Document Title: Comparing Safety Outcomes in Police Use-Of-Force Cases for Law Enforcement Agencies That Have Deployed Conducted Energy Devices and A Matched Comparison Group That Have Not: A Quasi-Experimental Evaluation

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Police Executive Research Forum

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Abstract

How law enforcement agencies (LEAs) manage the use-of-force by officers is perhaps one of the most important tasks that they will undertake. One weapon that has been advanced as a way to reduce injuries for officers and suspects is the Conducted Energy Device (CED). The purpose of our project, conducted from late 2006 to 2008, was to produce scientifically valid results that will inform LEA executives' decisions regarding CED use. The goal of our study was to produce practical information that can help LEAs establish guidelines that assist in the effective design of CED deployment programs that support increased safety for officers and citizens. We conducted one of the first quasi-experiments to compare LEAs with CED deployment (n=7) to a set of matched LEAs (n=6) that do not deploy CEDs on a variety of safety outcomes, controlling for a variety of incident factors (force used by officer, time frame of incident, suspect race/gender/age, suspect resistant behavior, and suspect weapon use) and agency-level factors (agency policy on CEDs, size/density of LEA, and population density for jurisdiction). For the LEAs that deployed CEDs, we collected two years of data before CED deployment and two years of data after CED deployment. For the non-CED sites, we collected four years of data over a similar period.

Overall, we found that the CED sites were associated with improved safety outcomes when compared to a group of matched non-CED sites on six of nine safety measures, including reductions in (1) officer injuries, (2–3) suspect injuries and severe injuries, (4–5) officers and suspects receiving injuries requiring medical attention, and (6) suspects receiving an injury that resulted in the suspect being taken to a hospital or other medical facility. (We refer to this last category as "hospitalization," although we have no data on the extent to which officers or suspects who went to a hospital or other medical facility were admitted and stayed overnight, as opposed to simply receiving an evaluation or treatment and being released.)

Also within CED agencies, in some cases the actual use of a CED by an officer is associated with improved safety outcomes compared to other less-lethal weapons. For five of the eight comparisons, the cases where an officer used a CED were associated with the lowest or second lowest rate of injuries, injuries requiring medical attention, or injuries officer was taken to a medical facility such as hospital or medical clinic for treatment of an injury due to a use-of-force incident requiring "hospitalization" (see comment in previous paragraph). There were no differences between the CED and the non-CED sites on the outcomes of the number of suspect deaths, officer severe injuries, and officer injuries requiring hospitalization.

The evidence from our study suggests that CEDs can be an effective weapon in helping prevent or minimize physical struggles in use-of-force cases. LEAs should consider the utility of the CED as a way to avoid up-close combative situations and reduce injuries to officers and suspects.

Executive Summary

The management of police officers' use of force is perhaps one of the most important tasks that a law enforcement agency (LEA) will undertake. LEA executives have to make important policy decisions on the types of force that will be authorized, technologies to deliver that force, and when and how often various types of force can be used. One of the key objectives in managing force is designing approaches to reduce incidents of police use of force and the injuries associated with force. One weapon that has been advanced as a way to reduce injuries for officers and suspects is the Conducted Energy Device (CED). Law enforcement executives have been overwhelmed with questions about the effectiveness of CEDs and the safety of these devices. The lack of available information and a full understanding of the effects of using CEDs has hampered the ability of police executives to make informed policy decisions about these devices. Police executives have been provided with little independent scientific evidence and guidance on the impact of using CEDs. While decades of research have documented the nature and extent of the force used by police and the conditions and correlates that affect its application (Smith et al., 2007), little research has been done isolating the effects of using CEDs on injuries to suspects and officers.

Project purpose, goals and objectives:

The **purpose** of our project was to produce scientifically valid results that will inform LEA executives' decisions regarding the use of CEDs. The **goal** of our study was to produce practical information that can help law enforcement executives make good decisions about whether to deploy CEDs, and if a decision is made to deploy them, to help the agencies develop CED policy and procedural guidelines that provide increased safety for officers and citizens. In order to accomplish this goal, our **objective** was to conduct an evaluation comparing LEAs that have deployed CEDs to a matched group of LEAs that have not deployed CEDs in terms of officer and suspect safety during use-of-force incidents.

Research design:

Our team used a quasi-experimental design (QED) to compare departments with CED deployment (n=7) to a set of matched departments (n=6) that do not deploy CEDs on a variety of outcomes. With our QED, we are able to isolate the safety outcomes to be expected if a department deploys CEDs, controlling for a variety of related organizational and individual/incident-level factors.

A key element for all QEDs is the process used to select a comparison group. In our study, we used a matching design. CED (n=7) and non-CED (n=6) sites were matched based on violent crime levels, police activity, agency size, and population size of jurisdiction. The inclusion of 13 departments allows us not only to assess incident-level factors, but also some important departmental/organizational-level factors that could affect outcomes. Our study is one of the first to examine LEAs that use CEDs to matched LEAs that do not use CEDs.

We collected four years of data on all incidents of use of force for all of the participating departments. For the LEAs that deployed CEDs, we collected at least two years of data before CED deployment and two years of data after CED deployment. For the LEAs that did not deploy CEDs, we collected at least four years of data over a similar period. While the focus of our study was on the use CEDs, we also collected data on all use-of-force incidents (not just CED cases) and examined the range of weapons and unarmed tactics that the police employ in exerting force to arrest suspects.

Site participants:

Our selection of cities was based on a matching analysis using a PERF nationally representative survey on use of force. We obtained our data from seven sites that have deployed CEDs and six sites that have not deployed CEDs.

Overall, we believe our CED and non-CED sites are comparable. We collected data from fairly comparable periods for the CED and non-CED sites, within a year or two. And while some differences emerged in our assessment of the comparability of our CED and non-CED sites, most of the differences were relatively small and did not seem to introduce any substantively important biases. When combined with our multivariate analyses, we believe that we have a reasonably comparable group of CED and non-CED sites with results that are interpretable.

Data analytic approach:

We conducted a series of analyses comparing CED and non-CED sites, including bivariate analyses to describe the basic raw differences between the CED and non-CED sites on our outcome measures, and a variety of multivariate analyses to attempt to assess the viability of the bivariate results and control for possible alternative explanations for the earlier raw differences. Our first multivariate analyses were done using logistic regression to isolate the effects of CED deployment on our safety-related outcomes where we included the following independent/control variables: Whether the agency deploys CEDs, the time frame of the incident, an interaction of CED multiplied by time-frame, suspect race, suspect gender, suspect age, whether the suspect used resistant behavior, and whether the suspect had a weapon at the force incident.

One of the concerns with examining multi-site data is that the individual use-of-force cases we analyze are clustered within 13 departments. In our study, individual cases of weapon use by officers are nested within specific police departments that have various policy guidelines on the use of force. Ignoring the nested structure of our data can potentially lead to biased estimates. To address this clustering issue we used two approaches. First, we conducted a modified logistic regression with a robust variance estimator to adjust for within-cluster correlation. However, with this approach we do not get aggregate-level coefficients to see the exact effects of aggregate-level conditions on our individual results. To examine and observe the effects of aggregate-level factors, we conducted a multi-level analysis using Hierarchical Linear Modeling (HLM). While we recognize our limited statistical power to conduct HLM analyses (n=13 LEAs), we are mainly using HLM to assess the robustness of our findings from our earlier analyses and take an initial step at assessing the possible problem of aggregate-level nesting. We focus our analyses of the HLM results on the direction and magnitude of the effects (as opposed to a focus on the statistical significance of the results).

Study Results:

Overall, we found that the CED sites were associated with improved safety outcomes when compared to a group of matched non-CED sites on six of nine safety measures, including reductions in:

- Officer injuries
- Suspect injuries
- Suspect severe injuries
- Officers receiving injuries requiring medical attention,
- Suspects receiving injuries requiring medical attention, and
- Suspects receiving an injury that resulted in their being sent to a hospital or other medical facility. (We refer to this as "hospitalization," but it does not necessarily mean that suspects were admitted and stayed overnight at a hospital; we were unable to obtain data on the extent to which officers or suspects who went to a hospital or other medical facility were admitted and stayed overnight, as opposed to simply receiving an outpatient evaluation and/or treatment.)

There were no differences between the CED and the non-CED sites on the outcomes of the other three measures: number of suspect deaths, officer severe injuries, and officer injuries requiring hospitalization.

For the six of nine significant outcomes, our data suggest that the magnitude of the effects of the improved safety outcomes for the CED sites (relative to the non-CED sites) was impressive. We found a strong effect of CEDs on reducing *officer injuries* based on our raw results (8% officer injuries in the post period to 20% for the non-CED sites), and our three multivariate models. For agencies that deploy CEDs, our data suggest that the odds of an officer being injured are reduced by over 70%. Also, for our CED-only site analyses, when officers actually use CEDs our data suggest that there is a 76% reduction in officer injuries. Similar reductions were observed for the CED sites on our measure of *suspect injuries*, as confirmed by our raw results (26% suspect injuries in the post period to 43% for the non-CED sites), and our three multivariate models. For an agency that deploys CEDs, our data suggest that the odds of a suspect being injured are reduced by more than 40%.

Along the same lines, our data suggest that CED sites were related to reductions in *suspect severe injuries* based on our raw results (5% suspect severe injuries in the post period to 7% for the non-CED sites), and our three multivariate models. For an agency that deploys CEDs, our data suggest that the odds of a suspect being severely injured are reduced by over 40%. For our CED-only site analyses, our data suggest that CEDs were associated with the lowest levels of suspect severe injuries compared to other forms of force.

Our data suggest that CED sites were related to reductions in *injuries to officers requiring medical attention* based on our raw results (8% for officer medical attention in the post period to 16% for the non-CED sites), and our three multivariate models. For an agency that deploys CEDs, our data suggest that the odds of an officer receiving an injury requiring medical attention is reduced by at least 80%. For our CED-only site analyses, when officers actually use CEDs our data suggest that there is a 63% reduction in the probability of an officer receiving an injury requiring medical attention.

Similarly, our data suggest that CED sites were related to reductions in *injuries to suspects requiring medical attention* based on our raw results (40% for suspect medical attention in the post period to 53% for the non-CED sites) and our three multivariate models. For an agency that deploys CEDs, our data suggest that the odds of a suspect receiving an injury requiring medical attention in the post period is reduced by more than 45%.

Our data suggest that CED sites were related to reductions in *injuries to suspects requiring hospitalization* (defined as being sent to a hospital, clinic, or other medical facility for evaluation or treatment, not necessarily being admitted for an overnight stay) based on our raw results (16% for suspect medical attention in the post period to 36% for the non-CED sites), and our three multivariate models. For agencies that deploy CEDs, our data suggest that the odds of a suspect receiving an injury requiring hospitalization in the post period is reduced by 52% for the logistic regression model or only 11% for the HLM models relative to agencies without CEDs. While there is a wide gap in these estimates, both models suggest that CED sites are associated with a reduced probability of suspects receiving injuries requiring hospitalization. For our CED-only site analyses, our data suggest that CEDs (30%) had the highest levels of suspects receiving injuries requiring hospitalization. Our data suggest that when officers use CEDs there was a 139% increase in the probability of a suspect receiving injuries requiring hospitalization (0.87, $p<.001$). This may reflect an informal police practice of sending suspects who have been subjected to a CED activation to a hospital as a precautionary measure—for example, to ensure that the skin punctures caused by the CED darts do not become infected. PERF's guidelines for use of CEDs, for example, developed in 2005 with support from the U.S. Justice Department, recommend that "all persons who have been exposed to a CED activation should receive a medical evaluation." (See further discussion of this in Chapter 5, "Discussion and Conclusion.") While overall, the CED sites led to better outcomes than the non-CED sites on this measure, this result needs to be explored further in future research.

Another concern raised by critics of CEDs is that they may lead to higher death rates for agencies that deploy CEDs. We found no support for this concern. CEDs seem to have a neutral effect on the number of suspect deaths related to officer use-of-force cases. Before implementation of CEDs, our data suggest that the CED sites had less than one percent of their cases (0.2%) involving a suspect killed by an officer. After CED implementation, our data suggest that this number remained about the same statistically (0.4%). During the same period, our data suggest that the non-CED sites did not change either statistically. The non-CED sites observed about one percent of their cases (0.9%) involving a suspect killed by an officer at the pre-test period, and observed no change in the number of suspects killed in force incidents at the post-period (0.9%). Our data suggest that we basically have a flat line for the CED sites (0.2% to 0.4%) and a flat line for the non-CED sites (0.9% at both time points). On balance, our study did not reveal a significant effect of CEDs on suspect deaths, but with a sample of only 44 suspect deaths we do not have a high level of statistical power to uncover statistically significant findings.

While our study did not reveal evidence of higher death rates for agencies that use CEDs, concerns still remain regarding a number of deaths that have followed use of CEDs. One of the most recent and influential studies of deaths following CED use, conducted by a high-level panel of medical experts for the National Institute of Justice (NIJ) and released in 2008, found that "the purported safety margins of CED deployment on normal healthy adults may not be applicable in small children, those with diseased hearts, the elderly, those who are pregnant, and other at-risk individuals," and that "the medical risks of repeated or continuous CED exposure are unknown and the role of CEDs in causing death is unclear in these cases." The NIJ panel also found that not *all* of the people who have died after being subjected to a CED activation were chemically dependent or had heart disease or mental illness; "some were normal healthy adults." Additional research should be conducted to explore these issues.

All in all, our data suggest that we found consistently strong effects for CEDs on increasing officer and suspect safety. Not only are CED sites associated with improved safety outcomes compared to a matched group of non-CED sites, but also within CED agencies, in some cases the actual use of a CED by an officer is associated with improved safety outcomes compared to use of other less-lethal weapons. For five of the eight comparisons, the cases where an officer uses a CED were associated with the lowest or second lowest rate of injury, injuries requiring medical attention, or injuries requiring hospitalization.

Implications of PERF results:

As other researchers have generally found in use-of-force studies, we found that most of our cases involved low levels of force and few if any injuries. However, our study also documented an important number of cases when officers had to use more force to gain control of a noncompliant suspect and take the person to the ground. These types of ground struggles carry an increased risk of injury for officers and suspects. According to our results, police equipment that allows officers to avoid these up-close struggles, such as CEDs and OC spray, hold the promise of preventing injuries for officers as well as suspects. These findings are consistent with the work by Smith and colleagues (2008) that CEDs and OC spray allow officers to control suspects from a distance without engaging in the hand-to-hand struggles that often result in injuries.

The evidence from our study suggests that CEDs can be an effective weapon in helping prevent or minimize physical struggles in use-of-force cases. LEAs should consider the utility of the CED as a way to avoid up-close combative situations and reduce injuries to officers and suspects. Similar results were obtained in a study by Smith et al. (2008), who recommended that CEDs should be authorized as a possible response in cases where suspects use defensive resistance (e.g., suspect struggles to escape physical control of officer) or higher levels of suspect resistance, in order to avoid up-close combative situations.

We do not take a position on the specific circumstances when an LEA should authorize the use of the CED. We believe such a policy decision needs to be made at the local level. It is not appropriate, based on a single study, to make a firm recommendation on when a CED should be authorized to be used. Each LEA has to consider a multitude of factors in assessing when to authorize use of CEDs, working closely with its full set of community partners to consider a range of local factors.

However, our study provides important data points to inform these policy decisions that LEAs need to make. For example, there is little support in our data for authorizing the use of CEDs in cases of passive resistance by suspects, because those types of cases rarely result in injuries to officers. Also, in terms of reducing injuries, there is little to gain by permitting use of CEDs against certain special populations (pregnant women, elderly citizens, and others who are clearly physically impaired); in our study, few of these persons were involved in force cases where officers were injured.

More work is also needed in the area of officer training in the use of CEDs. There is little attention in the CED literature to training of officers and sheriffs' deputies in the proper use of CEDs. While some CED manufacturers have developed CED training curricula and some have even provided CED training, there are few independent sources for agencies to consult for guidance on developing a CED training program (see Smith et al., 2008). As a result, there is little consensus on what training should be required, what it should encompass, or what its purpose should be beyond familiarization with the device (see Smith et al., 2008). More research is needed to identify which types of CED training are most effective (see Smith et al., 2008). Another training issue is the inappropriate use of the CED. Misuse can range from outright abusive or illegal use of the weapon to less obvious cases of officers turning to a CED too early in a force incident. These problems can be managed with policies, training, monitoring and accountability systems that provide clear guidance (and consequences) to officers regarding when and under what conditions CEDs should be used and when they should not be used (see Smith et al., 2008).

Conclusions and next steps

The management of officer use of force is perhaps one of the most important tasks that a law enforcement agency (LEA) will undertake. Our study has documented an important role for CEDs in this management task. Overall, our data suggest that CED sites were associated with improved safety outcomes when compared to a group of matched non-CED sites on six of nine safety measures. And within CED agencies, in some cases the actual use of a CED by an officer is associated with improved safety outcomes compared to other less-lethal weapons. The evidence from our study suggests that CEDs can be an effective weapon in helping prevent or minimize physical struggles in use-of-force cases. LEAs should consider the utility of the CED as a way to avoid up-close combative situations and reduce injuries to officers and suspects. Furthermore, use of CEDs by law enforcement agencies is a relatively recent development, and our research reflects the early experience with CEDs (early- to mid-2000s for most of the agencies in our study). Over time, it seems reasonable to conclude that LEAs will gain important insights into the use of CEDs and may be able to improve the safety outcomes associated with this weapon.

Despite the utility of our findings, many policy questions with the use of CEDs remain: Where on the body a CED should be used, the maximum safe number of CED activations and the duration of shocks, and the role of CEDs in contributing to deaths of suspects. In addition, the results of our study prompt further questions. For example, how generalizable are our results? Our study was made up of LEAs from urban areas. Would our results be replicated in smaller communities? While we were able to include some incident-level and agency-level control variables in our analyses, how would our results hold up if we included additional variables that might be available in the future? To answer these and other questions, a better approach to the collection of police use-of-force data is needed. One of the greatest barriers to conducting use-of-force research is the absence of uniformity and comprehensiveness in the collection of force data by LEAs across the country. We observed limitations in content (information about many of our areas of interest was not collected by the LEAs), and timing (many of the LEAs were limited in how long they kept their force records – limiting our team to no more than four years of analysis). Also, the use-of-force tracking systems we observed lacked a common architecture or set of definitions, making comparative analysis very difficult. We believe that a national use-of-force database, as recommended by Smith and colleagues (2008), would greatly assist the law enforcement community to produce reliable answers to the above and other questions.

CHAPTER 1: Introduction

Our society gives law enforcement the power to use force, even deadly force, against citizens. Law enforcement agencies make important decisions regarding this use of force, including: the types of force to use, technologies to deliver that force, when various types of force can be used, and how officers should be trained to use force. Many elected leaders and police executives have expressed a desire to reduce incidents of police use of force within their communities, especially deadly force. These leaders have sought to identify innovative strategies and use modern technology to achieve this objective.

One alternative that has been advanced is the use of a relatively new less-lethal weapon called the Conducted Energy Device (CED).[1] Police chiefs and sheriffs have communicated to the Police Executive Research Forum (PERF) that they need guidance in deciding whether to adopt CEDs or other less-than-lethal weapons. The purpose of this project was to produce scientifically valid results that will inform decisions about use of CEDs.

Compared to firearms, CEDs offer the promise of helping officers to control violent suspects without killing them or running the risk of a stray bullet killing a bystander. However, there is uncertainty within the law enforcement community about deployment of CEDs, especially with regard to deaths that have occurred following the use of CEDs. Law enforcement executives have been deluged with questions about the safety and effectiveness of CEDs, and some have been forced to explain a number of controversial tactical uses of CEDs by their officers. The lack of reliable information and a full understanding of CEDs and whether or how to best use them has hampered the ability of police executives to make informed policy decisions about the devices. Police executives have been provided with little independent scientific evidence and guidance on the impact of using CEDs, forcing them to make policy and operational decisions without being fully informed.

While several studies have examined the relationship between CED usage and arrestee handling, little work has been completed regarding the relationship between policies governing CED deployment and the risk of injuries. This gap exists despite the fact that the subject of police use of force has been studied for more than four decades (Smith, Kaminski, Rojek, Alpert and Mathis, 2007). While decades of research have documented the nature and extent of the force used by police and the conditions and the correlates that affect its application (Smith et al., 2007), little research has been done isolating the effects

[1] CED technology includes traditional stun guns and projectile weapons sold under the trade names Taser® and Stinger™ Handheld Projectile Stun Guns (see http://www.ojp.usdoj.gov/nij/topics/technology/less-lethal/conducted-energy-devices.htm).

of using CEDs on injuries to suspects and officers.

This Police Executive Research Forum (PERF) study, conducted from September 2006 to November 2008, is one of the first to compare law enforcement agencies (LEAs) that use CEDs with matched LEAs that do not use CEDs. The *purpose* of this study was to complete an objective analysis of the effects that department-wide deployments of CEDs by LEAs have on injuries. Our primary aim was to evaluate the effect of CED deployment on injuries and death to police and suspects, associated medical attention, and the need for hospitalization.

Overall, our *goal* was to produce practical information that can help LEAs establish policy and procedural guidelines that assist in the effective design of CED deployment programs that support increased safety for officers and citizens. In order to accomplish this goal, we examined the outcome of CED deployment in terms of officer and suspect safety. We compared outcomes for LEAs that have incorporated the use of CEDs (n=7) to outcomes in LEAs that have not incorporated the use of CEDs (n=6). This study contains important scientific information isolating the safety outcomes to be expected if a department deploys CEDs, controlling for a variety of related organizational and individual level factors. The focus of our study on injuries to police officers and suspects during use-of-force events should bring some clarity to this relatively understudied field.

Because the news media tend to provide heavy coverage of serious uses of force by police, it is easy to get the impression that police use of force is commonplace. But prior research suggests that these types of encounters are rare. Only 1.5 percent of police-citizen contacts involve the threat or application of physical force by the police[2], and 14 percent of these cases involve subjects who claim they sustained an injury (Durose, Schmitt, & Langan, 2005). Similar low levels of suspect injuries sustained during use-of-force encounters have also been found in single-agency analyses based on surveys of law enforcement officers (Kaminski, DiGiovanni, & Downs, 2004; Smith & Petrocelli, 2002). Alternatively, studies using agency records found higher rates of injuries to citizens during use-of-force encounters, with injuries reported in approximately 40 percent of the incidents (e.g., Alpert & Dunham, 2004; Henriquez, 1999). Despite these differences based on varying data sources, there seems to be agreement that most suspect injuries are relatively minor, typically consisting of consisting of bruises, abrasions, and muscle strains and sprains (Alpert & Dunham, 2000; Henriquez, 1999; Kaminski et al., 2004; Smith & Petrocelli, 2002).

The data on the prevalence of officer injuries in use-of-force encounters is less clear. Some studies have found that one in 10 officers were injured during use-of-force incidents (Henriquez, 1999; Kaminski et al., 2004; Smith & Petrocelli, 2002). Alternatively, analysis of force records from the Miami-Dade Police Department and the Baltimore County (Maryland) Police Department revealed substantially higher rates of officer injury, 38 and 25 percent, respectively (Alpert & Dunham, 2000; 2004; Kaminski, & Sorensen, 1995). Nevertheless, as with findings regarding suspect injuries, research on force-related officer injuries found that most injuries were relatively minor (Alpert & Dunham, 2000; Brandl, 1996; Brandl & Stroshine, 2003; Kaminski et al., 2004; Smith & Petrocelli, 2002).

[2] Also, it has been estimated that only 15–20 percent of arrests involve the use of force by police on non-complying suspects.

A key concern with the use of force by the police is the possibility of injury to suspects and officers, along with related costs for medical bills for suspects, worker's compensation claims for injured officers, or compensatory damages paid out in legal settlements or judgments. However, until recently, few revealing studies had been done on the frequency, causes, or correlates of force-related injuries (Smith et al., 2008).

Over the past couple of decades, new technologies have emerged that offer the promise of more effective control over suspects who resist police, with fewer or less substantial injuries (Smith et al., 2008). These technologies include oleoresin capsicum (OC or "pepper spray") found in use in most law enforcement agencies, and CEDs (such as Tasers®), reported to be in use in more than 11,500 LEAs (Smith et al., 2008). As with OC spray, CEDs have generated controversy (Amnesty International, 2004) and have been linked with in-custody deaths and allegations of overuse and even intentional abuse (Smith et al., 2008). The focus of our study is objectively assessing the experience of LEAs with CEDs and whether they can be deployed safely and effectively.

To follow, in Chapter 2, we review the prior work that has been done in the area of police use of force generally, and less-lethal weapons in particular. In Chapter 3, we provide a detailed account of how we conducted this study, including a review of the strengths and weaknesses of our research design, measures, and analytic procedures. In Chapter 4 we present all of the substantive data analytic results for the project. In Chapter 5, we summarize our main findings, discuss the implications of our results for LEA policy and training, and provide some recommendations for future research.

CHAPTER 2: Literature Review

Law enforcement officers are legally authorized to use force, including deadly force, in carrying out their mandate to preserve order and enforce the law. Ever since the issuing of the first less-lethal weapon—the truncheon or 'short billy' club – to officers in London's Metropolitan Police Department, police have sought safer and more effective tactics and technologies for controlling and subduing resistive and combative subjects. Over the last several decades, the law enforcement field has witnessed substantial improvements in unarmed methods of defense and control, as well as the development of new variants of the baton (e.g., side-handled and expandable batons), chemical irritants (OC spray, CS [2-chlorobenzalmalononitrile] spray, CN tear gas (Alphachloroacetaphenone) spray; pepper spray), and Conducted Energy Devices (e.g., Tasers®). These newer methods and technologies have been variously credited by some experts with reducing police shootings, the incidence of use of force generally, officer and suspect injuries, and excessive force complaints (Ederheimer, 2005). Their adoption and use have not been without controversy. Some have claimed that police use of various tactics or technologies such as the lateral-vascular (or carotid) neck restraint, OC spray, or CEDs have directly caused or contributed to deaths in police custody during encounters in which deadly force may not have been appropriate, or that these types of force have been used inappropriately to "punish" suspects.[3] For example, Amnesty International has documented over 245 deaths that occurred after the use of CEDs. Other civil liberties organizations have argued that a moratorium should be placed on CED use until research can determine a way for them to be safely used.

In this chapter, we provide (1) an overview of the literature on less-lethal weapons generally and then (2) a more detailed review of the use of CEDs. Next, (3) we review outcomes associated generally with the use of less-lethal weapons by the police, followed by (4) a more intensive review of outcomes associated with CEDs.

Less-lethal weapons (LLW)

To carry out their job, police officers rely on a range of weapons that are considered less lethal alternatives to firearms. The challenge for chiefs and sheriffs is to manage the use of these various weapons and to provide clear, firm guidance to officers/deputies in making appropriate decisions when choosing to use one of their weapons. Force

[3] In 2003, Amnesty International called for a moratorium on Tasers® until an independent inquiry on the use and effects of Tasers is completed, and in 2004 it reiterated this recommendation (Amnesty International 2003, 2004). In the 2004 report, Amnesty International offers recommendations to agencies that decline to suspend Taser usage. One of its significant recommendations is that police departments using Tasers strictly limit their use to situations where the alternative would be deadly force.

must be used cautiously and judiciously, and only to promote the safety of the community and officers (Adams 1995). In most instances, police officers are justified in their use of force to protect themselves or other citizens, but sometimes they use force that is unwarranted by the situation (Gaines, Kaune, and Miller, 2001).

For more than a century, advancements in technology have greatly changed the weapons used by police. In a sense, it appears that police weaponry has come full circle. During the mid-1800s, police officers in Boston and New York relied on LLWs (primarily wooden clubs). In the late 1800s, in response to better-armed criminals, police forces began issuing firearms to officers (Allison and Wardman 2004, 116). While firearms are still standard-issue tools, today's police departments once again emphasize the use of LLWs (albeit more advanced weapons than in the 1800s) rather than firearms in most situations, where lethal force is not justifiable.

For several decades, the LEA community has been in search of LLWs that would provide officers with the ability to manage use-of-force incidents effectively while at the same time reducing the potential for injury to suspects and officers (Smith et al., 2007). Over this period, policing experts recognized that a perilous gap existed in the types of weapons available to officers; that is, there are situations in which batons may be too weak an option, and guns are too strong.[4] This fact became clear in 1985 when the U.S. Supreme Court ruled in *Tennessee v. Garner* that the use of deadly force to apprehend apparently unarmed, nonviolent fleeing felons was an unreasonable seizure under the Fourth Amendment (Pearson, 2003).

[4] See http://www.iejs.com/TechnologyandCrime/Law_Enforcement_Technology/less_than_lethal_weapons.htm.

Today, LEAs have a wider range of less-lethal weapons, including:

- *Impact projectiles* (e.g., rubber bullets, bean bags, and other blunt trauma projectiles launched from a pump-action shotgun)
- *Electrical shock weapons* (e.g., CEDs and stun guns),
- *Chemical irritants* (e.g., OC spray, tear gas and stink bombs),
- *Physical restraints* (although they are not often considered "weapons," they are often used in conjunction with less-than-lethal devices and include nets, wire entanglement systems, sticky foams, and handcuffs/flexible cuffs),
- *Hard impact weapons* (e.g., retractable batons and flashlights),
- *Weapons that use extreme light* (e.g., bright white lights or lasers that produce a "wall of light" that may deter an assailant from attacking someone behind the light; these distraction devices can also confuse, frighten, or disorient violent suspects), and
- *Acoustic-based weapons* (e.g., acoustic energy, at both audible and inaudible frequencies, has been examined for potential use as a LLW, primarily for halting the advance of an aggressive or violent crowd in a riot scenario).

While LEAs have experimented with many of the aforementioned weapons, OC spray and CEDs are the most commonly used of these weapons (Smith et al., 2007). Similar to the current day controversy surrounding CEDs, as referenced earlier, in the early to mid-1990s, OC spray was spreading rapidly among U.S. police forces and concerns were being raised regarding its overall safety and cases of misuse (Amnesty International, 1997 and ACLU of South California, 1995). As pointed out by Smith and colleagues (2007), these concerns prompted the National Institute of Justice (NIJ) to fund a variety of studies on the safety and effectiveness of OC spray (Edwards et al., 1997; Granfield et al., 1994; Petty, 2004), and several other researchers examined its incapacitative effects and the relationship between OC use and officer/suspect injuries (Kaminski et al., 1998, 1999; Morabito and Doerner, 1997; Smith and Alpert, 2000; Lumb and Friday, 1997).

These studies found that the deaths occurring after the use of OC spray were generally the result of positional asphyxia, pre-existing health conditions, or drug-related factors (Granfield et al., 1994; Petty, 2004). The research data suggest that the use of OC spray by officers was associated with fewer attacks on officers and a reduction in related injuries to suspects and officers (Edwards et al., 1997; Gauvin, 1995; Kaminski et al., 1999; Lumb and Friday, 1997; NIJ, 2003; Nowicki, 1993; Smith and Petrocelli, 2002). Nevertheless, this above research suffered from a number of methodological problems, such as the lack of comparable control groups, measurement limitations, and the lack of statistical controls for the level of suspect resistance and the use of other tactics or weapons that may have been used in conjunction with OC. As a result, we are left with inconclusive evidence on the independent effect of OC spray on suspect and officer injuries after holding constant other types of force and resistance that may have been used (Smith et al., 2007).

Conducted Energy Devices (CEDs)

CEDs use electro-muscular disruption technology to cause neuromuscular incapacitation and strong muscle contractions through the involuntary stimulation of both the sensory nerves and the motor nerves, causing the suspect to be temporarily incapacitated and fall to the ground.[5] CEDs, such as Tasers®, use compressed nitrogen to fire two barbed probes/darts and 50,000 volts of electricity along thin wires attached to the probes.[6] The innovativeness of the CED weapon is that it is not dependent on pain compliance (like traditional stun guns), making it highly effective on suspects with high pain tolerance.

Until recently, TASER International's products were the main electronic incapacitating devices commercially available for police officers to carry (IACP 2003). As a result, the most common devices in use by law enforcement are the Taser M26 and X26 models.[7] According to TASER International, by 2005 more than 6,000 law enforcement agencies (primarily in the United States) were using Tasers, with more than 1,150 agencies deploying them to all officers on patrol. There are more than 100,000 Tasers in use by police officers in the field (Kelly 2004). Current industry figures place CEDs in the hands of more than 11,000 LEAs nationwide.[8]

TASER is an acronym for the Thomas A. Swift Electric Rifle, developed in the 1970s by Jack Cover. Swift was a fictional

[5] See http://www.ojp.usdoj.gov/nij/topics/technology/less-lethal/conducted-energy-devices.htm.
[6] See http://www.ojp.usdoj.gov/nij/topics/technology/less-lethal/how-ceds-work.htm.
[7] However, two companies have recently entered the market. In January 2005, Stinger Systems™ Inc. began offering its Stinger 4 Dart Less Lethal Gun, and in March 2005, Law Enforcement Associates™ (LEA) began offering its LEA Stun Gun. It is likely that more devices using this technology will be developed for sale to law enforcement agencies in the future.
[8] Taser International reports that more than 12,800 law enforcement, correctional and military organizations in 44 countries use its devices. Of these agencies, more than 4,500 of them equip all of their patrol officers with Tasers. Since 1998, more than 260,000 Taser brand immobilizers have been sold to law enforcement agencies.

character in a 1930s series of science fiction books by Victor Appleton (Sanchez 2004). The Taser fires darts that attach to (or penetrate) a person's skin or clothing and create an incapacitating electrical current. The Taser has evolved over the years. In 1999, the company developed the Advanced Taser M26, which was powered by an alkaline battery and used nitrogen cartridges, rather than gunpowder, which was used in earlier models, to fire projectiles. Shaped liked a handgun, the Advanced Taser M26 became popular with law enforcement officers. In 2003, the company introduced the Taser X26, more compact than the Advanced Taser M26 and, according to the company, more efficient. It is powered by a lithium battery and uses nitrogen cartridges to fire projectiles. Tasers are microprocessor-controlled. There is an onboard memory that records the dates and times of the most recent 585 times the unit has been fired (Nielsen, 2001). The M26 has a Microsoft Windows-compatible data port that allows the data to be downloaded to a computer using a special adapter cable (Nielsen, 2001). This allows an agency to monitor usage patterns, and in some cases helps police executives either to document an officer's unwarranted use of the CED, or to defend against unfair allegations of abuse of force. Tasers are laser-sighted and use cartridges attached to the end of the weapon's barrel.

The Taser has two modes: "probe" and "touch stun." In the probe mode, the cartridges project, via a set of wires, a pair of barbs (or darts with hooks) that attach to clothing or penetrate the skin after the Taser is fired, delivering an electrical charge (Association of Chief Police Officers, 2004). The Taser sends an electrical current down the wires and through the body between the two barb points. In the touch stun mode, electrical contacts on the Taser are pressed directly onto a person; there is a similar but reduced neuromuscular effect (Donnelly et al. 2002). Taser specifications indicate that the Taser is effective on persons up to 21 feet away; the ability of police to keep such a distance from a suspect during a confrontation improves their safety significantly.

Outcomes associated with the use of less-lethal weapons (LLWs) by the police

There have been a number of studies conducted over the past several decades focusing on LLWs (e.g., Kingshott, 1992; Edwards, Granfield, and Onnen, 1997; Gauvin, 1994; Phillips, 1994; IACP, 1995; Robin, 1996; Morabito and Doerner, 1997; Kaminski, Edwards and Johnson, 1998; Smith and Petrocelli, 2002; Kershaw, 2004; Adang, Kaminski, Howell, and Tilburg, under review). Of key concern to practitioners is the relationship between officer use of force and injuries to suspects and to the officers themselves. As different parts of the body differ in vulnerability, and because people vary in weight and fitness, any weapon powerful enough to incapacitate can kill under certain circumstances. Many weapons manufacturers and LEAs are now using the term "less-lethal" in place of the older terms "non-lethal" or "less-than-lethal," to emphasize that these weapons tend to kill or injure far fewer targets than firearms, which primarily incapacitate by killing or maiming.

Several studies have focused on the extent to which LLWs are "effective" in helping officers gain compliance over a subject. One such study found that OC was "effective" 70% to 85% of the time, depending on the definition and measure used. Earlier studies had found higher levels of effectiveness—ranging from 90% to 100% (Kaminski, Edwards, and Johnson, 1998). Since the 1970s, approximately 12 deaths have been attributed to impact weapons like the beanbag round (Wilmette, 2001). If used improperly, these rounds can penetrate subjects and cause serious injury and death. In one California case, a woman was threatening police with a knife when they shot her in the arm and torso with beanbag rounds, and she fell to the ground and died. The cause of death was cited as a laceration of the heart, due to severe focal blunt force trauma (Shin, 2002). Another danger of less-lethal weapons is confusing the less lethal ones with the lethal ones. One case has been documented in which an impact round shotgun was found to contain a one ounce slug. This was discovered after it was fired, and the slug severed the target's leg above the right knee (James, 2002). To overcome confusion, some departments use bright orange stocks on their less-lethal shotguns to distinguish them from the others.

A small number of studies have examined the extent to which various weapons cause injuries, as less-lethal weapons are most valuable to law enforcement if they can result in subject compliance while minimizing injury to both officer and subject. While there have been a number of studies that have examined police use of deadly force or officers killed in the line of duty, less research has been conducted on nonfatal injuries to suspects and officers (Smith et al., 2008). In studies by Alpert and Dunham (2000), Meyer (1992), and Smith and Petrocelli (2002), the researchers found that when officers used bodily force (e.g., takedowns, wrestling, and punching) to get control of a suspect, they had the greatest chance of getting injured. Other research also suggests that suspects have a higher likelihood of injury when officers use canines and impact weapons (such as batons or flashlights) (Smith et al., 2008). Overall, despite decades of research on use of force, much of the research on injuries related to police use of less-lethal weapons remains descriptive in nature or contains substantial data and analytic limitations that limit the utility of this research (Smith et al., 2008).

Outcomes associated with the use of CEDs by the police

CED specific non-medical studies: While CEDs are now in use by thousands of LEAs (GAO, 2005), the research on CEDs has been mostly descriptive and few have examined the relationship between CEDs and injuries (see Charlotte-Mecklenburg Police Department, 2006; Jenkinson, Neeson, & Bleetman, 2006; Seattle Police Department, 2002). The LEAs themselves conducted much of the early research on injury rates before and after CED implementation. LEAs in Austin, TX; Cape Coral, FL; Charlotte-Mecklenburg, NC; Cincinnati; Orange County, Phoenix; South Bend; and Topeka, based upon use-of-force reports, all reported substantial declines in either officer injuries (3 and 93 percent) or suspect injuries (between 40 and 79 percent) following the adoption of CEDs (Smith et al., 2008). Overall, these assessments indicate generally that CEDs are effective, but these estimates vary depending upon whether one evaluates the effectiveness of all instances in which CEDs are deployed against subjects or only the CED deployments that result in both darts making contact with the subject.[9] The Cincinnati Police Department reported CEDs were successful at gaining compliance from resistive/combative suspects approximately 84% of the time (Streicher, 2005). Of these, about 65% of suspects were "immobilized" and another 18% complied or were only partially affected (Streicher, 2005). In 102 instances where the CED was ineffective, an

[9] A subject will not receive an activation unless both darts make contact with the subject's skin.

inadequate circuit resulted in continued non-compliance (e.g., missed target, darts failed to penetrate subject's clothes), but there were also 61 instances in which the threat of CED use was enough to gain compliance (Streicher, 2005).

Next, TASER International claims that police departments have seen a decrease in officer and suspect injury rates after the introduction of the Taser. The company's website claims that injuries to suspects have dropped between 40 to 68 percent after the introduction of the Taser and injuries to officers have declined 41 to 93 percent. The site also reports a reduction in worker's compensation claims for one police department. The Grant City, Illinois Police Department introduced the Taser as part of a multi-faceted risk management program. The TASER International website reports that police department worker's compensation expenses were $454,192 and $740,172 for the two years prior to the introduction of the Taser. According to TASER International, based on two years of data after deployment of the Taser, the Grant City Police Department spent *zero* dollars on worker's compensation expenses[10]. However, these results have not been subjected to independent analysis, except for one analysis of data from TASER International that was subjected to the scrutiny of peer review. Based on data maintained by TASER International, researchers (Jenkinson, Neeson, & Bleetman, 2006) found a low level of injury associated with CED use (8%) compared to the use of CS spray (13%) and batons (24%).

Overall, questions have been raised about these CED studies because they are not the product of research produced by independent sources (Smith et al., 2008). Also, pre-post designs are generally considered weak research designs, especially considering that these studies did not statistically control for situational factors and other types of force used in conjunction with CEDs during any given force incident. Without a comparison group, such pre-test/post-test designs are not effective at isolating the effectiveness of CEDs. That is, there is no way of knowing if some other factor in the environment might have led to the observed changes between the "before" and "after" period.

In one of the more rigorous independent studies in this area, Smith, Kaminski, Rojek, Alpert and Mathis (2007) analyzed the relationship between CEDs and officer and suspect injuries from two law enforcement agencies while simultaneously controlling for the effects of other types of force used by officers as well as suspect resistance and other factors. The use of CEDs was associated with reduced odds of officer and suspect injury and the severity of suspect injury in one agency. In the other agency, CED use was unrelated to the odds of injury; however, the use of pepper spray was associated with reduced odds of suspect injury. Among other findings, in both agencies the use of hands-on tactics by police was associated with increased odds of officer and suspect injury, while the use of canines was associated with increased odds of suspect injury. A major concern with this study was the absence of comparison agencies that have not deployed CEDs, and this study was limited to only two CED deploying LEAs.

In another rigorous study of this issue, Smith, Kaminski, Alpert, Fridell, MacDonald, and Kubu (2008) collected more than 24,000 use-of-force records from 12 police agencies that have deployed CEDs.[11] These data were combined and analyzed using multilevel and fixed-effects

[10] See: http://www.taser.com/documents/TASERS_saving_lives_compilation-short.pdf

[11] One of these LEAs also participated in the current PERF study.

models to investigate the relationship between policy-related factors and the likelihood of injury to police and citizens in use-of-force incidents, adjusting for the demographic and situational differences between police use-of-force incidents. While controlling for the use of less-lethal weapons (OC spray and CEDs) in force encounters, they found that the use of physical force (hands, feet, fists) by police increased the odds of injury to suspects by more than 50 percent and substantially (by a factor of 3) increased the chances of injury to officers. Conversely, the use of OC spray or CEDs decreased the probability of injury to suspects by 65 and 70 percent respectively. Injuries to officers were unaffected by the use of CEDs, while the odds of officer injuries increased somewhat (by about 21 percent in the 12 agency models) when OC spray was used. Overall, CED use reduced the probability of injuries to suspects across the 12 agencies in the combined analysis and in two out of the three agencies, whose data were analyzed independently (Miami-Dade and Seattle). Likewise, the relationship between OC spray and suspect injuries in the multi-agency analysis is consistent with the injury reduction finding in Richland County; in Seattle, OC spray had no effect on suspect injuries, while the Miami-Dade Police Department does not issue OC spray.

Our study, the results of which are reported in Chapter 4, builds on the Smith et al. (2008) study, using comparable measures and including LEAs that have deployed CEDs and matched LEAs that have not deployed CEDs. The problem with using data only from CED agencies is that we have no counterfactual comparison to agencies that did not use CEDs, and are left with a simple pre/post design with all of its well-known flaws. Also, we are limited in observing the full effects of CEDS across similar types of force situations. In addition, some agencies reserve the use of CEDs only for certain types of more serious situations that justify higher levels of force, and tend to involve more danger to the officer, bystanders, or suspects. In these agencies, comparing CED use against situations involving lower levels of danger, in which other types of weapons may be used, could set up an unfair comparison. Thus, to the extent that our study includes pre-post analysis of agencies that have deployed CEDs, we are very cautious in our interpretation of these data.

CED-specific medical studies: A number of controlled medical studies have been conducted examining the physiological effects of CEDs on animals and humans. One of the vital issues regarding the use of CEDs is whether exposure can induce ventricular fibrillation (VF).[12] To address this issue, a number of controlled studies using sedated animals were conducted (see Smith et al. [2008] for a full summary of these studies). These studies found no VF of the heart using standard discharges of relatively short duration (e.g., 5–15 seconds), but higher-output discharges (e.g., 15–20 times the standard) or discharges of longer duration (two 40-second exposures) induced VF or increased heart rhythm (ventricular tachycardia) in some pigs (Dennis et al., 2007; Lakkireddy et al., 2008; Stratbucker et al., 2003; McDaniel et al., 2005; Walter et al., 2008), and longer duration exposures led to VF-induced death in three pigs (Dennis et al., 2007; Walter et al., 2008). Research by Nanthakumar and colleagues (2006) found that orienting TASER barbs across the hearts of pigs (simulating a “worst case scenario” of creating a current vector that directly passes through the heart) led to stimulation of the heart muscle (but not VF), while placement

[12] VF is a condition in which there is uncoordinated contraction of the cardiac muscle of the ventricles in the heart, making them tremble rather than contract properly, leading in some cases to a cessation of blood circulation and death.

across the abdomen did not (see also Lakkireddy et al., 2006; Roy & Podgorski, 1989). Although cardiac stimulation may be of little concern for healthy subjects, Nanthakumar et al. (2008) caution that heart stimulation might induce VF if preexisting conditions are present, such as heart disease, drug intoxication, and so forth.

Several controlled studies using healthy human subjects also have been conducted. For example, examining the impact of CEDs under highly controlled conditions, Levine et al. (2005) found that 20 human subjects exposed to approximately a 2.4-second shock from Tasers experienced no cardiac dysrhythmias. Also, Levine et al. (2007) monitored the hearts of 105 police trainees before, during and after exposure to the X-26 TASER for approximately 1 to 5 seconds. Although subjects experienced significant increases in heart rate following exposure, none experienced VF. An earlier study by Levine et al. (2005) reached similar conclusions. In a review of this literature, Smith and colleagues (2008) summarized these findings indicating that the evidence suggests that CEDs are relatively safe when used on healthy at-rest as well as physiologically stressed subjects, but that medical researchers caution that CEDs are not risk-free (National Institute of Justice, 2008; Vilke & Chan, 2007). Strote and Hutson (2008), for example, suggested that CEDs might cause physiologic and metabolic changes that are clinically insignificant in healthy individuals but that could be harmful or even life-threatening in at-risk populations (e.g., obese subjects with heart disease and/or those under the influence of drugs). Also, there are the secondary injuries that can occur from falling after exposure to a CED. For example, Kroll, Calkins, Luceri, Graham, and Heegaard (2008) reported six deaths due to head injuries suffered during falls following CED exposure.

While studies with animals and healthy volunteers are important, there is also a need for field studies in the actual population at risk of CED exposure. Below we examine three case review studies that explored cases involving suspects who were subjected to a CED activation. In a 1991 study, Kornblum and Reddy examined 16 CED-related deaths and reported that in all cases the subjects were behaving in a bizarre manner and that more than 80% of the subjects were under the influence of cocaine, PCP or amphetamine. Kornblum and Reddy (1991) found that in only one case was a CED possibly associated with a death of a suspect. They concluded that the CED did not *cause* death in this single case, but may have contributed to the death. In that case, the suspect had a heart disease and toxic levels of PCP in his system.

In a review of 37 CED-related deaths, Strote and Hutson (2006) found that autopsy reports indicated that CEDs were a possible cause of death in six and were a contributory cause in four of the 37 death cases. Strote and Hutson (2006) concluded that the fatal CED encounters involved subjects already at risk for sudden death from other causes, and that a common factor in the death cases was extreme agitation, often accompanied by stimulant drug use and/or preexisting heart disease.

In another study, Bozeman and colleagues explored whether CEDs contribute to or cause death (National Institute of Justice, 2008), and wrote that they found no conclusive medical evidence that indicates a high risk of death from the direct effects of CEDs (Bozeman, Winslow, Hauda, Graham, Martin, & Heck, 2008). In the study, six LEAs participated over two years. Independent physicians worked with each LEA. Researchers evaluated 962 incidents in which a CED was used on a suspect. While the large majority (99.7 percent) of the suspects in these 962 cases exposed to CEDs suffered no injuries or only mild injuries, a small number suffered potentially lethal injuries. This study did not observe any deaths occurring immediately after CED use that might suggest that the CED directly affected a suspect's heart rhythm.

Studies examining the positive and negative impacts of less-lethal weapons are critical for producing information to guide policy-makers. While the studies described above have produced some important information about various outcomes of CEDs, individually and as a group they are insufficient to guide decision-making. Our quasi-experiment provides comparative data from LEAs with CEDs and matched LEAs without CEDs. Our design allows us to isolate and rigorously evaluate the effects of using CEDs on officer and suspect safety outcomes.

CHAPTER 3: Research Design and Methods

Overview

Our research design allows our team to compare departments with CED deployment (n=7) to a set of matched departments (n=6) that do not deploy CEDs on a variety of outcomes. Matching was based on criteria such as violent crime levels, police activity (violent crime arrests), agency size, and population size of jurisdiction. The inclusion of 13 departments allows us not only to assess incident-level factors, but also some important departmental/organizational-level factors that could affect outcomes. To assure a fair comparison we collected at least four years of data on all incidents of use of force for all of the participating departments. For the LEAs that deployed CEDs, we collected at least two years of data before and two years after CED deployment. For the LEAs that did not deploy CEDs, we collected at least four years of data over a similar period.

While the focus of our study was on the use of CEDs, we also collected data on all use of force incidents (not just CED cases) and examined the range of weapons (including pepper spray and batons) and unarmed tactics (e.g., joint locking techniques) that the police employ in exerting force to arrest suspects. Agencies that do not deploy CEDs all have other forms of less-lethal options, and our study provides evidence on the relative effectiveness of CEDs to these other options, controlling for a variety of related organizational and incident-level factors.

Five of the sites in the study (three non-CED sites and two CED sites) did not have electronic use-of-force databases. For these five sites, we sent a team of three data collectors to collect random samples of 50 cases per year per site for four years (for a total sample of about 200 cases per site). Two individuals independently coded data from hard copies of use-of-force forms. The third person (a research supervisor) checked these data collectors' work, resolved any conflicts between the two coding sheets, and entered a reconciled sheet into a research database. Inter-rater reliability statistics were high across all five sites (on average .91 across all the sites).

Quasi-Experimental Design

While it might be preferable to assess the impact of CEDs through a randomized clinical trial (RCT), this type of design is not possible in this context. We are unaware of any police department that would randomly assign a CED (or any other weapon) to its officers, due to ethical concerns. Ethical considerations dictate that police chiefs develop use-of-force policies based on their best judgments of what will be safest and most effective in their communities. At this time, different chiefs have made different determinations on the question of whether to deploy CEDs; however, for each chief, there is no room for randomness in those calculations. An RCT with CEDs could potentially endanger the lives of officers, suspects, and bystanders.

One common alternative to the more inferentially powerful RCT design is a quasi-experimental design (QED). In this context, "quasi" means that the design is similar to an experiment, except that this design is characterized by a comparison group that receives either a different treatment or no explicit treatment at all (see Cook and Campbell, 1979). QEDs have a similar purpose as RCTs in terms of testing causal hypotheses and share many structural details (e.g., pre- and post-tests and comparison groups), but lack random assignment.

QEDs require the researcher to enumerate alternative explanations one by one, decide which are plausible, and then use logic, design and measurement to assess whether each one is operating in a way that might explain any observable effect (Shadish, Cook and Campbell, 2002: 14). Christensen (1988: 306) argues that many causal inferences can be made without using the RCT framework; they are made by rendering other rival interpretations implausible. For example, if someone unknowingly stepped in front of an oncoming car and was pronounced dead after being hit by the car, you would probably attribute the death to the moving vehicle. The person might have died as a result of numerous other causes happening at that same point in time (e.g., a long-term debilitating illness that finally killed the person), but such alternative explanations are not accepted because they are not likely to be plausible. In a like manner, the causal interpretations arrived at from quasi-experimentation are those that are consistent with the data in situations where rival interpretations have been shown to be implausible. Our design allows our team to isolate a number of injury/safety outcomes to be expected if an LEA deploys CEDs, controlling for a variety of organizational and incident-level factors.

Selection criteria for inclusion in study – We selected 18 police departments nationwide using a careful selection process to ensure comparability across these departments and to ensure that each department could provide the necessary outcome data regarding injuries in use-of-force incidents. Our goal was to have at least 12 departments in our study, and we were able to obtain 13. The selection criteria included: **(1)** being able to provide data on all incidents of use of force (including data such as type of force used and injury outcomes to both officer and suspects), **(2)** having a written policy identifying CED and other less-lethal weapon placement on the force continuum, **(3)** a willingness to share data with PERF for this study, and **(4)** having at least 100 sworn officers (we sought larger LEAs for participation in our study in order to obtain sufficient numbers of use-of-force incidents for a robust analysis).

Next, we needed to ensure that appropriate groups could be compared to each other and that the time-series (pre- and post-test) component of this study could take place. The final criterion **(5)** was that the departments in our study needed to have all of the necessary data available for at least 4 years (2 years pre- and 2 years post-CED deployment or 4 years of a comparable time period for the non-CED sites).

Matching to comparison cities – With QEDs, the comparison group is usually a naturally formed group that is similar in some ways to the experimental group but it does not receive the intervention that is the subject of the study. In our case, we have departments that use CEDs and a matched group of police departments that do not. The main concern with the QED design is that although differences that can be measured can be statistically controlled, unmeasured variables related to the outcome variable cannot be controlled. Therefore, our above selection criteria standards on data availability and PERF access had to be vigorously enforced to ensure the availability of all data necessary to create statistical controls for possible "pre-treatment" differences between our study groups. We originally had 18 potential sites, but five had to be dropped because of data availability issues.

To assure that we could identify sites into the study that could meet our study selection criteria (see above), we needed a methodology to screen sites for possible inclusion into our study. Fortunately, at the time, PERF had recently completed a survey that could be used for screening purposes. Our selection of cities was based on a PERF nationally representative survey on use of force conducted in 2006–2007. This survey was done as part of another NIJ-supported project (Smith et al., 2008) called the Use of Force survey. Briefly, this survey was developed to collect information on the current state of less-lethal weapon policy, practice, training and usage; and to empirically assess the positive and negative outcomes associated with less-lethal weapons (full details on the survey are provided in Smith et al., 2008). The University of South Florida, in collaboration PERF and the University of South Carolina, developed this survey. The final survey instrument contained a series of both open- and closed-ended questions. PERF drew a nationally representative stratified sample of LEAs using the 2005 National Directory of Law Enforcement Agencies database. PERF stratified the sample by type of LEA (i.e., local police departments and sheriffs' offices), region of the country, and the size of the population served by the department. The Use of Force survey was sent to a stratified random sample of law enforcement agencies (N = 950). Respondents were able to submit the survey via mail, facsimile, email, Federal Express, or the Internet (just under 60% of the sampled LEAs submitted a completed survey).

The same Use of Force Survey was used by Smith et al. (2008) to select 12 cities for their use-of-force study. To maximize the utility of the two studies, we selected a different group of sites in our study (only one site participated in both studies). Our selection process started with identifying LEAs that have full deployment of CEDs and that place CEDs in a low position on their use-of-force continuum. Then, we identified matched LEAs with full deployment of CEDs that place it high on the force continuum. To increase the generalizability of our study, we then selected a mix of CED type-sites, with some placing it low on the use-of-force continuum and some placing it high. Finally, we selected matched LEAs that do not use CEDs. Matching was based on criteria such as violent crime levels, police activity (violent crime arrests), agency size, and population size of jurisdiction. Table 1 shows the final list of LEAs (N = 13) that participated in our study. The table lists the participating LEAs, and the dates we collected data from each of these sites. For the CED agencies, we selected data two years before CED implementation and two years after CED implementation. For the non-CED agencies, we attempted to collect data from roughly comparable periods.

As can be seen from the table, the data collected from the CED and non-CED sites are from the same basic time period, within a year or two. For example, Pre-Period 1 starts in 1998 for the CED sites and 1999 in the non-CED sites, with an overall average of mid 2000 for CED sites and mid 2002 for non-CED sites. The second of our pre CED implementation periods averaged just about mid 2001 compared to year 2003 for the non-CED sites. The first of the post-CED

Table 1: Years for which data were collected in sites before and after CED deployment

Non-CED Sites	Pre-Period 1	Pre-Period 2	Post-Period 1	Post-Period 2
Site 1	1999	2000	2001	2002
Site 2	2002	2003	2005	2006
Site 3	2003	2004	2005	2006
Site 4	2003	2004	2005	2006
Site 5	2004	2005	2006	2007
Site 6	2005	N/A	2006	2007
Average	2002.7	2003.2	2004.7	2005.7

CED Sites	Pre-Period 1	Pre-Period 2	Post-Period 1	Post-Period 2
Site 7	1998	1999	2001	2002
Site 8	1999	2000	2002	2003
Site 9	2000	2001	2003	2004
Site 10	2000	2001	2005	2006
Site 11	2001	2002	2005	2006
Site 12	2001	2002	2004	2005
Site 13	2004	2005	2006	2007
Average	2000.4	2001.4	2003.7	2004.7

implementation periods for the CED sites averaged just about year 2004 compared to just about year 2005 for the non-CED sites. The second of the post-CED implementation periods for the CED sites averaged just about year 2005 compared to just about year 2006 for the non-CED sites. While it might have been preferable to have exactly the same start and end dates for the CED and non-CED sites, that was not possible due to data availability issues. However, given that we are within a year or two in most cases, we do not believe that any bias was introduced into the study based on temporal considerations.

It is worth noting that the CED sites in our sample have used CEDs for a relatively short time frame. All of the CED sites started using the CED weapon in the 21st century. While this is not surprising, given that the modern Taser that uses a nitrogen cartridge instead of gunpowder to fire the probes has been in use only since 1999, it does have implications about the nature of our study. Any conclusions that we draw from our research reflect the early experience with CEDs. Over time, it seems reasonable that LEAs will gain important insights into the use of CEDs and will be able to attain further improvements in safety outcomes associated with this weapon.

Table 2 (see below) presents the four-year average for police and census data for each of the above years for each participating LEA. The table includes data on the size of the residential population served by the LEA, number of officers in each LEA, number of arrests for violent offenses, number of violent crimes (using the UCR definition of violent crime), and number of homicides. We were able to find data for most of these measures; in cases where we were not able to secure these data, we place a dash in the cell of the table.

The main difference between the non-CED and CED sites is the participation of one CED site that is much larger than the other sites in our study. To address this issue, we estimated all of our models with and without the one unusually large site, and found no major differences in our results. For our residential population measure, we found that our non-CED sites had just over 600,000 people on average (612,354), and our CED sites had almost 2 million people on average, but just over 700,000 if the one largest site was excluded.[13] The non-CED and CED sites both had two agencies with populations below 500,000. The non-CED and CED sites each had three sites in the 500,000 to 900,000 range in population. The non-CED and CED sites each had one site in the one million-population range.

Next, the non-CED and CED sites were comparable in terms of the size of each of the agencies. All agencies selected for participation in our study were within the top 3% of LEAs in the United States in terms of number of officers, with our smallest CED site having 445 officers and our smallest non-CED site having 308 officers. We found that our non-CED sites had 1,324 sworn officers on average, and our CED sites had 2,123 sworn officers, but only 1,271 sworn officers if the one unusually large site was excluded.

The non-CED and CED sites were comparable in terms of the number of arrests for violent crime by the LEAs. Non-CED sites on average made 1,973 arrests for violent crimes compared to 1,638 violent crime arrests for CED sites (excluding the one unusually large site). Three of the non-CED sites and two of the CED sites had between 2,400 and 4,400 violent crime arrests and one of the CED sites had 1,437 violent crime arrests. Two non-CED sites and one CED site had between 650 and 850 violent crime arrests. Both non-CED and CED sites included one site each with fewer than 300 violent crime arrests.

The non-CED and CED sites were comparable in terms of the number of reported violent crimes. Non-CED sites had on average 4,374 violent crimes compared to 5,771 violent crimes for CED sites (not counting the especially large site, where we were not able to secure reliable data on violent crimes for this period). The non-CED and CED sites were also comparable in terms of the number of homicides. Non-CED sites had on average 56 homicides compared to 72 homicides for CED sites (again, not counting the especially large site, where we were not able to secure reliable homicide data for this period).

The final sets of comparisons were on demographic variables collected from the 2000 U.S. Census (see Table 2). The non-CED and CED sites were very similar on a full range of background aggregate-level factors. The non-CED sites averaged 8.5% of the population below the poverty level (ranging from 3.6% to 16.7%) compared to 10.1% for the CED sites (ranging from 3.3% to 23.5%). The non-CED sites averaged a household income of $50,386 (ranging from $37,752 to $61,768) compared to $48,190 for the CED sites (ranging from $23,483 to $71,551). The non-CED sites averaged 3.6% unemployment (ranging from 2.1% to 6.8%) compared to 3.9% for the CED sites (ranging from 2.2% to 5.9%). The population per square mile for the non-CED sites averaged 3,782 people per square mile (ranging from 530 to 9,316) compared to 3,466 people per square mile for the CED sites (ranging from 831 to 10,161). The

[13] There are some complications with examining residential population for cities that have large commuter populations. For example, one of our sites has a residential population of just under 600,000; however, due to commuters from the surrounding suburbs, its population rises to over one million during the workweek.

percent of female-headed households for non-CED sites averaged 7.6% (ranging from 5% to 9.9%) compared to 7.4% for CED sites (ranging from 5.9% to 9.2%). The percent of owner-occupied dwellings (which sometimes is used as a measure of an area's economic stability) for non-CED sites averaged 56.5% (ranging from 40% to 75.5%) compared to 56.7% for CED sites (ranging from 34.9% to 71.7%). Regarding the racial make-up of the jurisdictions, the percent of non-white residents for non-CED sites averaged 41.6% (ranging from 19% to 69%) compared to 36% for CED sites (ranging from 30.1% to 51.3%). The percentage of males in the population for the non-CED sites on average was 48.3% (ranging from 47.1% to 49.8%) compared to 49.5% for CED sites (ranging from 47.9% to 50.5%). The percentage of young people in the population between the ages of 15 and 24 for the non-CED sites on average was 13.4% (ranging from 11.4% to 15.7%) compared to 13.1% for CED sites (ranging from 10.9% to 14.6%).

On balance, we believe our CED and non-CED sites are comparable. We collected data from fairly comparable periods for the CED and non-CED sites, within a year or two. The main difference between the non-CED and CED sites is the participation of the one unusually large CED site in our study. However, when we estimated all of our models with and without this agency, we found no major differences in our results. With this site excluded from our analyses, there are no major aggregate-level differences between the CED and non-CED sites across a range of variables including: size of the residential population, number of officers, number of arrests for violent offenses, number of violent crimes, and number of homicides. Further evidence of the comparability of the CED and non-CED sites can be seen in our analyses of the demographic variables from the 2000 U.S. Census. The non-CED and CED sites were similar on a full range of background aggregate-level factors such as population below the poverty level, household income, unemployment, population density, female-headed households, residential stability, racial heterogeneity, percentage of males, and youths in the population. Overall, while differences were evident in a number of the variables, most of the differences were relatively small and did not seem to introduce any substantively important biases. Given that we were making comparisons based on U.S. Census data (i.e., population data from the same data collection system), any observed differences represent actual differences in the population. It would not be possible to select places with exactly the same characteristics. Therefore, the question is not whether there are differences (by definition quasi-experiments represent comparisons across non-equivalent groups), but whether the differences appear to be substantively important. We believe that there are no substantively important differences between the groups on relevant background factors. We believe that we have reasonably comparable CED and non-CED sites. When combined with our multivariate analyses, where we control for differences between the sites, we believe that the results of our study are interpretable.

Furthermore, we observed few contextual changes across the sites that might have affected the comparability of CED and non-CED sites. For example, in our interviews with police personnel and review of agency documents, all of the agencies provided detailed training for their officers on use-of-force issues. There is little evidence that additional refresher training was adopted after the CED weapon was introduced to the agency or that training efforts were otherwise intensified across the board after adopting CEDs. All of the agencies seemed to have sound training programs in place on use-of-force issues during the time frame of our study. All the agencies in our study required officers to report all use-of-force incidents before CEDs were introduced and there is no evidence agencies changed their reporting requirements during the time frames of our study (other than reporting on issues specific to the CED, such as how many times CEDs were activated against suspects).

Limitations and barriers to research

Conducting use-of-force research is a difficult undertaking. There are a number of barriers to conducting rigorous multi-site research in the area of police use of force. First, some LEAs do not systematically maintain use-of-force data. Due to the large number of reported crimes in most U.S. cities and the very large number of contacts that the police have with the public, the opportunities for the police to use force are vast. If the police do not have a reporting system for clearly documenting cases involving police use of force, the task for researchers to do this *post hoc* is very difficult. In our work, we came across LEAs that did not have separate use-of-force forms for officers to complete in force cases. Instead, the force incident was recorded within the narrative of the crime report or arrest report, with no data field or check box indicating that a force incident occurred. We were not able to use these agencies in our study, because the task of reviewing the narratives of hundreds of thousands of reports to identify force incidents is not possible in a typical research project, and while the internal affairs departments for these agencies have separate files on force cases that they investigate for possible officer wrongdoing, this is only a small percentage of cases involving force for a typical agency. Our study was interested in all force cases, not just those that were investigated by an agency internal affairs department. Thus, if an agency does not have a use-of-force tracking system of some type, researchers we will not typically be able to include them in a research study on police use of force.

Another problem is the lack of standardization of data collection methods for different LEAs. Some LEAs collect only a limited number of fields on use of force, and do not capture important information, such as the nature of the force incident, the nature of any injuries, the weapons available to the officer, suspect characteristics, and suspect actions prior to the officer's use of force. We were unable to use agencies that do not collect data on factors that were critical to our study, such as officer and suspect injuries. Some LEAs provided measures of suspect level of resistance, but many did not. Consequently, this variable had to be excluded from our analyses. As pointed out by Smith et al. (2008), there is a tradeoff between retaining the maximum number of agencies for analysis and the precision of the measures and/or the number of measures used in the analysis. As experienced by Smith et al. (2008) in similar research, the data analyzed in our study represent only records routinely captured by LEAs and are missing many qualitative features of the force events, such as the nature of the incident that spurred the initial contact between the police and the citizen (e.g., domestic disturbance, robbery, routine traffic stop, etc.), whether the suspect was under the influence of drugs, and the duration of the incident. These factors have been shown in prior research to be correlated with differences in the seriousness and consequences of force incidents (Adams, 1999; Alpert & Dunham, 2004; Kaminski & Sorensen, 1995). The consequence of this situation, as pointed out by Smith et al. (2008), is that like all analyses outside of an experimental setting, our models are to some degree misspecified.

In other cases, the LEA collects the general category of force data, but codes it in such a way that it cannot be readily compared with force data from other agencies. For example, instead of being able to use a precise scale of level of injuries an officer endured, a research team might need to code the data simply as whether any injury occurred (yes or no) to achieve cross-site comparability. Having more detail regarding injuries imparts a number of important analytical benefits, such as the ability to model predictors of injury severity as opposed to a more limited analysis of whether or not an injury occurred (Smith et al., 2008).

Also, as was done in the Smith et al. (2008) study, we conducted an examination of the injury narratives as a validity check on the other injury indicators in the same dataset. As an example, some LEAs counted skin irritation from pepper spray and CED dart punctures as injuries. However, this is inconsistent with how we operationalized injuries from these devices in this study and the way the Smith et al. (2008) operationalized injuries. The additional details in the narratives allowed us to recode these cases. (CED dart wounds to unapproved targets, such as the groin or face, were counted as injuries, however.) Unfortunately, this recoding could not be done in all datasets, due to the lack of data in some narratives regarding injuries.

Similar coding issues arose with one of our CED variables. As discussed earlier, CEDs can be used in touch-stun mode or dart-mode, and because each mode has a different effect and is activated from different distances from subjects, injury patterns could vary by the mode employed (Smith et al., 2008). Ideally, we would have been able to measure whether a CED use was done in touch-stun or dart-mode. However, as in the Smith et al. (2008) study, this was not possible, for many of the LEAs in our study did not provide this extra level of detail. Our only alternative was to use a simple yes/no variable on whether a CED was used by an officer.

Next, a large number of LEAs only have paper records of their force data. To include these LEAs in our research required PERF to send a team of researchers to the LEA site to code these paper records into a standardized database. In addition to being time consuming, this approach increases the chances for errors in the data (even though our team used various quality checks). Also, due to the time-consuming nature of such a task, our team was limited to taking a random sample of cases for selected years, as opposed to having all of the data available. In our study, for five of the sites we needed to code data due to the absence of an electronic use-of-force database. Three of the five sites were non-CED sites and two were CED sites. We conducted statistical tests to assess whether the use of these different methods of data collection might affect our results. That is, we introduced an additional covariate (use of a random sample of cases=0 or use of the population of cases=1) to our logistic regression and HLM tests for each of our outcome measures. None of the new covariates were statistically significant, nor did the other variables in the model change appreciably.

Finally, due to the sensitive nature of police force data, some LEAs feel obligated to decline to participate in this type of research project. Some of the cases could be in litigation or pending litigation, and some LEA attorneys prefer not to have those or possibly other cases involved in a research study. Even when the research team can demonstrate that the data will be protected and handled confidentially (as we did), some LEAs might still feel compelled to err on the side of caution and decline participation. Non-participation is an issue in almost all aspects of social science research, and it can be particularly salient in the arena of police use-of-force studies, due to the sensitivity associated with the requested LEA data.

Data Collection/Measures

PERF requested hard and electronic copies of departmental data that included: use of force policy (past and current policies); specific documentation of the placement of CEDs on the department's "use of force continuum" policy; and all use-of-force incident data (including cases with and without use of CED weapons by the police) for at least four years. The PERF team collected the force data in one of two ways: (1) we sent a two-person research team to the participating LEA to conduct onsite archival review and coding of use of force documents, or (2) we collected electronic use-of-force data maintained by the participating LEA. Our team also worked with each site to collect crime and demographic data for each participating city. The sources of these data were the FBI's Uniform Crime Report (UCR) system and the U.S. Census.

First, there was agreement across the agencies in our study on the definition of a use-of-force case. The agencies counted a case as officer use-of-force if it included any physical strike or instrumental contact with a person by an officer or any significant physical contact that restricted the movement of a person by an officer, including the discharge of firearms, use of a Conducted Energy Device, use of chemical spray, use of any other weapon, choke holds or hard hands, taking of the subject to the ground, and deployment of a canine.

From the above data sources, we created both departmental/organizational-level measures and incident-level measures for our statistical models.

Our outcome measures, at the incident level, included the following: (the first four below are officer measures and the final five are suspect measures)

1. Officer injuries. This was a dichotomous yes/no variable for any impairment of physical condition, or pain to an officer due to the suspect's actions, including physical damage produced by the transfer of energy, such as kinetic, thermal, chemical, electrical, and radiant energy.
2. Officer injury severity. This was a dichotomous minor/severe variable, in which broken bones, stab wounds, and gun wounds were classified as severe, and bruises, lacerations, and burns or punctures were classified as minor.
3. Officer injury from a force incident requiring medical attention. This was a yes/no variable indicating whether the officer was seen by any type of medical professional, such as an on-scene emergency medical technician or medical personnel in a hospital, related to an officer use-of-force incident.
4. Officer injury from a force incident requiring hospitalization. This was a yes/no variable indicating whether an officer was taken to a medical facility such as hospital or medical clinic for treatment of an injury due to a use-of-force incident. By using the term "hospitalization" we do not mean to being admitted to a hospital for an overnight stay; information was not available regarding how many of these incidents resulted in an overnight stay, as opposed to an outpatient evaluation and/or treatment.
5. Suspect injuries. As with the first outcome measure, officer injuries, this was a dichotomous yes/no variable for any impairment of physical condition, or

pain due to an officer's actions, including physical damage produced by the transfer of energy, such as kinetic, thermal, chemical, electrical, and radiant energy.

6. Suspect injury severity. This was a dichotomous minor/severe variable, in which broken bones, stab wounds, and gun wounds were classified as severe, and bruises, lacerations, and burns or punctures were classified as minor.
7. Suspect deaths. This was a dichotomous yes/no variable indicating whether the suspect died during or as a result of an officer use-of-force incident. We had no officer deaths in our sample and therefore did not assemble a similar officer death measure.
8. Suspect injury from a force incident requiring medical attention. This was a yes/no variable indicating whether the suspect was seen by any type of medical professional, such as an on-scene emergency medical technician or medical personnel in a hospital, related to an officer use-of-force incident.
9. Suspect injury from a force incident requiring hospitalization. This was a yes/no variable indicating whether a suspect was taken to a medical facility such as hospital, medical clinic or medical facility within a custodial environment for treatment of an injury due to the use-of-force incident. Again, our use of the term "hospitalization" does not imply being admitted to a hospital for an overnight stay; information was not available regarding how many of these incidents resulted in an overnight stay, as opposed to an outpatient evaluation and/or treatment.

One of the concerns was making sure to standardize across the datasets for all the agencies on the minimum occurrence that would be defined as an "injury." For example, if one agency defines any "hands-on" activity by an officer as an injury, and another has a minimum threshold of a physical sign such as a bruise, cut, or scrape, we would have a problem making comparisons across these agencies using different definitions. A review of each agency's reporting policies on use of force showed general agreement that an injury could be any impairment of physical condition, or pain. We also confirmed this definition by reading narratives at agencies that collected narratives on injuries. None of the agencies counted "mental injuries." Also, none of the agencies distinguished between accidental injuries (e.g., the suspect accidentally trips after being handcuffed) and injuries caused by the officer's deliberate actions. All the agencies counted both of these types of injuries. Therefore, while the agencies varied in the level of detail they collected and coded regarding the nature of the injuries, there was agreement on the basic definition of an injury used by the agencies.

Our individual-level covariate measures were intended to help control for potentially important incident-level differences across our participating departments that might explain our outcome measures. While we would have liked to include in our statistical models a full range of incident-level factors (e.g., suspect demeanor, suspect alcohol/drug impairment, and size of suspect relative to the size of the officer) that have received empirical support in prior use-of-force research (see Garner, Maxwell, and Heraux, 2002), only a limited number of variables were available to our team based on agency records. However, each of the variables that were available and included in our models were either shown to be important predictors of use of force in prior research (see Garner et al., 2002) or had the potential to be important. *Two of the variables (weapons and physical aggression* *by the suspect) represent situational factors that might influence whether an officer might use force. The other three variables are demographic factors that might be associated with whether an officer might use force (race, gender and age).* Our individual-level control/independent variables included the following:

1. Suspect race (1= white and 0=non-white)
2. Suspect gender (male=1, female=0)
3. Suspect age (under 25 years old=1 and over 25 years old=0)
4. Suspect behavior (whether the suspect used physical aggression or physical resistance of any type against the officer)
5. Suspect use of any weapon (yes=1, no=0)

Our aggregate-level measures were intended to help control for contextual differences across the participating sites that might impact our outcome measures. Our aggregate/departmental-level (control/independent variable) measures included the following:

1. Total # of sworn officers per 100,000 people in the population
2. Total # of arrests per 100,000 people in the population
3. Total # of violent crime arrests per 100,000 people in the population
4. Total # of Part I UCR crimes per 100,000 people in the population
5. Total # of violent crimes per 100,000 people in the population
6. Total # of homicides per 100,000 people in the population
7. Percentage of the population in the jurisdiction below poverty
8. Median household income for the population in the jurisdiction
9. Percent unemployed for the population in the jurisdiction
10. Population size/density for the population in the jurisdiction
11. Percent female-headed household with children for the population in the jurisdiction
12. Residential stability for the population in the jurisdiction
13. Racial heterogeneity for the population in the jurisdiction
14. Percent male for the population in the jurisdiction
15. Percent aged 15 to 24 for the population in the jurisdiction

Data Analysis

Descriptive/bivariate analyses – First, we cleaned all the data using standard data-cleaning processes to verify that the data are correct and conform to a set of rules. We wrote SPSS programs to remove errors and inconsistencies in all data files. Our first sets of analyses are descriptive statistics for all the main study variables for the entire sample (CED and non-CED site data combined). Second, we provide a graphical presentation of our bivariate results for the CED versus non-CED site comparisons on our outcome measures.

Multivariate analyses – Because QEDs involve comparison groups of unknown equivalence and tend to involve many different but interlocking relationships between variables, the development of statistical models becomes a critical process. Statistical models will control for possible pre-treatment differences between departments with CEDs and those without CEDs that could affect our outcome measures. A variety of modeling techniques exist (see Asher 1983), and a major problem in analyzing data from QEDs is model misspecification that can lead to biased estimates of treatment effects (Trochim, Cappelleri, and Reichardt, 1991). Modeling and theory will allow us to identify and remove from our models spurious variables that do not help predict the relationship between CED use/policies and our outcomes. It will also help find suppression effects when part of a variable affects part of another variable even though the bivariate relationship is not statistically significant.

To address the incident-level part of our data within each department, we will begin with the use of logistic regression. As discussed earlier, to assure the use of standard measures across all of our sites we were required to dichotomize our outcome measures. Logistic regression is an appropriate technique to assess such binary outcome measures. Logistic regression allows us to include an enormous amount of information, which will be necessary to control for all the potential confounding factors between police departments.

One of the concerns in analyzing data across multiple sites is the clustering/nesting of data. Nesting occurs when a unit of measurement is a subset of a larger unit and the units clustered in the larger unit might be correlated. In our study, individual cases of weapon use by officers are nested within specific police departments that have varying policy guidelines on the use of force. Ignoring the nested structure of our data (e.g., conducting only logistic regressions) can potentially lead to biased estimates. In the past, hierarchical data were analyzed using conventional regressions, but these techniques can yield biased standard errors and potentially spurious results (Hox, 2002). That is, using uni-level analysis methods on multilevel data can lead to parameter estimates that are unbiased but inefficient, and the standard errors are negatively biased, which results in spuriously ‘significant’ effects (see De Leeuw and Kreft, 1986; Snijders and Bosker, 1999; Hox, 1998, 2002).

First, the background circumstances of the individual cases of use of force may vary appreciably from department to department. Factors such as these can give rise to a degree of dependency or similarity among the observations nested within a department. Ignoring such dependencies (i.e., the intra-class correlational structure of multi-site data) can result in deflated standard errors for treatment effect estimates (Hox, 2002). Moreover, if we ignore the nesting of individuals in different departments in our analyses, we run the risk of inadvertently concealing potentially substantial between-department heterogeneity in the effects of CEDs. Such heterogeneity is likely given that the sites are likely to vary considerably.

We will use Hierarchical Linear Modeling (HLM) to obtain more appropriate standard errors for estimates of CEDs’ effects.[14] HLM will be used to assess how differences in agency-level and incident-level factors relate to differences in outcomes across departments. HLM will be used to assess differences in use of force and respective outcomes pre- and post-CED deployment.

We used HLM 6 software (developed by Raudenbush et al., 2004). HLM provides

[14] While our primary strategy was to run HLM models to address this clustering issue, before that we examined the results of using a logistic regression with a robust variance estimate to adjust for within-cluster correlation. We conducted these analyses using Stata statistical software with the vce (cluster *clustvar*) option. The robust variance estimator comes under various names in the literature and within the Stata software it is known as the Huber/White/sandwich estimate of variance. The names Huber and White refer to the seminal references for this estimator (Huber, 1967; White, 1980). The main limitation with using this approach is that we do not get aggregate-level coefficients like those produced using HLM, but we are still able to address the clustered nature of our data and produce unbiased estimates (Rogers, 1993; Williams, 2000; Wooldridge, 2002). We use this approach to examine more closely the viability of the standard logistic regression results, before examining a full multi-level HLM approach.

a conceptual framework and a flexible set of analytic tools to analyze the special requirements of our nested data. At level 1 of an HLM the analysis an outcome variable is predicted as a function of a linear combination of one or more level 1 variables, plus an intercept, as so:

$$Y_{ij} = \beta_{0j} + \beta_{1j}X_1 + \ldots + \beta_{kj}X_k + r_{ij}$$

where β_{0j} represents the intercept of group j, β_{1j} represents the slope of variable X_1 of group j, and r_{ij} represents the residual for individual i within group j. On subsequent levels, the level 1 slope(s) and intercept become dependent variables for level 2:

$$\beta_{0j} = \gamma_{00} + \gamma_{01}W_1 + \ldots + \gamma_{0k}W_k + u_{0j}$$

$$\beta_{1j} = \gamma_{10} + \gamma_{11}W_1 + \ldots + \gamma_{1k}W_k + u_{1j}$$

and so forth,

where γ_{00} and γ_{10} are intercepts, and γ_{01} and γ_{11} represent slopes predicting β_{0j} and β_{1j} respectively from variable W_1. Through this process, we accurately model the effects of level 1 variables (i.e., individual/incident-level variables) on the outcome (e.g., injuries), and the effects of level 2 variables (i.e., aggregate/site-level variables such as agency policy on use of CEDs) on the outcome. We will be examining differences in the above outcomes across two annual points in time after the CEDs were implemented, controlling for any observed pre-test differences in the comparison groups during the two year period before CEDs were implemented.

Statistical power: Statistical power provides an estimate of how often one would fail to identify a relationship that in fact existed (Weisburd, Petrosino and Mason, 1991; Cohen, 1988). Power is jointly determined by sample size and effect size. One of the most widely accepted methods of evaluating effect sizes is Cohen's formulation (treatment mean - control mean/shared variance): small effects= .25, medium effects= .75, and large effects= 1.25. For our logistic regression models, where we are working with just the incident-level data, we have more than enough statistical power. That is, we have thousands of use-of-force cases to analyze. At the individual/incident-level, our study would be able to detect small effect size differences (< 0.10) across the CED versus non-CED comparison groups (assuming an alpha of .05, a two-sided test and power of .95). What this means is that with our sample size, we have power of greater than 95% to yield statistically significant results even when the differences in proportions between the CED and non-CED sites are less than 10%.

However, for our HLM analyses, where we explicitly model the nested nature of our data, we only have 13 higher-level units. Given our need to use HLM, we conducted statistical power calculations to assess whether we had enough cases in our analysis to find statistically significant differences in CED (n=7) and non-CED (n=6) agencies if they existed. We used computer routines developed by Raudenbush and Liu (2000) to calculate statistical power for our HLM test. This program calculates approximate standard errors and optimal sample sizes for estimates of fixed effect parameters with multiple levels. Our study with 13 departments is able to detect only large effect size differences with a power level of .80 (assuming an alpha of .05, a one-sided test, Level-1 residual variance of 25, Level-2 residual variance of 10, and an intra-class correlation coefficient of .15). Overall, we have less statistical power to assess differences when explicitly modeling the nested nature of our data through HLM than when we conduct the logistic regressions and examine only the incident/individual-level data. That is, for the HLM analyses we were only able to detect large effects as opposed to our ability to detect very small effect size differences when we conducted our logistic regressions. Given the difficulty of on-site data collection and associated costs, we were not able to increase the number of departments beyond 13. Our purpose in using HLM is to assess the nested structure of our data and assure that our logistic regression estimates are not biased. We were able to produce unbiased HLM estimates – just with less statistical power across departments than within departments. In our HLM results, we were looking to confirm the direction and magnitude of our logistic regression results, and relied less on our tests of statistical significance (where our statistical power was fairly modest).

CHAPTER 4: Study Results

The first sets of analyses include descriptive statistics for all the main study variables. The second sets of analyses are our multivariate analyses using logistic regression. The third sets of analyses are our multi-level analyses using HLM to address potential nesting effects due to the fact that the individual use-of-force cases we analyze are clustered within 13 departments.

Descriptive statistics:

Univariate results: In Table 4 (see below), we present univariate results for each of our main outcome measures, comparing our pre- and post-test results for our CED sites and non-CED sites. In the section below, we review the overall sample results (CED and non-CED combined) to provide context for our later analyses. Immediately after this section, we conduct statistical tests to compare the pre- and post-CED implementation results for CED versus non-CED sites and present the results graphically.

Generally, our data suggest that the vast majority of officers are not injured in use-of-force cases (see Table 4). For the CED and non-CED sites, our data suggest that 11% of officers were injured in use-of-force cases in the pre-period and 9% in the post–period; and suspect injuries were more common in use-of-force cases (for the CED and non-CED sites 24% in the pre-period and 29% in the post-period) than officer injuries. Our data suggest that medical attention for officer injuries (pre-period 11% and 8% post-period) was much less common than medical attention for suspect injuries (pre-period 51% and 41% post-period). Likewise, hospitalization for officer injuries (pre-period 4.1% and 4.3% post-period) was less common than hospitalization for suspect injuries (pre-period 28% and 17% post-period). Our data suggest that the proportion of officers receiving severe injuries (4.5% pre-period and 5.6% post-period) was similar to the same measure for suspects (6.7% pre-period and 5.6% post-period). There were no recorded officer deaths in our sample, and fewer than 1% of the use-of-force cases in our sample had a suspect death (0.3% for the pre-period and 0.4% for the post-period).

The proportion of white suspects was just under one-third for the whole sample (32.7% in the pre-period and 30.7% in the post period) or conversely the non-white sample was just over two-thirds. The proportion of male suspects was over 85% across both time periods for the whole sample (85.1% in the pre-period and 86.3% in the post-period). The proportion of suspects under 25 years old was more than one-third for the whole sample (38.7% in the pre-period and 39.3% in the post-period). Our data indicate that the proportion of suspects using physical aggression against officers was about one-third for the whole sample (32.5% in the pre-period and 34.2% in the post-period). Our data indicate that the proportion of suspects with a weapon at the

Table 4: Descriptive Statistics for non-CED versus CED sites and all sites: Percentages for all study variables

	Non-CED Site	CED Site	All sites	N for all sites
Officer injury (pre period)	10.3	11.5	11.3	1,058
Officer injury (post period)	20.3	8.3	9.4	7,670
Suspect injury (pre period)	29.9	22.8	24.4	2,234
Suspect injury (post period)	42.5	26.6	29.4	9,131
Medical attention for officer injuries (pre period)	3.5	13.2	11.3	910
Medical attention for officer injuries (post period)	15.9	7.5	8.2	6,521
Medical attention for suspect injuries (pre period)	35.2	54.8	51.3	1,068
Medical attention for suspect injuries (post period)	53.2	39.8	40.8	8,944
Hospitalization for officer injuries (pre period)	3.3	4.3	4.1	847
Hospitalization for officer injuries (post period)	6.3	4.1	4.3	6,513
Hospitalization for suspect injuries (pre period)	30.5	26.8	27.5	762
Hospitalization for suspect injuries (post period)	36.3	16.2	16.9	8,875
Officer severe injury (pre period)	7.0	4.0	4.5	1,058
Officer severe injury (post period)	6.4	5.3	5.6	7,670
Suspect severe injury (pre period)	7.3	6.5	6.7	2,234
Suspect severe injury (post period)	7.2	5.0	5.6	9,131
Suspect deaths in force incidents (pre period)	0.9	0.2	0.3	1,952
Suspect deaths in force incidents (post period)	0.9	0.4	0.4	9,279
Suspects White/Caucasian (pre period)	43.8	30.9	32.7	1,379
Suspects White/Caucasian (post period)	35.0	30.3	30.7	11,922
Suspects male (pre period)	85.0	85.1	85.1	2,330
Suspects male (post period)	84.9	86.5	86.3	12,067
Suspects under 25 years old (pre period)	38.1	38.9	38.7	2,124
Suspects under 25 years old (post period)	40.1	39.2	39.3	8,873
Suspect used physical aggression against officer (pre period)	30.7	35.8	32.5	2,237
Suspect used physical aggression against officer (post period)	23.2	37.9	34.2	3,892
Suspect had weapon (pre period)	27.7	16.3	19.5	1,416
Suspect had weapon (post period)	50.5	10.7	15.7	6,444
Officer used CEDs only against suspect (pre period)	0.0	0.0	0.0	2,350
Officer used CEDs only against suspect (post period)	0.0	11.1	9.6	11,797
Officer used baton only against suspect (pre period)	4.1	1.4	2.0	2,350
Officer used baton only against suspect (post period)	7.0	0.8	1.7	11,797
Officer used OC spray only against suspect (pre period)	11.3	13.8	13.3	2,350
Officer used OC spray only against suspect (post period)	16.2	8.1	9.2	11,797
Officer used some weapon other than CEDs,OC,batons or used multiple weapons involving a CED, OC, or baton (pre period)	55.4	27.6	33.5	2,350
Officer used some weapon other than CEDs,OC,batons or used multiple weapons involving a CED, OC, or baton (post period)	67.5	38.3	42.3	11,797
Officer used other form of non-weapon force (pre period)	29.1	54.8	49.3	2,350
Officer used other form of non-weapon force (post period)	9.3	41.6	37.2	11,797

force incident was under 20% for the whole sample (19.5% in the pre-period and 15.7% in the post-period).

In terms of actual weapon use, no one in the sample used CEDs in the pre-period. In the CED sites, our data indicate that 11% of their force cases involved use of a CED only. For both CED and non-CED sites, our data indicate that use of batons by themselves is not common (2% in the pre-period and 1.7% in the post-period). For the sample as a whole, our results suggest that use of only OC spray is more common (13% in the pre-period and 9% in the post-period) than CED and baton use. For the sample as a whole, our data indicate that solo use of weapons other than CEDs, batons, or OC spray (or multiple weapon use involving CEDs, batons, OC spray or some other weapon) occurs in over one-third of the force cases (34% in the pre-period and 42% in the post-period). We also found evidence that officer use of non-weapon force (e.g., hands-on tactics) is common in force incidents (49% in the pre-period and 37% in the post-period).

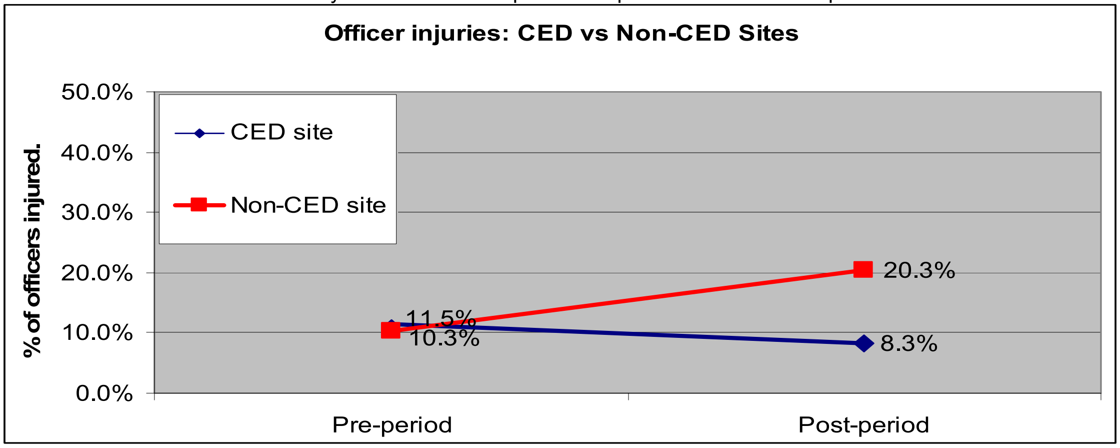
Bivariate results: The next sets of findings are for our bivariate results comparing outcomes for the CED and non-CED sites. Below we present the bivariate results graphically, and present chi-square statistics (in the text within parentheses) to assess the statistical significance of our bivariate comparisons.

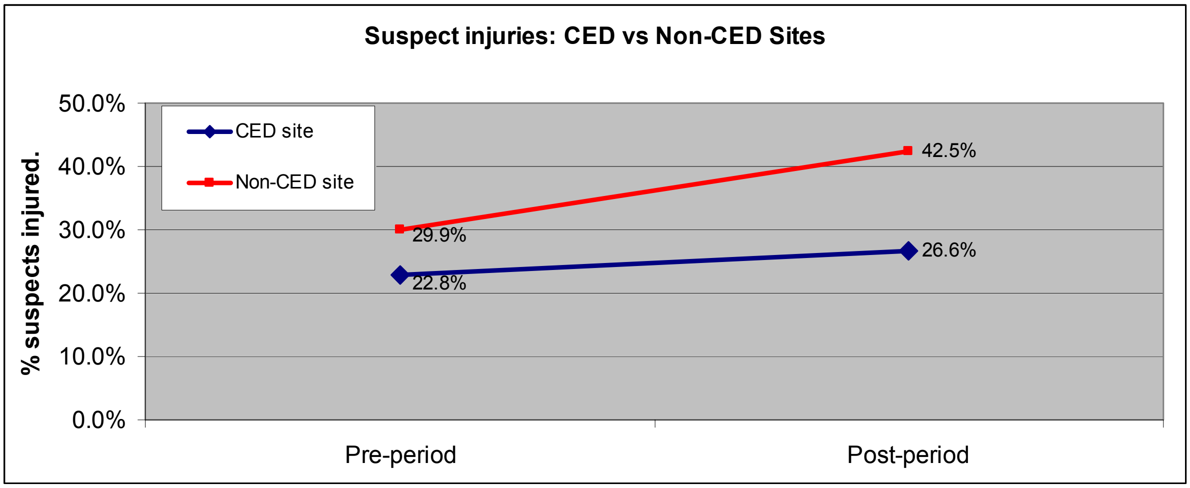
Officer injuries: Our first chart explores differences between CED and non-CED sites on the proportion of use-of-force cases where an officer was injured before CEDs were implemented and after CEDs were implemented. Before the CED sites had deployed CEDs, our data suggest that 11.5% of the officers were injured in force cases compared to a similar proportion of officers in the non-CED sites (10.3%) over the same reference period, representing no statistical difference (X^2= 0.78, df=1, p=.38). However, we found that the CED sites observed a reduction in officer injuries (8.3%) after they began their deployment of CEDs, while the non-CED sites observed an increase in officer injuries to 20.3% (X^2= 52.68, df=1, p<.001).

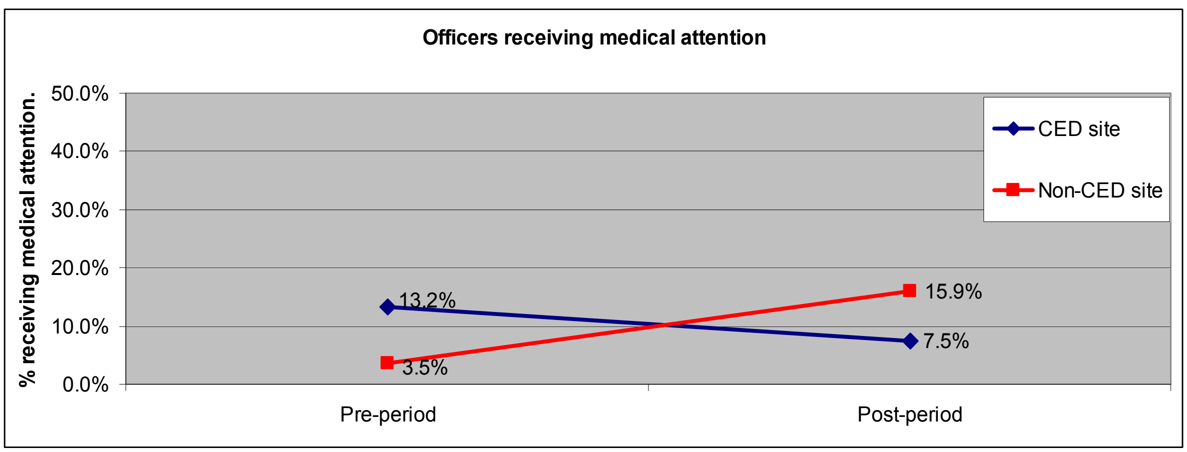
Suspect injuries: Before the CED sites deployed CEDs, our data suggest that 22.8% of their suspects were injured in force cases, compared to a slightly higher proportion of suspects in the non-CED sites (29.9%) over the same reference period, representing a statistically significant difference (X^2= 23.68, df=1, p<.001). The CED sites observed a small increase in suspect injuries (26%) after they began their deployment of CEDs, while the non-CED sites observed a much larger increase in suspect injuries to 42.5% (X^2= 102.02, df=1, p<.001). While the CEDs started out at a slightly lower rate of suspect injuries compared to the non-CED sites (22.8% to 29.9%), our data suggest that the CED sites were substantially lower at the post-period (26% to 42.5%), at a rate much greater than the initial differences would predict.

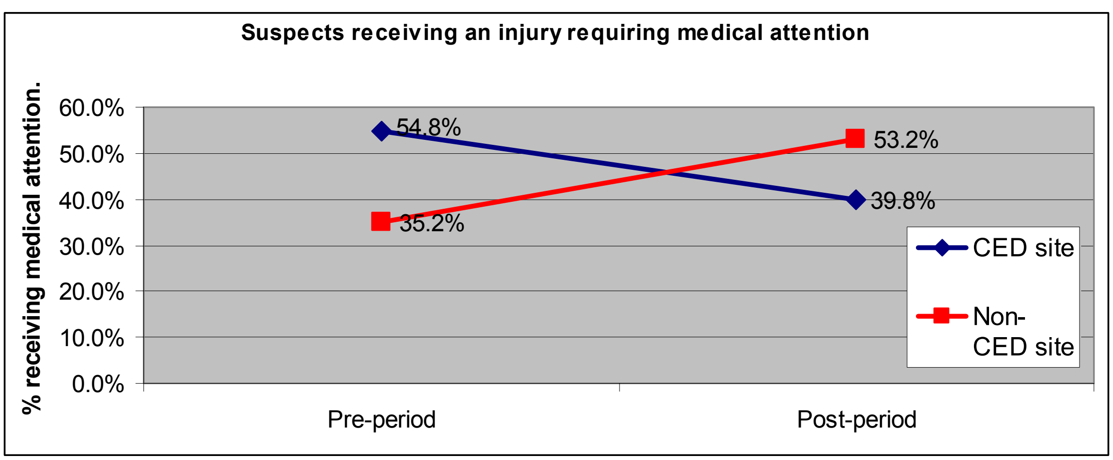
Officer injury requiring medical attention: Before the CED sites deployed CEDs, our data suggest that 13.2% of their officers received medical attention for injuries in force cases compared to a lower proportion of officers in the non-CED sites (3.5%) over the same reference period, representing a statistically significant difference (X^2= 45.07, df=1, p<.001). The CED sites observed a large decrease in officers receiving medical attention for injuries (7.5%) after they began their deployment of CEDs, while the non-CED sites observed a large increase in officers receiving medical attention for injuries to 15.9% (X^2= 29.78, df=1, p<.001). While the CEDs started out at a higher rate of officers receiving medical attention for injuries compared to the non-CED sites (13.2% to 3.5%), our data indicate that the CED sites were substantially lower in the post-period (7.5% to 15.9%).

Suspect injury requiring medical attention: Before the CED sites deployed CEDs, our data suggest that 54.8% of their suspects received medical attention for injuries in force cases, compared to a lower proportion of suspects in the non-CED sites (35.2%) over the same reference period, representing a statistically significant difference (X^2= 72.68, df=1, p<.001). The CED sites observed a large decrease in suspects receiving medical attention for injuries (39.8%) after they began their deployment of CEDs, compared to the non-CED sites that observed a large increase in suspects receiving medical attention for injuries to 53.2% (X^2= 33.97, df=1, p<.001). While the CEDs started out at a higher rate of suspects receiving medical attention for injuries compared to the non-CED sites (54.8% to 35.2%), our data suggest that the CED sites were substantially lower at the post-period (39.8% to 53.2%).


Officer injuries: CED vs Non-CED Sites
% of officers injured.
50.0%
40.0%
30.0%
20.0%
10.0%
0.0%
CED site
Non-CED site
11.5%
10.3%
20.3%
8.3%
Pre-period
Post-period


Suspect injuries: CED vs Non-CED Sites
% suspects injured.
50.0%
40.0%
30.0%
20.0%
10.0%
0.0%
CED site
Non-CED site
42.5%
29.9%
26.6%
22.8%
Pre-period
Post-period


Officers receiving medical attention
% receiving medical attention.
50.0%
40.0%
30.0%
20.0%
10.0%
0.0%
CED site
Non-CED site
13.2%
15.9%
7.5%
3.5%
Pre-period
Post-period


Suspects receiving an injury requiring medical attention
% receiving medical attention.
60.0%
50.0%
40.0%
30.0%
20.0%
10.0%
0.0%
54.8%
53.2%
39.8%
35.2%
CED site
Non-CED site
Pre-period
Post-period

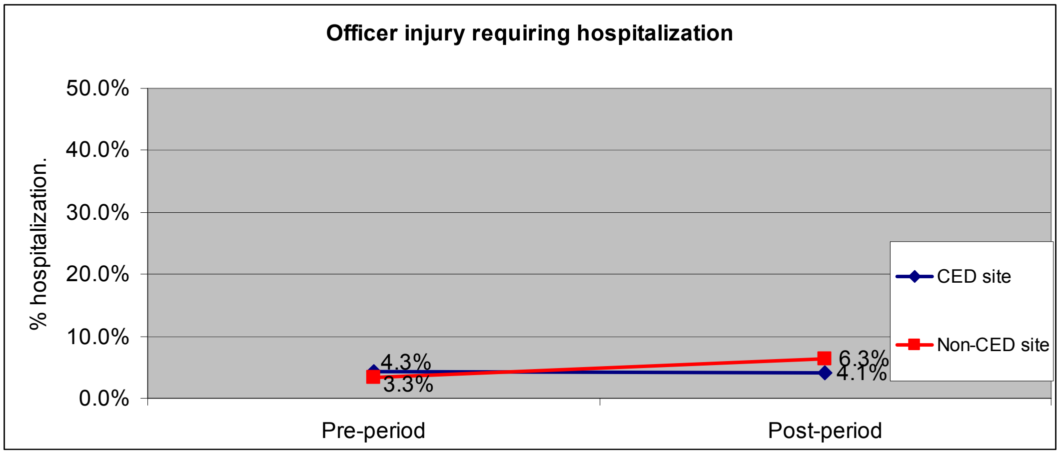
Officer injury requiring hospitalization: Before the CED sites deployed CEDs, our data suggest that 4.3% of the officers required hospitalization for injuries in force cases, compared to a similar proportion of officers requiring hospitalization in the non-CED sites (3.3%) over the same reference period, representing no statistical difference (X^2= 0.89, df=1, p=.35). The CED sites observed a very small decrease in officers requiring hospitalization for injuries (4.1%) after they began their deployment of CEDs, compared to the non-CED sites that observed an increase in officer requiring hospitalization for injuries to 6.3% (X^2= 3.9, df=1, p<.05). The CEDs started out at a similar rate of officers requiring hospitalization for injuries compared to the non-CED sites (3.3% to 4.3%), but the CED sites were significantly lower at the post-period (4.1% to 6.3%).

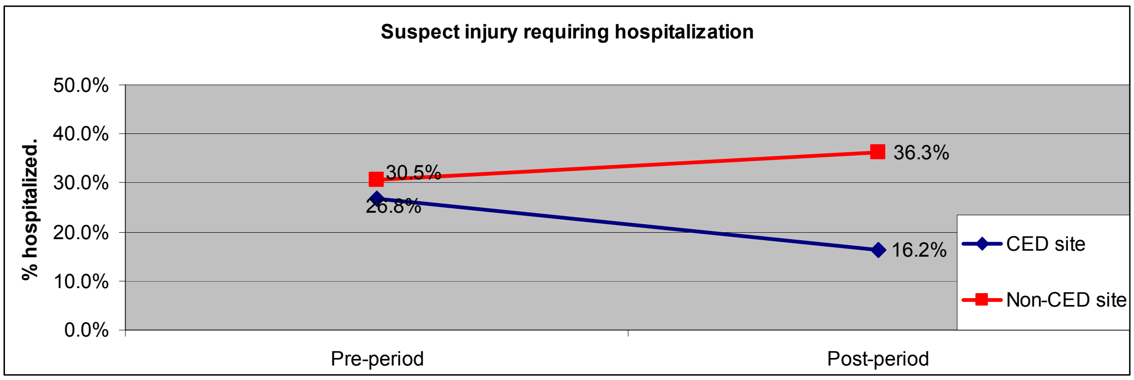
Suspect injury requiring hospitalization: Before the CED sites deployed CEDs, our data suggest that 26.8% of their suspects required hospitalization for injuries in force cases, compared to a similar proportion of suspects requiring hospitalization in the non-CED sites (30.5%) over the same reference period, representing no statistical difference (X^2= 2.57, df=1, p=.11). The CED sites observed a large decrease in suspects requiring hospitalization for injuries (16.2%) after they began their deployment of CEDs, compared to the non-CED sites that observed a small increase in suspects requiring hospitalization for injuries to 36.3% (X^2= 61.59, df=1, p<.05). The CEDs started out at a similar rate of suspects requiring hospitalization for injuries compared to the non-CED sites (26.8% to 30.5%), but our data suggest that the CED sites were significantly lower at the post-period (16.2% to 36.3%).

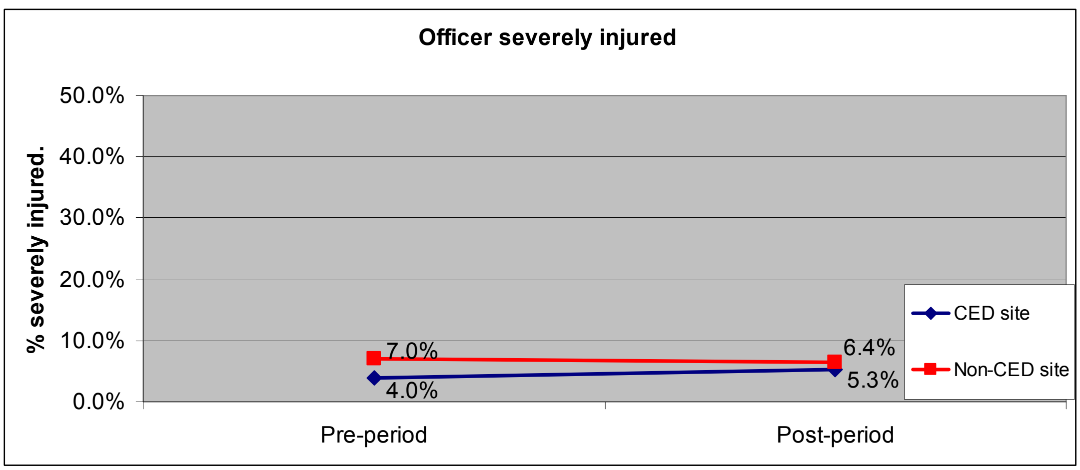
Officer severe injuries: Before the CED sites deployed CEDs, our data suggest that 4% of their officers were severely injured in force, cases compared to a similar proportion of officers in the non-CED sites (7%) over the same reference period, representing no statistical difference (X^2= 1.32, df=1, p=.25). Our data also suggest that the CED sites observed no significant change in officer severe injuries (5%) after they began their deployment of CEDs, compared to the non-CED sites that observed no change in officer severe injuries for the non-CED sites to 6.4% (X^2= 0.20, df=1, p=.66).

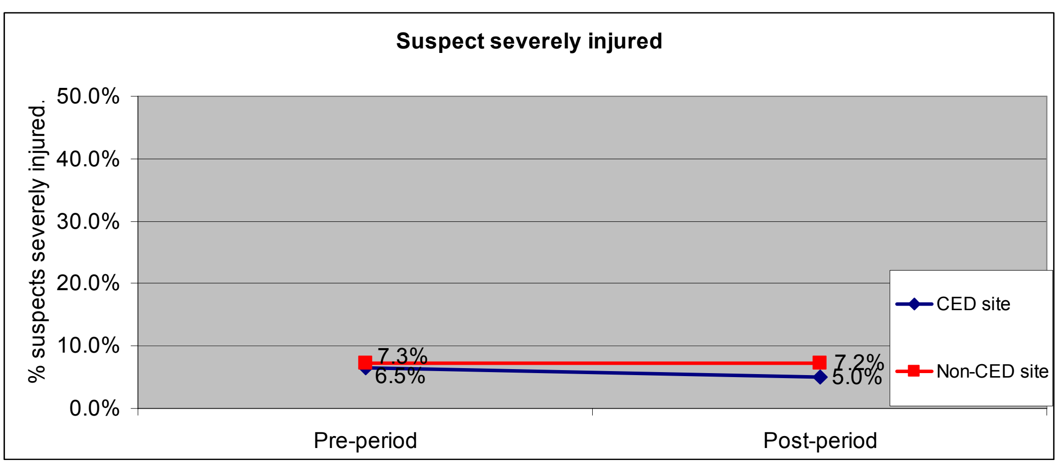
Suspect severe injuries: Before the CED sites deployed CEDs, our data suggest that 6.5% of their suspects were severely injured in force cases, compared to a similar proportion of suspects in the non-CED sites (7.3%) over the same reference period, representing no statistical difference (X^2= 0.23, df=1, p=.63). However, our data suggest that the CED sites observed a reduction in suspects' severe injuries (5%) after they began their deployment of CEDs, compared to the non-CED sites that observed no change in suspect severe injuries to 7.3% (X^2= 3.75, df=1, p<.05).

Suspect deaths: Due to the absence of any officer deaths in our sample, we were only able to examine suspect deaths occurring in relation to a police use-of-force incident. We had 28 suspect deaths in our sample, and our analysis of these cases is limited to the bivariate results we present below (multivariate modeling was not possible with our suspect death variable). Before the CED sites deployed CEDs, our data suggest that 0.2% of the suspects were killed in force cases, compared to a higher proportion of suspects in the non-CED sites (0.9%) over the same reference period, representing a small statistical difference (X^2= 8.7, df=1, p<.01). The CED sites


Officer injury requiring hospitalization
% hospitalization.
50.0%
40.0%
30.0%
20.0%
10.0%
0.0%
4.3%
3.3%
6.3%
4.1%
Pre-period
Post-period
CED site
Non-CED site


Suspect injury requiring hospitalization
% hospitalized.
50.0%
40.0%
30.0%
20.0%
10.0%
0.0%
30.5%
26.8%
36.3%
16.2%
Pre-period
Post-period
CED site
Non-CED site


Officer severely injured
% severely injured.
50.0%
40.0%
30.0%
20.0%
10.0%
0.0%
7.0%
4.0%
6.4%
5.3%
Pre-period
Post-period
CED site
Non-CED site


Suspect severely injured
% suspects severely injured.
50.0%
40.0%
30.0%
20.0%
10.0%
0.0%
7.3%
6.5%
7.2%
5.0%
Pre-period
Post-period
CED site
Non-CED site

observed about the same number of suspects killed in force incidents (0.4%) after they began their deployment of CEDs, compared to the non-CED sites that also observed no change in the number of suspects killed in force incidents (0.9%). While the post-period results for CEDs (0.4%) and non-CEDs (0.9%) represents a small statistical difference (X^2= 4.02, df=1, p<.05) we do not believe this difference is necessarily attributable to the presence of CEDs (more than likely it is just random noise in the data). The CED sites started out at a lower rate of suspect deaths compared to the non-CED sites (0.2% vs. 0.9%), and this difference simply held up over the post period (CED sites= 0.4% to non-CED sites= 0.9%).[15] On balance, our data suggest that CEDs do not appear to have much of an effect on suspect deaths. That is, while at the post-test the CED sites had fewer suspect deaths than the non-CED sites, this seems to reflect the fact that the CED sites had fewer suspect deaths prior to the deployment of CEDs.

To confirm our conclusion on suspect deaths, we also examined whether officer use of guns in force cases (including cases where suspects died as well as cases in which suspects lived) changed for the CED sites compared to the non-CED sites. We did not find evidence that CEDs had an effect on the proportion of use-of-force cases where an officer used a firearm. Before implementation of CEDs, our data suggest that the CED sites had less than one percent of their cases (0.6%) involving an officer using a firearm. After CED implementation, our data suggest that this number remained the same statistically and below one percent (0.9%). During the same period, the non-CED sites did not statistically change either on this measure. The non-CED sites observed about two percent of their cases (2.2%) involving an officer using a firearm at the pre-test period, and observed no statistical change in the number of officers using firearms in force incidents at the post-period (2.9%). On balance, our data suggest that CEDs do not appear to have much of an effect on officer use of firearms in force incidents, affirming the above finding regarding suspect deaths. Due to the small sample size of use of force cases involving firearms (overall, only about 1% of our cases involved an officer using a firearm) we limit our analyses of the firearms data to these bivariate results.[16]

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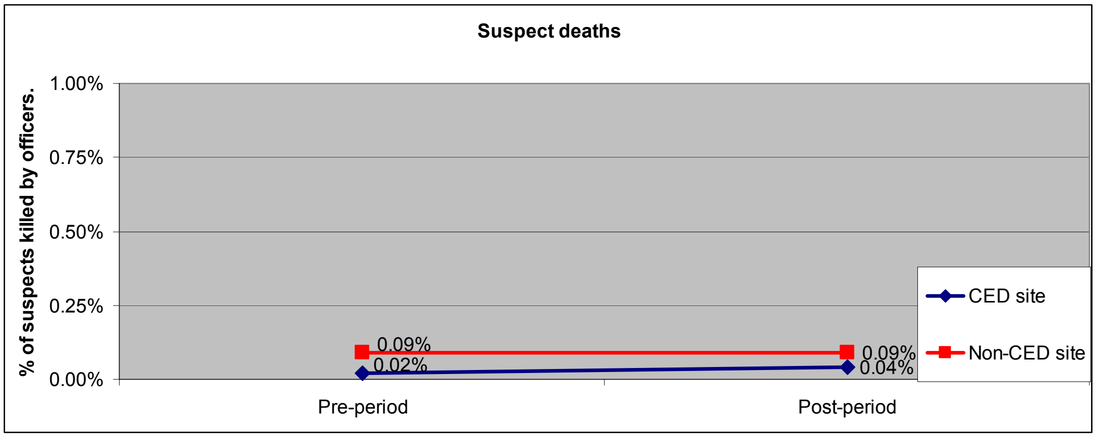
15 While we are very concerned about small sample size for this analysis, we did attempt to estimate a logistic regression to assess whether there was a statistical change from pre to post for the CED compared to non-CED sites. We found no statistical difference (B= -.02, p=.98) (further substantiating our conclusion above that there was no difference between CED and non-CED sites on the outcome of suspect deaths).

		Odds Ratio	SE	P value
Intercept	-5.04	0.01	1.18	0.00
Does Agency Deploy CED (1=yes, 0=no)	-1.79	0.17	1.21	0.14
Time frame of incident (post-CED/comparable period=1, pre-CED/ comparable period=0)	0.22	1.25	0.60	0.71
Interaction CED * Time Frame (1=CED and post period)	-0.02	0.98	0.77	0.98
Suspect race (White=1, Non-White=0)	0.21	1.23	0.39	0.60
Suspect gender (Male=1, female=0)	0.80	2.22	0.74	0.28
Suspect age (1=<25 years old, 0=>25 years old)	-1.13	0.32	0.49	0.02

16 While we are very concerned about small sample size for this analysis on officer gun use, we did attempt to estimate a logistic regression to assess whether there was a statistical change from pre to post for the CED compared to non-CED sites on officer use of guns. We found no statistical difference (B= -.004, odds ratio= .996, SE= .44, p=.99) (further substantiating our conclusion above that there was no difference between CED and non-CED sites on the outcome of officer use of gun in force cases).



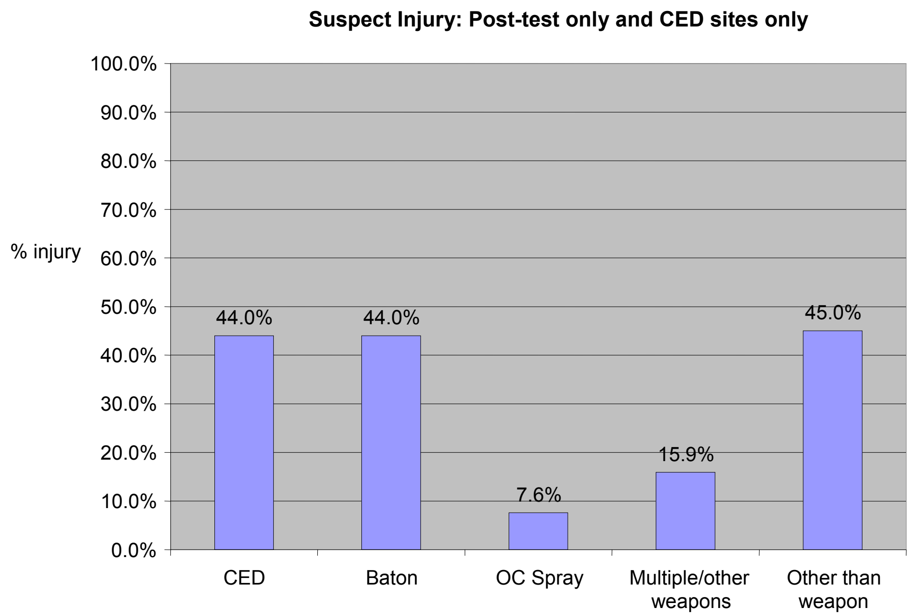
Comparing types of use-of-force by the police for CED sites only (and post-test only):

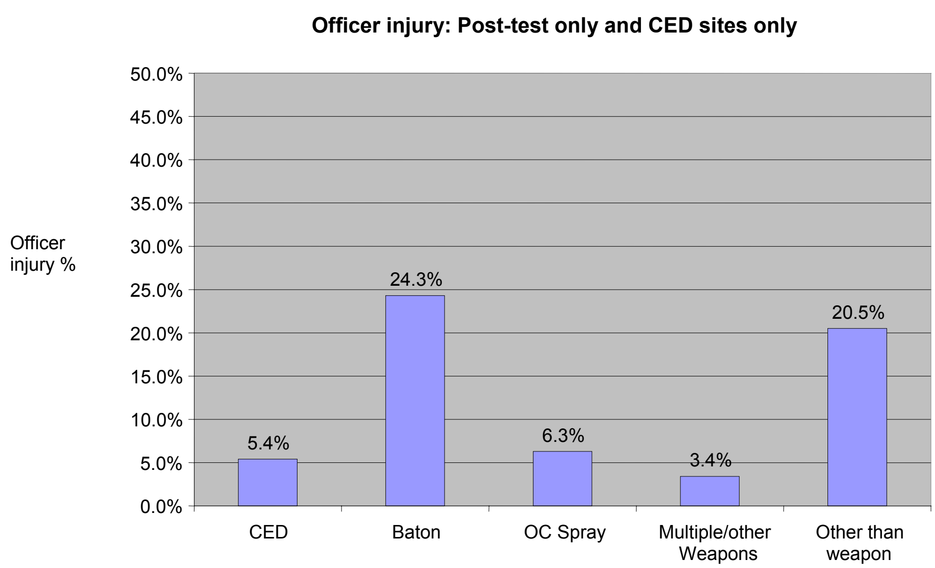
The next set of analyses focus on just the participating CED sites. For these analyses, we examine the period after CEDs have been deployed, comparing the actual use of CEDs by officers to other forms of use of force. Obviously we can only examine the use of CEDs in the post-period (after CEDs were introduced within the LEA), but we also limit our analyses of the non-CED force cases to the post-period to remove any potential temporal effects on our comparisons. That is, it could be potentially problematic to have the CED site data covering only a two-year period and the non-CED site data covering four years. Our first sets of analyses are bivariate models. Our second sets of analyses are multivariate models to confirm the earlier bivariate results. For these models, we coded our use-of-force data into five categories: CED use only, baton use only, OC spray use only, other weapon use or multiple weapon use, and non-weapon force by officers (hands-on tactics and other non-weapon approaches).

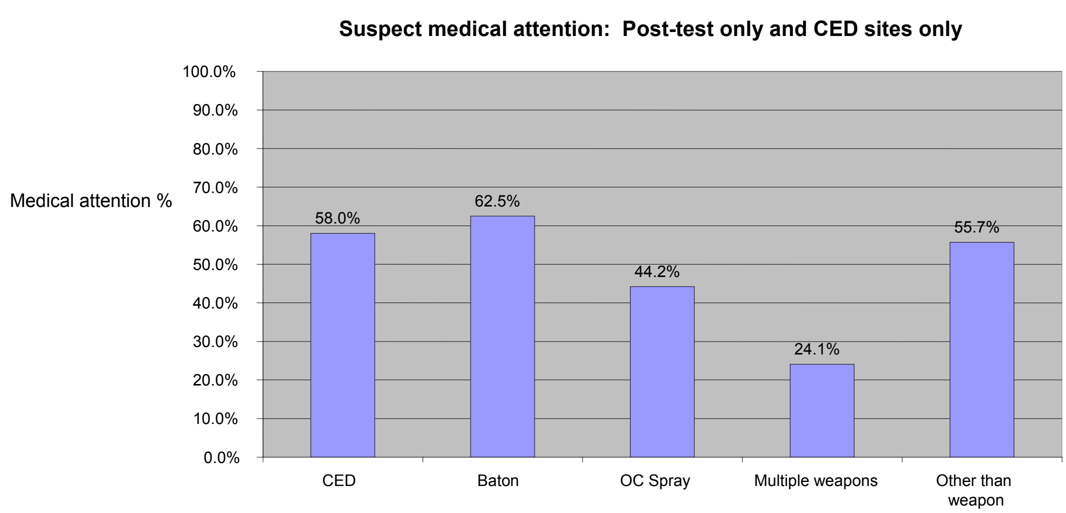
Suspect injuries: For suspect injuries (see chart below), non-weapon/hands-on tactics were associated with the highest levels of suspect injuries (45%), followed by batons (44%), CEDs (44%), multiple weapons (or weapons other than CEDs, batons, or OC spray) (15.9%), and OC spray (7.6%). For these analyses, our data suggest that OC spray and multiple/other weapons were associated with significantly lower suspect injuries than the other forms of force (X^2= 570.25, df=4, p<.001).

Officer injuries: For officer injuries (see chart below), batons (24.3%) and non-weapon/hands-on tactics (20.5%) were associated with the highest levels of officer injuries, followed by OC spray (6.3%), CEDs (5.4%), and multiple weapons (or weapons other than CEDs, batons, or OC spray) (3.4%). For these analyses, our data suggest that multiple/other weapons, CEDs, and OC spray and were associated with significantly lower officer injuries than the other forms of force (X^2= 264.97, df=4, p<.001).

Injuries requiring medical attention for the suspect: For suspect medical attention (see chart below), batons (62.5%), CEDs (58%) and non-weapon/hands-on tactics (55.7%) were associated with the highest levels of suspects requiring medical attention. For these analyses, our data suggest that multiple/other weapons and OC spray were associated with significantly lower number of cases where suspects required medical attention than the other forms of force (X^2= 644.98, df=4, p<.001).


Suspect Injury: Post-test only and CED sites only
% injury
100.0%
90.0%
80.0%
70.0%
60.0%
50.0%
40.0%
30.0%
20.0%
10.0%
0.0%
44.0%
44.0%
7.6%
15.9%
45.0%
CED
Baton
OC Spray
Multiple/other weapons
Other than weapon


Officer injury: Post-test only and CED sites only
Officer injury %
50.0%
45.0%
40.0%
35.0%
30.0%
25.0%
20.0%
15.0%
10.0%
5.0%
0.0%
5.4%
24.3%
6.3%
3.4%
20.5%
CED
Baton
OC Spray
Multiple/other Weapons
Other than weapon


Suspect medical attention: Post-test only and CED sites only
Medical attention %
100.0%
90.0%
80.0%
70.0%
60.0%
50.0%
40.0%
30.0%
20.0%
10.0%
0.0%
58.0%
62.5%
44.2%
24.1%
55.7%
CED
Baton
OC Spray
Multiple weapons
Other than weapon

Injuries requiring medical attention for the officer: For officer medical attention (see chart below), OC spray (12.6%), batons (12.3%), and other than weapons (8.9%) were associated with the highest levels of officers requiring medical attention. For these analyses, our data suggest that multiple/other weapons and CEDs were associated with significantly lower number of cases where officers required medical attention than the other forms of force (X^2= 56.19, df=4, p<.001).

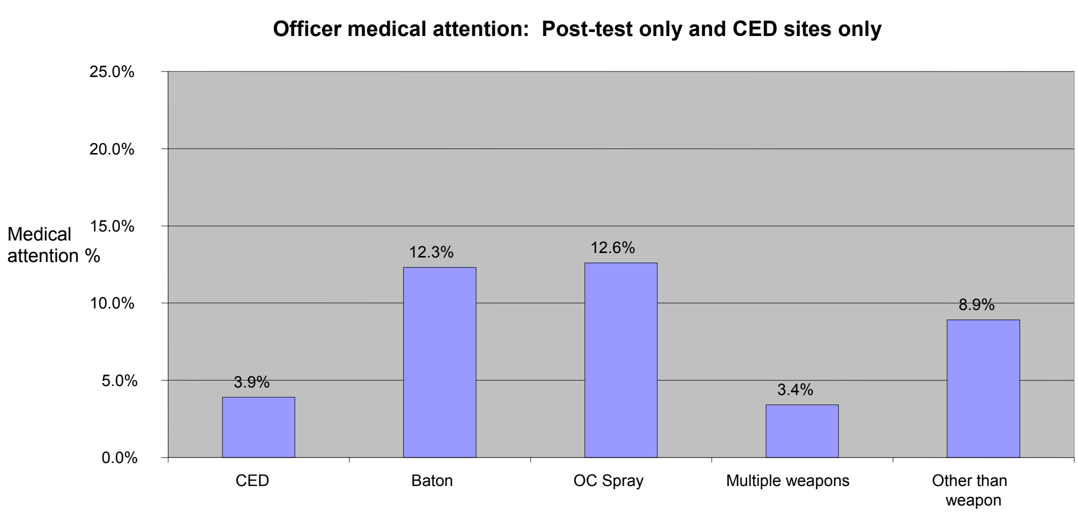
Injuries requiring hospitalization for the suspect: For suspect hospitalization (see chart below), CEDs (29.5%), batons (19.7%), and non-weapon/hands-on tactics (16.7%) were associated with the highest levels of suspects requiring hospitalization. For these analyses, our data suggest that OC spray (11.2%) and multiple/other weapons (12.3%) were associated with significantly lower number of cases where suspects required medical attention than the other forms of force (X^2= 126.77, df=4, p<.001).

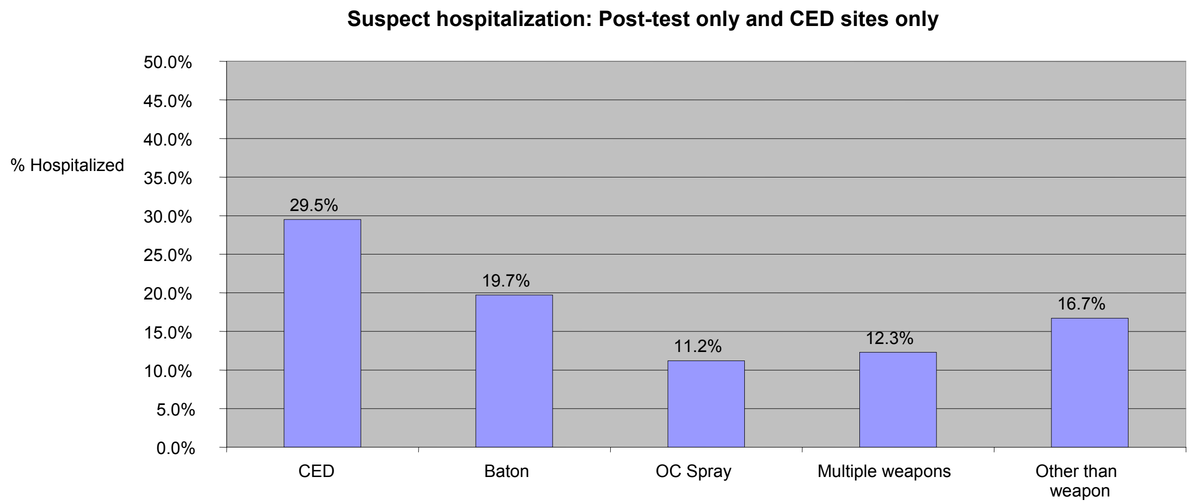
Injuries requiring hospitalization of the officer: For officer hospitalization (see chart below), our data suggest that there were no statistically significant differences across the various forms of use-of-force for officers requiring hospitalization from a force-related injury (X^2= 2.72, df=4, p=.61).

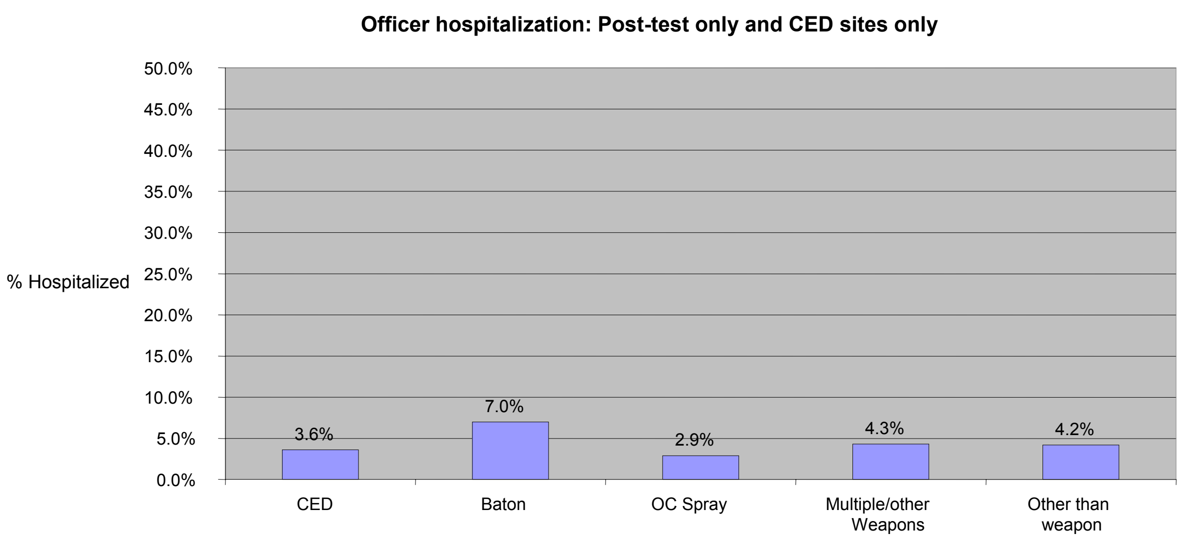
Suspect severe injuries: Our data suggest that for suspect severe injuries (see chart below), CEDs (2%) and OC spray (2.5%) were associated with lower levels of suspect severe injuries than multiple/other weapons (6.3%) and batons (5.9%) (X^2= 9.88, df=4, p<.05).

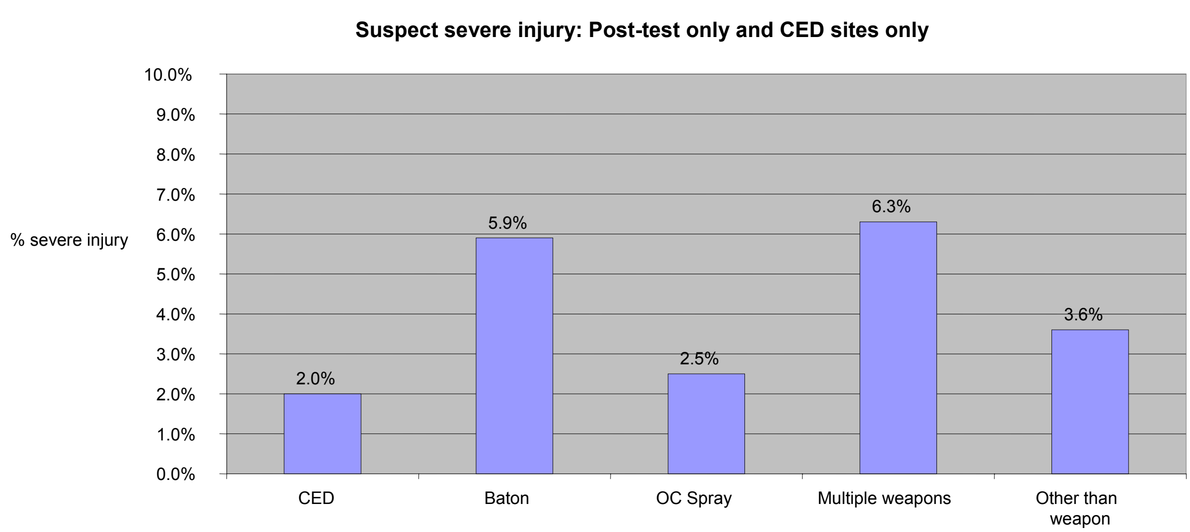
Officer severe injuries: For the officer severe injury variable (see chart below), our data suggest that there were no statistically significant differences across the various forms of use-of-force for officers receiving severe injuries (X^2= 8.49, df=4, p=.08).

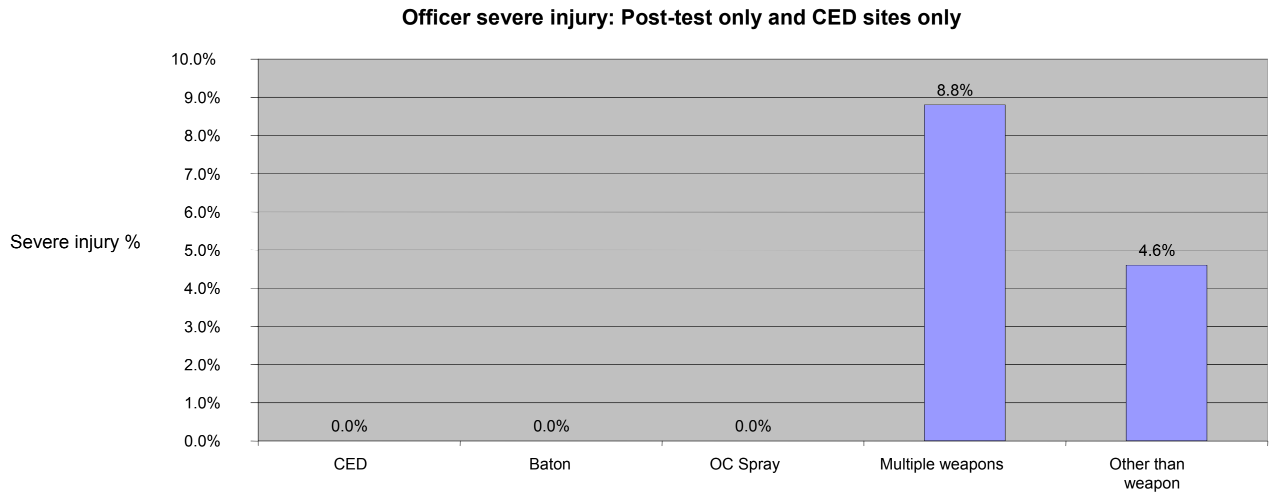
Overall, within our seven CED sites, our data suggest that OC spray was associated with the best outcomes. That is, for six of the eight comparisons, the cases where an officer uses OC spray were associated with the lowest or second lowest rate of injury or medical attention/ hospitalization. For five of the eight comparisons, our data suggest that the cases where an officer uses a CED were associated with the lowest or second lowest rate of injury or medical attention/ hospitalization. For three of the eight comparisons, our data suggest that the cases where an officer uses a baton were associated with the highest rate of injury or medical attention/hospitalization.




Suspect hospitalization: Post-test only and CED sites only
% Hospitalized
50.0%
45.0%
40.0%
35.0%
30.0%
25.0%
20.0%
15.0%
10.0%
5.0%
0.0%
29.5%
19.7%
11.2%
12.3%
16.7%
CED
Baton
OC Spray
Multiple weapons
Other than weapon


Officer hospitalization: Post-test only and CED sites only
% Hospitalized
50.0%
45.0%
40.0%
35.0%
30.0%
25.0%
20.0%
15.0%
10.0%
5.0%
0.0%
3.6%
7.0%
2.9%
4.3%
4.2%
CED
Baton
OC Spray
Multiple/other Weapons
Other than weapon


Suspect severe injury: Post-test only and CED sites only
% severe injury
10.0%
9.0%
8.0%
7.0%
6.0%
5.0%
4.0%
3.0%
2.0%
1.0%
0.0%
2.0%
5.9%
2.5%
6.3%
3.6%
CED
Baton
OC Spray
Multiple weapons
Other than weapon



Logistic regression model for "CED sites only" analyses: Next, we explore the above "CED sites only" results with a multivariate logistic regression model where we introduce variables to control for other factors that might affect the relationship between type of force used and various safety outcomes (see Table 5 below).[17] We only include the following variables[18]: suspect race, suspect gender, suspect age, and a categorical variable for type of force used (officer used a CED and no other type of force, baton use only, OC spray only, no weapon used/hands on tactics used, and a reference group [officer used multiple weapons or a weapon other than CEDs, batons, or OC spray]).[19]

For four of the eight models, our data suggest that OC spray was related to greater safety (see Table 5 below). Our data suggest that when officers use OC spray there is a 90% reduction in the probability of a suspect injury (-2.32, p<.001), a 73% reduction in the probability of an officer injury (-1.29, p<.001), a 35% reduction in the probability of a suspect receiving medical attention for an injury (-0.44, p<.001), and a 45% reduction in the probability of a suspect being hospitalized for an injury (-1.29, p<.001) compared to cases where other weapons or multiple weapons are used. Our data suggest that when officers use CEDs there is a 76% reduction in the probability of

[17] Due to our small sample size of CED only sites (n=7), we were not able to estimate robust variance estimates controlling for the nesting of standard errors nor we were able to estimate HLM models for our CED site only analyses.

[18] We did not include our variable for CED site because we only used CED sites in this analysis. We did not include our variable for time-period because we only used the post-period for this analysis when officers had CEDs or the other weapons available to use.

[19] As pointed out by one reviewer, it would have been preferable to control for different types of force situations (e.g., suspects with weapons or suspects exhibiting violent behavior) since different types of weapons are used for different types of situations officers might face. By excluding variables on the nature of the incident the officer(s) were facing, we run the risk of placing too much emphasis on the weapon the officer used in reducing negative outcomes. Unfortunately, three of the seven CED sites did not collect this information, and attempting to include theses variables in our model would drop our sample size down to four agencies. Despite this concern, we did explore this issue with the data we had for the four CED sites. We ran additional models with two additional control variables (did the suspect use violence against the officers and did the suspect use or threaten to use a weapon against the officers). While these new control variables were statistically significant in some of the models, the findings we reported in Table 5 generally did not change very much (the direction of the parameters did not change nor the magnitude). Given that little changes when we add these variables to our models and the missing data problems which emerge when we attempt to include these variables, we believe it is preferable to examine the data as we did in Table 5.

an officer injury (-1.45, $p<.001$), and a 63% reduction in the probability of an officer requiring medical attention for an injury (-0.99, $p<.001$) compared to cases where other weapons or multiple weapons are used. However, our data suggest that when officers use CEDs there was a 139% increase in the probability of a suspect requiring hospitalization (0.87, $p<.001$) compared to cases where other weapons or multiple weapons are used. This is one of the few negative/adverse findings for CEDs, and may reflect an informal police practice of sending suspects who have been subjected to a CED activation to a hospital as a precautionary measure—for example, to ensure that the skin punctures caused by the CED darts do not become infected. This is a concern requiring more attention in future research (see discussion section for more detail on this finding).

Multivariate analyses using logistic regression at individual/incident-level (CED compared to non-CED sites):

Using logistic regression, we explore differences between CED and non-CED sites on the following outcome measures for officers and suspects: injuries (yes/no), severity of injuries (minor injury or severe injury), injury requiring medical assistance (yes/no), and injury requiring hospitalization (yes/no). For each of these outcome measures we built two basic logistic regression models (called Model 1 and Model 2). Both models are presented in Appendix 1. Model 1 included the following independent/predictor variables: CED (whether the agency deploys CED: 1= yes, 0=no), time frame of incident (post-CED/comparable period= 1, pre CED/comparable period=0), interaction CED * Time Frame (1= CED and post period), and suspect race (White= 1, Non-White=0), suspect gender (male=1, female=0), and suspect age (1= < 25 years old, 0= > 25 years old).

Model 2 included all of the variables from Model 1 plus two additional independent variables: (a) whether the suspect used resistant behavior (1=physical aggression by suspect, 0= non-physical aggression [e.g., verbal attacks, assuming a fighting stance, etc.]) and (b) whether the suspect had a weapon at the force incident (1=yes, 0= no). The introduction of Model 2 allows us to analyze the impact of controlling for these two additional variables, but three of the sites (2 CED sites and 1 non-CED site) did not have any data on these variables. Therefore, Model 2 is based on 10 sites, not 13 sites. While we would have liked to include even more variables in our logistic regression model, we would have lost additional sites from our analyses if we attempted to include other independent variables. For the most part, despite the fact that the suspect resistant behavior and suspect possession of a weapon variables were generally statistically significant, our multivariate results in Model 2 were similar to our earlier results in terms of direction and statistical significance. The fact that our Model 1 results held up, even with the inclusion of two substantively significant variables, provides more confidence in our results. That is, even controlling for additional variables that might affect our outcome measures, the CED sites were still associated with a variety of positive outcomes. In the text below, we focus on presenting the results from Model 1, for the results from Model 2 are very similar both in the direction, magnitude, and statistical significance of the effects.

Table 5: CED only site logistic regression

Logistic Regression for Seven CED Sites Only (Post Period Only)

	SUSPECT INJURY				OFFICER INJURY			
	B	**S.E.**	**Sig.**	**Odds Ratio**	**B**	**S.E.**	**Sig.**	**Odds Ratio**
Suspect race (White= 1, Non-White=0)	0.13	0.08	0.11	1.14	0.02	0.13	0.87	1.02
Suspect gender (Male=1, female=0)	0.39	0.11	0.00	1.48	-0.11	0.16	0.49	0.89
Suspect age (1= < 25 years old, 0= > 25 years old)	0.21	0.07	0.00	1.23	0.14	0.12	0.25	1.15
Type of force used								
(Reference group= multiple weapon/other weapon)			0.00				0.00	
CED use only	0.24	0.11	0.06	1.27	-1.45	0.26	0.00	0.24
Baton use only	-0.21	0.34	0.53	0.81	0.30	0.40	0.45	1.35
OC spray only	-2.32	0.18	0.00	0.10	-1.29	0.21	0.00	0.27
No weapon used (hands on tactics)	-1.47	0.08	0.00	0.23	-2.01	0.14	0.00	0.13
Constant	-0.59	0.12	0.00	0.55	-1.32	0.17	0.00	0.27

	OFFICER MEDICAL ATTENTION				SUSPECT MEDICAL ATTENTION			
Suspect race (White= 1, Non-White=0)	-0.07	0.14	0.65	0.94	0.28	0.08	0.00	1.32
Suspect gender (Male=1, female=0)	0.07	0.20	0.72	1.08	0.36	0.10	0.00	1.43
Suspect age (1= < 25 years old, 0= > 25 years old)	-0.05	0.14	0.72	0.95	-0.14	0.07	0.03	0.87
Type of force used								
(Reference group= multiple weapon/other weapon)			0.00				0.00	
CED use only	-0.99	0.29	0.00	0.37	0.26	0.13	0.05	1.30
Baton use only	0.56	0.43	0.19	1.76	0.16	0.35	0.65	1.17
OC spray only	0.22	0.21	0.29	1.25	-0.44	0.11	0.00	0.65
No weapon used (hands on tactics)	-1.18	0.22	0.00	0.31	-1.12	0.08	0.00	0.33
Constant	-2.04	0.21	0.00	0.13	-0.18	0.11	0.11	0.84

	OFFICER HOSPITALIZATION				SUSPECT HOSPITALIZATION			
Suspect race (White= 1, Non-White=0)	-0.18	0.20	0.36	0.83	0.18	0.09	0.04	1.20
Suspect gender (Male=1, female=0)	-0.10	0.26	0.71	0.91	0.16	0.12	0.17	1.18
Suspect age (1= < 25 years old, 0= > 25 years old)	-0.02	0.18	0.92	0.98	-0.14	0.08	0.08	0.87
Type of force used								
(Reference group= multiple weapon/other weapon)			0.67				0.00	
CED use only	0.09	0.28	0.76	1.09	0.87	0.12	0.00	2.39
Baton use only	0.49	0.62	0.42	1.63	0.10	0.39	0.80	1.10
OC spray only	-0.38	0.38	0.31	0.68	-0.60	0.16	0.00	0.55
No weapon used (hands on tactics)	-0.14	0.22	0.51	0.87	-0.35	0.09	0.00	0.71
Constant	-2.74	0.27	0.00	0.06	-1.52	0.13	0.00	0.22

	SUSPECT SEVERE INJURIES				OFFICER SEVERE INJURIES			
Suspect race (White= 1, Non-White=0)	0.70	0.29	0.02	2.02	-1.38	0.77	0.07	0.25
Suspect gender (Male=1, female=0)	-0.27	0.40	0.49	0.76	0.57	0.77	0.46	1.77
Suspect age (1= < 25 years old, 0= > 25 years old)	-0.19	0.29	0.50	0.82	0.05	0.49	0.92	1.05
Type of force used								
(Reference group= multiple weapon/other weapon)			0.07				0.98	
CED use only	-0.34	0.52	0.51	0.71	-18.55	9677.64	1.00	0.00
Baton use only	0.59	1.07	0.58	1.80	-18.54	13307.02	1.00	0.00
OC spray only	-0.18	1.04	0.87	0.84	-18.26	7324.43	1.00	0.00
No weapon used (hands on tactics)	0.73	0.31	0.02	2.07	0.31	0.50	0.53	1.37
Constant	-3.40	0.46	0.00	0.03	-3.14	0.78	0.00	0.04

Each of our outcome measures includes the entire time frame (both the pre-CED/comparable period and post-CED/comparable period). To assess our outcome measures during the relevant post-period for CED versus non-CED sites, we introduce our CED variable and a variable representing the time frame of each use-of-force incident (pre or post) to form an interaction term. The main purpose of our logistic regression model is to attempt to isolate the effects of CED deployment on our safety-related outcomes after the implementation of CEDs, controlling for other factors that might affect levels of the various outcomes. The variable of main interest in our logistic regression models is the interaction variable of agency deployment of CED multiplied by time frame. A positive value on this interaction term would indicate that an agency that deploys the CED is associated with *more* injuries in the post-period than agencies without CEDs, controlling for other factors. A negative value on this interaction term would indicate that an agency that deploys the CED is associated with *fewer* injuries in the post-period than agencies without CEDs, controlling for other factors.

Any injury: For our suspect injury model, our results indicate that for an agency that deploys CEDs, the odds of a suspect being injured in the post-period is reduced by 44% relative to agencies without CEDs (β=-0.57, odds ratio= 0.56, $p<.0001$). To understand the nature of our effects we estimated prediction profiles for each of our logistic regression models. Our data suggest that for an average non-white male under the age of 25 years old (a group with a higher-than-average likelihood of being subjected to a CED activation), the predicted probability of an injury at the post-period for a non-CED agency equals 50% compared to a lower probability of only 28% for CED agencies. As we observed with most of our other Model 2's, our suspect resistant behavior (β= 0.37, odds ratio= 1.45) and suspect weapon (β= 0.86, odds ratio= 2.35) variables were statistically significant (suspects who used physical aggression against officers were 55% more likely to be injured than suspects who did not, and suspects who had weapons were 135% more likely to be injured than suspects who did not have weapons).

For our officer injury model, our results suggest that for an agency that deploys CEDs, the odds of an officer being injured in the post-period is reduced by 70% relative to agencies without CEDs (β=-1.20, odds ratio= 0.30, $p<.0001$). Our data suggest that for an average non-white male under the age of 25 years old, the predicted probability of an injury at the post-period for a non-CED agency equals 25% compared to a lower probability of only 9% for CED agencies.

Medical attention for injuries: For our suspect medical attention model, our results indicate that for an agency that deploys CEDs, the odds of a suspect needing medical attention for an injury in the post-period is reduced by 79% relative to agencies without CEDs (β =-1.54, odds ratio= 0.22, $p<.0001$). Our data suggest that for an average non-white male under the age of 25 years old, the predicted probability of a suspect needing medical attention for an injury at the post-period for a non-CED agency equals 52% compared to a lower probability of only 35% for CED agencies.

For our officer medical attention model, our results indicate that for an agency that deploys CEDs, the odds of a suspect needing medical attention for an injury in the post-period is reduced by 87% relative to agencies without CEDs (β=-2.04, odds ratio= 0.13, $p<.0001$). Our data suggest that for an average non-white male under the age of 25 years old, the predicted probability of an officer needing medical attention for an injury at the post-period for a non-CED agency equals 19% compared to a lower probability of only 9% for CED agencies.

Hospitalization required for injuries: For our suspect hospitalization model, our results indicate that for an agency that deploys CEDs, the odds of a suspect requiring hospitalization for an injury in the post-period is reduced by 52% relative to agencies without CEDs (β= -0.73, odds ratio= 0.48, p<.0001). Our data suggest that for an average non-white male under the age of 25 years old, the predicted probability of a suspect requiring hospitalization for an injury at the post-period for a non-CED agency equals 33% compared to a lower probability of only 16% for CED agencies.

For our officer hospitalization model, our results indicate that there were no differences for agencies that deploys CEDs and agencies that do not deploy CEDs in terms of the odds of an officer requiring hospitalization for an injury in the post-period (β= -0.23, odds ratio= 0.79, p=0.54). Our data suggest that for an average non-white male under the age of 25 years old (again, the group with a higher-than-average likelihood of being subjected to a CED activation), the predicted probability of an officer requiring hospitalization for an injury at the post-period for a non-CED agency equals 6.2% compared to a similar probability of only 5.2% for CED agencies.

Severity of injury (Minor vs. Severe): For our suspect injury severity model, our results indicate that there were no differences for agencies that deploys CEDs and agencies that do not deploy CEDs in terms of the odds of a suspect receiving a severe injury in the post-period (β=-0.58, odds ratio= 0.56, p=0.12). Our data suggest that for an average non-white male under the age of 25 years old, the predicted probability of a suspect receiving a severe injury at the post-period for a non-CED agency equals 7% compared to a similar probability of only 3.6% for CED agencies.

For our officer injury severity model, our results indicate that there were no differences for agencies that deploys CEDs and agencies that do not deploy CEDs in terms of the odds of an officer receiving a severe injury in the post period (β= 0.56, odds ratio= 1.75, p=0.42). Our data suggest that for an average non-white male under the age of 25 years old, the predicted probability of an officer receiving a severe injury at the post-period for a non-CED agency equals 5.2% compared to a similar probability of only 4.9% for CED agencies.

Multivariate analyses using logistic regression adjusting for nested standard errors (CED compared to non-CED sites):

As discussed earlier, one of the concerns with examining multi-site data is that the individual use-of-force cases we analyze are clustered within 13 departments. While we ran an HLM model next to address this clustering issue, before that we examined the results of using a logistic regression with a robust variance estimate to adjust for within-cluster correlation. We conducted these analyses using Stata statistical software with the vce (cluster *clustvar*) option. The robust variance estimator comes under various names in the literature, but within the Stata software it is known as the Huber/White/sandwich estimate of variance. The names Huber and White refer to the seminal references for this estimator (Huber, 1967; White, 1980). The main limitation with using this approach is that we do not get aggregate-level coefficients like those produced using HLM, but we are still able to address the clustered nature of our data and produce unbiased estimates (Rogers, 1993; Williams, 2000; Wooldridge, 2002). We use this approach to examine more closely the viability of our earlier logistic regression results, before examining our full multi-level HLM results. Also, given that we are attempting to assess the potential bias introduced by using nested data across multiple sites, we only examine our Model 1 results with variables of CEDs, time period, time period multiplied by CEDs, suspect race, suspect gender, and suspect age (see Appendix 2). We did not estimate another set of Model 2 results, for this involves dropping three sites from our analysis, which would weaken our tests of this nesting issue.

The full results of our logistic regression with robust variance estimates are presented in Appendix 2. Below (see Table 6 and 7) we present just our main variable of interest (our interaction variable for presence of CED at the post-period) for each of the eight outcome measures.[20] As seen below (see Table 6 and 7), three of the five statistically significant results from the earlier models remained significant under our logistic regression with robust variance estimates (including the variables of officer injury, suspect medical attention, and officer medical attention). In all three cases, as reported earlier, CED agencies were associated with lower post-test rates of officer injuries, suspects requiring medical attention for injuries, and officers requiring medical attention. Two of the five results rose above the .05 level of statistical significance, but remained in the predicted direction (that is, CED sites were still associated with fewer post-test suspect injuries and fewer suspects requiring hospitalization from an injury than non-CED sites, but the result was no longer statistically significant). However, one of the outcomes not previously statistically significant became statistically significant under the new regression model with robust variance estimates (that is, CED sites under this new model are associated with fewer post-test severe injuries for suspects than non-CED sites).

Multi-level analyses using Hierarchical Linear Modeling (HLM):

As discussed earlier, one of the concerns with examining multi-site force data is that the individual use-of-force cases we analyze are clustered within 13 departments, violating the independence assumption of traditional regression approaches. Our final approach is to use HLM modeling to examine more closely the viability of our earlier bivariate results and various logistic regression results. To follow, we compare our HLM models to our first set of logistic regression models and our second set of logistic regression models (which included a correction for nested standard errors). Each of the three types of statistical models includes a similar set of covariates to control for suspect age, gender, race, time period (post period after CEDs were implemented), and type of agency (CED sites or non-CED site).[21] We also included two additional aggregate-level variables in our HLM model: (1) the number of officers in the LEA per 100,000 in the population in the

[20] As discussed earlier, a positive value on this interaction term would indicate that an agency that deploys CED is associated with *more* injuries in the post-period than agencies without CEDs, controlling for other factors. A negative value on this interaction term would indicate that an agency that deploys CED is associated with *fewer* injuries in the post-period than agencies without CEDs, controlling for other factors.

[21] All three models included the following independent/predictor variables: CED (whether the agency deploys CED: 1= yes, 0=no), time frame of incident (post-CED/comparable period= 1, pre-CED/comparable period=0), suspect race (white= 1, non-white=0), suspect gender (male=1, female=0), and suspect age (1= < 25 years old, 0= > 25 years old).

Table 6: Comparison of two types of logistic regression models for injury and medical attention outcomes

Outcome measures/dependent variables

Variables in logistic regression model	Suspect Injury							
	Without standard error correction				With standard error correction			
	β	Odds Ratio	SE	P value	β	Odds Ratio	SE	P value
Interaction CED * Time Frame	-0.57	0.56	0.14	<.0001	-0.57	0.57	0.55	0.30

	Officer Injury							
	Without standard error correction				With standard error correction			
	β	Odds Ratio	SE	P value	β	Odds Ratio	SE	P value
Interaction CED * Time Frame	-1.20	0.30	0.20	<.0001	-1.20	0.30	0.51	0.019

	Suspect Medical Attention							
	Without standard error correction				With standard error correction			
	β	Odds Ratio	SE	P value	β	Odds Ratio	SE	P value
Interaction CED * Time Frame	-1.54	0.215	0.147	<.0001	-1.54	0.21	0.68	0.02

	Officer Medical Attention							
	Without standard error correction				With standard error correction			
	β	Odds Ratio	SE	P value	β	Odds Ratio	SE	P value
Interaction CED * Time Frame	-2.04	0.131	0.298	<.0001	-2.04	0.13	0.96	0.03

Table 7: Comparison of two types of logistic regression models for hospitalization severe injury outcomes

Outcome measures/dependent variables

Variables in logistic regression model	Suspect Hospitalization							
	Without standard error correction				With standard error correction			
	β	Odds Ratio	SE	P value	β	Odds Ratio	SE	P value
Interaction CED * Time Frame	-0.73	0.48	0.19	<.0001	-0.73	0.48	0.70	0.29
	Officer Hospitalization							
	Without standard error correction				With standard error correction			
	β	Odds Ratio	SE	P value	β	Odds Ratio	SE	P value
Interaction CED * Time Frame	-0.23	0.80	0.38	0.5439	-0.23	0.79	0.42	0.59
	Suspect Injury Minor vs. Severe							
	Without standard error correction				With standard error correction			
	β	Odds Ratio	Std Error	P value	β	Odds Ratio	Std Error	P value
Interaction CED * Time Frame	-0.58	0.56	0.37	0.118	-0.58	0.56	0.26	0.02
	Officer Injury Minor vs. Severe							
	Without standard error correction				With standard error correction			
	β	Odds Ratio	Std Error	P value	β	Odds Ratio	Std Error	P value
Interaction CED * Time Frame	0.56	1.75	0.69	0.4166	0.56	1.75	0.48	0.24

jurisdiction and (2) the population density per square mile (this aggregate level variable could only be added into the HLM model, for the HLM is the only multi-level technique of the three approaches). We present our full HLM results in Appendix 3.

While we considered estimating HLM models with other additional independent variables (whether the suspect used resistant behavior and whether the suspect had a weapon at the force incident), we are not in a good position to calculate such a set of models. The introduction of these two other variables would allow us to analyze the impact of controlling for these two additional incident-level factors, but three of the sites (2 CED sites and 1 non-CED site) did not have any data on these variables. Therefore, our sample size of aggregate level cases would drop to only 10 sites and further reduce the statistical power of our HLM analyses. While not presented, we did calculate HLM models with these additional variables, and the main results related to comparing CED sites to non-CED sites did not change from that presented below. We also examined other aggregate level variables in various combinations in our HLM model (e.g., measures of arrests and crimes in the jurisdiction, and various demographic measures of the jurisdiction) and achieved the same basic substantive finding. We included the two measures of population density and number of officers per 100,000, for we believe they serve as our most relevant area-level control variables.

Prior to running our HLM testing, we examined a variety of diagnostic plots and checked our data in terms of outliers, normality, linearity, and followed Tabachnick and Fidell's (2007) discussion of checking HLM assumptions (function forms are linear at each level, the model is specified correctly, the error term is not correlated with the independent variables, level-1 residuals are normally distributed and level-2 random effects have a multivariate normal distribution, level-1 residual variance is constant, level-1 residuals and level-2 residuals are uncorrelated, and observations at highest level are independent of each other). Our models generally met these underlying assumptions. While there are no agreed-upon exact standards on values for each of the tests associated with these assumptions (e.g., see Mass & Hox, 2002; Raudenbush & Bryk, 2002; Raudenbush & Willms, 1995), there is some evidence that HLM is fairly robust for modest violations of its assumptions (Delpish, 2006; Goldberger, 1991).

Officer injury: All three models (our logistic regression model, our logistic regression model with a correction for nested standard errors, and our HLM models) indicate that agencies that have deployed CEDs are associated with fewer injuries to officers compared to non-CED agencies. Our data suggest that the magnitude of the effect of CED agencies is similar across the three models (odds ratios .30 and .23) and in the same direction (negative coefficients). Our results indicate that for an agency that deploys CEDs, the odds of an officer being injured in the post-period are reduced by either 70% (logistic regression odds ratio= 0.30) or 77% (HLM odds ratio= 0.23) relative to agencies without CEDs. While we reached statistical significance with our first logistic regression model (where the analyses were solely based on the individual-level [p<.0001]) and our second logistic regression model [p=.019], we did not reach statistical significance for our HLM model (p=.38). As discussed earlier, due to our small sample size (n= 13 agencies) we did not expect to find statistical significance for our HLM findings.[22] Our main interest in running the

[22]To assess model fit we conducted a joint test that all coefficients are zero, and compared that to our

HLMs was to confirm the direction of our findings (i.e., whether CEDs were associated with an increase or a decrease in our safety-related outcome measures), and the approximate magnitude of the effect. For officer injuries, our HLM results have confirmed our earlier logistic regression findings.

Suspect injury: All three models indicate that agencies that have deployed CEDs are associated with fewer injuries to suspects compared to non-CED agencies. Our data suggest that the magnitude of the effect of CED agencies is similar across the three models (odds ratios .56 and .53) and in the same direction (negative coefficients). Our results indicate that for an agency that deploys CEDs, the odds of a suspect being injured in the post-period are reduced by either 43% (logistic regression odds ratio= 0.56) or 47% (HLM odds ratio[23]= 0.53) relative to agencies without CEDs. While we reached statistical significance with our first logistic regression model (where the analyses were solely based on the individual-level), we did not reach statistical significance for either of the other two models.

Officer injury requiring medical attention: All three models indicate that agencies that have deployed CEDs are associated with fewer cases of officers receiving medical attention for injuries related to use-of-force compared to non-CED agencies. Our data suggest that the magnitude of the effect of CED agencies is similar across the three models (.13 and.18) and in the same direction (negative coefficients). Our results indicate that for an agency that deploys CEDs, the odds of an officer receiving medical attention in the post-period are reduced by either 87% (logistic regression odds ratio= 0.13) or 82% (HLM[24] odds ratio= 0.18) relative to agencies without CEDs. While we reached statistical significance with both of our logistic regression models, we did not reach statistical significance for our HLM model.

Suspect injury requiring medical attention: All three models indicate that agencies that have deployed CEDs are associated with fewer cases of suspects receiving medical attention for injuries related to use of force compared to non-CED agencies. Our data suggest that the magnitude of the effect of CED agencies is similar across the three models (.21 and .54) and in the same direction (negative coefficients). Our results indicate that for an agency that deploys CEDs, the odds of a suspect receiving medical attention in the post-period are reduced by either 79% (logistic regression odds ratio= 0.21) or 46% (HLM[25] odds ratio= 0.54) relative to agencies without CEDs. While we reached statistical significance with both of our logistic regression models, we did not reach statistical significance for our HLM model.

specified HLM model outlined in Appendix 3. The result of our comparison is distributed as a Chi-Square distribution with degrees of freedom equal to the number of constrained coefficients. For our officer injury model test, that all the coefficients are zero, our result was statistically significant (X^2= 139.01 [DF = 20], p<.001), providing evidence that our model is a better fit than a fully constrained model.

[23] For our suspect injury model fit test, that all the coefficients are zero, our result was statistically significant (X^2= 40.12 [DF = 20], p<.01), providing evidence that our model is a better fit than a fully constrained model.

[24] For our officer medical attention model fit test, that all the coefficients are zero, our result was statistically significant (X^2= 110.22 [DF = 20], p<.001), providing evidence that our model is a better fit than a fully constrained model.

[25] For our suspect medical attention model fit test, that all the coefficients are zero, our result was statistically significant (X^2= 114.72 [DF = 20], p<.001), providing evidence that our model is a better fit than a fully constrained model.

Table 8: Comparison of three multivariate models for injury and medical attention outcomes

Outcome measures/dependent variables

Variables in Logistic/HLM model	Officer Injury											
	Logistic w/out standard error correction				Logistic w/ standard error correction				HLM			
	β	Odds Ratio	SE	P value	β	Odds Ratio	SE	P value	β	Odds Ratio	SE	P value
Interaction CED * Time Frame	-1.20	0.30	0.20	<.0001	-1.20	0.30	0.51	0.019	-1.49	0.23	1.59	0.379

	Suspect Injury											
	Logistic w/out standard error correction				Logistic w/ standard error correction				HLM			
	β	Odds Ratio	SE	P value	β	Odds Ratio	SE	P value	β	Odds Ratio	SE	P value
Interaction CED * Time Frame	-0.57	0.56	0.14	<.0001	-0.57	0.56	0.55	0.30	-0.64	0.53	1.63	0.71

	Officer Medicalization											
	Logistic w/out standard error correction				Logistic w/ standard error correction				HLM			
	β	Odds Ratio	SE	P value	β	Odds Ratio	SE	P value	β	Odds Ratio	SE	P value
Interaction CED * Time Frame	-2.04	0.13	0.298	<.0001	-2.04	0.13	0.96	0.03	-1.74	0.18	2.84	0.56

	Suspect Medicalization											
	Logistic w/out standard error correction				Logistic w/ standard error correction				HLM			
	β	Odds Ratio	SE	P value	β	Odds Ratio	SE	P value	β	Odds Ratio	SE	P value
Interaction CED * Time Frame	-1.54	0.21	0.147	<.0001	-1.54	0.21	0.68	0.02	-0.61	0.54	1.27	0.65

Suspect injury requiring hospitalization: All three models indicate that agencies that have deployed CEDs are associated with fewer cases of suspects having to be hospitalized for injuries related to use of force compared to non-CED agencies. Our data suggest that the magnitude of the effect of CED agencies is similar across the three models (.48 and .89) and in the same direction (negative coefficients). Our results indicate that for an agency that deploys CEDs, the odds of a suspect having to be hospitalized for injuries related to use of force in the post-period are reduced by either 52% (logistic regression odds ratio= 0.48) or 11% (HLM odds ratio[26]= 0.89) relative to agencies without CEDs. While we reached statistical significance with one of our logistic regression models (p<.0001), we did not reach statistical significance for our HLM model (p=.75).

Officer injury requiring hospitalization: All three models failed to reach statistical significance for the officer hospitalization. However, the logistic regression models and HLM model[27] had negative coefficients (suggesting that agencies that have deployed CEDs are associated with fewer cases of officers having to be hospitalized for injuries). On balance, our data suggest that the CED agencies do not seem to differ from the non-CED agencies in altering the number of officers requiring hospitalization for an injury during a force incident.

Suspect injury severity: All three models indicate that agencies that have deployed CEDs are associated with fewer severe injuries to suspects compared to non-CED agencies. Our data suggest that the magnitude of the effect of CED agencies is similar across the three models (.56 and .36) and in the same direction (negative coefficients). Our results indicate that for an agency that deploys CEDs, the odds of a suspect being severely injured in the post-period are reduced by either 44% (logistic regression odds ratio= 0.56) or 64% (HLM[28] odds ratio= 0.36) relative to agencies without CEDs. While we did not reach statistical significance with our first logistic regression model (where the analyses were solely based on the individual-level), we did reach statistical significance for our logistic regression model with a correction for nested standard errors (p<.02) and reached statistical significance for the HLM model (p=.05). Overall, the evidence suggests that agencies that have deployed CEDs are associated with fewer severe injuries to suspects compared to non-CED agencies.

Officer injury severity: All three models failed to reach statistical significance for the officer injury severity outcome measure. While all three models had positive coefficients (suggesting that agencies that have deployed CEDs are associated with more cases of officers receiving severe injuries) they were not even close to statistical significance for our logistic regressions and HLM[29] models (p=.42, p=.24, and p=.23). On balance, our data suggest that the CED agencies do not seem to differ from the non-CED agencies in altering the number of officers receiving severe injuries during force cases.

[26] For our suspect hospitalization model fit test, that all the coefficients are zero, our result was statistically significant (X^2= 120.81[DF = 20], p<.001), providing evidence that our model is a better fit than a fully constrained model.

[27] For our officer hospitalization model fit test, that all the coefficients are zero, our result was statistically significant (X^2= 922.37 [DF = 20], p<.001), providing evidence that our model is a better fit than a fully constrained model.

[28] For our suspect injury severity model fit test, that all the coefficients are zero, our result was statistically significant (X^2= 931.52 [DF = 20], p<.001), providing evidence that our model is a better fit than a fully constrained model.

[29] For our officer injury severity model fit test, that all the coefficients are zero, our result was statistically significant (X^2= 441.85 [DF = 20], p<.001), providing evidence that our model is a better fit than a fully constrained model.

Table 9: Comparison of three multivariate models for hospitalization and severe injury outcomes

Outcome measures/dependent variables

Variables in Logistic/HLM model	Suspect Hospitalization											
	Logistic w/out standard error correction				Logistic w/ standard error correction				HLM			
	β	Odds Ratio	SE	P value	β	Odds Ratio	SE	P value	β	Odds Ratio	SE	P value
Interaction CED * Time Frame	-0.73	0.48	0.19	<.0001	-0.73	0.48	0.70	0.29	-0.12	0.89	0.36	0.75

	Officer Hospitalization											
	Logistic w/out standard error correction				Logistic w/ standard error correction				HLM			
	β	Odds Ratio	SE	P value	β	Odds Ratio	SE	P value	β	Odds Ratio	SE	P value
Interaction CED * Time Frame	-0.23	0.80	0.38	0.5439	-0.23	0.80	0.42	0.59	-0.06	0.94	0.46	0.91

	Suspect Injury Minor vs. Severe											
	Logistic w/out standard error correction				Logistic w/ standard error correction				HLM			
	β	Odds Ratio	Std Err	P value	β	Odds Ratio	Std Err	P value	β	Odds Ratio	SE	P value
Interaction CED * Time Frame	-0.58	0.56	0.37	0.118	-0.58	0.56	0.26	0.02	-1.02	0.36	0.44	0.05

	Officer Injury Minor vs. Severe											
	Logistic w/out standard error correction				Logistic w/ standard error correction				HLM			
	β	Odds Ratio	Std Err	P value	β	Odds Ratio	Std Err	P value	β	Odds Ratio	SE	P value
Interaction CED * Time Frame	0.56	1.75	0.69	0.4166	0.56	1.75	0.48	0.24	1.07	2.92	0.82	0.23

CHAPTER 5
Discussion and Conclusion

The manner in which a policing agency manages its use of force, including the types of force it uses, technologies to deliver that force, and when various types of force can be used, are among the most important decisions that a LEA executive will have to make. One of the key objectives in managing force is designing approaches to reduce incidents of police use of force and the injuries associated with force. One weapon that has been advanced as a way to reduce injuries for officers and suspects is the Conducted Energy Device (CED). Police chiefs and sheriffs charged with making the decision whether to use CEDs or other less-lethal weapons need guidance about whether the weapons are in fact effective. Law enforcement executives have been deluged with questions about the effectiveness and safety of CEDs, and the lack of available information and a full understanding about the effects of using CEDs has hampered the ability of police executives to make informed policy decisions about the devices. Police executives have been provided with little independent scientific evidence and guidance on the impact of using CEDs, forcing them to make policy and operational decisions without being fully informed. While decades of research have documented the nature and extent of other types of force used by police and the conditions and correlates that affect the application of force (Smith et al., 2007), little research has been done isolating the effects of using CEDs on injuries to suspects and officers. The purpose of this project was to produce scientifically valid results that will inform LEA executives' decisions regarding the use of CEDs.

Our study is one of the first to compare LEAs that use CEDs to matched LEAs that do not use CEDs. The problem with evaluating data solely from CED agencies is that the inferences that can be made about the results are limited by the usual problems with pre/post designs and their inability to rule out rival explanations for any impacts of the intervention, in this case, the deployment of CEDs. That is, it is hard to control for alternative factors that could explain changes from the pre-test period to the post-period in those types of designs.[30] We completed an objective analysis of the effects that department-wide deployments of CEDs by LEAs have on injuries and deaths to police and suspects, associated medical attention, and need for hospitalization. The goal of our study was to produce practical information that can help law enforcement executives make good decisions about whether to deploy CEDs, and if a decision is made to deploy them, to help the agencies develop CED policy and procedural guidelines that provide increased safety for officers and citizens. In order to accomplish this goal, we examined the outcome of CED deployments in terms of

[30] While we also conduct a set of within CED site analyses, we are very cautious in our interpretation of these data and we rely more on our CED site to non-CED site comparisons.

officer and suspect safety. We then compared the differences in outcomes between police agencies that have incorporated the use of CEDs (n=7) to those found in police agencies that have not incorporated the use of CEDs (n=6). This study contains important scientific information isolating the safety outcomes to be expected if a department deploys CEDs, controlling for a variety of related organizational and individual/incident-level factors.

The first major methodological challenge in conducting our study was finding a set of comparison LEAs that have used CEDs and a matched group that did not use CEDs. Our selection of cities was based on a matching analysis using a PERF nationally representative survey on use of force. Overall, we believe our CED and non-CED sites are comparable. We collected data from roughly comparable periods (within a year or two) for the CED and non-CED sites. The main difference between the non-CED and CED sites is the participation of one CED site that is much larger than the other sites in our study. However, when we estimated all of our models with and without this large site, we found no major differences in our results. With this site excluded from our analyses, there are no major aggregate-level demographic differences between the CED and non-CED sites across a range of variables including: population size, size of agency, number of arrests for violent offenses, number of violent crimes, and number of homicides. The non-CED and CED sites were also similar on a full range of background aggregate-level factors measured through the 2000 U.S. Census (even with the especially large site included in the analysis). Overall, while some differences emerged in our assessment of the comparability of our CED and non-CED sites, most of the differences were relatively small and did not seem to introduce any substantively important biases. When combined with our multivariate analyses, we believe that we have a reasonably comparable group of CED and non-CED sites with results that are interpretable.

Another important point to recall is that all of the LEA sites with CEDs in our sample have had fairly limited experience with using the CED. None of the CED sites started using the CED weapon in the 20th century.[31] Therefore, any conclusions that we draw from our research reflect the early experience with CEDs. Over time, it seems reasonable to expect that LEAs will gain important insights into the use of CEDs and will be able to further improve safety outcomes associated with this weapon.

In the remainder of this section, we review our results and summarize the main findings. Following our review of our results, we discuss the implications of our results for LEA policy and training, and provide some recommendations for future research.

Review of results:

Earlier we presented a variety of analyses comparing CED and non-CED sites, including bivariate analyses to describe the basic raw differences between the CED and non-CED sites on our outcome measures, and a variety of multivariate analyses to attempt to assess the viability of the bivariate results and control for possible alternative explanations that might explain the earlier raw differences. Our first multivariate analyses were done using logistic regression to isolate the effects of CED deployment on our safety-related outcomes, where we included the following independent/control variables: whether the agency deploys CED, the time frame of the

[31] Only two sites started using the CED weapon in the early 2000s.

incident, an interaction of CED multiplied by time frame, suspect race, suspect gender, and suspect age.[32]

To examine the clustering issue described earlier, we used two approaches. First, we conducted a modified logistic regression with a robust variance estimator to adjust for within-cluster correlation. However, with this approach we do not get aggregate-level coefficients to see the exact effects of aggregate level conditions on our individual results. To examine and observe the effects of aggregate-level factors, we conducted a multi-level analysis using Hierarchical Linear Modeling (HLM).[33] While we recognize our limited statistical power to conduct HLM analyses (n=13 LEAs), we are mainly using HLM to assess the robustness of our findings from our earlier analyses. We focus our analyses of the HLM results on the direction and magnitude of the effects (as opposed to a focus on the statistical significance of the results).

Below we review our bivariate and multivariate results for each of our main outcome measures for the CED versus non-CED site comparisons: officer and suspect injuries, officer and suspect severe injuries, officer and suspect injuries requiring medical attention, officer and suspect injuries requiring hospitalization, and suspect deaths. For these outcomes, we review our results comparing CEDs to non-CED sites, followed by our results for CED sites only. For the analyses of only the CED sites, we review both raw bivariate results and multivariate logistic regression models for the period after CEDs have been deployed (comparing the actual use of CEDs by officers to other forms of use of force).

Officer injuries: Our results across all of our analyses suggest a strong effect of CEDs on reducing officer injuries. Our first set of raw bivariate results compared differences between CED and non-CED sites on the proportion of use-of-force cases where an officer was injured before CEDs were implemented and after CEDs were implemented in CED sites, and during a similar reference period for the non-CED sites. Before the CED sites deployed CEDs, the proportion of officers injured in force cases (12%) was similar compared to non-CED sites (10%) over the same reference period. The CED sites then went on to observe a reduction in officer injuries (to a level of 8%) after they began their deployment of CEDs, compared to the non-CED sites that observed an increase in officer injuries for the non-CED sites to 20%.

For our logistic regression officer injury model, we found that for an agency that deploys CEDs, the odds of an officer being injured in the post-period is reduced relative to agencies without CEDs ($p<.0001$). Our results held up when we estimated our logistic regression with robust variance estimates (the results were still statistically significant $p<.02$). While we did not reach statistical significance for our HLM model ($p=.38$), the effects were in the same direction and of a similar magnitude.[34]

[32] We also assessed two additional independent variables to our logistic regression model: (a) whether the suspect used resistant behavior and (b) whether the suspect had a weapon at the force incident. For the most part, despite the fact that the suspect resistant behavior and suspect possession of a weapon variables were generally statistically significant, our multivariate results were similar to our earlier univariate results in terms of direction and statistical significance. The fact that our Model 1 results held up, even with the inclusion of two substantively significant variables, provides more confidence in our results. That is, even controlling for additional variables that might affect our outcome measures, the CED sites were still associated with a variety of positive outcomes.

[33] We included two additional aggregate-level variables in our HLM model: (1) the number of officers in the LEA per 100,000 in the population in the jurisdiction and (2) the population density per square mile.

[34] As discussed earlier, due to our small sample size (n= 13 agencies) we did not expect to find statistical significance for our HLM findings. Our

All three multivariate models (our logistic regression model, our logistic regression model with robust variance estimates, and our HLM models) indicate that agencies that have deployed CEDs are associated with fewer injuries to officers compared to non-CED agencies. The magnitude of the effect of CED agencies is similar across the three models (.30 and .23) and in the same direction (negative coefficients). Our results indicate that for an agency that deploys CEDs, the odds of an officer being injured in the post-period is reduced by over 70% relative to agencies without CEDs.

Our final set of analyses on officer injuries focused on just the participating CED sites. Batons (24%) and non-weapon/hands-on tactics were associated with the highest levels of officer injuries (45%), followed by OC spray (6%), CEDs (5%), and multiple weapons (or weapons other than CEDs, batons, or OC spray) (3%). For these analyses, multiple/other weapons, CEDs, and OC spray and were associated with significantly lower officer injuries than the other forms of force ($p<.001$). Based on our logistic regression model, when officers use CEDs, there is a 76% reduction in the probability of an officer injury compared to cases where other weapons or multiple weapons are used.

Suspect injuries: Our results, across all of our analyses, demonstrate that CEDs are related to reductions in suspect injuries. Before the CED sites deployed CEDs, 23% of the suspects were injured in force cases, compared to a slightly higher proportion of suspects in the non-CED sites (30%) over the same reference period, representing a small statistical difference ($p<.001$). The CED sites observed a small increase in suspect injuries (to 26%) after they began their deployment of CEDs, compared to the non-CED sites that observed a much larger increase in suspect injuries for the non-CED sites to 43% ($p<.001$). While the CEDs started out at a slightly lower rate of suspect injuries compared to the non-CED sites (23% vs. 30%), the CED sites were substantially lower at the post period (26% vs. 43%), at a rate much greater than the initial differences would predict.

For our logistic regression model, our results indicate that for an agency that deploys CEDs, the odds of a suspect being injured in the post-period is reduced relative to agencies without CEDs ($p<.0001$). Our logistic regression with robust variance estimates and HLM model also indicated that agencies that have deployed CEDs are associated with fewer injuries to suspects compared to non-CED agencies. The magnitude of the effect of CED agencies is similar across the three models and in the same direction. Our results indicate that for an agency that deploys CEDs, the odds of a suspect being injured in the post-period is reduced by over 40% relative to agencies without CEDs.

For our CED-only site analyses, non-weapon/hands-on tactics were associated with the highest levels of suspect injuries (45%), followed by batons (44%), CEDs (44%), multiple weapons (or weapons other than CEDs, batons, or OC spray) (15.9%), and OC spray (7.6%). For these analyses, OC spray and multiple/other weapons were associated with significantly lower suspect injuries than the other forms of force ($p<.001$). Based on our logistic regression model, there was no difference between cases when officers use CEDs and cases where other weapons or multiple weapons are used in terms of suspect injuries.

Officer injury severity: Across all of our analyses, our results demonstrate that CEDs do not have an effect the severity of officer injuries. Before the CED sites

main interest in running the HLMs was to confirm the direction of our findings (i.e., whether CEDs were associated with an increase or a decrease in our safety-related outcome measures), and the approximate magnitude of the effect.

deployed CEDs, 4% of the officers were severely injured in force cases, compared to a similar proportion of officers in the non-CED sites (7%) over the same reference period, representing no statistical difference. The CED sites observed no significant change in officer severe injuries (5% from 4%) after they began their deployment of CEDs, compared to the non-CED sites that observed no change in officer severe injuries (6% to 7%). All three multivariate models failed to reach statistical significance for the officer injury severity outcome measure. The CED agencies do not seem to differ from the non-CED agencies in altering the number of officers receiving severe injuries during force cases. For our CED-only site analyses, we found no statistically significant differences in the proportion of officers receiving severe injuries across the various use-of-force tactics. Based on our logistic regression model, we found no difference between cases when officers use CEDs and cases where other weapons or multiple weapons are used in terms of officer severe injuries.

Suspect severe injuries: Before the CED sites deployed CEDs, 7% of the suspects were severely injured in force cases, compared to a similar proportion of suspects in the non-CED sites (7%). The CED sites went on to experience a reduction in suspect severe injuries (to 5% from 7%) after they began their deployment of CEDs, compared to the non-CED sites, which observed no change in suspect severe injuries. All three multivariate models also indicated that agencies that have deployed CEDs are associated with fewer severe injuries to suspects compared to non-CED agencies. The magnitude of the effect of CED agencies is similar across the three models (.56 and .36) and in the same direction (negative coefficients). Our results indicate that for an agency that deploys CEDs, the odds of a suspect being severely injured in the post-period is reduced by over 40% relative to agencies without CEDs. While we did not reach statistical significance with our first logistic regression model (where the analyses were solely based on the individual-level), we did reach statistical significance for our logistic regression model with a correction for nested standard errors ($p<.02$) and reached statistical significance for the HLM model ($p=.05$). Overall, the evidence suggests that agencies that have deployed CEDs are associated with fewer severe injuries to suspects compared to non-CED agencies.

For our CED-only site analyses, CED (2%) and OC spray (3%) forms of force were associated with lower levels of suspect severe injuries than multiple/other weapons (6%) and use of batons by officers (6%) ($p<.05$). Based on our logistic regression model, we found no difference between cases when officers use CEDs and cases where other weapons or multiple weapons are used in terms of suspect severe injuries.

Officer injury requiring medical attention: Before the CED sites deployed CEDs, 13% of the officers in the sites received an injury requiring medical attention in force cases, compared to a lower proportion of officers in the non-CED sites (4%) over the same reference period ($p<.001$). The CED sites observed a large decrease in officers receiving an injury requiring medical attention (to 8% from 13%) after they began their deployment of CEDs, compared to the non-CED sites, which observed a large increase in officer receiving an injury requiring medical attention (to 16% from 4%) ($p<.001$). All three multivariate models indicate that agencies that have deployed CEDs are associated with fewer cases of officers receiving an injury requiring medical attention related to use of force compared to non-CED agencies. The magnitude of the effect of CED agencies is similar across the three models and in the same direction. Our results indicate that for an agency that deploys CEDs, the odds of an officer receiving an injury requiring medical attention in the post-period is reduced by over 80% relative to agencies without CEDs.

For our CED-only site analyses, OC spray (13%), batons (12%), and tactics other than weapons (9%) were associated with the highest levels of officers receiving an injury requiring medical attention. For these analyses, multiple weapons/other weapons and CEDs were associated with significantly lower number of cases where officers received an injury requiring medical attention than the other forms of force ($p<.001$). Based on our logistic regression model, when officers use CEDs, there is a 63% reduction in the probability of an officer receiving an injury requiring medical attention ($p<.001$) compared to cases where other weapons or multiple weapons are used.

Suspect injury requiring medical attention: Before the CED sites deployed CEDs, 55% of the suspects received an injury requiring medical attention in force cases, compared to a lower proportion of suspects in the non-CED sites (35%) over the same reference period, representing a statistically significant difference ($p<.001$). The CED sites observed a large decrease in suspects receiving an injury requiring medical attention (to 40% from 55%) after they began their deployment of CEDs, compared to the non-CED sites, which observed a large increase in suspects receiving an injury requiring medical attention (to 53% from 35%) ($p<.001$). All three multivariate models indicate that agencies that have deployed CEDs are associated with fewer cases of suspects receiving injuries requiring medical attention related to use-of-force compared to non-CED agencies. Our results indicate that for an agency that deploys CEDs, the odds of a suspect receiving an injury requiring medical attention in the post-period is reduced between 79% (logistic) and 46% (HLM) relative to non-CED agencies.

For our CED-only site analyses, batons (62.5%), CEDs (58%) and non-weapon/hands-on tactics (55.7%) were associated with the highest levels of suspects receiving an injury requiring medical attention. For these analyses, multiple/other weapons and OC spray were associated with significantly lower number of cases where suspects received an injury requiring medical attention than the other forms of force ($p<.001$). Based on our logistic regression model, we found no difference between cases when officers actually use CEDs to cases where other weapons or multiple weapons are used in terms of suspects receiving an injury requiring medical attention.

Officer injury requiring hospitalization: Before the CED sites deployed CEDs, 4.3% of the officers received an injury requiring hospitalization in force cases, compared to a similar proportion of officers who received an injury requiring hospitalization in the non-CED sites (3.3%) over the same reference period, representing no statistical difference ($p=.35$). The CED sites observed a very small decrease in officers receiving an injury requiring hospitalization (to 4.1%) after they began their deployment of CEDs, compared to the non-CED sites, which observed an increase in officers receiving an injury requiring hospitalization (to 6.3%). The CED sites started out at a similar rate of officers receiving an injury requiring hospitalization compared to the non-CED sites (4.3% and 3.3%, respectively), but the CED sites were significantly lower at the post period (4.1% to 6.3%). All three multivariate models failed to reach statistical significance for the officer hospitalization measure. While the logistic regression models and HLM model had negative coefficients, on balance, the CED agencies do not seem to differ from the non-CED agencies in altering the number of officers requiring hospitalization for an injury during a force incident.

For our CED-only site analyses, we found no statistically significant differences across the various forms of use-of-force in terms of officers receiving injuries requiring hospitalization. Based on our logistic regression model, we found no difference between cases when officers actually use CEDs and cases where other weapons or multiple weapons are used in terms of officers receiving injuries requiring hospitalization.

Suspect injury requiring hospitalization: Before the CED sites deployed CEDs, 27% of the suspects received an injury requiring hospitalization compared to a similar proportion of the suspects in the non-CED sites (31%) over the same reference period (p=.11). The CED sites observed a large decrease in suspects receiving an injury requiring hospitalization (to 16%) after they began their deployment of CEDs, compared to the non-CED sites, which observed a small increase in suspects receiving an injury requiring hospitalization to 36% (p<.05). The CEDs started out at a similar rate of suspects receiving injuries requiring hospitalization compared to the non-CED sites (27% to 31%), but the CED sites were significantly lower at the post period (16% to 36%).

All three multivariate models indicate that agencies that have deployed CEDs are associated with fewer cases of suspects receiving an injury requiring hospitalization compared to non-CED agencies. While the direction of the effect of CED agencies is similar across the three models, the magnitude of the effect was quite different. Our results indicate that for an agency that deploys CEDs, the odds of a suspect receiving an injury requiring hospitalization in the post-period is reduced by 52% for the logistic regression model or only 11% for the HLM models relative to agencies without CEDs. While there is a wide gap in these estimates, both models suggest that CED sites are associated with a reduced probability of suspects receiving injuries requiring hospitalization.

For our CED-only site analyses, CEDs (29.5%), batons (19.7%), and non-weapon/hands-on tactics (16.7%) were associated with the highest levels of suspects receiving injuries requiring hospitalization. For these analyses, OC spray (11.2%) and multiple/other weapons (12.3%) were associated with significantly lower number of cases where suspects received injuries requiring hospitalization than the other forms of force (p<.001). Based on our logistic regression model, when officers use CEDs, there was a 139% increase in the probability of a suspect receiving injuries requiring hospitalization (0.87, *p*<.001) compared to cases where other weapons or multiple weapons are used.

We have explored this 139% increase and attempted to disentangle this result. For example, suspects who were subjected to a CED activation were not different from suspects who had other weapons used against them on our injuries measure. Also, these results apply only to agencies that have CEDs (the matched non-CED sites were not in these analyses). We discussed this finding with some of the police personnel at the sites. These personnel indicated that some agencies may have an informal practice in place where they send suspects who have been activated by a CED to a hospital more frequently compared to other types of force cases (perhaps due to the heavy news media coverage than can sometimes emerge with a CED case). Furthermore, as noted earlier, "hospitalization" in this context does not necessarily mean an overnight stay in a hospital; rather, it signifies only that suspects were sent to a hospital, clinic, or other medical facility, and in many cases may have simply received outpatient evaluation and/or treatment. It is also worth noting in this context that in 2005, PERF issued a set of 52 CED guidelines that, among other things, recommended that "all persons who have been exposed to a CED activation should receive a medical evaluation," and that "officers should not generally remove CED darts from a subject that have penetrated the skin unless they have been trained to do so." Unfortunately, we do not have the case narratives on each of the CED cases in this study to assess exactly what is occurring, and that is why more research is needed on this topic. Alternatively, this could be just an anomalous finding. Given that there is little precedence for collecting the type of data we collected, we were not aware of this potential complexity in our data and were not able to build this into the design of our study. Future researchers will be able to consider this finding and build in features to be able to explore this issue in their research.

Suspect deaths: Before implementation of CEDs, the CED sites had less than one percent of their cases (0.2%) involving a suspect killed by an officer. After CED implementation, this number remained about the same (0.4%). During the same period, the non-CED sites did not change either, observing about one percent of their cases (0.9%) involving a suspect killed by an officer at the pre-test period as well as the post-period. While the post-period results for CEDs (0.4%) and non-CEDs (0.9%) represents a small statistical difference ($p<.05$) we do not believe this difference is necessarily attributable to the presence of CEDs (it is likely just random noise in the data). We basically have a flat line for the CED sites (0.2% to 0.4%) and a flat line for the non-CED sites (0.9% at both time points). On balance, CEDs do not appear to have much of an effect on suspect deaths, but with a sample of only 44 suspect deaths we do not have a high level of statistical power to uncover statistically significant findings. One of the concerns that has been expressed by a number of organizations regarding CEDs is that they may lead to higher death rates for agencies that deploy CEDs. We found no support for this concern. CEDs seem to have a neutral effect on the number of suspect deaths related to officer use-of-force cases.

Summary of findings:

Overall, we found that the CED sites were associated with improved safety outcomes when compared to a group of matched non-CED sites on six of nine safety measures, including reductions in (1) officer injuries, (2–3) suspect injuries and severe injuries, (4–5) officers and suspects receiving injuries requiring medical attention, and (6) suspects receiving an injury requiring hospitalization. For the other three of nine measures, there were no differences between the CED and the non-CED sites on the outcomes of the number of suspect deaths, officer severe injuries, and officer injuries requiring hospitalization.

For the six of nine significant outcomes, the magnitude of the effects of the improved safety outcomes for the CED sites (relative to the non-CED sites) was impressive. We found a strong effect of CEDs on reducing *officer injuries* based on our raw results (8% officer injuries in the post–period, compared to 20% for the non-CED sites), and our three multivariate models. For agencies that deploy CEDs, the odds of an officer being injured are reduced by over 70%. Also, for our CED-only site analyses, when officers actually use CEDs there is a 76% reduction in officer injuries. Similar reductions were observed for the CED sites on our measure of *suspect injuries*, as confirmed by our raw results (26% suspect injuries in the post–period, compared to 43% for the non-CED sites), and our three multivariate models. For an agency that deploys CEDs, the odds of a suspect being injured are reduced over 40%. Along the same lines, CED sites were related to reductions in *suspect severe injuries* based on our raw results (5% suspect severe injuries in the post–period, compared to 7% for the non-CED sites), and our three multivariate models. For an agency that deploys CEDs, the odds of a suspect being severely injured are reduced by over 40%. For our CED-only site analyses, CEDs were associated with the lowest levels of suspect severe injuries compared to other forms of force.

CED sites were related to reductions in *injuries to officers requiring medical attention* based on our raw results (8% of use-of-force cases requiring officer medical attention in the post–period in CED sites, compared to 16% for the non-CED sites), and our three multivariate models. For an agency that deploys CEDs, the odds of an officer receiving an injury requiring medical attention is reduced by at least 80%. For our CED-only site analyses, when officers actually use CEDs there is a 63% reduction in the probability of an officer receiving an injury requiring medical attention. Similarly, CED sites were related to reductions in *injuries to suspects requiring medical attention* based on our raw results (40% of cases requiring suspect medical attention in the post-period in CED sites, compared to 53% for the non-CED sites), and our three multivariate models. For an agency that deploys CEDs, the odds of a suspect receiving an injury requiring medical attention in the post-period are reduced by over 45%.

CED sites were related to reductions in *injuries to suspects requiring hospitalization* based on our raw results (16% resulting in suspect medical attention in the post period, compared to 36% for the non-CED sites), and our three multivariate models. For agencies that deploy CEDs, the odds of a suspect receiving an injury requiring hospitalization in the post-period is reduced by 52% for the logistic regression model or only 11% for the HLM models relative to agencies without CEDs. While there is a wide gap in these estimates, both models suggest that CED sites are associated with a reduced probability of suspects receiving injuries requiring hospitalization. For our CED-only site analyses, CEDs (30%) had the highest levels of suspects receiving injuries requiring hospitalization. When officers use CEDs, there was a 139% increase in the probability of a suspect receiving injuries requiring hospitalization (0.87, $p<.001$). This is one of the few negative/adverse findings for CEDs, and may reflect an informal police practice of sending suspects who have been subjected to a CED activation to a hospital as a precautionary measure—for example, to ensure that the skin punctures caused by the CED darts do not become infected. While overall, the CED sites led to better outcomes than the non-CED sites on this measure, this result needs to be explored further in future research.

Another concern raised by proponents of CEDs is that they may lead to higher death rates for agencies that deploy CEDs. We found no support for this concern. CEDs seem to have a neutral effect on the number of suspect deaths related to officer use-of-force cases. Before implementation of CEDs, the CED sites had less than one percent of their cases (0.2%) involving a suspect killed by an officer. After CED implementation, this number remained about the same (0.4%). During the same period, the non-CED sites did not change either. The non-CED sites observed about one percent of their cases (0.9%) involving a suspect killed by an officer at the pre-test period, and observed no change in the number of suspects killed in force incidents at the post-period (0.9%). We basically have a flat line for the CED sites (0.2% to 0.4%) and a flat line for the non-CED sites (0.9% at both time points). On balance, CEDs do not appear to have much of an effect on suspect deaths, but with a sample of only 44 suspect deaths we do not have a high level of statistical power to uncover statistically significant findings. For officer severe injuries and injuries to officers requiring hospitalization, we also found no differences between the CED and non-CED sites.

All in all, we found consistently strong effects for CEDs on increasing officer and suspect safety. Not only are CED sites associated with improved safety outcomes compared to a matched group of non-CED sites, but also within CED agencies, in some cases the actual use of a CED by an officer is associated with improved safety outcomes compared to other less-lethal weapons. For five of the eight comparisons, the cases where an officer uses a CED were associated with the lowest or second lowest rate of injury or medical attention/ hospitalization.

While our study is one of the first to compare CEDs to matched non-CED sites, such quasi-experimental designs (QEDs) are not without limitations. As mentioned earlier, in the limitations section of Chapter 3, QEDs are not as strong as randomized experiments in isolating the effects of a policy (in our case the policy to either deploy or not deploy CEDs). The main concern is that, as opposed to randomized experiments, it hard to control for the many unmeasured variables related to the outcome variable (Shadish et al., 2002). Randomized experiments are typically considered the best method for eliminating threats to internal validity in evaluating social policies and programs (Berk et al., 1985; Boruch, et al., 1978; Campbell, 1969; Campbell and Stanley, 1963; Dennis and Boruch, 1989; Riecken et al., 1974). However, it was not possible in this study to randomly assign the use of various weapons to police officers.

With QEDs, the key is to determine all of the important covariates that might affect our outcome measures and statistically control for any observed differences on these measures in our matched participating agencies. We believe we have identified the most important covariates that might confound our comparison of CED and non-CED sites, and we have used these measures to effectively isolate the effects of various less-lethal weapons. We have considered various alternative explanations for our results, and believe the most plausible explanation is that the availability of CEDs to officers is a key factor in reducing injuries to officers and suspects. For example, differences between the CED and non-CED sites could be attributable to differences in time periods (this was controlled for in our selection of data from similar time frames across the sites), the presence of more aggravating incident-level factors in some agencies such as greater presence of weapon use by suspects or resistance used by suspects (we included incident-level factors in our models that statistically control for these factors), the absence of more detailed measures (while there are some concerns with the specificity of our measures, i.e. not having enough detail limits the number of additional outcomes we can assess, but does not affect the validity of our dichotomous outcomes), and variation in sampling across the comparison sites (this was assessed and ended up being non-significant in our statistical tests where a variable was added to our models to measure whether a sample or population data were used).

On balance the effect sizes evident in our results are substantively important and should be carefully considered by law enforcement executives. For example, for agencies participating in this evaluation that deploy CEDs, our results suggest that the odds of an officer being injured in the post-period are reduced by over 70% relative to agencies without CEDs. Also, the effect sizes are generally large enough to suggest that even if the comparability of the CED and non-CED sites is not perfect, there are still likely to be important safety gains for officers in agencies that deploy CED compared to those that do not.

Next, we discuss the implications of our results for LEA policy and training, and provide some recommendations for future research.

Implications of PERF results regarding when to use CEDs:

Prior research on police use of force, including our results, consistently shows that most use-of-force encounters involve low levels of force and few if any injuries for officers and suspects. However, it is not uncommon for officers to have to use more force to gain control of a noncompliant suspect and take the person to the ground, with the officer using the ground for leverage (see Smith et al., 2008). These types of ground struggles carry an increased risk for injury for officers and suspects. According to our results, police devices such as CEDs and OC spray that avoid these up-close struggles hold the promise of avoiding injuries for all concerned parties. These findings are consistent with the work by Smith and colleagues (2008) that CEDs and OC spray allow officers to control suspects from a distance without engaging in the hand-to-hand struggles that typically cause injuries.

The evidence from our study suggests that CEDs can be an effective weapon in helping prevent or minimize physical struggles in use-of-force cases. LEAs should consider the utility of the CED as a way to avoid up-close combative situations and reduce injuries to officers and suspects. Also, for agencies that do not deploy CEDs, our results suggest that they should consider the possible value of deploying CEDs, and the relevance of the CED for use by officers in their community. Also, similar results were also uncovered in a similar study by Smith et al. (2008). Faced with similar results, Smith et al. (2008) recommended that CEDs should be authorized as a possible response in cases where suspects use defensive resistance (e.g., suspect struggles to escape physical control of officer) or higher levels of suspect resistance, in order to help officers avoid up-close combative situations. We do not take a position on the specific circumstances when an LEA should authorize the use of the CED. We believe such a policy decision needs to be made at the local level. It is not appropriate, based on a single study, to make a firm recommendation on when a CED should be authorized to be used. Each LEA has to consider a multitude of factors in assessing when to authorize use of the CED, working closely with its full set of community partners to consider a range of local factors. However, our study provides important data points to inform these policy decisions that LEAs need to make. For example, there is little support in our data to consider authorizing the use of CEDs in cases of passive resistance from a suspect; these cases rarely results in injuries to officers. Also, in terms of reducing injuries, there is little to gain by permitting use of CEDs against certain special populations (pregnant women, elderly citizens, and others who are clearly physically impaired), for few of these persons were involved in force cases where officers were injured in our study. As pointed out in the Police Executive Research Forum CED guidelines, good CED policies and training will aid officers in evaluating the totality of the circumstances before using a CED, which would include considering the following factors: the age, size, gender, apparent physical capabilities, and health concerns of suspects, presence of flammable liquids, and circumstances where falling would pose unreasonable risks to the suspect.[35]

Many policy questions with the use of CEDs still remain. Where on the body should a CED be used? Do the number of CED activations and the duration of shocks impact safety? Does use of CEDs in combination with a flammable substance increase the possibility of ignition? Should the use of CEDs against the very young, pregnant women, and those suffering from medical problems or other special populations be prohibited? For example, some have raised concerns about the use of CEDs on seniors or individuals suffering from osteoporosis. A deputy sheriff suffering from this bone-weakening disease reportedly sustained a fracture after he was shocked during a training exercise (Anglen 2004).

Need for training for CEDs

There is little attention in the CED literature to training of officers and sheriffs' deputies in the proper use of CEDs. While some CED manufacturers have developed CED training curricula and some have even provided CED training, there are few independent sources for agencies to turn for guidance on developing a CED training program (see Smith et al., 2008). As a result, there is little consensus on what training should be required, what it should encompass, or what its purpose should be beyond familiarization with the device (see Smith et al., 2008). Officer training varies from familiarization training with the CED (sometimes including officers being shocked with the CED to experience the weapon's effects) to comprehensive scenario-based training where multiple weapons and other tools, including the CED, are available to deal with a simulated threat. However, research to identify which of these approaches is most effective has not been done (see Smith et al., 2008).

Another training issue is the inappropriate use of the CED. As with any service weapon, officers can misuse CEDs. Misuse can range from outright abusive or illegal use of the weapon to less obvious cases of officers turning to a CED too early in a force incident (e.g., bypassing verbal de-escalation skills and going right to the use of the CED). These problems can be managed with policies, training, monitoring, and accountability systems that provide clear guidance (and consequences) to officers regarding when and under what conditions CEDs should be used and when they should not be used (see Smith et al., 2008). Good CED policies and training should also require that officers evaluate the totality of the circumstances before using a CED, which would include the age, size, gender, apparent physical capabilities, health concerns of suspects, presence of flammable liquids, and circumstances where falling would pose unreasonable risks to the suspect.

Another issue that policing agencies may consider in light of this study is a phenomenon that has been called "weapon-option overload." Some police practitioners have expressed concern about officers having "too many tools on their belt," such as a CED, a collapsible baton, OC spray,

[35] See PERF guidelines on the use of CEDs (Police Executive Research Forum, 2005).

nunchuks, and a heavy flashlight in addition to a firearm. Some departments have discontinued the carrying of OC spray due to its potential for affecting persons other than the intended subject, or have discontinued use of the baton because it requires close contact. Police departments that provide CEDs for officers may consider the possibility of officers, in a fast-moving, highly charged situation, becoming temporarily confused if they have too many force options on their belts. A decision to deploy CEDs may cause some departments to discontinue use of other less-lethal options.

Next steps for researchers

As discussed in the limitations sections of this report, one of the greatest barriers to conducting use-of-force research is the absence of uniformity and comprehensiveness in the collection of data about uses of force by LEAs across the country. Our team was only able to identify a small group of LEAs that were able to participate readily in this study. While our team is very thankful that they participated, even for these agencies, the data available for analysis were limited. We observed limitations in content (information about many of our areas of interest was not collected by the LEAs), and timing (many of the LEAs were limited in how long they kept their force records, limiting our team to no more than four years of analysis). Also, the use-of-force tracking systems we observed lacked a common architecture or set of definitions. Similar barriers were reported by Smith et al. (2008).

One possible solution to this problem has been advanced by Smith and colleagues (2008) involving a federal incentive for agencies to collect use-of-force data using a common set of data elements and definitions to define what information is captured. Smith and colleagues (2008) suggest that Congress, with advice from the National Institute of Justice, could fund a grant program to state or local law enforcement agencies that collect and make available for research purposes data on the use of force by police. Smith and colleagues (2008) recommend that such a program could be jump-started by NIJ field-testing with volunteer LEAs a model use-of-force data collection protocol. The recommended approach by Smith et al. (2008) model would provide useful data from a select number of agencies and a model of how data collection and analysis can assist with agency policies and training, as well as providing critical information to the research community.

Smith and colleagues (2008) further suggest that LEAs could be encouraged to apply for grant funds to build the systems necessary to collect use-of-force data, which then would be used to support research and analysis aimed at reducing the need for and harmful consequences of police use of force. Smith et al. also call for developing a common software platform for data entry, storage, and transmission to a research team that would advise agency participants, audit the incoming data, and create a publicly available and non-proprietary dataset for research purposes.

We agree with Smith et al. (2008) that such a strategy would result in an important national-scale data source that could be maintained and updated regularly as new use-of-force technologies came online, and would likely spur new and better research on how to reduce the harm that can occur when LEAs use force.

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Appendix 1: Logistic regression Model 1 and Model 2 both without corrections for nested standard errors

	Outcome measures/dependent variables															
	Suspect Injury								Officer Injury							
	Model 1				Model 2				Model 1				Model 2			
Variables in logisitc regression model	β	Odds Ratio	SE	P value	β	Odds Ratio	SE	P value	β	Odds Ratio	SE	P value	β	Odds Ratio	SE	P value
Intercept	-1.45	0.23	0.12	<.0001	-1.39	0.25	0.17	<.0001	-2.26	0.10	0.18	<.0001	-3.00	0.05	0.30	<.0001
Does Agency Deploy CED (1= yes, 0=no)	-0.39	0.68	0.10	<.0001	0.25	1.28	0.13	0.0638	0.02	1.02	0.15	0.9131	-0.17	0.84	0.23	0.4576
Time frame of incident (post-CED/comparable period= 1, pre CED/comparable period=0)	0.77	2.17	0.13	<.0001	0.70	2.00	0.13	<.0001	0.89	2.43	0.19	<.0001	1.06	2.89	0.20	<.0001
Interaction CED * Time Frame (1= CED and post period)	-0.57	0.56	0.14	<.0001	-0.33	0.72	0.17	0.0511	-1.20	0.30	0.20	<.0001	-1.14	0.32	0.30	0.0001
Suspect race (White= 1, Non-White=0)	0.41	1.51	0.05	<.0001	0.18	1.20	0.09	0.053	0.23	1.26	0.08	0.0038	-0.25	0.78	0.17	0.1378
Suspect gender (Male=1, female=0)	0.55	1.73	0.08	<.0001	0.09	1.09	0.14	0.5086	0.03	1.03	0.11	0.7642	0.13	1.14	0.23	0.5708
Suspect age (1= < 25 years old, 0= > 25 years old)	0.14	1.15	0.05	0.0035	0.13	1.13	0.09	0.1419	0.23	1.26	0.07	0.002	0.08	1.08	0.15	0.6014
Suspect resistant behavior (1=physical aggression by suspect, 0= non-physical aggression)					0.37	1.45	0.09	<.0001					0.86	2.37	0.15	<.0001
Suspect had weapon (1=yes, 0= no)					0.86	2.35	0.11	<.0001					0.94	2.56	0.17	<.0001

	Outcome measures/dependent variables															
	Suspect Medicalization								Officer Medicalization							
	Model 1				Model 2				Model 1				Model 2			
Variables in logisitc regression model	β	Odds Ratio	SE	P value	β	Odds Ratio	SE	P value	β	Odds Ratio	SE	P value	β	Odds Ratio	SE	P value
Intercept	-1.00	0.370	0.120	<.0001	-1.39	0.25021463	0.188	<.0001	-3.21	0.040	0.282	<.0001	-5.07	0.01	0.47	<.0001
Does Agency Deploy CED (1= yes, 0=no)	0.84	2.308	0.105	<.0001	0.464	1.58993453	0.141	0.001	1.23	3.432	0.257	<.0001	1.07	2.92	0.35	0.0022
Time frame of incident (post-CED/comparable period= 1, pre CED/comparable period=0)	0.80	2.224	0.137	<.0001	0.933	2.54212921	0.143	<.0001	1.62	5.042	0.283	<.0001	2.21	9.12	0.30	<.0001
Interaction CED * Time Frame (1= CED and post period)	-1.54	0.215	0.147	<.0001	-0.64	0.5246766	0.185	0.0005	-2.04	0.131	0.298	<.0001	-2.94	0.05	0.41	<.0001
Suspect race (White= 1, Non-White=0)	0.42	1.523	0.052	<.0001	0.282	1.32629518	0.103	0.0064	0.09	1.091	0.094	0.3584	-0.20	0.82	0.21	0.3479
Suspect gender (Male=1, female=0)	0.41	1.506	0.070	<.0001	0.306	1.35749933	0.144	0.0333	0.07	1.074	0.135	0.5952	0.43	1.53	0.32	0.18
Suspect age (1= < 25 years old, 0= > 25 years old)	-0.13	0.879	0.048	0.0076	-0.32	0.72594582	0.093	0.0006	0.06	1.061	0.090	0.508	-0.05	0.95	0.19	0.7798
Suspect resistant behavior (1=physical aggression by suspect, 0= non-physical aggression)					0.858	2.35868545	0.094	<.0001					0.80	2.23	0.19	<.0001
Suspect had weapon (1=yes, 0= no)					0.485	1.62375671	0.123	<.0001					1.84	6.27	0.21	<.0001

Outcome measures/dependent variables

	Suspect Injury Minor vs. Severe								Officer Injury Minor vs. Severe							
	Model 1				Model 2				Model 1				Model 2			
Variables in logisitc regression model	β	Odds Ratio	Std Err	P value	β	Odds Ratio	Std Err	P value	β	Odds Ratio	Std Err	P value	β	Odds Ratio	Std Err	P value
Intercept	-2.50	0.08	0.36	<.0001	-3.28	0.04	0.46	<.0001	-2.35	0.10	0.58	<.0001	-2.76	0.06	0.79	0.0005
Does Agency Deploy CED (1= yes, 0=no)	-0.13	0.87	0.27	0.6215	0.50	1.65	0.34	0.1342	-0.62	0.54	0.49	0.2088	-0.24	0.79	0.64	0.7087
Time frame of incident (post-CED/comparable period= 1, pre CED/comparable period=0)	0.03	1.03	0.32	0.9169	-0.34	0.71	0.35	0.3317	-0.46	0.63	0.60	0.439	-0.55	0.58	0.63	0.3801
Interaction CED * Time Frame (1= CED and post period)	-0.58	0.56	0.37	0.118	-0.23	0.79	0.45	0.6107	0.56	1.75	0.69	0.4166	0.63	1.87	0.91	0.4888
Suspect race (White= 1, Non-White=0)	0.11	1.12	0.17	0.5068	0.13	1.14	0.23	0.5705	-0.14	0.87	0.34	0.679	0.56	1.75	0.49	0.2543
Suspect gender (Male=1, female=0)	0.04	1.04	0.27	0.8952	0.06	1.06	0.34	0.87	-0.08	0.92	0.42	0.8495	-0.07	0.93	0.65	0.9151
Suspect age (1= < 25 years old, 0= > 25 years old)	-0.14	0.87	0.17	0.4175	-0.34	0.71	0.24	0.1467	-0.01	0.99	0.31	0.9837	0.44	1.56	0.46	0.3334
Suspect resistant behavior (1=physical aggression by suspect, 0= non-physical aggression)					0.71	2.04	0.22	0.0014					0.05	1.05	0.45	0.92
Suspect had weapon (1=yes, 0= no)					1.21	3.37	0.27	<.0001					0.09	1.09	0.56	0.8731

Outcome measures/dependent variables

	Suspect Hospitalization								Officer Hospitalization							
	Model 1				Model 2				Model 1				Model 2			
Variables in logisitc regression model	β	Odds Ratio	SE	P value	β	Odds Ratio	SE	P value	β	Odds Ratio	SE	P value	β	Odds Ratio	SE	P value
Intercept	-0.85	0.43	0.14	<.0001	-0.70	0.50	0.18	0.0001	-2.91	0.05	0.32	<.0001	-3.37	0.03	0.43	<.0001
Does Agency Deploy CED (1= yes, 0=no)	-0.23	0.80	0.12	0.0536	0.54	1.71	0.15	0.0003	0.03	1.03	0.28	0.9077	0.02	1.02	0.35	0.9462
Time frame of incident (post-CED/comparable period= 1, pre CED/comparable period=0)	0.21	1.23	0.18	0.2417	0.12	1.13	0.18	0.5086	0.39	1.48	0.35	0.2645	0.41	1.50	0.36	0.2591
Interaction CED * Time Frame (1= CED and post period)	-0.73	0.48	0.19	<.0001	-0.70	0.49	0.21	0.0007	-0.23	0.80	0.38	0.5439	-0.48	0.62	0.43	0.2665
Suspect race (White= 1, Non-White=0)	0.18	1.20	0.06	0.0039	-0.19	0.83	0.10	0.0442	-0.37	0.69	0.15	0.0121	-0.38	0.68	0.23	0.0941
Suspect gender (Male=1, female=0)	0.21	1.24	0.09	0.0155	0.18	1.20	0.13	0.1738	-0.04	0.96	0.19	0.8204	0.11	1.11	0.30	0.7255
Suspect age (1= < 25 years old, 0= > 25 years old)	-0.27	0.76	0.06	<.0001	-0.24	0.79	0.09	0.0059	-0.15	0.86	0.13	0.2738	-0.02	0.98	0.19	0.9338
Suspect resistant behavior (1=physical aggression by suspect, 0= non-physical aggression)					0.20	1.22	0.09	0.0227					0.74	2.10	0.19	0.0001
Suspect had weapon (1=yes, 0= no)					-0.12	0.88	0.14	0.3723					-0.27	0.77	0.32	0.4041

Appendix 2: Logistic regression model with a correction for nested standard errors (Model 1 only)

Suspect Injury: Logistic Regression

Number of Observations = 9,324
Wald $X^2(6) = 138.30$
Prob > $X^2 = 0.0000$
Psuedo $r^2 = 0.024$

	Coefficient	Robust Std. Error	z	P>|z|	95% Conference Interval	
Agency deploys CED	-0.386	0.676	-0.57	0.569	-1.711	0.940
Post-test Period	0.774	0.523	1.48	0.139	-0.251	1.798
Agency used CEDs in post period	-0.572	0.554	-1.03	0.301	-1.658	0.513
Suspect White	0.413	0.188	2.19	0.028	0.044	0.783
Suspect Male	0.548	0.136	4.03	0.000	0.281	0.815
Suspect under 25 years of age	0.142	0.037	3.82	0.000	0.069	0.214
Constant	1.450	0.429	-3.38	0.001	-2.291	-0.608

Officer Injury: Logistic Regression

Number of Observations = 7,963
Wald $X^2(6) = 52.22$
Prob > $X^2 = 0.0000$
Psuedo $r^2 = 0.0162$

	Coefficient	Robust Std. Err.	z	P>|z|	95% Conference Interval	
Agency deploys CED	0.017	0.526	0.03	0.975	-1.015	1.048
Post-test Period	0.887	0.479	1.85	0.064	-0.052	1.826
Agency used CEDs in post period	-1.203	0.512	-2.35	0.019	-2.207	-0.200
Suspect White	0.032	0.157	0.20	0.838	-0.276	0.340
Suspect Male	0.230	0.098	2.34	0.019	0.037	0.423
Suspect under 25 years of age	0.231	0.082	2.81	0.005	0.070	0.392
Constant	-2.262	0.314	-7.22	0.000	-2.877	-1.648

Suspect Medical Attention: Logistic Regression
Number of Observations = 7,696
Wald $X^2(6) = 172.91$
Prob > $X^2 = 0.0000$
Psuedo $r^2 = 0.0334$

	Coefficient	Robust Std. Err.	z	P>|z|	95% Conference Interval	
Agency deploys CED	0.836	0.635	1.32	0.188	-0.407	2.080
Post-test Period	0.799	0.402	1.99	0.047	0.012	1.587
Agency used CEDs in post period	-1.539	0.685	-2.25	0.025	-2.881	-0.197
Suspect White	0.420	0.107	3.92	0.000	0.210	0.630
Suspect Male	0.410	0.178	2.30	0.021	0.061	0.759
Suspect under 25 years of age	-0.129	0.093	-1.39	0.164	-0.310	0.053
Constant	-0.996	0.635	-1.57	0.117	-2.241	0.250

Officer Medical Attention: Logistic Regression

Number of Observations = 5,303
Wald $X^2(6) = 46.66$
Prob > $X^2 = 0.0000$
Psuedo $r^2 = 0.0165$

	Coefficient	Robust Std. Err.	z	P>|z|	95% Conference Interval	
Agency deploys CED	1.233	0.392	3.14	0.002	0.465	2.002
Post-test Period	1.618	0.899	1.80	0.072	-0.144	3.380
Agency used CEDs in post period	-2.036	0.955	-2.13	0.033	-3.908	-0.164
Suspect White	0.087	0.109	0.79	0.427	-0.127	0.301
Suspect Male	0.072	0.070	1.03	0.304	-0.065	0.208
Suspect under 25 yearrs of age	0.059	0.097	0.61	0.539	-0.130	0.249
Constant	-3.212	0.253	-12.72	0.000	-3.707	-2.717

Suspect Hospitalization: Logistic Regression

Number of Observations = 6,996
Wald $X^2(6) = 173.45$
Prob > $X^2 = 0.0000$
Psuedo $r^2 = 0.0211$

	Coefficient	Robust Std. Err.	z	P>|z|	95% Conference Interval	
Agency deploys CED	-0.229	0.773	-0.30	0.767	-1.745	1.286
Post-test Period	0.206	0.491	0.42	0.675	-0.757	1.169
Agency used CEDs in post period	-0.732	0.704	-1.04	0.299	-2.113	0.649
Suspect White	0.182	0.223	0.82	0.415	-0.255	0.619
Suspect Male	0.214	0.085	2.51	0.012	0.047	0.380
Suspect under 25 yrs of age	-0.274	0.113	-2.42	0.016	-0.496	-0.052
Constant	-0.853	0.623	-1.37	0.171	-2.073	0.367

Officer Hospitalization: Logistic Regression

Number of Observations = 5,232
Wald $X^2(6) = 26.81$
Prob > $X^2 = 0.0002$
Psuedo $r^2 = 0.0051$

	Coefficient	Robust Std. Err.	z	P>|z|	95% Conference Interval	
Agency deploys CED	0.032	0.174	0.19	0.852	-0.308	0.373
Post-test Period	0.390	0.409	0.95	0.341	-0.412	1.191
Agency used CEDs in post period	-0.229	0.421	-0.54	0.586	-1.054	0.595
Suspect White	-0.374	0.110	-3.40	0.001	-0.589	-0.159
Suspect Male	-0.044	0.116	-0.38	0.706	-0.270	0.183
Suspect under 25 yrs of age	-0.145	0.119	-1.22	0.221	-0.378	0.087
Constant	-2.912	0.191	-15.24	0.000	-3.287	-2.538

Suspect Injury: Minor vs. Severe (Among those injured): Logistic Regression

Number of Observations = 2,929
Wald $X^2(6) = 57.53$
Prob > $X^2 = 0.0000$
Psuedo $r^2 = 0.0125$

	Coefficient	Robust Std. Err.	z	P>|z|	95% Conference Interval	
Agency deploys CED	-0.134	0.503	-0.27	0.791	-1.120	0.853
Post-test Period	0.034	0.219	0.15	0.877	-0.395	0.463
Agency used CEDs in post period	-0.582	0.257	-2.27	0.023	-1.086	-0.079
Suspect White	0.112	0.158	0.71	0.480	-0.198	0.421
Suspect Male	0.036	0.274	0.13	0.896	-0.501	0.573
Suspect under 25 yrs of age	-0.136	0.113	-1.20	0.231	-0.358	0.086
Constant	-2.505	0.602	-4.16	0.000	-3.685	-1.325

Officer Injury: Minor vs. Severe (Among those injured): Logistic Regression

Number of Observations = 956
Wald $X^2(6) = 12.16$
Prob > $X^2 = 0.0584$
Psuedo $r^2 = 0.0046$

	Coefficient	Robust Std. Err.	z	P>|z|	95% Conference Interval	
Agency deploys CED	-0.621	0.425	-1.46	0.144	-1.454	0.211
Post-test Period	-0.463	0.353	-1.31	0.189	-1.155	0.228
Agency used CEDs in post period	0.561	0.482	1.17	0.244	-0.383	1.506
Suspect White	-0.139	0.488	-0.29	0.775	-1.095	0.817
Suspect Male	-0.080	0.259	-0.31	0.757	-0.588	0.428
Suspect under 25 yrs of age	-0.006	0.241	-0.03	0.979	-0.478	0.465
Constant	-2.349	0.322	-7.29	0.000	-2.981	-1.718

Appendix 3: HLM Results

Officer injury: HLM model

Fixed Effect	Coefficient	Standard Error	T-ratio	P-value
For Intercept1, B0				
INTERCEPT2, G00	-2.411	0.411	-5.871	0.000
AGENCY DEPLOYS CED, G01	-0.031	0.559	-0.055	0.958
POPULATION DENSITY, G02	-0.000	0.000	-1.258	0.249
# OF OFFICERS per 100,000, G03	-0.000	0.000	-1.375	0.211
For POST_PERIOD slope, B1				
INTERCEPT2, G10	1.064	1.116	0.954	0.372
AGENCY DEPLOYS CED, G11	-1.494	1.591	-0.939	0.379
POPULATION DENSITY, G12	-0.000	0.000	-1.095	0.310
# OF OFFICERS per 100,000, G13	-0.000	0.000	-0.776	0.463
For SUSPECT WHITE slope, B2				
INTERCEPT2, G20	-0.648	0.302	-2.142	0.069
AGENCY DEPLOYS CED, G21	0.819	0.312	2.621	0.034
POPULATION DENSITY, G22	0.000	0.000	1.259	0.249
# OF OFFICERS per 100,000, G23	-0.000	0.000	-0.120	0.908
For SUSPECT MALE slope, B3				
INTERCEPT2, G30	0.189	0.494	0.383	0.713
AGENCY DEPLOYS CED, G31	-0.430	0.582	-0.738	0.484
POPULATION DENSITY, G32	-0.000	0.000	-0.378	0.716
# OF OFFICERS per 100,000, G33	0.000	0.000	1.092	0.311
For SUSPECT UNDER 25 YEARS OLD slope, B4				
INTERCEPT2, G40	-0.005	0.425	-0.012	0.991
AGENCY DEPLOYS CED, G41	-0.083	0.567	-0.146	0.888
POPULATION DENSITY, G42	-0.000	0.000	-0.105	0.920
# OF OFFICERS per 100,000, G43	0.000	0.000	0.049	0.963

Suspect injury: HLM model

Fixed Effect	Coefficient	Standard Error	T-ratio	P-value
For INTERCEPT1, B0				
INTERCEPT2, G00	-1.093	0.576	-1.896	0.099
AGENCY DEPLOYS CED, G01	-0.402	0.803	-0.500	0.632
POPULATION DENSITY, G02	-0.000	0.000	-1.052	0.328
AVERAGE NUMBER OF, G03	-0.000	0.000	-1.079	0.317
OFFICERS PER 100,000 IN POPULATION				
For POST_PERIOD slope, B1				
INTERCEPT2, G10	0.735	1.180	0.623	0.553
AGENCY DEPLOYS CED, G11	-0.639	1.632	-0.392	0.707
POPULATION DENSITY, G12	-0.000	0.000	-1.074	0.319
AVERAGE NUMBER OF, G13	-0.000	0.000	-0.669	0.525
OFFICERS PER 100,000 IN POPULATION				
For SUSPECT WHITE slope, B2				
INTERCEPT2, G20	0.032	0.216	0.147	0.888
AGENCY DEPLOYS CED, G21	0.458	0.258	1.778	0.118
POPULATION DENSITY, G22	0.000	0.000	1.718	0.129
AVERAGE NUMBER OF, G23	-0.000	0.000	-1.614	0.150
OFFICERS PER 100,000 IN POPULATION				
For SUSPECT MALE slope, B3				
INTERCEPT2, G30	0.404	0.412	0.982	0.359
AGENCY DEPLOYS CED, G31	0.181	0.531	0.342	0.742
POPULATION DENSITY, G32	0.000	0.000	0.249	0.811
AVERAGE NUMBER OF, G33	0.000	0.000	0.549	0.599
OFFICERS PER 100,000 IN POPULATION				
For SUSPECT UNDER 25 YEARS OLD slope, B4				
INTERCEPT2, G40	0.090	0.168	0.535	0.609
AGENCY DEPLOYS CED, G41	-0.162	0.183	-0.887	0.405
POPULATION DENSITY, G42	0.000	0.000	0.040	0.969
AVERAGE NUMBER OF, G43	0.000	0.000	0.245	0.814
OFFICERS PER 100,000 IN POPULATION				

Officer Medical Attention: HLM model

Fixed Effect	Coefficient	Standard Error	T-ratio	P-value
For INTERCEPT1, B0				
INTERCEPT2, G00	-3.554	0.801	-4.439	0.004
AGENCY DEPLOYS CED, G01	0.292	0.982	0.297	0.776
POPULATION DENSITY, G02	-0.000	0.000	-1.470	0.192
AVERAGE NUMBER OF, G03 OFFICERS PER 100,000 IN POPULATION	-0.000	0.001	-0.397	0.705
For POST_PERIOD slope, B1				
INTERCEPT2, G10	-0.779	2.940	-0.265	0.800
AGENCY DEPLOYS CED, G11	-1.744	2.842	-0.613	0.562
POPULATION DENSITY, G12	-0.000	0.000	-0.854	0.426
AVERAGE NUMBER OF, G13 OFFICERS PER 100,000 IN POPULATION	-0.003	0.003	-1.184	0.282
For SUSPECT WHITE slope, B2				
INTERCEPT2, G20	-0.439	0.501	-0.875	0.415
AGENCY DEPLOYS CED, G21	0.366	0.486	0.753	0.480
POPULATION DENSITY, G22	0.000	0.000	0.694	0.514
AVERAGE NUMBER OF, G23 OFFICERS PER 100,000 IN POPULATION	0.000	0.000	0.260	0.804
For SUSPECT MALE slope, B3				
INTERCEPT2, G30	0.794	1.017	0.781	0.464
AGENCY DEPLOYS CED, G31	-0.256	1.030	-0.249	0.812
POPULATION DENSITY, G32	0.000	0.000	0.981	0.365
AVERAGE NUMBER OF, G33 OFFICERS PER 100,000 IN POPULATION	-0.001	0.001	-1.368	0.220
For SUSPECT UNDER 25 YEARS OLD slope, B4				
INTERCEPT2, G40	-1.143	0.831	-1.376	0.218
AGENCY DEPLOYS CED, G41	-0.012	0.753	-0.016	0.988
POPULATION DENSITY, G42	-0.000	0.000	-0.960	0.375
AVERAGE NUMBER OF, G43 OFFICERS PER 100,000 IN POPULATION	-0.002	0.001	-2.241	0.065

Suspect Medical Attention: HLM model

Fixed Effect	Coefficient	Standard Error	T-ratio	P-value
For INTERCEPT1, B0				
INTERCEPT2, G00	-0.063	1.448	-0.043	0.967
AGENCY DEPLOYS CED, G01	-0.895	1.976	-0.453	0.664
POPULATION DENSITY, G02	-0.000	0.000	-0.925	0.386
AVERAGE NUMBER OF, G03 OFFICERS PER 100,000 IN POPULATION	0.000	0.001	0.170	0.870
For POST_PERIOD slope, B1				
INTERCEPT2, G10	0.584	0.940	0.621	0.554
AGENCY DEPLOYS CED, G11	-0.608	1.274	-0.478	0.647
POPULATION DENSITY, G12	-0.000	0.000	-0.803	0.449
AVERAGE NUMBER OF, G13 OFFICERS PER 100,000 IN POPULATION	-0.001	0.000	-1.909	0.097
For SUSPECT WHITE slope, B2				
INTERCEPT2, G20	-0.207	0.272	-0.764	0.470
AGENCY DEPLOYS CED, G21	0.705	0.323	2.184	0.065
POPULATION DENSITY, G22	0.000	0.000	0.634	0.546
AVERAGE NUMBER OF, G23 OFFICERS PER 100,000 IN POPULATION	-0.000	0.000	-0.086	0.934
For SUSPECT MALE slope, B3				
INTERCEPT2, G30	0.112	0.605	0.185	0.859
AGENCY DEPLOYS CED, G31	0.649	0.779	0.834	0.432
POPULATION DENSITY, G32	0.000	0.000	0.224	0.829
AVERAGE NUMBER OF, G33 OFFICERS PER 100,000 IN POPULATION	0.000	0.000	0.053	0.960
For SUSPECTS AGE UNDER 25 YEARS OLD slope, B4				
INTERCEPT2, G40	-0.359	0.271	-1.327	0.226
AGENCY DEPLOYS CED, G41	0.088	0.335	0.262	0.801
POPULATION DENSITY, G42	0.000	0.000	0.845	0.426
AVERAGE NUMBER OF, G43 OFFICERS PER 100,000 IN POPULATION	0.000	0.000	1.020	0.342

Officer Hospitalization: HLM model

Fixed Effect	Coefficient	Standard Error	T-ratio	P-value
For INTERCEPT1, B0				
INTERCEPT2, G00	-3.262	0.291	-11.197	0.000
AGENCY DEPLOYS CED, G01	0.062	0.316	0.196	0.852
POPULATION DENSITY, G02	-0.000	0.000	-1.480	0.199
AVERAGE NUMBER OF, G03 OFFICERS PER 100,000 IN POPULATION	0.000	0.000	0.506	0.634
For POST_PERIOD slope, B1				
INTERCEPT2, G10	0.304	0.402	0.758	0.483
AGENCY DEPLOYS CED, G11	-0.057	0.465	-0.123	0.908
POPULATION DENSITY, G12	-0.000	0.000	-0.829	0.445
AVERAGE NUMBER OF, G13 OFFICERS PER 100,000 IN POPULATION	0.001	0.000	1.873	0.119
For SUSPECT WHITE slope, B2				
INTERCEPT2, G20	-1.297	0.591	-2.196	0.078
AGENCY DEPLOYS CED, G21	1.111	0.601	1.847	0.123
POPULATION DENSITY, G22	-0.000	0.000	-0.129	0.902
AVERAGE NUMBER OF, G23 OFFICERS PER 100,000 IN POPULATION	0.000	0.001	0.687	0.522
For SUSPECT MALE slope, B3				
INTERCEPT2, G30	0.077	0.625	0.123	0.908
AGENCY DEPLOYS CED, G31	-0.128	0.737	-0.174	0.869
POPULATION DENSITY, G32	-0.000	0.000	-1.489	0.196
AVERAGE NUMBER OF, G33 OFFICERS PER 100,000 IN POPULATION	0.000	0.000	1.183	0.290
For SUSPECT AGE UNDER 25 YEARS OLD slope, B4				
INTERCEPT2, G40	0.016	0.405	0.040	0.970
AGENCY DEPLOYS CED, G41	-0.101	0.479	-0.211	0.842
POPULATION DENSITY, G42	0.000	0.000	1.467	0.202
AVERAGE NUMBER OF, G43 OFFICERS PER 100,000 IN POPULATION	-0.000	0.000	-1.459	0.204

Suspect Hospitalization: HLM model

Fixed Effect	Coefficient	Standard Error	T-ratio	P-value
For INTERCEPT1, B0				
INTERCEPT2, G00	-0.386	1.263	-0.306	0.769
AGENCY DEPLOYS CED, G01	-1.256	1.719	-0.731	0.489
POPULATION DENSITY, G02	0.000	0.000	0.422	0.685
AVERAGE NUMBER OF, G03	-0.001	0.001	-2.222	0.061
OFFICERS PER 100,000 IN POPULATION				
For POST_PERIOD slope, B1				
INTERCEPT2, G10	0.331	0.289	1.145	0.290
AGENCY DEPLOYS CED, G11	-0.121	0.362	-0.335	0.747
POPULATION DENSITY, G12	-0.000	0.000	-0.646	0.539
AVERAGE NUMBER OF, G13	0.000	0.000	0.279	0.788
OFFICERS PER 100,000 IN POPULATION				
For SUSPECT WHITE slope, B2				
INTERCEPT2, G20	-0.163	0.253	-0.644	0.540
AGENCY DEPLOYS CED, G21	0.490	0.272	1.803	0.114
POPULATION DENSITY, G22	-0.000	0.000	-2.031	0.081
AVERAGE NUMBER OF, G23	0.000	0.000	0.555	0.596
OFFICERS PER 100,000 IN POPULATION				
For SUSPECT MALE slope, B3				
INTERCEPT2, G30	0.206	0.391	0.526	0.615
AGENCY DEPLOYS CED, G31	0.038	0.469	0.081	0.938
POPULATION DENSITY, G32	-0.000	0.000	-0.888	0.404
AVERAGE NUMBER OF, G33	0.000	0.000	1.619	0.149
OFFICERS PER 100,000 IN POPULATION				
For SUSPECT AGE UNDER 25 YEARS OLD slope, B4				
INTERCEPT2, G40	-0.763	0.294	-2.592	0.036
AGENCY DEPLOYS CED, G41	0.469	0.365	1.285	0.240
POPULATION DENSITY, G42	0.000	0.000	0.624	0.552
AVERAGE NUMBER OF, G43	0.000	0.000	0.785	0.458
OFFICERS PER 100,000 IN POPULATION				

Officer Severity of Injury Minor/Severe: HLM model

Fixed Effect	Coefficient	Standard Error	T-ratio	P-value
For INTERCEPT 1, B0				
INTERCEPT2, G00	-2.130	0.508	-4.194	0.005
AGENCY DEPLOYS CED, G01	-1.069	0.619	-1.726	0.127
POPULATION DENSITY, G02	0.000	0.000	1.774	0.119
AVERAGE NUMBER OF, G03 OFFICERS PER 100,000 IN POPULATION	0.000	0.000	1.253	0.251
For POST_PERIOD slope, B1				
INTERCEPT2, G10	-0.804	0.688	-1.170	0.281
AGENCY DEPLOYS CED, G11	1.075	0.824	1.305	0.233
POPULATION DENSITY, G12	-0.000	0.000	-0.156	0.881
AVERAGE NUMBER OF, G13 OFFICERS PER 100,000 IN POPULATION	-0.000	0.000	-0.891	0.403
For SUSPECT WHITE slope, B2				
INTERCEPT2, G20	2.304	0.737	3.125	0.018
AGENCY DEPLOYS CED, G21	-2.906	0.942	-3.084	0.019
POPULATION DENSITY, G22	0.000	0.000	2.147	0.068
AVERAGE NUMBER OF, G23 OFFICERS PER 100,000 IN POPULATION	0.000	0.000	1.860	0.105
For SUSPECT MALE slope, B3				
INTERCEPT2, G30	-0.282	1.024	-0.276	0.791
AGENCY DEPLOYS CED, G31	0.665	1.131	0.588	0.574
POPULATION DENSITY, G32	0.000	0.000	1.000	0.351
AVERAGE NUMBER OF, G33 OFFICERS PER 100,000 IN POPULATION	-0.000	0.000	-0.739	0.484
For SUSPECT AGE UNDER 25 YEARS OLD slope, B4				
INTERCEPT2, G40	0.664	0.670	0.992	0.355
AGENCY DEPLOYS CED, G41	-0.624	0.764	-0.816	0.442
POPULATION DENSITY, G42	0.000	0.000	0.297	0.775
AVERAGE NUMBER OF, G43 OFFICERS PER 100,000 IN POPULATION	-0.000	0.000	-1.407	0.202

Suspect Severity of Injury Minor/Severe: HLM model

Fixed Effect	Coefficient	Standard Error	T-ratio	P-value
For INTERCEPT 1, B0				
INTERCEPT2, G00	-2.857	0.542	-5.268	0.000
AGENCY DEPLOYS CED, G01	-0.083	0.725	-0.115	0.912
POPULATION DENSITY, G02	0.000	0.000	0.353	0.734
AVERAGE NUMBER OF, G03	0.000	0.000	0.428	0.681
OFFICERS PER 100,000 IN POPULATION				
For POST_PERIOD slope, B1				
INTERCEPT2, G10	0.329	0.354	0.930	0.384
AGENCY DEPLOYS CED, G11	-1.028	0.437	-2.350	0.051
POPULATION DENSITY, G12	-0.000	0.000	-1.287	0.239
AVERAGE NUMBER OF, G13	-0.000	0.000	-0.922	0.388
OFFICERS PER 100,000 IN POPULATION				
For SUSPECT WHITE slope, B2				
INTERCEPT2, G20	-0.315	0.461	-0.684	0.516
AGENCY DEPLOYS CED, G21	0.334	0.541	0.617	0.557
POPULATION DENSITY, G22	-0.000	0.000	-0.884	0.406
AVERAGE NUMBER OF, G23	-0.000	0.000	-1.525	0.171
OFFICERS PER 100,000 IN POPULATION				
For SUSPECT MALE slope, B3				
INTERCEPT2, G30	0.894	0.940	0.950	0.374
AGENCY DEPLOYS CED, G31	-0.245	1.236	-0.199	0.848
POPULATION DENSITY, G32	0.000	0.000	0.151	0.885
AVERAGE NUMBER OF, G33	-0.000	0.000	-0.383	0.713
OFFICERS PER 100,000 IN POPULATION				
For SUSPECT AGE UNDER 25 YEARS OLD slope, B4				
INTERCEPT2, G40	-0.697	0.391	-1.780	0.118
AGENCY DEPLOYS CED, G41	0.581	0.457	1.272	0.244
POPULATION DENSITY, G42	-0.000	0.000	-0.779	0.461
AVERAGE NUMBER OF, G43	0.000	0.000	0.718	0.496
OFFICERS PER 100,000 IN POPULATION				

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