values for the amenities.

To further test the sensitivity of our results to signaling effects, we asked participants, "In each round of choices, when five different apartments all had the same violent crime rate, square footage, and school ratings, how did police use of force factor into your selection?" Of the 256 participants, 138 (54 percent) reported that they preferred lower police use of force because they or someone they knew might become an unfair victim of police actions, or because they believed lower police use of force would hurt the broader community, even if they were not personally affected. In contrast, 95 participants (37 percent) stated that police use of force influenced their decision because it served as a proxy for other neighborhood characteristics. Some participants selected both options, indicating that the use of force was viewed as both an intrinsic harm and a signal of other neighborhood factors. Of the 95 who viewed use of force as a proxy, 46 (18 percent) reported disliking police use of force because of its harmful impact on themselves or the community. This means that only 49 participants (19 percent of the total sample) indicated that police use of force mattered solely because it represented other aspects of the neighborhood.

Using the follow-up survey, we conducted an analysis that excluded participants who indicated that they used police use of force as a proxy for other neighborhood characteristics. In Panel B of Table II, we present the WTP estimates for all respondents and compare them to those who did not rely on this strategy. We find that estimated willingness to pay to reduce police use of force is qualitatively similar but slightly attenuated in this subsample, with an MRS between crime and use of force of 0.3. We view this as indicating that participants inherently disvalue police use of force.

Population Reweighting Next, we evaluate whether our results apply to two populations with different observable characteristics from our survey sample: the entire United States, and the city of Chicago. We first reweight our survey respondents to match the U.S. population in terms of race, age, income, and sex to help us extrapolate the results to the entire nation. The results for this procedure appear in Panel C of Table II. Column (1) reproduces our main results for comparison. Column (2) presents the reweighted results. Our reweighted estimates of willingness to pay to avoid violent crime and use of force are 40 and 30 percent larger respectively, suggesting that our baseline estimates are, if anything, underestimates relative to the national average. Importantly, the implied marginal rate of substitution between crime and force is very similar (about 10 percent larger) when reweighting.

Because we ultimately combine our survey estimates with quasi-experimental estimates from Chicago, we also reweight the survey sample to match Chicago's demographic distribution along the same dimensions, with results presented in Column (3). Willingness to pay for school quality, violent crime, and use of force is higher relative to the baseline sample. However, these increases are broadly proportional, leaving the estimated MRS between crime and force nearly unchanged relative to the U.S.-reweighted estimates.

Incentivized Survey Our next consideration is whether the hypothetical nature of the task introduces bias in our WTP estimates, preventing us from learning individuals' true preferences. While online samples allow for a large and broadly representative group, a concern remains that participants are making hypothetical rather than real choices with no incentive to reveal their true preferences (List and Gallet, 2001). There is an additional concern that making the information about police use-of-force more salient primed respondents to care more about police use of force than violent crime (List et al., 2006).

To address this concern, we followed Chan (2021) and designed a consequential survey in partnership with housing services at a large university in a major North American city with a population exceeding one million. Respondents to this survey were sent tailored recommendations based on their responses.[9] Because these recommendations are most useful when responses reflect true preferences, participants had a direct incentive to answer honestly.

The consequential design offers advantages relative to traditional approaches for valuing public goods. Like our baseline conjoint analysis, it requires respondents to make explicit trade-offs between housing prices and neighborhood safety. Unlike contingent valuation studies that directly elicit willingness to pay for policy changes, this design reduces incentives for strategic overstatement (Mitchell and Carson, 2013). Moreover, because higher stated willingness to pay leads to more expensive housing recommendations, respondents face implicit budget discipline, mitigating concerns that income constraints are ignored (Cohen, 2007).

We collected data for one year, from the summer of 2023 to the spring of 2024. This produced a sample of 66 respondents. To align with our recommendation system, options were presented as neighborhoods rather than specific apartments. This ruled out the use of square footage as a characteristic, but we replaced it with options more likely to be relevant to respondents seeking student housing: travel time from the university campus and the total number of day care options and restaurants in the neighborhood.

The results for the incentivized survey appear in Column (4) of Panel C of Table II. Estimates suggest that the WTP for school quality is much lower for the incentivized survey than for the non-incentivized survey. This is likely because of differences in demographic characteristics: incentivized survey respondents are younger and only three percent have children, compared to 30 percent of respondents in the non-incentivized survey.

Despite these differences, we see qualitatively similar responses for the WTP to avoid violent crime and the WTP to avoid the use of force. However, worth noting is the fact that, relative to participants in the hypothetical survey, incentivized survey respondents

[9] After each participant completed their survey, we followed the same approach as our main specification to estimate mixed logit models with the data collected up to that point to obtain a set of individual-level parameters for matching purposes. Each neighborhood was scored based on the participant's estimated choice probabilities. For each participant, we identified the ten neighborhoods with the highest scores and shared these recommendations with them.

value avoiding police use of force more highly and value avoiding violent crime less highly, leading to a MRS between crime and police use force that is 30 percent smaller than in the non-incentivized experiment. In other words, these respondents are willing to accept more crimes in return for a reduction in use of force than those in the hypothetical survey.

The evidence from the incentivized survey also allows us to address the concern that the framing of the conjoint study primed respondents to care more about police use of force than violent crime (List et al., 2006). Studies have shown that priming, due to factors such as framing or salience are more impactful in hypothetical choices than revealed choices (Hausman, 2012; List and Gallet, 2001). If priming caused our subjects to overemphasize police use of force and underemphasize crime, then we would expect subjects to have a higher WTP for crime reduction and a lower WTP for police use of force reduction in the incentivized survey. However, we find the opposite, suggesting that our estimates of the WTP for police use of force reductions are not driven by priming.

Discussion Overall, these results suggest that the WTP estimates we obtained from our primary survey are driven by the direct disamenities of crime and force. Furthermore, they are likely to generalize to the broader population and to those making real consequential apartment decisions. The similarity in results aligns with List et al. (2006) and Chan (2021), who find no evidence of hypothetical bias when estimating marginal attribute values.

III. Applying Survey Results

In this section, we will first contextualize our survey results within the wider literature on the costs of crime and police use of force. Then, we discuss how applying those results to previous cost-benefit analyses of policing may affect their conclusions.

III.A. Survey Results in Context

We begin with our estimate of the cost of violent crime. To facilitate comparison with existing estimates in the literature, we convert the \$9 monthly willingness to pay per household for a one-unit reduction in violent crime per 10,000 residents into a one-time social cost per violent crime. A single violent crime in a neighborhood with N residents increases the violent crime rate by $\frac{10{,}000}{N}$ incidents per 10,000 residents. Because households are willing to pay \$108 to reduce the crime rate by one unit per 10,000 residents, each household is willing to pay $108 \cdot \frac{10{,}000}{N}$ to prevent one additional violent crime. To obtain the total social willingness to pay, we multiply this amount by the number of households in the neighborhood. With an average household size of 2.3 residents, there are approximately $\frac{N}{2.3}$ households.[10] The implied social cost of a single violent crime is therefore:

$$108 \cdot \frac{10{,}000}{N} \cdot \frac{N}{2.3} \approx 470{,}000.$$

The estimate of the cost of violent crime that most closely matches our methodology comes from Cohen et al. (2004). That study administers a contingent valuation survey that asks participants about hypothetical programs that are said to reduce violent crime by a particular amount but will require participants to pay taxes to support. To make these results comparable to our own, we first weight cost estimates associated with particular types of violent crime with the national crime incidence statistics in Chalfin and McCrary (2018). We then adjust for inflation between 2003, when the Cohen et al. (2004) survey was conducted, and August 2022, when our survey was conducted. This yields an aggregate cost of crime of \$435,000, which falls within the 95% confidence interval of our own estimate.[11]

Both of these survey-derived estimates of the cost of violent crime are substantially higher than estimates that rely on totaling observed costs, such as value of lost property, awards to victims, and the statistical value of life. For example, Chalfin and McCrary (2018) use a cost of approximately \$150,000 in 2022 dollars. We do not speculate on why surveys of willingness to pay generate higher values, but we note that our finding's similarity to Cohen et al. (2004) using conjoint analysis rules out some forms of survey bias as an explanation.

Although no directly comparable benchmark exists for police use of force, a growing body of research documents substantial monetary and non-monetary costs associated with police violence, consistent with our finding that citizens place high disutility on force, both in absolute terms and relative to civilian violence.

First, lawsuits against the police tend to award settlements that are much larger than those for violent crime from other citizens. WTTW News and The Marshall Project (2025) reports that, excluding wrongful conviction cases, Chicago spent \$62.35 million across 112 lawsuits filed against CPD—about \$556,700 per case or \$3,700 per officer per year. These

[10] We take our count of residents per household from 2024 Census estimates for Chicago. We focus on Chicago for compatibility with our Chicago Police Department estimates in Section IV. Census estimates 2.5 residents per household nationwide. Using 2.5 in place of 2.3 yields an estimate of the cost of crime of approximately \$430,000, even closer to the estimate of Cohen et al. (2004).

[11] We estimate a standard error of \$0.4 for our cost of crime. Scaled up by the same factor as our main estimate, this is approximately \$21,000.

financial costs far exceed the typical civil damages awarded in cases of civilian-on-civilian assault, where the median settlement for a non-domestic assault is about \$54,600, \$87,000 for medically treated assaults, and \$440,600 for medically treated gunshot wounds (in 2024 dollars; Miller et al., 2017).

Second, Ba (2020) provides revealed-preference evidence from Chicago showing that civilians are willing to spend \$68.10 to complete a formal complaint involving a serious allegation against an officer. Given the strikingly low rates at which complaints are sustained, Ba (2020) interprets this estimate as indicating sizable willingness to pay to report police misconduct, thus suggesting a significant disutility from harm caused by the police.

Third, Ang (2021) finds that exposure to a nearby police killing causes significant declines in educational outcomes for Black and Hispanic students in Los Angeles, implying substantial long-run human capital costs. Importantly, Ang (2021) finds that the impacts of police-involved violence are twice as large as the impacts of local gang homicides, highlighting the additional social costs arising from police misconduct relative to civilian-on-civilian crime. Similarly, Bor et al. (2018) estimates that killings of unarmed Black Americans lead to an average of 1.7 additional "not good" mental health days per Black person per year, or roughly 55 million lost mental health days nationally.

Finally, long literatures in law and sociology recognize the unique importance of police legitimacy and the ways that police violence erodes this legitimacy. Individuals who perceive police as illegitimate feel trapped by a system where state power and violence are exercised excessively, unfairly, and unaccountably (Tyler et al., 2014). Likewise, neighborhoods with low perceptions of police legitimacy suffer from decreased willingness of residents to engage with the police, local government, or other social services organizations, even in situations where those institutions would be helpful (Brayne, 2014; Lerman and Weaver, 2014; Desmond et al., 2016; Ang et al., 2025).

In turn, individual exposure to police use of force (Jackson et al., 2021) and elevated rates of police use of force in the neighborhood (Owens and Ba, 2021) tend to reduce perceptions of police legitimacy. Desmond et al. (2016) find that following the publicized police beating of Frank Jude in Milwaukee, 911 calls fell by about 22,000 over the following year, a 17 percent decline concentrated in Black neighborhoods. This "trust shock" did not occur for medical or accident calls, indicating that the decline reflected disillusionment with law enforcement rather than lower need for police services. The resulting withdrawal from cooperation undermines crime deterrence and amplifies the downstream costs of misconduct.

Survey evidence reinforces these findings by documenting the broader social costs and subjective fears associated with police violence. An *Economist*/YouGov survey in 2019 found that 63 percent of Black Americans report fearing that they or a family member could be killed by police, compared with 52 percent who fear becoming victims of violent crime in general (Frankovic, 2019). This inversion of perceived risk highlights that many Black respondents experience police violence as a more salient and distressing threat than criminal violence itself.

Across these studies, the psychological, educational, and institutional externalities of police violence far exceed those observed for comparable civilian violence, consistent with a higher social willingness to pay to prevent such incidents. Combined with experimental willingness-to-pay evidence and the large revealed financial burden of police settlements, these findings suggest that citizens rationally assign a higher marginal value to reductions in police use of force than to reductions in ordinary violent crime.

III.B. Returning to Past Benefit-Cost Analyses

Existing studies of policing have frequently concluded that expansions in police force size are cost-effective by comparing the estimated social value of crime reduction with the cost of hiring the marginal officer. We now consider the implications of our survey estimates for these benefit-cost analyses. Because existing studies have considered the causal effects of police on crime but not on police use of force (a point we return to in section IV below), we construct benefit-cost ratios for hypothetical relationships between police force size and police use of force and ask how large the impact of police on force must be to reverse the conclusions about cost-effectiveness in the literature.

We focus our discussion on four recent papers reporting estimated causal effects of changes in police manpower on violent crime: Chalfin and McCrary (2018), Evans and Owens (2007), Mello (2019), and Weisburst (2019). All four studies rely on panel data at the municipality $\times$ year level. Three of the four studies rely on variation in force size generated by the receipt of federal police hiring grants for identification, while Chalfin and McCrary (2018) alternatively rely on measurement error correction methods. Estimated impacts of the marginal officer on the annual number of violent crimes in these four studies, reported in Table A2, range from -1.3 to -4.3.[12]

[12]Note that a separate strand of literature has examined deterrence effects of within-city deployment changes (e.g., Di Tella and Schargrodsky 2004; Klick and Tabarrok 2005; Draca et al. 2011; Weisburd 2021; Jabri 2021).

The benefit-cost ratio (BCR) associated with the marginal officer is given by:

$$\text{BCR} = \frac{-\Delta_c v_c}{w + \Delta_f v_f} = \frac{-\Delta_c}{\frac{w}{v_c} + \frac{v_f}{v_c}\Delta_f} \tag{2}$$

where Δ_c and Δ_f are the causal effects of police on violent crime and force, respectively, and v_c and v_f are society's valuations of one violent crime and one force event. In the second term, the numerator is the social value of crime reduction produced by an officer and the denominator is the social cost of an officer, including both their wage w and the social costs of police use of force $\Delta_f v_f$. Existing studies effectively report $-\Delta_c v_c/w$, which corresponds to the expression above when either $\Delta_f = 0$ or $v_f = 0$.

The third term above divides through by v_c in order to highlight that, given treatment effects Δ_c and Δ_f, the benefit-cost ratio depends only on the ratios w/v_c and v_f/v_c, which are the costs of an officer and a force incident, respectively, expressed in terms of the social cost of a violent crime. We view this framing as especially useful given that much of the policing literature has relied on meaningfully smaller estimates of the social costs of crime than our survey-based results, derived from alternative methodologies, when conducting benefit-cost analyses (as discussed briefly in section III.A). Our survey delivers an internally consistent estimate of the ratio $v_f/v_c \approx 4$, which can be directly applied to prior benefit-cost analyses regardless of the absolute level of v_c used.

With that in mind, we begin by revisiting existing cost-benefit analyses using only the ratio $v_f/v_c \approx 4$ from our survey, while taking estimates of Δ_c from the studies discussed above and setting $w/v_c \approx 1$ based on generally accepted values from the literature. Specifically, this ratio is based on values from Chalfin and McCrary (2018), who estimate the cost of a fully equipped police officer at $w = \$130,000$ and the social cost of a violent crime at $v_c = \$125,465$.

Based on these parameters, we plot the benefit-cost ratio of the marginal officer as a function of Δ_f, the impact of an officer on use of force in panel (a) of Figure II. The height of each line at $\Delta_f = 0$ corresponds to the benefit-cost ratio when officers have no impact on force, i.e., the cost-effectiveness typically assessed in the literature. Ignoring potential impacts on force, all four studies yield benefit-cost ratios above one, with the larger estimates from Evans and Owens (2007) and Mello (2019) implying benefit-cost ratios exceeding three. The point on the horizontal axis where the height of the BCR line equals one represents the "break-even" force effect, or the causal effect of the marginal officer on force such that the social value of their crime eduction exactly equals their social cost. These break-even force effects, which range from 0.08 to 0.82, are reported in Table A3.

Panel (b) repeats this exercise using our survey-based estimate of v_c and an inflation-adjusted wage w. Because our survey suggests a more significant social cost per violent crime, the benefit-cost ratios when assuming null effects on use of force are meaningfully larger, above ten when using the larger Δ_c estimates from Evans and Owens (2007) and Mello (2019). However, "break-even" impacts on force are only slightly larger in this case, ranging from 0.26 to roughly one.

A useful summary emerges from this exercise. Using our survey parameters, the ratio of the break-even Δ_f to Δ_c is approximately 0.2 across all four studies. In other words, if the marginal officer uses force at least once for every five violent crimes they prevent, their costs in the form of their wage bill and use of force outweighs the social benefit they produce in the form of crime reduction. On the other hand, the marginal officer is cost-effective if they can reduce more than five violent crimes for each time they use force.[13]

The valuation estimates from our survey therefore establish clear thresholds under hypothetical force effects. However, existing research estimates only Δ_c and not Δ_f. We now turn to quasi-experimental evidence from Chicago to estimate both causal effects directly.

IV. Effects of Marginal Police Officers on Violent Crime and Force

IV.A. Institutional Background

CPD Shift Organization Patrol officers in the CPD work in one of 22 non-overlapping patrol districts (henceforth "districts"), which cover the entire city (see Figure A1).[14] A typical patrol district employs approximately 320 personnel. Approximately 85 percent of district personnel are beat officers. These officers are assigned to small geographic areas called beats, typically comprising a few city blocks. Beat officers patrol these areas and respond to 911 calls. The CPD operates three standard shifts, known as watches: the first watch runs from 10 p.m. to 6 a.m., the second from 6 a.m. to 2 p.m., and the third from 2 p.m. to 10 p.m. Each beat officer is assigned to one of these watches and maintains this

[13]Note that the difference between this ratio of 0.2 and the valuation ratio from our survey of 0.25 arises because this cost-benefit analysis considers both an officer's salary costs and the social costs associated with their use of force. Ignoring officer salaries, the break-even Δ_f/Δ_c would be 0.25, aligning with the ratio of valuations in our survey.

[14]Before 2012, there were 25 districts, some of which were merged in 2012. We exclude district 5 because it operates under a special day-off group arrangement.

assignment throughout the year.

The remaining 15 percent of district personnel are tactical officers organized into tactical teams. Unlike beat officers, tactical teams are not tied to specific beats and are deployed flexibly in response to changing crime conditions. These officers engage in "hot spot" policing – they "do not answer service calls, but aggressively seek out problematic activity by conducting traffic stops, making contacts, and effecting arrests. Many drive unmarked vehicles and do not wear the traditional police uniform" (US Department of Justice, 2017). Tactical officers are generally described as more "aggressive and self-motivated," operating with greater discretion than beat officers.[15]

Each district maintains three tactical teams – teams A, B, and C – each typically consisting of roughly ten officers and one sergeant, though actual staffing varies due to transfers, leave, and other personnel changes. Tactical officers work longer shifts than beat officers and operate on either the second or third watch.[16] Unlike beat officers' fixed watch assignments, tactical teams rotate across watches according to a fixed, annually determined team-level schedule (see Figure A3).

CPD Operations Calendar and Day-off Groups The CPD operations calendar and day-off group system generates the day-to-day variation in staffing underlying our empirical strategy. Because the CPD operates every day, including weekends and holidays, all officers (beat and tactical) are assigned to regular day-off groups. Officers are assigned to one of six day-off groups (numbered 61 through 66). Officers in these groups work four consecutive days followed by two days off, cycling continuously. On any given day, two groups are off while four are on duty.

Figure A4 illustrates how this system operates during Period 1A of 2018 (January 7–20). For example, officers in group 64 were off on January 7–8, January 13–14, and January 19–20. Officers in the same group work together on all four of every six days; those in adjacent groups overlap three of every six days; and those in non-adjacent groups overlap two of every six days. Tactical teams are also assigned to fixed day-off groups—specifically, groups 61, 63, and 65. Because tactical teams adhere to these day-off group schedules, exactly two tactical teams are working on any given day, as depicted in Figure A5.

[15] Chicago Tribune, "Police Tactical Teams Tempt the Fates Daily," Jan 9, 1991.

[16] Second-watch tactical teams work from 9:30 a.m. to 6:30 p.m.; third-watch teams work from 6:00 p.m. to 3:00 a.m.

Day-off groups are assigned annually. In November, officers submit choices and are allocated by commanders based on seniority, preferences, and the need to balance manpower. Although officers may influence assignment at the margin (e.g., avoiding a particular holiday), they cannot systematically avoid working particular days of the week or holidays.

To the extent that day-off groups differ in size, the operations calendar generates rotating variation in the number of officers on duty across days. In principle, staffing should be balanced across day-off groups to ensure adequate coverage. In practice, some imbalance may arise due to transfers, medical leave, or other long-term absences. These imbalances are likely to be more pronounced for tactical teams than for beat officers. Beat staffing is more tightly regulated because CPD's community policing model requires minimum patrol coverage, and new recruits are typically assigned to beat positions, allowing imbalances to be corrected relatively quickly. Tactical staffing, by contrast, is less constrained by minimum coverage requirements, and tactical officers are selected based on experience, performance, and disciplinary history, making imbalances slower to correct. Finally, CPD directives explicitly require watch operations lieutenants to maintain balanced day-off group assignments among beat officers, whereas no analogous requirement exists for tactical lieutenants overseeing tactical teams. In light of these differences, we estimate our empirical models separately for beat and tactical officers.

IV.B. Data

We construct our dataset from CPD's administrative and personnel records covering 2010–2020. The core personnel files provide daily attendance and assignment information, including each officer's district and watch assignment, hours worked, and absences. We supplement these data with historical day-off group calendars (available from 1972–2021), which are central to our empirical strategy, and with individual-level day-off group assignments during the sample period.

To construct unit-level scheduled, rather than realized, staffing, we require two components: whether each officer is scheduled to be present and the unit to which the officer is effectively attached. Using the day-off group assignments and the operations calendar, we determine each officer's scheduled presence for every day in the month. For each month, we also assign each officer's modal unit from the preceding month as their scheduled unit.

Our primary outcomes are arrests, crimes, and use of force incidents. For each arrest, we observe charges, the FBI Uniform Crime Reporting (UCR) code, incident time, and location.

Crimes are classified as *index* (homicide, sexual assault, robbery, aggravated assault and battery, burglary, larceny, motor vehicle theft, and arson) or *non-index* (less severe crimes). Index crimes are further divided into violent and property crimes. Use of force incidents are reported through Tactical Response Reports (TRRs). Officers are required to submit a TRR whenever force is used, documenting officer demographics, officer actions, subject actions, and the time and location of the incident.

Our unit of analysis is the district-by-day. We aggregate outcomes such as total arrests and use-of-force incidents across watches within each district-day and construct predicted staffing by summing officers' scheduled presence at that level. We also calculate predicted officer demographic composition based on the operations calendar at the district-day level.

IV.C. Empirical Strategy

Our identification strategy exploits predetermined variation in daily staffing generated by CPD's day-off group scheduling system described in Section IV.A. This approach builds on prior work using the same rotation day-off group calendar (Ba et al., 2021; Gudgeon et al., 2025) and extends strategies that leverage administrative scheduling rules in other settings (Glover et al., 2017; Weisburd, 2021).

As detailed in Section IV.A, officers are assigned to day-off groups at the start of each year, and exactly four of six groups work each day. Because officers are often divided unevenly across groups – due to seniority preferences, transfers, and long-term absences – the operations calendar generates mechanical variation in the number of officers scheduled to work. For example, due to the rotation across day-off-groups and the size of each such group, a district might have 164 officers scheduled on one day but only 156 the next.

This variation is predetermined by the annual calendar established months in advance and is orthogonal to day-to-day crime fluctuations. We use predicted staffing based on these schedules as an instrument for realized officer presence, isolating variation unrelated to endogenous attendance decisions.

We estimate the following reduced-form specification for district j on day t:

$$y_{jt} = \beta_1 \,\text{Predicted Officers}_{jt} + \beta_2 \,\text{Officer Composition}_{jt} + \alpha_{j\times m(t)} + \delta_{dow(t)} + \delta_{holiday(t)} + \epsilon_{jt},$$

where y_{jt} represents daily outcomes (e.g., total crimes, violent crimes, use-of-force incidents),[17] *Predicted Officers*$_{jt}$ is the number of officers scheduled based on day-off group

[17]Our baseline specification considers counts of officers and crime or enforcement outcomes; Appendix

assignments, and *Officer Composition*$_{jt}$ includes the predicted share of Black, Hispanic, and male officers plus average tenure for beat and tactical officers.[18] We include district-by-year-month fixed effects ($\alpha_{j\times m(t)}$), day-of-week fixed effects ($\delta_{dow(t)}$), and indicators for 14 major holidays ($\delta_{holiday(t)}$). We cluster standard errors at the district level.

If unobservable determinants of daily outcomes are uncorrelated with the number of predicted officers, β_1 can be interpreted as the causal effect of having one additional officer scheduled to work. Our estimates capture short-run effects of transitory within-month staffing increases driven by day-off schedules. Note that this estimand differs from the longer-term effects of sustained staffing increases studied in the literature using city-year panel data (Chalfin and McCrary, 2018; Mello, 2019). Nonetheless, given the rotation-based scheduling of the CPD, our approach provides policy-relevant evidence on the impact of marginally deployed officers in a given district-day.

We also estimate corresponding instrumental variables specifications where actual officer presence is instrumented by predicted presence. This rescales the reduced form coefficient, β_1, by the first stage rate at which differences in scheduling actually become differences in staffing. The IV specification thus delivers an estimate of the causal effect of having an additional officer actually working, rather than simply scheduled to work. These estimates more directly measure the effect of an additional officer on violent crime and force, so we use them in our cost-benefit calculations below.

Our key assumption is that predetermined scheduled staffing is uncorrelated with unobservable determinants of daily outcomes, conditional on fixed effects and controls. This requires that day-off group assignments are not manipulated to target dates with expected high crime, and that within-month variation in scheduled staffing is as-good-as-random after conditioning on day-of-week and holidays. The annual assignment process based on seniority and preferences makes this plausible. District-by-year-month fixed effects absorb time-varying characteristics including average staffing levels and crime trends.

The instrument satisfies the relevance condition because day-off schedules are the primary determinant of daily attendance. Figure A6 shows the first stage relationship separately for beat and tactical officers. The horizontal axis displays daily officer counts normalized by each district's annual median, capturing relative staffing levels. The histogram (left

Table A4 illustrates similar results when alternatively scaling by 10,000 population.

[18]Motivated by evidence that officer demographics have important effects on policing outcomes (e.g., Ba et al. 2021), we include controls for predicted officer composition in our main specification. Appendix Table A5 illustrates similar results when omitting these controls.

axis) shows the distribution of predicted officer assignments, while the line and associated confidence intervals (right axis) illustrates the relationship between predicted and actual deployed officers, residualized of fixed effects and controls.

Panel (a) of Figure A6 demonstrates that predicted tactical officer assignments exhibit substantial variation across district-days, ranging from roughly 0.6 to 1.4 times the district-year median. This variation translates into a strong first stage relationship, with predicted assignments tightly predicting actual deployments. In contrast, panel (b) shows that beat officer assignments display much less variation, concentrated tightly around the median. This limited dispersion reflects the factors discussed above in Section IV.A, such as stronger efforts to maintain balance across day-off groups and quicker rebalancing mechanisms. Despite the narrower variation in beat officer staffing, the first stage relationship remains positive and statistically significant, with F-statistics well exceeding conventional thresholds for instrument strength in both specifications.

The exclusion restriction, which requires that predicted staffing based on day-off-group rotations impacts outcomes only through its effect on actual staffing, is plausible because day-off groups are assigned administratively months in advance and rotate mechanically, making daily variation in scheduled staffing within a month predetermined with respect to contemporaneous crime fluctuations. Although management may plan special operations when more officers are available, such coordination reflects one channel through which scheduled staffing affects outcomes, rather than a violation of exclusion.[19] We view violations of monotonicity as unlikely given the lack of clear mechanism by which an increase in scheduled staffing could cause a decrease in realized staffing.

Validation: Permutation Tests We verify our exogeneity assumption through permutation tests. A direct balance test examining the relationship between predicted staffing and observed, same-day crime outcomes is inappropriate as those outcomes may themselves respond to police presence. Instead, we examine whether scheduled officer deployment predicts crime outcomes drawn from other districts on the same day – outcomes that are less likely to be affected by policing activity in the focal district.

Specifically, for each district-day observation, we randomly reassign crime outcomes from

[19]In our analysis separately considering the impacts of beat and tactical officers, one relevant exclusion concern is the possibility that actual staffing of one type responds to scheduled staffing of the other (i.e., command staff ensures additional beat officers on duty when fewer tactical officers are scheduled and vice versa). As shown in Appendix Table A6, however, we find minimal support for this hypothesis.

a different district located in a different policing area on the same calendar day. The CPD is organized into three geographic areas; restricting reassignment across areas minimizes the possibility that reshuffled outcomes are affected by spatial spillovers from the focal district's policing activity. Reassignment is conducted without replacement within each day. We repeat this procedure 500 times and, in each iteration, estimate our baseline specification relating officer deployment to the reshuffled crime outcomes.

Under the null that scheduled officer levels are unrelated to underlying crime conditions, the estimated coefficients should be centered around zero, and approximately 5 percent of permutation iterations should yield statistically significant estimates at the 5 percent level.

Figure A7 displays the distribution of coefficients across the 500 permutation iterations, separately for all patrol officers and tactical officers. Across all outcomes, coefficient distributions are tightly centered around zero, and the share of statistically significant estimates closely matches the expected false-positive rate. For all patrol officers, between 3.4 percent and 8.2 percent of iterations yield p-values below 0.05 across the six outcomes, with an overall rate of 5.1 percent. For tactical officers, the corresponding range is 4.2 percent to 5.0 percent, with an overall rate of 4.7 percent.

These permutation tests illustrate that changes in predicted officer staffing in a given district cannot predict changes in crime in other districts, suggesting that fluctuations in staffing generated by the day-off group scheduling calendar are unrelated to changes in daily crime conditions and lending credence to our exogeneity assumption.

IV.D. Results

We estimate effects separately for beat and tactical officers by relating predicted staffing of each type to total district-day outcomes, given their different operational mandates and variation in scheduled staffing (as described in Section IV.A). Our discussion in the main text focuses on the impacts of tactical officers, while we report estimates for beat officers in Appendix Table A7. This decision is motivated by the fact that our empirical design has more statistical power to study effects of tactical officers because day-off group rotations generate more variation in tactical officer staffing.[20] We also view the impacts of tactical officers as especially interesting given the greater degree of discretion in their enforcement duties and the strong parallels between the activities of tactical officers and recent federal law

[20]While our design is better suited to the study of tactical officers, we also note that we find stronger effects of tactical officers than beat officers on both crime and use of force above and beyond differences in statistical precision across these two analyses.

enforcement deployments in major U.S. cities. Although tactical officers are likely engaging in more specialized deployments during any given shift, we emphasize again that our research design leverages only variation in available tactical officer staffing rather than the specifics of any given deployment.

The second and third columns of Table III consider the effect of assigning an additional tactical officer on police activity: arrests and force. Panel A shows the reduced form relationship between scheduling an additional tactical officer and these outcomes. Panel B shows the corresponding IV estimate of an additional tactical officer actually working. We find that an additional tactical officer is associated with statistically significant increases in both arrests and incidents of use of force.

To assist with interpreting magnitudes – given that outcome means are relatively small and that our analysis is at the district-day level – we also report the elasticity implied by the IV estimate, defined as $\hat{\beta}_{IV} \cdot (\mu_X/\mu_Y)$, where μ_X and μ_Y denote the average number of tactical officers and the average outcome, respectively. Elasticities for both arrests and use of force are around 0.19, suggesting modest effect sizes on both margins.

The remaining columns of Table III consider the effect of assigning an additional tactical officer on crime. Although we find no effect on overall crime, this is potentially confounded by the fact that many forms of contraband crime (e.g. drug crimes) are reported only following police action. Additional tactical officers do, however, reduce crimes with a victim – we find statistically significant declines in violent crime and index crime as defined by the FBI's Uniform Crime Reporting program. We also find a negative, although statistically indistinguishable from zero, effect of tactical officers on property crime. As suggested both by the size of the coefficients and the implied elasticities (-0.025 for index crime and -0.048 for violent crime), these estimates again suggest modest effects of marginal tactical officers.

V. Cost-Effectiveness of Marginal CPD Tactical Officers

Given the hypotheticals posed to our survey participants, two numbers from our quasi-experimental analysis are of particular interest: the effect of an additional tactical officer on use of force incidents and the effect of an additional tactical officer on violent crime. While much more could be said about the interpretation of these estimates, our cost-benefit framework requires only that we have estimates of these two parameters which are generated in the same context and using the same methodology.

In our IV specifications, we find that an additional tactical officer decreases violent crime by 0.0072 incidents per district-day and increases use of force by 0.003 incidents per district-day. The ratio of these two estimates is about 2.3, which is lower than the benchmark ratio of valuations (= 3.94). Hence, by comparing the point estimates for crime and force in light of the estimated civilian valuations from our survey, we would conclude that a marginal tactical officer generates more social costs in the form of use of force than social benefit in the form of crime reduction.

However, we also note that our quasi-experimental estimates are subject to sampling variation. We compute a confidence interval on the ratio of causal effects using a Bayesian bootstrap (Rubin, 1981), clustering at the district-level. The upper 95 percent confidence bound is 6.3 and 20 percent of the bootstrapped distribution lies above 3.94, meaning that we cannot statistically reject the null that the ratio of causal effects equals the benchmark valuation ratio at conventional levels.

We next combine the estimates derived from Section II and Section IV.D to recreate Figure II. This allows us to take officer salary into account and more directly compare our benefit-cost calculations to the prior literature. In Figure III, we calculate the benefit-cost ratio as in Section III using the ratio ${}^{v_f}/_{v_c} = 3.94$ from our survey estimates and Δ_f and Δ_c from our quasi-experimental design. We multiply the district-day estimates of changes in force and crime by the average 187 shifts a tactical officer works in a year. This gives $\Delta_f = 0.561$ and $\Delta_c = -1.346$. We calculate an officer's salary relative to the cost of crime, ${}^{w}/_{v_c} = 0.323$, based on an inflation-adjusted version of the annual cost of an officer from Chalfin and McCrary (2018), \$152,100, and our survey-based estimate of the cost of crime, \$470,000. Finally, we add bootstrapped 95% confidence intervals for the BCR at both an assumed force effect of 0 and our observed estimate of $\Delta_f = 0.561$.

Figure III shows that incorporating the cost of force into calculations of the BCR of marginal tactical officers has profound effects. When force is ignored, officers appear to have a substantial net benefit (BCR $\approx$ 4) relative to their salaries. When taking use of force into account, officers likely have a net negative effect on social welfare, even ignoring their salary costs. While imprecision in our quasi-experimental estimates precludes us from rejecting the break-even null, we can rule out BCR's which are well above one and our analysis clearly suggest meaningful differences in cost effectiveness when force is taken into account.

These conclusions are not particularly sensitive to assumptions about the fiscal costs of staffing. Our estimated BCR is 0.61 when ignoring salary, and incorporating our salary estimate only reduces the BCR to 0.53. Figure IV shows this directly in dollar units. Panel

A uses our survey estimate of the cost of police use of force, and Panel B instead assumes that the cost of police use of force is equal to the cost of violent crime (that is, $v_f/v_c = 1$). In both panels, the main contributor to the cost of policing is use of force, though only in Panel A is that force cost large enough to exceed the benefit of violent crime reduction. Previous evaluations of the cost-effectiveness of marginal officers have emphasized salary as the main cost to be considered, but we find it to only be second-order in Figure IV. Our results de-emphasize fiscal costs for two reasons. First, we estimate a much higher benefit of preventing violent crime, and second, we introduce the even larger cost of police use of force.

VI. Conclusion

In this paper, we study the potential tradeoff between crime reduction benefits and social costs associated with investments in policing. First, we estimate citizen valuations of violent crime and police use of force using conjoint analysis in an original survey which elicits individuals' choice of housing among options which vary in terms of prices, other characteristics, and neighborhood prevalence of crime and police use of force. Our survey results imply a willingness to pay to reduce violent crime of $470,000, larger than estimates commonly used in the literature but consistent with other estimates using comparable methodologies (Cohen et al., 2004), and a marginal rate of substitution between crime and force of 0.252. In other words, individuals are indifferent between an intervention preventing four violent crimes and one which prevents one incident of police use of force.

Next, we directly estimate the causal effect of marginal police officers on both violent crime and use of force, relying on administrative data from the Chicago police department and an identification strategy leveraging variation in officer staffing at the district $\times$ day level generated by officers' predetermined work schedules. We find that marginal tactical officers, those tasked with higher-stakes police work, reduce violent crime by 0.0072 incidents per district-day but increase use of force by 0.003 incidents per district-day.

Annualized over an officer's typical number of working days per year and combined with our survey estimates, these estimates imply that marginal officers provide a social benefit in terms of crime reduction well in excess of their salary, consistent with cost-benefit conclusions reached in the existing literature on police manpower (Chalfin and McCrary 2018; Chalfin and McCrary 2017). However, incorporating the social costs associated with additional police use of force reverses conclusions about cost-effectiveness.

While our analysis in Chicago focuses on the impacts of changes to police manpower, our survey findings have potentially important implications for the social desirability of a wide array of criminal justice policies, such as specific police deployment tactics (Braga and Bond, 2008; Braga et al., 2019; Jabri, 2021) or equipping the police with military equipment (Masera, 2021; Bove and Gavrilova, 2017; Harris et al., 2017). In particular, our findings highlight that implications for use of force need be considered when evaluating cost-effectiveness and suggest that future researchers should measure impacts on force whenever possible.

We emphasize that our finding — that marginally deployed tactical officers employ force in excess of what their crime-reduction benefits would justify — is specific to the setting and context we examine. Understanding variation across settings is, of course, important for drawing broader conclusions about the average benefit of increasing police staffing across the country. Nonetheless, an important takeaway from our analysis is that any policy which can increase the ratio of an officer's crime reduction benefit and their propensity to use force, such as improved police training (Dube et al., 2025; Adger et al., 2025) or changes in hiring practices (Saltiel and Tuttle, 2022), would carry significant social benefits.

Finally, worth noting is that our analysis captures only one dimension, albeit likely the most serious, of socially undesirable behavior by the police. The public may also have meaningful willingness to pay to avoid other forms of misconduct, such as failure to provide services, discrimination on the basis of race, age, or sex, or making legally invalid stops or arrests. Investigating the impacts of investments in policing on these other police actions, as well as society's valuation thereof, represents an interesting avenue for future research.

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Figures and Tables

FIGURE I. Survey Flow

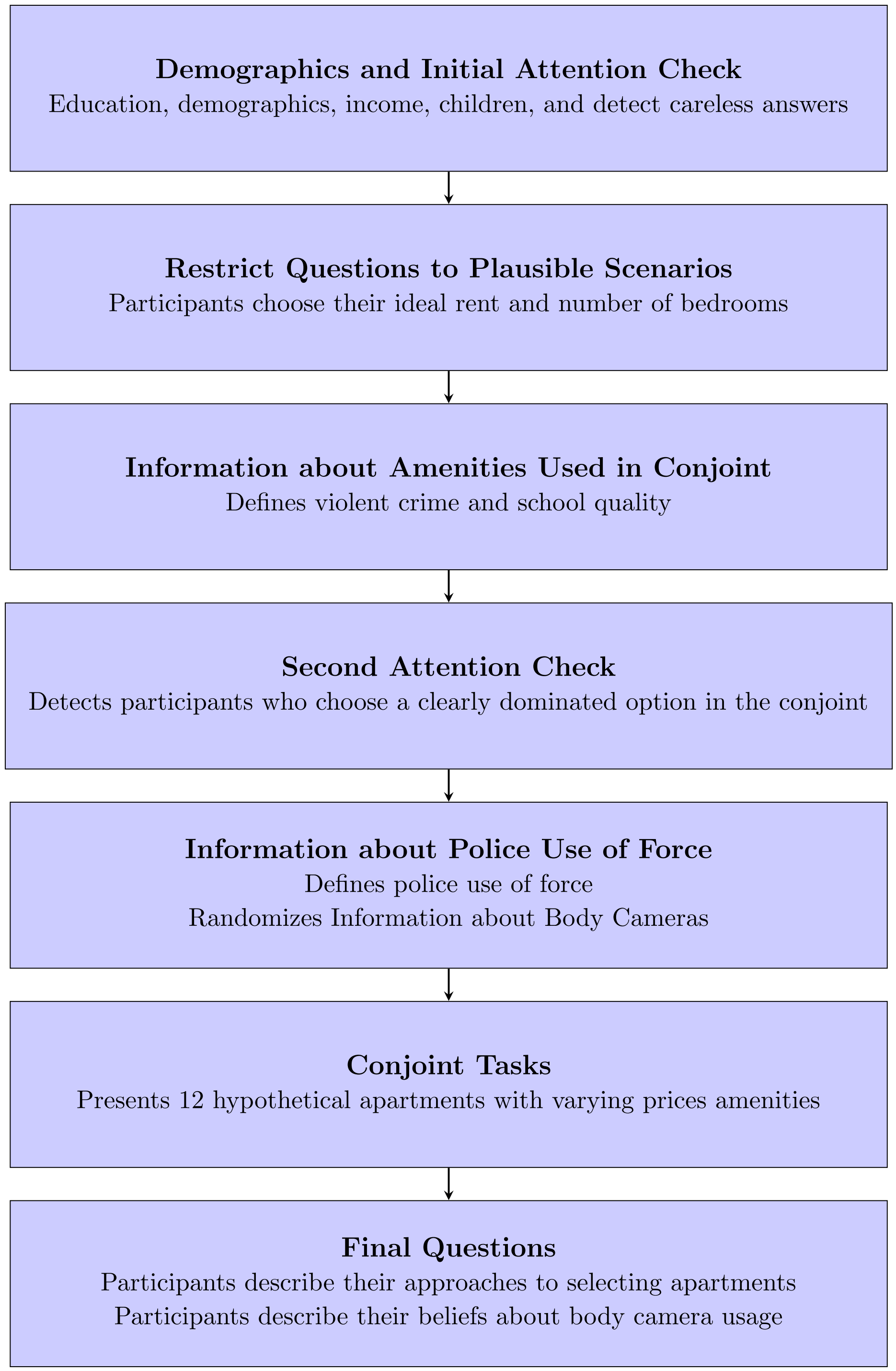
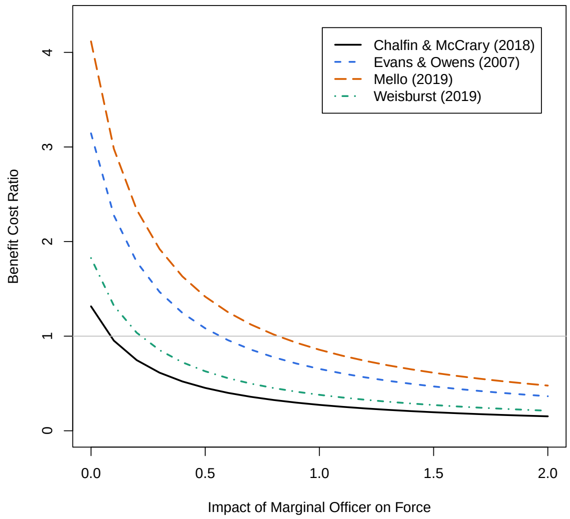
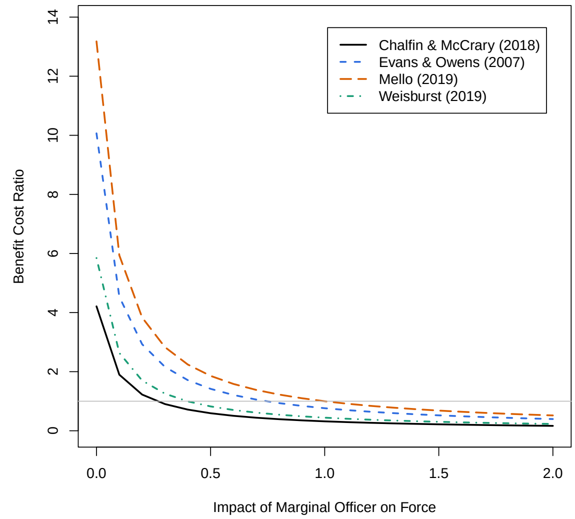


FIGURE II. Implications of force valuations for benefit-cost ratios from the literature



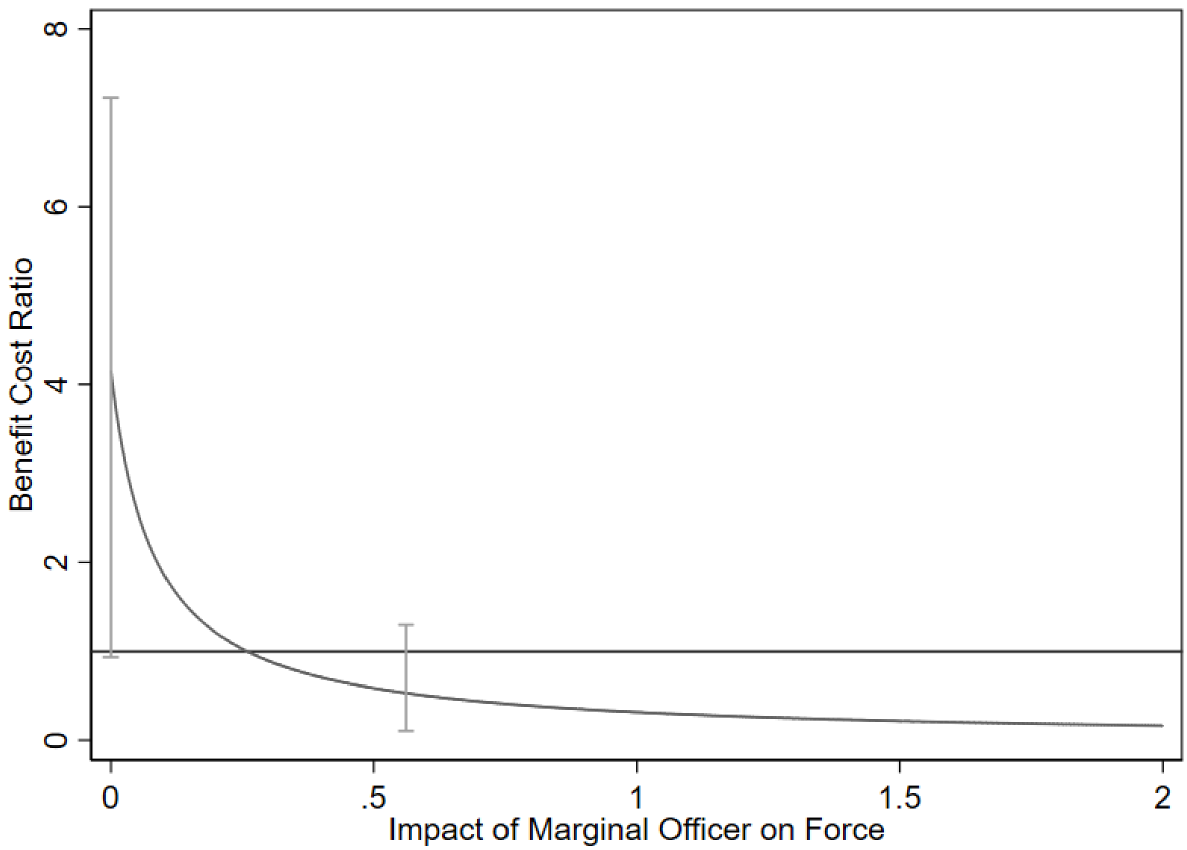
(a) Based on Chalfin and McCrary (2018)



(b) Based on Survey Estimates

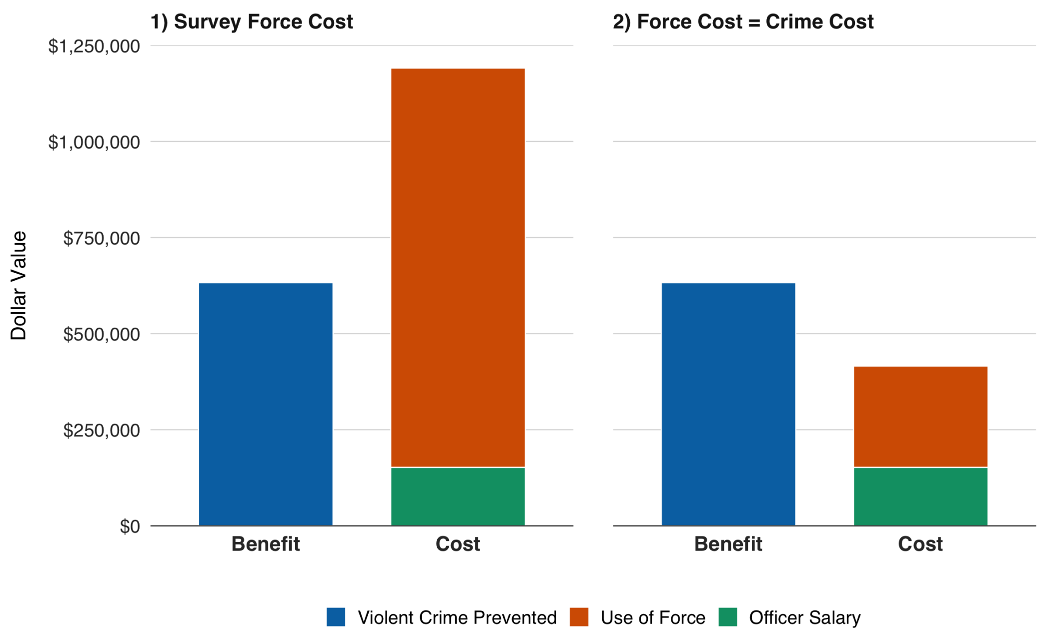
Notes: Each panel reports the benefit-cost ratio of a marginal police officer, defined as $v_c\Delta_c/(w + v_f\Delta_f)$ as a function of the impact of the marginal officer on use of force, Δ_f, using estimates of Δ_c, the impact of an officer on violent crime, from the literature. In panel (a), we use estimates of v_c (the value of a violent crime) and w from Chalfin and McCrary (2018) and use the ratio v_f/v_c from our survey estimates to construct the implied v_f (= \$494,888). In panel (b), we use our survey-based estimates of v_f and v_c and the inflation-adjusted w from Chalfin and McCrary (2018) (= \$152,100). Estimates of the impact of police on crime from the literature are reported in table A2 and the implied "break-even" levels of Δ_f are summarized in table A3.

FIGURE III. Benefit-Cost Ratio of a Marginal CPD Tactical Officer



Notes: This figure reports the benefit-cost (BCR) ratio of a marginal CPD tactical officer under various assumptions about their contribution to police use of force. We calculate the BCR using $\frac{-\Delta_c}{w/v_c + \Delta_f v_f/v_c}$ where we take $v_f/v_c = 3.94$ and $v_c = \$470,000$ from our survey estimates, $w = \$152,100$ from Chalfin and McCrary (2018) adjusted for inflation, and $\Delta_c = -0.0072 \cdot 187$ from our estimate of the marginal effect of a CPD tactical officer on violent crime in a single shift and the average number of shifts worked by tactical officers. The figure then plots this expression for different values of Δ_f. We present 95% confidence intervals at $\Delta_f = 0$ and $\Delta_f = 0.003 \cdot 187$, where the latter comes from our estimate of the marginal effect of a CPD tactical officer on use of force. The confidence intervals are based on bootstrap estimation that resamples both Δ_f and Δ_c.

FIGURE IV. Benefit-Cost Comparison of a Marginal CPD Tactical Officer



Notes: This figure compares the monetized benefits and costs of deploying a marginal Chicago Police Department tactical officer under two different valuation approaches. Benefits reflect the value of violent crimes prevented, calculated using the marginal effect estimate of 0.0072 fewer violent crimes per officer-day, multiplied by 187 deployment days and a $470,000 willingness-to-pay per violent crime prevented. Costs include officer salary ($152,100 annually) and the social cost of use of force incidents. Model 1 values use of force using the survey-estimated cost of $1.85 million per incident (3.94 times the crime prevention value). Model 2 assumes use of force and violent crime have equal social costs. Use of force costs are calculated using the marginal effect estimate of 0.003 additional force incidents per officer-day multiplied by 187 deployment days.

TABLE I. Willingnesses to Pay

	All
WTP for 100 Sq Ft	1.027
	(.058)
WTP for School Quality Quartile	1.294
	(.079)
WTP for Violent Crime (per 10,000 residents)	-.09
	(.004)
WTP for Use of Force (per 10,000 residents)	-.355
	(.02)
MRS of Crime and Force (per 10,000 residents)	.252
	(.018)

This table presents our results on willingness to pay from a mixed multinomial logit model. We produce estimates of willingness to pay for each of the apartment characteristics considered in the survey by dividing the estimated mean of its random coefficient by our estimate of disutility with respect to rent. Square footage is in units of 100 square feet, school quality in units of 1 quartile, violent crime and police use of force in units of 1 per 10,000 neighborhood residents. On the last row, we calculate the marginal rate of substitution between violent crime and police use of force.

TABLE II. Robustness of Willingness to Pay Estimates

	(1)	(2)	(3)	(4)
Panel A: Quality of Life (QOL) Information				
	QOL Shown	QOL Not Shown	P-value	
WTP for 100 Sq Ft	1.374	0.803	0.003	
	(0.242)	(0.133)		
WTP for School Quality Quartile	1.405	0.529	<0.001	
	(0.248)	(0.136)		
WTP for Violent Crime (per 10,000 residents)	-0.113	-0.060	<0.001	
	(0.017)	(0.007)		
WTP for Use of Force (per 10,000 residents)	-0.533	-0.209	<0.001	
	(0.100)	(0.036)		
MRS of Crime and Force	0.211	0.287	0.170	
	(0.051)	(0.060)		
WTP for QOL Index	0.169			
	(0.024)			
Panel B: Force as Proxy for Neighborhood Quality				
	All Respondents	Not Revealing		
WTP for 100 Sq Ft	1.151	0.891		
	(0.138)	(0.170)		
WTP for School Quality Quartile	1.052	0.982		
	(0.149)	(0.187)		
WTP for Violent Crime (per 10,000 residents)	-0.084	-0.065		
	(0.008)	(0.008)		
WTP for Use of Force (per 10,000 residents)	-0.332	-0.215		
	(0.040)	(0.045)		
MRS of Crime and Force	0.254	0.302		
	(0.038)	(0.073)		
Panel C: Population Reweighting and Incentivized Survey				
	Baseline	U.S. Weights	Chicago Weights	Incentivized
WTP for 100 Sq Ft	1.027	1.351	1.356	
	(0.058)	(0.129)	(0.186)	
WTP for School Quality Quartile	1.294	1.465	1.715	-0.030
	(0.079)	(0.159)	(0.224)	(0.024)
WTP for Violent Crime (per 10,000 residents)	-0.090	-0.129	-0.167	-0.078
	(0.004)	(0.015)	(0.027)	(0.012)
WTP for Use of Force (per 10,000 residents)	-0.355	-0.458	-0.596	-0.449
	(0.020)	(0.061)	(0.090)	(0.133)
MRS of Crime and Force	0.252	0.282	0.280	0.174
	(0.018)	(0.035)	(0.040)	(0.057)

Notes: This table reports robustness of WTP estimates. All WTP estimates are derived using the same estimation approach described in Table I. Panel A reports results from an additional hypothetical experiment (N=256, November 2024) in which half of respondents were randomly assigned to observe an additional apartment attribute: a Quality of Life (QOL) index. The QOL index was described as a comprehensive block-level measure provided by a local magazine. Column (1) reports results for respondents shown the QOL index, Column (2) for those not shown the index, and Column (3) reports p-values for the difference in WTP estimates across conditions. Panel B reports estimates from the same experimental sample. Column (1) includes all respondents, while Column (2) excludes respondents who indicated that they used police use of force as a proxy for other neighborhood characteristics when making choices. Panel C reports robustness checks for external validity. Column (1) reproduces baseline survey results. Columns (2) and (3) reweight the sample to match U.S. and Chicago Census distributions for race, age, income, and sex. Column (4) reports results from an incentivized survey conducted from summer 2023 through spring 2024 (N=66), in which respondents received personalized housing recommendations based on their responses.

TABLE III. Effects of Tactical Officers on Crime, Arrests, and Use of Force

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
			Panel A: Reduced Form				
	Actual Tactical Officers	Arrests	Force	Crime	Index Crime	Violent	Property
Pred. Tactical Officers	.692***	.0712***	.00209***	.00768	-.0119**	-.00498*	-.0069
	(.0155)	(.0127)	(.000727)	(.00822)	(.00543)	(.00242)	(.00514)
First-stage F-stat	2,007	.	.	.	.	.	.
Y mean	21.36	11.79	0.34	35.29	14.78	3.21	11.57
Observations	76,168	76,168	76,168	76,168	76,168	76,168	76,168
			Panel B: IV Estimates				
		Arrests	Force	Crime	Index Crime	Violent	Property
Actual Tactical Officers		.103***	.00302***	.0111	-.0172**	-.0072*	-.00997
		(.0184)	(.00106)	(.0119)	(.00776)	(.0035)	(.00738)
Y mean	.	11.8	.34	35.3	14.8	3.21	11.6
Implied elasticity	.	.186	.19	.00671	-.0248	-.0478	-.0184
Observations	.	76,168	76,168	76,168	76,168	76,168	76,168

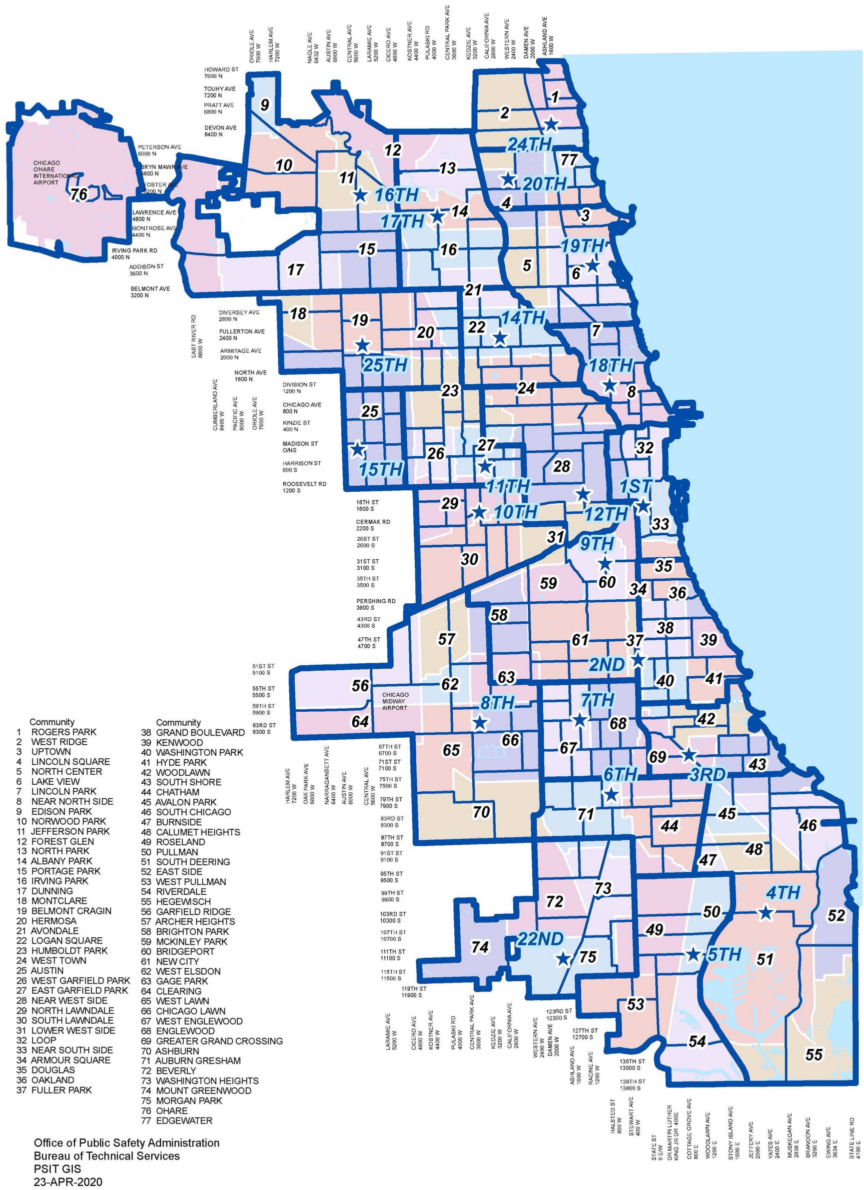
Notes: This table presents reduced form and instrumental variable estimates of tactical officers on daily district-level outcomes. Panel A reports reduced form estimates of the effect of predicted tactical officers on actual tactical officers and district-day-level outcomes. Panel B reports instrumental variable estimates where actual tactical officers are instrumented by predicted tactical officers. Predictions are based on assigned teams from the previous month and current day-off groups. All specifications control for predicted beat officers, officer composition (share Black, Hispanic, male, and average tenure), and include district-by-year-month, day-of-week, and holiday (14 categories) fixed effects. First-stage F-statistics are reported in Panel A to assess instrument strength. Standard errors, clustered at the district level, are in parentheses. Implied elasticities are computed as $\hat{\beta}_{IV} \cdot (\mu_X/\mu_Y)$, where μ_X and μ_Y denote the estimation-sample means of actual tactical officers and the outcome, respectively. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

(For Online Publication)

Justin Holz, Andrew Jordan, Taeho Kim, Steven Mello

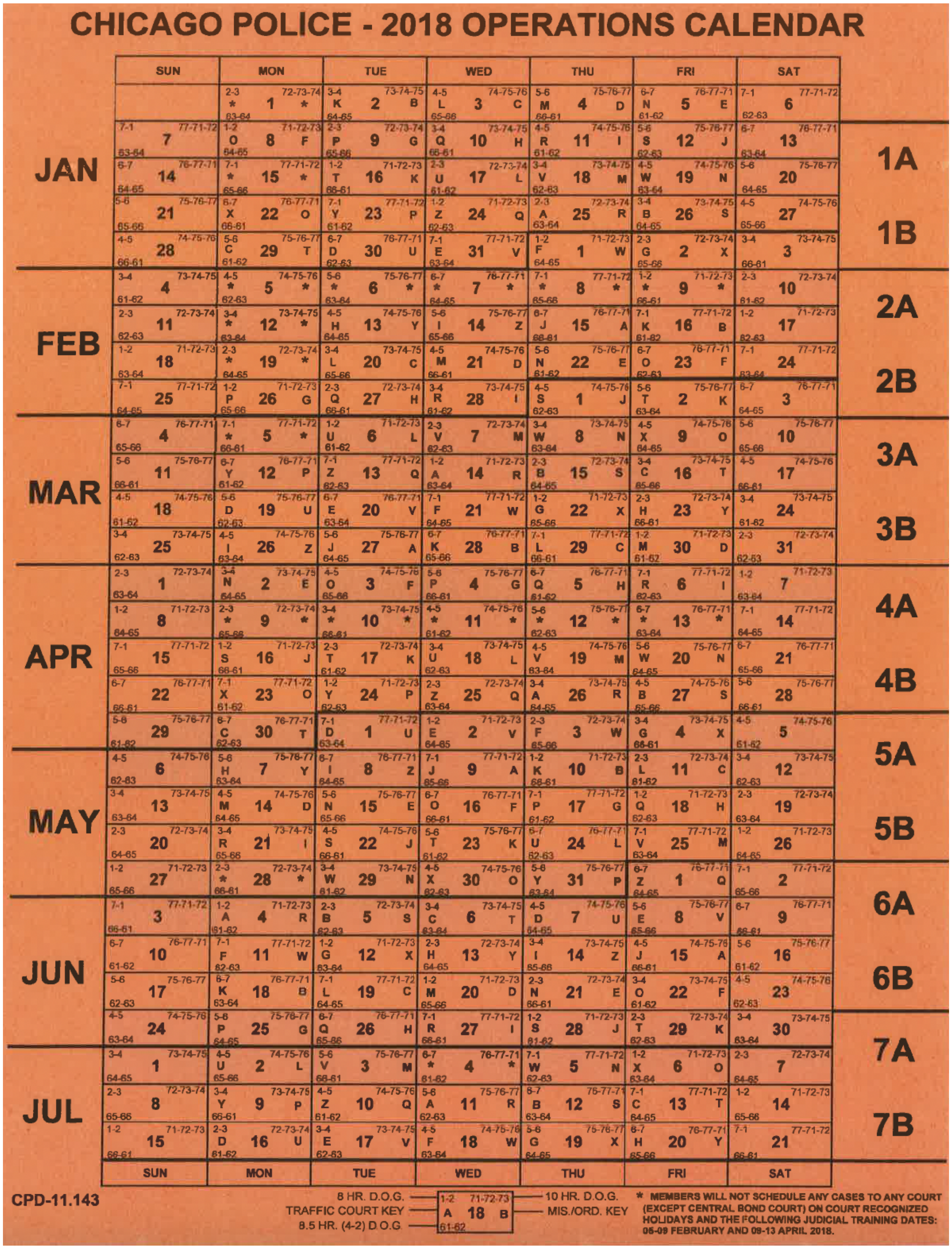
A. Appendix Figures and Tables

FIGURE A1. Map of CPD Districts



Note: This map, taken directly from `https://home.chicagopolice.org/wp-content/uploads/2020/07/CPD-Area-Map.pdf`, shows the 22 Chicago police districts (denoted by the solid, thicker blue lines). The district headquarters is denoted by a star. The thinner blue lines denote beats. The multi-colored areas numbered 1-77 are Chicago community areas.

FIGURE A2. Day-off Group Calendar in 2018



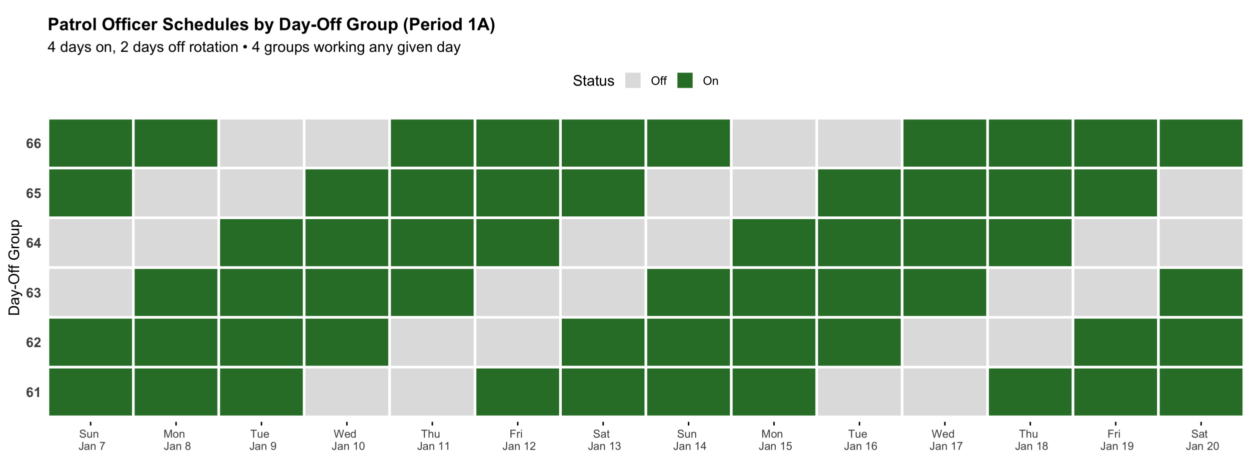
Note: This figure shows the 2018 day-off group calendar obtained from the CPD. The numbers between 61 and 66 in the bottom left corner of each box indicate whether that group has a day off on the given day. For example, on Sunday Jan 7, the 64-65 indicates that day-off groups 64 and 65 are not expected on duty on this day. The other numbers in the top left and right corners pertain to day-off groups for specialized personnel not under consideration in this study.

FIGURE A3. Tactical Team Watch Rotation

2018	Tactical Team A (61)	Tactical Team B (62)	Tactical Team C (63)
	Day Off Group	Day Off Group	Day Off Group
	61	63	65
PERIOD	Watch	Watch	Watch
1st	2nd	3rd	Relief
2nd	3rd	Relief	2nd
3rd	Relief	2nd	3rd
4th	2nd	3rd	Relief
5th	3rd	Relief	2nd
6th	Relief	2nd	3rd
7th	2nd	3rd	Relief
8th	3rd	Relief	2nd
9th	Relief	2nd	3rd
10th	2nd	3rd	Relief
11th	3rd	Relief	2nd
12th	Relief	2nd	3rd
13th	2nd	3rd	Relief

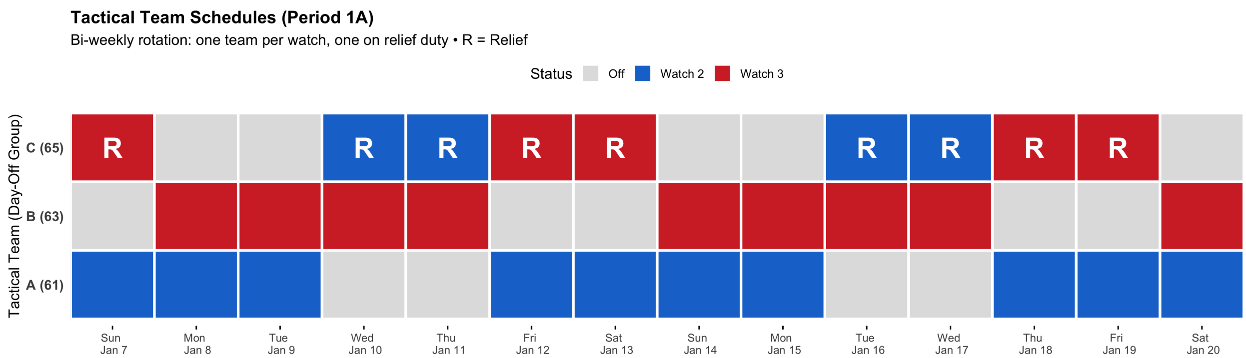
Note: This illustrates the monthly rotation schedule of tactical teams within a CPD district in 2018. Each month, one team was assigned to the second watch (09:30–18:30 hours), one to the third watch (18:00–03:00 hours), and one served as a relief team covering for the others during their regular days off. The rotation alternated systematically throughout the year according to the CPD's annual operations schedule

FIGURE A4. Day-off Group Rotation Example in 2018



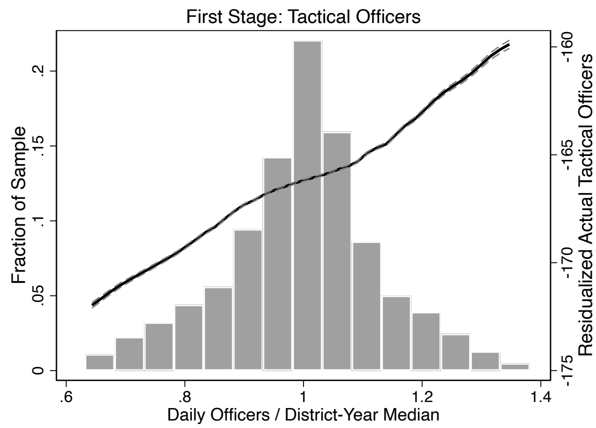
Note: This figure illustrates the CPD operations calendar for Period 1A (January 7–20, 2018). It shows work schedules for all six patrol officer day-off groups (61–66). Officers work four consecutive days followed by two days off in a continuous rotation. On any given day, exactly four groups are on duty (green) while two groups are off (gray).

FIGURE A5. Day-off Group Rotation Example in 2018: Tactical Teams

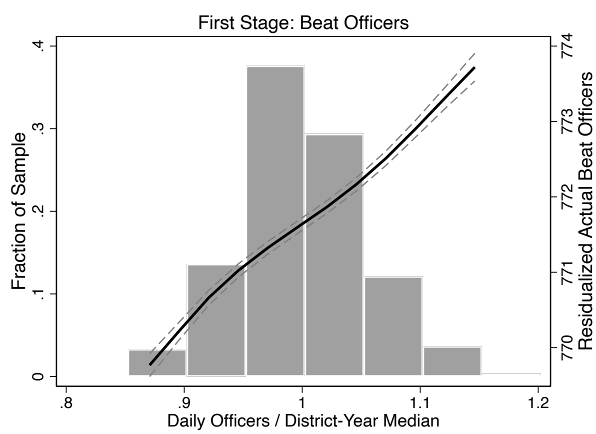


Note: This figure illustrates the CPD operations calendar for the tactical team in Period 1A (January 7–20, 2018). There are three tactical teams (A, B, C), which are assigned to day-off groups 61, 63, and 65 respectively. Each team follows its assigned day-off group pattern. In Period 1, Team A is assigned to Watch 2 (day, blue), Team B to Watch 3 (night, red), and Team C serves as the relief team. The relief team covers whichever watch is vacant due to the other team's days off, indicated by "R" markers. For example, on January 7, Team B (day-off group 63) is off, so Team C covers Watch 3 as relief. Team assignments to watches rotate every two weeks according to a pre-specified calendar.

FIGURE A6. First Stage: Instrument Variation and Relationship



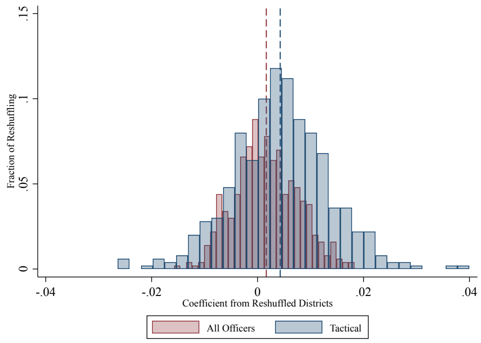
(a) Tactical Officers



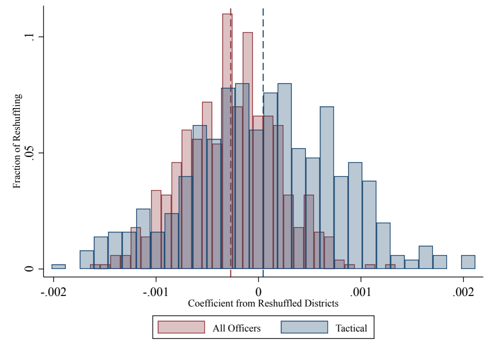
(b) Beat Officers

Notes: Each panel shows the distribution of assigned officers in the Chicago Police Department(histogram, left axis) and the first stage relationship between assigned and actual deployed officers (line and confidence intervals, right axis). Officers are normalized by district-year median. Sample trimmed at 2nd and 98th percentiles.

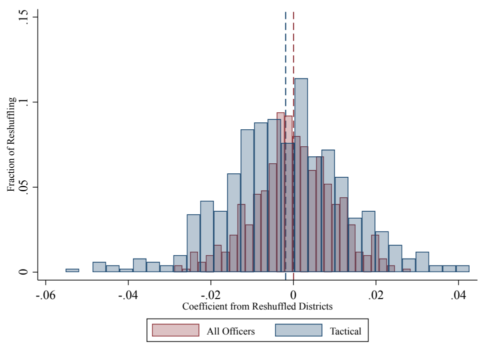
FIGURE A7. Permutation Test: Distribution of Coefficients from Reshuffled Districts



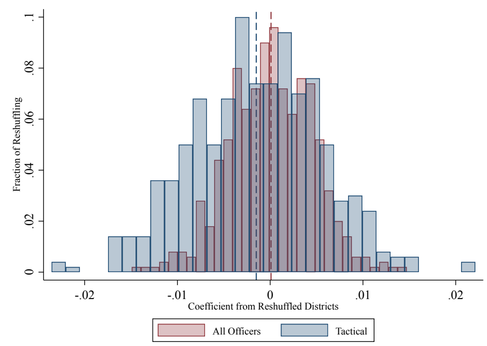
(a) Arrests



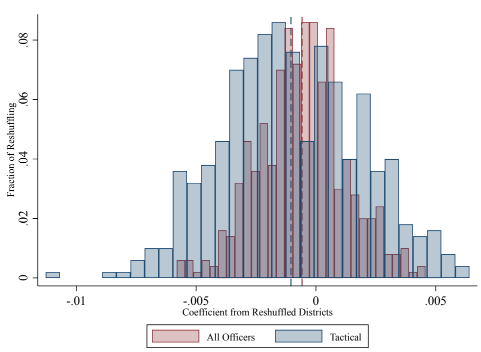
(b) Use of Force



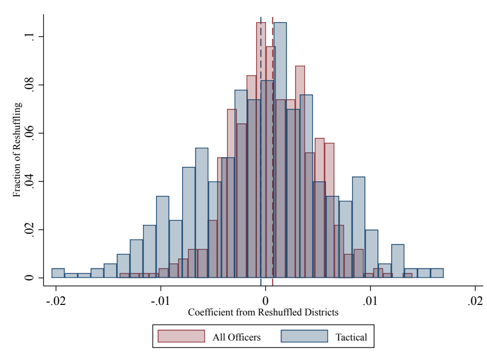
(c) Crime Incidents



(d) Index Crime



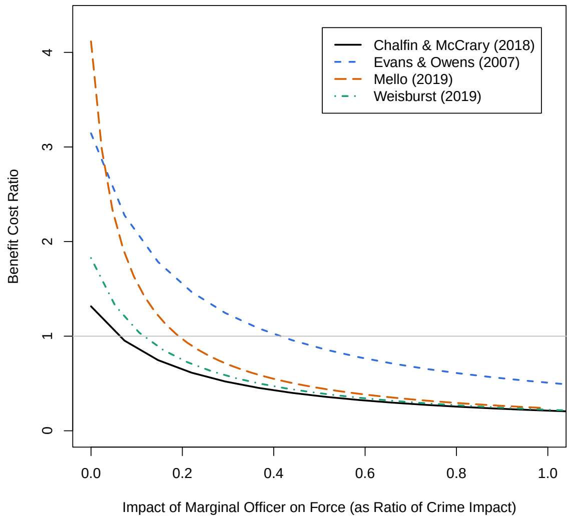
(e) Violent Crime



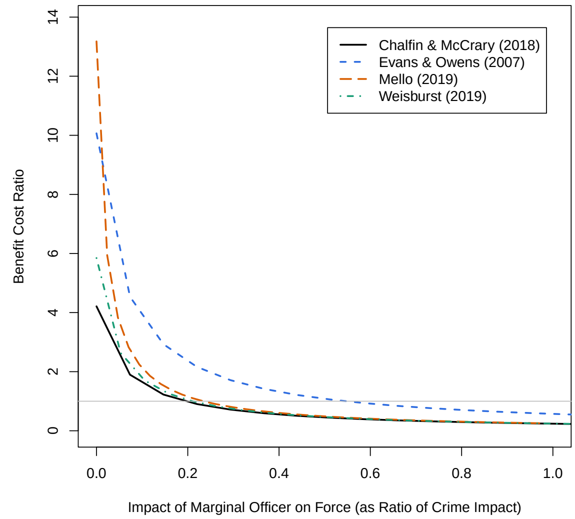
(f) Property Crime

Notes: Figures display the distribution of coefficients from permutation test iterations where district-level outcomes are randomly reassigned across districts on each day. The maroon (red) histogram shows the null distribution for all patrol officers, while the navy (blue) histogram shows the null distribution for tactical officers. Dashed vertical lines indicate the mean of each permutation distribution. Proportion of permutation iterations with $p < 0.05$: Arrests (All Officers 4.4%; Tactical 4.2%); Force (All Officers 8.2%; Tactical 5.0%); Crime (All Officers 5.6%; Tactical 4.8%); Index Crime (All Officers 3.4%; Tactical 5.0%); Violent (All Officers 5.2%; Tactical 4.6%); Property (All Officers 4.0%; Tactical 4.8%). Across all outcomes: All Officers 5.1%; Tactical 4.7%.

FIGURE A8. Implications of force valuations for benefit-cost ratios from the literature



(a) Based on Chalfin and McCrary (2018)



(b) Based on Survey Estimates

Notes: Same as Figure II except that we rescale the impact of the marginal officer on use of force (horizontal axis) as a ratio of the relevant study's estimated impact of the marginal officer on violent crime.

TABLE A1. Summary Statistics

	(1) All
Male	.345
	(.476)
Black	.286
	(.988)
Hispanic	.0792
	(.27)
Age	45.6
	(15.8)
Bachelor	.249
	(.433)
High school or less	.331
	(.471)
No Child	.699
	(.459)
One Bed	.275
	(.447)
Income: $<$25,000	.347
	(.476)
Income: 25k-50k	.365
	(.482)
Income: $>$50,000	.288
	(.453)
N. of participants	1,035
N. of obs.	72,450

Notes: This table summarizes the characteristics of the 1,035 survey respondents in our data for the hypothetical survey. We summarize demographics, education attainment, whether the respondent seeks a one-bedroom or studio apartment, and desired rental price range.

TABLE A2. Existing estimates of impact of police force size on violent crime

	(1) Chalfin & McCrary (2018)	(2) Evans & Owens (2007)	(3) Mello (2019)	(4) Weisburst (2019)
Estimated impact on violent crime				
IV estimate	-1.363	-3.258	-4.265	-1.892
	(0.38)	(1.084)	(2.022)	(0.75)
Elasticity	-0.34	-0.99	-1.3	-1.28
Sample means				
Police per 10,000	24.6	18.2	23.32	25.53
Violent crime per 10,000	97.3	59.8	75.16	34.8
Sample construction				
Population threshold	50,000+	10,000+	1,000+	1,000+
Cities	242	2,073	4,327	6,990
Years	1960–2010	1990–2001	2004–2014	2000-2014

Notes: This table reports estimated impacts of the marginal police officer on violent crime from the literature. Estimate from Chalfin and McCrary (2018) is from their table 3, column 9. Estimate from Evans and Owens (2007) is from their table 5, column 9. Estimate from Mello (2019) is from their table 5, column 1. Estimate from Weisburst (2019) is from their table 3, column 4. Note that Chalfin and McCrary (2018) report only elasticities; we convert their estimated elasticity into a "levels" estimate by multiplying the reported elasticity by the ratio of the sample mean violent crime rate and police rate.

TABLE A3. Implied Benefit-Cost Ratios and Break-Even Force Effects from Literature

	(1) Chalfin & McCrary (2018)	(2) Evans & Owens (2007)	(3) Mello (2019)	(4) Weisburst (2019)
Estimated impact on violent crime				
IV estimate	-1.363	-3.258	-4.265	-1.892
	(0.38)	(1.084)	(2.022)	(0.75)
Using Chalfin & McCrary (2018) valuations				
BCR without force effect	1.315	3.144	4.116	1.826
Break-even force effect	0.083	0.563	0.819	0.217
Using survey valuations				
BCR without force effect	4.212	10.067	13.179	5.846
Break-even force effect	0.264	0.744	0.999	0.398

Notes: This table reports salient quantities from figure II. Specifically, using the estimate of the impact of police on violent crime for each of the four studies, we report the implied benefit-cost ratio of the marginal officer assuming a null effect of the marginal officer on use of force, given by $v_c\Delta_c/w$ and the "break-even" impact of the marginal officer on use of force, or the impact of the marginal officer on use of force which sets the benefit-cost ratio equal to one, given by $(v_c\Delta_c - w)/v_f$. In the second panel, we report these quantities using estimates of w and v_c from Chalfin and McCrary (2018) and the associated estimate v_f implied by the ratio v_f/v_c in our survey estimates. In the third panel, we report these quantities using the v_c and v_f estimates directly from our survey and an inflation-adjusted version of w from Chalfin and McCrary (2018).

TABLE A4. Tactical Officer Results Adjusted for Population: Reduced Form + IV Estimates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Panel A: Reduced Form Per 10k Capita						
	Actual Tactical Officers	Arrests	Force	Crime	Index Crime	Violent	Property
Pred. Tactical Officers							
(10k capita)	.712***	.0955***	.00244**	.0186	-.007	-.00625**	-.000747
	(.0212)	(.0228)	(.00106)	(.0112)	(.0048)	(.00264)	(.00442)
First-stage F-stat	1,129	.	.	.	.	.	.
Y mean	2.12	1.24	0.04	3.47	1.42	0.33	1.09
Observations	76,168	76,168	76,168	76,168	76,168	76,168	76,168
	Panel B: IV Estimates Per 10k Capita						
		Arrests	Force	Crime	Index Crime	Violent	Property
Actual Tactical Officers							
(10k capita)		.134***	.00343**	.0262	-.00984	-.00879**	-.00105
		(.0309)	(.0015)	(.0154)	(.00681)	(.0037)	(.00622)
Y mean	.	1.24	.0358	3.47	1.42	.331	1.09
Observations	.	76,168	76,168	76,168	76,168	76,168	76,168

Notes: This table presents reduced form and instrumental variable estimates of tactical officers on daily district-level outcomes. Panel A reports reduced form estimates of the effect of predicted tactical officers on actual tactical officers and district-day-level per 10,000 capita outcomes. Panel B reports instrumental variable estimates where actual tactical officers are instrumented by predicted tactical officers. Predictions are based on assigned teams from the previous month and current day-off groups. All specifications control for officer composition (share Black, Hispanic, male, and average tenure), and include district-by-year-month, day-of-week, and holiday (14 categories) fixed effects. First-stage F-statistics are reported in Panel A to assess instrument strength. Standard errors, clustered at the district level, are in parentheses. *** p-value < 0.01, ** p-value < 0.05, * p-value < 0.1.

TABLE A5. Effects of Tactical Officers on Crime, Arrests, and Use of Force: No Team Composition Controls

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
			PANEL A: REDUCED FORM				
	Actual Tactical Officers	Arrests	Force	Crime	Index Crime	Violent	Property
Pred. Tactical Officers	.691***	.0702***	.00218***	.01	-.0112**	-.00499*	-.00625
	(.0154)	(.0131)	(.000734)	(.00862)	(.00518)	(.00251)	(.0047)
First-stage F-stat	2,016	.	.	.	.	.	.
Y mean	21.35	11.79	0.34	35.28	14.78	3.21	11.57
Observations	76,218	76,218	76,218	76,218	76,218	76,218	76,218
			PANEL B: IV ESTIMATES				
		Arrests	Force	Crime	Index Crime	Violent	Property
Actual Tactical Officers		.102***	.00313***	.0149	-.016**	-.0071*	-.00895
		(.019)	(.00108)	(.0126)	(.0074)	(.00358)	(.00677)
Y mean	.	11.8	.34	35.3	14.8	3.21	11.6
Observations	.	76,168	76,168	76,168	76,168	76,168	76,168

Notes: This table presents reduced form and instrumental variable estimates of tactical officers on daily district-level outcomes. Panel A reports reduced form estimates of the effect of predicted tactical officers on actual tactical officers and district-day-level outcomes. Panel B reports instrumental variable estimates where actual tactical officers are instrumented by predicted tactical officers. Predictions are based on assigned teams from the previous month and current day-off groups. The specifications are the same as Table III, except this Table does not control for predicted beat officers and officer composition (share Black, Hispanic, male, and average tenure). First-stage F-statistics are reported in Panel A to assess instrument strength. Standard errors, clustered at the district level, are in parentheses. *** p-value $<$ 0.01, ** p-value $<$ 0.05, * p-value $<$ 0.1.

TABLE A6. Cross-Effects of Predicted Tactical and Beat Staffing

	(1)	(2)
	Actual Tact Officers	Actual Beat Officers
Pred. Tactical Officers	.692***	-.0181
	(.0155)	(.0319)
Pred. Beat Officers	.00605	.244***
	(.00775)	(.0224)
First-stage F-stat	2,007	119
Y mean	21.36	119.13
Observations	76,168	76,168

Notes: The purpose of this table is to assess whether predicted tactical staffing mechanically correlates with actual beat staffing (and vice versa). Each column reports a regression of actual staffing on predicted staffing of the other team type. Predictions are based on assigned teams from the previous month and current day-off groups. All specifications control for predicted officer composition (share Black, Hispanic, male, and average tenure of tactical and beat officers), and include district-by-year-month, day-of-week, and holiday (14 categories) fixed effects. First-stage F-statistics are reported to assess instrument strength. Standard errors, clustered at the district level, are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

TABLE A7. Effects of All Patrol and Beat Officer Staffing on Crime and Police Activity

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
			Panel A: Patrol Officers – Reduced Form				
	Actual Patrol Officers	Arrests	Force	Crime	Index Crime	Violent	Property
Pred. Police Officers	.4***	.0338***	.000632	.00679	-.00284	-.00102	-.00183
	(.0214)	(.00827)	(.000475)	(.00648)	(.00313)	(.00101)	(.0031)
First-stage F-stat	351	.	.	.	.	.	.
Y mean	140.49	11.79	0.34	35.29	14.78	3.21	11.57
Observations	76,168	76,168	76,168	76,168	76,168	76,168	76,168
			Panel B: Patrol Officers – IV Estimates				
		Arrests	Force	Crime	Index Crime	Violent	Property
Actual Patrol Officers		.0843***	.00158	.017	-.00711	-.00255	-.00456
		(.0202)	(.00117)	(.0164)	(.00774)	(.00255)	(.00769)
Y mean	.	11.8	.34	35.3	14.8	3.21	11.6
Observations	.	76,168	76,168	76,168	76,168	76,168	76,168
			Panel C: Beat Officers – Reduced Form				
	Actual Patrol Officers	Arrests	Force	Crime	Index Crime	Violent	Property
Pred. Patrol Officers	.244***	.00879	-.000198	.00525	.0034	.00184	.00157
	(.0224)	(.00728)	(.000529)	(.00784)	(.00401)	(.00168)	(.00389)
First-stage F-stat	119	.	.	.	.	.	.
Y mean	119.13	11.79	0.34	35.29	14.78	3.21	11.57
Observations	76,168	76,168	76,168	76,168	76,168	76,168	76,168
			Panel D: Beat Officers – IV Estimates				
		Arrests	Force	Crime	Index Crime	Violent	Property
Actual Beat Officers		.036	-.000812	.0215	.0139	.00752	.00642
		(.0283)	(.00219)	(.0311)	(.0157)	(.00678)	(.0157)
Y mean	.	11.8	.34	35.3	14.8	3.21	11.6
Observations	.	76,168	76,168	76,168	76,168	76,168	76,168

Notes: This table presents reduced form and instrumental variable estimates of the effect of different officer types on daily district-level outcomes. Panels A and B examine patrol officers (beat and tactical police officers combined): Panel A reports reduced form estimates of predicted patrol officers on actual patrol officers and district-day outcomes, while Panel B reports instrumental variable estimates where actual patrol officers are instrumented by predicted patrol officers. Panels C and D examine beat officers: Panel C reports reduced form estimates of predicted beat officers on actual beat officers and district-day outcomes, while Panel D reports instrumental variable estimates where actual beat officers are instrumented by predicted beat officers. Predictions are based on assigned teams from the previous month and current day-off groups. All specifications control for officer composition (share Black, Hispanic, male, and average tenure), and include district-by-year-month, day-of-week, and holiday (14 categories) fixed effects. First-stage F-statistics are reported in Panels A and C to assess instrument strength. Standard errors, clustered at the district level, are in parentheses. *** p-value < 0.01, ** p-value < 0.05, * p-value < 0.1.

B. Heterogeneity in Willingness to Pay

Table B1 shows heterogeneity of the WTP along a few important dimensions. The first set of dimensions is subject characteristics, which describes how different segments of the population view crime and police use-of-force. The second set of dimensions reveals the causal interaction effect between crime and other amenities to examine the complementarity of the amenities.

First, we estimate WTPs separately by race, comparing Black respondents to all others. Both groups have similar WTPs for square footage. However, Black respondents have a higher willingness to pay for all neighborhood amenities, including positive amenities like schooling and for negative amenities like violent crime and police use of force.

Black respondents exhibit a substantially greater marginal disutility from police use of force than non-Black respondents. The estimated marginal utility of force is -0.617 for Black respondents, roughly double the -0.310 observed for non-Black respondents ($p < 0.001$), indicating that each additional use-of-force incident imposes about twice as much disutility for Black individuals. The marginal utility of crime is also 60% larger in magnitude for Black respondents ($p < 0.001$), suggesting Black respondents also get more disutility from local crime than non-Black residents. Combining these parameters, Black respondents experience use-of-force incidents as approximately 4.6 times more costly than equivalent increases in crime, while for non-Black respondents the corresponding ratio is 3.8. This translates into a crime–force MRS of 0.215 for Black respondents and 0.265 for non-Black respondents ($p = 0.084$), implying that Black respondents are willing to tolerate roughly 4.6 additional crimes for one fewer use-of-force incident, compared to 3.8 among non-Black respondents—evidence that Black respondents place greater relative value on reducing police use of force.

High- and low-income respondents also exhibit different preferences. The WTP for apartment size is 1.325 among high-income respondents compared to 0.50 among low-income respondents ($p < 0.001$), reflecting the expected income gradient for housing quality. Despite this, low-income respondents experience 2.3 times stronger disutility from police use of force ($p < 0.001$) and slightly weaker disutility from crime 2.6 times stronger disutility from crime ($p < 0.001$). These coefficients imply that use-of-force incidents are roughly 4.3 times more costly than equivalent increases in crime for low-income respondents, compared to 3.8 times for high-income respondents. The corresponding crime–force MRS values are not statistically distinguishable across income groups ($p = 0.233$).

Finally, we divide the sample by the randomly considered crime rate presented as part of the conjoint study. To form the “high crime” group for this analysis, we restrict the sample to the one choice made by each respondent with the highest fixed crime rate. Likewise, the "low crime" group is composed of the choices made under the lowest fixed crime rate.

Because the crime level is randomly assigned in the conjoint, the coefficients in this panel capture causal changes in marginal utilities rather than compositional differences. When local crime is high, respondents place greater value on housing space ($p = 0.002$), implying that higher crime causally increases WTP for higher-quality housing. This result may be due to respondents anticipating spending less time outdoors when crime is higher and is consistent with Albouy et al. (2020) who find that the WTP for parks is higher when crime is lower.

When the local crime level is higher, the marginal disutility of police use of force becomes more negative, from -0.266 to -0.366 ($p = 0.016$), indicating that people dislike force more intensely when crime is high. Because both crime and police use of force generate disutility and the pain from force grows in high-crime contexts, these results imply that crime and police use of force are complements in disutility. That is, in high-crime contexts, people dislike force more, not less. In other words, exposure to crime makes respondents both more protective of physical and residential security and more averse to aggressive policing, suggesting that fear of crime and concern over police behavior reinforce rather than offset one another.

TABLE B1. Heterogeneity in WTP Estimates

	Black	Non Black	p-val	High Income	Low Income	p-val	High Crime	Low Crime	p-val
100 Sq Ft	1.058	1.007	.717	1.325	.5	<0.001	1.169	.737	.002
	(.189)	(.059)		(.086)	(.059)		(.176)	(.098)	
School	1.96	1.168	<0.001	1.629	.693	<0.001	1.416	1.174	.209
	(.265)	(.081)		(.121)	(.075)		(.21)	(.174)	
Crime	-.133	-.082	<0.001	-.117	-.045	<0.001			
	(.015)	(.004)		(.006)	(.003)				
Force	-.617	-.31	<0.001	-.44	-.193	<0.001	-.366	-.266	.016
	(.08)	(.02)		(.031)	(.019)		(.047)	(.035)	
Crime-Force	.215	.265	.084	.265	.234	.233			
MRS	(.035)	(.021)		(.023)	(.029)				

This table presents our results on willingness to pay from a mixed multinomial logit model. In the first panel, we produce WTPs separately by race, separating black respondents from all others. In the second panel, we separate the sample by comparing high income to low income respondents. The latter have yearly incomes less than 25,000 dollars, and the former are all others. In the third panel, we divide the sample by the crime rate presented as part of the hypothetical. Recall that in many of the choice scenarios shown to respondents, all options are given the same randomly-selected crime rate. To form the group for this analysis, we restrict the sample to the one choice made by each respondent with the highest fixed crime rate. Likewise, the group is composed of the choices made under the lowest fixed crime rate. See note in Table I for details for estimation of WTPs.

C. Trust in Police and Demand for Amenities

How do changes in trust in police affect individuals' demand for various amenities? To half of the respondents, we gave random assignment to the body-worn camera information that weakens their beliefs about effectiveness of police accountability.

After filtering out inattentive participants with the first dominating option, we direct participants to consider police use of force as another feature of their decision-making. We choose to introduce police use of force as a feature only to participants who pass all the attention checks. This allows us to implement the randomized information provision described below without concerns that treatment affected inclusion in the sample. All respondents receive text about the police use of force and the use of body cameras in the hypothetical neighborhoods they consider. The activation rate that we display to the treatment group comes from Katz et al. (2014), who study officers' compliance with body camera policies in Phoenix, Arizona. The text about body cameras reads as follows:

> *"Assume that police officers wear body cameras as part of their uniforms to document what they see as they perform their duties. Department regulations require that use of force incidents are recorded, and citizens can request access to body camera footage involving themselves."*

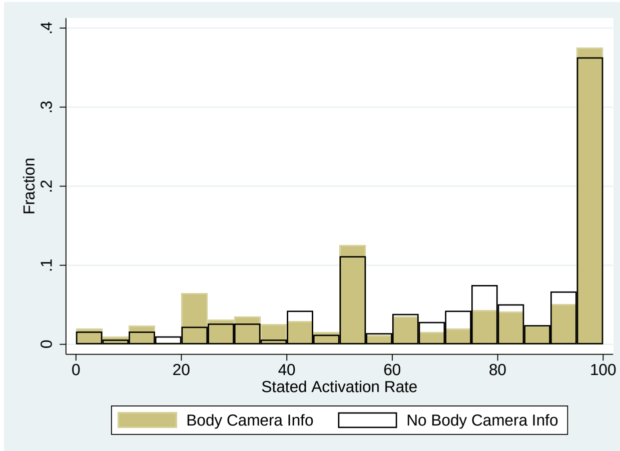
In addition, the treatment group receives text that weakens the participants' beliefs about the efficacy of body cameras. The treated group receives the following text:

> *"However, officers may fail to activate body cameras. A research study finds that police only activated these cameras around 24% of the time they were supposed to, and some officers disproportionally under-activated body cameras."*

Table C1 splits the sample of survey respondents between those randomized to the information treatment about body-worn camera activation rates and those in the control. Reassuringly, respondent characteristics are very similar across the treatment and control groups. The fourth column of Table C1 presents the difference between treatment and controls, and only one difference (income less than $25,000) is marginally significant.

We find that the treatment modestly decreases respondent perception of camera activation rates. This translates to small increases in willingness to pay for all amenities, including those unrelated to public safety. We do not measure a significant impact on the MRS between force and crime.

FIGURE C1. Respondent Estimates of BWC Activation Rates



The first question to ask about the information treatment is whether it actually changed the views of any survey respondents. At the end of the survey, we ask respondents how often they think police activate their body-worn cameras in settings that call for activation. Figure C1 compares the distribution of responses between treated and untreated respondents. Given that the treatment was randomly assigned, the control responses can be viewed as a prior distribution and the treatment responses as a posterior distribution that incorporates the information provided.

Recall that treated respondents were told that police activate their body-worn cameras in only 24% of the situations dictated by policy. Figure C1 reveals significant bunching at 50% and 100% for both treated and control groups. At both of these bunching points, there are slightly more treated respondents than untreated respondents. We think that at least some of the respondents who answer 100% are expressing an ideological belief and are unlikely to have been swayed by our information treatment. We therefore think that the over-representation of treated respondents in this mass point, which is not statistically significant, is merely noise.

Respondents who gave an answer other than 50% or 100% tended to shift their estimates from high values to low values. The exact value, 24%, given in the information treatment jumps from 0 estimates in the control to 17 in the treatment. Overall, the information treatment causes estimates of body-worn camera activation rates to fall by 3.8 percentage points (SE: 1.88) in the full sample and 7.2 percentage points (SE: 2) in the sample omitting responses of 100%. We therefore conclude that the information treatment generated a modest but real effect on respondent beliefs about body-worn camera activation rates.

Table C2 now asks whether and how this information affected willingness to pay for the studied amenities. We find that across the board that treated respondents are willing to pay more to increase positive amenities and decrease negative amenities, though the difference for use of force is only marginally statistically significant. Willingness to pay for 100 square feet is 1.21 among treated respondents and only 0.838 among untreated respondents. WTP for school quality rises from 1.16 to 1.38 among the treated. WTP for violent crime increases (in magnitude) from -0.84 to -0.96, and WTP for use of force increases from -0.33 to -0.38. The MRS between force and crime is unchanged.

We hypothesized that information about body-worn camera activation would cause respondents to be willing to pay more to avoid neighborhoods where violent crime and police use of force were high. Like high hypothetical crime rates, lower activation rates could signal that police are unprofessional or unaccountable, increasing the importance of living in a neighborhood where criminal and police violence are rare. This hypothesis is borne out by the data, though the difference in use of force is only significant at the 10% level.

We note that the same information also had a substantial effect on amenities unrelated to criminal justice. If anything, these effects were even stronger than those observed for criminal justice amenities. One hypothesis explaining this dynamic is that respondents understand that neighborhood quality may be heterogeneous within the boundaries used to calculate crime and force rates, and this quality is likely correlated with square footage and school quality. Thus, when information like the body-worn camera activation rate treatment causes them to doubt the accountability of the local police, they place extra emphasis on these amenities. Another hypothesis is that respondents anticipate spending less time outdoors in the neighborhood when that neighborhood is unsafe. They therefore place more direct value on the size of their apartment or the quality of the school their children attend because they will be spending more time in these places.

Overall, our findings underscore the significant role of trust in police accountability in shaping individuals' valuations of amenities, including those linked to police services.

TABLE C1. Summary Statistics and Balance

	(1) All	(2) Info Treat	(3) Control	(4) Difference
Male	0.34	0.35	0.34	0.00
	(0.48)	(0.48)	(0.48)	(0.03)
Black	0.29	0.33	0.24	0.09
	(0.99)	(1.10)	(0.86)	(0.06)
Hispanic	0.08	0.08	0.07	0.01
	(0.27)	(0.28)	(0.26)	(0.02)
Age: < 30	0.17	0.19	0.15	0.03
	(0.37)	(0.39)	(0.36)	(0.02)
Age: 30-45	0.35	0.33	0.37	-0.05
	(0.48)	(0.47)	(0.48)	(0.03)
Age: 45-60	0.24	0.25	0.23	0.03
	(0.43)	(0.44)	(0.42)	(0.03)
Age: > 60	0.24	0.23	0.25	-0.01
	(0.43)	(0.42)	(0.43)	(0.03)
Bachelor	0.25	0.27	0.23	0.04
	(0.43)	(0.44)	(0.42)	(0.03)
High school or less	0.33	0.32	0.34	-0.02
	(0.47)	(0.47)	(0.47)	(0.03)
No Child	0.70	0.72	0.68	0.03
	(0.46)	(0.45)	(0.47)	(0.03)
One Bed	0.28	0.28	0.27	0.02
	(0.45)	(0.45)	(0.44)	(0.03)
Income: <25,000	0.35	0.38	0.32	0.06*
	(0.48)	(0.49)	(0.47)	(0.03)
Income: 25-5,000K	0.37	0.34	0.39	-0.05
	(0.48)	(0.47)	(0.49)	(0.03)
Income: >50,000	0.29	0.28	0.29	-0.01
	(0.45)	(0.45)	(0.46)	(0.03)
High rent bucket	0.03	0.03	0.02	0.01
	(0.16)	(0.18)	(0.14)	(0.01)
Mid rent bucket	0.13	0.13	0.13	-0.00
	(0.34)	(0.33)	(0.34)	(0.02)
Low rent bucket	0.84	0.84	0.85	-0.01
	(0.36)	(0.37)	(0.36)	(0.02)
N. of participants	1,035	506	529	1,035
N. of obs.	72,450	37,030	35,420	

Note: The first column summarizes the characteristics of the 1,035 survey respondents in our data for the hypothetical survey. We summarize demographics, education attainment, whether the respondent seeks a one-bedroom or studio apartment, and desired rental price range. For each bedroom choice, we set two cutoffs - one that divides the price ranges between low-income and middle-income neighborhoods and the other between middle-income and high-income neighborhoods. These cutoffs are: for studios (900, 1400), for 1-bedrooms (1300, 2100), for 2-bedrooms (1600, 2500), and for 3-bedrooms (1700, 2600). The next two columns separately summarizes data for those in the information treatment and control group. The final column tests the difference.

TABLE C2. WTPs by Bodycam Treatment

	Treated	Untreated	P-Value of Difference
WTP for 100 Sq Ft	1.214	.839	<0.001
	(.089)	(.078)	
WTP for School Quality Quartile	1.374	1.157	.049
	(.109)	(.111)	
WTP for Violent Crime (per 10,000 residents)	-.096	-.084	.034
	(.006)	(.005)	
WTP for Use of Force (per 10,000 residents)	-.382	-.33	.07
	(.03)	(.027)	
MRS of Crime and Force (per 10,000 residents)	.252	.255	.898
	(.025)	(.026)	

We present WTP estimates separately for the treatment group that received additional information challenging body camera efficacy and the control group. All respondents were informed about police use of force and body cameras in the hypothetical neighborhoods they evaluated, with the treatment group receiving supplementary text to weaken their beliefs about body camera efficacy. See the note in Table I for details on WTP estimation.

D. Main Survey

Online Consent Form for Research Participation
Study Number: IRB22-0112
Researcher: Justin Holz

Description: We are researchers at the University of Chicago, Washington University in St. Louis, and the University of Pennsylvania doing an academic study about prices and preferences. Participation is voluntary and should take about 15 minutes. In this study, we will present you with a series of apartments with different attributes. You are offered an incentive payment for completing the survey. You may only fill out this survey once. You are responsible for all taxes that may correspond to payments from this study. PLEASE NOTE: This study contains attention checks to make sure that participants are finishing the survey honestly and completely. These attention checks are easy to spot if you read the questions. If you fail these checks, the response might be reversed.

Risks and Benefits: Your participation in this study does not involve any risk to you beyond that of everyday life.

Confidentiality: We will not collect any personally identifying information about you. If you decide to withdraw, data collected up until the point of withdrawal may still be included in analysis. De-identified information from this study may be used or shared with other researchers for future research without your additional informed consent.

Contacts & Questions: If you have questions or concerns about the study, you can contact the researchers at: justinholz@uchicago.edu. If you have any questions about your rights as a participant in this research, feel you have been harmed, or wish to discuss other study-related concerns with someone who is not part of the research team, you can contact the University of Chicago Social & Behavioral Sciences Institutional Review Board (IRB) Office by phone at (773) 702-2915, or by email at sbs-irb@uchicago.edu.

Consent: Participation is voluntary. Refusal to participate or withdrawing from the research will involve no penalty or loss of benefits to which you might otherwise be entitled. By clicking "Agree" below, you confirm that you have read the consent form, are at least 18 years old, live in the U.S., and agree to participate in the research. Please print or save a copy of this page for your records.

○ I consent to the above form and wish to proceed

○ I do not consent and wish to exit

→

Penn Carey Law
University of Pennsylvania

Please provide your demographic information: what is your gender?

- ◯ Male
- ◯ Female
- ◯ Non-binary / third gender
- ◯ Prefer not to say

What is your age (in years)?

→

Penn Carey Law
UNIVERSITY *of* PENNSYLVANIA

What is the highest degree or level of school you have completed?

- ◯ Less than a high school diploma
- ◯ High school degree or equivalent
- ◯ Some college, no degree
- ◯ Associate degree
- ◯ Bachelor's degree or higher

Choose one or more races that you consider yourself to be:

☐ White	☐ Asian
☐ Black or African American	☐ Native Hawaiian or Pacific Islander
☐ American Indian or Alaska Native	☐ Other

Penn Carey Law
UNIVERSITY of PENNSYLVANIA

Are you Spanish, Hispanic, or Latino, or none of these?

- ◯ Yes
- ◯ None of these

What is the level of your annual household income?

- ◯ Less than $25,000
- ◯ $25,000 - $50,000
- ◯ $50,000 - $100,000
- ◯ $100,000 - $200,000
- ◯ More than $200,000

→

How many school-aged children do you have?

- None
- 1-2
- More than 2
- Prefer not to say

In the past week, did you read any of the following newspapers, either online or in print? Please select all that apply. We also want to see whether people are reading the questions carefully. This is a data quality check. Regardless of your true answer, please select "Daily News" and "None of the above."

- USA Today
- The Wall Street Journal
- The New York Times
- The Washington Post
- Los Angeles Times
- Daily News
- Chicago Tribune
- None of the above.

For the rest of the questions in this survey, please imagine a scenario where you must relocate to a new apartment.

Select the number of bedrooms you would interested in:

- ◯ Studio
- ◯ 1 bedroom
- ◯ 2 bedroom
- ◯ 3 bedroom or more

Choose your preferred monthly rent:

400	1300	2200	3100	4000

Target Monthly Rent

→

PennCarey Law
UNIVERSITY of PENNSYLVANIA

In this survey, we will ask you 14 questions to understand what you are looking for in an apartment.

In each question, you will choose between 5 hypothetical apartment options. Just pick the option you like best.

Before we begin, here is some information about choice features that we want you to keep in mind when making decisions:

Violent Crime per 10,000 Residents

The violent crime rate describes the number of violent crimes recorded in a neighborhood in 2019 per 10,000 residents. For example, a neighborhood with 100 violent crimes and 50,000 residents has 20 violent crimes per 10,000 residents. Violent crimes include murder (including negligent homicide), sexual assault, assault & battery, kidnapping, and robbery.

Suppose 1/4 of neighborhoods have less than **53** violent crimes per 10,000 residents. About 1/4 of neighborhoods have more than **157** violent crimes per 10,000 residents.

School Quality

School quality is a summary measure of the quality of public schools in the area, using information provided by the local school district. Quality ranges from 1 to 4. 1 is the lowest score and 4 is the highest. The schools are created such that about 1/4 of schools get a score of 1, 1/4 get a score of 2, 1/4 get a score of 3, and 1/4 get a score of 4.

→

From the following list of housing choices, pick your most preferred option. Assume everything else is the same about the apartment. For each choice, only one home feature will vary across options. If you are using a mobile device, scroll right to see the entire set of options. Please make your choice below.

After making this choice, you have **13** more remaining.

	Option 1	Option 2	Option 3	Option 4	Option 5
Monthly Rent	$2,827	$2,610	$2,908	$2,852	$2,504
Violent Crime (per 10,000 people)	47	47	61	47	25
Square footage	633	633	633	633	633
School Ratings (Higher is better)	1.9	1.9	1.9	1.9	1.9

	Option 1	Option 2	Option 3	Option 4	Option 5
Your Choice	○	○	○	○	○

From the following list of housing choices, pick your most preferred option. Assume everything else is the same about the apartment. For each choice, only one home feature will vary across options. If you are using a mobile device, scroll right to see the entire set of options. Please make your choice below.

After making this choice, you have **12** more remaining.

	Option 1	Option 2	Option 3	Option 4	Option 5
Monthly Rent	$2,434	$2,570	$2,552	$2,832	$2,528
Violent Crime (per 10,000 people)	25	58	36	71	39
Square footage	931	931	931	931	931
School Ratings (Higher is better)	1.1	1.1	1.1	1.1	1.1

	Option 1	Option 2	Option 3	Option 4	Option 5
Your Choice	○	○	○	○	○

For the remaining set of questions, we want you to consider one additional neighborhood feature in making decisions:

Police Use of Force

Police use of force per 10,000 residents describes the number of times police used force in a neighborhood in 2019 per 10,000 residents of that area. For example, a neighborhood with 48 uses of force and 40,000 residents has 12 uses of force per 10,000 residents.

Police force refers to physical contact by a police officer, either directly or through the use of equipment, to compel a subject's compliance. These incidents include usages of firearms, other weapons, and unarmed force.

Suppose 1/4 of neighborhoods have less than **4** uses of force per 10,000 residents. About 1/4 of police neighborhood have more than **12** uses of force per 10,000 residents.

→

PennCarey Law
UNIVERSITY of PENNSYLVANIA

TREATMENT

Assume that police officers wear body cameras as part of their uniforms to document what they see as they perform their duties. Department regulations require that use of force incidents are recorded, and citizens can request access to body camera footage involving themselves.

However, officers may fail to activate body cameras. A research study finds that police only activated these cameras around 24% of the time they were supposed to, and some officers disproportionally under-activated body cameras.

→

CONTROL

Assume that police officers wear body cameras as part of their uniforms to document what they see as they perform their duties. Department regulations require that use of force incidents are recorded, and citizens can request access to body camera footage involving themselves.

→

From the following list of housing choices, pick your most preferred option. Assume everything else is the same about the apartment. Please make your choice below.

After making this choice, you have **11** more remaining.

	Option 1	Option 2	Option 3	Option 4	Option 5
Monthly Rent	$2,227	$2,495	$2,284	$2,779	$2,192
Violent Crime (per 10,000 people)	75	75	75	75	75
Police Use of Force (per 10,000 people)	3	2	2	8	2
Square footage	988	988	988	988	988
School Ratings (Higher is better)	1.5	1.5	1.5	1.5	1.5

	Option 1	Option 2	Option 3	Option 4	Option 5
Your Choice	○	○	○	○	○

From the following list of housing choices, pick your most preferred option. Assume everything else is the same about the apartment. Please make your choice below.

After making this choice, you have **10** more remaining.

	Option 1	Option 2	Option 3	Option 4	Option 5
Monthly Rent	$2,697	$2,917	$2,245	$2,763	$2,529
Violent Crime (per 10,000 people)	72	72	72	72	72
Police Use of Force (per 10,000 people)	7	1	8	3	5
Square footage	892	892	892	892	892
School Ratings (Higher is better)	3.1	3.1	3.1	3.1	3.1

	Option 1	Option 2	Option 3	Option 4	Option 5
Your Choice	◯	◯	◯	◯	◯

From the following list of housing choices, pick your most preferred option. Assume everything else is the same about the apartment. Please make your choice below.

After making this choice, you have **9** more remaining.

	Option 1	Option 2	Option 3	Option 4	Option 5
Monthly Rent	$2,152	$2,765	$2,749	$2,800	$2,741
Violent Crime (per 10,000 people)	47	47	47	47	47
Police Use of Force (per 10,000 people)	4	4	2	8	9
Square footage	771	771	771	771	771
School Ratings (Higher is better)	2.8	2.8	2.8	2.8	2.8

	Option 1	Option 2	Option 3	Option 4	Option 5
Your Choice	○	○	○	○	○

From the following list of housing choices, pick your most preferred option. Assume everything else is the same about the apartment. Please make your choice below.

After making this choice, you have **8** more remaining.

	Option 1	Option 2	Option 3	Option 4	Option 5
Monthly Rent	$2,046	$2,624	$2,359	$2,906	$2,332
Violent Crime (per 10,000 people)	53	53	53	53	53
Police Use of Force (per 10,000 people)	5	5	2	4	7
Square footage	899	899	899	899	899
School Ratings (Higher is better)	3.9	3.9	3.9	3.9	3.9

	Option 1	Option 2	Option 3	Option 4	Option 5
Your Choice	○	○	○	○	○

From the following list of housing choices, pick your most preferred option. Assume everything else is the same about the apartment. Please make your choice below.

After making this choice, you have **7** more remaining.

	Option 1	Option 2	Option 3	Option 4	Option 5
Monthly Rent	$2,953	$2,666	$2,494	$2,858	$2,129
Violent Crime (per 10,000 people)	43	43	43	43	43
Police Use of Force (per 10,000 people)	2	6	6	6	9
Square footage	632	632	632	632	632
School Ratings (Higher is better)	3.7	3.7	3.7	3.7	3.7

	Option 1	Option 2	Option 3	Option 4	Option 5
Your Choice	○	○	○	○	○

From the following list of housing choices, pick your most preferred option. Assume everything else is the same about the apartment. Please make your choice below.

After making this choice, you have **6** more remaining.

	Option 1	Option 2	Option 3	Option 4	Option 5
Monthly Rent	$2,597	$2,833	$2,912	$2,724	$2,387
Violent Crime (per 10,000 people)	54	39	48	60	25
Police Use of Force (per 10,000 people)	4	4	4	4	4
Square footage	775	775	775	775	775
School Ratings (Higher is better)	4	4	4	4	4

	Option 1	Option 2	Option 3	Option 4	Option 5
Your Choice	○	○	○	○	○

From the following list of housing choices, pick your most preferred option. Assume everything else is the same about the apartment. Please make your choice below.

After making this choice, you have **5** more remaining.

	Option 1	Option 2	Option 3	Option 4	Option 5
Monthly Rent	$2,675	$2,925	$2,924	$2,076	$2,718
Violent Crime (per 10,000 people)	69	69	69	69	69
Police Use of Force (per 10,000 people)	9	2	5	5	5
Square footage	754	754	754	754	754
School Ratings (Higher is better)	2	2	2	2	2

	Option 1	Option 2	Option 3	Option 4	Option 5
Your Choice	○	○	○	○	○

From the following list of housing choices, pick your most preferred option. Assume everything else is the same about the apartment. Please make your choice below.

After making this choice, you have **4** more remaining.

	Option 1	Option 2	Option 3	Option 4	Option 5
Monthly Rent	$2,591	$2,117	$2,671	$2,911	$2,673
Violent Crime (per 10,000 people)	73	65	34	64	64
Police Use of Force (per 10,000 people)	3	3	3	3	3
Square footage	757	757	757	757	757
School Ratings (Higher is better)	3.5	3.5	3.5	3.5	3.5

	Option 1	Option 2	Option 3	Option 4	Option 5
Your Choice	○	○	◉	○	○

From the following list of housing choices, pick your most preferred option. Assume everything else is the same about the apartment. Please make your choice below.

After making this choice, you have **3** more remaining.

	Option 1	Option 2	Option 3	Option 4	Option 5
Monthly Rent	$2,795	$2,417	$2,708	$2,901	$2,432
Violent Crime (per 10,000 people)	44	61	38	42	37
Police Use of Force (per 10,000 people)	9	9	9	9	9
Square footage	766	766	766	766	766
School Ratings (Higher is better)	3.3	3.3	3.3	3.3	3.3

	Option 1	Option 2	Option 3	Option 4	Option 5
Your Choice	○	○	○	○	○

From the following list of housing choices, pick your most preferred option. Assume everything else is the same about the apartment. Please make your choice below.

After making this choice, you have **2** more remaining.

	Option 1	Option 2	Option 3	Option 4	Option 5
Monthly Rent	$2,644	$2,556	$2,100	$2,664	$2,616
Violent Crime (per 10,000 people)	43	43	43	43	43
Police Use of Force (per 10,000 people)	1	1	1	1	1
Square footage	791	862	854	761	648
School Ratings (Higher is better)	1	1	1	1	1

	Option 1	Option 2	Option 3	Option 4	Option 5
Your Choice	○	○	○	○	○

From the following list of housing choices, pick your most preferred option. Assume everything else is the same about the apartment. Please make your choice below.

After making this choice, you have **1** more remaining.

	Option 1	Option 2	Option 3	Option 4	Option 5
Monthly Rent	$2,035	$2,221	$2,519	$2,346	$2,188
Violent Crime (per 10,000 people)	50	50	50	50	50
Police Use of Force (per 10,000 people)	7	6	2	5	2
Square footage	840	840	840	840	840
School Ratings (Higher is better)	1.4	1.4	1.4	1.4	1.4

	Option 1	Option 2	Option 3	Option 4	Option 5
Your Choice	○	○	○	○	○

From the following list of housing choices, pick your most preferred option. Assume everything else is the same about the apartment. Please make your choice below.

After making this choice, you have **0** more remaining.

	Option 1	Option 2	Option 3	Option 4	Option 5
Monthly Rent	$2,465	$2,107	$2,656	$2,539	$2,650
Violent Crime (per 10,000 people)	26	48	59	53	52
Police Use of Force (per 10,000 people)	5	5	5	5	5
Square footage	922	922	922	922	922
School Ratings (Higher is better)	2	2	2	2	2

	Option 1	Option 2	Option 3	Option 4	Option 5
Your Choice	○	○	○	○	○

Could you please describe your approach to selecting apartments from the options above?

→

Suppose a police department adopts body cameras. The department regulations require that officers activate their body cameras during all citizen encounters such as stops, searches, arrests, and use of force incidents.

In what percentage of such citizen encounters do you expect officers to *actually* activate their body cameras? Please note that the number should be between 0% and 100%.

→