



JOURNAL OF INSURANCE REGULATION

Cassandra Cole and Kathleen McCullough
Co-Editors

Vol. 35, No. 1

Cellphone Laws and Rear-End Accidents

Robert Puelz
Hanna E. Robertson



The NAIC is the authoritative source for insurance industry information. Our expert solutions support the efforts of regulators, insurers and researchers by providing detailed and comprehensive insurance information. The NAIC offers a wide range of publications in the following categories:

Accounting & Reporting

Information about statutory accounting principles and the procedures necessary for filing financial annual statements and conducting risk-based capital calculations.

Consumer Information

Important answers to common questions about auto, home, health and life insurance — as well as buyer's guides on annuities, long-term care insurance and Medicare supplement plans.

Financial Regulation

Useful handbooks, compliance guides and reports on financial analysis, company licensing, state audit requirements and receiverships.

Legal

Comprehensive collection of NAIC model laws, regulations and guidelines; state laws on insurance topics; and other regulatory guidance on antifraud and consumer privacy.

Market Regulation

Regulatory and industry guidance on market-related issues, including antifraud, product filing requirements, producer licensing and market analysis.

NAIC Activities

NAIC member directories, in-depth reporting of state regulatory activities and official historical records of NAIC national meetings and other activities.

Special Studies

Studies, reports, handbooks and regulatory research conducted by NAIC members on a variety of insurance-related topics.

Statistical Reports

Valuable and in-demand insurance industry-wide statistical data for various lines of business, including auto, home, health and life insurance.

Supplementary Products

Guidance manuals, handbooks, surveys and research on a wide variety of issues.

Capital Markets & Investment Analysis

Information regarding portfolio values and procedures for complying with NAIC reporting requirements.

White Papers

Relevant studies, guidance and NAIC policy positions on a variety of insurance topics.

For more information about NAIC publications, view our online catalog at:

 <http://store.naic.org>

© 2016 National Association of Insurance Commissioners. All rights reserved.

Printed in the United States of America

No part of this book may be reproduced, stored in a retrieval system, or transmitted in any form or by any means, electronic or mechanical, including photocopying, recording, or any storage or retrieval system, without written permission from the NAIC.

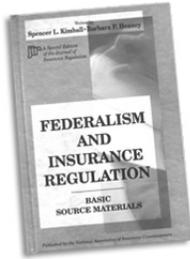
NAIC Executive Office
444 North Capitol Street, NW
Suite 700
Washington, DC 20001
202.471.3990

NAIC Central Office
1100 Walnut Street
Suite 1500
Kansas City, MO 64106
816.842.3600

NAIC Capital Markets
& Investment Analysis Office
One New York Plaza, Suite 4210
New York, NY 10004
212.398.9000

Companion Products

The following companion products provide additional information on the same or similar subject matter. Many customers who purchase the *Journal of Insurance Regulation* also purchase one or more of the following products:



Federalism and Insurance Regulation

This publication presents a factual historical account of the development of the framework for insurance regulation in the United States. It does so in part by using illustrative early statutes, presenting them chronologically, and in part by using cases that illustrate the interpretation of the crucial later statutes. Copyright 1995.



Regulation and the Casualty Actuary

This anthology reprints 20 important papers from past issues of the Journal of Insurance Regulation that are most relevant for practicing actuaries and state insurance regulators. It covers a wide range of issues, such as ratemaking, auto insurance pricing, residual markets, reserving and solvency monitoring. This invaluable reference explains these complex topics in straightforward, non-technical language. Copyright 1996.

How to Order

 816.783.8300

 prodserv@naic.org

 <http://store.naic.org>

Editorial Staff of the *Journal of Insurance Regulation*

Co-Editors

Cassandra Cole and Kathleen McCullough
Florida State University
Tallahassee, FL

Case Law Review Editor

Jennifer McAdam, J.D.
NAIC Legal Counsel II

Editorial Review Board

Cassandra Cole, Florida State University, Tallahassee, FL
Lee Covington, Insured Retirement Institute, Arlington, VA
Brenda Cude, University of Georgia, Athens, GA
Robert Detlefsen, National Association of Mutual Insurance Companies,
Indianapolis, IN
Bruce Ferguson, American Council of Life Insurers, Washington, DC
Stephen Fier, University of Mississippi, University, MS
Kevin Fitzgerald, Foley & Lardner, Milwaukee, WI
Robert Hoyt, University of Georgia, Athens, GA
Alessandro Iuppa, Zurich North America, Washington, DC
Robert Klein, Georgia State University, Atlanta, GA
J. Tyler Leverty, University of Iowa, Iowa City, IA
Andre Liebenberg, University of Mississippi, Oxford, MS
David Marlett, Appalachian State University, Boone, NC
Kathleen McCullough, Florida State University, Tallahassee, FL
Charles Nyce, Florida State University, Tallahassee, FL
Mike Pickens, The Goldwater Taplin Group, Little Rock, AR
David Sommer, St. Mary's University, San Antonio, TX
Sharon Tennyson, Cornell University, Ithaca, NY

Purpose

The *Journal of Insurance Regulation* is sponsored by the National Association of Insurance Commissioners. The objectives of the NAIC in sponsoring the *Journal of Insurance Regulation* are:

1. To provide a forum for opinion and discussion on major insurance regulatory issues;
2. To provide wide distribution of rigorous, high-quality research regarding insurance regulatory issues;
3. To make state insurance departments more aware of insurance regulatory research efforts;
4. To increase the rigor, quality and quantity of the research efforts on insurance regulatory issues; and
5. To be an important force for the overall improvement of insurance regulation.

To meet these objectives, the NAIC will provide an open forum for the discussion of a broad spectrum of ideas. However, the ideas expressed in the *Journal* are not endorsed by the NAIC, the *Journal's* editorial staff, or the *Journal's* board.

Cellphone Laws and Rear-End Accidents

Robert Puelz
Hanna E. Robertson*

Abstract

In an attempt to reduce accident frequency caused by distraction involving cellphone use, the state of California enacted two sets of laws banning cellphone use while driving. We examine the relationship between laws to ban cellphone use while driving and rear-end accident frequency in California. Using a panel data set that groups accidents by geographic location, we find evidence that the association between California's handheld cellphone 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.

* Edwin L. Cox School of Business, Southern Methodist University, Dallas, TX, 75275; rpuelz@smu.edu and hrobertson@smu.edu. We are grateful to Indraneel Chakraborty, Jim Carson Austin Nichols and anonymous referees for remarks on aspects of this research.

1. Introduction

Fifteen years ago, driving distracted because of cellphone 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 cellphone use is a major concern for legislators, public safety advocates and the insurance industry. The National Highway Traffic Safety Administration (NHTSA) estimates that 17% 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 distracted driving remains a liability for insurers in their contractual obligations to their clients and third-party claimants. If bans on handheld cellphone 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 cellphone while driving introduces an externality: the reduction in safety of innocent third parties. Previous studies have estimated that cellphone 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 cellphone while driving increased the risk of 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 cellphone use distracts drivers and increases the probability of accidents has led advocates for traffic safety to demand bans on cellphone use while driving. Regulation of cellphones has become a major policy issue and many states have enacted legislation to address distracted driving due to cellphone use. To date, using a handheld cellphone while driving is banned in 14 states.⁴

In addition, all cellphone use (handheld and hands-free) is restricted for novice drivers in 37 states and the District of Columbia. The definition of “novice driver” varies by state. Most states classify novice drivers as drivers younger than

1. www.distraction.gov/download/811737.pdf.

2. These are two of the six purposes for insurance regulation articulated by the NAIC, 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.

4. As of July 2015. See www.iihs.org/iihs/topics/laws/cellphonelaws?topicName=distracted-driving.

18, while other states designate novice drivers as learner's 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 all drivers younger than 19. All cellphone use while driving a school bus is prohibited in 19 states and the District of Columbia.⁵ Text messaging while driving is banned in 44 states and the District of Columbia. Novice drivers are prohibited from text messaging in four 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 cellphone use while driving (hereinafter referred to as "handheld ban") was put into effect.⁷ The handheld ban is made of up two laws: 1) the first law prohibits all drivers from using a handheld cellphone while operating a motor vehicle (California Vehicle Code [VC] §23123); and 2) the second law prohibits drivers under the age of 18 from using a handheld cellphone or a hands-free device while operating a motor vehicle (VC §23124). California also enacted a ban on text messaging (hereinafter referred to as "texting ban") that took effect 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 younger than 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 policymakers and economists are the costs and benefits of handheld cellphone bans. As we discuss, there is conflicting evidence about the association between cellphone 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 cellphone 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

5. "Hands-free" refers to a phone that does not require the user to hold it during operation.

6. Insurance Institute for Highway Safety, www.iihs.org/laws/maphandheldcellbans.aspx.

7. Note that hands-free devices are still allowed.

8. California drivers 18 and older may dictate, send or listen to text-based messages if they are using voice-activated, hands-free devices.

9. California Vehicle Code, 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 multitask when a driver wishes to simultaneously hold a phone conversation. See www.nsc.org/DistractedDrivingDocuments/Cognitive-Distraction-White-Paper.pdf.

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 this paper proceeds as follows. In the next section, we present a review of previous studies related to cellphone use and automobile incidents. Then, the California 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 cellphone use and accident frequency have produced conflicting results. Many studies conclude that the use of cellphones while driving increases the probability of accidents. Goodman, Tijerina, Bents and Wierwille (1999) conclude that there is sufficient evidence to claim that using a cellphone 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 the 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 regardless of 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 cellphone conversations on performance of a simulated driving task. Their research suggests that cellphone 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 cellphone 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 cellphone 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 cellphone 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 two years and

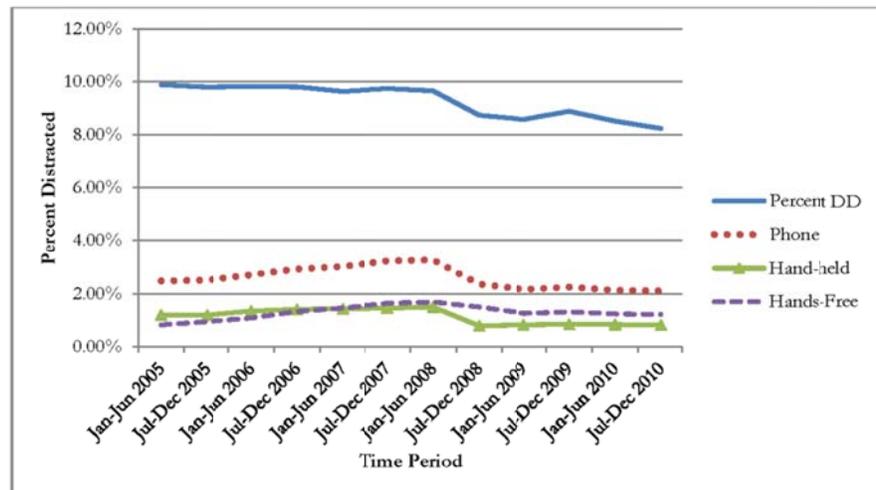
a control group of 100 randomly selected licensed drivers not involved in accidents within the past 10 years. Violanti and Marshall examined the amount of time per month spent talking on a cellular phone and 18 other driver inattention factors. They found that increased use of a cellphone while driving was associated with increased risk of being in a traffic accident. Although the study consisted of a small sample size, the results indicated that talking more than 50 minutes per month on a cellphone in a vehicle was associated with a 5.59-fold increased risk of a traffic accident. In addition, Violanti found in 1998 that cellphones 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 cellphone while driving. In addition, a two-fold increased risk for a fatality was found given the mere presence of a cellphone in the vehicle.

Redelmeier and Tibshirani (1997) further explored these findings by using the case-crossover design to study whether using a cellphone 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 cellphone was approximately four times higher than the risk when no cellphone was used. These results are the most highly cited in policy discussions about banning cellphone use while driving.¹¹ However, the field studies of Redelmeier and Tibshirani do not necessarily imply that the use of cellphones causes an increase in accident rates. People who use their cellphones 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% reduction in accidents from a cellphone ban, and Cohen and Graham (2003) found a 2% to 21% reduction in accidents with a central estimate of a 6% reduction in accidents.

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

11. Hahn and Preiger (2006).

Figure 1:
Percentage of Injuries by Distracted Driver Category



In contrast to these studies confirming a relationship between cellphone use and accident frequency, economists Hahn and Prieger (2006, 2007) report no significant effect of handheld or hands-free cellphone use on accidents based on survey data from more than 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 cellphones are inherently less safe drivers, even when not talking on the phone. They found that the impact of cellphone use on accidents varies across individuals; therefore, previous studies, based on accident-only samples, may be overstated by 36%. After controlling for selection bias and various risks across drivers, such as the statistically significant difference in cellphone effects between men and women in the sample, they found no significant effect of hands-free or handheld cellphone 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 cellphone use in California led to a reduction in traffic accidents. They tested whether the handheld cellphone 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 cellphone ban went into effect, making their analysis distinct from other studies. Their data set included more than 500,000 incidents as reported by the California Highway Patrol to the California Department of Transportation (Caltrans) 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, Burger et al. concluded that there is no evidence to support that the handheld

cellphone ban reduced accidents in California. While they 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 cellphone use does not impact accidents; that drivers used other disruptive hands-free or text-based technologies; or that weak compliance failed to reduce cellphone use overall.

By contrast, a University of California, Berkeley study found an overall reduction in percentages of fatalities and non-fatal injuries related to handheld cellphone use after implementation of California's handheld cellphone ban.¹² Using data from the California Statewide Integrated Traffic Records System (SWITRS) from January 2005 to December 2010, the study analyzed the number of fatalities and injuries overall due to distracted driving, cellphone use, handheld cellphone use and hands-free cellphone use. (The percentages of non-fatal injuries due to overall distracted driving, cellphone use, handheld cellphone use and hands-free cellphone 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 cellphone 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 handheld cellphone use after the July 1, 2008, effective date of the handheld cellphone ban in California. While Burger et al. had a rigorous analytical approach, the University of California, Berkeley study conducted a brief descriptive analysis of fatalities and injuries before and after the implementation of the handheld cellphone ban.

Several studies have focused on the relationship between cellphone 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 cellphone use and accidents or near accidents. Seo and Torabi found that the most frequently cited reason for drivers' accidents or near-accidents involving cellphones 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 impact 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 cellphone 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 cellphone use and all rear-end collisions in California.

12. See Ragland et al. (2012).

3. Analysis

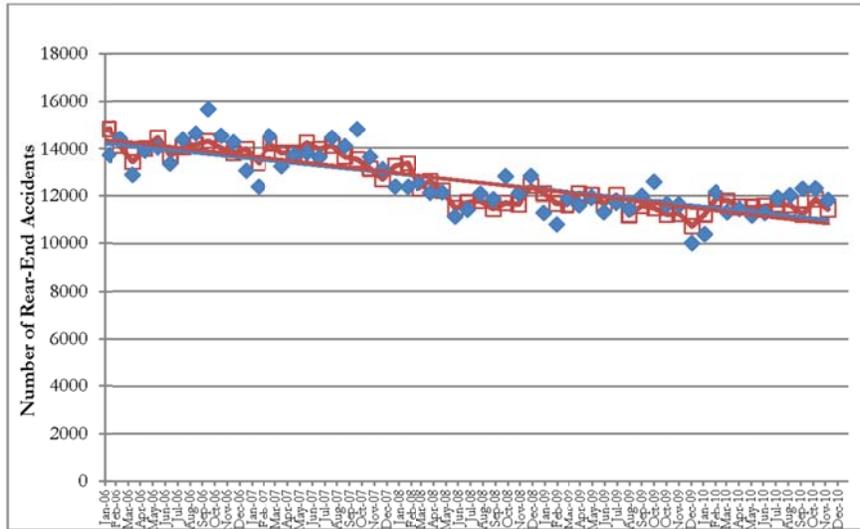
The accident data we use to test and evaluate the relationship between handheld and texting bans and the number of rear-end collisions comes from SWITRS. The SWITRS database houses collision scene data collected through the California Highway Patrol. Two separate laws that regulate cellphone use while 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 cellphone while operating a motor vehicle, but drivers 18 and older 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 cellphone ban. This period was chosen because it is 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 more than 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 cellphone distractions and rear-end accidents. This permits us to better isolate the efficacy of any legal change to distracted driving.

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.

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.

Figure 2:
Number of Rear-End Collisions by Month



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

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 cellphone use became effective 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 possible “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.

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 cellphone use became effective July 1, 2008.

H_1 : There has been a change in rear-end collisions in California after the ban on handheld cellphone use became effective July 1, 2008.

In building an empirical model, we undertook a series of preliminary steps. First, we 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, because across our overall time interval, there were only six months where the handheld ban was in effect and the texting ban was not. However, we view that of minor consequence because 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 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% 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 12 Caltrans districts in the California system, we found that District 1 and District 9 had few losses and Caltrans District 12 did not match well to the U.S. 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

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

where $g(t)$ is represented as, in Burger et al., a polynomial trend that “accounts for unobserved factors that evolved smoothly over time.”¹⁹ We include

18. Caltrans District 1 is in far northwest California (Eureka) and District 9 is in east central California (Bishop).

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

dv_{month_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 and be less distracted and, therefore, fewer accidents will occur.²⁰ A statistically significant negative coefficient on the 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 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. 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

discussed by a referee, a regression discontinuity (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”; i.e., “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; therefore, consideration of a texting ban variable was omitted from the final model.

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, CA, 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.

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.

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 handheld 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. (This is a proposition we test and report on later in the paper.)

22. Recognized U.S. holidays were obtained from the public archive files of the U.S. 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.

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 Caltrans to normalize monthly accidents by traffic district as an additional robustness check, but were unable to obtain the information.

**Table 2:
Descriptive Statistics**

Cal Trans District	rear-end collisions	handheld ban	gas price index	rainfall	unemprate (%)	net working days
2						
mean	89417	0.5	1.07076	2.403	10.898	21.633
std	15522	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	946217	0.5	1.05680	1.4087	8.1233	21.6333
std	108834	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	2356000	0.5	1.08284	1.459	6.812	21.633
std	258017	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	508067	0.5	1.08368	1.054	9.455	21.633
std	67240	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	600700	0.5	1.07667	0.917	11.775	21.633
std	78452	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	4159633	0.5	1.06515	0.877	7.778	21.633
std	317350	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	1407133	0.5	1.06263	1.033	9.300	21.633
std	236612	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	511167	0.5	1.05079	1.025	11.683	21.633
std	75178	0.5042	0.18165	1.261	4.022	1.207
min	310.00	0	0.605	0	6.30	19
max	683.00	1	1.579	5.25	18.30	24
11						
mean	751033	0.5	1.06925	0.723	6.928	21.633
std	98189	0.5042	0.18003	1.098	2.730	1.207
min	584.00	0	0.628	0	3.70	19
max	943.00	1	1.592	5.00	10.90	24

Moreover, care needs to be taken in interpreting the magnitudes of the handheld ban coefficients, because it is more likely that driving behavior may have been modified more continuously over time than at the specific effective 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

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

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)	-0.18313** (0.05437)	-0.164406** (0.05847)
ln(net working days)	0.33908** (0.11717)	0.323488** (0.1244)	0.315072** (0.12241)
month ₅	-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)	-0.0017919 (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.

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 are 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 six months before the ban and delaying the inclusion of post-handheld months until six months after the ban. As can be seen by Table 4, estimates of the handheld parameter remain consistent with the baseline 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.

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 www.ats.ucla.edu/stat/sas/faq/sas_interpret_log.htm.

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

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 Scenario 2 and Scenario 3, data are removed from around the key legislative date in July 2008 in two widening, symmetric intervals. By contrast, in Scenario 4 and Scenario 5, we remove months subsequent to July 1, 2008, because 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 four-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 Scenario 4 and Scenario 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 to the effective 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 effective 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 enacted 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.

Scenario 7 through Scenario 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 Scenario 8, Scenario 9 and Scenario 10 move the “effective” handheld ban out in six-month increments. Scenario 10 assumes the ban is effective as of January 2008, six months before the actual effective 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 the public’s awareness six months prior to the actual effective 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 Scenario 9, Scenario 8 and Scenario 7, in which different effective dates have been created farther away from the actual effective 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 ban variable and rear-end accidents and, in three instances, there is a positive relationship.³⁰

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 effective 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).

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 effect. It is reasonable to suspect that “noise” in such estimates would be prevalent.

**Table 5:
Robustness Tests II**

			Coefficients on handheld ban for estimation of equation (1) by time trend type		
			Linear	Quadratic	Cubic
Scenario		Time Trend 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			

The third set of robustness checks entails: 1) 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 2) determining 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 “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 occurred on roadways other than state highways and accidents that occurred 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.

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

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*** (0.02105)	-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 (0.014173)	0.00602 (0.01361)	0.005256 (0.013935)
month ₉	-0.102742*** (0.019823)	-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)	-0.01803 (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 $\alpha = 0.01$, 0.05 and 0.10 levels are denoted by ***, **, *, respectively. F-statistics for each estimated equation are strongly statistically significant.

Lastly, we kept the broader definition of “collision accident” and eliminated observations when the raw data did not report an associated traffic district; e.g., the accident did not occur on a state highway. In essence, the estimations are exploring whether analyzing 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.

**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* (0.00658)	0.022282** (0.00707)
ln(unemp rate)	-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** (0.022209)	-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 (0.01956)	0.036299* (0.02004)
time	-0.00028 (0.00068)	-0.00429** (0.001469)	-0.00262 (0.002513)
time ²	-	0.000063** (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 $\alpha = 0.01$, 0.05 and 0.10 levels are denoted by ***, **, *, respectively. F-statistics for each estimated equation are strongly statistically significant.

In summary, the first set of robustness checks shows a reduction in accidents post-ban relative to pre-ban for various snapshots of the data around its effective date. The subsequent set of robustness checks relaxes the formal definition of “effective date,” and the estimations are consistent with an inability to reject the null hypothesis except for the scenario that assumes that behavior in response to the handheld ban occurred six months prior to its effective date. The third set of robustness checks shows the handheld ban is associated with a reduction in a broader categorization of collisions and that isolating only on state highways, thereby 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.

4. Conclusion

The developing literature on cellphone use, driving and safety has yielded conflicting conclusions about whether such a distraction while driving is associated with higher odds of an accident. The results in this paper indicate that the presence of California’s handheld cellphone 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 a somewhat different definition of “accident” and different controls than Burger, our findings considered monthly data over a five-year time period centered about the handheld ban, rather than daily data up to a one-year time period around the handheld 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 enacting legislation that limits the use mobile communication devices. 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 public policies to exist that are effective in reducing accident frequency, regardless of the reason users become distracted.

References

- Burger, Nicholas Daniel Kaffine, and Bob Yu, (2014), Did California's Hand-Held Cell Phone Ban Reduce Accidents?, *Transportation Research Part A: Policy and Practice* 66: 162–172.
- Burger, Nicholas Daniel Kaffine, and Bob Yu, (2011), Did California's Hand-Held Cell Phone Ban Reduce Accidents?, RAND Corporation and <http://econbus.mines.edu/working-papers/wp201308.pdf>.
- California Climate Tracker, www.wrcc.dri.edu/monitor/cal-on/frames_data.html.
- California Department of Finance, www.dof.ca.gov/html/fs_data/latestecondata/FS_Misc.htm.
- California Department of Motor Vehicles, (2008), California Vehicle Code [VC] §23123, www.dmv.ca.gov/pub/vctop/dll/vc23123.htm.
- California Department of Motor Vehicles, (2008), California Vehicle Code [VC] §23124, www.dmv.ca.gov/pub/vctop/dll/vc23124.htm.
- California Department of Motor Vehicles, (2009), California Vehicle Code [VC] §23123_5, www.dmv.ca.gov/pub/vctop/dll/vc23123_5.htm.
- California Economic Indicators, www.dof.ca.gov/HTML/FS_DATA/indicatr/ei_home.htm.
- Cohen, J.T. and J.D. Graham, (2003), A Revised Economic Analysis of Restrictions on the Use of Cell Phones While Driving, *Risk Analysis*, 23: 5–17.
- Dreyer, N.A., J.E. Loughlin, and K.J. Rothman, (1999), Cause-Specific Mortality in Cellular Telephone Users, *JAMA*, 19: 1814-1816.
- Goodman, Michael J., Louis Tijerina, Frances D. Bents, and Walter W. Wierwille, (1999), Using Cellular Telephones in Vehicles: Safe or Unsafe?, *Transportation Human Factors*, 1: 3–42.
- Hahn, Robert W. and James E. Prieger, (2006), The Impact of Driver Cell Phone Use on Accidents, *The B.E. Journal of Economic Analysis and Policy* 6.
- Hahn, Robert W., Paul C. Tetlock, and Jason K. Burnett, (2000), Should You Be Allowed To Use Your Cellular Phone While Driving?, *Regulation*, 23: 46–55.
- Imbens, Guido W. and Thomas Lemieux, (2008), Regression Discontinuity Designs: A Guide to Practice, *Journal of Econometrics*, 142: 615–635.
- Insurance Institute for Highway Safety, (2012), *Highway Loss Data Institute*. www.iihs.org/laws/cellphonelaws.aspx.
- Klauer, Sheila G., Feng Guo, Bruce G. Simons-Morton, Marie Claude Ouimet, Suzanne E. Lee, and Thomas A. Dingus, (2014), Distracted Driving and Risk of Road Crashes among Novice and Experienced Drivers, *New England Journal of Medicine*, 370: 54–59.
- Kolko, Jed, 2008, What to Expect from California's New Hands-Free Law, Public Policy Institute of California, www.ppic.org/content/pubs/op/op_508jkop.pdf.
- Lee, David S. and Thomas Lemieux, (2010), Regression Discontinuity Designs in Economics, *Journal of Economic Literature*, 281–355.

- National Safety Council, (2012), "Understanding the Distracted Brain: Why Driving While Using Hands-Free Cell Phones is Risky Behavior." *White Paper April 2012*. www.nsc.org/DistractedDrivingDocuments/Cognitive-Distraction-White-Paper.pdf.
- Neyens, D.M. and L.N. Boyle, (2007), The Effect of Distraction on the Crash Types of Teenage Drivers, *Accident Analysis and Prevention*, 39: 206–212.
- Prieger, James E. and Robert W. Hahn (2007), Are Drivers Who Use Cell Phones Inherently Less Safe?" *Applied Economics Quarterly*, 53: 327–352.
- Ragland, David, (2012), Descriptive Analyses of Traffic Fatalities and Injuries Before and After California's Law Banning Hand-Held Cell Phone Use While Driving Was Implemented on July 1, 2008, *Safe Transportation Research and Education Center*, www.ots.ca.gov/Media_and_Research/Press_Room/2012/doc/CA_Cell_Phone_Law_Study.pdf.
- Redelmeier, Donald A. and Robert J. Tibshirani, (1997), Association Between Cellular-Telephone Calls and Motor Vehicle Collisions, *New England Journal of Medicine*, 336: 453–458.
- Redelmeier, Donald A. and M.C. Weinstein, (1999), Cost Effectiveness of Regulations Against Using a Cellular Telephone While Driving, *Medical Decision Making*, 19: 1–8.
- Seo, Dong-Chui and Mohammad R. Torabi, (2004), The Impact of In-Vehicle Cell-Phone Use on Accidents or Near-Accidents Among College Students, *Journal of American College Health*, 53, Issue 3: 101–108.
- Strayer, David L. and William A. Johnston, (2001), Driven to Distraction: Dual-Task Studies of Simulated Driving and Conversing on a Cellular Telephone, *Psychological Science*, 12: 462–466.
- United States Department of Transportation, www.fhwa.dot.gov/policyinformation/motorfuelhwy_trustfund.cfm.
- Violanti, J.M., (1998), Cellular Phones and Fatal Traffic Collisions, *Accident Analysis and Prevention*, 1998, 30: 519–524.
- Violanti, J.M. and J.R. Marshall, (1996), Cellular Phones and Traffic Accidents: An Epidemiological Approach, *Accident Analysis and Prevention*, 28: 265–270.

Journal of Insurance Regulation

Guidelines for Authors

Submissions should relate to the regulation of insurance. They may include empirical work, theory, and institutional or policy analysis. We seek papers that advance research or analytical techniques, particularly papers that make new research more understandable to regulators.

Submissions must be original work and not being considered for publication elsewhere; papers from presentations should note the meeting. Discussion, opinions, and controversial matters are welcome, provided the paper clearly documents the sources of information and distinguishes opinions or judgment from empirical or factual information. The paper should recognize contrary views, rebuttals, and opposing positions.

References to published literature should be inserted into the text using the “author, date” format. Examples are: (1) “Manders et al. (1994) have shown. . .” and (2) “Interstate compacts have been researched extensively (Manders et al., 1994).” Cited literature should be shown in a “References” section, containing an alphabetical list of authors as shown below.

Cummins, J. David and Richard A. Derrig, eds., 1989. *Financial Models of Insurance Solvency*, Norwell, Mass.: Kluwer Academic Publishers.

Manders, John M., Therese M. Vaughan and Robert H. Myers, Jr., 1994. “Insurance Regulation in the Public Interest: Where Do We Go from Here?” *Journal of Insurance Regulation*, 12: 285.

National Association of Insurance Commissioners, 1992. *An Update of the NAIC Solvency Agenda*, Jan. 7, Kansas City, Mo.: NAIC.

“Spreading Disaster Risk,” 1994. *Business Insurance*, Feb. 28, p. 1.

Footnotes should be used to supply useful background or technical information that might distract or disinterest the general readership of insurance professionals. Footnotes should not simply cite published literature — use instead the “author, date” format above.

Tables and charts should be used only if needed to *directly support* the thesis of the paper. They should have descriptive titles and helpful explanatory notes included at the foot of the exhibit.

Papers, including exhibits and appendices, should be limited to 45 double-spaced pages. Manuscripts are sent to reviewers anonymously; author(s) and affiliation(s) should appear only on a separate title page. The first page should include an abstract of no more than 200 words. Manuscripts should be sent by email in a Microsoft Word file to:

Cassandra Cole and Kathleen McCullough
jireditor@gmail.com

The first named author will receive acknowledgement of receipt and the editor's decision on whether the document will be accepted for further review. If declined for review, the manuscript will be destroyed. For reviewed manuscripts, the process will generally be completed and the first named author notified in eight to 10 weeks of receipt.

Published papers will become the copyrighted property of the *Journal of Insurance Regulation*. It is the author's responsibility to secure permission to reprint copyrighted material contained in the manuscript and make the proper acknowledgement.

NAIC publications are subject to copyright protection. If you would like to reprint an NAIC publication, please submit a request for permission via the NAIC Web site at www.naic.org. (Click on the "Copyright & Reprint Info" link at the bottom of the home page.) The NAIC will review your request.