The Impact of Texting Bans on Motor Vehicle Crash–Related Hospitalizations

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We used a panel design and the Nationwide Inpatient Sample from 19 states between 2003 and 2010 to examine the impact of texting bans on crash-related hospitalizations. We conducted conditional negative binomial regressions with state, year, and month fixed effects to examine changes in crash-related hospitalizations in states after the enactment of a texting ban relative to those in states without such bans.

Results indicate that texting bans were associated with a 7% reduction in crash-related hospitalizations among all age groups. Texting bans were significantly associated with reductions in hospitalizations among those aged 22 to 64 years and those aged 65 years or older. Marginal reductions were seen among adolescents.

States that have not passed strict texting bans should consider doing so. (*Am J Public Health.* 2015;105:859–865. doi: 10.2105/AJPH.2014.302537)

ROADWAY SAFETY, ALTHOUGH much improved over the years,[1,2] remains a major public health concern.[3] In 2009, more than 2.3 million adult drivers and passengers in the United States sought medical attention following involvement in a motor vehicle crash (MVC).[4] Of these individuals, 416 000 (18%) reported being injured in crashes involving a distracted driver.[5] Separate from the psychological[6–9] and physical consequences[10–12] of motor vehicle injuries are significant economic costs.[13,14] Naumann et al. found that the costs of motor vehicle fatality and injury exceeded $99 billion in 2005.[15] Because of limited resources and escalating health care costs, much attention has been devoted to exploring the factors that contribute to roadway safety hazards and to making the public aware of these threats.

Previous research has found that motor vehicular crashes between 2 or more vehicles are often the result of at least 1 driver having been distracted from the primary task of driving.[16,17] According to the National Highway Traffic Safety Administration, there are 3 main types of distraction: (1) taking one's hands off the wheel (i.e., manual distraction), (2) taking one's eyes off the road (i.e., visual distraction), and (3) taking one's mind off driving (i.e., cognitive distraction).[5] It is believed that the use of a cell phone while driving involves all 3 types of distraction.[5]

The negative relationship between cell phone use and driving performance is well documented. A recent systematic review of the distracted driving literature found that of 165 analyses examining the relationship between cell phone use and driving performance, 163 (98%) found a significantly negative association.[18] Thus, it is not surprising that state policymakers have enacted bans on cell phone use while driving.

In 2001, New York implemented the first state ban on talking on a handheld cell phone while driving. Several states, including California and Connecticut, followed suit. However, these early laws allowed handheld dialing and did not explicitly ban text messaging.[19] Some states subsequently enacted legislation explicitly banning drivers from texting (reading, manually composing, or sending text messages, instant messages, or e-mails via a portable electronic device)[20] while driving. However, because of the relative novelty of texting bans, little is known about their impact on roadway safety.

Two recent studies examined the impact of texting bans on motor vehicle fatalities[21,22] and insurance collision claims.[23] Abouk and Adams published the first nationwide study of the impact of texting bans on the occurrence of single-vehicle, single-occupant accidents between 2007 and 2010.[21] They found that texting bans that applied to all drivers were associated with decreases in single-vehicle, single-occupant car accidents. Ferdinand et al. conducted a national study that examined all crash-related fatalities between 2000 and 2010 and found that states with texting bans saw significant reductions in this outcome relative to states with no such bans.[22]

The Highway Loss Data Institute examined the relationship between collision claim frequencies and texting bans in 4 states (California, Louisiana, Minnesota, and Washington).[23] The researchers found that texting bans were associated with increased frequencies of collision claims. They posited that this increase may have stemmed from drivers lowering their phones from view to avoid citations and fines and, in so doing, taking their eyes off the road more than they did before the implementation of the bans. To our knowledge, no one has examined the impact of texting bans on hospitalizations resulting from MVCs.

We examined the overall effectiveness of texting bans in preventing MVC-related hospitalizations. In addition, we examined the effect of texting bans on MVC-related hospitalizations by different age cohorts because texting while driving is thought to occur most frequently among younger individuals. Our study will help inform policymakers, health care administrators and providers, and other stakeholders interested in decreasing the occurrence of MVC-related hospitalizations.

METHODS

Within each state are hospitals caring for MVC victims. Ideally, one would examine the number of hospitalizations owing to MVCs in each state-year. Such national data are not available. Data from each state are available, in principle, but not all states employ a coding system that allows identification of crash-related admissions, and the aggregation of such multistate data is prohibitively expensive.

Consequently, we employed a strategy of using the Health Care Cost and Utilization Project Nationwide Inpatient Sample (NIS)[24] to estimate the effects of state motor vehicle laws on hospital admissions. The NIS contains a 20% stratified random sample of US community hospitals in each year. The stratification is predicated on region, ownership type, urban versus rural location, bed size, and teaching status. A growing number of states have participated in the NIS over time. We limited our analysis to 19 states that were represented in the NIS sample each year. Each selected hospital reports hospital discharge data on 100% of its cases in that year. Thus, we included patients from all payer sources. We used 2003 to 2010 NIS data to construct the study panel.

Study Sample

We identified hospitalizations for which the *International Classification of Diseases, 9th revision* (*ICD-9*)[25] external cause of injury codes (E-codes) for motor vehicle traffic accidents were present (E810–E819). Because E-code reporting requirements and collection vary by state, we restricted the study sample to states with E-code reporting completion rates of 85% or higher.[26] Because of variations in E-code reporting rates from year to year, we checked these rates for each NIS-participating state in 2003, 2006, and 2010, and we included only those states that did not fall below the 85% completeness threshold in any of these years.

We quantified E-code reporting rates as the proportion of hospital discharges with an injury *ICD-9* code as a primary diagnosis with a valid E-code.[27] Additionally, because of previous research showing that states with secondarily enforced laws (i.e., laws stipulating that a police officer have another reason to stop a vehicle before citing a driver for texting while driving) did not see significant decreases in MVC-related fatalities,[22] we included only those states with primarily enforced texting laws (i.e., laws stipulating that an officer need not have another reason for stopping a vehicle) or no texting law at all (n = 19). We constructed age-specific cohorts to examine whether texting bans have affected different groups of individuals in varying ways. The final data set contains 96 months (8 years) of data from 19 states (n = 1824 state-month-year observations).

Texting Laws

To identify states with primary texting laws, we accessed a list of state bans (including statute numbers) on the use of mobile devices while driving from the Distracted Driving Laws data set developed by the Public Health Law and Research Program.[19] This data set provides information on activities regulated, persons banned, and the associated stringency levels.

Because this data set does not distinguish between overarching bans on the use of handheld devices (i.e., handheld bans) and explicit bans on texting while driving, we used the statute number to access and read each state law via the LexisNexis legal database. This allowed specific identification of states with explicit legislative language banning texting while driving. We characterized texting bans by using a binary indicator for whether, in a particular month-year, a state had a primary texting ban in place.

Other Variables

It has been shown that graduated driver licensing (GDL) programs have some bearing on motor vehicular injuries[28,29] and fatalities.[29–32] These laws limit adolescent and novice nighttime driving and the number of passengers that these drivers can transport. Furthermore, supervision by more experienced drivers is a key feature in GDL programs. The logic for including GDL programs as a control variable is that crash risk exposure is significantly diminished when such a program exists in a particular state. Thus, we included a binary measure in our models to account for the presence of a GDL program in a particular state-month-year.

Additionally, we included controls for laws regulating driving while under the influence of alcohol. Previous research has shown these laws to be important predictors of roadway safety.[33,34] Thus, we included a binary indicator for whether the state's licensing authority can suspend driving privileges after the first offense of drunk driving (administrative license revocation). Several states decreased their blood alcohol concentration limits from 0.10 to 0.08 during the study period. Consequently, we also included a binary indicator for whether it was illegal per se to drive with a blood alcohol concentration of 0.08 in a particular state-month-year.

Empirical research has also shown that seatbelt laws serve to enhance roadway safety.[35,36] Thus we included a binary variable indicating whether in a particular month and year a state had a primarily enforced seatbelt law in place. Additionally, because some states passed handheld bans on all drivers during the study period (California, Connecticut, New York, Oregon, and Washington), we included a binary indicator for whether, in a particular state, month, and year, there was a handheld ban in place regulating all drivers. Moreover, recognizing that speed limits have previously been shown to be associated with motor vehicular fatalities and to be consistent with previous research,[30,37,38] we included a binary variable that distinguishes those states with speed limits of 70 miles per hour or more from those without such speed limits.

We acquired information on implementation dates of GDL programs, per se administrative license revocation, interstate speed limits, seatbelt laws, and handheld bans from the Insurance Institute for Highway Safety. We obtained blood alcohol concentration implementation dates from the Alcohol Policy Information System.

It has been demonstrated that higher gasoline prices precede fewer miles driven and therefore reduced crash risk exposure.[38,39] Thus, we controlled for retail gasoline prices (obtained from the Energy Information Administration) in our models. Furthermore, researchers have shown that state macroeconomic factors are associated with road safety outcomes.[39,40] Thus, using US Bureau of Labor Statistics and Bureau of the Census data, we controlled for state annual unemployment rates by year and state per capita income by year, respectively. Finally, consistent with earlier traffic fatality research, we accounted for each state's exposure to crash risk by including state population estimates by year in our models.[31] We obtained population estimates from the US Census Bureau.

Models

We used pooled cross-sectional time series data and a difference-in-difference framework with state, month, and year dummy variables to examine the relationship between the presence of primarily enforced texting bans on traffic-related hospitalizations. The state-level dummy variables controlled for all state-specific factors that are potentially correlated with traffic-related hospitalizations that are generally time invariant, such as a state's level of law enforcement, roadway conditions, and weather patterns. The month dummy variables controlled for largely time-invariant factors that may be correlated with traffic-related hospitalizations, such as periods of widespread travel. The year dummy variables controlled for unobserved factors that vary from year to year that have some bearing on traffic-related hospitalizations, such as improved automotive technologies.

We used count data models for which the dependent variable was an MVC-related hospitalization count in a particular state, month, and year. We used this approach because many state-month-year cells contained very small numbers of MVC-related hospitalizations. More specifically, nearly 23% of our state-month-year observations had 10 or fewer traffic-related hospitalizations, and more than 47% had 25 or fewer such hospitalizations. Thus, because crash-related hospitalizations were not normally distributed in our data set and always took integer values and because the state-month-year conditional variances were larger than were the conditional means, we used conditional negative binomial regressions to estimate the relationship of interest.

Count data models often require an exposure variable that indicates the number of times the occurrence of interest could have happened. Because the NIS samples different numbers of hospitals each state-year, we used the total monthly number of nonelective hospitalizations in a particular state-year as the exposure variable.

Our model specifications took on the following basic functional form:

$$(1)\ Y_{imt} = f\,(Text_{imt} L_{imt} Z_{imt} S_i\, M_m\, T_t),$$

where Y_{imt} is the MVC-related hospitalization count for state i at month m and year t, $Text_{imt}$ is the presence of a primary texting ban for state i at month m and year t, L_{imt} is a vector of other laws affecting crash risk exposure (handheld bans, seatbelt laws, blood alcohol concentration laws, speed limits, and GDL programs), Z_{imt} is a vector of time varying covariates (gasoline prices, state unemployment rate, state per capita income, state population estimates, and total nonelective hospitalizations), S_i is a vector of state dummy variables, M_m is a vector of month dummy variables, and T_t is a vector of year dummy variables.

In addition to examining the effects of primary texting laws on the overall population, we conducted a series of subgroup analyses by constructing age cohorts to determine whether texting laws affect different age groups dissimilarly. Moreover, to further check the robustness of our results, we conducted a series of falsification analyses examining the effect of texting bans on subsets of non-MVC hospitalizations. These included diabetes, hypertension, influenza, and osteoarthritis-related hospitalizations as well as hospitalizations for injuries sustained in non-MVC accidents such as being accidentally struck by a falling object, accidents caused by explosion, and accidents caused by firearms.

The logic for conducting these falsification analyses is straightforward. The occurrence of other kinds of hospitalizations should be largely unaffected by texting bans, as these laws were not intended to reduce non-MVC hospitalizations. Thus, if the difference-in-difference models generated reliable estimates lending insight into the true relationship between texting bans on MVC hospitalizations, we would expect to observe statistically insignificant effects in similarly constructed models of non-MVC hospitalizations.

We conducted all analyses in Stata version 12 (StataCorp LP, College Station, TX). We have reported statistical significance at the .01 and .05 levels.

RESULTS

Nineteen NIS-participatory states met our inclusion criteria of 85% or more complete E-code reporting and the presence of a primarily enforced texting ban or no such ban at all. These states were California, Colorado, Connecticut, Massachusetts, Michigan, Minnesota, Missouri, Nebraska, New Jersey, New York, North Carolina, Oregon, Rhode Island, Tennessee, Texas, Utah, Vermont, Washington, and Wisconsin. Of these 19 states, 15 had passed a primarily enforced texting ban at some point during the study period. Oregon and Washington were the first to pass a primary texting ban between 2003 and 2010, with their laws having taken effect on January 1, 2008. The last state to enact a texting ban during the study period was Wisconsin (December 1, 2010).

Table 1 presents the descriptive statistics for our panel data

TABLE 1—Descriptive Statistics for State Panel Data in Effects of Texting Bans on Motor Vehicle Crash Hospitalizations: Health Care Cost and Utilization Project Nationwide Inpatient Sample, United States, 2003–2010

Variable	Mean (SD)
Texting ban	0.14 (0.34)
Motor vehicle crash hospitalizations per state-month-year	57.30 (78.21)
Gasoline prices in 2010 cents	234.99 (56.80)
State per capita income in 2010 dollars	41 153.46 (6 055.41)
State unemployment rate, %	6.19 (2.19)
Handheld ban, all drivers	0.10 (0.30)
Speed limit ≥ 70 mph	0.16 (0.37)
Primary seatbelt law	0.51 (0.50)
Administrative license revocation	0.79 (0.41)
Illegal per se at 0.08 BAC	0.95 (0.23)
Graduated driver licensing law	0.89 (0.31)

Note. BAC = blood alcohol concentration. The mean values for state laws are interpreted as the proportion of the 1824 state-month-years in which the law was in place. To be compliant with the Health Care Cost and Utilization Project Nationwide Inpatient Sample data use agreement, average age-specific hospitalization counts are not presented.

representing 1824 state-month-year observations. On average, there were 57.3 MVC-related hospitalizations in a particular state-month-year. The average gasoline retail price was approximately 235 cents (in 2010 cents) and the average state per capita income was $41 153.46 (in 2010 dollars). State laws stipulating a 0.08 blood alcohol concentration limit were in effect for the longest proportion of time (95%). Texting bans were in effect for 14% of the study period.

The evaluation results from the difference-in-difference models for all MVC-related hospitalizations are presented in Table 2. The column labeled "Model 1" presents results from the most parsimonious model specification, which included monthly MVC-related hospitalization counts as the dependent variable, a single binary variable representing the presence of a primary texting ban, and state, month, and year dummy variables as controls. Results from this model showed that, on average, sampled hospitals in states with a primarily enforced texting law saw marginally significant reductions in MVC-related hospitalizations among all age cohorts (incidence rate ratio [IRR] = 0.96; 95% CI = 0.91, 1.01; P = .086).

Model 2 introduces control variables for earlier laws geared toward reducing adverse traffic outcomes. This model indicated that, on average, the presence of a primary texting ban was associated with an estimated coefficient of –0.089, suggesting an approximately 9% reduction in MVC-related hospitalizations among all age cohorts in sampled hospitals (IRR = 0.91; 95% CI = 0.86, 0.96; P = .003). This implies that the average sampled hospital in a state with a primarily enforced law explicitly banning texting while driving saw 5.16 fewer MVC-related hospitalizations per month. Primary seatbelt laws (IRR = 0.82; 95% CI = 0.77, 0.89; P < .001) and GDL programs (IRR = 0.77; 95% CI = 0.68, 0.87; P < .001) were also associated with reductions in MVC-related hospitalizations in this model.

The economic and state population control variables were introduced in model 3. The results indicated a 7.2% reduction in MVC-related hospitalizations among all age cohorts in sampled hospitals in a state with a primarily enforced texting ban (IRR = 0.93; 95% CI = 0.88, 0.99; P = .017). Thus, the estimated effects of texting bans largely remained the same after controlling for both legal and economic factors and state population size. Primary seatbelt laws (IRR = 0.88; 95% CI = 0.82, 0.95; P = .001) and GDL programs (IRR = 0.74; 95% CI = 0.66, 0.84; P < .001) remained significantly

TABLE 2—Results of Multivariate Analysis of the Effects of Texting Bans on Motor Vehicle Crash Hospitalizations: Health Care Cost and Utilization Project Nationwide Inpatient Sample, United States, 2003–2010

Variable	Model 1, IRR (95% CI)	Model 2, IRR (95% CI)	Model 3, IRR (95% CI)
Primary texting law	0.96 (0.91, 1.01)	0.91** (0.86, 0.96)	0.93* (0.88, 0.99)
Handheld ban, all drivers		1.13** (1.06, 1.21)	1.26** (1.15, 1.39)
Speed limit ≥ 70 mph		0.81 (0.61, 1.07)	1.36* (1.00, 1.84)
Administrative license revocation		1.09 (0.86, 1.37)	1.49** (1.17, 1.90)
Primary seatbelt law		0.82** (0.77, 0.89)	0.88** (0.82, 0.95)
Illegal per se at 0.08 BAC		1.21** (1.10, 1.33)	1.15** (1.04, 1.27)
Graduated driver licensing law		0.77** (0.68, 0.87)	0.74** (0.66, 0.84)
Gasoline prices, 2010 cents			0.99 (0.99, 1.00)
Per capital income, 2010 dollars			1.00 (0.99, 1.00)
State unemployment rate			1.01 (0.99, 1.03)
State population estimates			1.00** (1.00, 1.00)

Note. BAC = blood alcohol concentration; CI = confidence interval; IRR = incidence rate ratio. The sample size was n = 1824 state-month-years. Each model includes state, month, and year dummy variables as controls and uses total state nonelective hospitalizations as the exposure variable.
*P < .05; **P < .01.

TABLE 3—Results of Multivariate Analysis of the Effects of Texting Bans on Motor Vehicle Crash Hospitalizations for Different Age Cohorts: Health Care Cost and Utilization Project Nationwide Inpatient Sample, United States, 2003–2010

Variable	Aged 15–21 Years, IRR (95% CI)	Aged 22–64 Years, IRR (95% CI)	Aged ≥ 65 Years, IRR (95% CI)
Primary texting ban	0.92 (0.84, 1.00)	0.91** (0.85, 0.97)	0.91* (0.85, 0.98)
Handheld ban, all drivers	1.09 (0.95, 1.24)	1.31** (1.19, 1.45)	1.10 (0.98, 1.24)
Speed limit ≥ 70 mph	0.72 (0.44, 1.15)	1.36 (0.99, 1.87)	0.72 (0.35, 1.46)
Administrative license revocation	1.22 (0.77, 1.94)	1.57** (1.21, 2.04)	0.99 (0.44, 2.18)
Primary seatbelt law	0.80** (0.71, 0.90)	0.88** (0.82, 0.95)	0.92 (0.83, 1.02)
Illegal per se at 0.08 BAC	1.23** (1.05, 1.43)	1.17** (1.04, 1.30)	1.10 (0.96, 1.27)
Graduated driver licensing law	0.79* (0.64, 0.98)	0.78** (0.68, 0.89)	0.79* (0.68, 0.99)
Gasoline prices, 2010 cents	1.00 (0.99, 1.00)	0.99 (0.99, 1.00)	0.99 (0.99, 1.00)
Per capita income, 2010 dollars	0.99 (0.99, 1.00)	0.99 (0.99, 1.00)	0.99 (0.99, 1.00)
State unemployment rate	1.01 (0.98, 1.05)	1.01 (0.99, 1.04)	1.01 (0.99, 1.04)
State population estimates	1.00** (1.00, 1.00)	1.00** (1.00, 1.00)	1.00** (1.00, 1.00)

Note. BAC = blood alcohol concentration; CI = confidence interval; IRR = incidence rate ratio. Each model includes state, month, and year dummy variables as controls and uses total state nonelective hospitalizations as the exposure variable.
*$P < .05$; **$P < .01$.

associated with MVC-related hospitalization reductions in this model.

Table 3 presents estimation results from our subgroup analyses in which we examined the effect of primary texting bans on total MVC-related hospitalizations among different age cohorts. Primary texting laws were marginally associated with MVC-related hospitalizations among those aged 15 to 21 years in sampled hospitals (IRR = 0.92; 95% CI = 0.84, 1.00; $P = .081$). Among those aged 22 to 64 years, primary texting bans were associated with significant MVC-related hospitalization reductions (IRR = 0.91; 95% CI = 0.85, 0.97; $P = .012$). Primary texting bans were also associated with decreases in MVC-related hospitalization counts among individuals aged 65 years or older (IRR = 0.91; 95% CI = 0.85, 0.98; $P = .02$).

Table 4 contains estimation results for our falsification analyses in which we used non-MVC hospitalizations, such as hospitalizations resulting from firearm and machinery accidents, complications of diabetes, hypertension, influenza, and osteoarthritis. Results indicated that primary texting laws are not associated with reductions in other non-MVC accident (IRR = 0.86; 95% CI = 0.65, 1.19), diabetes (IRR = 1.14; 95% CI = 0.79, 1.64), hypertension (IRR = 1.05; 95% CI = 0.73, 1.45), influenza (IRR = 1.05; 95% CI = 0.87, 1.28), and osteoarthritis (IRR = 0.97; 95% CI = 0.72, 1.31) -related hospitalizations.

DISCUSSION

Two main findings emerged from our analyses. First, primary texting laws were effective in reducing MVC hospitalizations among sampled hospitals in the NIS. Our results indicate that the average texting bans led to reductions in MVC hospitalizations of at least 4.3% among sampled hospitals. Because of the mean of 57 MVC-related hospitalizations in the average sampled hospital, this percentage decrease translates into approximately 30 MVC-related hospitalizations prevented per year in sampled hospitals following the enactment of a primary texting ban. Thus, our findings suggest that states that have not passed primarily enforced texting bans should consider doing so.

Second, when we examined the relationship between texting bans and MVC-related hospitalizations by age cohorts, results indicated that there were substantive differences in the effect of the laws on individuals in different age groups. However, the age is that of the hospitalized person, and not necessarily the driver, although it is conceivable that drivers constitute a considerable proportion of those hospitalized. Moreover, to the extent that the hospitalized person was indeed a driver, he or she may not have been the driver that caused the crash.

Nevertheless, primary texting bans had only a marginal effect on reducing MVC-related hospitalizations among younger individuals, whereas the same laws had significant effects in reducing MVC-related hospitalizations among the adult-aged cohorts. This suggests that further research should explore additional avenues for decreasing MVC-related hospitalizations among younger individuals. These additional avenues may include education to increase awareness and compliance with state texting bans, improved enforcement by law enforcement personnel, and technological initiatives to limit the ability of individuals to text while driving.

It should be noted that the estimates of some of the legal control variables contrasted with the findings of some previous studies. For example, alcohol-related policies have been shown to be associated with reductions in adverse traffic outcomes.[34,41] Because our study period contained only 8 very recent years, we captured primarily within-state variations in the enactment of texting bans, and not within-state variations in less recently enacted polices. Thus, we had very weak power for measuring the effect of these older laws on MVC-related hospitalizations. Additionally, handheld bans were associated with increases in MVC-related hospitalizations.

TABLE 4—Falsification Analyses, Results of Multivariate Analysis of Non–Motor Vehicle Crash Hospitalizations: Health Care Cost and Utilization Project Nationwide Inpatient Sample, United States, 2003–2010

Non–Motor Vehicle Crash Hospitalizations	Texting Ban, IRR (95% CI)
Other accidents,[a] E-codes 916–928	0.86 (0.65, 1.19)
Diabetes	1.14 (0.79, 1.64)
Hypertension	1.05 (0.73, 1.45)
Influenza	1.05 (0.87, 1.28)
Osteoarthritis	0.97 (0.72, 1.31)

Note. BAC = blood alcohol concentration; CI = confidence interval; IRR = incidence rate ratio. Each model includes state, month, and year dummy variables as controls and uses total state nonelective hospitalization counts as the offset variable. All models include gasoline prices in 2010 cents, state per capita income in 2010 dollars, state unemployment rate, and binary indicators for the presence of a handheld ≥ 70 miles-per-hour speed limits, administrative license revocation, illegal per se at 0.08 BAC, primary seat belt, and graduated driver licensing laws in a particular state-month-year. The dependent variable is the disease-specific hospitalization count.

[a]Includes hospitalizations associated with being struck accidentally by a falling object; struck accidentally by objects or persons, or caught accidentally in or between objects; and accidents caused by machinery, cutting and piercing instruments or objects, explosion of pressure vessel, firearm and air gun missile, explosive material, hot substances or objects, caustic or corrosive material and steam, electric currents, exposure to radiation, overexertion and strenuous movements, or other unspecified environmental and accidental causes.

This finding contrasts with previous findings that handheld bans are associated with decreases in traffic fatalities among adults.[22] However, because of the strict inclusion criteria in terms of E-code completeness, we were unable to include all the states that had implemented handheld bans during the study period, and 3 of the 5 states with handheld bans had effective dates of these bans toward the end of the study period. Thus, we likely lacked sufficient power to estimate the true effect of handheld bans on MVC hospitalizations. Inclusion of all states with handheld bans may have rendered different results.

Our study contained a few limitations. Because the NIS is designed to be nationally representative and not necessarily representative of participating states, statewide estimates could not be presented but rather only insight into hospitalization count changes among sampled hospitals in participating states. Furthermore, because of the relative novelty of texting bans in the United States, we did not examine the long-term impacts on MVC-related hospitalizations.

Moreover, because not all states participate in the NIS and some states have very low E-code reporting, we were unable to examine the impact of texting laws in several states that have enacted texting bans. Moreover, texting laws may have an impact on less serious traffic outcomes, such as acute injuries treatable at accident scenes or that result in emergency department visits. Future research should examine these relationships to add to the knowledge base on the impact of texting laws.

Lastly, we note that our study examined the presence of texting laws in states and not the extent to which these laws were actually enforced during the study period. The difficulties faced by law enforcement personnel in enforcing these bans have been noted.[42,43] Some states track texting-related citations, whereas others do not.[41] Thus, national data on law enforcement relative to texting violations was not available.

Despite these limitations, this study makes a contribution to the limited literature on the impact of texting laws. Although previous research examined the relationship between texting bans and collision claims and fatalities, no such examination had been done on crash-related hospitalizations. Our findings suggest that the efforts made by some states to implement strict texting bans are working to promote roadway safety and, by extension, public health. ■

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Contributors

A. O. Ferdinand drafted the article. A. O. Ferdinand, N. Menachemi, B. Sen, and M. Morrisey conceptualized the study design and participated in the analysis and interpretation of data. J. L. Blackburn participated in the analysis and interpretation of data. L. Nelson participated in the interpretation of regulations. All authors revised the article.

Human Participant Protection

This study was approved by the University of Alabama, Birmingham's institutional review board.

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