ESSAYS IN HEALTH ECONOMICS AND PUBLIC HEALTH POLICY

by

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ABSTRACT

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This dissertation consists of three essays. In the first essay I study the effect of texting bans on fatal accidents on roadways. 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 they are universally applied and enforced as a primary offense. Bans enforced as secondary offences, 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.

The second chapter studies the effect of homeschooling on child health. Homeschooling, which is becoming increasingly popular in the United States, has received some attention by researchers, but there has been no study of the potential health benefits. Given that homeschooled children receive more close supervision and guidance from parents, and

perhaps are less exposed to communicable illnesses, a benefit is possible. By adopting different identification strategies and using the Parent and Family Involvement (PFI) data from National Center for Education Statistics (NCES) for 2003 and 2007, I find that homeschooled children are healthier compared with their counterparts who go to public or private school. The effect is most pronounced for children between 8 to 12 years old. Finally, in the third essay, as a note, I study the effect of school shootings in the United States on private and public school enrollment. I find that school shootings are followed by a 10%t of school shootings in the United States on private and public school enrollment. The effects are most pronounced following shootings in nonurban areas, which is consistent with their more intense media coverage.

To my dear wife, Sahar

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Chapter 1: Texting bans on Roadways

I. Introduction

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 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 report, over 2 trillion text messages were sent in 2011, which is almost twenty times the number sent in 2006 (CTIA, 2012).

To mitigate the portion of fatal distracted driving accidents caused by sending or

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

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 non-vehicular 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 offence.[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 are 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-difference 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.

II. 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

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[2] By primary offense, we mean that law enforcement officers 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.

accident risk fourfold. Recently, the Virginia Tech Transportation Institute (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 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 et al. 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 show that 36% believe texting and driving can be fatal. Despite texting while driving being at least as dangerous as driving drunk, many more teenagers (55%) believe drinking and driving can be 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 a 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 accidents 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

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

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 devises 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 et al. (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 hand-held 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 et al. 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 Nickolaev et al. 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.

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

[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.

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 particular 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

[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.

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 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 et al. 2003; McCarrt and Geary, 2004).

III. 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 National Highway Traffic Safety Administration (NHTSA), which is a nationwide census for all fatal 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 (2010)'s 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

[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.

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 2352 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 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 offense. Text messaging while driving is typically considered a primary offence 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 delineate 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 will still allowed to

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[7] Blood alcohol content was previously estimated though 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 et al. (2011) for a discussion. No such detailed imputation exists for distracted driving.

[8] These were California, Connecticut, New Jersey, and New York. We exclude 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.

dial and talk on a phone while driving, we would suspect that enforcing text messaging bans would be even more difficult. Thus, we suspect the 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:

$$Y_{im} = \alpha + \gamma_i + \delta_m + X_{im}\beta + \omega B_{im} + \varepsilon_{im} \tag{1}$$

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 X matrix in equation (1) is 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

[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 et al. (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.

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. Additionally, one might suspect some non-congestion 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 also 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. (2011) 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

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[10] See data from the Hawaii Department of Transportation (2003) and the Washington Department of Highways (2008) for some representative statistics.

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 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

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[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.

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-difference 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:

$$Y_{im} = \alpha + \gamma_i + \delta_m + X_{im}\beta + \varphi' B_{im} * \gamma_i + \sum_{\tau=-5}^{+5} \omega_{SB\,\tau} SB_{\tau,im} + \sum_{\tau=-5}^{+5} \omega_{WB\,\tau} WB_{\tau,im} + \varepsilon_{im} \qquad (2)$$

The addition of $\varphi' B_{im} * \gamma_i$ 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 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 $\omega_{.,-5}$ through $\omega_{.,-1}$ 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 passage, giving a stronger sense of whether there were

[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 the same.

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 $\omega_{.,1}$ through $\omega_{.,5}$ 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 appendix. State-specific treatment effects are also perfectly collinear with a contemporaneous effect of texting bans, so the latter estimates are not identified.

IV. 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. We first consider all state texting bans, regardless of coverage or enforcement rules. Column (1) of Table 3 shows a 3.7% 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% 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 eight hundred 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

[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 estimate x 16.1 treatment state deaths x 21 treatment states x 12 months) + (0.081 estimate x 16.8 control state deaths x remaining 29 states x 12 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% - 30% 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

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 from 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 texting until a time when one is not driving to be of negligible cost. Thus, even the small $.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 bans 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 meaningful.

The estimates so far do not account for state specific-trends in single vehicle, single occupant crashes. 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 variation unique to single vehicle-single occupant crashes to identify the texting ban effect. The effect of strong bans remains negative and significant.

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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 U.S. Department of Transportation guidelines.

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

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

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 are larger.

In row (5), we undertake an additional falsification exercise. The imposition of bans should have no effect on vehicle miles travelled, 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 travelled. 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 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 re-estimate using a negative binomial, resulting in weaker effects. We explored the weakening effect of the negative binomial results in appendix Table 7 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]

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, Jan, Feb, 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.

[18] Specifically, once we limit attention to those states with population 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 quite similar. Since these nuances are not relevant for the main points of the paper, specifically the announcement effects, they are relegated to the appendix. In the course of assessing robustness, we conducted a number of additional tests, as well tests allowing for differential trends for the treatment group. These additional estimates are also added to appendix table A3.

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-difference 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 5 confirms that effects across states are highly variable.[21]

[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) results 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 et al. (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 Table 5, 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 Table A2), we find no effects of bans and certainly cannot rule out positive effects like those found by HLDI.

V. 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 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 $\omega_{.,\tau}$ 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. 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 both Figure 1a and 1b, however, the weak ban lead effects are positive and significant, lending perhaps less credibility to

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[22] Appendix Table A3 includes the full set of coefficient estimates for the leads and lags, as well as the control variables, for these specifications.

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% 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 4.

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.

As 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 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

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

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 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 suggestive 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 Tables A5 and 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.

VI. 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 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.

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

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.

Table 1 Effective date of text messaging bans across US states enacted 2007-2010

State	***Month effective***	***Enforcement***	***Universal Concurrent Handheld ban***
Arkansas	Oct 2009	Primary	No
California	Jan 2009	Primary	Yes
Colorado	Dec 2009	Primary	No
Connecticut	Oct 2010	Primary	Yes
Georgia	Aug 2010	Primary	No
Illinois	Jan 2010	Primary	No
Indiana	Jul 2009	Primary (only 18 and under)	No
Louisiana	Jul 2008	Primary	No
Maryland	Oct 2009	Primary	No
Massachusetts	Oct 2010	Primary	No
Michigan	Jul 2010	Primary	No
Minnesota	Aug 2008	Primary	No
Missouri	Aug 2009	Primary (only 21 and under)	No
Nebraska	Jul 2010	Secondary	No
New Hampshire	Jan 2010	Primary	No
New Jersey	Mar 2008	Primary	Yes
New York	Nov 2009	Secondary	Yes
North Carolina	Dec 2009	Primary	No
Oregon	Jan 2010	Primary	No
Rhode Island	Nov 2009	Primary	No
Tennessee	Jul 2009	Primary	No
Utah	May 2009	Primary	No
Vermont	June 2010	Primary	No
Virginia	July 2009	Secondary	No
Washington	Jan 2008	Secondary (until primary in June 2010)	No
Wisconsin	Dec 2010	Primary	No
Wyoming	Jul 2010	Primary	No

Note: 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.

Table 2 Summary statistics for relevant variables

	Control states	Treatment 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	1056	1296	894	402

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

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

	(1)	(2)	(3)	(4)	(5)
Texting ban in place	-0.0374				
	(0.0272)				
x universally applied, primarily enforced		-0.0807	-0.0764	-0.0712	-0.0253
		(0.0255)	(0.0252)	(0.0445)	(0.0414)
x limited coverage/enforcement		0.0753	0.0751	0.0372	0.1158
		(0.0374)	(0.0360)	(0.0294)	(0.0327)
Log of population	-0.3346	-0.2376	-0.0279	-2.7000	1.1811
	(1.3172)	(1.1800)	(1.1800)	(1.8214)	(1.3073)
Log of unemployment rate	-0.1972	-.01798	-0.0128	-0.0863	0.2978
	(0.1215)	(0.1205)	(0.1186)	(0.06547)	(0.1838)
Percent male	-0.0130	-0.0179	-0.0144	-0.0010	-0.0712
	(0.0400)	(0.0254)	(0.0259)	(0.0377)	(0.0323)
Log of gas tax	-0.0605	-0.0421	-0.0362	-0.1113	0.0232
	(0.1012)	(0.0854)	(0.0781)	(0.1051)	(0.0778)
Other accidents	…	…	0.1797	0.4128	0.1649
			(0.0485)	(0.0498)	(0.0487)
Including 48 month fixed effects	Yes	Yes	Yes	No	Yes
Including differential monthly trend for all states	No	No	No	Yes	Yes

Note: 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 non-independence of observations from the same state.

Table 4 Additional estimates of the effect of texting bans, with robustness checks

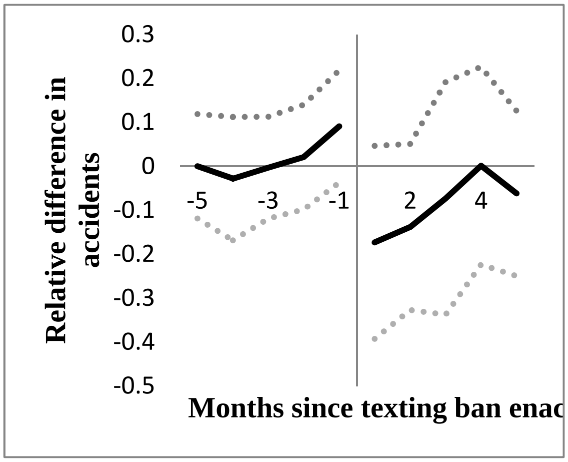
		With state dummies	With state-specific trends	With both
(1)	Table 3 estimates	-0.0764 (0.0252)	-0.0712 (0.0445)	-0.0253 (0.0414)
(2)	<u>Alternative dependent variables and falsification</u> Total of all crashes as dependent variable	-0.0378 (0.0143)	-0.0506 (0.0315)	-0.0024 (0.0303)
(3)	Multiple vehicles or multiple occupants As dependent variable	-0.0240 (0.0152)	-0.0343 (0.0299)	0.0076 (0.0314)
(4)	Difference-in-difference-in-differences (single vehicle, occupant vs. multiple vehicle, occupant)	-0.1443 (0.0610)	-0.1443 (0.0610)	-0.1443 (0.0613)
(5)	Vehicle miles travelled as dependent variable	-0.0029 (0.0064)	-0.0278 (0.0235)	-0.0199 (0.0172)
(6)	Accidents per million vehicle miles travelled as dependent variable	-0.0735 (0.0269)	-0.0423 (0.0455)	-0.0054 (0.0488)
(7)	<u>Alternative legislation/enforcement</u> Handheld cell phone ban also in place	-0.1108 (0.0388)	-0.1352 (0.0616)	-0.0897 (0.0802)
(8)	Handheld ban not in place	-0.0253 (0.0380)	-0.0204 (0.0654)	0.0065 (0.0595)
(9)	<u>Alternative modeling</u> Negative Binomial	-0.0050 (0.01916)	-0.0082 (0.0287)	0.0078 (0.0271)
(10)	Year fixed effects, month of year fixed effects	-0.0755 (0.0256)	…	-0.0274 (0.0421)
(11)	Data through 2009	-0.0778 (0.0211)	-0.1254 (0.0364)	-0.0469 (0.0526)

Note: Each cell is from a separate regression. The specification in the left 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 add 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 appendix.

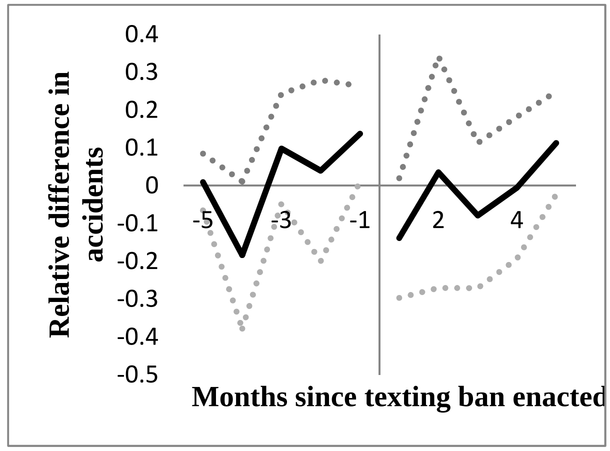
Figure 1 The announcement effect: Impacts of texting bans over

1a. Estimates without state-specific trends

Universally applied, primarily enforced bans

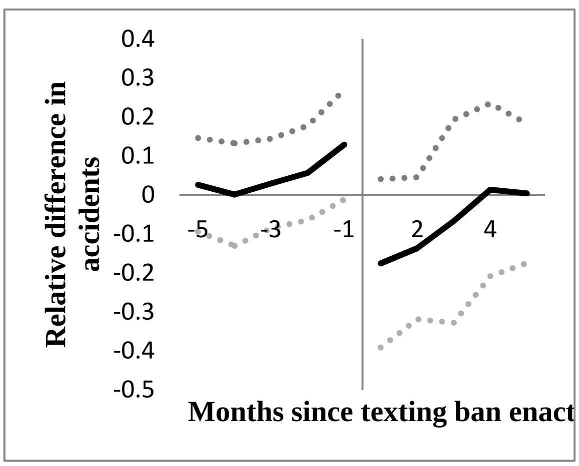


Bans with limited coverage and enforcement

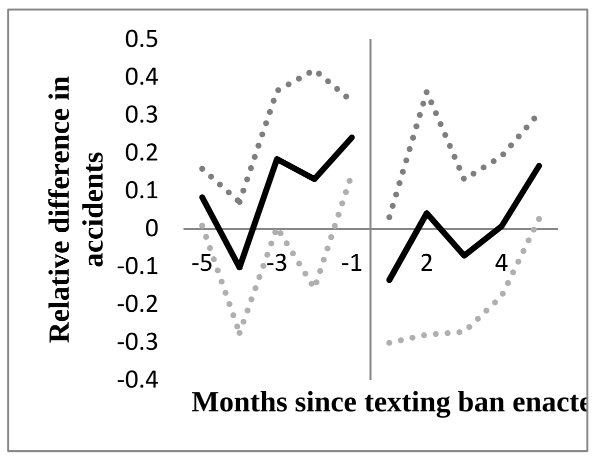


1b. Estimates with state-specific trends

Universally applied, primarily enforced bans



Bans with limited coverage and enforcement

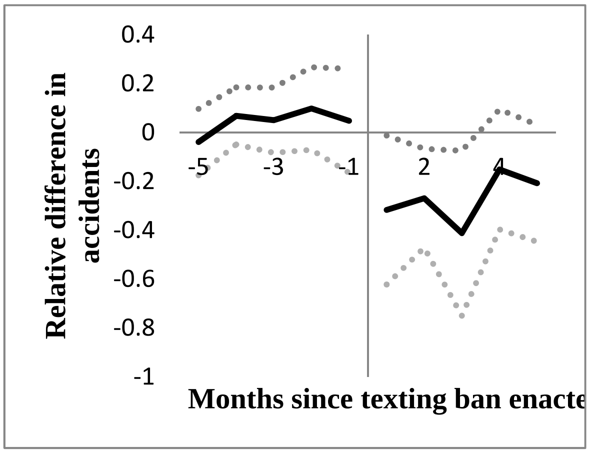


Note: These figures plot the estimated lead and lag coefficients from equation (2), along with 95% confidence bands. Panel (a) is derived from an estimation without state trends and panel (b) derived from an estimation with state trends . Regressions include both 49 state and 48 month fixed effects and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

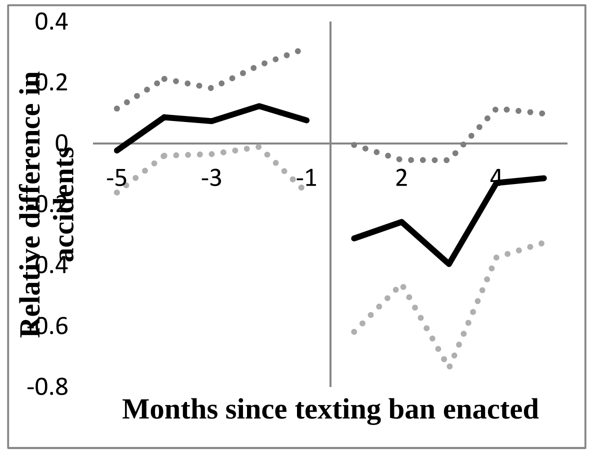
Figure 2 Impacts of universal, primarily enforced texting bans by presence of handheld bans

2a. Effects in states with bans on all handheld cell phone use

Without state-specific trends

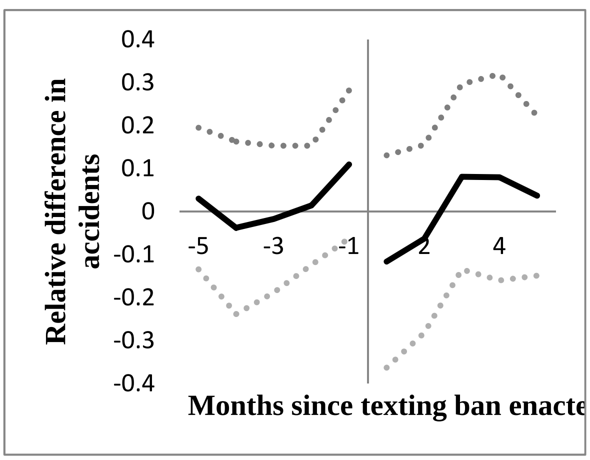


With state-specific trends

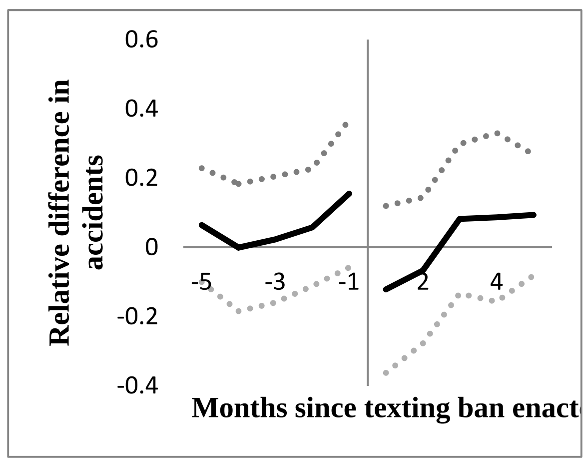


2b. Effects in states with a texting ban only (i.e., no handheld ban)

Without state-specific trends



With state-specific trends



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

Table 5 Effects of texting bans by state

Arkansas	0.0475 (0.0768)	New Hampshire	0.4732 (0.1399)
California	-0.0991 (0.0420)	New Jersey	-0.1941 (0.0823)
Colorado	0.0137 (0.1047)	New York	0.1829 (0.0761)
Connecticut	-0.3754 (0.3552)	North Carolina	-0.1799 (0.1136)
Georgia	0.0119 (0.0679)	Oregon	-0.3380 (0.1204)
Illinois	0.0011 (0.1092)	Rhode Island	0.3523 (0.1825)
Indiana	0.0571 (0.0721)	Tennessee	-0.0150 (0.0810)
Louisiana	-0.1759 (0.0732)	Utah	-0.0662 (0.1538)
Maryland	-0.0391 (0.0836)	Vermont	0.2945 (0.2749)
Massachusetts	0.0342 (0.2252)	Virginia	-0.0744 (0.0809)
Michigan	0.3271 (0.0778)	Washington	0.0828 (0.0666)
Minnesota	-0.1494 (0.1089)	Wisconsin	-0.9781 (0.0743)
Missouri	0.1042 (0.0822)	Wyoming	0.4859 (0.1394)
Nebraska	0.0119 (0.1045)		

Note: Reported are coefficients on an interaction of the texting ban variable with state dummy. The dependent variable is the natural logarithm of the number of fatal accidents plus one. Newey-West (1987) standard errors are reported in parentheses correcting for heteroskedasticity and allowing autocorrelation up to one lag. Each regression includes 49 states and 48 month dummy variables, as well as controls listed in Table 2 and used in the third column of Table 3.

Table 6 Effects in selected states

Accidents	Single vehicle, single occupant	All crashes
California (vs. Arizona, Nevada, and Oregon)	-0.1395 (0.1080)	-0.0148 (0.0500)
Louisiana (vs. Arkansas, Mississippi, and Texas)	-0.1128 (0.0695)	-0.0637 (0.0423)
Minnesota (vs. Iowa and Wisconsin)	-0.0429 (0.2263)	0.0574 (0.0832)
Washington (vs. Idaho and Oregon)	0.0090 (0.1816)	0.1093 (0.0902)

Note: Each cell is from a separate regression. Reported are coefficients from a weighted least squares regression, weighted by state population size. The dependent variable is the natural logarithm of the number of fatal accidents plus one. Robust standard errors are in parentheses. Each regression includes state and month dummy variables, as well as controls listed in Table 2 and used in the third column of Table 3. The control states are those chosen by HLDI (2010)

Table 7 Additional estimates of the effect of texting bans, with robustness checks

		With state dummies	**With state-specific trends**	**With both**
(1)	Preferred Table 3 estimates	-0.0764 (0.0252)	-0.0712 (0.0445)	-0.0253 (0.0414)
	Different weights and population sizes			
(2)	Unweighted OLS	-0.0099 (0.0493)	-0.0147 (.0696)	0.0149 (0.0644)
(3)	Unweighted OLS for states with at least 2 million residents (drops 14 small states; total of 35)	-0.0945 (0.0378)	-0.1212 (0.0624)	-0.0929 (0.0559)
(4)	Unweighted OLS for states with at least one accident in every month (drops 12 small states; total of 37)	-0.0780 (0.0456)	-0.0935 (0.0756)	-0.0625 (0.0677)
(5)	Poisson for states with at least one accident in every month	-0.0613 (0.0300)	-0.0648 (0.0459)	-0.0198 (0.0425)
(6)	Negative binomial for states with at least one accident in every month	-0.0606 (0.0311)	-0.0714 (0.0499)	-0.0226 (0.0434)
	Removing questionable states from Control group			
(7)	Illinois because of Chicago ban (42 states)	-0.0838 (0.0261)	-0.0803 (0.0459)	-0.0340 (0.0434)
(8)	New Mexico because of Albuquerque, Las Cruces, and Santa Fe	-0.0766 (0.0253)	-0.0713 (0.0445)	-0.0257 (0.0414)
	Alternative estimations			
(9)	Balanced set of states with laws in effect for at least 6 months	-0.0611 (0.0256)	-0.0478 (0.0435)	0.0041 (0.0404)

Note: See Table 4 notes.

Table 8 Full Lead and lag specifications

	Without state-specific trends		With state-specific trends	
	Weak bans	**Strong bans**	**Weak bans**	**Strong bans**
Lead 5	0.0097 (0.0373)	-0.0001 (0.0591)	0.0825 (0.0374)	0.0259 (0.0598)
Lead 4	-0.1834 (0.0967)	-0.0282 (0.0699	-0.1028 (0.0861)	0.0011 (0.0653)
Lead 3	0.0982 (0.0722)	-0.0031 (0.0576)	0.1833 (0.0897)	0.0295 (0.0571)
Lead 2	0.0396 (0.1191)	0.0210 (0.0593)	0.1304 (0.1434)	0.0566 (0.0601)
Lead 1	0.1375 (0.0626)	0.0908 (0.0636)	0.2404 (0.0477)	0.1291 (0.0699)
P-value leads	0.83	0.04	0.54	<0.01
Lag 1	-0.1385 (0.0787)	-0.1733 (0.1092)	-0.1362 (0.0825)	-0.1754 (0.1074)
Lag 2	0.0357 (0.1523)	-0.1382 (0.0939)	0.0404 (0.1595)	-0.1367 (0.0905)
Lag3	-0.0789 (0.0955)	-0.0729 (0.1318)	-0.0717 (0.1001)	-0.0671 (0.1296)
Lag4	-0.0052 (0.0933)	0.0014 (0.1118)	0.0062 (0.0918)	0.0133 (0.1103)
Lag 5	0.1128 (0.0676)	-0.0619 (0.0935)	0.1654 (0.0696)	0.0040 (0.0889)
Log of population	1.4408 (1.1378)		2.5868 (1.4878)	
Log of unemployment rate	-0.0753 (0.1161)		0.3490 (0.1732)	
Percent male	-0.0145 (0.0357)		-0.06077 (0.0491)	
Log of gas tax	-0.0486 (0.0825)		0.0430 (0.0758)	
Other accidents	0.1742 (0.0488)		0.1586 (0.0497)	

Note: Each column is from a separate regression (equation (3)). Each specification includes both 49 state and 48 month fixed effects, as well as controls listed in Table 2 and used in column (2) of Table 3. The lead and lag coefficients are plotted in Figure 1.

Table 9 Tests of significance of leads and lags in alternative models

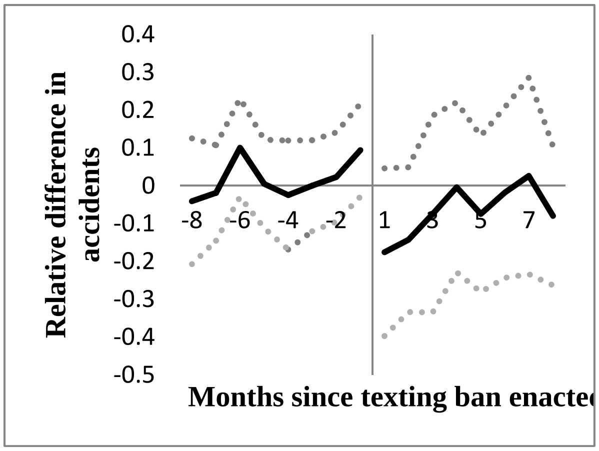
	Universally applied and primarily						Limited coverage or secondary enforcement					
	p-vl. leads	lag 1	lag 2	lag 3	lag 4	lag 5	p-vl. leads	lag 1	lag 2	lag 3	lag 4	lag 5
Alternative dependent variables												
Multiple vehicles or multiple occupants	<.01	.022	-.076	-.009	.005	.003	<.01	-.248	-.043	-.137	-.206	-.185
		(.041)	(.049)	(.052)	(.063)	(.044)		(.077)	(.136)	(.096)	(.102)	(.067)
Accidents per million vehicle miles travelled	.49	-.193	-.155	-.118	.006	-.033	<.01	-.180	.026	-.104	-.044	.126
		(.108)	(.101)	(.150)	(.115)	(.094)		(.087)	(.146)	(.115)	(.078)	(.064)
Alternative legislation/ enforcement												
Handheld cell phone ban also in place	.09	-.312	-.257	-.396	-.129	-.114	<.01	-.002	.257	.035	.295	.413
		(.149)	(.099)	(.166)	(.119)	(.103)		(.116)	(.113)	(.133)	(.118)	(.136)
Handheld ban not in place	.76	-.122	-.067	.082	.086	.093	<.01	-.181	-.036	-.029	-.079	.087
		(.120)	(.105)	(.107)	(.121)	(.087)		(.125)	(.246)	(.117)	(.111)	(.053)
Alternative modeling												
Negative Binomial	.35	-.090	-.082	.023	.006	.002	.14	-.073	-.031	-.006	-.017	.040
		(.067)	(.044)	(.041)	(.044)	(.038)		(.040)	(.064)	(.035)	(.039)	(.020)
Additional tests												
Removing the control for state-specific effects	.68	-.116	-.075	.006	.085	.052	<.01	-.017	.147	.035	.111	.266
		(.060)	(.060)	(.081)	(.075)	(.051)		(.087)	(.144)	(.071)	(.101)	(.068)
Balanced set of states with laws in effect for at least 6 months	.02	-.174	-.164	-.065	.009	-.005	<.01	-.132	.063	-.041	.023	.190
		(.104)	(.092)	(.136)	(.112)	(.089)		(.087)	(.161)	(.098)	(.094)	(.070)

Note: Each row is from a separate regression estimation of equation (2). Each specification includes state and month fixed effects, as well as controls listed in Table 2 and used in the fifth column of Table 3. These include state-specific trends.

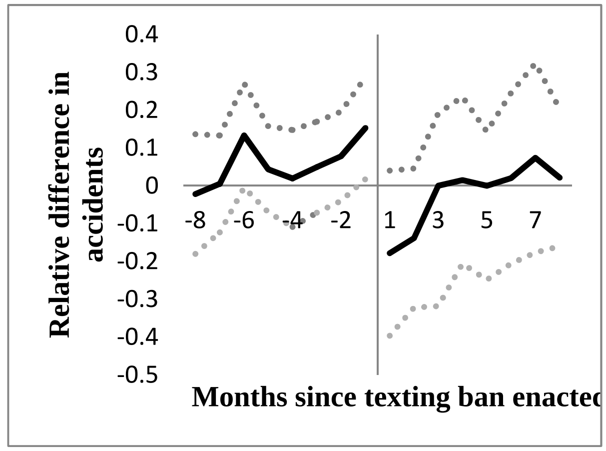
Figure 3: Lead and lag effect

1a. Universally applied and primarily enforced bans; lags extended to 8 months

Without state-specific trends

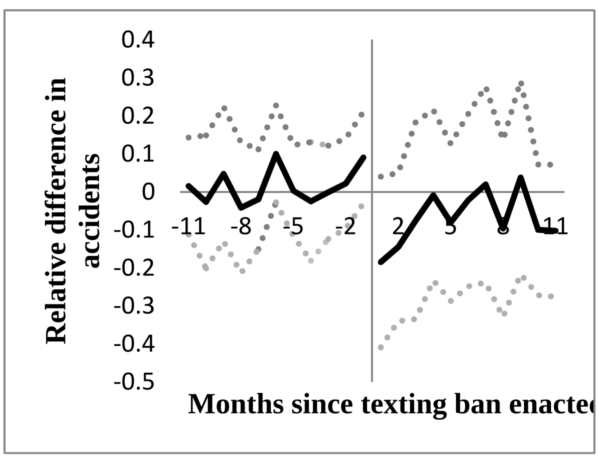


With state-specific trends

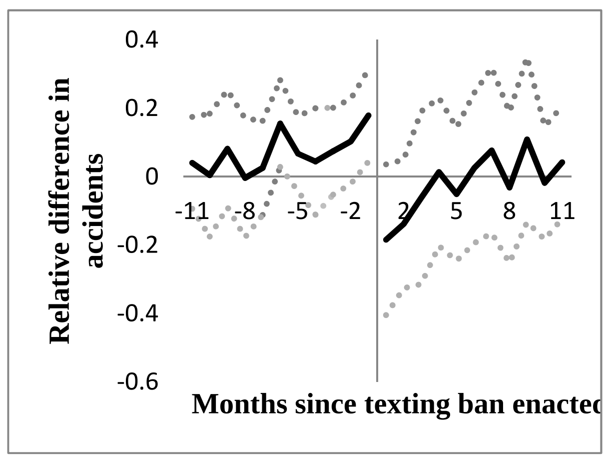


1b. Universally applied and primarily enforced bans; lags extended to 11 months

Without state-specific trends



With state-specific trends



Note: These figures plot the estimated lead and lag coefficients from equation (2) extended to additional leads and lags. Regressions include both 49 state and 48 month fixed effects and the control variables from the third column of Table 3.

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Chapter 2: Homeschooling and Child Health

I. Introduction

Poor health during childhood can affect health in later stages of life. Blackwell et al. (2001) show that poor childhood health increases morbidity during adulthood. In addition, unhealthy children have lower educational attainment, which can lead to poor adult health. Case et al. (2005) find a significant relationship between child health and their socioeconomic status. Their study emphasizes that children born into poor families have poor health and low investment in human capital, which leads to health deterioration when they become older. There is a broad literature examining a variety of determining factors of children's health, such as parents' education and employment status, family income, child's birth weight, child's gender, mother's age at birth of child, and other factors (See Currie and Madrian, 1999; Case et al, 2002; Currie, 2009). One potentially overlooked determinant of childhood health is the method of schooling, particularly if that schooling method increases the time a parent spends with a child.

Child homeschooling has become widespread during the past ten years in many developed countries, including the United States. In the United States, the number of homeschooled students grew from an estimated 1.3 million in 1999 to 2.4 million in 2010. Bielick (2008) estimates the growth rate of homeschooling at around 8 percent per year. Despite this growth, there is little economic research on home education and almost no previous investigation on the health aspects of homeschooling. This study fills that void.

Grossman's health production model (1972) characterizes health as a type of capital, which depreciates at a constant rate. One can invest in health by allocating time to health improvement or purchasing a set of market goods such as medical care, diet, housing or recreation. In his model, the utility of the representative agent in the economy depends on his health status. Jacobson (2000) extends the Grossman model by considering the family as both producer and consumer of health. She also adds the child's health in the family utility function emphasizing that better child health increases the parent's utility. Therefore, parents use the market good inputs and their own time to upgrade their child's health.

According to the Grossman and Jacobson models, homeschooling could be taken into account in production of health by either affecting the productivity parameters or by increasing the parental time spent on the production of child health. The latter path seems to fit the provision of homeschooling well. There are obligations on the minimum number of hours that parents should homeschool their child. For example, in Pennsylvania and Kansas, parents have to teach their children 900 to 990 hours per year in at least 180 days of instruction. Therefore, parents (especially mothers) who homeschool their children have more direct contact with them, allowing greater time to devote to observing and fostering healthy behaviors. Therefore, the allocated time by parents could result in better health directly or could upgrade health via improving productivity factors in the health production function.

There are several other reasons to expect a homeschooling-health link. Because of less exposure to the public, it is less likely that homeschooled students suffer from infectious diseases. According to Cai et al. (2002), another threat for the non-homeschooled students, especially teenagers, is exposure to drugs. The most important reason, however, for why homeschooled children would be healthier is that they are under close attention and training of their parents, especially their mothers. Therefore, parents can control their child's health-related behaviors, as well as observe their health problems and take them to the physician early on to prevent more severe issues. On the other hand, regular students spend 6 to 10 hours out of the home every day and therefore their parents are less likely to be vigilant regarding their health. Moreover, the homeschooled students are provided with better nutrition than regular students. During the school hours, students usually get low quality foods that normally have higher fat content. Instead, their counterparts benefit from fresh and healthy homemade foods (See Perry, 2008).

In this paper, by adopting two different identification strategies, one IV and one quasi-experimental, I estimate the effect of homeschooling on the health status of children between 5 and 17 years old. There is a strong effect of homeschooling, particularly for students ages 8-12. This concentrated effect suggests increased parental involvement and time spent is the likely mechanism. Homeschooling does not affect the health of teenagers. Since drug usage and deficiency occurs mostly in the teenage years, it is unlikely home education enhances children's health through this route. The limited impact on younger children also suggests the infectious disease route is also of secondary importance.

The novelty of this research is that it introduces the method of schooling itself as another determinant of child's health. Previous research typically considers education's effect in two stages. First, individuals achieve a given level of education. Then, their health status is determined based on how that education can improve their production of health (Grossman, 1972). In other words, this literature assesses the long term effect of education on adult health rather than the more immediate question of whether a child's health status varies with the schooling method that his or her parents chose.

In Section II, I review the literature on child health that identifies its determinant factors. Section III discusses the reasons of choosing home education by parents. Data used in this study are discussed in Section IV. I explain estimation methods and discuss results in Section V. Finally, Section VI concludes.

II. Literature Review

The application of health production to homeschooling requires one to believe that homeschooled students are receiving better health inputs than their non-homeschooled counterparts. Although studies on the comparative health of the homeschooled are limited, there is some evidence to suspect they might be benefitting from a healthier environment. By studying a sample of 65 homeschooled children and 47 public school children between 7-11 years old, Perry (2008) tests if there is any difference in nutrient intake of public school children compared with their homeschooled counterparts. The study shows that homeschooled children get 15 percent more calories, 17 percent more protein and 20 percent more fiber than the regular students. In this study, 81 percent of the hoeschooled children were lean, 13 percent were overweight and 6 percent were classified as obese. On the other hand, 68 percent of regular students were lean, 19 percent were overweight and 13 percent were obese.

Neuville et al. (2006) studied a sample of 26 children between 10 to 14 years old, 9 of which were homeschooled, who were timed running for one mile. Using the recorded time, along with other individual characteristics such as height and weight, students' aerobic capacity was calculated. The results show that the difference in cardiovascular fitness between homeschooled children and the public school children was significant. It was shown that home-educated students on average have 15 percent higher aerobic capacity (VO_2 max) value than public school students.[25] In addition to a small sample size, there were some problems in the design of the experiment. The homeschooled children knew that they were part of the experiment, and the regular students did not. The other issue is that for the homeschooled children, the test was done indoors while regular students did the test in an outdoor soccer field. Despite these facts, the experiment is certainly suggestive that the homeschooled children are likely more physically fit.[26]

Although the particular question of the health effects of homeschooling has not been tested in large-scale studies using population data, ample study of the effect of education and socioeconomic factors on health have been undertaken. Although one might suspect income to be critical, the one message that these studies seems to have in common is that mothers' characteristics are critically important for child health. Khanam et al. (2009), for example, recently showed that the effect of family income diminishes when more maternal characteristics are added to the model. Propper et al. (2007) suggest that family income becomes ineffective on child health when maternal mental health is taken into account.

Over the past several decades of study, two maternal characteristics that are likely relevant to homeschooling have emerged as particularly important. The first is mother's education, which has long been observed to be negatively correlated with child mortality (Caldwell, 1979). The second is the amount of time a mother spends with a child, which is inversely related to maternal education but would be positively correlated with homeschooling.

The literature on the effect of maternal education on child health is more extensive than the literature on time spent with a child. Although a complete review is beyond the scope of the paper, it is useful to discuss a few highlights of this literature. Desai and Alva (1998), using data from 22 countries, conclude that mother's education is correlated with child health. Using Brazilian demographic and health survey, Thomas et al. (1991) show that mother's education

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[25] VO_2 max is the maximum rate of oxygen that body can take up and utilize. This index could be regarded as a measure of healthiness.

[26] The study was also limited in controlling for other reasons for the differences in the results, including socioeconomic status.

could affect child health through access to more information in the form of reading newspapers, watching television and listening to radio. Barrera (1990) suggests other reasons for the maternal education-child health link, such as changing the productivity of health inputs via an increase in mother's non-market efficiency, changing household preferences, and increasing the chance of marrying a wealthier husband. He also concludes that well-educated mothers improve the health of children under 2 years old since their health has not yet stabilized. Case et al. (2002) indicates that educated mothers have healthier children and suggests that maternal health has a greater effect on child health compared with paternal health for all age groups. Currie and Stabile (2003) use the National Longitudinal Survey of Children and Youth (NLSCY) data to estimate the effect of family income and maternal education on health of children between ages 3 and 15 in Canada. Their results are quite similar to those in Case et al. (2002). Chen and Li (2009) consider a sample of adopted children in China, which helps them remove the genetic determinants of child health, and find that adopted children with well-educated mothers are healthier even after controlling for income.

Contrary to the above findings, Schultz (1984) suggests a well-educated mother has a higher opportunity cost, which limits the time she can spend with her child as she might intend to work more. This hints that there may be other pathways for mothers' characteristics to improve health. Particularly, the time mothers spend with children might prove crucial. A series of studies have offered evidence that speak to this issue. Using 1997-2002 data from Health Survey of England (HSE), Currie et al. (2007) finds no evidence to support that parental unemployment might deteriorate child health, which at first might seems to run counter to the expected effects of the lower income and education of the unemployed. Although generous unemployment benefits in the UK are a mitigating factor, parental unemployment also increases the time parents spend with their child, which may have an offsetting positive effect on child health. Currie et al. (2007) also conclude that nutrition and family lifestyle choices play an important role in child's health. They conclude that children who eat vegetables regularly are healthier regardless of family income.

Other studies have looked more directly at maternal employment's effect on child health. Using the National Longitudinal Survey of Youth (NLSY), Anderson et al. (2003) find that there is a positive causal relationship between the number of hours that a mother works per week and the likelihood that her child is overweight. Their results indicate that it is hours per week worked that affects the likelihood that a child is overweight, rather than the number of weeks worked. Fertig et al. (2009) replicate the Anderson et al. (2003) study using the Panel Survey of Income Dynamics (PSID) and obtain very similar results. Their study also indicates that maternal employment is related to children's Body Mass Index (BMI) through the average number of meals consumed and time spent on sedentary activities. Cawley and Liu (2007) find that employed women spend significantly less time cooking, eating with their children, and playing with their children. They are also more likely to purchase prepared foods, which could increase the likelihood of child obesity.

As these studies imply, childhood health could be sensitive to the time mothers spend with their children. Homeschooling, as an alternative form of educational provision, leads mothers to allocate more time to their own child and might enhance child health. The results in Section V support this conjecture.

III. What is known about decision to homeschool a child?

Most children receive education by going to a public or private school outside of the home. However, due to perceived delinquencies in various aspects of public and private education systems, parents may prefer to homeschool their children. According to the NCES report in 2007, 85 percent of students used the public system and 11.4 percent used private schools. Apart from this, 2.9 percent of students were homeschooled.

According to Bielick (2008), 35 percent of homeschooling parents cite that the most important reason they chose homeschooling was to provide their children with religious or moral instruction. Later, we take into account the degree of religiosity as a determining factor of homeschooling. In addition, 20 percent of them believe that their incentive for homeschooling was to protect their children from the negative aspects of the public school environment, especially issues such as safety, drug use, and negative peer group pressure. According to Grady et al. (2010), 77 percent of families involved in homeschooling are white, 10 percent are Hispanic and only 4 percent are from black. This report also shows that 45 percent of homeschooled children are in grades 1 to 5. In addition, this type of education is more popular in suburbs and rural areas in which 77 percent of US homeschooling takes place.

Isenburg (2002) investigated the effect of public or private school quality on parents' decisions to homeschool their children. The findings are consistent with poor quality of traditional schooling options being to blame. Parents also believe that by homeschooling their children, they could reduce the likelihood that their children engage in risky behaviors, such as alcohol consumption, drugs, violence and premarital sex.

Houston and Toma (2003) discuss how maternal education and heterogeneous income distributions within public school districts increase the rate of homeschooling, but tight regulations negatively affect it. In addition, poor quality has a similar effect to that found in other studies.

Belfield (2002) studied the major reasons for why parents end up choosing a schooling method among public, private-independent, private-religious and homeschooling. He finds that the families involved in homeschooling are not that different from those who choose other schooling methods. However, he finds that mothers' characteristics are very important in parental choice of homeschooling, especially her employment status as well as religiosity.

Howell and Sheran (2008) use the same data that I use to study the determinants of the homeschooling decision in the United States. Their findings show that the probability of homeschooling increases by 1.5 percentage points in families in which the mother is not in the labor force, assuming maternal employment to be exogenous. Maternal employment, along with a set of socioeconomic variables, however, could be both determinants of child health as well as homeschooling decision. On the other hand, the degree of religiosity of a family likely does not affect child health. Therefore, I will propose using religiosity, among other instruments, to address what is likely to be the endogenous choice to homeschool a child. Endogeneity of homeschooling and my empirical strategy are discussed more extensively in Section 5.

IV. Data

I use the Parent and Family Involvement (PFI) survey of NCES to test my hypothesis about the association of homeschooling and child health. This survey is a random digit dialing phone survey, which is carried out nationally every four years across all states in the United States. NCES changes the panel of households each year and interviews a new cross section of them. In the PFI survey, parents answer some questions about the type of schooling they choose for their kids, including public and private schools, as well as home education. Since 2003, the parents were asked about their child's health status and some other health related questions.

Unfortunately, the health status and homeschooling variables are not available for years prior to 2003 and they are jointly available only for 2003 and 2007. Table 10 illustrates the summary statistics of data both for homeschooled and non-homeschooled children. The health status of the child is reported by parent or guardian in an ordinal manner from 1 to 5. This measure is similar to those used in Case et al. (2002) and Currie and Stabile (2003).[27] I rescale the health status variable such that 1 represents poor health, 2: fair, 3: good, 4: very good and 5: excellent. For some specifications, I also generate a dummy for health status that equals one for those with very good or excellent health and zero otherwise. I restrict attention to children in the traditional schooling age rage 5 to 17. The survey includes 21,701 children who were sent to public or private schools, and 537 homeschooled children in the combined 2003 and 2007 samples.

Child age, a gender dummy, and a set of race and ethnicity dummy variables indicating whether a child is white, black or Hispanic are included as covariates to serve as controls for socioeconomic status (SES). Household income is available in the survey in a categorical format. I consider the midpoint of the income category as household income for each individual and use the log of income in the regressions.[28] In addition, to take into account maternal characteristics, a dummy for her educational background is considered. The rest of Table 10 reports mother's education , as well as year and region dummies. Column 2 of Table 10 presents the summary statistics for homeschooled students.

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[27] For more explanation, see Currie, 2008.

[28] Since the last category is unbounded, I assume that the highest income is $150,000.

Table 11 shows the unconditional child health variation for three age groups: 5-7, 8-12 and 13-17 years old. According to Table 2, 59.54 percent of non-homeschooled children between 8 to 12 years old are reported to have excellent health status while 72.96 percent for their homeschooled counterparts have excellent health. On the other hand, 59.08 percent of non-homeschooled teenagers (those older than 12) benefit from excellent health while this number is 57.40 for homeschooled kids in this age group. The test for homogeneity of proportions is significant, suggesting that child health is different across homeschooled and non-homeschooled children. This does not take into account the influence of the covariates listed in Table 1, nor does it consider the endogeneity of homeschooling. Yet, it does stand as an important pre-condition that the health of homeschooled children ages 8-12 is better than their traditionally-schooled counterparts.

V. Method and Estimation

A. Identification strategies

Each strategy begins by using an ordered Probit analysis, with the dependent variable being child health status on a 1-5 scale. The covariate of interest is a dummy equal to one if the child is homeschooled and zero otherwise. A set of other explanatory variables are typically included, including child age and gender, race and ethnicity, family income, child disability, mother's education , and year and region dummies.[29]

The first means to identify plausibly exogenous variation in homeschooling is an instrumental variable ordered Probit model. Such an approach requires identifying variables that affect the decision to homeschool but are free of correlation with unobserved factors affecting health. While finding a perfect IV is very difficult, I introduce a set of IVs. Each IV has merit in terms of explaining homeschooling and passing the typical statistical tests for instrument validity.

I consider two instruments. The first instrument is based on a question, which reflects the degree of religiosity of the family. The question is "During this school year, has (CHILD)

[29] Inclusion of a dummy for living in urban area and family size in the reduced form regression results in statistically insignificant estimated coefficients. While I do not report in Table 3, I use this point to employ these variables as the second set of IVs for homeschooling given that they can explain homeschooling decision.

participated in any of the following activities (outside of school)? Church or temple youth group or religious instruction”. This question is asked from all individuals in the sample, including homeschooled as well as non-homeschooled children. This variable might be a suitable instrumental variable because according to Bielick (2008), religion is a determining factor of homeschooling decision. On the other hand, there are a number of studies which address the region-health link (Levin 1994, Deaton 2009). However, as we will see later, homeschooling is only positively associated with a better child health for children between 8 to 12 years old and ineffective to the health of children in other age groups. This differential effect might be suggestive in that religiosity is a valid IV and could be used in our analysis with less concern. This assumption is of course testable with additional instruments.

The second instrumental variable is related to extracurricular activities that might indicate a parent would home school a child. Specifically, the question “During this school year, has (CHILD) participated in any of the following activities (outside of school)? Scouting or other group and club activities”. Again, this question was asked of all individuals in the sample regardless of their schooling type. Many states give equal right to homeschoolers to utilize the public school districts’ extracurricular activities.[30] Scouting and other group activities might have a positive effect on the decision making process to homeschool, as families observe successful experiences of homeschooling children among other families in these group activities. As we see later, the first stage results in Table 12 Column (4) support this idea.[31]

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[30] For a list of states that have such laws see http://www.hslda.org/docs/nche/Issues/E/Equal_Access.pdf.

[31] In the appendix, Table A1 illustrates the results including three IVs in this first set. It includes log of number of regional internet users per regional population as another IV. I appeal to the fact that much information on the benefits of homeschooling comes from the internet. Homeschoolers can learn about the benefits, as well as receive the curricular materials through the internet. For example, Illinois and Florida provide some online high school courses designed for homeschoolers. There are also some private companies that produce online materials for home education. In order to utilize these online products, it is essential for the families to have an internet connection. Therefore, it is reasonable to consider internet penetration in US families as an instrumental variable (IV) for homeschooling. In order to do so, the number of internet users in different regions in the United States weighted by regional population in 2003 and 2007 is construed as a proxy for internet penetration among US families. In addition, the information of whether the family has access to the internet at home or not is also available in the survey. This variable itself may be endogenous as an instrument, which I verified using the Hansen J statistics when I conduct robustness check using Two-Stage Least Square (2SLS). As I explain later, regional internet penetration does not suffer from these issues. However, comparing the p-value of Hansen J statistic including the two first IVs proposed in the main text preferred to all three. Therefore, the main results are based on including Church and scouting/club activities as IVs. But I report the three IV case in the appendix. Appendix Figure 1 illustrates the percentage of home internet users by income for 2003 and 2005 and shows that it is not correlated with the household income. Hence, this IV might not be correlated with family income, which itself can affect child health.

In order to make sure that IVs are not weak, I conduct a test of joint significance of IVs for the first stage. In addition to that, I carry out an overidentification test for each set of IVs in which the null hypothesis of valid IVs (not correlated with the error term) could not be rejected. I discuss these tests in detail in section 5.2.

The econometric model, with individual subscripts suppressed, is as follows:

$$y = \alpha_1 + \beta_1 HS + X\gamma_1 + \varepsilon_1 \qquad (1)$$

$$HS = \alpha_2 + Z\beta_2 + X\gamma_2 + \varepsilon_2 \qquad (2)$$

The variable *y* represents child health, *HS* is the homeschooling dummy variable and *X* is the vector of other explanatory variables. In the first stage regression, *Z* are the IVs that predict homeschooling. Column (4) of Table 12 illustrates the IVs are significant and the F-test of joint significance of IVs in the first stage regression is reported separately. I recognize there are a number of alternatives to estimating my model in this fashion and take these up in a series of robustness checks later in the paper.

B. Quasi-experimental approach

While the IV approach illustrates strong evidence of positive effect of homeschooling on child health, one might question the validity of the results obtained, especially on conceptual grounds with regard to the exogeneity of the IVs used. In this section, by adopting a different identification strategy, I try to mitigate this concern. I implement a quasi-experiment in which I restrict the sample to families with two children, only one of which is being homeschooled. This holds family background fixed. What remains as the only difference is that one of the children is being homeschooled but the other one is being sent to a public or private school. This approach helps tackle the endogeneity problem we faced in previous section and provides validation that those findings were indeed correct. To conduct such analysis a data set that includes children in the same family is needed. The 2003 PFI survey from NCES is the only known survey that gives the flexibility to carry out such experiment. In that year, there were fifty families with one homeschooled child and one not homeschooled. Although this leaves me with just hundred observations, it is a more plausible exogenous experiment.

VI. Results

Column (1) of Table 12 shows the results considering health as a function of homeschooling only. Column (2) adds a series of socioeconomic covariates, as well as mother's education. The first two columns of Table 12 show that, assuming homeschooling is exogenous, it has a positive but only marginally significant impact. The explanatory variables predict health as expected; child health depreciates by age and boys are healthier compared to girls assuming all other factors to be constant. In addition, white children are healthier regardless of which estimation method we use. The results also indicate that household income positively affects health.

An important factor that stands out is mother's education. This factor has long been considered important determinants of child health. My results show that child health could be adversely affected by mothers' low level of education (less than a college degree). We will return to this variable and its possible relationship with home schooling's effect later.

Column (3) and (4) take endogeneity into account by using the two IVs. The coefficient of homeschooling becomes more pronounced and statistically significant. The estimation results indicate that homeschooling positively affects the child health. Using IV approach necessitates a series of post-estimation diagnostic tests. We should verify that the IVs are jointly significant and they are valid and not correlated with the error term. The results from the F-test of joint significance of IVs are reported in Column (4) of Table 12 and show that the excluded IVs (*Church*, *Scouting activities*) are jointly significant when we control for other explanatory variables in the first stage. To test the validity of IVs, I use the Hansen test of overidentification restrictions in 2SLS estimation since a similar test is not available for IV ordered Probit model, and the estimation results from 2SLS is similar to that of IV ordered Probit. Table 16 reports the results for the Hansen J statistic. The p-value is large enough for both sets of IVs to imply that the null hypothesis of valid IVs is not rejected.[32]

In Table 13, I conduct a subsample analysis for children within the age groups of 5 to 7, 8 to 12 and 13 to 17 years old. Panel A considers no instrument while Panel B corrects for endogeneity. The results indicate that homeschooling has the more pronounced effect on child

[32] In Table 16, Rows (1) and (3) report the p-values of Hansen test of overidentification restrictions.

health for children of age 8 to 12 across all two panels. This result is in accord with the existing literature on the association of children's health and time they spend with their mothers. During the pre-teen ages, children would likely be more influenced by the additional time spent with a mother. This result is also very suggestive in that although the homeschooling does not have an immediate effect on child health, it improves the child's health after two or three years of positive health inputs. This might be related to healthy nutrition at home. It is also the case that the homeschooled children ages 8-12 likely have more limited contact with other students, but the lack of an effect for younger children ages 5-7 suggests it is the maternal contact that is likely causing the better health.

A. Maternal characteristics and homeschooling effect

Table 5 explores the mechanisms of the effect of homeschooling and child health further by presenting the estimated coefficient of homeschooling on child health given information about one's potential maternal influence. I use the same set of IVs and sample from Table 12. I first am interested in whether mother's education, which has its own strong influence on health, matters in terms of whether homeschooling is more or less influential on health. Interestingly, children of less educated mothers are the ones whose health is improved by homeschooling. Row (2) analyzes the effect of homeschooling on child health for two income levels: below $50,000 and above $50,000. The results show that homeschooling has a larger effect on child health for families at or below the national average income. It also indicates that homeschooling does not affect child health in families with income above $50,000.[33] Taken together, these results suggest that the maternal involvement that comes with home schooling has a strong effect on health but only for the less educated, lower income families. This is indicative that homeschooling somewhat substitutes for these families what higher education and income buys for other children. Specifically, there are multiple ways mothers can improve the health of their children. It can be achieved through highly educated mothers being better able to provide

[33] Unfortunately, I could not find a way to test if the estimated coefficients of homeschooling in two different subsamples are statistically different using IV ordered Probit models. However, I use another strategy for estimating the effect of homeschooling in which I generate a dummy from the interaction of homeschooling dummy and another dummy equal to one if the child belongs to the subsample of interest. Since the homeschooling dummy is endogenous, so is the interaction variable. Therefore, in the simplest case two more IVs are added which are themselves interactions of initial IVs (Church and Scouting), with the dummy for the subsample of interest. The results support the findings in Table 14, especially those related to mothers' education and family income and are reported in the Appendix Table A4. However, there are still some concerns that the IVs for endogenous interaction dummies are not valid conceptually, although Hansen J test validates them mechanically.

positive health inputs to their children. Alternatively, those inputs could also be fostered with time spent with children. I conduct some additional estimations to confirm this point in rows (3) and (4). When a mother works full time, homeschooling is ineffective on child health. However, homeschooling enhances health of a child whose mother either does not work or works part time. Row (4) considers the presence and the absence of mothers in the family and indicates that, in the presence of a mother, homeschooling affects the child health positively. When a mother is absent, homeschooling becomes ineffective. Although the sample size is small for the latter subsample, the result is suggestive that mothers' time spend with a child is a key determinant of child health.

Results in Table 14 imply that the time a less educated mother or a parent in a low-income family spends with the child (in the form of homeschooling) matters to the child health. In other words, time spent by a less educated mother or a parent in a low-income family makes up part of the child's health gap with a child living in a family with high socioeconomic backgrounds. It could also be that part of the gain to mother's education is mitigated by the higher opportunity cost of a mother's time in providing inputs to their children. In either case, these results are strongly supportive of the notion that maternal time with children is valuable to child health.

B. Alternative models and robustness checks

Table 15 illustrates the results for the quasi-experimental approach. Column (1) shows the estimated coefficients of homeschooling using an ordered Probit regression for the sample of hundred children in fifty families. It includes age, gender dummy, disability, race and ethnicity dummies, maternal education and log of household income. Column (2) reports the results from the same set of regressions but in a sample including homeschooled children not older than 12 year of age. This sample is very similar to the subsample analysis in Section V.A, However, I was unable to split the sample into three age categories due to the lack of enough observations. The effect of homeschooling is positive and more pronounced for the restricted sample, and it is statistically significant. This strongly suggests that the homeschooled children under 13 are healthier than their siblings who are not being homeschooled. This finding is consistent with the result I obtained using the first set of IVs, confirming that it is very likely that homeschooling could lead to better health outcome for children.

In Table 16, I present batteries of additional robustness checks. Given that there are reasonable alternatives to the IV ordered Probit model, I test whether alternative models yield similar results. In panel A, Row (1), I show the estimated coefficient of homeschooling on child health from 2SLS using church and scouting activities as IVs. The 2SLS results in a larger coefficient, which is also statistically significant. The p-value of Hansen J statistic also implies that the instruments are valid and are uncorrelated with the error term. Since child health is an ordinal outcome, it is recommended to use ordered Probit rather than 2SLS. However, the diagnostic tests available for 2SLS method enable us to confirm the appropriateness of the IVs.

In Row (2), using an IV ordered Probit model, I exclude the students attending private schools. Since many private schools have religious affiliations, excluding private school attendees should help us identify the effect of homeschooling on child health better. The estimated coefficient of homeschooling does not change notably.

In panel B, results are reported using the same models as in rows (1) and (2) but with a different measure of child health. It also adds Row (4) in which I report the results using an IV Probit model. We use a binary variable equal to one if child health is excellent or very good and zero otherwise. Again the results are consistent with those in panel A, each confirming the positive association of child health and homeschooling.

VII. Conclusion

Homeschooling plays a small, but growing role in the education of children. Since 1999 the number of homeschooled children has increased by almost 60 percent. The results of this paper show that being homeschooled significantly enhances the health status of children between 8 to 12 years old relative to their private and public school counterparts. The findings suggest that home education does not affect health of those of age 13 and older.

The likely pathway from homeschooling to better early childhood health is the increased time, spent with a child, particularly by a mother. The results also indicate that mother's education is a crucial factor in determining child health. Mothers with higher level of education have healthier children. Homeschooling, however, can partially alleviate the gap between the early health of children with educated mothers and children with less educated mothers. This result shows that by spending more time with her child and providing him with healthier foods and other positive physical and mental health inputs, s a mother can partially make up forthe benefits that come with higher education. Additional estimations confirm that itis indeed the presence of the mother and the time spent with a child that likely explains the health-homeschooling correlation.

This study is one of the first of what will likely be a large literature on the effects of homeschooling on children. The study is far from concluding whether homeschooling is a socially desirable means of schooling kids. It does highlight one important aspect of home schooling, however, which is the health benefit of increased contact between mother and child at early ages.

Table 10 Summary statistics

	(1) Non-homeschooled		(2) Homeschooled		(3)	
Variable	**Mean**	**Std. Dev.**	**Mean**	**Std. Dev.**	**Min**	**Max**
Health	4.41	0.84	4.46	0.89	1	5
Homeschooling	0	0	1	0	0	1
Age	11.28	3.69	11.27	3.84	5	17
White	0.60	0.48	0.75	0.43	0	1
Black	0.12	0.33	0.06	0.23	0	1
Hispanic	0.20	0.40	0.11	0.31	0	1
Disability	0.24	0.43	0.24	0.43	0	1
Income	69144	47694	61419	40801	2500	150000
Mom education $\leq$ some college	0.63	0.48	0.63	0.48	0	1
Mom employed	0.68	0.47	0.40	0.49	0	1
Church involvement	0.58	0.49	0.73	0.44	0	1
Scouting and/or Clubs activities	0.23	0.42	0.33	0.47	0	1
Midwest	0.22	0.41	0.18	0.39	0	1
West	0.25	0.43	0.25	0.43	0	1
South	0.36	0.48	0.44	0.50	0	1
yr07	0.46	0.50	0.55	0.50	0	1
Obs.	21701		537			

Table 11 Percentage of homeschooled and non-homeschooled children with different health status in two age groups.

Health / Age	Not homeschooled					Homeschooled					Chi-squared test
	1	**2**	**3**	**4**	**5**	**1**	**2**	**3**	**4**	**5**	
5-7											
Percentage	0.40	2.63	9.96	24.06	62.94	0.85	1.69	7.63	18.64	71.19	4.238 [0.375]
Frequency	18	119	450	1087	2843	1	2	9	22	84	
8-12											
Percentage	0.40	2.94	11.21	25.91	59.54	0.51	2.55	6.12	17.86	72.96	15.207 [0.004]
Frequency	33	242	923	2133	4902	1	5	12	35	143	
13-17											
Percentage	0.58	3.52	11.66	25.16	59.08	1.79	6.28	11.66	22.87	57.40	10.42 [0.034]
Frequency	52	315	1044	2252	5288	4	14	26	51	128	

Note: Numbers in parenthesis are P-value of the homogeneity test.

Table 12 Effect of homeschooling on child health

	Ordered Probit		IV Ordered Probit	
	(1)	(2)	(3)	(4)
			Stage 2	Stage 1
Homeschooling	0.124	0.086	1.514	
	(0.080)	(0.089)	(0.266)***	
Age		-0.011	-0.010	0.000
		(0.003)***	(0.003)***	(0.006)
Male		0.071	0.077	-0.057
		(0.022)***	(0.022)***	(0.047)
White		0.208	0.188	0.118
		(0.034)***	(0.034)***	(0.085)
Black		-0.002	0.040	-0.443
		(0.045)	(0.045)	(0.111)***
Hispanic		-0.267	-0.213	-0.401
		(0.033)***	(0.035)***	(0.076)***
Disability		-0.182	-0.174	0.027
		(0.024)***	(0.024)***	(0.053)
Log of income		0.086	1.514	0.000
		(0.089)	(0.266)***	(0.006)
Some college or lower		-0.011	-0.010	-0.057
		(0.003)***	(0.003)***	(0.047)
Excluded IVs				
Church activities				0.328
				(0.050)***
Scouting activities				0.182
				(0.049)***
F-test				61.17
				[0.000]
Obs.	22,238	22,238	22,238	22,238

Note: The coefficients for year and region dummies are included but not reported in the table. All regressions are weighted and numbers in parentheses are robust standard errors. F-test reports the F statistic for testing the joint significance of IVs in the first stage regression with the p-value reported in the brackets. *, ** and *** indicate $p<0.1$, $p<0.05$ and $p<0.01$, respectively.

Table 13 Effect of homeschooling on health across different age groups

	(1)	(2)	(3)
Panel A: No instrument	5-7	8-12	13-17
Homeschooling	-0.046	0.309	-0.011
	(0.280)	(0.104)***	(0.107)
Panel B: With instrument			
Second stage			
Homeschooling	-1.345	1.983	0.760
	(0.832)	(0.095)***	(0.623)
First stage (Excluded IVs)			
Church activities	0.334	0.480	0.224
	(0.106)***	(0.079)***	(0.082)***
Scouting activities	0.250	0.219	0.150
	(0.156)	(0.069)***	(0.092)
F-test	10.84	51.65	9.28
	[0.004]	[0.000]	[0.010]
Obs.	4,635	8,429	9,171

Note: Each column reports the result from an IV Ordered Probit regression for a specified age group. The first row shows the estimated coefficient of homeschooling on child health followed by the first stage results for the estimated coefficient of excluded IVs. Variables included in both stages are age, a gender dummy, race, and disability dummy, log of family income, region and year dummy. F-test reports the F statistic for testing the joint significance of IVs in the first stage regression with the p-value reported in the brackets. All regressions are weighted and the numbers in parenthesis are robust standard errors. *, ** and *** indicate $p<0.1$, $p<0.05$ and $p<0.01$, respectively.

Table 14 Subsample analysis of the effect of homeschooling on child health

With instrument, IVs: Church, scouting activities		
	Mothers with some college or lower education	**Mothers with college degree or higher education**
(1)	1.629 (0.154)***	0.233 (2.179)
Obs.	14,956	7,282
	Income < $50K	**Income > $50K**
(2)	1.445 (0.197)***	1.162 (1.931)
Obs.	9,501	12,737
	Mothers do not work or work part time	**Mothers work full time**
(3)	1.532 (0.260)***	0.161 (0.466)
Obs.	10,966	11,272
	Mother is present	**Mother is absent**
(4)	1.441 (0.304)***	0.015 (0.722)
Obs.	20,668	1570

Note: All coefficients are coefficients of homeschooling on child health estimated using IV ordered Probit. All regressions are weighted and numbers in parentheses are robust standard errors. *, ** and *** indicate p<0.1, p<0.05 and p<0.01, respectively.

Table 15 Effect of homeschooling on child health, quasi-experimental approach

	(1) Full sample	(2) Homeschooled=1 & Age < 13 Yrs.
Panel A	***Ordered child health (1=poor to 5=excellent)***	
Homeschooling	0.112 (0.252)	0.776 (0.330)**
Panel B	***Binary child health (0=good or below, 1=very good & excellent)***	
Homeschooling	0.068 (0.294)	0.830 (0.394)**
Obs.	100	88

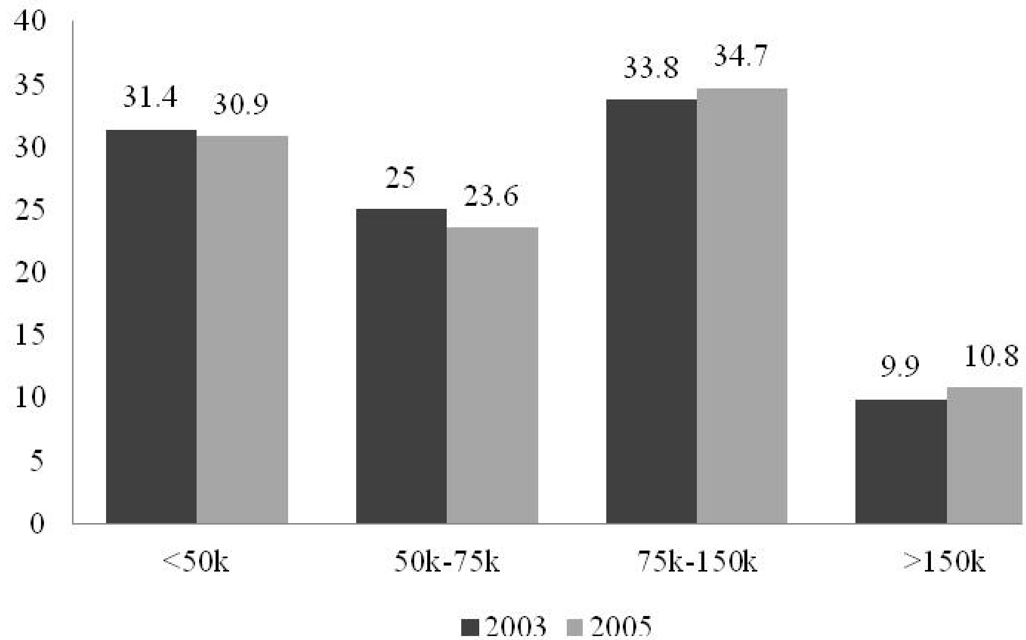
Note: All regressions are weighted and numbers in parenthesis are within family clustered standard errors. Region and year dummies are excluded to achieve convergence due to lack of enough observations. The family background variables are included in the regression but are not reported. *, ** and *** indicate p<0.1, p<0.05 and p<0.01, respectively

Table 16 Robustness checks

Panel A		***Ordered child health (1=poor to 5=excellent)***
2SLS		
(1)	IVs: Church and scouting activities	3.607 (0.975)***
	P-value of Hansen J statistic	[0.479]
IV ordered Probit		
(2)	Excluding private schools IVs: Church and scouting activities	1.480 (0.275)***
Panel B		***Binary child health (0=good or below, 1=very good & excellent)***
2SLS		
(3)	IVs: Church and scouting activities	1.802 (0.455)***
	P-value of Hansen J statistic	[0.739]
IV Probit		
(4)	IVs: Church and scouting activities	5.097 (0.514)***
(5)	Excluding private schools IVs: Church and scouting activities	4.873 (0.482)***

Note: Each row shows the estimated coefficient of homeschooling for the specified regression. P-value of Hansen J statistic is reported for 2SLS regression in the brackets in Rows (1) and (3) both implying that the IVs are valid. Bivariate ordered Probit and Bivariate Probit in the absence of endogeneity represent ordered Probit and Probit, respectively. Rows (2) and (4) show the effect of homeschooling on child health excluding children who go to private schools. All regressions are weighted and numbers in parentheses are robust standard errors.

Figure 4 Internet home users by income (%)



Source: U.S. Census Bureau

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Note: School shootings and private school enrollment

I. Introduction

School shootings, through excessive media coverage, create panic (Jemphrey and Berrington 2000; Muschert 2007) and generate a perception of a public health problem that exceeds its actual danger (Burns and Crawford 1999). According to a post-Columbine survey, two-thirds of Americans believed that it is at least somewhat likely that a similar shooting could happen in their area (Saad 1999). Such widespread fear may prompt reactions from the public that surpass the scope of these isolated events. We offer evidence of one such reaction to school shootings by showing a significant increase in private school enrollments the school year following a shooting. At the same time, public school enrollments decline.34

We confirm that these reactions are likely media-driven. Since socioeconomic factors drive the nature of how school violence is covered by the media, with shootings in urban and predominantly minority schools receiving more limited coverage (See, e.g., Menifield et al., 2001), we would expect stronger effects to follow shootings in nonurban settings. This is supported by our data. We also find that the impact of school shootings is temporary, with a post-shooting effect only observed in the Fall following the shooting. No effects are found in subsequent years, confirming that these enrollment decisions are likely heat-of-the-moment in nature.

[34] The only study of the effect of school shootings by economists that we are aware of showed that test scores for high school students fell following school shootings in Finland. The results were linked to post-traumatic stress syndrome (Poutvaara and Ropponen, 2010).

These results suggest school shootings serve as a viable source of exogenous variation in private school enrollments for future studies wishing to employ a natural experiment approach for assessing the economic implications of private school enrollment.

III. Basic empirical approach and data

To assess the impact of school shootings on enrollments, we estimate weighted least squares regressions on state-level data, summarized by:

$$lprivate_{it} = \alpha_i + \gamma_t + \delta_1 SS_{it} + \delta_2' X_{it} + \varepsilon_{it} \qquad (1)$$

and

$$lpublic_{it} = \alpha_i + \gamma_t + \beta_1 SS_{it} + \beta_2' X_{it} + \varepsilon_{it} \qquad (2).$$

These regressions are weighted by population, which allows for less weight to be placed on smaller states that have higher variability in enrollment. The variables lprivate and lpublic are logs of private and public school enrollment in state i in year t for the school year beginning in the Fall. State (α) and year (γ) fixed effects are included. The variable SS is a dummy variable indicating that a shooting occurred in the previous academic year. We also separate the SS variable into urban shootings and nonurban shootings. Specifically, we define urban shootings as those in places with more than 100,000 people.35

The variable X comprises the control variables in all specifications, including the log of a state's population of those ages 14-18. The log of unemployment is also included since a poorer economic climate might limit private school enrollments. In some specifications, each state dummy variable is interacted with a linear time trend. This allows for identification of state enrollment effects free of confounding trends. 36

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[35] There were two cases (Antioch, CA and El Cajon, CA) that were close to the 100,000 cutoff but circumstances of the shootings clearly guided the classifications into urban and nonurban, respectively.

[36] Adding a richer set of control variables such as mean personal income, percentage of people with

We consider shootings to be exogenous so the controls only add to the efficiency of the estimations. To verify that shootings are exogenous, we use the 2007-2008 School Survey on Crime and Safety from the National Center for Education Statistics to test if school characteristics such as school size, location (urban vs. nonurban) and racial characteristics are statistically different between schools in which a shooting has happened versus other schools. They are not. Moreover, since shootings in our database included wealthy states (CA and MD), poor states (AL and TN), and quintessentially average states (GA and WI), these shootings can and do occur anywhere.

We aggregate private and public high school enrollments using data from the 1998-2009 October Current Population Surveys (CPS) and the CPS population weights. Annual unemployment rates are obtained from the Bureau of Labor Statistics. Our final data consist of fifty states and the District of Columbia from 1998-2009, which totals 612 observations.

III. Results

A. Basic weighted least squares estimation

Panel A and Panel B of Table 17 present the basic private and public enrollment results, respectively. Starting in column (1), where only state and year fixed effects are included, and proceeding through column (3), which adds state-specific trends to the population and unemployment controls, the effect on private school enrollment is a 9.7%-11.6% increase. The public school enrollment effect shows a 0.4%-1.3% decrease.

Despite being statistically insignificant and small, the public school enrollment decrease is consistent with the private school enrollment increase in both sign and magnitude. According to Snyder and Dillow (2011), 1,389,000 students were enrolled in private high schools and 14,807,000 were enrolled in public high schools in 2009. A 9.7%-11.6% increase in private school enrollment therefore corresponds to an 135,000-161,000 increase in the number of students enrolled. A 0.4%-1.3% decline in public school enrollment corresponds to a 59,000-192,000 decrease in public school enrollment.

Bachelor's degree, and percentage of African American people only makes the estimated coefficient of shootings on private school enrollment stronger.

The mechanism by which we suspect school shooting to translate into changes in enrollment is excessive media coverage. Such coverage is more likely following shootings in suburban and rural areas, as opposed to urban areas where youth violence is more expected and not overplayed. In column (4), we report different effects for the urban vs. suburban shootings, and the results are consistent with the expected mechanism.

B. Additional estimates

Table 18 presents additional estimates and robustness checks applied specifically to the private school enrollment results, which were the effects that proved significant in the basic specifications. We first probe the media coverage explanation more deeply by eliminating those shootings that resulted in no deaths, which we would suspect might prompt more limited reaction from parents. The difference in the nonurban and urban effects on enrollment becomes more pronounced, with only the former being negative.

The log specifications using weighted least squares provide easily interpretable elasticities, but we recognize there was an obvious alternative. In the second row of Table 18, we create a measure of the proportion of enrollments among those ages 14-18 that were in private high schools in each state-year cell. The effect of nonurban shootings is a 0.011 increase in the proportion enrolled in private schools. Given that the proportion enrolled in private high schools is just under 9% in the sample, the magnitude of this estimation is comparable in size to the log enrollment specification estimates presented in Table 17. We also present unweighted OLS estimations, confirming weighted least squares was providing, if anything, more conservative estimates.

We used school shootings in the previous academic year to explain enrollment in the subsequent year. We suspect these reactions are heat-of-the-moment decisions amid intense coverage and do not last long. The last three rows of Table 18 are consistent with these expectations. Private school enrollments surge in the school year immediately following a shooting but quickly return to old levels.

IV. Conclusion

We establish a previously overlooked result linking school shootings to private school enrollments. Despite the fact that shooting incidents are relatively rare, their impacts are widely felt. Parents overestimate the potential for such events to be repeated, particularly those that occur in suburban and rural areas, because of intense media coverage. Because this manifests itself in changes in enrollment, we provide a potential source of exogenous variation in school enrollment type for future studies wishing to assess the implications of private school enrollment through a natural experiment approach. For example, the literature on the effect of private schools on student achievement is wrought with conflicting results stemming from issues related to the unobservable factors that explain private school enrollment decisions.37 Currently, the best evidence on the benefit of private schools comes from randomized field trials.38 As an alternative, one could presumably track students over time that switch schools following school shootings. These students might provide a reliable treatment group for studies of private schooling.

[37] See Vandenberghe and Robin (2004) for a comprehensive review of the international evidence.

[38] For example, voucher experiments provide compelling evidence of the potential benefits of private school (see, for example, Angrist et al., (2002) and Wolf et al. (2011)).

Table 17 Weighted least squares regression

Panel A: Private high school enrollment

	(1)	(2)	(3)	(4)
x nonurban shooting				0.151
				(0.080)
x urban shooting				0.071
				(0.082)
Log of 14 to 18 yrs old pop		0.631	-0.195	-0.263
		(0.437)	(0.971)	(0.987)
Log of unemployment		0.123	0.082	0.102
		(0.131)	(0.183)	(0.184)
State specific trends	No	No	Yes	Yes

Panel B: Public high school enrollment

	(1)	(2)	(3)	(4)
x nonurban shooting				-0.010
				(0.026)
x urban shooting				0.001
				(0.023)
Log of 14 to 18 yrs old pop		0.707	0.533	0.543
		(0.201)	(0.334)	(0.334)
Log of unemployment		0.017	-0.045	-0.048
		(0.047)	(0.053)	(0.054)
State specific trends	No	No	Yes	Yes

Note: Each column from each panel is from a separate regression, weighted by the population of the state, with the log of state private school enrollment the dependent variable in the top panel and log of state public school enrollment the dependent variable in the bottom panel. State and year fixed effects are included in all regressions. The level of observation is the state-year. Each regression includes 612 observations (50 states and the District of Columbia from 1998-2009). The numbers in parenthesis are clustered (at the state level) standard errors.

Table 18 Robustness checks for enrollment effects in private schools

	(1) Nonurban	(2) Urban
Robustness checks		
Excluding shootings with no deaths	0.185	-0.047
	(0.084)	(0.110)
Proportion enrolled in private	0.011	0.005
high schools as dependent variable	(0.006)	(0.009)
Unweighted OLS	0.217	0.110
	(0.090)	(0.108)
Lagged effect of shootings		
Subsequent school year	0.154	0.038
Two school years later	-0.073	0.070
	(0.075)	(0.135)
Three or more school years later	-0.017	-0.209
	(0.134)	(0.194)

Note: Each row is from a separate regression, with shooting effects separated into those that occur in non-urban and urban areas, as in the last column of Table 17. State and year fixed effects, state-specific trends, and controls listed in Table 17 are included in all estimations in this table. The numbers in parenthesis are clustered (at the state level) standard errors. The number of observations in the first row, which removes observations where a non-fatal shooting occurred, is 605. Results in other rows include the full 612 observations. The results in the last part of the table (lagged effects) comes from a single regression in which we include first lag, second lag and third or more lag in the model.

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Fields of Interest

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Publications

- Abouk, R., Adams, S.J., "Texting Bans on Roadways: Do They Work? Or Do Drivers Just React to Announcements of Bans?", American Economic Journal: Applied Economics, 5(2), 2013, 179-199.
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Working Papers

- Abouk, R., "Homeschooling & Child Health" (Job Market Paper).
- Abouk, R., Bartholomew, K.S., "Evaluating the Effect of Local Smoking Restrictions on Birth Outcomes and Prenatal Smoking".
- Abouk, R., Goranova, M., Nystrom, P. & Soofi, E. "Activists at the Gate: Zero-Inflated Count Process of Shareholder Activism".

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- Abouk, R., "Do Health Houses Improve Birth Outcomes? Case Study of Health Houses in Iran".

Media Mention

- The Atlantic Cities, "Do Texting Bans Really Prevent Fatal Accidents?", March 27, 2013.
- NPR, Austin, Texas, March 2013.
- The Palm Beach Post, "Once-shunned Anti-texting Bill Gains Renewed Momentum in Florida Legislature", April 10, 2013.
- The Tampa Tribune, "Intextication Ban Won't Fix Anything", April 7, 2013.

- Green Bay Press Gazette, "Editorial: Consider Banning All Cellphone Use by Drivers", April 3, 2013.

Honors and Awards

- Finalist for Graduate Paper Competition, Illinois Economic Association Meeting, DePaul University, 2012.
- UWM Graduate School Travel Award, 2012.
- NBER Travel Award, 2011.
- Graduate Teaching Assistant Award, Department of Economics, University of Wisconsin Milwaukee, Spring 2007 through Spring 2011.

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Conferences and Workshops

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