The Effect of Cell Phone Bans on Driver Behavior, Accidents, and Casualties

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November 6, 2012

Abstract

In response to concerns that distracted driving due to cell phone use has become a threat to roadway safety, many states have passed cell phone bans that prohibit drivers from texting and talking on handheld cell phones. This paper empirically examines the impact of these bans on driver cell phone usage, traffic accidents, and casualties. To distinguish the effect of the bans from confounding factors, I use a difference-in-differences approach that exploits the within-state variation in the adoption of cell phone bans. Results indicate that prohibiting drivers from texting and talking on handheld cell phones reduces drivers' handheld device manipulation and cell phone use by 60 and 40 percent, respectively. This effect is largely driven by adult drivers and drivers with passengers. However, cell phone bans do not appear to have any meaningful impact on accidents and casualties. Several robustness and falsification exercises, including permutation tests using placebo ban estimates, are used to test the validity of both the estimates and their statistical significance.

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1. Introduction

In 2009, an estimated 995 people lost their lives in motor vehicle crashes in the United States due to the use of cell phones while driving (National Highway Traffic Safety Administration, 2009). Despite the fact that an increasing body of research indicates that texting and talking on cell phones while driving is risky, such behavior is still prevalent among drivers.[1] According to the 2011 National Phone Survey on Distracted Driving Attitudes and Behaviors (Tison, Chaudhary and Cosgrove, 2011), over 60% drivers regularly send text messages while driving. This survey also finds that 66% respondents indicate that their most common action when receiving calls while driving is to answer and keep driving.

In response to concerns over distracted driving due to cell phone use, states have started to pass cell phone bans to prohibit drivers from using cell phones behind the wheel in order to reduce motor vehicle accidents that cause injury, death, and property damage. This paper focuses on whether the two most important cell phone bans that apply to all drivers—texting bans and handheld bans —are effective in reducing drivers' cell phone usage and subsequently reduce traffic accidents and casualties. Texting bans prohibit drivers from sending or reading text messages on cell phones; handheld bans prohibit all drivers from engaging in phone calls, either talking or listening, on handheld cell phones when operating motor vehicles. These bans impose significant penalties for violations, including fines (ranging from $20 to $500 in adopting states), license suspension, and even jail time. By raising the expected cost of using cell phones while driving, one might expect the bans to reduce drivers' cell phone use. However, it is also possible that the bans could have the opposite effect of increasing the concealed use of cell phones when drivers attempt to avoid being ticketed. As a result, whether cell

[1] Studies find that drivers talking or texting on handheld devices are more likely to get involved into crashes (e.g., Housking, Young, and Regan, 2009; Olson, Hanowski, Hickman and Bocanegra, 2009) and experiments were also conducted to reveal the mechanism(e.g., Just, Keller and Cynkar, 2008; Olson, Hanowski, Hickman, and Bocanegra, 2009)

phone bans ultimately reduce traffic accidents is ambiguous. The purpose of this paper is to empirically evaluate whether the bans reduce visible driver cell phone usage and estimate the net effect on traffic accidents and casualties.

To address these questions, this paper applies a difference-in-differences (DD) strategy to two panel datasets: individual-level observational survey data on drivers' visible cell phone usage, and state-level data on traffic accidents and casualties from 2004-2010. By doing so, I exploit the within-state variation in the adoption of texting and handheld bans among the 23 adopting states. Intuitively, I compare the *relative* changes in outcome measures, including drivers' visible cell phone usage (handheld device manipulation and handheld cell phone usage), traffic accidents, and traffic casualties, between states that passed the cell phone bans (treatment states) and states that did not (control states), from before and after the adoption of cell phone bans. The identifying assumption is that states where drivers are prohibited from using cell phones would have followed similar trajectories in cell phone usage, accidents, and casualties to other non-adopting states, in the absence of the adoption of cell phone bans. To assess the validity of this assumption, I show that DD estimates are robust to the inclusion of time-varying covariates that are used in previous studies, such as unemployment rate, median income, violent and property crime rates, and demographics (Abouk and Adams, forthcoming; Cohen and Einav, 2003; Carpenter and Stehr, 2011). In particular, I also control for the number of full-time equivalent police and government spending on both highway and public welfare to capture the enforcement of cell phone bans and other policies that could simultaneously affect highway safety. In addition, graphical and regression analyses suggest that there is no evidence that the three outcomes in treatment states diverged before cell phone bans were adopted. Similarly, permutation tests show that there is no evidence of systematic differences in the trends of outcomes in adopting and non-adopting states in the 15-year period prior to adoption. Moreover, there is no

evidence that non-cell phone-related safety behaviors—proxied by seat belt usage—differed between adopting and non-adopting states over the examination period. Finally, results are robust to various robustness checks such as allowing for region-year or region-year-quarter fixed effects to account for common regional shocks, excluding states where cell phone bans are upgraded from secondary to primary enforcement, and using alternative definition of cell phone bans.

In examining the effects of cell phone bans, this paper is closely related to two recent studies in economics that look at texting bans and handheld bans, respectively. Abouk and Adams (forthcoming) focus on the effect of texting bans on single-vehicle-single-occupant accidents. They use monthly data from 2007 to 2010 and find that accidents only reduce within a few months after the adoption of texting bans and then return to former levels. Bhargava and Pathania (forthcoming) examine handheld bans instead. By analyzing data from 1989 to 2007, they show that banning drivers from talking on cell phones while driving has no effect on fatal crashes.[2] This paper also joins a literature of single-state studies that evaluate the effectiveness of cell phone bans by using neighboring states as control groups. The majority of these studies focus on early adopters of handheld bans such as Connecticut (Cosgrove, Chaudhary and Roberts, 2010), New York (McCartt, Braver, and Geary, 2003; McCartt and Geary, 2004), and Washington DC (McCartt, Hellinga, and Geary, 2006; McCartt and Hellinga, 2007) and find that handheld bans do induce drivers to use cell phones less often. Two recent reports from the Highway Loss Data Institute (HLDI) find that generally neither texting bans nor handheld bans are effective in reducing collision claim frequencies.[3] More broadly, this paper joins a larger literature of evaluating of mandatory state laws on roadway safety (e.g., Cohen and Einav,

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[2] In their main analysis, Bhargava and Pathania (forthcoming) cleverly exploit the discontinuity in cellular plans that transition from "peak" to "off-peak" pricing at 9pm on weekdays from 2002 to 2005 in California. They find that although the call likelihood increases by 7.2% during 9-10pm from Mondays to Thursdays, this sharp local rise in call likelihood does not lead to more crashes.

[3] In particular, this 2009 report finds that handheld bans increase collision claim frequencies in Connecticut and New York but not in California and DC. The 2010 report finds that texting bans increase collision claim frequencies in California, Louisiana, and Minnesota, but not in Washington.

2003; Carpenter and Stehr, 2011).

The primary contribution of this study is to combine first-stage evidence of the average effect of state cell phone bans on drivers' cell phone usage along with a thorough analysis of how the bans affect accidents and casualties. Results provide strong evidence that drivers reduce the visible cell phone use when cell phone bans increase the expected cost of doing so. Specifically, cell phone bans significantly lower a driver's probability of talking and texting while driving by 40 and 60 percent, respectively. Furthermore, I find that the two groups of drivers—drivers with passengers and adult drivers—respond the most to cell phone bans by reducing their cell phone use. However, cell phone bans do not appear to have any meaningful effect on accidents and casualties. One potential explanation for this seemingly contradictory finding is that the bans may cause more hidden cell phone use, which is likely more dangerous and could lead to an increase in accidents and casualties that offsets any reductions elsewhere. It is also possible that the use of cell phones does not necessarily result in more accidents and casualties because drivers compensate for this distracted driving behavior by simply driving more carefully.

2. Identification Strategy

I apply the difference-in-differences (DD) strategy to estimate the effect of state cell phone bans during 2004-2010 from two data sources by examining if states with cell phone bans have drivers use cell phones less frequently which ultimately results in fewer accidents and casualties.

The annual state-level panel data model based on the individual observational survey is:

(1) $$Outcome_{isy} = \beta_0 + \beta_1 Texting\ Ban_{sy} + \beta_2 Handheld\ Ban_{sy} + X_{isy}\gamma + c_s + u_y + \varepsilon_{isy},$$

where i indexes individuals, s indexes states, and y indexes years. The dependent variable $Outcome_{isy}$ is the dummy variable which equals one if the individual driver is observed using or manipulating a

cell phone and otherwise equals 0. $Texting\ Ban_{sy}$ is an indicator variable that equals one if the texting ban is effective in state *s* in year *y*.[4] $Handheld\ Ban_{sy}$ is similarly defined. Individual-level control variables in X_{isy} include observed race and median age, as well as a set of indicators that correspond to weekday non-rush hour, weekend, rural and urban areas, and weather condition. c_s and u_y are state- and year- fixed effects. ε_{isy} is the idiosyncratic term. The parameters of interest are β_1 and β_2, which measure the average effect of texting and handheld bans, respectively. Due to potential error correlations within states, standard errors are clustered at the state level (Bertrand, Duflo and Mullainathan, 2004).

The quarterly state-level panel data model of accidents and casualties is:

(2) $$Outcome_{syq} = \beta_0 + \beta_1 Texting\ Ban_{syq} + \beta_2 Handheld\ Ban_{syq} + X_{syq}\gamma + \Pi_{sy}\lambda + c_s + u_{yq} + \varepsilon_{syq},$$

where *q* indexes quarters. $Outcome_{syq}$ is the natural log of either accident/casualty count or accident/casualty rate (count per 100 million vehicles miles travelled (VMT)).[5] Formally, accident is defined as the number of vehicles involved in collisions, and casualty is defined as the sum of incapacitating injuries and fatalities in accidents. $Texting\ Ban_{syq}$ is an indicator variable that equals one if the texting ban is effective in state *s* in quarter *q* of year *y*. $Handheld\ Ban_{syq}$ is similarly defined. X_{syq} is a vector of quarterly time-varying covariates, including unemployment rate and median income. Π_{sy} is a vector of annual time-varying covariates, containing demographics, violent and property crime rates, government spending on highway and public welfare, police and population. u_{yq} is year- fixed effects. ε_{syq} is the idiosyncratic term. Other than clustering standard errors at the state level, I will perform different kinds of permutation tests to investigate the validity of the magnitudes and standard

[4] Since the survey is conducted in June each year, in the year when the texting ban is adopted, $Texting\ Ban_{sy}$ equals one if the texting ban is adopted after June and equals zero otherwise.

[5] The primary reason for estimating both count and rate models is to demonstrate that estimates from both models are almost identical. This is important, as only the data for the count model are available for the purpose of the permutation tests performed in Section 4.4. .

errors of the DD estimates.[6] Specifically, I will estimate the placebo effects of placebo cell phone bans, which are randomly assigned to states before 2001 in a way that is similar to how the actual cell phone bans were adopted during 2004-2010, and then ask how often I can obtain the same levels of magnitudes and statistical significance of the estimated effects of the real cell phone bans (Abadie, Diamond, and Hainmueller, 2010; Bertrand, Duflo, and Mullainathan, 2004; Chetty, Looney, and Kroft, 2009). In addition, I also assign the placebo cell phone bans to states that have passed cell phones to investigate if the estimated effects represent a structural break in the placebo effects (Nunn and Qian, 2011).

The crucial identifying assumption for the DD strategy used here is that states that adopted cell phone bans and other non-adopting states would have *trended* similarly in outcomes, including cell phone usage, accidents, and casualties, in the absence of the adoption of cell phone bans. Therefore, if this assumption holds, concerns such like that states that have higher accident rates are more likely to pass cell phone bans pose no threat to the identification.[7] To provide evidence of the validity of this identifying assumption, I will perform several tests. The first is to compare estimates of *Texting Ban* and *Handheld Ban* with and without controlling for time-varying covariates. If these two sets of estimates differ significantly, it indicates the time-varying determinants of outcomes varied systematically over time across treatment and control groups, which casts doubts on the identifying assumption. More specifically, this would cause me to worry that unobserved time-varying determinants, variables that I cannot directly control for, could vary in a similar way and potentially bias the DD estimates. Second, I will examine if there is any graphical evidence of divergence in outcomes before the adoption of cell phone bans, as well as formally test for divergence in the

[6] Such tests are also called "refutability" tests in Angrist and Krueger (1999).
[7] In addition, the program evaluation literature (e.g., Friedberg, 1998) has argued that even though *whether* a state adopted a law is correlated with initial levels of outcome measures that the law aims at, *when* such law is adopted is not.

regression analysis by including leading indicators of both cell phone ban indicators. Third, I will use permutation tests to investigate if similar divergence in outcomes already existed before cell phone bans were adopted.

In addition, given that states that have adopted handheld bans are only in the Northeastern and Western Census regions as shown in Figure 1, one might worry that the estimates may simply pick up regional common shocks. To account for this concern, I will include region-year or region-year-quarter fixed effects and therefore identify effects by comparing changes in outcomes between states that adopted cell phone bans and other non-adopting states within the same Census region of the country.

Finally, I will perform two more tests to provide more evidence that the estimated effects of cell phone bans are not confounded with other factors. First, I will show that VMT is not affected by cell phone bans, indicating that cell phone bans indeed do not affect the overall level of driving. This suggests that the reduction in accidents and casualties is not due to a reduction in traffic congestion. Second, I will show that seat belt usage is not affected by cell phone bans either, which provides some evidence that the effect of cell phone bans on accidents and casualties will not be due to differential trends in other safe driving habits.

3. Data

3.1 Texting and Handheld Bans

In this paper, cell phone bans refer to texting and handheld bans that apply to all drivers in all locations.[8,9] Also, I focus on texting and handheld bans with primary enforcement and do not

[8] Due to their limited coverage, other cell phone bans that are only applicable to specific drivers and locations are not considered to be handheld or texting bans discussed in this paper. Some state bans only target inexperienced drivers who are either below the statutory age (usually 18 years old) or provisional license holders. For example, Missouri has a texting ban that only applies to drivers who are 21 years old and younger; Kansas's handheld ban is only applicable to learner's permit and intermediate license holders. In some other

consider cell phone bans with secondary enforcement in the main analysis.[10] Primary enforcement in the context of cell phone bans means that law enforcement officials can stop drivers who are observed violating cell phone bans by either making phone calls or texting while driving, without having other primary reasons such as running a red light or speeding. In contrast, under secondary enforcement drivers can only be pulled over for a primary offense first.[11] Therefore, *ex ante* it makes sense to focus on the primary cell phone bans that substantially increase the cost of using cell phones while driving due to the strong enforcement. In fact, most adopting states adopted primary cell phone bans. Up to 2010, only Nebraska, New York and Virginia adopted secondary texting bans; Maryland is the only state that has secondary handheld ban. During the examination period, two states also updated the enforcement level of their cell phone bans from secondary to primary: New Jersey upgraded its secondary handheld ban in 2008 and Washington upgraded its secondary texting ban in 2010.

To determine if and when a state passed either a texting ban or a handheld ban between 2004 and 2010, I found the text of the actual laws or bills and then cross-checked with other sources such as Abouk and Adams (forthcoming), Bhargava and Pathania (forthcoming), National Conference of State Legislatures (NCSL), the Insurance Institute for Highway Safety (IIHS), and Ibrahim, Anderson, Burris and Wagenaar (2011).[12] Table 1 lists the 23 states that have passed primary cell phone bans as

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states the bans are only effective in specific areas. Illinois, for instance, restricts all drivers from talking on handheld devices in construction and school speed zones. Also, several states prohibit bus drivers from texting when a passenger of 17 years old or younger is present, such as Texas.

[9] Both bans do allow for the possibility of using cell phones for interactive communications in emergencies. Also, persons who perform their official duties such as certified law enforcement officers, firefighters, ambulance drivers are also exempt from the requirement of the cell phone bans.

[10] I also look at the effect when including secondary cell phone bans and results are not statistically different from those reported in Section 4.

[11] For example, in a state where cell phone bans have secondary enforcement and seat belt law has primary enforcement, a driver could be pulled over for not wearing seat belt and then get citations for not wearing seat belt and using cell phones. But the driver cannot be stopped for just using cell phones without any other primary enforcement offense.

[12] I thank Abouk and Adams (2012) for helpful conversations that correct the coding of cell phone bans in some states in the earlier version of this paper.

of 2010 along with the corresponding effective dates.[13] New York was the first state to pass a handheld ban in 2001 and New Jersey became the first to pass a texting ban in 2008.[14] For the five states that passed both cell phone bans, all of them adopted handheld bans no later than they adopted texting bans. Therefore, it is difficult to distinguish between the effect of handheld bans on their own and the effect of handheld bans when texting bans are already in effect, which is important when interpreting the estimated effect of handheld bans.

Figure 1 shows the geographic distribution of texting bans and handheld bans as of 2010: the 22 states with texting bans appear to be equally distributed across the U.S, and the 6 states with handheld bans are only located in the northeastern and western regions.

3.2 Outcome Measures

Empirical analysis in this paper relies on the outcome measures of state cell phone bans. To construct these variables, I use data from two distinct datasets.

The first set of outcome measures is the observed cell phone usage by individual drivers, which essentially provides the first-stage evidence on the mechanism through which cell phone bans may affect accidents and casualties. The observed handheld cell phone usage contains two main categories. The first category is "handheld cell phone usage", which refers to the situation in which the driver "[hold] cell phones to their ears while driving". The second is "visible handheld device manipulation". In this scenario, drivers are observed visibly text-messaging or manipulating other hand-held device. The raw data come from the 2004-2010 National Occupant Protection Use Survey

[13] The effective dates are the dates when the full enforcement begins rather than the initial announcement dates. The reason to choose the effective dates based on whether the enforcement starts is consistent with the purpose of this paper: to evaluate if drivers respond to the incentive change in using cellphones while driving, which only makes sense when there is full enforcement of cell phone bans. For example, Georgia's texting ban was supposed to become effective on July 1, 2010, but due to the ticketing issue the full enforcement began one month later.

[14] District of Columbia passed its both texting ban and handheld ban on July 1, 2004. But due to missing data, it is excluded from this analysis. Also, Washington was the first state to pass a texting ban with secondary enforcement on January 1, 2008.

(NOPUS) and are kindly provided by the National Highway Traffic Safety Administration (NHTSA) for this project. Unlike usual surveys that are done through interviews or questionnaires, NOPUS is based on the observations of sophisticated data collectors and is conducted at stop signs or stoplights between 7 a.m. and 6 p.m. at different state observational sites each June. Observers collect information on multiple characteristics of drivers and observational sites, such as age range and weather condition.[15] The survey covers around 30 states each year, though the composition of states covered varies slightly over time.[16] This dataset also includes data on individual use of headsets and seat belts, which can be used as further tests.

The second set of outcomes is the number of roadway accidents and casualties. They are aggregated to the state and quarterly levels using data from the Fatality Analysis Reporting System (FARS). FARS is a nationwide census provided by NHTSA, containing accidents and casualties information at the individual, vehicle, and the crash levels. Formally, accidents are measured as the number of motor vehicles involved in crashes; casualties are defined to be the sum of corresponding "incapacitating injuries" and "fatalities".[17] When I estimate rate models based on Model (2), the rate of accident and casualty is normalized using vehicle miles travelled (VMT). VMT data are from the Federal Highway Administration (FHA).

There are two advantages of the FARS data. The first is that because the accidents and casualties data are available at the quarterly level, I can more accurately identify the pre- and post- treatment periods. Second, accidents and casualties data are available back to 1975, which allows me to further evaluate the validity of the magnitudes and statistical significance of the estimated effect through permutation tests.

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[15] Only stopped vehicles are observed to allow time for observers to collect the wide range of information required by the survey.
[16] On average, 80% of the treatment states with handheld bans and 60% of the treatment states with texting bans are included each year.
[17] I exclude other minor or unknown injuries such as "possible injuries" and "non-incapacitating evident injuries".

3.3. Time-varying Control Variables

I have also obtained data on determinants of the outcome measures to serve as controls. To estimate the effect of cell phone bans on cell phone usage, I utilize the NOPUS data which contain characteristics of individual drivers (median age groups, race, and gender) and observational sites (whether the observation time is weekend or weekday rush hour, whether the site is urban or rural, and whether the weather is clear or not). To gauge the effect of cell phone bans on accidents and casualties, I have also collected quarterly and annual data on a set of control variables. Quarterly variables include unemployment rate and median income, with data collected from Bureau of Labor Statistics and the Census Bureau, respectively. Annual population and demographic (proportion of male in the 15-24 and 25-44 age groups) data are also obtained from the Census Bureau; annual violent crime and property crime data are from Federal Bureau of Investigation's Uniform Crime Reports (UCR). Importantly, I also include the number of full-time equivalent police from UCR to capture the effect of the enforcement of cell phone bans. In addition, I obtain the government spending data (US Census Bureau) on highway and public welfare to distinguish the effect of cell phone bans from the effect of other policies that could be implemented simultaneously when cell phone bans are adopted for the purpose of increasing roadway safety.

3.4 Sample Period

The sample period used in this paper is the 2004-2010 period. I choose the year 2004 as the starting point because the quarterly state-level VMT data are only available starting from 2004, which are used in Model (2) to normalize the count of accidents and casualties.[18] This examination period covers all states that have passed texting bans and only leaves out New York's handheld ban, which

[18] The 2004 data on "visible handheld device manipulation" are not available.

was passed in 2001.[19]

4. Results

4.1 Do Cell phone Bans Reduce Drivers' Cell Phone Usage?

In this section, I examine whether cell phone bans make drivers use cell phones less often. Regression results using the NOPUS data from Model (1) are reported in Table 3. Panel A presents the effects of cell phone bans on the behavior of handheld cell phone usage while driving. In Column 1, in which state and year fixed effects are controlled for, estimates of *Texting Ban* are insignificant and close to 0, indicating that texting bans have no effect. In contrast, handheld bans reduce the drivers' probability of talking on handheld cell phones by 2.8 percentage points, which is significant at the 1% level. This represents a 40% drop from the rate of 7 percent in states with handheld bans the year before these bans are adopted. Column 2 is the preferred specification, in which I additionally control for time-varying covariates, including driver characteristics (e.g., gender, age, and race), observational site features (e.g., rural or urban areas) and weather condition. Estimates of *Texting Ban* and *Handheld Ban* remain almost unaffected after controlling for time-varying controls compared to estimates in Column 1. This indicates that the within-state variation in cell phone bans is orthogonal to known determinants of handheld device manipulation, which is consistent with the idea that the within-state variation in bans is as-good-as-random. It also gives me some reason to believe that the within-state variation would also be orthogonal to unobserved determinants (Altonji, Elder, and Taber, 2005).

In Column 3 I include leading indicators of *Texting Ban* and *Handheld Ban* to directly test if cell

[19] As a check, I include New York's 2001 handheld ban by using the 2000-2010 sample and estimate the count model. I find that it does not drive the estimated effects of cell phone bans on accidents and casualties. For example, when including New York, the estimates of *Texting Ban* and *Handheld Ban* for accidents are are -0.0256 (s.e.=0.023) and -0.012 (s.e.=0.0257), respectively, which are similar to the corresponding estimates of -0.0148 (s.e.=0.0221) and -0.0249 (s.e.=0.034) reported in Column 1 of Panel A in Table A2.

phone usage trends diverge one year before bans are enacted. The two insignificant leading indicator estimates show no evidence of such divergence. In Column 4 I also include region-year fixed effects. The estimates are unchanged, which suggests that the effects are not confounded by regional common shocks. In Column 5 I include state-specific linear time trends to relax the assumption that state-level unobservable covariates are constant from 2004 to 2010 and find that results stay robust (Friedberg, 1998). Finally, I exclude California in Column 6 and find that it does not drive the results.

From Column 7 to Column 12, I re-estimate the effects of cell phone bans using weighted OLS in which state population is used to adjust the sampling weight because the chosen sample depends on observational sites that are not necessarily representative.[20] Results are not statistically different from estimates in the first six columns.

In panel B, I examine if cell phone bans have any effect on handheld device manipulation while driving, such as sending text messages. Results indicate that texting bans significantly reduce such behavior by about 0.6 to 1.9 percent points, which is about 25 to 80 percent drop compared to the manipulation rate the year before texting bans are adopted. Meanwhile, there appears to be a substitution from manipulating handheld devices towards using handheld cell phones caused by handheld bans as almost all the estimates of *Handheld Ban* are positive and significant at the 1% level.

Thus, the evidence suggests that both texting bans and handheld bans are quite effective in reducing visible handheld device manipulation and handheld cell phone use, respectively, by increasing the expected cost of using a cell phone while driving. Moreover, handheld bans appear to induce drivers to further shift towards more texting. One interpretation consistent with this finding is that it is much easier to avoid detection by police while texting than to avoid detection when talking on a handheld phone, as one can always text by holding the cell phone in a concealed way. But a caveat

[20] I use the pweight procedure of Stata (version 12.0) to apply the sampling weight using state population to the regression.

in interpreting the effect of handheld bans is that, as discussed in Section 3.1, it is difficult to distinguish between whether handheld bans matter on their own and whether banning both texting and talking while driving is what really works. In addition, given the limitation of the way NOPUS data are collected, the estimated effect of cellphone bans could potentially be biased if on average drivers in adopting and non-adopting states use cell phones differentially from 6pm to 7am or in locations other than the stop sign and spotlight areas.

4.2 Do Cell phone Bans Reduce Accidents and Casualties?

After establishing the mechanism through which cell phone bans matter, I now turn to the quarterly state-level panel data to investigate the effect of cell phone bans on accidents and casualties.

I begin by examining the effect of cell phone bans on accidents. Panel A in Figure 2 shows logged accident rates for treatment and control states before and after the adoption of texting bans. In order to remove seasonal effects and other trends common to all states, the vertical axis shows the average residuals after partialing out year-by-quarter fixed effects. The horizontal axis shows the number of quarters after the adoption of the cell phone ban. For example, treatment states are assigned a zero in the quarter in which handheld bans are enacted, while control states are assigned a zero in the 1st quarter of 2010, which is the average time at which treatment states passed handheld bans.[21] While more formal regression results follow, two things are clear from this figure. First, both treatment and control states appear to follow the same *trend* before handheld bans are adopted. Though on average control states have higher accident rates, one should not worry that the difference in the *levels* of accident rates would bias the estimated effects given the difference-in-differences research design I am using. There is no evidence of divergence between adopting and non-adopting

[21] For example, 0 for Arkansas stands for the fourth quarter of 2009, the time when Arkansas passed its texting ban.

states prior to handheld bans, so it is hard to believe that there would have been any divergence afterward in the absence of the bans.

Second, it is clear that the two accident paths for treatment and control states consistently track each other closely after the adoption of texting bans, indicating that there is no evidence of the effectiveness of texting bans on accidents.[22] Similarly, Panel A in Figure 3 suggests that handheld bans might not reduce accidents as well.

Regression results in Panel A of Table 4 confirm this graphical evidence, in which estimates of *Texting Ban* and *Handheld Ban* are in general statistically insignificant and close to zero.

I now move to the effect of cell phone bans on casualties, defined as the sum of incapacitating injuries and fatalities. Graphical evidence in Panel B of Figure 2 and 3 tells a similar story as in the case of accidents: neither texting bans nor handheld bans seem to reduce casualties. Estimates in Panel B of Table 4 indicate that texting bans have no meaningful effects on casualties.[23] In Table A1 in the appendix, I look at incapacitating injuries and fatalities separately and find that the effects of texting and handheld bans are similar.

Combined with the evidence from how drivers respond to texting bans and handheld bans, it seems quite interesting that cell phone bans have no effect on traffic accidents and casualties even though they reduce cell phone use while driving. There are two possible interpretations for this. One is that cell phone bans could reduce accidents and casualties by reducing some drivers' use of cell phones. But meanwhile they could also make drivers more likely to text or make phone calls in a concealed way. This type of behavior is likely more dangerous and would thus lead to more accidents and casualties, offsetting any reduction due to less overall cell phone use while driving.

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[22] Due to the fact that states generally adopted the handheld ban after the texting ban, as discussed in Section 2, the post-adoption effect of texting ban could be confounded with handheld bans. Therefore, the treatment states in Figure 2 do not include states with handheld bans.

[23] I use the aweight procedure of Stata (version 12.0) to apply the analytic weight using state population to the regression.

The other interpretation is that drivers compensate for distracted cell phone-related driving behavior by driving more cautiously in other ways (Bhargava and Pathania, forthcoming), similar to the "Peltzman Effect" (Peltzman, 1975) in the setting of safety-belt use. Therefore, texting or talking on handheld cell phones does not necessarily lead to more accidents and casualties, because rational drivers simply drive more cautiously when doing so. As a result, it is plausible that cell phone bans do not reduce accidents and casualties either, although they do make drivers use cell phones less often.

4.3 Differential Effects

To this point, I have shown the average effect of cell phone bans on drivers' cell phone usage, and subsequently on accidents and casualties. Here I investigate if cell phone bans have differential effects by driver or accident type.

Two kinds of differential effects are of particular interest. The first is that cell phone bans could affect a driver's behavior differentially depending on whether the driver is accompanied by passengers or not. For example, a driver may be able to substitute passengers' cell phone use for her own, thereby making it easier for her to comply with the ban. In short, raising the cost to using a cell phone while driving is likely to change behavior the most when there are good substitutes available, and drivers with passengers have more substitutes available. Similarly, risk-averse passengers who are aware of the cell phone bans are more likely to remind the drivers to comply with the bans. Thus, it is reasonable to expect that cellphone bans would have the biggest effect on drivers accompanied by passengers.

In Panel A of Table 5, results show that drivers who are driving with passengers do respond more to cell phone bans than those who are driving alone. Since the estimates in Columns 1 and 3 based on Model (1) only represent absolute changes in probabilities of handheld cell phone use and handheld

device manipulation, respectively, in Columns 2 and 4 I convert them to proportional changes relative to the corresponding probabilities the year before cell phone bans were adopted in order to better compare these estimates. The results imply texting bans reduce manipulation of handheld devices three times as much for drivers with passengers as for single drivers. There is a similarly disproportionate effect for handheld bans. One somewhat peculiar finding is that texting bans appear to induce drivers with passengers to be much less likely to talk on handheld cell phones. This could be because passengers talk or text on their phones in lieu of the driver doing so when a ban is in effect.

The second kind of differential effect worth further examining is whether cell phone bans have different effects on young and adult drivers. Results from Panel B suggest that cell phone bans only differentially alter the way young and adult drivers send text messages, not whether or not they make phone calls while driving. In particular, texting bans significantly reduce the probability of manipulating handheld devices among adult drivers by 75 percent but have no significant effect on young drivers. Also, handheld bans induce more adult drivers to shift from talking to texting while driving than young drivers.

Results in Panel C suggest that there are no differential effects on accidents as in each column estimates are insignificant and are not significantly different from each other. Again, this could be explained by more hidden use of cell phones or the compensating driving behavior, as discussed in Section 4.2.

4.4 Permutation Tests

So far I have shown that cell phone bans have no meaningful impact on accidents and casualties. Though it could be explained by more concealed use of cell phones or the compensation effect, it is also possible that the statistical inference is incorrect due to, for example, over-estimated standard

errors. To account for this concern, in this section, I perform three kinds of permutation tests to ensure that the statistical inference in Tables 4 is correct, in addition to reporting levels of statistical significance based on clustered standard errors.

In the first test, I randomly pick a 7-year state panel of accidents and casualties between 1975 and 2000, and then assign the placebo cell phone bans to 23 randomly picked states without replacement in the order of Table 1.[24] Specifically, the first randomly selected state simulates Arkansas and is assigned a texting ban that becomes effective on October 1st in the 6th year. Similarly, the second randomly selected state simulates California and is assigned a texting ban and a handheld ban on January 1st in the 6th year and July 1st in the 5th year, respectively. The third selected state simulates Colorado, and so on. I then repeat this randomization strategy 1,000 times and estimate the effect of the placebo cell phone bans on accidents and casualties using Specifications 1 and 7 in Table A2 in the appendix which are based on the count data model, as well as drawing the empirical distribution of the placebo estimates.[25] The reasons to rely on the count data model are that placebo estimates corresponding to Table 4 are infeasible since the state-level quarterly VMT data are unavailable before 2004 and that the rate model (Table 4) and the count data model (Table A2) generate similar estimates.

Results in Figure 4 through Figure 7 indicate that the estimated effects of cell phone bans on accidents and casualties from the count data model are not significant based on the empirical distributions. For example, in Panel A of Figure 4, 30.9% of placebo estimates of *Texting Ban* from unweighted OLS are smaller than -0.0148 (the estimate of Texting Ban in Column 1 of Panel A in Table A2), which is equivalent to a two-sided empirical p-value of 0.618. In particular, Figure 6 and Figure 7 further suggest that the OLS using analytic weights tend to exaggerate the statistical

[24] I choose the 1975-2000 period for this permutation test because (1) the historical data of accidents and casualties in FARS can only be traced back to 1975 and (2) there was no cell phone ban passed during this period. The first cell phone can was New York's handheld ban in 2001.

[25] The count data model uses the count of accidents and casualties as dependent variables based on Model (2), rather than the rate of accidents and casualties which is adjusted by VMT.

significance.[26]

In addition, results from these placebo estimates imply that in Tables 4 the standard errors and levels of statistical significance are correctly specified only for *Texting Ban* estimates from unweighted OLS in the spirit of Bertrand, Duflo, and Mullainathan (2004). Specifically, based on unweighted OLS, 11.8% of *Texting Ban* placebo estimates for accidents and 12% of *Texting Ban* placebo estimates for casualties are significant at the 10% level. Meanwhile, 17.7% of *Handheld Ban* placebo estimates for accidents and 18.2% of *Handheld Ban* placebo estimates for casualties are significant at the 10% level. Estimates from weighted OLS appear to overstate the degree of statistical significance in general: about 20% of *Texting Ban* placebo estimates and 30% of *Handheld Ban* placebo estimates are significant at the 10% level, respectively.

Finally, similar to Nunn and Qian (2011), I perform rolling estimation to examine the causal relationship between cell phone bans and accidents and casualties by looking at whether the actual estimates represent a structural break of the pattern of the placebo estimates. In other words, I test whether the divergence observed at the time of the cell phone bans was significantly larger than any divergences that had occurred previously, or if it merely reflects a general tendency for these groups of states to diverge over time. To do so, I assign placebo cell phone bans to the *same* treatment states in 7-year panels from 1975 to 2010 also as in Table 1 and then estimate the effect using Specification 8 of Table 4 and 5. Specifically, in each 7-year panel, Arkansas is assigned the texting ban on October 1st in the 6th year; California is assigned the texting ban on July 1st in the 6th year and the handheld ban on January 1st in the 5th year, and etc. Under the identifying assumption of the difference-in-differences approach, I would not expect that the placebo estimates from before the bans were enacted to be different from zero. Panel A of Figure 8 plots the unweighted placebo estimates of *Texting Ban* for

[26] Three out of four estimates in Column 7 of Table A2 are significant. But the empirical distributions in Figure 6 and Figure 7 indicate that these estimates should have been insignificant.

accidents and casualties, respectively, from the 1975-1981 period to the 2003-2009 period, as well as the estimates for the 2004-2010 period. As expected, prior to the 2004-2010 period, placebo estimates are indeed close to zero, which show that there was no historical difference in trends between adopting and non-adopting states. By comparison, the estimates from the real treatment period (count estimates in Column 1 of Table A2), -0.0148 and -0.00189, do not appear to represent a structural break of the placebo estimates, implying that texting bans during 2004-2010 have no effect on accidents and casualties at all. Along similar lines, Panel B shows that the two count estimates of *Handheld Ban*, -0.249 and -0.0405, are close to zero as well as the average of the corresponding placebo estimates, suggesting that handheld bans have no effect either. Figure 9 tells a similar story using weighted OLS.

Taken together, I have found strong evidence from the three permutation tests that neither texting ban nor handheld ban has a meaningful impact on accident and casualties. Moreover, the findings suggest that in Table 4 unweighted OLS correctly specifies the level of statistical significance while weighted OLS tends to overstate it.

4.5 Additional Tests and Robustness Checks

In this section I perform different kinds of tests and robustness checks to validate the estimated effects of cell phone bans. First, to understand whether cell phone bans could affect traffic and casualties through channels other than reducing drivers' cell phone usage, I look at if the general driving behavior is affected. To do so, I focus on the effect of cell phone bans on vehicle miles travelled (VMT), which is a measure of the total vehicle mileage. Estimates in Columns 1 through 6 of Table 6 are all insignificant and close to zero, which suggests that it is unlikely that the effect of cell phone bans on accidents and casualties could be due to whether people drive more or less during the

examination period. [27]

Second, the NOPUS data allow me to perform a falsification test by examining whether cell phone bans change drivers' use of seat belt that ought not to be affected by cell phone bans. Again, I find zero effect as shown in Columns 7 through 12. This suggests that the estimated effect of cell phones bans will not be confounded with factors such as safety driving campaigns that could coincide with the adoption and implementation of cell phone bans and therefore bias the DD estimates.

Third, I further test if cell phone bans make drivers turn to using headsets. Results in Columns 13 through 18 suggest no evidence of this substitution behavior. Rather, it confirms the previous finding that the substitution is only between the use of handheld cell phones and handheld device manipulation.

Finally, I check if different definitions of cell phone bans could drive the results in Table 7. In Panel A I drop two states (New Jersey and Washington) where cell phone bans were upgraded to the primary enforcement. In Panel B, to single out the effect of texting bans, I exclude the six states that passed handheld bans. In Panel C, I redefine *Texting Ban* and *Handheld Ban* to be fractions of days during a quarter rather than binary variables in order to better capture the effects of cell phone bans in the quarter when they are enacted.,[28] Results indicate that the estimated effects are robust to these changes in definition.

5. Conclusion

Texting and handheld bans are the major response from state legislatures to distracted driving, but evidence on the effectiveness of such bans from previous studies is limited. This paper uses a

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[27] Carpenter and Stehr (2011) finds that state youth bicycle helmet laws have unintended consequence of reducing youth bicycling.

[28] For example, California adopted handheld ban on July 1st, 2008. Therefore, in terms of fractions of days with texting ban in 2008, the value of *Handheld Ban* for California in 2008 should be 0.5, compared to 1 using the binary definition.

difference-in-differences method and performs a particularly comprehensive analysis of the average effect of the U.S. state cell phone bans on both drivers' use of cell phones and accidents and casualties. Results indicate that driver behavior is very responsive: texting bans reduce visible texting while driving by around 60% and handheld bans reduce the probability of talking on handheld cell phones while driving by around 40%. Specifically, cell phone bans have a larger effect on adult drivers and drivers accompanied by passengers. However, these apparent changes in behavior do not lead to meaningful reductions in accidents or casualties. This could be because cell phone use while driving is less dangerous than commonly believed, or because individuals drive less carefully when not using a cell phone than they do while using a cell phone, or because cell phone bans induce more hidden and likely dangerous use of cell phones while driving.

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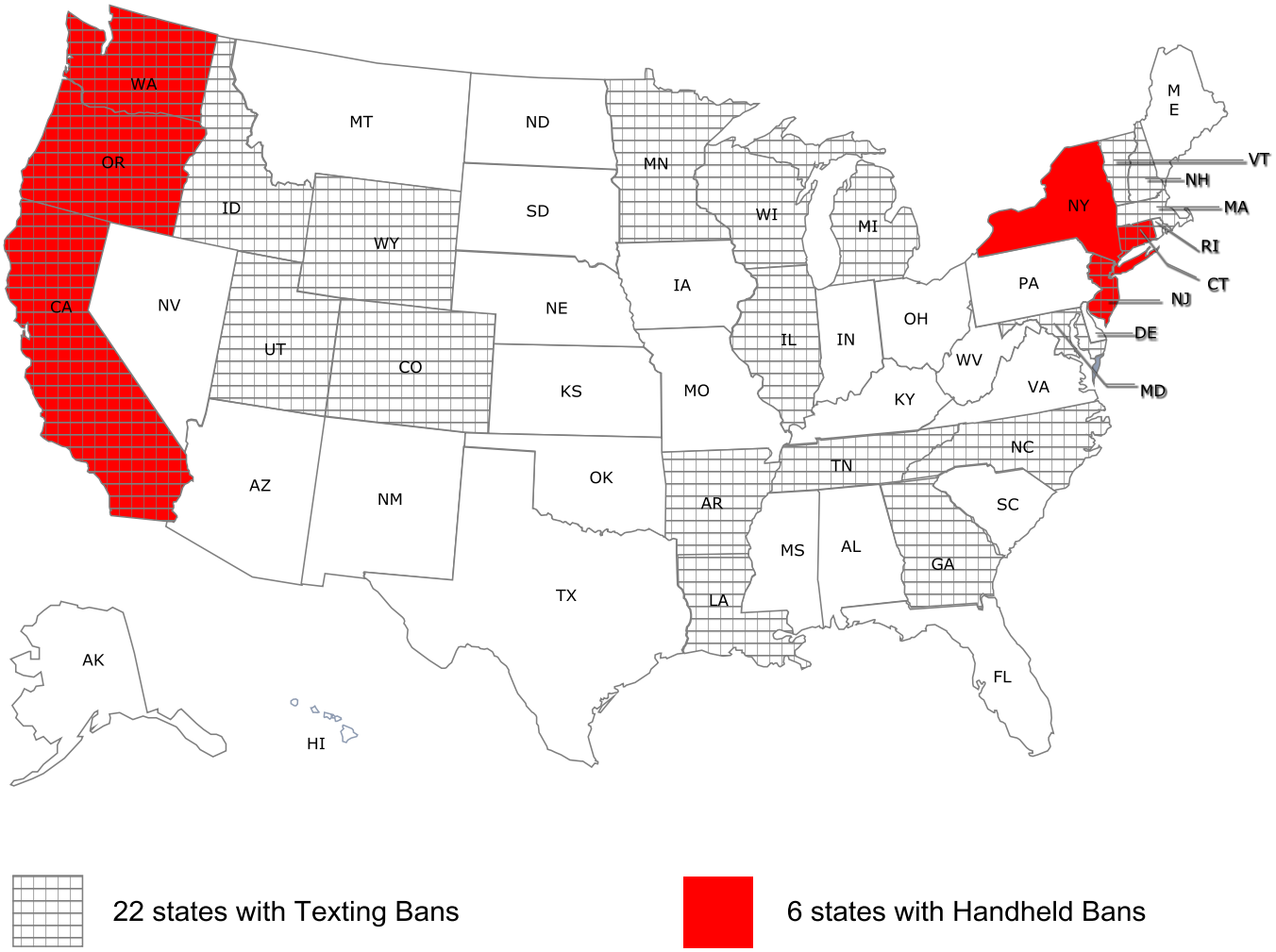
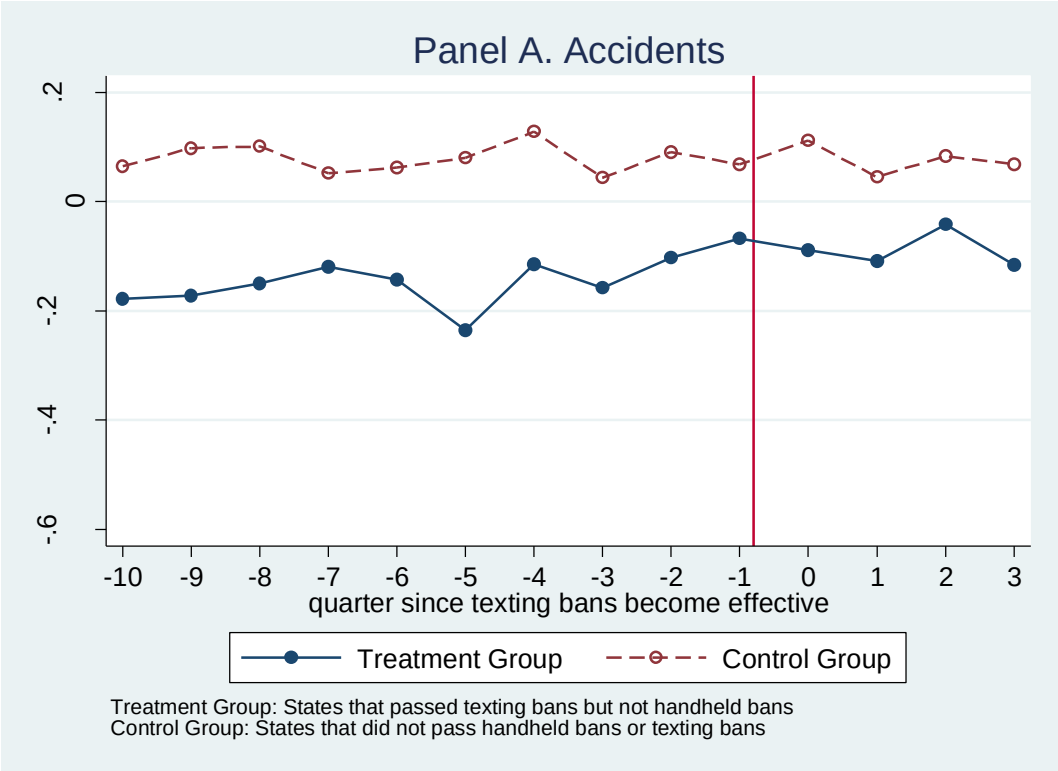


Figure 1. State Texting nad Handheld Bans with Primary Enforcement as of 2010



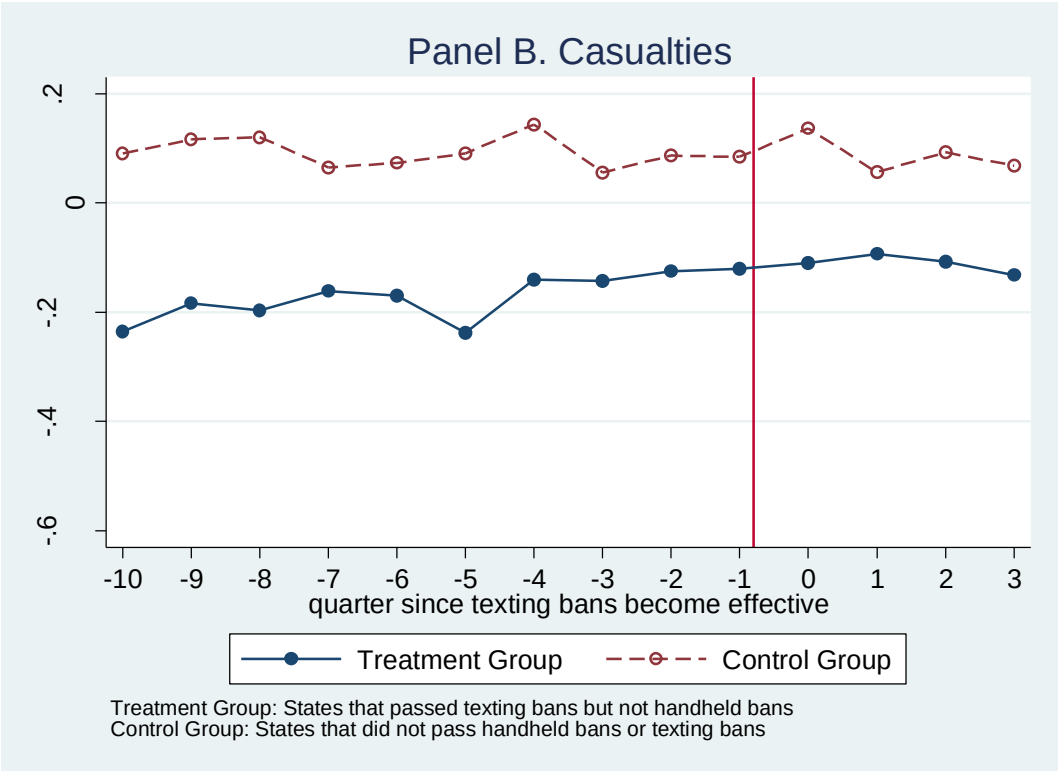
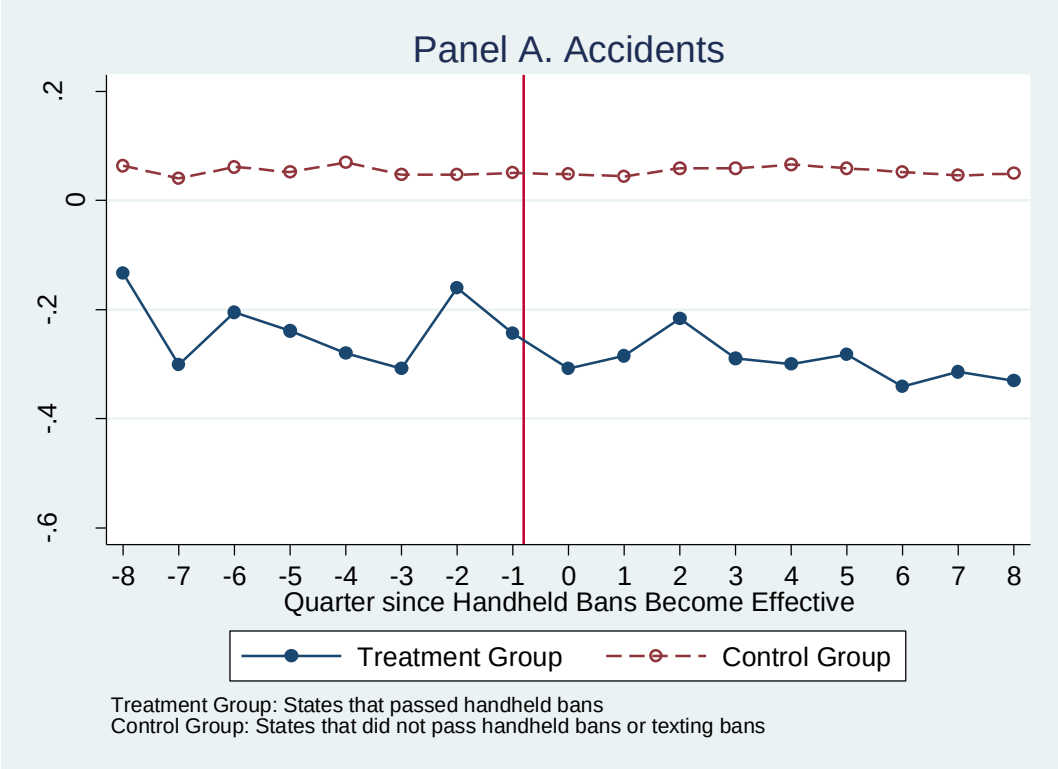


Figure 2. Effects of Texting Bans on Accidents and Casualties

Notes: Each data point represents the average residual from regressing the logged accidents or casualties on fixed year-quarter effects. 0 on the horizontal axis corresponds to the time treatment states adopted texting bans. For control states 0 corresponds to the 1st quarter of 2010, which is the average time for treatment states to adopt only texting bans.



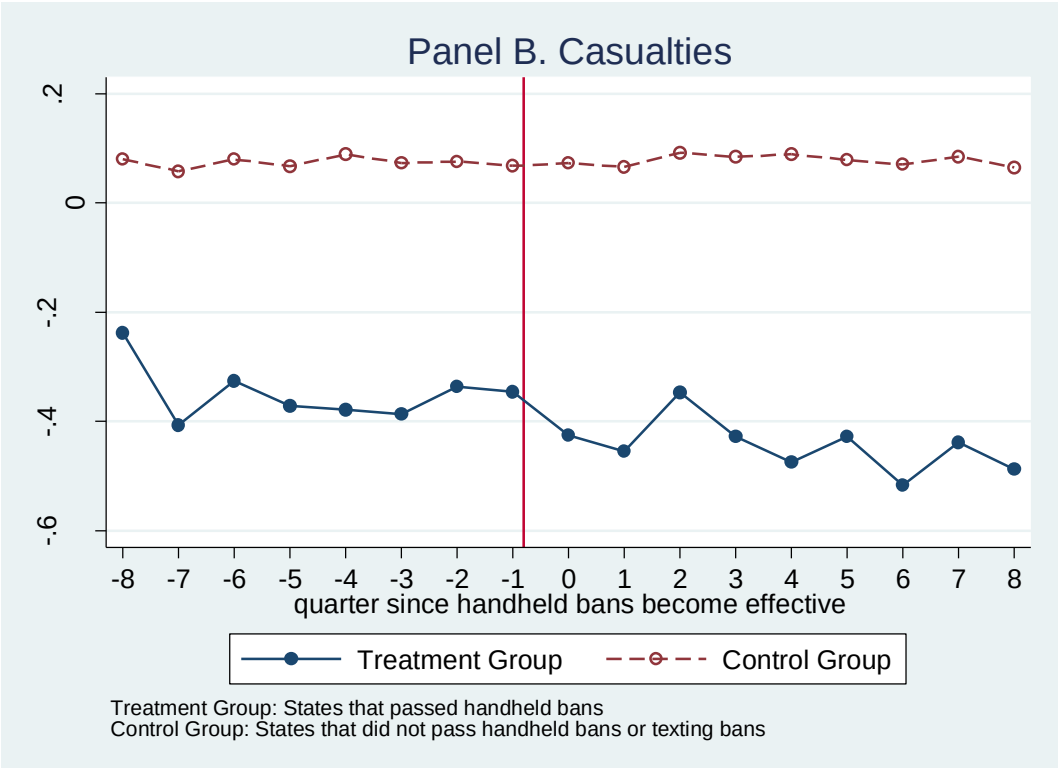
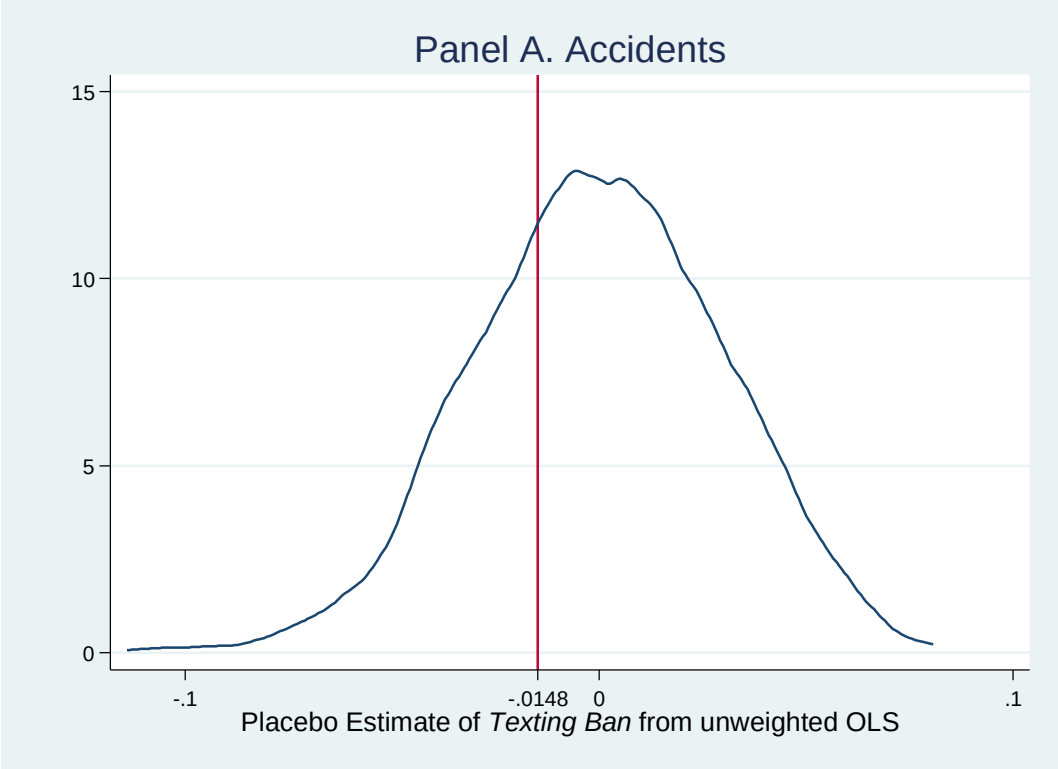


Figure 3. Effects of Handheld Bans on Accidents and Casualties

Notes: Each data point represents the average residual from regressing the logged accidents or casualties on fixed year-quarter effects. 0 on the horizontal axis corresponds to the time treatment states adopted handheld bans. For control states 0 corresponds to the 2nd quarter of 2008, which is the average time for treatment states to adopt handheld bans...



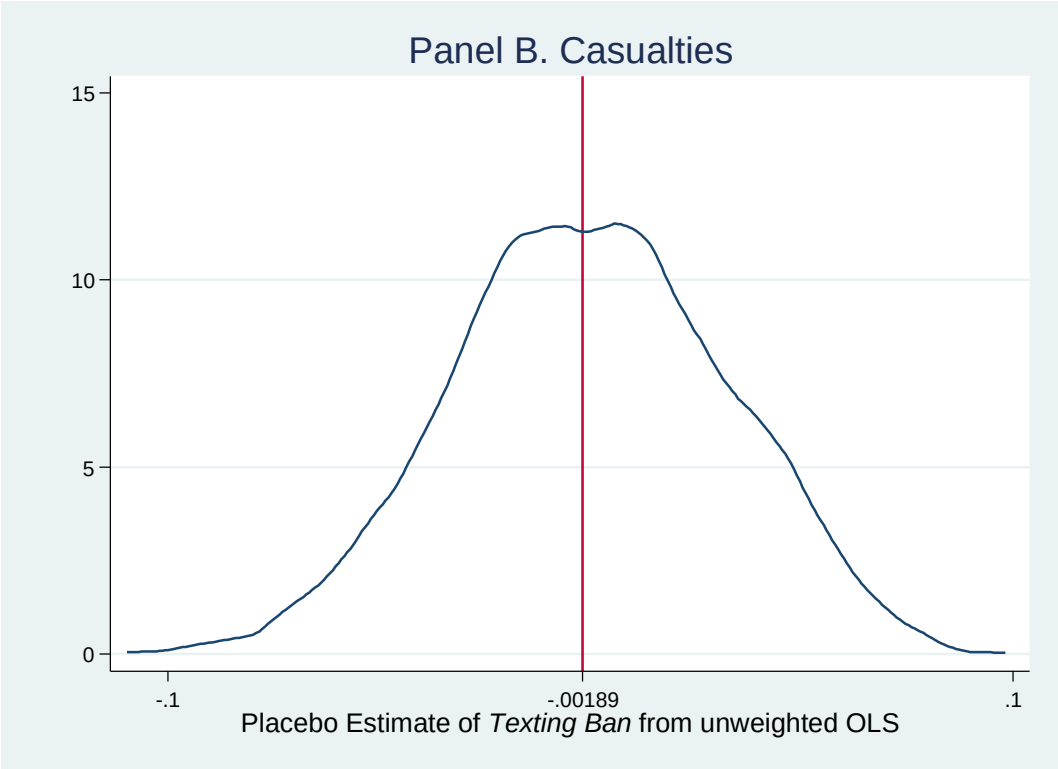
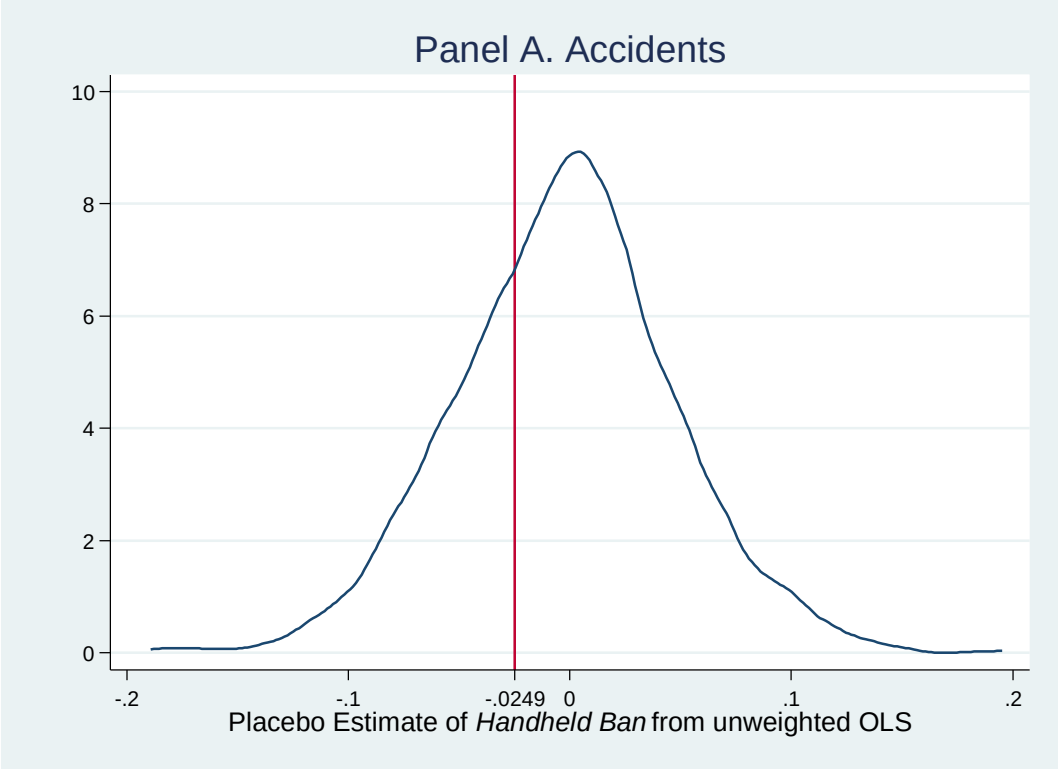


Figure 4. Empirical Density Distribution of Placebo Estimates of *Texting Ban* Based on Unweighted OLS

Notes: In Panel A, 30.9% of placebo estimates are smaller than -0.0148, the estimate of *Texting Ban* in Column 1 of Panel A in Table A2. In Panel B, 47.9% of placebo estimates are smaller than -0.00189, the estimate of *Texting Ban* in Column 1 of Panel B in Table A2.



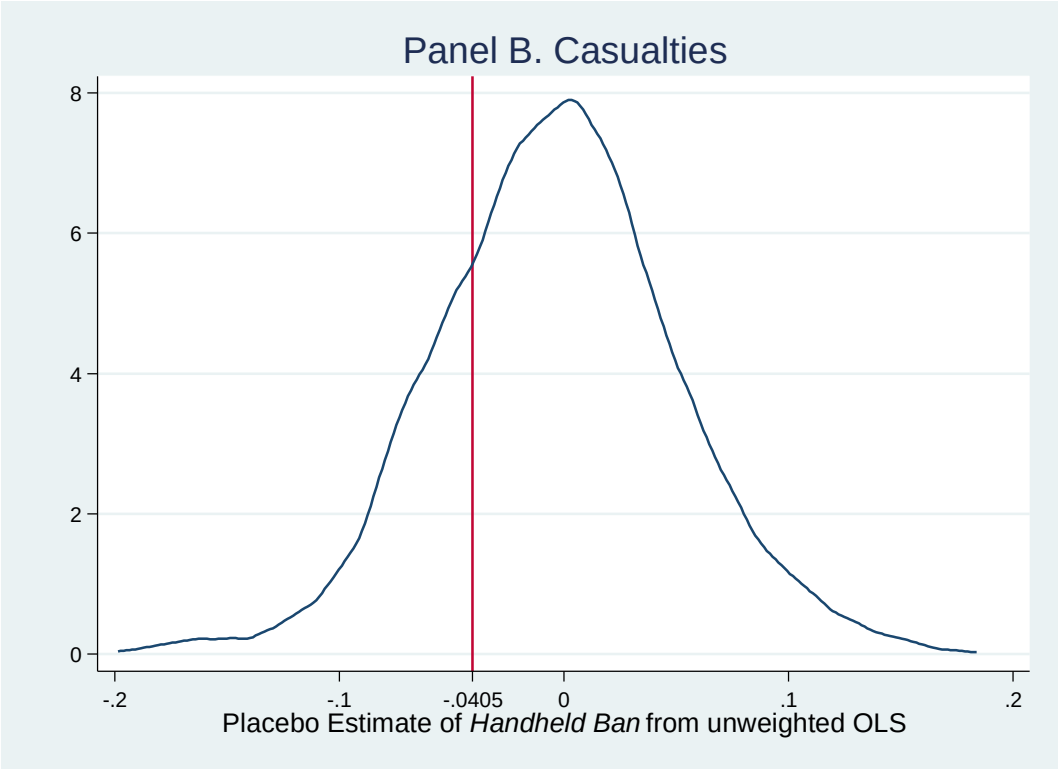
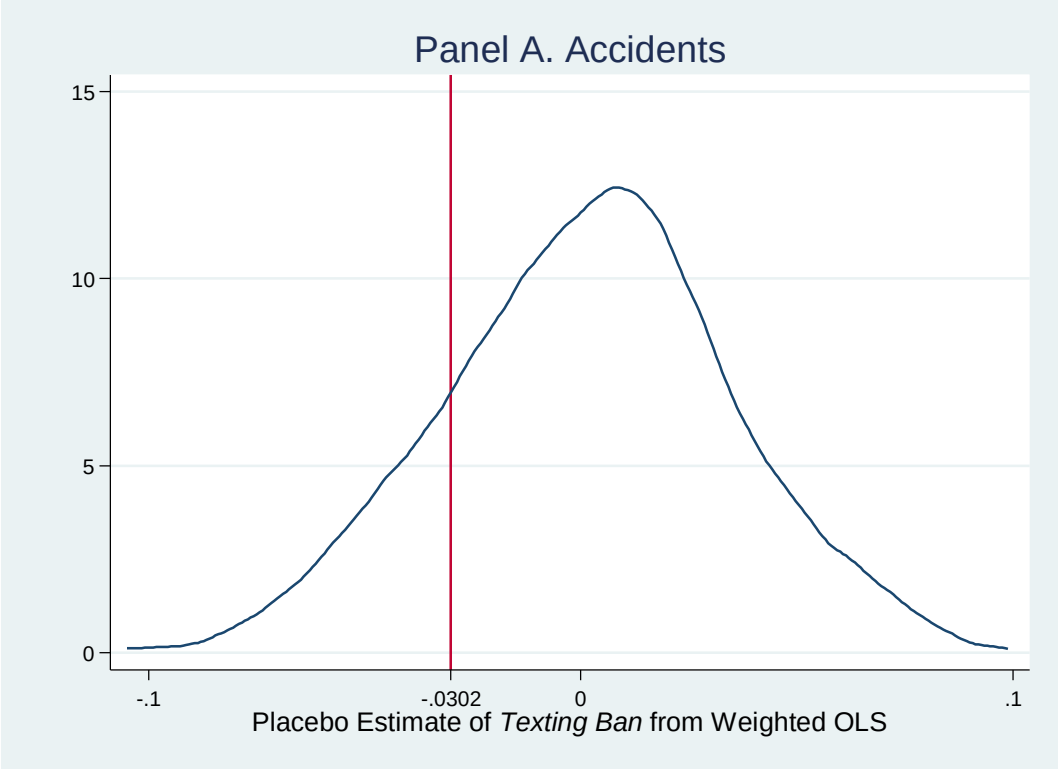


Figure 5. Empirical Density Distribution of Placebo Estimates of *Handheld Ban* Based on Unweighted OLS

Notes: In Panel A, 30.8% of placebo estimates are smaller than -0.0249, the estimate of *Handheld Ban* in Column 1 of Panel A in Table A2. In Panel B, 22.8% of placebo estimates are smaller than -0.0405, the estimate of *Handheld Ban* in Column 1 of Panel B in Table A2.



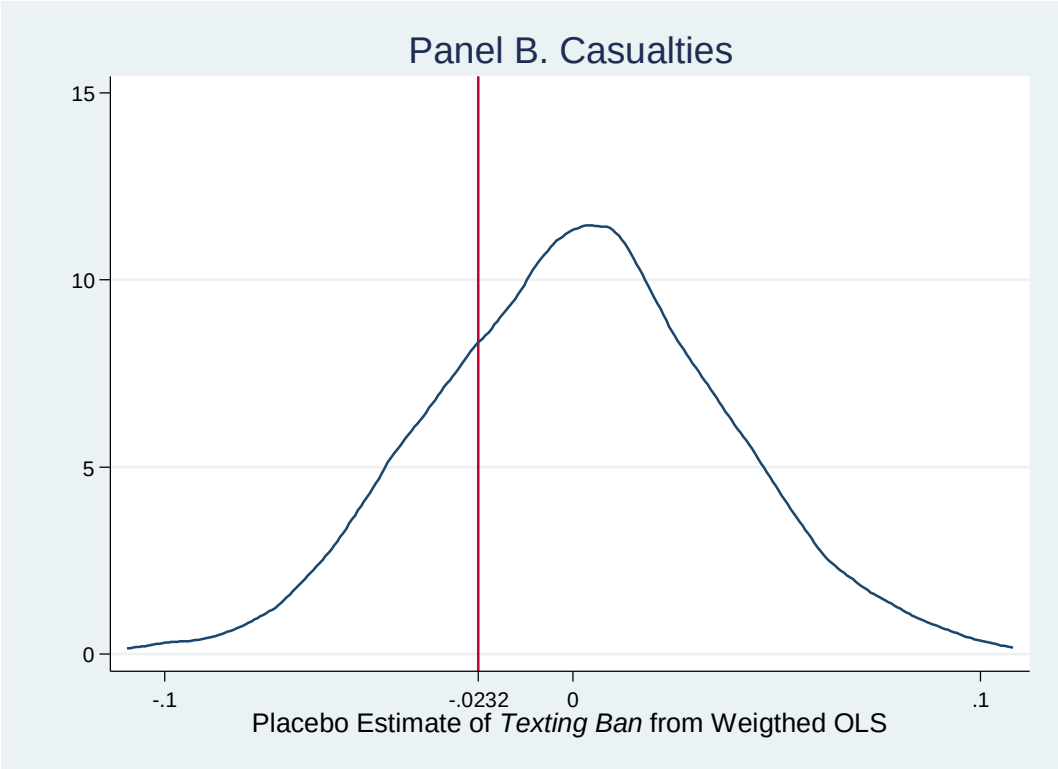
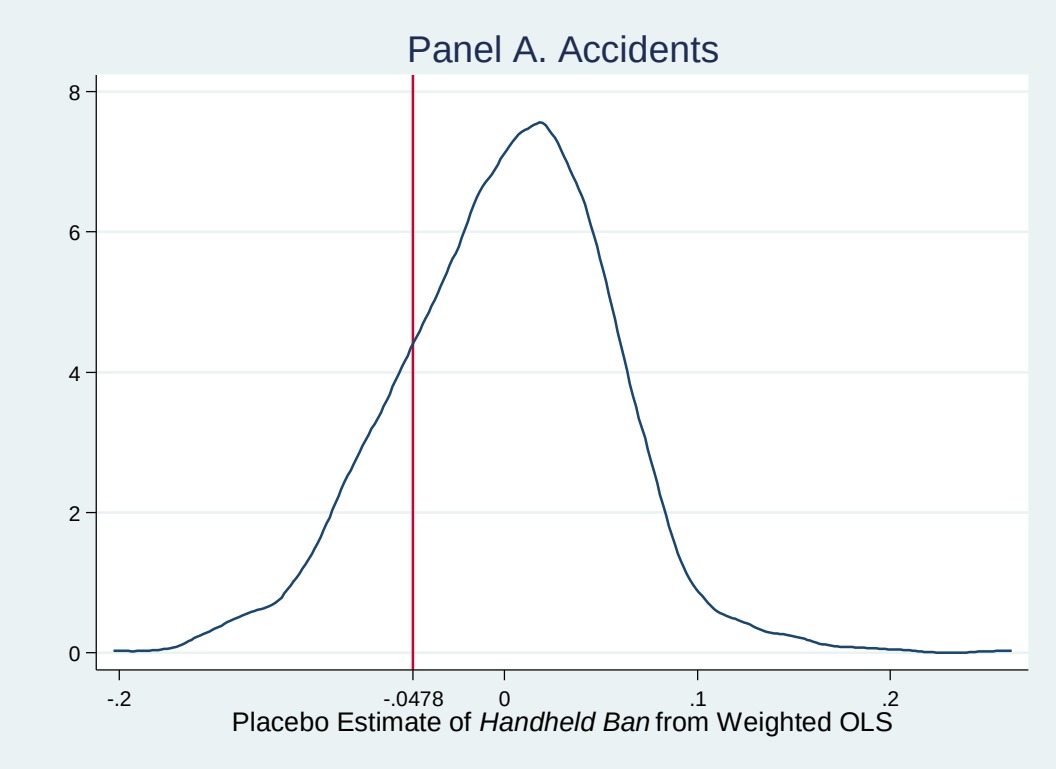


Figure 6. Empirical Density Distribution of Placebo Estimates of *Texting Ban* Based on Weighted OLS

Notes: In Panel A, 17.6% of placebo estimates are smaller than -0.0302, the estimate of *Texting Ban* in Column 7 of Panel A in Table A2. In Panel B, 24.6% of placebo estimates are smaller than -0.0232, the estimate of *Texting Ban* in Column 7 of Panel B in Table A2.



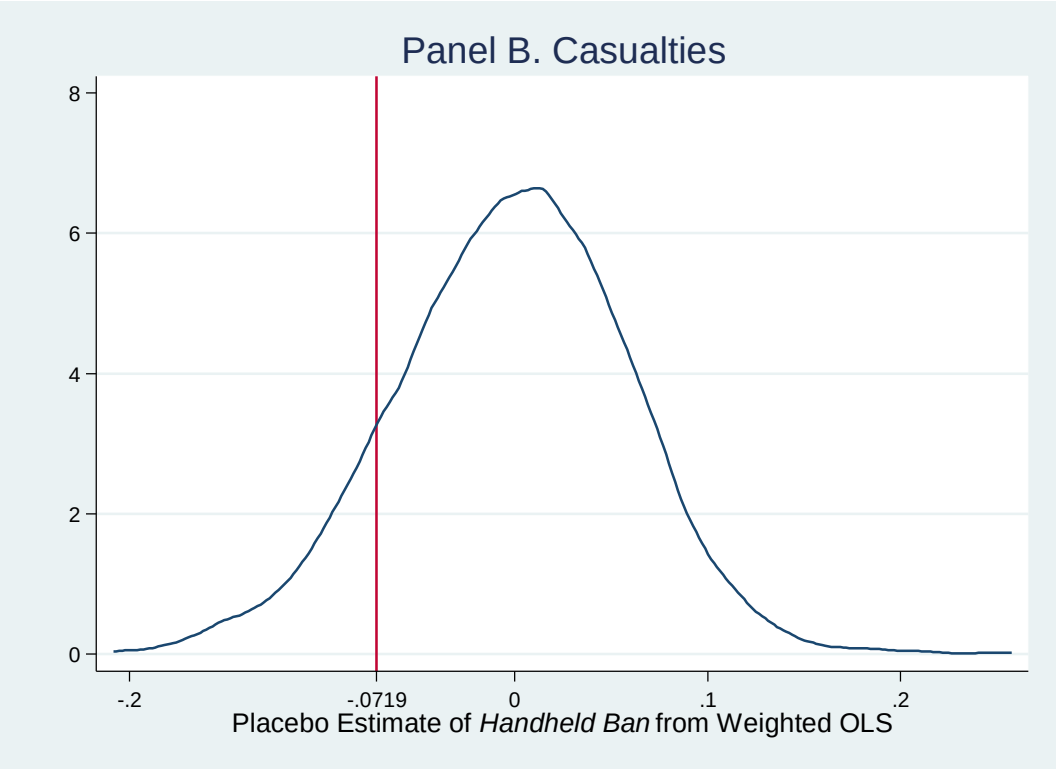
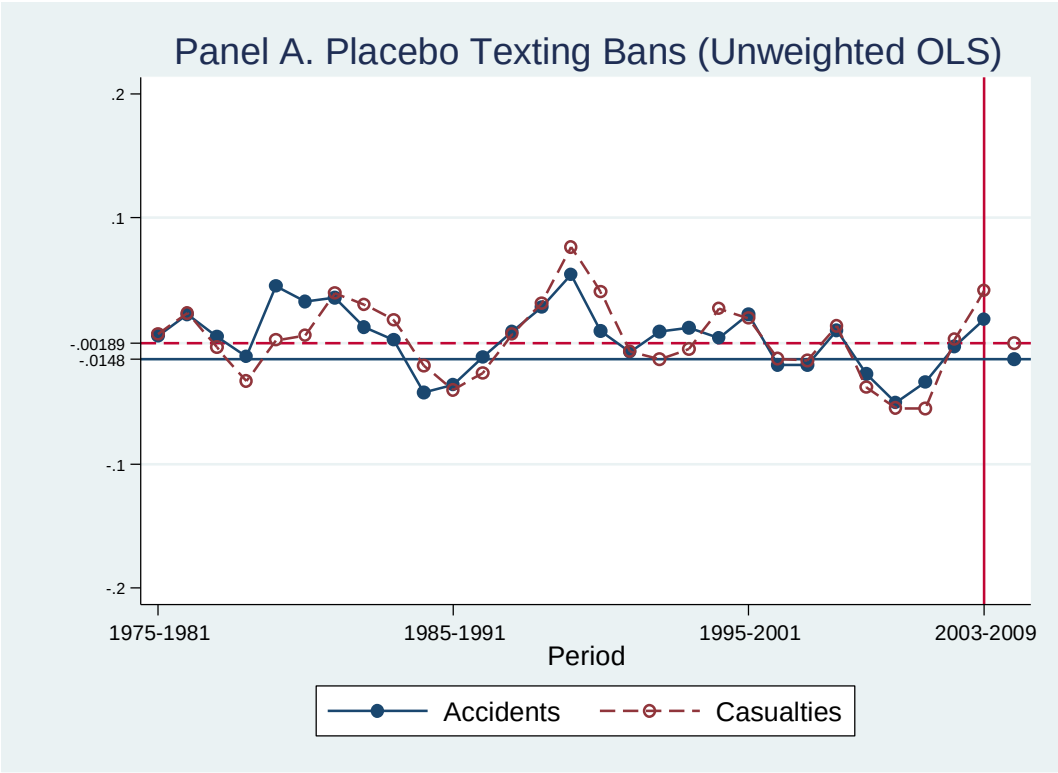


Figure 7. Empirical Density Distribution of Placebo Estimates of *Handheld Ban* Based on Weighted OLS

Notes: In Panel A, 19.1% of placebo estimates are smaller than -0.0478, the estimate of *Handheld Ban* in Column 7 of Panel A in Table A2. In Panel B, 12.1% of placebo estimates are smaller than -0.0719, the estimate of *Handheld Ban* in Column 7 of Panel B in Table A2.



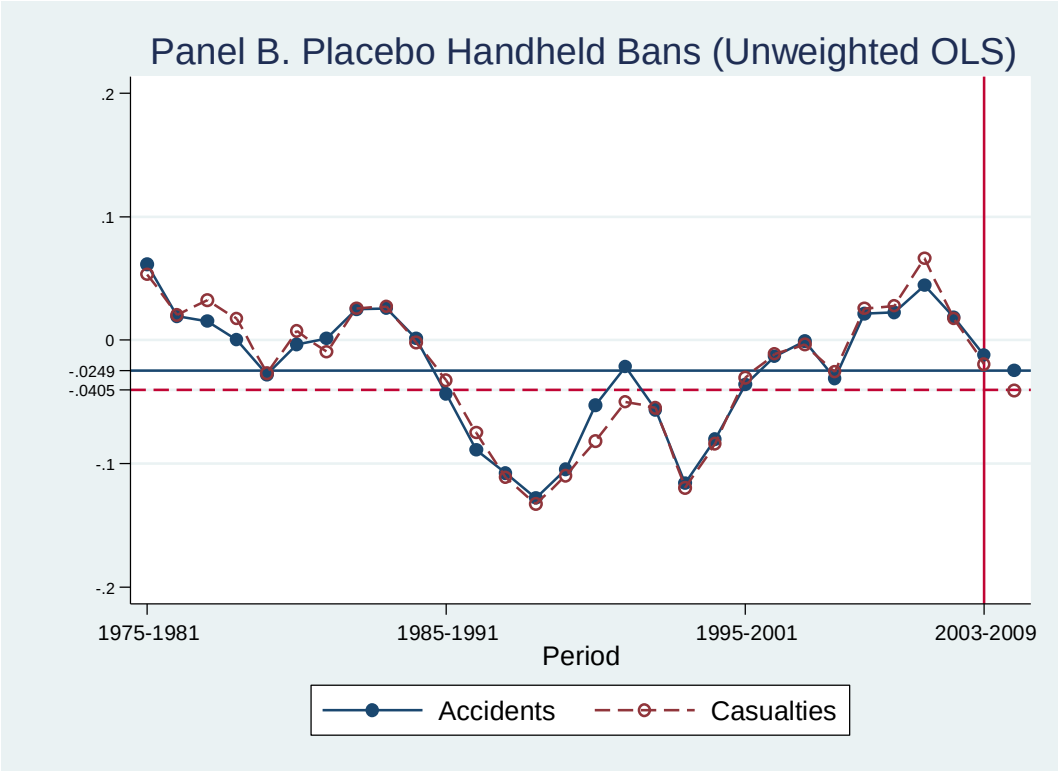
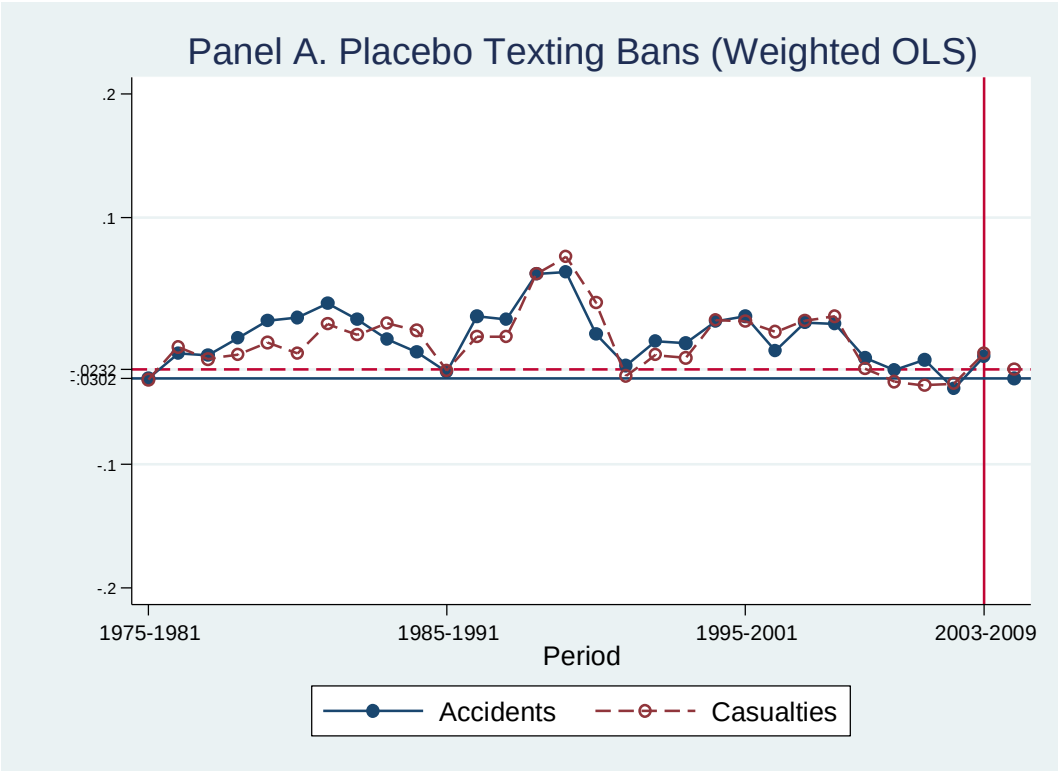


Figure 8. Rolling Estimation of the Effect of Placebo Cellphone Bans Based on Unweighted OLS

Notes: Placebo estimates are from the 1975-1981 period to the 2003-2009 period in which placebo cellphone bans are assigned to the same treatment states. The average effect of placebo texting bans for accidents and casualties are 0.00174 and 0.0013; the average effect of placebo handheld bans for accidents and casualties are -0.023 and -0.0227. The estimates based on the real texting and handheld bans during the 2004-2010 period on accidents (-0.0148 and -0.0249) and casualties (-0.00189 and -0.0405) are indicated by horizontal solid and dash lines, respectively.



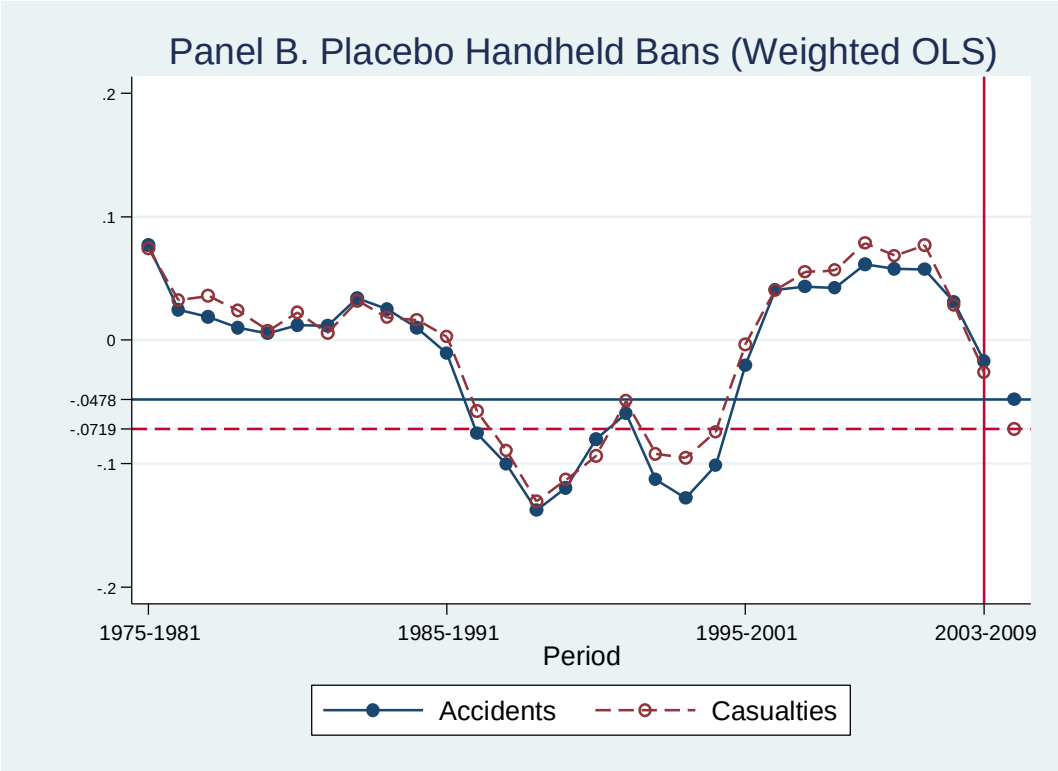


Figure 9. Rolling Estimation of the Effect of Placebo Cellphone Bans Based on Weighted OLS

Notes: Placebo estimates are from the 1975-1981 period to the 2003-2009 period in which placebo cellphone bans are assigned to the same treatment states. The average effect of placebo texting bans for accidents and casualties are -0.00285 and -0.0004; the average effect of placebo handheld bans for accidents and casualties are -0.0138 and -0.0051. The estimates based on the real texting and handheld bans during the 2004-2010 period on accidents (-0.0302 and -0.0478) and casualties (-0.0232 and -0.0719) are indicated by horizontal solid and dash lines, respectively

Table 1. State Texting and Handheld Bans with Primary Enforcement as of 2010

State	Effective Date	
	Texting Ban	Handheld Ban
Arkansas	2009/10/1	
California	2009/1/1	2008/7/1
Colorado	2009/12/1	
Connecticut	2010/10/1	2005/10/1
Georgia	2010/7/1	
Illinois	2010/1/1	
Louisiana	2008/7/1	
Maryland	2009/10/1	
Massachusetts	2010/9/30	
Michigan	2010/7/1	
Minnesota	2008/8/1	
New Hampshire	2010/1/1	
New Jersey	2008/3/1	2008/3/1*
New York		2001/11/1
North Carolina	2009/12/1	
Oregon	2010/1/1	2010/1/1
Rhode Island	2009/11/10	
Tennessee	2009/7/1	
Utah	2009/5/12	
Vermont	2010/6/1	
Washington	2010/6/10*	2008/7/1
Wisconsin	2010/12/1	
Wyoming	2010/7/1	

* States that have upgraded the enforcement type from secondary to primary

Table 2. Descriptive Statistics

Dependent Variable	Mean	Observations
Accident Rate (per 100 Million VMT)	2.01	1428
	(0.60)	
Casualty Rate (per 100 Million VMT)	1.81	1428
	(0.64)	
Fatality Rate (per 100 Million VMT)	1.39	1428
	(0.44)	
Injury Rate (per 100 Million VMT)	0.42	1428
	(0.25)	
Accidents	294	1428
	(313)	
Casualties	252	1428
	(255)	
Accident Rate by Drivers with at least One Passenge	0.81	1428
	(0.28)	
Accident Rate by Drivers without Any Passenger	1.21	1428
	(0.36)	
Accident Rate by Young Drivers	0.42	1428
	(0.15)	
Accident Rate by Adult Drivers	1.41	1428
	(0.44)	
Handheld Device Manipulation	0.0062	331883
	(0.08)	
Handheld Cellphone Usage	0.0541	331883
	(0.23)	
Headset Usage	0.0066	331883
	(0.08)	
Seat Belt Usage	0.84	331883
	(0.37)	
VMT (Millions)	14,741	1428
	(15136)	

Independent Variable	Mean	Observations
Texting Ban	0.08	1428
	(0.26)	
Handheld Ban	0.06	1428
	(0.24)	
Unemployment Rate (%)	5.88	1428
	(2.28)	
Median Income ($)	228,613	1428
	(268641)	
Population	6,013,147	357
	(6608285)	
Police	19,916	357
	(23805)	
Government Spending on Highway	2,332,007	357
	(2309445)	
Government Spending on Public Welfare	8,428,193	357
	(10800000)	
Male Aged 15-24 (%)	7.23	357
	(0.54)	
Male Aged 25-44 (%)	13.66	357
	(0.79)	
Violent Crime Rate	400	357
	(169)	
Property Crime Rate	3164	357
	(745)	
Age	45	331883
	(12)	
White	0.79	331883
	(0.41)	
Black	0.11	331883
	(0.31)	
Male	0.58	331883
	(0.49)	
Weekend	0.23	331883
	(0.42)	
Weekday Non-Rush Hour	0.43	331883
	(0.49)	
Urban Area	0.20	331883
	(0.40)	
Rural Area	0.21	331883
	(0.41)	
Clear Weather	0.90	331883
	(0.31)	

Notes: Each cell contains the mean with the standard deviation in the parentheses. Quarterly state-level variables have 1428 observations and annual state-level variables have 357 observations.Variables from the individual observational survey have 331883 observations.

Table 3. The Effect of Cell Phone Bans on Drivers' Cell Phone Usage

	1	2	3	4	5	6	7	8	9	10	11	12
	Unweighted OLS						Weighted OLS					
Panel A. Handheld Cell Phone Usage (Talking on Handheld Cellphones)												
Texting Ban	-0.00562	-0.00716	-0.00972	-0.00544	-0.0140	-0.00616	-0.0117	-0.0118	-0.0178*	-0.0105	-0.0110	-0.0114
	(0.00857)	(0.00805)	(0.00951)	(0.00848)	(0.0101)	(0.00812)	(0.0106)	(0.00965)	(0.0102)	(0.0103)	(0.0106)	(0.00990)
One Quarter Before Adoption of			-0.00700						-0.0198			
Texting Ban			(0.0112)						(0.0130)			
Handheld Ban	-0.0284***	-0.0284***	-0.0254***	-0.0333***	-0.0297***	-0.0237***	-0.0178	-0.0205**	-0.0152	-0.0246**	-0.0305***	-0.0170**
	(0.00797)	(0.00746)	(0.00862)	(0.00642)	(0.00870)	(0.00587)	(0.0107)	(0.00963)	(0.00933)	(0.00956)	(0.0106)	(0.00786)
One Quarter Before Adoption of			0.00895						0.0222			
Handheld Ban			(0.0110)						(0.0147)			
Observations	331883	331883	331883	331883	331883	286564	331883	331883	331883	331883	331883	286564
Panel B. Handheld Device Manipulation (Sending Text Messages, etc.)												
Texting Ban	-0.0140***	-0.0142***	-0.00672***	-0.0135***	-0.0325**	-0.0141***	-0.0101***	-0.0102***	-0.00639***	-0.00950***	-0.0189*	-0.00977***
	(0.00365)	(0.00372)	(0.00178)	(0.00342)	(0.0138)	(0.00384)	(0.00337)	(0.00342)	(0.00175)	(0.00319)	(0.00962)	(0.00357)
One Quarter Before Adoption of			0.0198**						0.0128			
Texting Ban			(0.00783)						(0.00775)			
Handheld Ban	0.0124***	0.0123***	0.0107***	0.0129***	0.0187	0.0100**	0.00981***	0.00958***	0.0102***	0.00922**	0.00929	0.00861***
	(0.00368)	(0.00378)	(0.00248)	(0.00455)	(0.0131)	(0.00433)	(0.00299)	(0.00308)	(0.00155)	(0.00402)	(0.00937)	(0.00304)
One Quarter Before Adoption of			-0.00713						-0.00164			
Handheld Ban			(0.00704)						(0.00729)			
Observations	293566	293566	293566	293566	293566	252435	293566	293566	293566	293566	293566	252435
State and Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time-Varying Controls		Yes	Yes	Yes	Yes	Yes		Yes	Yes	Yes	Yes	Yes
Region-Year Fixed Effects				Yes						Yes		
State Linear Trends					Yes						Yes	
Excludes California						Yes						Yes

Notes: The table reports panel data estimates using linear probility model. Each column in each panel represents a separate regression. The unit of observation is an individual. Weighted OLS uses state population to reweight the sample. Standard errors are clustered at the state level. Time-varying controls include indicators for gender, age group, race, rural and urban areas, weekday non-rush hour, weekend, and clear weather.

* Significant at the 10% level

** Significant at the 5% level

*** Significant at the 1% level

Table 4. The Effect of Cell Phone Bans on Traffic Accidents and Casualties

	1	2	3	4	5	6	7	8	9	10	11	12
	Unweighted OLS						Weighted OLS					
Panel A. Accident Rate												
	log(Accident Rate)						log(Accident Rate)					
Texting Ban	-0.00487	-0.00777	0.00179	-0.00470	0.00328	-0.00962	-0.0234	-0.0176	-0.0110	-0.0160	-0.00374	-0.0289
	(0.0225)	(0.0242)	(0.0259)	(0.0231)	(0.0289)	(0.0248)	(0.0171)	(0.0174)	(0.0190)	(0.0176)	(0.0187)	(0.0174)
One Quarter Before Adoption of Texting Ban			0.0718**						0.0352*			
			(0.0290)						(0.0185)			
Handheld Ban	-0.0213	-0.00382	-0.00947	0.00771	-0.0735**	-0.00133	-0.0373*	-0.0174	-0.0257	0.0174	-0.0415*	-0.0113
	(0.0338)	(0.0291)	(0.0315)	(0.0354)	(0.0338)	(0.0337)	(0.0190)	(0.0189)	(0.0201)	(0.0330)	(0.0237)	(0.0249)
One Quarter Before Adoption of Handheld Ban			-0.0159						-0.0337			
			(0.0369)						(0.0226)			
Panel B. Casualty Rate												
	log(Casualty Rate)						log(Casualty Rate)					
Texting Ban	0.00806	0.00911	0.0152	0.0107	0.0162	0.00722	-0.0163	-0.00773	-0.00834	-0.00391	0.0164	-0.0220
	(0.0263)	(0.0262)	(0.0278)	(0.0239)	(0.0323)	(0.0269)	(0.0233)	(0.0217)	(0.0229)	(0.0201)	(0.0224)	(0.0219)
One Quarter Before Adoption of Texting Ban			0.0454						-0.00318			
			(0.0301)						(0.0219)			
Handheld Ban	-0.0370	-0.0159	-0.0172	-0.00634	-0.0996**	-0.0130	-0.0614**	-0.0328	-0.0330	-0.00906	-0.0683***	-0.0234
	(0.0438)	(0.0359)	(0.0367)	(0.0430)	(0.0426)	(0.0431)	(0.0237)	(0.0227)	(0.0235)	(0.0343)	(0.0253)	(0.0346)
One Quarter Before Adoption of Handheld Ban			0.0165						-0.00682			
			(0.0322)						(0.0220)			
Observations	1400	1400	1400	1400	1400	1372	1400	1400	1400	1400	1400	1372
State and Year-Quarter Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time-Varying Controls		Yes	Yes	Yes	Yes	Yes		Yes	Yes	Yes	Yes	Yes
Region-Year-Quarter Fixed Effects				Yes						Yes		
State Linear Trends					Yes						Yes	
Excludes California						Yes						Yes

Notes: The table reports panel data estimates. Each column in each panel represents a separate regression. The unit of observation is state-year-quarter. Weighted OLS uses state population to reweight the sample. Standard errors are clustered at the state level. Time-varying controls include unemployment rate, demographics, seat belt law dummy, state median income, violent and property crime rates, government spending on highway and public welfare, police, and population.

* Significant at the 10% level

** Significant at the 5% level

*** Significant at the 1% level

Table 5. Differential Effects of Cell Phone Bans by Driver Types

	1	2	3	4
Panel A. Differential Effects by the Presence of Passengers on Cell Phone Use				
	Handheld Cellphone Usage (Talking on Handheld Cellphones)	% Change compared to Rate of Handheld Cellphone Use One Year prior to Adoption of Cellphone Bans	Handheld Device Manipulation (Sending Text Messages, etc.)	% Change compared to Rate of HandheldDevice Manipulation One Year prior to Adoption of Cellphone Bans
Texting Ban x Driver with at least One Passenger	-0.0205***	-74%	-0.0171***	-155%
	(0.00697)		(0.00402)	
Texting Ban x Driver without Passenger	-0.00132	-1%	-0.0129***	-43%
	(0.00990)		(0.00363)	
Handheld Ban x Driver with at least One Passenger	-0.0329***	-107%	0.0104**	149%
	(0.00658)		(0.00431)	
Handheld Ban x Driver without Passenger	-0.0256***	-30%	0.0133***	83%
	(0.00844)		(0.00365)	
Observations	331883	-	2935666	-
Panel B. Differential Effects by Driver Age on Cellphone Use				
	Handheld Cellphone Usage (Talking on Handheld Cellphones)	% Change compared to Rate of Handheld Cellphone Use One Year prior to Adoption of Cellphone Bans	Handheld Device Manipulation (Sending Text Messages, etc.)	% Change compared to Rate of HandheldDevice Manipulation One Year prior to Adoption of Cellphone Bans
Texting Ban x Young Driver (16<Driver Age<24)	-0.00343	-3%	-0.00660	-14%
	(0.0100)		(0.00496)	
Texting Ban x Adult Driver (Driver Age>25)	-0.00725	-12%	-0.0152***	-75%
	(0.00783)		(0.00363)	
Handheld Ban x Young Driver (16<Driver Age<24)	-0.0463***	-45%	0.0119***	46%
	(0.00827)		(0.00365)	
Handheld Ban x Adult Driver (Driver Age>25)	-0.0254***	-40%	0.0120***	109%
	(0.00736)		(0.00384)	
Observations	331883	-	2935666	-
Panel C. Differential Effects on Accidents				
	log(Accident Rate) (Drivers with at least One Passenger)	log(Accident Rate) (Drivers without Passenger)	log(Accident Rate) (16<Driver Age<24)	log(Accident Rate) (Driver Age>25)
Texting Ban	0.0118	-0.0159	-0.0221	-0.00132
	(0.0268)	(0.0264)	(0.0397)	(0.0272)
Handheld Ban	-0.0122	-0.0249	0.00442	-0.0285
	(0.0384)	(0.0326)	(0.0288)	(0.0330)
Observations	1400	1400	1400	1400
State and Year/Year-Quarter Fixed Effects	Yes	Yes	Yes	Yes
Time-Varying Controls	Yes	Yes	Yes	Yes

Notes: The table reports unweighted panel data estimates. Standard errors are clustered at the state level. Time-varying controls for Panels A and B include indicators for gender, age group, race, rural and urban areas, weekday non-rush hour, weekend, and clear weather. Time-varying controls for Panel C include unemployment rate, demographics, state median income, violent and property crime rates, government spending on highway and public welfare, police, and population.

* Significant at the 10% level

** Significant at the 5% level

*** Significant at the 1% level

Table 6. Additional Tests: The Effect of Cell Phone Bans on VMT, Seat Belt Usage and Headset Usage

	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18
	log(VMT)						Seat Belt Use						Headset Use					
Texting Ban	-0.00995	-0.00647	-0.00577	-0.00439	-0.00365	-0.00225	0.0102	0.0133	0.0215	0.0168	0.00114	0.0146	0.00317	0.00300	0.00402	0.00204	0.00175	0.00291
	(0.00830)	(0.00663)	(0.00673)	(0.00631)	(0.00791)	(0.00824)	(0.0138)	(0.0135)	(0.0156)	(0.0132)	(0.0220)	(0.0118)	(0.00345)	(0.00352)	(0.00380)	(0.00384)	(0.00521)	(0.00383)
One Quarter Before Adoption of Texting Ban			0.00518						0.0252						0.00431			
			(0.0143)						(0.0160)						(0.00261)			
Handheld Ban	-0.00354	0.00350	0.00405	0.00444	-0.00424	-0.00109	-0.00892	-0.0114	-0.0126	-0.00782	-0.0179	-0.00466	0.000337	0.000490	0.00355	0.00214	-0.00587	-0.000317
	(0.0110)	(0.00949)	(0.0113)	(0.0100)	(0.0111)	(0.0122)	(0.0189)	(0.0181)	(0.0196)	(0.0202)	(0.0272)	(0.0278)	(0.00325)	(0.00331)	(0.00396)	(0.00258)	(0.00603)	(0.00364)
One Quarter Before Adoption of Handheld Ban			0.0105						-0.00859						0.00655			
			(0.0250)						(0.0174)						(0.00434)			
Observations	331883	331883	331883	331883	331883	286564	1428	1428	1428	1428	1428	1400	331883	331883	331883	331883	331883	286564
State and Year/Year-Quarter Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time-Varying Controls		Yes	Yes	Yes	Yes	Yes		Yes	Yes	Yes	Yes	Yes		Yes	Yes	Yes	Yes	Yes
Region-Year/Region-Year-Quarter Fixed Effects				Yes						Yes						Yes		
State Linear Trends					Yes						Yes						Yes	
Excludes California						Yes						Yes						Yes

Notes: The table reports unweighted panel data estimates. Standard errors are clustered at the state level. Time-varying controls include unemployment rate, demographics, seat belt law dummy, state median income, violent and property crime rates, government spending on highway and public welfare, police, and population.

* Significant at the 10% level

** Significant at the 5% level

*** Significant at the 1% level

Table 7. Additional Robustness Checks

	1	2	3	4	5
	Handheld Cellphone Usage	Handheld Device Manipulation	Headset Usage	Log (Accident Rate)	Log (Casualty Rate)
Panel A. Excluding States with Cellphone Bans Enforcement Upgrade					
Texting Ban	-0.00722	-0.0142***	0.00348	-0.00579	0.0130
	(0.00808)	(0.00364)	(0.00354)	(0.0252)	(0.0270)
Handheld Ban	-0.0290***	0.0121***	0.00151	0.0256	0.00166
	(0.00738)	(0.00404)	(0.00312)	(0.0385)	(0.0559)
Observations	314133	277208	314133	1344	1344
Panel B. Excluding States with Handheld Bans					
Texting Ban	-0.00505	-0.0152***	0.00304	0.00555	0.0281
	(0.00909)	(0.00404)	(0.00431)	(0.0269)	(0.0292)
Observations	242025	211844	242025	1232	1232
Panel C. Defining Cellphone Ban Dummies as Fractions					
Texting Ban	-	-	-	-0.00990	0.00652
	-	-	-	(0.0249)	(0.0270)
Handheld Ban	-	-	-	-0.000215	-0.0148
	-	-	-	(0.0290)	(0.0360)
Observations	-	-	-	1400	1400
State and Year/Year-Quarter Fixed Effects	Yes	Yes	Yes	Yes	Yes
Time-Varying Controls	Yes	Yes	Yes	Yes	Yes

Notes: The table reports unweighted panel data estimates. Each column in each panel represents a separate regression. Standard errors are clustered at the state level. Time-varying controls for Column 1 through Column 3 include indicators for gender, age group, race, rural and urban areas, weekday non-rush hour, weekend, and clear weather. Time-varying controls for Column 4 through Column 5 include unemployment rate, demographics, seat belt law dummy, state median income, violent and property crime rates, government spending on highway and public welfare, police, and population.

* Significant at the 10% level

** Significant at the 5% level

*** Significant at the 1% level

Appendix

Table A1. Effects of Cell Phone Bans on Fatalities and Injuries

	1	2	3	4	5	6	7	8	9	10	11	12
	Unweighted OLS						Weighted OLS					
Panel A. Fatality Rate												
	log(Fatality Rate)						log(Fatality Rate)					
Texting Ban	0.0124	0.00974	0.0175	0.00781	0.0205	0.00882	-0.0229	-0.0241	-0.0223	-0.0270	-0.00158	-0.0110
	(0.0232)	(0.0236)	(0.0251)	(0.0209)	(0.0293)	(0.0242)	(0.0289)	(0.0255)	(0.0275)	(0.0215)	(0.0204)	(0.0213)
One Quarter Before Adoption of Texting Ban			0.0580**						0.0113			
			(0.0237)						(0.0183)			
Handheld Ban	-0.0355	-0.0181	-0.0225	-0.00851	-0.0971**	-0.0110	-0.0765***	-0.0403	-0.0410	-0.00474	-0.0684*	-0.0775
	(0.0431)	(0.0364)	(0.0373)	(0.0436)	(0.0415)	(0.0429)	(0.0260)	(0.0253)	(0.0268)	(0.0405)	(0.0386)	(0.0527)
One Quarter Before Adoption of Handheld Ban			-0.0113						0.00542			
			(0.0277)						(0.0246)			
Panel C. Injury Rate												
	log(Injury Rate)						log(Injury Rate)					
Texting Ban	-0.0487	-0.0419	-0.0467	-0.0347	-0.0309	-0.0494	0.00954	-0.0184	-0.0279	-0.0157	0.00301	-0.00254
	(0.0537)	(0.0603)	(0.0621)	(0.0660)	(0.0607)	(0.0618)	(0.0484)	(0.0537)	(0.0567)	(0.0483)	(0.0550)	(0.0594)
One Quarter Before Adoption of Texting Ban			-0.0365						-0.0588			
			(0.0830)						(0.0718)			
Handheld Ban	-0.0693	-0.0339	-0.0219	-0.0321	-0.0709	-0.0496	-0.120*	-0.0190	-0.0143	0.0171	0.0148	0.0491
	(0.0484)	(0.0470)	(0.0503)	(0.0685)	(0.0767)	(0.0490)	(0.0657)	(0.0572)	(0.0561)	(0.0584)	(0.106)	(0.161)
One Quarter Before Adoption of Handheld Ban			0.118						-0.0218			
			(0.0786)						(0.0644)			
Observations	1428	1428	1428	1428	1428	1400	1428	1428	1428	1428	1428	1400
State and Year-Quarter Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time-Varying Controls		Yes	Yes	Yes	Yes	Yes		Yes	Yes	Yes	Yes	Yes
Region-Year-Quarter Fixed Effects				Yes						Yes		
State Linear Trends					Yes						Yes	
Excludes California						Yes						Yes

Notes: The table reports panel data estimates. Each column in each panel represents a separate regression. The unit of observation is state-year-quarter. Weighted OLS uses state population to reweight the sample. Standard errors are clustered at the state level. Time-varying controls include unemployment rate, demographics, seat belt law dummy, state median income, violent and property crime rates, government spending on highway and public welfare, police, and population.

* Significant at the 10% level

** Significant at the 5% level

*** Significant at the 1% level

Table A2. The Effect of Cellphone Bans on Traffic Accidents and Casualties (Count Data Model)

	1	2	3	4	5	6	7	8	9	10	11	12
	Unweighted OLS						Weighted OLS					
Panel A. Accident Count												
	log(Accident Count)						log(Accident Count)					
Texting Ban	-0.0148	-0.0142	-0.00398	-0.00910	-0.00217	-0.0150	-0.0302*	-0.0231	-0.0142	-0.0195	-0.0125	-0.0262
	(0.0221)	(0.0239)	(0.0260)	(0.0229)	(0.0287)	(0.0245)	(0.0159)	(0.0157)	(0.0176)	(0.0164)	(0.0171)	(0.0182)
One Quarter Before Adoption of Texting Ban			0.0769**						0.0472**			
			(0.0362)						(0.0185)			
Handheld Ban	-0.0249	-0.000314	-0.00541	0.0122	-0.0781***	0.00503	-0.0478*	-0.0142	-0.0228	0.0206	-0.0496**	0.000374
	(0.0340)	(0.0288)	(0.0313)	(0.0322)	(0.0278)	(0.0324)	(0.0245)	(0.0195)	(0.0219)	(0.0300)	(0.0211)	(0.0230)
One Quarter Before Adoption of Handheld Ban			-0.00543						-0.0187			
			(0.0277)						(0.0155)			
Panel B. Casualty Count												
	log(Casualty Count)						log(Casualty Count)					
Texting Ban	-0.00189	0.00264	0.00940	0.00632	0.0107	0.00188	-0.0232	-0.0132	-0.0116	-0.00740	0.00759	-0.0193
	(0.0252)	(0.0252)	(0.0271)	(0.0230)	(0.0318)	(0.0260)	(0.0214)	(0.0190)	(0.0212)	(0.0185)	(0.0203)	(0.0219)
One Quarter Before Adoption of Texting Ban			0.0506						0.00877			
			(0.0387)						(0.0211)			
Handheld Ban	-0.0405	-0.0124	-0.0132	-0.00190	-0.104**	-0.00664	-0.0719**	-0.0296	-0.0301	-0.00588	-0.0764***	-0.0117
	(0.0444)	(0.0340)	(0.0349)	(0.0379)	(0.0406)	(0.0403)	(0.0295)	(0.0220)	(0.0236)	(0.0302)	(0.0253)	(0.0292)
One Quarter Before Adoption of Handheld Ban			0.0270						0.00822			
			(0.0189)						(0.0171)			
Observations	1400	1400	1400	1400	1400	1372	1400	1400	1400	1400	1400	1372
State and Year-Quarter Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time-Varying Controls		Yes	Yes	Yes	Yes	Yes		Yes	Yes	Yes	Yes	Yes
Region-Year-Quarter Fixed Effects				Yes						Yes		
State Linear Trends					Yes						Yes	
Excludes California						Yes						Yes

Notes: The table reports panel data estimates. Each column in each panel represents a separate regression. The unit of observation is state-year-quarter. Weighted OLS uses state population to reweight the sample. Standard errors are clustered at the state level. Time-varying controls include unemployment rate, demographics, seat belt law dummy, state median income, violent and property crime rates, government spending on highway and public welfare, police, and population.

* Significant at the 10% level

** Significant at the 5% level

*** Significant at the 1% level