Militarization fails to enhance police safety or reduce crime but may harm police reputation

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The increasingly visible presence of heavily armed police units in American communities has stoked widespread concern over the militarization of local law enforcement. Advocates claim militarized policing protects officers and deters violent crime, while critics allege these tactics are targeted at racial minorities and erode trust in law enforcement. Using a rare geocoded census of SWAT team deployments from Maryland, I show that militarized police units are more often deployed in communities with large shares of African American residents, even after controlling for local crime rates. Further, using nationwide panel data on local police militarization, I demonstrate that militarized policing fails to enhance officer safety or reduce local crime. Finally, using survey experiments—one of which includes a large oversample of African American respondents—I show that seeing militarized police in news reports may diminish police reputation in the mass public. In the case of militarized policing, the results suggest that the often-cited trade-off between public safety and civil liberties is a false choice.

police militarization | public safety | crime | race and policing | bureaucratic reputation

As thousands marched in Ferguson, MO to protest the police shooting of Michael Brown in 2014, many Americans were surprised and alarmed by the character of law enforcement's response. For days, national news networks broadcast images of armored vehicles, snipers taking aim at unarmed Black and brown civilians, and officers clad in battle armor, deployed by state and local police agencies (1).

To some people, American police appeared to have suddenly transformed into a wartime occupying force. But to scholars of race and policing, and perhaps to many citizens of color, these images were less surprising. More than half a century earlier, James Baldwin described urban police as "occupying forces" in Black communities (2). And decades of research in the intervening years have documented the ways in which policing efforts like "stop and frisk" and the "war on drugs" have served to maintain race- and class-based social hierarchies (3–6). In part due to this history, critical race scholars have characterized police militarization as another means by which the state exercises social control over racial minorities (7).

But despite a prolonged and vigorous national debate, there is little systematic evidence demonstrating the consequences of militarized police tactics or whether they are more prevalent in communities of color. Because of heterogeneity in the way thousands of local law enforcement agencies in the United States document the presence and activities of their militarized units (if they document them at all), the study of police militarization has been hampered by data constraints (8, 9). In the absence of scientific analysis, the arguments of both advocates and critics are largely informed by anecdotal and journalistic accounts. Proponents argue that militarized police units enhance officer safety and deter violent crime (10), while critics allege that these tactics are disproportionately applied in the policing of racial minorities (11–13), potentially eroding the already-anemic levels of trust between citizens and law enforcement in highly policed communities (14). The implications of police militarization for civil rights, public safety, and the exercise of state power depend crucially on the empirical validity of these claims.

This study leverages previously unavailable data to describe the communities affected by militarized policing and to estimate its effects on crime, officer safety, and public perceptions of police. I first use a rare census of "special weapons and tactics" (SWAT) team deployments in Maryland to characterize the ways in which militarized police units are used and the characteristics of the communities in which they deploy. I show that militarized police units are more often deployed in communities with high concentrations of African Americans, a relationship that holds at multiple levels of geography and even after controlling for social indicators including crime rates. I then use an original nationwide panel measuring the presence of active SWAT teams in roughly 9,000 US law enforcement agencies, as well as the Maryland SWAT deployment data, to test whether militarized policing lowers crime rates and promotes officer safety. Using within-agency comparisons that greatly mitigate concerns over omitted variable bias, I find no evidence that obtaining or deploying a SWAT team reduces local crime rates or lowers the rates at which officers are killed or assaulted.

Finally, using survey experiments that randomly assign images of police officers in news reports, I show that seeing more militarized officers—relative to traditionally equipped police—can inflate perceptions of crime and depress support for police

Significance

National debates over heavy-handed police tactics, including so-called "militarized" policing, are often framed as a trade-off between civil liberties and public safety, but the costs and benefits of controversial police practices remain unclear due to data limitations. Using an array of administrative data sources and original experiments I show that militarized "special weapons and tactics" (SWAT) teams are more often deployed in communities of color, and—contrary to claims by police administrators—provide no detectable benefits in terms of officer safety or violent crime reduction, on average. However, survey experiments suggest that seeing militarized police in news reports erodes opinion toward law enforcement. Taken together, these findings suggest that curtailing militarized policing may be in the interest of both police and citizens.

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funding and presence. This analysis includes a large oversample of African American respondents—an important feature given the high rate at which militarized police units deploy in Black neighborhoods.

On average, militarized police units do not appear to provide the safety benefits that many police administrators claim. And police may suffer reputational damage when they deploy militarized units. These results suggest that the often-cited tradeoff between public safety and civil liberties is, in the case of militarized policing, a false choice.

Defining Police Militarization

Police militarization is a continuum defined by a combination of equipment, tactics, and culture that centers on violent conflict (7, 15, 16). In recent decades, local police agencies have militarized their departments to varying degrees, adopting weapons, attire, tactics, and organizational structures developed for theaters of war. The proliferation of militarized policing is due in part to an expansion of the war on drugs and federal initiatives that supplied localities with excess military equipment and funds to purchase arms (17, 18). Heterogeneity in agency capacity makes it difficult to precisely code police agencies as "militarized" or not. Recently publicized data on military gear disbursements have been used in some studies to estimate the effects of militarization on police violence, crime, and officer safety (19–21). But these data convey only the receipt of equipment from one of several programs that help supply agencies with militarized gear (22). The data also appear incomplete (see *SI Appendix*, section 1A for details).

As an alternative approach, this paper analyzes the effects of a substantively important threshold on the militarization continuum: the use of SWAT teams. Both popular and scholarly debates over police militarization have focused on the activities of SWAT teams, their pronounced role in conducting the drug war (17), and their high-profile crowd-control efforts (23). SWAT teams often receive advanced combat training and exhibit a command structure modeled on military special forces units (15). In general the formation of a SWAT team represents a heightened commitment to the use of militarized equipment and tactics.

While it is plausible that SWAT deployments deter violent crime—and, in turn, improve officer safety—previous research on other heavy-handed tactics, including some that were much more widely applied such as stop and frisk, has found little evidence of resulting crime reductions (24–26). It is therefore crucial to empirically evaluate the assumed benefits of militarized policing.

Data and Methods

To characterize where and why SWAT teams deploy, I obtained data on every SWAT deployment in the state of Maryland over a 5-y period via a public records request. These data exist because of an unusual statute requiring every Maryland agency to uniformly record all SWAT activity. Because the statute has since sunset, the data represent a rare, complete accounting of militarized police units' activities and contain the date, postal zip code, and agency of each SWAT deployment between FY2010 and FY2014, as well as the reasons for and outcomes of each deployment ($n \approx 8{,}200$ deployments).

To estimate the effects of police militarization on crime and officer safety, I use a nationwide panel measuring the presence of active SWAT teams. I generated the national panel by merging (27) three waves of the federal Census of State and Local Law Enforcement Agencies (CSLLEA) surveys—which measure whether agencies supply SWAT services—with FBI data on violent crimes and the FBI's Law Enforcement Officers Killed and Assaulted (LEOKA) database. The resulting panel includes roughly 9,000 agencies, each observed in 2000, 2004, and 2008. About 29% of agencies vary on SWAT status during this period (*SI Appendix*, Table S2). I then use an agency-month panel in Maryland to test whether the increased deployment of SWAT teams affects crime and officer safety outcomes.

To estimate the effects of militarized policing on public perceptions of law enforcement, I conducted two survey experiments: one using a convenience sample from Amazon's Mechanical Turk ($n=$ 1,566) and one conducted by Survey Sampling International (SSI) ($n=$ 4,465). (Survey experiments were approved by Stanford University's Institutional Review Board, protocol no. 32534. All respondents supplied informed consent.) These experiments provided brief news articles accompanied by a randomly assigned image that conveyed different levels of militarization, allowing for an estimate of the causal effect of seeing militarized police on attitudes toward law enforcement relative to seeing more traditionally equipped police forces. Because of the prevalence of SWAT deployments in Black communities, the SSI data include an oversample of roughly 1,850 African Americans to test whether treatment effects varied with respondent race.

Where and Why SWAT Teams Deploy

The rare census of SWAT activity available in Maryland offers a valuable opportunity to study the stated reasons for, and geographic correlates of, militarized policing. Maryland also exhibits large variation in the racial composition of localities [e.g., the percentage of Blacks in zip-code tabulation areas (ZCTAs) ranges from 0% to more than 95%], allowing for an evaluation of the relationship between militarized tactics and neighborhood racial composition with minimal extrapolation.

Before examining that relationship, Table 1, generated using the pooled Maryland SWAT census, first displays the reasons SWAT teams are deployed. Table 1 shows that roughly 90% of SWAT deployments in that state over 5 fiscal years were conducted to serve search warrants. Previous work has shown that the use of SWAT teams to serve warrants, a practice which escalated as a result of the war on drugs (17), is an extremely disruptive event in the lives of citizens and often involves percussive grenades, battering rams, substantial property damage, and in rare cases deadly altercations stemming from citizens' mistaken belief that they are experiencing a home invasion (28, 29). Table 1 also shows that less than 5% of deployments involved a "barricade" scenario, which typically involves an armed suspect refusing to surrender to police. Violence to people and animals is rare, and gun shots are fired 1.2% of the time—roughly 100 deployments during this period. While the data suggest that indiscriminate violence is less common than some anecdotal reports suggest, they also show that the vast majority of SWAT deployments occur in connection with nonemergency scenarios, predominately to serve search warrants.

Table 1. SWAT deployments, Maryland FY2010–2014

Reason, legal authority	%	Outcome	%
Search warrant	91.06	Property taken	84.38
Barricade	4.92	Forcible entry	68.36
Other	2.67	Arrest	63.69
Arrest warrant	0.89	Shot fired	1.20
Exigent circumstances	0.45	Person injured	1.15
		Officer injured	0.42
		Animal killed	0.17
		Animal injured	0.12
		Person killed	0.11

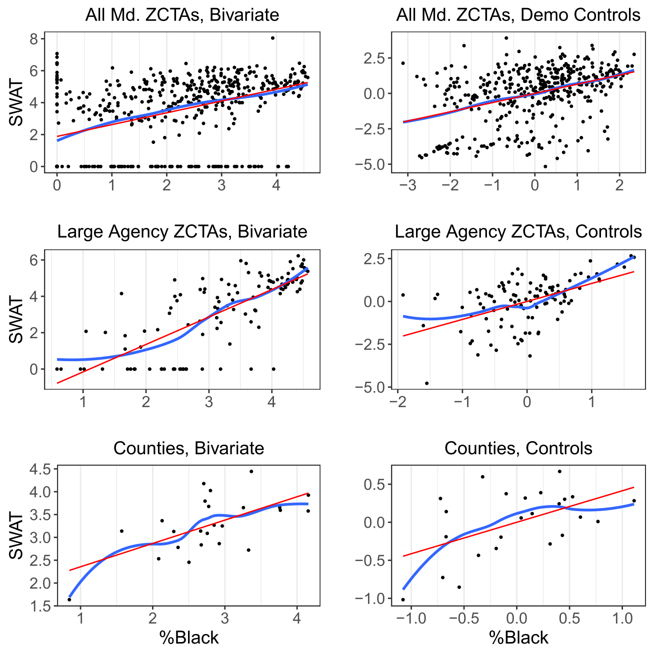

Fig. 1. SWAT deployments are more frequent in areas with high concentrations of African Americans. (*Top* row) Maryland ZCTAs. (*Middle* row) ZCTAs from three large Maryland agencies. (*Bottom* row) Maryland counties. *Left* column shows bivariate relationships. *Right* column controls for social indicators in corresponding geographic unit. Locally weighted and linear regression fits are shown. Variables are plotted on log scales.

Critics allege that militarized tactics are more often applied in communities of color, a pattern that would be in line with decades of evidence indicating disparate treatment of racial minorities by police (4, 6, 30). To test this claim, Fig. 1 shows the relationship between the percentage of Black residents in a geographic unit and the volume of SWAT deployments per 100,000 residents in that unit during FY2010–2014, both logged. Fig. 1, *Top Left* and *Top Right* is generated using ZCTAs in Maryland and shows a strong positive correlation, even after controlling for local unemployment, education, and household income levels (Fig. 1, *Top Right*). Fig. 1, *Middle Left* and *Middle Right* is generated using data from three large agencies—the Baltimore City Police Department and the Prince George's County and Montgomery County Police Departments—which publish incident-level crime data that can be mapped to ZCTAs. Even after controlling for the aforementioned social indicators and local crime rates (Fig. 1, *Middle Right*), the percentage of Blacks still strongly predicts the volume of SWAT activity in a ZCTA. The slope in Fig. 1, *Middle Right* ($\beta = 1.05$, $P < 0.001$; *SI Appendix*, Tables S8 and S9) implies that a 10% increase in the percentage of Blacks in a ZCTA is associated with a 10.53% increase in SWAT deployments per 100,000 residents during this period. Fig. 1, *Bottom Left* and *Bottom Right* shows a similar pattern at the county level, although the slope is not statistically significant in the county model with controls. This analysis comes from only one state, so caution is warranted when generalizing beyond Maryland. We also cannot confidently infer a causal relationship from these cross-sectional comparisons. However, these results are consistent with the descriptive claim that Black residents face a pronounced risk of experiencing militarized policing.

Effects on Crime and Officer Safety

Proponents claim that militarized tactics deter violent crime and protect police (10). To test these claims, I use a nationwide panel to estimate the effects of acquiring a SWAT team on violent crime and officer fatalities and assaults and an agency-month panel in Maryland to test whether the increased deployment of SWAT teams affects these outcomes. I focus on violent crime because of the asserted link between militarized policing and public safety. To gauge robustness, I estimate two models per outcome: a generalized difference-in-differences (DID) estimator containing agency and time period fixed effects and a second model including fixed effects but also allowing each agency to trend uniquely over time. By comparing agencies to themselves over time, both approaches greatly mitigate concerns over omitted variable bias that plague cross-sectional comparisons (31). All outcomes are logged to reduce the influence of extreme values (see *SI Appendix*, sections 2–4 for alternative specifications).

Fig. 2 displays estimates of the effect of obtaining a SWAT team on violent crime and officer safety measures. The top coefficient implies a statistically significant 6.5% increase in within-agency violent crimes, on average. This result is consistent with anecdotal evidence of suspects reacting violently to SWAT teams (29) or with militarized policing lowering trust in police, thereby hindering criminal investigations and promoting crime (14). However, once we allow agencies to trend differentially over time, the effect shrinks to a statistically insignificant 4.3%. Estimates for officer deaths, both accidental and felonious, are precise and near zero, partly because they are so rare (i.e., there is little variation in these outcomes to explain). Estimates for assaults are noisier, and one shows a statistically significant 3.2% increase in noninjurious assaults, although this result misses statistical significance in the time trends specification. In sum, estimated effects are generally positive and often indistinguishable from zero, and there is no evidence that acquiring a SWAT team lowers crime or promotes officer safety.

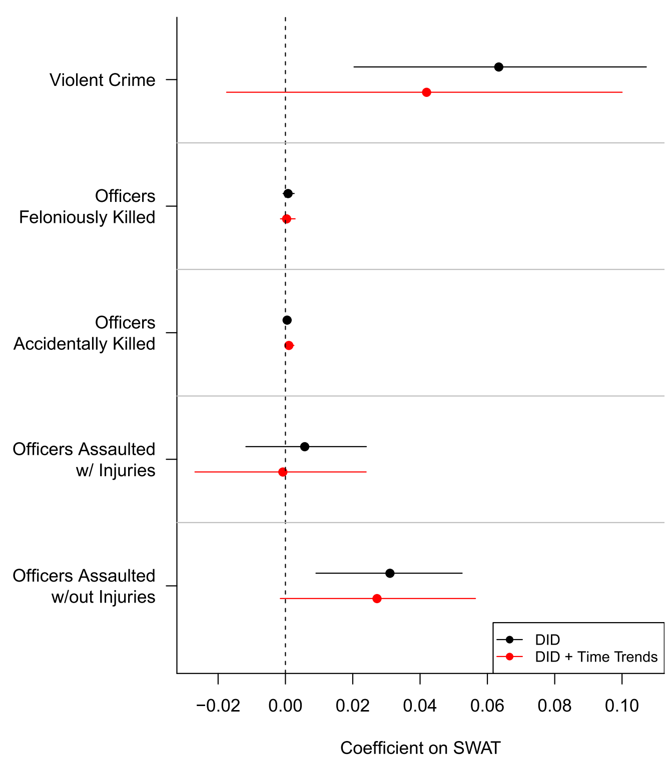

Fig. 2. Obtaining a SWAT team does not reduce crime or improve officer safety. Shown are coefficients from regressions estimating the effect of obtaining a SWAT team on crime and officer safety. Outcomes are logged. Bars are 95% confidence intervals.

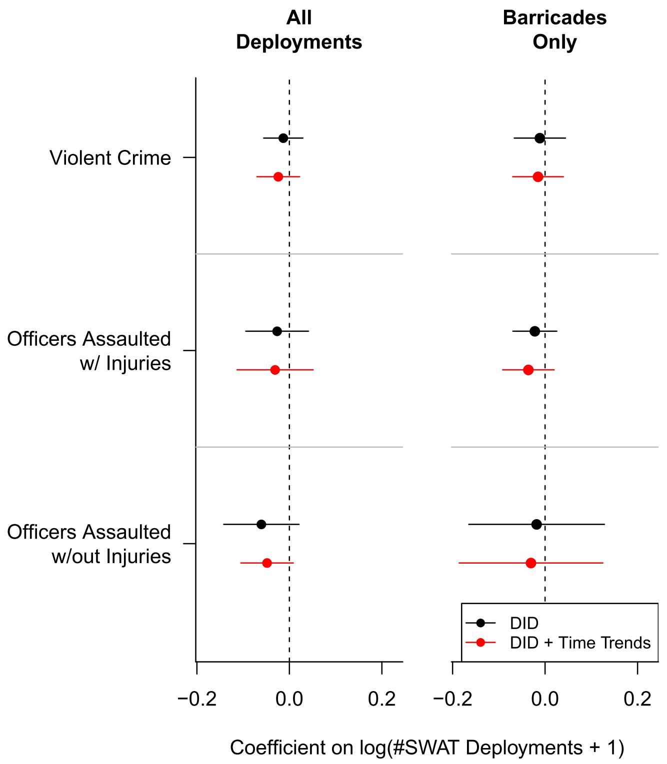

Fig. 3. SWAT team deployments exert no detectable effects on crime or officer safety. Shown are coefficients on *log*(#SWAT Deployments + 1) from panel regressions using data from Maryland, FY2010–2014. All outcomes are logged. Bars are 95% confidence intervals.

Fig. 3 displays the effects of increased SWAT deployments in Maryland on crime and officer safety. Fig. 3, *Left* shows results from models using all deployments, summed within agency months and logged, while Fig. 3, *Right* uses deployments only for barricade situations to test whether using SWAT in emergencies might be especially effective. (I thank an anonymous reviewer for this suggestion.) (Officer fatalities are omitted as an outcome here because they almost never occur in these data; *SI Appendix*, Table S7.) As Fig. 3 shows, point estimates are negative but near zero, none are statistically significant, and all estimates imply a 10% increase in deployments exerts changes smaller than 1% in all outcomes. Some small declines in officer assaults are detectable if the data are weighted by agency size (*SI Appendix*, Table S35), suggesting the Maryland results are more sensitive to model specification than the national analysis which makes it difficult to draw firm conclusions. Using the available data, the benefits of increased deployments appear to be either small or nonexistent.

The adoption and deployment of SWAT teams is not randomly assigned, and we should therefore make causal interpretations with some caution (see *SI Appendix*, section 2B, for placebo tests assessing the risk of endogeneity). The Maryland results may also not generalize to other states. But the lack of any robust association between militarized policing and public and officer safety shown here calls the validity of these claimed benefits into question.

Effects on Police Reputation

I conducted two survey experiments to estimate effects on public perceptions of police, the first one via Amazon's Mechanical Turk (M-Turk) and the second one via SSI. Respondents read a mock news article concerning an unnamed police chief seeking a budget increase. Text remained constant while the accompanying image randomly varied across respondents (Fig. 4). Images were randomly assigned within racial groups of respondents in the second survey (which contained an oversample of African American respondents), with equal probability. The control image (Fig. 4*A*) featured five male, traditionally uniformed officers (e.g., blue uniforms, brimmed caps, and standard side arms). The "low" militarization conditions (Fig. 4*B* and Fig. 4*C*) showed five male officers with "riot gear" and batons or with body armor and assault rifles. The "high" militarization condition (Fig. 4*D*) added an armored vehicle. (Because it elicited very similar treatment effects to the other low militarization image in Fig. 4*C*, the Fig. 4*B* image was dropped from the SSI survey to enhance statistical power.)

In each case, the caption beneath the photo read, "Above: Five city police officers stand guard during a local protest." All images were tightly cropped to ensure that any differences in responses were due to the appearance of police officers and not the surrounding area in which they were deployed. Following the article, respondents answered questions measuring perceived crime levels, support for police spending, and confidence in police. All images and text were borrowed or adapted from real online news content (32–36).

Experimental Results. Fig. 5 shows average differences in responses between each treatment condition and the control condition in the unweighted samples. All outcomes were rescaled to range from 0 to 100 so effects can be interpreted as percentage-point increases on each outcome's respective scale. Militarized images in the M-Turk survey caused clear increases between roughly 8 points and 15 points in perceived levels of crime in the vignette city. The high militarization condition in the SSI survey caused a statistically significant 2.2-point increase in the perceived level of crime in the vignette city and, strikingly, a 3.2-point drop in respondents' desire for more police patrols in their own neighborhoods.

The high militarization treatment also caused support for police funding in the United States to fall by roughly four points in the M-Turk survey and two points in the SSI survey. Support

Fig. 4. Experimental manipulation [from *Top Left* (*A*) to *Bottom Right* (*D*)]. Reprinted with permission from Reuters Pictures/Jessica Rinaldi, Shutterstock/JPL Designs, Associated Press/Jeff Roberson, and The New York Times/Redux Pictures/Whitney Curtis.

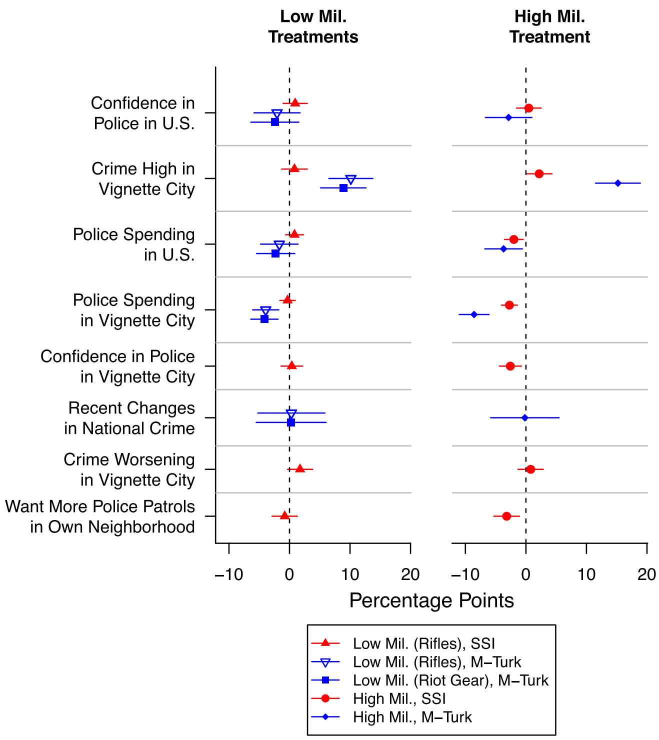

Fig. 5. Survey experimental results: seeing militarized police may tarnish police reputation. Shown are effects of militarized images in M-Turk (blue) and SSI (red) surveys. Bars are 95% confidence intervals.

for funding the department in the news article also fell. A close reading and text analysis of open-ended responses suggest that treated respondents were less supportive of police funding because militarized equipment gave the impression that the agency was already well funded (see *SI Appendix*, Fig. S10 and discussion in *SI Appendix*, section 2E).

Effects on confidence in police in the United States are generally negative but not statistically significant, although the high militarization treatment in the SSI survey did lower confidence in the police portrayed in the experimental vignette, suggesting some of militarized images' effects may be confined to the agency in question and not law enforcement at large. The effects of the high militarization treatment are also substantially smaller in the SSI sample, and the low militarization treatment effects from the M-Turk sample did not replicate, so we should be cautious about endorsing the large effect sizes from the M-Turk survey. Despite these limitations, the SSI results generally replicate the M-Turk results in terms of direction, and all effects appear after only a single brief exposure to militarized images. Repeated exposure to similar news items over time could help to cement negative views of law enforcement in the mass public.

Effects by Race of Respondent. Fig. 6 shows treatment effects in the SSI survey estimated on non-Hispanic white and African American respondents (with respondent race/ethnicity measured via self-reports) separately, as well as the difference in effects. In some cases the group effects have opposite signs, such as the effects of high militarization on perceived crime in the vignette city, but this difference is not statistically significant. In general, these results reveal little evidence of heterogeneous responses. Larger samples may allow for the detection of disparate effects, but the small point estimates in Fig. 6 suggest that any differences in effects are likely to be small in magnitude.

This relatively uniform pattern of response is surprising given extensive prior work indicating disparate treatment of Black Americans by police and depressed levels of trust in police in communities of color. Indeed, in the control condition in the SSI data, when asked how much confidence they had in police in the United States, the mean response among Black participants was 21 percentage points lower than among white respondents ($P < 0.001$). Why then, do we observe such similar effects among the two groups? One explanation is that different mechanisms are operating within each group to produce effects of similar magnitude. For example, white respondents may react negatively to militarized images because they clash with their baseline perception of law enforcement, while Black residents may react negatively because they conjure memories of discrimination. These results are also consistent with previous work showing that perceived discrimination is not well predicted by the increased probability of exposure to racial hostility (37). Adjudicating between these explanations is beyond the scope of this study. But what we learn from the present analysis is that militarized policing can impose reputational costs on law enforcement, likely in unintended ways. This is troubling, since prior work shows that negative views of police inhibit criminal investigations and are associated with stunted civic participation (38).

Discussion and Conclusion

Aggressive policing strategies have historically been disproportionately applied to citizens of color in ways that serve to preserve race- and class-based social hierarchies (3). The normalization of militarized policing in the United States (15, 16) has raised concerns that a new, heavy-handed policing strategy is being used in similar ways and is eroding public opinion toward law enforcement, but law enforcement administrators defend the tactics claiming they can deter violent crime and protect police. This study marshals an array of data sources and analytical techniques to systematically evaluate these claims.

Consistent with anecdotal evidence (11), militarized police units are more often deployed in areas with high concentrations of African Americans, even after adjusting for local crime rates

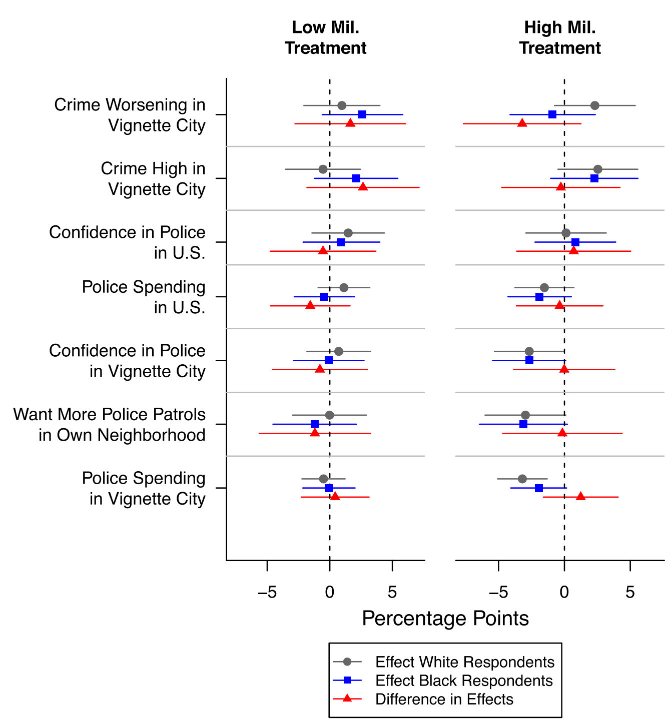

Fig. 6. No heterogeneous effects by race of respondent. Bars are 95% confidence intervals.

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and other community traits. But I find no firm evidence that SWAT teams lower an agency's violent crime rate or the rates at which officers are killed or assaulted. Using survey experiments, I show that citizens react negatively to the appearance of militarized police units in news reports and become less willing to fund police agencies and less supportive of having police patrols in their own neighborhoods.

Given the concentration of deployments in communities of color, where trust in law enforcement and government at large is already depressed (14, 38), the routine use of militarized police tactics by local agencies threatens to increase the historic tensions between marginalized groups and the state with no detectable public safety benefit. While SWAT teams arguably remain a necessary tool for violent emergency situations, restricting their use to those rare events may improve perceptions of police with little or no safety loss.

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Correction

POLITICAL SCIENCES

Correction for "Militarization fails to enhance police safety or reduce crime but may harm police reputation," by Jonathan Mummolo, which was first published August 20, 2018; 10.1073/pnas.1805161115 (*Proc. Natl. Acad. Sci. U.S.A.* **115**, 9181–9186).

The authors note that: "This correction addresses recently discovered code errors that affect one section of this paper's analysis. Fortunately, after correcting these errors, reported results are not substantively altered. All corrected results also remain statistically significant ($P < 0.05$). Fig. 1 analyzes predictors of SWAT team deployments in Maryland. A recent code review showed crime data for one of the three jurisdictions was incorrectly merged. This affects two reported results. The first is displayed graphically in Fig. 1 (*Middle* row, *Right* panel), a zip-code (ZCTA)-level analysis that adjusts for covariates including local crime rates (corresponding numeric results in *SI Appendix* Table S8, column 4). The originally published coefficient on racial composition, discussed in the main text, was 1.05, and the revised coefficient after fixing the merge error is 1.08. The predicted increase in SWAT deployments for a 10% increase in %Black, also discussed in the main text, changes from 10.53% to 10.85% after the correction. The second affected result appeared in *SI Appendix* Table S9, column 4, which is the same analysis with population weights added. Here, the coefficient of interest remained 0.85, but the standard error changed from 0.19 to 0.18. In addition, the weights were incorrectly specified in the model corresponding to *SI Appendix* Table S9, column 1. Correcting this error changes the previously reported coefficient on %Black from 0.74 (SE = 0.07) to 0.60 (SE = 0.05)." The corrected Fig. 1 appears below. The *SI Appendix* has been updated online to show the corrected Tables S8 and S9.

CORRECTION

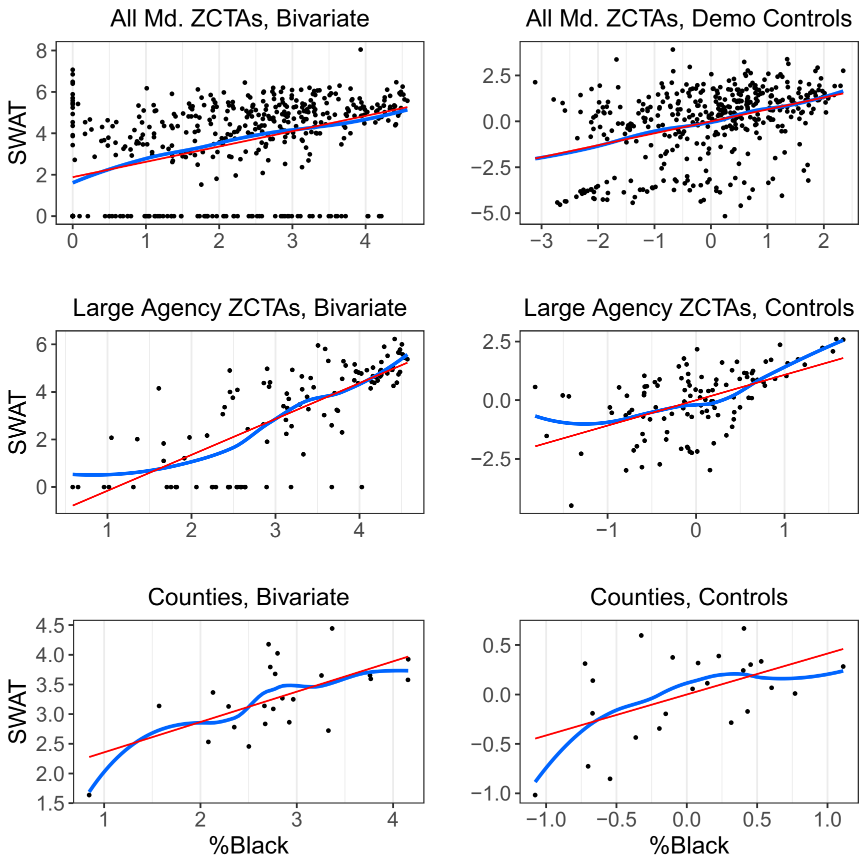

Fig. 1. SWAT deployments are more frequent in areas with high concentrations of African Americans. (*Top* row) Maryland ZCTAs. (*Middle* row) ZCTAs from three large Maryland agencies. (*Bottom* row) Maryland counties. *Left* column shows bivariate relationships. *Right* column controls for social indicators in corresponding geographic unit. Locally weighted and linear regression fits are shown. Variables are plotted on log scales.

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

Jonathan Mummolo

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

Contents

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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:

$$log(Outcome_{i,t} + 1) = \tau SWAT_{i,t} + \theta_i + \gamma_t + \epsilon_{i,t} \quad [1]$$

$$log(Outcome_{i,t} + 1) = \tau SWAT_{i,t} + \theta_i + \gamma_t + \beta_i Agency_i \cdot time + \epsilon_{i,t} \quad [2]$$

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, $\text{SWAT}_{i,t}$ 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:

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

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.

$$log(Outcome_{i,t} + 1) = \tau SWAT_{i,t} + \sigma SWAT_{i,t+1} + \theta_i + \gamma_t + \epsilon_{i,t} \quad [5]$$

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 int he 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 manpow 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.

U.S. Department of Justice
Bureau of Alcohol, Tobacco, Firearms and Explosives

OMB No. 1140-0015 (03/31/2014)

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 2h)*:

☐ Firearm is Unserviceable and is Being Transferred as a Curio or Ornament
☒ Firearm is Being Transferred to or from a Government Entity
☐ Firearm is Being Transferred to a Lawful Heir
☐ Other (Specify) ______

3a. Transferor's Name and Address *(Include trade name, if any) (Executors: 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

3c. If Applicable: Decedent's Name, Address, and Date of Death

3d. Number, Street, City, State and Zip Code of Residence *(or Firearms Business Premises)* If Different from Item 3a.

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 i)*

a. Name and Address of Manufacturer and/or Importer of Firearm	b. Type of Firearm *(See instruction 1c)*	c. Caliber, Gauge or Size *(Specify)*	d. Model		
			M16A1		
			Length *(Inches)*	e. Of Barrel: 20	f. Overall: 39
COLT MANUFACTURING CO INC P. O. BOX 1868 HARTFORD, CT 06144-1868	MACHINEGUN	5.56 MM	g. Serial Number: SEE ATTACHED LISTING		

h. Additional Description or Data Appearing on Firearm *(Attach additional sheet if necessary)*

i. Is the Firearm Unserviceable as Defined in Instruction 1I? Yes ☐ No ☐ *(If "No," describe any other method by which firearm has been rendered unserviceable. Use additional sheets.)*

5. Transferee's Federal Firearms License *(If any)* *(Give complete 15-digit number) (See instruction 2c)*

First 6 digits	2 digits	2 digits	5 digits

6. Transferee's Special (Occupational) Tax Status *(If any)*
a. Employer Identification Number | b. Class

7. Transferor's Federal Firearms License *(If any)* *(Give complete 15-digit number) (See instruction 2e)*

First 6 digits	2 digits	2 digits	5 digits

8. Transferor's Special (Occupational) Tax Status *(If any)*
a. Employer Identification Number | b. Class

Under Penalties of Perjury, I Declare that I have examined this application, and to the best of my knowledge and belief it is true, correct and complete, and that the transfer of the described firearm to the transferee and receipt and possession of it by the transferee are not prohibited by the provisions of Chapter 44, Title 18, United States Code; Chapter 53, Title 26, United States Code; or Title VII of the Omnibus Crime Control and Safe Streets Act, as amended; or any provisions of State or local law.

9. Consent to Disclosure of Information to Transferee *(See instruction 8)*. I Do or Do Not *(circle one)* Authorize ATF to Provide Information Relating to this Application to the Above-Named Transferee.

10. Signature of Transferor *(Or authorized official)*

11. Name and Title of Authorized Official *(Print or type)*
Gregory R. Zembsch / Sergeant

12. Date
6-27-14

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 Interstate Movement of that Firearm, When Applicable, to the Transferee are:

☐ Approved *(With the following conditions, if any)*

☐ Disapproved *(For the following reasons)*

Signature of Authorized ATF Official | Date

ATF Form 5 (5320.5)
Revised November 2004

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.

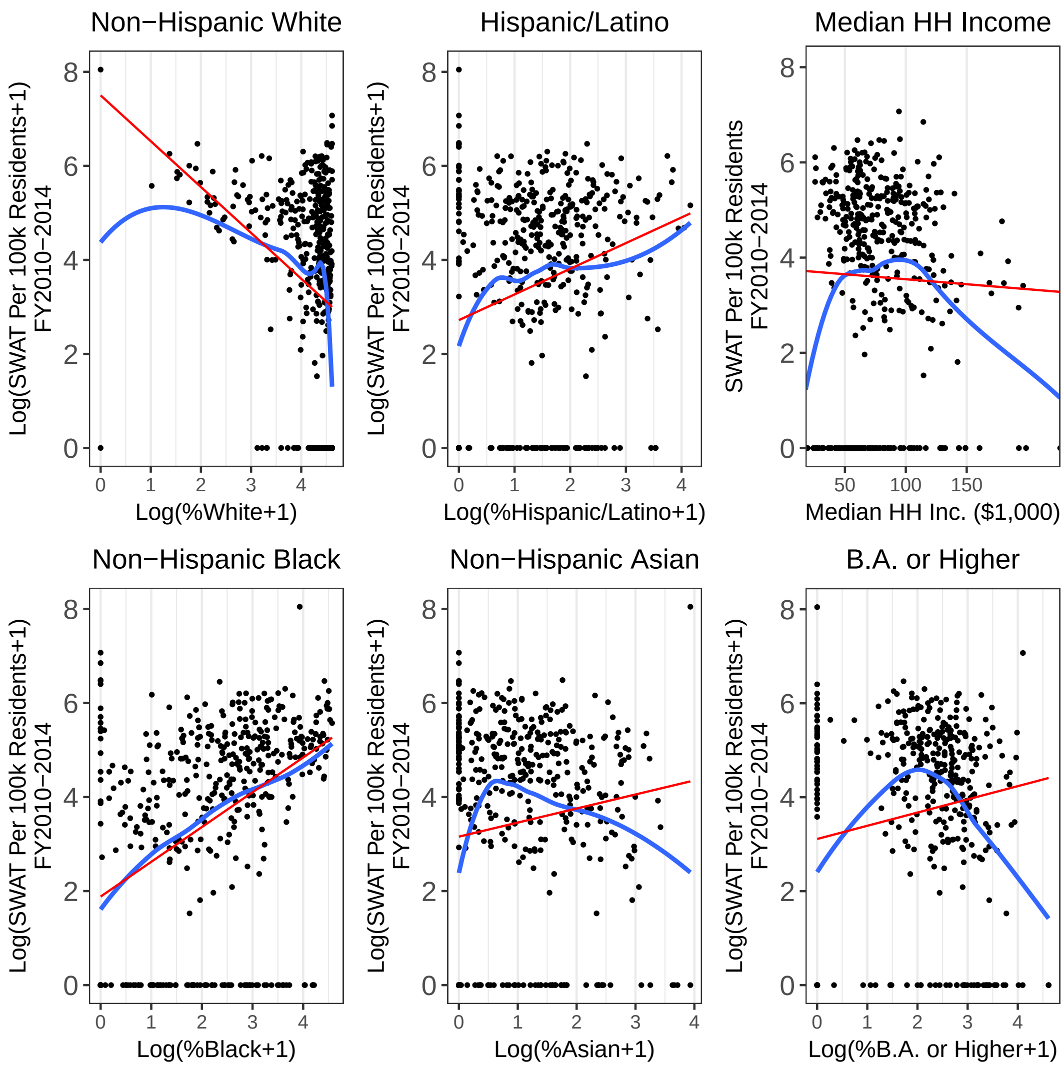

Fig. S2. SWAT Deployments in Maryland ZCTAs. ZCTA-level demographics in Maryland by volume of SWAT deployments, FY2010-2014. LOESS and OLS fits.

C. Placebo Tests With “Lead” SWAT Treatments.

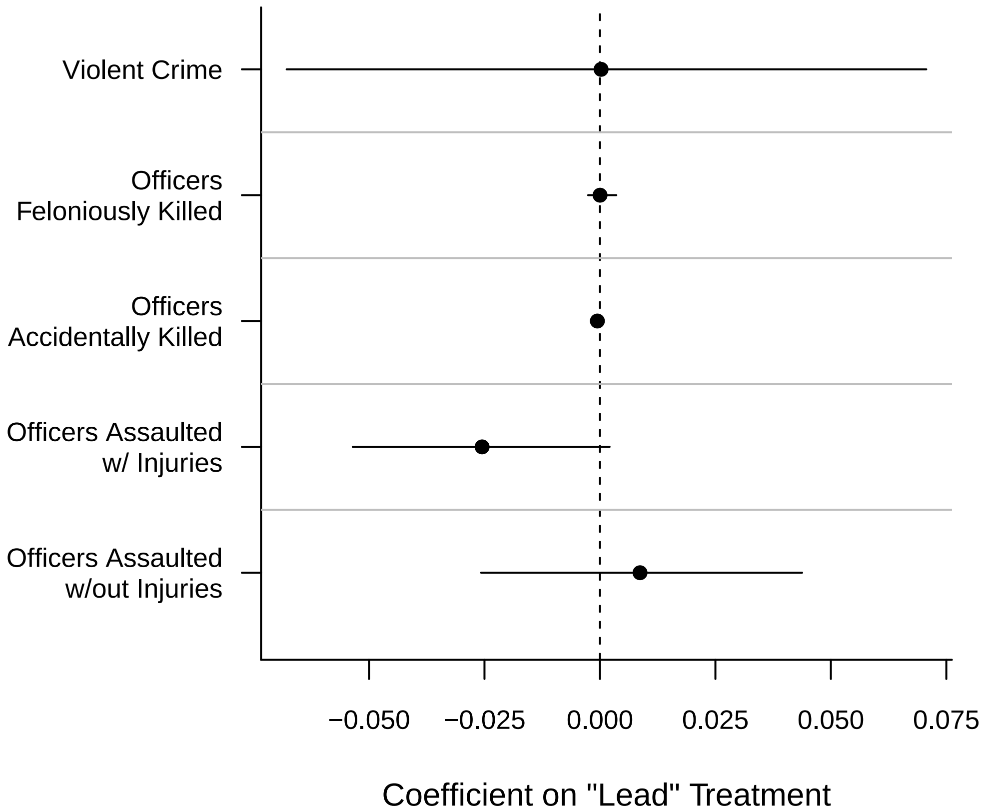

Fig. S3. Placebo test: Future SWAT Status Cannot Predict Current Outcomes in National SWAT Panel. Effects of obtaining a SWAT Team on crime and officer safety; placebo test using "lead" treatment indicators. Dots denote point estimates, bars denote 95% CIs. Future treatment status cannot predict contemporaneous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

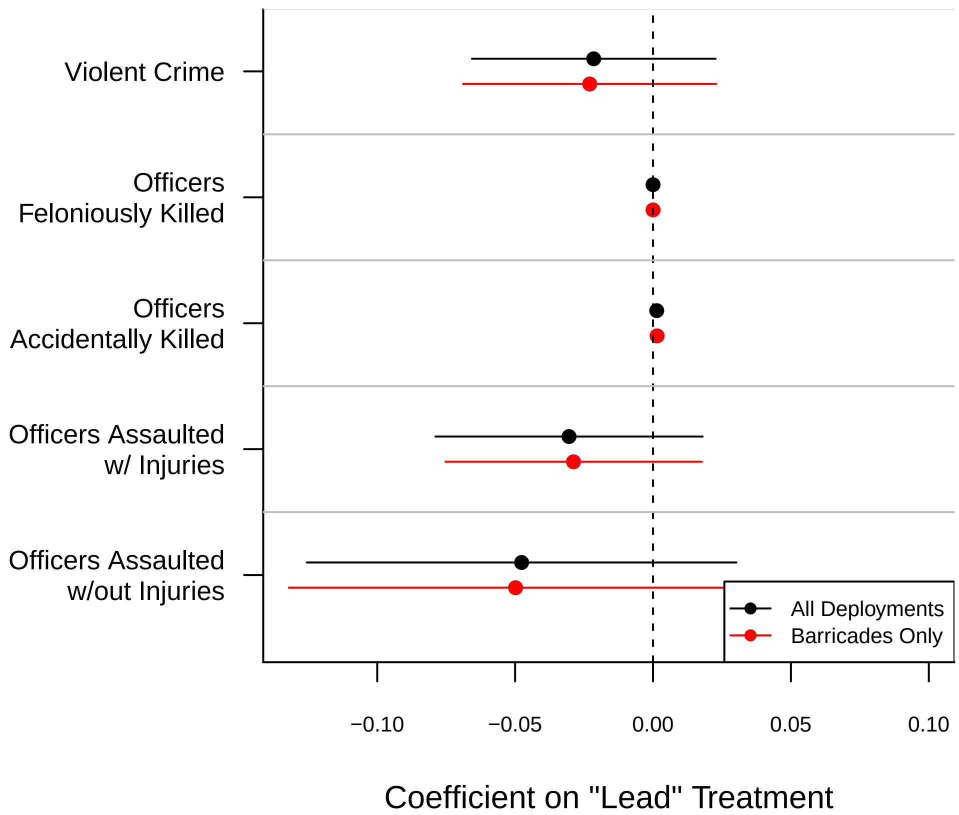

Fig. S4. Placebo Test: Future SWAT Deployments Cannot Predict Current Outcomes In Maryland SWAT Panel. Effects of deploying a SWAT Team on crime and officer safety in Maryland, FY2010-2014; placebo test using “lead” treatment indicators. Dots denote point estimates, bars denote 95% CIs. Future treatment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

D. Sensitivity to Dropping Agencies.

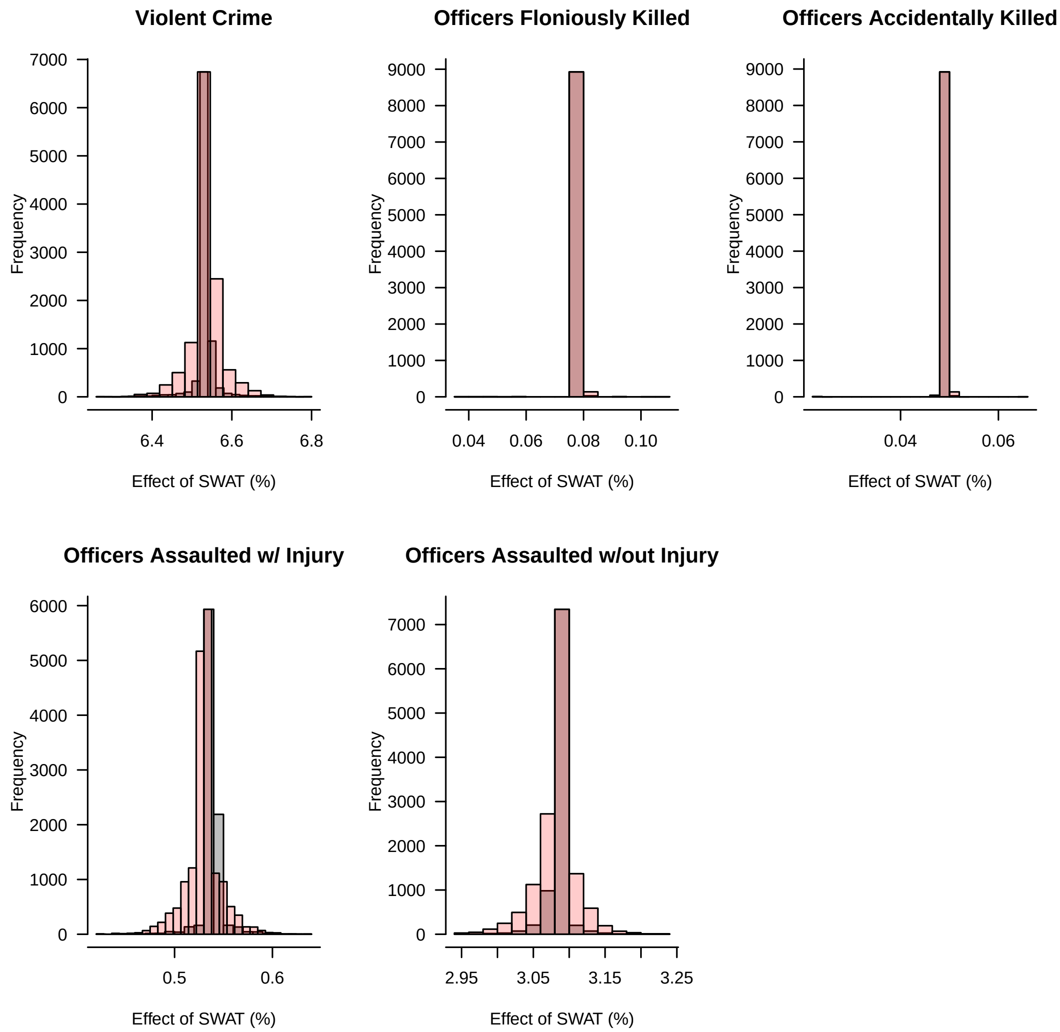

Fig. S5. Sensitivity Analysis, DID Model, National SWAT Panel. To test whether particular agencies are driving results, perhaps due to measurement error, the plot shows the distributions of estimates obtained for each outcome when dropping one agency's data at a time (gray bars), and when dropping a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

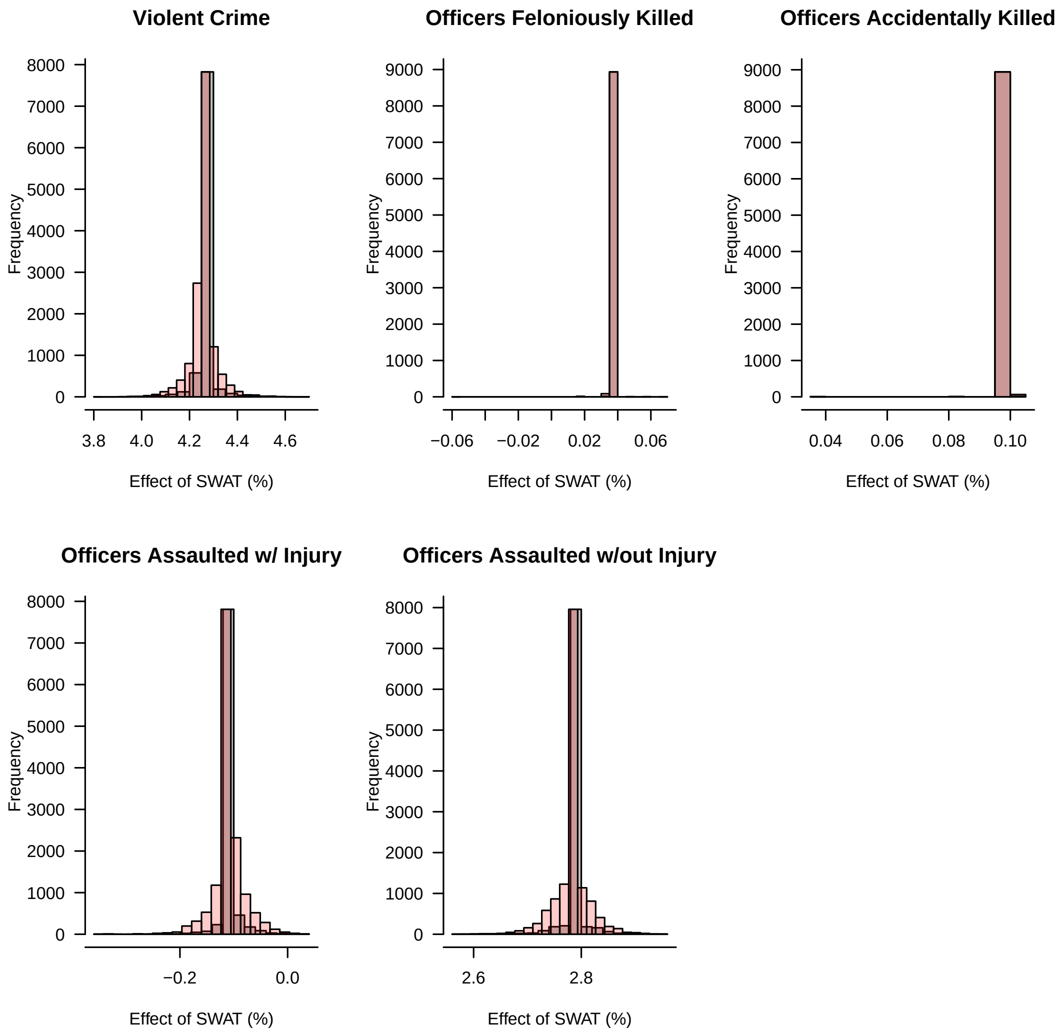

Fig. S6. Sensitivity Analysis, Agency FE + Time Trends Model, National SWAT Panel. To test whether particular agencies are driving results, perhaps due to measurement error, the plot shows the distributions of estimates obtained for each outcome when dropping one agency's data at a time (gray bars),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

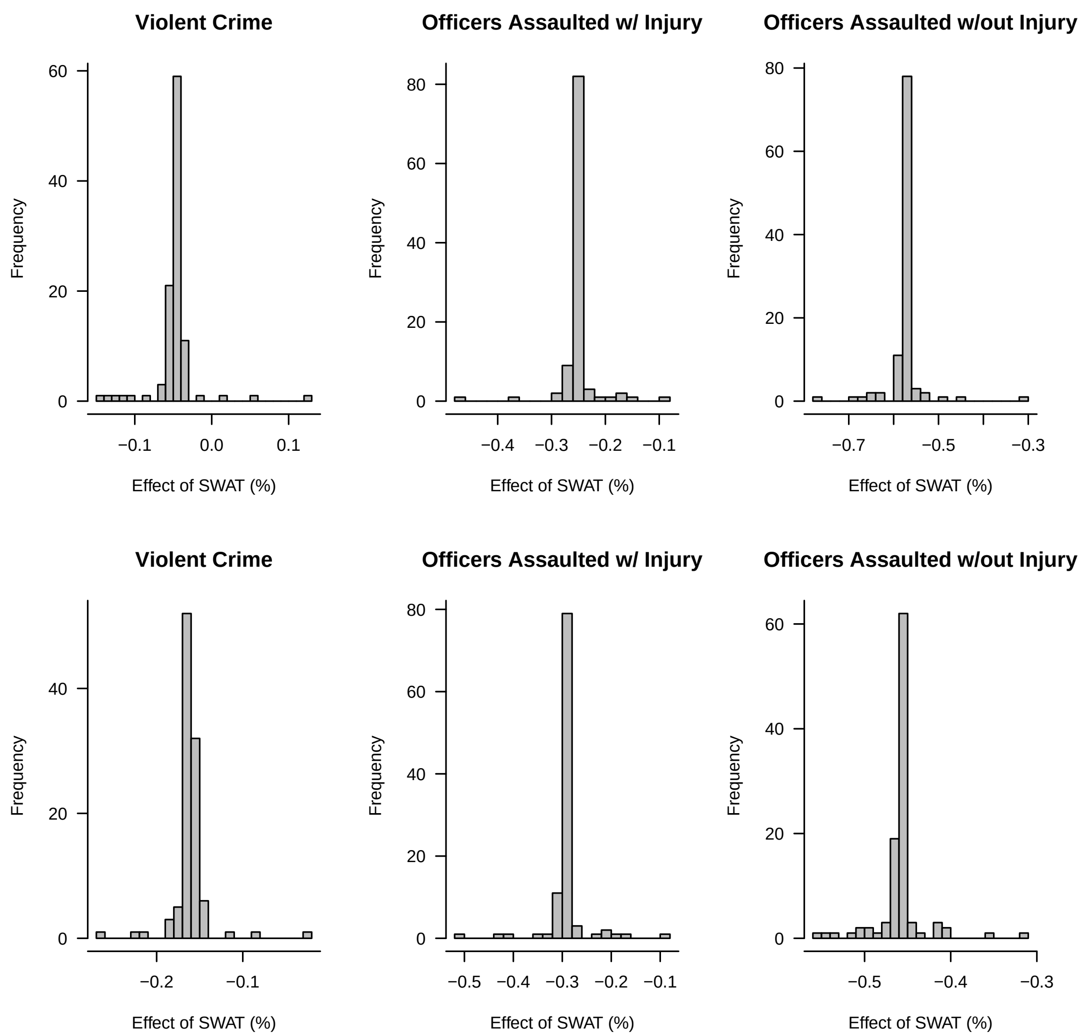

Fig. S7. Sensitivity Analysis, Maryland SWAT Deployment Panel. To test whether particular agencies are driving results, perhaps due to measurement error, the plot shows the distributions of estimates (effect of a 10% increase in deployments) obtained for each outcome when dropping one agency's data at 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.

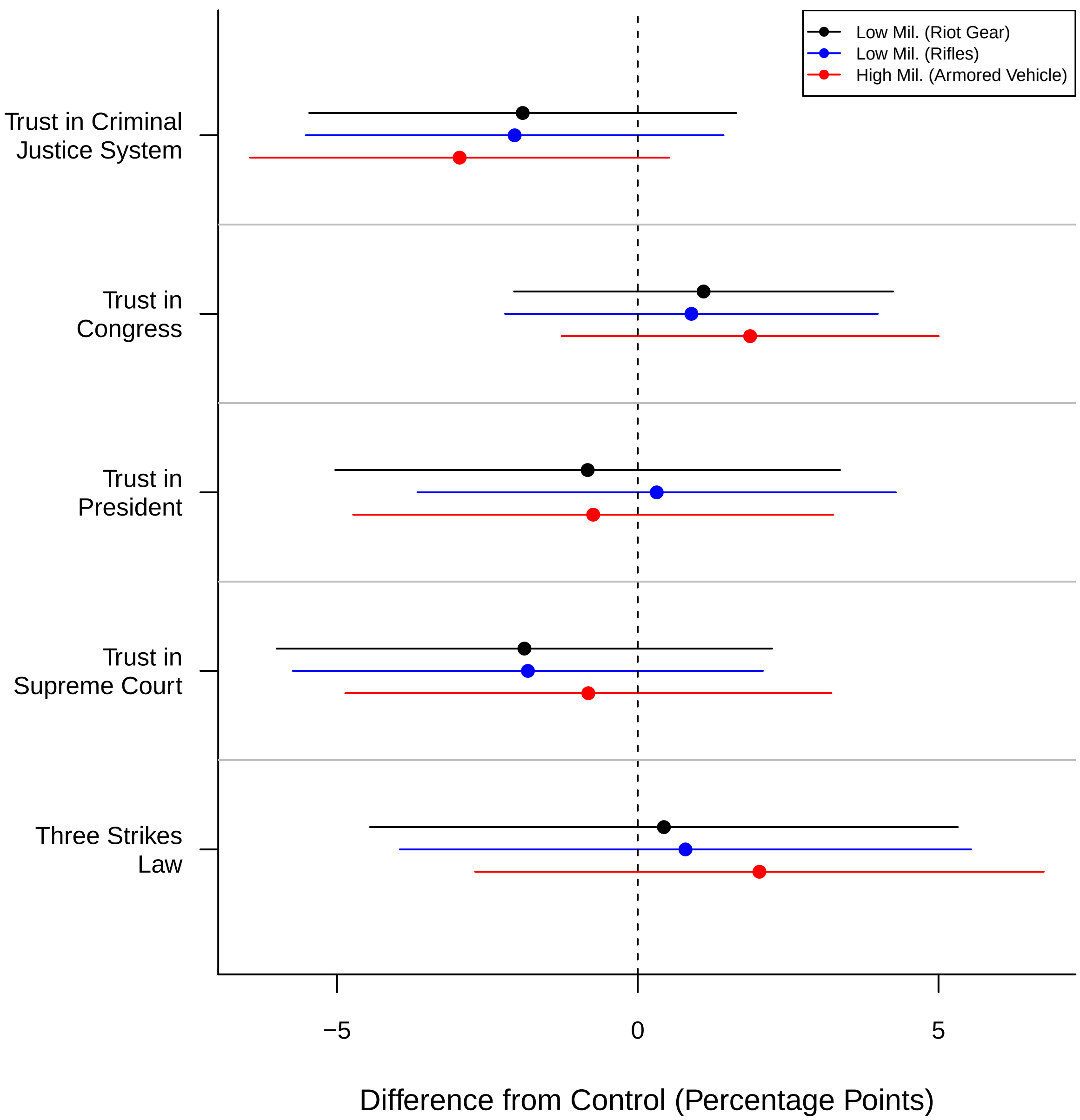

Fig. S8. Treatment Effects on Additional Dependent Variables in M-Turk Sample. These dependent variables were not included on the SSI survey. Bars denote 95% confidence intervals.

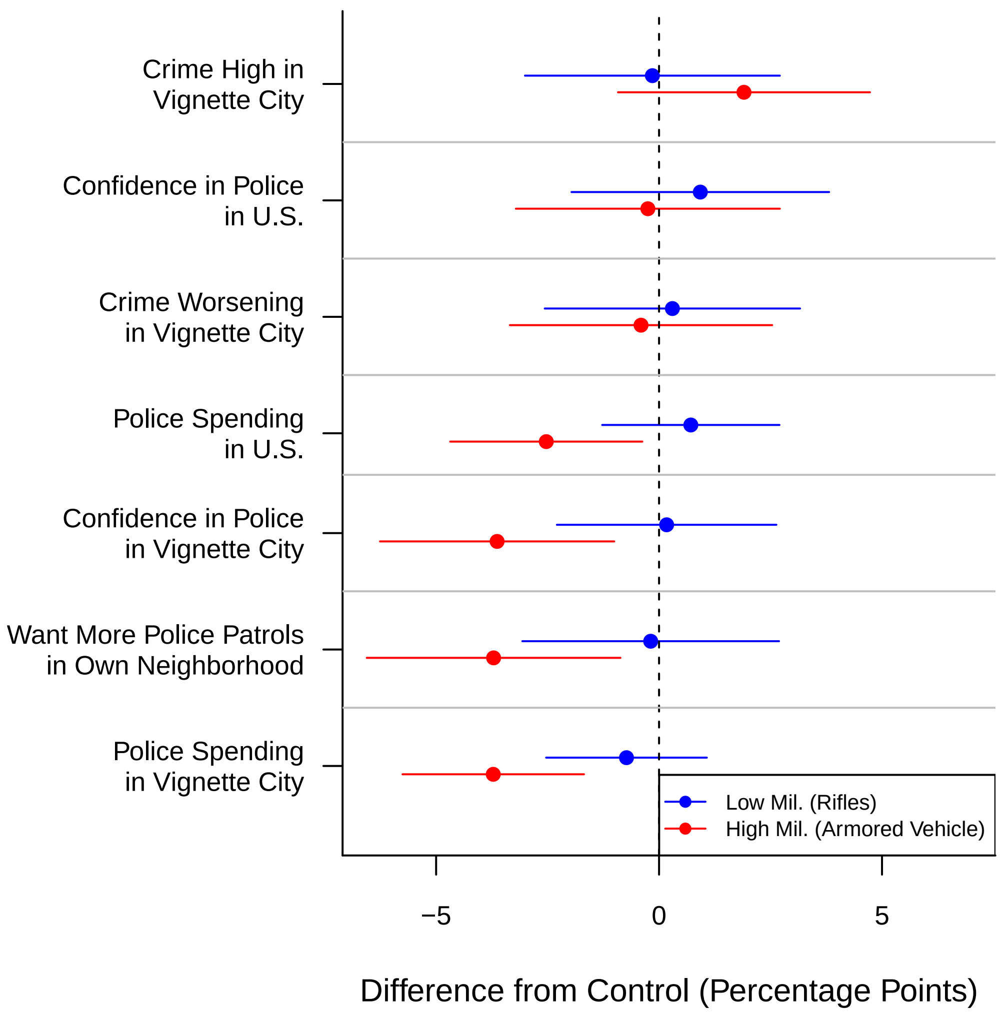

Fig. S9. Population-weighted Treatment Effects. Results of the SSI experiment after re-weighting the data to be nationally representative in terms of race, gender and party according to proportions in the population-weighted 2016 Cooperative Congressional Election Study (66).

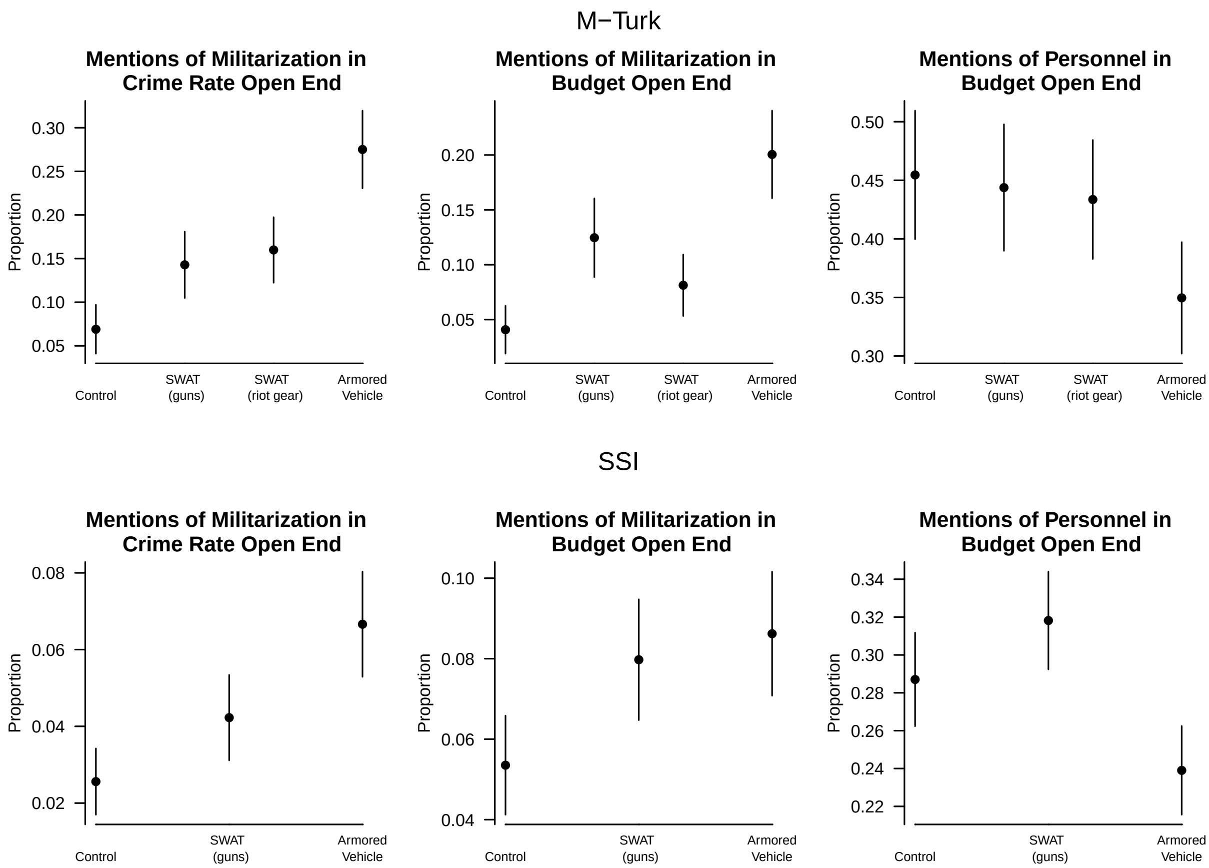

Fig. S10. Open End Response Dictionary Analysis. Words relating to militarization are much more prevalent among treated respondents. Words relating to personnel shortages are much more prevalent among control respondents.

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)
N	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)
N	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)
N	4,702	4,702	4,702	1,563	1,563	1,563	1,563
F-stat	1.984	1.585	1.254	0.568	1.644	1.644	1.602
p-value on F-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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