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Texting Bans and Fatal Accidents on Roadways: Do They Work? Or Do Drivers Just React to Announcements of Bans?†

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Since 2007, many states passed laws prohibiting text messaging while driving. Using vehicular fatality data from across the United States and standard difference-in-differences techniques, bans appear moderately successful at reducing single-vehicle, single-occupant accidents if bans are universally applied and enforced as a primary offense. Bans enforced as secondary offenses, however, have at best no effect on accidents. Any reduction in accidents following texting bans is short-lived, however, with accidents returning to near former levels within a few months. This is suggestive of drivers reacting to the announcement of the legislation only to return to old habits shortly afterward. (*JEL* D12, K42, R41)

Using the text message feature of mobile devices while driving is thought by some to be the most dangerous thing one can do while attempting to operate a motor vehicle. According to an experiment carried out in 2009 by *Car and Driver*, text messaging has a greater negative impact on safely operating a motor vehicle than being drunk.[1] Text messaging is part of what has generally been seen as the broad new scourge of the roadways, distracted driving. While accidents in general are on the decline, and those attributable to drunk driving have been somewhat curbed by a myriad of legislative actions and public awareness campaigns, the National Highway Traffic Safety Administration (NHTSA) reported a steady increase in fatalities caused by distracted drivers from 2004–2008. During the period, over 25,000 fatalities were estimated to be caused by a distracted driver (NHTSA 2009). Distracted driving is a rather broad concept, however, including drivers preoccupied with texting, talking to a passenger, eating, reading, or using global positioning systems.

Although encompassing a variety of sources, the recent upward swing in the fatalities attributable to distracted driving has coincided with an upward trend in text messaging in particular. In 2000, the number of cell phone subscribers was

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[1] See http://wheels.blogs.nytimes.com/2009/06/25/texting-is-more-dangerous-than-driving-drunk/.

under 100 million, but by the end of 2008, this number reached over 250 million. According to an International Association for the Wireless Telecommunications Industry (CTIA) report, over 2 trillion text messages were sent in 2011, which is almost 20 times the number sent in 2006 (CTIA 2012).

To mitigate the portion of fatal distracted driving accidents caused by sending or receiving text messages, many states have banned texting while driving. Washington was the first state to do so, and they were followed by 32 other states through January 2012. In this paper, we conduct a set of tests to determine whether there was a reduction in fatal accidents following state bans on text messaging. By limiting attention to those crashes that are most likely the result of distracted drivers sending messages, specifically single-vehicle accidents with a sole occupant crashing into a nonvehicular object, we isolate whether the bans have their expected effect. The evidence is highly suggestive that the bans can reduce the number of such crashes if legislation is universally applied and enforced as a primary offense.[2]

The most important finding, however, is that while the reduction in the number of accidents is substantial in the month following a ban, the effect begins to decline rapidly. Thus, drivers appear to be reacting to bans by initially altering their behavior, only to return to normal behavior later. Drivers are likely reacting to limited enforcement of bans or learning new ways to evade detection. We present evidence consistent with these explanations as bans with more limited enforcement or coverage seem to result in drivers returning to old behaviors more rapidly.

The rest of the paper is organized as follows. Section I provides background on related literature. In Section II, we introduce the data and methodology we employ in our study. Section III presents and discusses the basic difference-in-differences results. Section IV analyzes effects over time, providing the evidence that drivers are likely reacting to the announcement of a ban rather than permanently changing their behavior. Section V concludes.

I. Background

A number of studies have assessed the risk of cell phones, with McEvoy et al. (2005) presenting compelling evidence that using a phone while driving increases the accident risk fourfold. Recently, the Virginia Tech Transportation Institute (VTTI) (Klauer et al. 2006; VTTI 2009) completed several naturalistic driving studies to assess the risk of cell phones. This technique uses cameras and instrumentation in vehicles to determine the heightened risk of certain driving behaviors. Cell phones were shown to increase the risk of accidents and near-accidents by anywhere from 1.3 times in the case of talking on a phone to 5.9 times in the case of dialing a phone. They also looked at text messaging specifically, which they found increases the likelihood of a crash or near crash event by 23 times. The fact that text messaging could

[2] By primary offense, we mean that law enforcement officials can stop someone suspected to be texting while driving. No other offense needs to be committed. Texting bans that are secondarily enforced require a driver be stopped for a separate infraction. We find no evidence suggesting these latter types of secondarily enforced bans reduce accidents.

be nearly 20 times more dangerous than talking on a cell phone means that it likely merits particular attention from policy makers and researchers.

Despite the widespread belief and evidence that cell phone use while driving is dangerous, drivers still continue to engage in the risky behavior (Nelson, Atchley, and Little 2009). This may be suggestive that drivers underestimate the risk associated with their own use of phones while driving. In fact, a recent survey of new and prospective teen drivers performed by State Farm Insurance and Harris Interactive shows that 36 percent believe texting and driving can be more fatal. Despite texting while driving being at least as dangerous as driving drunk, many more teenagers (55 percent) believe drinking and driving can be more fatal than text messaging.[3] This underestimation of risk may lead to more texting than is socially desirable and a market failure that legislation could potentially correct.

If those who text and drive underestimate the risk to themselves, they likely would not internalize the costs they impose on others. Pedestrians and cyclists are at risk of being injured or killed by distracted drivers. Property damage could be caused by those texting and driving. Moreover, costs associated with responses to accident scenes, emergent care, and increased traffic congestion following accidents all likely lead to a negative externality that could justify government intervention.

Wilson and Stimpson (2010) find substantial linkages between cell phone texting volumes and deaths from distracted driving. They conclude that in the absence of text messaging, predicted fatalities from distracted driving would have declined from 2001 to 2007 instead of increasing. Their estimates suggest about 2,690 deaths per year were attributable to drivers text messaging.

Given that texting while driving has only recently been banned by states, studies of the effectiveness of bans like the one we undertake are limited. Prior to banning texting, however, a few states acted to limit speaking on cell phones while driving with the hope of encouraging drivers to use hands-free devices more generally.[4] The studies of the impact of these regulations have been narrow as well because of the limited number of laws and the peculiarities of their provisions. Nikolaev, Robbins, and Jacobson (2010) is one exception, as they investigated accident rates in New York. New York was the first state to pass a comprehensive ban on the use of handheld cell phones while driving in late 2001. Using a cross-county analysis, they find significant reductions in fatal accident rates. Sampaio (2010) correctly note, however, that Nikolaev, Robbins, and Jacobson (2010) only looked at New York and failed to account for underlying trends in accident rates. Proper analyses of policies restricting cell phone use necessitate cross-state analyses to infer a causal effect, and Sampaio (2010) shows that the Nikolaev, Robbins, and Jacobson's (2010) findings are a combination of a ban effect and factors that are unobservable in their study. We also note that those speaking on a cell phone are only 1.3 times more likely to get into an accident or near accident (VTTI 2009), so general restrictions on speaking on cell phones may have limited impact unless there are some specific text message provisions.

[3] See http://www.harrisinteractive.com/vault/State-Farm-Teens-Texting-2010-09-20.pdf for the complete study details.

[4] Variation of such general cell phone legislation was insufficient during our sample period to include as a control variable, but we did use information on existing handheld bans to perform several additional tests later in the paper.

The Highway Loss Data Institute (HLDI 2010) provides the only study known to us that specifically tests the effect of texting bans on crashes. The HLDI use collision claim frequencies in four states to assess the impact of bans. Considering California, Louisiana, Minnesota, and Washington as treated states and using neighboring states as control states, the authors find that bans on text messaging actually were followed by an increase in collision claims. They control for collision-level variables, such as vehicle model year, driver age groups, gender, marital status, garaging state, vehicle density, and year and month. The most notable case in their data was California, for which they find a large and significant increase in the number of collisions after the state passed a ban. This increase was also observed in Louisiana and Minnesota. The increase in collisions is an unexpected result, and the justification provided by the authors is that texting bans encourage drivers to hide their phones while they are texting. Consequently, they are even less cognizant of the road than they would be had they been allowed to text in the open.

The HLDI (2010) study has received some attention in the popular press because of its surprising findings, but there are several reasons to question the approach. First, the authors use insurance collision claims as a measure of accident rates. Although they have the advantage of isolating accidents in which a driver is culpable, they miss all accidents for which claims were not filed. This selected sample partially explains the curious finding. Most existing research on the effects of public policies on traffic accidents focuses on censuses of fatal accidents rather than self-reported claims (e.g., Dee 2001, Eisenberg 2003, and Carpenter and Dobkin 2009). Second, HLDI combines all types of accidents, whether they involve single or multiple drivers or vehicles. This includes many accidents for which no effect is expected, a point we take up later using our data.[5] Finally, any analysis of the effect of texting bans needs to assess the lead and lagged effects of the legislation. Given the bans HLDI studied were passed in January, July, and August, with December and July being particularly accident-heavy months, the lead effects are likely needed. Moreover, the lagged effects would assess whether the bans took a few months to become effective or waned in terms of impact after some time.

In our study, we aim to advance the understanding of the effect of texting on traffic safety by modeling our strategy after the strengths of the existing studies while overcoming some of the limitations. Specifically, we exploit cross-state variation in the implementation of texting bans to identify the unique effects of texting on driving safety, separating the more strongly enforced bans from the weaker bans. Most importantly, we test for effects of the legislation over time, with the aim of assessing whether there is an announcement effect of the legislation. That is, we are concerned that a texting ban is followed by an immediate reaction by drivers and law enforcement, only to have everyone revert to prior behavior after a number of months. Announcement effects have long been recognized in financial markets, as investors react immediately to new information (e.g., Barclay and Litzenberger 1988). It is not surprising that drivers react similarly to investors, as they observe the extent to

[5] We also add that limiting attention to just a handful of states has problems as well, particularly given the variation in the impact of legislation we observe in Table 5. Considering that bans have recently extended to dozens of states, a nationwide analysis will allow for more data points to assess the effect of bans.

which the law will be enforced or learn new ways to not be detected, such as hiding their phone from view.[6] Given past experience with similar legislation curbing cell phone use, this pattern of behavior is certainly plausible (McCarrt, Braver, and Geary 2003; McCarrt and Geary 2004).

II. Data and Methodology

A. *Crash Data and Information of Texting Bans*

The crash data used in this study come from the Fatality Analysis Reporting System (FARS) of the NHTSA, which is a nationwide census for all motor vehicle crash fatalities. We are interested in using the crash-level information to determine whether the accident included a single-vehicle with a single-occupant. Wilson and Stimpson's (2010) data show that the large jump in distracted accidents from 2001–2007 is mirrored by increases in the proportion of distracted accidents involving single-vehicles and single drivers. These patterns in the data also make intuitive sense. A driver with passengers might be less willing to put them in danger by texting. Moreover, they may find less need to text if someone is there to speak with them or stop them from texting if they perceived the risk to be dangerous. Multiple vehicle accidents typically are caused by more than one factor since there are multiple drivers. In picking a single group of accidents or a set of accident types to assess the effect of a policy is similar to the approach taken in the drunk driving literature before more advanced means of imputing blood alcohol content from crash scene variables were developed. For example, Eisenberg (2003) used crashes that occurred at night or on the weekend to infer those most likely to be associated with driving drunk.[7] We use monthly data from 2007–2010 on fatal accidents because all texting bans were passed after 2007 and the latest data available were from 2010. After removing Alaska from the sample because of some missing data, our final sample consists of 49 states over a 48 month period for a total of 2,352 observations.

We merge crash data to information on the enactment of text message bans. Table 1 lists each state with a ban, along with the month the ban became effective and some basic enforcement information. Most text message laws are similar in wording, and there are no remarkable differences in the size of the penalties associated with text messaging as the penalties are typically small fines.

There are two distinctions that allow us to classify bans as "weak" or "strong." Bans in Indiana and Missouri covered only younger drivers during our sample period but had considered universal bans, thus likely rendering the scope of coverage confusing to some drivers. Nevertheless, bans in these states are likely "weak" in terms of effectiveness. The second distinction is whether text messaging is a primary or secondary

[6] Anecdotal evidence suggests that such a pattern of results is to be expected for these reasons. For example, see http://www.gazette.com/articles/texting-89993-tuesday-entirely.html.

[7] Blood alcohol content was previously estimated through discriminant analyses because of the infrequency and inconsistency of actual measurement of blood alcohol content at accident scenes. More recent analyses have used the NHTSA's new multiple imputation procedure. See Adams, Blackburn, and Cotti (2012) for a discussion. No such detailed imputation exists for distracted driving.

Table 1—Effective Date of Text Messaging Bans across US States Enacted 2007–2010

State	Month effective	Enforcement	Universal concurrent handheld ban
Arkansas	October 2009	Primary	No
California	January 2009	Primary	Yes
Colorado	December 2009	Primary	No
Connecticut	October 2010	Primary	Yes
Georgia	August 2010	Primary	No
Illinois	January 2010	Primary	No
Indiana	July 2009	Primary (only 18 and under)	No
Louisiana	July 2008	Primary	No
Maryland	October 2009	Primary	No
Massachusetts	October 2010	Primary	No
Michigan	July 2010	Primary	No
Minnesota	August 2008	Primary	No
Missouri	August 2009	Primary (only 21 and under)	No
Nebraska	July 2010	Secondary	No
New Hampshire	January 2010	Primary	No
New Jersey	March 2008	Primary	Yes
New York	November 2009	Secondary	Yes
North Carolina	December 2009	Primary	No
Oregon	January 2010	Primary	No
Rhode Island	November 2009	Primary	No
Tennessee	July 2009	Primary	No
Utah	May 2009	Primary	No
Vermont	June 2010	Primary	No
Virginia	July 2009	Secondary	No
Washington	January 2008	Secondary (until primary in June 2010)	No
Wisconsin	December 2010	Primary	No
Wyoming	July 2010	Primary	No

Notes: Alaska enacted a ban in September 2008 but is excluded because of inconsistent information for some control variables. Delaware, Iowa, Kansas, and Kentucky passed laws for which official enforcement did not begin until 2011.

offense. Text messaging while driving is typically considered a primary offense by most states. That is, law enforcement officials can pull over a driver suspected of text messaging even if another infraction or crime has not been committed. There were four states (Nebraska, New York, Virginia, and Washington) for which texting is enforced only as a secondary offense during our sample period. These state bans are also likely "weak" in terms of effectiveness. Our research design will separate effects by ban type, with the strong bans expected to have a measurable effect.

There were also a few states that had concurrent handheld cell phone bans for all drivers.[8] We use this variation across states in a number of additional tests in the paper in an attempt to determine the relevance of ban heterogeneity. If drivers are still allowed to dial and talk on a phone while driving, we would suspect that enforcing text messaging bans would be even more difficult. Thus, we expect the

[8] These were California, Connecticut, New Jersey, and New York. We exclude Washington, DC given its unique driving conditions and long-existing cell phone ban. Washington's handheld ban came six months after their texting ban, so we do not consider it concurrent for our estimations. We did not code the two states with secondary enforcement of their handheld cell phone bans (Maryland and Utah) as part of this group since this would not be relevant to the enforcement issues we bring up later in the paper.

states with concurrent bans on cell phone use might have the stronger impact and perhaps one that lasts longer.

B. *Basic Empirical Model*

Our first step is to determine whether there is any evidence of reduced accidents following texting bans in a standard difference-in-differences framework by estimating

$$(1) \qquad Y_{im} = \alpha + \gamma_i + \delta_m + \mathbf{X}_{im}\boldsymbol{\beta} + \omega B_{\mathrm{im}} + \varepsilon_{im}.$$

Y_{im} represents the log (number of fatal accidents + 1) for state i in month m. We chose log accidents since this would provide an easy way to interpret the effects of the policies in percentage terms. State and month fixed effects are γ and δ, respectively. B indicates whether a state has a texting ban in place in a month, and the estimate of ω is our coefficient of interest.[9] We weight our estimations by state population because of the greater variation in accidents in smaller states. The ωB_{im} can also easily be expanded to account for strong bans (universal, primarily enforced) and weak bans (secondarily enforced or applicable just to a subset of the population). To do so, a dummy variable for the strong ban states is interacted with B, as is a dummy for the weak ban states, to yield $\omega_{SB} SB_{im} + \omega_{WB} WB_{im}$. This replaces ωB_{im}.

The $\mathbf{X}$ matrix in equation (1) contains the set of controls. We include a control for the log of the population in the state and the proportion male in the state. These data are available annually from the Census Bureau. Population, once state fixed effects are included, will likely be related to population density and congestion, which could increase the risk of accidents. However, an increase in density would most likely affect accidents in general, rather than single-vehicle, single-occupant accidents. The proportion male may heighten the potential for accidents as males typically are more likely to be involved in fatal accidents.[10] Also, we control for two other factors that might be related to accidents: the real prevailing gasoline tax and the state unemployment rate. Gas taxes did not vary by much during the sample period, but the unemployment rate did. The unemployment rate may reduce accidents if fewer drivers are on the road because of less economic activity (Cotti and Tefft 2011). There is no reason to think that the imposition of a texting ban should be related in any way to these control variables, however, so their inclusion is only expected to improve the efficiency of our estimates.

Given that the control variables most expected to be relevant are effectively determinants of traffic congestion, another approach would simply redefine the dependent variable Y as a measure of accidents per vehicle miles driven in a month. As shown later, the results are robust to this redefinition of the dependent variable.

[9] A concern with estimating equation (1) is that accident data from within a jurisdiction are correlated, raising problems with inference and necessitating clustering standard errors as a simple correction (Bertrand, Duflo, and Mullainathan 2004). The HLDI (2010) study examining the effect of texting bans on collision claims did not consider the potential for observations to be correlated within state.

[10] See data from the Hawaii Department of Transportation (2003) and the Washington State Department of Health (2012) for some representative statistics.

Table 2—Summary Statistics for Relevant Variables

		Treatment states		
	Control states	All months	Pre-ban months	Post-ban months
Number of single-vehicle, single-occupant accidents (monthly)	16.84	16.13	16.12	16.16
Population (annual)	5,157,694	7,064,738	6,614,487	8,066,044
Unemployment rate (monthly)	6.51	6.83	6.01	8.63
Proportion male (monthly)	49.32	49.34	49.37	49.26
Real gas tax in 1983 cents (monthly)	19.94	20.57	20.50	20.73
Sample size	1,056	1,296	894	402

Note: The treatment states are those listed in Table 1 and the control states are the remainder of states (less Alaska).

Additionally, one might suspect some noncongestion-related factors to have a role in accidents, such as weather or construction. We think that the geographic dispersion of the passage of the state laws and the timing of laws render this a second order concern, given that we include state and month fixed effects in our specifications. For example, there is little reason to believe that an extreme weather month would hit the states in our treatment group systematically around the time they pass texting bans, but not somehow be captured by nearby states that did not change their texting ban status in that month. Nevertheless, we use a control that has proved useful in studies that look at the effects of policies on particular types of accidents (Adams et al. 2012 and Cotti and Walker 2010). Specifically, for some specifications we add to the **X** vector the log of other types of accidents, namely those involving multiple vehicles or multiple occupants. The same factors that might affect accidents in general, like weather, should affect all types of accidents. Including other accidents as a control holds constant this confounding variation in single accidents.

Although including a control for other accidents is one way to control for confounding influences on accidents over time, a more complete means of controlling for these changes would add a unique time trend for each state. Although this limits some identifying information, particularly if we also include a fixed effect for every month in the sample, state-specific time trends are most robust. We will consider results with and without these state-specific time trends.

Table 2 presents summary statistics for the variables in the analysis. We first report the number of single-vehicle, single-occupant crashes for both the treatment and control group. This will serve as our primary variable to test for the fatal impact of texting and the efficacy of bans. The control states that do not pass a ban during our sample period had an average of 16.84 fatal single-vehicle, single-occupant accidents per month. The treatment states showed no notable change in the raw number of accidents, but the population of the post-ban sample in the treatment group is notably larger.[11] Therefore, assuming a state with a constant population of 6 million in both the pre- and post-ban sample, the bans actually were followed by a reduction of over 2.5 fatalities a month. This decline does not account for the general downward trend in such accidents that was occurring nationwide and necessitates the

[11] The larger post-ban population reflects the fact that the large states in the treatment group, namely California and New York, enacted bans fairly early.

difference-in-differences research design described above. Moreover, the bulk of this decline might be concentrated in just a few months following the ban, which we discuss in the next subsection.

The remaining control variables summarized in Table 2 suggest no extreme differences between the treatment and control states. There is nothing notably different about the unemployment rate, proportion male, or gas tax. The unemployment rate was rising in all states over this sample period so our post-ban period will naturally have higher unemployment rates. Given that there may be a relationship between unemployment rates and traffic accidents, controlling for unemployment is sensible but only necessary if we expect texting bans to be systematically passed by states in a deeper (or shallower) economic downturn. This is unlikely.

In addition to estimating equation (1), we examine whether the estimated effects of texting bans on accidents are robust to several assumptions concerning the distribution of fatal accidents across the states in the sample and other empirical decisions we made in the research design. As part of this, we engage in a series of checks where we test for the effects of texting bans on other types of accidents, some of which might be less likely to be affected by texting bans. These amount to falsification exercises. We also utilize different features of accidents, including whether there is a concurrent handheld cell phone ban, to further assess the impact of the legislation.

C. *Additional Estimation of Lead and Lag Effects*

We suspect that a simple difference-in-differences test might mask effects of texting bans in the months leading up to their effective dates and the pattern of results after passage. To test for these possibilities, we estimate

$$Y_{im} = \alpha + \gamma_i + \delta_m + \mathbf{X}_{im}\boldsymbol{\beta} + \varphi'\mathbf{B}_{im} \cdot \boldsymbol{\gamma}_i + \sum_{\tau=-5}^{+5} \omega_{SB_\tau} SB_{\tau,im} + \sum_{\tau=-5}^{+5} \omega_{WB_\tau} WB_{\tau,im} + \varepsilon_{im}. \tag{2}$$

The addition of $\varphi'\mathbf{B}_{im} \cdot \boldsymbol{\gamma}_i$ allows for a differential treatment effect for each state. Given that some states are observed for fewer periods post ban than others, this allows for lagged effects to be estimated free of concerns about composition bias.[12]

The summation of leads and lags are essentially a series of dummy variables. SB_{-5} and WB_{-5} are set to one for a month if the state will enact a "strong" (SB) or "weak" (WB) texting ban, respectively, five months in the future, and zero otherwise. Likewise, SB_{-4} through SB_{-1} (and WB_{-4} through WB_{-1}) are dummy variables that are similarly defined for months leading up to enactment. The estimates of ω_{-5} through ω_{-1} jointly measure the lead effects of the texting bans and will capture any unusual activity in states just prior to the actual effective month of the texting ban. This will also provide a test of whether the treatment and control states differ just prior to

[12] Note that the coefficient estimates ω are therefore not to be interpreted relative to no legislation. Rather, they are interpreted relative to an average treatment effect. An alternative would be to limit attention to a balanced panel of states that are in the sample for all of the lagged periods. In all cases, the pattern of announcement effects are similar.

passage, giving a stronger sense of whether there were confounding trends in the data and whether the imposition of bans are exogenous. Also added are a dummy for one (SB_1 and WB_1) through five or more months (SB_5 and WB_5) following enactment. Estimates of ω_1 through ω_5 measure the lagged effects of the ban and answer the essential question of the paper—that is, whether the effects are sustained over time or whether they merely reflect an announcement effect. We experiment with lags of different lengths in the online Appendix. State-specific treatment effects are also perfectly collinear with a contemporaneous effect of texting bans, so the latter estimates are not identified.

III. Results

A. *Basic Difference-in-Differences Estimates*

We first present estimates of the effect of texting bans on fatal accidents to establish whether the results have any detectable impact considering all state texting bans as the same, regardless of coverage or enforcement rules. Column 1 of Table 3 shows a 3.7 percent reduction in single-vehicle, single-occupant crashes in states after they pass a texting ban compared with states not passing a ban, but the effect is not significant.

In column 2, we separate the effects by "weak" and "strong" bans with the simplest set of controls used from column 1. The strong ban effects are negative and significant, suggesting an 8.1 percent reduction in accidents. Given there were 16.1 single-vehicle, single-occupant accidents a month in states before a ban was put in place, this suggests that accidents are reduced by about 1.3 per month per state or roughly 800 lives per year nationally if there were a national ban with universal coverage and primary enforcement.[13] This of course assumes the effect is sustained.[14] If we assume $6 million as an approximation of the value per life saved, this amounts to $4.8 billion saved annually from texting bans.[15] Given 2.12 trillion text messages are currently sent a year according to CTIA-The Wireless Association, this amounts to 0.2 cents per text message of benefit.[16] The proportion of text messages sent from roadways is unknown, however, so the cost to drivers of prohibiting them from texting from their vehicle cannot be estimated. Therefore, any assessment of welfare implications of texting bans would be incomplete. That said, we suspect delaying

[13] This is a very rough calculation based on the weighted average of accident totals of states in the treatment and control group and assumes one death per single-vehicle, single-occupant accident. Specifically, 802 lives saved $= (0.081 \text{ estimate} \times 16.1 \text{ treatment state deaths} \times 21 \text{ treatment states} \times 12 \text{ months}) + (0.081 \text{ estimate} \times 16.8 \text{ control state deaths} \times \text{remaining } 29 \text{ states} \times 12 \text{ months})$.

[14] Given that Wilson and Stimpson (2010) estimates suggest about 2,690 fatal accidents per year were associated with texting from 2002–2007, the harm from texting could be cut by just about 25 percent–30 percent following a ban. We note that these totals are only for single-vehicle, single-occupant crashes. Given the results later in the paper suggest that ban effects on other types of accidents are limited, however, the estimated effects on single-vehicle, single-occupant crashes in this section represent a substantial proportion of the effect of texting bans on accident reduction.

[15] See http://www.nytimes.com/2011/02/17/business/economy/17regulation.html?_r=2 for the justification of the value of a life saved calculation, which is taken from the US Department of Transportation guidelines.

[16] See http://www.ctia.org/advocacy/research/index.cfm/aid/10323 for the number of text messages sent.

Table 3—Determinants of Fatal, Single-Vehicle, Single-Occupant Crashes

	(1)	(2)	(3)	(4)	(5)
Texting ban in place	−0.0374 (0.0272)				
× universally applied, primarily enforced		−0.0807 (0.0255)***	−0.0764 (0.0252)***	−0.0712 (0.0445)	−0.0253 (0.0414)
× limited coverage/enforcement		0.0753 (0.0374)**	0.0751 (0.0369)**	0.0372 (0.0294)	0.1158 (0.0327)***
Log of population	−0.3346 (1.3172)	−0.2376 (1.1800)	−0.2792 (1.1800)	−2.7000 (1.8214)	1.1811 (1.3073)
Log of unemployment rate	−0.1972 (0.1215)	−0.1798 (0.1205)	−0.1277 (0.1186)	−0.0863 (0.0655)	0.2978 (0.1838)
Percent male	−0.0130 (0.0400)	−0.0179 (0.0254)	−0.0144 (0.0259)	−0.0010 (0.0377)	−0.0712 (0.0323)**
Log of gas tax	−0.0605 (0.1012)	−0.0421 (0.0854)	−0.0362 (0.0781)	−0.1113 (0.1051)	0.0232 (0.0778)
Other accidents	—	—	0.1797 (0.0485)***	0.4128 (0.0498)***	0.1649 (0.0487)***
Including 48 month fixed effects	Yes	Yes	Yes	No	Yes
Including differential monthly trend for all states	No	No	No	Yes	Yes

Notes: Reported are coefficients from weighted least squares regressions, weighted by state population size for 49 states over 48 months. The dependent variable is the natural logarithm of the number of fatal accidents + 1. Each specification includes state fixed effects. Standard errors are in parentheses and are clustered to allow for nonindependence of observations from the same state.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

texting until a time when one is not driving to be of negligible cost. Thus, even the small $0.002 benefit per text message likely supports an economic rationale for legislation.

On the other hand, column 2 reports an estimated effect of the weak texting bans that is positive. This means a poorly enforced ban might be worse than no ban at all. This finding is consistent with anecdotes of law enforcement officials being frustrated with bans that are difficult to enforce because of limited coverage.[17] There are two other points to be made about these positive effects for weak bans. First, we acknowledge that the limited number of cases of weakly enforced bans limits how much weight should be placed on these results. Second, the overall positive effect masks a meaningful pattern of effects over time that we revisit in the next section. For most of the estimations presented in the remainder of the paper, we keep focus on the strong ban cases but also consider the weak bans where appropriate.

The estimates so far do not account for trends in single-vehicle, single-occupant crashes that might be confounding. In the third column, we add the control for other types of accidents in a state-month. The aim here is to control for all factors that might affect accidents in general, regardless of type. This will leave only accident

[17] See, for example, http://www.daytondailynews.com/news/crime/enforcing-texting-ban-could-be-tricky-for-police-1377406.html.

variation unique to single-vehicle, single-occupant crashes to identify the texting ban effect. The effect of strong bans remains negative and significant.

We next explicitly allow for state-specific trends in columns 4 and 5. In column 4, we replace the time dummy variables with a linear time trend over the 48 sample months, which is then interacted with each state variable. In column 5, we include both the time dummies and unique linear time trends. Column 4 results reveal a significant accident reduction for the "strong" bans, suggesting both time dummies and linear time trends for each state reveal similar results. The column 5 results, however, suggest substantially reduced effects for strong bans and a very large weak ban result. Column 5 is the most robust estimation, but it comes with a cost in terms of limiting identifying variation in a sample with one observation per time period for each state. Thus, the coefficient estimates for the texting bans (and other variables) are highly affected. Throughout the remainder of the paper, we will always present results comparable to at least column 3 and column 5. We therefore illustrate the tradeoff between a more robust specification and one that allows for more identifying variation. Our results should be interpreted with these limitations in mind.

B. *Falsification Exercises and Sensitivity Checks for the "Strong" Ban Results*

If texting bans are effective, they would be most likely to reduce single-vehicle, single-occupant crashes, with less clear effects on other types of accidents. We verify this is true in the second row of Table 4. Compared with the estimates for single-vehicle, single-occupant crashes, which we repeat in the first row, the effects of texting bans on all types of accidents falls. In the third row, we consider the log of multiple vehicle or multiple occupant accidents and find virtually no effect of the texting bans. This essentially amounts to a falsification exercise, as this is the accident type we expect least likely to be affected by texting. We take this point a step farther in row 4 by pooling the single-vehicle, single-occupant crash counts and other crash counts by state-month into one sample and perform a difference-in-difference-in-differences estimation. A dummy for a single-vehicle, single-accident count observation is added and interacted with the texting bans. The estimates are strongly suggestive that the relative effect on single-vehicle, single-occupant accidents is a relatively large reduction.

In row 5, we undertake an additional falsification exercise. The imposition of bans should have no effect on vehicle miles traveled, and this is verified. Since the control variables we added in previous estimations are essentially proxy variables for traffic volume, we also divide the log of the total number of accidents, which is our dependent variable throughout most of the paper, by millions of vehicle miles traveled. In row 6, the alternative definition of the dependent variable yields very similar results.

The next two rows consider whether the lack of a concurrent restriction on the use of handheld cell phones influence the efficacy of texting bans. This is consistent with the concerns of law enforcement officials. Without a handheld cell phone ban, there is no way to know whether someone is texting, which is not legal, or dialing a phone, which is legal. It appears that this concern has some merit, as the states with the handheld bans experience larger reductions in accidents. Unfortunately, dividing the sample in this fashion leaves us with relatively few states to identify an effect,

TABLE 4—ADDITIONAL ESTIMATES OF THE EFFECT OF UNIVERSAL, PRIMARILY ENFORCED TEXTING BANS, WITH ROBUSTNESS CHECKS

	With state dummies	With state-specific trends	With both
(1) Table 3 estimates	−0.0764	−0.0712	−0.0253
	(0.0252)***	(0.0445)	(0.0414)
Panel A. Alternative dependent variables and falsification			
(2) Total of all crashes as dependent variable	−0.0378	−0.0506	−0.0024
	(0.0143)**	(0.0315)	(0.0303)
(3) Multiple vehicles or multiple occupants as dependent variable	−0.0240	−0.0343	0.0076
	(0.0152)	(0.0299)	(0.0314)
(4) Difference-in-difference-in-differences (single-vehicle occupant vs. multiple-vehicle occupant)	−0.1443	−0.1443	−0.1443
	(0.0610)**	(0.0610)**	(0.0613)**
(5) Vehicle miles traveled as dependent variable	−0.0029	−0.0278	−0.0199
	(0.0064)	(0.0234)	(0.0172)
(6) Accidents per million vehicle miles traveled as dependent variable	−0.0735	−0.0434	−0.0054
	(0.0269)***	(0.0458)	(0.0488)
Panel B. Alternative legislation/enforcement			
(7) Handheld cell phone ban also in place	−0.1108	−0.1352	−0.0897
	(0.0388)***	(0.0616)**	(0.0802)
(8) Handheld ban not in place	−0.0253	−0.0204	0.0065
	(0.0380)	(0.0654)	(0.0595)
Panel C. Alternative modeling			
(9) Negative binomial	−0.0050	−0.0082	0.0078
	(0.0192)	(0.0287)	(0.0271)
(10) Year fixed effects, month of year fixed effects	−0.0755	—	−0.0274
	(0.0256)***		(0.0421)
(11) Data through 2009	−0.0778	−0.1254	−0.0469
	(0.0211)***	(0.0364)***	(0.0526)

Notes: Each cell is from a separate regression. The specification in the leftmost column includes both 49 state and 48 month fixed effects and controls listed in Table 2 and used in the third column of Table 3. The middle column replaces the time dummies with state-specific time trends and the rightmost column adds the dummies back in. For row 1, as well as rows 7–11, the dependent variable is constructed from single-vehicle, single-occupant crashes. Additional robustness checks are reported in the online Appendix.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

and the results are imprecise. We will explore this distinction more in the next section when we plot effects over time.

In the remainder of Table 4, we confront other potential critiques of the basic approach used in the earlier estimates of the paper. Given we used a dependent variable that is the log of count data, we reestimate using a negative binomial, resulting in weaker effects. We explored the weakening effect of the negative binomial results in online Appendix Table A3 and determine this is likely due to the undue influence of smaller states when we estimate count data models, which are not weighted by population.[18]

[18] Specifically, once we limit attention to those states with populations above 2 million or states with at least one accident in every month, the effects of the Poisson model, negative binomial or unweighted/weighted OLS are all

The results presented thus far employ individual month fixed effects. We also could have used year fixed effects (2007–2010) and month-of-year (i.e, January, February, March, etc.) fixed effects as well. This would allow us to capture both seasonality and annual changes in accidents common across states, but it would also allow more identifying variation than in our original estimation. We find an effect that is not substantially different than in our basic estimates.

Finally, as a precursor to looking at the lagged effects of the legislation, we consider a specification that only includes the bans that were enacted through 2009 and exclude 2010 data.[19] The effects are stronger, suggesting that adding the 2010 data weakened the estimates. There are two potential reasons. First, the effects of bans enacted through 2009 have a smaller impact by 2010, resulting in lower estimated effects of the legislation. Second, bans enacted in 2010 have less of an influence than those enacted earlier. Both estimates hint that drivers might learn over time that ban enforcement is limited. We return to more formal estimates of lagged effects in the next section.[20]

We return briefly to the only paper that has also assessed state-level bans (HLDI 2010). Although their outcome of interest is a sample of collisions, rather than fatal accidents, the positive effect on accidents estimated for three out of the four states they analyze stand in contrast to the basic difference-in-differences evidence we present for the nation as a whole. Given fatal accident data are a census of all accidents across the nation and are not self-reported collisions, however, we are more confident that our results represent more credibly the effects of texting bans. Additionally, inferring any meaningful effect from analyzing one state at a time is problematic. Table A1 (online Appendix) confirms that effects across states are highly variable.[21]

IV. Effects over Time and Evidence of the Announcement Effect

Up until this point, we established that texting bans are followed by reduced traffic accidents in states with primary enforcement of legislation. This says nothing about whether the effects are sustained, however, nor does it say anything about whether there were changes in accident levels prior to ban enactment. In this section, we

quite similar. Since these nuances are not relevant for the main points of the paper, specifically the announcement effects, they are relegated to the online Appendix. In the course of assessing robustness, we conducted a number of additional tests. These additional estimates are also added to online Appendix Table A3.

[19] An earlier version of the paper used data only through 2009. We thought it useful to discuss briefly the changes to the results from adding the 2010 data, and what this might imply about the lagged effects of legislation.

[20] Simple estimations limiting attention to bans only passed in 2010 (and dropping older bans) result in weaker estimates, as does dropping new bans and assessing the impact of adding 2010 data for earlier bans. We also point out that the announcement effect results of the next section, however, are present in both the data through 2009 and 2010.

[21] There are some additional issues with the HLDI study that render the study difficult to evaluate, some of which were noted earlier. First, HLDI do not adjust their standard errors for the likely correlation of observations from the same state. This adjustment would likely increase their standard errors notably, and perhaps change their assessment of significance. Such adjustment has become standard in difference-in-differences estimations since Bertrand, Duflo, and Mullainathan (2004). Second, HLDI focus on collision claims rather than fatal accidents. The makeup of collision claims may differ drastically from fatal accidents. The former is self-reported and may be less likely to be divulged by someone who feels entirely at fault and wishes not to see his insurance premiums rise. This is likely why most existing studies of traffic safety focus on the census of fatal accidents. Looking at online Appendix Table A1, the results for California, Minnesota, Illinois, and Washington again show a difference between our results and those of HLDI. Interestingly, when we use the selected control groups used by HLDI and estimate effects for all accidents (see online Appendix Table A2), we find no effects of bans and certainly cannot rule out positive effects like those found by HLDI.

explore lead and lagged effects of the bans. The lead effects are meant to deal with a concern that the estimates presented thus far might reflect some unexpected change in accidents just prior to the laws taking effect, which may lead to a spurious finding. Most of the texting bans listed in Table 1 became effective in the winter or summer, which are typically more dangerous driving months. Thus, it is useful to assess whether there are any differences in accidents just prior to the laws taking effect.

Figure 1A plots the coefficients $\omega_{\mathrm{SB}_\tau}$ and $\omega_{\mathrm{WB}_\tau}$ from equation (2), where the dependent variable is the log of single-vehicle, single-occupant accidents. Effects for bans that are universally applied and enforced (SB) and effects for bans that have limited coverage or enforcement (WB) are presented separately (although are generated from the same regression). The **X** vector variables are those from column 3 of Table 2, which does not include state-specific time trends. Looking at the months before laws are enacted, there are no anticipatory effects or remarkably different trends in accidents between the treatment and control states for the universally applied and enforced legislation. There are a moderately larger number of accidents in the month preceding bans, but combined with the other lead effects, a joint test of the lead coefficients concludes they are not significant. This is strongly suggestive that the treatment and control groups are comparable in terms of single-vehicle, single-occupant accidents and that the imposition of texting bans is exogenous in the case of the stronger bans. Figure 1B repeats the analysis adding controls for state-specific linear time trends. Again, there are no significant lead effects of the "strong" bans.[22] For Figures 1A and 1B, however, the weak ban lead effects are positive and significant, lending perhaps less credibility to these estimates. The relatively higher number of accidents in these localities after bans reflects what was a generally higher level of accidents prior to the bans.

Figures 1A and 1B also plot the effect of legislation during the months following enactment. The estimated effect in the month following a ban is substantial, suggesting a 17–18 percent relative reduction in accidents that is statistically significant for the strongly enforced bans. Figures 1A and 1B also reveal a rapid decline in the effect of bans in subsequent months. For the stronger bans, the effect declines substantially by the second month and has essentially disappeared by month four.

This announcement effect could be explained in two ways. First, drivers may initially alter behavior by reducing texting but soon learn ways to evade detection, such as texting while out of view of police or hiding their phones. This explanation was advanced by the HLDI (2010) to explain their curiously positive impact on accidents. An explanation we view as more likely is that enforcement is not sufficient. This point is illustrated in Figures 1A and 1B where we consider the lagged effects for the bans that are enforced as secondary offenses or are limited in terms of coverage. After a slight decline the month after these bans were passed, it appears that drivers returned to the relatively high level of texting they had been engaging in even before the bans. Compared with drivers in states with primary enforcement, it appears that drivers return more quickly to past behaviors where laws are difficult to enforce.

[22] Online Appendix Table A4 includes the full set of coefficient estimates for the leads and lags, as well as the control variables, for these specifications.

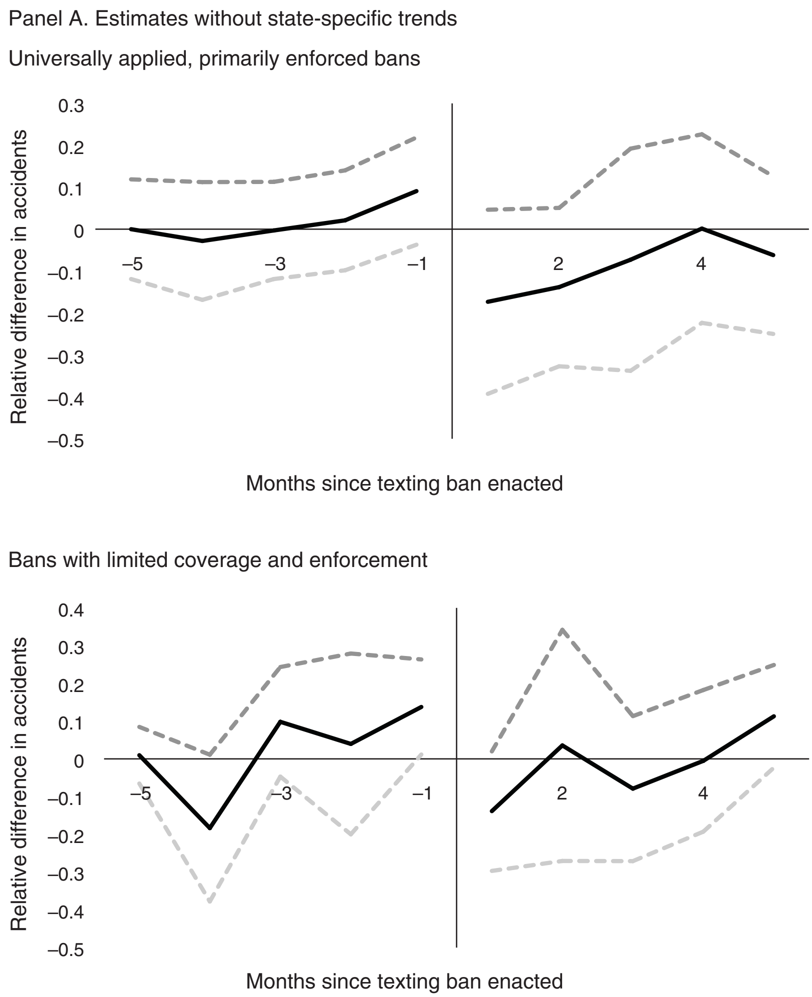


Figure 1A. The "Announcement Effect": Impacts of Texting Bans over Time

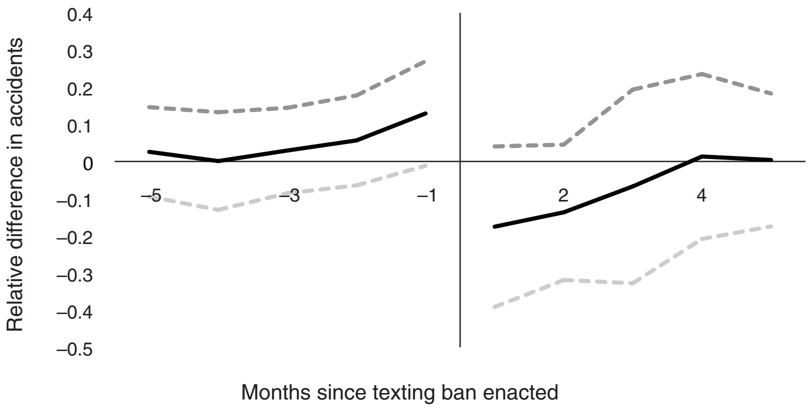
Notes: These figures plot the estimated lead and lag coefficients from equation (2), along with 95 percent confidence bands. Panel A is derived from an estimation without state trends and panel B derived from an estimation with state trends. Regressions include both 49 state and 48 month fixed effects and control variables from the third column of Table 3.

For further evidence of lax enforcement as an explanation for only a short-term impact of bans, we turn to another test. Specifically, law enforcement officials have expressed frustration over a number of aspects of the legislation. Primary among these is the inability to tell the difference between drivers who are actually texting and drivers that are using a handheld cell phone.[23] This should therefore be reflected in a more pronounced announcement effect in those states without handheld cell

[23] See for example http://www.gazette.com/articles/texting-89993-tuesday-entirely.html.

Panel B. Estimates with state-specific trends

Universally applied, primarily enforced bans



Bans with limited coverage and enforcement

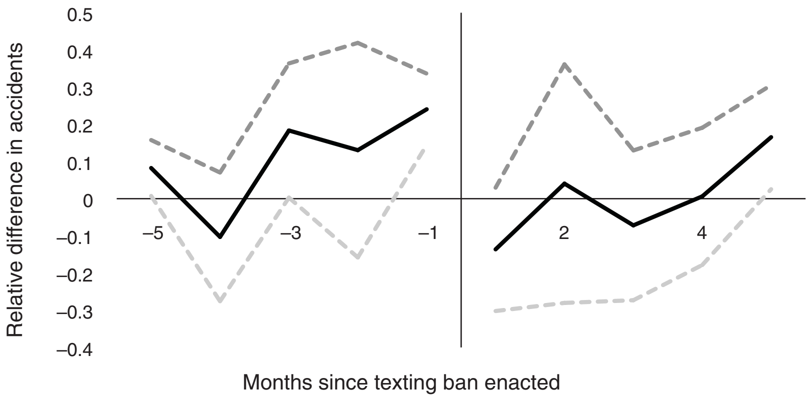


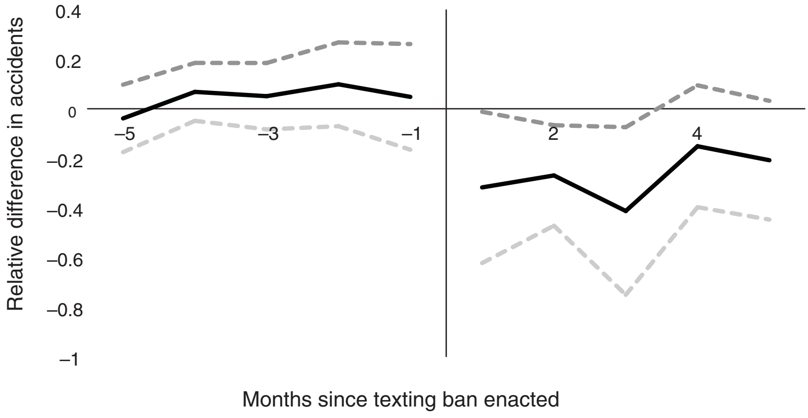
FIGURE 1B. THE "ANNOUNCEMENT EFFECT": IMPACTS OF TEXTING BANS OVER TIME

Notes: These figures plot the estimated lead and lag coefficients from equation (2), along with 95 percent confidence bands. Panel A is derived from an estimation without state trends and panel B derived from an estimation with state trends. Regressions include both 49 state and 48 month fixed effects and control variables from the third column of Table 3.

phone bans that are in place when texting bans are passed. We illustrate this possibility in Figure 2 by dividing the states in our sample into those with bans on handheld cell phone use and those without. We also limit attention in Figure 2 to only those estimates of effects of universal, primarily enforced bans. It is in the case of no handheld bans (Figure 2B) where the announcement effect is clearly stronger. The Figure 2A results actually have significant leads and lags, the former of which likely questions any inference drawn about the unique effects of texting bans post enactment. The results of Figures 2A and 2B suggest that law enforcement officials

Panel A. Effects in states with bans on all handheld cell phone use

Without state-specific trends



With state-specific trends

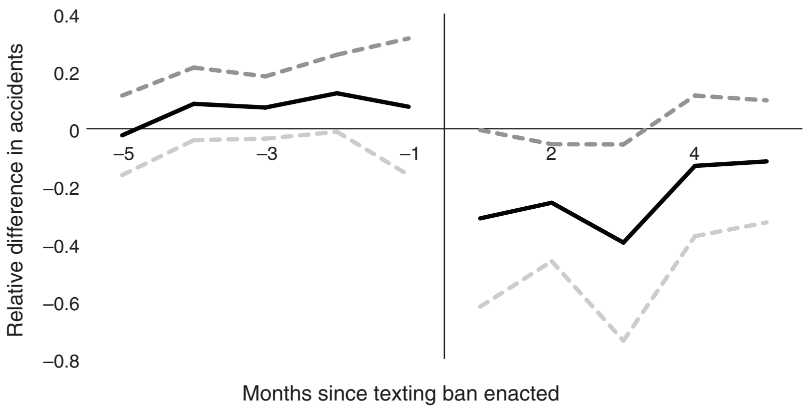


Figure 2A. Impacts of Universal, Primarily Enforced Texting Bans by Presence of Handheld Bans

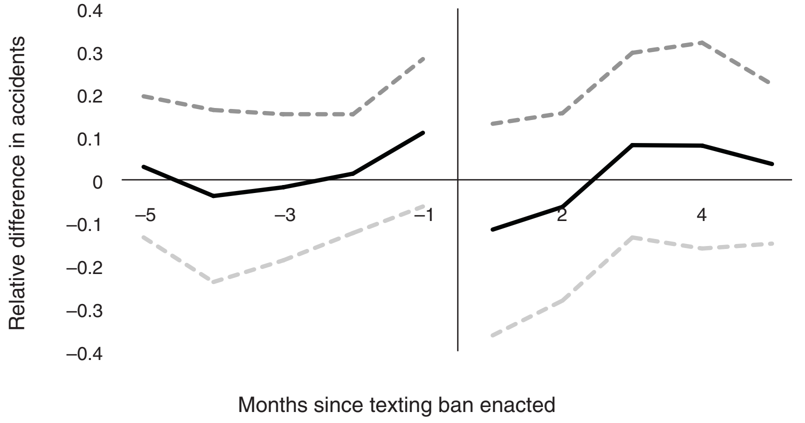
Note: Each graph is from a separate estimation of equation (2), including all control variables from column (3) of Table 3.

find it difficult to discern what a driver holding a cell phone is doing, and thus limit the impact a texting ban might have.

While ultimately indicative of lax enforcement being to blame, the results cannot rule out other explanations, such as drivers learning to circumvent the laws. Nevertheless, we add that the announcement effect pattern of the results exhibited in Figures 1 and 2 are robust. We show under a wide variety of tests in online Table A5 and online Appendix Figure A1 that effects in the first month or two tend to be substantial (as well as significant or nearly significant) in the case of primarily enforced bans, and the results after the third month tend not to be. When we add in the secondarily enforced ban, it is basically just the first lag that shows a meaningful reduction.

Panel B. Effects in states with a texting ban only (i.e., no handheld ban)

Without state-specific trends



With state-specific trends

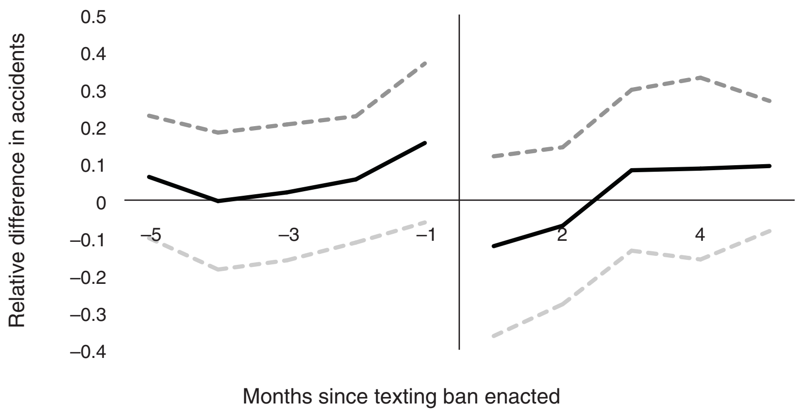


Figure 2B. Impacts of Universal, Primarily Enforced Texting Bans by Presence of Handheld Bans

Note: Each graph is from a separate estimation of equation (2), including all control variables from column (3) of Table 3.

V. Conclusion

We provide the first national-level study of the effect of texting bans imposed by states on the incidence of fatal automobile accidents. Texting while driving is now considered a major public health issue, with Senator Charles Schumer (D-NY) recently pushing for a nationwide ban.[24] By targeting a specific group of drivers (solo drivers) and a specific group of crashes (those involving just one vehicle), we isolated the

[24] See http://schumer.senate.gov/record.cfm?id=318484&.

accidents most likely to be affected by text message bans. Our evidence suggests fatal accidents are reduced by bans if they are enforced as a primary offense and cover all drivers. Alternatively, accidents less likely to be related to text messaging, particularly multiple vehicle or multiple occupant accidents, are not reduced significantly.

The strong impact of texting bans on single-vehicle, single-occupant crashes is short-lived. While the effects are strong for the month immediately following ban imposition, accident levels appear to return toward normal levels in about three months. This suggests that a texting ban immediately saves lives, but the positive effect cannot be sustained. The declining impact of traffic safety policies over time is not uncommon and has been observed in other regulations. Given the large impact of texting bans in the initial months following enactment, however, the evidence of the paper suggests greater enforcement of these laws likely can save more lives. More complete bans on handheld devices for all purposes might also lead to texting bans being more effective. The latter solution would impose additional costs on drivers, however, rendering the welfare effect of such legislation uncertain.

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