Police Demilitarization and Violent Crime

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Abstract

Policymakers and advocates make contradictory claims about the effects of providing military equipment for local law enforcement. But this intervention is not well understood because of severe data limitations and inferential challenges. I use 3.8 million archived inventory records to estimate the magnitude of sources of bias in existing studies of the 1033 program. I show most variation in militarization comes from previously unobserved sources, which implies studies that show crime-reduction benefits are unreliable. I then leverage recent policy changes to evaluate the effect of military equipment: the Obama administration recalled property under Executive Order 13688, which resulted in a forced demilitarization of several hundred departments. Difference-in-difference estimates of agencies that retained similar equipment show negligible or undetectable impacts on violent crime or officer safety. These findings do not suggest similar scale federal reforms designed to demilitarize police would have the downside risks proposed by proponents of military transfers.

Introduction

Federal transfers of billions in military equipment to local law enforcement fuel an ongoing debate about the "militarization" of police.[1,2,3,4] Proponents refer to this equipment as "life-saving"[5] and critical to fighting violent crime. Announcing the reversal of Obama-era restrictions in August of 2017, Jeff Sessions said, "studies have shown this equipment reduces crime rates, reduces the number of assaults against police officers, and reduces the number of complaints against police officers." Meanwhile, opponents suggest the equipment is to blame for excessive use-of-force by police and that its acquisition is motivated by racial threat.[6,7]

But systematic evidence evaluating these claims faces severe data limitations and inferential challenges. Numerous studies in circulation link military equipment to improved law enforcement outcomes.[8,9,10,11,12] Others link military equipment to worse law enforcement outcomes.[8,11,13] In a replication of two published studies, Gunderson et al. show the previous claims that suggest militarization reduces crime are not robust to agency-level analysis and alternative calculations of key instrumental variables.[14]

More fundamentally, the publicly released data—on which all past work based—is subject to significant error. These data omit military equipment returned or destroyed before the first available records released in 2014, and conflate inter-agency transfers with new distributions. The severity of this error led Mummolo[15], for example, to investigate the impact of the deployment of SWAT teams as an alternative measure of militarization. SWAT teams themselves have also received considerable scholarly attention.[16,17] Nonetheless, transfers of military weapons and equipment remain a highly contested aspect of policing in the U.S., as well as the programmatic feature of police militarization most subject federal-level reforms.

This study reports estimates of the scale of error in past research on the effects of the 1033 program. I leverage all active inventories of military equipment under the program from 2014-2020, which were obtained via Freedom of Information Act (FOIA) request (Supplementary Tables 1 and 2). Comparing within-agency inventories over time makes parsing attrition, inter-agency transfers, and new distributions possible. I find attrition within law enforcement inventories is common—about 1 in 5 weapons are returned or destroyed within 5 years—and that it is associated with some policing outcomes. This implies the estimated effects of equipment on crime are biased in all previous research that transforms one publicly available active inventory into a time series. As of May 4, 2020, there were at least 17 such studies (10 published in peer-reviewed journals) publicly available in the fields of economics, political science, public policy, and policing. They have been cited 154 times according to Google Scholar, and the earliest was produced in 2015. The peer-reviewed work has also been cited by journalists and policymakers, and mostly includes studies that find evidence in support of violence reduction claims.

To avoid this bias, I then use recent regulatory changes to the 1033 program to estimate the impact of demilitarization on violent crime and officer safety. Public criticism of the program in 2015 led the Obama administration to recall select property under Executive Order 13688.[18] Particular items were selected for recall and shipped, free of charge, back to federal processing facilities or otherwise destroyed (see Supplementary Table 8). In effect, this forced the demilitarization of agencies who held this equipment, while allowing others to retain equipment criticized by groups opposed to the program as operationally equivalent. By identifying these items and tracking their attrition, I show that these orders were implemented effectively by administrators and led to a sudden demilitarization that was out of the hands of law enforcement agencies (LEAs; see Figure 3, Supplementary Tables 4 and 5, and Supplementary Figure 5).

I leverage this sudden change to estimate difference-in-differences (DiD) by comparing over-time trends in crime and officer safety across demilitarized LEAs and unaffected LEAs matched on a set of pre-order characteristics (see Figure 4). With demilitarization pre-defined by the order, I also avoid vast researcher degrees-of-freedom available in past work—attributable to estimating the effects of a handful of the roughly 35,000 unique items of military equipment in these data.[19] I find little evidence that this demilitarization led to changes in either rates of violent crime or officer deaths and assaults. These findings are not sensitive to matching methodology, measurement strategies, estimation procedure, or outcome measure. Put simply, the executive order had no detectable impact on violent crime or officer safety in these data.

This information is valuable for those considering future reform. In the United States, the acquisition of federal military equipment monotonically increased beginning in the late-1990s. I provide the first counter-factual estimates of what would occur under similar, hypothetical demilitarization policies. Alongside studies that suggest retaining military equipment poses risks to public support of LEAs[15,20], my findings imply the downside risks of returning certain types of weapons and equipment are undetectable for many outcomes law enforcement and the public care about.

Police militarization has been defined as the adoption of weapons, equipment, tactics and organizational patterns of the military by law enforcement agencies.[21] While there is no scholarly consensus on the definition of police militarization, this definition is broadly inclusive.[22] There are, however, some aspects of militarization (e.g., the adoption of military culture) that this study does not directly address. In the United States, the documented rise in police militarization[1,16] has been attributed to the War on Drugs[2], the War on Terror[23], and U.S. military interventions abroad.[3] Beyond explanations for this increase, critical debates center around its impact on crime and public trust in law enforcement.

This study investigates one aspect of militarization—the use of military equipment—for several reasons. First, though organizational features of law enforcement agencies receive public scrutiny (e.g., civilian oversight boards and investigation procedures for officer misconduct), most are not adopted by LEAs as a result of a military analogue. Second, public debate on policing often centers around equipment, weapons, and tactics. Weapons and equipment have received considerable attention from Congress and news organizations. In addition, though some argue there is no clear political avenue for reversing trends in police militarization[24], the distribution of weapons and equipment through the 1033 program presents one of the few opportunities for a centralized policy response within the executive branch. In addition, materiel may have secondary effects on other aspects of militarization. Reduction in the use of military uniforms, for example, has been suggested as a means of affecting the cultural aspects of police militarization.[25] And while recent work has investigated the effects of controversial, military-style tactics, such as no-knock warrants and the deployment of SWAT teams[15,26,27,28], the effects of weapons and equipment are contested because of straightforward—but critical–data integrity challenges.

The basic problem is that the current scientific record, published and unpublished, treats data on equipment released by the Defense Logistics Agency (DLA) as a time series—when, in fact, it represents a point-in-time snapshot of active inventories. See Supplementary Figure 13 to visualize one such snapshot. This conflates three sources of variation:

- New Distribution: Equipment shipped from the DLA to a participating LEA.
- Attrition: Equipment either destroyed or returned to the DLA or a state coordinator. Attrition prior to 2014 is unobservable in the public records.
- Transfer: Equipment shipped from one LEA to another with a state coordinator as intermediary. When weapons are transferred, they retain their original shipment date in the DLA inventory. Transfer prior to 2014 is unobservable in the public records.

Attrition and transfers are sources of unmodeled error in existing research. For example, the December 2014 DLA release indicates the Cambridge Police Department held twenty-five 5.56mm assault rifles. These gradually disappear from their inventories until March of 2017, when all drop out. Inventory records from the LEAs themselves confirm attrition like this is a regular part of the program. See Supplementary Figures 3 and 4 in the Supplementary Information. Earliest records indicate the Yale Police Department held no equipment shipped in 2014, but by 2019, sixty-nine rail-mounted optics appear. Before coming to New Haven, this equipment was likely held by another LEA in Connecticut. While both are examples measurement error in the treatment, attrition is most problematic, as the decision to return or destroy an item may be driven by crime, officer safety, or citizen complaints.

Results

The scale of error in prior research is substantial. I plot this error in Supplementary Figures 1 and 2, recovered based on a procedure described in detail in the Methods section. In the median 3-month period, more than 15,000 controlled items vanish from agency inventories and more than 4,000 items are received in transfer. Most of the variation in militarization during this period is due to these previously unobserved sources. Due to secular changes in defense procurement patterns after the height of the Iraq War (which influences the supply of equipment), the preceding 5-7 years are likely similar. A striking illustration of this pattern is AR-15 style 5.56mm rifles, the most common weapon requested by agencies and the item on which some estimates of the effect of militarization on crime are based. Between 2016-2019, all 5.56mm assault rifle variation is either the transfer of those already circulation among LEAs, or the disposal of weapons that have fallen out of use. As Figure 1 suggests, five-year rates of attrition for other weapons and armored vehicles are similar.

Importantly, this approach also reproduces known patterns based on top-down policy changes. The first quarter of 2015 sees little variation as it underwent review by a federal task force. Aside from the end of 2017 (when a subset of items was no longer designated as controlled), transfer and attrition track one another and transfers never outnumber attrition, suggesting many items are returned to state coordinators for use in other departments. This suggests the strategy for recovering this variation is valid.

Most problematically, attrition appears to be correlated with policing outcomes, even after accounting for time-invariant differences across LEAs. Specifically, Supplementary Table 3 reports coefficients from two-way fixed effects models that account for over time variation in inventory and recalled property. They suggest detectable, negative associations between attrition and four policing outcomes, including murder ($\beta = -0.0016$, 95% $ci = (-0.0002, -0.0031)$, $p = 0.048$) and assaults against police officers with injury ($\beta = -0.0023$, 95% $ci = (-0.0011, -0.0036)$, $p = 0.000$). Thus, even the relatively short time series from the contemporary period (e.g., March 2015 to March 2020) suggests that increases in crime are associated with whether LEAs return or destroy equipment, potentially biasing in favor of the positive relationships uncovered by past research. This also suggests the measurement error in prior research is systematic, and thus, instrumental variable approaches cannot correct for it.

It is also important to note that these associations do not imply that the studies finding reductions in crime as a result of property distribution are underestimating the magnitude of its impact. In this case, transfers also occur, and are difficult to model. In addition, previous studies generally select some subset of complete weapons and equipment, which may have different attrition patterns in the periods of study. This poses another research challenge not discussed by these studies, as parts for weapons and equipment are also distributed, but often left out of analyses. For example, the upper receiver of an AR-15 style assault rifle is a controlled item, serialized, and legally considered the firearm itself. But this item is excluded from studies' measures of police militarization. When there is more than one source of error, and that error will likely exhibit temporal variation, it is difficult to be confident about the direction of the bias in any particular study. Instead, this should be taken as a demonstration that attrition and transfer occur, are significant in scale, and are likely not random.

Thus, existing estimates of the effects of militarization based on these data are seriously flawed. Moreover, the DLA did not retain active inventories prior to the first data release in 2014. This means that accurate time series records of military equipment on hand can only be obtained through another source—the records of LEAs themselves, which are highly decentralized and may be missing. This necessitates an alternative means of estimating the effect of militarization over the contemporary period for which more accurate records exist.

Effects on Violent Crime and Officer Safety

To assess the claims of policymakers and past research, I estimate the effect of demilitarization in panels of demilitarized and matched agencies. As I describe in more detail in the Methods section, I present estimates from generalized difference-in-difference models with intercept shifts for agency and time. Diagnostic checks suggest the demilitarized and matched agencies have similar pre-treatment trends in crime (see Supplementary Table 7 and Supplementary Figures 6 and 7) This accounts for fixed features of law enforcement agencies and their jurisdictions that influence crime. Pre-processing to comparable sets reduces model dependence[29] and the incidence of false-negatives. Moreover, relying on a plausibly exogenous source of variation addresses inferential challenges common in studies of police militarization. The dependent variables are logged to reduce the influence of extreme cases.

Figure 2 plots estimates of the 2015 demilitarization on violent crime and officer safety, leveraging various matched sets and the three quarters before and after the change. The treatment variable is a dichotomous indicator of when the police department complied with the demilitarization order (as described in the Methods section). Coefficients for violent crime are mixed. Estimates for the effect of demilitarization on murder and manslaughter allow us to statistically reject effect sizes of more than +/-5 percentage points. Estimates for effects on the incidents of robbery imply a 6.6 percentage point increase ($\beta = 0.066$, 95% $ci = (-0.00, 0.13)$, $p = 0.063$). But these effects are highly uncertain—even if conventionally distinguishable from zero under some model specifications and robustness checks reported in the supplementary information. Estimates for officer safety are near zero and are more consistent. Officer deaths are sufficiently rare that the effects are near (or in some specifications, at) zero. These allow us to reject effect sizes of greater than +/- 1 percentage point. Officer assault estimates are near zero, suggestive of only minor effects, and inconsistently signed.

This substantive interpretation of the findings is consistent across numerous robustness checks. Most obviously, the results are stable across different specifications of comparable departments. Estimated effects among the inclusive, baseline, and restrictive samples are very similar, and narrowing or broadening the inclusion criteria is not associated with any consistent effect pattern. I describe each of these samples in detail in the Methods section. Another potential explanation for the estimated effects near zero is that demilitarization is thought to have an impact on crime, in general, rather than particular offenses. In addition, unmodeled variation in the reporting and classification of particular offenses can make them less reliable.[30] But in robustness checks that use linear, additive indices of violent crime and officer safety, we again cannot be confident about the sign of these effects (see Supplementary Figure 8).

I adopted several approaches to ensure the results were not a function of dichotomous specification of the key independent variable. I use the amount of recalled equipment (log scale) and a threshold value of recalled equipment ($1,000) as alternative treatment variables. The estimates reported in Supplementary Figure 9 show that neither specification provides evidence that the presence of recalled equipment had an impact on outcomes. Put differently, the results are robust to accounting for the magnitude of demilitarization within affected agencies. Some specifications for incidence of robbery are statistically distinguishable (by convention) from zero, but the scale of these effects are vanishingly small. The coefficients for the continuous treatment imply that the loss of all weapons and equipment led to a 0.8 percentage point increase in the incidence of robbery (95% $ci = (-0.003, -0.017)$, $p = 0.012$). The estimated coefficient for these outcomes is similar for the threshold version of the treatment, which essentially recodes departments with very little recalled equipment (typically several dozen bayonets) as untreated.

These estimates do vary depending upon the length of the pre- and post- demilitarization period (Supplementary Figures 11 and 12). The results reported in Figure 2 include the second quarter of 2015 to the third quarter of 2016, which is two quarters prior to demilitarization and two quarters after all demilitarization was completed. But lengthening the post-treatment study period does not impact these estimates in a consistent direction, which would not be the case if the order had systematic, long-term effects.

A narrower study period also lowers the chance that treated agencies will have "re-militarized" via some unobserved means. In the contemporary period, for example, homeland security grants enable law enforcement agencies to purchase weapons and equipment. There is no centralized record of what is acquired through these grants. In addition, many grants "pass through" other jurisdictions (i.e., counties or states) and are then used for purchases at the municipal-level. This is a persistent challenge for estimating the effect of weapons and equipment generally, which further suggests examining the impact of a top-down policy change is more credible. In addition, for this reason, estimates that include a longer post-treatment time horizon should be treated with more skepticism.

The results presented in Figure 2 rely on the timing of observed compliance with the executive order as a treatment. Though this likely introduces additional error, since it is known that some agencies did not immediately demilitarize (see Methods section and Supplementary Table 5), an alternative specification would be to use the timing of the executive order itself. I report these results in Supplementary Figure 10. Coefficients that use the release of the guidelines are consistent with the effects reported in Figure 2, but moving the treatment timing one quarter later (until complete compliance is achieved for all agencies) reverses the sign of most coefficients. This again, underscores the lack of consistent evidence of significant effects of demilitarization on crime.

This set of mostly null effects, of course, does not itself imply that demilitarization has no effect. To estimate the power of this research design, I simulated from the observed covariate distributions in the baseline matched sample. Given a true effect size, sample size, treatment assignment, and research design, the simulation drew unit, time and error term effects from a standard normal distribution. The observed data was then regressed against the simulated dependent variable, which was then repeated 1,000 times to obtain false-negative rates by effect size and outcome. As Supplementary Table 6 reports, this design should be capable of detecting effects as small as 0.1 stan. dev. for the conventional power threshold of 80% with $\alpha = 0.05$. This means that for the highly variable outcomes (i.e., assault, robbery, and rape), the effects would have to be quite large to be reliably recovered with this research design. For other outcomes, like murder and officer deaths or assaults, more confidence in the lack of even small, positive effects for demilitarization is warranted.

In summary, there is little evidence from these data that the 2015 demilitarization of police departments participating in the 1033 program had substantively significant impacts on violent crime or officer safety.

Discussion

Over the previous three decades, local law enforcement agencies in the U.S. have acquired and routinized the use of weapons, equipment, and tactics previously exclusive to the military. Weapons and equipment acquisition has been subsidized by federal authorities, but fundamental limitations in record-keeping about these transfers have rendered studies of their effects not credible. This has not, however, limited their reach and potential influence among policymakers. This study analyzed archived federal inventory records to gauge the reliability of this work, while providing estimates of the effect of a instance of demilitarization with weaker identification assumptions.

I find that most changes in police militarization in the contemporary period are the result of weapons and equipment already in circulation, and that attrition of these weapons is common. This is highly problematic for studies that estimate the effect of militarization (on any outcome) using data series that include years prior to 2014. In addition, this suggests the secular trends in militarization through this program have leveled off—possibly due to the draw down of U.S. military personnel abroad. Notably, however, there was a non-negligible increase in new distributions in late 2019 and early 2020 (Supplementary Figure 2).

Importantly, this means that the operative question facing interested parties is the potential effects of policy interventions that demilitarize departments with already acquired equipment. This study examined the only known effort to do so, which was implemented by the Obama administration in 2015. Contrary to the claims of the administration's critics, this policy significantly reduced the level of militarization in select agencies. This suggests demilitarization by presidential fiat is indeed feasible for the set of 1033 program participants. Finally, I find little-to-no evidence that demilitarization had an impact on violent crime or officer safety. Put differently, contrary to the claims of those that support the transfer of equipment, these data do not support the conclusion that militarization "saves lives"—or that demilitarization risks them.

There are important limitations to this study and the conclusions that can be drawn from it. The demilitarization examined was limited to several categories of items and several hundred of the roughly 6,000 departments participating in the 1033 program (which only comprise less than half of the LEAs in the United States). This means that we cannot confidently state that this study provides evidence against small (and for some outcomes moderate) sized effects. This is also a substantive scope condition for the present study. It examines the effects of recalling specific types of weapons and equipment for participants of a particular government program—not demilitarization, in general.

These limitations notwithstanding, this study provides valuable evidence for those considering reform. Numerous comparable weapons, vehicles and types of equipment remain in circulation today. In companion with evidence about reputational costs of militarization[15,20], these findings suggest that the downside risks of demilitarization would not be detectable for similar policy interventions.

Methods

To parse variation in attrition and transfers between departments, I obtained every quarterly inventory from 2014-2020 from the DLA via Freedom of Information Act (FOIA) request. In total, these contain $\sim$3.8 million shipment records from roughly 9,300 unique LEAs in 22 data releases (see Supplementary Table 1). Individual shipments are identified and tracked by their shipment date, recipient, and national stock number (NSN). Within agency inventories, I define a new distribution as a controlled item that appears in active inventory $q+1$, but not q, with a shipment date after q. The technical definition of controlled item is one that has a demilitarization (DEMIL) Code of B, C, D, E, F, G, and Q3. In general, a controlled item is one that requires some degree of physical destruction prior to disposal. Uncontrolled items do not have to be tracked by recipient agencies. The modal distribution in the 1033 program is uncontrolled, and can include items like exercise equipment, socks, computers, or wiffle bats. Attrition is a controlled item that appears in inventory at q but not $q+1$, while a transfer as a new item with a shipment date prior to q. I identify each type of shipment variation for each agency and quarter after December 2014, and then adjust the algorithm to account for the most common record-keeping errors to ensure that these are not counted as attrition or transfer (see SI page A3). While this provides descriptive information about sources of error, it also allows us to validate reported patterns of attrition as a result of changes in policy.

Identification strategy: Executive Order 13688

My approach for estimating the effect of military weapons and equipment relies on regulatory changes implemented by the Obama administration in response to public scrutiny of the 1033 program in late 2015. This allows us to estimate the average effect of the order for demilitarized agencies by comparing over-time changes in measures of violent crime and officer safety. This improves upon existing strategies that are not applicable to the contemporary period and which have themselves been critiqued.[14]

This strategy addresses a significant challenge for inference—that the distribution of weapons and equipment is endogenous to policing outcomes. Harris et al.[31] and Bove and Gavrilova[12] rely on instruments—county location and variation in military spending, respectively—to address this concern. Proximity to DLA distribution centers is thought to impact the likelihood of obtaining equipment because LEAs pay shipping costs. However, as Supplementary Figure 2 shows, transfers account for most distributions, and these occur within-state. Moreover, when transfers are present, changes in military spending will have very little purchase on the presence of equipment in individual LEAs.

President Obama signed Executive Order 13688 on January 15, 2015, which created an inter-agency working group tasked with developing reform proposals for the distribution of military surplus. Their final report was released May 2015[32], and led to the recall of tracked armored vehicles, grenade launchers, and bayonets (see Supplementary Table 8). Also banned was the distribution of small arms greater than or equal to .50 caliber, but no such weapons were ever distributed according to DLA inventories. I argue this led to a demilitarization that was exogenous to policing outcomes. This is supported by key features of the regulations. First, the May 2015 report indicates that its authors did not know which agencies held recalled equipment, and only 7 of the nearly 10,000 LEAs in the program submitted comments. According to the report, at the time of its release "a Government-wide assessment [...] to identify the LEAs that have acquired the types of equipment identified on the recommended prohibited equipment list" was ongoing. This is evidence that the choice of recalled items could not have been influenced by lobbying or foreknowledge the LEAs that would be impacted.

Second, the items recalled have operationally equivalent analogues that suggest the LEAs that held this equipment at the time of the change were not unique among LEAs (Supplementary Table 8. The administration recalled bayonets, but not combat knives—though some officers reported that bayonets were used as "big, sturdy, knives."[33] It prohibited .50 caliber weapons and ammo, but not other cartridges widely used by the military but not law enforcement. It recalled tracked vehicles, but allowed departments to retain mine resistant vehicles with similar exterior armor. The reforms at the time were criticized by advocates for demilitarization as insufficient—with the implication that the items were selected for recall because of their appearance, rather than their use in the context of policing. One scholar referred to the reforms as "nothing more than symbolic politics."[33] Nonetheless, at the time of the order, the recall affected nearly 350 LEAs and $30.3 million in equipment. This necessarily means that this study does not speak to all types of demilitarization via weapons and equipment. The policy intervention was significant for the LEAs that were impacted because it tended to be a large share of their 1033 inventory. But it was also limited in scope, in that a small share of LEAs were demilitarized and several dozen items were recalled. Recalling different types of equipment with more impacted departments could, of course, result in different effects.

This approach to estimating the effect of demilitarization assumes the Obama administration's directive achieved full compliance from affected LEAs. But executive orders are non-justicable, which implies a private citizen cannot sue their police department because it did not comply.[34] This renders the most common legal enforcement mechanism moot. However, active inventories of recalled and banned equipment from 2014-2020 demonstrate that near full compliance was achieved. Figure 3 plots the drop in this equipment following the release of the final recommendations. Recalled items that remain in circulation are concentrated in local branches of federal agencies still permitted to retain them. In addition, there were 88 departments that retained recalled property for either one or two additional quarters. But this delayed demilitarization is not associated with any observed feature of agencies or jurisdictions (Supplementary Table 4 and 5, Supplementary Figure 5). There is no corresponding remilitarization after the revocation of these restrictions in 2017, which can be explained by differences in the public management strategies of each administration (see Supplementary Methods section). Thus, it is appropriate to compare affected and unaffected agencies over the available time series.

I link military equipment records with monthly policing outcomes recorded by the FBI's Uniform

Crime Reporting Program from 2014-2017.[35,36] Most LEAs that appear in the 1033 data can be matched, however, the majority of LEAs nationwide do not participate (see Supplementary Table 2). Note, this is an additional source of error for studies that aggregate to county or other geographic level. I then acquire a vector of pre-treatment covariates on which to prune the data into matched sets of demilitarized and comparable unaffected agencies. Specifically, I have available data on department size and 1033 inventory, as well as plausibly relevant data from the 2010 Census and 2014 American Community Survey: jurisdiction population size, demographics (e.g., % African American or Hispanic), median household income, and unemployment. I also match LEAs to measures of jurisdiction political ideology: the average Democratic presidential candidate voteshare over the previous four elections, and MRP estimates of conservatism.[37]

I use coarsened exact matching[38] to reweight agencies in these data with recalled equipment at the time of the order and comparable LEAs. To ensure the results are not sensitive to this step, I iteratively impose additional covariates and coarsening strategies to produce three comparison sets, which I label "inclusive", "baseline", and "restrictive." The purpose of these three sets is illustrative. There are numerous potential matching strategies, but I have found the results are mostly insensitive to the choice of matched set. The reported results are meant to demonstrate that imposing additional matching restrictions does not alter the main findings. I present matching diagnostics and additional descriptive information about the matched sets and procedure in the Supplementary Results section.

In general, matching based on these characteristics renders more plausible the parallel trends assumption necessary for valid DiD estimates. In this instance, their are two concerns worth noting. First, the 1033 program represents only one avenue for the acquisition of military weapons and equipment. Departments may obtain similar weapons through other federal grants, or with their own budgets. Second, banned equipment itself might be associated with policing jurisdictions with trends in violent crime that diverge significantly from others. Put differently, there may be different patterns of unobserved error in agencies that did and did not have recalled equipment. Thus, the most credible set of comparisons will attempt to select agencies with similar program take-up and similar jurisdictions to police.

The inclusive set matches agencies within state based on LEA size and the total value of their 1033 inventory. All departments are matched within state since transfers are thought to occur through state-coordinators. Department size, measured as the number of sworn officers, and the present value of their 1033 inventory help ensure comparisons across units with similar levels of demand for equipment. Importantly, this present inventory value is not subject to the measurement error concerns laid out in the previous section. Matching within state will also approximate matching based on distance to relevant distribution centers. In addition to these variables, the baseline set matches based on % African American and % Hispanic in jurisdiction, since the distribution of particular types of equipment has been linked to the policing of minority communities.[6,39] The restrictive set adds median household income and Democratic presidential voteshare to the set of matching covariates. Income has well-established associations with crime rates. I have shown there is no association between Democratic voteshare and time to compliance, but it is possible that some other, unobservable aspect of compliance (e.g., usage or actual deployment) might be associated with local-level political preferences.

Figure 4 locates the set of demilitarized and baseline control LEAs. Agencies in the South and Midwest held most recalled equipment. This differs from overall distribution of equipment, which—in addition to representing city-centers—also shows concentrations in border counties that have no recalled equipment (Supplementary Figure 13). The spatial distribution may be partly determined by storage location of recalled equipment.[31] Many armored units, for example, are stationed in the South (e.g., Georgia, North Carolina, and Texas).

Though the parallel trends assumption is unverifiable, initial diagnostic checks in the pre-treatment period among matched agencies are reassuring. Supplementary Figures 6 and 7 report mean outcome values by group and quarter along with bootstrapped confidence intervals. In addition, Supplementary Table 7 reports coefficients for time and treatment group interactions for the pre-treatment period. While demilitarized and unaffected agencies tend to have different baseline levels of crime, trends in each outcome do not differ across treated and matched sets during the pre-treatment period.

Data availability statement
The data supporting the findings reported in this study, along with the underlying 1033 program inventory records, have been deposited in the Harvard Dataverse, and can be found at: `https://doi.org/10.7910/DVN/XCTPNR`.

Code availability statement
Replication code for the findings reported in this study, have been deposited in the Harvard Dataverse, and can be found at: `https://doi.org/10.7910/DVN/XCTPNR`.

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Author contributions
K.L. designed research, performed research, analyzed data, and wrote the paper.

Competing interests
The author declares no competing interests.

Figure legends

Figure 1: Roughly one in five controlled weapons turnover within 4 years. Plots proportion of December 2014 inventory remaining within police department as of December 2019. In the initial inventory, there were 82,634 assault rifles, 8,494 handguns, 625 mine resistant vehicles (MRVs), and 1,074 shotguns.

Figure 2: Demilitarization did not lead to substantively significant increases or decreases in violent crime or officer safety. Plots point predictions and 90%, 95% and 99% confidence interval estimates from linear models with two-way fixed effects for the effect of demilitarization on policing outcomes, with observations weighted by matched LEA sample weights. Dataset includes six panels (three quarters before and after the Executive Order was implemented). "Sample" refers to the number and degree of coarsening in pre-treatment covariates; restrictive includes 2,509 LEAs ($n = 17,570$), baseline includes 5,315 LEAs ($n = 37,233$), inclusive includes 7,495 LEAs ($n = 52,514$).

Figure 3: EO 13688 achieved near-perfect compliance and resulted in demilitarization among affected LEAs. Plots summaries of recalled property (i.e., grenade launchers, 50 caliber weapons, tracked APCs, and bayonets) in active inventories from December 2014 to March 2020. Shaded bar indicates issuance of restrictions.

Figure 4: Local law enforcement agencies with recalled equipment were concentrated in the South, Midwest, and California. Plots the locations of demilitarized agencies ($n = 315$) and baseline set of unaffected agencies ($n = 5,000$) assigned greater than zero weight, which are matched within state based on pre-order department size, amount of 1033 equipment, and non-white population in jurisdiction. Shapefiles from U.S. Census Bureau.

Supplementary Information

Police Demilitarization and Violent Crime

Kenneth Lowande

Supplementary Methods

Militarization Data

1033 program data were first released in 2014 in response to FOIA requests from journalists. Earliest versions of these data aggregated active inventories to county-level. Beginning in December 2014, and quarterly thereafter, the DLA released active inventories of shipments to individual LEAs with their original shipment dates (Supplementary Table 1). The most current inventory can be viewed at the Law Enforcement Support Office (LESO) public information page: `https://www.dla.mil/DispositionServices/Offers/Reutilization/LawEnforcement/PublicInformation/`.

These versions are not archived in public repositories. To obtain all releases, I submitted a FOIA request to the DLA. This was FOIA Case Number DLA-DSERV-2019-003512, resolved September 25, 2019. Prior to this, I obtained as many versions which were available through archived links on the Wayback Machine (`https://archive.org/web/`) in order to minimize the burden of our request. Ultimately, 9 versions of the dataset could not be obtained through archived webpages, and thus, were included in the request.

Studies that use these data typically do not note the version of the dataset they have obtained. However, without this information, it is difficult to determine the scale of the error induced by attrition. In general, however, the presence of attrition and transfer implies that the pre-2014 record will be subject to more error, the more recent the version.

Supplementary Table 1 – 1033 Active Inventories, Dec. 2014 - Mar. 2020

Quarter	Year	Shipments	LEAs
4	2014	199653	7809
1	2015	194792	7817
2	2015	201978	7826
3	2015	192311	7803
4	2015	198657	7748
1	2016	207123	7708
2	2016	198240	7640
3	2016	190348	7535
4	2016	197063	7562
1	2017	189292	7512
2	2017	183218	7495
3	2017	180001	7473
4	2017	161771	7134
1	2018	156480	7148
2	2018	154780	7143
3	2018	148111	6910
4	2018	151513	6890
1	2019	145945	6813
2	2019	144285	6746
3	2019	141646	6631
4	2019	146374	6635
1	2020	141068	6522

The versions above contain two identifiers for LEAs: a string name, and a state abbreviation. Because the LEAs are contained in separate Excel spreadsheets, there are very few errors in state location. However, in general, the names used from version to version are not constant, and sometimes contain misspellings. In some cases, the name and state render the LEA ambiguous, particularly in jurisdictions policed by multiple agencies (e.g., sheriff, police, or constable), or within states that have cities and townships with identical names. Other non-identifiable LEAs include task forces or other inter-agency groups with no web presence or defined geographic jurisdiction. Some units are identifiable, but do not report crime statistics—most often, higher education LEAs or school districts. Thus, though most agencies can be matched to measures of crime and other covariates, some do not have corresponding ORI9 codes and are dropped (Supplementary Table 2). In general, these unmatched departments retain minor active inventories and do not account for a significant portion of the program participants. Those who do participate make up a minority of the local LEAs operating in the U.S., and do not make up a majority of the LEAs that report data to the FBI.

Supplementary Table 2 – 1033 participants make up less than half of local LEAs. Reports LEAs with matched ORI-9 codes, and their appearance in UCR data files and the 1033 data releases. There were 361 LEAs that could not be matched with an ORI-9 code based on name and state.

	1033 Program	
UCR	No	Yes
No	–	532
Yes	14116	7916

Measurement of Attrition, Transfers, and New Distributions

Attrition, transfers, and new distributions are not noted in the active inventories released by the DLA. Attrition occurs when a department destroys or returns an item. Transfers occur when an agency receives an item already in circulation among participating LEAs. New distributions are surplus received by LEAs from the DLA. Tracking this variation is only possible for controlled items—those the military has determined require demilitarization and must be recorded in agency self-audits. Thousands of other items (e.g., socks, computers, exercise equipment) may drop out of an inventory for reasons not associated with attrition or transfer. To track these sources of variation, I adopt the following definitions:

New distribution. A controlled item that appears in active inventory $q+1$, but not q, with a shipment date after q.

Attrition. A controlled item that appears in inventory at q, but not $q+1$.

Transfer. A controlled item that appears in active inventory $q+1$, but not q, with a shipment date prior to q.

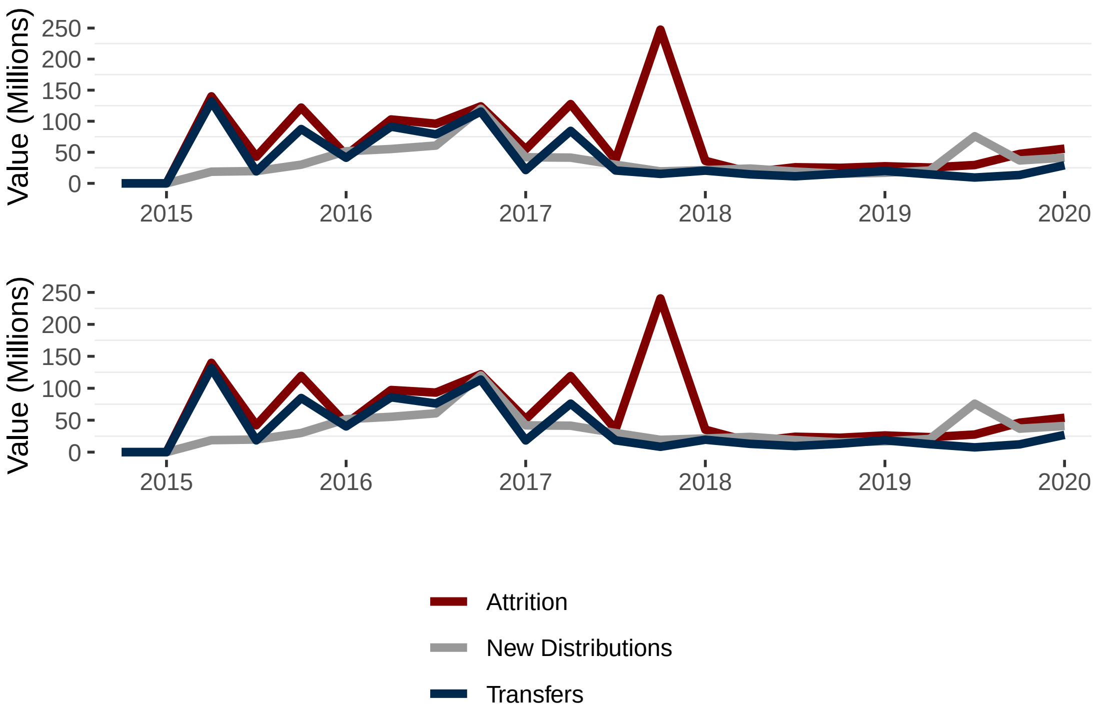
Within LEA, from inventory to inventory, I classify shipments as either one of the above or an existing shipment. This makes several assumptions that may be inaccurate in practice. First, it assumes that the shipment date for a shipment indicates the first date the equipment was transferred from the DLA (e.g., transfers retain their original shipment date). Second, it assumes that the records contain no errors from year to year (e.g., errors in shipment dates, item quantities, etc.). I therefore adopt two additional assumptions to rule out the most minor record-keeping errors.

Shipment splitting. Shipments of multiple items in q, split into multiple shipments of the same total quantity in $q+1$, are assumed to be an existing shipment rather than attrition and transfer.

No shipment exchanges. Shipments that change dates, but are otherwise identical in q and $q+1$, are assumed to be an existing shipment rather than attrition and transfer.

These additional conditions assume that departments, in the same three month period, would not decommission or return and then acquire the same items. I regard this as conservative, in that it will likely under-estimate the amount of attrition and transfer. In practice, it is possible that departments wait to decommission items until replacement items are available and can be shipped. However, the amount of attrition and transfer without these additional restrictions, along with their concentration in particular quarters, suggests (to us) that some of this variation is due to simple record keeping errors.

In Supplementary Figure 1, I reproduce Supplementary Figure 2 without the additional restrictions to show the measurement of error in militarization assuming no record keeping errors. In peak quarters, such as 2015-4, these restrictions reclassify around $100 million in equipment as existing shipments.



Supplementary Figure 1 – Attrition and transfers outnumber new distributions in military equipment from 2014-2020. Plots quarterly estimates of each category across active inventories from December 2014 to March 2020 under alternative assumptions. Top time series assumes no record keeping errors. Bottom figure allows split shipments. Estimates from Figure 2 also enforce no shipment exchange restriction.

Supplementary Figure 2 – Attrition and transfers outnumber new distributions in military equipment from 2014-2020. Plots quarterly estimates of each category across active inventories from December 2014 to March 2020.

Third-party records suggest this means of measuring attrition is valid. As part of an ongoing project to verify federal data with third-party records, I have obtained agency records of item attrition in Michigan. For demonstration purposes, I include two snapshots of these records in Supplementary Figures 3 and 4. In agency records, attrition often presents as "closed" items. Records of closure are not maintained by the DLA. As these snapshots suggest, attrition is common—such that the estimates of error I present are plausible.

LESO FEPMIS: Inventory Search Results User ID: [redacted]

Inventory Search | Inventory Search Results | Inventory Item

Place cursor over *Pending* status of an item to view additional information

Previous Show All 165 Next

Images	Documents	Requisition#	Property#	DTID	State	Station	NSN	Item Name	Dmil Code	Dmil Integrity Code	Creation Date	Quantity	Status	Tracked
2	1	2YTPDD-3016-6640	13260MI138	W56U1R-2152-0029A	MI	BRIGHTON POLICE DEPT	2320-01-107-7155	TRUCK,UTILITY	Q	6	September 17, 2013	1	CLOSED	Y
		2YTPDD-5089-4880	15098MI00028	FB2039-5086-2119	MI	BRIGHTON POLICE DEPT	7920-00-292-2365	BROOM,PUSH	A	1	April 8, 2015	2	CLOSED	Y
		2YTPDD-5089-4941	15098MI00029	W15GK8-5085-T188L	MI	BRIGHTON POLICE DEPT	6130-01-527-2726	CHARGER,BATTERY	A	1	April 8, 2015	1	CLOSED	Y
		2YTPDD-5091-7900	15098MI00030	W25MWY-5083-Z036	MI	BRIGHTON POLICE DEPT	6630-01-527-0969	ANALYZER,BLOOD GL...	A	1	April 8, 2015	1	CLOSED	Y
		2YTPDD-5091-7900	15098MI00031	W25MWY-5083-Z036	MI	BRIGHTON POLICE DEPT	6630-01-527-0969	ANALYZER,BLOOD GL...	A	1	April 8, 2015	1	CLOSED	Y
						DEPT	601-7506							
		2YTPDD-6245-1483	16277MI00002	FU6131-61750-043	MI	BRIGHTON POLICE DEPT	6910-DS-TRA-INAI	TRAINING AIDS	A		October 3, 2016	3	CLOSED	Y
		2YTPDD-6250-3021	16277MI00003	2YTH5H-62030-001	MI	BRIGHTON POLICE DEPT	2541-01-555-4876	ARMOR,TRANSPARENT...	D	1	October 3, 2016	2	ASSIGNED	Y
		2YTPDD-6250-3037	16277MI00004		MI	BRIGHTON POLICE DEPT	7010-01-528-9875	COMPUTER SYSTEM,D...	A	1	October 3, 2016	2	CLOSED	Y
		2YTPDD-6253-4641	16277MI00005	W9166W-62172-505	MI	BRIGHTON POLICE DEPT	8465-01-569-8802	KIT,MODERN COMBAT...	A	1	October 3, 2016	2	CLOSED	Y
		2YTPDD-6258-6299	16277MI00006		MI	BRIGHTON POLICE DEPT	8105-01-505-1463	STATIC SHIELD BAG	A	1	October 3, 2016	150	CLOSED	Y
2		2YTPDD-6287-7440	16295MI00002	W56HZW-6279-W501G	MI	BRIGHTON POLICE DEPT	2355-01-553-4634	MINE RESISTANT VE...	C	1	October 21, 2016	1	ASSIGNED	Y
1		2YTL45-6348-9553	16364MI00003		MI	BRIGHTON POLICE DEPT	5855-01-524-4314	SIGHT,THERMAL	D	0	December 29, 2016	1	ASSIGNED	Y
		2YTPDD-7037-1330	17046MI00001		MI	BRIGHTON POLICE DEPT	1240-00-137-7768	CASE,OPTICAL INST...	A	1	February 15, 2017	2	CLOSED	Y
		2YTPDD-	17046MI00002		MI	BRIGHTON	1240-	CASE,OPTICAL INST...	A	1	February	2	CLOSED	Y

Supplementary Figure 3 – Attrition in Brighton, MI Police Department Inventory. "Assigned" indicates the item is still in active inventory.

	A	B	C	D	E	F	G	H
1	GRAND BLANC TOWNSHIP POLICE DEPARTMENT							
2	LESO EQUIPMENT LIST							
3	Station	NSN	Item Name	Dmil Code	Dmil Integrity Code	Creation Date	Quantity	Status
4	GB TWP POLICE	6515-00-364-0560	SCISSORS,GENERAL ...	A	1	25-Sep-13	7	CLOSED
5	GB TWP POLICE	1005-01-363-0207	POUCH,RIFLE ACCES...	A	1	25-Sep-13	1	CLOSED
6	GB TWP POLICE	1005-01-363-0207	POUCH,RIFLE ACCES...	A	1	25-Sep-13	1	CLOSED
7	GB TWP POLICE	1005-01-456-9772	BASKET,STORAGE	A	1	25-Sep-13	4	CLOSED
8	GB TWP POLICE	1005-01-555-8427	BRUSH,CLEANING,SM...	A	1	25-Sep-13	15	CLOSED
9	GB TWP POLICE	2540-01-361-3057	TARPAULIN	A	1	1-Oct-13	1	CLOSED
10	GB TWP POLICE	5180-01-483-0250	TOOL KIT,GENERAL ...	A	1	3-Oct-13	1	CLOSED
11	GB TWP POLICE	6510-01-540-6484	DRESSING,COMPRESSION	A	1	21-Oct-13	100	CLOSED
12	GB TWP POLICE	1240-01-044-4572	CASE,OPTICAL INST...	A	1	21-Oct-13	30	CLOSED
13	GB TWP POLICE	8465-01-515-8620	FIELD PACK	A	1	21-Oct-13	20	CLOSED
14	GB TWP POLICE	5120-00-293-3336	SHOVEL,HAND	A	1	31-Oct-13	20	CLOSED
15	GB TWP POLICE	6230-DS-LIG-HT01	ELECTRIC PORTABLE...	A		31-Oct-13	2	CLOSED
16	GB TWP POLICE	5130-00-357-5135	WRENCH SET,SOCKET	A	1	31-Oct-13	1	CLOSED
17	GB TWP POLICE	8415-01-472-6914	OVERALLS,COLD WEA...	A	1	31-Oct-13	28	CLOSED
18	GB TWP POLICE	6230-DS-LIG-HT01	ELECTRIC PORTABLE...	A		31-Oct-13	9	CLOSED
19	GB TWP POLICE	6510-01-210-4453	DRESSING,OCCLUSIV...	A	1	4-Nov-13	100	CLOSED
20	GB TWP POLICE	4240-01-399-3349	CARRIER ASSEMBLY	A	1	4-Nov-13	50	CLOSED
21	GB TWP POLICE	6230-01-588-8427	LIGHT KIT,WEAPONS	B	3	12-Nov-13	4	ASSIGNED
22	GB TWP POLICE	1240-01-490-7312	SIGHT,REFLEX	D	1	12-Nov-13	2	ASSIGNED
23	GB TWP POLICE	1005-00-056-2237	MAGAZINE,CARTRIDGE	D	1	12-Nov-13	70	ASSIGNED
24	GB TWP POLICE	7830-DS-ELL-IPTI	ELLIPTICAL	A		12-Nov-13	1	CLOSED

Supplementary Figure 4 – Attrition in Grand Blanc, MI Police Department Inventory. "Assigned" indicates the item is still in active inventory.

Supplementary Table 3 – Attrition is correlated with some policing outcomes. Reports coefficients from linear models predicting attrition with unit (LEA) and time (quarter) fixed effects, controlling for present 1033 inventory and the attrition of recalled items. Dependent variable is a dichotomous indicator for attrition. Includes all participating agencies from 2015-2020. Two-tailed tests.

Beta	SE	P	N	Outcome
-0.001573	0.000794	0.047555	119025.000000	Murder
-0.000014	0.000007	0.043362	119025.000000	Assault
-0.000351	0.000172	0.041112	119025.000000	Rape
-0.000953	0.004466	0.831039	119025.000000	Manslaughter
-0.000061	0.000056	0.279694	119025.000000	Robbery
0.011135	0.020863	0.593551	119025.000000	Felonious Officer Deaths
-0.009508	0.052318	0.855792	119024.000000	Accidental Officer Deaths
-0.002329	0.000639	0.000270	119025.000000	Officer Assaults (Injury)
-0.000402	0.000235	0.086472	119025.000000	Officer Assaults (No Injury)

Differences in the Implementation of Presidential Directives

President Obama signed Executive Order 13688 in January 2015, amid congressional oversight hearings on equipment transfers in both 2014 and 2015. Contemporaneous oversight is typically thought to aid in bureaucratic compliance by revealing additional information to principals[40]. The Order created an inter-agency working group, which involved officials from a dozen executive departments and agencies. This included officials from the Department of Defense tasked with administering 1033 program and implementing any policy changes. Though none was required, the working group conducted a notice-and-comment process, mirroring formal rulemaking under the Administrative Procedures Act. This led to participation by 47 stakeholders named in the final report. There is evidence that interest group participation in public comment processes influence the content of regulations[41]. The final policy recalled equipment at no cost to affected LEAs.

President Trump signed Executive Order 13809, revoking 13688 in August 2017. No formal congressional oversight of this action or the topic occurred at the time. The action was announced and developed by the Department of Justice without known consultation of other departments and agencies—including the bureau responsible for implementing the 1033 program. There was no public comment process, and the order itself was announced at a meeting of the Fraternal Order of Police (FOP)—the only apparent stakeholder from the previous comment process that was consulted.[1] The Order revoked all regulations associated with the Obama order, including those publicly favored by the FOP (e.g., providing LEAs training for use of transferred equipment, and creating a comprehensive list of available equipment). To re-militarize, LEAs would have to pay shipping costs—knowing this same equipment might be, again, recalled by a future administration.

These differences in context likely account for the discontinuity following the Obama Order, and the subsequent lack of re-militarization following the Trump Order. The Obama administration's public management strategy incentivized local LEAs to comply with a national directive through top-down oversight, administrative and stakeholder buy-in, and reduction of the material cost of compliance. None of these strategies were adopted by the Trump administration.

[1] "Attorney General Sessions Delivers Remarks at the 63rd Biennial Conference of the National Fraternal Order of Police," Justice News, Nashville, TN Monday, August 28, 2017. URL: `https://www.justice.gov/opa/speech/attorney-general-sessions-delivers-remarks-63rd-biennial-conference-national-fraternal` (Accessed Oct. 16, 2019).

Variation in Compliance

Full compliance was eventually achieved for all agencies that were subject to the order and held recalled equipment at the time of the task force recommendations. However, as Supplementary Figure 5 suggests, there was some variation in the timing of compliance. One potential concern is that variation in this timing could be driven by the same or similar selection mechanisms that make inferences about the causal effect of militarization difficult. Additionally, it is possible that local compliance was conditional on political disagreements over the order. The order was a top-down, presidential mandate, and the program itself became politicized after the events in Ferguson, MO. Bureaucratic compliance is often thought to be conditional on ideological disagreement[42].

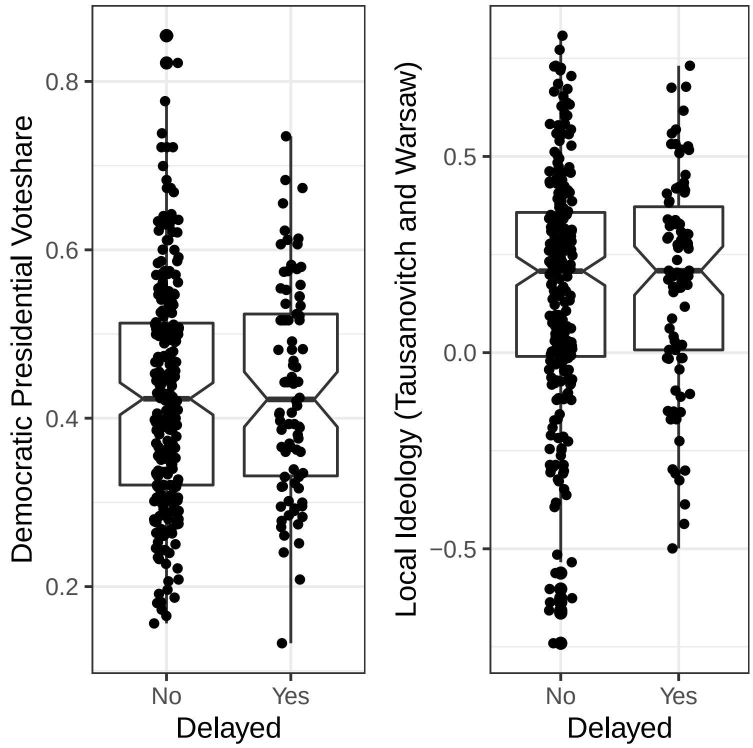
To assess this concern, I produce a dichotomous variable, compliance delay, which equals one if recalled equipment remained in an LEAs inventory after the first quarterly data release following the recommendations. In most cases, this means a 3 month delay in attrition. I examine distributions of delay and potential observable confounders. I report raw correlations in Supplementary Table 4, and one logit specification in Supplementary Table 5. Delay and % Black are the only significant correlation, and the magnitude is relatively minor, and runs counter to standard expectations of racial threat theory (i.e., if anything, departments with larger minority populations returned their equipment more quickly). Moreover, agencies that delayed did not reside in jurisdictions that were significantly more conservative or liberal, as measured by Democratic presidential voteshare or public opinion based estimates (Supplementary Figure 5).

Supplementary Table 4 – Correlates of Compliance Delay

	Delayed	Dem. Vote	Ideology	Population	Dept. Size	% Black	% Hispanic	Income
Delayed	1.00	0.01	0.03	-0.02	-0.01	-0.16	-0.05	0.02
Dem. Vote	0.01	1.00	-0.80	0.25	0.22	0.32	0.16	0.12
Ideology	0.03	-0.80	1.00	-0.26	-0.22	-0.27	-0.26	-0.18
Population	-0.02	0.25	-0.26	1.00	0.94	0.09	0.16	0.05
Dept. Size	-0.01	0.22	-0.22	0.94	1.00	0.06	0.15	0.05
% Black	-0.16	0.32	-0.27	0.09	0.06	1.00	-0.09	-0.31
% Hispanic	-0.05	0.16	-0.26	0.16	0.15	-0.09	1.00	0.03
Income	0.02	0.12	-0.18	0.05	0.05	-0.31	0.03	1.00

Supplementary Table 5 – Logit Estimates of Compliance Delay

	Dependent variable:
	DV: Delay $\{0, 1\}$
Dem. Voteshare	0.35 (0.98)
	p = 0.72
Population (Thousands)	0.0000 (0.0003)
	p = 0.95
Log(Banned Inventory)	-0.19 (0.05)
	p = 0.0001
Constant	0.23 (0.55)
	p = 0.69
Observations	337
Log Likelihood	-182.46
Akaike Inf. Crit.	372.92



Supplementary Figure 5 – Variation in compliance is not associated with local support for President Obama. Plots compliance delay (1 or 2 quarters) and average votshare for Obama or local level ideology in LEA jurisdiction. Delay is is defined as maintaining an inventory of recalled equipment (e.g. tracked vehicles, grenade launchers, etc.) after September 2015.

Simulated False-Negative Rates in Research Design

Statistical power is, of course, a central issue for studies that report null effects for policy interventions. I estimated the power of this generalized DiD design by simulating from the observed data in the baseline matched sample. The exact procedure and code can be found in the replication files that accompany this study. But in summary, my approach followed the following steps:

1. Store observed treatment assignment, unit, and quarter.
2. Assume true treatment effect (standard deviation scale).
3. Simulate dependent variable:
   (a) Draw error term, unit and time effects from a standard normal distribution.
   (b) Generate dependent variable as a linear function of treatment effect, treatment condition, unit and time intercept shifts, and error term.
4. Regress simulated dependent variable on observed treatment with time and unit fixed effects.
5. Repeat steps 3-4 1,000 times to obtain false negative rate.
6. Repeat step 5 for each effect size (0.01–0.15 sd).
7. Report the minimum detectable effect size for a given false negative rate by outcome (Supplementary Table 6)

In general, the results suggest that this study should be able to detect effect sizes at small as 0.1 sd at the conventional power threshold of 80%. What this means, of course, depends upon the variability of the outcome. The observed outcomes can provide some guidance. This design should be able to detect relatively small changes in outcomes like murder, manslaughter, and officer deaths or assaults. In contrast, treatment effects on rape, robbery, and most significantly—assault, would have to be quite large to be reliability detected.

As I indicate in the main text, this means that this study is not general evidence demilitarization has no effect. Some policing outcomes are inherently more variable than others (i.e., rape, robbery, and assault), which means that a study of this kind can only speak to whether there are large intervention effects.

Supplementary Table 6 – Simulated Power by Outcome. Reports minimum coefficient effect size by outcome and simulated false negative rate for the baseline sample ($N = 37,233$). Given a true effect size, sample size, treatment assignment, and research design, the simulation draws unit, time and error term effects from a standard normal distribution. Observed data is then regressed against the simulated dependent variable, and these steps are repeated 1,000 times to obtain false-negative rates by effect size and outcome.

	71%	83%	91%
% Stan. Dev.	0.090	0.102	0.113
Murder	0.033	0.038	0.042
Assault	0.177	0.200	0.224
Manslaughter	0.007	0.008	0.009
Rape	0.073	0.083	0.093
Robbery	0.088	0.100	0.112
Felonious Officer Deaths	0.002	0.002	0.003
Accidental Officer Deaths	0.001	0.001	0.001
Officer Assaults (Injury)	0.033	0.038	0.042
Officer Assaults (No injury)	0.048	0.054	0.060

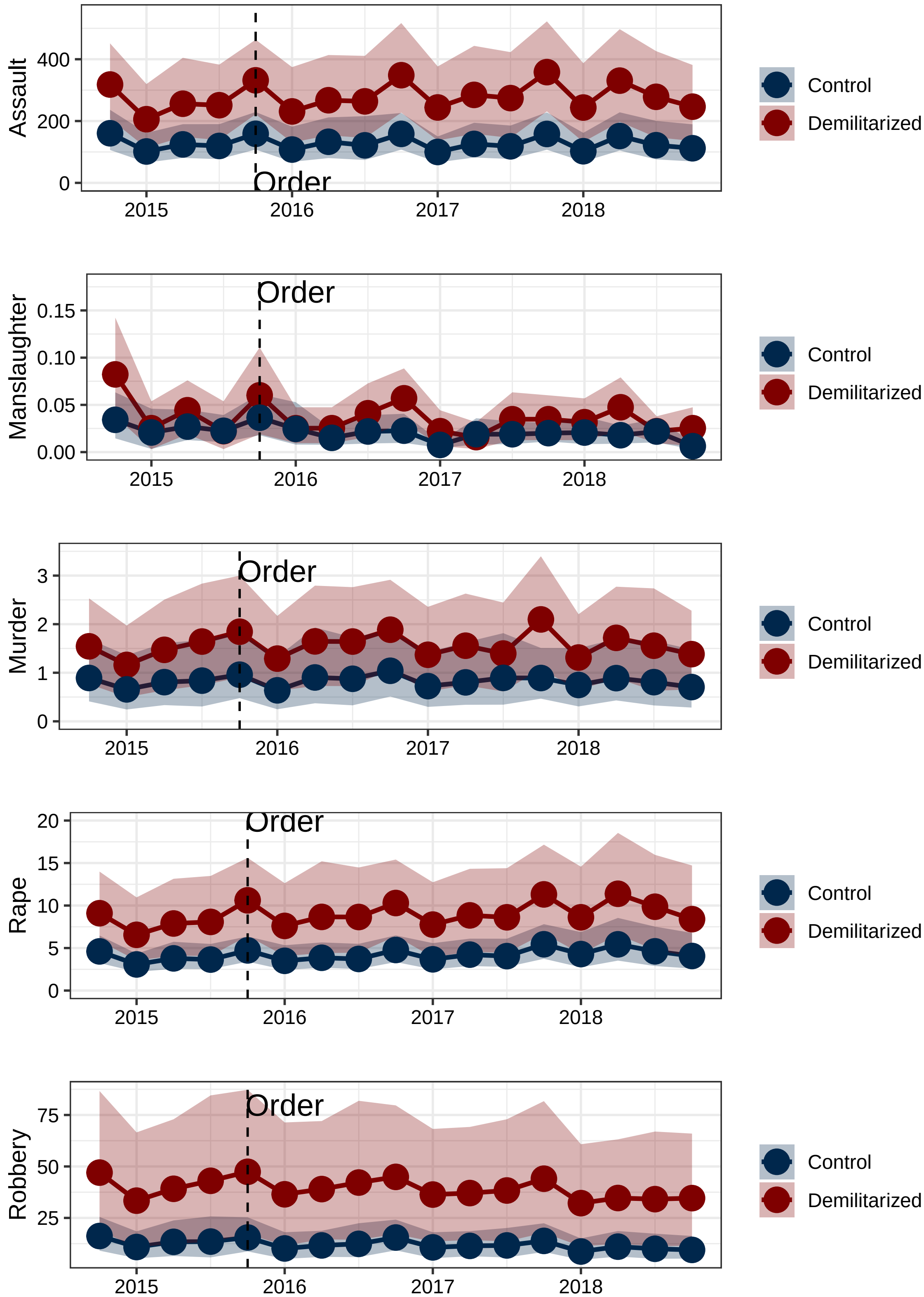
Supplementary Results

This section reports additional analyses meant to gauge the robustness of the findings reported in the main text. Supplementary Table 7 investigates whether there are significant differences in pre-treatment trends across demilitarized and unaffected agencies. Trends in each dependent variable to not differ systematically. The parallel trends assumption required or DiD estimates is unverifiable. This is not a test of that assumption. However, it is reassuring that the matching procedure results in sets of agencies with similar pre-treatment over-time variation in violent crime. Supplementary Figures 6 and 7 provide additional reassurance that these trends do not diverge by plotting mean and bootstrapped confidence intervals for these outcomes. This straightforward, non-parametric summary of the data provide additional evidence that the demilitarization had little impact.

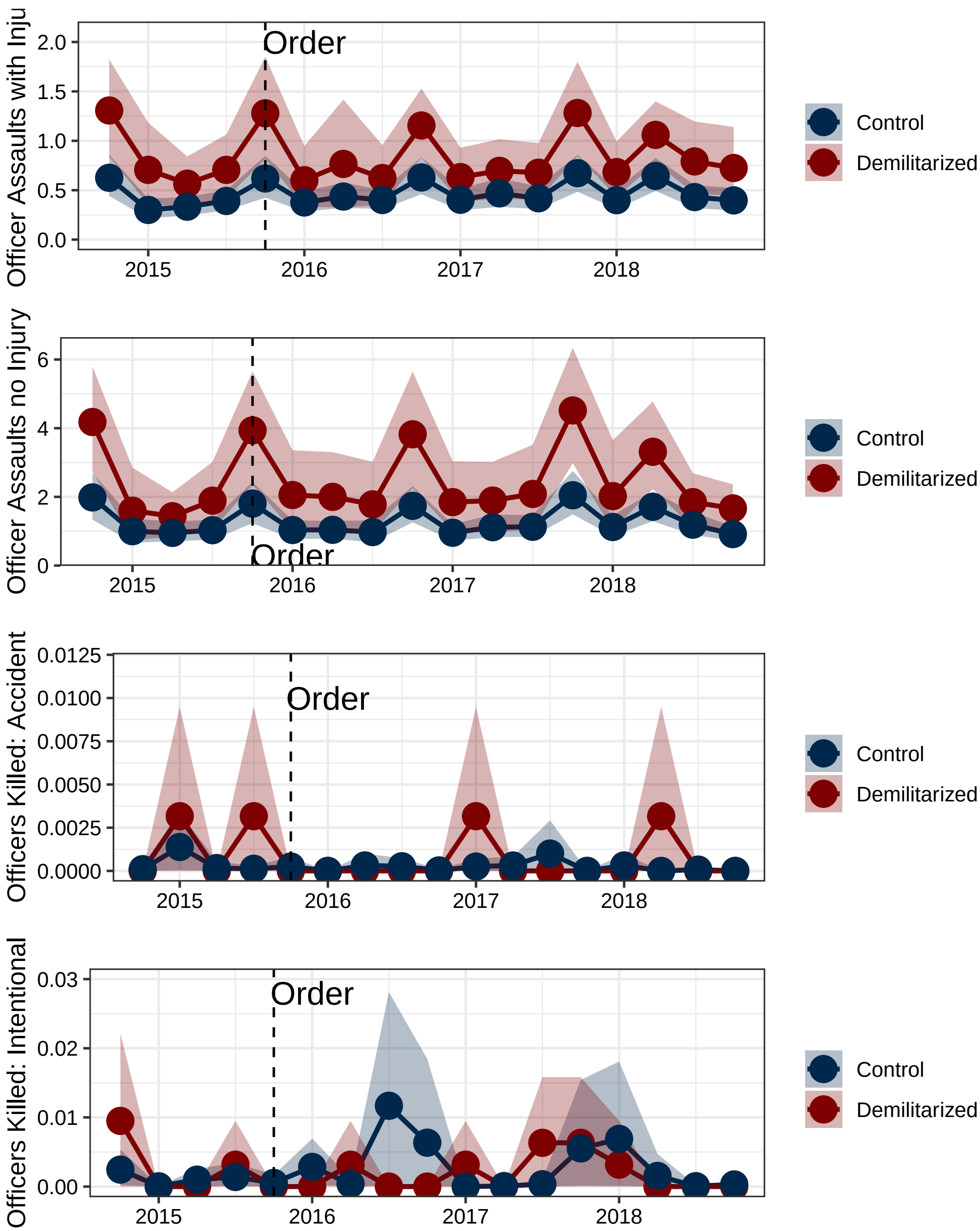
Supplementary Figure 8 replicates the main regressions with linear, additive violent crime indices. Supplementary Figure 9 replicates the main regressions with alternative versions of the treatment: a continuous effect of the presence of banned equipment, and a dichotomous threshold of $1,000 in equipment. The substantive interpretation of the results are unchanged in these alternative specifications.

Supplementary Table 7 – Pre-treatment quarters are not significantly different across LEA groups. Reports interaction coefficients between pre-treatment quarters and group, along with associated standard errors and number of obersvations.

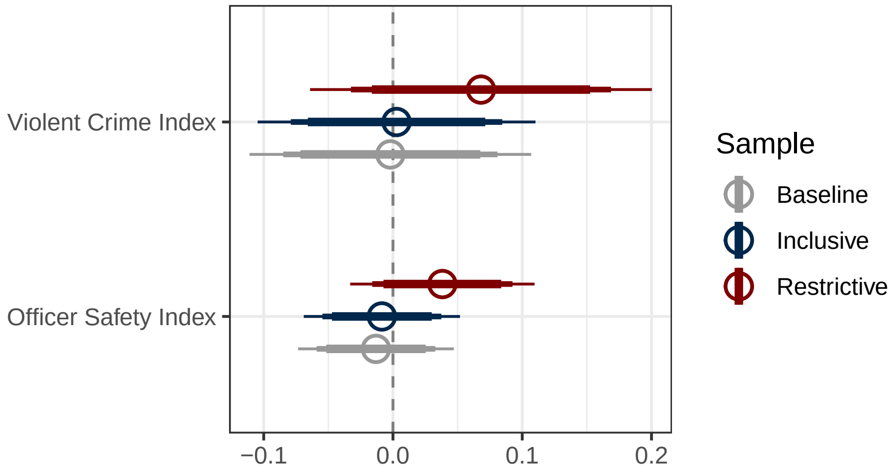
qtr	outcome	b	se	N	p
2014-4	Murder	0.131	0.032	42552	0.000
2015-1	Murder	-0.071	0.046		0.118
2015-2	Murder	-0.054	0.046		0.237
2015-3	Murder	-0.044	0.046		0.332
2015-4	Murder	-0.010	0.046		0.831
2014-4	Assault	0.756	0.125	42552	0.000
2015-1	Assault	-0.273	0.177		0.123
2015-2	Assault	-0.225	0.177		0.203
2015-3	Assault	-0.259	0.177		0.143
2015-4	Assault	0.010	0.177		0.955
2014-4	Rape	0.291	0.060	42552	0.000
2015-1	Rape	-0.109	0.084		0.194
2015-2	Rape	-0.078	0.084		0.356
2015-3	Rape	-0.077	0.084		0.360
2015-4	Rape	0.059	0.084		0.485
2014-4	Manslaughter	0.025	0.007	42552	0.000
2015-1	Manslaughter	-0.023	0.010		0.018
2015-2	Manslaughter	-0.014	0.010		0.141
2015-3	Manslaughter	-0.028	0.010		0.004
2015-4	Manslaughter	-0.015	0.010		0.123
2014-4	Robbery	0.418	0.075	42552	0.000
2015-1	Robbery	-0.231	0.106		0.029
2015-2	Robbery	-0.184	0.106		0.083
2015-3	Robbery	-0.212	0.106		0.045
2015-4	Robbery	-0.027	0.106		0.800
2014-4	Felonious Officer Deaths	0.005	0.002	42552	0.020
2015-1	Felonious Officer Deaths	-0.005	0.003		0.099
2015-2	Felonious Officer Deaths	-0.006	0.003		0.061
2015-3	Felonious Officer Deaths	-0.004	0.003		0.219
2015-4	Felonious Officer Deaths	-0.005	0.003		0.077
2014-4	Accidental Officer Deaths	-0.000	0.001	42552	0.925
2015-1	Accidental Officer Deaths	0.001	0.001		0.223
2015-2	Accidental Officer Deaths	-0.000	0.001		0.967
2015-3	Accidental Officer Deaths	0.002	0.001		0.042
2015-4	Accidental Officer Deaths	-0.000	0.001		0.921
2014-4	Officer Assaults (Injury)	0.136	0.027	42552	0.000
2015-1	Officer Assaults (Injury)	-0.057	0.039		0.143
2015-2	Officer Assaults (Injury)	-0.088	0.039		0.023
2015-3	Officer Assaults (Injury)	-0.073	0.039		0.059
2015-4	Officer Assaults (Injury)	0.001	0.039		0.983
2014-4	Officer Assaults (No Injury)	0.225	0.040	42552	0.000
2015-1	Officer Assaults (No Injury)	-0.164	0.057		0.004
2015-2	Officer Assaults (No Injury)	-0.143	0.057		0.012
2015-3	Officer Assaults (No Injury)	-0.128	0.057		0.025
2015-4	Officer Assaults (No Injury)	-0.013	0.057		0.819



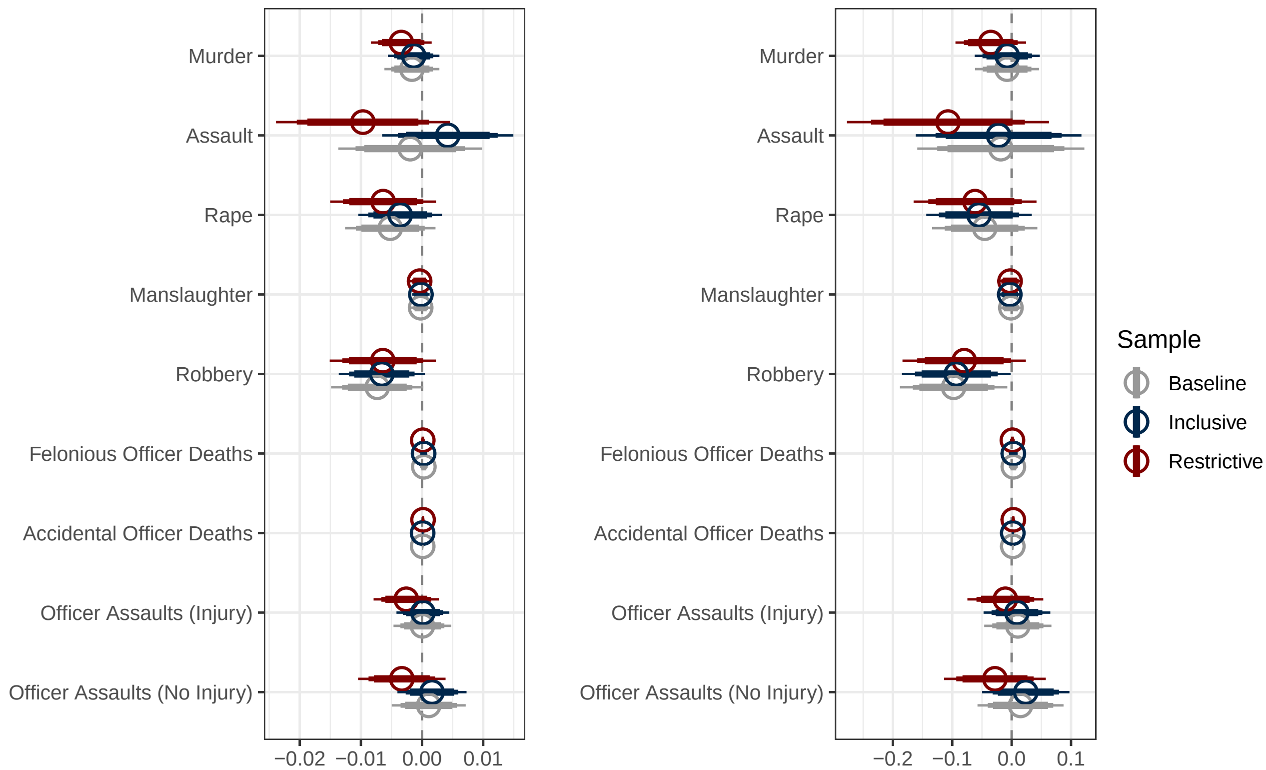
Supplementary Figure 6 – Trends in violent crime by treatment and control group. Reports weighted mean and bootstrapped 95% confidence intervals in outcome measures by demilitarized and unaffected agencies, using the "baseline" matched set.



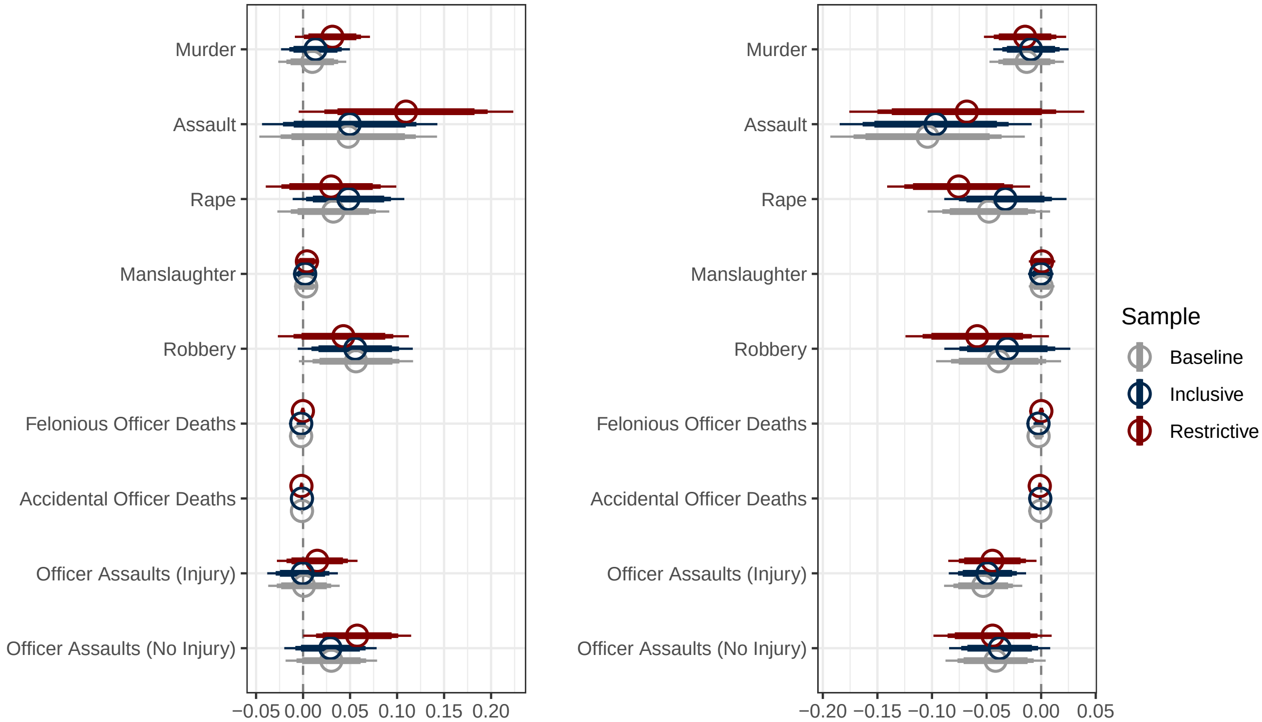
Supplementary Figure 7 – Trends in officer safety by treatment and control group. Reports weighted mean and bootstrapped 95% confidence intervals in outcome measures by demilitarized and unaffected agencies, using the "baseline" matched set.



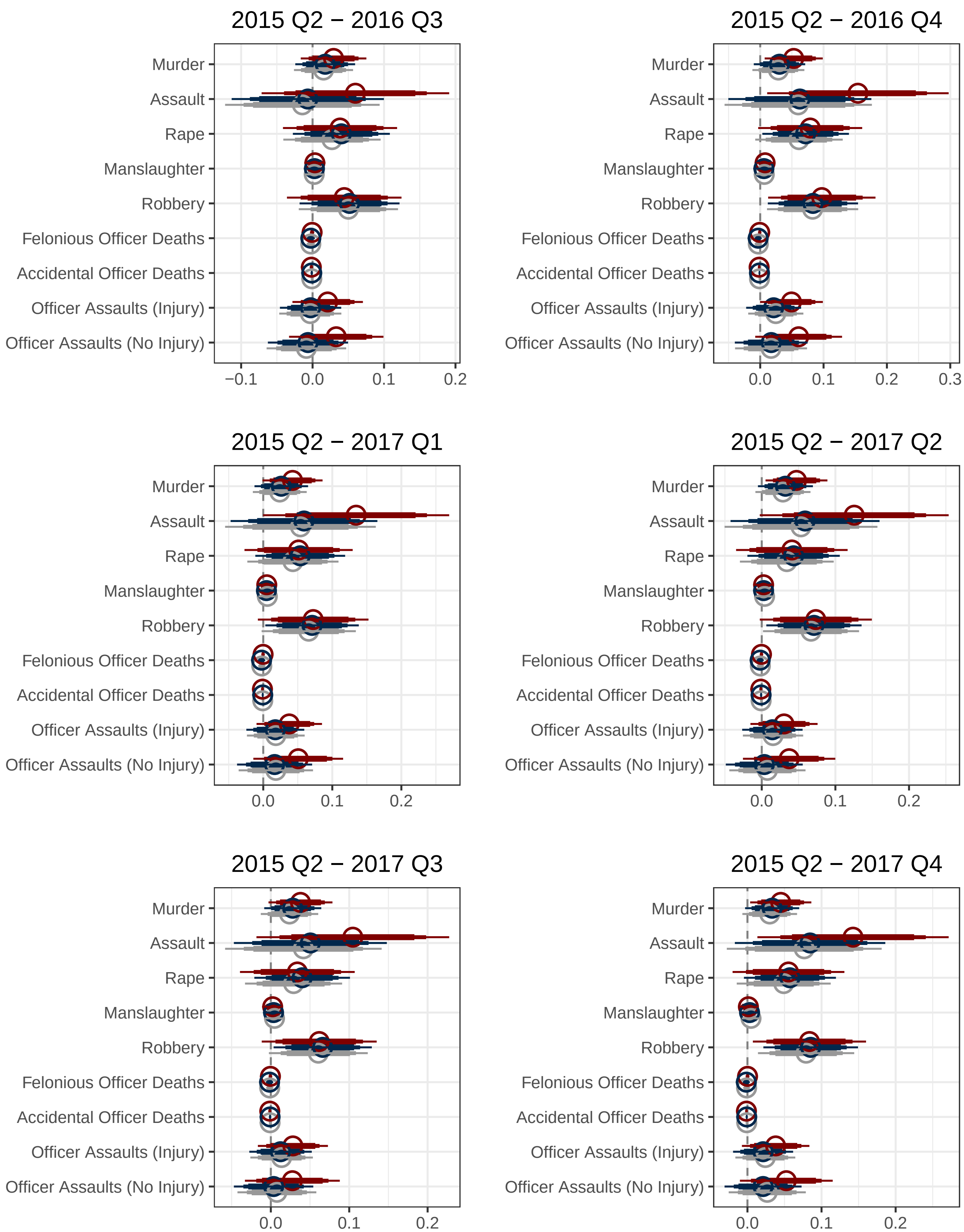
Supplementary Figure 8 – Demilitarization did not lead to significant increases or decreases in violent crime or officer safety indices. Plots point predictions and 90%, 95% and 99% confidence interval estimates from linear models with two-way fixed effects for the effect of demilitarization on policing outcomes, with observations weighted by matched LEA sample weights. Dataset includes six panels (three quarters before and after the Executive Order was implemented). "Sample" refers to the number and degree of coarsening in pretreatment covariates; restrictive includes 2,509 LEAs ($n = 17,570$), baseline includes 5,315 LEAs ($n = 37,233$), inclusive includes 7,495 LEAs ($n = 52,514$).



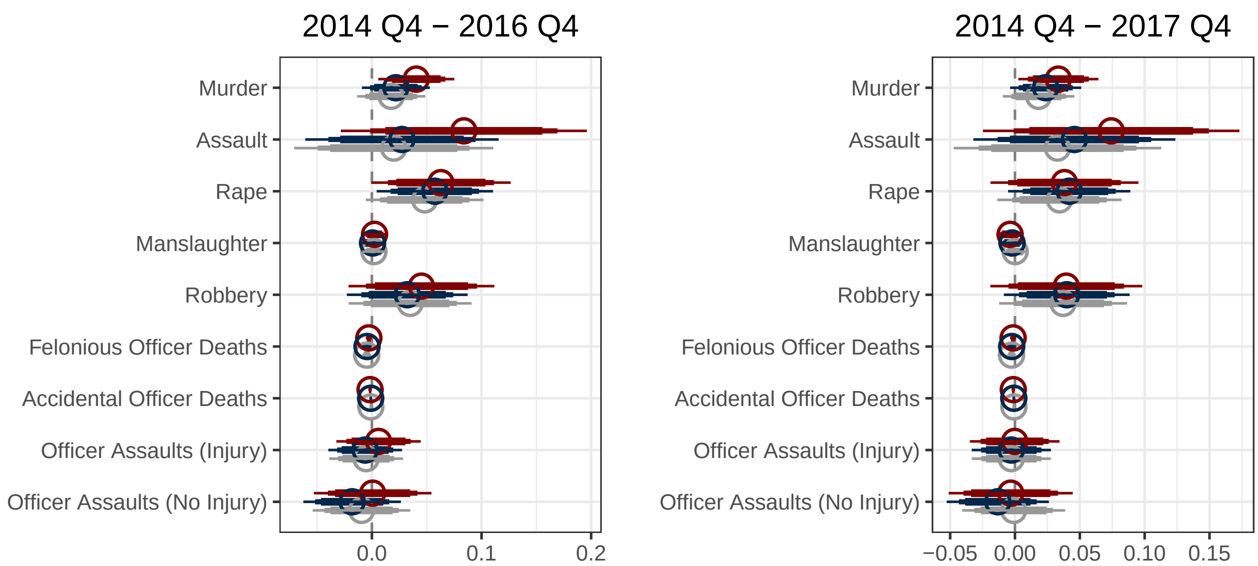
Supplementary Figure 9 – Demilitarization, as either a continuous treatment or with a different dichotmous threshold of recalled equipment, did not lead to significant increases or decreases in violent crime or officer safety. Plots point predictions and 90%, 95% and 99% confidence interval estimates from linear models with two-way fixed effects for the effect of demilitarization on policing outcomes, with observations weighted by matched LEA sample weights. Note, the key treatment is the presence of the equipment, so the interpretation of this figure is inverted compared to other estimate plots. Right-hand plot takes any department that had more than $1,000 in recalled equipment as demilitarized; left-hand is a continuous effect with the logged value of recalled property as the treatment variable. Dataset includes six panels (three quarters before and after the Executive Order was implemented). 'Sample" refers to the number and degree of coarsening in pre-treatment covariates; restrictive includes 2,509 LEAs ($n = 17,570$), baseline includes 5,315 LEAs ($n = 37,233$), inclusive includes 7,495 LEAs ($n = 52,514$).



Supplementary Figure 10 – Results with the timing of the executive order as the treatment produces are inconsistent. Plots point predictions and 90%, 95% and 99% confidence interval estimates from linear models with two-way fixed effects for the effect of demilitarization on policing outcomes, with observations weighted by matched LEA sample weights. Left panel: treatment is a dichotomous indicator that takes the value of 1 after the third quarter of 2015. Right panel: treatment is a dichotomous indicator that takes the value of 1 after the fourth quarter of 2015. Dataset includes six panels (three quarters before and after the Executive Order was implemented). 'Sample" refers to the number and degree of coarsening in pre-treatment covariates; restrictive includes 2,509 LEAs ($n = 17,570$), baseline includes 5,315 LEAs ($n = 37,233$), inclusive includes 7,495 LEAs ($n = 52,514$).



Supplementary Figure 11 – Results not sensitive to the inclusion of different quarterly snapshots. Reproduces the findings in Figure 2 with different pre- and post- treatment windows, as noted in each panel.



Supplementary Figure 12 – Results not sensitive to the inclusion of different quarterly snapshots (continued). Reproduces the findings in Figure 2 with different pre- and post-treatment windows, as noted in each panel.

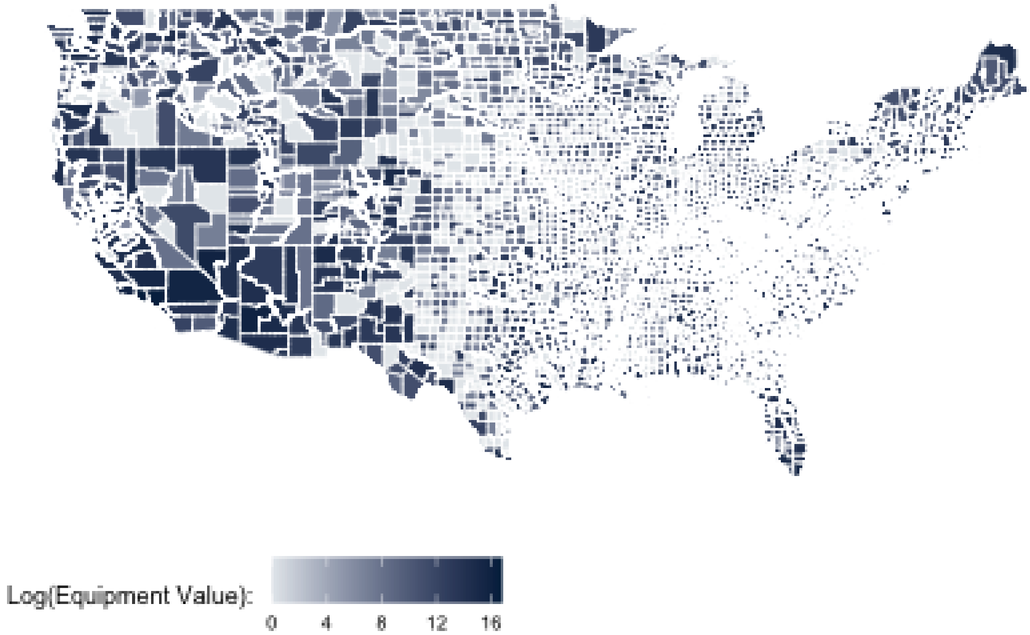
Balance Across Demilitarized and Unaffected LEAs

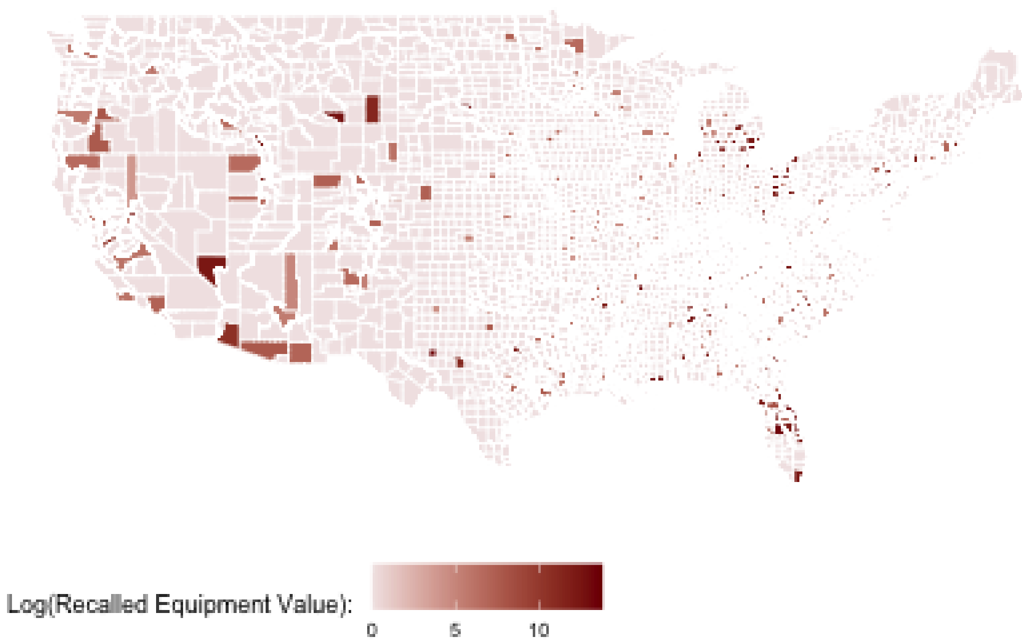
This section provides additional information regarding the reweighting procedure used prior to estimation of difference-in-differences among demilitarized and unaffected agencies. The primary substantive justification for this procedure is that demilitarization did not ban classes of equipment. Instead, it banned select items that had drawn public scrutiny, despite the presence an operationally equivalent item. I report these in Supplementary Table 8. In some cases, the analogue is an item. In others, parts for the banned item continue to be distributed.

Another potential concern is that the spatial concentration of banned or not banned equipment renders the point estimates I report narrow in substantive application. As Supplementary Figure 13 reports, that is clearly not the case. There are concentrations of equipment at the geographic borders of the U.S., in part, because expensive equipment such as vehicles are often approved to be deployed because of the demands of the LEA's jurisdiction. Supplementary Tables 9, 10, and 11 report diagnostic information about the progressive matching procedure, which reweights observations within states based on 1033 inventory, department size, % Hispanic, % Black, Democratic voteshare, and median household income. Not surprisingly, increasing the dimensionality of the matching procedure reduces local common support.

Supplementary Table 8 – Banned equipment typically has at least one direct analogue among still circulating items. Reports the national stock numbers (NSN) for a set of recalled and banned items, along with classes of not banned items.

Category	Banned Item NSN	Not Recalled
Bayonets	1095000179701	Combat Knives
	1095000739238	Bayonet Sheaths
	1095003172460	
	1095007162787	
	1095009132602	
	1095012271739	
	1095015063424	
	1095015216087	
Grenade Launchers	1010006911382	81mm Mortar Parts
	1010001796447	107mm Mortar Parts
	1010015669083	120mm Mortar Parts
	1010015572542	Grenade Launcher Parts
Tracked Vehicles	2350000566808	Mine Resistant Vehicles
	2350008602349	Fast Attack Vehicles
	2350010451123	Light Armored Vehicles
	2350010684077	Armored Trucks
	2350010684089	
	2350010696931	
.50 Caliber Small Arms	1005000136944	5.56mm Rifles
	1005001229339	7.62mm Rifles
	1005001229368	Parts for .50 Caliber Small Arms
	1005009371831	
	1005009371832	
	1005010612459	
	1005011442845	
	1005014692133	





Supplementary Figure 13 – 1033 equipment is typically concentrated in border counties because of local branches of federal agencies. Plots the logged distribution of total and recalled equipment as of the first agency-level quarterly inventory in December 2014. Shapefiles from U.S. Census Bureau.

Supplementary Table 9 – Matching diagnostics for inclusive set of LEAs. Reports differences in demilitarized and unaffected LEAs by matching variable; all LEAs matched within state.

	Diff	L_1	Min.	25%	50%	75%	Max.
Inventory	-534293.79	0.00	0.00	-11538	-244844.00	-786974.5	-1941133.37
Dept. Size	-406.41	0.00	0.00	-15	-46.67	-123	-35946.33
				L_1^{pre}	0.65	% LCS^{pre}	20.00
				L_1^{post}	0.51	% LCS^{post}	27.94

Supplementary Table 10 – Matching diagnostics for baseline set of LEAs. Reports differences in demilitarized and unaffected LEAs by matching variable; all LEAs matched within state.

	Diff	L_1	Min.	25%	50%	75%	Max.
Inventory	-456267.85	0.00	0.00	-13473	-244844.00	-694139.7	10890253.51
Dept. Size	-336.38	0.00	0.00	-18.33	-44.00	-115.67	-35946.33
% Hispanic	0.00	0.03	0.00	-0.01	-0.01	0	0.04
% Black	-0.02	0.08	0.00	-0.01	-0.03	-0.06	0.05
				L_1^{pre}	0.81	% LCS^{pre}	2.88
				L_1^{post}	0.61	% LCS^{post}	14.33

Supplementary Table 11 – Matching diagnostics for restrictive set of LEAs. Reports differences in demilitarized and unaffected LEAs by matching variable; all LEAs matched within state.

	Diff	L_1	Min.	25%	50%	75%	Max.
Inventory	-534293.79	0.00	0.00	-11538	-244844.00	-786974.5	-1941133.37
Dept. Size	-406.41	0.00	0.00	-15	-46.67	-123	-35946.33
% Hispanic	0.02	0.04	0.00	0	0.00	0.01	0.04
% Black	-0.02	0.02	0.00	0	-0.01	-0.05	0.05
Dem. Voteshare	0.00	0.00	-0.01	0.01	-0.00	0.01	0.00
Income	-1732.94	0.00	-5343.00	-997	-1226.00	-3031	17101.00
				L_1^{pre}	0.98	% LCS^{pre}	0.34
				L_1^{post}	0.92	% LCS^{post}	3.89