The association between strengthened cellphone laws and police-reported rear-end crash rates

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Ian J. Reagan
Jessica B. Cicchino
Eric R. Teoh
Aimee E. Cox

Insurance Institute for Highway Safety
4121 Wilson Boulevard, 6th floor
Arlington, VA 22203
researchpapers@iihs.org
+1 703 247 1500

iihs.org

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Abstract

Introduction: Prior evaluations of the relationship between cellphone bans and crash outcomes show unclear effects. Advances in smartphone functionality have coincided with legislation to comprehensively define and prohibit cellphone use while driving. California, Oregon, and Washington enacted legislation (effective in 2017) to update earlier bans specific to handheld phone calls and texting. The current study evaluated the relationship between strengthening cellphone laws and the rates of rear-end crashes, a crash type sensitive to visual-manual cellphone use, in California, Oregon, and Washington.

Method: Negative binomial regression compared the change in monthly per capita rear-end crash rates in California, Oregon, and Washington before and after the law changes relative to the rates in two control states, Colorado and Idaho, during 2015–2019. Analyses examined (a) rear-end crashes with injuries in all three study states, including minor to fatal injuries; and (b) rear-end crashes of all severities in California and Washington, including property-damage-only crashes and crashes with injuries; Oregon was excluded from this analysis because of a 2018 change to its reporting criteria for property-damage-only crashes.

Results: Washington's strengthened law was associated with a significant 7.6% reduction in the rate of monthly rear-end crashes of all severities relative to the rates in the control states. The law changes in Oregon and Washington were associated with significant reductions of 8.8% and 10.9%, respectively, in the rates of monthly rear-end crashes with injury relative to the rates in the control states. In contrast, California did not experience changes in rear-end crash rates of all severities or with injuries associated with the strengthened law.

Conclusion: The results of this study are mixed, with law changes associated with significant reductions in rear-end crash rates in two of the three study states. Differences in the wording of the laws, levels of enforcement, and sanction severity may help explain the divergent results.

Practical application: The crash reductions in Oregon and Washington suggest that enacting legislation that comprehensively bans practically all visual-manual cellphone activity may have made the laws easier to enforce and clarified to drivers that handheld cellphone use is unacceptable in these states.

Keywords: distraction driving, strengthened cellphone laws, visual distraction, rear-end crash rate

1. Introduction

Research in controlled laboratory or test track settings indicates that visual-manual cellphone use by drivers reduces reaction time, narrows visual scanning, and degrades lane-keeping abilities (Caird et al., 2014). Studies based on naturalistic observation have established reliable associations between visual-manual cellphone interaction and increased crash risk (Dingus et al., 2016; Guo et al., 2017; Kidd & McCartt, 2015; Klauer et al., 2006). Legislation banning cellphone behaviors began to be enacted in the United States around the turn of the century to reduce crashes associated with their use. Currently, 48 states and the District of Columbia ban all drivers from text messaging, and 24 states and the District of Columbia ban handheld phone conversations or prohibit all drivers from holding a phone (Insurance Institute for Highway Safety, 2022).

It is unclear whether these laws initially achieved their intended effects. Roadside observational research has shown reductions in the percentage of drivers seen holding their phone during post-ban relative to pre-ban time periods, which suggest the laws were associated with a reduction in the targeted behavior of handheld cellphone conversations (McCartt et al., 2010); other studies show that drivers in states with handheld bans are less likely to report or be observed conducting handheld phone calls than in states without them (Braitman & McCartt, 2010; Reagan & Cicchino, 2020; Rudisill & Zhu, 2017). However, evaluations that measured the relationships between cellphone bans and crash outcomes show mixed support. Two early studies that measured insurance claims in jurisdictions with bans on handheld phone calls or texting found either no differences or increases in claims after the laws were enacted relative to the comparison jurisdictions without bans (Highway Loss Data Institute, 2010; Trempel et al., 2011). In a review of research that analyzed relationships between cellphone bans and crashes, McCartt et al. (2014) reported inconsistent effects and methodological limitations across the studies.

More recent studies have identified significant fatality reductions associated with laws banning handheld phone calls or texting while driving (Flaherty et al., 2020; French & Gumus, 2018; Rocco & Sampaio, 2016; Rudisill et al., 2018), although a study by Tsai et al. (2015) reported cellphone bans were associated with a small decrease in the traffic fatality rate per vehicle miles traveled that was not statistically significant. These studies all varied in how they treated cellphone bans; for example, by dichotomizing states into those with a handheld or texting ban versus those without (e.g., Tsai et al.) or by coding states as having strong, moderate, or weak bans depending on whether they had handheld or texting bans and whether they were considered primary or secondary laws. The range of estimated crash reductions provided by these authors was quite large (i.e., a 3.5% to 18% reduction in the fatality metrics), and the studies were limited in their lack of adequate control groups. A separate study of the effect of text messaging bans that assessed motor-vehicle-related emergency department visits in 16 states found a significant reduction in visits in states with primary texting bans; however, all-driver bans on handheld cellphone conversations, which were included as a covariate, were linked to increased motor-vehicle-related visits to emergency departments (Ferdinand et al., 2019).

These recent studies suggest that the legislation's targeted behavior and enforceability affect the relationship with crash outcomes. As expected, primary enforcement laws, which allow officers to initiate a traffic stop and issue a citation when they witness a violation (e.g., texting), were associated with larger crash reductions than secondary enforcement laws, which require the driver to commit some other primary offense (e.g., speeding) before an officer can cite for the secondary offense (e.g., texting). Likewise, crash reductions reported for all-driver bans on handheld phone calls were generally larger than those for texting bans, although presently all states that ban handheld phone calls also ban texting.

The original laws banning cellphone use focused on handheld phone conversations or text messages (sending or reading texts) with a handheld phone. Smartphones introduced functionality (e.g., live streaming technology, social media and video-calling applications, connectivity to the Internet of Things) that the legislators who introduced the initial bans had never anticipated. This technology explosion combined with laws that only banned handheld cellphone calls or sending or reading text messages or emails introduced loopholes that made enforcing the cellphone laws challenging (for example, many states that banned texting permitted manually dialing a 10-digit phone number). As a result, states began to use more comprehensive wording in legislation designed to ban drivers from nearly all handheld cellphone behaviors. A study by Zhu et al. (2021) found states that implemented such comprehensive bans were associated with significant decreases in driver fatality rates but not in the fatality rates of nondrivers or total fatalities.

The current study assessed 2017 revisions to the existing cellphone laws of California, Oregon, and Washington. The revisions strengthened the wording of the laws in each. The similarities in timing of the states' initial and strengthened laws, the adopted wordings that describe the traffic offense, and the states' geographical proximity factored in their selection.

California introduced its first ban on all driver handheld phone conversations in July 2008, followed by a ban on sending or receiving text messages and email in January 2009. Notable exceptions to prohibited behaviors in California included reading, selecting, or entering a telephone number or name. California's initial laws permitted primary enforcement for adult drivers.

Oregon's initial law, effective January 2008, applied only to drivers under age 18, prohibited using mobile communication devices to transmit voice or text communication, and allowed only secondary enforcement of the law. In January 2010, Oregon extended the prohibition to all drivers and allowed primary enforcement, defined a hands-free accessory as one that permitted the driver to keep both hands on the steering wheel, and listed as an exception using the device with a hands-free accessory if the driver was at least 18 years old.

Washington prohibited drivers from text messaging in January 2008, followed by prohibiting drivers from holding a wireless communication device to their ear in July 2008. Both laws allowed only secondary enforcement, and Washington also specifically excluded reading, selecting, or entering a phone number or name for the purposes of making a call. Washington changed its law in 2010 to allow primary enforcement.

The strengthened laws and their effective dates are summarized in Table 1. It is noteworthy that the 2017 changes implemented by each study state included more general cellphone terminology and/or more broadly defined the traffic offense rather than trying to enumerate an exhaustive list of all prohibited cellphone behaviors. For example, each state added language that ensured the only acceptable cellphone interaction was via hands-free systems that required minimal manual input. The unambiguous wording used for the strengthened laws in California, Oregon, and Washington may be a stronger deterrent to driver cellphone distraction and was the motivation for the current study.

Table 1. Revised 2017 cellphone bans and effective dates in California, Oregon, and Washington

State	Effective date of strengthened law	Description of offense under strengthened law
California	January 1, 2017	Driving a vehicle while holding and operating a handheld wireless telephone or an electronic wireless communications device unless the wireless telephone or electronic wireless communications device is specifically designed and configured to allow voice-operated and hands-free operation, and it is used in that manner while driving.
Oregon	October 1, 2017	Driving while using a mobile electronic device if the driver holds a mobile electronic device in the driver's hand or uses a mobile electronic device for any purpose. "Driving" means to operate on a highway or premises open to the public and while temporarily stationary because of traffic, traffic control device, or other momentary delays. "Using" includes, but is not limited to, text messaging, voice communication, entertainment, navigation, accessing the Internet or producing electronic mail.
Washington	July 23, 2017	Driving a vehicle while using a personal electronic device. "Driving" means to operate a motor vehicle on a public highway, including while temporarily stationary because of traffic, a traffic control device, or other momentary delays. It does not include when the vehicle has pulled over to the side of, or off of, an active roadway and has stopped in a location where it can safely remain stationary. "Use" or "uses" means holding a personal electronic device in either or both hands; using hand or finger to compose, send, read, view, access, browse, transmit, save, or retrieve email, text messages, instant messages, photographs, or other electronic data; however, this does not preclude the minimal use of a finger to activate, deactivate, or initiate a function of the device; watching video on a personal electronic device.

The current analyses used police-reported rear-end crashes as the basis for our outcome measure, which was a departure from recent evaluations that analyzed measures based on fatal crashes. O'Neill and Kyrychenko (2006) demonstrated that variables unrelated to traffic safety such as urbanization and demographic factors explain the majority of the year-to-year variability in fatality rates. They suggested this sensitivity to macro-level factors may be related to the rare and unpredictable nature of fatal crashes and encouraged evaluations of traffic laws to use crash measures expected to be related to the behavior the law targeted. We also avoided focusing on crashes coded by police as involving driver distraction, as the data are noted to be unreliable due to significant underreporting (National Center for Statistics and Analysis, 2021). The decision to focus on rear-end crashes was largely influenced by a crash risk analysis from a large naturalistic driving study that suggested rear-end crashes are sensitive to cellphone use (Owens et al., 2018). They found visual-manual cellphone use was associated with a significant increase of 1.8 times the odds of being involved in any crash, but with a much larger increase of 7.8 times the odds of being the striking vehicle in a rear-end crash. Bálint et al. (2020) used data from the same study to demonstrate that engaging in multiple secondary tasks was associated with a larger increase in the odds of a rear-end crash relative to analyses that examined broader groupings of crashes and near crashes. For these reasons, we focused our analysis on rear-end crashes as a surrogate for distracted driving crashes.

2. Method

2.1 Research design

We used a pre-post study with control group design to test the effect of the law changes on rear-end crashes in the *study* states of California, Oregon, and Washington. The states of Colorado and Idaho served as the *control* states. Colorado and Idaho had texting bans in place beginning in 2009 and 2012, respectively, but did not prohibit other cellphone behaviors.

2.2 Data

The analyses are based on annual files of police-reported crashes maintained by California, Colorado, Idaho, Oregon, and Washington. The data range was the 5-year period spanning from January 1, 2015 to December 31, 2019. Rear-end crashes of all severities and with injuries were summed to create monthly counts for each state. We obtained monthly counts of vehicle miles traveled (VMT) (in billions) by state from the Federal Highway Administration, monthly unemployment rates by state from the Bureau of Labor Statistics, and annual state population counts from the U.S. Census Bureau.

2.3 Analysis

We used negative binomial regression models to analyze the association of the strengthened cellphone laws with monthly rear-end crash rates in each study state relative to the experiences of Colorado and Idaho, with one pair of models conducted per study state—one model for all police-reported crashes, and one for crashes resulting in injury/fatality. A log link function was used with each model with the log of state population as an offset term, so that the outcome measure was the rate of rear-end crashes per 100,000 population.

The primary indicator variable used to estimate the law effects denoted when the strengthened laws were in place. In the study states, this variable was equal to 0 during the months before the strengthened law in that state became effective (see Table 1) and 1 afterwards, and in control states it was always equal to 0. Additional independent variables included in unadjusted models accounted for time (i.e., months since the starting point) and state. Thus, the effect of the law change was determined by the change in rear-end crash rates in the months after each law change became effective, while including main effects for state and time that controlled for between-state differences and the monthly trend in rear-end crash rates from 2015–2019. Of these three terms, only the law indicator is interpreted in the results.

Adjusted models included these terms and additional covariates for VMT, unemployment rate, and the onset of recreational marijuana sales to control for other factors associated with crash rates. Research indicates that legalized recreational marijuana sales are associated with increased police-reported crash rates (Monfort, 2018). However, this covariate was only used in analyses for California and Oregon, as there were no changes in recreational marijuana sales during the study period in Washington.

In 2018, Oregon increased the threshold for estimated property damage that obligated law enforcement to complete a police crash report from $1,500 to $2,500. Given this change would likely lead to a decrease in property-damage-only crash reports, two sets of regression models measured the effects of law changes on rear-end crash rates: one that estimated change in rates of rear-end crashes of all severity levels, including property-damage-only, and a second that assessed the change in rates of rear-end crashes with injury (i.e., including minor to fatal injuries). The analyses based on rear-crash rates of all severities was limited to California and Washington due to Oregon's 2018 reporting threshold increase.

Maximum likelihood parameter estimates were considered significant when $p < 0.05$. Parameter estimates and 95% confidence limits were converted to rate ratios (RR) by calculating the inverse of the natural log of the estimate (i.e., RR = Exp[X]), where X equals the parameter estimate. The ratios were then converted by the formula ([RR − 1] × 100), so effects could be discussed in terms of the estimated percent change in per capita rear-end crash rates. Thus, for example, a parameter estimate of –0.039 for the state by law date interaction term would have translated to a rate ratio of 0.962 and equated to a 3.8% estimated reduction in the rear-end crash rate after the implementation of the strengthened law relative to the change in rear-end crash rates in the control states during the same period. For continuous covariates, such an estimate was interpreted as the effect of a one unit increase in the covariate, while holding all other variables constant.

3. Results

3.1 Police-reported rear-end crash rates of all severities

Figures 1a and 1b present the observed monthly rear-end crash rates per 100,000 population in California and Washington relative to Colorado and Idaho. During 2015 to 2019, Colorado's monthly rear-end crash rates were the highest and Idaho's were the lowest. Figure 1b suggests a decreasing trend in Washington's rear-end crash rates after the law went into effect in July 2017. In California (Figure 1a), there was no apparent change in rear-end crash rates after its law became effective in January 2017, nor was there clear change in the control states during the study period.

Figure 1a. Monthly rear-end crash rates per 100,000 population in California and control states, January 2015–December 2019

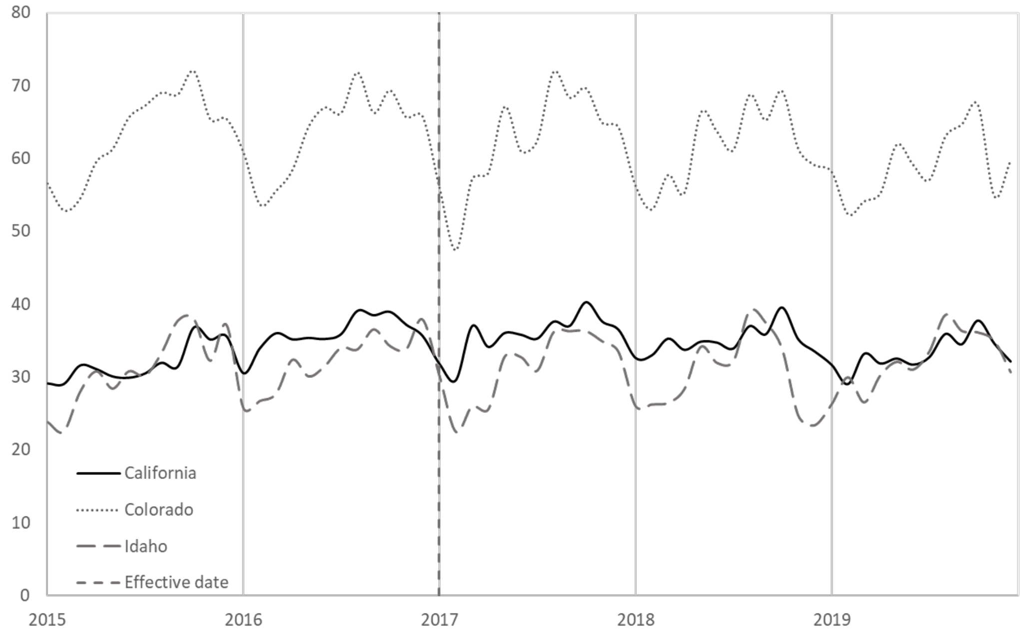
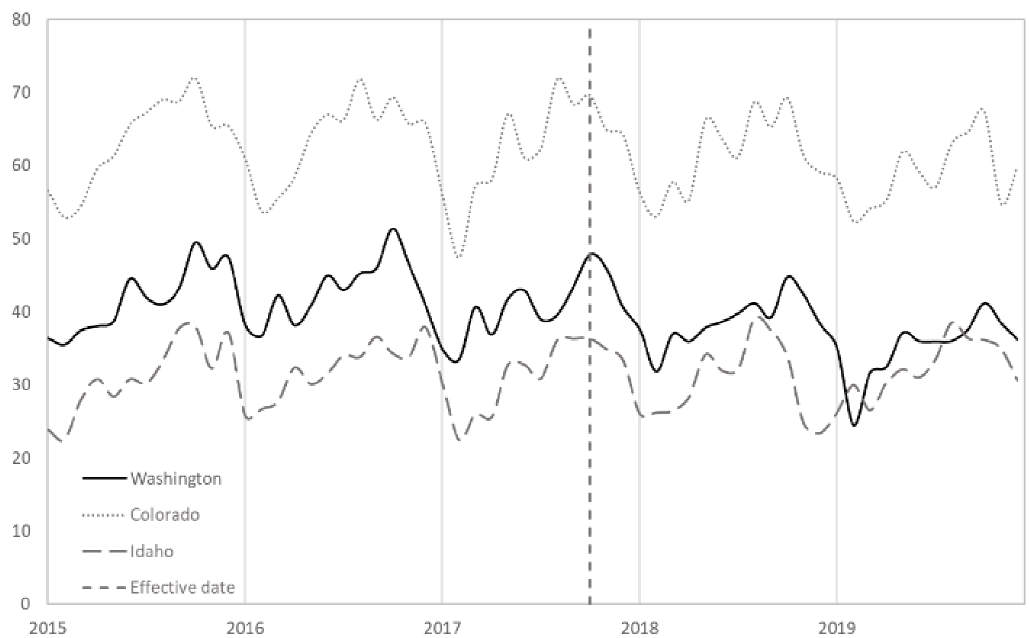


Figure 1b. Monthly rear-end crash rates per 100,000 population in Washington and control states, January 2015–December 2019



The unadjusted model results in Table 2 show that the introduction of California's strengthened cellphone ban was associated with a 1.0% increase in monthly rear-end crash rates relative to the change in the control states, but this increase was not statistically significant. In contrast, the analysis for Washington indicates a significant decrease in rear-end crash rates of 7.8% in the period after its strengthened law became effective relative to the change in in the control states.

After adjusting for VMT, the unemployment rate, and legalized marijuana, the results for California indicate that the period after the strengthened law became effective was associated with no change in monthly rear-end crash rates. The adjusted model for Washington estimated that the post-law period coincided with a significant 7.6% decrease in rear-end crash rates relative to the change in Colorado and Idaho.

Table 2. Negative binomial regression models of the effects of strengthened cellphone laws on monthly rear-end crash rates

Parameter	Estimate	Standard error	Effect (% change)	Lower CL	Upper CL	χ^2	*p* value
California unadjusted model							
Strengthened law	0.010	0.031	1.0	−5.00	7.4	0.10	0.75
Colorado (vs. CA)	0.597	0.027	81.7	72.5	91.4	507.84	<.0001
Idaho (vs. CA)	−0.081	0.027	−7.8	−12.6	−2.8	8.96	0.003
Time	0.000	0.001	0.0	−0.1	0.1	0.46	0.50
California adjusted model							
Strengthened law	0.000	0.041	0.0	−7.7	8.2	0.00	0.99
Colorado (vs. CA)	0.830	0.145	129.4	72.6	204.9	32.70	<.0001
Idaho (vs. CA)	0.138	0.154	14.8	−15.1	55.1	0.80	0.37
Time	−0.001	0.001	−0.1	−0.3	0.0	2.18	0.14
Unemployment rate	−0.074	0.032	−7.2	−12.9	−1.1	5.31	0.02
VMT	0.014	0.005	1.4	0.5	2.3	9.54	0.002
Recreational marijuana	−0.072	0.036	−6.9	−13.3	−0.1	3.97	0.046
Washington unadjusted model							
Strengthened law	−0.082	0.034	−7.8	−13.9	−1.4	5.62	0.02
Colorado (vs. WA)	0.404	0.027	49.8	42.2	57.9	228.78	<.0001
Idaho (vs. WA)	−0.274	0.027	−23.9	−27.9	−19.7	100.17	<.0001
Time	0.000	0.001	0.0	−0.1	0.1	0.02	0.90
Washington adjusted model							
Strengthened law	−0.079	0.033	−7.6	−13.5	−1.38	5.64	0.02
Colorado (vs. WA)	0.497	0.078	64.4	41.0	91.6	40.38	<.0001
Idaho (vs. WA)	0.100	0.113	10.5	−11.4	37.8	0.78	0.38
Time	0.000	0.001	0.0	−0.2	0.2	0.03	0.87
Unemployment rate	0.014	0.039	1.4	−6.1	9.4	0.12	0.73
VMT	0.096	0.023	10.0	5.1	15.1	16.94	<.0001

Note. CL = confidence limit. VMT = vehicle miles traveled.

With regard to the covariates, the increases in rear-end crash rates associated with VMT were expected based on historical patterns. In California, the effect of monthly unemployment rate showed the expected increase in crash rates with decreased unemployment , but there was no significant relationship in Washington . The onset of legal recreational marijuana sales in California was associated with a significant decrease in monthly rear-end crash rates.

3.2 Rates of rear-end crashes with injuries

Figures 2a–c display the observed monthly rates of rear-end crashes with injury during the study period. On average, rear-end injury crash rates were highest in Oregon. Figure 2a suggests California's monthly rates of rear-end crashes with injury were generally flat from 2015–2019, whereas the trend lines for Oregon (Figure 2b) and Washington (2c) indicate decreasing trends in the monthly rates of rear-end crashes with injury, which is most apparent from 2017–2019. In Colorado and Idaho, the monthly rates of rear-end crashes with injury did not follow clear trends across the study period.

Figure 2a. Monthly rear-end injury crash rates per 100,000 population in California and control states, 2015–2019

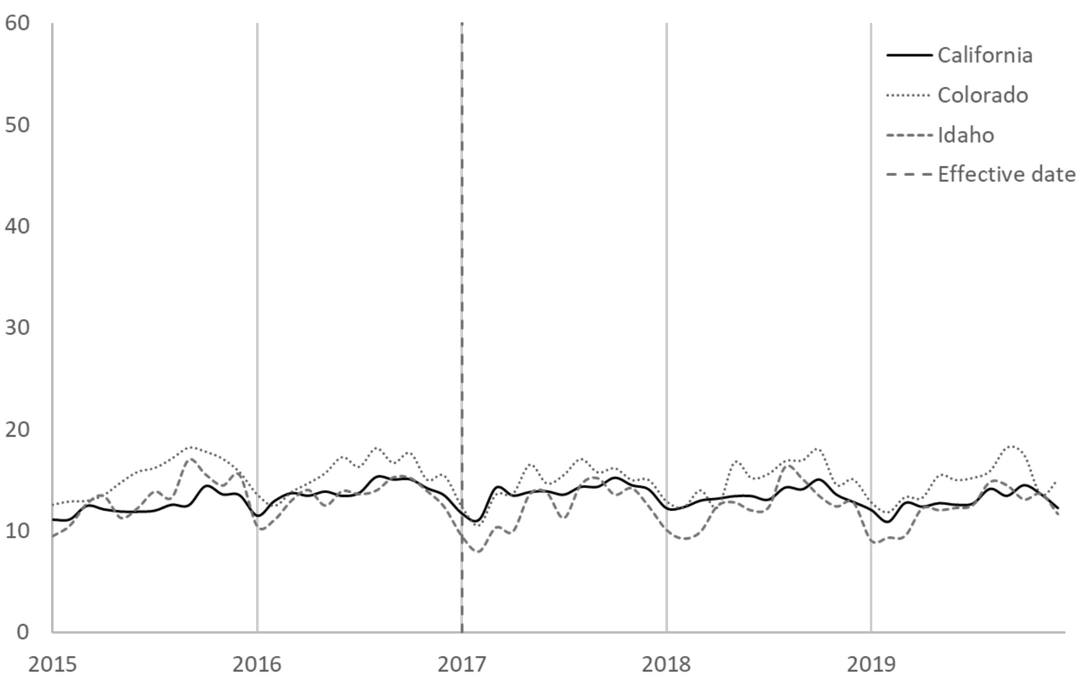


Figure 2b. Monthly rear-end injury crash rates per 100,000 population in Oregon and control states, 2015–2019

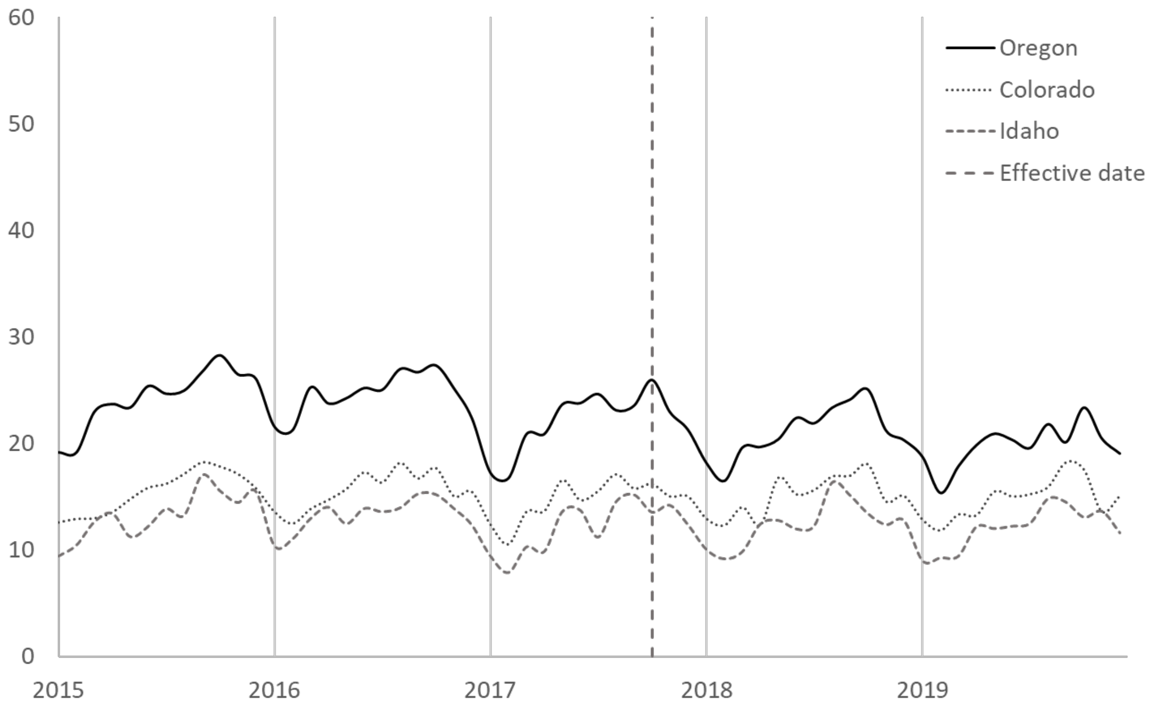
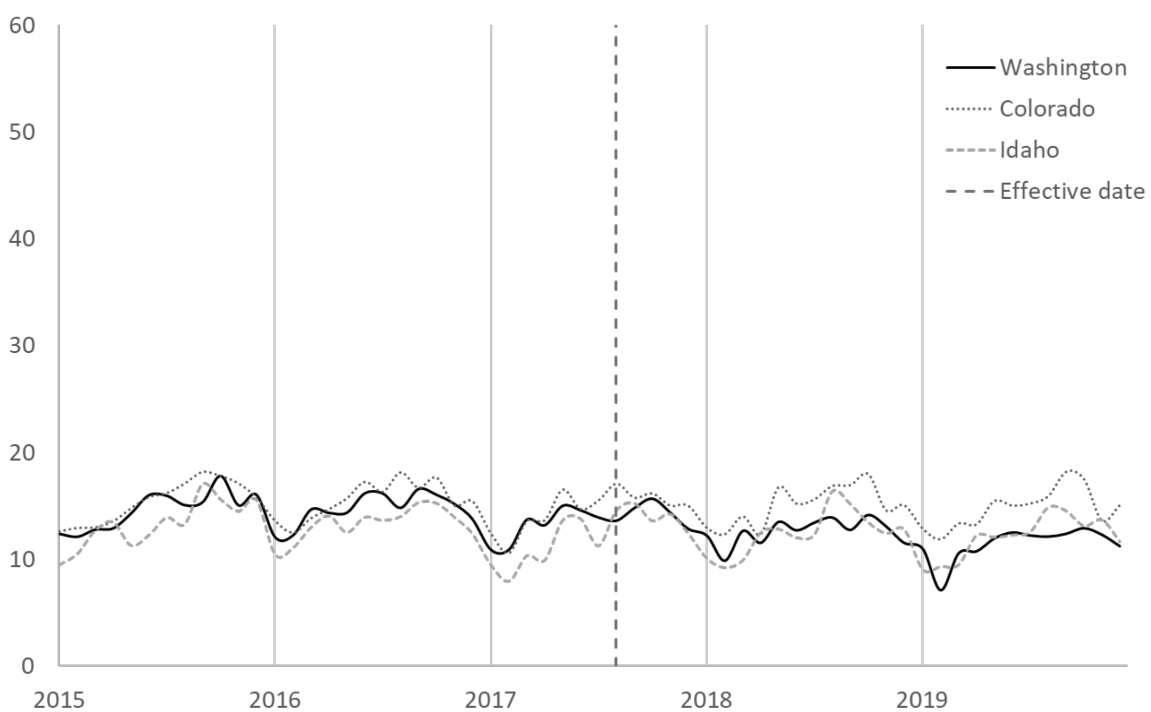


Figure 2c. Monthly rear-end injury crash rates per 100,000 population in Washington and control states, 2015–2019



The results of the unadjusted negative binomial regression models indicate that the strengthened bans in Oregon and Washington were associated with significant 10.8 and 11.7% decreases in their respective monthly rates of rear-end crashes with injury relative to the control states (Table 3). After adjusting for covariates, the estimated crash reductions were slightly smaller but still statistically significant in Oregon (8.8%) and Washington (10.9%). In contrast, the unadjusted and adjusted models for California indicated 1.5% and 1.7% nonsignificant increases in the monthly rate of rear-end crashes with injury compared with Colorado and Idaho.

Table 3. Negative binomial regression models of the effect of strengthened cellphone laws on monthly rates of rear-end crashes with injury

Parameter	Estimate	Standard error	Effect (% change)	Lower CL	Upper CL	χ^2	*p* value
California unadjusted model							
Strengthened law	0.015	0.04	1.5	−5.2	8.8	0.18	0.67
Colorado	0.142	0.03	15.2	8.6	22.2	22.35	<.0001
Idaho	−0.037	0.03	−3.6	−9.3	2.4	1.44	0.23
Time	0.000	0.00	0.0	−0.1	0.1	0.01	0.92
California adjusted model							
Strengthened law	0.017	0.05	1.7	−7.1	11.2	0.13	0.72
Colorado	0.515	0.16	67.3	21.2	130.9	9.79	0.002
Idaho	0.345	0.17	41.2	0.3	98.6	3.91	0.048
Time	−0.001	0.00	−0.1	−0.3	0.1	1.15	0.28
Unemployment rate	−0.044	0.04	−4.3	−11.0	3.0	1.37	0.24
VMT	0.017	0.01	1.7	0.7	2.7	11.72	0.0006
Recreational marijuana	−0.052	0.04	−5.1	−12.3	2.8	1.65	0.20
Oregon unadjusted model							
Strengthened law	−0.115	0.04	−10.8	−17.4	−3.7	8.54	0.003
Colorado	−0.440	0.03	−35.6	−39.2	−31.7	221.2	<.0001
Idaho	−0.618	0.03	−46.1	−49.2	−42.8	411.39	<.0001
Time	−0.001	0.00	0.0	−0.2	0.1	0.64	0.42
Oregon adjusted model							
Strengthened law	−0.092	0.04	−8.8	−14.9	−2.2	6.7	0.01
Colorado	−0.665	0.07	−48.6	−54.9	−41.3	97.61	<.0001
Idaho	−0.094	0.10	−9.0	−25.1	10.6	0.9	0.34
Time	−0.000	0.00	0.0	−0.2	0.2	0.03	0.86
Unemployment rate	0.060	0.04	6.2	−1.2	14.2	2.64	0.10
VMT	0.229	0.03	25.7	18.0	33.8	51.09	<.0001
Recreational marijuana	0.085	0.05	8.9	−1.7	20.5	2.68	0.10
Washington unadjusted model							
Strengthened law	−0.124	0.04	−11.7	−18.3	−4.5	9.66	0.002
Colorado	0.0601	0.03	6.3	0	13.0	3.83	0.05
Idaho	−0.118	0.03	−11.1	−16.5	−5.4	13.58	0.0002
Time	−0.001	0.00	−0.1	−0.2	0.1	1.06	0.30
Washington adjusted model							
Strengthened law	−0.115	0.04	−10.9	−17.0	−4.2	9.92	0.002
Colorado	0.273	0.09	31.4	11.0	55.6	10.09	0.001
Idaho	0.563	0.12	75.5	37.8	123.6	20.71	<.0001
Time	−0.000	0.00	0.0	−0.2	0.2	0.03	0.86
Unemployment rate	0.053	0.04	5.4	−3.1	14.7	1.51	0.22
VMT	0.162	0.03	17.5	11.9	23.5	40.75	<.0001

Note. CL = confidence limit. VMT = vehicle miles traveled.

Results for the covariates followed a pattern similar to the analysis of rear-end crashes of all severities. VMT was associated with expected significant increases in monthly rates of rear-end crashes with injury in each adjusted model. The estimates for unemployment and the onset of retail sales of recreational marijuana diverged across the models, but none of the effects were associated with significant changes in monthly rear-end injury crash rates.

4. Discussion

The current evaluation of strengthened cellphone bans in California, Oregon, and Washington found that the strategy of comprehensively banning visual-manual cellphone use was associated with significantly lower rates of rear-end crashes in two of the three study states relative to the control states. When accounting for covariates, the strengthened laws were associated with significant reductions in the rate of rear-end crashes with injury in Washington (10.9%) and Oregon (8.8%). Washington also experienced a 7.6% decrease in the rates of police-reported rear-end crashes of all severities relative to Colorado and Idaho after Washington's strengthened law became effective. However, the analyses for California indicated small changes to the rates of rear-end crashes of all severities and rear-end crashes with injury that were not statistically significant. The current results partially align with and build upon Zhu et al. (2021) who found that comprehensive bans were associated with significantly lower driver fatality rates and called for future research to examine the relationship between such bans and less severe crashes.

The relationships between comprehensive cellphone laws and reduced rear-end crash rates in Oregon and Washington may reflect a ripple effect associated with the wording of the law. As hypothesized by Zhu and colleagues (2021), the use of plain, direct language to ban all handheld cellphone use, including holding a phone, and to close loopholes used to successfully contest citations under the earlier laws may boost driver compliance through increased understanding or perceived likelihood of being cited, and it may improve law enforcement officers' willingness to cite due to the facility of identifying infractions or belief that a citation is less likely to be dismissed in court.

Despite similarities in how the current study states comprehensively banned driving while using a handheld cellphone, there are notable differences in the wording of the law that may explain why the strengthened bans were associated with lower crash rates for Oregon and Washington but not California. For example, drivers who are holding a phone but otherwise are not using it are violating the law in Oregon and Washington but are following California's law. Oregon and Washington define "driving" to include operating a vehicle on a highway even while the vehicle is temporarily stopped due to traffic, a traffic control device, or other delays. California's law does not explicitly state whether driving includes stopped conditions. The clarification that a temporary stop in traffic is considered driving removes ambiguity for those trying to follow or enforce the law. A report by the Oregon Department of Transportation (2017) noted that representatives from the state police indicated that a law that allowed for enforcement in stopped traffic would be easier to enforce than one that required officers to witness the violation while the vehicle is moving. Allowing police officers to cite drivers for the act of holding a phone may also be easier to enforce than requiring the officer to document visual-manual interaction. Such wording may also send a clearer message that drivers should never use a handheld cellphone when operating a vehicle.

Factors related to enforcement and the judicial system may have influenced results, as prior research has identified relationships between crash reductions and increased volume of enforcement of traffic laws to address impaired driving, seat belt use, and speeding (Fell et al. 2014; Rezapour et al., 2017). In Oregon, where the law became effective in October 2017, distracted driving convictions decreased by 15% from 2016–2017 and then increased year-to-year by 50% in 2018 and 27% in 2019 (Oregon Department of Transportation, 2021). Washington's numbers on citations followed a similar pattern to Oregon's convictions, with year-to-year decreases before the law took effect and year-to-year increases of 74% and 11% from 2017–2019 (Washington Traffic Safety Commission, 2022). In California, distracted driving convictions increased year-to-year by 19% in 2017 and by 20% in 2018 but then decreased by 0.3% from 2018–2019 (California Office of Traffic Safety, 2020). The associations between the reductions in rear-end injury crash rates and sustained year-to-year increases in enforcement in Oregon and Washington warrant further attention, given the association of these measures with crash outcomes in other domains. However, it is equally important to emphasize that the comparisons of these enforcement data are not like-for-like, and these data likely represent an incomplete picture of effects related to enforcement and the court system overall. For example, California and Oregon reported convictions, whereas Washington provided counts of citations, and only California had three full years of enforcement data available after the strengthened ban became effective.

The severity of the fine amounts and other sanctions associated with the different law changes may also have contributed to the current results. In California, where there were not significant reductions in rear-end crash rates associated with the strengthened law, base fines for first and second offenses ($20 and $50, respectively) are lower than the comparable fines in Washington ($88, $136 [1]) and Oregon ($265, $440). In Oregon, if the first offense leads to a crash, then a fine of $440 is incurred, and a third offense within 10 years is an automatic Class B misdemeanor, which results in a minimum fine of $2,000 and can lead to 6 months in jail. It is noteworthy that effective January 2020, after the conclusion of this analysis, California added one penalty point on driver license records for a second offense occurring on or after July 1, 2021, if it occurred within 3 years of the first (California Vehicle Code, 2020).

Research has identified relationships between increased fine amounts for traffic safety offenses and compliance. Nichols et al. (2010) reported that increases in fines for seat belt citations from $25 to $60 and from $25 to $100 were associated with significant 4 and 7 percentage point increases in observed seat belt use. With respect to enforcing handheld cellphone bans, Zhu et al. (2016) reported that fines of $100 or more were associated with reduced odds of observing drivers younger than age 25 using a handheld phone, but this finding was based on only two states with high fine amounts. Other work has shown more nuanced relationships between sanction severity and behavior change. For example, Moolenaar (2014) reported that increased speeding fine amounts corresponded to a decrease in the rate of speeding offenses detected by automated enforcement, but speeding offenses caught by police officers

[1] Washington fines for first and second offenses are $48 and $96. Both include an additional $40 in fees.

showed no relationship with the fine increases during the same time period. Sagberg and Ingebrigtsen (2018) found evidence for a deterrence effect associated with a penalty point system only when drivers had accrued a level of points where a subsequent offense would trigger a suspension. However, underprivileged individuals assessed large fines often face harsh and unintended consequences (license suspension, imprisonment, or job loss) if they are unable to pay (Colgan, 2017; Lawyers' Committee for Civil Rights of the San Francisco Bay Area, 2017; Watts et al., 2021). Given this concern, many argue that jurisdictions considering increased fine amounts should incorporate income-based sliding scales, payment plans, or other safeguards to promote equitable penalties for traffic law infractions across the driving population.

A limitation of this study is that the study states had laws that banned text messaging and handheld cellphone conversation prior to the strengthened laws that were enacted in each state in 2017. Although the initial laws in the three states were similarly worded, the effects of the strengthened law could have been obscured in California if the state experienced crash reductions from its earlier bans on cellphone behaviors that were different from the initial experiences in Washington and Oregon. Indeed, Liu et al. (2019) found that California's earlier legislative efforts were related to lower crash frequency, but the authors used police-reported cellphone crashes as the outcome of interest, so their findings should be interpreted with caution given the data quality concerns about "cellphone-related crashes."

In conclusion, the current findings provide partial support for the use of language to comprehensively ban drivers' visual-manual interaction with handheld cellphones as a countermeasure that can reduce crashes. Although the reductions in the rates of rear-end crashes with injury in Oregon and Washington are promising, more research is needed to determine whether these crash rate reductions remain in the years ahead. Work is also needed to understand whether between-state differences in the wording of laws, sanction severity, or the level of enforcement might affect crash rates. The recent addition of penalty points to driver records for violating California's cellphone ban provides another inflection point for such follow-on work. Similarly, nine states have enacted bans on handheld cellphone use since 2018, some which were comprehensive, and analysis of the rear-end crash experience in these jurisdictions may also be insightful.

5. References

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