Taser Devices: Reconsidering the Future of Policing Technology

Exploring Officer Safety Measures Across 18 U.S. Police Departments

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Abstract

Many of the largest police departments in the U.S. have received significant funding in the past two decades to improve police equipment and acquire cutting-edge technology. One such device is the Taser, advertised as a non-lethal alternative capable of gaining submission for police officers. The existing literature has demonstrated that Tasers have had mixed results in reducing officer injuries. I consider a balanced dataset of 18 police departments in the U.S. along with officer safety measures collected through the Law Enforcement Officers Killed and Assaulted (LEOKA) database from 2000-2019. I examine how the introduction of Taser devices and the expansion of Taser devices within a department affect six officer safety measures – officer deaths by felony, total assaults, unarmed assaults with and without injury, and gun assaults with and without injury. Using a Poisson regression model, I apply difference-in-differences estimation techniques. I find that the provisioning of Tasers to a select group of officers led to a 38% decrease in injuries from unarmed assaults with decreases across all safety categories. When Taser devices are expanded so that all patrol officers have access to a device, injuries to officers from unarmed assaults increase by an offsetting amount such that the additive effect is a 7% increase. Across officer safety measures, the expansion of Taser devices to all patrol officers negates the safety improvements of having any Taser program.

1 Introduction

Crime and policing in the U.S. have commanded the national spotlight for significant parts of the past decade as police departments have grappled with the challenge of balancing order while maintaining proper levels of force. From Ferguson in 2014 to Minneapolis in 2020, among dozens of other incidents that have led to calls for police reform, police departments have come under significant scrutiny. To address controversies in the use of force and improve officer safety, Axon Enterprise Inc., formerly known as Taser International, has touted its Taser products as the future of policing: a nonlethal, conductive electrical weapon capable of gaining physical compliance and helping officers de-escalate. If Tasers function as advertised, police departments that use Tasers may see improvements in officer safety outcomes.

In addition to improving officer safety, one motivation to support Taser use is that Tasers claim to provide sufficient force to subdue resistors while mitigating civilian injury. Policing costs continue to escalate, and excessive use of force stands out as one area where departments are keen to cut costs accumulated through legal fees and settlements.[1]

There are several mechanisms by which Tasers may influence officer safety. The most direct of these is that Taser devices, when used properly, can subdue a resistor and provide control for a police officer more effectively than alternative intermediate force options. The theoretical literature on crime economics offers several additional motivations as Tasers may change civilian and police behavior. In his foundational piece, Becker (1968) notes that rational agents who consider committing crimes weigh the probability of getting caught against the severity of punishment. Tasers could increase the probability of arrest if they help officers more rapidly gain control in situations where

[1] The VERA Institute reports local governments cumulatively spent over $115 billion across the U.S. in 2020 with many spending over one-third of their annual budget on policing (VERA Institute of Justice, 2020). The National Police Funding Database reports that departments have paid over $2.2 billion in reported settlements across the nation since 2004; Los Angeles paid excesses of $190 million from 2005 to 2018 in legal costs from use of force cases while Washington, D.C. spent $30.5 million on police misconduct from 2010-2014 (Thurgood Marshall Institute, 2020). While not the focus of my paper, examining how Tasers affect policing costs stands as another avenue for future research.

firearm use would violate department use of force guidelines. Furthermore, if officer injuries decrease after Tasers are implemented, then long-term police productivity will increase as fewer take injury leave, which could indirectly improve the safety of other officers. However, there is also some literature on policing that notes the potential for abuse of Tasers; officers may become overly reliant on Tasers and risk increasing total force incidents (Alpert & Dunham, 2010; Bulman, 2010).

A number of studies have empirically explored Taser programs and the effects these devices have on officer safety measures. These studies can be split into two groups, based on methodology. The first collection assesses Taser effects using descriptive statistics and comparing safety outcomes within a given department. This group of studies finds that Taser devices are associated with lower officer injury rates per use of force (i.e., among force incidents, those in which a Taser is used are associated with fewer officer injuries). A second group of studies leverages causal inference techniques. Two of the studies with the largest sample sizes reach different conclusions. Taylor et al. (2012) collects data from 13 departments with four years of data from each and finds that Tasers reduce officer injuries. A separate study of 12 agencies across 10 years finds that Tasers have no effect on officer total injuries (Smith et al., 2010). One study of the Chicago Police Department concludes that expanding Taser devices to all patrol officers had no effect on overall injuries or the injury rate per use of force (Ba & Grogger, 2018).

I expand the existing literature in two ways. First, I consider Taser programs across a wider sample of agencies and years, examining 18 of the largest police departments in the U.S. from 2000-2019. In considering annual aggregate data, I allow for the possibility that Tasers may affect overall use of force incident counts rather than just considering how Tasers affect officer injury rates per use of force incident. The second contribution is considering one aspect of Taser adoption heterogeneity - whether departments deploy Taser devices to all patrol officers, or if they restrict Taser use to more senior officers or tactical units.

Data on Taser program implementations and expansions come from several sources. Departments that publish annual reports or use of force summaries detail their Taser programs. Archived local news reports confirm information about Taser programs and police use of force policies, particularly for early years in the sample time period. Axon quarterly reports detail Taser sales to agencies to identify departments that expand their Taser devices to all patrol officers. I collect measures of officer safety from Law Enforcement Officers Killed and Assaulted (LEOKA) data to assess how Tasers have impacted officer death rates and assault rates by firearm or unarmed. I consider assaults that cause officer injuries as well as reported assaults not linked to officer injuries. I collect data on several county-level demographic and socioeconomic variables using data from the 1-year American Community Survey (ACS) estimates.

I conduct a difference-in-differences analysis using Poisson regression models to compare several measures of police safety among police departments that implement a Taser program versus those that do not. I also consider the effects that expanding Tasers have on officer safety, further distinguishing between departments that restrict Taser use to more senior officers versus those that equip all patrol officers with them. I include county and year fixed effects to control for unobserved factors that differ by county or over time. I include an additional set of control variables across models, including the percentage of the county population that is at or below the federal poverty line; the percentage of each county's population that is Black, White, or other; and the percentage of the population that is between the ages of 15 and 30. Use of these covariates, or some variation thereof, is consistent in the crime literature (Ulmer & Steffensmeier, 2014). My specifications model the following officer safety measures: deaths by felony, total assaults, unarmed assaults with and without officer injury, and gun assaults with and without officer injury. All outcome variables are given as rates per 100,000 population served per year. Lastly, I include a robustness check for my preferred specification on police officer deaths by accident. As these deaths are almost entirely determined by car accidents, I expect the use of Tasers to have no effect on this outcome.[2]

I find that police departments with any Taser implementation, compared to departments without Tasers, receive statistically significant reductions in officer injuries, including a 38% decrease in injuries from unarmed assaults. When Taser devices are expanded to all patrol officers, the effects are reversed. I find several statistically significant increases in officer injury categories, offsetting gains incurred with the limited deployment. Overall, the net effect of expanding Tasers to all patrol officers is a non-significant increase in officer injuries. I consider a second model to consider the effects of Tasers over time. Several safety category-specific trends emerge, but no clear conclusions can be made. I repeat my main analysis on a separate group of departments for which I have data and that have never expanded Taser devices to all patrol, and my results again suggest that limited Taser deployment is associated with significant reductions of officer injuries. I end my analysis with a robustness check that uses officer deaths by accident. The robustness check shows no relationship between Taser devices and accidental officer deaths in three of four specifications, but one robustness model raises potential concerns that I address.

The rest of the paper is structured as follows. Section 2 describes Taser

[2] In my additional specifications, I compare results using an ordinary least squares regression model, finding similar coefficient magnitudes and significance. These are reported in the OLS Specifications and Full Sample Results subsection of the appendix.

devices, reviews Taser policies across police departments, and introduces some theoretical expectations for how Tasers affect police safety. In Section 3, I review the existing literature on Tasers and the effects these devices have on policing outcomes. I describe my data in greater detail and provide some initial descriptive statistics in Section 4. I describe my full empirical specifications and different methods of evaluating the effects that Tasers have on police safety in Section 5, and I provide my main results in Section 6. I describe a robustness check in Section 7. I conclude in Section 8.

2 Institutional Background and Theory

2.1 Taser Devices and Policing

Taser devices, also known as conductive energy devices or electronic control weapons, are policing tools designed to provide law enforcement officers with a non-lethal weapon alternative to firearms. Tasers are typically one of several intermediate use of force options available to officers, along with batons and pepper spray (OC spray). Tasers can be used in two ways, the first of which is by firing two electrical probes leading to involuntary muscle contraction (Baliatsas et al., 2021). When fired, Tasers use an internal compressed gas chamber to fire darts up to 15 feet. Tasers, when used correctly, produce as much as 50,000 volts of electricity, which disable individuals and facilitate law enforcement officials attempting to subdue and arrest them. When hit with electrical current, some individuals may collapse, potentially leading to additional injuries (Bulman, 2010). Tasers can also be applied in drive stun mode, in which the Taser is directly applied to the skin. Drive stun mode lacks the ability to cause the same muscle failure in subjects because electrical probes are not properly spaced far enough apart across a muscle group, and officers who use drive stun mode rely on pain compliance (Somers et al., 2020).

Tasers have spread throughout 15,000 military and civilian law enforcement agencies (Bulman, 2010). Despite their ubiquity in U.S. policing, Tasers can cause serious injuries. One news organization has documented over 1,000 civilian deaths following Taser use, and Tasers have been cited as a contributing cause of death in 153 of these (Reuters, 2017). Tasers have become controversial and generated significant public protest as well from police accidents in which firearms are mistakenly used instead of Tasers (New York Times, 2022).

2.2 Taser Policies

There are some Taser use guidelines established by Axon that police departments generally follow, but departments maintain the final authority in setting their own use of force policies. Some widely implemented regulations include that Tasers should not be used on the neck, face, or genital areas. Most departments recommend Tasers not be used against certain groups like pregnant women or the elderly. Police departments may still differ in their Taser policies across several important dimensions.

There is significant variation in how Tasers may be used across police departments in the U.S., starting with the circumstances under which officers may deploy Taser devices. Generally, departments outline three tiers of resistance: active aggression, active resistance, and passive resistance. One sample definition can be found in the Baltimore Police Department 2019 Use of Force Policy 1115, which defines active aggression as "when a person attacks or attempts to attack a member or another person. . . strikes, kicks, or attempted strikes or kicks with hands, fists, the head, elbows, knees, or an instrument." Active resistance is "when a person moves to avoid detention or arrest but does not attack" and can include fleeing or physical resistance to being handcuffed or grasped.

Finally, passive resistance is a "failure to comply with the member's [officer's] commands without the attempt to flee." Definitions have remained essentially common across departments and throughout iterations of use of force policies.

The departments with the most permissive Taser use of force guidelines allow officers to use Taser devices on passively resisting civilians. Baltimore Police Department, from 2010 until a policy revision in 2016, allowed Taser use when "verbal persuasion" proved "ineffective. . . or the subject refuse[d] to comply" (Baltimore Police Department Training Bulletin Guideline 2010). Other departments restrict officer use of Tasers to "a situation where the subject is escalating from passive resistance to active resistance" (Atlanta Police Department Policy Manual 2020, 4.2.1). Finally, the most stringent policies only permit Taser use against active aggressors. For example, a 2011 report on 20 Arizona law enforcement agencies noted that Tucson only permitted Taser use against active aggressors (American Civil Liberties Union of Arizona 2011).

In my primary dataset with 18 departments, three departments allow Taser use against passive resistors for some period of time; one of these, Dekalb County, permits use against passive resistors throughout the time period, so its policy is absorbed by fixed effects. I describe this further in my Empirical Methods. The other two departments that permit Taser use against passive resistors are Philadelphia and Los Angeles; Philadelphia permits use of Tasers against passive resistors for nine years, and Los Angeles permits use for six years.

The final dimension for examining Taser use concerns Taser availability. Departments that outfit only some of their officers with Tasers may find that Tasers provide different results than departments that outfit all patrol officers. This decision varies most often by funding availability. Often, departments will use a pilot program to test Taser use and performance in a limited part of their police force for several months to a year. Others departments purchase a large quantity of Tasers to outfit all patrol officers with one purchase.

2.3 Theory

Becker established key foundations in considering economic models of crime and policing that apply to considerations of Taser use. Becker claims that "an increase in a person's probability of conviction or punishment if convicted would generally decrease... the number of offenses he commits," establishing two mechanisms by which Tasers may influence the production of crime (Becker, 1968). Tasers could increase the probability of conviction for a crime. Tasers may help officers more rapidly gain control in situations where firearm use would violate department use of force guidelines (i.e., where there is resistance or aggression but not lethal force directed against officers). The most direct effect of these devices is to assist officers in more safely subduing resistors. While Taser use against fleeing individuals is now largely prohibited, it was permitted in nearly 20% of the department-year observations from 2000-2019. One advantage of Tasers is that Taser probes can be used to gain compliance from farther distances than other intermediate force techniques available, so Taser use against someone fleeing, when permitted, could have increased the probability of being apprehended by police. Tasers may also increase the immediate punishments for crime. The anecdotal evidence of the severe reactions from Taser use, including death or hospitalization, suggests that Tasers may in fact be a more severe punishment than batons or OC spray (Reuters, 2017). Becker's function relating an individual's number of crimes committed as dependent upon the probability of conviction, punishment if convicted, and other variables such as income and risk aversion (Equation 12) suggests that Tasers may lower the number of crimes (Becker, 1968).

This discussion of criminal behavior and activity is relevant to considering officer safety. Owens connects Becker's model to police production; if force can "incapacitate an individual who would otherwise continue to offend," then there are potential gains in police officer future productivity attributable to health improvements (Owens, 2022). Officers are most likely to be assaulted and killed by felonious acts when responding to calls regarding criminal activity. An increase in probability of conviction or severity of punishment reduces utility gained from crime and therefore reduces crime output, according to Becker's rational crime model. If criminals commit fewer crimes, there will be fewer officer injuries in a given time period.

A second causal pathway by which Tasers affect policing outcomes occurs through their ability to reduce officer injury rates per use of force incident. If Tasers allow officers to subdue resistors from a farther distance or gain compliance more readily, then officer injuries decrease. This may be apparent theoretically when comparing force from baton use (in which officers must be close enough to strike) or hand tactics. Even OC spray requires officers to be closer than a Taser device used in probe mode. Police officers are an input in the police production curve, so reductions in officer injuries per force incident would act as a positive shift.

While there are theoretical justifications that support Taser use, Tasers could also prove harmful to effective policing. Because Tasers are easy to use, these devices may lead officers to escalate an incident up the use of force continuum and use a Taser when they could instead resolve an incident without force. One study interviewed officers and resistors following the use of force and noted multiple instances where individuals believed the officers used a Taser early in their interaction (Bulman, 2010). Others have described a phenomenon labeled 'lazy cop syndrome' by which officers become overly reliant on their Taser technology at the expense of using proper de-escalation techniques (Alpert & Dunham, 2010). If officers use a Taser device early in an interaction, then Tasers could potentially increase the use of force interactions in a given department per year. Indirectly related to officer safety, high-profile Taser abuses can also undermine police legitimacy, reducing voluntary cooperation with the law and law-abidance in a given area; if this manifests in higher crime rates, there is potential for officer injuries to increase (Tyler, 2004).

Returning to remarks from the policy background, there are differences in whether departments equip all officers or instead supply only more senior police officials with Taser devices. This differential deployment raises an additional consideration – whether officers can be expected to use Tasers similarly, even within a single department (Ba & Grogger, 2018). There has been some research on officer profiles and Taser use. Psychologists who have considered Tasers write that, due to the description-experience gap identified in psychology, older officers are less likely to overreact to possible threats due to their experience resolving incidents without resorting to force (Harman et al., 2019). Empirically, however, Dymond (2020) found that officers with 6-10 years of experience were more likely to use a Taser than officers with five or fewer years of experience. Less theoretical and empirical research has been done as to whether female police officers use Tasers differently than their male counterparts; one study concluded that female officers generally use less force (Schuck & Rabe-Hemp, 2005).

3 Literature Review

The existing literature on Tasers and the effects these devices have on officer safety can be divided into two broad categories: studies that rely on descriptive analysis, and studies that use causal inference methods. In general, studies use data from one or several police departments. Taylor et al. (2012) has the largest sample that I have identified, using data from 13 different departments between 1999-2007.[3] The majority of studies appear to focus on changes resulting from Taser implementation within a single department.

The first set of studies uses descriptive statistics to compare officer injuries in a time period preceding Taser implementation to a period after implementation. The first three studies in this category are authored by police departments or authors affiliated with a department. Blair (2007) reports data for the Toronto Police Service following a limited deployment of Tasers and concludes that Tasers were 94% effective at subduing a resistor, and that no officers were injured in incidents where they used a Taser.[4] Campagna (2004) of the Charlotte Police Department examines use of force data from 2002 and 2004 and concludes that after Tasers were deployed at the start of 2004, there was a 59% reduction in officer injuries by the end of 2004 compared to 2002. The Seattle Police Department (2002) compares injuries in 2001 to injuries after Taser deployment in 2002 and reports that Tasers had the fewest instances of officer injury compared to other intermediate weapons. Two studies from departments in the United Kingdom (UK) also use descriptive statistics. One study uses data from a UK department from 1999 to 2002 and reports that Tasers deployed with darts were associated with a 7.8% injury rate, compared to a 12.6% injury rate for OC spray and a 23.7% injury rate for baton use (Jenkinson et al., 2006). A second study finds that Tasers were associated with fewer officer and civilian injuries after comparing the percentage of occasions upon which an officer injury was reported after a Taser is used (4%) compared to batons (11%) and

[3] One study on Tasers illustrates the difficulty of conducting a national-level analysis due to data limitations. One British study attempted to collect data on police use of intermediate force from 2007-2011 in 50 law enforcement agencies in the UK; the authors report that they were only able to collect aggregate data on 10 of the agencies, and they were unable to connect the data to officer injury datasets (Payne-James et al., 2014).

[4] I.e., in 94% of incidents in which a Taser was used, no additional force was required.

OC spray (16%) (Stevenson & Drummond-Smith, 2020).[5] Studies that examine officer injury rates may neglect mechanisms by which Tasers may increase the total use of force incidents for a given department.

One study in the literature cautions against drawing conclusions from studies that rely on descriptive statistics (Adams & Jennison, 2007). The authors reference one review of Tasers from the Orlando Police Department that reported a reduction in officer injuries following a year of Taser implementation, but injury rates had risen from pre-Taser levels only two years after the report. Additionally, studies that focus on officer injury rates per use of force method (Taser, baton, etc.) do not consider how providing Tasers may change officer behaviors, criminal behaviors, or overall force incidents.

A second category of studies in the literature uses causal inference methods. I first outline the studies that find positive safety effects associated with Taser implementation. One longitudinal study estimates officer injuries with Taser data from four years across 13 agencies, collected at different intervals between 1999 and 2007 depending on an agency's Taser implementation (Taylor et al., 2012). The study's principle conclusion is that Tasers reduce officer injuries and civilian injuries, and that Tasers were the most effective intermediate use of force option. The study uses a matching design and data from two years before and after Tasers were deployed in each department in the treatment group. A logistic regression model estimates that the probability of an officer being injured after Tasers are deployed is reduced by 70%. However, the authors also note that for injuries that required officers to be hospitalized, Tasers had no effect. Taylor et al. (2012) uses the largest sample from the research I have identified. One limitation is that the study does not consider hetero-

[5] One caution is that existing studies present a potentially biased sample of departments. Departments that had positive benefits associated with Tasers have greater incentive to publish these results than departments that acquired significant taxpayer funds but did not see safety improvements.

geneous implementation of Taser devices across departments (whether Tasers were restricted in deployment or available to all officers), and the anonymity of the participating agencies precludes additional research.

Within the group of studies that use causal inference methods, there is also a category of studies that finds no association between Taser use and officer safety. Smith et al. (2010) examines 24,000 instances of police force across 12 agencies from 1998-2007 that used Tasers. The authors find that there are no effects on overall officer injuries from Tasers (Smith et al., 2010). However, it is important to note that these authors again consider the effects of Tasers on officer safety using incident-level data and consequently consider Taser use within the context of how Tasers affect safety relative to other use of force methods.

Two other studies also in the same category as Smith et al. (2010) illustrate how expanding Taser access complicates evaluating their effects. Smith et al. (2007) examine use of force data from two police departments – the Richland County (SC) Sheriff's Department (which provided Tasers to about 60% of patrol officers with its initial Taser deployment) and the Miami-Dade County Police Department; the use of Tasers had no statistically significant effect on injuries in Richland County while Tasers were found to reduce police officer injuries in Miami-Dade (Smith et al., 2007). However, this study collects data from 2003-2006, but the Miami-Dade Police Department transitioned from the M-26 to the X-26 Taser model during the time period and expanded Taser access to reach 70% of patrol officers; Smith et al. (2007) does not examine how the expansion of Tasers affected officer injury rates, likely due to data limitations. Ba and Grogger (2018) exploit a change in the Chicago Police Department Taser implementation. In 2010, the Chicago Police gave Tasers to all patrol officers after they had previously been restricted to sergeant use. The authors consider incident-level data from 2005 to 2015 and find that the expansion of Tasers to all patrol officers had no statistically significant effect on officer injuries or injury rates until officers were retrained in late 2012, at which point there was a reduction in officer injuries (Ba & Grogger, 2018).

Finally, there is limited research regarding police department policy and training with regards to Taser use. While Ba and Grogger (2018) consider a Taser re-training program that occurred in 2012 in the Chicago Police Department, the authors do not indicate that any use of force guidelines changed. Womack et al. (2016) examines a Taser policy in the Dallas Police Department use of force in 2005, after which Tasers were only allowed in response to active aggression as opposed to either passive or active resistance. They find that the policy change, which restricted Taser use, was associated with a statistically significant increase in officer injuries, although they only examine seven months of injury data before and after the policy took effect. Overall, much remains to be done regarding the relationship between the Taser policies and officer injuries. Specifically, as I describe in my Empirical Methods, I assume that differences in Taser policies do not lead to differences in Taser effectiveness or use across departments in my sample. Given that the vast majority of departments appear similar in the sample time period, I argue this assumption is unlikely to bias results.

The existing literature highlights some of the ambiguities that exist in the effects of Taser use on police injuries. There are limitations in the scope of many studies, either restricting data to one police department or analyzing a limited time period of several years, making it difficult to generalize to policing throughout the U.S. (Adams & Jennison, 2007). Several studies that leverage incident-level data highlight the distinctions that must be made between determining the effects of officer injuries per use of force incident and overall officer injuries, emphasizing that Tasers may affect the total number of use of force incidents in a department and could change officer safety outcomes conditional on the use of force incidents remaining relatively constant. Furthermore, the policies guiding police officer conduct with Tasers may affect officer safety and certainly serve as an additional source of heterogeneity across departments' Taser usages.

I build on the literature in two main ways. I consider a larger sample of departments (18) over a wider time period (2000-2019). Of the studies that examine multiple departments, I examine another source of heterogeneity and consider Taser implementation (restricted vs. expanded to all patrol). Several avenues for additional research remain, including a national-level analysis of Taser use in relation to Taser policies (building on Womack et al. (2016)) and a study that considers civilian injuries.[6]

4 Data Description

My analysis uses three sets of data: Taser policy and incident data, LEOKA data, and controls from the ACS. I describe the collection of data for each of these respective parts, and I provide some initial summary statistics and figures.

4.1 Taser Policy and Incident Data

A significant accomplishment of this paper was constructing the first national database on Taser policies and Taser incident data. While I primarily

[6]The literature is limited on the role of Tasers in reducing civilian injuries. Ba and Grogger (2018) find no reduction in civilian injuries per incident. Other studies (Bozeman et al., 2008; Kaminski et al., 2015; Stevenson & Drummond-Smith, 2020) use descriptive statistics and note decreases in civilian injuries. The LEOKA dataset focuses on law enforcement safety measures, and I lack injury data on civilians. There still exists a need to evaluate the effects of Tasers on civilian injuries using causal inference at a national level.

use a balanced dataset of 18 departments, I collect data for the 50 largest police departments in the U.S from 2000-2019.[7] Because of ambiguities regarding when Tasers are available to all patrol officers for many in this larger dataset, I restrict my main analysis to a balanced subsample of 18 departments for which I have complete data on initial Taser implementation and subsequent Taser expansions (the primary dataset). Previously, there was no national database that had compiled which police departments had Taser programs or that catalogued the policy heterogeneity regarding Taser use in the U.S.[8]

I additionally classify whether Taser devices are available to all patrol officers or only some, such as officers ranked sergeant and above, as occurred in the Chicago Police Department (Ba & Grogger, 2018). I constructed this data through purchase orders released by Axon quarterly reports, annual purchase orders of county-level governments, and historical records of local news agencies for the county of a respective department. I catalogued source materials for all departments. I define the availability of Tasers for all officers based on whether a department purchased enough Tasers to outfit approximately 70% of all officers, or if the department has announced that there are enough Tasers to place one in each patrol car. Purchasing Tasers to outfit every officer is not necessary because it is common for departments to rotate these devices across officer shifts.

Although I do not directly estimate the effects of different Taser policies on officer safety measures, I collect data on Taser policies to consider an assumption in the analysis. I collect details, as available, on the following Taser

[7] I initially helped compile a significant part of this dataset during my research assistantship to Professor Melissa Moore.

[8] The Police Use of Force Project is currently the closest publicly available resource to the dataset I have constructed, but it focuses on broader policing use of force policies (whether there are requirements for de-escalation, presence of a use of force continuum, bans on chokeholds and strangleholds, etc.); it does not describe Taser-specific policies (Campaign Zero, 2022). Thus, the database lacks a historical dimension to characterizing policing that my dataset provides.

policies: whether these weapons can be used against passively resisting individuals, active resistors, or active aggressors; whether Taser can be used for pain compliance (equivalently, whether Tasers can be used in drive stun mode, in which the Taser is directly applied to the skin rather than shot from the device); whether Tasers can be used on handcuffed persons; and whether Tasers can be used against handcuffed individuals. One initial concern was that department definitions of the above categories would vary, but there appears to be a general consensus on the aforementioned terms.[9] As Womack et al. (2016) finds, differences regarding when Tasers can be used may lead to differences in officer safety outcomes; I find that in my primary dataset, approximately 97.3% of department-year observations occur with all departments having the same guidelines on Taser use against passive and actively resisting civilians. Exploring the effects of different Taser policies with a large sample remains an avenue for future research. When manuals are not publicly available, a particular difficulty in early years of the dataset, I search local news reports that detail particular Taser incident encounters and confirm police department use of force policies.

For the 18 departments in my balanced dataset, only five departments acquire Tasers for the first time after 2010; most purchase Tasers early in the sample period. Identifying when a department expands its Taser access relative to its first purchase is critical to the analysis and justifies my use of the smaller dataset; using the larger dataset is more likely to bias the Taser policy variables in my models because changes in certain safety outcomes may be falsely attributed to a Taser expansion or lack thereof. In the 50 department dataset, I am missing 203 observations on Taser expansion, mainly from years early in the dataset. This is evident in Figure 1, which displays trends in initial Taser

[9] I describe these policies in Section 2.2. These policy data are described in police department use of force manuals, which are updated as a department sees fit.

purchase and expansion for all 50 departments. It is typical for several years to pass between a department's initial Taser purchase and when it expands Taser access (for those that do).

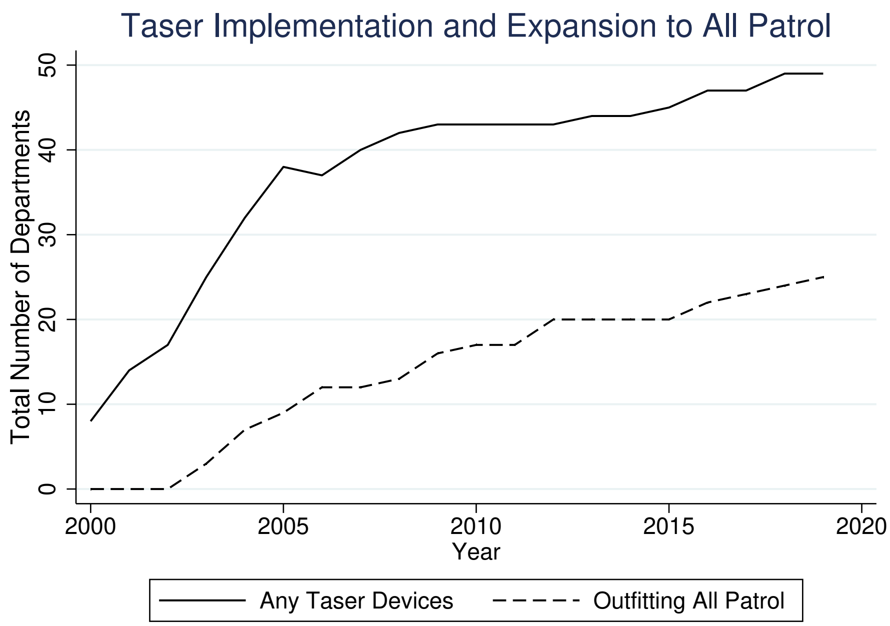

Figure 1: Taser Implementation in Sample Period

4.2 LEOKA Data

The primary outcome data – focusing on officer safety - comes from the LEOKA dataset.[10] All agencies in the sample report data to LEOKA from 2000-2019 with the exception of the Louisville Metro Police Department, which began reporting to LEOKA in 2003.[11]

I estimate my models with several variables of police officer safety. First,

[10]I use Jacob Kaplan's concatenated files obtained through Open ICPSR, which contain all data for all 50 police departments from 2000-2019 (Kaplan, 2021).

[11]The Louisville (KY) Police Department did not report from 2000-2002 as the department was not formed until 2003 when the Jefferson County Police Department and Louisville Division of Police merged (Louisville Metro Police Department, 2022).

I collapse data for three measures of officer fatalities – officers killed in total, officers killed by felony, and officers killed by accident. The majority of officer accidental deaths occur by motor vehicle crashes, so it is important to separate these from deaths that involve force directed against officers (Kaplan, 2022). Next, I consider the following categories of assaults, separating incidents within each category by whether or not an injury occurred: assaults by firearm, unarmed assaults (equivalently, assaults by hand or foot), and total assaults. I collect data from these different categories to explore the differential impact Tasers may have regarding whether an officer faces a life-threatening situation (assault by firearm) compared to what may simply be an escalating confrontation (unarmed assault).

4.3 Control Data

Following the research examining police officer injuries, I control for several additional covariates. I control for poverty level (representing the number of families that have an income at or below 100% of the federal poverty line), race (percentage of population that is Black, White, other), male (percentage of population that is male), and age. Following Ulmer and Steffensmeier (2014), I create a *youth* variable for the percentage of the population that is between the ages of 15 and 30.[12]

Table 3 displays summary statistics for the primary dataset and the full sample of departments. Departments in each have similar assault and death rates. The balanced dataset departments report slightly fewer unarmed assaults (with and without officer injury) than the full dataset. Departments in the 50 department dataset report slightly more gun assaults (with and without injury).

[12] This coding for the *youth* variable, while arbitrary, splits the difference between two articles that examine decades of crime and age data to specify the peak crime population age range (Kanazwa & Still, 2000; Steffensmeier & Harer, 1999; Ulmer & Steffensmeier, 2014).

Covariates are similar in both datasets, suggesting that the departments for which I have complete Taser implementation are located in areas with similar demographics. Officer deaths by felony and accident are similar in both datasets.

The table also illustrates some differences in Taser implementation within departments in the balanced dataset compared to all departments. In the balanced dataset, a department has some Taser program in 65% of county-year observations (234 of 360) and a fully expanded Taser program in 25% of the observations. In contrast, in the full dataset, a higher percentage have some Taser program (76.6%) and a fully expanded program (36%).

Table 1: Summary Statistics for Full and Balanced Datasets

	Full Dataset					Balanced Dataset				
	N	Mean	Std Dev	Min	Max	N	Mean	Std Dev	Min	Max
Poverty Line	1000	.1689	.0458	.0494	.3219	360	.1887	.0378	.1105	.3121
White	1000	.6064	.1323	.1564	.8518	360	.5698	.1145	.2771	.8226
Black	1000	.2191	.1375	.0197	.6731	360	.2497	.1462	.0279	.6730822
Other Race	1000	.1746	.1139	.0358	.7743	360	.1805	.0943	.0425	.4957
Male	1000	.4877	.0087	.4633	.5136	360	.4862	.0087	.4633	.5136
Youth	1000	.2301	.0209	.1791	.3548	360	.2353	.0226	.1858	.3548
Total Assaults	997	36.39	33.56	0	216.1	357	35.44	27.17	0	153.5
Unarmed - Inj.	997	8.148	7.518	0	45.48	357	7.241	6.733	0	34.38
Unarmed - No Inj.	997	20.25	22.28	0	130.3	357	19.13	18.65	0	120.4
Gun - Inj	997	.1620	.2967	0	2.584	357	.1880	.2992	0	1.922
Gun - No Inj	997	1.742	2.876	0	26.11	357	2.328	2.806	0	18.74
Killed - Felony	997	.0166	.0550	0	.5958	357	.0178	.0527	0	.3116
Killed - Accident	997	.0102	.0456	0	.6864	357	.0114	.0538	0	.6864
Any Taser Devices	979	.7661	.4235	0	1	360	.65	.4756	0	1
All Patrol	776	.3608	.4805	0	1	360	.25	.4368	0	1

Data source: LEOKA (2000-2019), ACS 1-Year Estimates

5 Empirical Methods

To evaluate the effects of Taser devices on officer safety outcomes, I use a difference-in-difference Poisson regression with the following specification (Greene, 2003). In the appendix, I estimate models using ordinary least squares (OLS); the regression equation for OLS is similar to the Poisson but removes the logarithm.

$$\ln E[Y_{c,t}] = \beta_0 + \beta_1 Any + \beta_2 Patrol + \beta_3 X_{c,t} + \alpha_c + \lambda_t$$

The outcome variables represented by the vector $Y_{c,t}$ include the following: officers killed by felony, total assaults, unarmed assaults with and without injury, and gun assaults with and without injury. The primary variables of interest are *Any* and *Patrol*. The variable *Any* notes whether a department has partial or full Taser deployment. The variable *Patrol* compares departments that have deployed Taser devices to all on patrol against those that have partial deployment or no Tasers in use. The vector $X_{c,t}$ includes control variables including variables for county-level percentages on race (Black and other with base group White omitted), male, and youth (defined to be between the ages of 15 and 30) along with department controls that include the number of female officers and total number of officers in a given department.

A Poisson model is preferred over ordinary least squares given the nature of count data and the high volume of zero counts in the outcome variables. For the data on officers killed by felony, approximately 86% of values are zero; the injury by gun assault outcome has 51% of observations equal to zero, and the remaining outcome variables of interest have between 10%-20% of the observations as zeros. The Poisson regression model I use is estimated using maximum likelihood; only perfectly predicted zero values are dropped from the model

(Correia et al., 2019).[13]

Difference-in-differences estimation relies on several assumptions, the first of which is parallel trends; i.e., in the absence of Taser devices, there would not have been any changes in the post-intervention trends for the officer safety measures (Angrist & Pischke, 2008).

Assessing these conditions for difference-in-differences estimation was not possible for all departments in my dataset, including for both my balanced dataset and for the entire sample of 50 departments for which I have incomplete data. By 2000, some departments already had Tasers deployed in some capacity. Additionally, 17 departments had implemented some Taser program by 2002. To better assess pre-trends of safety outcomes prior to Taser implementation, I consider a subsample of departments that implement in 2003, offering three years of pre-implementation data. Note that this subsample is not equivalent to the balanced dataset upon which I focus my analysis; my balanced dataset is too small to have one subgroup of departments implementing in the same year and another department for which Taser implementation was delayed (past 2007, for example). By 2007, 42 departments had a Taser program, leaving eight departments that implemented post-2007 (sometime between 2008 and 2019). Thus, I consider a subsample of 16 departments – eight that implement in 2003, and eight that implement after 2007. I consider pre-intervention and post-intervention trends in several safety outcomes to assess the parallel trends assumptions.

I first consider average total assaults per 100,000 population served. Figure 2 offers evidence that the parallel trends assumption holds. It is apparent that the pre-trends are remarkably similar for departments that implement Taser

[13] The consequence is that, instead of the model omitting single observations of zero injuries, the model only drops the observations if a department reports zero for a given outcome throughout the whole time period. This does not occur.

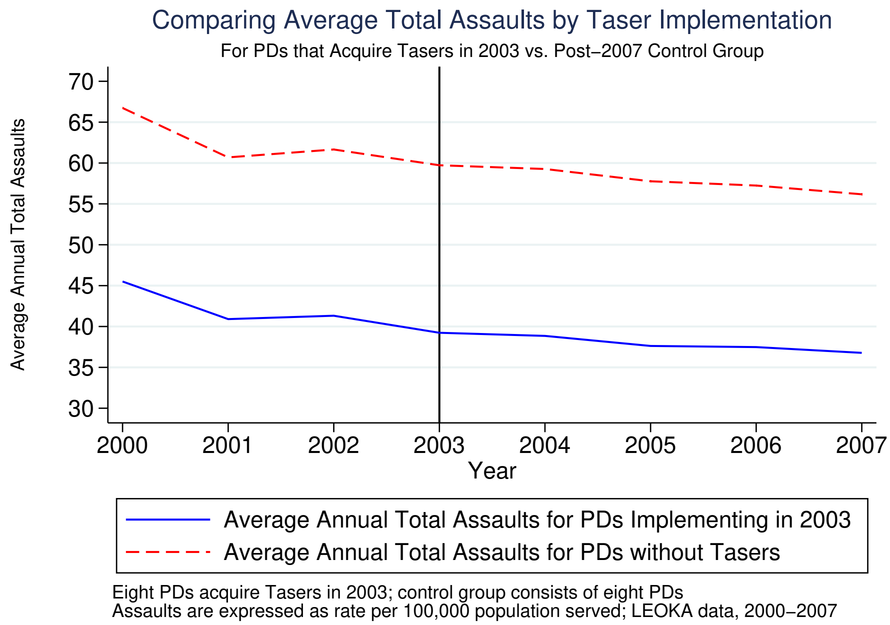

Figure 2: Avg. Annual Total Assaults per 100,000 for PDs that Implement in 2003 vs. Non-Implementing PDs

programs in 2003 and those that do not. Although this does not prove the parallel trends assumption, it offers support that it may hold. Even though the levels are different, the important part is that the trends are similar.[14] The similarity of post-trends suggests that, in this sample, the introduction of Tasers had no effect on annual total assaults in the treatment group. This descriptive analysis does not account for changes in covariates included in the regression analysis and is a separate sample from the one considered in my results section.[15]

The theoretical expectation is that departments with higher officer assault numbers would be the ones that acquire Tasers, but the scatterplot inter-

[14]Note that there is no overlap of departments in a county; each department is in its own county for the balanced dataset.

[15]To better assess whether these assumptions hold for the full sample or my primary dataset, I would need data from several pre-implementation years (1990s). Future research should evaluate this if incorporating the same methodological as this analysis. Therefore my estimates are not certainly causal effects.

estingly suggests the opposite.[16] Including fixed effects accounts for the level differences between the treatment and control groups in their outcome variables. County α_c and annual λ_t fixed effects are incorporated in the previous model; these absorb all variables that stay constant within a county or across years, respectively.

I make several additional assumptions in this analysis. First, I assume that differences in Taser training are similar across departments. Axon releases standards and provides training for those who purchase their devices, so this is a plausible assumption. A review of the available training guidelines described in use of force manuals released by departments further supports this assumption. I also have to assume that Taser policies are similar across departments. Taser policies have been found to affect officer injuries (Womack et al., 2016). As I highlight in Section 2.2, policies regarding Taser use against passive resistors are nearly the same for departments in my balanced dataset. Three of the 18 departments allowed Taser use against passive resistors at some point in the sample period. Dekalb County police are allowed to use Tasers against passive resistors during the entire time period, so this is differenced out by fixed effects. There is some variation in the Philadelphia and Los Angeles Departments, but this represents a very small portion (2.7%) of the balanced dataset. Because there are few changes in Taser policies in the sample, there is less cause for concern that changes in officer safety could lead to Taser policy changes. Another important assumption that this paper makes – as occurs throughout the literature, due to data challenges – is that the quality or model of a Taser device has no effect on officer safety. Axon releases new Taser devices that vary in their voltage, probe distance, or accuracy; no study in the literature has sought to account for how changes in the quality of Tasers provided to officers affect officer

[16] Plotting different outcomes produces similar graphs because gun assaults and unarmed assaults are subcategories of total assaults. See Additional Plots for 2003 Implementing Group.

safety. I assume in this paper that a Taser from 2000 functions equivalently to a Taser from 2019, which is not necessarily true and remains unstudied. This remains an avenue for future research. Regarding the outcome data, I note the possibility of reporting bias and reporting errors.[17]

6 Results

6.1 Officer Safety Measures with Balanced Panel

Table 2 presents my primary results estimating the effects of Taser implementation on officer safety outcomes. All outcomes are expressed in terms of officer injuries or deaths per 100,000 population served. The data corresponds to 18 police departments for which I have complete data from 2000-2019. The foremost result is that departments that provide some Taser program see decreases, some statistically significant, in officer injuries and deaths compared to departments without Tasers.

Given that this is a Poisson estimation, interpreting results involves exponentiating coefficients. The expected number of unarmed officer injuries changes by a multiplicative factor of $e^{-0.472} = 0.623$ for departments that implement some form of Taser devices compared to those that do not, holding other variables constant. This corresponds to approximately an expected 38% decrease in injuries by unarmed assault. Similarly, injuries from LEOKA's gun assaults category reflect a decrease in injuries by about 66%. The signs on the coefficients are negative for both unarmed and gun assaults without injury (as well as for the broader total assaults category), suggesting there may be several mechanisms attributable to the presence of a restricted Taser program. One explanation

[17] The existing literature documents some irregularities in the UCR data, particularly for years preceding 2000 (Kaplan, 2022). A more relevant concern is that departments may have incentive to overreport or underreport statistics as UCR data is used to determine police budgets, police performance, and resource allocation (Federal Bureau of Investigation, 2017).

could be that Taser devices are deterring criminals who are considering escalating force, indicating that Tasers raise the costs of a crime (Becker, 1968). It is also possible that Tasers improve officers' de-escalation abilities when officers are trained properly, providing additional insurance to an officer that if someone were to resist, the officer would have a viable non-lethal alternative to control the situation.

Although restricted Taser implementation decreases injuries, departments that ultimately expand Tasers to all patrol officers have no associated improvements in officer safety outcomes. I calculate the linear combination for *Any* + *Patrol*, listed at the bottom of Table 2. The additive effect of the coefficient with the most theoretical interest – unarmed injuries – is approximately zero. Three of the coefficient signs fully switch from negative to positive after Tasers are expanded (killed by felony, injuries from gun assaults, and gun assaults without injury). None of the additive effects are statistically significant at standard levels. Several explanations previously identified in the literature may apply. Alpert and Dunham (2010) argue that police officers may default to Taser use too quickly, a concern noted by a national policing organization (Bulman, 2010). As Tasers can provide officers with the ability to quickly subdue a resistor, there may be incentive to use Tasers before properly progressing through de-escalation techniques. Another explanation argues that officers who use Tasers may undermine policing legitimacy, creating an antagonistic relationship between a police department and the community it serves (Tyler, 2004). Officer training and experience may be important if more experienced officers are the ones equipped with Tasers in the limited Taser deployment.

Table 2: Officer Safety Outcomes in Balanced Panel Dataset

	(1) Killed - Felony	(2) Total Assaults	(3) Unarmed - Inj.	(4) Unarmed - No Inj.	(5) Gun - Inj	(6) Gun - No Inj
Any Taser Devices	−1.0747	−0.3155*	−0.4718***	−0.2366	−1.0749***	−0.5566***
	(0.6774)	(0.1641)	(0.1732)	(0.2570)	(0.2547)	(0.2025)
All Patrol	0.6810	0.4963*	0.5476***	0.5513	0.5910	0.4930
	(0.6037)	(0.2783)	(0.1577)	(0.3482)	(0.4348)	(0.3139)
Female Officers Percentage	−0.1476	0.0258	0.0176	0.0633	−0.0551	0.0517
	(0.1126)	(0.0524)	(0.0256)	(0.0679)	(0.0770)	(0.0760)
Total Officers	0.0010**	−0.0003	−0.0007***	−0.0003	0.0000	0.0000
	(0.0004)	(0.0002)	(0.0003)	(0.0004)	(0.0002)	(0.0003)
Taser Additive Effects	−0.394	0.181	0.076	0.315	−0.484	−0.064
P-Value	(0.566)	(0.493)	(0.642)	(0.350)	(0.269)	(0.840)
Any Taser % Change	−65.85%	−27.06%	−37.61%	−21.07%	−65.87%	−42.68%
All Patrol % Change	97.56%	64.26%	72.91%	73.55%	80.58%	63.72%
Net % Change	31.71%	37.20%	35.3%	52.48%	14.71%	21.04%
N	255	335	335	335	335	335

Standard errors in parentheses

Sample include 18 departments, 2000-2019

Data source: LEOKA; department policy data; ACS 1-year estimates.

Controls include: percentages of county population that is Black, other (ACS race category), below federal poverty line, male, and between ages 16-30.

* $p < .10$, ** $p < .05$, *** $p < .01$

6.2 Taser Effects over Time

I also consider the effects that Taser devices have over time in Table 3, defining three time periods to consider Taser use. I designate early, middle, and late time periods to be periods in which a department has a Taser for 1-2 (Early), 3-4 (Middle), or 5+ years (Late) for each implementation variable. There are some changes in the magnitudes of the Taser implementation variables within an officer safety category, but there are no clear trends across all outcomes. For example, the coefficient for *Any* increases in magnitude for the unarmed assaults with injury category, suggesting a stronger safety benefit over time. When Tasers are expanded to all officers, the reverse trend emerges (Column 3). There are several explanations as to why Tasers may become less effective over time, such as time since last Taser training (Ba & Grogger, 2018). However, this trend for the Taser expansion variable does not hold for officer fatalities or either of the gun assault categories. Few categories show changes in significance across time periods. Repeating this analysis with the entire sample does not notably change results or conclusions about time interaction effects (see Full Sample Results).

Overall, this model highlights that there are no easily generalizable changes in Taser effects on officer safety outcomes over time. Further research may consider left-censoring biases for these estimates, and extending the policy and outcome data collection into the 1990s (when the very first departments acquired Tasers) is one approach. Additionally, to consider changes in Taser effectiveness over time, future researchers should collect data on the specific Taser models in use for a given department. Axon releases new versions of the Taser, and the literature has not examined whether these affect officer safety outcomes differently.

Table 3: Time Interactions with Taser Implementation

	(1) Killed - Felony	(2) Total Assaults	(3) Unarmed - Inj.	(4) Unarmed - No Inj.	(5) Gun - Inj	(6) Gun - No Inj
Any-Early	−0.9041	−0.1974	−0.2539	−0.0993	−1.0033**	−0.6131***
	(0.9410)	(0.1555)	(0.1557)	(0.2329)	(0.4338)	(0.1703)
Any-Middle	−1.9613*	−0.2407	−0.5472***	−0.1052	−1.0123***	−0.4109
	(1.0370)	(0.2219)	(0.1725)	(0.3397)	(0.3531)	(0.2894)
Any-Late	−1.4838*	−0.1495	−0.5886***	−0.1246	−0.8881***	−0.1748
	(0.7635)	(0.2488)	(0.1901)	(0.3542)	(0.2520)	(0.3629)
Patrol-Early	1.0136	0.0550	0.0273	0.0371	0.4818	0.3708*
	(0.6610)	(0.2066)	(0.2775)	(0.2481)	(0.5686)	(0.2046)
Patrol-Middle	−0.6265	0.5503*	0.3282*	0.7200*	−0.4499	0.3791
	(1.3065)	(0.2954)	(0.1958)	(0.3832)	(0.5043)	(0.4568)
Patrol-Late	0.0797	0.7928*	0.9821***	0.9224	0.8432	0.5794
	(0.8301)	(0.4578)	(0.2405)	(0.5786)	(0.5811)	(0.5448)
Female Officers Percentage	−0.0965	0.0277	0.0034	0.0605	−0.0623	0.0623
	(0.1275)	(0.0495)	(0.0285)	(0.0656)	(0.0713)	(0.0749)
Total Officers	0.0014**	−0.0003	−0.0008***	−0.0003	−0.0001	−0.0000
	(0.0006)	(0.0003)	(0.0002)	(0.0004)	(0.0002)	(0.0004)
Taser Additive Effects	−3.882	0.811	−0.052	1.351	−2.029	0.131
P-Value	(0.139)	(0.427)	(0.919)	(0.323)	(0.143)	(0.914)
N	255	335	335	335	335	335

Standard errors in parentheses

Sample include 18 departments, 2000-2019

Data source: LEOKA; department policy data.

Controls include: percentages of county population that is Black, other (ACS race category), below federal poverty line, male, and between ages 16-30.

* $p < .10$, ** $p < .05$, *** $p < .01$

6.3 Subgroup Analysis

To isolate the effects of introducing Tasers to a select group of officers before Tasers are expanded throughout a department, I consider a subgroup of 12 departments that do not expand Tasers during the sample time period. I present these results in Table 4. These results are generally consistent with those from Table 2. The coefficients for injuries by unarmed assault are similar in magnitude and significance, and the estimates for unarmed assaults without injury are similar even though these are not significant (Column 5).

There are several differences between the two models. While the Taser implementation variables (*Any*) have the same sign across most outcomes, the models differ in sign for unarmed assaults without officer injury. However, Taser implementation is not associated with a statistically significant change for this outcome in either model. The two regressions also differ in how they evaluate the effects of Taser devices on gun assaults (with and without injury). In Table 2, Taser devices are associated with statistically significant reductions for both gun assault outcomes with 99% confidence (Columns 5-6). In this subgroup analysis, Taser devices are not associated with statistically significant reductions in officer assaults for either firearm category even though the coefficients are similar in magnitude. It is unclear to what degree these differences can be explained by the fact that I have a different (although not disjoint) sample of departments, or whether this can be attributed to a smaller sample size. The similarity of the results lends credibility to the conclusion that when Tasers are deployed in limited capacity, officers in a department face fewer injuries, particularly from unarmed assault.

Table 4: Subgroup Analysis with Limited Taser Implementation

	(1) Total Assaults	(2) Unarmed - Inj.	(3) Unarmed - No Inj.	(4) Gun - Inj.	(5) Gun - No Inj.
Any Taser Devices	−0.137	−0.420***	0.106	−0.721	−0.443
	(0.212)	(0.116)	(0.399)	(0.443)	(0.299)
Female Officers Percentage	−0.00866	0.0356**	0.0110	−0.164*	0.0778
	(0.0229)	(0.0154)	(0.0531)	(0.0848)	(0.118)
Total Officers	0.000358*	−0.000238	0.000387	0.00103	0.00166***
	(0.000189)	(0.000197)	(0.000416)	(0.000742)	(0.000438)
Any Taser % Change	−12.80%	−34.30%	11.18%	−51.37%	−35.79%
Observations	140	140	140	140	140

Standard errors in parentheses

Sample includes 12 departments specified in text, 2000-2019

Data source: LEOKA; department policy data.

Controls include: percentages of county population that is Black, other (ACS race category), below federal poverty line, male, and between ages 16-30.

* $p < .10$, ** $p < .05$, *** $p < .01$

7 Robustness Checks

One additional outcome reported in the LEOKA dataset is officers who died by accident. These deaths overwhelmingly consist of officers who have died in vehicular crashes, as noted in Section 4.2. As a result, there is no theoretical link between Taser availability and officer deaths by accident.

Table 5 repeats the regression analysis from my primary model (Table 2) using officer deaths by accident as the outcome. Each of the four columns offers a different sample or regression specification. Column 1 is a Poisson regression for the balanced sample of 18 police departments. Column 2 is a Poisson specification for the entire sample of 50 departments, and columns 3 and 4 offer OLS regressions using the balanced and full sample, respectively. The Taser implementation coefficients are nonsignificant in all models except for *Any* implementation in the balanced Poisson specification. The OLS coefficients in columns 3 and 4 are additionally estimated close to zero. While the significance of the *Any* implementation coefficient is concerning, much is likely attributable to the limited observations of officer deaths by accident. There are only 135 agency-year observations for deaths by accident from 2000-2019 while there are 355 observations for officers killed by felony.

Table 5 highlights that models may be unreliable at smaller sample sizes. This means that the subgroup analysis in Table 4 should be viewed with caution, but the results from the subgroup analysis appear largely consistent with those in Table 2. Table 2 does not present statistically significant results on effects regarding officer deaths, and the assault categories have more nonzero data entries, improving the reliability of those estimates.

In contrast, there is a clear inconsistency between the OLS specification and the Poisson estimation for the balanced dataset when officer deaths by

Table 5: Robustness Check Using Deaths from Accident

	(1) Killed - Accident	(2) Killed - Accident	(3) Killed - Accident	(4) Killed - Accident
Any Taser Devices	-1.8463^{**}	-0.8289	-0.0153	-0.0094
	(0.9406)	(0.6678)	(0.0145)	(0.0106)
All Patrol	0.0811	-0.5475	0.0021	-0.0020
	(0.5229)	(0.6392)	(0.0085)	(0.0057)
Female Officers Percentage	0.3645	0.0979	0.0010	0.0002
	(0.2709)	(0.1196)	(0.0015)	(0.0012)
Total Officers	0.0034	0.0018^{*}	-0.0000	0.0000
	(0.0021)	(0.0009)	(0.0000)	(0.0000)
Taser Additive Effects	-1.765	-1.376	-0.013	-0.011
P-Value	(0.180)	(0.139)	(0.521)	(0.384)
N	135	370	355	768

Standard errors in parentheses
Model (1): Poisson, balanced sample; Model (2): Poisson, full sample; Model (3): OLS, balanced sample; Model (4): OLS, full sample
Standard controls omitted.
Data source: LEOKA; department policy data; ACS 1-year estimates.
* $p < .10$, ** $p < .05$, *** $p < .01$

accident is the outcome in Table 5. This is not the case for the assault categories discussed in the results section. For the primary specification, I compare Table 2 (Poisson) with Table 9 (OLS); five of the six significant coefficients are significant in the OLS model, and the signs of coefficients are identical for the policy variables. Regarding Taser effects over Time, there are consistencies between Table 3 (Poisson) and Table 10 (OLS). All coefficients significant at 1% or 5% in the Poisson specification remain significant in OLS, and some of the coefficients significant at 10% confidence in Poisson remain significant in OLS. For the subgroup analysis, I compare Table 4 (Poisson) and Table 11 (OLS). The unarmed assault (significant) coefficient in the Poisson model is similarly significant in OLS, and control coefficients are similar in this specification as well.

8 Discussion and Conclusion

In this study, I further the existing research on the effects of Tasers on police safety by using an expanded dataset that covers a greater number of departments over a longer time period. I also consider different types of Taser programs to differentiate departments that restrict Taser use to certain officers or units from departments that provide Taser devices to all patrol officers.

Across the models I present, I find that Taser devices may have significant effects on assaults, but these devices do not have a statistically significant effect on officer deaths. These devices are associated with significant effects in several assault categories. In Table 2, I find that departments that implement Taser devices to a subgroup of officers see a decrease in injuries from unarmed assault by 38%. The implementation of Tasers on a limited scale is also associated with statistically significant decreases in officer injuries by gun assault, gun assaults without injury, and total assaults. There is a non-significant decrease in officers killed by felony. When Tasers are expanded throughout a department to all patrol officers, the effects are offsetting for these outcomes. There is an increase in unarmed assaults with injuries such that the additive effect is a non-significant increase of 7.9%. There are no statistically significant reductions for the additive effect in any officer safety outcome. In Table 4, I use data from a different, although not disjoint, sample of 12 departments that never fully expand Tasers to all patrol officers. My results mirror those of Table 2, lending additional support to the conclusion that limited Taser implementation can improve officer safety.

In Table 3, I consider the effects of Tasers over three time periods. I find a number of significant effects; for example, injuries from unarmed assaults decrease over time for departments with Tasers compared to those without. Overall, some officer safety measures improve over time, but other outcomes trend in the opposite direction. Only two coefficients see a sign change over different time periods, and neither sign change is statistically significant. Neither Taser variable switches from helping to then harming officers (or vice versa) in a statistically significant manner over time periods.

This study extends the existing literature in several ways. While I have not found studies that consider Taser expansion as a source of heterogeneity across multiple departments, Ba and Grogger (2018) study the expansion of Tasers in the Chicago Police Department and find that the expansion of Tasers in 2010 had no statistically significant effect on officer injury rates or overall injuries. In my primary analysis, I find a statistically significant increase in several categories of officer injuries, a contradictory result.

There are several theoretical arguments as to how Tasers may improve officer safety when deployed in a limited capacity. Departments may be able to dedicate more resources and individual training on Taser use, equip more senior and experienced officers, and closely monitor Taser use when Tasers are deployed in a more limited capacity. When Taser access is expanded, training and oversight may be diminished. Officers may favor Taser use as an easier way to gain compliance instead of progressing through de-escalation techniques (Alpert & Dunham, 2010). Improper Taser use can also erode trust in the police and escalate the probability of confrontation (Tyler, 2004).

There remain several avenues for future research on how Tasers and Taser policies affect policing. One argument that Axon touts to sell its devices is that Tasers will reduce instances of excessive force and thereby reduce the legal costs of policing.[18] Examining how the implementation of Tasers affects lawsuits against police could provide better information as to the true costs and benefits of Taser programs. Other avenues for exploration include how Taser policies affect Taser use on a national scale, whether Taser devices affect civilian injuries, and whether differences in Taser product models affect officer safety.

[18] Data on legal expenditures and reasons for cases brought against some of the largest departments are available for over 100 police departments in the U.S. (Thurgood Marshall Institute, 2020).

9 Appendix

Additional Plots from Empirical Methods

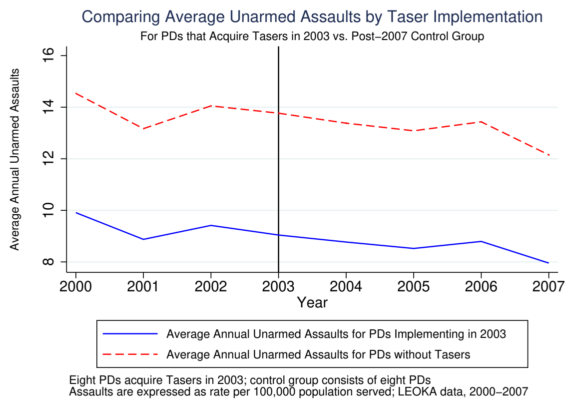

Figure 3: Average Annual Unarmed Assaults for Departments that Implement in 2003 vs. Non-Implementing Departments

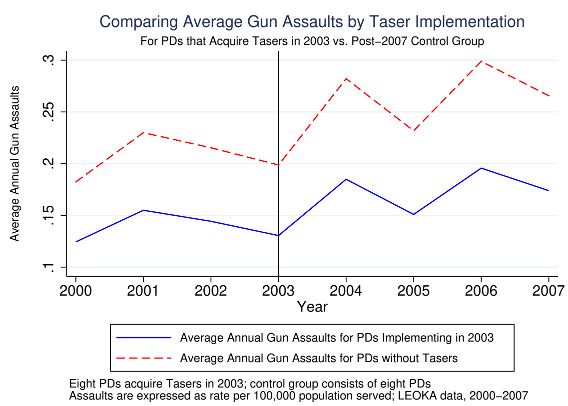

Figure 4: Average Annual Gun Assaults for Departments that Implement in 2003 vs. Non-Implementing Departments

Results Section Extended Output

These are the full models (including covariates) presented in the Results section.

Table 6: Officer Safety Outcomes in Balanced Panel Dataset

	(1) Killed - Felony	(2) Total Assaults	(3) Unarmed - Inj.	(4) Unarmed - No Inj.	(5) Gun - Inj	(6) Gun - No Inj
Any Taser Devices	−1.0747	−0.3155*	−0.4718***	−0.2366	−1.0749***	−0.5566***
	(0.6774)	(0.1641)	(0.1732)	(0.2570)	(0.2547)	(0.2025)
All Patrol	0.6810	0.4963*	0.5476***	0.5513	0.5910	0.4930
	(0.6037)	(0.2783)	(0.1577)	(0.3482)	(0.4348)	(0.3139)
Poverty Level	−0.0011	0.0204	0.0263	0.0081	0.0931	0.0145
	(0.1351)	(0.0288)	(0.0256)	(0.0398)	(0.0643)	(0.0256)
Other	−0.0691*	−0.0022	−0.0163	0.0002	−0.0500**	−0.0135
	(0.0388)	(0.0094)	(0.0102)	(0.0106)	(0.0228)	(0.0169)
Black	−0.0005	−0.0164	−0.0116	−0.0217	−0.0147	−0.0131
	(0.0247)	(0.0116)	(0.0076)	(0.0167)	(0.0172)	(0.0122)
Male	−0.1491	−0.1104	−0.0704	−0.1608	0.0804	−0.2776**
	(0.6385)	(0.1302)	(0.1159)	(0.1697)	(0.2268)	(0.1185)
Ages 15-30	−0.1780	−0.0098	−0.0305	0.0238	−0.0083	−0.0640
	(0.2514)	(0.0339)	(0.0411)	(0.0566)	(0.0741)	(0.0788)
Female Officers Percentage	−0.1476	0.0258	0.0176	0.0633	−0.0551	0.0517
	(0.1126)	(0.0524)	(0.0256)	(0.0679)	(0.0770)	(0.0760)
Total Officers	0.0010**	−0.0003	−0.0007***	−0.0003	0.0000	0.0000
	(0.0004)	(0.0002)	(0.0003)	(0.0004)	(0.0002)	(0.0003)
Constant	9.6811	9.7253	8.0455	10.5275	−4.3699	15.6349**
	(32.7562)	(6.7509)	(5.8079)	(8.7364)	(13.3009)	(6.6586)
Taser Additive Effects	−0.394	0.181	0.076	0.315	−0.484	−0.064
P-Value	(0.566)	(0.493)	(0.642)	(0.350)	(0.269)	(0.840)
N	255	335	335	335	335	335

Standard errors in parentheses

Sample include 18 departments, 2000-2019

Data source: LEOKA; department policy data. See appendix for additional details.

* $p < .10$, ** $p < .05$, *** $p < .01$

Table 7: Time Interactions with Taser Implementation

	(1) Killed - Felony	(2) Total Assaults	(3) Unarmed - Inj.	(4) Unarmed - No Inj.	(5) Gun - Inj	(6) Gun - No Inj
Any-Early	−0.9041	−0.1974	−0.2539	−0.0993	−1.0033**	−0.6131***
	(0.9410)	(0.1555)	(0.1557)	(0.2329)	(0.4338)	(0.1703)
Any-Middle	−1.9613*	−0.2407	−0.5472***	−0.1052	−1.0123***	−0.4109
	(1.0370)	(0.2219)	(0.1725)	(0.3397)	(0.3531)	(0.2894)
Any-Late	−1.4838*	−0.1495	−0.5886***	−0.1246	−0.8881***	−0.1748
	(0.7635)	(0.2488)	(0.1901)	(0.3542)	(0.2520)	(0.3629)
Patrol-Early	1.0136	0.0550	0.0273	0.0371	0.4818	0.3708*
	(0.6610)	(0.2066)	(0.2775)	(0.2481)	(0.5686)	(0.2046)
Patrol-Middle	−0.6265	0.5503*	0.3282*	0.7200*	−0.4499	0.3791
	(1.3065)	(0.2954)	(0.1958)	(0.3832)	(0.5043)	(0.4568)
Patrol-Late	0.0797	0.7928*	0.9821***	0.9224	0.8432	0.5794
	(0.8301)	(0.4578)	(0.2405)	(0.5786)	(0.5811)	(0.5448)
Poverty Level	0.0293	0.0166	0.0272	0.0044	0.0847	0.0051
	(0.1421)	(0.0288)	(0.0269)	(0.0387)	(0.0625)	(0.0293)
Other	−0.0897*	0.0090	−0.0105	0.0129	−0.0390	−0.0028
	(0.0509)	(0.0122)	(0.0080)	(0.0157)	(0.0242)	(0.0187)
Black	−0.0196	−0.0091	−0.0115*	−0.0128	−0.0091	−0.0032
	(0.0273)	(0.0099)	(0.0059)	(0.0146)	(0.0205)	(0.0124)
Male	−0.1778	−0.1011	−0.0857	−0.1623	0.1113	−0.2216**
	(0.7170)	(0.1136)	(0.1063)	(0.1564)	(0.2458)	(0.1107)
Ages 15-30	−0.1736	−0.0010	−0.0152	0.0325	0.0028	−0.0752
	(0.2282)	(0.0284)	(0.0327)	(0.0506)	(0.0663)	(0.0786)
Female Officers Percentage	−0.0965	0.0277	0.0034	0.0605	−0.0623	0.0623
	(0.1275)	(0.0495)	(0.0285)	(0.0656)	(0.0713)	(0.0749)
Total Officers	0.0014**	−0.0003	−0.0008***	−0.0003	−0.0001	−0.0000
	(0.0006)	(0.0003)	(0.0002)	(0.0004)	(0.0002)	(0.0004)
Constant	9.5447	8.7415	8.8286	10.0374	−5.9590	12.7351**
	(36.4201)	(5.6760)	(5.3910)	(7.8336)	(14.1295)	(6.1818)
Taser Additive Effects	−3.882	0.811	−0.052	1.351	−2.029	0.131
P-Value	(0.139)	(0.427)	(0.919)	(0.323)	(0.143)	(0.914)
N	255	335	335	335	335	335

Standard errors in parentheses; Sample include 18 departments, 2000-2019;Data source: LEOKA; department policy data

* $p < .10$, ** $p < .05$, *** $p < .01$

Table 8: Subgroup Analysis with Limited Taser Implementation - Extended Output

	(1) Total Assaults	(2) Unarmed - Inj.	(3) Unarmed - No Inj.	(4) Gun - Inj.	(5) Gun - No Inj.
Any Taser Devices	−0.137	−0.420***	0.106	−0.721	−0.443
	(0.212)	(0.116)	(0.399)	(0.443)	(0.299)
Poverty Level	−0.0139	0.0259	−0.0339	−0.0711	−0.0267
	(0.0211)	(0.0172)	(0.0310)	(0.0674)	(0.0418)
Other	−0.0196**	−0.0109	−0.00709	−0.0715	−0.0854**
	(0.00919)	(0.0204)	(0.0223)	(0.0611)	(0.0358)
Black	−0.0232***	−0.0158**	−0.0157	−0.0709***	−0.0489***
	(0.00882)	(0.00742)	(0.0201)	(0.0223)	(0.0138)
Male	−0.399***	−0.139	−0.390*	−1.290***	−0.958***
	(0.0660)	(0.127)	(0.213)	(0.269)	(0.196)
Ages 15-30	0.0115	0.0202	0.0189	0.0419	−0.0196
	(0.0272)	(0.0149)	(0.0900)	(0.0892)	(0.101)
Female Officers Percentage	−0.00866	0.0356**	0.0110	−0.164*	0.0778
	(0.0229)	(0.0154)	(0.0531)	(0.0848)	(0.118)
Total Officers	0.000358*	−0.000238	0.000387	0.00103	0.00166***
	(0.000189)	(0.000197)	(0.000416)	(0.000742)	(0.000438)
Constant	23.34***	9.136	21.37**	66.96***	44.56***
	(3.289)	(7.116)	(10.15)	(15.35)	(12.90)
Observations	140	140	140	140	140

Standard errors in parentheses

Sample include 18 departments, 2000-2019

Data source: LEOKA; department policy data. See appendix for additional Taser policy details.

* $p < .10$, ** $p < .05$, *** $p < .01$

OLS Specifications and Full Sample Results

I offer several alternative specifications to supplement my primary analysis in the Results section. First, I compare Table 2 with Table 9 for the initial analysis on my balanced dataset of 18 departments. Note that the coefficients for the two Taser policy variables are consistent in sign for all outcomes, and every Taser policy coefficient appears to be closely matched in significance in the OLS model. The coefficients from the Table 9 provide count changes in officer injuries; for example, a department that implements any Taser program is associated with approximately 4.73 fewer officer injuries by unarmed assault per 100,000 population served per year compared to a department without Tasers (Column 3). I repeat my regression using OLS for both my Time Interactions and Subgroup Analysis Poisson models. These are presented in Table 10 and Table 11, respectively. The OLS estimates appear consistent with the Poisson models. The estimates for time interactions are consistent in all coefficient signs and in significance for those significant at 5%.

Table 12 and Table 13 are Poisson regressions that use data from all 50 departments. Table 12 is the same specification as Table 2 from the Results section but increases the sample size. The model with all 50 departments produces generally similar results to the 18 department dataset, but it does not show any statistically significant effects for unarmed injuries or total assaults. I favor the balanced dataset because I cannot precisely identify Taser expansions for all 50 departments. For example, in some departments, I may only be able to identify that a Taser program has expanded to all patrol officers several years after an expansion occurred, and this may bias estimates. Table 13 shows that many coefficients are no longer significant in different time intervals compared to Table 3 when I use 50 departments.

Table 9: Officer Safety Outcomes in Balanced Panel Dataset - OLS

	(1) Killed - Felony	(2) Total Assaults	(3) Unarmed - Inj.	(4) Unarmed - No Inj.	(5) Gun - Inj	(6) Gun - No Inj
Any Taser Devices	−0.0161	−9.8747	−4.7261***	−1.3520	−0.2088***	−1.0660*
	(0.0145)	(6.1018)	(1.2582)	(5.1957)	(0.0644)	(0.5519)
All Patrol	−0.0000	16.1942**	3.1679**	10.4862*	0.0628	0.9095
	(0.0107)	(7.3853)	(1.3740)	(5.2024)	(0.0754)	(0.5516)
Female Officers Percentage	−0.0015	1.0384	0.4452	1.2582	−0.0097	0.1069
	(0.0026)	(1.9734)	(0.2695)	(1.1450)	(0.0203)	(0.2248)
Total Officers	0.0000	−0.0019	−0.0007**	−0.0011	−0.0000	0.0000
	(0.0000)	(0.0014)	(0.0003)	(0.0011)	(0.0000)	(0.0001)
Taser Additive Effects	−0.016	6.320	−1.558	9.134	−0.146	−0.157
P-Value	(0.461)	(0.508)	(0.297)	(0.205)	(0.160)	(0.857)
N	355	355	355	355	355	355

Standard errors in parentheses

Sample include 18 departments, 2000-2019

Data source: LEOKA; department policy data; ACS 1-year estimates. See appendix for additional Taser policy details.

OLS Specification

* $p < .10$, ** $p < .05$, *** $p < .01$

Table 10: Time Interactions with Taser Implementation - OLS

	(1) Killed - Felony	(2) Total Assaults	(3) Unarmed - Inj.	(4) Unarmed - No Inj.	(5) Gun - Inj	(6) Gun - No Inj
Any-Early	−0.0115	−7.1002	−3.5164**	0.3030	−0.1933**	−1.1240**
	(0.0157)	(5.7743)	(1.3000)	(4.6198)	(0.0693)	(0.4945)
Any-Middle	−0.0261	−7.7225	−5.4406***	0.7820	−0.2139**	−0.7853
	(0.0162)	(8.4279)	(1.5535)	(6.9625)	(0.0873)	(0.7632)
Any-Late	−0.0234	−6.8190	−5.9019***	−1.2482	−0.2097**	−0.5823
	(0.0200)	(9.2135)	(1.5950)	(7.0702)	(0.0845)	(0.8428)
Patrol-Early	0.0078	5.4524	1.8064	2.5914	0.0513	0.6489
	(0.0118)	(5.2605)	(1.1189)	(3.5500)	(0.0741)	(0.5004)
Patrol-Middle	−0.0124	17.9788**	2.1509	13.8376**	−0.0096	0.4910
	(0.0124)	(7.0302)	(1.4715)	(5.4837)	(0.0799)	(0.6808)
Patrol-Late	0.0006	29.7363*	6.6579***	20.0610	0.1436	1.4605
	(0.0116)	(16.0413)	(2.1989)	(11.9974)	(0.1229)	(0.9777)
Female Officers Percentage	−0.0015	0.8226	0.3716	1.0500	−0.0104	0.1093
	(0.0026)	(1.8440)	(0.2779)	(1.1048)	(0.0188)	(0.2240)
Total Officers	0.0000	−0.0022	−0.0008**	−0.0012	−0.0000	0.0000
	(0.0000)	(0.0018)	(0.0003)	(0.0013)	(0.0000)	(0.0001)
Taser Additive Effects	−0.065	31.526	−4.244	36.327	−0.432	0.109
P-Value	(0.329)	(0.406)	(0.449)	(0.200)	(0.211)	(0.971)
N	355	355	355	355	355	355

Standard errors in parentheses

Balanced panel dataset (2000-2019), OLS specification

Data sources: LEOKA; department policy data; ACS. See appendix for additional Taser policy details.

Controls include: percentages of county population that is black, other (ACS race category), below federal poverty line, male, and between ages 16-30.

* $p < .10$, ** $p < .05$, *** $p < .01$

Table 11: Subgroup Analysis - OLS

	(1) Total Assaults	(2) Unarmed - Inj.	(3) Unarmed - No Inj.	(4) Gun - Inj.	(5) Gun - No Inj.
Any Taser Devices	−0.915	−5.505**	6.023	−1.060	2.217
	(10.04)	(1.562)	(7.781)	(0.810)	(17.49)
Female Officers Percentage	1.441	1.004***	1.388	0.0658	9.100
	(1.641)	(0.176)	(0.935)	(0.102)	(5.630)
Total Officers	0.0426***	0.00492	0.0243**	0.00140	0.0792***
	(0.00670)	(0.00263)	(0.00673)	(0.000966)	(0.0183)
Observations	140	140	140	140	140

Standard errors in parentheses

Sample include 18 departments, 2000-2019

Data source: LEOKA; department policy data. See appendix for additional Taser policy details.

Controls include: percentages of county population that is black, other (ACS race category), below federal poverty line, male, and between ages 16-30.

* $p < .10$, ** $p < .05$, *** $p < .01$

Table 12: Officer Safety Outcomes in 50 Department Dataset

	(1) Killed - Felony	(2) Total Assaults	(3) Unarmed - Inj.	(4) Unarmed - No Inj.	(5) Gun - Inj	(6) Gun - No Inj
Any Taser Devices	−0.4842	−0.0884	−0.1056	−0.0325	−0.8107***	−0.4457**
	(0.4852)	(0.1132)	(0.1405)	(0.1762)	(0.2324)	(0.1780)
All Patrol	0.6451	0.3028	0.3593**	0.3487	0.5877*	0.4163*
	(0.4526)	(0.2027)	(0.1713)	(0.2513)	(0.3335)	(0.2166)
Female Officers Percentage	−0.0923	0.0266	0.0466	0.0553	−0.0760	0.0462
	(0.1076)	(0.0412)	(0.0338)	(0.0475)	(0.0730)	(0.0642)
Total Officers	−0.0001	−0.0001	−0.0004**	0.0000	−0.0003*	0.0000
	(0.0004)	(0.0002)	(0.0002)	(0.0003)	(0.0001)	(0.0002)
Taser Additive Effects	0.161	0.214	0.254	0.316	−0.223	−0.029
P-Value	(0.776)	(0.294)	(0.188)	(0.216)	(0.559)	(0.897)
N	507	721	721	721	695	721

Standard errors in parentheses

Sample includes 50 departments, 2000-2019

Data source: LEOKA; department policy data; ACS 1-year estimates.

Controls include: percentages of county population that is Black, other (ACS race category), below federal poverty line, male, and between ages 16-30.

* $p < .10$, ** $p < .05$, *** $p < .01$

Table 13: Time Interactions with 50 Department Dataset

	(1) Killed - Felony	(2) Total Assaults	(3) Unarmed - Inj.	(4) Unarmed - No Inj.	(5) Gun - Inj	(6) Gun - No Inj
Any-Early	−0.4906	−0.0361	−0.0856	0.0507	−0.8545***	−0.4594***
	(0.5361)	(0.1060)	(0.1410)	(0.1600)	(0.3150)	(0.1413)
Any-Middle	−1.2151	−0.0671	−0.0638	−0.0214	−0.6900**	−0.3080
	(0.7406)	(0.1358)	(0.1723)	(0.2072)	(0.2841)	(0.2641)
Any-Late	−0.0517	0.0474	0.0614	0.0461	−0.4097	−0.2249
	(0.7187)	(0.1579)	(0.1839)	(0.2255)	(0.2578)	(0.3414)
Patrol-Early	0.6657	0.1167	0.2589	0.0844	0.5497*	0.2687
	(0.4331)	(0.1391)	(0.1606)	(0.1714)	(0.3199)	(0.1772)
Patrol-Middle	0.5296	0.3232	0.2937*	0.4422*	0.1200	0.2930
	(0.6540)	(0.2074)	(0.1706)	(0.2688)	(0.4593)	(0.2989)
Patrol-Late	−0.1234	0.4011	0.3950*	0.5415	0.6362	0.5303
	(0.6218)	(0.2858)	(0.2219)	(0.3651)	(0.4368)	(0.3270)
Female Officers Percentage	−0.0878	0.0290	0.0481	0.0579	−0.0766	0.0494
	(0.1072)	(0.0379)	(0.0314)	(0.0461)	(0.0639)	(0.0611)
Total Officers	0.0000	−0.0001	−0.0005**	−0.0000	−0.0004**	−0.0001
	(0.0006)	(0.0002)	(0.0002)	(0.0003)	(0.0002)	(0.0003)
Taser Additive Effects	−0.685	0.785	0.860	1.144	−0.648	0.100
P-Value	(0.737)	(0.257)	(0.168)	(0.200)	(0.621)	(0.903)
N	507	721	721	721	695	721

Standard errors in parentheses

Sample includes 50 departments, 2000-2019

Data source: LEOKA; department policy data.

Controls include: percentages of county population that is Black, other (ACS race category), below federal poverty line, male, and between ages 16-30.

* $p < .10$, ** $p < .05$, *** $p < .01$

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