Cell Phone Laws and Rear-End Accidents

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Cell Phone Laws and Rear-End Accidents in California

Abstract

In an attempt to reduce accident frequency caused by distraction involving cell phone use, the state of California enacted two sets of laws banning cell phone use while driving. We examine the relationship between laws to ban cell phone use while driving and rear-end accident frequency in California. Using a panel dataset that groups accidents by geographic location, we find evidence that the association between California's handheld cell phone ban and the number of rear-end accidents is negative and statistically significant. In robustness testing, we find this result is unaltered when we use a broader definition of collision accident. These results have implications for public safety and transportation, automobile insurance markets and insurance regulation.

Keywords. Legal rules, Automobile Insurance, Transportation, Distracted driving.

1. Introduction

Fifteen years ago driving distracted because of mobile phone use was a new risk created through innovation. The risk has matured and today is recognized as broadly impactful. Consequently, the potential for distraction caused by cell phone use is a major concern for legislators, public safety advocates and the insurance industry. The National Highway Traffic Safety Administration (NHTSA) estimates that 17 percent of accidents with injuries in 2011 were "distraction affected crashes."[1]

From the point-of-view of insurance regulation distracted driving touches upon potential product regulation and market regulation.[2] The efficacy of state laws to limit distracted driving while using a cellphone and a better understanding of distraction effects are key because driving distracted remains a liability for insurers in their contractual obligations to their clients and third-party claimants. If bans on handheld use prove effective, then risks borne by insurers will be less important. If, on the other hand, bans are ineffective, then insurers' loss ratios may become higher and/or more variable which could lead to some insurers pushing for alternative contract designs in their auto insurance policy forms. Thus, regulations focused on safety and the subsequent outcomes of such regulation can become informative to insurance regulators who must weigh simultaneous effects on insurance consumers and their insurers.

Using a cell phone while driving introduces an externality: the reduction in safety of innocent third parties. Previous studies have estimated that cell phone use while driving may cause between 10 and 1,000 fatalities per year in the U.S. and many more non-fatal accidents.[3] Klauer et al (2014) found that among both young and experienced drivers, using a cell phone while driving increased the risk of

[1]http://www.distraction.gov/download/811737.pdf

[2]These are two of the six purposes for insurance regulation articulated by the NAIC, http://www.naic.org/documents/consumer_state_reg_brief.pdf

[3]Hahn, Tetlock, and Burnett (2000) calculated a range of 10 to 10,000 fatalities with a best estimate of 300 deaths per year.

a crash or near-crash. The heightened risk will eventually trickle through to insurance pricing as long as insurers continue to embrace first-party and third-party coverage for distracted driving incidents.

Concern that cell phone use distracts drivers and increases the probability of accidents has led advocates for traffic safety to demand bans on cell phone use while driving. Regulation of cell phones has become a major policy issue and many states have passed legislation to address distracted driving due to cell phone use. Using a handheld cell phone while driving is banned in fourteen states.[4]

In addition, all cell phone use (handheld and hands-free) is restricted for novice drivers in 37 states and the District of Columbia. The definition of novice drivers varies by state. Most states classify novice drivers as drivers younger than 18, while other states designate novice drivers as learners permit and intermediate license holders. For example, novice drivers in Alabama are 16-year-old drivers and 17-year-old drivers who have held an intermediate license for fewer than six months, and novice drivers in Illinois are drivers younger than 19. All cell phone use while driving a school bus is prohibited in 19 states and the District of Columbia.[5] Text messaging while driving is also banned in 44 states and the District of Columbia. Novice drivers are prohibited from text messaging in 4 states (Mississippi, Missouri, Oklahoma, and Texas).[6]

Most relevant to this research is the California experience where on July 1, 2008 a ban on handheld cell phone use while driving, hereafter referred to as "handheld ban.," was put into effect.[7] The handheld ban is made of up two laws; the first law prohibits all drivers from using a handheld cell phone while operating a motor vehicle (California Vehicle Code [VC] §23123), and the second law prohibits drivers under the age of 18 from using a handheld cell phone or a hands-free device while operating a motor vehicle (VC §23124). California also enacted a ban on text messaging (hereafter

[4]As of July 2015. See http://www.iihs.org/iihs/topics/laws/cellphonelaws?topicName=distracted-driving
[5]"Hands-free" refers to a phone that does not require the user to hold it during operation.
[6]*Insurance Institute for Highway Safety*, <http://www.iihs.org/laws/maphandheldcellbans.aspx>.
[7]Note that hands-free devices are still allowed.

referred to as "texting ban") that took effect on January 1, 2009, which prohibits all drivers from writing, sending, or reading text-based communication on an electronic wireless communications device while driving a motor vehicle (VC §23123.5).[8] The handheld and texting laws are enforced as primary enforcement laws, which allow law enforcement officers to stop drivers solely for talking on a handheld phone or texting. However, the use of hands-free devices by drivers under the age of 18 is enforced as secondary enforcement, which requires an officer to have some other reason for making a traffic stop.[9]

Of particular interest to policy makers and economists are the costs and benefits of handheld cell phone bans. As we discuss, there is conflicting evidence about the association between cell phone use, distracted driving and automobile accidents. This paper stems from significant new research by Burger, Kaffine and Yu (BKY, 2014) who find no significant decline in accidents around the time of California's change.[10] We extend BKY's work using California accident data by initially focusing on what we believe to be a better set of accidents to investigate this question. We apply a finding by Neyens and Boyle (2007), who showed that cell phone distractions resulted in a higher likelihood of rear-end accidents, and we are able to expand the state highway accident data used by BKY to include accidents off of state highways. We find and report a strong relationship between the introduction of the handheld ban and a decline in monthly rear-end accidents that is robust across a variety of empirical specifications.

The direction of the paper proceeds as follows. In the next section of the paper we present a review of previous studies related to cell phone use and automobile incidents. Then, the California

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[8]California drivers over the age of 18 may dictate, send, or listen to text-based messages if they are using voice-activated, hands-free devices.

[9]*California Vehicle Code* [VC], http://www.dmv.ca.gov/pubs/vctop/d11/vc23123.htm>.

[10]In a 2012 white paper, the National Safety Council asserts that distraction still exists in hands-free environments because of the inability of the brain to effectively multi-task when a driver wishes to simultaneously hold a phone conversation. See http://www.nsc.org/DistractedDrivingDocuments/Cognitive-Distraction-White-Paper.pdf

accident data used in the study and other control variables are introduced and analyzed. We find a negative relationship between the ban on handheld use and accident count, and extend the analysis by three robustness checks. We conclude with a discussion of how the study results may be interpreted by policymakers and we offer thoughts for future research.

2. Background

Previous studies that have analyzed data involving cell phone and accident frequency have produced conflicting results. Many studies conclude that the use of cell phones while driving increases the probability of accidents. Goodman, Tijerina, Bents, and Wierwille (1999) conclude that there is sufficient evidence to claim that using a cell phone while driving increases the risk of being involved in an accident. Various factors may contribute to the increased frequency of collisions, including dialing, answering, and talking on the phone, along with sending and receiving text messages. These activities divert driver's attention away from the driving task and reduce the amount of attention capacity available for processing and dealing with potential hazards that may occur; therefore, the likelihood of the driver being involved in an accident increases (Goodman et al., 1999). A driver's cognitive load is increased whether he or she uses handheld or hands-free equipment.

Strayer and Johnston (2001) supported this notion with their dual-task studies to assess the effects of cell phone conversations on performance of a simulated driving task. Their research suggests that cell phone use disrupts performance by diverting attention to an engaging cognitive context other than the one immediately associated with driving. Their data implies that legislative initiatives that restrict handheld devices but permit hands-free devices are not likely to reduce cognitive interference from the phone conversation, because the interference is due to central attention processes.

Several studies attempt to find a statistical relationship between cell phone use and accidents using individual-level data. Violanti and Marshall (1996), Violanti (1998), Redelmeier and Tibshirani (1997), and Dryer, Loughlin, and Rothman (1999) conclude that using a cell phone while driving increases the risk of being involved in an accident.

Violanti and Marshall (1996) used an epidemiological case-control design and logistic regression techniques to examine the association of cell phone use in motor vehicles and traffic accident risk. Their data were obtained from a case group of 100 randomly selected drivers involved in accidents within the past 2 years and a control group of 100 randomly selected licensed drivers not involved in accidents within the past 10 years. Violanti and Marshall examined the amount of time per month spent talking on a cellular phone and 18 other driver inattention factors. They found that increased use of a cell phone while driving was associated with increased risk of being in a traffic accident. Although the study did consist of a small sample size, the results indicated that talking more than 50 minutes per month on a cell phone in a vehicle was associated with a 5.59-fold increased risk of a traffic accident. In addition, Violanti found in 1998 that cell phones increase the risk of certain accident characteristics in fatal collisions more than those same characteristics in non-fatal collisions. A case-control study found an approximate nine-fold increased risk for a fatality given the use of a cell phone while driving. In addition, a two-fold increased risk for a fatality was found given the mere presence of a cell phone in the vehicle.

Redelmeier and Tibshirani (1997) further explored these findings by using the case-crossover design to study whether using a cell phone while driving increased the risk of a motor vehicle collision. The case-crossover method is used in medical literature to study determinants of rare events, which are accidents in this case. Redelmeier and Tibshirani found that the risk of a collision when using a cell phone was approximately 4 times higher than the risk when no cell phone was used. These results are the most highly cited in policy discussions about banning cell phone use while driving.[11] However, the field studies of Redelmeier and Tibshirani do not necessarily imply that the use of cell phones causes an increase in accident rates. People who use their cell phones while driving may be more likely to drive erratically or to engage in risky driving behavior, such as speeding. This increased risk-taking may underlie the correlation, which could be a limitation of this field study. Based on Redelmeier and Tibshirani (1997), Redelmeier and Weinstein (1999) estimated a 2 percent reduction in accidents from a cell phone ban, and Cohen and Graham (2003) found a 2 to 21 percent reduction in accidents with a central estimate of a 6 percent reduction in accidents.

Supporting the association between cell phones and collisions, Dreyer, Loughlin, and Rothman (1999) found an association between cell phone use and death from automobile collisions. In their research on health risks of cellular telephones, Dreyer et al found evidence that the heaviest users of cell phones had more than double the mortality from collision incidents than the lightest users. Interestingly, the effect of cell phone use on mortality was not as strong for longer-term users, which could suggest a greater degree of caution or perhaps a learning effect.

In contrast to these studies confirming a relationship between cell phone use and accident frequency, economists Hahn and Prieger (2006, 2007) report no significant effect of handheld or hands-free cell phone use on accidents based on survey data from over 7,000 individuals. Hahn and Prieger used a large sample of individual-level data and tested for selection effects, such as whether drivers who use cell phones are inherently less safe drivers, even when not talking on the phone. They found that the impact of cell phone use on accidents varies across individuals; therefore, previous studies, based on accident-only samples, may be overstated by 36 percent. After controlling for selection bias and various risks across drivers, such as the statistically significant difference in cell

[11] Hahn and Preiger (2006).

phone effects between men and women in the sample, they found no significant effect of hands-free or handheld cell phone use on accidents.

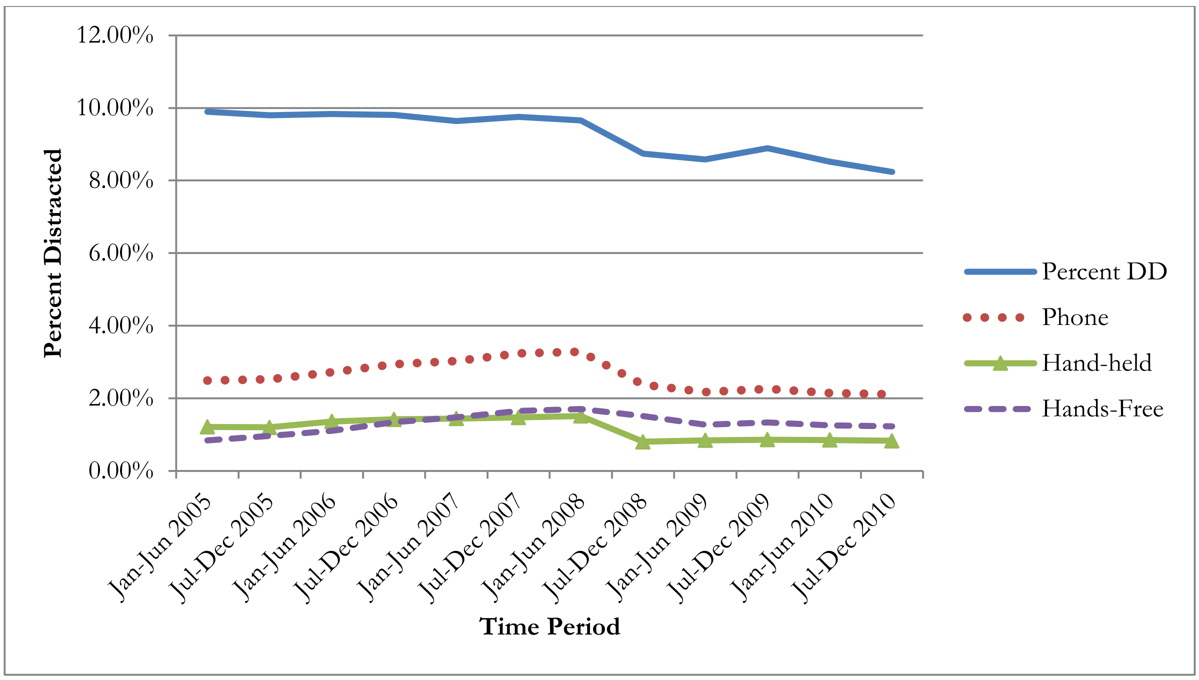
Most closely related to the research in this paper is the work of Burger, Kaffine and Yu (2014). Burger et al found no evidence that the ban on handheld cell phone use in California led to a reduction in traffic accidents. They tested whether the handheld cell phone ban in California reduced the number of accidents generally but they did not consider accident type. Their analysis was based on daily, high-frequency vehicle accident data around the time the cell phone ban was enacted, making their analysis distinct from other studies. Their data set included over 500,000 incidents as reported by the California Highway Patrol to the Performance Management System (PeMS). PeMS collects, manages and publishes data on California highway conditions. Using total daily accidents regardless of accident type on major freeways and highways in nine of California's traffic districts, they concluded that there is no evidence to support that the handheld cell phone ban reduced accidents in California. While Burger et al provide evidence that the ban failed to reduce accidents, the researchers were unable to discover whether compliance with the law was associated with a reduction in accidents. That is, they were unable to explain the ban's lack of effect on accident frequency by any one of the following possibilities: that cell phone use does not impact accidents, that drivers used other disruptive hands-free or text-based technologies, or that weak compliance failed to reduce cell phone use overall.

By contrast, a Berkeley study found an overall reduction in percentages of fatalities and non-fatal injuries related to handheld cell phone use after implementation of California's handheld cell phone ban.[12] Using data from the Statewide Integrated Traffic Records System (SWITRS) from January 2005 to December 2010, the study analyzed the number of fatalities and injuries overall for distracted driving, cell phone use, handheld cell phone use, and hands-free cell phone use. The

[12]See Ragland et al (2012).

percentages of non-fatal injuries due to overall distracted driving, cell phone use, handheld cell phone use, and hands-free cell phone use for the period of January 2005 to December 2010 are shown in Figure 1. Two-year periods before and after the implementation of the handheld cell phone ban were compared to provide before and after collision patterns. The results of the analysis show a consistent reduction in fatalities and injuries related to hand-held cell phone use after the enactment of the handheld cell

Figure 1: Percentage of Injuries by Distracted Driver Category



phone ban in California on July 1, 2008. While Burger et al had a rigorous analytical approach, the Cal-Berkeley study conducted a brief descriptive analysis of fatalities and injuries before and after the implementation of the handheld cell phone ban.

Several studies have focused on the relationship between cell phone use and collisions for specific age groups. Using a survey sample of 1,185 college students, Seo and Torabi (2004) examined the association between in-vehicle cell phone use on accidents or near accidents. Seo and Torabi found that the most frequently cited reason for drivers' accidents or near-accidents involving cell phones was actually talking while driving, not dialing or answering the phone. This is consistent with Strayer and Johnson (2001).

Neyens and Boyle (2007) determined how different distraction factors impacted the crash types that are common among teenage drivers specifically. They developed a model to predict the likelihood that a driver will be involved in one of three common crash types: an angular collision with a moving vehicle, a rear-end collision with a moving lead vehicle, and a collision with a fixed object. Their results showed that cell phone distractions resulted in a higher likelihood of rear-end collision. We utilize this finding in the next section and broaden the analysis to examine the relationship between cell phone use and all rear-end collisions in the state of California.

3. Analysis

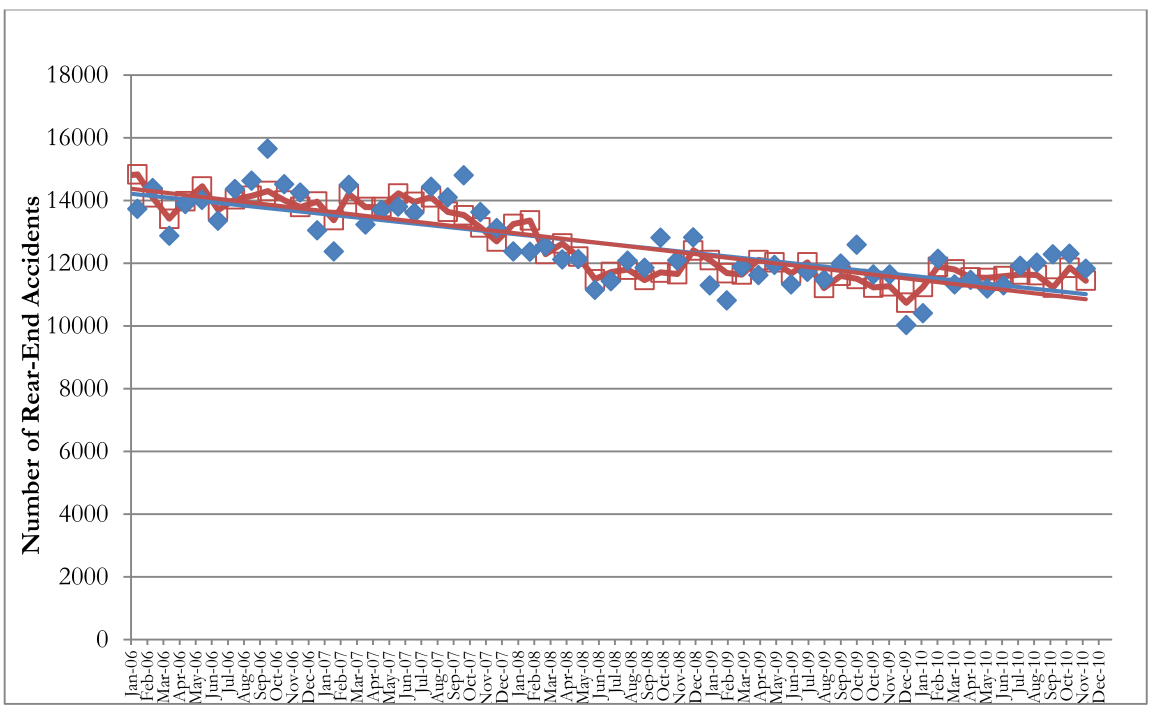
The accident data we use to test and evaluate the relationship between handheld and texting bans and the number of rear-end collisions comes from the California Statewide Integrated Traffic Records System (SWITRS). The SWITRS database houses collision scene data developed through the California Highway Patrol (CHP). Two separate laws that regulate cell phone use while driving in California are germane to the analysis. The handheld ban (VC §23123) took effect July 1, 2008, and it prohibits all drivers from using a handheld cell phone while operating a motor vehicle, but drivers 18 and over are allowed to use a hands-free device. The separate texting regulation (VC 23123.5) took effect January 1, 2009, and it prohibits all drivers from texting while driving.[13] The dates chosen for this study encompass five years, from January 1, 2006 to December 31, 2010, with two years and six months on either side of the state's handheld cell phone ban. This period was chosen because it is

[13]<http://dmv.ca.gov/cellularphonelaws/>. There were no grace periods for enforcement after the laws went into effect July 1, 2008 and January 1, 2009, respectively.

symmetric on either side of the handheld ban (July 1, 2008) while also providing a window on either side of the additional texting ban (January 1, 2009).[14]

The raw data set for each of the five years contained over 500,000 collisions. The observations contain detailed information about each collision, including the location of the incident, the date and time the accident occurred, the collision severity, and the type of collision. Each collision can be classified into one of the following categories according to type of collision: head-on; sideswipe; rear-end; broadside; hit object; overturned; vehicle/pedestrian; other; or not stated. We segregated the data into 60 months from January 2006 to December 2010 and focused on rear-end accidents given the Neyens and Boyle evidence on cell phone distractions and rear-end accidents. This permits us to better isolate the efficacy of any legal change to distracted driving.

Figure 2: Number of Rear-End Collisions by Month



[14]The raw data are available in three separate files: Collision Data File, Party Data File and Victim Data File. All three files are linked together with the same case identification number of each collision. Victim data are linked to the collision file by the case identification number and to their respective party by the party number.

An initial look at statewide data is displayed in Figure 2 with the raw data noted as diamonds and the seasonality considered in the squares connected by a solid line. A general negative trend in accident count over time is apparent in Figure 2. Moreover, if one follows the actual monthly accidents over time there also appears to be a seasonal effect.[15] Periodically, the months of August through December appear to have a larger than average number of collisions with a peak high in the month of October. The months of January and February have a lower than average number of collisions.[16]

3.1 The Hypotheses and the Empirical Model

Our primary interest is to document how a change in law has been associated with accidents. The null and alternative hypotheses for the initial test are as follows:

> H_0: There has been no change in rear-end collisions in California after the ban on handheld cell phone use became effective on July 1, 2008.
>
> H_1: There has been a change in rear-end collisions in California after the ban on handheld cell phone use became effective on July 1, 2008.

In building an empirical model we undertook a series of preliminary steps. First, we initially began by looking at monthly accident data at the "state-level," which permitted us to utilize more accessible control variables related to information about gas prices, new vehicle registrations and fuel consumption thought to be related to accident frequency. However, observation at the state level has the obvious severe limitation of omitting more specific geographic location, important to accident frequency, which needed to be addressed. Second, we elected to focus only on the handheld ban

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[15]We looked at the average number of rear-end collisions from January 2006 through June 2008 (approximately 13,552) and compared them to the average number of rear-end collisions from July 1, 2008 to December 2010 (approximately 11,708). Utilizing a chi-square test, there is sufficient evidence at the $\alpha = 0.05$ level to infer there has been a change in rear-end collisions in California after the ban on handheld cell phone use became effective on July 1, 2008. Of course, the result is also consistent with the general negative trend in rear-end collisions regardless of changes in the law.

[16]The outlier months could be attributable to a tourism effect. Perhaps there are more visitors to the state and therefore more cars on the road during the August through December time frame than during January and February. More cars on the road could be associated with a higher number of rear-end collisions, and a tourism effect would suggest that these drivers may be unfamiliar with the roads and, therefore, more likely to be involved in an accident.

because across our overall time interval there were only six months were the handheld ban was in effect and the texting ban was not. However, we view that of minor consequence since we are most interested in general distracted driving effects.[17] Third, consideration of intra-state accident location and our ultimate treatment of the California data provide an important and direct contrast of our findings to the contribution of Burger et al. These researchers considered accident location when the underlying data recorded the California Department of Transportation (CalTrans) district venue of the incident. However, many data points were eliminated from their study because only accidents that occur on state highways have a recorded and associated CalTrans district in the SWITRS data system. For instance, in 2010, the number of accidents available for analysis would decline by 63 percent if the data were limited to only those accidents with CalTrans information, and this has the potential of severely biasing any analysis that explores texting behavior and auto accident outcomes when more localized and urban start and stop driving behaviors are excluded.

To retain as many accident data points as possible we constructed a panel data set with the SWITRS information that would account for location by using the county and city information in the accident record or the traffic district information. When an accident had county and city information, we assigned the accident to its specific traffic district in order to merge these accidents with those that had traffic district information. The traffic districts we considered are represented in Table 1 along with the associated major MSA in the district,

[17]Estimations that included a texting ban variable were undertaken with the empirical model offered in the forthcoming section. The texting ban variable was statistically insignificant. Results are available from the authors upon request.

Table 1

Major MSA in CalTrans District	CalTrans District
Redding MSA	2
Sacramento-Arden Arcade-Roseville MSA	3
San Francisco-Oakland-Fremont MSA	4
Salinas MSA	5
Fresno MSA	6
Los Angeles-Long Beach-Santa Ana MSA	7
Riverside-San Bernardino-Ontario MSA	8
Stockton MSA	10
San Diego-Carlsbad-San Marcos MSA	11

Among these CalTrans districts we identified the major metropolitan statistical area (MSA) within each district and used the MSA definition to assist our gathering of values on exogenous factors in our model. While there are twelve (12) CalTrans district in the California system, we found that districts 1 and 9 had very few losses and CalTrans district 12 did not match well to the Census Bureau's MSA definition.[18] The resulting data set comprises 60 monthly observations across the remaining 9 geographic locations over the time period January 2006 through December 2010. The handheld ban became effective in July 2008. Therefore, in our initial estimations we indicate this legal change by giving the handheld ban variable a value of 0 for each month in the time series through June 2008, then a value of 1 for each month from July 2008 through December 2010.

We follow a regression discontinuity design approach of Burger et al (2014) where a change in the law at a specific point in time can be viewed as a threshold, once crossed, that may impose a discontinuous change on monthly rear-end accidents over the time period of the study.

In our model, the relationship between monthly rear-end collisions is stated in (1) as

$$\ln(\text{rear-end collisions}_{i,t}) = \beta_0 + \beta_1 \cdot \text{handheld ban} + \beta_2 \cdot \ln(\text{gas price index}_{i,t}) + \beta_3 \cdot \text{rainfall}_{i,t} + \beta_4 \cdot \ln(\text{unemprate}_{i,t}) + \beta_5 \cdot \ln(\text{net working days}_{i,t}) + \sum_{j=6}^{16} \beta_j \cdot \text{dvmonth}_j + g(t) + \varepsilon_{i,t} \qquad (1)$$

[18]CalTrans district 1 is in far northwest California (Eureka) and district 9 is in east central California (Bishop).

where g(t) is represented as in Burger a polynomial trend that "accounts for unobserved factors that evolved smoothly over time."[19] We include $dvmonth_j$ to represent the first j months in a calendar year to control for any seasonal effects. When controlling for traffic district fixed effects and other factors, we expect the handheld legal change to be negatively related to monthly rear-end accidents consistent with the expectation that some proportion of drivers will follow the law, be less distracted, and fewer accidents will occur.[20] A statistically significant negative coefficient on handheld ban could be revealed by non-reporting if negligent drivers perceive that an additional penalty attributable to the cause of incident will be added to incidents where handheld use was a factor. Yet, any non-reporting would require that the injured party who was rear-ended would be willing to settle a claim without an insurer. That is less a problem for rear-end incidents where the attribution of fault is clear.

We include four additional exogenous factors that are expected to be related to monthly rear-end collisions: a gas price index, rainfall, the unemployment rate, and net working days. We view gas prices as a proxy for exposure to loss via traffic density on California roadways. Changes in gas prices are expected to be negatively related to marginal differences in traffic density and therefore in rear-end collisions. To control for gas prices by location, a retail gas price index was created for this study by the California Energy Commission. The gas price index for location i at time t is the average price in the MSA i during month t divided by the difference between the highest monthly average price observed across all MSAs and months from January 2006 through December 2010 and the lowest

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[19]For comparison purposes with Burger et al we adopt the same specification of the polynomial function and report results under linear, quadratic and cubic functional forms. As discussed by a referee an RD approach to a problem involving a legal change, such as a handheld ban, is imperfect because before and after groupings are likely to be "fuzzy." "A flexible enough polynomial could absorb the impact of the ban, leaving little for the discrete jump in the mean accident rate from the ban coefficient to pick up. This is an especially important consideration given that it is questionable how sharp the RD actually is here." In section 3.2, we explore different subsets of the data and different definitions for the handheld ban to provide additional perspectives on the initial findings.

[20]In preliminary testing, we did not find statistically significant marginal changes attributable to this legal change so consideration of a texting ban variable was omitted from the final model.

average monthly price across all cities and months in the same time frame. The index captures relative differences in gas prices during the time period of the study while being sensitive to geography. The rainfall data are taken from weather stations near each MSA area. The data are available on the website of the Western Regional Climate Center.[21] Higher amounts of monthly rainfall are expected to be related to higher counts of rear-end accidents to reflect higher physical hazard risk. The unemployment rate is considered as a reflection of economic activity and, consequently, transportation flows, which we would expect to be negatively related to rear-end accidents. Lower unemployment is expected to be related to a higher count of rear-end accidents because of increased traffic flows, particularly during rush hours. Finally, we create the variable, net working days, which captures the number of working days in a month that accounts for recognized U.S. holidays during the time frame of this study. More working days in a month imply higher monthly traffic frequency which is expected to be positively related to the number of rear-end accidents.[22] For the model estimations we take natural logs of these variables, except for rainfall, because of a high number of data points (139) with no rainfall.[23] Table 2 reports sample statistics of the variables by CalTrans district considering the entire time period of the study.

[21]Monthly precipitation totals for specific weather stations can be found at http://wrcc.dri.edu/climatedata/climsum/. The Riverside, CA MSA presented us with a problem. We used the Redlands weather station and found that two data points, March and August 2006 had missing data. We used the historical average for those months as our data points.

[22]Recognized U.S. Holidays were obtained from the public archive files of the Office of Personal Management (www.opm.gov). We first were made aware of this variable in a traffic estimation problem in Jeffrey Woolridge's "Introductory Econometrics" text.

[23]At the suggestion of a referee, we ran the model using the cube root of Rainfall to address potential skewness issues and found the results were not fundamentally different from those reported in Table 3.

Table 2: Descriptive Statistics

Cal Trans District	rear-end collisions	handheld ban	gas price index	rainfall	unemprate (%)	net working days
2						
mean	89.417	0.5	1.07076	2.403	10.898	21.633
std	15.522	0.5042	0.18412	2.851	3.812	1.207
min	59.00	0	0.639	0	5.30	19.00
max	129.00	1	1.60	9.98	17.50	24.00
3						
mean	946.217	0.5	1.05680	1.4087	8.1233	21.6333
std	108.834	0.5042	0.18047	1.7148	3.1839	1.2069
min	727.00	0	0.624	0	4.30	19
max	1,183.00	1	1.581	6.67	12.90	24.00
4						
mean	2356.000	0.5	1.08284	1.459	6.812	21.633
std	258.017	0.5042	0.18550	1.944	2.674	1.207
min	1,796.00	0	0.648	0	3.70	19.00
max	2,929.00	1	1.583	7.830	10.70	24
5						
mean	508.067	0.5	1.08368	1.054	9.455	21.633
std	67.240	0.5042	0.18040	1.309	3.530	1.207
min	329.00	0	0.658	0	4.50	19
max	666.00	1	1.599	5.02	17.70	24
6						
mean	600.700	0.5	1.07667	0.917	11.775	21.633
std	78.452	0.5042	0.18211	1.302	3.696	1.207
min	419.00	0	0.655	0	6.30	19
max	780.00	1	1.605	5.920	18.60	24
7						
mean	4159.633	0.5	1.06515	0.877	7.778	21.633
std	317.350	0.5042	0.18121	1.531	3.165	1.207
min	3,411.00	0	0.628	0	4.00	19
max	4,933.00	1	1.588	8.83	12.50	24
8						
mean	1407.133	0.5	1.06263	1.033	9.300	21.633
std	236.612	0.5042	0.18163	2.023	3.918	1.207
min	1,042.00	0	0.621	0	4.50	19
max	1,891.00	1	1.586	12.60	15.00	24
10						
mean	511.167	0.5	1.05079	1.025	11.683	21.633
std	75.178	0.5042	0.18165	1.261	4.022	1.207
min	310.00	0	0.605	0	6.30	19
max	688.00	1	1.579	5.25	18.30	24
11						
mean	751.033	0.5	1.06925	0.723	6.928	21.633
std	98.189	0.5042	0.18003	1.098	2.730	1.207
min	584.00	0	0.628	0	3.70	19
max	948.00	1	1.592	5.00	10.90	24

Table 3: Estimates
(n = 540; 9 traffic districts over 60 months)

Variable	Dependent Variable: ln(rear-end collisions)		
intercept	6.11*** (0.336)	6.189*** (0.0374)	6.163*** (0.3894)
handheld ban	-0.07094*** (0.01509)	-0.06669*** (0.01498)	-0.04753* (0.0213)
ln(gas price index)	-0.22251*** (0.2420)	-0.21552*** (0.02582)	-0.21351*** (0.0256)
rainfall	0.00873** (0.00285)	0.00727* (0.00321)	0.00704* (0.00326)
ln(unemprate)	-0.17551** (0.54512)	-0.18318** (0.05437)	-0.164406** (0.05847)
ln(net working days)	0.33908** (0.11717)	0.323488** (0.1244)	0.315072** (0.12241)
$month_6$	-0.1061*** (0.02133)	-0.10223*** (0.02305)	-0.09833*** (0.022601)
$month_7$	-0.0114 (0.0176)	-0.010206 (0.01786)	-0.010314 (0.01818)
$month_8$	-0.05744** (0.01927)	-0.05098 (0.02342)	-0.052216* (0.02427)
$month_9$	-0.11761*** (0.02667)	-0.11211*** (0.02701)	-0.110268*** (0.02717)
$month_{10}$	-0.15931*** (0.02387)	-0.15273*** (0.02652)	-0.150311*** (0.02685)
$month_{11}$	-0.06113 (0.03323)	-0.06043 (0.03313)	-0.061724 (0.034229)
$month_{12}$	-0.08811*** (0.01992)	-0.08660*** (0.02027)	-0.083159*** (0.019722)
$month_{13}$	-0.07975 (0.04022)	-0.07421 (0.04315)	-0.071519 (0.04316)
$month_{14}$	-0.05938** (0.02371)	-0.05783** (0.02423)	-0.053641* (0.0236)
$month_{15}$	-0.00496 (0.0176)	-0.00333 (0.01843)	-0.004633 (0.01891)
$month_{16}$	0.020802 (0.01721)	0.022694 (0.01792)	0.022984 (0.01780)
time	-0.00028 (0.0007)	-0017919 (0.00148)	0.00222 (0.0025)
$time^2$	-	0.000025 (0.00002)	-0.00016 (0.00011)
$time^3$	-	-	2.12e-06 (1.12e-06)
adj. R^2			
within	0.6884	0.6907	0.6923
between	0.3424	0.3570	0.3530
overall	0.0778	0.0840	0.0740

Clustered standard errors via *Stata* are reported in parentheses. Statistical significance at the $\alpha = 0.01, 0.05$ and 0.10 levels are denoted by ***, **, *, respectively. The unit of clustering is the traffic district. F-statistics for each estimated equation are strongly statistically significant.

We approach equation (1) following Burger, Kaffine and Yu (2011) and estimate the model by treating the time trend as linear, quadratic or cubic, accounting for "unobserved factors that evolved smoothly over time and are unrelated" to a handheld ban.[24] The three estimations are reported in Table 3. For each estimation, the parameter estimates with standard errors in parentheses are reported controlling for traffic district fixed effects.

The results in Table 3 support the hypothesis that a hand-held ban has been associated with a reduction in rear-end accidents for the initial estimation where the ban is interpreted strictly as July 2008. Using the cubic model, the coefficient value indicates that the handheld ban is associated with a 4.75% reduction in rear-end accidents, while the linear model estimates an associated reduction in rear-end accidents of 7.09%. While we have attempted to take a similar tack to Burger et al, this finding, while controlling for traffic district fixed effects and other factors, could be different from Burger et al because of our assertion that changes in rear-end accidents better measure initiatives to reduce distracted driving and/or our efforts to include data points associated with non-highway incidents. A proposition we test and report on later in the paper. Moreover, care needs to be taken in interpreting the magnitudes of the handheld ban coefficients since it is more likely that driving behavior may have been modified more continuously over time than at the specific enactment date initially modeled. It is plausible that the time polynomial captures some of the influence of handheld ban in its association with rear-end accidents.[25]

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[24]We, following Burger et al, did not normalize the monthly accident count in a traffic district by measures that more directly reflect accident frequency such as vehicles miles traveled (VMT). Burger et al motivated their use of a regression discontinuity design by stating that certain unobservable variables such as VMT could alter accident count and, as in their case, "countervailing the policy effect." At the end of this research we did attempt to gather VMT data from the California Department of Transportation to normalize monthly accidents by traffic district as an additional robustness check but were unable to obtain the information.

[25]As noted by one referee, "there is more continuity in the effect of the ban than the authors expect and it would be absorbed into the continuous time trend." We appreciate the remarks of anonymous referees who emphasized caution about the estimation approach being a true RD design.

Other estimated parameters line up with our expectations. Higher levels of rear-end accidents are associated with lower fuel prices perhaps reflecting fewer cars on roadways in response to marginal changes in the demand for fuel. A 1% change in the gas price index is associated with about a 0.215% reduction in rear-accidents. Higher levels of rainfall lead to less safe driving conditions and are associated with higher levels of rear-end accidents. More specifically, the rate of increase in rear-end accidents is 0.873% for a 1% change in rainfall under the model with linear time. More working days in a month were related to a higher number of rear-end accidents in a month: a 1% change in working days is associated with about a 0.33% increase in rear-end accidents depending on how time is included. Lower rates of unemployment are assumed to be aligned with higher rates of economic activity which should impact transportation. We found a statistically significant negative relationship between monthly rear-end accidents and unemployment rates consistent with this premise. The range of reduction in rear-end accidents associated with a 1% increase in the unemployment rate is between 0.164% and 0.183%.[26]

3.2 Robustness Checks

To further explore the strength of the results reported in Table 3, we estimated the model with different time dependent subsets of the data. In Table 4, we report only the estimated coefficient on handheld ban for estimations in which various time windows before and after the legal change was implemented.[27] For ease of comparison purposes we repeat the reported estimated value from our benchmark case from Table 3 noting it as Benchmark. In Scenario 2, we omitted from our 60-month sample the 12-month time period surrounding July 1, 2008 including only pre-handheld months up to 6 months before the ban and delaying the inclusion of post-handheld months until 6 months after the

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[26]A reminder of interpretations of the parameters in a log-linear model may be found at http://dss.princeton.edu/online_help/stats_packages/stata/log.html and http://www.ats.ucla.edu/stat/sas/faq/sas_interpret_log.htm

[27]Estimates of the entire model are available from the authors upon request.

ban. As can be seen by Table 4, estimates of the handheld parameter remain consistent with the base-line model while controlling for other factors. For Scenario 2 we find that rear-end accidents are associated with a reduction of between 11.7% and 13.6% when the handheld ban is in place, *ceteris paribus*. In scenario 3 we expand that omission window to 24 months and our estimation is undertaken including the pre-handheld ban months from January 1, 2006 through July 2007 and the post-handheld ban months from July 1, 2009 through December 2010. Negative and statistically significant parameter estimates of the handheld ban remain evident.

Table 4
Robustness Tests I

			Coefficients on handheld ban for estimation of equation (1) by time trend type		
		Time Trend	Linear	Quadratic	Cubic
Scenario		**Time Interval**			
Benchmark	No Handheld Ban	Jan-06 thru June-08	-0.07094*** (0.01509)	-0.06669*** (0.01498)	-0.04753* (0.0213)
	Handheld Ban	July-08 thru Dec-10			
2	No Handheld Ban	Jan-06 thru Dec-07	-0.11703** (0.03701)	-0.11964** (0.03619)	-0.13668** (0.04271)
	Handheld Ban	Dec-08 thru Dec-10			
3	No Handheld Ban	Jan-06 thru July-07	-0.13085** (0.0497)	-0.12935** (0.04843)	-0.4825** (0.1578)
	Handheld Ban	July-09 thru Dec-10			
4	No Handheld Ban	Jan-06 thru June-08	-0.08378*** (0.0195)	-0.0878*** (0.0184)	-0.0666** (0.0235)
	Handheld Ban	Nov-08 thru Dec-10			
5	No Handheld Ban	Jan-06 thru June-08	-0.08616** (0.03804)	-0.09975** (0.03304)	-0.06999* (0.0354)
	Handheld Ban	Jan-09 thru Dec-10			

In scenarios 2 and 3, data are removed from around the key legislative date in July 2008 in two widening, symmetric intervals. By contrast, in Scenarios 4 and 5 we remove months subsequent to July 1, 2008 since we expect there could be some response lag to the legal change. In Scenario 4, we omit months from July 1, 2008 through October 2008, a 4-month time period, and in Scenario 5 we omit months from July 1, 2008 through December 2008. The contrast between handheld and no handheld regions should be more evident and the relative magnitudes of the negative coefficients on the handheld variable support this presumption. In scenarios 4 and 5 data are omitted post-legal change in three month windows beyond July 2008. Both model estimations yield statistically significant negative coefficients on the handheld ban variable.

The second set of robustness checks is motivated by the potential for distracted driving behavior to be altered prior enactment date of the law. While it is reasonable to expect that the transmission of information around the change in handheld law was most frequent at the enactment date, it is equally plausible that information and public awareness about the potential of the handheld law would be expected to occur over a time range.[28] Indeed, the law in question was passed in September 2006 with information in the public domain prior to that date.[29] Thus, utilizing the entire 60-month period, we report in Table 5 the estimated coefficients of a handheld ban during the estimation of the entire model in (1) across four additional scenarios where we change how we treat the handheld ban. We include the benchmark scenario 1 for comparison purposes.

Scenarios 7 through 10 report the coefficient estimates with different *assumed* dates of the handheld ban being the distinguishing attribute. Scenario 7 considers an effective handheld ban beginning in August 2006 and scenarios 8, 9 and 10 move the "effective" handheld ban out in 6-month increments. Scenario 10 assumes the ban is effective as of January 2008, six months before the enactment date of the law. Contrast the results in scenario 10 versus the original estimations. They are notably similar. The assumption that the prospective ban would have penetrated some of

[28]We are grateful to one reader who suggested that dates prior to July 1, 2008 could serve as a "placebo" test for comparisons to the initial estimation results reported in Table 3 to accommodate differences in driving behavior away from the enactment date of the law.

[29]See "Schwarzenegger Calls for Banning Hand-Held Cellphone Use While Driving," *Los Angeles Times*, July 13, 2006 and Kolko (2008).

the public's awareness six months prior to the actual enactment date leads to the expectation that such an awareness is related to fewer rear-end driving accidents. Indeed, the negative coefficient estimates largely follow this expected outcome. Moreover, as one moves upward in Table 5 from scenario 10 to 9, 8, and 7 in which different enactment dates have been created farther away from the actual enactment date, the coefficient estimates on the effective handheld variable become inconclusive. More often than not there is no evidence of a significant relationship between the created handheld van variable and rear-end accidents and in three instances there is a positive relationship.[30]

Table 5
Robustness Tests II

			Coefficients on handheld ban for estimation of equation (1) by time trend type		
		Time Trend	Linear	Quadratic	Cubic
Scenario		**Time Interval**			
Benchmark	No Handheld Ban	Jan-06 thru June-08	-0.07094*** (0.01509)	-0.06669*** (0.01498)	-0.04753* (0.0213)
	Handheld Ban	July-08 thru Dec-10			
7	No Handheld Ban	Jan-06 thru Jul-06	-0.00221 (0.02378)	0.03537** (0.01291)	-0.01516 (0.02366)
	Handheld Ban	Aug-06 thru Dec-10			
8	No Handheld Ban	Jan-06 thru Dec-06	0.01813 (0.0252)	0.0952*** (0.01470)	0.0803*** (0.01604)
	Handheld Ban	Jan-07 thru Dec-10			
9	No Handheld Ban	Jan-06 thru June-07	-0.02654 (0.0164)	-0.00976 (0.01236)	0.00765 (0.01478)
	Handheld Ban	Jul-07 thru Dec-10			
10	No Handheld Ban	Jan-06 thru Dec-07	-0.07480*** (0.01852)	-0.07450*** (0.01904)	-0.06175** (0.0261)
	Handheld Ban	Jan-08 thru Dec-10			

[30]As noted by a reader, there are instances in Table 5 in which the coefficient on handheld ban is positive. However, these positive estimates correspond with dates that were at least a year before the actual handheld ban going into law. It is reasonable to suspect that noise in such estimates would be prevalent.

The third set of robustness checks entails a) analyzing the assumption that focusing on rear-end accidents only is important to testing whether the handheld policy change is associated with a declining accident frequency, and b) whether considering only collisions in which the incident took place on a state highway would lead to a different inference.[31] We expanded the data points in the model to include a definition of a collision accident that included rear-end accidents *and* all other collision accident descriptors including "not stated" in the SWITRS data while maintaining in the data both accidents that were incurred on roadways other than state highways and accidents that were incurred on state highways. The results are reported in Table 6. The coefficient and statistical significance of the handheld ban is consistent with the results in Table 3 revealing that focusing only on rear-end accidents is inconsequential to the implications from this empirical analysis. Other model variables are largely consistent with the findings reported in Table 3.

Lastly, we kept the broader definition of collision accidents and eliminated observations when the raw data did not report an associated traffic district, e.g., the accident was not incurred on a state highway. In essence, estimations are exploring whether exploring all reported collision incidents but limiting them to only state highways would change the original findings. In Table 7 the estimate of the handheld ban coefficient is negative and statistically significant across all model specifications, and other model variables are largely consistent with previous findings.

In summary, the first set of robustness checks shows a reduction in accidents post-law relative to pre-law for various snapshots of the data around the enactment date. The subsequent set of robustness checks relaxes the formal definition of enactment date and the estimations are consistent with an inability to reject the null hypothesis except for the scenario that assumes that behavior to a handheld ban occurred six months prior to the enactment date. The third set of robustness checks

[31]We appreciate the comments of an anonymous referee who directed us to explore these issues.

shows the handheld ban is associated with a reduction in a broader categorization of collisions and that isolating only on state highways, thus altering the definition of an observation, does not change the findings of the original benchmark estimation. For the model specification employed in this paper across different sets of data, lower collision accident frequency is associated with the enactment of California's handheld ban.

Table 6: Estimates
Robustness Checks IIIa
(n = 540; 9 traffic districts over 60 months)

Variable	**Dependent Variable: ln(collisions)**		
intercept	7.95*** (0.146)	7.989*** (0.1676)	7.9742*** (0.17454)
handheld ban	-0.05746*** (0.0055)	-0.055341*** (0.0049)	-0.043489*** (0.010147)
ln(gas price index)	-0.15747*** (0.02109)	-0.15397*** (0.02162)	-0.152734*** (0.021301)
rainfall	0.014056*** (0.00342)	0.01332* (0.00411)	0.013182** (0.004158)
ln(unemprate)	-0.151568*** (0.0382)	-0.155404** (0.03859)	-0.143788** (0.04203)
ln(net working days)	0.11192** (0.0493)	0.104119** (0.05238)	0.098912* (0.05226)
$month_6$	-0.0637*** (0.013905)	-0.06185*** (0.01507)	-0.05943*** (0.01527)
$month_7$	0.018356 (0.13023)	0.018954 (0.01245)	0.0188873 (0.012563)
$month_8$	0.002784 (0.014178)	0.00602 (0.01361)	0.005256 (0.013935)
$month_9$	-0.102742*** (0.019828)	-0.09998*** (0.021061)	-0.098846*** (0.02125)
$month_{10}$	-0.082796*** (0.018287)	-0.0795*** (0.01866)	-0.078002*** (0.018644)
$month_{11}$	-0.008114 (0.0124)	-0.007765 (0.012101)	-0.008561 (0.012745)
$month_{12}$	-0.03467*** (0.01992)	-0.033915*** (0.009593)	-0.031781*** (0.00915)
$month_{13}$	-0.02104 (0.01833)	-0.018269 (0.01987)	-0.016604 (0.020127)
$month_{14}$	-0.003802 (0.00782)	-0.003107 (0.007825)	-0.0005107 (0.00774)
$month_{15}$	-0.00311 (0.00945)	-0.002301 (0.00927)	-0.003101 (0.00972)
$month_{16}$	0.03618*** (0.01015)	0.03713*** (0.010609)	0.037313*** (0.010603)
time	-0.001047* (0.00054)	-001803 (0.00102)	0.0006795 (0.001768)
$time^2$	-	0.000012 (0.000016)	-0.000107 (0.00008)
$time^3$	-	-	1.31e-06 (9.06-07)
adj. R^2			
within	0.8097	0.8105	0.8113
between	0.2208	0.2299	0.2223
overall	0.0592	0.0620	0.0566

Clustered standard errors via *Stata* are reported in parentheses. Statistical significance at the α = 0.01, 0.05 and 0.10 levels are denoted by ***, **, *, respectively. . F-statistics for each estimated equation are strongly statistically significant.

Table 7: Estimates
Robustness Checks IIIb
(n = 540; 9 traffic districts over 60 months)

Variable	**Dependent Variable: ln(collisions)**		
intercept	6.804*** (0.16115)	7.0027*** (0.19407)	6.9922*** (0.19274)
handheld ban	-0.089734*** (0.010562)	-0.078443*** (0.00954)	-0.07048*** (0.01354)
ln(gas price index)	-0.205005*** (0.021588)	-0.186409*** (0.02032)	-0.18557*** (0.02027)
rainfall	0.02625*** (0.005697)	0.0223792* (0.00698)	0.022282** (0.00707)
ln(unemprate)	-0.13827** (0.042188)	-0.15865** (0.04016)	-0.150853*** (0.04376)
ln(net working days)	0.145626** (0.057399)	0.104175** (0.061304)	0.10068 (0.06301)
$month_6$	-0.072436*** (0.02109)	-0.062134** (0.0222209)	-0.060515** (0.023035)
$month_7$	0.054817*** (0.01461)	0.057991*** (0.013488)	0.057946*** (0.013598)
$month_8$	-0.004116 (0.020422)	0.013065 (0.01845)	0.012553 (0.01865)
$month_9$	-0.1219571*** (0.032432)	-0.10733** (0.03358)	-0.106569** (0.03396)
$month_{10}$	-0.114709*** (0.02461)	-0.097206*** (0.024819)	-0.0962*** (0.02512)
$month_{11}$	0.048098** (0.0166257)	0.049955** (0.015659)	0.04942** (0.016148)
$month_{12}$	-0.027649** (0.012018)	-0.023642* (0.01143)	-0.022209* (0.011939)
$month_{13}$	-0.04523* (0.02377)	-0.030505 (0.024944)	-0.029388 (0.02545)
$month_{14}$	-0.023233 (0.01442)	-0.019129 (0.01417)	-0.017386 (0.01472)
$month_{15}$	0.030803 (0.017176)	0.035111* (0.01663)	0.034574* (0.016933)
$month_{16}$	0.0311508 (0.019435)	0.036178 (0.01996)	0.036299* (0.02004)
time	-0.00028 (0.00068)	-0.00429** (0.001469)	-0.00262 (0.002513)
$time^2$	-	0.000068** (0.00002)	-0.000012 (0.00012)
$time^3$	-	-	8.81e-07 (1.35e-06)
adj. R^2			
within	0.6544	0.623	0.6726
between	0.168	0.231	0.2221
overall	0.0517	0.0674	0.0632

Clustered standard errors via *Stata* are reported in parentheses. Statistical significance at the α = 0.01, 0.05 and 0.10 levels are denoted by ***, **, *, respectively. . F-statistics for each estimated equation are strongly statistically significant.

4. Conclusion

The developing literature on mobile phone use, driving and safety has yielded conflicting conclusions about whether such a distraction while driving is associated with higher odds of an incident. The results in this paper indicate that the presence of California's handheld cell phone ban has been associated with a reduction in the number of accidents generally consistent with our expectations and contrary to Burger et al (2014). While our approach had somewhat different definitions of an accident and different controls than Burger, our findings considered monthly data over a five year time period centered about the ban, rather than daily data up to a one year time period around the ban.

While this primary finding is limited to the state of California, there are broader implications for policymakers in other jurisdictions who are charged with evaluating the efficacy of mobile communication device legislation. Our findings can contribute to any discussion of technological innovation within a vehicle that may deviate driver attention. Indeed, technological innovation around any element of vehicle operation has the potential of altering the auto insurance landscape and the subsequent regulation of this market. Due to the high costs imposed on society by accidents, it is important for policies to exist which are effective in reducing accident frequency regardless of the reason users become distracted.

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