

Supplementary Information Appendix: Militarization Fails to Enhance Police Safety or Reduce Crime, But May Harm Police Reputation

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All replication files posted at <https://dataverse.harvard.edu/>.

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1. Materials and Methods

A. Censoring in Data on Disbursement of Military Equipment. Several recent studies have measured the military capacity of local law enforcement agencies using the Department of Defense “1033” database, which contains details on shipments of excess military weapons and equipment from the federal government to localities (1–3). The reason these data are inappropriate for over-time analyses is that equipment from earlier years is often censored, making it appear that agencies are receiving more gear as time goes on.

The degree of censoring appears severe. As of 2014, more than \$4.3 billion in equipment had reportedly been distributed to law enforcement agencies through this program (4), but program data from 2014 show less than \$2 billion. There are several causes. For most of the program’s history, the federal government has only tracked what it deems “controlled” equipment (typically weapons and armored vehicles) but has allowed many other items to drop out of the database one year after receipt by the agency. Many of these items are mundane, such as office supplies, but others include weapon accessories. In addition, a 2015 article in *Mother Jones* stated, “Law enforcement agencies across the country have quietly returned more than 6,000 unwanted or unusable items to the Pentagon in the last 10 years, according to Defense Department data provided to *Mother Jones* by a spokeswoman for Sen. Claire McCaskill (D-Mo.). . . Thousands more unwanted items have been transferred to other police departments,” (5). There have also been recorded cases of departments acquiring equipment with the apparent aim of selling it for profit, another source of error in the data (6). These returns, sales and inter-agency transfers are not noted in the public “1033” data, and result in an inaccurate portrait of what gear agencies have possessed and when. Over time, items simply drop out of the database, or appear as if they always belonged to an agency that receives an inter-agency transfer (see Figure S1 for one such example of M16 assault rifles switching agencies in California). As a result of these data curation policies, the “1033” data represent, at best, a snapshot of what agencies *currently* possess, not a record of everything they have received from the federal government over time. Treating this dataset as a panel is therefore a strategy prone to measurement error, censoring and bias.

The use of agency fixed effects (i.e., within-agency, over-time comparisons) in analyses of the effect of receipt of gear on various outcomes is unlikely to solve this issue. Both crime and officer deaths have been trending down in recent decades (7, 8), even within agencies. The data censoring in the “1033” data will likely make it appear as if the acquisition of more equipment over time has led to declines in crime and officer deaths, when in fact the apparent increase in equipment over time is simply exaggerated by the censoring in early years.

Further, 1033 data represent an incomplete picture of the acquisition of militarized equipment, as departments have been acquiring such equipment through other means. For example, *The New York Times* reports that the, “Department of Homeland Security provides funding for arms. It has distributed more than \$34 billion through ‘terrorism grants,’ enabling local police departments to acquire such absurd items as a surveillance drone and an Army tank,” (9). For this reason, even if censoring were not an issue within the 1033 data set, it would still fail to accurately measure the extent to which local agencies have been equipped with militarized gear over time, unless the “1033” data represent an unbiased proxy for all military-style equipment an agency has received (a weighty assumption).

I therefore rely on alternative administrative data on militarized police units over time, outlined below.

B. CSLLEA. The federal Census of State and Local Law Enforcement Agencies (CSLLEA) surveys from 2000, 2004 and 2008 contain records of whether more than 15,000 state and local law enforcement agencies throughout the U.S. provide SWAT services in their jurisdictions (10–12). The SWAT variable is not available in other years. Indicators for SWAT status are derived from the following survey items:

2000: Variable name in CSLLEA: “V60”. “Which of the following functions did your agency perform on a routine basis during the 12-month period ending June 30, 2000?” Options: “TACTICAL OPERATIONS (SWAT)” [Yes/No]

2004: Variable name in CSLLEA: “SWAT”. “During 2004, which of the following functions has your agency performed on a regular basis or had primary responsibility for performing when needed?” Options: “TACTICAL OPERATIONS” [Yes/No]

2008: Variable name in CSLLEA: “Q1H3”. “During 2008, which of the following functions did your agency perform on a regular basis or have primary responsibility for performing when needed?” Options: “Tactical operations (SWAT)”. [Yes/No]

Note that the question wording in the CSLLEA measuring SWAT team status listed above differs slightly across waves, and suggests different periods of temporal coverage. I confirmed with the Bureau of Justice Statistics that the intent of the SWAT questions was to measure SWAT status during July 1, 1999 - June 30, 2000 in the 2000 survey, and during the calendar year in 2004 and 2008. Accordingly, I aggregate violent crimes and officer deaths/injuries during these respective 12-month periods. Across the three waves in the CSLLEA, roughly 29% of agencies vary on the indicator for having a SWAT team during this period (see Table S2). I also conduct a robustness check (see Table S18) which drops data from the 2000 survey and recomputes all core models, since the question wording in 2000 differed from the other two years. The same pattern of results holds: we recover no evidence of a statistically significant drop in violent crimes or officer injuries/deaths.

C. Merging the CSLLEA Waves. Because the CSLLEA does not contain consistent agency identifiers across these three waves, merging these datasets into a three-wave panel required fuzzy string and probabilistic matching techniques. To find matches, I first cleaned agency names by making all names lowercase and by standardizing common words (e.g. both “Department” and “Dept.” became “dept”). I then paired agencies across the three waves which had the exact same name, state, agency type (e.g., sheriff or police dept.) and zip code. For remaining mismatched agencies, I used `amatch`, an approximate string distance matching function in R, to locate additional matches based on agency names within states. I used the `fastLink` function, a probabilistic matching algorithm (13), on the remaining set to locate additional matches using agency names and zip codes in the pooled (multi-state) data set. Agencies which were merged across waves based on non-exact matches (usually due to a discrepancy in zip codes, which may have stemmed from typos in the CSLLEA survey responses) were manually verified for accuracy by myself and research assistants.

Only agencies which appeared in all three waves were retained. Tribal police departments and agencies with “special” jurisdictions (such as fish and wildlife authorities) were excluded, as were agencies with zero full-time sworn officers and agencies which could not be paired with the FBI’s Uniform Crime Reporting Data. I also dropped agencies which indicated that they pool their data with other agencies when filing UCR reports, since it is impossible to attribute changes in outcomes to any particular agency in these cases. The bulk of observations come from local police and sheriff’s departments. The final panel consists of 8,961 agencies. After constructing viable agency identifiers, I was then able to use a crosswalk file (14) that contains standardized agency IDs present in the 2008 wave of the CSLLEA to merge the entire panel with the data on violent crimes and officer deaths/injuries maintained by the FBI.

As Table S1 shows, the agencies used in the final sample are broadly representative of local police, sheriff and state police agencies at large. Compared with all agencies in the 2008 CSLLEA data which have at least one full-time-sworn officer, the agencies in my data are slightly smaller (fewer officers) and have lower budgets, but roughly the same share have SWAT teams and roughly the same are located in Southern states. The composition of agency types (police, sheriff and state agency) is also highly comparable.

The number of agencies remaining after each step in the merge/cleaning process is detailed below:

Original CSLLEA 2000: 16,376

Subset to exact matches CSLLEA 2000/2004: 11,895

Conduct Fuzzy String Matching between 2000/2004 using remaining mismatches; add likely matches: 12,669

Conduct Probabilistic String Matching (13) between 2000/2004 using remaining mismatches, add likely matches: 12,910

Remove Agencies that do not Exactly Match 2008 Data: 10,820

Conduct Fuzzy String Matching between 2000/2008 using remaining mismatches; add likely matches: 11,649

Conduct Probabilistic String Matching (13) between 2000/2008 using remaining mismatches, add likely matches: 12,133

Remove Agencies Without Valid ORI Number: 12,046

Remove Tribal and Special Jurisdictions, and Agencies With Missing Outcome Data/Agencies Which Report Outcome Data as Group: 8,961

D. Maryland SWAT Data. I obtained data on every SWAT deployment in the state of Maryland between FY2010-FY2014 via a public records request to Maryland’s Governor’s Office of Crime Control & Prevention, which published annual reports of descriptive statistics using the data during the years the law requiring the data’s collection was active (15). The raw data I received contained the date, postal zip-code and agency of each SWAT deployment in the state, as well as the reasons for and outcomes of each deployment.

I transformed this data into several different data sets to serve different analyses. These are:

1. **Deployment-Level Data.** This takes the deployment as the unit of analysis allowing for a descriptive inquiry into why and how SWAT teams are deployed (Table 1 in the main text).
2. **ZCTA-Level Data, All Md.** This takes the zip code tabulation area (ZCTA) as the unit of analysis, and allows for an inquiry into the geographic correlates of SWAT deployments. The postal code SWAT data was merged with ZCTA-level U.S. Census demographic data from the 2013 American Community Survey (16–19) using a ZCTA/zipcode crosswalk file published online by the UDS Mapper project (info here: <https://www.udsmapper.org/about.cfm>; $n \approx 470$ zip-codes). (see Figure 1 in the main text and Figure S2 below).
3. **ZCTA-Level Data, Large Md. Jurisdictions.** Generates the same data structure as 2. but for Baltimore City, Prince George’s County and Montgomery County Police Depts. These agencies post incident-level, geocoded crime data (longitude and latitude) online (see here: <https://data.baltimorecity.gov/Public-Safety/BPD-Part-1-Victim-Based-Crime-Data/wsfq-mvij/data>; <https://data.princegeorgescountymd.gov/login>; <https://data.montgomerycountymd.gov/Public-Safety/Crime/icn6-v9z3/data>). I used census shape files to map crimes to ZCTAs. The crime data obtained from Baltimore, Prince George’s County and Montgomery County Maryland were not uniformly coded, so it was necessary to classify violent crimes in the data. The FBI classifies murder, manslaughter, rape, robbery and assault as violent crimes. In the Baltimore data, crimes took on 15 distinct categorical values. I coded any crime event containing the FBI terms, as well as the word “shooting,” as a violent crime. In the Prince George’s County data, where crimes were coded into 21 categories, I used the same technique. The crime categories in the Montgomery County data took over 300 distinct values. I hand coded these as violent or not based on the FBI definitions, and also counted events with terms like “simple assault,” “bomb threat” and “explosive device” as violent crimes. In addition, the Montgomery County web site noted that single rows in the data could represent multiple offenses, but did not specify which rows, so they are assumed to be single incidents by necessity. The data from all three sources are periodically refreshed and do not extend back in time indefinitely, but I downloaded versions of these incident-level data sets in late 2015 and early 2016 that contained data from Baltimore ranging from 2011-2016; data from Prince George’s County from Jan. 1, 2011-December 24, 2015; and data from Montgomery County that ranged from July 1, 2013-December 31, 2015. Data from all crime data sets were trimmed to overlap with the Maryland SWAT data in time, which covers FY2010-2014, though the Montgomery crime data could not be paired in the earliest years. These three agencies represent three of the top five largest agencies in the state in terms of total full-time sworn officers according to the 2008 CSLLEA.
5. **County-Level Data.** Allows for a replication of the ZCTA-level analysis in Figure 1 using (20–23) and the Maryland SWAT data merged with U.S. Census data on counties (24–27).
6. **Agency-Month Data.** Allows for a panel analysis of the effects of deployments on crime and officer safety (Figure 3); merges in the FBI crime data and LEOKA data on officers killed and assaulted (see Figure 3 in main text). As with the nationwide panel, agencies which report crime and officer safety data to the FBI as a group were excluded. The Maryland State Police were also excluded since their various substations’ crime and officer safety data could not be paired with the SWAT data. Finally, agency-months in which an agency did not report data on officers assaulted in the LEOKA data are excluded from estimation of models of assaults on officers. This means some of the agency-month analyses use unbalanced panels (not all agencies are observed in the same number of time periods). It is possible agencies did not report assaults on officers in these months because none occurred. See Table S34 for a robustness check treating unreported agency-month assault data as zero assaults, which leads to similar results.

E. Crime Data. For the core analysis in Figure 2, I sum violent crimes over the 12-month periods specified above corresponding to the 2000, 2004 and 2008 CSLLEA surveys using the FBI’s agency-month level crime data (28–42). I merged these data with the CSLLEA using a common agency identifier (“Originating Agency Identification” (ORI7) numbers). The 2008 CSLLEA included an ORI number which I then appended to the the 2004 and 2000 waves after matching agencies across waves using the procedure described above. The same crime data were merged with the Maryland SWAT data by agency and month to generate Figure 3.

F. LEOKA Data. The FBI Law Enforcement Officers Killed and Assaulted (LEOKA) database contains incident-level records on police officers killed or injured in the line of duty. After summing incidents of felonious killings, accidental killings, and assaults over the appropriate 12-month periods, the LEOKA data were merged with the 2000, 2004 and 2008 CSLLEA by a standardized agency identifier (ORI number), and with the Maryland SWAT data by agency and month (43–57).

G. SWAT Analysis: Model Specifications. The results of models explaining violent crime and officer safety using the national and Maryland SWAT panels in the main text are estimated via the following ordinary least squares model specifications:

$$\log(\text{Outcome}_{i,t} + 1) = \tau \text{SWAT}_{i,t} + \theta_i + \gamma_t + \epsilon_{i,t} \quad [1]$$

$$\log(\text{Outcome}_{i,t} + 1) = \tau \text{SWAT}_{i,t} + \theta_i + \gamma_t + \beta_i \text{Agency}_i \cdot \text{time} + \epsilon_{i,t} \quad [2]$$

where the $Outcome_{i,t}$ is a count of the number of violent crimes or officer killings/assaults that occurred in a unit (agency) i in a time period (year for national panel; year-month for Maryland panel) t , $SWAT_{i,t}$ is an indicator for having a SWAT team for a given agency and year in the national panel, and the logged number of SWAT deployments in a month-year in the Maryland panel (i.e. $\log(\#Deployments + 1)$), θ_i and γ_t are agency and period-specific fixed effects, respectively, and $\epsilon_{i,t}$ is an error term. All outcomes for results in the main text are specified as $\log(Outcome_{i,t} + 1)$ to reduce the influence of extreme values, but results using alternative specifications of the outcome appear below.

Equation (1) is the generalized difference-in-differences (DID) model and Equation (2) adds agency-specific linear time trends, where $time$ is a continuous variable ranging from the minimum to the maximum number of the periods, and $Agency_i$ is an indicator for an observation belonging to one of J agencies. The second model imposes a different safeguard against endogeneity, since agency-specific linear time trends help correct for the possibility that agencies were not trending in parallel over time. However, this safeguard comes at a cost—precision—since Model 2 trades away degrees of freedom due to the additional parameters requiring estimation.

The quantity of interest in each case is τ , which represents the average change in the outcome for a one-unit increase in the treatment, (i.e., obtaining or deploying a SWAT team), within agencies over time, net of common time shocks/trends. Both models provide causal leverage by making comparisons within agencies over time rather than attempting to control for the many unobservable differences between agencies (58).

In addition to these core specifications, Tables S12-S34 display the results of tests for lagged effects and for specification errors using various alternative codings of the outcomes and weighting schemes. The pattern of results across these alternate specifications remains highly similar to those reported in the core results.

Given the thousands of parameters being estimated in these models, standard regression packages in R and STATA proved computationally inadequate to compute the correct standard errors. As an alternative, coefficients and standard errors for the national SWAT panel analysis were estimated using an agency-blocked bootstrap (59) with 1,500 iterations per model. The procedure is as follows. Consider a data set with k agencies. To start, k agencies (all of their respective observations) are randomly resampled from the data with replacement. The treatment effect is estimated on this bootstrapped sample, the coefficient is stored, and the process is repeated 1,500 times. The mean of these bootstrapped estimates serves as the point estimate and the 2.5th and 97.5th quantiles of this distribution of estimates serve as the bounds of the 95% confidence interval. This procedure simulates the data generating process of error clustering by agency while relaxing parametric assumptions about uncertainty (such as Normal errors). Standard regression software packages were used to estimate coefficients and agency-clustered standard errors with the much smaller Maryland panel.

H. Survey Experimental Design. The M-Turk survey was conducted September-October 2015. The SSI survey was conducted in March 2016. The M-Turk survey consists of a convenience sample of adult volunteers. The SSI survey was quota-targeted to match the U.S. Census in terms of age race and gender, and also included an oversample of 1,849 African-American respondents who were targeted to match Census figures on age and gender. See Table S8 for demographics of samples.

I. Sources of Image Manipulations. The control image of Boston police officers in traditional blue uniforms appeared in (60). The original source was listed as Jessica Rinaldi/Reuters. The “riot gear” image portrays an image of police in Portland during a protest wearing heavy armor and equipped with batons and face shields, and appeared in (61). The same image was located on Shutterstock.com, a stock photo warehouse, where a high-resolution version was purchased. The photo is credited to JPL Designs. The “assault rifles” image accompanied (62) and portrays officers deployed in Ferguson, MO in body armor with rifles. The photo was credited to Jeff Roberson of the Associated Press. The “armored vehicle” image also portrays officers deployed in Ferguson, MO and, in addition to SWAT team members, includes a mine-resistant ambush-protected vehicle (MRAP). This image accompanied (63). The image was credited to Whitney Curtis/The New York Times and was purchased for reprinting from Redux Pictures.

J. Survey Experiments: Model Specifications. In the M-Turk survey, treatment assignment was globally randomized. In the SSI survey, treatment assignment was blocked by the race of the respondent, but treatment assignment probabilities were constant across blocks (i.e., treatment assignment is uncorrelated with block status). Indicators for blocks are included as covariates during estimation for the SSI survey in order to increase efficiency (64). The model estimating treatment effects is:

$$Outcome = \alpha + \sum_j \tau_j Image_j + \sum_k Race_k + \epsilon \quad [3]$$

where $Image_j$ are indicators for each of the militarized treatment images (with the control image serving as a reference category) and $Race_{i,k}$ are indicators for each racial category of respondents (non-Hispanic white, non-Hispanic Black and Hispanic, with “non-Hispanic white” serving as a reference category.) All models use robust (HC1) standard errors (65).

To estimate differences in treatment effects between Black and white respondents, Equation 4 was re-estimated with the addition of interaction between treatment indicators and indicators for respondent race (see Equation 5 below). The coefficient τ_j below represents the treatment effect of a given image j among white respondents (the omitted category). The quantity $\tau_j + \gamma_{j,k}$ represents the treatment effect of image j for racial group k . Finally, $\gamma_{j,k}$ represents the difference in treatment effects between racial group k and white respondents. These quantities are displayed in 6 in the main text for non-Hispanic white and African American respondents.

$$Outcome = \alpha + \sum_j \tau_j Image_j + \sum_k Race_k + \sum_{j,k} \gamma_{j,k} Image_j * Race_k + \epsilon \quad [4]$$

Figure S9 displays treatment effects after re-weighting the data to be nationally representative in terms of race, gender and party based on the proportions in population-weighted 2016 Cooperative Congressional Election Study (66). The results are highly similar.

K. Balance on Observables. OLS models in Table S11 predict treatment assignment in the SSI and M-Turk surveys as a function of respondent demographics. The F statistic and accompanying p -value at the bottom of each model correspond to the null hypothesis that all coefficients are jointly zero, which should be true if balance was achieved through random assignment. As the table shows, no p -value on any F -statistic allows us to reject this null (i.e., $p > .05$), indicating that the randomization was successful.

L. Text of Experimental News Article, M-turk Survey. The article provided to respondents was based on an online news article from *The Advertiser News*, a local newspaper in New Jersey (67).

City police ask for budget increase

Feb. 23, 2015

(Image Manipulation Here)

Above: Five city police officers stand guard during a local protest.

The chief of the city police department requested a 7 percent budget increase for his agency at Thursday's City Council meeting, arguing that additional officers are needed in light of recent personnel changes.

According to the police chief, retirements and promotions in the police department made it necessary to hire additional officers in 2014. This led to an increase in costs associated with officer training and implementation.

The council is expected to vote on a final budget some time next month.

M. Question Wording, M-turk Survey. Respondents were not initially told that the article was fictitious, but were fully debriefed at the end of the survey. To help ensure receipt of the treatment, the article appeared on the screen for 30 seconds before respondents were allowed to advance in the survey. Following the news article, the following questions were asked to measure outcomes.

Crime in vignette city:

"Compared to a typical city in the United States, would you say the city in the news article you just read has less violent crime, the same amount of violent crime, or more violent crime?" [Less violent crime; A typical amount of violent crime; More violent crime] (Note: The response option "A typical amount of violent crime" was changed to "The same amount of violent crime" in the SSI version of this survey in order to more closely match the wording of the question.)

Police budget in vignette city:

"Thinking about the article you just read, do you think the City Council should decrease the police department's budget, keep funding the same, or increase funding?" [Significantly decrease budget; Decrease budget; Keep budget the same; Increase budget; Significantly increase budget]

Violent crime open end:

Please briefly explain why you think the rate of violent crime in the city discussed with the news article was lower, about the same, or higher than a typical neighborhood in the U.S. You may answer in one sentence or less." [Text box]

Budget open end:

"Please briefly explain why you think the police budget in the city discussed in the news article should be increased, kept the same, or decreased. You may answer in one sentence or less." [Text box]

Crime worsening in United States: “Over the last ten years, would you say that the number of violent crimes per person in the United States has fallen, remained about the same, or risen?” [Fallen; Remained about the same; Risen]

Police budgets in U.S. “Please indicate whether you would like to see more or less government spending on the police and law enforcement in the United States.” [Spend much less; Spend less, Spend the same as now, Spend more, Spend much more]

Support for Three-Strikes Law: “As you may know, several states have recently passed laws commonly known as “three strikes and you’re out” laws. These laws say that anyone with two serious felony convictions on their record who is convicted of a third serious crime must be given a sentence of life in prison. Do you support or oppose ‘three strikes and you’re out’ laws?” [Strongly oppose, Oppose, Neither support nor oppose; Somewhat support; Strongly support] (Note: Note, the response option here read “Oppose” instead of “Somewhat oppose” due to an error.)

Confidence in Institutions in U.S.

“Now you will read a list of institutions in American society. Please tell me how much confidence you, yourself, have in each one – a great deal, quite a lot, some, or very little?” [Institutions listed: “The police”; “Congress”; “The criminal justice system”; “The presidency”; “The U.S. Supreme Court”]*

N. Text of Experimental News Article, SSI Survey.

City police ask for budget increase

Feb. 26, 2016

(Image Manipulation Here)

Above: Five city police officers stand guard during a local protest.

The chief of the city police department requested a 7 percent budget increase for his agency at Thursday’s City Council meeting, arguing that additional funds are necessary in light of recent personnel changes.

According to the police chief, retirements and promotions last year mean the department will need to hire additional officers to serve the city, which has roughly 200,000 residents. The additional funds will be used for training, equipment and salaries, the chief said.

The council is expected to vote on a final budget later this year.

O. Question Wording, SSI Survey. Crime in vignette city:

“Compared to a typical U.S. city of roughly the same size, would you say the city you just read about has less violent crime, the same amount of violent crime, or more violent crime?” [Less violent crime; The same amount of violent crime; More violent crime]

Worsening crime in vignette city:

“In the past year, would you say that the amount of violent crime in the city you just read about has decreased, stayed about the same, or increased?” [Decreased; Stayed about the same; Increased]

Police budget in vignette city:

“Thinking about the article you just read, do you think the City Council should decrease the police department’s budget, keep the budget the same, or increase the budget?” [Significantly decrease budget; Decrease budget; Keep budget the same; Increase budget; Significantly increase budget]

Confidence in police in vignette city:

“Please tell us how much confidence you, yourself, have in the police department in the city you just read about – a great deal, quite a lot, some, or very little?” [Very little; Some; Quite a lot; A great deal]

Crime in vignette city open end:

“Please briefly explain why you think the amount of violent crime in the city you just read about was lower, about the same, or higher than a typical U.S. city of roughly the same size?” [Text box]

Worsening violent crime open end:

“Please briefly explain why you think the amount of violent crime in the city you just read about has decreased, stayed about the same, or increased in the past year.” [Text box]

*Note: the wording of these questions, and the questions measuring confidence in police, is similar to standard survey items used by Gallup and other polling firms.

Budget open end: “Please briefly explain why you think the police budget in the city you just read about should be decreased, kept the same, or increased.” [Text box]

Police budgets in U.S.

“In your opinion, should local governments in the United States be spending less, about the same, or more on police agencies?” [Spend much less; Spend less; Spend the same as now; Spend more; Spend much more]

Confidence in police in U.S.:

“Please tell us how much confidence you, yourself, have in police in the United States – a great deal, quite a lot, some, or very little?” [Very little; Some; Quite a lot; A great deal]

Police patrols in respondent neighborhood:

“In general, would you be in favor of police patrolling your neighborhood less often, more often, or would you rather the amount of patrols remain the same?” [Fewer patrols; Remain the same; More patrols]

Perceived discrimination:

“Have you ever been treated unfairly by police?” [Yes; No; I don’t know] (Note: Responses of “I don’t know” were coded as missing. This question was asked in the final block of the survey.)

P. Changes between the M-Turk and SSI Surveys. The M-turk survey included several dependent variables pertaining to trust in government institutions and support for punitive policy that were not repeated in the SSI survey. The estimated treatment effects on these omitted outcomes are displayed in Figure S8. In addition, the SSI survey added items gauging perceived *changes* in crime within the vignette city, perceived fairness in treatment by police, and support for police patrols in respondents’ own neighborhoods. The SSI survey also included an attention screen at the start of the survey which screened out respondents who were unable to correctly use a sliding bar scale, a measurement tool used by other researchers who were sharing space on the survey instrument. The police experiment appeared first in the survey before the other researchers’ content. In the M-Turk survey, the prompts for the open-ended questions stated, “you may answer in one sentence or less.” This was omitted in the SSI survey in order to encourage more complete responses. In addition, the font size of the text of the mock news article was made larger in the SSI survey than in the M-turk survey to make it easier to read.

Demographic items were measured at the end of the M-Turk survey, but were measured at the start of the SSI survey in order to accommodate another experiment that was included in the same module by other researchers. The SSI survey also fully randomized the order of all questions measuring dependent variables, while the M-turk survey listed questions pertaining to the vignette city, including perceived crime in that city, immediately following the mock news article.

2. Additional Analysis and Robustness Checks

This section describes several additional tests that were conducted to gauge the robustness of the core results for the crime and officer safety analysis presented in the main text.

A. Demographics of SWAT Deployments in MD. Figure S2 displays the relationships between the volume of deployments in a given Maryland zip-code (logged and adjusted for population size) and various social indicators. As the figure shows, zip-codes with higher shares of non-Hispanic Black residents, and lower shares of non-Hispanic white residents, tend to see more SWAT team deployments per capita, while SWAT deployments appear to have nonlinear relationships with the %Non-Hispanic White, %Non-Hispanic Asian, % with a B.A. and median household income in a ZCTA.

B. The Parallel Trends Assumption. Differences-in-differences models identify causal effects given a parallel trends assumption, which states that outcomes in the treatment and control groups would have trended in parallel over time but for the treatment (58). A common robustness check of this assumption involves examining pre-treatment trends across groups. If groups are not trending in parallel prior to the treatment, there is good reason to suspect that the parallel trends assumption is violated. In this case, an example of this violation would be if agencies where crime was worsening adopted SWAT teams at differential rates compared with agencies where crime was stable.

To test for this possibility, I conduct additional placebo tests that add “lead” treatment indicators—which measure an agency’s SWAT status one time period in the future—to the DID models in both the national and Maryland panels (see Equation 3 below). If selection bias of this sort is not a problem, we should not be able to predict contemporaneous violent crime and officer safety levels with the *future* SWAT status of an agency.

$$\log(\text{Outcome}_{i,t} + 1) = \tau \text{SWAT}_{i,t} + \sigma \text{SWAT}_{i,t+1} + \theta_i + \gamma_t + \epsilon_{i,t} \tag{5}$$

Figures S3 and S4 display the results of these placebo tests in the national and Maryland SWAT panels, respectively. All “lead” effects are indiscernible from zero (statistically insignificant). There is no indication from this test that endogenous selection into adopting SWAT teams is producing these null results.

One plausible reason why the parallel trends assumption appears to hold in these data is that militarized policing has become a routine facet of local law enforcement (68–70). Local agencies are adopting militarized police units as a matter of course under the assumptions that these tactics help protect police officers and deter crime, but not necessarily as a response to changes in conditions.

C. Measurement Error and Influential Observations. As previous scholars have noted, FBI data on crime often contains error. Prior work has developed procedures for identifying values likely to be erroneous (71), such as observations which substantially deviate from an agency’s average value. The problem with this strategy is that it is difficult to tell whether an observation is truly erroneous or simply an outlier.

I took several alternative measures to gauge the robustness of results to data errors, summarized below.

1. **Iteratively dropping agencies.** I reanalyzed the data after iteratively dropping agencies from the data to test whether results were being driven by deviant observations. In one analysis, I iteratively drop each agency from the national and Maryland SWAT panels, reestimate effects on crime and officer safety, and store results. In another, I iteratively drop five random agencies from the national SWAT panel (10,000 iterations) and do the same. If extreme values due to measurement error are driving results, we should see a large variance in the resulting distribution of estimates. Figures S5- S7 display the results of this sensitivity analysis. The grey bars in the histograms correspond to the “leave one out” analysis and the pink bars correspond to the “leave five out” analysis. The results show no indication that a small number of discrepant agencies are driving results. In general these distributions of estimates are concentrated around the treatment effects reported in the main text.
2. **Dropping agencies with “zero” violent crimes.** An additional robustness check appears Tables S19 and S30. This analysis drops all agencies from the data which report zero violent crimes in at least one year in the national panel, or go a whole year without reporting a violent crime in the agency-month Maryland panel. The reasoning behind this test is that, in the FBI UCR data, the value of zero is listed for agencies who either did not report any crimes or that actually experienced zero crimes. Dropping these cases can therefore serve as another check that measurement error is not driving results. As the results show, dropping these agencies leads to a similar pattern of results, and no statistically significant negative estimates are recovered, i.e., the general conclusion that SWAT teams fail to reduce violent crime or enhance officer safety holds.
3. **Accounting for underreporting of assaults on officers.** The LEOKA monthly data set containing officer safety outcomes indicates whether an agency failed to report assaults in a given month. In the national panel analysis, where the agency-year is the unit of observation, I conduct a robustness check that weights all regression results by the proportion of months per agency in each wave of the CSLLEA that have complete assault data. The results appear in Table S22. The results still show no support for the claims that the acquisition of SWAT teams or SWAT deployments reduce violent crime or promote officer safety. In the Maryland panel analysis, where the agency-month is the unit of observation, I only use agency-months where assaults were reported in the core analyses. However, it is possible agencies did not report assaults on officers in these months because none occurred. See Table S34 for a robustness check treating unreported agency-month assault data as zero assaults, which leads to similar results.
4. **Dropping influential observations.** I identify influential observations by computing the Cook’s Distance for each observation in the core DID models and reestimating those models after dropping agencies with a Cook’s Distance greater than $\frac{4}{n-k-1}$, where n is the number of observations in the data and k is the number of parameters being estimated, (a common rule-of-thumb threshold for identifying influential observations (72)). Tables S21 and S31 show that the core results are largely unchanged after dropping these observations.

D. Spillover. Some might worry that the above results in Figure 1 are driven by spillover effects, i.e., jurisdictions with SWAT teams may deploy in neighboring jurisdictions which do not have SWAT teams. This is a common concern in all evaluations of law enforcement tactics, even in randomized controlled trials, since emergency scenarios often lead treatment assignment protocols to be violated in law enforcement studies.

As a robustness check, I reestimate core models after subsetting to only the largest agency (most full-time sworn officers) in each county. Because nearly all SWAT deployments are within-county in the Maryland data, this subset of the data should be largely free of spillover from other SWAT teams in other counties. And because large agencies would be less likely to receive SWAT assistance from smaller agencies in the county than vice versa, this subset should also be largely free of spillover from other agencies within counties. Table S20 displays the results of these tests, which yield conclusions highly similar to the main analysis (i.e., no significant reductions in crime or officer deaths/assaults).

E. Results: Open-Ended Survey Responses. The responses to open-ended questions concerning why police should get more/less funding can help shed light on the potential mechanisms behind the causal effects reported in the main text. A close reading of a sample of responses suggested that treated respondents cited the militarized gear in the images they saw as justification for funding police less, reasoning that the agency in question must be well-funded already if they have access to such equipment. For example, one respondent in one of the low militarization conditions in the M-Turk sample wrote that the agency’s budget should be reduced, “[b]ecause the gear and equipment in the picture looked very expensive and up to date so I think that they

could do with less." Another respondent in the SSI high militarization condition wrote, "from the picture, they were buying equipment to fight a war in another country. they are not fighting in the middle east. i think they are wasting money."

It also appeared from the open ended responses that *untreated* individuals were more likely to cite the department's stated justification for additional funds: that it needed to hire and train officers to make up for retirements and promotions. For example, one respondent in the control condition wrote that police deserved more money because, "Staff turnover is expensive." This suggests that, had the agency in question not deployed militarized police officers, respondents would have been more receptive to the agency's argument to the city council.

In order to systematically test whether these lines of reasoning were more or less prevalent among treated individuals, I conducted a dictionary analysis as follows. I randomly sampled 10% of the pooled SSI and M-Turk open ended responses and developed two dictionaries: one that included words commonly used in answers that noted how militarized the officers appeared, and one containing words common to those who cited personnel issues. The words in these dictionaries, displayed below, as well as the text of the open ended responses, were stemmed, made lowercase, and stripped of punctuation. Words which did not appear more than twice in the sample were discarded. In order to avoid over-fitting, I then discarded these 1,160 responses and computed the proportion of *remaining* responses across all treatment conditions which contained at least one of these words.

The results are displayed in Figure S10. The estimates in this figure are consistent with the results of the initial reading above: mentions of words relating to police militarization in the budget open ends are far more common in treated conditions than in the control condition in both the M-Turk and SSI samples. There is also evidence that responses pertaining to the department's stated justification for its budget request—personnel—were more common in the control condition than in treated conditions, though the clearest difference appears between the control and high treatment conditions.

Note: Open-ended responses were also asked about the reasoning behind the perceived levels of crime and text analysis of these responses revealed similar results. The full contents of the dictionaries used to produce Figure S10, which included corrected versions of words misspelled by respondents, are as follows:

Militarization Dictionary:

battl gear suit war militar gun arm guard riot equip power militari armi tank milit cloth armor uniform apparel combat swat outfit dress technic rifl artilleri

Personnel Dictionary:

personnel retir promot number hire turnov manpower pentsion salari retire newbi train wage staff retrain presenc replac retain recruit understaf new pension

3. SI Figures

A. Inter-Department Transfers of Military Equipment.

**Application for Tax Exempt Transfer and
Registration of Firearm**

ATF Control Number		SUBMIT in DUPLICATE to: National Firearms Act Branch Bureau of Alcohol, Tobacco, Firearms and Explosives, 244 Needy Road, Martinsburg, WV 25405	
2a. Transferee's Name and Address (Include trade name, if any) (See instruction 2) EUREKA POLICE DEPARTMENT 604 C STREET EUREKA, CA 95501		2b. County HUMBOLDT	1. Type of Transfer: Tax Exempt I Believe That I am Entitled to Exemption From the Payment of the Transfer Tax Imposed By Section 5811 (26 U.S.C. Chapter 53) on the Firearm Described Herein for the Following Reason (See instruction 2b): <input type="checkbox"/> Firearm is Unserviceable and is Being Transferred as a Curio or Ornament <input checked="" type="checkbox"/> Firearm is Being Transferred to or from a Government Entity <input type="checkbox"/> Firearm is Being Transferred to a Lawful Acir <input type="checkbox"/> Other (Specify) _____
2c. Transferor's Name and Address (Include trade name, if any) (Exception: see instruction 2j) SUTTER COUNTY SHERIFF'S DEPARTMENT 1077 CIVIC CENTER BLVD YUBA CITY, CA 95993		3b. Transferor's Telephone Number and Area Code (530) 822-2254	3d. Number, Street, City, State and Zip Code of Residence (or Firearm's Business Premises) if Different from Item 3c.
2e. If Applicable: Decedent's Name, Address, and Date of Death			
The above-named and undersigned transferor hereby makes application as required by Section 5812 of the National Firearms Act to transfer and register the firearm described below to the transferee.			
4. Description of Firearm (Complete items a through g)			
a. Name and Address of Manufacturer and/or Imposer of Firearm COLT MANUFACTURING CO INC P. O. BOX 1868 HARTFORD, CT 06144-1868	b. Type of Firearm (See instruction 1c) MACHINEGUN	c. Caliber, Gauge or Size (Specify) 5.56 MM	d. Model M16A1
		e. Length (Inches)	f. Overall (Inches)
		g. Serial Number	SEE ATTACHED LISTING
5. Additional Description or Data Appearing on Firearm (Attach additional sheet if necessary)		4. Is the Firearm Unserviceable as Defined in Instruction 1f? Yes <input type="checkbox"/> No <input checked="" type="checkbox"/> If "No," describe any other method by which firearm has been rendered unserviceable. Use additional sheets.	
6. Transferee's Federal Firearms License (If any) (Give complete 15-digit number) (See instruction 2c)		6. Transferee's Special (Occupational) Tax Status (If any)	
7. Transferor's Federal Firearms License (If any) (Give complete 15-digit number) (See instruction 2c)		8. Transferor's Special (Occupational) Tax Status (If any)	
9. Consent to Disclosure of Information to Transferee (See instruction 8j. I Do or Do Not leave and Authorize ATF to Provide Information Relating to this Application to the Above-Named Transferee.		a. Employer Identification Number b. Class	
10. Signature of Transferor (Or authorized official)		11. Name and Title of Authorized Official (Print or type) Gregory R. Demore 6-27-14	
12. Date			
The Space Below is for the use of the Bureau of Alcohol, Tobacco, Firearms and Explosives			
By Authority of the Director, This Application Has Been Examined, and the Transfer and Registration of the Firearm Described Herein and the Inherent Movement of that Firearm, When Applicable, to the Transferee are:			
<input type="checkbox"/> Approved (With the following conditions, if any)		<input type="checkbox"/> Disapproved (For the following reason(s))	
Signature of Authorized ATF Official			Date

ATF Form 5 (5-2013)
Revised November 2014

Fig. S1. Record of Inter-Agency Transfer of Weapons in California from 2014. The record, which I obtained through a public information request, shows the transfer of 20 M-16 rifles between two agencies in California in 2014. Though the weapons appear in the inventory of the receiving agency in the 2015 1033 data, there is no indication that these weapons once belonged to the transferring agency. Interagency transfers of this nature thus render the 1033 data an inaccurate record of which agencies held what over time.

B. Geographic Correlates of SWAT in Maryland.

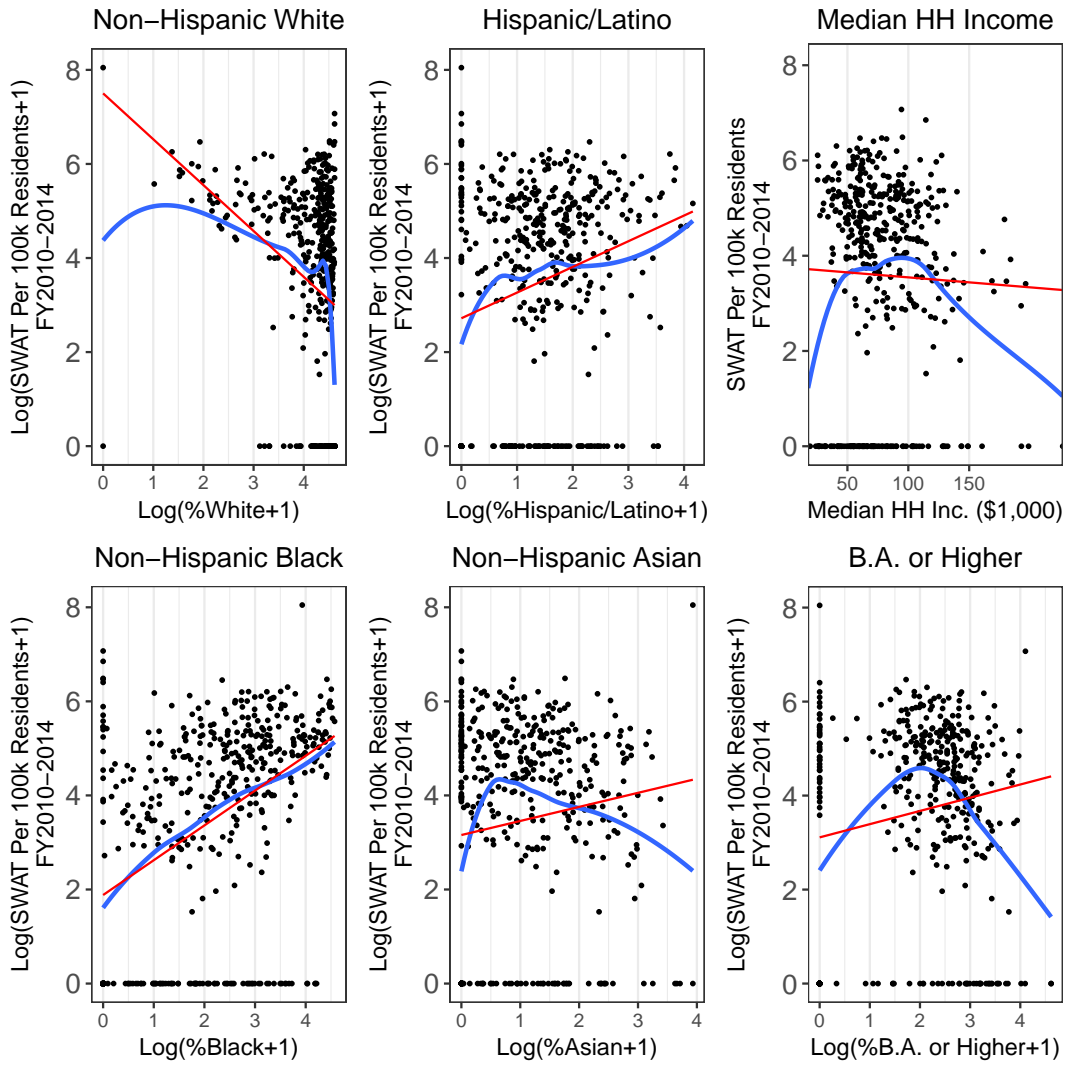


Fig. S2. SWAT Deployments in Maryland ZCTAs. ZCTA-level demographics in Maryland by volume of SWAT deployments, FY2010-2014. LOESS and OLS fits.

C. Placebo Tests With “Lead” SWAT Treatments.

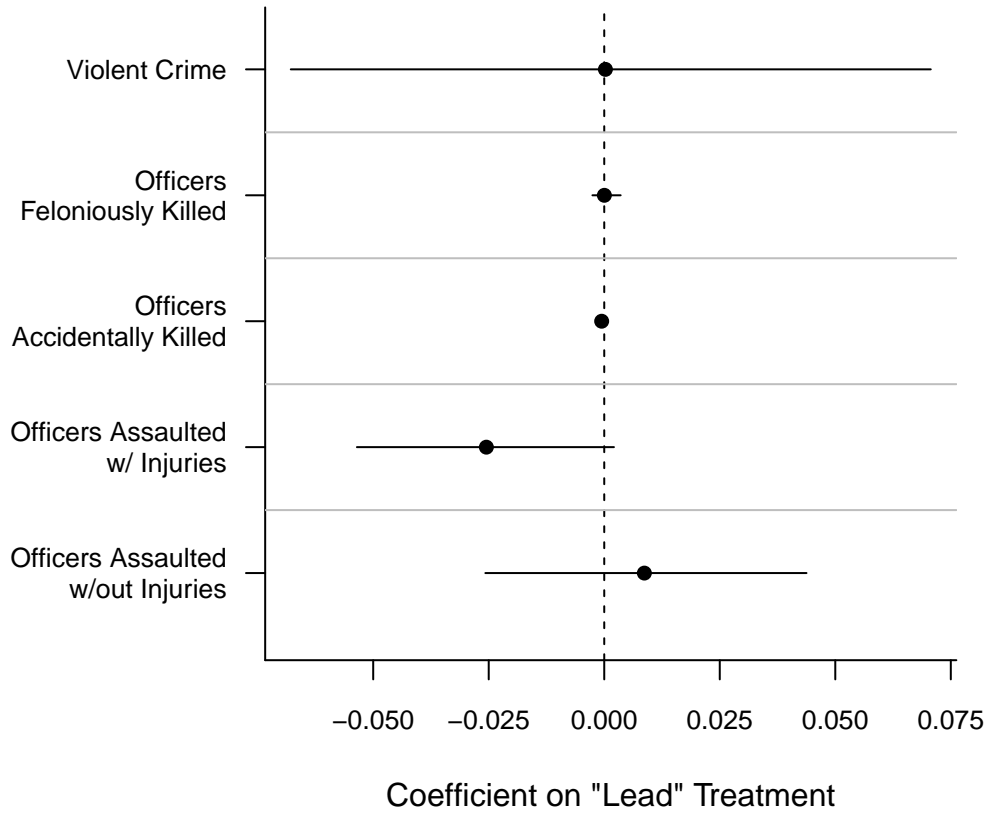


Fig. S3. Placebo test: Future SWAT Status Cannot Predict Current Outcomes in National SWAT Panel. Effects of obtaining a SWAT Team on crime and officer safety; placebo test using "lead" treatment indicators. Dots denote point estimates, bars denote 95% CIs. Future treatment status cannot predict contemporaneous outcomes, suggesting results are not driven by endogenous selection into obtaining a SWAT team.

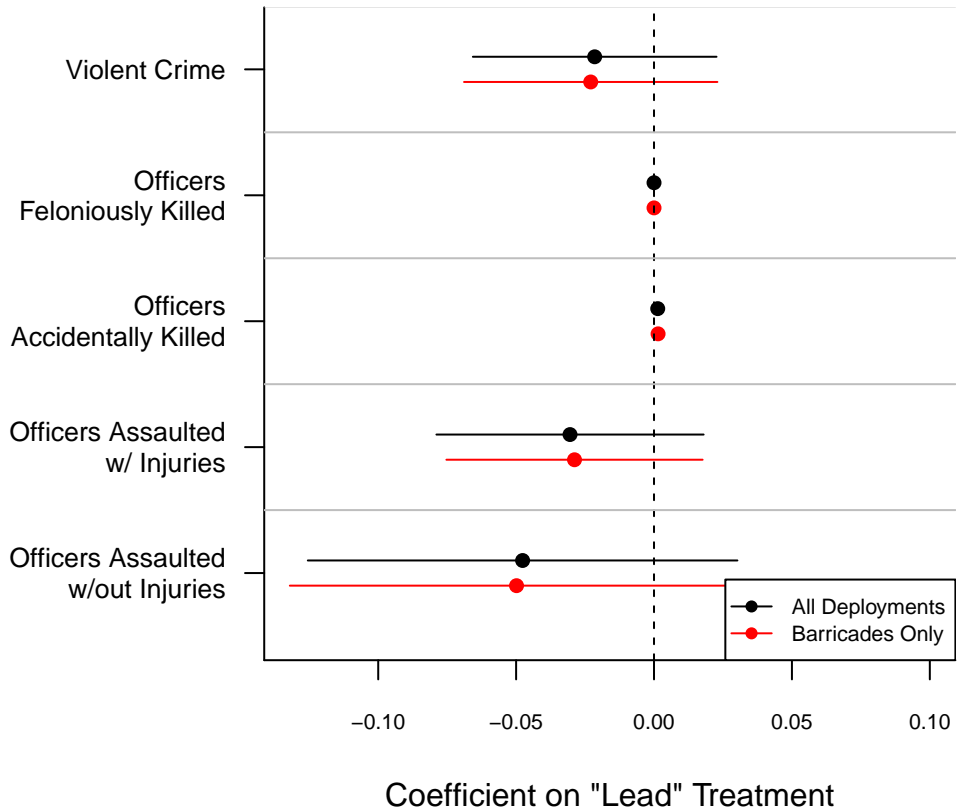


Fig. S4. Placebo Test: Future SWAT Deployments Cannot Predict Current Outcomes In Maryland SWAT Panel. Effects of deploying a SWAT Team on crime and officer safety in Maryland, FY2010-2014; placebo test using "lead" treatment indicators. Dots denote point estimates, bars denote 95% CIs. Future treatment status cannot predict contemporaneous outcomes, suggesting results are not driven by endogenous selection into deploying SWAT teams.

D. Sensitivity to Dropping Agencies.

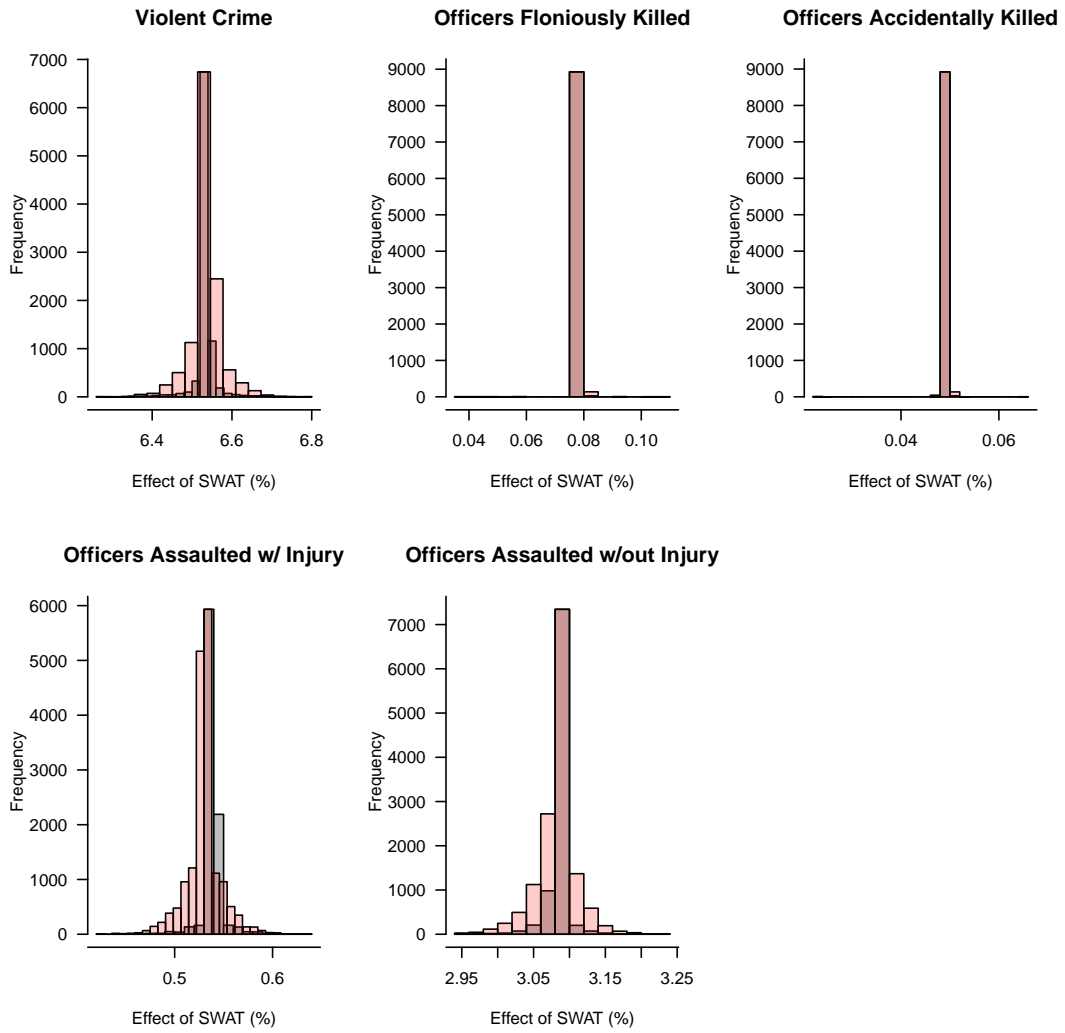


Fig. S5. Sensitivity Analysis, DID Model, National SWAT Panel. To test whether particular agencies are driving results, perhaps due to measurement error, the plot shows the distributions of estimates obtained for each outcome when dropping one agency's data at a time (gray bars), and when dropping a random 5 agencies at a time (red bars).

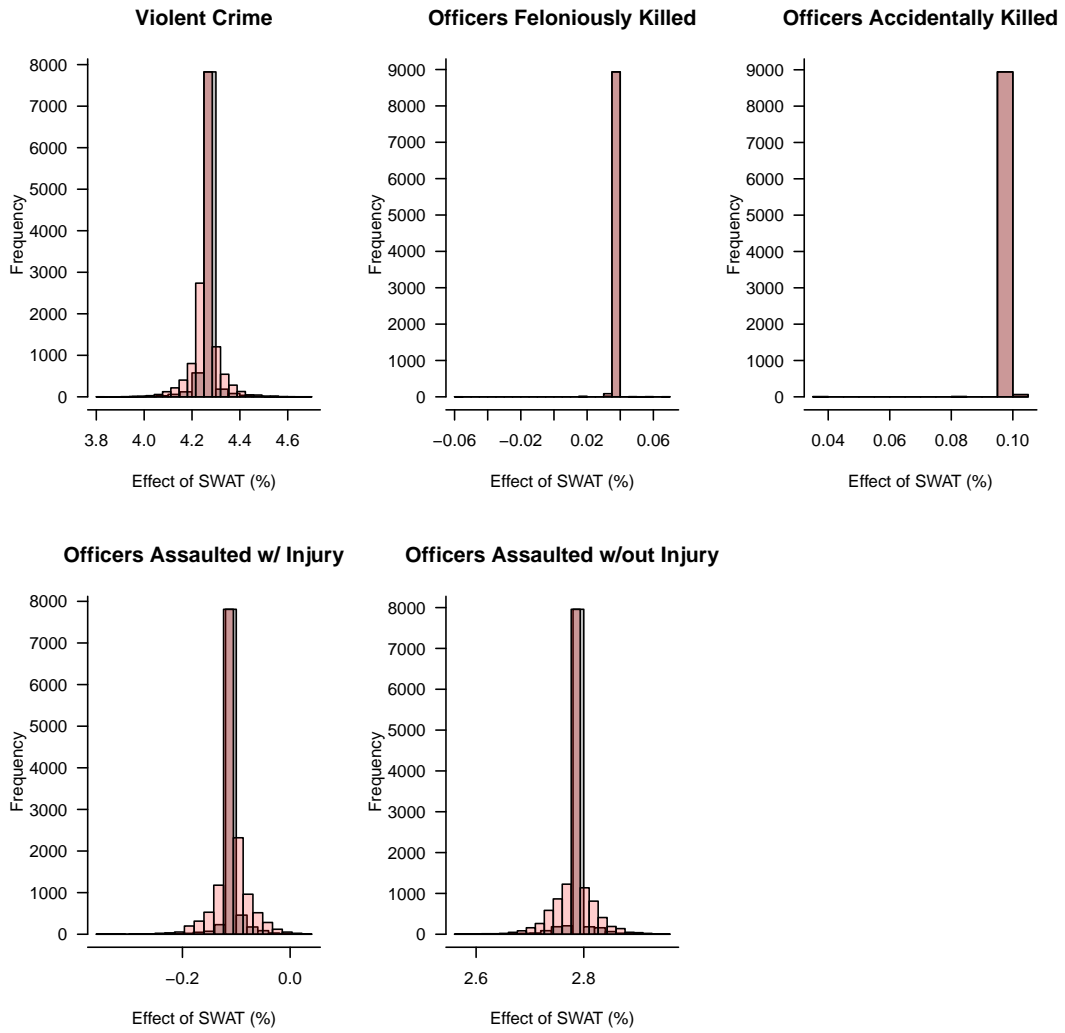


Fig. S6. Sensitivity Analysis, Agency FE + Time Trends Model, National SWAT Panel. To test whether particular agencies are driving results, perhaps due to measurement error, the plot shows the distributions of estimates obtained for each outcome when dropping one agency's data at a time (gray bars), and when dropping a random 5 agencies at a time (red bars).

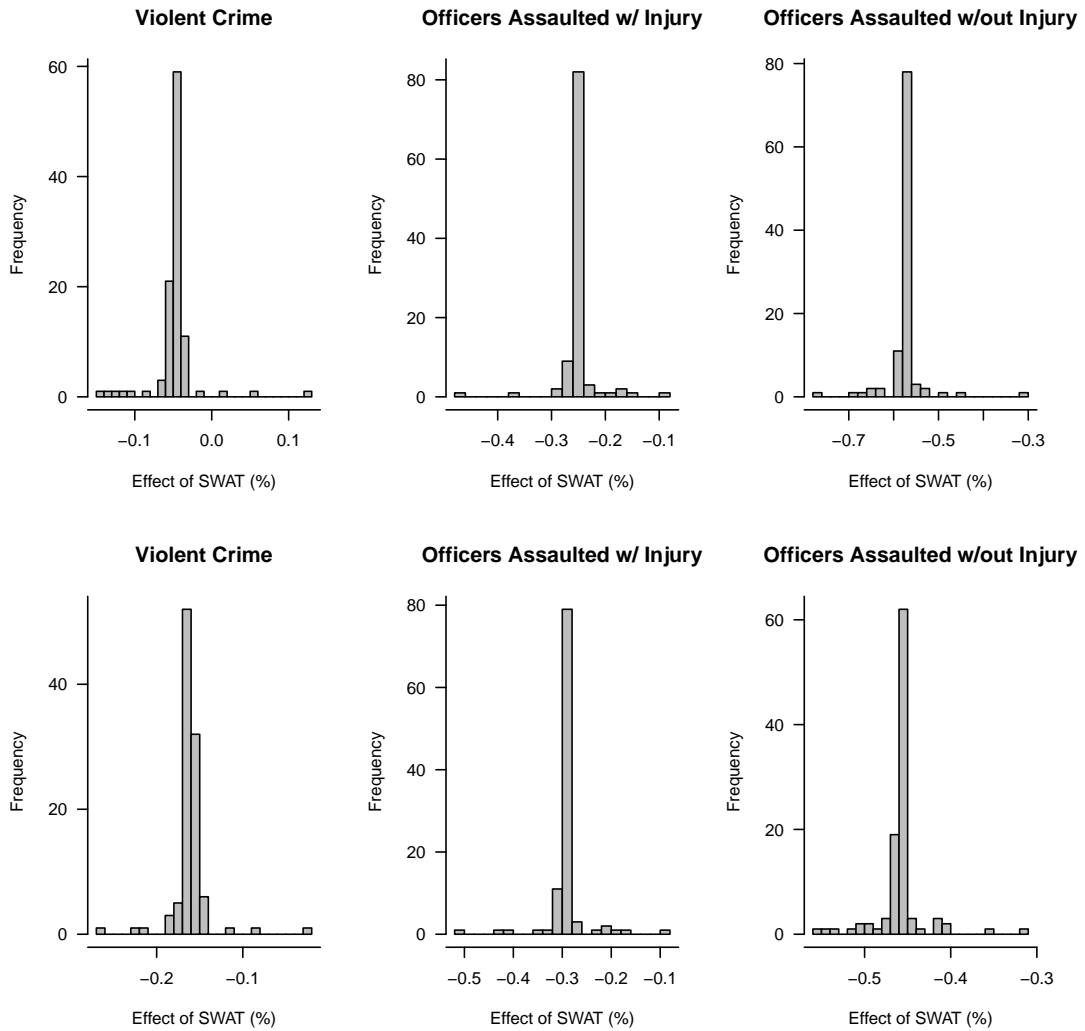


Fig. S7. Sensitivity Analysis, Maryland SWAT Deployment Panel. To test whether particular agencies are driving results, perhaps due to measurement error, the plot shows the distributions of estimates (effect of a 10% increase in deployments) obtained for each outcome when dropping one agency's data at a time. The top panels show results from the DID model and the bottom panels show results from the DID + Time Trends models.

E. Additional Survey Results.

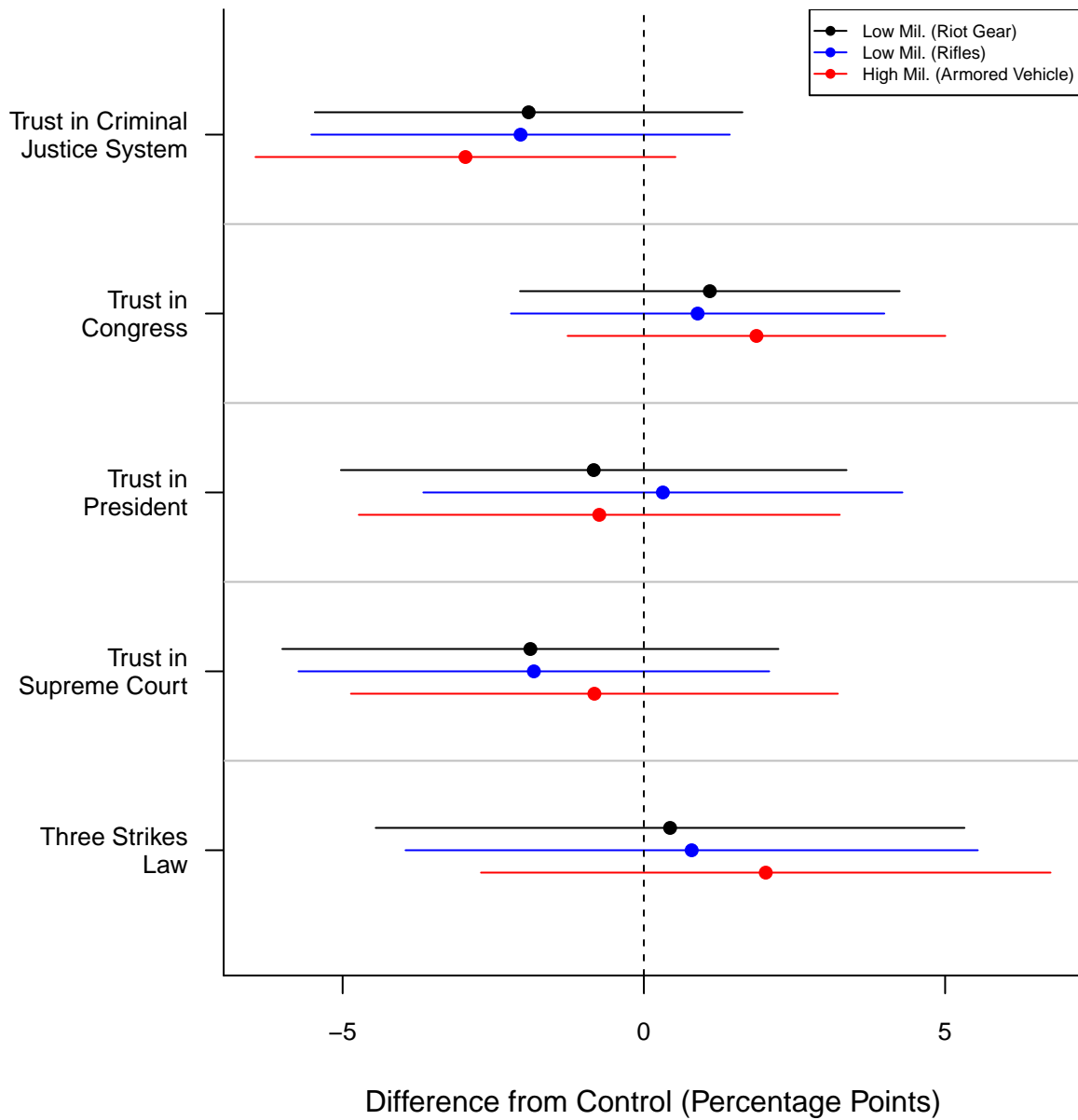


Fig. S8. Treatment Effects on Additional Dependent Variables in M-Turk Sample. These dependent variables were not included on the SSI survey. Bars denote 95% confidence intervals.

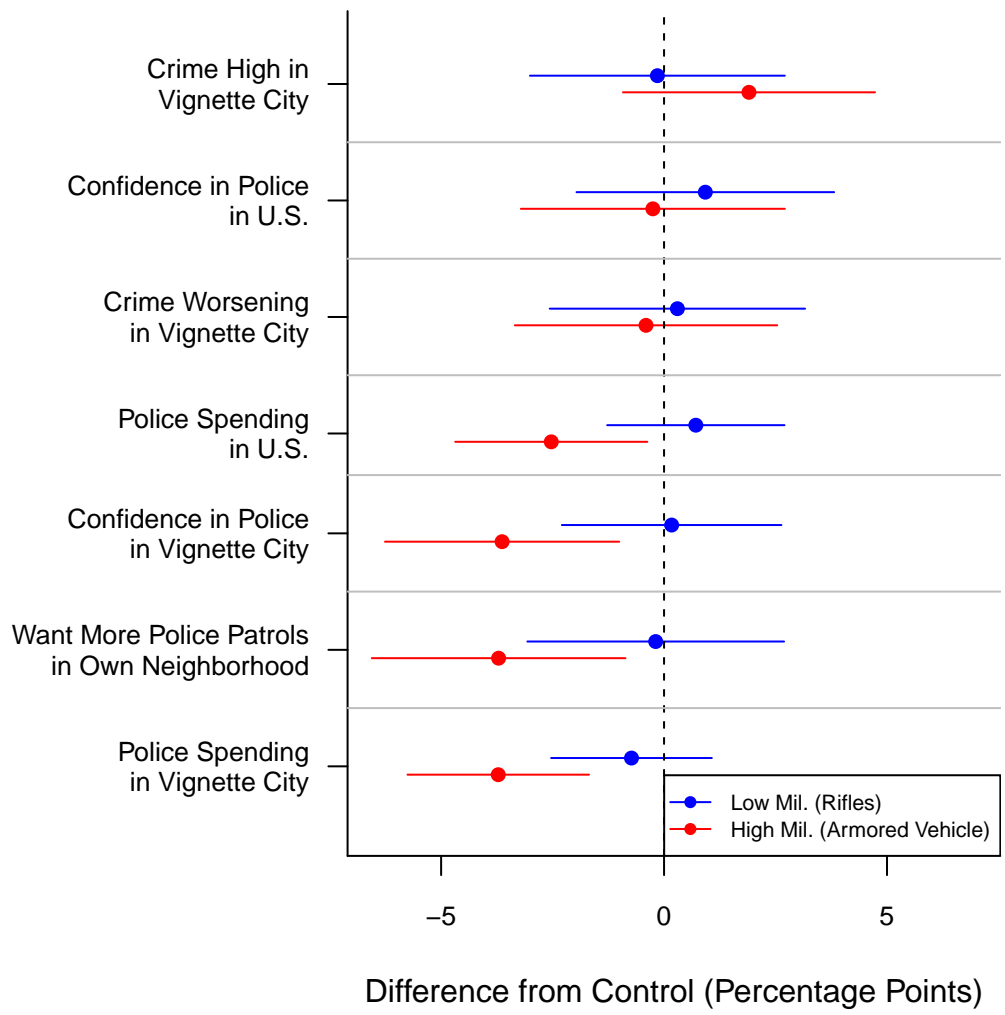


Fig. S9. Population-weighted Treatment Effects. Results of the SSI experiment after re-weighting the data to be nationally representative in terms of race, gender and party according to proportions in the population-weighted 2016 Cooperative Congressional Election Study (66).

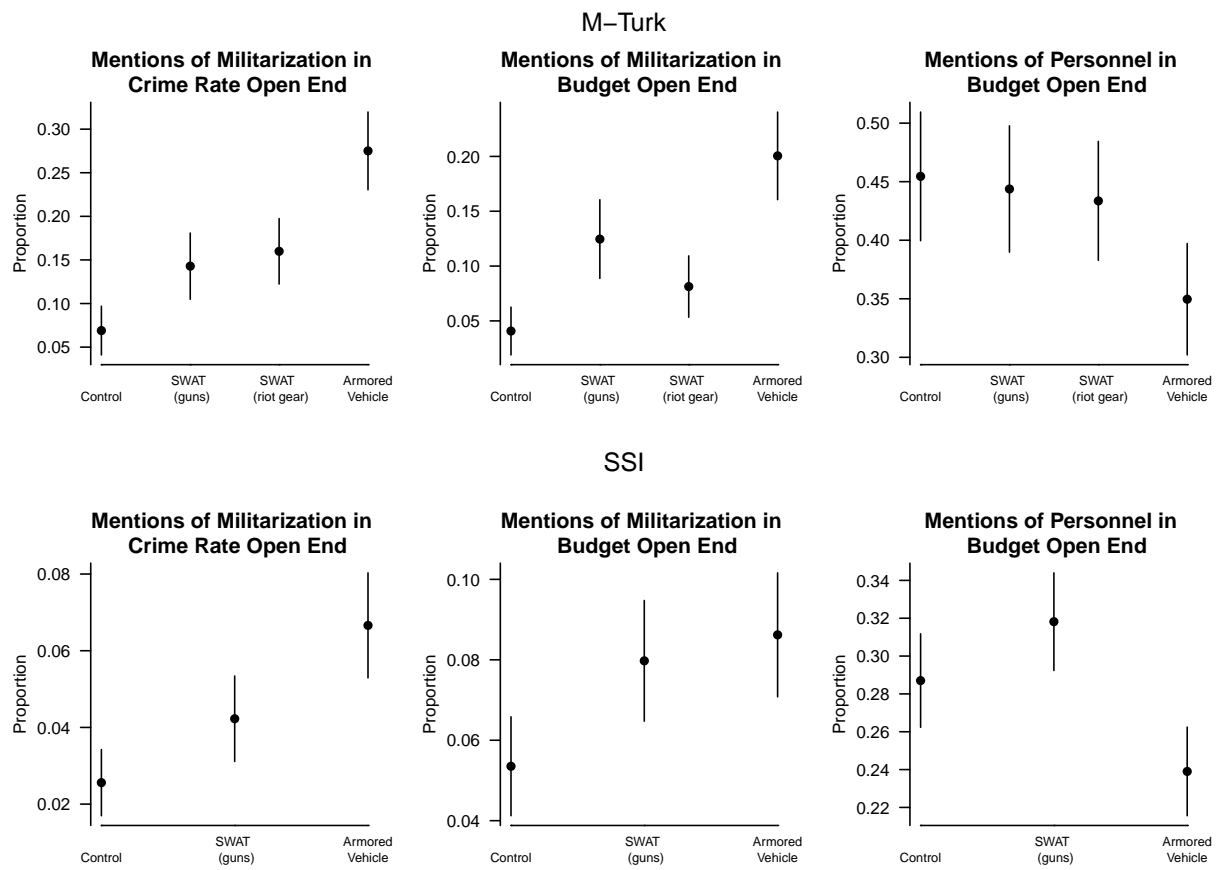


Fig. S10. Open End Response Dictionary Analysis. Words relating to militarization are much more prevalent among treated respondents. Words relating to personnel shortages are much more prevalent among control respondents.

4. SI Tables

A. Descriptive Statistics.

Table S1. Comparison Between 2008 CSLLEA Agencies and 2008 Agencies Used for Estimation. The first column displays mean values for the agencies in the 2008 CSLLEA sample. The second column contains means for agencies in the 2008 estimation data.

	Mean in 2008 CSLLEA	Mean in 2008 Estimation Data
SWAT	0.338	0.376
#Full-Time Sworn Officers	45.796	39.484
Police Agency	0.799	0.816
Sheriff's Agency	0.197	0.181
State Police Agency	0.003	0.002
Annual Budget (\$)	6,497,758	5,326,525
South	0.361	0.395
N	15331	8961

Table S2. Distribution of Treatment Status in CSLLEA Data. The table shows the number and percent of agencies in the three-wave CSLLEA panel which always had a SWAT team, never had a SWAT team, obtained a SWAT team in 2004 or 2008, or switched between having and not having a SWAT team during this period.

	count	percent
always	1842	20.56
never	4520	50.44
start 2004	695	7.76
start 2008	691	7.71
switcher	1213	13.54

Table S3. Descriptive Statistics: National SWAT Panel, 2000, 2004 and 2008

	N	min	median	mean	max	sd
SWAT	26883	0.00	0.00	0.34	1.00	0.47
#Full-Time Sworn Officers	26883	1.00	13.00	38.12	4458.00	134.71
#Violent Crimes	26883	0.00	41.00	261.70	45860.00	1175.38
log(#Violent Crimes + 1)	26883	0.00	3.74	3.50	10.73	2.20
#Officers Feloniously Killed	26883	0.00	0.00	0.00	4.00	0.06
log(#Officers Feloniously Killed+1)	26883	0.00	0.00	0.00	1.61	0.03
#Officers Accidentally Killed	26883	0.00	0.00	0.00	1.00	0.04
log(#Officers Accidentally Killed+1)	26883	0.00	0.00	0.00	0.69	0.03
#Injurious Assaults on Officers	26883	0.00	0.00	1.03	221.00	5.84
log(#Injurious Assaults on Officers+1)	26883	0.00	0.00	0.27	5.40	0.64
#Non-Injurious Assaults on Officers	26883	0.00	0.00	2.22	805.00	16.23
log(#Non-Injurious Assaults on Officers+1)	26883	0.00	0.00	0.38	6.69	0.82

Table S4. Descriptive Statistics: Maryland ZCTAs

	N	min	median	mean	max	sd
Population	468	0.00	5031.50	12466.42	69893.00	15322.56
#SWAT Deployments per 100k	466	0.00	73.17	125.26	3125.00	202.02
log(#SWAT Deployments per 100k+1)	466	0.00	4.31	3.46	8.05	2.23
Unemployment Rate	459	0.00	6.90	8.12	100.00	7.26
log(Unemployment Rate+1)	459	0.00	2.07	1.97	4.62	0.76
% w/ BA or Higher	425	0.00	9.30	11.81	100.00	12.50
log(% w/ BA or Higher+1)	425	0.00	2.33	2.02	4.62	1.18
%Non-Hispanic White	466	0.00	79.32	71.57	100.00	26.57
log(%Non-Hispanic White+1)	466	0.00	4.39	4.14	4.62	0.69
%Non-Hispanic Black	466	0.00	9.00	17.19	95.61	21.62
log(%Non-Hispanic Black+1)	466	0.00	2.30	2.13	4.57	1.36
%Non-Hispanic Asian	466	0.00	1.37	3.71	50.00	6.43
log(%Non-Hispanic Asian+1)	466	0.00	0.86	1.01	3.93	0.96
%Latino	466	0.00	3.00	5.39	62.94	8.06
log(%Latino+1)	466	0.00	1.39	1.36	4.16	0.97
Median Household Income (\$)	446	17708.00	72179.00	78678.21	226786.00	32593.45

Table S5. Descriptive Statistics: Maryland ZCTAs, Three Large Maryland Agencies Only

	N	min	median	mean	max	sd
Population	114	115.00	25502.00	25875.45	69893.00	17271.14
#SWAT Deployments per 100k	114	0.00	56.13	84.37	506.09	101.64
log(#SWAT Deployments per 100k+1)	114	0.00	4.04	3.21	6.23	2.05
#Violent Crimes per 100k	114	0.00	215.86	326.90	1961.41	429.58
log(#Violent Crimes per 100k +1)	114	0.00	5.38	3.73	7.58	2.95
Unemployment Rate	114	0.00	8.05	8.98	28.40	4.70
log(Unemployment Rate+1)	114	0.00	2.20	2.19	3.38	0.51
% w/ BA or Higher	112	0.00	13.25	15.89	51.90	11.73
log(% w/ BA or Higher+1)	114	0.59	3.35	3.23	4.57	1.02
%Non-Hispanic Black	114	0.80	27.64	36.79	95.61	28.63
log(%Non-Hispanic Black+1)	112	0.00	2.66	2.58	3.97	0.75
Median Household Income (\$)	112	19375.00	76300.00	84825.85	192891.00	38785.18

Table S6. Descriptive Statistics: Maryland Counties

	N	min	median	mean	max	sd
Population	24	20477.50	107615.25	250912.45	1030670.00	305044.58
#SWAT Deployments per 100k	24	4.14	25.04	30.58	84.15	18.75
log(#SWAT Deployments per 100k+1)	24	1.64	3.26	3.28	4.44	0.63
#Violent Crimes per 100k	24	0.00	0.00	0.00	0.01	0.00
log(#Violent Crimes per 100k)	24	0.00	0.00	0.00	0.01	0.00
%Non-Hispanic Black	24	1.33	14.90	20.48	62.87	16.51
log(%Non-Hispanic Black+1)	24	0.84	2.77	2.81	4.16	0.76
Unemployment Rate	24	5.20	7.75	8.32	13.90	2.27
log(Unemployment Rate+1)	24	1.82	2.17	2.21	2.70	0.23
% w/ BA or Higher	24	1.60	9.50	10.08	20.60	4.60
log(% w/ BA or Higher+1)	24	0.96	2.35	2.31	3.07	0.48
Median Household Income (\$)	24	38447.00	66587.50	69403.92	109865.00	20871.61

Table S7. Descriptive Statistics: Maryland SWAT Panel, Agency-Months

	N	min	median	mean	max	sd
#SWAT Deployments	6240	0.00	0.00	0.58	40.00	2.36
log(#SWAT Deployments+1)	6240	0.00	0.00	0.19	3.71	0.53
#SWAT Deployments for Barricades	6240	0.00	0.00	0.03	5.00	0.23
log(#SWAT Deployments for Barricades+1)	6240	0.00	0.00	0.02	1.79	0.13
#Violent Crimes	6240	0.00	4.00	34.96	1797.00	158.05
log(#Violent Crimes + 1)	6240	0.00	1.61	1.78	7.49	1.55
#Officers Feloniously Killed	6240	0.00	0.00	0.00	0.00	0.00
log(#Officers Feloniously Killed+1)	6240	0.00	0.00	0.00	0.00	0.00
#Officers Accidentally Killed	6240	0.00	0.00	0.00	1.00	0.03
log(#Officers Accidentally Killed+1)	6240	0.00	0.00	0.00	0.69	0.02
#Injurious Assaults on Officers	2038	0.00	0.00	0.74	31.00	2.72
log(#Injurious Assaults on Officers+1)	2038	0.00	0.00	0.25	3.47	0.57
#Non-Injurious Assaults on Officers	2038	0.00	0.00	2.33	48.00	6.12
log(#Non-Injurious Assaults on Officers+1)	2038	0.00	0.00	0.63	3.89	0.86

B. Models of SWAT Deployments by Local Geographic Attributes.

Table S8. OLS Models of Volume of SWAT Deployments, Maryland

	MD ZCTAs	MD ZCTAs	Large Agencies, ZCTAs	Large Agencies, ZCTAs	MD Counties	MD Counties
(Intercept)	1.88 *	1.49 *	-1.67 *	-0.37	1.85 *	-0.37
	(0.19)	(0.62)	(0.42)	(1.24)	(0.41)	(2.26)
log(%Black + 1)	0.74 *	0.65 *	1.51 *	1.08 *	0.51 *	0.37 *
	(0.07)	(0.08)	(0.11)	(0.18)	(0.13)	(0.18)
Median HH Inc		-0.00		-0.00 *		0.00 *
		(0.00)		(0.00)		(0.00)
log(%BA or Higher + 1)		0.15		-0.07		-0.53
		(0.11)		(0.16)		(0.31)
log(Unemployment Rate + 1)		0.42		0.47		1.83
		(0.23)		(0.33)		(1.02)
log(Violent Crimes per 100k + 1)				0.03		-0.28
				(0.06)		(0.33)
<i>N</i>	466	411	114	110	24	24

Robust standard errors in parentheses

* indicates significance at $p < 0.05$

Table S9. OLS Models of Volume of SWAT Deployments, Maryland, Weighted by Population

	MD ZCTAs	MD ZCTAs	Large Agencies, ZCTAs	Large Agencies, ZCTAs	MD Counties	MD Counties
(Intercept)	2.71 *	2.16 *	-0.88	-0.75	1.82 *	-2.41
	(0.18)	(0.59)	(0.48)	(1.84)	(0.38)	(2.37)
log(%Black + 1)	0.60 *	0.44 *	1.35 *	0.85 *	0.44 *	0.17
	(0.05)	(0.06)	(0.12)	(0.18)	(0.12)	(0.21)
Median HH Inc		-0.00		-0.00		0.00 *
		(0.00)		(0.00)		(0.00)
log(%BA or Higher + 1)		-0.12		-0.03		-0.77 *
		(0.08)		(0.23)		(0.20)
log(Unemployment Rate + 1)		0.65 *		1.11		1.88
		(0.21)		(0.59)		(0.96)
log(Violent Crimes per 100k + 1)				-0.04		0.13
				(0.05)		(0.19)
<i>N</i>	466	411	114	110	24	24

Robust standard errors in parentheses

* indicates significance at $p < 0.05$

C. Survey Sample Demographics.

Table S10. Mean Values on Demographic Traits in Survey Samples. Note: *N* computed as number of respondents who answered violent crime question about vignette city. Due to non-response, these proportions may vary depending on the dependent variable being analyzed.

	M-turk	SSI
male	0.52	0.47
age	34.25	41.74
nhwhite	0.77	0.45
nhblack	0.07	0.41
latino	0.08	0.08
other race	0.09	0.05
Has B.A.	0.50	0.39
N	1,566	4,465

Table S11. OLS Models Predicting Treatment Assignment in Survey Experiments

	SSI, Control	SSI, Treatment 1	SSI, Treatment 2	Turk, Control	Turk, Treatment 1	Turk, Treatment 2	Turk, Treatment 3
(Intercept)	0.27 *	0.33 *	0.40 *	0.23 *	0.35 *	0.14 *	0.28 *
	(0.04)	(0.04)	(0.04)	(0.05)	(0.05)	(0.05)	(0.05)
male	0.02	-0.03	0.01	0.01	-0.03	0.00	0.02
	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)
age	0.00	0.00	-0.00	0.00	-0.00 *	0.00 *	-0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
nhwhite	0.07 *	-0.02	-0.05	-0.00	-0.05	0.06	-0.01
	(0.03)	(0.03)	(0.03)	(0.04)	(0.04)	(0.04)	(0.04)
nhblack	0.07 *	-0.01	-0.06	-0.08	0.01	0.04	0.03
	(0.03)	(0.03)	(0.03)	(0.06)	(0.06)	(0.06)	(0.06)
latino	0.01	0.04	-0.06	-0.01	-0.07	0.09	-0.01
	(0.04)	(0.04)	(0.04)	(0.05)	(0.05)	(0.06)	(0.06)
has B.A.	-0.01	-0.01	0.02	0.01	0.02	-0.02	-0.00
	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)
<i>N</i>	4,702	4,702	4,702	1,563	1,563	1,563	1,563
<i>F</i> -stat	1.984	1.585	1.254	0.568	1.644	1.644	1.602
<i>p</i> -value on <i>F</i> -stat	0.064	0.147	0.275	0.756	0.131	0.131	0.143

Standard errors in parentheses. * indicates $p < 0.05$.

D. Robustness Checks: National SWAT Panel Analysis.

Tables display untransformed coefficients. Treatment is dichotomous. All outcomes coded as $\log(x + 1)$ unless otherwise specified.

Table S12. Effect of Obtaining a SWAT Team on Crime and Officer Safety, Estimates from Main Text

model	coef	95% CI	N
Violent Crime, DID	0.063	[0.02,0.107]	26883
Violent Crime, Time Trends	0.042	[-0.017,0.1]	26883
Officers Feloniously Killed, DID	0.001	[-0.001,0.003]	26883
Officers Feloniously Killed, Time Trends	0.000	[-0.001,0.003]	26883
Officers Accidentally Killed, DID	0.001	[0,0.002]	26883
Officers Accidentally Killed, Time Trends	0.001	[0,0.003]	26883
Officers Assaulted w/ Injuries, DID	0.006	[-0.012,0.024]	26883
Officers Assaulted w/ Injuries, Time Trends	-0.001	[-0.027,0.024]	26883
Officers Assaulted w/out Injuries, DID	0.031	[0.009,0.052]	26883
Officers Assaulted w/out Injuries, Time Trends	0.027	[-0.002,0.056]	26883

Table S13. Effect of Obtaining a SWAT Team on Crime and Officer Safety, Weighted by Agency Size

model	coef	95% CI	N
Violent Crime, DID	0.227	[0.037,0.551]	26883
Violent Crime, Time Trends	0.146	[-0.02,0.383]	26883
Officers Feloniously Killed, DID	0.001	[-0.008,0.009]	26883
Officers Feloniously Killed, Time Trends	-0.008	[-0.024,0.003]	26883
Officers Accidentally Killed, DID	-0.002	[-0.009,0.005]	26883
Officers Accidentally Killed, Time Trends	0.003	[-0.002,0.008]	26883
Officers Assaulted w/ Injuries, DID	0.031	[-0.018,0.081]	26883
Officers Assaulted w/ Injuries, Time Trends	0.020	[-0.043,0.086]	26883
Officers Assaulted w/out Injuries, DID	0.048	[-0.02,0.113]	26883
Officers Assaulted w/out Injuries, Time Trends	0.036	[-0.039,0.114]	26883

Table S14. Effect of Obtaining a SWAT Team on Crime and Officer Safety, Placebo Test with Lead Treatments

model	coef	95% CI	N
Violent Crime, Coef on Treatment	0.052	[-0.015,0.121]	17922
Violent Crime, Coef on Lead Treatment	0.000	[-0.068,0.071]	17922
Officers Feloniously Killed, Coef on Treatment	0.000	[-0.002,0.003]	17922
Officers Feloniously Killed, Coef on Lead Treatment	0.000	[-0.003,0.004]	17922
Officers Accidentally Killed, Coef on Treatment	-0.000	[-0.001,0.001]	17922
Officers Accidentally Killed, Coef on Lead Treatment	-0.001	[-0.001,0]	17922
Officers Assaulted w/ Injuries, Coef on Treatment	-0.020	[-0.048,0.009]	17922
Officers Assaulted w/ Injuries, Coef on Lead Treatment	-0.026	[-0.054,0.002]	17922
Officers Assaulted w/out Injuries, Coef on Treatment	0.034	[-0.001,0.071]	17922
Officers Assaulted w/out Injuries, Coef on Lead Treatment	0.009	[-0.026,0.044]	17922

Table S15. Effect of Obtaining a SWAT Team on Crime and Officer Safety, Outcomes Lagged One Year

model	coef	95% CI	N
Violent Crime, DID	0.041	[-0.005,0.087]	26883
Violent Crime, Time Trends	0.023	[-0.041,0.088]	26883
Officers Feloniously Killed, DID	-0.001	[-0.002,0]	26883
Officers Feloniously Killed, Time Trends	-0.001	[-0.003,0.001]	26883
Officers Accidentally Killed, DID	-0.000	[-0.002,0.001]	26883
Officers Accidentally Killed, Time Trends	-0.001	[-0.003,0.001]	26883
Officers Assaulted w/ Injuries, DID	-0.011	[-0.028,0.007]	26883
Officers Assaulted w/ Injuries, Time Trends	-0.016	[-0.041,0.01]	26883
Officers Assaulted w/out Injuries, DID	0.021	[-0.002,0.042]	26883
Officers Assaulted w/out Injuries, Time Trends	0.005	[-0.025,0.034]	26883

Table S16. Effect of Obtaining a SWAT Team on Officer Safety, Outcomes Coded Dichotomously

model	coef	95% CI	N
Violent Crime, DID	0.008	[-0.004,0.019]	26883
Violent Crime, Time Trends	0.006	[-0.01,0.021]	26883
Officers Feloniously Killed, DID	0.001	[-0.001,0.002]	26883
Officers Feloniously Killed, Time Trends	0.000	[-0.001,0.003]	26883
Officers Accidentally Killed, DID	0.001	[0,0.002]	26883
Officers Accidentally Killed, Time Trends	0.001	[0,0.004]	26883
Officers Assaulted w/ Injuries, DID	0.011	[-0.005,0.025]	26883
Officers Assaulted w/ Injuries, Time Trends	-0.007	[-0.032,0.015]	26883
Officers Assaulted w/out Injuries, DID	0.020	[0.004,0.035]	26883
Officers Assaulted w/out Injuries, Time Trends	0.016	[-0.006,0.039]	26883

Table S17. Effect of Obtaining a SWAT Team on Crime and Officer Safety, Outcome Scaled by Agency Size

model	coef	95% CI	N
Violent Crime, DID	0.005	[-0.018,0.028]	26883
Violent Crime, Time Trends	0.002	[-0.028,0.034]	26883
Officers Feloniously Killed, DID	0.000	[0,0]	26883
Officers Feloniously Killed, Time Trends	0.000	[0,0]	26883
Officers Accidentally Killed, DID	0.000	[0,0]	26883
Officers Accidentally Killed, Time Trends	0.000	[0,0]	26883
Officers Assaulted w/ Injuries, DID	-0.001	[-0.003,0.002]	26883
Officers Assaulted w/ Injuries, Time Trends	-0.001	[-0.005,0.002]	26883
Officers Assaulted w/out Injuries, DID	0.003	[-0.001,0.007]	26883
Officers Assaulted w/out Injuries, Time Trends	0.002	[-0.003,0.007]	26883

Table S18. Effect of Obtaining a SWAT Team on Crime and Officer Safety, Dropping Observations from 2000

model	coef	95% CI	N
Violent Crime, DID	0.055	[-0.006,0.116]	17922
Officers Feloniously Killed, DID	0.001	[-0.001,0.003]	17922
Officers Accidentally Killed, DID	0.001	[0,0.003]	17922
Officers Assaulted w/ Injuries, DID	0.016	[-0.008,0.041]	17922
Officers Assaulted w/out Injuries, DID	0.029	[0,0.056]	17922

Table S19. Effect of Obtaining a SWAT Team on Crime and Officer Safety, Dropping Agencies with “Zero” Violent Crimes

model	coef	95% CI	N
Violent Crime, DID	0.033	[0.006,0.061]	19611
Violent Crime, Time Trends	0.023	[-0.012,0.058]	19611
Officers Feloniously Killed, DID	0.001	[-0.001,0.003]	19611
Officers Feloniously Killed, Time Trends	0.000	[-0.002,0.004]	19611
Officers Accidentally Killed, DID	0.001	[0,0.002]	19611
Officers Accidentally Killed, Time Trends	0.001	[0,0.003]	19611
Officers Assaulted w/ Injuries, DID	-0.001	[-0.023,0.02]	19611
Officers Assaulted w/ Injuries, Time Trends	-0.009	[-0.042,0.023]	19611
Officers Assaulted w/out Injuries, DID	0.030	[0.003,0.058]	19611
Officers Assaulted w/out Injuries, Time Trends	0.028	[-0.007,0.063]	19611

Table S20. Effect of Obtaining a SWAT Team on Crime and Officer Safety, Only Largest Agency in Each County

model	coef	95% CI	N
Violent Crime, DID	0.054	[-0.041,0.15]	8280
Violent Crime, Time Trends	0.045	[-0.069,0.165]	8280
Officers Feloniously Killed, DID	0.002	[-0.001,0.007]	8280
Officers Feloniously Killed, Time Trends	0.001	[-0.004,0.009]	8280
Officers Accidentally Killed, DID	0.001	[-0.001,0.004]	8280
Officers Accidentally Killed, Time Trends	0.003	[0,0.007]	8280
Officers Assaulted w/ Injuries, DID	0.006	[-0.03,0.041]	8280
Officers Assaulted w/ Injuries, Time Trends	0.007	[-0.041,0.058]	8280
Officers Assaulted w/out Injuries, DID	0.026	[-0.017,0.065]	8280
Officers Assaulted w/out Injuries, Time Trends	0.039	[-0.018,0.099]	8280

Table S21. Effect of Obtaining a SWAT Team on Crime and Officer Safety, Influential Observations Dropped

model	coef	95% CI	N
Violent Crime, DID	0.066	[0.015,0.116]	23448
Officers Feloniously Killed, DID	0.001	[-0.001,0.002]	26748
Officers Accidentally Killed, DID	0.000	[0,0.001]	26763
Officers Assaulted w/ Injuries, DID	0.003	[-0.016,0.023]	22788
Officers Assaulted w/out Injuries, DID	0.028	[0.004,0.053]	23250

Table S22. Effect of Obtaining a SWAT Team on Crime and Officer Safety, Weighted by Completeness of LEOKA Assault Data

model	coef	95% CI	N
Violent Crime, DID	0.010	[-0.037,0.058]	26883
Officers Feloniously Killed, DID	0.000	[-0.001,0.001]	26883
Officers Accidentally Killed, DID	-0.000	[-0.001,0]	26883
Officers Assaulted w/ Injuries, DID	-0.002	[-0.038,0.036]	26883
Officers Assaulted w/out Injuries, DID	0.036	[-0.003,0.078]	26883

E. Robustness Checks: Maryland SWAT Panel Analysis.

Tables display untransformed coefficients. All outcomes and treatments coded as $\log(x + 1)$ unless otherwise specified.

Table S23. Effect of SWAT Team Deployments on Crime and Officer Safety, All Deployments, Estimates from Figure 3

model	coef	95% CI	N
Violent Crime,	-0.013	[-0.056,0.029]	6240
Violent Crime,	-0.024	[-0.07,0.022]	6240
Officers Assaulted w/ Injuries,	-0.026	[-0.094,0.041]	2038
Officers Assaulted w/ Injuries,	-0.031	[-0.113,0.052]	2038
Officers Assaulted w/out Injuries,	-0.061	[-0.142,0.021]	2038
Officers Assaulted w/out Injuries,	-0.048	[-0.105,0.009]	2038

Table S24. Effect of SWAT Team Deployments on Crime and Officer Safety, Barricade Deployments, Estimates from Figure 3

model	coef	95% CI	N
Violent Crime,	-0.011	[-0.067,0.044]	6240
Violent Crime,	-0.015	[-0.07,0.04]	6240
Officers Assaulted w/ Injuries,	-0.022	[-0.07,0.025]	2038
Officers Assaulted w/ Injuries,	-0.036	[-0.092,0.02]	2038
Officers Assaulted w/out Injuries,	-0.018	[-0.165,0.129]	2038
Officers Assaulted w/out Injuries,	-0.030	[-0.186,0.125]	2038

Table S25. Effect of "Lead" SWAT Team Deployments on Crime and Officer Safety, Placebo Test

model	coef	95% CI	N
Violent Crime, DID	-0.022	[-0.066,0.023]	6136
Officers Assaulted w/ Injuries, DID	-0.034	[-0.118,0.051]	1934
Officers Assaulted w/out Injuries, DID	-0.032	[-0.148,0.085]	1934

Table S26. Effect of SWAT Team Deployments on Crime and Officer Safety, Outcomes Lagged One Month

model	coef	95% CI	N
Violent Crime,	0.034	[0.001,0.067]	6136
Violent Crime,	0.029	[-0.003,0.061]	6136
Officers Assaulted w/ Injuries,	-0.039	[-0.091,0.012]	2014
Officers Assaulted w/ Injuries,	-0.046	[-0.103,0.012]	2014
Officers Assaulted w/out Injuries,	-0.023	[-0.092,0.045]	2014
Officers Assaulted w/out Injuries,	-0.011	[-0.076,0.053]	2014

Table S27. Effect of SWAT Team Deployments on Crime and Officer Safety, Outcomes Lagged Two Months

model	coef	95% CI	N
Violent Crime,	0.000	[-0.035,0.035]	6032
Violent Crime,	-0.006	[-0.034,0.022]	6032
Officers Assaulted w/ Injuries,	-0.001	[-0.069,0.066]	1989
Officers Assaulted w/ Injuries,	-0.011	[-0.083,0.061]	1989
Officers Assaulted w/out Injuries,	-0.004	[-0.09,0.082]	1989
Officers Assaulted w/out Injuries,	0.014	[-0.072,0.1]	1989

Table S28. Effect of SWAT Team Deployments on Crime and Officer Safety, Outcomes Coded Dichotomously

model	coef	95% CI	N
Violent Crime,	-0.002	[-0.008,0.003]	6240
Violent Crime,	0.000	[-0.005,0.005]	6240
Officers Assaulted w/ Injuries,	-0.007	[-0.081,0.066]	2038
Officers Assaulted w/ Injuries,	-0.017	[-0.101,0.067]	2038
Officers Assaulted w/out Injuries,	-0.031	[-0.079,0.016]	2038
Officers Assaulted w/out Injuries,	-0.023	[-0.061,0.016]	2038

Table S29. Effect of SWAT Team Deployments on Crime and Officer Safety, Outcomes Scaled by Agency Size

model	coef	95% CI	N
Violent Crime,	-0.006	[-0.032,0.021]	6240
Violent Crime,	-0.009	[-0.035,0.017]	6240
Officers Assaulted w/ Injuries,	-0.000	[-0.003,0.003]	2038
Officers Assaulted w/ Injuries,	0.000	[-0.002,0.003]	2038
Officers Assaulted w/out Injuries,	-0.003	[-0.008,0.003]	2038
Officers Assaulted w/out Injuries,	-0.001	[-0.005,0.002]	2038

Table S30. Effect of SWAT Team Deployments on Crime and Officer Safety, Drop Agencies that Go Year Without Violent Crime

model	coef	95% CI	N
Violent Crime,	-0.013	[-0.056,0.03]	5580
Violent Crime,	-0.024	[-0.071,0.023]	5580
Officers Assaulted w/ Injuries,	-0.026	[-0.094,0.042]	1877
Officers Assaulted w/ Injuries,	-0.030	[-0.112,0.053]	1877
Officers Assaulted w/out Injuries,	-0.060	[-0.14,0.021]	1877
Officers Assaulted w/out Injuries,	-0.048	[-0.105,0.009]	1877

Table S31. Effect of SWAT Team Deployments on Crime and Officer Safety, Dropping Influential Observations

model	coef	95% CI	N
Violent Crime,	0.011	[-0.014,0.037]	1920
Violent Crime,	-0.001	[-0.021,0.019]	1920
Officers Assaulted w/ Injuries,	0.024	[-0.015,0.064]	662
Officers Assaulted w/ Injuries,	0.026	[-0.023,0.075]	674
Officers Assaulted w/out Injuries,	-0.057	[-0.153,0.039]	1028
Officers Assaulted w/out Injuries,	0.050	[-0.038,0.137]	636

Table S32. Effect of SWAT Team Deployments on Crime and Officer Safety, SWAT deployments coded dichotomously

model	coef	95% CI	N
Violent Crime,	-0.024	[-0.084,0.035]	6240
Violent Crime,	-0.027	[-0.09,0.037]	6240
Officers Assaulted w/ Injuries,	0.001	[-0.084,0.086]	2038
Officers Assaulted w/ Injuries,	0.004	[-0.092,0.099]	2038
Officers Assaulted w/out Injuries,	-0.091	[-0.183,0.002]	2038
Officers Assaulted w/out Injuries,	-0.062	[-0.129,0.004]	2038

Table S33. Effect of SWAT Team Deployments on Crime and Officer Safety, SWAT deployments and outcomes coded dichotomously

model	coef	95% CI	N
Violent Crime,	-0.004	[-0.01,0.003]	6240
Violent Crime,	0.001	[-0.007,0.01]	6240
Officers Assaulted w/ Injuries,	-0.002	[-0.105,0.102]	2038
Officers Assaulted w/ Injuries,	-0.004	[-0.113,0.105]	2038
Officers Assaulted w/out Injuries,	-0.054	[-0.126,0.017]	2038
Officers Assaulted w/out Injuries,	-0.030	[-0.092,0.031]	2038

Table S34. Effect of SWAT Team Deployments on Crime and Officer Safety, Agency-months Where Assaults Not Reported Coded as Zero Assaults

model	coef	95% CI	N
Violent Crime,	-0.013	[-0.056,0.029]	6240
Violent Crime,	-0.024	[-0.07,0.022]	6240
Officers Assaulted w/ Injuries,	0.007	[-0.037,0.051]	6240
Officers Assaulted w/ Injuries,	0.008	[-0.044,0.061]	6240
Officers Assaulted w/out Injuries,	-0.028	[-0.089,0.034]	6240
Officers Assaulted w/out Injuries,	-0.023	[-0.075,0.029]	6240

Table S35. Effect of SWAT Team Deployments on Crime and Officer Safety, All Deployments, Weighted by Agency Size in 2008 CSLLEA

model	coef	95% CI	N
Violent Crime, DID	0.021	[-0.018,0.06]	6240
Violent Crime, Time Trends	0.004	[-0.024,0.032]	6240
Officers Assaulted w/ Injuries, DID	-0.135	[-0.199,-0.071]	2038
Officers Assaulted w/ Injuries, Time Trends	-0.126	[-0.176,-0.077]	2038
Officers Assaulted w/out Injuries, DID	-0.088	[-0.138,-0.039]	2038
Officers Assaulted w/out Injuries, Time Trends	-0.064	[-0.105,-0.023]	2038

Table S36. Effect of SWAT Team Deployments on Crime and Officer Safety, Barricade Deployments, Weighted by Agency Size in 2008 CSLLEA

model	coef	95% CI	N
Violent Crime, DID	-0.004	[-0.031,0.023]	6240
Violent Crime, Time Trends	-0.001	[-0.025,0.023]	6240
Officers Assaulted w/ Injuries, DID	-0.063	[-0.136,0.01]	2038
Officers Assaulted w/ Injuries, Time Trends	-0.095	[-0.173,-0.018]	2038
Officers Assaulted w/out Injuries, DID	0.062	[0.012,0.111]	2038
Officers Assaulted w/out Injuries, Time Trends	0.031	[-0.017,0.079]	2038

F. Survey Experimental Effect Estimates.

Table S37. M-Turk Results: Effects Relative to Control Condition (Percentage Points)

dv	treatment	coef	se	lb	ub
Crime High in Vignette City	riot_gear	8.90	1.90	5.10	12.70
Crime High in Vignette City	swat_w_guns	10.10	1.90	6.50	13.80
Crime High in Vignette City	swat_plus_mrap	15.20	1.90	11.50	18.90
Recent Changes in National Crime	riot_gear	0.30	2.90	-5.50	6.00
Recent Changes in National Crime	swat_w_guns	0.30	2.80	-5.20	5.90
Recent Changes in National Crime	swat_plus_mrap	-0.20	2.90	-5.80	5.50
Police Spending in Vignette City	riot_gear	-4.10	1.20	-6.40	-1.90
Police Spending in Vignette City	swat_w_guns	-3.90	1.10	-6.10	-1.80
Police Spending in Vignette City	swat_plus_mrap	-8.60	1.30	-11.00	-6.10
Police Spending in U.S.	riot_gear	-2.30	1.60	-5.40	0.90
Police Spending in U.S.	swat_w_guns	-1.70	1.60	-4.80	1.40
Police Spending in U.S.	swat_plus_mrap	-3.70	1.60	-6.80	-0.60
Confidence in Police in U.S.	riot_gear	-2.40	2.00	-6.40	1.50
Confidence in Police in U.S.	swat_w_guns	-2.10	1.90	-5.90	1.70
Confidence in Police in U.S.	swat_plus_mrap	-2.90	2.00	-6.70	1.00
Confidence in Criminal Justice System	riot_gear	-1.90	1.80	-5.50	1.60
Confidence in Criminal Justice System	swat_w_guns	-2.00	1.80	-5.50	1.40
Confidence in Criminal Justice System	swat_plus_mrap	-3.00	1.80	-6.40	0.50
Confidence in Congress	riot_gear	1.10	1.60	-2.10	4.20
Confidence in Congress	swat_w_guns	0.90	1.60	-2.20	4.00
Confidence in Congress	swat_plus_mrap	1.90	1.60	-1.30	5.00
Confidence in Presidency	riot_gear	-0.80	2.10	-5.00	3.40
Confidence in Presidency	swat_w_guns	0.30	2.00	-3.70	4.30
Confidence in Presidency	swat_plus_mrap	-0.70	2.00	-4.70	3.30
Confidence in Supreme Court	riot_gear	-1.90	2.10	-6.00	2.20
Confidence in Supreme Court	swat_w_guns	-1.80	2.00	-5.70	2.10
Confidence in Supreme Court	swat_plus_mrap	-0.80	2.10	-4.90	3.20
Three Strikes Law	riot_gear	0.40	2.50	-4.50	5.30
Three Strikes Law	swat_w_guns	0.80	2.40	-4.00	5.50
Three Strikes Law	swat_plus_mrap	2.00	2.40	-2.70	6.70

Table S38. SSI Results

dv	treatment	coef	se	lb	ub
Crime High in Vignette City	swat_w_guns	0.80	1.10	-1.30	2.90
Crime High in Vignette City	swat_plus_mrap	2.20	1.10	0.10	4.30
Crime Worsening in Vignette City	swat_w_guns	1.70	1.10	-0.30	3.80
Crime Worsening in Vignette City	swat_plus_mrap	0.80	1.10	-1.30	2.90
Confidence in Police in U.S.	swat_w_guns	0.90	1.00	-1.10	2.90
Confidence in Police in U.S.	swat_plus_mrap	0.50	1.00	-1.60	2.50
Police Spending in U.S.	swat_w_guns	0.80	0.80	-0.70	2.30
Police Spending in U.S.	swat_plus_mrap	-2.00	0.80	-3.50	-0.50
Confidence in Police in Vignette City	swat_w_guns	0.40	0.90	-1.40	2.20
Confidence in Police in Vignette City	swat_plus_mrap	-2.60	0.90	-4.40	-0.80
Police Spending in Vignette City	swat_w_guns	-0.30	0.70	-1.60	1.00
Police Spending in Vignette City	swat_plus_mrap	-2.70	0.70	-4.10	-1.40
Want More Police Patrols in Own Neighborhood	swat_w_guns	-0.80	1.10	-2.90	1.30
Want More Police Patrols in Own Neighborhood	swat_plus_mrap	-3.20	1.10	-5.30	-1.10

Table S39. SSI Results: Effects by Race of Respondent

dv	treatment	coef.white	lb.white	ub.white	coef.black	lb.black	ub.black	coef.diff	lb.diff	ub.diff
Crime Worsening in Vignette City	swat_w_guns	1.00	-2.10	4.00	2.60	-0.60	5.80	1.60	-2.80	6.10
Crime Worsening in Vignette City	swat_plus_mrap	2.30	-0.80	5.40	-0.90	-4.10	2.30	-3.20	-7.70	1.30
Crime High in Vignette City	swat_w_guns	-0.50	-3.50	2.50	2.10	-1.20	5.40	2.70	-1.80	7.10
Crime High in Vignette City	swat_plus_mrap	2.50	-0.50	5.60	2.30	-1.00	5.60	-0.30	-4.80	4.20
Confidence in Police in U.S.	swat_w_guns	1.50	-1.40	4.40	0.90	-2.10	4.00	-0.50	-4.80	3.70
Confidence in Police in U.S.	swat_plus_mrap	0.10	-2.90	3.20	0.80	-2.20	3.90	0.70	-3.60	5.00
Police Spending in U.S.	swat_w_guns	1.10	-0.90	3.20	-0.40	-2.80	2.00	-1.60	-4.70	1.60
Police Spending in U.S.	swat_plus_mrap	-1.50	-3.80	0.70	-1.90	-4.30	0.50	-0.40	-3.70	2.90
Confidence in Police in Vignette City	swat_w_guns	0.70	-1.80	3.30	-0.10	-2.90	2.70	-0.80	-4.60	3.00
Confidence in Police in Vignette City	swat_plus_mrap	-2.70	-5.30	-0.00	-2.70	-5.50	0.10	-0.00	-3.90	3.80
Want More Police Patrols	swat_w_guns	-0.00	-3.00	2.90	-1.20	-4.50	2.10	-1.20	-5.60	3.30
Want More Police Patrols	swat_plus_mrap	-3.00	-6.00	0.10	-3.10	-6.50	0.20	-0.20	-4.70	4.40
Police Spending in Vignette City	swat_w_guns	-0.50	-2.20	1.20	-0.10	-2.20	2.00	0.40	-2.30	3.10
Police Spending in Vignette City	swat_plus_mrap	-3.20	-5.10	-1.30	-2.00	-4.10	0.20	1.20	-1.60	4.10

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