How Cellphone Bans Affect Automobile Insurance Markets

In this paper, we examine the effect of laws prohibiting the hand-held use of a cellphone while driving on the automobile insurance market. Our research is motivated by the fact that prior studies present evidence that the enactment of such laws alters drivers' behaviors in ways that lead to fewer automobile accidents. We posit that that, by extension, these laws should also lead to reductions in the amount of losses paid by automobile insurers. Our analysis is consistent with this expectation and suggests that the enactment of a ban on the hand-held use of a cellphone while driving leads to a 1.3 percent reduction in the average amount of losses incurred by automobile insurers. We also find evidence that these laws lead to reductions in incurred loss ratios but have no impact on premiums.

Introduction

The safety of individuals operating automobiles is a paramount public health concern. Automobile crashes are the leading cause of death in the U.S. and, each year, millions of motorists are given emergency treatments for injuries arising out of automobile accidents (Centers for Disease Control, 2015). In response, policymakers have enacted a variety of measures aimed at reducing the frequency and/or severity of automobile accidents, the most recent of which involves restricting the use of cellphones while driving. Proponents of these laws argue that the use of a cellphone to talk or text while driving distracts the driver, thereby increasing the likelihood that a traffic accident will occur. As a result, it is suggested that policies banning the use of cellphones while driving alter the behaviors of drivers in a way that results in fewer traffic accidents. Indeed, the fact that many states enacted laws banning the hand-held use of cellphones for all persons operating motor vehicles suggests that policymakers believe restricting the use of cellphones will alter drivers' behaviors in a manner that improves public safety.

Many researchers have sought to provide empirical evidence as to whether there are actually any benefits to statutory restrictions on cellphone use. The approach taken by the majority of these studies is to examine how rates of traffic fatalities, injuries, or collisions change in a given geographic area (e.g. state-level, county-level, etc.) after a law that restricts the use of a cellphone while driving is enacted. A few studies find no evidence of changes in fatality, injury, or collision rates following the enactment of a law limiting cellphone use while driving (e.g. Trempel et al., 2011). Many other researchers, however, provide evidence that cellphone laws impact driver behavior in ways that lead to fewer accidents and traffic fatalities (e.g. Nikolaev et al., 2010; Sampaio et al., 2010). Thus, there is a degree of evidence that the enactment of laws limiting cellphone use while driving may be associated with real public health benefits.

However, there currently exists little evidence as to whether there are any *economic* benefits associated with banning cellphone use for motorists. While a variety of economic markets are conceivably influenced by cellphone bans, the automobile insurance market appears especially poised to be affected by laws that alter the behaviors of drivers. More specifically, because automobile insurers pay

for the losses associated with automobile accidents, any changes in drivers' behaviors resulting from cellphone bans should be reflected in the amount of losses paid by automobile insurers. It therefore follows that, if laws restricting the use of cellphones while driving reduce the frequency and/or severity of traffic accidents in a given marketplace, automobile insurers will incur fewer losses in the marketplace following the enactment of the law. Yet, due to a lack of empirical research on the subject, it is not known whether the enactment of cellphone laws have any appreciable effect on the automobile insurance industry.

We therefore explore the relationship between cellphone bans and automobile insurance markets by examining the regulatory filings of automobile insurers reporting to the National Association of Insurance Commissioners (NAIC) during the years 2000 – 2010. This database captures virtually all of the private automobile insurers participating in the U.S. marketplace and contains detailed information, including losses incurred and premium volume, pertaining to the operations of a given insurer, in a given state, during a given year. When combined with other sources of state-level demographic and regulatory data, the NAIC data therefore allows us to test whether a given insurer incurs fewer losses in a given state that enacts a law restricting the use of cellphones while driving. Similarly, the data also permit us to consider automobile insurers' premium levels and incurred loss ratios, both of which are important when evaluating the economic effects of these laws on consumers and insurers.

Our empirical approach is to exploit the heterogeneity that exists in states' decisions to enact laws that prohibit handheld cellphone conversations by teen drivers or all drivers. During our sample period, 32 states enacted such a law and the literature presents strong evidence that these laws have a significant effect on drivers' behaviors and lead to a lasting reduction in the number of drivers using hand-held cellphones while driving (McCartt et al., 2014). When placed alongside other studies that suggest talking on a hand-held increases the risk of a crash (Klauer et al., 2006), we might expect automobile insurers to incur fewer losses in a given state in the time period following a ban on hand-held cellphone conversations. We test this expectation using a multivariate differences-in-differences study design similar to those frequently employed by insurance economists evaluating the effects of state-specific laws

on insurance markets (e.g. Viscusi and Born, 2005; Heaton, 2015). We also implement a variety of specification checks to verify the robustness of our analysis.

Our results suggest that automobile insurers experience a 1.3 percent reduction in the amount of losses incurred in a given state following the enactment of a law banning the hand-held use of cellphones while driving. While we find no evidence that the laws impact premiums in any statistically meaningful way, our analysis suggests that incurred loss ratios decline by approximately 1.2 percent in states that enact a cellphone ban. As such, our analysis suggests that automobile insurers may benefit from cellphone bans via a slight increase in underwriting performance.

Overall, our evidence is consistent with the notion that laws banning the hand-held use of cellphones alters drivers' behaviors in ways that reduce the amount of losses paid by automobile insurers. Indeed, the small magnitude of the decreases in losses incurred and loss ratios we find in our analysis is in-line with what we would expect from a behavioral response. That is, we would not expect prohibiting hand-held cellphone conversations while driving to dramatically reduce the frequency and/or severity of traffic accidents. A more reasonable expectation, in view of the literature, is that the behavior change would result in a relatively small reduction in the frequency and/or severity of traffic accidents. As such, insurers would experience an equally small, but significant, reduction in the amount of losses incurred – which is exactly what we find.

Further, while the magnitude of the economic impact of cellphone laws on insurers is small, it does not diminish the importance of our results. Slight improvements in the underwriting performance of insurers implies that insurers have more capital to invest in capital markets, operational growth opportunities, dividend payments to policyholders, or similar activities associated with economic benefit. In addition, as noted by Grace and Leverty (2013), as incurred losses fall, premiums may 1) be driven lower by market competition or 2) increase due to an increase in the quantity of insurance sold. As such, the authors argue that, if a law reduces the losses incurred by insurers but does not raise premiums, both consumers and insurers benefit. Since we find no evidence that cell-phone bans influence premium levels,

it follows that automobile insurers and drivers insured in the private marketplace both benefit from the enactment of such laws.

Another important aspect of our study is that it provides unique and robust insight into the effect of cellphone bans on accident rates. As noted earlier, while a significant amount of literature suggests these laws reduce collision rates, fatality rates, or injury rates, some studies find the laws have no effect on such outcomes. As noted by McCartt et al. (2014), the discrepancies in findings arise from challenges in data sources and study design. Our analysis overcomes many of these challenges by using a nationally representative dataset and a study design that allows for clean identification of the effects of cellphone laws. To the extent that automobile insurance losses are highly correlated with the frequency and/or severity of traffic accidents, our study may, indirectly, help advance the public health literature as well.

Our paper proceeds as follows. We first provide background on the effects of hand-held cellphone bans and their potential impact on the automobile insurance market. We then discuss the sample we use to examine the potential relation between hand-held cellphone laws and the losses incurred by automobile insurers. After that, we discuss our methods and results and provide concluding remarks.

Background

During the past three decades, cellphone ownership grew dramatically among Americans and it is estimated that over 90 percent of individuals owned a cellphone in 2014. As a result, researchers began to consider how the proliferation of cellphones in the U.S. affected driving performance and several studies identified a positive correlation between the hand-held use of a cellphone while driving and the risk of an automobile accident. For example, Redelmeier and Tibshirani (1997) and McEvoy et al. (2006) both find evidence indicating that the hand-held use of a cellphone increases the risk of an automobile crash by a factor of four. Other researchers, using a variety of study designs and data sources, provide additional evidence that the use of a cellphone while driving significantly increases the likelihood of an automobile accident (e.g. McCartt, Hellinga, and Braitman, 2006; Klauer, et al., 2006; Klauer et al., 2014)

Motivated by the apparent link between hand-held cellphone use and accident risk, many policymakers began considering enacting laws that would prohibit drivers from using cellphones while operating a motor vehicle. These policymakers recognized the empirical evidence indicating other laws, unrelated to cellphone use, had significantly changed drivers' behaviors in ways that reduced the risk of an accident by, for example, increasing seat-belt use or decreasing drunk-driving (e.g. Wells, et al., 1992; Dinh-Zarr et al., 2001; Farmer and Williams, 2006). Laws banning the hand-held use of a cellphone while driving were expected to have a similar, beneficial effects on drivers' behaviors and traffic accidents. As a result, in 2001, New York became the first state to implement a ban on the hand-held use of cellphones for all drivers and, in the ensuing time, many more states implemented laws that restrict the use of cellphones while driving.

A variety of empirical studies examine whether hand-held cellphone bans have the desired effect of changing drivers' behaviors and reducing automobile accidents.[1] As noted by McCartt et al. (2014), research in this area is hindered by data availability and the appropriateness of study designs and, consequently, there is no clear consensus in the literature regarding the magnitude of the effect of cellphone bans on drivers' behaviors. Nevertheless, there exists a considerable amount of evidence that cellphone use while driving decreases when states enact laws prohibiting this behavior. For example, McCartt et al. (2010) report that driver hand-held cellphone use declined significantly in three states that enacted hand-held cellphone bans while Braitman and McCartt (2010) find that fewer drivers report using cellphones while driving in states with hand-held bans compared to states without bans.

Other studies also suggest that the change in driver behavior elicited by hand-held cellphone bans ultimately leads to reductions in traffic accidents. For example, Nikolaev et al. (2010) and Sampaio et al.

[1] While, in more recent years, states have enacted laws prohibiting texting while driving, the effects of these laws on drivers' behaviors remains unclear. As noted by McCartt et al. (2014), this is due to the fact that researchers in this area face many challenges that make it difficult to evaluate the effectiveness of texting laws. For example, little evidence exists regarding the extent to which drivers text, making it difficult to empirically examine changes in texting behavior after the enactment of a law. In addition, many states enacted texting bans after a hand-held cellphone ban was already in place, making it difficult to isolate the specific effects of texting laws on drivers' behavior.

(2010) provide evidence that mean automobile crash rates declined in New York in the time period after the state enacted a ban on hand-held cellphone use while driving. In addition, using data pertaining to automobile accident and fatality rates across most or all of the states in the U.S., Kolko (2009), Anyanwu (2012) and Lim and Chi (2013) all provide some degree of empirical support for the notion that hand-held cellphone bans reduce traffic fatalities and/or traffic accidents. A small number of studies, examining data pertaining only to a small number of states, find no evidence that banning the hand-held use of a cellphone while driving affects drivers' behaviors or accident rates (e.g. Trempel et al., 2011; Bhargava and Pathania, 2013). However, when considered in its entirety, the overall empirical evidence presented in the in the literature provides strong support for the notion that hand-held cellphone bans have a real effect on drivers' behaviors that results in reductions in traffic accidents and fatalities.

However, while understanding how laws prohibiting the use of hand-held cellphones affects accident rates is an important public health concern, understanding the extent to which the effects of these laws spillover into the economic marketplace is an equally important public policy concern. Yet, to date, the economic effects of cellphone bans remain largely unexplored in the literature. With this in mind, we note that the U.S. automobile insurance market appears to be an ideal setting for considering the extent to which laws banning drivers from using hand-held cellphones impact the economic marketplace. This is due to the fact that automobile insurers pay for losses incurred by drivers in an automobile accident. In this way, the total amount of losses incurred by a given insurer in a given marketplace is directly correlated with the frequency and severity of automobile accidents in that marketplace. We therefore expect that, if cellphone laws alter drivers' behavior in ways that reduce accident rates in a given marketplace, these laws should also reduce the amount of losses incurred by automobile insurers operating in that marketplace.

The insurance literature does provide evidence that legislative activity, unrelated to the use of a cellphone while driving, affects drivers' behaviors in ways that have consequences for the automobile insurance marketplace. This is most notable in studies that examine no-fault automobile insurance. For example, Cummins et al. (2001) provide evidence that the enactment of no-fault automobile insurance

laws led to higher fatal accident rates. In addition, Heaton (2015) provides evidence that automobile insurance expenditures fall when no-fault laws are repealed.

However, to our knowledge, no study in the insurance literature has considered the effect of hand-held cellphone bans on the automobile insurance market despite the fact that the sheer size and economic importance of the automobile insurance market suggests it is important to do so. Thus, even slight decreases in accident rates brought about by cellphone bans could potentially amount to millions of dollars in reduced loss payments and free up capital for insurers to do any number of things, such as invest in new positive NPV projects, improve the bottom line, acquire additional capital market assets, or even reduce premiums for policyholders. The automobile insurance market, then, provides an excellent non-laboratory setting to evaluate the economic consequences of individuals' behavioral responses to laws banning the hand-held use of a cellphone while driving. The remainder of this paper is therefore devoted to performing an empirical analysis of the extent to which hand-held cellphone bans reduce the amount of losses incurred by automobile insurers.

Discussion of Sample

Similar to previous studies of how the enactment of various laws affect insurance markets (e.g. Viscusi and Born, 2005), we study how the enactment of a cellphone ban influences the operations of a given insurer, in a given state, during a given year and we utilize data from various sources to compile our firm-state-year dataset. Data pertaining to the financial operations of automobile insurers are acquired from the National Association of Insurance Commissioners' (NAIC) Property-Liability database, which captures the operations of virtually all of the automobile insurers operating in the U.S. From this database, we obtain the dollar amount of premiums earned (*Premiums*) and losses incurred (*Losses*) in the line private passenger auto physical damage by a given insurer, in a given state, during a given year. When we divide *Losses* by *Premiums*, we are also able to obtain the private passenger automobile physical damage *Loss Ratio*, which is a common measure of underwriting performance examined in the insurance literature (e.g. Grace and Leverty, 2013).

Focusing on the private passenger automobile insurance market has the important advantage of allowing for a cleaner identification of the effects of hand-held cellphone bans. This is because the amount of automobile liability losses incurred by a given insurer is influenced by factors in the tort and medical system, such as attorneys' fees, length of litigation, the presence of tort reform, or the cost of medical care. Isolating the effects of cellphone laws on automobile liability losses in the presence of these confounding factors is difficult, at best. Since physical damage losses paid by insurers are for the actual cash value of the damages to an insured's owned automobile, far fewer confounding factors could explain variation in the amount of losses paid by automobile insurers and greatly increases the likelihood that we will be able to correctly identify the effects of cellphone bans on the automobile insurance market.

In addition to *Losses*, *Premiums*, and *Loss Ratio*, the NAIC data allows us to collect other information that will serve as controls in our econometric analysis. This information includes whether the insurer is organized as a stock insurance company (*Stock*), whether the insurer is affiliated with an insurer group (*Group*), and the total number of states that the insurer has positive private passenger automobile physical damage premiums (*States*). We also use the A.M. Best database to collect additional firm-specific information, including whether the firm uses a direct writer marketing system (*Direct*) and whether the firm has an AM Best rating of "A+" or better (*A_Rating*).

We then collect additional state-level data that will serve as additional controls in our analysis. We examine state statutes, prior studies (e.g. Grace and Phillips, 2008; Anderson et al., 2010), and various years of the NAIC's Compendium of State Laws and Insurance Topics to identify whether a state has a no-fault automobile insurance law (*No_Fault*) or a prior approval law for automobile insurance (*Prior_Approval*) in effect during a given year. From the Bureau of Labor and Statistics (BLS) we collect information regarding the number of persons residing in a given state (*Population*), the proportion of the population that is unemployed (*Unemployment*), and the median income of the population (*Income*). We also use the Federal Highway Administration's Highway Statistics Series Publications for various years to collect the total number of licensed drivers (*Drivers*), the number of licensed drivers per square mile (*Density*), and the proportion of licensed drivers under the age of 19 years old (*Young)*. The Database of

State Tort Law Reforms (Avraham, 2014; DSTLR 5th) is also used to identify the years in which caps on noneconomic damages (*NE*), caps on punitive damages (*CP*), reforms to collateral source rules (*CS*), and modifications to joint and several liability rules (*JS*) became effective in a given state.[2]

Finally, we obtain data pertaining to the hand-held cellphone laws from the appendix provided in McCartt et al. (2014). We verify these data by examining other studies as well as data provided by the Insurance Institute for Highway Safety. Using these data, we identify states that enacted a ban on the hand-held use of a cellphone for teenage and/or all drivers. We then create *HH_Ban*, which indicates whether a given state had a hand-held cellphone ban in effect during a given year.

When we combine all of our data sources, we institute the standard filters imposed in insurance studies. Specifically, we exclude from our sample all firms with illogical or missing values for assets, surplus, AM Best ratings, and premiums and we also exclude all professional reinsurers. Additionally, to ensure that our sample consists of insurers actively participating in the automobile insurance market, we exclude all firm-state observations of insurers with less than $100,000 in private passenger automobile physical damage premiums written. After applying the filters, we arrive at a dataset consisting of 52,582 firm-state-year observations of private passenger automobile physical damage insurers operating across all 50 states (and the District of Colombia) between the years 2000 and 2010. Summary statistics are reported in Table 1 and Appendix A provides more detail on the definitions of each variable in our dataset.

Our sample period is choses to generally align with the sample periods of prior studies of cellphone bans on accident rates (e.g. Lim and Chi, 2013; Jacobson et al., 2012; Kolko, 2009). While it is possible that cellphone laws enacted after the end of the sample period affected driver behavior, the

[2] Caps on noneconomic damages place limits on amounts awarded to injured parties for pain and suffering, emotional distress, loss of consortium, and similar non-pecuniary losses. Punitive damage caps limit the amount that defendants are required to pay as a result of intentional or malicious conduct. Collateral source reforms place restrictions on the sources from which plaintiffs can collect awards. Joint and several liability reforms place restrictions on the assignment of liability to two or more parties that are potentially liable for a tort and require each guilty party to pay damages commensurate with their level of responsibility for the tort. For more detailed discussion of specific tort liability reforms in insurance markets, see, for example, Grace and Leverty (2013) and Viscusi and Born (2005).

literature strongly suggests that this behavioral change did, in fact, occur during our chosen sample period. Thus, 2000 – 2010 is the most appropriate period to examine whether the behavior changes brought about by the enactment of hand-held cellphone bans had any effect on automobile insurers' losses, premiums, and underwriting performance.

Our sample period is also characterized by considerable heterogeneity in states' decisions to institute a law prohibiting the hand-held use of a cellphone while driving. Figure 1 depicts the cellphone law ban activity during our sample period and Appendix B reports the years that each state enacted any type of hand-held cellphone ban up to 2010. In total, 32 states enacted a law that prohibited part or all of the population from using a hand-held cellphone while driving. As is apparent from the figure, there is also heterogeneity in the years during which states enacted cellphone laws. Our empirical strategy is exploit this heterogeneity in both the number and timing of states' decisions to ban drivers from using hand-held cellphones in order to estimate the effect of the laws on the automobile insurance market.

Methods and Results

Our empirical model examines the extent to which hand-held cellphone bans, via their effect on drivers' behaviors, influence automobile insurer's losses, premiums, and underwriting performance. To investigate this relationship for firm *i* in state *j* during year *t*, we estimate a series of regression models that take the general form of:

$$Y_{ijt} = \alpha + \beta_1 HH\_Ban_{jt} + \theta' F_{it} + \eta' X_{jt} + \sum_{t=2000}^{2010} \tau_t T_t + \sum_{j=1}^{51} \delta_j S_j + \varepsilon_{ijt} \qquad 1)$$

Here, the variable *Y* is the natural logarithm of *Losses*, *Premiums*, or *Loss Ratio*, depending on the specification. *HH_Ban* is, as previously defined, an indicator of the presence of a hand-held cellphone law ban in effect in state *j* during year *t* and is our variable of interests in the model. If cellphone laws do alter

drivers' behaviors in ways that reduce the losses, premiums, and loss ratios of automobile insurers, we would observe a negative and statistically significant coefficient on *HH_Ban*.

F is a vector of time-varying firm characteristics and *X* is a vector of time-varying state characteristics, both of which serve as firm-specific and state-specific controls. The variables included in each vector were chosen with the aid of prior literature (e.g. Cummins et al., 2001; Viscusi and Born, 2005; Grace and Phillips, 2008; Born et al., 2009; Weiss et al., 2010). The variables specifically included in *F* are *Stock*, *Group*, *States*, *Direct*, and *A_Rating* while the vector *X* is comprise of *No_Fault*, *Prior_Approval*, *Drivers*, *Density*, *Young*, *Unemployment*, *Income*, *Gas_Tax*, *NE*, *JS*, *CP*, and *CS*. Per Viscusi and Born (2005), we also include the lag of the dependent variables in *F* to control for the fact that the particular risk or pricing portfolio of a particular insurer is likely to continue in the future. Finally, when *Losses* is the dependent variable, *Premiums* is included in *F* to control for the fact that total levels of losses incurred will be conditional on the amount of business a given insurer writes (e.g. Viscusi and Born, 2005).

All models also contain state and year dummies to control for unobserved factors, unrelated to hand-held cellphone laws, which could affect any or all of our dependent variables. A time trend is also included in all models to control for general trends in marketplace conditions over time. Note that, with the exception of indicator variables, all variables are transformed by taking the natural logarithm, so as to improve normality and easily facilitate economic interpretations. Finally, for all models presented in our analysis, standard errors are clustered at the firm-state level and are robust to heteroskedacity.

Note that our modeling approach assumes that *HH_Ban* is exogenously determined, an assumption that every study of hand-held cellphone bans and accident rates, discussed previously, also makes. Other studies of cellphone-related laws offer further evidence that suggests our assumption of exogeneity is valid (e.g. Abouk and Adams, 2012), as do other studies of regulatory activity surrounding insurance markets (e.g. Viscusi and Born, 2005; Born and Neale, 2013; Heaton, 2015).

Table 2 reports the results of estimating equation 1 and the first column reports the specification when the dependent variable is *Losses*. In that model, the coefficient on *HH_Ban* is -.013 and is

statistically significant at the 10 percent level (p-value = .066) which suggests that automobile insurers incur fewer losses in states that prohibit the hand-held use of a cellphone by drivers. In particular, the coefficient indicates that, in the post cellphone ban period, the average automobile insurer incurs approximately 1.3 percent fewer losses in states with a ban on hand-held cellphone use relative to states with no ban in place. As given in column 2 of the table, we also find evidence that the presence of a hand-held cellphone ban reduces the loss ratio of automobile insurers. Here, the coefficient on *HH_Ban* is negative and statistically significant at the 5 percent level (p-value = .046) and the magnitude of the coefficient suggests loss ratios in states enacting a ban are reduced, on average, by approximately 1.2 percent in the years following the ban. As given in column 3, we find no evidence of a statistically significant relation between the enactment of a hand-held cellphone ban and *Premiums*.

Among all three models given in Table 2, many of our control variables are statistically significant. In particular, we find that *Stock*, *States*, *Density, Unemployment, Gas_Tax*, and *NE* are, in general, inversely related to *Losses, Loss Ratios,* and *Premiums* in statistically meaningful ways. *Direct* and *CS* are generally positively related to all of the dependent variables considered in our analysis. In addition, we find evidence that other controls are correlated with *Losses, Loss Ratios*, or *Premiums* in varying ways, depending on the specification. The fact that *HH_Ban* is statistically significant in two of our models, despited the presence of many significant control variables, increases the likelihood that our models properly identify the effect of hand-held cellphone bans on the automobile insurance market.

Considered jointly, the models presented in Table 2 suggest that laws prohibiting drivers from using cellphones alter drivers' behaviors in ways that reduce the losses incurred and loss ratios of private passenger automobile physical damage insurers. The same laws, however, do not appear to have any meaningful effect on premiums. As noted by Grace and Leverty (2013), when a law reduces losses but has no impact on premiums, the law is said to benefit both consumers and insurance companies. Thus, our evidence suggests that the effects of hand-held cellphone bans spillover into the automobile insurance market and are associated with economic benefit. In the ensuing section, we explore the robustness of our results and the associated conclusions.

Specification Checks

Table 3 presents the results from several alternative model specifications in an effort to examine the robustness of the results presented in Table 2, above (henceforth the "main" models). For the sake of brevity, only the results of the variable of interest are displayed and the estimated coefficients of all other covariates are not included. However, unless otherwise noted, all specifications reported in Table 3 include the full vector of firm controls (*F*) and state controls (*X*) described in the previous section as well as year dummies, state dummies, a time trend, and firm-state clustered standard errors.

The first robustness check we perform is to estimate the main models but additionally include firm dummy variables, so as to ensure that some unobserved firm effect is not responsible for our results. Note that *Stock*, *Group*, and *Direct* are removed from the specification because they vary little between years within a given firm. However, all other control variables are included. As given in Table 3, *HH_Ban* remains negative (positive) and statistically significant (insignificant) in the *Loss* and *Loss Ratio* (*Premium*) equation. In fact, when compared to the main model, the statistical significance of *HH_Ban* slightly improves to the 5 percent level when firm effects are included. In addition, including the firm effects only slightly changes the magnitude of the coefficient by between .02 and .03, meaning that the economic interpretation reported in the main models remains largely unchanged.

Our next robustness check is to remove the lag of the dependent variables (i.e. *Lag_Losses*, *Lag_Loss_Ratio*, and *Lag_Premiums*) in each specification. While the lags were included as controls to be consistent with prior studies, they could create some econometric concerns regarding the model specification. However, as given in the second row of Table 3, removing the lags does not alter our main result that mean levels of losses incurred and incurred loss ratios are lower for insurers operating in states with bans on the hand-held use of a cellphone while driving. Omitting lags also does not significantly change the magnitude of the coefficients.

In the third row of Table 3, we report an additional specification check whereby we control for the presence of a law that prohibits texting while driving. As noted earlier, there exist sparse empirical

evidence pertaining to drivers' texting behaviors, making it difficult for researchers to document the effect of these laws. In addition, banning the hand-held use of a cellphone while driving also implicitly bans texting while driving, meaning that *HH_Ban* should account for much the accident-reducing effects of bans on texting. Nevertheless, some studies do find evidence that texting laws may lead to a slight reduction in accident rates (e.g. Ferdinand et al, 2015), therefore potentially affecting automobile insurance losses. We therefore reestimate the main models but include a dummy variable indicating the presence of a texting ban in a given state during a given year and find that, even after controlling for texting laws, a negative and significant relation remains between hand-held cellphone bans and *Losses* as well as *Loss_Ratio*. In addition, the magnitude of *HH_Ban* increases slightly, suggesting that the effect of hand-held bans is greater for insurers once the effects of texting laws are held constant.

As given in the fourth row of Table 3, we also reestimate our main models on a sample of private passenger automobile physical damage insurers operating between 2003 and 2013. While we chose our original sample period of 2000 – 2010 to be consistent with prior empirical evidence on the effects of cellphone bans, we recognize that cellphone ownership is potentially more prevalent during the latter part of our sample period and the effects of hand-held bans might be different in more recent years. However, we find that insurers incur fewer losses and lower loss ratios in states with hand-held cellphone bans, even after updating the sample period. Interestingly, the magnitude of the coefficient on *HH_Ban* in the *Loss* equation indicates losses are reduced by approximately 2.7 percent in the presence of a hand-held cellphone ban between 2003 and 2013, which is approximately 1.4 percent greater than the effect observed in the main models.

Our final robustness check is to reestimate the main models but additionally include a set of variables indicating the time period before and after a hand-held ban was enacted in a given state. This is a robustness check frequently employed in studies of the effects of laws on economic marketplaces (e.g. Frakes, 2013; Heaton, 2015). Table 4 displays the results of this specification and the variables of interest are *HH_Ban_N*, which indicate the years before/after a ban was enacted in a state that a given insurer

operates.[3] As given in the table, only one of the pre-law time indicator variables is statistically significant and a joint f-test of the pre-law indicator coefficients fail to reject the null of joint significance. Thus further suggests that our assumption that the enactment of hand-held cellphone bans is exogenous is valid because, if our results were due to mean-reversion or reverse causality, we would expect the pre-law coefficients to be statistically relevant.

Another important result in Table 4 is that, in the *Loss* and *Loss Ratio* models, virtually all of the post-law indicator variables are negative and statistically significant. This indicates that the enactment of a hand-held ban on cellphone use while driving has the real and persistent effect of reducing losses incurred and loss ratios. This result becomes evident when we graph the coefficients of the law time indicator variables, as is given in Figure 2. The figure clearly shows that losses and loss ratios of insurers operating in states about to enact a hand-held cellphone ban are not significantly different from insurers operating in states not about to enact a similar ban (i.e. time -4 to -1). However, once the ban is enacted, insurers experience a sudden and sharp decline in losses and loss ratios (i.e. time 0) and the levels of losses incurred and loss ratios continue to remain lower in the post-law period (i.e. time 1 to 3) for insurers operating in states with bans on the hand-held use of cellphones.

Conclusion

Our analysis examines whether the presence of a law banning the hand-held use of a cellphone while driving has any influence on the automobile insurance market. Our research is motivated by prior studies that suggest these laws alter drivers' behaviors in ways that reduce the risk of an automobile insurance accident. Because automobile insurers incur losses on behalf of insureds in automobile accidents, it follows that hand-held cellphone bans should also lead to reductions in the amounts of losses incurred by automobile insurers. However, no prior study has considered this topic.

[3] For example, *HH_Ban_t-3* is equal to 1 if a given insurer operates in a given state that enacted a ban on hand-held cellphones three years later. Concurrently, *HH_Ban_t+3* is equal to 1 if a given insurer operates in a given state that enacted a ban on hand-held cellphones three years prior.

Our analysis of private passenger automobile physical damage insurers from 2000 to 2010 suggests that hand-held cellphone bans to have positive effects that spillover into the insurance market. In particular, we find that insurers incur approximately 1.2 percent fewer losses in in states with a hand-held cellphone ban. We also find similar beneficial effects of these bans on loss ratios but no significant impact on premiums. Our conclusion is that laws banning the hand-held use of a cellphone while driving elicit a behavioral response from drivers that ultimately leads to fewer loses paid by automobile insurers.

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Table 1: Summary Statistics (N = 52,582)

Variable	Mean	Std. Dev.
Losses	7.06	1.81
Loss Ratio	-0.64	0.42
Premiums	7.71	1.70
HH_Ban	0.22	0.41
Stock	0.81	0.39
Group	0.96	0.20
Direct	0.21	0.41
Size	13.34	1.95
A_Rating	0.46	0.50
States	2.72	1.10
No_Fault	0.24	0.42
Prior_Approval	0.39	0.49
Drivers	14.98	0.94
Density	3.83	1.33
Young	-3.00	0.28
Unemployment	1.69	0.34
Income	10.72	0.16
Gas_Tax	3.01	0.29
NE	0.31	0.46
JS	0.74	0.44
CP	0.47	0.50
CS	0.66	0.48

Table 2: Main Models

VARIABLES	(1) Losses	(2) Loss_Ratio	(3) Premiums
HH_Ban	-0.013*	-0.012**	0.013
	[0.007]	[0.006]	[0.011]
Premiums	1.013***		
	[0.006]		
Lag_Losses	0.032***		
	[0.005]		
Lag_Loss_Ratio		0.361***	
		[0.011]	
Lag_Premiums			0.838***
			[0.004]
Stock	-0.035***	-0.043***	-0.050***
	[0.007]	[0.005]	[0.011]
Group	-0.008	-0.002	0.021
	[0.013]	[0.009]	[0.018]
Direct	0.071***	0.058***	0.115***
	[0.007]	[0.005]	[0.010]
Size	-0.021***	-0.006***	0.023***
	[0.002]	[0.001]	[0.003]
A_Rating	-0.012**	0.004	0.109***
	[0.006]	[0.004]	[0.009]
States	-0.005	-0.011***	-0.065***
	[0.004]	[0.003]	[0.005]
No_Fault	-0.004	0.002	0.046
	[0.029]	[0.022]	[0.043]
Prior_Approval	0.025	0.032*	-0.068**
	[0.023]	[0.017]	[0.033]
Drivers	-0.037	0.022	-0.089
	[0.061]	[0.053]	[0.093]
Density	-0.069***	-0.084***	0.037
	[0.022]	[0.021]	[0.034]
Young	-0.005	-0.005	-0.029
	[0.018]	[0.016]	[0.024]
Unemployment	-0.113***	-0.068***	-0.082***
	[0.018]	[0.016]	[0.025]
Income	-0.053	0.024	0.179***
	[0.047]	[0.041]	[0.068]
Gas_Tax	-0.091***	-0.067***	-0.024
	[0.029]	[0.024]	[0.045]
NE	-0.009	-0.013*	-0.017

	[0.010]	[0.008]	[0.015]
JS	0.018	0.010	-0.015
	[0.012]	[0.010]	[0.020]
CP	0.018	0.011	0.063***
	[0.015]	[0.012]	[0.023]
CS	0.029**	0.026**	-0.012
	[0.014]	[0.012]	[0.023]
Constant	0.718	-0.662	0.555
	[0.943]	[0.824]	[1.422]
Year Dummies?	Yes	Yes	Yes
State Dummies?	Yes	Yes	Yes
Time Trend?	Yes	Yes	Yes
Observations	52,582	52,582	52,582
R-squared	0.954	0.234	0.888

Robust standard errors in brackets

*** p<0.01, ** p<0.05, * p<0.1

Table 3: Robustness Checks

			Dependent Variables		
	Specification	**Variable of Interest**	**Losses**	**Loss Ratio**	**Premiums**
1	Include Firm Effects	*HH_Ban*	-0.016**	-0.014**	0.011
			[0.007]	[0.006]	[0.010]
2	Remove Lags of Y	*HH_Ban*	-0.013*	-0.013*	0.019
			[0.007]	[0.007]	[0.023]
3	Control for Texting Laws	*HH_Ban*	-0.020**	-0.021***	-0.011
			[0.009]	[0.008]	[0.014]
4	Examine 2003 to 2013 Sample	*HH_Ban*	-0.027***	-0.015***	0.002
			[0.007]	[0.006]	[0.010]
	Firm Controls Included?		Yes	Yes	Yes
	State Controls Included?		Yes	Yes	Yes
	Year Dummies Included?		Yes	Yes	Yes
	State Dummies Included?		Yes	Yes	Yes
	Firm-State Clustered Errors		Yes	Yes	Yes

Table 4: Dynamic Specification Results

VARIABLES	(1) Losses	(2) Loss_Ratio	(3) Premiums
HH_Ban_t-4	0.004	0.010	-0.008
	[0.008]	[0.008]	[0.011]
HH_Ban_t-3	0.019**	0.018**	0.022*
	[0.008]	[0.008]	[0.013]
HH_Ban_t-2	-0.006	-0.009	0.002
	[0.008]	[0.008]	[0.013]
HH_Ban_t-1	0.001	0.005	0.005
	[0.008]	[0.007]	[0.012]
HH_Ban_t	-0.034***	-0.033***	0.023*
	[0.008]	[0.008]	[0.013]
HH_Ban_t+1	-0.033***	-0.018**	0.014
	[0.009]	[0.008]	[0.014]
HH_Ban_t+2	-0.022**	-0.012	0.001
	[0.010]	[0.009]	[0.015]
HH_Ban_t+3	-0.021**	-0.021**	0.007
	[0.010]	[0.010]	[0.017]
Premiums	1.013***		
	[0.006]		
Lag_Losses	0.032***		
	[0.005]		
Lag_Loss_Ratio		0.361***	
		[0.011]	
Lag_Premiums			0.838***
			[0.004]
Stock	-0.035***	-0.043***	-0.050***
	[0.007]	[0.005]	[0.011]
Group	-0.009	-0.003	0.021
	[0.013]	[0.009]	[0.018]
Direct	0.071***	0.058***	0.116***
	[0.007]	[0.005]	[0.010]
Size	-0.021***	-0.006***	0.023***
	[0.002]	[0.001]	[0.003]
A_Rating	-0.012**	0.004	0.109***
	[0.006]	[0.004]	[0.009]
States	-0.004	-0.011***	-0.065***
	[0.004]	[0.003]	[0.005]
No_Fault	-0.007	0.000	0.035
	[0.029]	[0.022]	[0.043]
Prior_Approval	0.027	0.035**	-0.074**
	[0.022]	[0.017]	[0.033]
Drivers	-0.011	0.039	-0.094

	[0.062]	[0.054]	[0.093]
Density	-0.074***	-0.090***	0.044
	[0.022]	[0.021]	[0.035]
Young	-0.001	-0.002	-0.030
	[0.018]	[0.016]	[0.024]
Unemployment	-0.116***	-0.072***	-0.081***
	[0.018]	[0.016]	[0.025]
Income	-0.054	0.017	0.187***
	[0.047]	[0.042]	[0.068]
Gas_Tax	-0.092***	-0.071***	-0.021
	[0.030]	[0.024]	[0.046]
NE	-0.010	-0.013*	-0.019
	[0.010]	[0.008]	[0.015]
JS	0.017	0.010	-0.015
	[0.012]	[0.010]	[0.020]
CP	0.015	0.009	0.062***
	[0.015]	[0.012]	[0.023]
CS	0.029**	0.027**	-0.014
	[0.014]	[0.012]	[0.023]
Constant	0.404	-0.793	0.556
	[0.945]	[0.824]	[1.433]
Year Dummies?	Yes	Yes	Yes
State Dummies?	Yes	Yes	Yes
Time Trend?	Yes	Yes	Yes
Observations	52,582	52,582	52,582
R-squared	0.954	0.235	0.888

Robust standard errors in brackets

*** p<0.01, ** p<0.05, * p<0.1

Figure 1: Number of States Enacting Hand-Held Cellphone Bans in a Given Year (2000 – 2010)

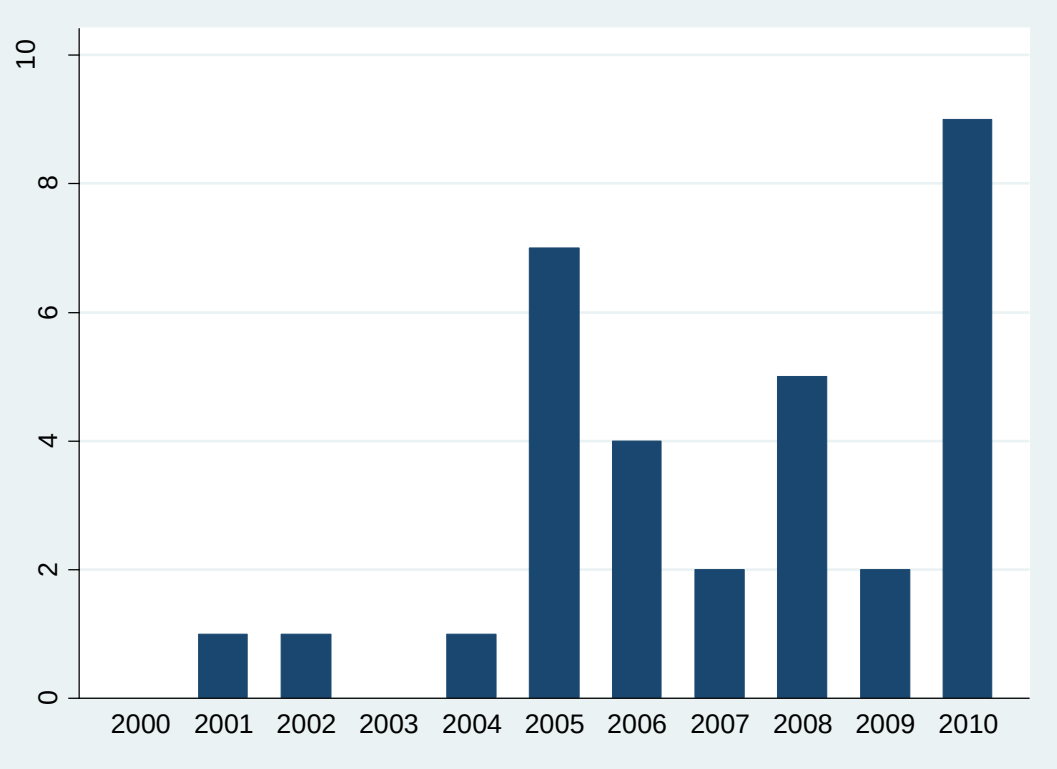
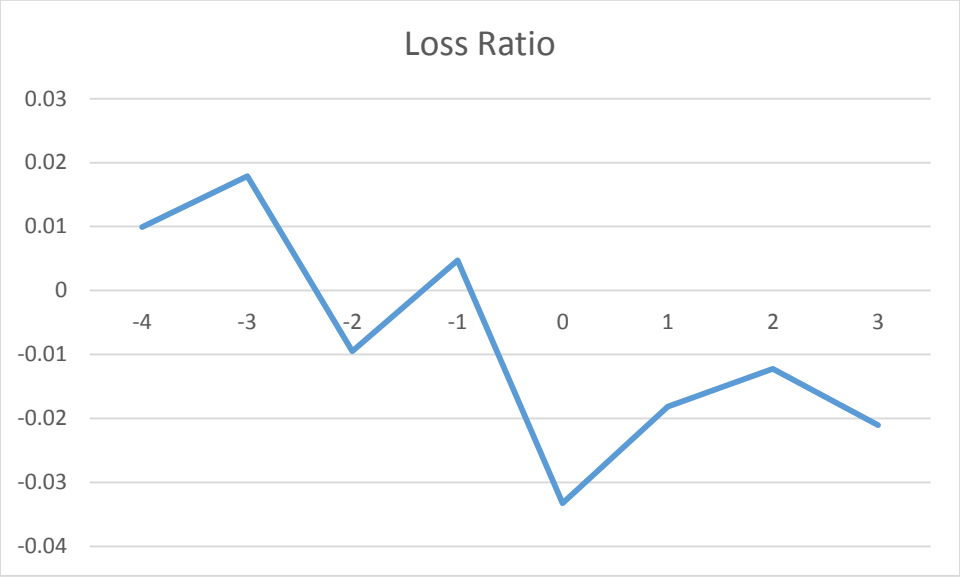


Figure 2: Dynamic Effects of Hand-Held Cellphone Bans on Automobile Losses and Loss Ratios



Appendix

State	Year of Hand-Held Ban Enactment
AL	2010
AR	2009
CA	2008
CO	2005
CT	2005
DC	2004
DE	2005
GA	2010
IA	2010
IL	2005
IN	2009
KS	2010
KY	2010
LA	2008
MA	2010
MD	2005
ME	2010
MN	2006
NC	2006
NE	2008
NJ	2002
NY	2001
OK	2010
OR	2008
RI	2006
TN	2005
TX	2005
VA	2007
VT	2010
WA	2008
WV	2006
WY	2007