Review of recent literature on the effectiveness of distracted driving laws

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Abstract

A review of 16 studies examining the relationship between laws banning cellphone use and crash outcomes found an encouraging reduction in crashes. On average, bans of texting or cellphone use were associated with an approximate 6% reduction in fatal crash metrics, and those laws with a primary enforcement component were associated with a 12% reduction. The results do not represent strong evidence of a causal relationship, however, due to methodological limitations.

Introduction

This review is a summary of studies published during 2015–2020 that evaluated the effectiveness of bans on cellphone use. It is an update to the work of McCartt, Kidd, and Teoh (2014), who reviewed 13 studies published through 2013 that analyzed the relationship between handheld phone or texting bans and various crash outcome measures and found mixed results. The current review includes 16 studies that measured whether crash reductions could be associated with statewide bans on handheld phone or texting behaviors (Table 1). Of the 16, 11 focused on the effectiveness of cellphone bans at the national level, and the remaining five examined law changes in a limited number of jurisdictions.

Table 1. Studies included in this review

	Primary outcome measure
National studies	
Benzaman (2017)	Fatality count and rate
Dong et al. (2017)	Fatal crashes
Dong et al. (2018)	Fatal crashes
Flaherty et al. (2020)	Fatality rate (population)
French and Gumus (2018)	Fatality rate (population)
Karl and Nyce (2019)	Insurance losses
Karl and Nyce (2020)	Insurance claims
Noland and Zhou (2017)	Fatality count
Rocco and Sampaio (2016)	Fatality count
Rudisill et al. (2018)	Fatality rate (population)
Tsai et al. (2015)	Fatality rate (VMT)
Small-scale studies	
Curran et al (2019)	Cellphone-related crashes
Ferdinand et al (2019)	Vehicle-related ER crashes
Liu et al. (2019)	Cellphone-related crashes
Roper (2017)	Cellphone-related crashes
Shuster (2018)	Injury crashes, fatal crashes

National studies

The national studies (Benzaman, 2017; Dong, Nambisan, Xie, Clarke, & Yan, 2017; Dong, Xie, Zeng, & Li, 2018; Flaherty, Kim, Salt, & Lee, 2020; French & Gumus, 2018; Karl & Nyce, 2019, 2020; Noland & Zhou, 2017; Rocco & Sampaio, 2016; Rudisill, Chu, & Zhu, 2018; and Tsai, Alhwiti, Swartz, & Megahed, 2015) varied considerably in their use of outcome measures (e.g., counts of fatally injured occupants/fatal crashes, fatality rates based on population or vehicle miles traveled [VMT], insurance claim rates), subpopulations (all drivers, age-specific ranges, motorcyclists), covariates, and operational definitions regarding factors such as law strength. The majority of these studies used variations on time series analysis.

Each study except those by Benzaman (2017) and Tsai et al. (2015) found at least one statistically significant effect that indicated crash or insurance claim reductions for some ban on cellphone use. The studies by Karl and Nyce (2019, 2020) were the only ones to examine insurance records rather than data from the Fatality Analysis Reporting System (FARS). They found a 3% reduction in total insurance losses for states that enacted primary bans (2019) and an estimated 9% reduction in injury liability claims in states that enacted a primary handheld ban (2020)

Of the nine FARS-based studies, four (Benzaman, 2017; Dong et al., 2017; Dong et al., 2018; Noland & Zhou, 2017) were methodologically flawed or produced unexplained, illogical results.

Benzaman (2017) over-relied on imputing values for covariates and used over-specified models (e.g., including number of registered vehicles, average daily traffic, and annual VMT in the same analysis). The author reported significant changes in fatalities or fatal crash rates for cellphone and texting bans, but effects for the bans were in opposite directions depending on the modeling approach. A regression approach showed reductions in fatalities for cellphone bans, but a panel analysis indicated significant increases in the fatal crash rate for texting and cellphone bans.

Dong and colleagues (2017, 2018) also used an excessive number of covariates in their work (e.g., 48 predictor variables in the 2017 study). Their 2018 results indicated an unrealistic 45% reduction in fatal crash counts for texting bans and a smaller but still difficult-to-believe 34% reduction for handheld cellphone bans. Dong et al. (2017) reported no significant effect for handheld cellphone bans but a 27% reduction in fatal crashes for enacting a texting ban.

Noland and Zhou (2017), who estimated that any handheld or texting ban was associated with a 2.2% reduction in fatality counts, used too many covariates and provided insufficient information to the reader (e.g., omitting data for nonsignificant results). Noland and Zhou also presented illogical results: In evaluating traffic fatalities from 1983–2013, they included a covariate for seat belt laws, found no significant effect for them, and then dismissed the result by saying that the finding was of no concern as prior studies found belt laws were effective.

The five remaining national studies (Flaherty et al., 2020; French & Gumus, 2018; Rocco & Sampaio, 2016; Rudisill et al., 2018; Tsai et al., 2015) relied on adequate methods (e.g., used a reasonable number of covariates, with each at least controlling for mileage driven or fuel consumption, employment, and major safety laws) and provided enough detail to transform various fatal crash metrics (population-based fatality rates, fatality rate per VMT, or fatality counts) into percent changes. The nature of these studies — their small number, varying outcome measures and reliance on the same FARS data over similar time periods — did not support a full meta-analysis. Instead, a simple combined measure of how these laws affect fatal crash outcomes was calculated as an unweighted average of their reported effects (Table 2).

Tsai et al. (2015) compared the effect of any statewide texting or handheld ban for all drivers against the absence of any such ban and reported that the 0.8% reduction in fatality rate that they estimated occurred after the ban was not statistically significant. The authors of the other four studies included at least one factor to estimate different aspects of bans, for example, handheld versus texting bans or primary versus secondary enforcement, but they did so in dissimilar ways.

To arrive at the five-study average of a 5.8% decrease in fatal crash metrics for any statewide ban listed in Table 2, effects for different types of bans within a study were combined to create an overall average effect for that study, and these overall effects were then averaged together.

Table 2. Average reduction in fatal crash metrics for enacting statewide bans on cellphone behaviors

Study	Time period analyzed	Average effect for primary handheld ban for all drivers	Overall average effect for any texting or handheld ban
Flaherty et al. (2020)[1]	2007–2017	−25.0%	−18.2%
French and Gumus (2018)[2]	2005–2015	−4.5%	−2.5%
Rocco and Sampaio (2016)	1991–2009	−9.3%	−4.2%
Rudisill et al. (2018)	2000–2014	−10.0%	−3.5%
Tsai et al. (2015)	2005–2011	—	−0.8%
Five-study average	—	−12.2%	−5.8%

[1] Flaherty et al. (2020) values are based on averaging together the effects for 16- to 19-year-old and 40- to 55-year-old drivers.

[2] French and Gumus (2018) values are based on those provided for nonmotorcyclist passenger-vehicle fatality rate.

The five studies differed in how various aspects of cellphone legislation were modeled, which limited summarizing marginal effects across the studies. Primary enforcement of a statewide handheld ban for all drivers was the only other legislation-related factor that was consistently evaluated across these studies, with all but Tsai et al. (2015) including an effect. As indicated in Table 2, among the four studies that examined primary enforcement of an all-driver handheld ban, there was an average 12.2% reduction in fatal crash outcomes. Like the results of Dong and colleagues discussed above, the 25% and 18% reductions in the fatality metric reported by Flaherty are too large to believe. (Whether the metric was crashes or occupants is unclear in the manuscript.)

Small-scale studies

Curran, Graham, and Burk (2019); Ferdinand, Aftab, and Akinlotan (2019); Liu, Lu, Wang, Sharma, and Shaw (2019); Roper (2018); and Shuster (2018) conducted less-comprehensive evaluations of cellphone laws on various crash outcomes.

Curran et al., Liu et al., and Roper used police-reported crashes due to electronic device use as their primary outcome measure. Curran et al., who found fatalities and police-reported traffic crashes increased significantly in Oklahoma after cellphone legislation was introduced, and Liu et al., who found that handheld bans reduced police-reported cellphone crashes in California by 66%, were single-state case studies without controls.

Shuster conducted two case studies without controls to assess the effect of Ohio's secondary and Pennsylvania's primary texting bans on police-reported fatal and injury crashes over time. Shuster reported no significant change in these metrics in either state after the legislation.

The study by Ferdinand et al. (2019) focused on motor-vehicle-related emergency room visits in 11 states that enacted universal texting bans. Ferdinand and colleagues seemed to use an appropriate statistical model in terms of specifying covariates, but they only included one state (Arizona) as a comparison group. In addition, some of the authors' results for covariates are unreasonably large. For example, they estimated a 3,000% decrease in emergency room visits for having a speed limit below 70 mph.

Discussion

Averaging the overall effects of the five national studies that used reasonable methods and comparable measures shows fatal crash outcomes went down about 6% after enacting statewide handheld or texting bans for all drivers. When focusing on the four suitable studies that evaluated legislation permitting primary enforcement of a handheld cellphone ban, the average reduction in fatal crash outcomes doubles to just over 12%. This increased effectiveness for primary enforcement was also noted by Karl and Nyce who used acceptable methods but not comparable measures. Likewise, primary enforcement of seat belt laws also has been shown to be more effective than secondary enforcement (Dinh-Zarr, 2001).

The five-study averages are clearly affected by the large values reported by Flaherty et al. (2020). As noted in the footnote under Table 2, the separate effects Flaherty reported for 16- to 19-year-old drivers and 40- to 55-year-old drivers were combined for this review. The estimates for 40- to 55-year-old drivers were considerably larger than the effects reported in the other studies. It's likely that the Flaherty results would have been different if the authors had included the whole age range.

The major methodological limitation of the five studies included in calculating the average reductions in fatal crash outcomes was the lack of a within-state comparison group of drivers or crashes unaffected by distraction. For example, in IIHS evaluations of GDL laws, adult drivers within states implementing laws served as a comparison for teen drivers in those states (McCartt, Teoh, Fields, Braitman, & Hellinga, 2010). This limitation makes the reductions reported across studies less convincing. Identifying appropriate in-state comparisons in future research on all-driver cellphone bans will remain a challenge, however, as the effect of a ban should be expected to be similar among drivers within a given subpopulation, and connecting cellphone use to specific crashes is problematic.

The McCartt et al. (2014) review highlighted that the existing studies at the time had divergent outcomes, with a relatively even distribution of studies that showed increased, decreased, or unchanged crash outcomes associated with handheld cellphone or texting bans. The current review indicates that recent research has more consistently found an association between statewide bans and reductions in fatal crash outcomes, but methodological concerns such as less-than-adequate comparison groups make it impossible to infer a cause-and-effect relationship.

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