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# Synthesis of Studies That Relate Amount of Enforcement to Magnitude Of Safety Outcomes - Technical Appendix

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16. Abstract
This report covers the findings of a research project carried out by the Volpe Center on behalf of NHTSA and the Governors Highway Safety Association (GHSA) under the National Cooperative Research and Evaluation Program (NCREP), which identifies and funds research and evaluation projects for improving and expanding State highway safety countermeasures. One such topic identified is measuring the impact of various amounts of traffic enforcement on changes in safety outcomes. The project team identified 80 relevant studies for inclusion in the synthesis. Current literature only supported findings related to occupant protection enforcement. No relationship between levels of enforcement and safety outcomes could be identified for distracted driving, alcohol-impaired driving, speeding, or aggressive driving. However, for all targeted behaviors, the enforcement campaigns evaluated in the available literature were effective in improving safety outcomes even though the combination of these evaluations could not provide sufficient evidence to establish a relationship between the level of resources used and the magnitude of the safety improvement. This document is a technical appendix to the main report, *Synthesis of Studies that Relate Amount of Enforcement to Magnitude of Safety Outcomes*

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# 1 Executive Summary

This document covers the findings of a research project carried out by the Volpe Center on behalf of NHTSA and the GHSA under NCREP. This is the technical appendix to the main report. The focus of the research is to study the relationship between levels of enforcement (including publicity of those efforts) and safety outcomes. The available literature has not explored a relationship between the intensity or amount of enforcement and the magnitude of observed safety impacts. This research seeks to fill that information gap by investigating the research question: What is the impact of various amounts of enforcement on safety outcomes?

Analyzing the effectiveness of different levels of enforcement covers a wide range of potential topics. In consultation with NHTSA the decision was made to focus on enforcement that target occupant protection, distracted driving, alcohol-impaired driving, speeding, and aggressive driving. These driving behaviors are the among the most common focus of the grant funding provided under Sections 402 and 405 of Title 23, U.S. Code. The scope is further limited to enforcement of existing traffic laws through additional police officer activity and publicity of those activities. As such, it does not consider the effectiveness of changing traffic safety laws or the effectiveness of using new technologies for enforcement. For example, the impacts of graduated driving licensing or the effectiveness of red light cameras are outside of the scope of this research effort.

The initial literature review uncovered only a few studies that have addressed the impact of safety outcome from incremental amounts of enforcement (Elvik, 2001; Erke et al., 2009; Phillips et al., 2011; Shults et al., 2004; Bergen et al., 2014). None of the existing studies provided results relevant to this effort. First, these studies tended to use old data that could not be considered relevant to the current time-period. Second, the available studies related to countries with driving cultures and laws that are very different than those of the United States.

However, the existing studies provided a useful methodology and framework for this project. First, the existing studies take a meta-analysis approach, which is a methodology that involves examination of the results from individual studies of the same subject to determine overall trends. Second, they highlight a potential challenge of applying a meta-analysis approach to the study of the effectiveness of enforcement; namely, that evaluations of individual enforcement tend to use a variety of measures and metrics to describe both the safety outcomes and the intensity of the enforcement (Elvik, 2001). The variety of different measures and metrics makes comparing the results of individual studies challenging because they cannot be plotted or analyzed in the same terms.

The research team developed a methodology for this project that responded to the initial literature review. The research methodology focused on gathering information from existing evaluations and analyses described in the research literature culminating in a literature scan to identify all potentially relevant studies in the available literature. No new primary data collection was undertaken as part of this research.

The scope was limited to enforcement since 1990 and in the United States or Canada. In the end, however, no literature from Canada was included in this synthesis.

This technical appendix continues with a discussion of the strategies and techniques used to perform the literature search and a discussion of the screening process used to discern which of the over 15,000 potential studies were relevant to the research questions and should be included in the synthesis. The subsequent five chapters each focus on a single targeted behavior—occupant protection, distracted driving, alcohol-impaired driving, speeding, and aggressive driving. Finally, the conclusion summarizes the findings from each section as well as some avenues for future research.

In many cases, conclusions regarding the impacts of additional enforcement on safety outcomes could not be made. Thus, to provide additional information for understanding the expected change in safety outcomes for certain levels of enforcement effort, this report includes detailed tables describing past enforcement. The detailed tables can be used to support a case study approach whereby a reader can browse descriptions of past enforcement to find an example that is like a current context of interest and gain insight on appropriate amounts of enforcement to be used in that context. The detailed tables describe the context of the enforcement effort (location, time-period, baseline safety conditions), the enforcement effort itself (strategy, resources devoted to the effort), and the observed changes in safety outcomes.

# 2 Literature Search

This research examined the enforcement strategies and statistical findings from independent studies to identify trends and draw general conclusions. An electronic search of different search engines was performed between February 12 and May 31,st 2018, targeted on the areas mentioned above. In addition to the literature search results, forwards referencing was conducted between June 6 and June 22, 2018. The search covered material from 1990 to 2018.

The main area of interest was how enforcement affects a given safety outcome, where the resources used in the enforcement can be measured in scalable units (i.e., on a continuous spectrum). For example, the measures might be the dollars spent on publicity, the number of checkpoints per mile of roadway or per capita, the number of officers staffing checkpoints, or police deployment rates to represent visibility or presence. Likewise, the safety outcomes should also be measured on a continuous spectrum. The safety measures might relate to final outcomes such as avoided crashes or fatalities. Or they may relate to intermediate safety outcomes such as seat belt use rates, typical speeds, or percent of drivers driving while impaired.

Every effort was made to avoid bias by casting a wide net to identify all potentially available studies. The literature search included studies published in peer-reviewed journals, technical reports, conference proceedings, books, and unpublished manuscripts. Finally, the large collection of potentially relevant studies was then screened to eliminate non-relevant items before synthesizing the enforcement and safety outcome data extracted from relevant items.

## 2.1 Search Words

The study focused on efforts targeting the following behaviors: occupant protection, distracted driving, alcohol-impaired driving, speeding, and aggressive driving. Studies involving changes in law/legislation (examples: graduated driver licensing, driving penalty points, speed limits, blood alcohol concentration [BAC] limits) or technology (examples: red light cameras, photo radars, automated speed enforcement) were not included.

As part of the literature search methodology, the evaluation team developed a series of word groupings that were expected to be associated with each of those four broad concepts. First, the study must attempt to measure an impact. Thus, Group 1 terms are those referring to the methods to assess safety impacts. The search terms were Impact, Effect, Effectiveness, and Evaluation.

Second, the study must address the targeted behaviors in the scope of this research. Group 2 terms are those that refer to the behaviors the enforcement is trying to influence. The search terms were Speeding, Aggressive Driving, Distracted Driving, Cell Phone, Handheld Device, Texting, Impaired Driving, Drunk Driving, Drowsy Driving, Intoxicated Driving, Alcohol, Occupant Protection, Seat Belt, Helmet, Child Restraint, Car Seats, and Child Seats

Third, the study must relate to the type of enforcement that is in the scope of this research. Terms that refer to actual enforcement activities or expenditure on those activities are categorized under Group 3. The search terms were Enforcement, Patrols, Visibility, Police Presence, Spotters, Publicity, Campaign, Advertisement, Checkpoint, Breath Test, and Expenditure.

Finally, the study must provide a quantified measure of a safety outcome. Group 4 terms refer to safety outcomes. The search terms were Safety, Incidents, Crashes, Accidents, Fatalities/Deaths, Injuries, Property Damage, Deterrence, Seat Belt Usage, Speed, and BAC Test Results.

## 2.2 Search Engines

The literature scan was conducted using resources that represent a mix of human-curated databases and internet search engines.

1. Transportation Research Board (TRB) Transportation Research International Documentation (TRID) including Research in Progress (RiP)
2. Google Scholar Search
3. ScienceDirect
4. National Transportation Library's Repository and Open Science Access Portal (NTL-ROSAP)
5. NHTSA's Compendium of Traffic Safety Research Projects
6. SafetyLit

The exact search terms and Boolean operators differed according to the specifics of the user interfaces for each search engine and database. However, the searches were collectively exhaustive of the search terms listed above. In all, 10,814 studies were evaluated for relevance after the initial literature scan. The values presented below in Table 1 represent the full universe of studies considered for inclusion in this synthesis, separated by search engine.

*Table 1. Initial Count of Studies Considered for Inclusion*

| Source | Count of Studies |
|---|---|
| TRID/RiP | 880 |
| Google Scholar | 5,206 |
| ScienceDirect | 3,442 |
| NTL-ROSAP | 255 |
| Compendium | 552 |
| SafetyLit | 479 |
| TOTAL | 10,814 |

## 2.3 Levels of Screening

The search results were reviewed using four levels of screening. At each stage studies were rated as relevant, not relevant, or possibly relevant. Screening was carried out by three team members who reviewed studies across all safety outcomes; most studies were reviewed by at least two team members. Where screeners were not in agreement about whether to include a study, the entire research team met to review the study and collectively decide its relevance.

The Level 1 screen focused on filtering out research results based on titles and key words to determine whether the study investigated the impacts or effects of enforcement of any of the five behaviors. Table 2 presents the counts of studies, by search engine, that were deemed relevant or possibly relevant after completion of the Level 1 scan; 10,089 studies were found not to meet the relevance criteria of the Level 1 screen.

*Table 2. Count of Studies Remaining After Level 1 Screen Completion*

| Source | Count of Studies |
|---|---|
| TRID/RiP | 84 |
| Google Scholar | 230 |
| ScienceDirect | 298 |
| NTL-ROSAP | 29 |
| Compendium | 29 |
| SafetyLit | 55 |
| TOTAL | 725 |

The Level 2 screen involved carefully reading the *abstracts* to determine whether the study examined one of the five proscribed behaviors listed in the work plan (Group 2 terms) and whether it related enforcement activities to a safety outcome (i.e., Groups 3 and 4 terms). Examples of studies that were filtered out at this stage include those studies involving policy or technological enforcement strategies that are not the focus of this research, such as speed cameras, red light cameras, graduated driver licensing, or changes from secondary to primary seat belt enforcement. Table 3 presents the counts of studies, by search engine, that were deemed relevant or possibly relevant after completion of the Level 2 scan; 378 studies were found not to meet the relevance criteria of the Level 2 screen.

*Table 3. Count of Studies Remaining After Level 2 Screen Completion*

| Source | Count of Studies |
|---|---|
| TRID/RiP | 76 |
| Google Scholar | 145 |
| ScienceDirect | 46 |
| NTL-ROSAP | 26 |
| Compendium | 29 |
| SafetyLit | 25 |
| TOTAL | 347 |

The Level 3 screen involved combining all results from the search engines and removing the duplicate entries. In addition, studies were restricted to the United States and Canada. Additionally, at this stage, the text was scanned to ensure the study included quantitative measures of enforcement activities and outcomes (i.e., Groups 3 and 4 terms). Examples of studies that were filtered out at this stage include those that were strictly qualitative discussions of enforcement, During Level 3 screening, the studies were combined and no longer disaggregated by search engine. Following the Level 3 screen, 37 studies were found to have appeared in several search engines, 82 studies were excluded based on geographic focus, and 86 studies were excluded by scanning the article for Level 3 screening criteria. Thus, the remaining 142 studies met or possibly met Level 3 screening criteria.

To identify all relevant literature, the team employed both backwards and forwards referencing. Backwards referencing refers to examining all literature cited by the relevant studies, checking whether they have already been included in the retrieved literature, and, if not, adding them to the analysis. The team's initial efforts indicated that the additional studies identified through backwards referencing were older studies, not within the time frame of interest. As such the research team's efforts shifted to forwards referencing.

Forwards referencing refers to examining all literature that cites the identified relevant studies. The team used Google Scholar's "Cited by" feature to perform forwards referencing to all the studies identified as relevant from the six search engines. Studies that met all relevance criteria were added to the analysis. The same screening steps were performed on the studies identified through forwards referencing. Combining the original and forwards referencing study results and dropping the duplicates, 201 of the 228 possible studies met the Level 3 screening criteria. Table 4 describes the study counts by screening level.

*Table 4. Count of Studies Identified Through Forwards Referencing at Levels of Screening*

| Screening Level | Count of studies |
|---|---|
| Initial Scan | 4,440 |
| Level 1 | 237 |
| Level 2 | 117 |
| Level 3 | 59 |

The Level 4 screen, or the final screen, involved a complete reading of the articles, including supplemental information. Data extraction was conducted at the same time, and studies were grouped according to targeted behavior. At this stage, studies that described general enforcement that affected targeted behaviors were grouped into a separate, non-specific enforcement section. Eighty studies were identified as meeting Level 4 screening criteria.

A total of 10,814 articles from the six search engines listed above were identified from the literature search and an additional 4,440 articles were identified through the forwards referencing search; in total 15,254 studies were screened for inclusion in this synthesis. After several levels of screening, based first on title and key words, then abstracts, and finally the entire text of the study, 80 studies were deemed relevant for inclusion in this literature synthesis.

# 3 Occupant Protection

This chapter reviews the 38 relevant studies on occupant protection enforcement programs in the United States. Each study varied in terms of time-period, location, method of enforcement, presence of publicity activities (such as paid media), and analysis techniques. A general overview of the studies is given in the Description of Occupant Protection Evaluation Studies section. The next section (Methods of Enforcement) discusses the different methods used in the programs reviewed within the chapter, the following section (Publicity) discusses the different types of other program activities (such as paid media, earned media, and slogans), and the next section (Safety Outcomes) explains the different ways that studies measure safety outcomes. The section titled Relationship between Enforcement and Safety Outcomes analyzes the studies in various subsections and attempts to quantify, where possible, the effect of enforcement and media on seat belt use. The Conclusion section summarizes the main findings.

## 3.1 Description of Occupant Protection Evaluation Studies

This chapter focuses on the effect of police enforcement strategies on occupant protection. Occupant protection generally means seat belt use; however, a few studies in this chapter also looked at the use of child safety seats, as well as general seat belt use. The majority of these 38 studies are before-after studies that did not use control locations. The studies that do have control locations are discussed in a separate section.

Nearly half of these studies looked at the effects of *Click It or Ticket* (*CIOT*) campaigns. As will be discussed more fully later, *CIOT* is a Selective Traffic Enforcement Program (STEP) that uses HVE to increase seat belt use. Many of the studies that did not look at *CIOT* examined other campaigns that also used HVE and/or were STEP programs, as these are common strategies promoted by NHTSA to improve occupant protection.

The studies varied widely by the target location. Studies that looked at *CIOT* campaigns, for example, looked at very large areas—a State, several States, or the entire United States Other studies focused on smaller areas such as a city or county level program. One study (Elliotet al/, 2014) went narrower, focusing on one stretch of roadway. The studies also vary by time period. The review includes studies from 1990 to 2017, thus allowing for studies to vary by over two decades. The oldest study included in this review is by Decina et al. (1994), which focused on a child safety seat and seat belt program in two communities in Philadelphia. The most recent study included in this review is by Thomas et al. (2017), although this study looked at slightly older data from 2007 to 2009 to evaluate a nighttime seat belt program in Washington. Further details about individual studies are presented in the subsection titled Relationship between Enforcement and Safety Outcome."

## 3.2 Methods of Enforcement

Occupant protection campaigns use a variety of enforcement methods. The main strategies for occupant protection campaigns are HVE and STEPs, which often use checkpoints, safety zones, and/or saturation patrols. This section presents brief explanations of how some of these strategies work in occupant protection campaigns.

The first location in the United States to use an occupant protection STEP was Elmira, New York, in 1985. Since then, NHTSA has funded many STEPs across the United States, including the *CIOT* campaign (Solomon et al., 2007). While a STEP does not have to be an HVE program, STEPs are typically HVE programs. The most well-known STEPs such as *CIOT* are also HVE programs because they use enforcement, visibility elements, and media strategies. The specific type of enforcement used in a STEP can vary, and each of the enforcement strategies, discussed in the remainder of this section, can be a part of a STEP.

Checkpoints are more commonly used to check driver sobriety than to check seat belt use, and some States have laws that prevent law enforcement agencies from conducting seat belt checkpoints. However, several of the studies examined use checkpoints, sometimes called "safety checkpoints," as a method of enforcing seat belt use.

While drivers at checkpoints can be stopped even if no safety violation has been spotted, safety zones require a violation to pull over a driver. In States with secondary seat belt laws these zones tend to focus on safety more broadly, as opposed to just occupant protection, since officers are not allowed to pull someone over if the only noticeable violation is failure to wear a seat belt (Solomon et al., 2009).

## 3.3 Publicity

Most enforcement campaigns included a publicity component, which supplements enforcement. These activities help raise awareness of targeted enforcement activities and inform the public of the importance of wearing a seat belt. While not part of enforcement activities, like checkpoints or patrols, these publicity activities are a component of the enforcement campaign and likely have an impact on the observed safety outcome. Due to media inclusion in these campaigns, the study results cannot be attributed only to increased enforcement. Rather, the results are likely due to both enforcement and media activities as part of the HVE model. The following is a list of studies by publicity activities.

***Paid Media (television, radio, billboards, etc.) – 36 studies***

Agent et al., 2003, 2005, 2006, 2007, 2008; Agent and Green, 2004; Amiotte et al., 2016; Eby and Vivoda, 2003, 2004; Eby et al., 2003; Kim and Yamashita, 2003; Ledingham et al., and Preusser, 2009; Morgan, 2015; Nichols et al., 2007; Nichols et al., 2009; Nichols et al., 2011; Nichols et al., 2016; Solomon et al., 2002; Solomon et al., 2007; Solomon et al., 2007; Solomon et al., 2009; Solomon et al., 2009; Solomon et al., 2013; Thomas et al., 2011; Thomas et al., 2017; Tison et al., 2008; Tison and Williams, 2010; Turner and Alex, 2002; Vasudevan et al., 2009; Vivoda et al., 2004; Vivoda et al., 2007; Vivoda et al., 2007; Williams et al., 1994; Williams et al., 1996; Williams et al., 1997; Williams et al., 2000.

***Slogans (e.g., "*****Click It or Ticket*****") – 35 studies***

Agent et al., 2003, 2005, 2006, 2007, 2008; Agent and Green, 2004; Chaudhary et al., 2005; Eby and Vivoda, 2003, 2004; Eby et al., 2003; Kim and Yamashita, 2003; Ledingham et al., 2009; Morgan, 2015; Nichols et al., 2007; Nichols et al., 2009; Nichols et al., 2011; Nichols et al., 2016; Solomon et al., 2002; Solomon, Chaffe, and Cosgrove, 2007; Solomon et al., 2007; Solomon, Preusser, Tison, and Chaudhary, 2009; Solomon et al., 2013; Thomas et al., 2011; Thomas et al., 2017;Tison et al., 2008; Tison and Williams, 2010; Turner and Alex, 2002; Vasudevan et al., 2009; Vivoda et al., 2004; Vivoda, Eby, et al., 2007; Vivoda, St. Louis, et al., 2007; ; Williams et al., 1994; Williams et al., 1996; Williams et al., 1997; Williams et al., 2000.

***Earned Media – 28 Studies***

Agent et al., 2003, 2005, 2006, 2007, 2008; Agent and Green, 2004; Chaudhary, Alonge, and Preusser, 2005; Decina et al., 1994; Eby and Vivoda, 2003, 2004; Eby et al., 2003; Elliot et al., 2014; Nichols et al., 2007; Nichols et al., 2009; Nichols et al., 2011; Nichols et al., 2016; Solomon et al., 2002; Solomon, Chaffe, and Cosgrove, 2007; Solomon et al., 2007; Solomon, Chaffe, and Preusser, 2009; Solomon, Preusser, Tison, and Chaudhary, 2009; Solomon et al., 2013; Thomas et al., 2017; Tison et al., 2008; Tison and Williams, 2010; Turner and Alex, 2002; Vasudevan et al., 2009; Williams et al., 2000.

***Awareness Surveys – 27 Studies***

Agent et al., 2003, 2005, 2006, 2007, 2008; Agent and Green, 2004; Chaudhary et al., 2005; Elliot et al., 2014; Ledingham et al., 2009; Nichols et al., 2007; Nichols et al., 2009; Nichols et al., 2011; Nichols et al., 2016; Solomon et al., 2002; Solomon, Chaffe, and Cosgrove, 2007; Solomon et al., 2007; Solomon, Chaffe, and Preusser, 2009; Solomon, Preusser, Tison, and Chaudhary, 2009; Solomon et al., 2013; Thomas et al., 2017; Tison et al., 2008; Tison and Williams, 2010; Turner and Alex, 2002; Vasudevan et al., 2009; Williams et al., 1994; Williams et al., 1996; Williams et al., 2000.

***Public Service Announcements (PSAs) – 8 Studies***

Chaudhary et al., 2005; Decina et al., 1994; Kaye et al., 1995; Nichols et al., 2007; Solomon et al., 2002; Thomas et al., 2017; Turner and Alex, 2002; Vasudevan et al., 2009.

***Trainings and Education Programs – 2 Studies***

Turner and Alex, 2002; Williams et al., 1997.

## 3.4 Safety Outcomes

Seat belt use rates are the most common way that occupant protection studies measure safety outcomes. Use rates are typically measured by observing certain road segments, recording how many vehicle occupants are or are not wearing seat belts during a certain observation period, and calculating a percentage. The seat belt use rate may be specific to where in the vehicle the occupant is sitting (driver, front seat passenger, back seat passenger), the type of vehicle (passenger vehicles, large trucks), or the time of day (daytime, nighttime). A few of the examined studies used data from the National Occupant Protection Use Survey (NOPUS), which is conducted annually by NHTSA's National Center for Statistics and Analysis and is the only nationwide survey that provides probability-based observed data on seat belt use rates. The results are based on observations at randomly selected roadway sites without stopping or interviewing any vehicle occupants (NHTSA, 2018).

Many studies use their own observations to get more accurate estimates of seat belt use in their study areas at specific times. Several studies conducted their own observations of seat belt use to differentiate between daytime and nighttime seat belt use. Other studies distinguished between drivers and passengers, or between cars and trucks (the specific distinctions and terminology varied based on the study). Many, but not all, of the studies report the number of observations in their seat belt surveys. Despite the differences in the precise metrics for seat belt use, almost all of the occupant protection studies included in this section had at least some measure of seat belt use rates in their study area. All studies that reported seat belt use in this review used observational data to measure use as opposed to self-reported data.

Crashes are a less common measure of safety in occupant protection studies, yet there are a few in this review that included various measures of crashes. These studies measured total crashes, total injury crashes, total injuries, total fatal crashes, and total fatalities. Unfortunately, they did not focus on injuries to unbelted occupants.

### 3.5 Relationship Between Enforcement and Safety Outcomes

Ideally, all included studies would provide comprehensive information on all the resources used as part of an enforcement effort. Such a description would include: number of officer enforcement hours by type of enforcement (patrols, checkpoints, safety zones, etc.); cost of those enforcement hours (i.e., officer wages, cost of overtime patrols, etc.); number of checkpoints, safety zones, or patrols; amount of paid media measured in number of airings; cost of the paid media; amount of earned media; etc.

Unfortunately, the descriptions of the intensity of the enforcement available in the literature were often incomplete. That lack of detailed information negatively affected the ability to identify a quantitative relationship between amount or intensity of enforcement and magnitude of changes in safety outcomes. Nonetheless, certain insights were obtained.

In the following sections, studies have been grouped for analysis based on the type of enforcement analyzed and the metrics used to describe the intensity of the enforcement, Some studies appear in several sections. To account for population differences across geographic locations, the measures of the sizes of the enforcement, including media costs, have been normalized and are presented per 1,000 residents, with population estimates taken from the Census Bureau (see Appendix A).

It is also important to remember throughout this synthesis that each program is often comprised of several elements. The analysis attempts to account for this where possible, but it is difficult to separate out the effect of different aspects of a campaign. However, given that most studies discussed in this section fall under the category of HVE, positive results can broadly be interpreted as an indication that HVE is a successful strategy for occupant protection programs, even when it is impossible to separate out the effects of the different elements of the program.

Finally, some of the studies report decreases in seat belt use after the enforcement campaign. This does not necessarily mean that those campaigns (or that enforcement campaigns in general) were ineffective. Rather, simple random variation would suggest that occasionally, results will show a decrease in seat belt use (regression to the mean). This could be because outside factors were causing seat belt use to decrease. Potentially, the enforcement campaign helped reduce the amount by which seat belt use fell, but the campaign was not able to fully counteract the other factors causing seat belt use to decrease.

The remainder of this chapter will discuss findings related to specific measures of enforcement (officer enforcement hours, number of checkpoints and amount of media spending) followed by summaries of the impacts of specific types of enforcement campaigns (*Click it or Ticket*, *Buckle Up Kentucky*, and nighttime enforcement). The last section discusses the results of studies that included a control location when analyzing the impact of an enforcement effort.

### 3.5.1 Officer Enforcement Hours

To describe the intensity of an enforcement effort, 10 studies provided the number of hours that officers spent conducting activities within the various enforcement programs. These officer enforcement hours only cover the additional hours of enforcement used specifically for the enforcement program—these hours do not account for the baseline level of hours used during routine law enforcement activities. Conclusions only relate to these additional officer enforcement hours, and they are not meant to provide analysis of regular hours of enforcement.

These 10 studies covered 20 different locations, had different durations of enforcement, and used different enforcement strategies (patrols, checkpoints, etc.). But, all of them reported information on hours worked and an observational measure of seat belt use. These studies can therefore be analyzed to explore the relationship between additional officer enforcement hours and changes in seat belt use. General background information on these studies can be found in Table 5.

*Table 5. Occupant Protection Studies That Quantified Hours of Enforcement*

| **Study** | **Location** | **Dates** | **Type of enforcement** |
|---|---|---|---|
| Elliot et al., 2014) | Roanoke Corridor, 24-mile stretch of Blue Ridge Parkway, Virginia | Spring 2010; Fall 2010 | Saturation Patrols |
| Nichols et al., 2007) | Illinois, Indiana, Ohio, Minnesota, Michigan, and Wisconsin | May 2005 | Enforcement Zones<br>Patrols |
| Nichols et al., 2011) | Colorado and Nevada | September 2007 to September 2008 | Enforcement Zones<br>Saturation Patrols |
| Solomon et al., 2009) | Asheville and Greenville, North Carolina;<br>Charleston, West Virginia | 2007 | Checkpoints<br>Saturation Patrols |
| Thomas et al., 2017) | Washington | May 2007 to May 2009 | Nighttime Patrols |
| Tison et al., 2008) | Nationwide | May 2006 | Checkpoints |
| Tison & Williams (2010) | Nationwide | 2003 to 2006 | Checkpoints |
| Vasudevan et al., 2009) | Nevada | 2003 to 2005 | Not Specified |
| Williams et al., 1994) | Elizabeth City, Haywood County, and High Point, North Carolina | 1993 | Checkpoints |
| Williams et al., 2000) | Elmira, New York | October 4 to 22, 1999 | Checkpoints |

All studies reported both dedicated officer enforcement hours and seat belt use pre- and post-enforcement; some studies also reported the total cost of paid media. Paid media can have an important effect on enforcement campaigns and needs to be considered when examining the results of any campaign. Overall, the enforcement produced positive changes in seat belt use: 17 of the 19 enforcement locations resulted in positive changes in the seat belt use rate. In those locations where the impact was positive, the use rates increased between 0.3 percent and 30.4 percent. For the two locations where the impact was negative, the magnitude was small, roughly 1 percent. Table 6 reports the dedicated hours of enforcement, total span of time, cost of paid media, and change in the seat belt use rate for these studies ordered by the percent change, starting with the largest positive change.

*Table 6. Results From Occupant Protection Studies That Quantified Officer Enforcement Hours*

| Location (study) | Dedicated hours of enforcement per 1,000 residents | Number of weeks/number of waves | Paid media $ per 1,000 residents in 2018 $ | Seat belt use pre-program | Seat belt use post-program | Change (pre- to post-) |
|---|---|---|---|---|---|---|
| Elmira, NY (Williams et al., 2000) | 0.02 | 2 weeks/ 1 wave | Not specified | 69.0% | 90.0% | 30.4% |
| Nevada (Vasudevan et al., 2009) | 3.06 | 12 weeks/ 3 waves | $322.62 | 74.9% | 94.8% | 26.6% |
| High Point, NC (Williams et al., 1994) | 2.79 | 3 weeks/ 1 wave | $1,115.40 | 65.0% | 78.0% | 20.0% |
| Elizabeth City, NC (Williams et al., 1994) | 10.24 | 3 weeks/ 1 wave | $4,765.81 | 69.0% | 79.0% | 14.5% |
| Wisconsin (Nichols et al., 2007) | 5.85 | 2 weeks/ 1 wave | $113.65 | 65.6% | 73.3% | 11.7% |
| Nevada (Nichols et al., 2011) | 2.59 | 8 weeks/ 4 waves | $221.31 | 79.0% | 87.0% | 10.1% |
| U.S. (Tison & Williams, 2010) | 5.90 | 8 weeks/ 3 waves | $346.78 | 75.0% | 82.0% | 9.3% |
| Blue Ridge Parkway, Virginia (Elliot et al., 2014) | 0.02 | 4 weeks/ 2 waves | No paid media | 82.5% | 90.1% | 9.2% |
| Colorado (Nichols et al., 2011) | 1.40 | 14 weeks/ 4 waves | $191.76 | 72.0% | 77.0% | 6.9% |

| Location (study) | Dedicated hours of enforcement per 1,000 residents | Number of weeks/number of waves | Paid media $ per 1,000 residents in 2018 $ | Seat belt use pre-program | Seat belt use post-program | Change (pre- to post-) |
|---|---|---|---|---|---|---|
| Indiana (Nichols et al., 2007) | 2.99 | 4 weeks/ 2 waves | $265.18 | 76.3% | 81.2% | 6.4% |
| Minnesota (Nichols et al., 2007) | 1.60 | 2 weeks/ 1 wave | $164.16 | 78.1% | 82.6% | 5.8% |
| Illinois (Nichols et al., 2007) | 1.93 | 4 weeks/ 2 waves | $164.16 | 83.5% | 88.3% | 5.7% |
| Michigan (Nichols et al., 2007) | 4.51 | 2 weeks/ 1 wave | $126.27 | 89.4% | 93.2% | 4.3% |
| Ohio (Nichols et al., 2007) | 9.63 | 4 weeks/ 2 waves | $618.75 | 75.5% | 78.7% | 4.2% |
| Asheville, NC (Solomon et al., 2009) | 19.75 | 8 weeks/ 4 waves | $1,894.76 | 86.3% | 89.4% | 3.6% |
| Greenville, NC (Solomon et al., 2009) | 16.04 | 8 weeks/ 4 waves | $1,030.34 | 89.6% | 89.9% | 0.3% |
| Washington (Thomas et al., 2017) | 4.37 | 10 weeks/ 5 waves | $249.56 | 95.4% | 95.7% | 0.3% |
| Charleston, WV (Solomon et al., 2009) | 47.62 | 8 weeks/ 4 waves | $1,985.91 | 72.7% | 71.9% | -1.1% |
| U.S. (Tison et al., 2008) | 2.08 | 2 weeks/ 1 wave | $107.83 | 82% | 81% | -1.2% |

Although the table above provides a helpful overview of the results, it does not provide a clear answer as to whether the size or intensity of the enforcement effort affected the magnitude of the change in seat belt use. To explore whether the magnitude of the change in seat belt use rates could be linked to the size or intensity of the enforcement effort, a linear regression was used. The regression equation is shown below in Equation 1.

$$Y_{ij} = \beta_1 X_{1,ij} + \beta_2 X_{2,ij} + \beta_3 X_{3,ij} + \beta_4 X_{4,ij} + \gamma_1 D_{1,ij} + \gamma_2 D_{2,ij} + \gamma_3 D_{3,ij} + \gamma_4 D_{4,ij} + \gamma_5 D_{5,ij} + \varepsilon_{ij} \quad (1)$$

Where,

- the index $i$ = individual observations and $j$ = studies
- Y, the dependent variable, is the percentage-point change in seat belt use;[1]
- $X_1$ is seat belt use pre-enforcement. It is included to control for the possibility that at higher levels of seat belt use, additional improvements are likely more difficult to produce;
- $X_2$ is the amount of paid media (normalized by population) and is included to examine whether the amount of resources used in the supplementary enforcement effort impacts the success of the effort in improving safety outcomes;
- $X_3$ is the number of dedicated officer enforcement hours (normalized by population) and is the critical variable of interest that is included to examine whether the number of hours spent on the enforcement effort by law enforcement personnel impacts the success of the effort in improving safety outcomes;[2]
- $X_4$ is a time trend related to the year that the enforcement effort occurred, with efforts in 1990 coded as zero, efforts in 1991 coded as 1, etc. It is included to control for external factors that could be changing over time and influencing the results of these studies;
- $\beta_1$, $\beta_2$, $\beta_3$, and $\beta_4$ are (slope) coefficients to be estimated;
- $\gamma_1$ through $\gamma_5$ are coefficients to be estimated on indicator variables taking values 1 or 0 for each of the following categories of enforcement, $D_1$ for checkpoints only, $D_2$ for patrols only, $D_3$ for checkpoints and patrols, $D_4$ for safety zones, and $D_5$ for unspecified. These categories are mutually exclusive and collectively exhaustive of all the results included in this regression, and as such the model does not contain an intercept (alpha). The resulting estimates can be used to determine if different types of enforcement produce differing safety outcomes; and
- $\varepsilon_{ij}$ is the error term which is decomposed into a cluster specific error term (indexed with "$j$" for each study) and individual observation level error term (indexed with "$i$" for individual observation). In some cases observations are derived from the same study. Observations from the study may share similar unobserved attributes. If so, the observation specific error terms are not independently distributed, and estimates derived from Ordinary Least Squares (OLS) are still unbiased but variances are incorrect. The regression results below incorporate robust standard errors to produce correct estimates of the precision of the coefficient estimates.

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[1] It is possible to create a result such that the model predicts a change that would push seat belt use above 100 percent. To help avoid such a scenario, this model, and any model in this chapter, should only be used in similar contexts as those of the original studies.

[2] Note that X(2) and X(3) were tested for multicollinearity (that is, they were tested to see if media and hours were strongly correlated) using the variance inflation factor. The variance inflation factor values for media and hours were both below 2, which is low enough to conclude that the variables are not highly correlated.

Because these studies often involved waves of enforcement accompanied by appropriate descriptive data, it was possible to analyze the data at wave-level of detail. Not every study provided appropriate wave-level descriptive data, but, in total, 40 wave-level observations were available. The results of the linear regression involving the 40 available results can be found in the table below.

*Table 7. Occupant Protection Regression Results for Officer Enforcement Hours*

| **Independent Variables** | **Coefficient Estimate** | **Robust Std. Error** | **t-Value** | **p-Value** | **95% Confidence Interval** | |
|---|---|---|---|---|---|---|
| $\hat{b}_1$ (Use of Checkpoints) | 21.61396 | 4.64971 | 4.65 | 0.002 | 10.61914 | 32.60878 |
| $\hat{b}_2$ (Use of Patrols) | 21.16368 | 5.35290 | 3.95 | 0.006 | 8.50608 | 33.82127 |
| $\hat{b}_3$ (Use of Checkpoints and Patrol) | 19.94479 | 4.96252 | 4.02 | 0.005 | 8.21030 | 31.67928 |
| $\hat{b}_4$ (Use of Safety Zones) | 20.80579 | 4.35261 | 4.78 | 0.002 | 10.51350 | 31.09809 |
| $\hat{b}_5$ (Use of Unknown Enforcement Tactics) | 20.42836 | 4.32989 | 4.72 | 0.002 | 10.18980 | 30.66691 |
| $\hat{b}_6$ (Seat Belt Use Pre-Enforcement) | -0.14634 | 0.05515 | -2.65 | 0.033 | -0.27675 | -0.01593 |
| $\hat{b}_7$ (Media $ (in 2018 $)/Thousand Residents) | 0.00019 | 0.00089 | 0.21 | 0.838 | -0.00192 | 0.00230 |
| $\hat{b}_8$ (Hours of Enforcement/Thousand Residents) | -0.09629 | 0.21680 | -0.44 | 0.670 | -0.60894 | 0.41636 |
| $\hat{b}_9$ (Time Trend) | -0.39337 | 0.30483 | -1.29 | 0.238 | -1.11417 | 0.32743 |

*Note*. R-squared was 0.7034. Number of observations was 40. Robust standard errors, adjusted for the 8 studies. Population estimates were taken from United States Census Bureau (via Google Population Search Module) in March 2019. See Appendix A for more information.

The linear regression found no statistically significant relationship between officer enforcement hours and percentage-point changes in seat belt use rates. However, recall that overall, these studies resulted in positive increases in seat belt use, indicating that these campaigns were effective. The regression result simply indicates that the number of hours spent on this dedicated enforcement do not explain the magnitude of the effectiveness. Additionally, these officer enforcement hours are only the overtime hours specifically associated with the campaign being analyzed in the study. Officers are still doing routine enforcement activities, and the studies do not report the number of hours spent on these routine activities. For this subset of studies, the impact of paid media was also not statistically significant.

In the absence of identifying a statistical relationship between the size of the enforcement effort and safety outcomes, Table 6, as previously explained, shows the results by study, and it provides information on the length of the enforcement effort, number of dedicated hours of enforcement per capita, and paid media spending per capita that may suggest an effect size for similar types of programs.

### 3.5.2 Checkpoints

Thirteen studies included data on the number of checkpoints used in their campaigns. Background information on the studies that provided data on the number of checkpoints can be found in the table below.

*Table 8. Occupant Protection Studies That Quantified Checkpoints*

| **Study** | **Treatment Locations** | **Number of Enforcement Waves** | **Dates of Enforcement** |
|---|---|---|---|
| Chaudhary et al., 2005 | Reading, PA | 1 Wave | September 2004 |
| Decina et al., 1994 | Tredyffrin Township, PA | Not Specified | 1991 |
| | Haverford Township, PA | | |
| Ledingham et al., 2009) | Part of Queens in New York City | 4 Waves | June 2007–April 2008 |
| Nichols et al., 2016 | Oklahoma City and Tulsa designated market areas, OK | 6 Waves | November 2011–August 2013 |
| | Nashville, Memphis, and Chattanooga DMAs, TN | | |
| Solomon et al., 2009 | Asheville, NC | 4 Waves | 2007 |
| Tison et al., 2008 | All of United States | 1 Wave | May 2006 |
| Tison & Williams, 2010 | All of United States | 4 Waves | 2003–2006 |
| Turner & Alex,2002 | Alabama | 1 Wave | April–June 2002 |
| Williams et al., 1996 | North Carolina | 2 Waves | November 1993–July 1994 |
| Williams et al., 1997 | Durham, NC | 1 Wave | March–April 1996 |
| Williams et al., 2000 | Elmira, NY | 1 Wave | October 4–22, 1999 |

All studies reported both the number of checkpoints used in the enforcement effort and observational seat belt use pre- and post-enforcement, but some studies also reported the total cost of paid media. As such, the number of checkpoints used, total span of time, cost of paid media, and change in the seat belt use rate are all reported in the table below. The table is ordered by the percent change, starting with the largest increase in seat belt use.

*Table 9. Results from Occupant Protection Studies that Quantified Checkpoints*

| **Location (study)** | **Dedicated hours of enforcement per 1,000 residents** | **Number of weeks/ number of waves** | **Paid media $ per 1,000 residents in 2018 $** | **Seat belt use pre-program** | **Seat belt use post-program** | **Change (pre- to post-)** |
|---|---|---|---|---|---|---|
| North Carolina (Williams et al., 1996) | 4 weeks/ 2 waves | 0.895 | $136.08 | 64% | 81% | 26.6% |
| Elmira, NY (Williams et al., 2000) | 2 weeks/ 1 wave | 1.034 | Not Specified | 73% | 90% | 23.3% |
| Treddyfrin, PA (Decina et al., 1994) | Unclear | 0.107 | Not Specified | 47.9% | 57% | 19.0% |
| Haverford, PA (Decina et al., 1994) | Unclear | 0.241 | Not Specified | 43.7% | 50% | 14.4% |
| Alabama (Turner & Alex, 2002) | 2 weeks/ 1 wave | 0.0179 | Not Specified | 70.3% | 78.8% | 12.1% |
| Reading, PA (Chaudhary et al., 2005) | 4 weeks/ 1 wave | 1.119 | No Paid Media | 50% | 56% | 12.0% |
| Durham, NC (Williams et al., 1997) | 2 weeks/ 1 wave | 0.06 | Not Specified | 79.2% | 87.7% | 10.7% |
| Asheville, NC (Solomon et al., 2009) | 8 weeks/ 4 waves | 0.495 | $1,894.76 | 83.5% | 91% | 9.0% |
| U.S. (Tison & Williams 2010) | 6 weeks/ 3 waves | 0.117 | $346.80 | 75% | 82% | 9.3% |
| Tennessee (Nichols et al., 2016) | 20 weeks/ 6 waves | 0.057 | $360.32 | 81.6% | 85.8% | 5.1% |
| Queens, New York City (Ledingham et al., 2009) | 4 weeks/ 4 waves | 0.029 | $29.84 | 87% | 89% | 2.3% |
| Oklahoma (Nichols et al., 2016) | 20 weeks/ 6 waves | 0.068 | $303.34 | 85.3% | 86.8% | 1.8% |
| U.S. (Tison et al. 2008) | 2 weeks/ 1 wave | 0.0225 | $107.83 | 82% | 81% | -1.2% |

Although the table above provides a helpful overview of the results, it does not indicate whether the number of checkpoints had a statistically significant impact on seat belt use rate. A linear regression was initially run using 24 wave-level results from 7 of the checkpoint studies—not all checkpoint studies were used as not all studies reported data on the paid media element, which was a variable included in the regression analysis. Then, the finding from Chaudhary et al. (2005) was dropped as it was an outlier relative to the other data points due to the high number of checkpoints relative to the population, and the decision was made to focus only on the studies that had more similar levels of checkpoints, meaning less than 1 checkpoint per 1,000 residents. This left the regression with 23 data points from 6 studies. The regression equation is shown below in Equation 2.

$$Y_{ij} = \alpha\, \beta_1 X_{1,ij} + \beta_2 X_{2,ij} + \beta_3 X_{3,ij} + \beta_4 X_{4,ij} + \varepsilon_{ij} \qquad (2)$$

Where,

- the index $i$ = individual observations and $j$ = studies
- Y, the dependent variable, is the percentage-point change in seat belt use;
- $\alpha$ is the constant, which controls for an overall average impact;
- $X_1$ is seat belt use pre-enforcement. It is included to control for the possibility that at higher levels of seat belt use, additional improvements are likely more difficult to produce;
- $X_2$ is the amount of paid media (normalized by population) and is included to examine whether the amount of resources used in the enforcement effort impacts the success of the effort in improving safety outcomes;
- $X_3$ is the number of checkpoints (normalized by population and the number of weeks in the campaign) and is the critical variable of interest that is included to examine whether the number of checkpoints impacts the success of the effort in improving safety outcomes;
- $X_4$ is a time trend related to the year that the enforcement effort occurred, with efforts in 1990 coded as zero, efforts in 1991 coded as 1, etc. It is included to control for external factors that could be changing over time and influencing the results of these studies;
- $\beta_1$, $\beta_2$, $\beta_3$, and $\beta_4$ are coefficients to be estimated; and
- $\varepsilon_{ij}$ is the error term which is decomposed into a cluster specific error term (indexed with "*j*" for each study) and individual observation level error term (indexed with "*i*" for individual observation). The regression results incorporate robust standard errors to produce correct estimates of the precision of the coefficient estimates.

The results of the linear regression involving the 23 available results can be found in the table on the following page.

*Table 10. Occupant Protection Regression Results for Checkpoints*

| Independent Variables | Coefficient Estimate | Robust Std. Error | t-Value | p-Value | [95 % Conf. Interval] | |
|---|---|---|---|---|---|---|
| $\hat{\alpha}_1$ (Constant) | 27.50292 | 19.97477 | 1.38 | 0.227 | -23.84387 | 78.8497 |
| $\hat{b}_2$ (Seat Belt Use Pre-Enforcement) | -0.37355 | 0.31447 | -1.19 | 0.288 | -1.18192 | 0.43482 |
| $\hat{b}_3$ (Media $ (in 2018 $)/10,000 Residents) | -0.00284 | 0.00203 | -1.40 | 0.220 | -0.00805 | 0.00237 |
| $\hat{b}_4$ (Checkpoints per Week/10,000 Residents) | 7.60503 | 1.95583 | 3.89 | 0.012 | 2.57740 | 12.63266 |
| $\hat{b}_5$ (Time Trend) | 0.20304 | 0.33000 | 0.62 | 0.565 | -0.64538 | 1.05145 |

*Note*. R-squared was 0.8437. Number of observations: 23. Robust standard errors, adjusted for 6 studies. Population estimates are taken from Census Bureau (via Google Population Search Module) in March 2019. See Appendix A for more information.

While the regression did not find a statistically significant effect for paid media, baseline levels of seat belt use, or the time trend, the number of checkpoints per 10,000 people in the campaign area per week of the campaign was statistically significant. The positive coefficient suggests that more checkpoints lead to greater increase in seat belt use, which supports the hypothesis that the size of the enforcement effort affects safety outcomes. The coefficient of 7.6 means that for a fictional town of 100,000 people, increasing the number of checkpoints by 1 per week would increase seat belt use by 0.76 percentage points. These results should only be considered valid within the range present in the synthesis dataset. In the dataset the average value for checkpoints per 10,000 residents per week was 0.29, although most enforcement had fewer than 0.2 checkpoints per 10,000 residents per week. The impact of changing from 0.04 checkpoints per 10,000 residents per week (the 25th percentile value in the analysis dataset) to 0.24 checkpoints per 10,000 residents per week (the 75th percentile value in the analysis dataset) is expected to be a 1.5 percentage-point increase in seat belt use.

The coefficient on paid media was insignificant in this regression. While this could mean that there is no relationship between media spending and seat belt use, this is only a small sample of studies. With 23 results, an insignificant finding might not be representative of the larger population of occupant protection campaigns. The next section, Paid Media, analyzes more data points on media spending and finds a significant relationship. Readers are encouraged to look at the results of that section for a more complete understanding of the effect of media in occupant protection campaigns. Both the time trend and the baseline seat belt use were not statistically significant in this regression, suggesting that for this subset of results, these variables did not impact the observed change in seat belt use.

The main finding of this regression was that the number of checkpoints had a statistically significant positive impact on seat belt use. The results suggest that an enforcement campaign with checkpoints will have greater success at increasing seat belt use if the campaign uses a larger number of checkpoints. The regression did not find statistical significance for the other variables; however, the next section on paid media analyzes more studies and finds statistical significance for some of these variables.

### 3.5.3 Paid Media

The majority of occupant protection programs are HVE campaigns, which have some element of paid media. Many studies reported quantitative information on this media. Some studies reported metrics such as the number of advertisements, TV spots, radio spots, or flyers, but the most common measurement of paid media was the amount of money spent. While not all studies provided a measure of spending, 27 studies provided a dollar value for the paid media, producing 90 wave-level results (several studies examined programs in several locations and over several waves). The table below shows the number of results from the 27 studies by the number of waves of enforcement and by the general size of the location studied.

*Table 11. Count of Occupant Protection Paid Media Results by Number of Waves and Location*

| Criteria | Count of Results | Percentage of All Results |
|---|---|---|
| One Total Wave | 32 | 35.56% |
| Two Total Waves | 16 | 17.78% |
| Three or More Total Waves | 42 | 46.67% |
| Location: Statewide | 67 | 74.44% |
| Location: Nationwide | 9 | 10.00% |
| Location: Smaller Than Statewide | 14 | 15.56% |

The next table shows summary statistics for some of the key quantifiable measures found in these studies, which included total duration of the enforcement campaign, the number of officer enforcement hours involved in the enforcement campaign, the amount spent on paid media, the observed baseline seat belt use, and the percentage-point change in seat belt use. For a more detailed table on the results, see Appendix B.

*Table 12. Summary Statistics for Occupant Protection Paid Media Studies*

| Area | Statistic | Value |
|---|---|---|
| Duration | Minimum | 1 Week |
| | Maximum | 6 Weeks |
| | Average | 2 Weeks |
| Hours of Enforcement per Thousand Residents | Number Unknown | 49 Results |
| | Minimum | 0.22 Hours |
| | Maximum | 20.48 Hours |
| | Average | 3.89 Hours |
| Amount Spent on Paid Media (in 2018 Dollars) per Thousand Residents | Minimum | $14.77 |
| | Maximum | $4,765.81 |
| | Average | $229.05 |
| Baseline Seat Belt Use | Minimum | 53.80% |
| | Maximum | 97.10% |
| | Average | 77.92% |

| Area | Statistic | Value |
|---|---|---|
| Percentage-Point Change | Minimum | -4.30% |
| | Maximum | 38.00%[3] |
| | Average | 3.53% |

*Note*: N=90.

Most results relate to HVE campaigns that involved three or more waves. The results of each wave of an enforcement effort were considered separately. That is, each wave produced an individual result. The studied enforcement campaigns tended to be State-wide efforts, but nationwide efforts and city-level efforts were also included. As mentioned previously, to control for differences in geographic scope of enforcement, the measures of enforcement were normalized using the population of the study location. Around one-third of the results came from studies that looked at *Click It or Ticket* mobilizations, a program that will be further analyzed in a later section. The studies also looked at a few different types of enforcement: checkpoints (31 results), patrols (8 results), checkpoints and patrols (22 results), and safety zones (10 results). There were also 19 results that did not specify the type of enforcement used.

The baseline pre-enforcement seat belt use rate was 77.9 percent on average across these estimates, which is considerably lower than the current nationwide estimate of 90 percent (NHTSA, n.d.-h). This is because many of the enforcement campaigns studied were from several years ago—the years studied range from 1993 to 2013. Overall, these enforcement campaigns were successful, producing on average a 3.5 percentage-point improvement in seat belt use rates. Thus, the average pre-enforcement seat belt use rate of 77.9 percent would be expected to increase to roughly 81 percent post-enforcement. Note that these pre- and post-enforcement seat belt use rates were estimated based on observations typically a week or two prior to the enforcement campaign and a week or two following the enforcement campaign. This means that the longer-lasting effects of seat belt use could not be determined by these studies.

To explore whether the magnitude of the change in seat belt use rates could be linked to the size or intensity of the enforcement effort, a linear regression was used, with robust standard errors. The regression equation is shown below in Equation 3.

$$Y_{ij} = \beta_1 X_{1,ij} + \beta_2 X_{2,ij} + \beta_3 X_{3,ij} + \beta_4 X_{4,ij} + \beta_5 X_{5,ij} + \gamma_1 D_{1,ij} + \gamma_2 D_{2,ij} + \gamma_3 D_{3,ij} + \gamma_4 D_{4,ij} + \gamma_5 D_{5,ij} + \gamma_6 D_{6,ij} + \gamma_7 D_{7,ij} + \varepsilon_{ij} \quad (3)$$

Where,

- the index $i$ = individual observations and $j$ = studies
- Y, the dependent variable, is the percentage-point change in seat belt use;
- $\gamma_1$ through $\gamma_5$ are dummy variables taking values 1 or 0 for each of the following categories of enforcement : $D_1$ for checkpoints only, $D_2$ for patrols only, $D_3$ for checkpoints and patrols, $D_4$ for safety zones, and $D_5$ for unspecified;

[3] The 38% maximum percentage-point change was approximately 20 percentage points larger than the next biggest change. It represents an increase from 43% use to 81% reported in Williams et al., 1994. Since this result appears to be an outlier, it is not included in the analysis.

- $X_1$ is seat belt use pre-enforcement. It is included to control for the possibility that at higher levels of seat belt use, additional improvements are likely more difficult to produce;
- $X_2$, $X_3$, and $X_4$ are the amount of paid media (normalized by population and presented in dollars per thousand person), subdivided into three categories based on the size of the media and the time period;
- $X_2$ is the amount of paid media spent up to $500 before 2008;
- $X_3$ is the amount of paid media spent above $500 before 2008;
- $X_4$ is the amount of paid media spent during or after 2008;[4]
- $X_5$ is a time trend related to the year that the enforcement effort occurred, with efforts in 1990 coded as zero, efforts in 1991 coded as 1, etc. It is included to control for external factors that could be changing over time and influencing the results of these studies;
- $\beta_1$, $\beta_2$, $\beta_3$, $\beta_4$, and $\beta_5$ are coefficients to be estimated;
- $\gamma_6$ and $\gamma_7$ are dummy variables taking values of 1 or 0 for different categories of media spending. $D_6$ is a 1 when media spending is at or below $500 (per thousand person) pre-2008, and $D_7$ is a 1 when media spending is above $500 (per thousand person). These categories are mutually exclusive, the omitted category is media spending during 2008 or later. These dummy variables control for unobservable attributes of enforcement campaigns that happened to spend larger than normal amounts on media; and
- $\varepsilon_{ij}$ is the error term which is decomposed into a cluster specific error term (indexed with "*j*" for each study) and individual observation level error term (indexed with "*i*" for individual observation). The regression results below incorporate robust standard errors to produce correct estimates of the precision of the coefficient estimates.

While ideally the regression would include a measure of the size of the enforcement effort, this section takes a different approach by retaining the measure of paid media while excluding the size of the enforcement effort. Instead, indicator variables control for the type of enforcement. With this less strict data requirement, 89 study-location-wave level results were available for analysis. Some of these observations were included in the regressions in the previous sections. The results of the linear regression can be found in the table below. Alternative specifications that analyzed the data at study-location level, rather than at the study-location-wave level, indicated that the wave-level specification was more appropriate as there did not appear to be a cumulative effect of enforcement across waves.

[4] In the synthesis dataset, this value is always below $500. The decision to use a $500 threshold was based upon the distribution of paid media over time.

*Table 13. Occupant Protection Paid Media Regression Results*

| Independent Variables | Coefficient Estimate | Robust Std. Error | t-Value | p-Value | 95% Confidence Interval | |
|---|---|---|---|---|---|---|
| $\hat{b}_1$ (Use of Checkpoints[a]) | 22.91343 | 3.25159 | 7.05 | 0.000 | 16.22970 | 29.59720 |
| $\hat{b}_2$ (Use of Patrols[a]) | 22.24492 | 3.62550 | 6.14 | 0.000 | 14.79260 | 29.69730 |
| $\hat{b}_3$ (Use of Checkpoints and Patrols[a]) | 23.03819 | 3.51215 | 6.56 | 0.000 | 15.81886 | 30.25750 |
| $\hat{b}_4$ (Use of Safety Zones[a]) | 20.99419 | 3.45028 | 6.08 | 0.000 | 13.90204 | 28.08630 |
| $\hat{b}_5$ (Use of Unknown Enforcement Tactics[a]) | 21.39913 | 3.41518 | 6.27 | 0.000 | 14.37913 | 28.41910 |
| $\hat{b}_6$ (Seat Belt Use Pre-Enforcement) | -0.08775 | 0.03526 | -2.49 | 0.020 | -0.16022 | -0.01530 |
| $\hat{b}_7$ (Media $ (in 2018 $)/Thousand Residents up to $500, Pre-2008)[b] | 0.01100 | 0.00474 | 2.32 | 0.028 | 0.00126 | 0.02075 |
| $\hat{b}_8$ (Media $ (in 2018 $)/Thousand Residents Above $500, pre-2008)[b] | -0.00044 | 0.00028 | -1.56 | 0.132 | -0.00101 | 0.00014 |
| $\hat{b}_9$ (Media $ (in 2018 $)/Thousand Residents Post-2007)[b] | -0.01539 | 0.01699 | -0.91 | 0.373 | -0.05032 | 0.01954 |
| $\hat{b}_{10}$ (Time Trend) | -0.68071 | 0.05603 | -12.15 | 0.000 | -0.79588 | -0.56550 |
| $\hat{b}_{11}$ (Use of Media Less Than $500, pre-2008)[c] | -3.11538 | 1.76798 | -1.76 | 0.090 | -6.74950 | 0.51875 |
| $\hat{b}_{12}$ (Use of Media Greater than $500, pre-2008)[c] | -5.14810 | 1.82380 | -2.82 | 0.009 | -8.89696 | -1.3992 |

[a] Variable is a dummy: 1 when the named enforcement tactic was present, 0 otherwise.
[b] Population estimates are taken from Census Bureau (via Google Population Search Module) in March 2019. See Appendix A for more information.
[c] Variable is a dummy: 1 when the media condition was met, 0 otherwise
Note: R-squared was 0.7516. 89 observations. Standard errors adjusted by studies.

Many of the coefficients were significant at the 0.05 level, meaning that those coefficients are likely different from zero. An important overall conclusion is that enforcement that used both patrols and checkpoints produced the largest increases in seat belt use rates, followed by enforcement that used patrols alone or checkpoints alone. Enforcement that used safety zones or that were not described adequately in the study produced relatively lower improvements in safety outcomes.

One caveat is that although the estimated coefficients differed on these enforcement strategies, not all enforcement strategies were statistically significantly different from each other. The combination of checkpoints and patrols was significantly more effective than checkpoints alone, safety zones, and unspecified enforcement strategies. No other pairs of enforcement activities were statistically different from the effects of the separate activities. For instance, patrols and checkpoints and use of patrols alone were not statistically different from each other.

The statistically significant estimate of the seat belt use pre-enforcement coefficient indicates that a larger baseline (pre-wave) level of seat belt use results in a smaller percentage-point change after an occupant protection campaign. Specifically, compared to a town where the baseline use is 80 percent (Town A), a typical occupant protection program in a town where the baseline use is 70 percent (Town B) would produce a 0.88 percentage-point larger increase in Town B than in Town A.

The estimated coefficient on the time trend was statistically significant and negative, indicating that more recent years saw smaller increases in seat belt use. Specifically, each year since 1990 reduces the seat belt improvement by an additional .68 percentage points. This value must be combined with the baseline value, as well as the enforcement effort coefficients, to be fully understood in context.

To produce an estimate, one needs to specify a baseline seat belt use rate as well as a year. The sample average baseline seat belt use rate of 77.9 percent and the sample median year of 2007 are used in these illustrative calculations. These estimates assume that no funds are spent on paid media. Note, however, these estimates are based on study locations where baseline seat belt use typically ranged between 66 and 87 percent. In using these results they should likewise be applied only in contexts with similar baseline levels of use. At baseline seat belt use levels of greater than 90 percent, the formula will return negative impacts (a decrease in seat belt use) for some types of enforcement, The table below shows the steps to calculate expected percentage-point changes in seat belt use by type of enforcement effort based on the results of the regression.

*Table 14. Analysis of Occupant Protection Paid Media Regression Result*

| **Adjustment for Baseline Seat Belt Use of 77.9% Pre-Enforcement** | **Adjustment for Year, Using Median Year of 2007** | **Adjustment for Type of Enforcement Effort** | **Result: Percentage-Point Change in Seat Belt Use Post-Enforcement (No Media Spent)** |
|---|---|---|---|
| *$\beta_6$*(average baseline level) +* | *$\beta_{10}$*(Median Year-1990) +* | *$\beta_t$, where t depends on the enforcement type* | *= expected percentage point change in seat belt use rates* |
| -0.088*77.9% = -6.84% + | -0.68*17 = -11.57% + | 22.91% (Checkpoints) | = 4.502% |
| -0.088*77.9% = -6.84% + | -0.68*17 = -11.57% + | 23.038% (Checkpoints and Patrols) | = 4.63% |
| -0.088*77.9% = -6.84% + | -0.68*17 = -11.57% + | 22.244% (Patrols) | = 3.836% |
| -0.088*77.9% = -6.84% + | -0.68*17 = -11.57% + | 20.994% (Safety Zones) | = 2.586% |
| -0.088*77.9% = -6.84% + | -0.68*17 = -11.57% + | 21.399% (Unknown) | = 2.991% |

Of the three types of media spending represented in the regression, only one was statistically significant, which was the variable for media spending pre-2008 under $500 per thousand people. The estimated coefficient was 0.011, meaning that increasing media spending by $1 per 1,000 residents would increase seat belt use by 0.011 percentage points. This finding suggests that during the period of 1993 to 2008 each additional cent ($0.01) per resident up to 50 cents per resident spent on media in an occupant protection HVE campaign increased seat belt use by an additional 0.11 percentage points. Extrapolating to a fictional town of 100,000 people, including paid media spending of approximately $9,091 (or $0.09 per person) in an enforcement effort prior to 2008 would be expected to have increased seat belt use rates by 1 percentage point.

The amount of money spent above $500 per thousand people was not statistically significant. This suggests that beyond 50 cents per resident, media spending produced no additional impact on safety outcomes. The lack of significance on media spending after 2008 indicates that the available data did not show an impact on safety outcomes from additional media spending after 2008. It is difficult to know why an impact from increased media spending was not observed after 2008, but the nature of the available data was one factor that may explain the result. After 2008, amounts spent on media were confined to a small range and were on the lower end of amounts spent prior to 2008. Thus, there was not much variation among studies related to media spending from which to determine a relationship with safety outcomes. Another possibility is that the impact of currently used safety messages spread through paid media has diminished over time. No one message can appeal to all audiences (Schmid et al., 2008). Therefore, the next generation of safety messages may need to be tailored to specific audiences, as suggested in a recent report published by NHTSA titled *Expanding the Seat Belt Program Strategies Toolbox: A Starter Kit for Trying New Program Ideas* (Thomas et al., 2016).

The indicator variables for media spending greater than $500 per thousand residents was negative and statistically significant at the conventional 0.05 level. The indicator variable for spending less than $500 was negative but only statistically significant at the 0.10 level. These dummy variables were included to account for unobservable characteristics of enforcement campaigns. The statistical significance of these variables suggests that there are some systematic similarities among enforcement that spend larger amounts on media and among studies that spend smaller amounts on media and these differences are not otherwise associated with the exact amount spent. Thus, these dummy variables were retained in this preferred specification to control for those unobserved characteristics.

What the regression has shown is that during the period prior to 2008, media spending had a significant impact on seat belt use, regardless of the type of enforcement. However, those incremental impacts related to amount spent on media have not been observed in the literature after 2008. Most of the enforcement types were not statistically different from one another; however, the combination of checkpoints and patrols was significantly more effective than checkpoints alone, safety zones, and unspecified enforcement strategies. The results suggest that a well-rounded HVE campaign will be more successful than a simple campaign that only uses one type of enforcement and does not include media. Campaigns are more successful with a strong media component and with, visible enforcement strategies.

### 3.5.4 Click It or Ticket (CIOT)

*CIOT* is an occupant protection HVE campaign that has been implemented across the United States. Annual national campaign mobilization began in 2003, but *CIOT* originally started in North Carolina in 1993 (Tison & Williams, 2010). Modeled after the first American STEP in Elmira, New York, North Carolina's *CIOT* used periodic enforcement waves that were augmented by extensive paid, earned, and public service media. The program increased seat belt use in North Carolina by about 20 percent over 5 years, and that success contributed to *CIOT*'s spread across the United States. In 2000 South Carolina initiated its own *CIOT* program, followed by another 7 States in 2001, 18 additional States in 2002, and then 45 States in the first national campaign in 2003. Since then, there has been full nationwide participation in the NHTSA-led program.

Unfortunately, it is difficult to accurately quantify the effect of *CIOT* as there have often been data availability issues. An evaluation of the national *CIOT* campaign found that States tend to provide limited and inconsistently collected data on their *CIOT* activities; therefore, it was impossible to know aspects of the program like the total number of officer enforcement hours or the total amount of earned media (Tison & Williams, 2010).

Despite how difficult it can be to properly assess the *CIOT* campaigns, there were 21 studies that focused on *CIOT* activities. Some of these 21 studies were analyzed in previous subsections. These 21 studies vary in terms of what States they examine and what data they report; however, all studies have some measure of seat belt use as an outcome variable. All studies used observational data to estimate seat belt use. The location and years of the *CIOT* programs analyzed can be found in the table below.

*Table 15. Background Information on CIOT Studies*

| Study | Location | Years Studied |
|---|---|---|
| Agent et al., 2008 | Kentucky | 2008 |
| Eby et al., 2003 | Michigan | 2003 |
| Eby & Vivoda, 2003 | | 2002 |
| Eby & Vivoda, 2004 | | 2004 |
| Kim & Yamashita, 2003 | Hawaii | 2002 |
| Morgan, 2015 | Ohio | 2005–2009 |
| Nichols et al., 2007 | Illinois; Indiana; Ohio; Minnesota; Michigan; Wisconsin | 2005 |
| Solomon et al., 2002 | Alabama; Florida; Illinois; Indiana; Mississippi; Nevada; Texas; Vermont; Washington; West Virginia; Colorado; Michigan; Ohio; Rhode Island | 2002 |
| Solomon et al., 2007 | All of United States | 2004 |
| | | 2005 |
| Solomon et al., 2009 | | 2007 |
| Solomon et al., 2013 | | 2008 and 2009 |
| Thomas et al., 2011 | Utah | 2009 |
| Tison et al., 2008 | All of United States | 2006 |
| Tison & Williams, 2010 | | 2002–2006 |

| Study | Location | Years Studied |
|---|---|---|
| Turner & Alex, 2002 | Alabama | 2002 |
| Vasudevan et al., 2009 | Nevada | 2003–2005 |
| Vivoda et al., 2004 | Michigan | 2004 |
| Vivoda, Eby, et al., 2007 | Indiana | 2006 |
| Vivoda, St. Louis, et al., 2007 | Florida | 2007 |
| Williams et al., 1997 | North Carolina | 1996 |

Due to the aforementioned data issues, one cannot explore or estimate a quantitative relationship between the intensity or size of the *CIOT* effort and the magnitude of the resulting change in seat belt use. However, a simple vote count is possible; if a *CIOT* program was active in an area, did that area experience an increase in seat belt use?

Some studies reported several results—from several years of *CIOT* or several States—such that the 21 studies produced 78 total findings on seat belt use. Of these 78 results, 60 showed a positive improvement in seat belt use while 18 showed a decline, as can be seen in Figure 2. A simple sign test can test the odds of getting at least 60 positive increases out of 78 results due to random chance. Testing the hypothesis that the result is due to random chance results in a very small p-value, less than 0.000001, meaning that the hypothesis can be rejected, indicating that it is very likely that *CIOT*, has a positive effect on seat belt use.

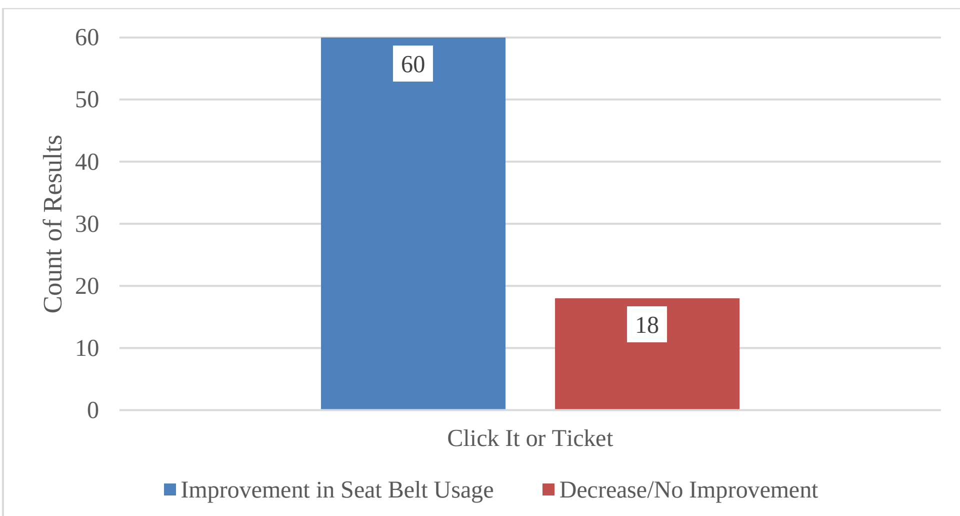



*Figure 1. Vote count of CIOT results*

Changes in seat belt use will be discussed here in terms of percentage-point changes: a change from 50 to 100 percent would be a 50 point increase. Among the positive *CIOT* findings, the change in seat belt use ranged from a 0.1 percentage-point increase to an 18.7 percentage point increase with an average increase of 3.34 points. In contrast, the decreases were less variable, ranging from a 0.1 percentage-point decrease to a 5.3 percentage-point decrease. Of the 60 results that saw an improvement, 52 of them had an increase larger than 1 percentage point. Of the 18 results that did not see an improvement, 12 results showed a 1 percentage point or smaller decrease.

The findings from these studies would suggest that *CIOT* programs are generally successful in increasing seat belt use. It is difficult to assess which *CIOT* activities are the most useful, given that many of the studies did not report full data on enforcement tactics. Nevertheless, the promising results from the vote count suggest that *CIOT* produces positive results overall. These results also broadly suggest that occupant protection STEPs that use HVE strategies tend to increase seat belt use.

### 3.5.5 Buckle Up Kentucky

Six studies have examined the same HVE campaign in Kentucky for 6 years, from 2003 to 2008. For the first 5 years, the program was known as "Buckle Up Kentucky: It's the Law & It's Enforced," while in 2008 the program used the *Click It or Ticket* branding. The program was a STEP aimed at increasing seat belt use rates across the entire State of Kentucky. The enforcement campaigns were conducted near Memorial Day every year and included paid media and earned media. Kentucky's enforcement program included saturated patrols and checkpoints. It is also important to note that during the first 4 years, from 2003 to 2006, Kentucky only had secondary enforcement laws, which means that drivers must be first pulled over for a different violation before they can be ticketed for failing to wear a seat belt. Seat belt use rates in States with primary laws hover close to 10 percentage points above States with secondary seat belt laws (NHTSA, n.d.-a). In late 2006 the law in Kentucky was changed to primary enforcement, meaning that the last 2 years of campaigns (in 2007 and 2008) were fundamentally different from the first 4 years. Observational data was used to measure seat belt use in all 6 years of the campaign.

All 6 years saw an improvement in the observed seat belt use, and there was a 14 percent increase in seat belt use from before the 2003 campaign was conducted to after the 2008 campaign. Figure 2 shows the baseline seat belt use rate before and after the STEP for all 6 years of the Kentucky program.

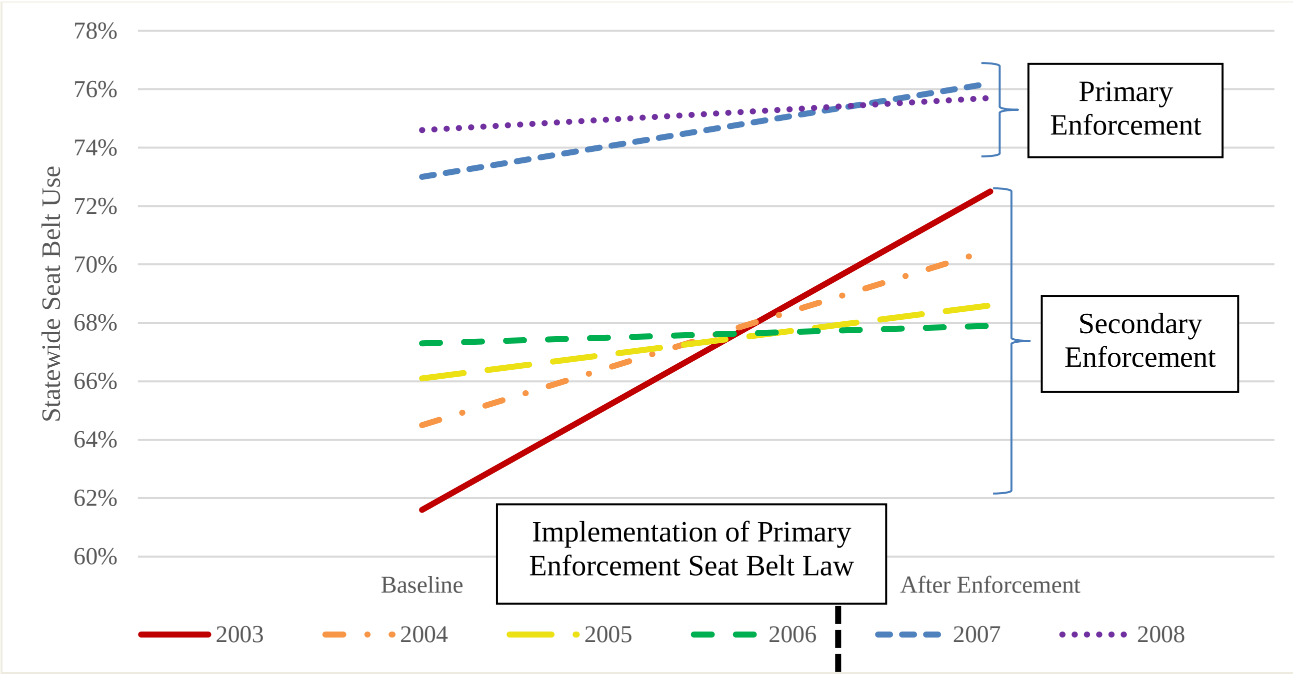



*Figure 2. Buckle Up Kentucky: Results*

The figure suggests that each enforcement campaign was effective at increasing seat belt use during the enforcement time-period, and that the increase was at least partially sustained from year-to-year, as the baseline (pre-enforcement) seat belt use values steadily increased every year. The large jump from 2006 to 2007 may also be showing the effect of changing the seat belt laws from secondary enforcement to primary enforcement. The data also suggest that the enforcement campaigns became less effective from year-to-year, perhaps due to the baseline levels or an increasing sense of ambivalence toward the enforcement program after it had been repeated for several years. These 6 studies show that HVE campaigns and STEPs were effective in Kentucky, and appear to have been effective both when Kentucky had secondary enforcement laws and primary enforcement laws, meaning that STEPs and HVE campaigns could be effective in both types of States.

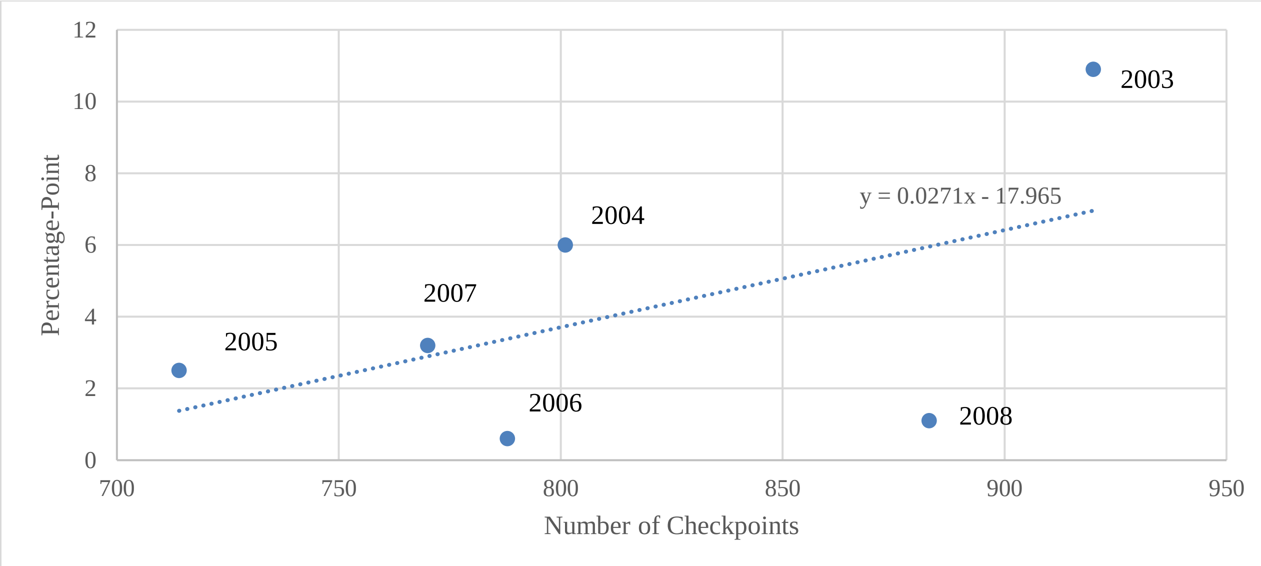



*Figure 3. Changes in Kentucky seat belt use by the number of checkpoints. Note: This upward trend line is not statistically significant due to the small sample size (only 6 points), which is shown in that the regression coefficient (0.0271) is not significantly different from zero (p=0.2768)*

Figure 3 shows a scatter plot relating the number of checkpoints employed in a specific year of the campaign with the resulting percentage-point change in seat belt use. The trend line suggests that more checkpoints led to a greater increase in seat belt use. For each additional 100 checkpoints in the State, the seat belt use rate increased 2.71 percentage points.

### 3.5.6 Nighttime Seat Belt Use

Most of the seat belt enforcement programs analyzed were conducted during the day, but a few studies focused on how those daytime programs and specific nighttime programs have affected seat belt use at night. Historical data showed that nighttime fatalities are a large portion of all U.S. fatalities, indicating that improving the rate of occupant protection at night could help achieve significant progress in reducing U.S. fatalities in crashes (Solomon et al., 2009).

There were 4 unique studies that looked at nighttime seat belt use. However, because one study looked at several programs, there were results from 6 programs discussed in this section. The background information on the studies can be found in the table below.

*Table 16. Studies That Examined Nighttime Seat Belt Use*

| Study | Location | Dates | Number of waves |
|---|---|---|---|
| Chaudhary et al., 2005 | Reading, PA | September 2004 | 1 |
| Solomon et al., 2009 | Asheville, NC<br>Greenville, NC<br>Charleston, WV | 2007 | 4 |
| Thomas et al., 2017 | Washington | May 2007–May 2009 | 5 |
| Vivoda, Eby, et al., 2007 | Indiana | May 2006 | 1 |

The reviewed programs had the goal of increasing observed seat belt use, but each enforcement program was unique. Some programs used checkpoints, while others did not. All programs generally used patrols, although some studies provided more detail on the amount of patrols than others. And some studies provided very minimal information on the type of enforcement used during the program. The results from the studies can be seen below.

*Table 17. Results From Nighttime Seat Belt Use Studies*

| **Location** | **Enforcement details** | **Number of weeks/Number of waves** | **Paid media $ per 1,000 residents in 2018 $** | **Nighttime seat belt use (%) pre-wave 1** | **Nighttime seat belt use (%) post-final wave** | **% Change (pre-to-post-final wave)** |
|---|---|---|---|---|---|---|
| Reading, PA (Chaudhary et al., 2005) | Checkpoints | 4 weeks/1 wave | No Paid Media | 50% | 56% | 12% |
| Asheville, NC (Solomon et al., 2009) | 48 Checkpoints | 8 weeks/4 waves | $1,894.76 | 85.6% | 92.5% | 8.1% |
| Greenville, NC (Solomon et al., 2009) | 140 Saturation Patrols | 8 weeks/4 waves | $1,030.34 | 83.4% | 87.1% | 4.4% |
| Charleston, WV (Solomon et al., 2009) | 46 Traffic Safety Zones | 8 weeks/4 waves | $1,985.91 | 58.4% | 60.2% | 3.1% |
| Washington (Thomas et al., 2017) | $1,530,188 spent on enforcement | 20 weeks/5 waves | $249.56 | 94.6% | 96.4% | 1.9% |
| Indiana (Vivoda, Eby, et al., 2007) | Not Specified | 4 weeks/1 wave | Not Specified | 79% | 74% | -6.3% |

Because the 4 studies that look at nighttime seat belt use did not have the same enforcement strategies or measures, it was not possible to generalize a relationship between intensity or size of the enforcement effort and magnitude of the safety outcomes. However, a qualitative analysis found that the two enforcement programs that explicitly stated they used checkpoints—the one in Reading and the one in Asheville—resulted in having the two largest positive increases in nighttime seat belt use. Since there were only a few studies in the sample, it is impossible to draw a robust conclusion. However, the studies with checkpoints reported greater positive changes in nighttime seat belt use than the other studies that did not explicitly state that they used checkpoints.

It also appears that nighttime occupant protection programs were more effective in smaller, targeted areas. The two programs that targeted entire States were the least successful programs, while the ones that targeted smaller areas had more success. Again, with only 4 studies, this conclusion was very tentative.

### *3.5.7 Control Locations*

A limited number of the studies reviewed in this chapter included control locations (sometimes called a comparison location). In these instances the study looked at how safety (in this case, the safety outcome for all studies was seat belt use) was changing in a similar community that did not receive the same enforcement and media efforts as the study location.

Nine studies included controls: Chaudhary et al. (2005), Decina et al. (1994), Kaye et al. (1995), Ledingham et al. (2009), Nichols et al. (2016), Schaechter & Uhlhorn (2011), Solomon et al. (2009), Thomas et al. (2017), and Williams et al. (2000). Four of these studies—Decina et al. (1994), Kaye et al. (1995), Solomon et al. (2009), and Williams et al. (2000)—provided data on the control locations but did not do a statistical analysis to determine whether the treatment locations had significantly better improvements in seat belt use than the control locations. The remaining 5 studies provided statistical analysis on the difference.

Looking at all 9 studies, the treatment locations generally saw larger increases in seat belt use than the control locations. In 5 studies (Chaudhary et al., 2005; Decina et al., 1994; Ledingham et al., 2009; Solomon et al., 2009; and Williams et al., 2000), the treatment locations experienced a large increase in seat belt use while the control locations experienced either a decline in seat belt use or virtually no change. Both Chaudhary et al. (2005) and Ledingham et al. (2009) found that the difference between the treatment and control locations was statistically significant. The results from these studies support the theory that occupant protection campaigns have been effective at increasing seat belt use.

Schaechter and Uhlhorn (2011) studied efforts focused on Latino communities in the United States and found an increase in seat belt use in both their treatment locations and their control locations. Their statistical test, however, showed that the increase in the treatment location was significantly larger than the increase in the control location. This result therefore still supports the idea that occupant protection campaigns are effective at increasing seat belt use.

Neither Nichols et al. (2016) nor Thomas et al. (2017) found statistically significant differences between their treatment locations and their control locations. In both studies seat belt use increased in all the locations. The results, therefore, suggested that the campaigns were not effective at increasing seat belt use, since seat belt use increased by similar amounts in locations without the campaigns. It is worth noting, however, that both studies targeted large areas. Nichols et al. (2016) looked at large areas in Oklahoma and Tennessee, while Thomas et al. (2017) studied all of Washington State. The other studies that included control locations examined programs in smaller areas. This could suggest that programs more targeted to small areas may be more effective than broader, larger programs, but it is difficult to draw a strong conclusion without more studies.

The final study, Kaye et al. (1995), had mixed results. In one treatment location (Escambia and Santa Rosa, Florida), seat belt use increased. In the other treatment location (Hernando and Pasco, Florida), seat belt use decreased. The control location for both treatments was Volusia County, Florida, and seat belt use remained unchanged there. In the first treatment location it seemed the campaign was effective at increasing seat belt use beyond the control location. In the second treatment location seat belt use decreased more than in the control location. Since this study had mixed results, it is difficult to draw conclusions on the campaign's effectiveness.

Overall, despite some negative results, studies that included control locations generally showed that their occupant protection programs increased seat belt use significantly more than any change in the control location. The results suggest that occupant protection campaigns that were conducted in large areas were less effective than the ones conducted in smaller areas, but only 2 of the 9 studies looked at large programs.

## 3.6 Conclusion

Overall, the main findings from these occupant protection studies were: (1) higher baseline levels of seat belt use decrease the positive change that the program will provide; (2) during earlier time periods, paid media was more effective at further increasing seat belt use rates; (3) more checkpoints lead to greater increases in seat belt use; (4) NHTSA's *Click It or Ticket* program has been generally successful at increasing seat belt use; and (5) based on some of the other findings, HVE has been an effective program model for occupant program campaigns. Based upon existing research, this synthesis was not able to determine the effect of different methods of enforcement, as well as how different campaigns may have unique impacts on daytime and nighttime seat belt use. Additionally, as these studies also showed decreasing returns on enforcement over time, it could indicate that future occupant protection campaigns will need to try new tactics to make them more successful, as people may potentially become resistant to the older campaign models.

# 4 Distracted Driving

This chapter reviews the 5 relevant studies on distracted driving enforcement programs in the United States. Distracted driving is classed as any activity that diverts attention from driving, which includes manual distractions such as eating, cognitive distractions such as talking to people in the vehicle or on a phone, or visual distractions due to entertainment or navigation systems. Activities may include several forms of distraction; for example, texting is both a manual and visual distraction and, potentially, a cognitive one (NHTSA, n.d.-b). Many States have enacted laws aimed at reducing the prevalence of distracted driving by banning texting, banning handheld cell phone use, or using a graduated driver licensing system for teen drivers; however, the available literature on distracted driving enforcement campaigns examined only the cell phone type of distracted driving. While 24 States plus the District of Columbia, Guam, Puerto Rico, and the Virgin Islands ban handheld phone use for all drivers, 48 States and the District of Columbia ban texting while driving. A full list of distracted driving laws by State, retrieved from the Governors Highway Safety Association (n.d.-b), is provided below:

***Handheld Ban (24 States Plus DC)***

Arizona, California, Connecticut, Delaware, District of Columbia, Georgia, Hawaii, Idaho, Illinois, Indiana, Maine, Maryland, Massachusetts, Minnesota, Nevada, New Hampshire, New Jersey, New York, Oregon, Puerto Rico, Rhode Island, Tennessee, Vermont, Virginia, Washington, and West Virginia

***Texting Ban (48 States Plus DC)***

Alabama, Alaska, Arizona, Arkansas, California, Colorado, Connecticut, Delaware, District of Columbia, Florida, Georgia, Hawaii, Idaho, Illinois, Indiana, Iowa, Kansas, Kentucky, Louisiana, Maine, Maryland, Massachusetts, Michigan, Minnesota, Mississippi, Nebraska, Nevada, New Hampshire, New Jersey, New Mexico, New York, North Carolina, North Dakota, Ohio, Oklahoma, Oregon, Pennsylvania, Puerto Rico, Rhode Island, South Carolina, South Dakota, Tennessee, Texas, Utah, Vermont, Virginia, Washington, West Virginia, Wisconsin, and Wyoming.

## 4.1 Description of Distracted Driving Enforcement Evaluation Studies

There is limited literature available on the impact of distracted driving enforcement. After screening studies for relevance and content, a total of 5 distracted driving studies were appropriate for inclusion in the analysis. All were before-after studies with one or more control locations. The studies are considered quasi-experimental because they lack random assignment of participants, and, while the treatment and control locations are similar, they may not in fact be directly comparable (or identical) at baseline conditions. Studies that used control locations looked at how safety was changing in similar communities that did not receive the same enforcement and media efforts as the study locations. The 5 U.S. studies, published between 2010 and 2017, discussed the impacts of two enhanced enforcement campaigns and span 5 States.

Four of these studies examined the impact of an HVE campaign, *Phone in One Hand, Ticket in the Other*, implemented in 4 States: California, Delaware, Connecticut, and New York. This campaign combined awareness surveys, publicity, and enhanced HVE actions to target distracted driving. The impacts of these campaigns were measured primarily through observed phone use rates, specifically handheld phone use and cell phone manipulation rates. In addition, one study measured changes in the number of crashes and the percentage of distraction-related crashes

(Chaudhary et al., 2015). During the study analysis period, California, Delaware, Connecticut, and New York prohibited texting and handheld device use while driving.

Schick et al. (2014) and Chaudhary et al. (2015) examined all three waves of a single distracted driving campaign in California and Delaware. For waves one and two, the studies reported pre-/post-data; for wave three, only post-final data was reported. Cosgrove et al. (2010) and Chaudhary et al. (2012) both examined a single distracted driving program conducted in both Connecticut and New York; Chaudhary et al. (2012) examined all four program waves while Cosgrove et al. (2010) examined only the first two waves. Studies with the same enforcement data for the same enforcement program and waves are combined into a single observation; the 4 studies examining *Phone in One Hand, Ticket in the Other* represented only two unique campaign observations, covering four program sites.

The fifth study, Retting et al. (2017), examined targeted distracted driver enforcement across 19 cities and towns in Connecticut and Massachusetts. State and local police used a combination of spotters and other self-initiated enforcement strategies (e.g., stationary covert or overt, roving, or motorcycle patrols) targeted at reducing observed instances of distracted driving. Consistent with the aforementioned HVE studies, the impact of enforcement was measured through observed phone use rates, specifically handheld phone use and texting rates. While the campaign described by Retting et al. (2017) did not include media spending, it did include awareness surveys and other strategies to raise awareness of the campaign and, for the purposes of this report, is classed as HVE. It is important to note that Connecticut and Massachusetts had different distracted driving laws; in Massachusetts, only texting was illegal during the time of the evaluation, whereas, in Connecticut, both texting and handheld phone use were prohibited.

The following table provides summary information about each of the 5 distracted driving studies.

*Table 18. Distracted Driving Studies*

| Study | Capture Area | State | Specific Locations | Number of Enforcement Waves | Dates of Enforcement |
|---|---|---|---|---|---|
| Schick et al., 2014) Chaudhary et al., 2015) | Several Counties | California | El Dorado, Sacramento, San Joaquin, Stanislaus, Solano, Sutter, Placer, Yuba, Yolo | 3 Waves | November 2012-June 2013 |
| Schick et al., 2014) Chaudhary et al., 2015) | State | Delaware | NA | | |
| Cosgrove et al., 2010) Chaudhary et al., 2012) | City | Connecticut | Hartford | 4 Waves (Cosgrove covers Waves 1 & 2) | April 2010-July 2011 |
| Cosgrove et al., 2010) Chaudhary et al., 2012) | | New York | Syracuse | | |
| Retting, R., Sprattler, K., Rothenberg, H., & Sexton, T. (2017) | Several Towns | Connecticut | Bethel, Brookfield, Danbury, Monroe, Newton, Redding, and Ridgefield | 4 Waves | May/June 2013-June 2014 |
| | | Massachusetts | Andover, Dracut, Dunstable, Lawrence, Lowell, Methuen, North Andover, North Reading, Reading, Tewksbury, Tyngsborough, Wilmington | | |

Each study varied in terms of treatment duration, location, enforcement strategy employed, and analysis techniques. The next section, Methods of Enforcement, describes the enforcement strategies employed across the different campaigns. The following section, Publicity, describes like publicity strategies and visibility elements, which help raise awareness of enhanced enforcement activities. The Safety Outcomes section describes the variety of ways the studies measured safety outcomes related to distracted driving. The subsequent section synthesizes the literature, linking enforcement strategy to safety outcome, and attempts to quantify, where possible, the effect of targeted distracted driving enforcement campaigns on distracted driving-related safety outcomes. Finally, the Conclusion section summarizes the main findings.

## 4.2 Methods of Enforcement

The following table provides a brief overview of the enforcement strategies and their prevalence in the 5 distracted driving studies. The count of studies describes the number of studies in which the enforcement type appears while the count of observations describes the number of observations obtained for each type. Patrols and Spotters show no observation counts because, while 4 studies mentioned that they were a component of the enforcement campaign, no information quantitatively, such as number of patrols or spotters, was given.

*Table 19. Distracted Driving Enforcement Measures*

| Enforcement Type | Count of Studies | Studies | Count of Observations |
|---|---|---|---|
| HVE | 4 | Schick et al., 2014), Chaudhary et al., 2015), Chaudhary et al., 2015), Cosgrove et al., 2010) | 47 |
| Patrols | | Chaudhary et al., 2012), Chaudhary et al., 2015), Cosgrove et al., 2010), Retting et al., 2017) | 0 |
| Spotters | | | |

## 4.3 Publicity

The following table provides a brief overview of the publicity activities and their prevalence in the 5 distracted driving studies. The count of studies describes the number of studies in which the activity appeared. As seen in the table, enforcement campaigns often involve publicity that supplement the enforcement, such as raising awareness of targeted enforcement activities and informing the public of the dangers of distracted driving. While not part of enforcement activities, these supplementary activities are a component of the enforcement campaign and likely have an impact on handheld phone use rates and other measures of the prevalence of distracted driving.

*Table 20. Distracted Driving Publicity*

| Publicity Effort | Count of Studies | Studies |
|---|---|---|
| Paid Media (television, radio, billboards, etc.) | 4 | Schick et al., 2014), Chaudhary et al., 2015), Chaudhary et al., 2012), Cosgrove et al., 2010) |
| Earned Media | 2 | Chaudhary et al., 2015), Chaudhary et al., 2012) |
| Awareness Surveys | 5 | Chaudhary et al., 2015), Chaudhary et al., 2012), Cosgrove et al., 2010), Schick et al., 2014), Retting et al., 2016) |
| PSAs | 1 | Chaudhary et al., 2012) |

| Publicity Effort | Count of Studies | Studies |
|---|---|---|
| Slogan (e.g., "Phone in one hand, ticket in the other") | 4 | Schick et al., 2014), Chaudhary et al., 2015), Chaudhary et al., 2012), Cosgrove et al., 2010) |

The next table shows a breakdown of what four SHSOs spent on paid media broken down by waves for their HVE campaigns (Chaudhary et al., 2015); the table is sorted first by location then by wave to allow for comparison of spending across individual campaign waves.

*Table 21. Distracted Driving Paid Media by State and Enforcement Wave*

| Wave | Location | TV Cost, 2018$ | Radio Cost, 2018$ | Online Cost, 2018$ | Total Cost, 2018$ |
|---|---|---|---|---|---|
| Wave 1 | Delaware | $18,479.13 | $30,588.38 | $16,801.46 | $65,868.97 |
| Wave 2 | | $13,816.82 | $22,163.26 | $10,848.29 | $46,828.38 |
| Wave 3 | | $14,056.57 | $22,022.63 | $10,848.29 | $46,927.50 |
| Wave 1 | Hartford | $124,790.36 | $31,244.97 | $5,310.86 | $161,346.19 |
| Wave 2 | | $65,579.52 | $20,198.28 | $4,307.04 | $90,084.83 |
| Wave 3 | | $80,678.27 | $16,800.89 | $2,871.36 | $100,350.51 |
| Wave 4 | | $36,281.14 | $9,317.51 | $5,625.16 | $51,223.81 |
| Wave 1 | Sacramento | $286,536.79 | $47,255.17 | $8,278.81 | $342,070.77 |
| Wave 2 | | $247,310.46 | $39,876.43 | $8,136.22 | $295,323.12 |
| Wave 3 | | $272,306.83 | $46,165.84 | $8,136.22 | $326,608.89 |
| Wave 1 | Syracuse | $42,378.95 | $14,170.73 | $5,082.30 | $61,631.97 |
| Wave 2 | | $24,713.20 | $10,831.91 | $4,307.04 | $39,852.15 |
| Wave 3 | | $24,816.57 | $5,970.13 | $2,871.36 | $33,658.06 |
| Wave 4 | | $36,281.14 | $9,317.51 | $5,625.16 | $51,223.81 |

During the time that the 5 distracted driving studies took place, considerable media attention was given to the topic of distracted driving (Chaudhary et al., 2012). Insurance companies, mobile phone providers, and safety and advocacy organizations at the time were very active in raising awareness about the dangers of mobile phone use and other distractions while driving. There were several large events, public service announcements (outside of NHTSA grant funding), and special reports from national, regional, and local media outlets. Celebrities and public figures, including former DOT Secretary Ray LaHood, were also active in garnering national media attention at the time, which included a live TV broadcast and rallies in several cities across the United States (Chaudhary et al., 2012).

## 4.4 Safety Outcomes

The table below provides a brief overview of the safety outcomes measures and their prevalence in the 5 distracted driving studies. The count of studies describes the number of studies in which the safety outcome appears while the count of observations describes the number of observations obtained for each outcome measure. While the change in the number of crashes and associated injuries attributable to distracted driving would be preferable measurements for the safety outcome, attributing crashes to distracted driving is challenging as many police reports do not include information on distracting events and those that do depend on secondary information like after-the-fact reconstructions, self-reporting, or other physical evidence, such as observing a phone in the car. Only one study reported the number of crashes attributable to distracted driving.

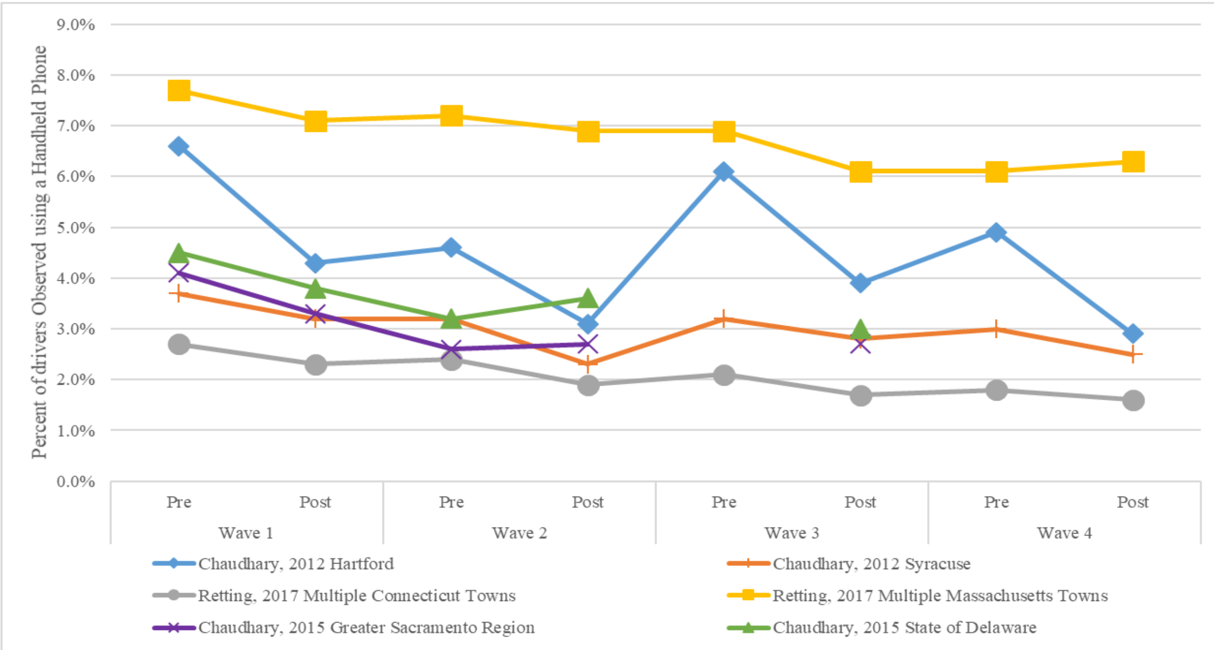
*Table 22. Measures of Distracted Driving Safety Outcomes*

| Safety Outcome | Count of Studies | Studies | Count of Observations |
|---|---|---|---|
| Percentage of drivers observed using a handheld phone | 5 | Schick et al., 2014, Chaudhary et al., 2015, Cosgrove et al., 2010, Chaudhary et al., 2012, Retting et al., 2016 | 47 |
| Percentage of drivers observed manipulating a handheld phone | 3 | Schick et al., 2014, Chaudhary et al., 2015, Chaudhary et al., 2012 | 47 |
| Percentage of drivers observed using an earpiece | 2 | Chaudhary et al., 2015, Chaudhary et al., 2012 | 31 |
| Number of Crashes | 1 | Chaudhary et al., 2015 | 8 |
| Proportion of distraction-related crashes (measured as a percent) | | | |

*Note.* As discussed previously, studies reporting information on the same HVE campaigns have been grouped, reporting only one observation per wave per location. (Group 1: Schick et al., 2014 and Chaudhary et al., 2015, and Group 2: Cosgrove et al., 2010 and Chaudhary et al., 2012)

All studies reported the percentage of drivers observed performing behaviors associated with distracted driving. These behaviors included: using a handheld phone (likely talking on the phone), manipulating a handheld phone (potentially texting, reading an email, or dialing a number), and using an earpiece (hands-free calling device). All studies described behavior changes for individual waves of enforcement, reporting pre- and post-wave safety outcome data.

Typically, studies measured handheld phone use rates with in-person observations. Both Chaudhary et al. studies (2012 and 2015) used spotters who recorded data on paper forms about three types of cell phone use: handheld phone, in-ear device, or manipulating a device. Similarly, Retting et al. (2017) used trained data collectors who made street-side observations while in plain view. They recorded data on three types of phone use: driver holding handheld phone to ear, driver holding handheld phone to mouth, or driver manipulating a handheld electronic device. If the exact type of phone use was ambiguous, it was coded as manipulation. While all 5 studies reported the percentage of drivers observed using a handheld phone, the other measures were not reported by all studies. Using that metric, the studies tended to observe gradual decreases in the number of distracted drivers from several waves of enforcement (see Figure 4).




*Note.* As discussed previously, studies reporting information on the same HVE campaigns have been grouped, reporting only one data point per wave per treatment location. (Group 1: Schick et al., 2014, and Chaudhary et al., 2015, and Group 2: Cosgrove et al., 2010, and Chaudhary et al., 2012)

*Note:* Chaudhary et al. (2015) did not report pre-wave 3 data for Delaware or the Greater Sacramento Region, indicated by a green triangle and a purple X for post-wave 3 data with no corresponding symbols present for pre-wave 3.

*Figure 4. Percentage of drivers observed using a handheld device, by wave and location*

## 4.5 Relationship Between Enforcement and Safety Outcomes

While not all studies explicitly define the campaign enforcement type as HVE, the 4 studies examining the impact of "Phone in One Hand, Ticket in the Other" have been grouped as such because each campaign combined enforcement, visibility elements, and a publicity strategy. Each study took an individualized approach to enforcement. California used overtime roving patrols, spotters, and motorcycle patrols. Connecticut used spotters. Due to concern over the legality of spotters, Delaware favored use of stationary and roving patrols (Chaudhary et al., 2015). Meanwhile, New York preferred roving patrols where police could discretely observe distracted drivers. The studies did not report information on how much enforcement was conducted during the baseline period prior to enforcement, so a change in enforcement (baseline to campaign) cannot be calculated.

Ideally, enforcement would be measured by officer enforcement hours or dollars. However, for all 5 studies, their most prevalent measure of enforcement was the number of tickets, citations, or violations. This is problematic because the goal of HVE is deterrence. While a successful HVE campaign will likely have more citations at the start of the program than in the period right before it started, it should show a decrease in citations over time as individuals are deterred from driving while distracted. Consider a successful HVE campaign where increased enforcement in the form of more officer hours spent on enforcement leads to fewer people engaged in distracted driving, fewer citations being written, and better safety outcomes. In this example, however, measuring enforcement based on number of citations would suggest that a decline in enforcement (fewer citations) leads to improved safety outcomes (lower rates of distracted driving) while enforcement effort actually increased (more officer hours) and contributed to improved safety.

Measuring the number of personnel or amount of money spent on enforcement is a more accurate measure when trying to assess the relationship between changes in enforcement to a change in safety outcome. Therefore, this analysis focused on cases that included primary information on the amount of enforcement used.

### 4.5.1 Crashes Attributed to Distracted Driving

Aside from number of violations, Chaudhary et al. (2015) reported the cost of overtime roving patrols and the cost of media buys to enhance visibility of the campaign. To finance overtime roving patrols, Delaware allocated roughly $382,300, while California allocated roughly $513,100. Both values have been adjusted to 2018 dollars using the *Gross Domestic Product: Implicit Price Deflator* available from the Federal Reserve Bank of St. Louis (Bureau of Economic Analysis, 2019).

For media buys, Delaware allocated $158,500, while California allocated over $958,000 (both values have been adjusted to 2018 dollars). Chaudhary et al. (2015) reported various safety outcomes, including the total number of crashes that occurred during the program as well as the percentage of crashes attributed to distracted driving (see Table 26). To account for population differences across geographic locations, the total media cost and dollars spent on overtime roving patrols were normalized and presented per 1,000 residents. All population estimates were taken from the Census Bureau, via the Google Public Data Search Module (Census Bureau, 2019).

*Table 23. Distracted Driving Enforcement and Safety Outcomes, Chaudhary et al. (2015)*

| Location | Total Cost (Media) per 1,000 Residents, 2018$ | Dollars Allocated to Finance Overtime Roving Patrols per 1,000 residents, 2018$ | Distracted Driving-Attributed Crashes, Pre-Wave 1 | Distracted Driving-Attributed Crashes, During/Post-Wave 3 | Absolute Change | Percentage Change |
|---|---|---|---|---|---|---|
| Greater Sacramento Region | $237.50 | $127.19 | 7.7% | 6.6% | -1.1% | -14% |
| Delaware | $171.59 | $413.88 | 1.0% | 0.5% | -0.5% | -50% |

Both HVE programs devoted funds to increasing awareness of distracted driving laws as well as targeted enforcement, and both the Greater Sacramento Area and Delaware saw a decrease in the percentage of distracted driving-attributed crashes. California saw an increase in the absolute number of crashes, both total and distracted driving-related, but a smaller percentage of crashes were attributed to distracted driving during the program year. Notably, when looking across program waves, there was much more variation—for some waves, crashes increased, while for other waves, crashes decreased; however, the overarching trend was a decrease in the percentage of crashes attributed to distracted driving from pre-wave 1 to during/post-wave 3.

### 4.5.2 Dedicated Hours of Enforcement and Hand-Held Phone Use Rate Changes

Several studies also reported total number of dedicated officer enforcement hours by wave (see table below). These were additional officer enforcement hours specific to the program and, as such, do not measure the typical hours of enforcement still being worked during routine law enforcement activities. For programs with a media component, the studies also reported the dollars spent on media buys during the enforcement campaign. Again, no baseline measure was provided, so the reported hours and media spending are for the "during" portion of a before-during-after study. Because both dedicated officer enforcement hours and dollars spent on overtime roving patrols measure some of the same enforcement inputs, the decision was made only to report dedicated officer enforcement hours per 1,000 residents.

*Table 24. Distracted Driving Dedicated Officer Hours of Enforcement*

| Location | Dedicated Hours of Enforcement per 1,000 Residents | Total Paid Cost (Media) per 1,000 Residents, 2018$ | Handheld Phone Use (%) Pre-Wave 1 | Handheld Phone Use (%) Post-Final Wave | % Change Pre-to-Post-Final Wave |
|---|---|---|---|---|---|
| Towns in Connecticut | 21.21 | $0.00 | 2.7% | 1.6% | -41% |
| Delaware | 8.05 | $172.82 | 4.5% | 3.0% | -33% |
| Hartford | 39.93 | $3,213.68 | 6.6% | 2.9% | -56% |
| Towns in Massachusetts | 6.81 | $0.00 | 7.7% | 6.3% | -18% |
| Greater Sacramento Region | 2.34 | $238.99 | 4.1% | 2.7% | -34% |
| Syracuse | 36.89 | $1,282.78 | 3.7% | 2.5% | -32% |

Across waves of enforcement, outcomes fluctuated. However, overall the enforcement campaigns were successful at reducing the percentage of drivers using handheld phones while driving. To summarize the general results of the 6 enforcement, it is possible to perform a simple vote-count. Looking strictly at the observed change in overall handheld phone use between the pre-period of the first wave and the post-period of the final wave, 6 of 6 locations showed a decrease. The likelihood (probability) that this would occur by chance alone, calculated with a simple sign test, is 1 out of 64, or 0.0156.

Figure 5 shows the results from the six data points from the previous table with the officer enforcement hours on the x-axis and the percentage-point change in handheld phone use on the y-axis. As the graph shows, five of the six results had very similar changes in handheld phone use, despite the variation in additional hours of enforcement. Generally, this graph indicates that these campaigns all achieved similar outcomes, regardless of the amount of additional hours dedicated to enforcement, with the exception of the one outlier from Chaudhary et al. (2012).

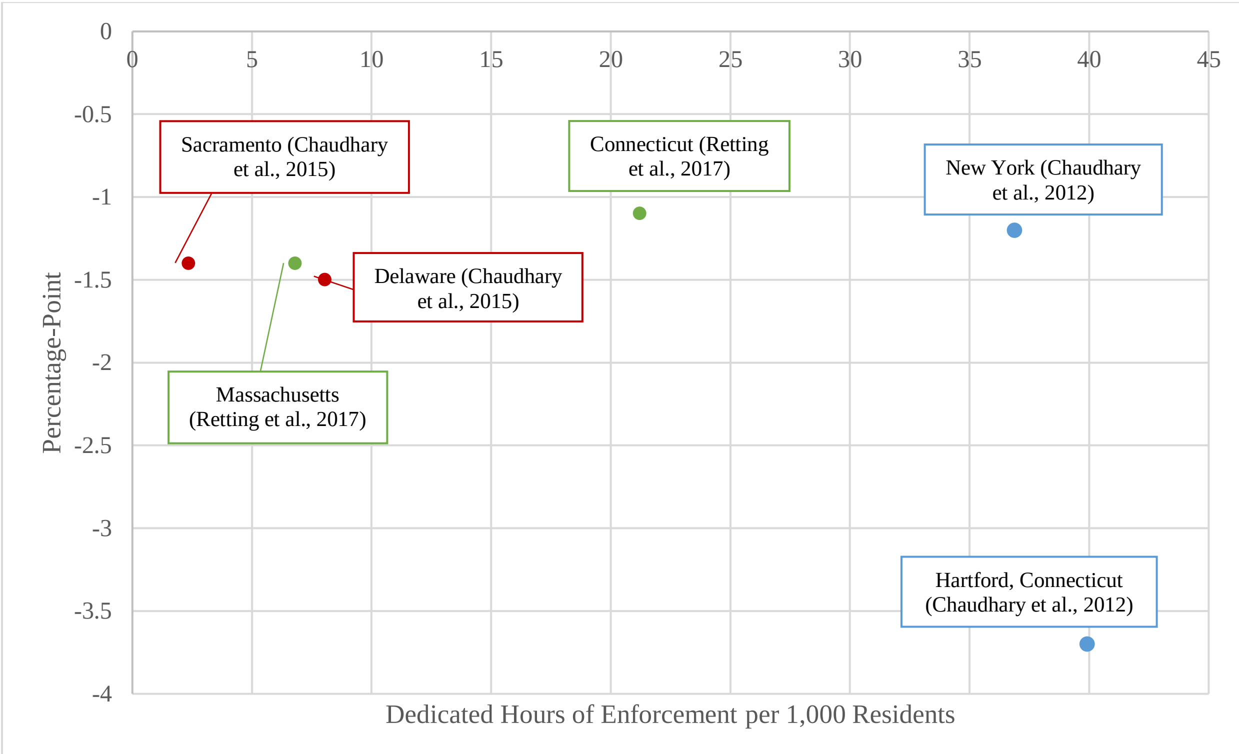



*Figure 5. Officer enforcement hours versus change in handheld phone use*

Figure 6 shows the results from the six data points with the paid media spending on the x-axis and the percentage-point change in handheld phone use on the y-axis. This graph shows a somewhat similar trend as Figure 5, in that for five of the data points, there are similar changes in handheld phone use for various amounts of paid media spending. There are also two data points that had no paid media spending, which further complicates the analysis of this data, as that is one-third of the entire data set. The one outlier from Chaudhary et al. (2012) appears to indicate that greater paid media spending can lead to greater reductions in handheld phone use, but that finding is based on a single data point and is therefore not robust. There could be other features of the Connecticut effort studied in Chaudhary et al. (2012) that account for its success. Additionally, the available data only cover the amount spent on paid media by the campaign, and they do not include any measure of earned media or publicity that was conducted outside the campaign.

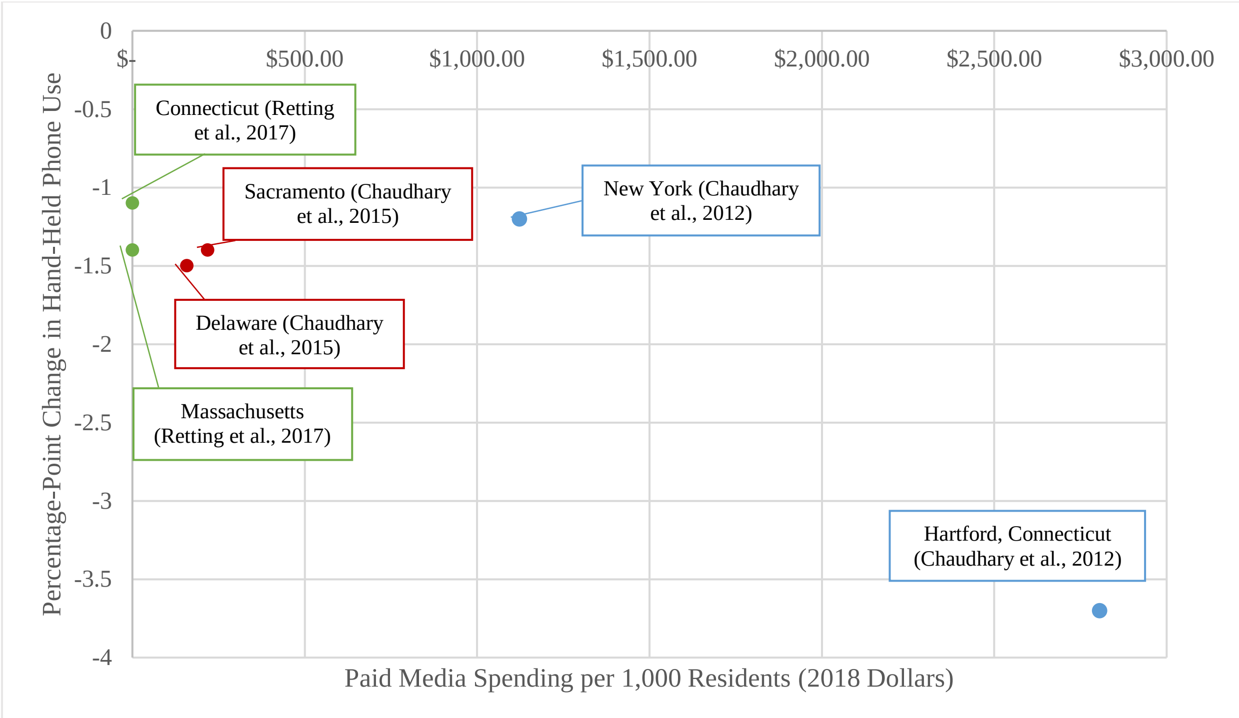



*Figure 6. Paid media spending versus handheld phone use*

With only six data points, it is difficult to draw strong conclusions about the results. The paid media findings in Figure 6 seem to support the idea that increasing paid media spending will further reduce handheld phone use, but it is due to the influence of a single study. However, given that all six programs were successful at reducing handheld phone use, the range of enforcement levels used in these efforts could serve as a useful guide for future efforts. On average, these six programs used 19.2 additional officer enforcement hours per 1,000 residents, spent $718 per 1,000 residents on paid media, and had a baseline handheld phone use rate of 4.9 percent. Although this section was unable to determine the specific impact of incremental increases in enforcement or media, the results indicated that these programs were successful at decreasing handheld phone use.

### 4.5.3 Control Locations: Difference-in-Difference Tests

The previous discussion relied on simple comparison of the distracted driving rates at a certain location before and after a series of enforcement waves. There may have been external factors that caused changes in distracted driving rates over that time span that are unrelated to the enforcement, For instance, a nationwide anti-distracted driving campaign could have been deployed during that same period. To control the impact of such external factors, the available studies included measures of distracted driving at both the treatment locations (as analyzed in the previous section) and at control locations. In the interest of completeness, analysis that considers the observations at the control locations is presented here.

This analysis uses a "difference-in-difference" approach to assess the impact of introducing targeted distracted driving enforcement in the treatment location relative to one or more control locations. Difference-in-difference analysis compares the means of two populations across time, before and after the treatment is applied to the control location. The general equation is shown below in Equation 4 on the next page.

$$(Avg.Pre \ - Avg.Post)_{Control\ Location} - (Avg.Pre \ - Avg.Post)_{Treatment\ Location} \quad (4)$$

Broadly speaking, the average handheld phone use rate of the treatment locations was compared to the average handheld phone use rate in the control locations, both before and after the treatment took place. The difference describes the impact of targeted distracted driving enforcement. The table below provides a list of treatment and control locations used in this synthesis.

*Table 25. List of Distracted-Driving Treatment and Control Locations With Populations*

| Study | Treatment Locations | Treatment Population | Control Locations | Control Population |
|---|---|---|---|---|
| Chaudhary et al., 2012 | Hartford, CT | 125,403 | Stamford and Bridgeport, CT | 267,762 |
| | Syracuse, NY | 145,283 | Albany, NY | 97,756 |
| Chaudhary et al., 2015 | State of Delaware | 923,638 | Atlantic County, NJ and New Haven County, CT | 1,140,512 |
| | Greater Sacramento Region: El Dorado, Sacramento, San Joaquin, Stanislaus, Solano, Sutter, Placer, Yuba, and Yolo Counties, CA | 4,034,207 | Portland, OR | 609,059 |
| Retting et al., 2017 | Bethel, Brookfield, Danbury, Monroe, Newton, Redding, and Ridgefield, CT | 202,563 | East Lyme, Groton, Ledyard, Montville, New London, Norwich, Stonington, and Waterford, CT | 168,609 |
| | Andover, Dracut, Dunstable, Lawrence, Lowell, Methuen, North Andover, North Reading, Reading, Tewksbury, Tyngsborough, and Wilmington, MA | 441,248 | Chicopee and Springfield, MA | 210,037 |

Two difference-in-difference equations were used, testing both wave level data and baseline to post-final data. The wave level data included two observations per wave, pre- and post-data, and per location, treatment or control, for a total of 44 observations. For this analysis, the difference-in-difference coefficient was small, just -0.003 (or -0.3%), and not statistically significant ($p$ = 0.676).

The data was also analyzed by aggregating across waves to compare the baseline observed handheld phone use rates (pre-Wave 1) to post-final wave rates resulting in two observations per location (one for the control location and one for the treatment locations). There were 13 observations (one treatment location had two control locations). Using these data, the difference-in-difference coefficient was still found to be negative, small, and not statistically significant ($p$ = 0.84).

These two difference-in-difference approaches applied to study-level results did not find that the HVE produced reductions in distracted driving behavior compared to control locations.

## 4.6 Conclusion

In examining the impact of HVE on handheld phone use rates, all estimates show improvement when comparing pre- and post-enforcement campaign values. Specifically, across the 6 enforcement that were studied in the available literature, HVE have reduced drivers' handheld phone use rates. Baseline rates averaged 4.9 percent and enforcement produced reductions of 1.1 to 3.7 percentage points, averaging a 36 percent decrease in drivers' handheld phone use. In summary, while in some cases wave to wave results may have shown a mix of improvement and decline, distracted driving HVE campaigns were overall successful at reducing the percentage of drivers using handheld phones while driving.

In examining the impact of HVE on crashes, the percentage of crashes due to distraction decreased in both cases (the Greater Sacramento Region and in the State of Delaware), suggesting that targeted distracted driving enforcement was effective. However, caution should be taken when interpreting this result since it is based on just two data points. Another factor to be aware of when attempting to attribute changes in enforcement to changes in number of distracted driving crashes is the difficulty in ascribing crashes as being caused by distracted driving. As discussed earlier, it is likely that the available data underestimate the number of crashes attributable to distracted driving, so no conclusion can be drawn regarding the effectiveness of HVE at reducing distraction-related crashes.

Overall, the main findings from these distracted driving studies were:

- HVE has shown a reduction in drivers' handheld phone use rates, ranging from 18 to 56 percent. Baseline handheld phone use rates averaged 4.9 percent; HVE reduced drivers' handheld phone use an average of 1.7 percentage points. However, in cases where the analysis allowed for comparisons with a control group, no statistically significant difference was detected. Consequently, this result should be taken with caution.
- No relationship was detected for dedicated officer enforcement hours or media spending, but additional analysis using a larger dataset would be needed to verify that finding.

# 5 Impaired Driving

This chapter reviews the 19 relevant studies on alcohol-impaired driving enforcement programs in the United States. Many substances can impair driving, including alcohol, some over-the-counter and prescription drugs, and illegal drugs. This chapter focuses on alcohol-impaired driving. Alcohol-impaired driving or drunk driving is the act of operating any motor vehicle with impaired ability as a result of alcohol consumption, or with a BAC in excess of the legal limit (NCSA, 2018b). The table below provides a State-specific summary of DUI laws (as of December 2018).

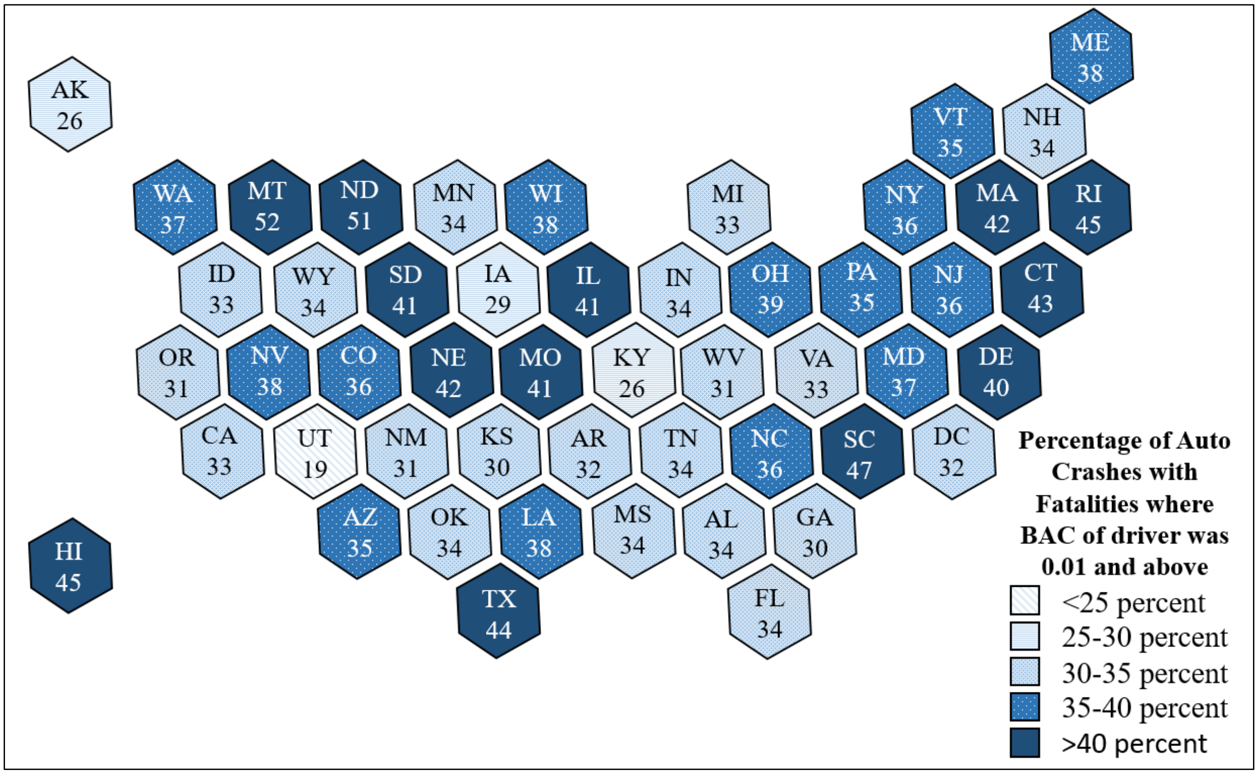
*Table 26. DUI Laws by State*

| States | BAC Limit for People Under Age 21 | BAC Limit for People 21 and Older |
|---|---|---|
| Alaska, Arizona, District of Columbia, Illinois, Maine, Maryland, Michigan, Minnesota, North Carolina, Oklahoma, Oregon, Pennsylvania, Texas, Wisconsin | .00 | .08 |
| California, New Jersey | .01 | .08 |
| Alabama, Arkansas, Colorado, Connecticut, Delaware, Florida, Georgia, Hawaii, Idaho, Indiana, Iowa, Kansas, Kentucky, Louisiana, Massachusetts, Mississippi, Missouri, Montana, Nebraska, Nevada, New Hampshire, New Mexico, New York, North Dakota, Ohio, Rhode Island, South Carolina, South Dakota, Tennessee, Vermont, Virginia, Washington, West Virginia, Wyoming | .02 | .08 |
| Utah | .00 | .05 |

*Note:* Table sourced from the Governors Highway Safety Association (GHSA) (n.d.-a)

*Note:* With States that have .00 as the limit, the State is a true "zero tolerance" State in that any detectible amount of alcohol in their system would be a violation of the statute. In other words, .00 is the only acceptable BAC for a driver under age 21. Some States have opted for slightly higher thresholds of .01 or .02 BAC. The arguments for having a slightly higher limit vary (e.g., using cough syrup); however, the violation for "zero tolerance" is still the same. BACs are reported as grams per deciliter (g/dL), but States uses differing measurement terms.

Drivers are considered alcohol-impaired when their BACs are equal to or greater than the State-specific limits. Any crash involving a driver with a BAC over the limit is considered an alcohol-impaired crash, any fatal crash involving a driver with a BAC over the limit is considered an alcohol-impaired fatal crash, and any fatalities (including those of drivers, passengers, non-occupants) occurring due to those crashes are considered alcohol-impaired fatalities. The total number of alcohol-impaired driving fatalities in 2017 was 10,874, which is 29.3 percent of total crash fatalities (NHTSA, 2017). Figure 8 shows the prevalence of alcohol-impaired crashes by State.




*Note.* Graphic created using 2017 data from the Fatality Analysis Reporting System (FARS)

*Figure 7. Percentage of auto crashes with fatalities where BAC of the driver was .01 g/dL and above, by State*

This chapter reviews the available literature on alcohol-impaired driving enforcement in the United States. Each study varied in terms of time-period, location, type of enforcement, presence of supplementary activities (publicity, training, education and outreach, etc.), and analysis techniques. The next section (Description of Impaired Driving Enforcement Evaluation Studies) provides a summary of the impaired driving studies included in this report. The section titled Methods of Enforcement discusses the different types of enforcement activities used in the programs reviewed in this report, the Publicity section discusses the types of publicity activities conducted, and the Safety Outcomes section explains the different ways that studies measure their safety outcome. Finally, the last section summarizes the studies' findings regarding the effect of impaired driving enforcement activities and publicity on different safety outcomes, divided by enforcement types.

## 5.1 Description of Impaired Driving Enforcement Evaluation Studies

After screening studies for relevance and content, 19 impaired driving studies were found to be appropriate for inclusion in this analysis. The studies explored various sizes of treatment locations, including specific intersection, towns, counties, States, and the entire country. In terms of methods, 7 are before-after or before-during comparison studies, 8 studies are interrupted time-series studies, and 4 studies use multivariate regression analysis.

- Before-after or before-during comparison studies are those studies that present safety outcome measures prior to the enforcement effort and compare it to the same metric as measured either during the enforcement effort or after the effort is completed.

- Interrupted time-series method is an approach to assess the impact of a policy change with longitudinal or time series data (Lagarde, 2011; Grimshaw et al., 2003). These studies use data collected at equally-spaced intervals of time before and after an intervention, and do not necessarily require a control site (Grimshaw et al., 2003). Data series are usually long to allow for various techniques to test for statistical significance of changes in trends or patterns of safety outcomes over time. In an autoregressive integrated moving average model, a form of an interrupted time series approach, the rule of thumb is that there should be at least 50 but preferably more than 100 observations (Box & Tiao, 1975). The primary driving force behind these numbers is that if there are seasonal components, more observations are needed to analyze trends and patterns. The approach has been used to assess the consequences of a variety of policy issues in various fields, such as environmental policies (Box & Tiao, 1975), financial economics (Ho & Wan, 2002), and, in some cases, health policies (Van Driel et al., 2008; Chan et al., 2009; Zhang et al., 2009).
- Multivariate regression analysis is a statistical method that allows one to control for several factors that may have an impact on an outcome measure of interest. The results of a multivariate regression allow one to understand which factors impact an outcome, the magnitude of the impacts, and to perform tests of statistical significance.

The table below provides a description of the available government reports and journal articles relating to impaired driving, including the study location, the time-period analyzed, and the analysis method.

*Table 27. Background Information on Alcohol-Impaired Driving Studies*

| Study | Geographic Size of Analysis | Specific Location | Period of Enforcement | Method |
|---|---|---|---|---|
| Agent et al., 2002 | State | Kentucky | August 22, 2002–September 3, 2002 | Before-During Comparison Study |
| Beck, 2009 | | Maryland | 2002–2004 | |
| Beck et al., 2018 | County | 4 Counties in Maryland: Anne Arundel, Baltimore, Montgomery, Prince Georges | May 2013–Oct 2013 | Interrupted Time-Series Study |
| Creaser et al.2007 | | 13/14 Counties in Minnesota (specific counties vary by year) | Initial implementation of Operation NightCAP (1998–2002); 13 Deadliest Impaired Driving Counties (2003–2005) | Multivariate Regression |
| Fell et al., 2005 | State | Georgia, Louisiana, Pennsylvania, Tennessee | July 2000–September 2001 | Interrupted Time-Series Study |

| Study | Geographic Size of Analysis | Specific Location | Period of Enforcement | Method |
|---|---|---|---|---|
| Fell et al., 2014 | National | National | January 2007–December 2007 | Multivariate Regression |
| Lacey et al.1999 | State | Tennessee | April 1994–March 1995 | Interrupted Time-Series Study |
| Lacey et al., 2000 | | New Mexico | December 1993–December 1995 | Interrupted Time-Series Study |
| Lacey et al., 2006 | County | 4 Counties in West Virginia: Harrison and Monongalia, Raleigh and Greenbrier | August 2003–August 2004 | Before-During Comparison Study |
| McCartt et al., 2009 | City | 2 Cities in West Virginia: Huntington, WV (Treatment) and Morgantown, WV (Control) | Winter 2006–Fall 2007 | Interrupted Time-Series Study |
| Niederdeppe et al., 2017 | National | National | 1996–2010 | Multivariate Regression |
| Nunn, & Newby, 2011 | Intersection | 18 intersections/locations in Indianapolis, Indiana | Oct 2008–Nov 2009 | Interrupted Time-Series Study |
| Ramirez et al., 2014, March | State | New Mexico | July 2005–March 2009 | Before-During Comparison Study |
| Solomon et al., 2008 | | All 50 States | August 2006–September 2006 | |
| NHTSA, 2007 | | | 2003–2005 | |
| Syner et al., 2008 | | Alaska, Arizona, California, Florida, Georgia, Louisiana, Mississippi, Montana, Missouri, New Mexico, Ohio, Pennsylvania, South Carolina, Texas, West Virginia | 2002–2004 | Before-During Comparison Study |
| Yao et al., 2016 | Several States | 30 States and DC | 1996–2006 | Multivariate Regression |
| Zwicker et al., 2007 | State | Connecticut | March 2003–January 2004 | |

| Study | Geographic Size of Analysis | Specific Location | Period of Enforcement | Method |
|---|---|---|---|---|
| | | West Virginia | July 2003–September 2005 | Interrupted Time-Series Study |

## 5.2 Methods of Enforcement

This section contains a brief overview of the enforcement strategies and their prevalence in the alcohol-impaired driving studies. For the tables in this section, each method of enforcement is linked to the studies that included a discussion of it. An important reminder is that HVE is an overarching enforcement strategy, in which law enforcement agencies have discretion of what type of enforcement to deploy. Enforcement type options include, but are not limited to, saturation patrols and checkpoints.

### *5.2.1 Sobriety Checkpoints*

A type of enforcement, sobriety checkpoints (also called DUI checkpoints) are obstructions placed across a road for halting or hindering traffic, used by police to facilitate compliance checks (NHTSA, n.d.-d). Sobriety checkpoints, specifically, are locations where law enforcement officers are stationed to check drivers for signs of intoxication and impairment (GHSA, n.d.-c).

Many jurisdictions utilize sobriety checkpoints as part of their larger drunk driving deterrence program. However, not all States conduct sobriety checkpoints due to State-level legal issues surrounding their use with some State laws authorizing their use and others forbidding them or being silent on the issue. States with no explicit statutory authority may or may not conduct checkpoints. The lists below from GHSA (n.d.-a) show the States that allow and do not allow checkpoints.

***Sobriety Checkpoints Allowed***

Alabama, Arizona, Arkansas, California, Colorado, Connecticut, Delaware, District of Columbia, Florida, Georgia, Hawaii, Illinois, Indiana, Kansas, Kentucky, Louisiana, Maine, Maryland, Massachusetts, Mississippi, Missouri, Nebraska, Nevada, New Hampshire, New Jersey, New Mexico, New York, North Carolina, North Dakota, Ohio, Oklahoma, Pennsylvania, South Carolina, South Dakota, Tennessee, Utah, Vermont, Virginia, West Virginia

***Sobriety Checkpoints Not Allowed***

Alaska, Idaho, Iowa, Michigan, Minnesota, Montana, Oregon, Rhode Island, Texas, Washington, Wisconsin, and Wyoming

### 5.2.2 HVE

HVE is a hybrid enforcement strategy combining enforcement, visibility elements, and a publicity strategy to both educate the public and encourage voluntary compliance with existing laws. Checkpoints, saturation patrols, and other enforcement activities are bolstered by increased publicity and warnings to the public in an HVE campaign. HVE incorporates visibility elements (e.g., electronic message boards, road signs, command posts, blood alcohol testing mobiles also known as "BAT mobiles," etc.) designed to make enforcement obvious to the public. It is supported by a coordinated communication strategy and publicity. HVE may also be enhanced through multi-jurisdictional efforts and partnerships between people and organizations dedicated to the safety of their community. An example of a multi-jurisdictional effort is the Strategic Evaluation States (SES) initiative. More details on this initiative will be provided in the section "Impacts of HVE: Multi-State Results From Before-During Comparison Studies." The table below provides the summary of different enforcement types used in the studies.

*Table 28. Summary of Alcohol-Impaired Driving Enforcement Types and Studies*

| Enforcement Type | Studies |
|---|---|
| Checkpoint | Lacey et al., 2006; Nunn and Newby, 2011 |
| HVE | Agent et al., 2002; Beck et al., 2018; Beck, 2009; Creaser et al., 2007; Fell et al., 2005; Lacey et al., 1999; Lacey et al., 2000; McCartt et al., 2009; Ramirez et al., 2014; Solomon et al., 2008; NHTSA, 2007; Syner et al., 2008; Zwicker, Chaudhary, Maloney, et al., 2007; Zwicker, Chaudhary, Solomon, et al., 2007 |
| Publicity | Niederdeppe et al., 2017 |
| Unspecified | Fell et al., 2014; Yao et al., 2015 |

## 5.3 Publicity

Often, enforcement campaigns involve publicity activities that supplement the enforcement, such as raising awareness of targeted enforcement activities and informing the public of the dangers of impaired driving. While not part of enforcement activities, like checkpoints or covert patrols, these publicity activities are a component of the enforcement campaign and likely have an impact on the observed safety outcome.

Because these publicity activities often occur concurrently with enforcement activities, it is difficult to attribute changes in the safety outcome to either the presence of enforcement or the presence of the publicity. The table below provides a summary of all the publicity used with the impaired driving enforcement in the reviewed studies.

*Table 29. Publicity Activities in Alcohol-Impaired Driving Enforcement*

| Publicity Types | Studies |
|---|---|
| Paid Media (television, radio, billboards, etc.) | Agent et al., 2002; Beck, 2009; Creaser et al., 2007; Fell et al., 2005; Lacey et al., 1996; Lacey et al., 1999; Lacey et al., 2000; Lacey et al., 2006; McCartt et al., 2009; NHTSA, 2007; Niederdeppe et al., 2017; Nunn and Newby, 2011; Ramirez et al., 2014; Solomon et al., 2008; Syner et al., 2008; Zwicker, Chaudhary, Maloney, et al., 2007; Zwicker, Chaudhary, Solomon, et al., 2007 |
| Earned Media (press releases, project kickoff press conferences) | Beck, 2009; Fell et al., 2005; Lacey et al., 1996; Lacey et al., 1999; NHTSA, 2007; Niederdeppe et al., 2017; Ramirez et al., 2014; Solomon et al., 2008; Syner et al., 2008; Zwicker, Chaudhary, Maloney, et al., 2007; Zwicker, Chaudhary, Solomon, et al., 2007 |
| PSAs | Creaser et al., 2007; Lacey et al., 1999; NHTSA, 2007; Niederdeppe et al., 2017; Ramirez et al., 2014; Solomon et al., 2008; Zwicker, Chaudhary, Maloney, et al., 2007; Zwicker, Chaudhary, Solomon, et al., 2007 |
| Slogans | Agent et al., 2002; Creaser et al., 2007; McCartt et al., 2009; NHTSA, 2007; Niederdeppe et al., 2017; Ramirez et al., 2014; Solomon et al., 2008; Syner et al., 2008; Zwicker, Chaudhary, Solomon, et al., 2007 |

## 5.4 Safety Outcomes

Impaired driving studies included in this synthesis discussed a wide variety of safety outcome measures. Possible reasons for the wide variation include the following:

1. Safety outcomes may come in the form of reduction in prevalence of the proscribed behavior or reduction in impaired crashes or fatalities.
2. Because different States have different laws, different measures may be considered in different studies (i.e., BAC levels of .01, .02, .05, and .08 g/dL).
3. Different crash severity levels may also be looked at (i.e., crashes resulting in injury, crashes resulting in fatality, or overall count of crashes or fatalities). Note that alcohol-impaired fatalities are not the same as alcohol-impaired fatal crashes as there may be more than one fatality in a single fatal crash.
4. Within broad categories of safety outcomes, the units used to measure the safety outcomes displayed considerable variation. In some cases the *total* number of alcohol-impaired crashes or fatalities was reported, in others the *percent* of all crashes or fatalities that were alcohol-impaired was reported. In some cases the study authors reported rates such as the number of alcohol-impaired crashes *per* capita or *per* VMT. Scales of measure also differed. Some measures reported *monthly* measures while the majority reported *annual* measures. This diversity is due to variability in program durations—some programs ran only for a few months while others lasted for a few years. In some cases the original metrics reported by the study authors have been converted to allow for comparisons among studies.

In addition, a single study may report several of these safety outcome measures; hence, the count of observations is greater than the number of studies. The alcohol-impaired crash metrics included different units (percent, total), scales (year, month), and levels of severity (alcohol-impaired fatal crashes or injury crashes). Sixteen studies resulted in 24 unique unit measures related to crashes and 122 observations that are summarized in the table below.

*Table 30. Measures of Crash Related Safety Outcomes in Alcohol-Impaired Driving Studies*

| **Safety Outcome Measure** | **Studies** | **Count of Studies** | **Count of Observations** |
|---|---|---|---|
| Alcohol-Impaired Crashes, Percentage Annual | Agent et al., 2002; Creaser et al., 2007 | 2 | 4 |
| Alcohol-Impaired Crashes, Total Annual | Agent et al., 2002; Beck, 2009; Creaser et al., 2007; Nunn and Newby, 2011 | 4 | 6 |
| Alcohol-Impaired Crashes, per 100,000 Residents Adults | Niederdeppe et al., 2017 | 1 | 3 |
| Alcohol-Impaired Crashes, per 100,000 Residents Underage | | 1 | 3 |
| Alcohol-Impaired Fatal Crashes, Percentage Annual | Creaser et al., 2007; Ramirez et al., 2014 | 2 | 3 |
| Alcohol-Impaired Fatal Crashes, Total Annual | Beck, 2009; Creaser et al., 2007; Lacey et al., 1999. | 3 | 3 |
| Alcohol-Impaired Fatal Crashes, Total Annual (BAC $\geq$ .10) | Lacey et al., 2000 | 1 | 1 |
| Alcohol-Impaired Fatal Crashes, Total Month | Beck et al., 2018 | 1 | 1 |
| Alcohol-Impaired Injury Crashes, Percentage Annual | Agent et al., 2002; Creaser et al., 2007 | 2 | 5 |
| Alcohol-Impaired Injury Crashes, Total Annual | Agent et al., 2002; Beck, 2009 | 2 | 3 |
| Alcohol-Impaired Injured Drivers, Total Annual | Beck, 2009 | 1 | 1 |
| Alcohol-Impaired Injured Pedestrians, Total Annual | | 1 | 1 |
| Alcohol-Impaired Fatalities, Percentage Annual | Syner et al., 2008 | 1 | 15 |

| Safety Outcome Measure | Studies | Count of Studies | Count of Observations |
|---|---|---|---|
| Alcohol-Impaired Fatalities, Total Annual | Beck, 2009; Solomon et al., 2008; NHTSA, 2007 | 3 | 1 |
| Alcohol-Impaired Fatalities, Total Month | Zwicker, Chaudhary, Maloney, et al., 2007; Zwicker, Chaudhary, Solomon, et al., 2007 | 2 | 2 |
| Alcohol-Impaired Fatalities, Rate Annual | Yao et al., 2015 | 1 | 52 |
| Alcohol-Impaired Fatalities, Rate per 100 Million VMT | Nunn and Newby, 2011 | 1 | 2 |
| Ratio of Alcohol-Impaired Fatalities to Annual VMT | Fell et al., 2005 | 1 | 1 |
| Single-Vehicle Nighttime (SVN) Injury Crashes, Total Annual | Lacey et al., 1999 | 1 | 4 |
| Ratio of SVN Crashes to Multi-Vehicle Daytime Crashes | Beck et al., 2018 | 1 | 1 |
| Crash Incidence Ratio (Ratio of Impaired Driving Crashes to Non-Impaired Driving Crashes) BAC > .00 g/dL | Fell et al., 2014 | 1 | 2 |
| Crash Incidence Ratio (Ratio of Impaired Driving Crashes to Non-Impaired Driving Crashes) BAC ≥ .01 g/dL | Fell et al., 2005 | 1 | 4 |
| Crash Incidence Ratio (Ratio of Impaired Driving Crashes to Non-Impaired Driving Crashes) BAC ≥ .05 g/dL | Fell et al., 2014 | 1 | 2 |
| Crash Incidence Ratio (Ratio of Impaired Driving Crashes to Non-Impaired Driving Crashes) BAC ≥ .08 g/dL | Fell et al., 2014 | 1 | 2 |

Four studies resulted in 17 unique behavior-related safety outcome measures. Two of the measures relate to counts of drivers with positive BACs and the other 15 measures relate to prevalence (measured as a percentage of drivers). The measures exhibited additional variation in terms of the BAC level evaluated (.01, .02, .05, and .08 g/dL) and age groups. In total, 118 observations of behavior-related safety outcomes were available from 4 studies. The next table provides a summary of all safety outcomes measuring alcohol-impaired behavior.

*Table 31. Measures of Behavior Related Outcomes in Alcohol-Impaired-Driving Studies*

| Safety Outcome Measure Percentage Annual | Studies | Count of Studies | Count of Observations |
|---|---|---|---|
| Drivers 16 - 20 With BAC > .00 | McCartt et al., 2009 | 1 | 1 |
| Drivers 16 - 20 With BAC ≥ .02 | | | |
| Drivers 16 - 20 With BAC ≥ .05 | | | |
| Drivers 16 - 20 With BAC ≥ .08 | | | |
| Drivers 21 - 24 With BAC > .00 | | | |
| Drivers 21 - 24 With BAC ≥ .02 | | | |
| Drivers 21 - 24 With BAC ≥ .05 | | | |
| Drivers 21 - 24 With BAC ≥ .08 | | | |
| Drivers 25+ With BAC > .00 | | | |
| Drivers 25+ With BAC ≥ .02 | | | |
| Drivers 25+ With BAC ≥ .05 | | | |
| Drivers 25+ With BAC ≥ .08 | | | |
| Drivers With BACs ≥ .01 | Lacey et al., 2006 | 1 | 1 |
| Drivers With BACs ≥ .01, Total Annual | Solomon et al., 2008; NHTSA, 2007 | 2 | 50 |
| Drivers With BACs ≥ .05 | Lacey et al., 2006 | 1 | 2 |
| Drivers With BACs ≥ .08 | Lacey et al., 2006 | 1 | 2 |
| Drivers With BACs ≥ .08, Total Annual | Solomon et al., 2008; NHTSA, 2007 | 2 | 51 |

## 5.5 Relationship Between Enforcement Activities and Safety Outcomes

Ideally, a study would provide comprehensive information on all the resources used as part of an enforcement effort. Such information would include: number of officer enforcement hours by type of enforcement (patrols, checkpoints, etc.), the cost of wages for those enforcement hours, number of checkpoints or patrols, amount of paid media measured in number of airings, the cost of the paid media, the amount of earned media, etc. However, this information is not provided for most studies. In addition, a clear baseline measure for both the enforcement levels and safety outcomes (i.e., before and after measures) is warranted in measuring impacts of enforcement activities. The descriptions of the intensity of the enforcement available in the literature were often incomplete. Only 44 observations from 13 impaired driving studies provide enforcement baselines. Nonetheless, certain insights have been obtained.

This section is organized by type of enforcement, The first section discusses the impacts of HVE, followed by the impacts of sobriety checkpoints, the impacts of publicity, and, finally, the Impacts of Unspecified Enforcement on Alcohol-Impaired Driving Safety Outcomes, which includes efforts described only by increased spending or staffing of officers without a description of what types of activities those resources were used for.

### *5.5.1 Impacts of HVE on Alcohol-Impaired Safety Outcomes*

A total of 14 studies investigated enforcement aimed at impaired driving using HVE. HVE implies combinations of different enforcement activities, i.e., checkpoints, patrols (which may be of several types), and publicity; hence, the way the enforcement is measured is diverse. In addition to diverse units of measure for enforcement, combinations of enforcement activities differ as well. Some studies have both checkpoints and patrols with publicity, some only have checkpoints with publicity, and others only patrols with publicity. With the limited number of studies in this area, identifying the contribution of each individual prong of an HVE effort was not possible. Figure 9 shows the different combinations of enforcement activities considered in the available impaired driving studies.

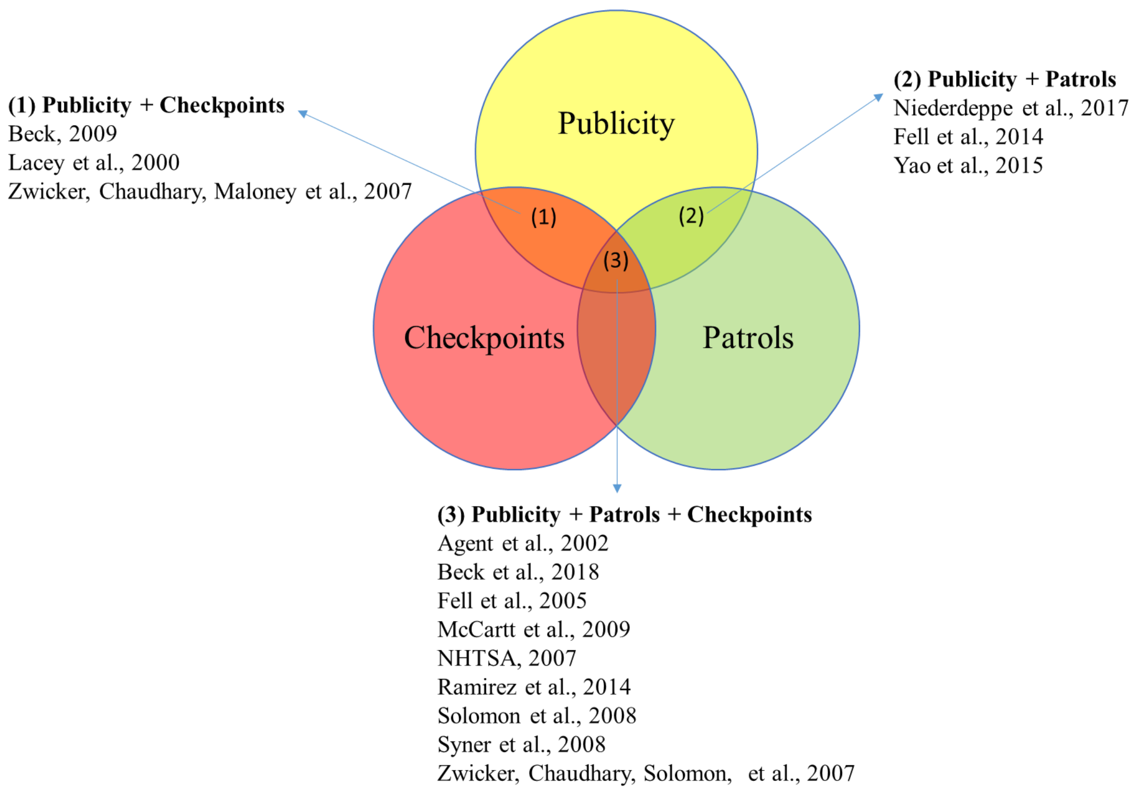



*Figure 8. Summary of enforcement activities combination for the HVE studies*

In addition to diversity in enforcement activity combinations (Figure 9) and units of measures for enforcement and geographic scale of these HVE studies, the methods of analysis also varied. Different methods of analysis tested different types of hypotheses; hence, what one can infer from their results also differs. Before-during comparison studies provide safety outcome measures before and during enforcement periods, which allows one to calculate a percent change or percentage-point change in safety outcomes resulting from an enforcement effort. However, these studies do not provide any statistical testing of the differences of the safety outcomes between the time-periods. On the other hand, interrupted time-series tests the null hypothesis of whether safety outcome measures before and during enforcement periods are statistically different. In the two tables that summarize the results found in the interrupted time series literature, significance levels are reported as they appeared in the studies. Finally, multivariate regressions test the significance of each factor or variable that may impact the safety outcome. This method enables one to measure incremental effects of additional amounts of enforcement activities while controlling for other factors that may impact observed results. Hence, studies based on multivariate regression analysis provide a stronger result upon which to infer the impacts of enforcement.

Because of the issues described above, the attempt to group the observations by enforcement measure and safety outcome measure pairings resulted in sparse data not suitable for quantitative analysis. Thus, this HVE impacts section presents a qualitative discussion of impacts of HVE activities on alcohol-impaired safety outcomes where comparable results are grouped by scale and analysis method.

### *5.5.2 Impacts of HVE: State-level Results From Before-During Comparison Studies.*

Three studies explored the impact of HVE on alcohol-impaired driving using State-level data and before-during analysis. The results from Kentucky and New Mexico show that by all the available measures, the HVE had the intended impact of reducing alcohol-impaired crashes. In Kentucky alcohol-impaired crashes were reduced by 9 percent while in New Mexico alcohol-impaired fatal crashes were reduced by 7 percent during the time-period of the enforcement, However, the results for the HVE effort in Maryland were mixed. While all alcohol-impaired crashes increased by 2 percent, the more severe injury and fatal crashes decreased by 5 percent and 3 percent, respectively. On the other hand, the number of alcohol-impaired fatalities increased by 15 percent, suggesting that, while there were fewer fatal crashes, the number of fatalities per crash increased. Thus, Beck (2009) found no evidence that alcohol-impaired fatalities or crashes improved during the first 3 years of the Maryland campaign. Beck suggested that the reasons for this finding included insufficient levels of enforcement (e.g., too few sobriety checkpoints and vehicle contacts occurred to raise public perceptions of the risk pertaining to impaired driving) and inadequate publicity surrounding this campaign. He supported the assertion regarding insufficient enforcement levels by examining alcohol-impaired citations from those enforcement activities and found that, on average, there were fewer State-wide citations during the enforcement campaign period than before the enforcement campaign. However, NHTSA has noted the goal of HVE campaigns is deterrence, which could result in fewer citations being issued (NHTSA, n.d.-f). The 3 studies are summarized in the table on the following page.

*Table 32. Summary of State-level Alcohol-Impaired Driving Results, HVE Studies Using Before-During Enforcement Comparison*

| Specific Location/Period of Enforcement | Publicity | Checkpoints, Enforcement Roadblocks | Saturation Patrols, Roving Patrols, Mobile Awareness Patrols | Enforcement (dollars) | Outcome Measure | Baseline | During | Change |
|---|---|---|---|---|---|---|---|---|
| Kentucky (Agent et al., 2002)<br><br>August 22, 2002 – September 3, 2002 | Total paid media: $322,825 | 465 Checkpoints using 4,513 officers working 129,576 hours | 3,830 officers were deployed in saturation patrols involving 125,832 working hours | unspecified | Alcohol-Impaired Crashes, Annual Percentage | 23% | 22% | -1% pt |
| | | | | | | 34% | 32% | -2% pt |
| | | | | | Alcohol-Impaired Crashes, Total Annual | 107 | 97 | -9% |
| | | | | | | 84 | 80 | -5% |
| New Mexico (Ramirez et al., 2014)<br><br>July 2005 – March 2009 | Total paid media $ spent on television and radio ads from 2006 - 2008 was $3,540,379 | 178 Total checkpoints and 7 low-staff checkpoints from October 2005 - March 2009 | 187 saturation patrols from October 2005 - March 2009 | $5,604,282 | Alcohol-Impaired Fatal Crashes, Annual Percentage | 29% | 27% | -2% pt |
| Maryland (Beck, 2009)<br><br>2002 – 2004 | Total paid media: $355,000 | 206 Checkpoints | None | unspecified | Alcohol-Impaired Crashes, Total Annual; | 8,811 | 9,001 | 2% |

| Specific Location/Period of Enforcement | Publicity | Checkpoints, Enforcement Roadblocks | Saturation Patrols, Roving Patrols, Mobile Awareness Patrols | Enforcement (dollars) | Outcome Measure | Baseline | During | Change |
|---|---|---|---|---|---|---|---|---|
| | | | | | Alcohol-Impaired Fatal Crashes, Total Annual; | 189 | 184 | -3% |
| | | | | | Alcohol-Impaired Injury Crashes, Total Annual; | 3,705 | 3,531 | -5% |
| | | | | | Alcohol-Impaired Fatalities, Total Annual; | 245 | 281 | 15% |
| | | | | | Alcohol-Impaired Injured Drivers, Total Annual | 2,378 | 2,287 | -4% |

### *5.5.3 Impacts of HVE: Multi-State Results From Before-During Comparison Studies.*

Three studies performed multi-State comparisons to evaluate the effectiveness of HVE using before-during comparisons: Syner et al. (2008); Solomon et al. (2008); and NHTSA (2007).

Syner et al. (2008) provided a summary of the impaired driving enforcement and communication activities for 13 (eventually, 15) States that participated in the Strategic Evaluation States (SES) program between 2002 and 2005. Solomon et al. (2008) and NHTSA (2007) studied the same program, but the analysis in this section is based on Solomon et al. (2008), since that study provides more years of data. Solomon et al. (2008) provided year-by-year observations from 2001 to 2006 of alcohol-impaired fatalities for 50 States. These multi-State studies analyzed HVE programs that covered the three elements: publicity, checkpoints, and patrols (i.e., group 3 in Figure 9).

In 2002 NHTSA undertook a new approach that focused strategically on reducing alcohol-impaired crashes, injuries, and deaths in States with especially high numbers or rates of alcohol-impaired fatalities. NHTSA identified 13 States to participate in the SES initiative: Alaska, Arizona, California, Florida, Georgia, Louisiana, Mississippi, Montana, New Mexico, Ohio, Pennsylvania, Texas, and West Virginia. In 2005 NHTSA invited Missouri and South Carolina to join the program, bringing the total number of States participating to 15. These 15 States accounted for more than half of the alcohol-impaired fatalities in the United States. The HVE program under the SES initiative included: (1) high-visibility, multi-agency enforcement operations on a monthly basis and year-round, with a focus on areas that accounted for 65 percent of the alcohol fatality problem; (2) “charismatic” leadership that secured commitments from law enforcement agencies and provided clear guidance on the direction of the DWI enforcement program; (3) law enforcement training; and (4) targeted messaging through earned and paid media along with outreach, As described in Syner et al. (2008), the SES enforcement resulted in an average 4.3 percent reduction in alcohol-impaired fatalities in the States that implemented the program. This reduction translates to roughly 12 avoided fatalities per State per year on average. This reduction was not found to be significant at the 5 percent level using a two-tailed t-test. This finding suggests that while there is variation among the success of the SES efforts across States, overall, the SES effort was not successful at significantly reducing alcohol-impaired fatalities.

In 2006 NHTSA modeled a national Labor Day holiday campaign entitled *Drunk Driving. Over the Limit. Under Arrest.* after a previously successful national program on seat belt use. In contrast to the year-long efforts analyzed in Syner et al. (2008), this campaign only lasted for 18 days. The Labor Day holiday campaign had three main components: (1) DUI enforcement, (2) public awareness efforts, and (3) evaluation. The 2006 program used approximately $10 million in Federally funded television and radio advertisements. The message was that police would arrest drivers if they were caught driving drunk. Thirty States reported spending an additional $8 million locally on similar messages. All States engaged in 18 nights of enforcement focused on apprehending intoxicated drivers. Forty-eight States reported over 40,000 DWI arrests during the 18 nights. As detailed in Solomon et al. (2011) the percentage changes in alcohol-impaired fatalities during the enforcement period compared to before the enforcement averaged 2.0 percent. This very small magnitude is not statistically significant. This result suggests that the overall State-wide efforts were not effective in reducing alcohol-impaired fatalities. However, while some States implementing HVE strategies experienced increases in alcohol-impaired fatalities, others experienced declines. Unfortunately, data describing the intensity of the HVE effort in each State were not available, so this analysis could not determine if the variation in outcomes among States could be attributed to differences in the amount of enforcement conducted by each State.

*Table 33. T-test for the Percentage Change in Alcohol-Impaired Fatalities by State*

| Statistic | Alcohol-Impaired Alcohol-Related Fatalities (percentage change before and during enforcement) from Syner et al., 2008) | Alcohol-Impaired Fatalities (percentage change before and during enforcement) from Solomon et al., 2008) |
|---|---|---|
| Mean Percentage Change | -0.0452 | -0.0038 |
| Sample Size | 15 | 51 |
| Standard Error | 0.0295 | 0.0270 |
| t-Statistic | -1.85309 | -0.1424 |
| Significance – Two-Tailed t-Test | $0.05 < p < 0.10$ | $p > 0.10$ |

### 5.5.4 Impacts of HVE: Interrupted Time-Series Analysis of Crash-Related Outcomes.

Seven studies used interrupted time-series analysis to determine whether the change in safety outcomes before and during enforcement periods was statistically significant for HVE studies. Six out of the 7 studies looked at crash-related outcomes while one looked at prevalence of proscribed behavior as the safety outcome. All statistically significant results were negative, which means the efforts reduced alcohol-impaired crashes. Significant reductions in alcohol-impaired fatal crashes ranged from -9 percent to -20 percent, while reduction in alcohol-impaired fatalities ranged from -8 percent to -17 percent. Changes in crash incidence ratios (the ratio of impaired driving crashes to non-impaired driving crashes) varied by State and ranged from -0.07 to -0.18. The effort in Tennessee resulted in single-vehicle nighttime injury crashes being reduced by 5 percent. The effort in four Maryland counties reduced the ratio of single-vehicle nighttime to multi-vehicle daytime crashes by -0.01. Overall, the impacts of HVE on crash-related outcomes evaluated using interrupted time series methodologies are negative, statistically significant, and slightly larger than the impacts derived from before-during comparison studies. The following table provides a summary of crash-related outcomes based on interrupted time-series analysis.

*Table 34. Summary of Results for Alcohol-Impaired Driving Crash-Related Outcomes, HVE Studies Using Interrupted Time-Series Analysis*

| Specific Location/Period of Enforcement | Publicity | Checkpoints, Enforcement Roadblocks | Saturation Patrols, Roving Patrols, Mobile Awareness Patrols | Enforcement $ | Outcome Measure | Change in Safety Outcome |
|---|---|---|---|---|---|---|
| Connecticut (Zwicker, Chaudhary, Maloney, et al., 2007)<br>March 2003 – January 2004 | Earned Media and paid media, $1,582,568 | At least 109 | None | $2,199,533 | Alcohol-Impaired Fatalities | -17%** |
| Counties in Maryland: Anne Arundel, Baltimore, Montgomery, Prince Georges (Beck et al., 2018)<br>May 2013 – October 2013 | Earned Media and paid media | None | A team of 7 Maryland State Police officers were selected and dedicated to this campaign | Unspecified | Alcohol-Impaired Fatal Crashes | -9%** |
| | | | | | Ratio of SVN crashes to multi-vehicle daytime crashes | -0.01** |
| Georgia (Fell et al., 2005)<br>July 2000 – September 2001 | | 2,837 | None | | Crash Incidence Ratio (BAC $\geq$ .01) | -0.07** |
| | | | | | Ratio of Alcohol-Impaired Fatalities to Annual VMT | -0.05 |
| Louisiana (Fell et al., 2005) | Earned Media | Unspecified | Conducted 217 saturation patrols, then | | Crash Incidence Ratio BAC $\geq$ .01 | -0.11** |

| Specific Location/Period of Enforcement | Publicity | Checkpoints, Enforcement Roadblocks | Saturation Patrols, Roving Patrols, Mobile Awareness Patrols | Enforcement $ | Outcome Measure | Change in Safety Outcome |
|---|---|---|---|---|---|---|
| July 2000 – September 2001 | | | sobriety checkpoints after they became legal | | Ratio of Alcohol-impaired Fatalities to Annual VMT | -0.08** |
| New Mexico (Lacey et al., 2000) | Paid Media | 910 | None | | Alcohol-Impaired Fatal Crashes | -19%** |
| Pennsylvania (Fell et al., 2005)<br>July 2000 – September 2001 | Earned Media | 150 sobriety checkpoints and 150 reduced staffing checkpoints | 480 roving patrols and 360 mobile awareness patrols with trailer | | Crash Incidence Ratio BAC ≥ .01 | -0.004 |
| | | | | | Ratio of Alcohol-Impaired Fatalities to Annual VMT | 0.09 |
| Tennessee (Fell et al., 2005)<br>November 2000 – January 2002 | | 535 sobriety checkpoints and 270 enforcement roadblocks | 270 roving and saturation patrols | | Alcohol-Impaired Fatal Crashes | -20%** |
| | | | | | Crash Incidence Ratio BAC ≥ .01 | -0.18** |
| Tennessee (Lacey et al., 1999)<br>April 1994 – March 1995 | Paid and Earned Media | 10 to 15 sobriety checkpoints per year | 576 checkpoints | | Ratio of alcohol-impaired Fatalities to Annual VMT | -0.03 |
| | | | | | SVN injury crashes | -5%** |

| Specific Location/Period of Enforcement | Publicity | Checkpoints, Enforcement Roadblocks | Saturation Patrols, Roving Patrols, Mobile Awareness Patrols | Enforcement $ | Outcome Measure | Change in Safety Outcome |
|---|---|---|---|---|---|---|
| West Virginia (Zwicker, Chaudhary, Solomon, et al., 2007)<br>July 2003 – September 2005 | Paid Media: $416,838 | Checkpoints and saturation patrols increased from 46 to 74 during the enforcement period. | Yes | $2,943,601 | Alcohol-Impaired Fatalities | -8%** |

*Note*. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

### *5.5.5 Impacts of HVE: Interrupted Time-Series Analysis of Behavior-Related Outcomes.*

Only one study (McCartt et al., 2009) examined impacts of HVE on behavior-related outcomes using interrupted time-series analysis. McCartt et al. (2009) looked at a program of publicized intensive enforcement of the minimum drinking age law and the drinking and driving laws that was implemented at a college community. The effects on driving at various BACs were evaluated, particularly for drivers 16 to 24, who were targeted by the program. Objective measures of driver BACs were collected through nighttime roadside surveys before and during the program in the treatment college community and a comparison college community. Logistic regression models estimated the program's effects on the likelihood of driving at various BAC thresholds at the treatment location, after accounting for BAC patterns in the comparison location. Total sample size was 3,783 for the treatment city and 4,770 for the comparison city.

The results show that relative to the comparison community, consistent reductions in driving at various BAC levels were achieved in the experimental community. The greatest reductions in drinking and driving were for 16- to 20-year-olds (66% reduction of drivers with positive BACs and 94% reduction of drivers with BACs ≥ .05), followed by 21- to 24-year-olds (32% reduction of drivers with positive BACs and 71% reduction of drivers with BACs ≥ .08). There also were reductions for drivers 25 and older (23% reduction of drivers with positive BACs and 53% reduction of drivers with BACs ≥ .08), but these reductions were only marginally significant at the 90 percent level. The results imply the HVE program with a strong enforcement component targeted to a community college produced substantial reductions in drinking and driving among teenagers and young adults and smaller reductions among older adults. Results are summarized in the following table.

*Table 35. Summary of Results for Alcohol-Impaired Driving Behavior Related Outcomes, HVE Studies Using Interrupted Time-Series Analysis*

| **Specific Location** | **Period of Enforcement** | **Publicity** | **Enforcement Activities** | **Outcome Measure (Odds ratio of treatment and control groups)** | **Change Outcome (Odds ratios)** |
|---|---|---|---|---|---|
| Cities in West Virginia: Huntington and Morgantown (McCartt et al., 2009) | Winter 2006 – Fall 2007 | Paid and Earned Media | Sobriety checkpoints and special DUI patrols per month during baseline is 15 per month increasing to 60 | Drivers 16 - 20 with BAC > .00 | -0.66** |
| | | | | Drivers 16 - 20 with BAC ≥ .02 | -0.76** |
| | | | | Drivers 16 - 20 with BAC ≥ .05 | -0.94** |
| | | | | Drivers 16 - 20 with BAC ≥ .08 | -0.91** |
| | | | | Drivers 21 - 24 with BAC > .00 | -0.32* |
| | | | | Drivers 21 - 24 with BAC ≥ .02 | -0.44** |
| | | | | Drivers 21 - 24 with BAC ≥ .05 | -0.69* |
| | | | | Drivers 21 - 24 with BAC ≥ .08 | -0.71* |
| | | | | Drivers 25+ with BAC > .00 | -0.23 |
| | | | | Drivers 25+ with BAC ≥ .02 | -0.33 |
| | | | | Drivers 25+ with BAC ≥ .05 | -0.39* |
| | | | | Drivers 25+ with BAC ≥ .08 | -0.53* |

*Note.* *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

### 5.5.6 Impacts of HVE: Multivariate regressions.

Only one HVE study performed a multivariate regression. Creaser et al. (2007) used Poisson regressions to study the effects of an overtime enforcement program entitled Operation NightCAP that used saturation patrols to identify impaired drivers. Poisson regression is a form of regression analysis used to model count data, meaning that the outcome is measured in integers (Wooldridge, 2015). Since 2003, Operation NightCAP funds enforcement programs for the 13 Minnesota counties with the highest numbers of alcohol-impaired fatal and severe-injury crashes. The counties involved varied each year.

Unemployment rate was included in the model as a predictor of alcohol-impaired fatal crashes. Unemployment rates have been shown to be significantly inversely correlated with fatal traffic crashes, meaning that higher unemployment rates are correlated with a lower number of fatal traffic crashes (Wilde, 1994). A trend variable (Time = 1991 - 2005) was also included in the models to identify any trends in crash rates that may be attributable to various road safety interventions or other factors that may result in decreases in crash rates over time.

The study found that increasing the number of saturation patrols conducted in a given year resulted in a marginally statistically significant decrease in the alcohol-impaired fatal crash rates in the group of counties (the number of alcohol-impaired fatal crashes as a percent of all crashes). The regression results suggest that an additional saturation patrol in the collection of 13 counties would decrease the alcohol-impaired fatal crash rate for those counties by 0.1 percent. The average number of fatal alcohol-impaired crashes from 1991 to 2005 (combined across all 13 counties) was 193 crashes. A 0.1 percent reduction is less than one crash. A 1 percent reduction, requiring 10 additional saturation patrols, would decrease the alcohol-impaired fatal crash rate by almost two crashes.

This small magnitude indicates that a large number of saturation patrols are probably required to see significant decreases in the alcohol-impaired fatal crash rate. A 10 percent reduction would require 100 additional saturation patrols across the 13 counties. The average number of saturation patrols in the sample was gradually increased from 54 to 260 from 1998 to 2005 and averaged 132 over the enforcement period.

### 5.5.7 Impacts of Sobriety Checkpoints on Alcohol-Impaired Driving Safety Outcomes

Two studies looked at the impact of sobriety checkpoints as measured by total number of checkpoints per year (Nunn & Newby, 2011, and Lacey et al., 2006). Lacey et al. used a before-during comparison approach, while Nunn and Newby used interrupted time-series analysis. Lacey et al compared behavior-related safety outcomes before and during enforcement in terms of proportion of drivers with different BAC concentration levels. The enforcement effort related to the use of so-called "low manpower" checkpoints that used only 3 to 5 officers compared to typical checkpoints that use 15 to 20 officers. Resulting percentage-point changes were non-significant.

On the other hand, the checkpoint effect on alcohol-impaired crash outcomes explored in Nunn and Newby (2011) for 18 intersections in Indianapolis was statistically significant. Poisson and negative binomial regressions were applied to determine significant differences in the data with the checkpoint effect as the variable of interest. Poisson regression was explained above in relation to Creaser et al. (2007). Negative binomial regression is a form of Poisson regression but has a less restrictive assumption on data distribution that allows for under-dispersion (Wooldridge, 2015).

If the application of a sobriety checkpoint at a location has a deterrent impact on the subsequent number of alcohol-impaired crashes within the zone around the checkpoint, the checkpoint effect variable (taking the value of one or zero) would be negative. Other independent variables control for the overall volume of crashes, the monthly trend, the number of previous checkpoint applications at a given location, the fixed-effects for different checkpoint locations, and whether the checkpoint location is downtown or not (due to the higher volume of traffic in the downtown area). With other variables held at their means, the checkpoint effect coefficient of 0.209 translates into an 18.8 percent reduction in the expected count of alcohol-impaired crashes in checkpoint zones after checkpoints have taken place. Thus, controlling for the monthly trend and total number of crashes within the checkpoint zones, checkpoints have a statistically significant dampening effect on the total number of alcohol-impaired crashes. The table below provides the summary of results from studies that looked at the impacts of checkpoints in isolation (that is, not an HVE effort that also involved publicity).

*Table 36. Summary of Impacts of Sobriety Checkpoints on Safety*

| Specific Location/ Period of Enforcement | Enforcement Baseline | Enforcement During | Outcome Measure | Baseline | During | Change Outcome |
|---|---|---|---|---|---|---|
| 2 Counties in West Virginia: Harrison and Monongalia (Lacey et al., 2006)<br><br>August 2003 – August 2004 | 13 checkpoints | 19 checkpoints | Drivers with BACs ≥ .01; | 5.1% | 4.5% | -0.6 % pt |
| | | | Drivers with BACs ≥ .05; | 1.4% | 2.8% | -1.4% pt |
| | | | Drivers with BACs ≥ .08 | 0.9% | 1.5% | -0.6% pt |
| 2 Counties in West Virginia: Raleigh and Greenbrier (Lacey et al., 2006)<br><br>August 2003 – August 2004 | 25 low manpower checkpoints | 106 low manpower checkpoints | Drivers with BACs ≥ .01; | 4.5% | 3.6% | -0.9% pt |
| | | | Drivers with BACs ≥ .05; | 1.6% | 1% | -0.6% pt |
| | | | Drivers with BACs ≥ .08 | 1.1% | 0.7% | -0.4% pt |
| Indianapolis; 9 Non-downtown and 9 downtown locations (Nunn and Newby, 2011)<br><br>October 2008 – November 2009 | 9 checkpoints | 22 checkpoints | Alcohol-Impaired Crashes, Total Annual | | | -20.9%** |

*Note.* *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

### 5.5.8 Impacts of Publicity on Alcohol-Impaired Driving Safety Outcomes

Only 1 study out of 18 alcohol-impaired driving studies evaluated the impacts of publicity by itself. This is different from HVE studies where publicity was combined with other enforcement activities such as checkpoints, patrols, or training and education. Niederdeppe et al. (2017) examined the relationship between the volume and timing of alcohol-control PSAs and rates of alcohol-impaired fatal crashes in the United States at the State and month level for a 14-year period (1996–2010). Simple linear regression models were used to predict rates of alcohol-impaired fatal crashes by State and month as a function of the volume of alcohol-control PSAs aired during the previous 8 months. Because it is a regression, unlike before-after or before-during comparison studies, the model included other controls for State anti-drunk driving laws and regulations, State demographic characteristics, State taxes on alcohol, calendar year, and seasonality. Results showed that increased volumes of anti-drunk driving PSAs aired were significantly related to reduced rates of alcohol-impaired fatal crashes. The results show that PSA volumes up to 3 months prior to when the alcohol-impaired fatal crash rates were measured still had a significant effect. The effects range from 9 percent to 17 percent reductions in alcohol-impaired fatal crashes, suggesting that PSAs could play an important, contributing role in reducing alcohol-impaired fatal crashes over time. However, as these results are from one study, caution should be taken in interpreting them.

### 5.5.9 Impacts of Unspecified Enforcement on Alcohol-Impaired Driving Safety Outcomes

The last group of studies did not indicate the particular enforcement activities undertaken as part of alcohol-impaired driving enforcement programs but rather used the number of deployed officers per year as a measure of enforcement intensity. Fell et al. (2014) analyzed the influence of enforcement presence using the number of sworn officers per capita on safety outcomes from the 2007 National Roadside Survey (NRS) and crashes from the General Estimates System in the same locations as the 2007 NRS. The study analyzed the relationship between the intensity of enforcement and the prevalence of impaired driving crashes in 22 to 26 communities with complete data (the number of communities included varied depending on completeness of data for a particular analysis). Hence, the authors noted that the study should be considered a multi-site study with a convenience sample of communities. The study found that a one percent increase in sworn officers per 10,000 residents reduced crash incidence ratios (the ratio of impaired driving crashes to non-impaired driving crashes) by between 0.09 and 0.10 regardless of blood alcohol concentration limits. Given the average number of officers per 10,000 drivers was 12.76, this means that with 1.3 additional officers (i.e., a 10 percent increase), the Crash Incidence Ratio (CIR) of 0.25 would drop by 1 percent (.0025) to .2475, suggesting that the percent of crashes that involve alcohol would fall from 20 to 19.8 percent. Increasing officers by 10 percent (roughly 1 officer per 10,000 drivers) resulted in the percent of alcohol involved driving crashes falling by ~0.2 percent, which was a fairly small impact.

On the other hand, Yao et al. (2016) collected data from 30 States and the District of Columbia that experienced the greatest changes in alcohol-impaired fatal crashes from 1996 to 2006. Aggregate State-level data from a total of 279 State and year combinations were analyzed. The dependent measure was the ratio of drivers involved in fatal crashes with BAC ≥ .08 over drivers involved in fatal crashes with BACs = .00. per capita DUI arrests and traffic enforcement funding were the primary predictors. Additional controls included vehicle miles traveled; the proportional distributions of gender and race/ ethnicity; geographic distribution; the proportion of drivers 21– to 34 years old; median family income; and education level. Analysis revealed that level of enforcement funding was not statistically significant.

## 5.6 Conclusion

Overall, the effort to derive a quantified relationship between enforcement and impaired driving outcomes was hampered by several issues. First, the impaired driving literature uses a variety of safety outcome measures, which impedes the ability to compare results between studies. Second, very few studies provided quantitative measures of the enforcement activities so that the level of enforcement intensity could not be related to the magnitude of observed safety outcomes. Nonetheless, certain conclusions can be made based on the available literature.

The table below provides a simple summary of the direction of the observed impacts from the various alcohol-impaired driving enforcement discussed above. As a reminder, a single study may have examined several efforts and therefore may be represented several times in the table. The table shows that of the 90 study locations that analyzed the impacts of HVE, 58 percent resulted in a decrease of either crashes or proscribed behavior, while just 40 percent showed an increase. The remaining 2 percent of enforcement locations showed mixed results. Other types of enforcement (non-HVE) resulted in reductions in either crashes or proscribed behavior exclusively. A simple sign test can determine whether the probability of getting at least 57 reductions in crashes or proscribed behaviors out of 95 results is greater than would be expected by chance (i.e., 50:50). Testing the hypothesis that the result is due to random chance resulted in a very small p-value of 0.0188, which indicated that it is very likely that alcohol-impaired driving enforcement was effective at improving safety outcomes.

*Table 37. Summary of Safety Outcome Vote Counts*

| Enforcement | Number of study locations | Reduction in crashes or proscribed behavior | Mixed results | Increases in crashes or proscribed behavior |
|---|---|---|---|---|
| HVE | 90 | 52 (58%) | 2 (2%) | 36 (40%) |
| Checkpoints | 2 | 2 (100%) | | |
| Publicity | 1 | 1 (100%) | | |
| Unspecified | 2 | 2 (100%) | | |
| All | 95 | 57 (60%) | 2 (2%) | 36 (38%) |

A closer look at the characteristics of the studies that led to reductions in crashes reveals that the observed reductions in crashes varied based on both the methods chosen and the scale of the enforcement effort. Regarding analysis methods, multivariate regressions (and in some cases interrupted time-series analysis) controlled for other factors or variables besides the enforcement effort being analyzed, which could affect the impact the safety outcome measures. In contrast, before-during studies are likely to ascribe all observed changes in the safety outcome measure to the enforcement effort because they often lack a means of controlling for external factors. As a consequence, the estimated impacts from regression analysis tend to be smaller than from before-during comparison studies.

In addition, the effects of enforcement on crash-related outcomes were generally negative and significant for smaller scale studies focusing only on certain States, counties, or intersections and with use of prior information on prevalence of impaired driving. Examples include Creaser et al. (2007) focusing on 13 Minnesota counties with the highest rate of alcohol-impaired crashes, McCartt et al. (2009) targeting college communities in West Virginia, and Beck et al. (2018) targeting four counties in Maryland. The one checkpoint study (Nunn & Newby, 2011), which yielded a significant result, looked at a small-scale enforcement that identified hotspot intersections.

On the other hand, the overall impact of enforcement on behavior-related outcomes was more difficult to identify. For one, the studies utilized a wide variety of measures. A few studies used the proportion of drivers operating over a certain BAC (Lacey et al., 2006, and Solomon et al., 2008), while another compared the odds ratios to measure the likelihood of driving over certain BAC limits for different age groups (McCartt et al., 2009). Second, the applicability of the overall results largely depends on the age group and State being studied as different States may have different legal BAC limits for underage drivers. Finally, data for BAC measurements may not be a random sample. A driver needs to be pulled over to get BAC measurement. Unless the data come from random and standardized checkpoints, impaired behavior data should be scrutinized.

# 6 Speeding

This chapter reviews the 13 relevant studies on speeding enforcement programs in the United States. Speeding is a type of aggressive driving behavior characterized by drivers traveling faster than the posted speed limit, or driving at or below the speed limit, but traveling too fast for roadway conditions (e.g., bad weather, work zones, or at night through poorly lit areas) (NCSA, 2018a). NHTSA estimates that speeders account for 3 out of every 10 drivers and, based on 2017 FARS data, are a contributing factor in 26 percent of crashes (NCSA, 2018a; USDOT, n.d.-d). Some of these crashes may have other contributing factors, such as impairment or distraction, and, as such, crash counts may include crashes discussed in other sections of this report. Due to advancements in vehicle safety and passenger protection, there has been an overall decrease in the number of deaths and injuries attributed to speed; however, thousands of Americans continue to die in speed-related crashes each year (NCSA, 2018a).

Since the repeal of the National Maximum Speed Limit in 1995, setting speed limits has been the responsibility of State and local governments, which has led to variation across States (Forbes et al., 2012). As of April 2019, there were 41 States that have speed limits of at least 70 mph on some portion of their roadway systems (IIHS, 2019). However, the available literature on speeding enforcement describes enforcement on a variety of roadway types (from rural interstates to residential streets) and conditions (from free-flowing traffic to work zones), and, as such, speed limit and baseline speeds vary greatly across studies.

A brief description of the studies can be found in the next section, Description of Speeding Enforcement Evaluation Studies. The Methods of Enforcement section discusses the different types of enforcement used in the programs reviewed in this chapter; the Publicity section discusses the different types of other program activities (such as paid media, earned media, and slogans); the Safety Outcomes section explains the different ways that studies measure safety outcomes; the section titled Relationship between Enforcement and Safety Outcomes analyzes the studies in various subsections and attempts to quantify, where possible, the effect of enforcement on speeding; and the final section, Conclusion, summarizes the main findings.

## 6.1 Description of Speeding Enforcement Evaluation Studies

The screening process resulted in 13 studies related to speeding enforcement that are in the scope of this synthesis. The synthesis did not consider studies that looked at the effect of legal changes, such as a change in the speed limit, on speeding. The synthesis includes only studies that looked specifically at the impact of police enforcement strategies, and not the impact of technology, such as variable speed signs or drone radar. Some of the studies in this review may have looked at such elements (drone radar, specifically, appeared in several studies), but such studies also included police enforcement separate from technology.

Most studies looked at speeding on highways, and a non-trivial number of these studies looked specifically at highway work zones. Work zone safety is a major area of concern for agencies dealing with roadway operations. In 2015 there were 642 fatal crashes related to work zones in the United States, 28 percent of which involved speeding (Ravani & Wang, 2018). Since work zones require workers to be very near live traffic that is moving at high speeds, work zones create a dangerous situation for those workers. Speed limits are typically reduced near work zones, and work zones often take up one or more lanes of traffic, creating a unique situation for drivers. It is plausible that drivers behave differently in work zones than they otherwise would, which means that their response to enforcement may also differ. Thus, the analysis later in this chapter will separate out the impacts of enforcement in work zones from the impacts in non-work zones.

Basic background information on these 13 studies can be found in the table below. This includes information on the location and dates of enforcement.

*Table 38. Background Information on Speeding Studies*

| **Study** | **Capture Area** | **State** | **Specific Locations** | **Work Zone?** | **Dates of Enforcement** |
|---|---|---|---|---|---|
| Benekohal et al.,2010 | Highway Segments | Illinois | I-64; I-55 | Yes | June 2006; June and July 2007 |
| Blomberg & Cleven, 2006 | Street Segments | Arizona | Peoria; Phoenix | No | End of 2002–early 2003 |
| Chen & Tarko, 2012 | Highway Segments | Indiana | I-65; I-70; I-465; US-31; I-69 | Yes | 2011 |
| Cunningham et al., 2011 | | North Carolina | I-85 | No | Unspecified |
| Haas et al., 2003 | | Oregon | Gates; Alsea; Monmouth; Aumsville; Oak Knoll; Noti | No | July 2001–January 2003 |
| Hajbabaie et al., 2009 | | Illinois | I-64; I-55 | Yes | Summer 2006; Summer 2007 |
| Medina et al., 2009 | | | | Yes | June 2006; June and July 2007 |
| Ravani & Wang, 2018 | | California | Stockton; San Diego; Redding; Weed | Yes | 2011 |
| Sisiopiku & Patel, 1999 | | Michigan | I-96 | No | October–November 1996 |
| Streff et al., 1995 | | | I-96; US-23 | Yes; No | August and September 1993 |
| Stuster, 1995 | Counties | California | San Bernardino; Modesto | No | June–November 1994 |
| Stuster, 2004 | Counties | Arizona; Indiana | Tucson; Marion County | No | July–December 2001 |
| Talebpour & Mahmassani, 2014 | Highway Segments | Illinois | I-64; I-55/I-70 | No | June–July 2011; August–September 2011 |

## 6.2 Methods of Enforcement

Visibility strategies involve deployment of police in a highly visible manner, such as stationing police cars or other overt police presence in target locations (NHTSA, 2008). However, unlike stationary or roving patrols, there is no effort made both to detect and then to intercept someone who is speeding. Specifically, stationing is when law enforcement agencies place marked or unmarked police cars in areas where speeding is common or speeding crashes have previously occurred. In the studies sometimes these vehicles had their emergency lights flashing, which would identify an unmarked car as a police vehicle. At other times, police vehicles were conspicuously parked without lights on. Studies that employed visibility strategies sometimes combined other non-enforcement aspects such as variable message signs, drone radar, or speed trailers. For analysis purposes, studies that did not explicitly label their efforts as a patrol (detect and intercept) or those that highlighted the use of enforcement as a deterrent (as opposed to enforcement used to actively pursue and prosecute speeding drivers) were classified as using visibility strategies. The table below shows the methods of enforcement used in the various speeding studies.

*Table 39. Speeding Enforcement Measures*

| Enforcement Method | Count | Studies |
|---|---|---|
| Police presence, unspecified | 6 | Chen & Tarko (2013), Cunningham et al., 2011, Haas et al., 2003), Ravani & Wang (2018), Streff et al., 1995), Stuster (1995) |
| Visibility strategies | 6 | Benekohal et al., 2010), Hajbabaie et al., 2009), Medina et al., 2009), Ravani & Wang (2018), Sommers et al., 2013), Streff et al., 1995) |
| Patrols, unspecified | 2 | Blomberg & Cleven (2006), Sisiopiku & Patel (1999), Stuster (2004) |
| Patrols, roving | 1 | Talebpour & Mahmassani (2014) |
| Patrols, stationary | 1 | Talebpour & Mahmassani (2014) |

## 6.3 Publicity

Because publicity activities often occur concurrently with enforcement activities, it is difficult to attribute changes in the safety outcome to either the presence of enforcement or the presence of other program elements. The publicity measures used in the reviewed speeding studies can be seen in the table below. This list of program elements that supplement enforcement activities is not exhaustive and is limited to supplementary activities discussed in the reviewed studies that are within the scope of this synthesis.

*Table 40. Publicity Measures for Speeding Enforcement*

| Publicity Type | Count of Studies | Studies |
|---|---|---|
| Paid Media | 3 | Blomberg & Cleven, 2006, Stuster, 1995, 2004 |
| Earned Media | 3 | Blomberg & Cleven, 2006, Stuster, 1995, 2004 |
| Slogan | 2 | Blomberg & Cleven, 2006, Stuster, 2004 |

Several speeding campaigns employed media as an awareness or publicity tool. Stuster (1995) discussed a campaign that included both paid and earned media. The campaign employed posters, brochures, bus bench display advertising, and television and radio PSAs. Similarly, Blomberg and Cleven (2006) described the educational material developed for the program, which included print material for homeowners, parents, and drivers as well as radio spots. Neither study provided a cost estimate for paid media associated with the publicity campaigns. Stuster (2004), however, did indicate the percentage of overall program funds that were spent on publicity.

For the publicity campaigns discussed in Stuster (1995), earned media included press conferences, supermarket drop-ins, media events, and public speakers. Similarly, for the education campaign discussed in Blomberg and Cleven (2006), earned media included inputs for homeowner's association newsletters, press releases, and lawn signs. In Stuster (2004) earned media included interviews, press releases, and news coverage of a fatal crash that occurred during a high-speed pursuit by an officer assigned to the aggressive driving patrol. No study provided a cost estimate for events that generated earned media coverage.

## 6.4 Safety Outcomes

There were two main ways in which the reviewed speeding studies measured safety: average speed and the prevalence of speeding drivers. Average speed appeared in 11 of the 13 reviewed studies, making it the most common measure. There was variation in the types of average speed reported, as some studies separated by type of vehicle, travel lane, and time of day. Three of 13 studies reported the percentage of drivers traveling above the speed limit. For a list of the studies that used each safety measure, as well as a count of the number of observations that will be used in later analyses, see the table below.

*Table 41. Safety Outcome Measures for Speeding Enforcement*

| Type of Safety Outcome Measure | Count of Studies | Studies | Count of Observations Used in Analysis |
|---|---|---|---|
| Average speed | 11 | Benekohal et al., 2010), Blomberg & Cleven (2006), Chen & Tarko (2012), Cunningham et al., 2011, Hajbabaie et al., 2009), Medina et al., 2009), Ravani & Wang (2018), Sisiopiku & Patel (1999), Streff et al., 1995), Stuster (2004) Talebpour & Mahmassani (2014) | 40 |
| Percentage of vehicles traveling above the speed limit | 3 | Benekohal et al., 2010), Haas et al., 2003), Stuster (1995) | 16 |

All 11 studies that looked at average speed collected their own speed data, measuring vehicle speeds and generating vehicle counts. Five studies used only video-based evaluation (Benekohal et al., 2010; Chen & Tarko, 2013; Cunningham et al., 2011; Hajbabaie et al., 2008; and Medina et al., 2009). These studies used video cameras to record vehicles and calculated average speed for a subset of vehicles based on a known distance between two points on the video. Four studies used only some form of automated traffic counter (Blomberg & Cleven, 2006; Sisiopiku & Patel, 1999; Streff et al., 1995; and Stuster, 2004). One study, Ravani and Wang (2018), used both video-based imaging and automated traffic counters to record traffic data.

Three studies, Benekohal et al. (2010), Hajbabaie et al. (2009), and Medina et al. (2009), were written by the same combination of five authors, Benekohal, Chitturi, Hajbabaie, Medina, and Wang. These 3 studies explored the effects of automated speed photo-radar enforcement (SPE) and various combinations of more traditional speed reduction treatments (speed feedback trailer, presence of police vehicles with emergency lights on/off, etc.) on average speed at two work zone locations, one near East St. Louis, Illinois, and a second near Joliet, Illinois. Two studies (Benekohal et al., 2010, and Hajbabaie et al., 2000) described instances of speeding at the treatment location while two studies (Benekohal et al., 2010, and Medina et al., 2009) described instances of speeding 1.5 miles downstream of the treatment location. The studies used identical treatments and, for the studies that list the dates/times of treatment (Medina et al., 2009 and Benekohal et al., 2010), 12 dates/times out of a possible 17 results are identical. Based on how the data are reported (sample size and mean speed), it is not feasible to separate out the data such that duplicate observations between the 2 studies could be removed. For the purposes of this report and to avoid possible double-counting, data from Medina et al. (2009) and Hajbabaie et al. (2009) are excluded from t-tests and other subsequent analyses. Only Benekohal et al. (2010) will appear in the review. This dropped the total number of studies reviewed from 13 to 11.

## 6.5 Relationship Between Enforcement and Safety Outcomes

Studies reporting changes in speed tended to report the change between baseline data and data gathered *during* the enforcement campaign, and did not report measures of speeding *after* the enforcement ended. So, no conclusion regarding the duration of the effects can be drawn.

Ideally, a study would provide comprehensive information on all the resources used as part of an enforcement effort, providing measures before, during, and after program implementation. Such a description would include: number of officer enforcement hours by type of enforcement, the cost of wages for those enforcement hours, and the number of patrols or other activities conducted. Unfortunately, the descriptions of the intensity of the enforcement available in the literature were often incomplete.

Additionally, many of these studies looked mainly at the impact of police visibility or police presence, which does not necessarily lend itself to being measured in levels. The police presence was either at the site or it was not. Often these studies used a police car (or cars) parked in a work zone (a visibility strategy) employed in conjunction with an assortment of other enforcement strategies (variable message signs, drone radar, speed trailers, etc.). Of these studies, only estimates resulting from enforcement combinations that included police presence, not simply technology, were included in this analysis. In the subsequent tables these studies are labeled as using visibility strategies and their enforcement is measured as police presence—either there was some form of police presence, or not.

While some of the studies did provide a quantifiable measure of the amount of enforcement used, the measures were too varied and observation counts too sparse for analysis. Nevertheless, the synthesis obtained certain insights. In the following sections studies have been grouped based on the metric used to measure the safety outcome. The sections discuss the average change in the safety outcome, and analyze whether the change was statistically significant—did enforcement reduce speeding (as measured by changes in average speed, percentage of vehicles traveling above the speed limit, or speed-attributed fatalities)?

Nine studies provided the change in average speed due to the enforcement, The studies differed in key ways, including the location and enforcement strategy. However, since they all reported the same safety outcome, they can provide insight into how average speed changes based on enforcement strategies.

Studies generally produced observations that were relevant to this analysis. In other words, several observations may have been drawn from a single study, which examined different travel lanes, vehicle types, and/or time periods. Background information on these studies is provided in the table below, which includes information on the treatment location, time-period, duration of the enforcement effort, type of enforcement, how enforcement was measured, and the average safety outcome. The table reports averaged results for each study location, sorted by enforcement strategy, then by author's last name. Studies discussing patrols tended not to specify the specific type of patrols used and listed them, generally, as patrols.

*Table 42. Speeding Enforcement Study Results*

| Study | Location | Time-Period (Duration) | Enforcement Strategy | Enforcement Measure | Average Change in Speed |
|---|---|---|---|---|---|
| Benekohal et al., 2010)<br>al., al., | I-64, proximate to East St. Louis, Illinois (work zone) | June 2006; 6 days of different enforcement combinations and varying time of day plus peak-morning and peak-afternoon baseline periods | Visibility Strategies | Presence of police vehicle | -5.42 mph |
| | I-64, proximate to East St. Louis, Illinois (work zone, downstream from treatment location) | | | | -0.59 mph |
| | I-55, proximate to Chicago, Illinois (work zone) | June and July 2007; 4 days of different enforcement combinations plus a baseline period | | | -5.38 mph |

| Study | Location | Time-Period (Duration) | Enforcement Strategy | Enforcement Measure | Average Change in Speed |
|---|---|---|---|---|---|
| | I-55, proximate to Chicago, Illinois (work zone, downstream from treatment location) | | | | -0.03 mph |
| Ravani & Wang (2018) | Six locations in California (All work zones)—2 urban locations (San Diego and Stockton) and two rural locations (Redding and Weed) | 2011; number of test days varied by site<br><br>Stockton: 2 days<br>San Diego: 6 days<br>Redding: 1 day<br>Weed: 2 days | Visibility Strategies/ Police presence, unspecified | | -5.76 mph |
| Streff et al., 1995) | I-96 in Livingston County, Michigan (work zone) | August to September 1993; 13 days of patrols (drone radar use varied) out of a total of 42 possible enforcement or baseline days | | Hours of enforcement (patrols took place 7-9 a.m. and 3-5 p.m. across 14 days) | -2.03 mph |
| | US-23 in Livingston County, Michigan (Not a work zone) | | | | -0.37 mph |
| Blomberg & Cleven (2006) | Peoria 84th Avenue, Arizona (not a work zone) | End of 2002 to early 2003; special enforcement took place over a 3-month period, but no level of daily enforcement is given | Patrols | Presence of patrols | -1.94 mph |
| | Peoria 85th Lane, Arizona (Not a work zone) | | | | 0.67 mph |

| Study | Location | Time-Period (Duration) | Enforcement Strategy | Enforcement Measure | Average Change in Speed |
|---|---|---|---|---|---|
| | Peoria 91st Avenue, Arizona (Not a work zone) | | | | -0.77 mph |
| | Peoria 95th Avenue, Arizona (not a work zone) | | | | -4.33 mph |
| | Phoenix – Clarendon, Arizona (not a work zone) | | | | -2.51 mph |
| | Phoenix – Sweetwater, Arizona (not a work zone) | | | | -3.41 mph |
| | Moon Valley Drive East/West Segment, Phoenix, Arizona (not a work zone) | | | | -1.21 mph |
| | Moon Valley Drive North/South Segment, Phoenix, Arizona (not a work zone) | | | | -1.96 mph |
| | Coral Gables Drive East/West Segment, Phoenix, Arizona (not a work zone) | | | | -3.10 mph |

| Study | Location | Time-Period (Duration) | Enforcement Strategy | Enforcement Measure | Average Change in Speed |
|---|---|---|---|---|---|
| | Coral Gables Drive North/South Segment, Phoenix, Arizona (not a work zone) | | | | -0.95 mph |
| Sisiopiku & Patel (1999). | I-96 in Ionia County, Michigan (Not a work zone) | October to November 1996; selected time intervals over a total of 6 days | | 2 patrol cars circulating within certain mileposts over selected time intervals for a total of 6 days | -2.65 mph |
| Stuster (2004). | Marion County, Indiana | March to August 2001; 61 days of special enforcement | | Presence of patrols | -0.4 mph |
| | Tucson, Arizona | July to December 2001; 168 days of special enforcement | | | -0.73 mph |
| Talebpour & Mahmassani (2014 | I-64 between North Kings highway and North Illinois Street, Illinois (Not a work zone) | June to July 2011; 4 weeks of enforcement, 3 different strategies, in a 5 week period | | | -1.82 mph |
| | I-55/I-70 between IL 203 and Vandalia Street, Illinois (Not a work zone) | August to September 2011; 3 weeks, each a different enforcement strategy | | | 0.12 mph |
| Chen & Tarko (2012 | Six sites in Indiana (All work zones); 5 rural sites and 1 urban | 2011; 2 days of enforcement per site with five combinations of two treatment factors per day | Police presence, unspecified | Number of stationary police vehicles | -2.19 mph |

| Study | Location | Time-Period (Duration) | Enforcement Strategy | Enforcement Measure | Average Change in Speed |
|---|---|---|---|---|---|
| Cunningham et al., 2011 | I-85 in Durham County, North Carolina (Not a work zone) | Unspecified; Video recorded from 9 a.m.-5 p.m., TACT program enforcement operations took place 10 a.m.-3:30 p.m.; number of days measured is unknown | | Presence of the TACT enforcement program | 2.45 mph |

Within these locations, some studies used several enforcement strategies. When considering both the unique locations and enforcement strategies, there are 40 observations related to change in average speed that can be compared across studies. Some of these observations are averages, since, as previously mentioned, some studies reported several observations for a single enforcement effort, meaning they reported changes in average speed for different lanes or vehicle types. For the purposes of this analysis, however, it was determined that each enforcement effort should be weighted equally, and thus cases of several observations were averaged to produce a single observation for inclusion in this analysis.

Some of the studies focused on speeding in areas with work zones, while other studies looked more generally at speeding. Given that a work zone could cause drivers to react differently than they otherwise would, the observations from the 9 studies were also split into work zone observations and non-work zone observations. These two subgroups were then analyzed separately. Of the 40 observations, 17 were from work zones and 23 were not from work zones.

The observations are grouped by size in Figure 9. Average speed observations As the figure shows, most of the observations for both categories (work zone and non-work zone) showed decreases in average speed due to the various enforcement strategies. The figure shows that work zones tended to have larger decreases in speed than non-work zones. Only non-work zone areas experienced any increases in average speed, and the increases where small, just 3 mph or less.

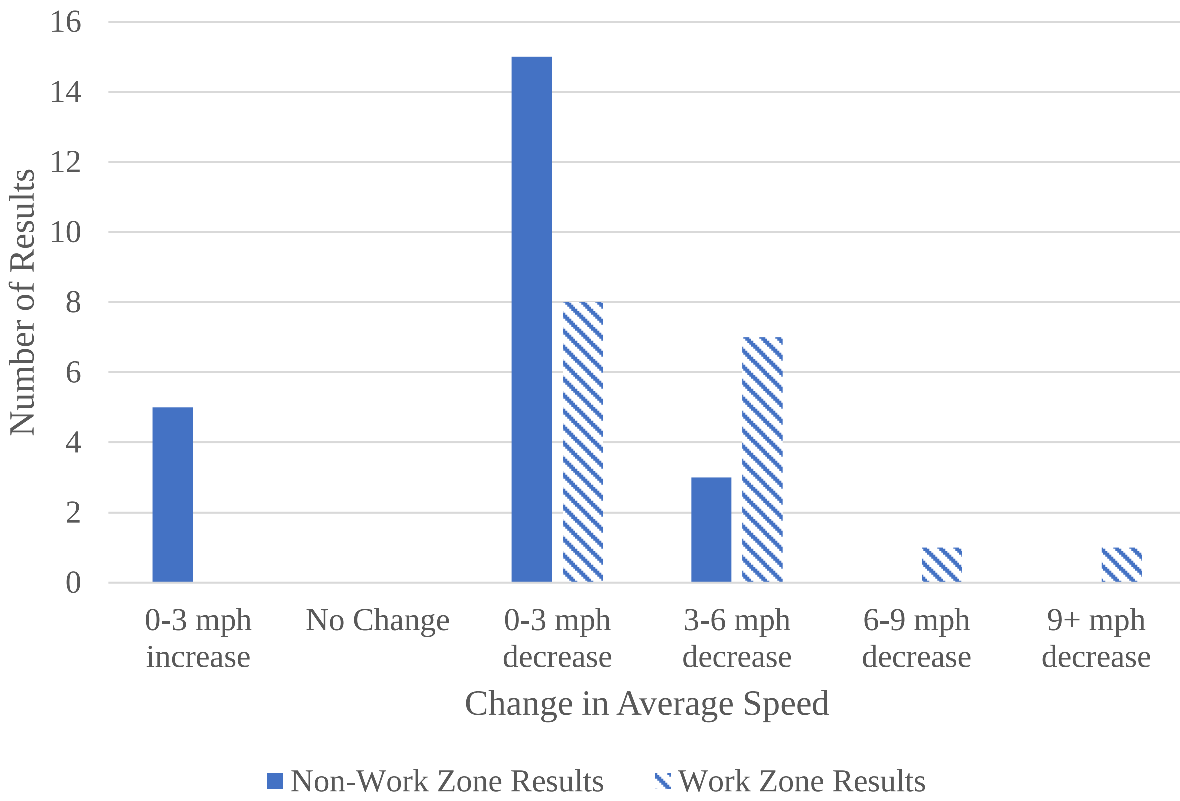



*Figure 9. Average speed observations*

A two-tailed t-test was conducted to test for statistical significance for the work zone observations and the non-work zone observations. This analysis found an average change of -4.16 mph in work zones and an average change of -0.99 mph in non-work zones. Both values were statistically significant at the 0.01 level. A change in speeds of over 4 mph in work zones may produce significant safety benefits for highway construction workers. Not only does reducing speed allow more reaction time to potentially avoid a collision between a vehicle and worker, but pedestrians are more likely to survive a collision at slower speeds. The change of less than one mph for non-work zones, while statistically significant, may not be meaningfully significant in producing improved safety outcomes, such as decreasing speed-related fatalities. The values used in the significance tests can be found in Table 43.

*Table 43. Average Speed Results*

| **Statistic** | **Work Zones** | **Non-Work Zones** |
|---|---|---|
| Average Change (mph) | -4.162 | -0.99 |
| Sample Size | 17 | 23 |
| Standard Error | 0.683 | 0.29 |
| t-Statistic | -6.09 | -3.424 |
| Significance – Two-Tailed t-Test | $p < .01$ | $p < .01$ |

The synthesis also tested to see if the average changes in speed within and outside of work zones were statistically different from each other. The two-tailed two-sample t-test found that the values were statistically different from each other at the 0.01 level. The results indicate that enforcement reduced average vehicle speeds but that enforcement reduced vehicle speeds more in work zones than outside of work zones.

## 6.6 Results by Enforcement Strategy

There was not enough information about the enforcement strategy in each study to do a robust analysis to control for both work zone and strategy impacts. However, it can still be useful to look at the observations from each study, sorted by the general enforcement strategy and by whether the study location was (or was not) in a work zone. Of the previously analyzed 9 studies that reported average change in speed, there were two main strategies: (1) visibility strategies (police presence) and (2) patrols.

The 40 speeding observations were also separated into four groups based on these two enforcement types and the presence of a work zone to see if there was any significant change that could be found due to the unique enforcement. For patrols in work zones, there was only one result, preventing any analysis. For police presence in non-work zones, the average impact was positive, meaning speeds increased, but the change was not statistically significant. Both police presence in work zones and patrols in non-work zones showed statistically significant decreases in speed. The average change by enforcement strategy and by whether the location was a work zone can be seen in the table below.

*Table 44. Results by Speeding Enforcement Strategy*

| Enforcement Strategy | Work Zone? | Number of Results (Number of Studies) | Average Change | Significant? |
|---|---|---|---|---|
| Visibility Strategies and/or Police Presence, Unspecified | Yes | 16 results (4 studies) | -4.26 | Yes ($p < .01$) |
| Visibility Strategies and/or Police Presence, Unspecified | No | 5 results (2 studies) | 0.197 | No ($p > .10$) |
| Patrols | Yes | 1 result (1 study) | -2.65 | N/A |
| Patrols | No | 18 results (3 studies) | -1.33 | Yes ($p < .01$) |

### *6.6.1 Prevalence of Speeding Drivers*

Three studies reported the prevalence of speeding drivers (measured as the percentage of vehicles going above the speed limit) before and during the enforcement, Similar to the studies that reported average speed, these studies differed in terms of location, time-period, and enforcement strategies. Benekohal et al. (2010) used video-based evaluation to generate traffic counts and estimate speed while Haas, Jones, and Kirk (2003) used automated traffic recorders. Stuster (1995) received data from local law enforcement agencies and gave no further information on data collection practices.

However, since they all reported the same safety outcome, an analysis was done to test whether enforcement generally reduced the percentage of speeding drivers across these 3 studies. Background information on the 3 studies can be found in the table below. Note that Benekohal et al. (2010) is included in both this section as well as in the previous section on average speeds.

*Table 45. Background Information on Studies Using Prevalence of Speeding Drivers*

| Study | Location | Time-Period (Duration) | Enforcement Strategy | Measure of Enforcement |
|---|---|---|---|---|
| Benekohal et al., 2010 | I-64, proximate to East St. Louis, Illinois | June 2006; 6 days of different enforcement combinations and varying time of day plus peak-morning and peak-afternoon baseline periods | Visibility Strategies | Police Presence |
| | I-55, proximate to Chicago, Illinois | June and July 2007; 4 days of different enforcement combinations plus a baseline period | | |
| Haas et al., 2003 | Six sites on highways in Oregon; variety of rural State highway classifications | July 2001 to January 2003; enhanced patrols over a 2-week period, followed by 6 weeks without enhanced patrols; repeated over a period of 18 months; deployed on weekdays only | Police presence, unspecified | Patrol intensity and predictability (Fixed or random schedule; assigned between 10 and 25 hours of enforcement) |
| Stuster (1995 | San Bernardino and Modesto, CA | June to November 1994; 22 months total, but no information on how many days special enforcement were conducted | | Total officer hours; average hours per zone per week |

There were 16 total observations from the 3 studies, and the number of observations by magnitude of the change in speeding behavior can be seen in Figure 10. Percentage of vehicles traveling above the speed limit observations. The observations are broken out by work zone and non-work zone observations, although only 4 of the 16 observations came from a work zone. The figure demonstrates that most of the observations showed a decrease in the percentage of vehicles that were speeding, but there were a few instances in which there was an increase in the percentage of speeding vehicles. There were also a few instances in which there were large reductions in the percentage of vehicles speeding, which primarily occurred at work zone locations.

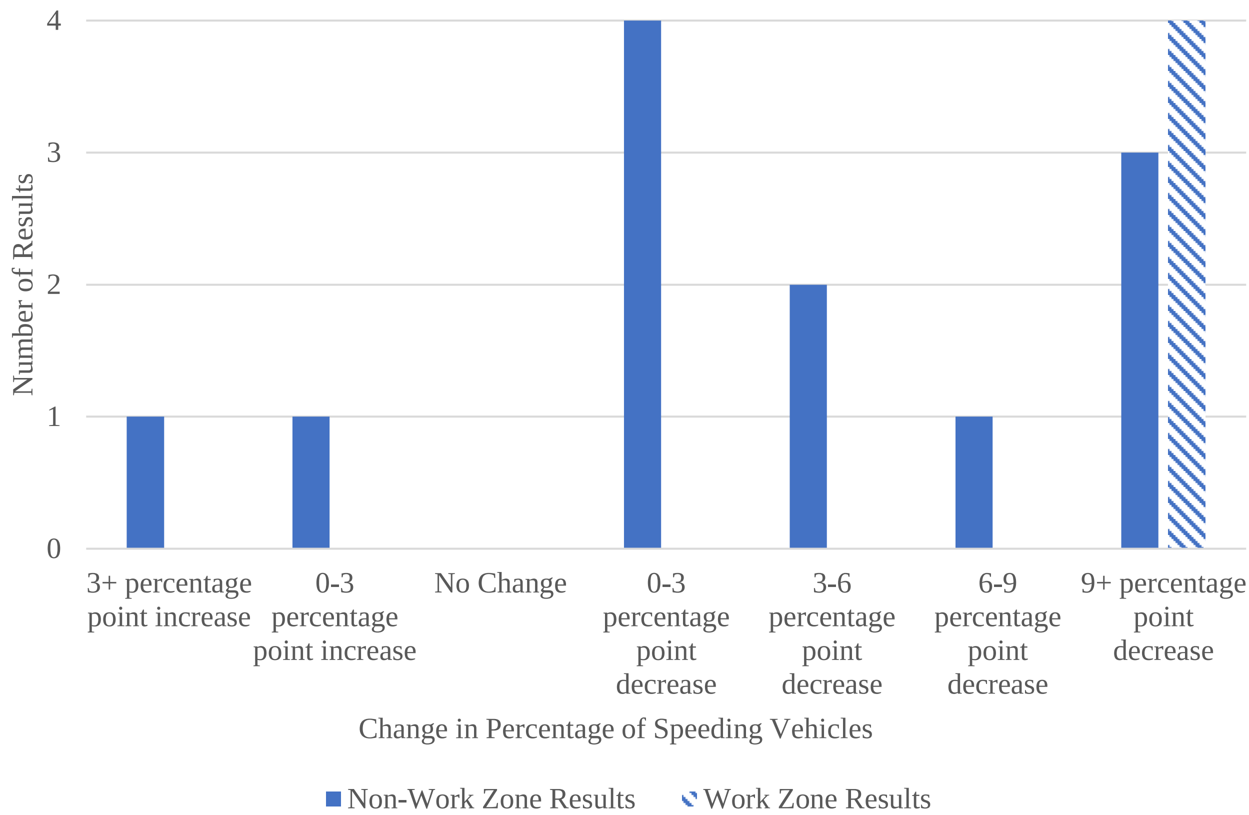



*Figure 10. Percentage of vehicles traveling above the speed limit observations*

The next table reports the results of the analyses for both work zones and non-work zones. The speed enforcement at work zones appear to reduce the percent of vehicles speeding by 50 percentage points, although caution should be used in applying this dramatic finding since it is based on four observations from just one study. In non-work zones the impact is more modest: a 4.8 percentage-point drop in percent of vehicles speeding.

*Table 46. Percentage of Speeding Vehicles Results*

| **Statistic** | **Work Zones** | **Non-Work Zones** |
|---|---|---|
| Average Change | -50.53 % pts | -4.82 % pts |
| Sample Size | 4 | 12 |
| Standard Error | 4.977 | 2.104 |
| t-Statistic | -10.152 | -2.289 |
| Significance – Two-Tailed t-Test | $p < 0.01$ | $p < 0.05$ |

### 6.6.2 Spatial and Temporal Effects

In terms of speeding, spatial and temporal effects may exist if placing a police car in a certain location, such as the beginning of a work zone, has additional effects beyond the immediate treatment area. Ideally, the highly visible placement of the police car would deter speeders, decreasing the number of vehicles speeding or average speeds outside the work zone, and would have a lasting impact, even after the treatment is removed (Benekohal et al., 2010, and Chen & Tarko, 2013). Two studies, Benekohal et al. and Chen and Tarko examined spatial and/or temporal effects of police enforcement in work zones.

For spatial effects, Benekohal et al. (2010) examined impact of police presence on vehicle speed at a location 1.5 miles downstream of the treatment location. The effects of different treatments on cars and trucks were studied separately and further disaggregated into free-flowing vehicles (those that can choose their desired speed) and general traffic stream vehicles (those that must drive with the flow of traffic). Since the focus of the Benekohal study was on the effectiveness of SPE, the study only qualitatively reported the spatial effects of police presence relative to SPE. Although SPE is outside the scope of this analysis (the focus of this report is non-technological enforcement), it is worth noting that Benekohal et al. did not find that police presence had spatial effects of any great magnitude and the spatial effects of SPE consistently exceeded any spatial effects of police presence.

Chen and Tarko (2013) placed police enforcement vehicles at different locations within the treatment location and upstream of the speed measurement segment. Positioning the vehicle 0.5 miles upstream of the speed measurement segment reduced average speed by 1.97 mph, but this is less than the speed reductions when the police vehicle was placed closer to or within the treatment site and the decrease was not sustained. Consistent with Benekohal et al. (2010), Chen and Tarko found that the effect of police enforcement does not have a strong spatial effect—the decrease in vehicle speeds is not maintained at locations downstream from the treatment location.

For temporal effects, Benekohal et al. (2010) compared the speed of vehicles after the treatment was removed from the work zone (data collection continued for 40 to 60 minutes after the treatment was removed) to the speed when the treatment was in place. Again, the effects of different treatments on cars and trucks were studied separately and further disaggregated into free-flowing vehicles and those that must drive with the flow of traffic. While the study found that SPE had a limited effect on free-flowing cars, police presence did not have temporal effects; the presence of a police car did not decrease speeds (at a statistically significant level) after police presence was removed.

## 6.7 Conclusion

The speeding studies analyzed in this section generally showed that enforcement reduced speeding. Although it was not possible to equate levels or intensity of enforcement to a change in speeding, the results generally indicate that, compared to no enforcement, having some level of enforcement leads to reductions in vehicle speeds. By analyzing average speed, the analysis found that enforcement reduced speed, on average, 2.3 mph, and that the effect was larger in work zones than non-work zones (a 4.2 mph reduction compared to a 0.99 mph reduction). Additionally, analysis that separated the results by enforcement categories found that in non-work zones, visibility strategies and general police presence were slightly less effective at reducing speeds than patrols.

Overall, the results of this section would suggest that speeding enforcement is effective, particularly in work zones. But the change in speed was often small, suggesting that perhaps larger concentrations of enforcement, or different types of enforcement and awareness messages surrounding speeding, might be necessary to create a large reduction in speeding fatalities, injuries, and crashes. There was, unfortunately, not enough information available to do a full quantitative analysis of the effects of different types and levels of enforcement on speeding.

# 7 Aggressive Driving

This chapter reviews the 5 relevant studies on aggressive driving enforcement programs in the United States. Aggressive driving is defined by the National Cooperative Highway Research Program (NCHRP) as operating a motor vehicle in a selfish, pushy, or impatient manner that directly affects other drivers, often unsafely (Neuman et al., 2003). Examples of aggressive driving include: tailgating, speeding, cutting in, changing lanes, changing lanes without using turn signals, and driving in a blind spot (Tarko et al., 2011). These are not the only behaviors that can be classified as aggressive driving, but they are some of the most common ones. While aggressive driving can also apply to all types of vehicles, it is most often discussed in the context of cars driving safely around trucks.

## 7.1 Description of Aggressive Driving Enforcement Evaluation Studies

The 5 studies reviewed in this section each examined the impact of the Ticketing Aggressive Cars and Trucks (TACT) program on aggressive driving behaviors. TACT is a program that was established specifically to try to reduce fatalities and injuries that come from cutting off trucks, tailgating trucks, and speeding around trucks (Nerup et al., 2006). TACT was originally conceived as a response to the Consolidated Omnibus Appropriations Act of FY2004, in which Congress mandated that NHTSA work with the Federal Motor Carrier Safety Administration (FMCSA) to educate the public on how to safely drive near commercial motor vehicles (Nerup et al., 2006). The TACT program model includes both media elements as well as intense enforcement,

Each study varied in terms of treatment duration, location, and analysis techniques. The analyzed time-periods ranged from 2005 to 2015, and each study took place in a different State: Alabama, Indiana, Kentucky, Utah, and Washington. Additionally, despite all programs targeting aggressive driving, not all studies measured the impact on aggressive driving in the same way. These variations in how studies measured the safety outcome will be discussed in the Safety Outcomes section.

Four of the 5 studies (Dye, 2016; Green, 2010; Nerup et al., 2006; and Tarko et al., 2011) looked at TACT programs that focused on highways. TACT was originally developed for highways and has had more limited testing on other types of roadways (Telfordet al., 2018). However, the fifth study, Telford et al., specifically examined whether TACT could be effective on city streets. The general principles of the program remained the same, even though the location differed. More detailed information on the location, time-period, and results of each individual study will be presented later in the section Relationships between Enforcement and Safety Outcome.

The next section, Methods of Enforcement, discusses TACT enforcement, the following section, Publicity, discusses how media is an aspect of TACT campaigns, and the next section, Safety Outcomes, explains the different ways that studies measure safety outcomes. The subsequent section, titled Relationship between Enforcement and Safety Outcomes, analyzes the studies. Finally, the Conclusion section summarizes the main findings.

## 7.2 Methods of Enforcement

TACT is a STEP focusing on aggressive driving, which begins with a publicity campaign and then introduces enforcement later during the campaign. Some of these TACT programs also included several waves of enforcement, meaning the same program was enacted several times. Dye (2016), Green (2010), and Nerup et al. (2006) all had several waves of enforcement.

## 7.3 Publicity

TACT programs always include a media campaign, so all 5 studies examined in this section have paid media and earned media. These publicity activities raise awareness of targeted enforcement activities, such as patrols or police visibility, and inform the public of the dangers of aggressive driving. While not a part of enforcement activities, these publicity activities are a component of the enhanced enforcement and likely have an impact on the observed safety outcome. Because these publicity activities typically occur concurrently with the enforcement activities, it is difficult to determine whether changes in the safety outcome are due to the presence of enforcement, the presence of the media, or the combination of all program elements.

## 7.4 Safety Outcomes

The table below shows the different measures of safety outcomes used in the various studies. Only a few safety outcomes were present in more than one study. One of these was the number of crashes, which was found in 2 of the studies. Studies looked at the total number of crashes and at subsets of crashes, such as injury crashes or fatal crashes. All studies used video data to measure their safety outcomes, except Telford et al. (2018), which used direct observations.

*Table 47. Safety Outcome Measures for Aggressive Driving Enforcement*

| Safety Outcome | Count of Studies | Studies | Count of Observations |
|---|---|---|---|
| Number of crashes | 2 | Green, 2010; Telford et al., 2018 | 9 |
| Percentage of drivers tailgating | 2 | Green, 2010; Tarko et al., 2011 | 74 |
| Number of unsafe events | 1 | Dye, 2016 | 27 |
| Violation rates | 1 | Nerup et al., 2006 | 1 |

The percentage of drivers tailgating was also reported in 2 studies (Green, 2010; and Tarko et al., 2011). Data were collected through camera observations, which determined the distance or time between two vehicles. Studies had different cut-offs to decide at what distance a vehicle was considered tailgating.

Dye (2016) looked at the number of unsafe events per minute, which included measures of the number of drivers tailgating. There were also other types of unsafe events included in the analysis. All unsafe events were meant to be measures of typical aggressive driving behaviors. Dye (2016) reported the total number of events, while Green (2010) and Tarko et al. (2011), the other 2 studies that looked at tailgating, only reported the percentage of tailgating drivers, meaning the studies are not directly comparable.

One study, Nerup et al. (2006), measured the number of violations to determine changes in driver behavior. Violations were measured by State and local law enforcement monitoring a section of roadway and documenting any observed violations.

## 7.5 Relationship Between Enforcement and Safety Outcomes

While some of the studies did provide a quantifiable measure of the amount of enforcement used, the measures were too varied and observation counts too sparse for a substantive analysis. Nevertheless, the synthesis obtained certain insights.

Each of the 5 TACT studies are presented in the following table. The table shows a quantifiable measure of enforcement, if there was one, or gives a more qualitative description of the enforcement strategy. The table also presents one of the main findings from each study. As the table shows, the studies had differing safety outcomes and measures of enforcement, which makes it difficult to directly compare them.

*Table 48. Study Results for Aggressive Driving Enforcement*

| Study | Location | Time-Period (Duration) | Enforcement Measure | Safety Outcome |
|---|---|---|---|---|
| Dye, 2016 | Alabama | January 2010 to August 2015 (68 Months) | Presence of TACT enforcement | 0.1548 fewer unsafe events per passenger car equivalency |
| Green, 2009 | I-75 and I-65 in Kentucky | 2007 to 2008 (18 Months) | Three enforcement blitzes | 11.83% reduction in total crashes |
| Nerup et al., 2006 | Sites in Washington | July 2005 (2 weeks) and September 2005 (2 weeks) | 4,737 contacts | 2.75 fewer violations per hour |
| Tarko et al., 2011 | Marion County, Indiana | November/ December 2009 (3 Weeks) | Saturated Patrols | 8% reduction in tailgating by non-trucks |
| Telford et al., 2018 | An unspecified city in Utah | Spring 2014 (3 Weeks) | Citations | 13 fewer passenger car crashes in the city |

Although each study had its own measure of safety, all 5 studies attempted to understand the effects of the TACT program on aggressive driving. Therefore, all the reported safety outcomes were meant to be a proxy for aggressive driving. In this sense the studies can generally be compared to see how many of the TACT programs resulted in a decrease in aggressive driving.

Of the 5 studies, 4 of them showed a reduction in aggressive driving and an increase in safety. These studies were Green (2009), Nerup et al. (2006), Tarko et al. (2011), and Telford et al. (2018). Dye (2016) had mixed results, showing both reductions and increases in aggressive driving across the various reported results. Thus, most studies found that TACT had some success at reducing instances of aggressive driving behavior.

## 7.6 Conclusion

Out of the 5 TACT studies included in this review, 4 found that TACT reduced instances of aggressive driving while one study found mixed results, suggesting that TACT is an effective program. However, the available literature does not support any conclusions regarding the impact of increased amounts of TACT enforcement on safety outcomes.

# 8 Discussion

This chapter discusses suggestions for future research and for practitioners, based on the findings of this report. The research team struggled to conduct certain analyses in this report due to data constraints, and future research would benefit from implementing the suggestions described in this chapter. Similarly, practitioners and grant reviewers may find the results of this synthesis, as well as the limitations of the analysis, when making funding decisions. This chapter splits the discussion into two subgroups, based on the intended audience for the suggestion, but all parties could likely benefit from both subgroupings.

## 8.1 Suggestions for Researchers

The first suggestion is that studies that evaluate or analyze the safety impacts from enforcement should endeavor to collect and report a more detailed description of the enforcement using quantitative measures, such as number of officer enforcement hours, number of checkpoints, number of patrols, dollar amount of paid media, etc. This may be difficult information to acquire for some campaigns, particularly those involving many law enforcement agencies, but acquiring this information would be particularly beneficial in understanding the true scope of an enforcement campaign. In fact, some studies provided almost no quantitative measure of the enforcement campaign, making it impossible to determine how the size of a campaign might affect safety outcomes. If studies more regularly reported quantitative measures of enforcement, it would be possible to conduct a more robust meta-analysis that could determine how the number of officer enforcement hours affects safety outcomes.

Chaudhary et al. (2012) is an example of a study that provided this information. The authors described two distracted driving campaigns and provided information on both the officer enforcement hours and the amount spent on paid media for both campaigns by wave of enforcement. Providing quantitative information, such as officer enforcement hours, allows for comparisons across studies to determine the effect of increasing the officer enforcement hours. Unfortunately, this literature synthesis was unable to make any conclusions about the effects of increased officer enforcement hours. If more studies had reported this type of data, perhaps analyses would have shown a relationship between officer enforcement hours and safety outcomes.

Similarly, the second suggestion is that evaluation should describe the baseline levels of enforcement that exist prior to the specific enforcement effort. A campaign that adds 10 checkpoints could have a very different effect in a town that is already conducting significant numbers of checkpoints compared to one that currently has no checkpoints. Researchers should endeavor to understand what normal police operations look like prior to an enforcement campaign. This will also help researchers discover if there is a point at which running additional enforcement in the form of a targeted enforcement campaign will not provide additional safety benefits. Without information on baseline enforcement levels, it is difficult to fully analyze the effect of enforcement campaigns.

The third suggestion is for researchers to report safety outcomes several weeks or months after an enforcement campaign has concluded. In this synthesis, studies often reported safety outcomes during a campaign or immediately following the campaign, just days after it ended. This does not provide a full picture of how long term the effects of a campaign might be. There is evidence to suggest that the improvements in safety from an enforcement campaign do not fully persist over time, but there is not information on how long the effect is sustained or how quick the drop-off occurs, which could provide relevant information for local law enforcement officers. If the effects of a campaign are mostly sustained for only a month, then perhaps there needs to be a campaign every month or two to maximize safety. If the effects of a campaign are sustained for several months, then campaigns can be run less frequently, saving money without sacrificing safety.

As an example, one study that provided some long-term data was Kayeet al. (1995), who reported seat belt data from before the enforcement campaign, immediately following the campaign, and 3 months after the campaign ended. Providing these three sets of data allowed for a more in-depth look at the effects of this campaign as opposed to only providing the data only referencing before and immediately after the study. An even better model would be providing data regularly after the conclusion of the campaign, such that safety outcomes are recorded one month after the campaign, 2 months after the campaign, and so on, for several months.

The fourth suggestion is that researchers consider research plans informed by concepts of experimental design that would randomly assign sites to be either control locations (experiencing no change in enforcement ) and test locations (receiving additional enforcement ) then randomly assign a specified level of enforcement effort to the test locations to more thoroughly explore the relationship between incremental levels of enforcement and safety outcomes. Without random assignment, there could be (and likely are) external factors that affect both the amount of enforcement and the changes in safety outcomes. For instance, wealthier communities may be more likely to have the resources to conduct more intense enforcement and may also be more receptive to traffic safety messages. As a result, researchers cannot be confident that the observed relationship between enforcement and safety outcomes are a true cause and effect relationship, or are simply correlations with outside unobserved factors. An increase in the variety of enforcement strategies and locations would allow researchers to develop a more robust picture of the impacts of different campaigns.

The implementation of these suggestions could help improve research on the effects of law enforcement on traffic safety. Providing more detailed information is useful in general, but if researchers across the discipline more consistently provide the same detailed information, that will allow future meta-analyses and literature reviews to do intense cross-study comparisons and analyses.

## 8.2 Suggestions for Practitioners

This report had findings that could be relevant for practitioners. This first suggestion is when conducting HVE programs, conduct complete HVE programs; complete HVE is a hybrid enforcement strategy combining enforcement, visibility elements, and a publicity strategy to both educate the public and encourage voluntary compliance with existing laws/statutes. Although this report was generally unable to do substantial statistical analyses, there were a few findings that support this recommendation. The occupant protection analysis showed the significance of enforcement-related paid media spending at improving seat belt use. In addition, the HVE targeting distracted driving produced reductions in handheld cell phone use.

These findings suggest that enforcement programs with large amounts of paid media should be prioritized over programs with low levels of media, or even no media at all. This is not to say that enforcement programs cannot be effective without a media component, but rather that media tends to further improve the results. If SHSOs are deciding between spending more on a media campaign and spending more on direct enforcement action, the findings of this report could validate the decision to choose to focus on additional media. These findings suggest that SHSOs should coordinate with the NHTSA calendar to ensure their enforcement align with large national media buys (e.g., Click It or Ticket).

The second suggestion is to support programing in areas with low baseline levels of safety; this could mean, for example, areas with high crashes, low seat belt use, or high observed handheld phone use. Focusing on these low baseline of safety areas allows SHSOs to get a greater safety return on their investment and prioritize their grant awards on areas in the most need of intervention. The occupant protection analysis showed that programs in areas with higher baseline levels of seat belt use resulted in smaller changes in seat belt use than programs in areas with lower baseline levels of seat belt use.

The third suggestion is that practitioners should collect robust data on their activities. Developing a more complete picture of baseline enforcement activities is necessary to further understand the effectiveness of additional levels of enforcement on safety outcomes. As part of grant applications, applicants could provide information on the typical traffic enforcement strategies currently in place outside of grant funding, which could further aid reviewers in choosing the most appropriate places to fund enforcement programs. Collecting these data demonstrate support of traffic safety initiatives outside of grant funding; it creates the opportunity to identify departments who have a commitment to transportation safety and therefore may be more active during their grant-funded enforcement.

# 9 Synthesis Conclusion

This literature synthesis investigated the relationship between enforcement and changes in safety outcomes. After an exhaustive literature search that considered over 15,000 studies, 80 studies were identified as relevant to this effort. The results of the literature synthesis are provided individually for each of five targeted behaviors: occupant protection, distracted driving, alcohol-impaired driving, speeding, and aggressive driving.

Among those relationships between enforcement activities and safety outcomes that could be analyzed by combining results from several studies, only two relationships were found to be significant, both related to occupant protection. The findings are limited to occupant protection because the available literature on that subject was much more plentiful and consistent than for other targeted behaviors. First, based on an analysis of 27 studies, findings suggest that increasing media spending as part of HVE significantly increases the effectiveness of enforcement on occupant protection (seat belt use). Second, based on the results of 23 enforcement, it was found that increasing the number of checkpoints used during an occupant protection campaign also increases seat belt use rates. Specifically, the results indicate that the impact of changing from 0.04 checkpoints per 10,000 residents per week (the 25th percentile value in the analysis dataset) to 0.24 checkpoints per 10,000 residents per week (the 75th percentile value in the analysis dataset) is expected to be a 1.5 percentage-point increase in seat belt use. This finding supports the theory that increasing the presence of law enforcement will help improve safety outcomes.

In addition to the findings from the synthesis, there were individual articles or groups of articles that directly related to this project's primary research question: what is the impact of various amounts of enforcement on safety outcomes? First, in analyzing the data from a series of 6 annual studies that evaluated the "Buckle Up Kentucky" occupant protection enforcement campaign, a simple scatter plot shows that each additional 100 checkpoints for the statewide campaign increased seat belt use by 2.7 percentage points. (The average number of checkpoints used during the campaign was 813.)

Second, Fell et al. (2014) found that a 1 percent increase in sworn officers per 10,000 residents reduced the rate of impaired driving crashes relative to non-impaired driving crashes by between 2 and 3 percent in a study of 26 communities. Thus, as an illustration of this finding, if 20 percent of crashes involve alcohol-impaired driving, increasing the number of sworn officers by 10 percent would reduce the percent of alcohol-impaired driving crashes to 19.8 percent. This is a very small reduction, especially when considering the large change in officers. The study did not find a relationship between the use of sobriety checkpoints and alcohol-impaired driving, but the authors noted that only a few police departments included in the study reported using checkpoints.

Third, Creaseret al. (2007) studied the effects of an overtime enforcement program that used saturation patrols to identify impaired drivers in the 13 Minnesota counties with the highest numbers of alcohol-impaired fatal and severe-injury crashes. The study results suggested that one additional saturation patrol in the collection of 13 counties would decrease the alcohol-impaired crash rate for those counties by 0.1 percent. This very small magnitude indicates that a large number of saturation patrols are probably required to see significant decreases in the alcohol-impaired fatal crash rate. In fact, a 10 percent reduction would have required 100 additional saturation patrols across the 13 counties.

Due to significant limitations in the details found in the available literature, this research was unable to support additional conclusions about the relationship between increased levels of traditional enforcement, such as number of hours of enforcement by police officers or number of patrols, and changes in safety outcomes. For example, in the case of occupant protection, a measure of hours of enforcement was included in the regression analysis along with various other independent variables. The estimate of the relationship between the number of hours of additional enforcement and the size of the increase in seat belt use was not statistically significant. That finding should not be interpreted to mean that police officer time dedicated to patrols is not impactful. Rather, it means that the available data were not sufficient to determine the relationship between the size of the increase in these activities and the size of the change in safety outcomes.

In the case of alcohol-impaired driving, the analysis was constrained by the large variety of ways safety outcomes were measured in the available literature. Only a limited number of impaired driving studies shared the same safety outcome measure to allow for cross-study comparisons, and those that did tended not to report information that could be used to quantify the level of enforcement that was used to generate the observed safety outcomes.

For speeding, enforcement that were studied in the available literature typically involved placing some sort of visibility element at a certain highway site and observing the resulting speeds of passing vehicles. Thus, the impact of the enforcement effort was observed in relation to the presence or absence of a visibility element and did not lend itself to being measured on a continuous scale, which would allow assessment of the incremental impact of additional enforcement.

Although not directly related to the primary research question, this synthesis produced additional findings related to the effectiveness of enforcement. First, the types of enforcement investigated in the available literature overwhelmingly resulted in improved safety outcomes:

- On average across the 27 studies that explored the impact of HVE on seat belt use rates, *occupant protection* HVE campaigns increased seat belt use rates by 3.5 percent compared to an average baseline seat belt use rate of 77.9 percent;
- Relatedly, analyzing 78 results from 21 studies that investigated the *Click It or Ticket* campaign found that 60 out of the 78 observations resulted in increases in seat belt use. A simple sign test rejects the hypothesis of this result occurring due to random chance, with a p-value of less than 0.0001;
- Across the 6 enforcement examined for this report, *distracted driving* HVE have reduced drivers' handheld phone use rates. Baseline rates averaged 4.9 percent and enforcement produced reductions of 1.1. to 3.7 percentage points, averaging a 36 percent decrease in drivers' handheld phone use;
- Among the studies that investigated the effectiveness of alcohol-impaired driving enforcement, 57 out of 95 efforts resulted in a positive safety outcome, such as a decrease in BAC levels. A simple sign test rejects the hypothesis of this result occurring due to random chance, with a p-value of 0.0188;
- In speed enforcement studies, enforcement produced statistically significant reductions in average speeds. For efforts aimed at work zones, speeds were reduced by 4.2 mph on

average while in non-work zones the reduction was smaller (though still statistically significant) at 0.99 mph;

- Speed enforcement also reduced the percentage of vehicles speeding. Non-work zone efforts comprised most of the results and showed a statistically significant 4.8 percentage-point decline in the percent of vehicles speeding;

Second, analyses showed that the baseline conditions impact the effectiveness of enforcement campaigns:

- In the analysis of occupant protection enforcement across several studies, the baseline level seat belt use rate was found to be a significant predictor of the observed impact of an enforcement effort. Efforts in locations that had higher levels of baseline seat belt use experienced smaller impacts from enforcement, suggesting that the marginal impact of additional enforcement declines with greater effort.

Finally, using enforcement focused on smaller geographic areas appeared to result in better safety outcomes than broad-based State-level initiatives. The effect was particularly noticeable when the target location was picked based on prior information that the area had poor safety outcomes. Alcohol-impaired enforcement campaigns that concentrated on smaller geographic areas, such as a small number of counties or college communities, experienced above average reductions in alcohol-impaired driving (See Creaser et al., 2007; McCartt et al., 2009; Beck et al., 2018; and Nunn & Newby, 2011).

This research was constrained by the lack of consistent details reported in the available literature. In the future, studies that evaluate or analyze the safety impacts from enforcement should collect and report a more detailed description of the enforcement using quantitative measures such as number of officer enforcement hours, number of checkpoints, number of patrols, dollar amount of paid media, etc. In addition, the evaluation should describe the baseline levels of enforcement that exist prior to the specific enforcement effort.

Another recommendation for researchers is to report safety outcomes several weeks or months after an enforcement campaign has ended. An extended reporting period would provide information on the long-term effects of a campaign, whereas most of the studies in the available literature were concerned with only the immediate effects. The last recommendation for researchers is to adopt a research plan informed by concepts of experimental design that would randomly select test sites and assign specified levels of enforcement in a pre-determined manner to better explore the dose-response relationship between incremental levels of enforcement and safety outcomes.

This report also had findings that could be of use to practitioners. One suggestion is that when jurisdictions conduct HVE programs, conduct complete HVE programs; the available literature provides substantial evidence that combining enforcement, visibility elements, and publicity is an effective strategy. Practitioners should also endeavor to collect robust data on their activities, even when they are not conducting a specialized enforcement program; this would allow for greater data availability for researchers.

The implementation of these suggestions would accomplish two goals: first, they would help improve the success of enforcement programs and second, they would help increase data availability such that future research will be able to better understand the relationship between enforcement and safety. While improving safety is the goal, an important intermediate step is understanding which strategies are the most effective for improving safety. Without more data, it is difficult to identify those strategies and estimate the resources necessary to effectively implement them.

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# Appendix A. Occupant Protection Population Estimates

This appendix presents the estimated population values taken from the Census Bureau via the Google Population Search Module in March 2019. The estimates can be seen by study, location, and year in Table A-1. If a study reported enough information on population such that an outside reference was unnecessary, then the study is not reported in this table. Only studies/locations for which it was necessary to look up population information are presented in this table.

*Table A-1. Population Estimates Used in Regression Analyses*

| Study | Location | Year | Estimated Population |
|---|---|---|---|
| Agent et al., 2003 | Kentucky | 2003 | 4,117,000 |
| Agent & Green, 2004 | | 2004 | 4,146,000 |
| Agent et al., 2005 | | 2005 | 4,183,000 |
| Agent et al., 2006 | | 2006 | 4,219,000 |
| Agent et al., 2007 | | 2007 | 4,257,000 |
| Agent et al., 2008 | | 2008 | 4,290,000 |
| Chaudhary et al., 2005 | Reading, PA | 2004 | 80,400 |
| Eby & Vivoda, 2004 | Michigan | 2004 | 10,060,000 |
| Kim & Yamashita, 2003 | Hawaii | 2002 | 1,240,000 |
| Ledingham et al., 2009 | Queens, New York City | 2007 | 2,229,379 |
| | | 2008 | 2,229,379 |
| Nichols et al., 2009 | Iowa | 2006 | 2,983,000 |
| | | 2007 | 2,999,000 |
| | Kansas | 2006 | 2,763,000 |
| | | 2007 | 2,784,000 |
| | Missouri | 2006 | 5,843,000 |
| | | 2007 | 5,888,000 |
| | Nebraska | 2006 | 1,773,000 |
| | | 2007 | 1,783,000 |
| Nichols et al., 2011 | Colorado | 2007 | 4,804,000 |
| | | 2008 | 4,804,000 |
| | Nevada | 2007 | 2,601,000 |
| | | 2008 | 2,601,000 |
| Nichols et al., 2016. | Oklahoma | 2011 | 2,550,000 |
| | | 2012 | 2,550,000 |
| | | 2013 | 2,550,000 |
| | Tennessee | 2011 | 3,860,000 |
| | | 2012 | 3,860,000 |
| | | 2013 | 3,860,000 |
| Solomon et al., 2002 | Alabama | 2002 | 4,480,000 |
| | Florida | 2002 | 16,690,000 |
| | Illinois | 2002 | 12,530,000 |

| Study | Location | Year | Estimated Population |
|---|---|---|---|
| | Indiana | 2002 | 6,156,000 |
| | Mississippi | 2002 | 2,859,000 |
| | Nevada | 2002 | 2,174,000 |
| | 10 Largest Cities in Texas | 2002 | 21,690,000 |
| | Vermont | 2002 | 615,442 |
| | Washington | 2002 | 6,052,000 |
| | West Virginia | 2002 | 1,805,000 |
| Solomon et al., 2007 | United States | 2004 | 292,800,000 |
| Solomon et al., 2009 | | 2007 | 301,200,000 |
| | Asheville, NC | 2007 | 76,748 |
| | Greenville, NC | 2007 | 76,996 |
| | Charleston, WV | 2007 | 50,822 |
| Solomon et al., 2013 | United States | 2008 | 304,100,000 |
| | | 2009 | 306,800,000 |
| Thomas et al., 2017 | Washington | 2007 | 6,462,000 |
| | | 2008 | 6,562,000 |
| | | 2009 | 6,667,000 |
| Tison et al., 2008 | United States | 2006 | 298,400,000 |
| Tison & Williams, 2010 | | 2003 | 292,800,000 |
| | | 2004 | 295,500,000 |
| | | 2006 | 298,400,000 |
| Vasudevan et al., 2009 | Nevada | 2003 | 2,249,000 |
| | | 2004 | 2,346,000 |
| | | 2005 | 2,432,000 |
| Vivoda et al., 2004 | Michigan | 2004 | 2,249,000 |
| Vivoda, St. Louis, et al., 2007 | Florida | 2007 | 18,370,000 |
| Williams et al., 1994 | Elizabeth City, NC | 1993 | 16,804 |
| | High Point, NC | 1993 | 71,799 |
| Williams et al., 1996 | North Carolina | 1993 | 7,043,000 |
| | | 1994 | 7,187,000 |

# Appendix B. Occupant Protection Paid Media Observations

Appendix B shows all observations analyzed in the paid media section, separated by study, wave number, and location. There are a few instances in which not all waves are reported for a study due to a lack of complete data for those waves. The data is organized according to method of enforcement. For some studies, data were collected during the enforcement campaign, not after. The decision was made to not separate these studies from the ones that collected data after the enforcement period. This is because the studies that collected data after the enforcement were often looking immediately after the enforcement (e.g., the day after the campaign ended and were not attempting to draw conclusions about how the effects of a campaign linger—or do not linger—after the end of a campaign. Although studies varied in when they were making observations, they were attempting to measure the more immediate impact of the occupant protection campaign, not the long-term effects.

*Table B-1. Observations from Studies that Reported Paid Media Data*

| Study | Location | Wave # | Weeks | Methods | Hours | Paid Media per 1,000 residents in 2018 $ | % Seat Belt Use Pre- | % Seat Belt Use Post | Seat Belt Use Pre- to Post |
|---|---|---|---|---|---|---|---|---|---|
| Agent et al., 2003 | Kentucky | 1 | 2 | Checkpoints | Unknown | $112.33 | 61.6 | 72.5 | 17.7 |
| Agent & Green, 2004 | | | | | | $125.62 | 64.5 | 70.5 | 9.3 |
| Agent et al., 2005 | | | | | | $211.68 | 66.1 | 68.6 | 3.8 |
| Agent et al., 2006 | | | | | | $73.49 | 67.3 | 67.9 | 0.9 |
| Agent et al., 2007 | | | | | | $79.00 | 73.0 | 76.2 | 4.4 |
| Agent et al., 2008 | | | | | | $89.41 | 74.6 | 75.7 | 1.5 |
| Nichols et al. (2016) | Oklahoma | | | | | $80.74 | 85.30 | 85.70 | 0.5 |
| | | 2 | 1 | | | $37.66 | 85.60 | 85.50 | -0.1 |
| | | 3 | 1 | | | $37.66 | 87.30 | 87.60 | 0.3 |
| | | 4 | 2 | | | $78.78 | 86.90 | 85.70 | -1.4 |
| | | 5 | 1 | | | $33.61 | 85.50 | 86.20 | 0.8 |
| | | 6 | 1 | | | $34.88 | 86.50 | 86.80 | 0.3 |
| | Tennessee | 1 | 2 | | | $100.26 | 81.60 | 82.00 | 0.5 |
| | | 2 | 1 | | | $48.61 | 82.80 | 83.40 | 0.7 |
| | | 3 | 1 | | | $47.47 | 83.20 | 84.90 | 2.0 |
| | | 4 | 2 | | | $72.64 | 84.00 | 84.20 | 0.2 |
| | | 5 | 1 | | | $45.53 | 84.40 | 85.20 | 0.9 |
| | | 6 | 1 | | | $45.81 | 85.90 | 85.80 | -0.1 |

| Study | Location | Wave # | Weeks | Methods | Hours | Paid Media per 1,000 residents in 2018 $ | % Seat Belt Use Pre- | % Seat Belt Use Post | Seat Belt Use Pre- to Post |
|---|---|---|---|---|---|---|---|---|---|
| Solomon et al., 2009 | Asheville, NC | 1 | 2 | | 518 | $970.51 | 83.5 | 85.5 | 2.4 |
| | | 3 | | | 592 | $498.07 | 85.9 | 89.3 | 4.0 |
| | | 4 | | | 406 | $426.18 | 83.7 | 91.0 | 8.7 |
| Tison et al., 2008 | United States | 1 | | | 621,736 | $107.83 | 82 | 81 | -1.2 |
| Tison & Williams, 2010 | | 1 | | | 580,361 | $108.23 | 75.0 | 79.0 | 5.3 |
| | | 2 | | | 546,871 | $132.19 | 79.0 | 80.0 | 1.3 |
| | | 4 | | | 617,990 | $106.37 | 82.0 | 81.0 | -1.2 |
| Williams et al., 1994 | Elizabeth City, NC | 1 | 3 | | 172 | $4,765.81 | 69.0 | 79.0 | 14.5 |
| | Haywood County, NC | | | | 850 | $1,624.60 | 43.0 | 81.0 | 88.4 |
| | High Point, NC | | | | 200 | $1,115.40 | 65.0 | 78.0 | 20.0 |
| Ledingham et al., 2009. | Queens, New York City | | 1 | Checkpoints and Roving Patrols | Unknown | $15.06 | 87 | 89 | 2.3 |
| | | 4 | 1 | | | $14.77 | 85 | 89 | 4.7 |
| Williams et al., 1996 | North Carolina | 1 | 6 | | | $101.45 | 64 | 80 | 25.0 |
| | | 2 | 3 | | | $34.63 | 73 | 81 | 11.0 |
| Solomon et al., 2002 | Alabama | 1 | | Checkpoints and Saturation Patrols | | $76.46 | 70.3 | 78.7 | 11.9 |
| | Florida | | | | | $172.41 | 66.5 | 75.1 | 12.9 |
| | Illinois | | | | | $108.69 | 70.6 | 74.3 | 5.2 |
| | Indiana | | | | | $213.07 | 69.2 | 72.2 | 4.3 |

| Study | Location | Wave # | Weeks | Methods | Hours | Paid Media per 1,000 residents in 2018 $ | % Seat Belt Use Pre- | % Seat Belt Use Post | Seat Belt Use Pre- to Post |
|---|---|---|---|---|---|---|---|---|---|
| | Mississippi | | | | | $157.20 | 53.8 | 61.5 | 14.3 |
| | Nevada | | | | | $181.67 | 70.6 | 76.4 | 8.2 |
| | 10 Largest Cities in Texas | | | | | $65.67 | 80.5 | 86.4 | 7.3 |
| | Vermont | | | | | $442.58 | 66.2 | 84.9 | 28.2 |
| | Washington | | | | | $112.52 | 80.8 | 89.5 | 10.8 |
| | West Virginia | | | | | $188.63 | 56.5 | 71.6 | 26.7 |
| Vasudevan et al., 2009 | Nevada | | 2 | Checkpoints and STEP | 2,906 | $112.96 | 74.9 | 78.7 | 5.1 |
| | | 2 | | | 2,828 | $111.01 | 78.7 | 86.6 | 10.0 |
| | | 3 | | | 1,374 | $98.65 | 86.6 | 94.8 | 9.5 |
| Eby & Vivoda, 2004 | Michigan | 1 | | Enforcement Zones | Unknown | $103.55 | 84.5 | 87.1 | 3.1 |
| Nichols et al., 2007 | Illinois | 1 | | | 4,774 | $75.76 | 83.5 | 85.5 | 2.4 |
| | | 2 | | | 2,902 | $88.39 | 85.5 | 88.3 | 3.3 |
| | Indiana | 1 | | | 520 | $227.29 | 76.3 | 77.0 | 0.9 |
| | | 2 | | | 14,393 | $37.88 | 77.0 | 81.2 | 5.5 |
| | Michigan | 1 | | | 44,708 | $126.27 | 89.4 | 93.2 | 4.3 |
| Nichols et al., 2011 | Colorado | 1 | | Enforcement Zones and Saturation Patrols | 1,075 | $49.44 | 72.0 | 77.0 | 6.9 |
| | | 2 | | | 1,669 | $42.16 | 77.0 | 74.0 | -3.9 |
| | | 3 | | | 1,678 | $48.98 | 74.0 | 75.0 | 1.4 |
| | | 4 | | | 2,303 | $51.17 | 75.0 | 77.0 | 2.7 |
| | Nevada | 1 | | | 1,687 | $63.33 | 79.0 | 80.0 | 1.3 |
| | | 2 | | | 1,687 | $49.51 | 80.0 | 79.0 | -1.3 |

| Study | Location | Wave # | Weeks | Methods | Hours | Paid Media per 1,000 residents in 2018 $ | % Seat Belt Use Pre- | % Seat Belt Use Post | Seat Belt Use Pre- to Post |
|---|---|---|---|---|---|---|---|---|---|
| | | 3 | | | 1,687 | $49.96 | 79.0 | 83.0 | 5.1 |
| | | 4 | | | 1,687 | $58.51 | 83.0 | 87.0 | 4.8 |
| Thomas et al., 2010 | Washington | 1 | | Patrols | 5,715 | $53.16 | 94.6 | 95.3 | 0.7 |
| | | 2 | | | 5,362 | $47.70 | 96.6 | 96.1 | -0.5 |
| | | 3 | | | 6,248 | $53.38 | 94.1 | 95.7 | 1.7 |
| Thomas et al., 2017 | | 4 | | | 5,586 | $47.94 | 95.3 | 97.0 | 1.8 |
| | | 5 | | | 5,650 | $47.38 | 97.1 | 97.2 | 0.1 |
| Vivoda et al., 2004 | Michigan | 1 | 3 | Safety Belt Enforcement Zones | Unknown | $231.59 | 83.8 | 83.6 | -0.2 |
| Solomon et al., 2009 | Greenville, NC | 1 | 2 | Saturation Patrols | 536 | $105.64 | 83.4 | 84.6 | 1.4 |
| | | 3 | 2 | | 329 | $321.95 | 86.2 | 87.6 | 1.6 |
| | | 4 | 2 | | 370 | $602.74 | 86.8 | 87.1 | 0.3 |
| | Charleston, WV | 1 | 2 | Traffic Safety Zones | 739 | $1,009.36 | 58.4 | 61.8 | 5.8 |
| | | 3 | 2 | | 1,041 | $222.47 | 66.1 | 70.2 | 6.2 |
| | | 4 | 2 | | 640 | $754.08 | 64.5 | 60.2 | -6.7 |
| Kim & Yamashita, 2003 | Hawaii | 1 | 3 | Unspecified | Unknown | $384.41 | 83.5 | 90.4 | 8.3 |
| Nichols et al., 2007 | Ohio | 1 | 2 | | 1,204 | $555.61 | 75.5 | 78.7 | 4.2 |
| | | 2 | | | 94,791 | $63.14 | 78.7 | 78.7 | 0.0 |
| | Minnesota | 1 | | | 8,024 | $164.16 | 78.1 | 82.6 | 5.8 |
| | Wisconsin | 1 | | | 32,397 | $113.65 | 65.6 | 73.3 | 11.7 |
| | Iowa | 1 | | | Unknown | $59.58 | 87.8 | 87.6 | -0.2 |

| Study | Location | Wave # | Weeks | Methods | Hours | Paid Media per 1,000 residents in 2018 $ | % Seat Belt Use Pre- | % Seat Belt Use Post | Seat Belt Use Pre- to Post |
|---|---|---|---|---|---|---|---|---|---|
| Nichols et al., 2009 | | 2 | | | | $114.62 | 91.4 | 90.9 | -0.5 |
| | Kansas | 1 | | | | $65.21 | 59.3 | 59.6 | 0.5 |
| | | 2 | | | | $121.33 | 70.6 | 73.5 | 4.1 |
| | Missouri | 1 | | | | $49.29 | 70.1 | 74.5 | 6.3 |
| | | 2 | | | | $37.10 | 66.9 | 72.4 | 8.2 |
| | Nebraska | 1 | | | | $83.64 | 68.5 | 64.2 | -6.3 |
| | | 2 | | | | $167.35 | 74.5 | 74.9 | 0.5 |
| Solomon et al., 2007 | United States | 1 | | | | $113.65 | 80.0 | 82.0 | 2.5 |
| | | | | | | $133.41 | 76.6 | 79.0 | 3.1 |
| Solomon et al., 2009 | | | | | | $107.39 | 81.0 | 82.0 | 1.2 |
| Solomon et al., 2013 | | | | | | $91.24 | 82.0 | 83.0 | 1.2 |
| | | | | | | $79.15 | 83.0 | 84.0 | 1.2 |
| Vivoda, St. Louis, et al., 2007 | Florida | | | | | $123.45 | 74.1 | 74.2 | 0.1 |

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