

Controlling for the Non-Controlled: Revisiting Police Militarization & Police Violence

Drew N. Leonard*

March 11, 2019

Abstract

Since 1997, America's Department of Justice (DOJ) has sold nationwide law enforcement agencies (LEA) over \$6B of excess military equipment—such as assault rifles and aircraft—at little to no cost through its “1033” grant-in-aid program. The 1033 Program's purpose is increasing the public's trust and safety by better-equipping police officers with tools to deter and control crime. However, recent studies reveal that the program might have the opposite effect by increasing police violence: militarized police officers might be *more* prone to use force due to military-equipment's enabling effects. However, the primary study on police militarization's effects on police violence is limited in two ways: first, it measures the aggregate effects of DOD-sold items both with and without military attributes; and second, its scope includes just four of 50 American states. In this paper, I re-estimate police militarizations' effects on police violence by measuring the marginal effects of items with military attributes on police killings while controlling for a battery of plausibly confounding variables. I recover the original study's conclusion that police militarization increases police violence significantly and show that items without military attributes bias estimate effects.

*GOVT019/QSS030, 19W, Dartmouth College

Contents

| | | |
|----------|------------------------------------|-----------|
| 1 | Introduction and Hypotheses | 1 |
| 2 | Data and Methods | 4 |
| 2.1 | Dependent Variable | 5 |
| 2.2 | Explanatory Variable | 6 |
| 2.3 | Control Variables | 7 |
| 2.4 | Methods | 8 |
| 3 | Results | 10 |
| 4 | Conclusion | 12 |
| | Appendix A Models | 15 |
| | Appendix B Figures | 16 |
| | Appendix C Tables | 23 |

1 Introduction and Hypotheses

In 1990 and 1997, the United States Congress passed National Defense Authorization Acts (NDAA) that allow the Department of Defense (DOD) to supply nationwide law enforcement agencies (LEA) with surplus military equipment for mixed law enforcement purposes.¹ Police militarization’s express purpose was (and remains) increasing the public’s trust and safety by providing police officers with better leverage to deter or control crime.² The Department of Justice (DOJ) and DOD have since created multiple grant-in-aid programs that provision LEAs with military-grade weapons and vehicles (among other items) at little to no cost. The DOJ’s Law Enforcement Support Office (LESO) or “1033” Program is the largest such program, having transferred over \$6B of excess equipment to 8,600+ LEAs since its 1997 inception.³ The DOJ initially required that LESO Program transfers service drug law enforcement purposes, but today the program permits transfers with few use-case constraints.⁴ Importantly, the LESO Program decommissions DOD items both with and without military attributes (the former are “controlled” and the latter are “non-controlled”). Example controlled items are mine-resistant ambush protected (MRAP) vehicles and assault rifles, and example non-controlled items are fax machines and storage cabinets. Most LESO-provisioned items are non-controlled, but controlled items’ roles in militarizing LEAs have awarded them public scrutiny.⁵

Research on police militarization offers mixed conclusions about its effects and falls into two general camps. Scholarship from the first camp reports that police militarization achieves *positive* effects across public trust and safety metrics (in other words, fulfilling its mandate). Masera (2016) finds that it is responsible for most violent crime reduction since 2007, and Bove and Gavrilova (2017) and Harris et al. (2017) find that it decreases citizens’ aggression toward and complaints against police officers (in addition to reducing crime). The argument

1. Gunderson et al. (2019)

2. Gunderson et al. (2019)

3. Office (2017)

4. Gunderson et al. (2019)

5. Office (2017)

here is that military-grade equipment makes police officers more effective at preventing or fighting crime. Conversely, scholarship from the second camp reports that police militarization achieves *negative* effects across public trust and safety metrics. Delehanty et al. (2017) find that it makes police officers more likely to kill civilians, and Mummolo (2018) finds that it decreases trust in police officers. The argument here is that military-grade equipment makes law enforcement more aggressive or proactive, with militarized police officers likely to use force and engage in deterrent behavior (such as stopping-and-frisking).

It is possible that police militarization both reduces crime and increases police violence. However, Gunderson et al. (2019) argue that conclusions from both camps depend on study-specific methods that plausibly bias results. Gunderson et al. (2019) list the variety of metrics available for measuring police militarization (such as county-year level total expenditures) and describe advantages and disadvantages associated with each. One important distinction is whether police militarization metrics include controlled items only or both controlled *and* non-controlled items. Non-controlled items do not have military attributes and, consequently, do not have controlled items' same theoretical link to police violence (i.e., making police officers more aggressive or proactive). It follows that Gunderson et al. (2019) advocate that scholars exclude non-controlled items from police militarization metrics to isolate controlled items' effects, given that police militarization's ties to trust and safety depend on items' military attributes.

However, Delehanty et al. (2017) include both controlled and non-controlled items in their study on police militarization's effects on police violence. Specifically, they estimate the effects that expenditures on *all* (rather than controlled only) LESO Program items have on counties' police killings. This police militarization metric is indirect due to its inclusion of non-controlled items and possibly biases Delehanty et al. (2017)'s conclusions (for example, if expenditures on non-controlled items correlate with expenditures on controlled items). In addition, Delehanty et al. (2017) include just four of 50 American states in their study due to then-limited police violence data, which geographic limitation might bias their conclusions

as well (for example, if their study’s states do not represent nationwide police militarization or violence trends).

I replicate Delehanty et al. (2017)’s study by defining the police militarization metric as controlled items only, as well as using nationwide police militarization and violence data, to back-check their conclusion that police militarization increases police violence. Specifically, I estimate the effect that increased spending on controlled LESO Program items has on police killings at the county level across all 50 states. Here, controlled-item LESO Program expenditures and police killings are my respective proxy variables for police militarization and violence. I hypothesize that increased police militarization causes increased police violence for two reasons. First, theoretical arguments that military-grade equipment makes police officers more prone or able to use force are plausible and convincing. Second, Delehanty et al. (2017) report that increased LESO Program expenditures cause increased police killings using data with non-controlled items that should not affect police violence, giving reason to believe that controlled items’ isolated effects on police killings should be positive as well. In addition, I hypothesize that non-controlled items impact controlled items’ estimated effects on police killings. It is plausible that expenditures on non-controlled items correlate with expenditures on controlled items and, as a result, affect the magnitude of and confidence in controlled items’ estimated effects. I interpret this potential interaction effect as evidence that non-controlled items’ inclusion in Delehanty et al. (2017)’s police militarization metric bias their conclusions (given that I do not identify convincing theoretical links between non-controlled items and police violence).

I fit three regression models to test my hypotheses (see Appendix A). To test my first hypothesis that police militarization has significant and positive effects on police violence, I re-fit Delehanty et al. (2017)’s primary regression—police killings on LESO Program expenditures—with separate variables for controlled and non-controlled items and counties from all states. To test my second hypothesis that non-controlled items impact controlled items’ measured effects, I regress police killings on *all* items’ expenditures—as Delehanty et al. (2017) do—to

observe how that affects estimates from my first hypothesis. I expect that the inclusion rather than exclusion of non-controlled items in the police militarization metric will shift expected police killings. Also, I test this hypothesis by regressing police killings on expenditures for controlled items that interact with expenditures for non-controlled items. For credibility and replication purposes, I control for Delehanty et al. (2017)’s same battery of plausibly confounding variables. In the end, I recover Delehanty et al. (2017)’s original conclusion that police militarization affects police violence significantly, and report that non-controlled items do impact controlled items’ effects, making it justifiable and important to exclude non-controlled items from police militarization metrics.

2 Data and Methods

In this section, I describe the data and methods (and attendant choices) that I use to estimate police militarization’s effects on police violence. My dependent variable and proxy for police violence is the number of police killings and my explanatory variable and proxy for police militarization is spending on controlled LESO Program items. While I use Delehanty et al. (2017)’s same sources for police killings and LESO Program expenditures, my data-sets include all (rather than just four) American states and label whether items are controlled or non-controlled. The battery of confounding variables that I control for includes violent crime, income, race, and population. I perform the analysis at the county level, which risks potential biases but allows me to replicate Delehanty et al. (2017)’s study. Importantly, my data-set includes counties with (rather than without) police killings or LESO Program expenditures for given years, and often counties have records for one or the other but not both for given years. Consequently, I base my analyses on the set of counties from my controlling data, which hedges against biases introduced by assuming that all American counties feature police militarization and violence.

2.1 Dependent Variable

I leverage police killings from the Fatal Encounters project, which provides the most comprehensive police killings data-set available. The project claims to offer fully complete records for all American states between 2000 and present, and aggregates data through paid research, public records requests, and crowdsourcing. Delehanty et al. (2017) use Fatal Encounters’ police killings data as well, but their version includes just four of 50 American states because Fatal Encounters’ records were then incomplete. Fatal Encounters includes *any* police killings rather than unlawful police killings only, making these records good proxies for police violence or the use of force generally. Other available police violence data-sets include the FBI’s Use of Force project and competing publicly-assembled data-sets, but records from both are lesser-complete.⁶

The Fatal Encounters data-set records individual police killings between 2000 and present with victims’ race, ages, and genders, as well as the killings’ date, location, and cause ($n = 25,635$). The only step I used to prepare the records for analysis was geocoding their locations to label the data with county names.⁷ I performed this step for each of my data-sets with the same geocoding service to standardize county-level naming conventions and make combining data-sets easy. Notably, African Americans are *over-represented* in the data-set relative to their share of the American populace: while they make up just $\approx 13\%$ of the country’s population (using 2017 estimates), they account for $\approx 31\%$ of police killings victims (between 2000 and present) (see Table 1).⁸ White Americans are underrepresented in the data-set (they make up $\approx 76\%$ of the country’s population and $\approx 46\%$ of police killings victims) and Hispanic Americans are about equally represented (at $\approx 19\%$ of the country’s population and police killings victims). In addition, police killings rise from 2000 until around 2014, at which time there the national spotlight on police brutality was strong and President Obama issued Executive Order (EO) 13688 to curb LESO Program sales (revoked by President Trump in

6. See Investigation (2019a) and, for example, “Mapping Police Violence” (2017).

7. I used the Google Maps API’s geocoding service.

8. For Census estimates, see Bureau (2018).

2017) (see Figure 1).⁹

2.2 Explanatory Variable

I leverage the Defense Logistic Agency’s (DLA) LESO Program expenditures data-set, which includes controlled and non-controlled item-level records between 1990 and present ($n = 151,390$). The DLA is the primary source for records on LESO Program expenditures, and provides the data for all scholarship on police militarization that I overview in Section 1.¹⁰ This data-set includes item-level LESO Program expenditures with items’ names, acquiring LEAs, quantities, values (\$US), and dates. I prepared this data-set for analysis with multiple steps. First, I geocoded items’ acquiring LEAs to label records with county names. Second, I labelled items’ groups because their names were non-standard and arbitrary. Doing so required using their 13-digit National Stock Number (NSN) codes that include items’ Federal Supply Group (FSG) codes (among other information) (see Figure 2 for the NSN schema). There are 78 Federal Supply Groups (such as *Nuclear Ordinance*). To label items’ FSGs, I pulled their FSG codes from the NSN codes and mapped FSG codes to FSG labels. Third, I used the FSG labels to bin items into four manually-created sub-groups (*Weapons*, *Vehicular*, *Mechanical*, *Office*) to make data analysis and interpretation easier. Fourth, I used the FSG labels to label items as either controlled or non-controlled. I labelled Weapons and Vehicular sub-group items as *controlled* and Mechanical and Office items as *non-controlled*.¹¹

Most dollars spent on the LESO Program across the time period went toward vehicles, and most acquired items were weapons (see Table 2). Vehicles and weapons are controlled, revealing that LEAs prioritized acquiring items with military attributes through the LESO

9. Delehanty et al. (2017), House (2017).

10. Mummolo (2018) is the exception because he does not use LESO Program expenditures to measure police militarization.

11. I did not find information describing processes for using NSN codes to label items’ control status. While Gunderson et al. (2019) discuss the importance applying that label at length, they do not describe their steps to do it. Given that Gunderson et al. (2019) describe how study-specific data choices bias conclusions about police militarization, it is important that they describe their labelling process to facilitate research and replication.

Program. In addition, expenditures for controlled items began rising around 2010 and peaking around 2014: the same relative peak of police killings and EO 13688 (see Figure 3). However, non-controlled items make up substantive portions of total acquisitions as well ($\approx 80\%$ quantity and $\approx 15\%$ cost (\$US)). Taken together, these trends suggest: first, that controlled items might have some significant relationship with police killings due to their relatively similar time-series trends; and second, that estimating that relationship with both controlled and non-controlled items might bias that relationship due to non-controlled items' non-trivial proportion of the LESO Program records.

2.3 Control Variables

I collect control variable data from two sources: the FBI's Uniform Crime Reporting (UCR) Program for violent crime data and the American Community Survey (ACS) for income, race, and population data.¹² Controlling for this specific set of variables is important for two reasons. First, each control variable has some theoretical ability to explain variation in police killings between counties. For example, it is plausible (or likely) that more populous counties have more police killings. Holding population (and each other control variable) constant allows me to isolate police militarization's effects. Second, Delehanty et al. (2017) includes these specific control variables, requiring me to include them to replicate his study credibly. The UCR Program reports county-level crime rate data between 1995 and 2017 ($n = 29,701$). Importantly, it includes self-reported crime rates only (i.e., data that counties voluntarily report). While the UCR Program reports rates from multiple crime sub-categories (such as burglary or arson), I follow Delehanty et al. (2017)'s practice in controlling for *violent* crime rates only. To prepare violent crime statistics for analysis, I aggregated and geocoded the UCR program's records from 2005 to 2017 (all years available digitally and currently). Interestingly, violent crimes drop substantially from 2007 to 2013 (see Table 3), which Masera (2016) attributes to police militarization's simultaneous rise but Gunderson et al. (2019)

12. Investigation (2019b), Bureau (2019).

argues against. The ACS provides yearly county-level demographics data between 2000 and 2017. Given that the ACS *surveys* Americans, preparing the data involved aggregating and weighting data from each county’s surveyed individuals (using either household or personal weights) to estimate each county’s total values for each demographic metric.

2.4 Methods

Testing my hypotheses involves three steps: first, I aggregate my each of four data-sets (Fatal Encounters police killings, LESO Program expenditures, UCR Program crime rates, and ACS demographics) to the county–year level; second, I combine my four aggregated data-sets; and third, I fit multiple regressions of police violence on militarization. Aggregating data-sets is straightforward and involves summing measures of interest (such as violent crime) to the county–year level. I choose counties as my geographic unit of analysis because it is straightforward to match with county-level records from my other three data sources and, importantly, because Delehanty et al. (2017) aggregate data to the county–year level. However, aggregating to the county rather than LEAs acquiring items and killing citizens obscures potential explanatory variation between observations and, as a result, possibly weakens confidence in estimated effects or biases them (likely toward the null). Gunderson et al. (2019) aggregate to the LEA–year level, but doing so here would violate my ability to replicate Delehanty et al. (2017)’s study and attribute differences between their and my estimated effects to the police militarization metric only.

Combining data-sets is less straightforward because each data-set includes different sets of county–year observations. For example, not all counties with LESO Program expenditures have police killings for the same years (or vice versa). The underlying problem here is that my data includes observations for counties *with* either LESO Program expenditures or police killings for select years, requiring that I locate counties *without* either metric to hedge against biased estimations. For example, estimating effects using counties with police killings only would discount the marginal effects that police militarization has on counties without police

killings. To solve this problem, I select counties that are present in my two control data-sets (UCR Program crime rates and ACS demographics) for analysis. That set of counties does not depend on my police militarization or violence data-sets, allowing me to retain county-year observations with and without LESO Program expenditures and police killings. I start by combining my control data-sets and dropping non-matching county-year observations. This produces county-year observations between 2005 and 2016, which are the UCR Program data-set's starting and ending years. Then, I add my police militarization and violence data-sets (Fatal Encounters police killings and LESO Program expenditures) and record zero-values for non-matching county-year observations. After this combination process, I have 3,360 county-year observations.

To test whether police militarization increases police violence and explore the effects that non-military items have on that relationship, I first regress police killings on military-item LESO Program expenditures while controlling for non-military items (see the Controlled Items Model in Appendix A). Controlling for non-military items allows me to isolate military items' marginal effects while also observing non-military items' effects. I log expenditures to make interpreting results easier because the data are skewed, with few county-year records taking on very high values (see Figure 4). Also, I use a negative binomial regression (as do Delehanty et al. (2017)), which is appropriate because the logged expenditures can be treated as count variables and many county-year observations have zero values for expenditures and killings. In addition to controlling non-military items, I control for violent crime, income, race, and population. This regression informs about both military and non-military items' marginal effects on police killings.

To explore whether and how non-military items affect estimates in greater depth, I run two additional negative binomial regressions. The first regresses police killings on logged expenditures for all items (military and non-military items combined), which is Delehanty et al. (2017)'s primary regression (see the All Items Model in Appendix A). The second regresses police killings on logged expenditures for military items interacting with logged

expenditures for non-military items (see the Interactions Model in Appendix A). Each informs about the impact that including non-military items in the police militarization metric has on estimated effects. Finally, I estimate the correlation coefficient between logged expenditures for military and non-military items to infer whether the two are co-linear.

3 Results

From the Controlled Items Model’s results, I find that police militarization increases police violence when regressing military-item LESO Program expenditures increase the expected number of police killings ($\beta = 0.011$; $p = 0.021$) (see Table 4). This suggests that Delehanty et al. (2017)’s observation that police militarization increases police violence holds when using the improved police militarization metric that includes controlled items only. Figure 5 informs that counties without LESO Program expenditures for given years can expect 1.08 police killings, whereas counties with the maximum amount of LESO Program expenditures can expect 1.31 police killings. Put differently, receiving the maximum rather than minimum amount of military equipment increases expected police killings by about 21 percentage points. In line with Delehanty et al. (2017), I find that income and population affect police killings significantly while—to my surprise—violent crime does not. In addition, this first regression informs that LESO Program expenditures on non-military items affect estimates for expected police killings as well. Given that there is no strong theoretical link between non-military items and police violence, this suggests expenditures on non-military items might correlate with those on military items. Consequently, including non-military items might bias or add unhelpful noise to estimated effects.

While I find that military and non-military items’ expenditures do not correlate *very* strongly (Pearson’s $r = 0.427$), the All Items and Interactions Models inform that non-military items impact military items’ estimated effects. Specifically, the All Items Model suggests that defining the police militarization metric as both military and non-military items inflates

expected police killings estimates (See Table 2 and 7). This is likely because both military and non-military items register positive marginal effects on police killings: including non-military items in the police militarization metric (the All Items Model) boosts the Controlled Items Model's effects. In addition, the Interactions Model suggests that non-military items interact with military items: the marginal effects of military items on police killings *depend* on the value of non-military items, with higher expenditures for non-military moderating the effects that varied expenditures for military items have on police killings (see Figure 6). Put differently, the presence of expenditures for non-military items affects how strongly military items increase police violence.

The All Items and Interactions Models provide relatively clear evidence that including non-military items in the police militarizing metric impacts estimated effects, which suggests that excluding non-military items from police militarization metrics is important. Items with military attributes make up the theoretically interesting and direct definition of police militarization, so excluding non-military items that are not proxy variables for police militarization but do impact its estimates effects has clear value. Generally, I interpret the inclusion of non-military items in police militarization metrics as unhelpful noise that bias Delehanty et al. (2017)'s conclusions for two reasons. First, while while Table 4 and Figures 7 and 6 suggest that spending on non-military items affects police militarizations' estimated effects on police killings, there is no convincing theoretical link between non-military items and police killings. Second, given that there is some correlation between the two metrics, it is plausible that non-military items confound the relationship between military items and police killings (rather than that non-military items themselves cause police killings). If that argument is reasonable, I would interpret those figures not as evidence that non-military items affect police killings, but rather that it is justifiable and important to include military items only in police militarization metrics (as Gunderson et al. (2019) argue).

4 Conclusion

In this paper, I make two primary points: first, excluding non-controlled LESO Program items from Delehanty et al. (2017)’s police militarization metric retains their conclusion that police militarization increases police violence; and second, including non-controlled LESO Program items in Delehanty et al. (2017)’s police militarization metric impacts police militarization’s estimated effects on police violence. The key takeaway here is that defining police militarization directly using controlled items only is important, and doing so suggests that police militarization does increase police violence. This finding is important because police–citizen interactions are foundations to the public’s trust and safety, along with other civic engagement metrics. Gunderson et al. (2019) Violating that relationship through militarization-induced violence can harm not only those involved but also democracy generally. Consequently, it is worthwhile to reconsider the enabling federal grant-in-aid programs (specifically, the LESO Program), given the relative freedom that LEAs have to acquire and use military-grade equipment during daily law enforcement.

To be sure, several data and methods details limit my findings. For one, I measure variation at the county level, which Gunderson et al. (2019) explain might obscure variation present at the LEA level. In addition, I base my analysis on counties present in the UCR Program’s crime rate and ACS’s demographics data. While this helps measure variation between counties with *and* without either police militarization or killings records for given years, the sample is neither random nor complete, and it is difficult to understand how this choice affects estimates. These are some of the largest challenges associated with understanding police militarization’s effects: it is interpret research that depends on limited data and subjective choices. Consequently, I think one next step is performing smaller-scale but more in-depth case studies that inform about police militarization’s effects in select areas. For example, researchers might gather their own data in for several cities rather than rely on observational data that is necessarily limited. Still, this research is important in bolstering prior scholarship that suggests both that police militarization might be an ineffective and harmful tool.

References

- Bove, Vincenzo, and Evelina Gavrilova. 2017. "Police officer on the frontline or a soldier? the effect of police militarization on crime." *American Economic Journal: Economic Policy* 9 (3): 1–18.
- Bureau, United States Census. 2018. "QuickFacts." <https://www.census.gov/quickfacts/fact/table/US/PST045217>.
- . 2019. "American Community Survey (ACS)." <https://www.census.gov/programs-surveys/acs>.
- Delehanty, Casey, Jack Mewhirter, Ryan Welch, and Jason Wilks. 2017. "Militarization and police violence: The case of the 1033 program." *Research & Politics* 4 (2): 2053168017712885.
- Gunderson, Anna, Elisha Cohen, Kaylyn Jackson, Tom S Clark, Adam Glynn, and Michael Leo Owens. 2019. "Does Military Aid to Police Decrease Crime? Counterevidence from the Federal 1033 Program and Local Police Jurisdictions in the United States."
- Harris, Matthew C, Jinseong Park, Donald J Bruce, and Matthew N Murray. 2017. "Peace-keeping force: Effects of providing tactical equipment to local law enforcement." *American Economic Journal: Economic Policy* 9 (3): 291–313.
- House, White. 2017. "Presidential Executive Order on Restoring State, Tribal, and Local Law Enforcement's Access to Life-Saving Equipment and Resources." <https://www.whitehouse.gov/presidential-actions/presidential-executive-order-restoring-state-tribal-local-law-enforcements-access-life-saving-equipment-resources/>.
- Investigation, Federal Bureau of. 2019a. "National Use-of-Force Data Collection." <https://www.fbi.gov/services/cjis/ucr/use-of-force>.

Investigation, Federal Bureau of. 2019b. “Uniform Crime Reporting (UCR) Program.” <https://www.fbi.gov/services/cjis/ucr>.

“Mapping Police Violence.” 2017. <https://mappingpoliceviolence.org/>.

Masera, Federico. 2016. “Bringing war home: Violent crime, police killings and the overmilitarization of the US police.”

Mummolo, Jonathan. 2018. “Militarization fails to enhance police safety or reduce crime but may harm police reputation.” *Proceedings of the National Academy of Sciences* 115 (37): 9181–9186.

Office, Government Accountability. 2017. “DOD Excess Property: Enhanced Controls Needed for Access to Excess Controlled Property.” <https://www.gao.gov/assets/690/685916.pdf>.

Appendix A Models

$$Killings = \beta_0 + \beta_1 E_{CI} + \beta_2 E_{NCI} + \beta_3 X_{c,i,p,bp} \quad (\text{Controlled Items})$$

$$Killings = \beta_0 + \beta_1 \beta_2 E_{CI} \cdot \beta_2 \beta_2 E_{NCI} + \beta_3 X_{c,i,p,bp} \quad (\text{Interactions})$$

$$Killings = \beta_0 + \beta_1 \beta_2 E_{AI} + \beta_2 X_{c,i,p,bp} \quad (\text{All Items})$$

where:

$Killings$ = expected police killings

E_{CI} = controlled-item expenditures (logged)

E_{NCI} = non-controlled-item expenditures (logged)

E_{AI} = all-item expenditures (logged)

X_c = Violent crime rate

X_i = Median income

X_p = Population

X_{bp} = Black population

Appendix B Figures

Figure 1: Police Killings by Race and Time

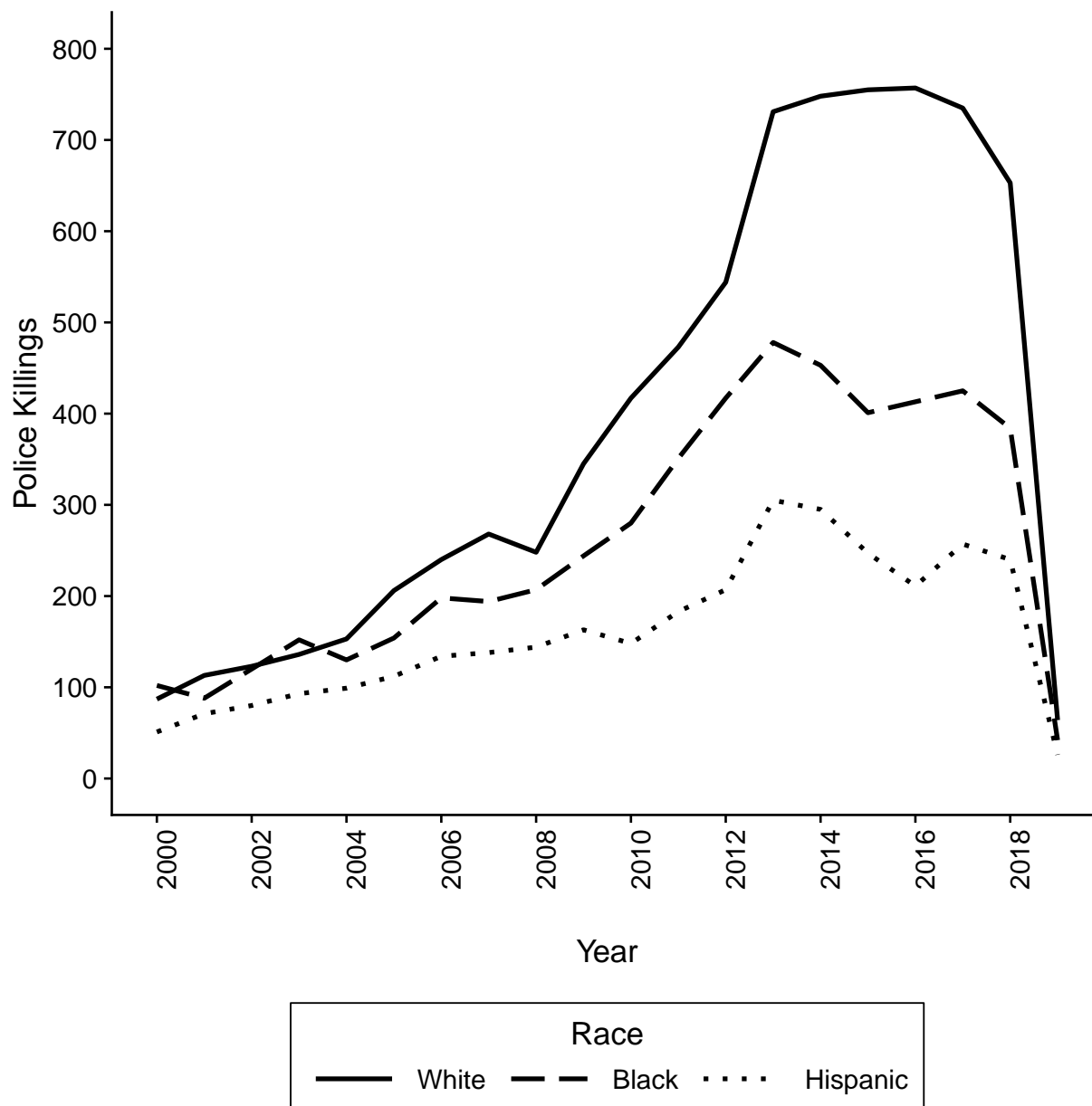
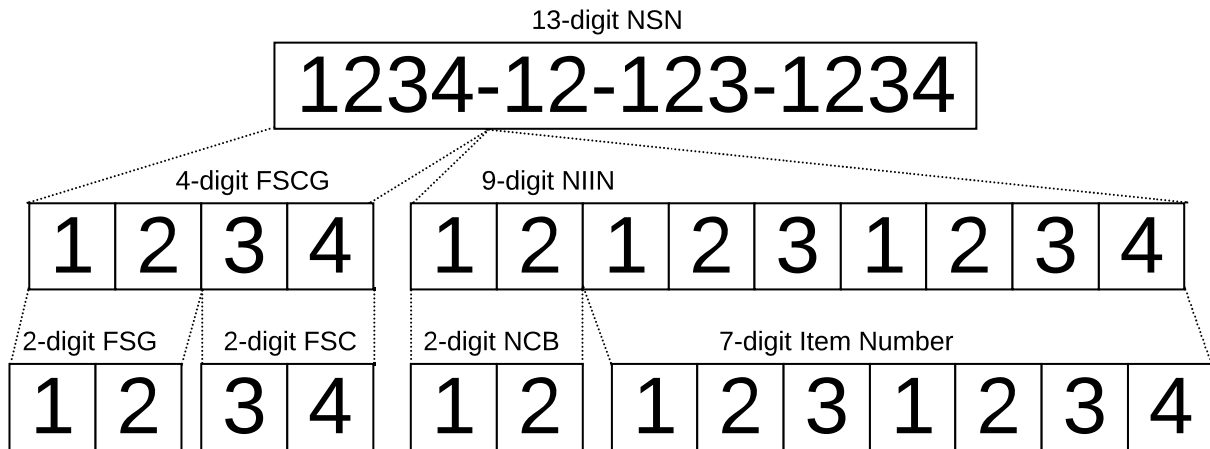


Figure 2: National Stock Number (NSN) Schema



This is the schema for mapping NSN codes to items' FSG codes. I use this schema to label items with their FSG codes.

Figure 3: LESO Program Expenditures by Control Status and Time

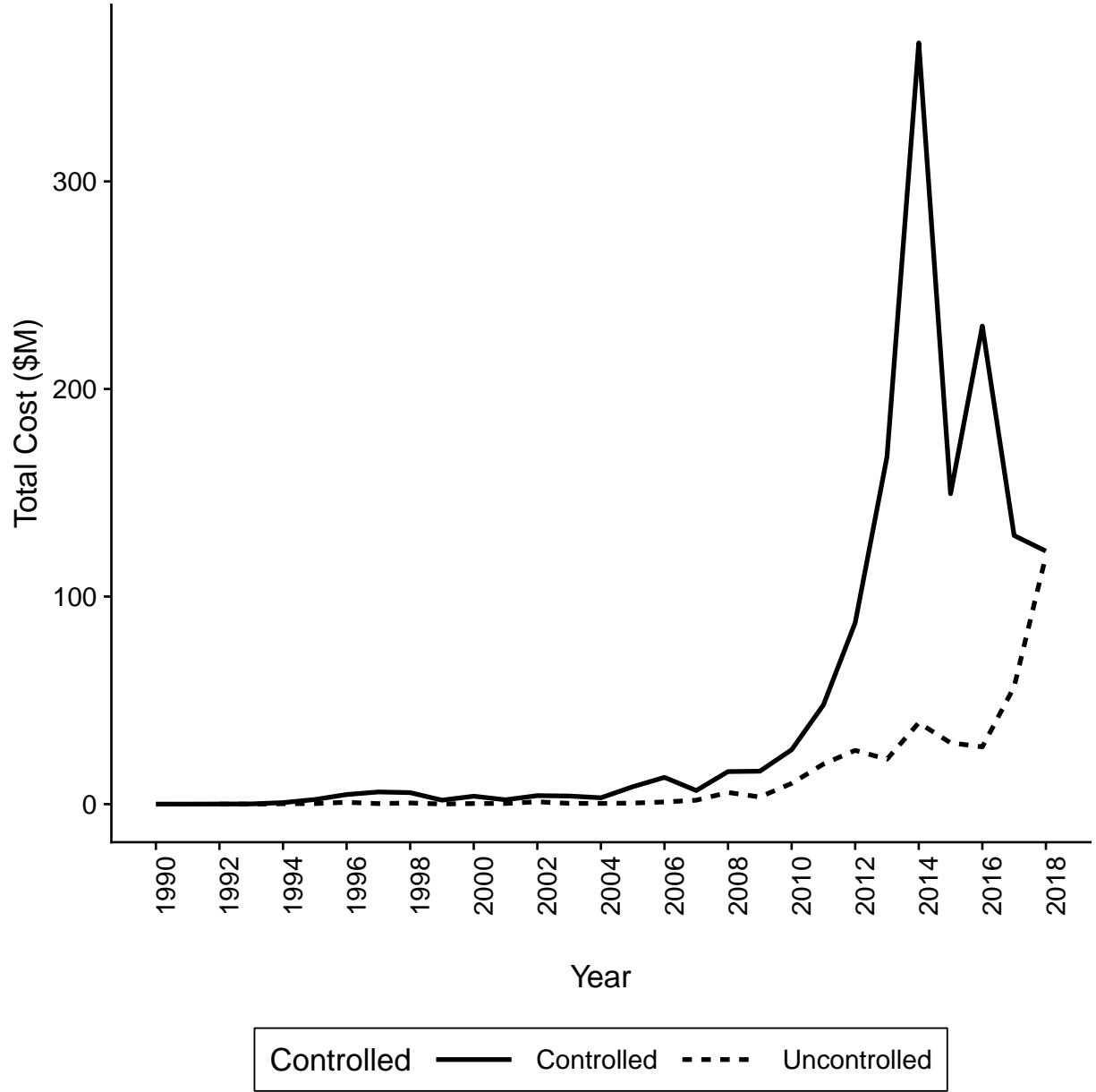
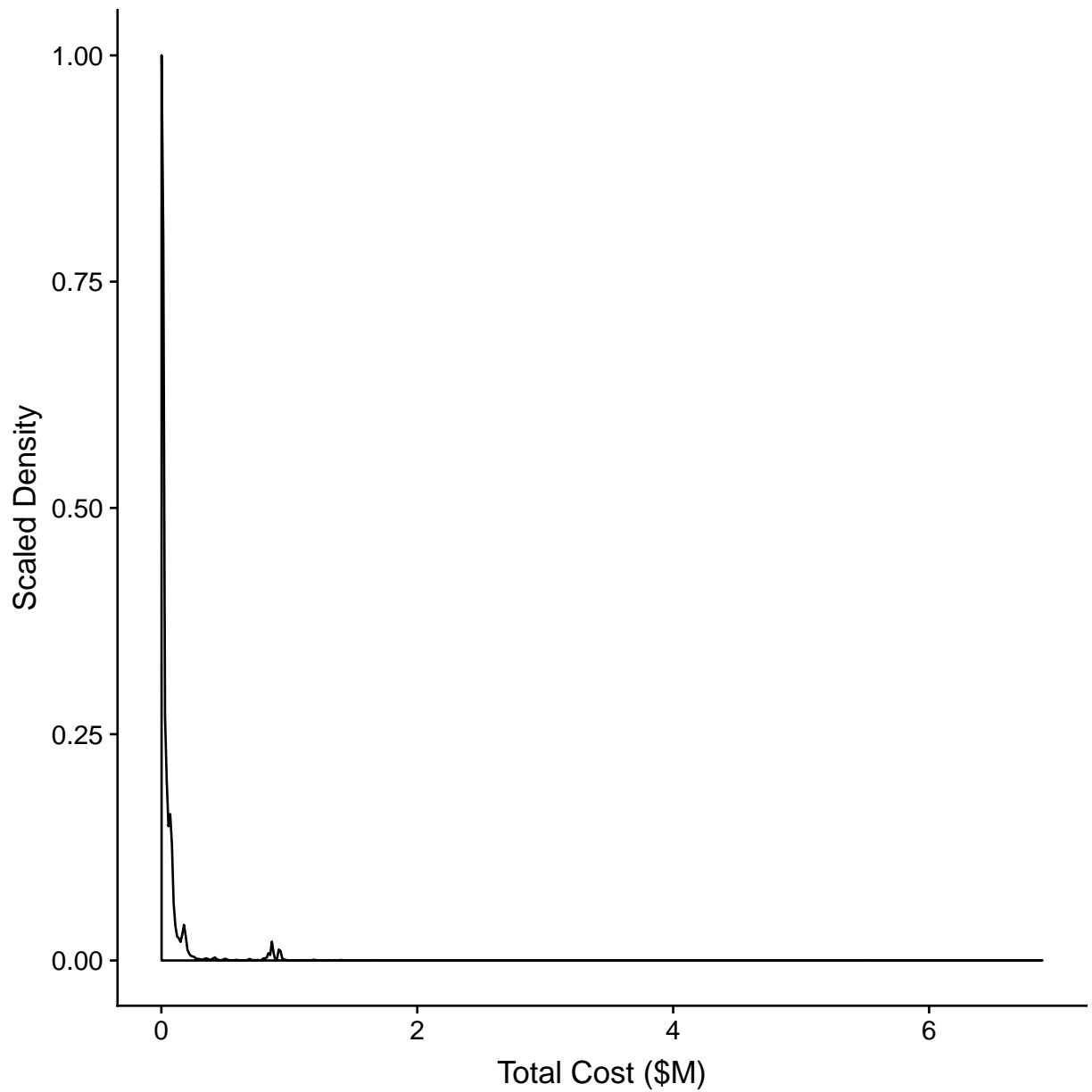
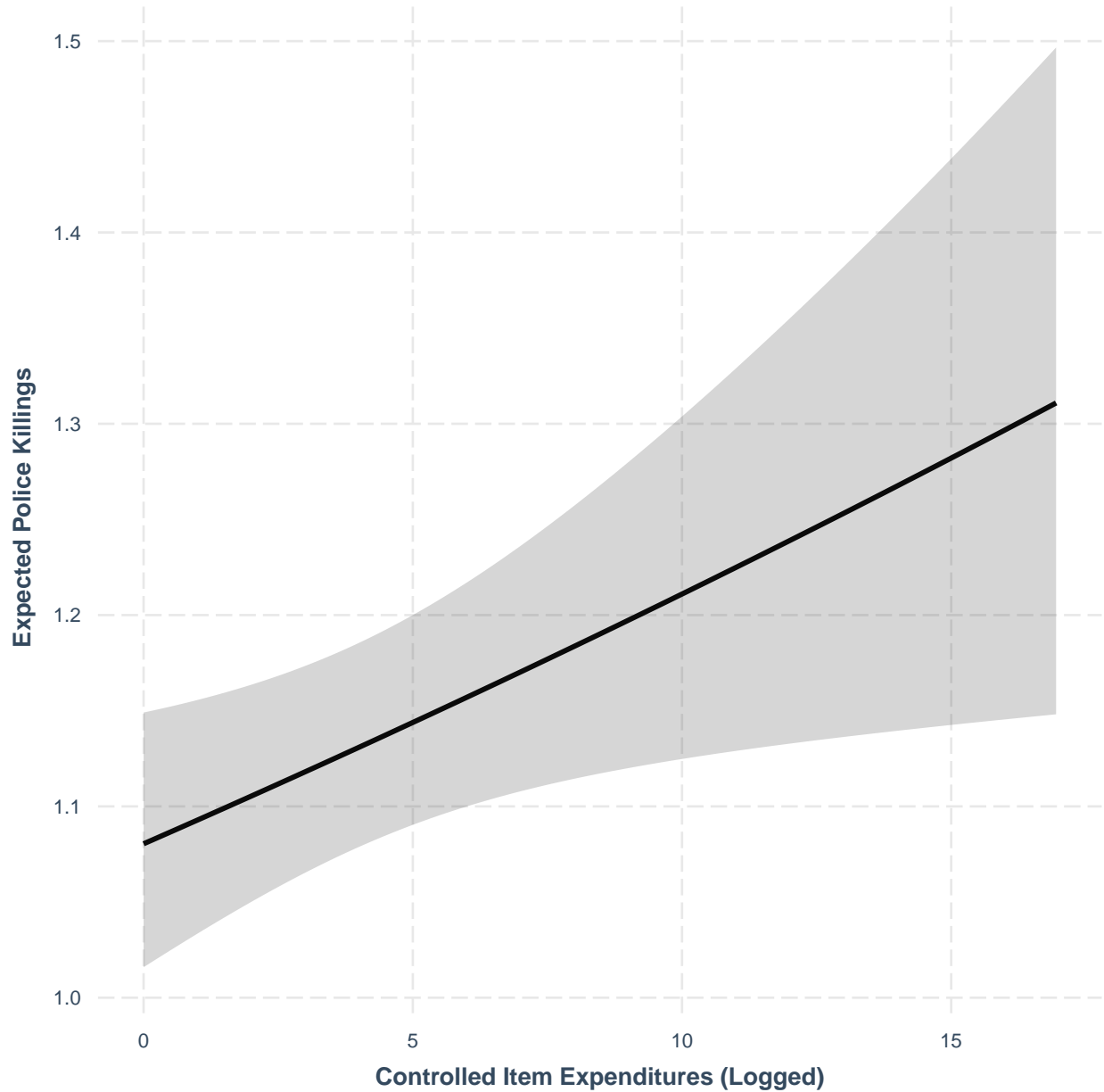


Figure 4: LESO Program Expenditures Density



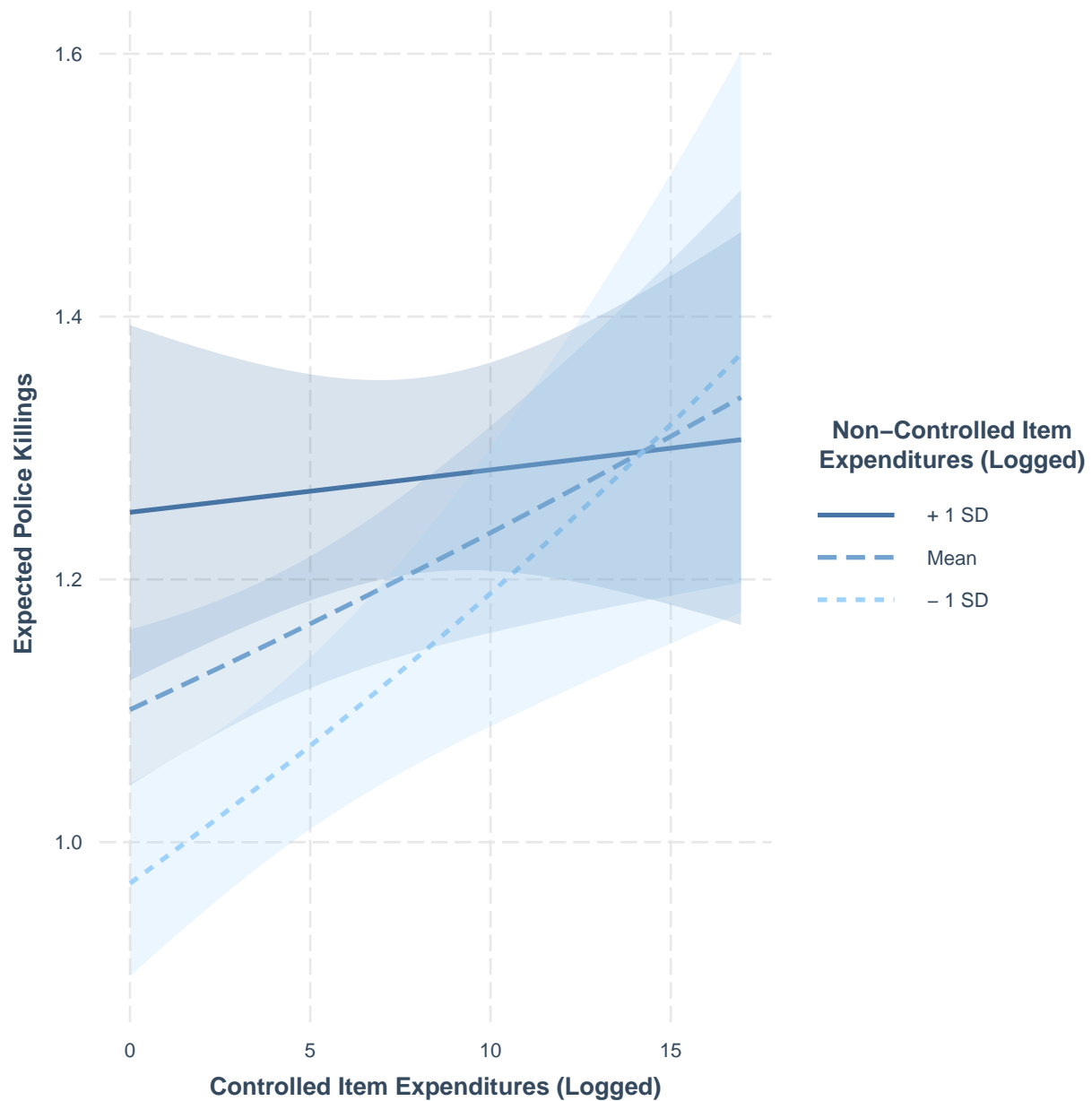
This figure reveals that the costs of the LESO Program expenditures are highly skewed, with most expenditures clustered around the origin (below \$1M) and few taking on large values (above \$1M).

Figure 5: Controlled Items' Marginal Effects on Expected Police Killings



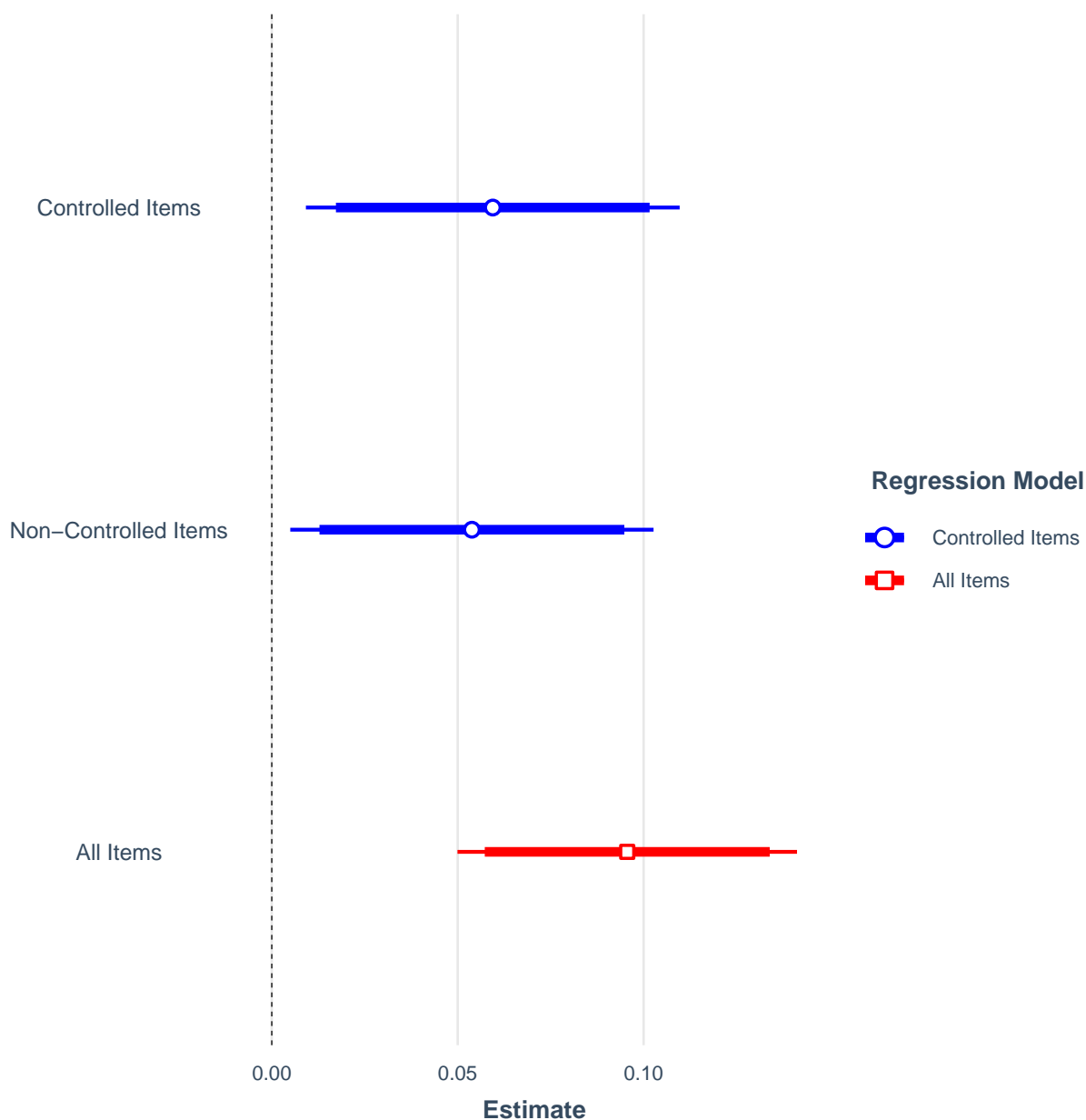
This figure plots the Controlled Items Model's estimated effects (i.e., expected police killings) across the interval of controlled item expenditures (logged). These expenditures are based on item's *costs* (quantity · value). The graph shows that county's that spend more on LESO Program military items can expect more civilian killings.

Figure 6: Controlled and Non-Controlled Items' Interaction Effects on Expected Police Killings



This figure plots the Interactions Model's estimated interaction effects expenditures on controlled and non-controlled LESO Program items. Specifically, larger expenditures for non-controlled items moderate controlled items' effects on police violence. This is not to say that non-controlled items actually do affect police violence, but rather that non-controlled items have some impact on controlled items' estimated effects.

Figure 7: Controlled and All Items' Marginal Effects on Expected Police Killings



This figure's blue "Controlled Items" model plots the Controlled Items Model's estimated effects of expenditures on controlled and non-controlled items on police violence. The red "All Items" model plots the All Items Model's estimated effects of expenditures on all items on police violence. The plot shows estimates' 90% and 95% confidence intervals along the bars' respective inner and outer bands. This graph is meant to contrast my Controlled Items Model against the All Items Model that follows Delehanty et al. (2017)'s regression specification. The important takeaway here is that both controlled and non-controlled items have statistically significant and positive effects on police violence, which suggests that the All Items Model might inflate estimated effects on police killings.

Appendix C Tables

Table 1: Police Killings by Race

| | Deaths | (%) |
|-----------------|--------|-------|
| | 16,889 | 100.0 |
| White | 7,801 | 46.2 |
| Black | 5,235 | 31.0 |
| Hispanic | 3,205 | 19.0 |
| Asian | 360 | 2.1 |
| Native American | 247 | 1.5 |
| Middle Eastern | 41 | 0.2 |

Table 2: LESO Program Expenditures

| | | Cost | | Value | | Quantity | |
|----------------|------------|-------|-------|-------|-------|----------|-------|
| | | \$M | (%) | \$M | (%) | <i>n</i> | (%) |
| | | 1,525 | 100.0 | 0.81 | 100.0 | 616,568 | 100.0 |
| Controlled | Vehicular | 1,113 | 72.9 | 1,078 | 79.9 | 20177 | 3.3 |
| | Weapons | 128 | 8.4 | 92 | 6.8 | 275646 | 44.7 |
| Non-Controlled | Mechanical | 188 | 12.3 | 128 | 9.5 | 103793 | 16.8 |
| | Office | 96 | 6.3 | 52 | 3.9 | 216952 | 35.2 |

This table describes LESO Program expenditures across sub-groups. The DLA's data-set includes the Value and Quantity metrics, which describe each item's worth and purchase quantity. I multiply those to measure the amount that LEAs spent on that item (Cost). Note that the LESO Program subsidizes these expenditures.

Table 3: Violent Crime Rates by Time

| Year | Violent Crime | Change (%) |
|------|---------------|------------|
| 2005 | 240,320 | — |
| 2006 | 242,929 | 1.1 |
| 2007 | 245,181 | 0.9 |
| 2008 | 238,963 | -2.6 |
| 2009 | 235,347 | -1.5 |
| 2010 | 219,932 | -7.0 |
| 2011 | 204,065 | -7.8 |
| 2012 | 206,542 | 1.2 |
| 2013 | 200,850 | -2.8 |
| 2014 | 202,414 | 0.8 |
| 2015 | 209,988 | 3.6 |
| 2016 | 221,678 | 5.3 |

Table 4: Regression Results, Police Killings on LESO Program Expenditures

| | Controlled Items | | All Items | |
|----------------------------------|-------------------------------|---------------------------|--------------------------|--------------------------------------|
| | (1) Controlled Items Model | (2) Interactions Model | (3) All Items Model | (4) Delehanty et al. (2017) Model |
| <i>LESO Program Expenditures</i> | | | | |
| Controlled Items | 1.14e-2* (4.92e-3) | 1.55e-2** (5.29e-3) | | |
| Non-Controlled Items | 1.48e-2* (6.87e-3) | 3.53e-2** (1.27e-2) | | |
| Controlled-Non-Controlled | | -2.48e-3 (1.29e-3) | | |
| All Items | | | 1.27e-2*** (3.10e-3) | 5.50e-2** (2.30e-2) |
| <i>Controls</i> | | | | |
| Violent Crime | 4.05e-5 (2.51e-5) | 3.92e-5 (2.50e-5) | 4.02e-5 (2.50e-5) | 2.30e-2 (4.40e-2) |
| Median Income | -1.13e-5*** (1.56e-6) | -1.13e-5*** (1.56e-6) | -1.13e-5*** (1.56e-6) | 9.10e-1* (5.49e-1) |
| Population | 1.21e-6*** (3.64e-8) | 1.20e-6*** (3.64e-8) | 1.21e-6*** (3.62e-8) | 8.72e-1** (3.39e-1) |
| Black Population | 9.84e-7*** (2.25e-7) | 9.89e-7*** (2.25e-7) | 9.76e-7*** (2.25e-7) | 4.00e-2 (1.62e-1) |
| Constant | 8.13e-2 (9.73e-2) | 7.00e-2 (9.75e-2) | 7.86e-2 (9.69e-2) | 3.87e-1 (5.59) |
| Observations | 3,360 | 3,360 | 3,360 | 390 |

*p < 0.05; **p < 0.01; ***p < 0.001. Standard errors parenthesized.