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Injuries to Officers and Suspects in Police Use-of-Force Cases: A Quasi-Experimental Evaluation

Bruce Taylor[1] and Daniel J. Woods[1]

Abstract

The Conducted Energy Device (CED) weapon holds the potential to reduce injuries for officers/suspects. However, the dearth of research on CEDs makes it difficult to make informed decisions about its deployment. We conducted a quasi experiment to compare 4 years of data from seven law enforcement agencies (LEAs) with CED deployment with six matched LEAs without CED deployment. Compared with non-CED sites, CED sites had lower rates of officer injuries, suspect severe injuries, and officers and suspects receiving injuries requiring medical attention. Our results suggest that CEDs can be effective in helping minimize physical struggles and resulting injuries in use-of-force cases.

Keywords

injuries to officers and suspects, police use-of-force, conducted energy devices, nonlethal weapons

How law enforcement agencies (LEAs) manage the use of force by officers is perhaps one of the most important tasks that they 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. Although the news media tends to provide heavy coverage of force used by police, it is easy to get the impression that police use of force is commonplace. However, prior research suggests that these types of encounters are rare.

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Research based on a variety of data sources consistently demonstrates that a very small number of police-to-citizen contacts include any use of force by the officers (U.S. Department of Justice, 1999). Based on an analysis of the 2002 national *Police-Public Contact Survey* and the 2002 *Survey of Inmates in Local Jails*, Hickman, Piquero, and Garner (2008) found that the police use or threaten to use force in 1.7% of all contacts and in about 20% of all arrests. Also, most suspect injuries are relatively minor, typically consisting of bruises, abrasions, and muscle strains and sprains (Alpert & Dunham, 2000; Henriquez, 1999; Kaminski, DiGiovanni, & Downs, 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 was 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%, respectively (Alpert & Dunham, 2000, 2004; Kaminski & Sorensen, 1995). Nevertheless, research on force-related officer injuries suggests that most injuries were relatively minor (Alpert & Dunham, 2000; Brandl, 1996; Brandl & Stroshine, 2003; Kaminski et al., 2004; Smith & Petrocelli, 2002).

A number of nonlethal weapons have been advanced in recent years as a way to reduce injuries for officers and suspects. Although 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, Kaminski, Rojek, Alpert, & Mathis, 2007), little research has been done isolating the effects of using nonlethal weapons on injuries to suspects and officers. Compared with firearms, nonlethal weapons offer the promise of helping officers to control violent suspects without killing them or running the risk of a stray bullet killing a bystander. 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 LEAs and conducted energy devices (CEDs or Tasers®) reported to be in use in more than 11,500 LEAs (Smith et al., 2008). However, there is uncertainty within the law enforcement community about deployment of some of these weapons. As with OC spray, CEDs have generated controversy (Amnesty International, 2004). Although some law enforcement organizations, such as the Police Executive Research Forum (PERF; 2005), have established guidelines for the safe and effective use of CEDs, other groups have attempted to link these weapons with in-custody deaths and allegations of overuse and even intentional abuse (see Smith et al., 2008 for a review). For example, Amnesty International has documented more than 245 deaths that occurred after the use of CEDs and has argued that a moratorium should be placed on CED use until research can determine a way for them to be safely used (Amnesty International, 2004).

Our study, conducted from 2006 to 2008, is one of the first to compare LEAs that use CEDs with matched LEAs that do not use CEDs. The purpose of our 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 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. 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) with outcomes in LEAs that have not incorporated the use of CEDs (n = 6), statistically controlling for a variety of organizational and individual-level factors.

Literature Review

Nonlethal Weapons and Injuries

Although 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). There have been a number of studies conducted over the past several decades focusing on nonlethal weapons (e.g., Adang, Kaminski, Howell, & Mensink, 2006; Edwards, Granfield, & Onnen, 1997; Gauvin, 1994; IACP, 1995; Kaminski, Edwards, & Johnson, 1998; Kershaw, 2004; Kingshott, 1992; Morabito & Doerner, 1997; Phillips, 1994; Robin, 1996; Smith & Petrocelli, 2002). Several studies have focused on the extent to which nonlethal weapons are "effective" in helping officers gain compliance over a suspect. One such study found that OC spray 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 et al., 1998). If used improperly, impact weapons like the beanbag rounds can penetrate suspects and cause serious injury and death. Since the 1970s, nearly 20 deaths have been attributed to impact weapons (Wilmette, 2001).

OC Spray and Injuries

Although LEAs have experimented with a number of nonlethal weapons, OC spray and CEDs are among the most commonly used of these weapons (Smith et al., 2007). Similar to the current day controversy surrounding CEDs, 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; 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, Onnen, & Petty, 1994;

Petty, 2004), and several other researchers examined its incapacitative effects and the relationship between OC use and officer/suspect injuries (Kaminski, Edwards, & Johnson, 1999; Kaminski et al., 1998; Lumb & Friday, 1997; Morabito & Doerner, 1997; Smith & Alpert, 2000). These studies found that the deaths occurring after the use of OC spray were generally the result of positional asphyxia, preexisting 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 & Friday, 1997; NIJ, 2003; Nowicki, 1993; Smith & 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).

CEDs and Injuries

Although CEDs are now in use by thousands of LEAs (GAO, 2005) and guidelines have emerged for their safe use (PERF, 2005), the research on CEDs has been mostly descriptive and few studies 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, Texax; Cape Coral, Florida; Charlotte-Mecklenburg, North Carolina; Cincinnati, Ohio; Phoenix, Arizona; South Bend, Indiana; and Topeka, Kansas, based on use-of-force reports, all reported substantial declines in either officer injuries (between 3% and 93%) or suspect injuries (between 40% and 79%) following the adoption of CEDs (Smith et al., 2008). Overall, these assessments indicate generally that CEDs are effective, but these estimates vary depending on whether one evaluates the effectiveness of all instances in which CEDs are deployed against suspects or only the CED deployments that result in both darts making contact with the suspect.

Also, TASER International, the main manufacturer of CEDs, 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 the drop in suspect injuries has ranged from a 40% reduction for some agencies up to a 68% reduction for other agencies and officer injuries have dropped from a range of a 41% reduction for some agencies up to as high as a 93% reduction for other agencies. 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 et al., 2006) found a low level of officer injury associated with CED use (8%) compared with 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 pretest/posttest 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 et al. (2007) analyzed the relationship between CEDs and officer and suspect injuries from two LEAs 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 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, whereas 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 et al. (2008) collected more than 24,000 use-of-force records from 12 police agencies that have deployed CEDs. These data were combined and analyzed using multilevel and fixed-effects 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% 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%, respectively. Injuries to officers were unaffected by the use of CEDs, whereas the odds of officer injuries increased somewhat (by about 21% 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 multiagency analysis is consistent with the injury reduction finding in Richland County; in Seattle, OC spray had no effect on suspect injuries, whereas the Miami-Dade Police Department does not issue OC spray.

Our study builds on the Smith et al. (2008) study, using comparable measures and including LEAs that not only have deployed CEDs but also a matched group of LEAs that have not deployed CEDs. The problem with using data only from CED agencies, as in the Smith et al. (2008) study, is that there is no counterfactual comparison group, and we are left with a simple pre/post design with all of its well-known flaws. Also, we are possibly limited in observing the full effects of CEDs across similar types of force situations. That is, 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. Overall, our study fills some of the gaps that exist in the force literature. That is, 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).

Method

We used a quasi-experimental design, comparing seven departments with CED deployment with a set of six matched departments that do not deploy CEDs on a variety of injury outcomes. We collected 4 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 2 years of data before and 2 years after CED deployment. For the LEAs that did not deploy CEDs, we collected at least 4 years of data over a similar period. Although 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 (e.g., 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 4 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. Interrater reliability statistics were high across all five sites (on average .91 across all the sites).

Although it might be preferable to assess the impact of CEDs through a randomized clinical trial, 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 because of ethical concerns. One common alternative to the RCT design is a quasi-experimental design (QED). 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, & Campbell, 2002, p. 14).

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 (a) 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), (b) having a written policy identifying CED and other less lethal weapon placement on the force continuum, (c) a willingness to share data with PERF for this study, and (d) having at least 100 sworn officers (we sought larger LEAs for participation in our study to obtain sufficient numbers of use-of-force incidents for a robust analysis). Next, we needed to ensure that appropriate groups could be compared with each other and that the time-series (pretest and posttest) component of this study could take place. The final criterion (e) 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

To assure that we could identify sites into the study that could meet our study selection criteria (as mentioned earlier), 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 (see Smith et al., 2008 for details on this other project). Our selection process started with identifying LEAs that have full deployment of CEDs. Next, we selected matched LEAs that do not use CEDs. Matching was based on the following criteria: violent crime levels, police activity (violent crime arrests), agency size, and population size of jurisdiction. To protect the confidentiality of the participating sites, we do not reveal the names of the participating agencies. As documented in the Taylor et al. report (2009), comparability between the CED and non-CED sites was achieved for a full range of variables, including dates of analyzed data, 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, number of homicides, and a full range of background aggregate-level factors from the 2000 U.S. Census (poverty level, household income, unemployment, population density, female-headed households, residential stability, racial heterogeneity, percentage of men and youths in the population).

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 that 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 multisite research in the area of police use of force. First, some LEAs do not systematically maintain use-of-force data. 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. During the preliminary/screening phase of our research, 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 although 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 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.[1] 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 trade-off 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, 1995; 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 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, we had 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 seriousness of injury, as opposed to a more limited analysis of whether an injury occurred or not (Smith et al., 2008).

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 (0 = *use of a random sample of cases* or 1 = *use of the population of cases*) to our logistic regression for each of our outcome measures. None of the new covariates were statistically significant nor did the other variables in the model change appreciably.

Data Collection/Measures

PERF requested electronic and hard copies of departmental data that included all use-of-force incident data (including cases with and without use of CED weapons by the police) for 4 years. Most of the analyzed data were from the years 2000 to 2005 for most of the sites.[2] The PERF team collected the force data in one of two ways: (a) a three-person research team was sent to the participating LEA to conduct onsite archival review and coding of use-of-force documents or (b) electronic use-of-force data maintained by the participating LEA were collected. Our team also collected crime and demographic data for each participating city. The sources of these data were the 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 CED, use of chemical spray, use of any other weapon, choke holds or hard hands, taking the suspect to the ground, and deployment of a canine.

From the above data sources, we created the measures for our statistical models. Our outcome measures included the following:[3]

1. Injuries: This was a dichotomous yes/no variable for any impairment of physical condition or pain to an officer (for the officer measure) or suspect (for the suspect injury measure) due to the officer's or suspect's actions, including

physical damage produced by the transfer of energy, such as kinetic, thermal, chemical, electrical, and radiant energy.

2. Injury severity: This was a dichotomous variable in which broken bones, stab wounds, and gun wounds were classified as severe and bruises, lacerations, burns, or punctures were classified as minor.
3. Injury from a force incident requiring medical attention: This was a yes/no variable indicating whether the officer (for the officer measure) or suspect (for the suspect medical attention measure) was seen by any type of medical professional, such as an on-scene emergency medical technician or medical personnel in a hospital, related to a use-of-force incident.
4. Injury from a force incident requiring hospitalization: This was a yes/no variable indicating whether an officer (for the officer measure) or suspect (for the suspect hospitalization measure) 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*, this does not require the person to be 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.

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. Although 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, & 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. Our independent/control variables included the following:

Agency deploys CEDs (1 = *yes* and 0 = *no*)
Suspect race (1 = *White* and 0 = *non-White*)
Suspect gender (1 = *male* and 0 = *female*)
Suspect age (1 = *below 25 years* and 0 = *above 25 years*)

Analytic Approach

As 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 pretreatment 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, & 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.

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 can potentially lead to biased estimates.

We analyzed our data 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* (Huber, 1967; White, 1980). This approach addresses the clustered nature of our data and produces unbiased estimates (Rogers, 1993; Williams, 2000; Wooldridge, 2002). We will be examining differences in the above outcomes after CEDs were implemented, controlling for any observed pretest differences in the comparison groups during the 2-year period before CEDs were implemented.

Results

To provide an overview of the data, we first present descriptive statistics on the raw comparisons of CED with non-CED sites. Later, we present our multivariate models comparing CED with non-CED sites controlling for a host of variables.

Descriptive Statistics

In Table 1, we present univariate results for each of our main outcome measures, comparing our pretest and posttest results for our CED sites and non-CED sites. Generally, our data suggest that the vast majority of officers are not injured in use-of-force cases (see Table 1). For the whole sample, CED and non-CED sites, our data suggest that 11% of officers were injured in use-of-force cases in the preperiod and 9% in the postperiod; and suspect injuries were more common in use-of-force cases (24% in the preperiod and 29% in the postperiod) than officer injuries. Our data suggest that medical attention for officer injuries (11% in the preperiod and 8% in the postperiod) was much less common than medical attention for suspect injuries (51% in the preperiod and 41% in the postperiod). Likewise, hospitalization for officer injuries (4.1% in the preperiod and 4.3%

Table 1. Descriptive Statistics for Non-CED Versus CED Sites and All Sites

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

Note: CED = conducted energy device; OC = oleoresin capsicum.

in the postperiod) was less common than hospitalization for suspect injuries (28% in the preperiod and 17% in the postperiod). Our data suggest that the proportion of officers receiving severe injuries (4.5% in the preperiod and 5.6% in the postperiod) was similar to the same measure for suspects (6.7% in the preperiod and 5.6% in the postperiod).

The proportion of White suspects was just less than one third for the whole sample for both time periods. The proportion of male suspects was more than 85% across both time periods for the whole sample. The proportion of suspects above 25 years of age was more than one third for the whole sample for both time periods. Our data indicate that the proportion of suspects using physical aggression against officers was about one third and suspects with a weapon at the force incident was less than 20% for the whole sample for both time periods. Our data indicate that use of only batons by officers is not common (2% or less for both time periods) but use of only OC spray is more common (about 10% in both time periods). We also found evidence that officer use of nonweapon force (e.g., hands-on tactics) is common in force incidents (49% in the preperiod and 37% in the postperiod).

Officer injuries. Our first chart (Figure 1) 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. Pre-CED implementation, our data suggest that 11.5% of the officers in the CED sites were injured in force cases compared with a similar proportion of officers in the non-CED sites (10.3%) over the same reference period, representing no statistical difference ($\chi^2 = 0.78$, $df = 1$, $p = .38$). However, we found that the CED sites observed a reduction in officer injuries (8.3%) after they began their deployment of CEDs, whereas the non-CED sites observed an increase in officer injuries to 20.3% ($\chi^2 = 52.68$, $df = 1$, $p < .001$).

Suspect injuries. Before the CED sites deployed CEDs, our data suggest that 22.8% of their suspects were injured in force cases compared with a slightly higher proportion of suspects in the non-CED sites (29.9%) over the same reference period, representing a statistically significant difference ($\chi^2 = 23.68$, $df = 1$, $p < .001$). The CED sites observed a small increase in suspect injuries (26%) after they began their deployment of CEDs, whereas the non-CED sites observed a much larger increase in suspect injuries to 42.5% ($\chi^2 = 102.02$, $df = 1$, $p < .001$). Although the CEDs started out at a slightly lower rate of suspect injuries compared with the non-CED sites (22.8% to 29.9%), our data suggest that the CED sites were substantially lower at the postperiod (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 with a lower proportion of officers in the non-CED sites (3.5%) over the same reference period, representing a statistically significant difference ($\chi^2 = 45.07$, $df = 1$, $p < .001$). The CED sites observed a large decrease in officers receiving medical attention for injuries (7.5%) after they began their deployment of CEDs, whereas the non-CED sites observed a large increase in officers receiving medical attention for injuries to 15.9% ($\chi^2 = 29.78$, $df = 1$, $p < .001$). Although the CEDs started out at a higher rate of officers receiving medical attention for injuries compared with the non-CED sites

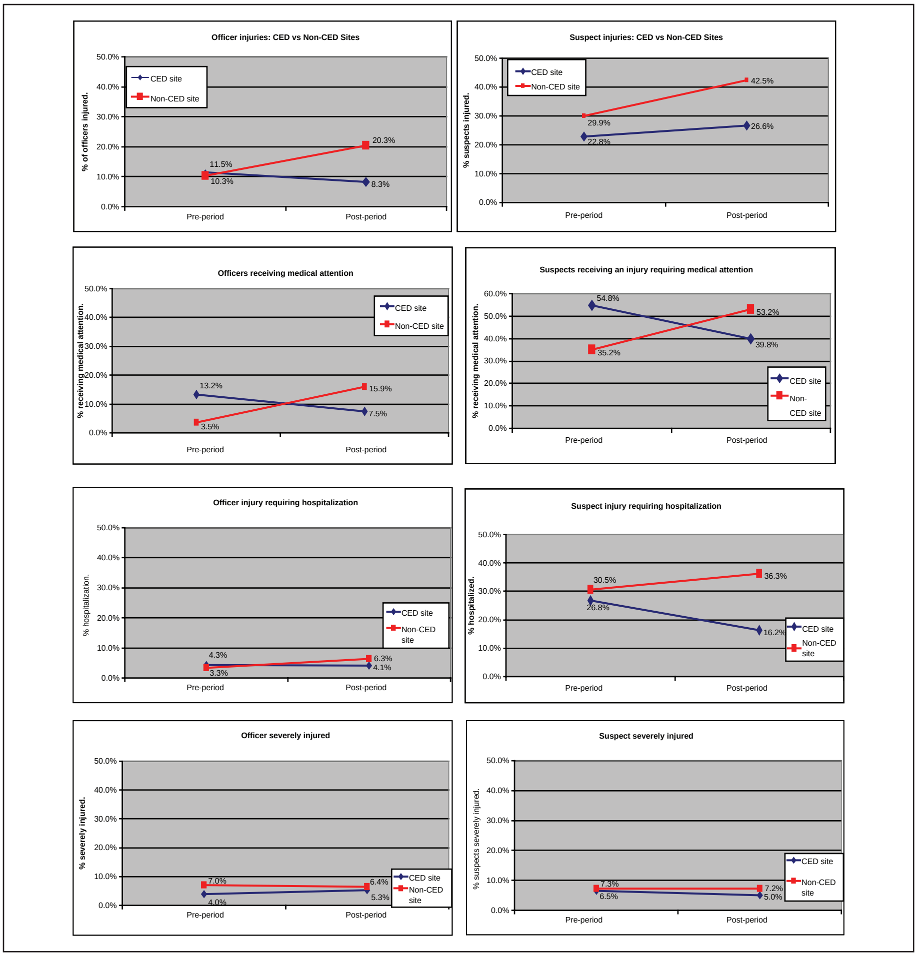

Figure 1. Bivariate results comparing outcomes for the CED and non-CED sites.

(13.2% to 3.5%), our data indicate that the CED sites were substantially lower in the postperiod (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 with a lower proportion of suspects in the non-CED sites (35.2%) over the same reference period, representing a statistically significant difference ($\chi^2 = 72.68$, $df = 1$, $p < .001$). The CED sites observed a large decrease in suspects receiving medical attention for injuries (39.8%) after they began their deployment of CEDs compared with the non-CED sites that observed a large increase in suspects receiving medical attention for injuries to53.2% ($^2 = 33.97$, $df = 1$, $p < .001$). Although the CEDs started out at a higher rate of suspects receiving medical attention for injuries compared with the non-CED sites (54.8% to 35.2%), our data suggest that the CED sites were substantially lower at the postperiod (39.8% to 53.2%).

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 with a similar proportion of officers requiring hospitalization in the non-CED sites (3.3%) over the same reference period, representing no statistical difference ($\chi^2 = 0.89$, $df = 1$, $p = .35$). The CED sites observed a very small decrease in officers requiring hospitalization for injuries (4.1%) after they began their deployment of CEDs compared with the non-CED sites that observed an increase in officer requiring hospitalization for injuries to 6.3% ($\chi^2 = 3.9$, $df = 1$, $p < .05$). The CEDs started out at a similar rate of officers requiring hospitalization for injuries compared with the non-CED sites (3.3% to 4.3%), but the CED sites were significantly lower at the postperiod (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 with a similar proportion of suspects requiring hospitalization in the non-CED sites (30.5%) over the same reference period, representing no statistical difference ($\chi^2 = 2.57$, $df = 1$, $p = .11$). The CED sites observed a large decrease in suspects requiring hospitalization for injuries (16.2%) after they began their deployment of CEDs compared with the non-CED sites that observed a small increase in suspects requiring hospitalization for injuries to 36.3% ($\chi^2 = 61.59$, $df = 1$, $p < .05$). The CEDs started out at a similar rate of suspects requiring hospitalization for injuries compared with the non-CED sites (26.8% to 30.5%), but our data suggest that the CED sites were significantly lower at the postperiod (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 with a similar proportion of officers in the non-CED sites (7%) over the same reference period, representing no statistical difference ($^2 = 1.32$, $df = 1$, $p = .25$). 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 with the non-CED sites that observed no change in officer severe injuries (6.4%) for the non-CED sites ($\chi^2 = 0.20$, $df = 1$, $p = .66$).

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 with a similar proportion of suspects in the non-CED sites (7.3%) over the same reference period, representing no statistical difference ($^2 = 0.23$, $df = 1$, $p = .63$). However, our data suggest that the CED sites observed a significant reduction in suspects' severe injuries (5%) after they began their deployment of CEDs compared with the non-CED sites that observed no change in suspect severe injuries (7.3%; $^2 = 3.75$, $df = 1$, $p < .05$). The CEDs started out at a similar rate of suspect severe injuries compared with the non-CED sites (6.5% to 7.3%), but our data suggest that the CED sites were significantly lower at the postperiod (5% to 7.3%).

Multivariate Analyses Using Logistic Regression Adjusting for Nested Standard Errors

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 models, we included the following independent/predictor variables: CED (whether the agency deploys CED: 1 = *yes*, 0 = *no*), time frame of incident (1 = *post-CED/comparable period*, 0 = *pre-CED/comparable period*), interaction CED × Time frame (1 = *CED* and postperiod), suspect race (1 = *White*, 0 = *non-White*), suspect gender (1 = *male*, 0 = *female*), and suspect age (1 = *less than 25 years,* 0 = *more than 25 years*). Each of our outcome models includes the entire time frame (both the pre-CED/comparable period and post-CED/comparable period). To assess our outcome measures during the relevant postperiod 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 (see Long, 1997) 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 postperiod 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 postperiod than agencies without CEDs, controlling for other factors.

As seen below (see Tables 2-5), four of the seven statistically significant results from the earlier bivariate models remained significant under our logistic regression with robust variance estimates (including the variables of officer injury, suspect severe injury, suspect medical attention, and officer medical attention). In all four cases, as reported earlier, CED agencies were associated with lower posttest rates of officer injuries, suspect severe injuries, suspects requiring medical attention for injuries, and officers requiring medical attention. Three of the seven positive results at the bivariate level rose above the .05 level of statistical significance at the multivariate level but remained in the predicted direction (that is, CED sites were still associated with fewer posttest suspect injuries and fewer suspects and officers requiring hospitalization from an injury than non-CED sites, but the result was no longer statistically significant).

Any injury. For our suspect injury logistic regression model, our results indicate that for an agency that deploys CEDs, the odds of a suspect being injured in the postperiod is not statistically reduced. That is, where we earlier reported a bivariate statistical difference, this difference is no longer statistically significant when we estimate the results with a logistic regression adjusting for nested standard errors ($\beta = -0.57$, odds ratio = 0.56, $p = .30$).[4] However, for our officer injury model, our results are still statistically significant

(text continued on p. 280)

Table 2. Logistic Regression Model for Injuries With a Correction for Nested Standard Errors

	Suspect injury				Officer injury			
	Coefficient	Robust *SE*	*z*	$p > \|z\|$	Coefficient	Robust *SE*	*z*	$p > \|z\|$
Agency deploys CED	−0.386	0.676	−0.57	.569	0.017	0.526	0.03	.975
Posttest period	0.774	0.523	1.48	.139	0.887	0.479	1.85	.064
Agency used CEDs in postperiod	−0.572	0.554	−1.03	.301	−1.203	0.512	−2.35	.019
Suspect White	0.413	0.188	2.19	.028	0.032	0.157	0.20	.838
Suspect male	0.548	0.136	4.03	.000	0.230	0.098	2.34	.019
Suspect below 25 years of age	0.142	0.037	3.82	.000	0.231	0.082	2.81	.005
Constant	1.450	0.429	−3.38	.001	−2.262	0.314	−7.22	.000
Number of observations		9,324				7,963		
Wald $\chi^2(6)$		138.30				52.22		
Prob > χ^2		0.0000				0.0000		
Pseudo R^2		.024				.0162		

Note: CED = conducted energy device.

Table 3. Logistic Regression Model for Medical Attention With a Correction for Nested Standard Errors

	Suspect medical attention				Officer medical attention			
	Coefficient	Robust *SE*	*z*	$p > \|z\|$	Coefficient	Robust *SE*	*z*	$p > \|z\|$
Agency deploys CED	0.836	0.635	1.32	.188	1.233	0.392	3.14	.002
Posttest period	0.799	0.402	1.99	.047	1.618	0.899	1.80	.072
Agency used CEDs in postperiod	−1.539	0.685	−2.25	.025	−2.036	0.955	−2.13	.033
Suspect White	0.420	0.107	3.92	.000	0.087	0.109	0.79	.427
Suspect male	0.410	0.178	2.30	.021	0.072	0.070	1.03	.304
Suspect below 25 years of age	−0.129	0.093	−1.39	.164	0.059	0.097	0.61	.539
Constant	−0.996	0.635	−1.57	.117	−3.212	0.253	−12.72	.000
Number of observations		7,696				5,303		
Wald $\chi^2(6)$		172.91				46.66		
Prob > χ^2		0.0000				0.0000		
Pseudo R^2		.0334				.0165		

Note: CED = conducted energy device.

Table 4. Logistic Regression Model for Hospitalization With a Correction for Nested Standard Errors

	Suspect hospitalization				Officer hospitalization			
	Coefficient	Robust *SE*	z	$p > \|z\|$	Coefficient	Robust *SE*	z	$p > \|z\|$
Agency deploys CED	−0.229	0.773	−0.30	.767	0.032	0.174	0.19	852
Posttest period	0.206	0.491	0.42	.675	0.390	0.409	0.95	.341
Agency used CEDs in postperiod	−0.732	0.704	−1.04	.299	−0.229	0.421	−0.54	.586
Suspect White	0.182	0.223	0.82	.415	−0.374	0.110	−3.40	.001
Suspect male	0.214	0.085	2.51	.012	−0.044	0.116	−0.38	.706
Suspect below 25 years of age	−0.274	0.113	−2.42	.016	−0.145	0.119	−1.22	.221
Constant	−0.853	0.623	−1.37	.171	−2.912	0.191	−15.24	.000
Number of observations		6,996				5,232		
Wald $\chi^2(6)$		173.45				26.81		
Prob > χ^2		0.0000				0.0002		
Pseudo R^2		.0211				.0051		

Note: CED = conducted energy device.

Table 5. Logistic Regression Model for Minor Versus Severe Injuries (Among Those Injured) With a Correction for Nested Standard Errors

	Suspect severe injuries				Officer severe injuries			
	Coefficient	Robust *SE*	z	$p > \|z\|$	Coefficient	Robust *SE*	z	$p > \|z\|$
Agency deploys CED	−0.134	0.503	−0.27	.791	−0.621	0.425	−1.46	.144
Posttest period	0.034	0.219	0.15	.877	−0.463	0.353	−1.31	.189
Agency used CEDs in postperiod	−0.582	0.257	−2.27	.023	0.561	0.482	1.17	.244
Suspect White	0.112	0.158	0.71	.480	−0.139	0.488	−0.29	.775
Suspect male	0.036	0.274	0.13	.896	−0.080	0.259	−0.31	.757
Suspect below 25 years of age	−0.136	0.113	−1.20	.231	−0.006	0.241	−0.03	.979
Constant	−2.505	0.602	−4.16	.000	−2.349	0.322	−7.29	.000
Number of observations		2,929				956		
Wald $\chi^2(6)$		57.53				12.16		
Prob > χ^2		0.0000				0.0584		
Pseudo R^2		.0125				.0046		

Note: CED = conducted energy device.

suggesting that for an agency that deploys CEDs, the odds of an officer being injured in the postperiod is reduced by 70% relative to agencies without CEDs (β = –1.20, odds ratio = 0.30, p = .019).

Medical attention for injuries. For our suspect medical attention logistic regression model, our results indicate that for an agency that deploys CEDs, the odds of a suspect needing medical attention for an injury in the postperiod is statistically reduced by 79% relative to agencies without CEDs (β = –1.54, odds ratio = 0.22, p = .02). For our officer medical attention model, our results indicate that for an agency that deploys CEDs, the odds of an officer needing medical attention for an injury in the postperiod is reduced by 87% relative to agencies without CEDs (β = –2.04, odds ratio = 0.13, p = .03).

Hospitalization required for injuries. For our suspect hospitalization logistic regression model, our results indicate that for an agency that deploys CEDs, the odds of a suspect requiring hospitalization for an injury in the postperiod is not statistically reduced (β = –0.73, odds ratio = 0.48, p = .29). 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 postperiod (β = –0.23, odds ratio = 0.79, p = .59).

Severity of injury (minor vs. severe). For our suspect injury severity logistic regression model, our results indicate that for an agency that deploys CEDs, the odds of a suspect receiving a severe injury in the postperiod is statistically reduced by 44% relative to agencies without CEDs (β = –0.58, odds ratio = 0.56, p = .02). For our officer injury severity model, our results indicate that there were no differences for agencies that deploy CEDs and agencies that do not deploy CEDs in terms of the odds of an officer receiving a severe injury in the postperiod (= 0.56, odds ratio = 1.75, p = .24).

Discussion

How LEAs manage their officers' 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 an LEA executive will have to make. One weapon that has received significant attention as a way to reduce injuries for officers and suspects is the CED. 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. The purpose of this study was to produce scientifically valid results that will help guide LEA executives' decisions regarding the use of CEDs.

Our study is one of the first to compare LEAs that use CEDs with 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 pretest period to the postperiod in those types of designs. We compared the differences in outcomes between police agencies that have incorporated the use of CEDs ($n = 7$) with a matched group of 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 factors.

Overall, we found that the CED sites were associated with improved safety outcomes when compared with a group of matched non-CED sites on four of eight safety measures, including reductions in (a) officer injuries, (b) suspect severe injuries, (c) officers receiving injuries requiring medical attention, and (d) suspects receiving an injury requiring medical attention. Three of the four other results, which were statistically significant under bivariate testing (see graphs in Figure 1), rose above the .05 level of statistical significance at the multivariate level. However, they remained in the predicted direction (i.e., CED sites were still associated with fewer posttest suspect injuries and fewer suspects and officers requiring hospitalization from an injury than non-CED sites, but the result was no longer statistically significant).

For the four statistically significant outcomes, the magnitude of the effects of the improved safety outcomes for the CED sites (relative to the non-CED sites) was impressive. For agencies that deploy CEDs, the odds of an officer being injured are reduced by more than 70%. For an agency that deploys CEDs, the odds of a suspect being severely injured are reduced by more than 40%. For an agency that deploys CEDs, the odds of an officer receiving an injury requiring medical attention is reduced by more than 85%. For an agency that deploys CEDs, the odds of a suspect receiving an injury requiring medical attention in the postperiod are reduced by more than 75%.

Although our study is one of the first to compare CEDs with matched non-CED sites, such QEDs are not without limitations. As mentioned earlier, 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 is 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, Boruch, Chambers, Rossi, & Witte, 1985; Boruch, McSweeny, & Soderstrom, 1978; Campbell & Stanley, 1963; Dennis & Boruch, 1989). 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. However, here our study was limited in that we only had a small number of covariates available for analysis across all of our sites. Nevertheless, we believe we have identified some important covariates that might confound our comparison of CED and non-CED sites, and we have used these measures to help 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).

There are some other concerns about our study, but we do not believe they interfere with the interpretation of the results we presented. There are some concerns with the specificity of our measures. That is, we would have preferred more detailed, interval-level outcome measures. Although this limits exploration of our data, this does not affect the validity of our dichotomous outcome analyses. In addition, we would have liked to have had all the use-of-force cases (i.e., the universe of cases) from all of our participating sites. However, this was not possible because some of the sites had only paper record forms (necessitating that a random sample be drawn). We analyzed this potential variation across sites and added variables to our statistical models on whether a sample or population data were used. This variable was nonsignificant in all of our statistical tests. Although it is preferable to work with population data, sampling is a well-accepted science and no apparent differences emerged between our sites that provided samples and population data.

Next, our results were limited to 4 years of data (2 years before and 2 years after implementation of CEDs). Once again, it would have been preferable to have a longer longitudinal period to assess the effects of CED implementation. Nevertheless, the shorter timeframe does not invalidate our results. Our project is simply providing short-term (as opposed to long-term) results. Another important point 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. 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.

Future research could also benefit from exploring some alternative outcome measures. We observed limitations in content of the data that were available from our participating agencies, for a number of other important areas of interest were not collected by the LEAs in our study or for legal reasons could not be shared with our team. For example, we were not able to collect liability data on law suits against the agencies, for many of the agencies either did not collect these data systematically or could not release results on cases pending litigation. We were not able to collect data on injuries to victims and bystanders because these data were missing from most of the use-of-force reports which focus on the activities of the officer and suspect. We could not get access to worker's compensation claims, for these data were considered private personnel matters and not available for analysis.

Funding agencies should also emphasize the need to triangulate the results of use-of-force form research (like what our team conduced and Smith and colleagues, 2008, used in their study) with other research methods. For example, Hickman and colleagues (2008) reviewed several dozen studies on nonlethal force and pointed out the merits of alternative methodologies, such as observational studies, household surveys, and suspect surveys. Given the inherent limitations present in each approach, the law enforcement community will be able to have even greater levels of confidence in the results from nonlethal force research if a number of these alternative methods are used and demonstrated to lead to similar results.

In designing this research, there also was little theoretical guidance that could be derived from the extant literature on police use of force. For example, the recent nonlethal force studies by Smith and colleagues (2007, 2008) or Jenkinson and colleagues (2006) were largely data-driven studies without an underlying theoretical test. Similarly, our study is not a theoretical test. Given the complexity of our data, a theoretical framework might have been helpful for synthesizing our findings. Nevertheless, we do acknowledge the work of Wilson (1975) and other later theorists (e.g., Handberg, Unkovic, & Feuerstein, 1986) in studying patrol officer behavior and its potential application to our research. That is, although our inclusion of multiple agencies in our study builds on Wilson's emphasis on the large degree of variation across agencies in the way officers performed their functions, because of the absence of data from our agencies, we did not measure this variation. We recognize that our absence of measures in this area is a limitation in our study.

Although not directly concerned with officer use of force, Wilson (1975, pp. 19-20) did recognize a link between levels of enforcement and order maintenance activities and officer exposure to negative citizen encounters and other dangers (Kaminski, 2002). In Wilson's (1975, pp. 145-146) "watchman-style" departments, patrol officers are encouraged to look past the "little stuff" or handle minor offenses informally, but "legalistic" departments encourage officers to handle routine situations as if they were matters of law enforcement rather than order maintenance and to intervene formally rather than informally by making arrests. In between these two styles are the "service style" agencies. In these agencies, officers are encouraged to intervene frequently but not formally (Wilson, 1975).

Research by Kaminski (2002) builds on Wilson's (1975) work, and he argues that agencies that fit Wilson's legalistic typology (or a similar aggressive typology where officers are encouraged to maximize the number of interventions and enforce the law) may put officers in more negative citizen-encounter situations. In these legalistic agencies, it could be argued that officers may be placed in more situations where force is needed or these agencies with their aggressive style may inadvertently encourage the use of force. Kaminski (2002) also argues that more "passive" agencies (watchman and service), where officers may less frequently stop suspicious persons or motor vehicles and respond more informally, may be less likely to have negative citizen encounters. Conversely, these agencies may place their officers in fewer situations that require force.

Anecdotally, we learned that we had variation in our participating agencies and likely had all three of Wilson's typologies represented in our study. However, we are not able to assess if there was a balance across our CED and non-CED study sites on Wilson's typology. The fact that we cannot measure Wilson's typologies or other typologies is a limitation of our study. Although this is a limitation, we do not believe that any typology differences in our CED and non-CED study sites explains the large effect sizes that emerged on our injury and related medical outcomes. Future research will need to be grounded in more explicit theoretical frameworks and associated measurement models. A theoretical framework could help provide greater insight into our findings.

Conclusion

On balance, the effect sizes evident in our results are substantively important and should be carefully considered by LEAs. For example, for agencies participating in this evaluation that deploy CEDs, our results suggest that the odds of an officer being injured in the postperiod are reduced by more than 70% relative to agencies without CEDs. Moreover, the effect sizes are generally large enough to suggest that even if the comparability of the CED and non-CED sites is not perfect (as could be possibly achieved with a randomized experiment), the odds are that there are still likely to be important safety gains for officers in agencies that deploy CED compared with those that do not.

Although research on police use of force consistently shows that most use-of-force encounters involve low levels of force and few, if any, injuries for officers and suspects, 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 of injury for officers and suspects (see Smith et al., 2008). According to our results, police devices such as CEDs 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 allow officers to control suspects from a distance without engaging in the hand-to-hand struggles that typically cause injuries. LEAs should consider the utility of the CED as a way to avoid hand-to-hand combative situations and reduce injuries to officers and suspects. 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 in our non-CED sites. Moreover, 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 one of our non-CED sites. As pointed out in the PERF (2005) 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.

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? 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. In addition, there is little attention in the CED literature to training of officers and sheriffs' deputies in the proper use of CEDs. Although 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 yet been done (see Smith et al., 2008).

Another issue that policing agencies may consider in light of this study is a phenomenon that has been called *weapon-option overload*. During our study, we learned anecdotally that some police practitioners are deeply concerned about officers having "too many tools on their belt," such as a CED, a collapsible baton or other impact weapon, OC spray or other chemical agent, and a heavy flashlight in addition to a firearm. Some departments have discontinued the carrying of OC spray because of its potential for affecting persons other than the intended one 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 should also trigger for the agency a decision on whether they may want to discontinue the use of other less lethal options to "make room" for the CED.

Author's Note

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Notes

1. Not only do agencies measure nonlethal force differently but also is there little consensus among researchers on how to measure nonlethal force (see Hickman et al., 2008, for a discussion of the various methods used by researchers to measure nonlethal force).

2. A few of the Conducted Energy Device (CED) and non-CED sites had data dating back to 1998 and as recent as 2007. However, we were able to achieve a balance between the CED and non-CED sites on the years for their analyzed data, with each CED site and its corresponding matched non-CED site being within about 1 year of each other.
3. Our article does not address the issue of whether deaths are connected to CEDs.
4. See Long (1997) for a discussion of odds ratios.

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