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Abstract

This paper assesses the effectiveness of child safety seat laws. These laws progressively increased the mandatory age up to which children must be restrained in safety seats in cars. We use US Fatality Analysis Reporting System (FARS) data from 1978 to 2011 and rich state-time level variation in the implementation of these child safety seat laws for children of different ages. Increasing legal age thresholds is effective in increasing the actual age of child safety seat use. Across the child age distribution, restraint rates increase by about 30ppt in the long-run when the legal minimum age increases. However, we cannot reject the null hypothesis that restraining older children in safety seats does not reduce their likelihood to die in fatal accidents. We estimate that parents of 8.6M young children are "legal compliers." They compose an important target group for policymakers because these parents alter their parenting behavior when laws change.

JEL Classifications: I18, K32, R41

Keywords: Child safety seats, age requirements, fatalities, FARS

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1. INTRODUCTION

In the US, as well as in over 90 countries worldwide, traffic safety regulations require use of specific, approved child safety seats for children in automobiles (WHO 2013). Currently, all US states mandate the use of child safety seats. Forty-three states require the use of child safety seats up to at least age four; but—as Figure 1 shows—statutory age regulations have increased significantly over the last 30 years. In the 1980s and 1990s, children were required to be restrained in safety seats up to age two, or at most three. Since then, the average upper age requirement has almost doubled from 3 years in 2000 to almost 6 years in 2012. The costs of these laws are not negligible: First, prices of safety seats for children above age 2 vary greatly; roughly half of these seats cost more than $200 with the most expensive ones costing up to $500.[1] Second, fines for non-compliance vary between $20 in West Virginia up to $500 in Nevada for first time offenders; the mass point lies around $100 (IIHS 2013).

[Insert Figure 1 about here]

Despite their prevalence and increasing stringency, there exists surprisingly little empirical evidence on parents' compliance with child safety seat laws as well as their effectiveness under real world conditions. Whereas several papers list guidelines (Roberts and Turner 1984), document the heterogeneity in state laws (Bae et al. 2014), or discuss the effectiveness of potential interventions such as parent education (Zaza et al. 2001; Simpson et al. 2002), we were unable to identify empirical studies that evaluate the causal effects of more stringent child safety laws on parents' behavior, and their eventual impacts on child safety in cars. [2]

[1] Based on a search for "child safety seat" for "months 24 and up" on www.amazon.com as of September 30, 2015.

[2] The case for seatbelt usage of adults is different. Several empirical studies in and outside of economics empirically study the impact of mandatory seatbelt laws on usage, injuries and deaths (Loeb 1995; Hakes and Viscusi 2007; Carpenter and Stehr 2008). Other studies assess the effect of motorcycle helmet laws (French et al. 2009; Dee 2009) and bicycle helmet laws (DeJong 2010; Newbold 2012, Carpenter and Stehr 2011; Markowitz

The first, main objective of this paper is therefore to assess whether parents have changed their behavior in response to the evolving child safety regulations. We test for effectiveness of child safety laws by exploiting changes in state laws, which have progressively increased the age up to which children are required to ride in child safety seats.

Whether the laws are effective in changing behavior, and who responds, depends on how parents evaluate the costs and benefits of child safety seat use. In a classical economic framework with no missing information, rational and forward-looking parents would form beliefs about the expected benefits and costs of using safety seats (both for parents and children). The benefits of use include actual safety benefits, as well as utility derived from feeling safe. The costs of use would include pecuniary costs of buying safety seats, or non-pecuniary costs like discomfort for the children, inconvenience, or discomfort for the parent (e.g. because of potential parent-child conflict of restraining children). If benefits outweigh costs, parents would choose to restrain their children in safety seats even in the absence of any formal law. When laws requiring use are implemented or become more stringent, standard economic theory would predict an increase in safety seat use among marginal parents of children of that age. This is because the laws increase the costs of not using safety seats by introducing fines for non-compliant parents. Under the perfect information assumption, any behavior changes resulting from the laws should be generated exclusively through their impact on the costs of not complying (fines, eventual legal measures). However, if parents do not have perfect information about the safety benefits of the seats, then the laws could also impact use

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and Chatterji 2015). Moreover, several papers assess the impact of drunk driving (laws) (Levitt and Porter 2001b; Dills 2009, Lovenheim and Slemrod 2010, Lovenheim and Steefel 2010, Cotti and Walker 2010), medical marijuana laws (Anderson et al 2013), or driver licensing (Dee et al. 2005) on traffic fatalities. Research on the SUV "arms race" on American roads finds that vehicle crashes involving both passenger cars and SUVs have an increased fatality probability (White, 2004; Daly et al., 2006, Anderson 2008; Li, 2012; Anderson and Auffhammer 2014).

by signaling the effectiveness of child safety seats. In this case, the introduction of a new law could change parents' behavior by altering their perceived safety benefit of using a safety seat.

Covering the years 1975 to 2011, this paper uses US Fatality Analysis Reporting System (FARS) data on the universe of children aged 0 to 7 who are involved in accidents where at least one person died. Using a difference-in-difference-in-difference (DDD) approach, we are able to identify the effects of the laws by comparing child safety seat use in state-years where children of a given age were covered by the law versus not—net of child age, year and state effects. The models also consider time-invariant, state-specific differences in child safety seat use among children of similar ages, and state-specific time trends in use. Because only children involved in fatal accidents are included in our sample, we make use of the Porter-Levitt (2001a, b) sample selection correction method.

Our findings show the following: First, increasing legal age thresholds is effective in increasing the actual age of child safety seat use. Our preferred specification shows that, among children subject to a law, the share in a safety seat roughly doubles in the short- to medium-run from a baseline level of 17 percent. Second, in the long-run, the rates of safety seat use increase by almost 30 percentage points (ppt) among covered children. This effect translates into an additional 8.6 million children under the age of 8 who ride in safety seats as a result of the more stringent laws. Parents of these children are an important target population for policymakers because they respond to legislative changes by altering their parenting behavior (cf. Case and Paxson 2002; Kuziemko 2014).

Third, our findings suggest that child safety seat laws are effective across the entire child age distribution. That is, safety seat use increased by about 30ppt when laws mandated parents to restrain their infants under the age of 1 in safety seats in the 1980s. Likewise, the more recent laws that apply to children between ages 5 and 6 have also increased use of child safety seats by about 30ppt. However, because the pre-reform baseline restraint levels differed greatly between younger and older children, the law effect size in percent has increased substantially over time.

Fourth, this paper assesses whether child safety seat laws are effective in reducing children's fatality risk when involved in a fatal accident. Based on data from the 1980s, early reports by the National Highway Traffic Safety Administration (NHTSA) find clear evidence that, when used correctly, child safety seats reduce fatality risk by more than 50 percent (Kahane 1986; Partyka 1988; Hertz 1996). A meta-analysis of 72 studies published as of 1998 confirmed the reported effectiveness of safety seats (Zaza 2001). More recent empirical studies similarly confirm these early findings, although the magnitude of the fatality risk protection effect seems to have decreased over time (Evans and Graham 1990; Cohen and Einav 2003; Starnes 2005; Sen and Mizzen 2007, Rice et al. 2009; Jones and Ziebarth 2016). In general, there is a societal and scientific consensus that restraining children is highly effective in saving lives.

However, these studies all compare children in safety seats to *unrestrained* children. Among studies where the relevant comparison group is children restrained in traditional *seatbelts*, results are inconclusive: On the one hand, medical and public health studies do find significant reductions in severe injuries and child fatalities as a result of children being restrained in safety seats (Durbin et al. 2003; Arbogast et al. 2004; Elloitt et al. 2006). On the other hand, economic papers fail to find such statistically significant risk protection effects—at least for children aged two to six (Levitt 2008; Levitt et al. 2010; Jones and Ziebarth 2016). We add to the evidence on the effectiveness of child safety seats by exploiting the exogenous variation generated by changes to safety seat laws. In line with the economics literature, we are unable to reject the null hypothesis of no additional safety protection for our preferred specification. When analyzing the shifts in restraint use as a result of changes in laws, we provide convincing evidence that marginally complying parents move from traditional seatbelt use to child safety seat use—rather than from leaving their children unrestrained to restraining them in safety seats.

Finally, we characterize parents by identifying predictors of both non-compliance (not using a child safety seat when one is required) and over-compliance (using a child safety seat when one is not required). We find evidence that indictors of risk preferences, like riding a van or SUV, are important predictors of compliance behaviors. Furthermore, we uncover two pieces of evidence to suggest that the laws do not operate through the direct costs of not restraining. First, our one quasi-measure of wealth—possession of a new car—is negatively related to non-compliance, suggesting that wealthier parents for whom fines are relatively less costly are *more* likely to comply. And second, the legal fine incurred for non-compliance is *not* a statistically significant predictor of non-compliance. These pieces of evidence suggest that child safety legislation may change behavior by signaling to parents the effectiveness of safety best practice.

The next section describes the data used in this study. Section 3 summarizes the empirical approach and Section 4 discusses the findings. The final section concludes.

2. DATA

2.1 FARS Data on the Universe of Fatal Accidents in the US from 1975-2011

The main data used in this study are publicly available US FARS data from 1975 to 2011. The data include the universe of all car accidents in which at least one person died in the US. Moreover, they include information on the type of restraint used by each vehicle occupant involved in a fatal crash. Following similar studies in the literature (Levitt 2008; Levitt et al.

2010; Jones and Ziebarth, 2016), we restrict the data as follows: We disregard crashes in which the only fatality were pedestrians, motorcyclists, or occupants of nonstandard vehicles. We also limit the analysis to occupants of automobiles, minivans and SUVs with model years newer than 1969. We discard observations with missing values on relevant variables and cases in which the occupant did not sit in the first three rows of the vehicle. Finally, we restrict the sample to children aged seven and younger. This means that each observation in the dataset refers to one child under the age of eight who was involved in an accident where at least one person died on US roads between 1975 and 2011.

After the sample restrictions, our total sample has 77,837 child-car accident observations. The first two columns of Table 1 list the characteristics of these children—along with characteristics of their vehicles, drivers and crashes. As seen, the child mean age is 3.4 but the standard deviation is a substantial 2.2. About half of the children are male. Thirty-four percent of all children involved in fatal car accidents were restrained in child safety seats, whereas 24 percent were restrained by an ordinary seatbelt; the remaining 43 percent were unrestrained. About 22 percent of all children involved in a fatal accident eventually died.

[Insert Table 1 about here]

Data on the type of accident reveal that the majority were two vehicle crashes and almost half of all crashes (44 percent) were front-impact crashes. About two thirds of all crashes occurred during the week and one third on weekends. Only 10 percent of all accidents happened on rural roads.

There is significant heterogeneity in the type of car involved in accidents. On average, over the entire sample time frame, 69 percent were passenger cars. However, this share has decreased from about 70 percent at the beginning of the 1990s to just over 40 percent in more recent years (Figure 1b in Jones and Ziebarth, 2016). Over the same time period, the share of

SUVs has tripled from about 10 to 30 percent. The mean vehicle year in our dataset is 1986 and the mean vehicle weight 2.520lbs. About 37 percent of vehicles are five years old or newer.

In terms of driver characteristics, the first column of Table 1 shows that the average driver was 32 years old. Fifty-six percent of drivers were female, 29 percent had a previous low-level traffic violation (i.e., speeding tickets) and 10 percent had a high-level traffic violation (i.e., driving while impaired).

2.2 Data on US Child Safety Seat Laws from 1978-2011

The law data on child safety seat restraint ages comes from the Insurance Institute for Highway Safety (IIHS) Child Safety Laws (2016) documents. We create a dataset from the IIHS documents that lists, for each state and every year, the age up to which children have to be restrained in safety seats. We also use the IIHS documents to collect information on the legal fines for not complying with the child safety seat laws for every state and year. Whenever the IIHS documentation was lacking information, we conducted additional research for the according states and years.

[Insert Figure 2 about here]

Figure 2 illustrates the evolution of child safety seat laws over time. Each of the lines plotted indicates for every year the share of states that required children of a given age to be restrained in child safety seats. The first state law was implemented in Tennessee, effective January 1978. Subsequently, Rhode Island and West Virginia implemented laws that required children up to age 3 to be restrained in safety seats (in 1980 and 1981). Shortly after, by 1984, almost all US states had laws requiring infants below 12 months to be restrained, and a majority of states had set the age threshold to age one or two. Whereas the share of states setting the thresholds at ages 1, 2, or 3 increased sharply to almost 100 percent between 1985 and 2005, it was only in the new millennium that a significant number of US states passed laws forcing older children, up to age 7, to be restrained in safety seats. As Figure 2 shows, the share of states with laws mandating that children up to age 6 to be put in safety seats increased from below 10 percent to above 40 percent in just a decade. In 2011, 25 states had laws that even required children up to age 7 to be restrained in safety seats.

It should be noted that some state laws are relatively complex (cf. Bae et al. 2014) which makes it unlikely that parents are actually perfectly informed about the laws. The complexity of some laws may be an explanation for why compliance is not perfect, as we will discuss in the Results Section. For example, the wording of some laws makes the age threshold conditional (or substitutive) on children's height and/or weight. The state of Louisiana specifies that "a child younger than six years of age or weighing sixty pounds or less shall be restrained in a child restraint system." In contrast, the "age 0" line in Figure 2 does not reach 100 percent because Maine and Kentucky have no formal age thresholds at all, but require that children who weigh less than 40 pounds or are shorter than 40 inches have to be restrained in child safety seats. In our empirical analysis, we abstain from differentiating between laws with and without height-weight conditions for two main reasons: First, whereas age thresholds are relatively salient for both parents and the police, it is unclear if this also applies to height and weight thresholds. Because height and weight changes rapidly when children are young, it is very plausible that parents mainly follow the age thresholds. The same applies for law enforcement units who are unlikely to take measures in order to fine parents. Second, our data does not include height or weight measures for children, only their age and whether they were restrained in safety seats or not. To the extent that ignoring the height and weight restrictions leads to (classical) mismeasurement of the treatment variable, it attenuates our estimates such that they represent lower bounds (Lewbel 2007).

A further complication of some laws is the possibility to use booster seats which have become particularly popular for older children in more recent years. In our analysis, we do not differentiate between traditional child safety seats and booster seats in our interpretation of the laws or the response in compliance rates.[3]

3. EMPIRICAL APPROACH AND IDENTIFICATION

3.1 Empirical Model

Our empirical approach exploits rich variation in child safety seat laws across the 51 US states, over 37 years. We specify the following difference-in-differences-in-differences (DDD) model, where the third difference stems from the fact that, in a given state, the laws change differentially for children of different ages. Hence, our comparison group is composed not only of children in states without laws in a given year, but also of older children in the same state who are not required to use safety seats in the given year. We estimate the following model:

$$y_{iast} = \alpha + \delta law_s * Age_a * Effective_t + \boldsymbol{X_{iast}}'\boldsymbol{\beta}$$

$$+\gamma_a + \theta_t + \lambda_s + \theta_t * \lambda_s + \gamma_a * \lambda_s + \varepsilon_{ist} \qquad (1)$$

where y_{iast} equals 1 if a child *i* of age *a* in state *s* and year *t* is restrained in a child safety seat, and 0 otherwise. Note that in our main specification, both unrestrained children and children restrained with traditional seatbelts are coded as zero. In refined model specifications, we further differentiate between the types of alternative restraints (cf. Table 4).

The triple dichotomous interaction term $law_s * Age_a * Effective_t$ is the main variable of interest. It equals one if in year *t*, state *s* has a safety seat law for children of age *a*. Hence, this

[3] Booster seat use has only been recorded in the FARS as a separate restraint category since 2008.

main binary treatment indicator varies at the state *(s)*, year *(t)*, and child age level *(a)*. The coefficient estimate δ represents the DDD treatment effect of changes in age requirements in US states over time. It captures the change in the probability that a child of a specific age is restrained in a child safety seat when law requirements for that age group change. In other words, δ yields the difference in the probability that a child of age *a* being restrained in a child safety seat when there exists a law mandating it vs. not.

To accurately estimate the DDD coefficient, we include the single elements of the triple interaction term as controls. They net out potential confounding factors. The set of seven child age fixed effects, γ_a, controls for time- and state-invariant age differences in safety seat use. Common year fixed effects, θ_t, are added for the entire time period from 1975 to 2011. These year fixed effects control for general increases in child safety seat use for all child age groups across all states, e.g., through social norms or nationwide recommendations by the NHTSA (e.g. NHTSA, 2010). A set of state fixed effects, λ_s, nets out persistent differences in child safety seat use in the heterogeneous US states. For example, it could be that safety seat use has always been lower in Texas as compared to New York State.

In addition, the model in equation (1) includes a full set of state-year fixed effects, $\theta_t * \lambda_s$, which control for state-*specific* time shocks in safety seat use that apply to children of all ages. It is imaginable be that state-specific events or policies—which are either unrelated or part of the law requiring older children to be restrained—increase the general safety seat use in a state. For example, if a state law increases the mandatory safety seat age from 3 to 4 years but also includes other provisions that increase safety seat use among *all* children aged 0 to 7, our model estimates the effect of the law on 4-year olds, net of the general increase. Including this full set of state-year fixed is a more thorough variant of state-specific time trends, λ_s*t, which we substitute for $\theta_t * \lambda_s$ in robustness checks.

Our models also include age-state fixed effects, $\gamma_a * \lambda_s$, which account for state-specific factors that persist over time and lead to different restraint use rates among children of difference ages. For example, if safety seat use among 4 year olds is traditionally very high in New York but very low in Texas and a law increases the requirements for 3 year olds in Texas, then ignoring $\gamma_a * \lambda_s$ can make a crucial difference in the evaluation of the law in DDD models. In practice, including $\gamma_a * \lambda_s$ makes a difference in our estimates. The magnitude of the triple interaction estimate δ increases once we control for $\gamma_a * \lambda_s$—but neither the sign nor the significance of the estimated coefficient change.

We estimate models both including and excluding age-year effects, $\gamma_a * \theta_t$. Because the expansion of the child safety seat laws occurred in fits and starts, with some age requirements increasing quickly in many states around the same time, including time-age effects may generate a conservative specification underestimating the true compliance rate among parents. Figure 2 illustrates that almost all states introduced child safety seat laws for infants between 1982 and 1984. Age-year fixed effects capture much of the variation in restraint use for these early changes. The remaining estimate then only reflects the differential (higher) safety seat use rates in states that adopted the laws very shortly before other states did. As we will discuss in the Results Section, controlling for $\gamma_a * \theta_t$ produces an estimate that remains statistically significant, but that decreases by a factor of between 3 and 4. Our preferred specification excludes these controls.

As an additional test of whether we are justified in excluding the age-by-year controls, we estimate models by child age. These reduce our DDD model to a series of difference-in-difference (DD) estimates. For each child age group, we test separately whether safety seat use is greater in state-years where it is required. These models allow us to estimate the impact of the laws net of age-year controls with an alternative strategy that does not directly compare effects by child age.

The vector $\boldsymbol{X_{ist}}$ includes a comprehensive set of control variables as listed in Table 1. These include the gender of the child, the position of the child in the car[4], car- and crash-level characteristics, and a set of driver characteristics such as the age, gender, and previous traffic violations of the driver. We estimate (1) as a linear probability model but the results are robust to estimating probit models and calculating marginal effects (available upon request). Standard errors are routinely clustered at the state level where the laws vary over time (Bertrand et al. 2004, Cameron and Miller 2015).

We additionally seek to identify whether the increasingly stringent age requirements for child safety seats have resulted in a reduced fatality likelihood for children involved in fatal crashes. To answer this question, we estimate (1) but y_{iast} now represents an indicator of whether the child *i* of age *a* in state *s* and year *t* died in the fatal accident or not (i.e. *Child Died in Crash* in Table 1). Then, δ yields changes in the child death probability after laws have become more stringent. To examine further whether the laws are likely to have been effective in preventing deaths, we also estimate (1) on the outcomes that identify alternative restraint options. Specifically, we construct indicators that equal one if a child was restrained in a traditional seatbelt, and if a child was unrestrained at the time of the crash. The results of this exercise give us information of how the laws impacted use of alternative restraint types, and buttress our results on the effectiveness of the laws in preventing death.

[4] It has also been found that the positioning of child safety seats matters. For toddlers up to age two, rear-facing child safety seats seem to significantly reduce serious injuries and fatalities when compared forward-facing seats (Hertz, 1996; Lund, 2005; Henary et al. 2007; O'Neil et al. 2014).

3.2 Investigating Compliance

Another important facet in the discussion of whether child safety seat laws are effective is what type of parent is the marginal complier. To characterize parents, we estimate a variant of (1) on two outcomes: *over-compliance* and *non-compliance*. Over-compliant drivers are identified as those who use child safety seats in law environments where they are not required to do so. Non-compliant drivers are those who do not use a safety seat when, by law, they should. Estimating model (1) identifies which child and driver characteristics predict compliance. The non-compliance analysis additionally investigates whether the fine for breaking the law (which varies at the state-year level) deters law-breaking behavior. We do this by including the natural log of the fine amount in real 2014 dollars as a regressor. Note that the compliance analysis restricts the sample to (a) children who are not required to be in a child safety seat (*over-compliance*) and, (b) children who are required to be in a child safety seat (*non-compliance*).

3.3 Correcting for Sample Selection

One potential issue with our empirical approach is that that our data only represent a subset of children traveling in vehicles—namely, those involved in fatal traffic accidents and included in the FARS dataset. It is likely that the characteristics of these children (and their drivers) differ significantly from those of the general population.

To account for this source of selection bias, we use the Porter and Levitt (2001a, b) sample selection correction technique. It eliminates the sample selection bias by focusing only on children who are included in the sample because someone in the *other* car was fatally wounded in the accident. The assumption here is that the restraint choices of the person in the first car are uncorrelated with the driving behavior of the person in the second car. As such, we approximate the results for the full population and also test the sensitivity of the findings with respect to this sample selection correction. Whereas the full sample includes 77,837 child-accident observations, the sample size in the selection-corrected sample decreases to 22,018 child-accident observations. We estimate our main models using both the full and selection-corrected samples, but focus all our extensions on the selection-corrected sample. However, results for the full sample are generally consistent with those reported herein (available upon request).

Comparing columns (3) and (4) with columns (1) and (2) in Table 1 yields evidence on the selection into the FARS dataset. At first sight, many of the mean characteristics look surprisingly similar. However, the selection corrected sample indicates a larger share of children who were restrained in safety seats (41 percent vs. 33 percent) or restrained in traditional seatbelts (32 percent vs. 24 percent). This documents that the full FARS sample clearly represents a negatively selected sample as compared to the full population, at least when it comes to driver safety concerns. Children in the full sample are also more likely to have been seated in the front (29 percent vs. 26 percent) and are significantly more likely to have died in the crash (22 percent vs. 4 percent). The cars are older and more likely to be passenger cars.

4. RESULTS

4.1 Child Safety Seat Laws, Use and Effectiveness

Baselines Regression Results

Table 2, Panel A shows results that address the first research question: namely, how large is the share of marginal parents who change their behavior when laws affecting their children change? Each column represents one regression model as in equation (1). The models differ by the sets of covariates included. Columns (5) and (6) show results for models estimated with the full sample of children, while the first four columns show results for the selection-corrected sample—our preferred sample.

[Insert Table 2 about here]

Column (1) shows the most parsimonious specification of the DDD model which includes just year, child age, and state fixed effects and is estimated using the selection-corrected sample; the equivalent result using the full sample is reported in column (5). First, we note that both estimates are very similar and not statistically different from one another. Second, the highly significant point estimate in column (1) indicates that child safety seat use increases by 18 percentage points (ppt) when laws require restraint of children in safety seats. The introduction and expansion of safety seat laws more than doubles the share of children who are restrained in safety seats when compared to the pre-reform baseline restraint rate of 17 percent.

Column (2) reports results from a model that includes all first order interactions of child age, state and year covariates. As seen, including all two-way interactions cuts the size of the coefficient in half to 8.7ppt. However, this estimate still represents an increase in safety seat use of 50 percent relative to the baseline. Because the expansions of many state laws occurred in close succession for children of similar ages—as discussed in Section 3—column (2) likely underestimates the true compliance effect. As such, we interpret the 8.7ppt increase in column (2) as a lower bound estimate.

Next we estimate models that exclude the set of 7*37 age-year ($\gamma_a * \theta_t$) fixed effects (columns (3), (4) and (6)). Column (3) shows an estimated law effect of 31ppt while column (4) reveals an almost identical estimate of 29ppt. Column (4) is our preferred specification: we report an estimate using this specification for the full sample in column (6).

Overall, we learn the following from Table 2: First, correcting the sample with respect to covariates barely alters the coefficient estimate (column (3) vs. column (4)). Obviously, observable features of drivers and accidents are not systematically and simultaneously correlated with changes in laws and compliance. Second, comparing column (4) to column (6), one finds that the coefficient estimates are very robust to the sample selection correction: the confidence intervals largely overlap. Third, the preferred result in column (4) lets us conclude that when laws mandate older children to be restrained in safety seats, the share of children who ride in safety seats increases by 29ppt from a baseline level of 17 percent, or by 173 percent. Note that this estimate can be interpreted as the long-run effect because the model identifies average post-reform increases in restraint rates for *all* laws—those that were passed 30 years ago and applied to infants as well as those that were passed recently and apply to older children.

According to the American Community Survey (ACS), in 2014, there were 29.8 million children under the age of eight living in US households who owned at least one car (United States Department of Commerce, 2015). To the extent that compliance behavior has remained stable over time, the marginal compliance rate of 29ppt would translate into 8.6 million marginal young children whose parents altered their parenting behavior when laws changed.

Testing for Endogenous Law Changes: Event Study Graph

Our identification strategy only produces unbiased estimates if legislated changes in the required restraint age are uncorrelated with other state-specific confounding factors that may affect restraint use. Our preferred specification in column (4) of Table 2 routinely includes state-year ($\theta_t * \lambda_s$) fixed effects, in addition to age-state ($\gamma_a * \lambda_s$) fixed effects, which should eliminate many potential concerns.

Figure 3 plots an event study graph. Event study graphs are common tools to evaluate the validity of the unconfoundedness assumption in settings where many treatments are implemented at different points in time. We estimate the following variant of the main model in equation (1):

$$y_{iast} = \alpha + \delta \sum_{e=-6}^{6} Law_{ase} + \boldsymbol{X_{ist}}'\boldsymbol{\beta} + \gamma_a + \theta_t + \lambda_s + \theta_t * \lambda_s + \gamma_a * \lambda_s + \varepsilon_{ist} \quad (2)$$

We define event time equal to 0 in the first year when children of age a are required to be restrained in child safety seats in state *s*. Years before the law change are defined as event times -6, -5,…,-1 for children of age *a* in state *s*, whereas years subsequent to the law change are defined as 1, 2,…, 6. In model (2), Law_{ase} represents a series of indicator variables, one for each event time period in the twelve years surrounding each law change. Law_{ase} is coded 1 for children of age a, from state *s*, with event time *e*, and 0 otherwise. We treat event time equal to 0 as the excluded time period, and estimate (2) after controlling for the same fixed effects and covariates as our preferred speciation.

The plotted Law_{ase} coefficients construct the event study graph. They show average restraint use in each year before and after law changes for affected children, after controlling for year, state, child age, year-state and state-age effects in restraint use. The shape of the curve allows us to assess the plausibility of the assumption that laws were not passed in reaction to rising or falling trends in the outcome variable. The graph also helps to assess whether systematically correlated unobservable factors could drive trends in the outcome variable that are also correlated with the timing of the laws. Figure 3 shows the event study graph for the selection corrected sample. The reference point is the year when the law was passed, *Event Time '0'* on the x-axis.

[Insert Figure 3 about here]

We make the following observations: (a) In the six years priors to a law change, restraint use was significantly lower as compared to post-reform years. (b) There appears to be a slight upward trend in restraint use in the three pre-reform years. However, the trend is relatively moderate as compared to the overall post-reform increase. A slight pre-reform upward trend or anticipation effects would attenuate our compliance estimates. (c) We observe a discrete and clear increase of about 10ppt. in the probability that children are restrained in safety seats in the first post-reform year. (c) The effect of the law continues to grow in subsequent years. One observes an additional 10ppt. increase in the probability of safety seat use during the next four post-reform years. (d) The magnitude of the plotted coefficients and the shape of the curves are very similar for both the full sample and the selection corrected sample (graph for full sample available upon request).

Note that the increase in compliance plotted in Figure 3 is solely based on six pre and six post-reform years. It can thus be interpreted as the short- to medium-run effect of the laws, whereas the estimated effect in column (4) of Table 2 (Panel A) represents the long-run effect. In sum, Figure 3 provides little evidence of endogenous law changes, but clear evidence for a causal effect of the laws on child safety seat use.

Effects of the Laws on Child Fatality

Panel B of Table 2 assesses the impact of stricter child safety seat regulations on the likelihood that a child dies in a fatal road accident. Fortunately, this is a relatively rare event. For example, between 2001 and 2011, only 5,779 children below the age of eight died on American roads, an average of 525 per year. Relative to the total of an estimated 29.8 million children under the age of eight in families with cars, this equals a fatality rate of just 0.001 percent.

The six regression results reported in Panel B show the estimated impact of the laws on the outcome variable *child died in crash* (Table 1), and are otherwise identically specified as the models in Panel A. As seen, except for one marginally significant coefficient in column (5), all other five coefficient estimates are small in size and not statistically significant. However, the effects are relatively imprecisely estimated: relative to the mean fatality rate of just 4 percent in the selection corrected sample, the point estimate in column (4) only let us exclude with 95% statistical probability an effect size of more than a negative 1.5ppt, or 39 percent of the mean. Extrapolating to the entire US, we may only say with relative certainty that the more stringent child safety seat requirements did not save more than 197 children's lives per year. The precisely estimated point estimate in column (5), by contrast, indicates that more stringent safety seat laws saved 26 young lives per year. Twenty-six saved lives would translate into an economically monetized benefit of $260M per year, whereas the upper bound estimate from our preferred specification in column (4) translates into a value of$1,970M—assuming a value of a statistical life of $10M per child under the age of eight (Viscusi and Aldy 2003).

Note that this estimate includes unintended effects which are potentially triggered when passing safety laws and which operate against the law's intention to save children's lives. Unintended effects include the Peltzman effect, which predicts that parents become riskier drivers because they feel safer when their children are restrained (Peltzman 1975). Unintended consequences may also arise from the fact that many parents do not restrain their children correctly in child safety seats, which can increase the likelihood that a child dies in an accident (Howland et al., 1965; Bull et al., 1988; Decina et al. 1997; Children's Safety Network, 2005; Duchossois et al. 2008; Snowdon et al. 2008; New York Times, 2013; Mirman et al. 2014).

4.2 Mechanisms and Heterogeneity in Effects

Effects by Child Age

Next, we take a closer look at the operating mechanisms of the laws and investigate heterogeneity in their effects. First, we differentiate by child age. Parental compliance with child safety laws may differ by child age because of heterogeneity in the assessed potential benefits of the seats. The costs of restraining the child may also differ by child age. In addition, as Figure 2 shows, laws requiring children up to age three to be restrained in safety seats were primarily passed in the 1980s, whereas laws affecting older children were primarily passed in the new millennium.

[Insert Table 3 about here]

We repeat our baseline analyses separately for children of ages 0 to 7. Because we run the model in equation (1) for each age group separately, the DDD model reduces to a simple DD model where the laws vary across states and over time for each age group. Each column of Table 3 show a result from one of the eight separate DD models by child age. All results are produced using the selection corrected sample.

Panel A reports results from standard DD models which control for a full set of state and year fixed effects. As seen, the coefficient estimates vary in size from 2.5ppt (for age 0) to 18.5ppt (for age 6). Further, the average pre-reform restraint use rate ("over-compliance") varies highly by child age. We find significant point estimates for laws affecting children aged 1 (7.5ppt, 16 percent), aged 3 (9.0ppt, 32 percent), aged 5 (12.0ppt, 100 percent), aged 6 (18.5ppt, 300 percent) and aged 7 (11.7ppt, 400 percent). The estimated effect for children aged below 1, aged 2 or 4 are smaller, and not significantly different than 0.

Two important factors may contribute to differences in law effectiveness rates expressed in percent by child age. First, the share of children in safety seats in pre-reform years differs significantly by age. Whereas among one year olds, nearly 50 percent were restrained before they were required to be by law, among seven year olds, only 3 percent rode in safety seats before the law required them to. The differences in the baseline over-compliance rates are a crucial determinant of the calculated differences in law effectiveness rates in percent. It is reasonable to assume, and supported by the pre-reform over-compliance rates, that parents generally see a higher necessity to restrain infants as compared to seven year old children in the *absence* of a law. Another interpretation could be that increasingly stringent laws have crowded out parents' personal motivation or ability to make decisions that deviate from legal norms (Frey 2002; Fehr and Schmidt 2007).

Second, because laws affecting children of different ages were implemented in different decades, it is econometrically difficult to disentangle the effectiveness of laws from the changes due to evolving social norms. For example, it may be less socially acceptable today to not restraint a child if legally required as compared to the 1980s. It could also be that parents have become more or less law-abiding.

Because essentially all laws requiring children aged 0 to 2 to be restrained in safety seats were passed at the same time within 3 years of each other, econometric models with year fixed effects absorb much of the variation in child safety seat use in this legislative era. In the extreme, if all states had implemented laws for one-year olds in the same year, then the year dummy would net out the common increase in safety seat use even if the law was highly effective. Hence, we estimate our models after replacing all year fixed effects with state-specific linear time trends, and show results in Panel B of Table 3.

The exclusion of the year fixed effects increases the magnitude of the estimated effects for all age groups. As expected, this holds particularly for younger age groups, for whom law enactment dates are much more closely clustered than for older children. In absolute terms, and in line with intuition, the marginal compliance rates are very high for infants below the age of one (39ppt) and one year olds (26ppt). Maybe surprisingly, very high marginal compliance rates are also found for children aged five (30ppt) and six (29ppt). Laws impacting children aged 2 and 3, however, do not appear to have induced as many parents to take up child seat use.

Shift in Alternative Restraint Use

The policy implications of the law changes—as well as any effect they may have had on children's fatality rates—are highly dependent on the alternative restraint choices of parents. If the law was effective primarily for parents who otherwise would have driven with unrestrained children, then we should expect the law changes to be effective in reducing fatalities. However, if the increased age requirements primarily shifted parents from use of traditional seatbelts to child safety seats, the results may be more nuanced. Previous research has suggested that child safety seats are no better than seatbelts at preventing fatalities (Levitt 2008; Levitt et al. 2010; Jones and Ziebarth 2016).

[Insert Tables 4 about here]

Table 4 investigates how the law affected the use of traditional seatbelts and the probability of children being unrestrained. We estimate model (1) on two new outcomes variables: *traditional seat belt* use (column (2)), and *no restraint* use (column (3)). The estimate in column (1) is the estimate from our preferred model in column (4) of Table 2 [Panel A] using state, year and age fixed effects in addition to state-year and state-age fixed effects as well as socio-demographic controls.

The empirical evidence is very clear. While there is a small decrease in the probability that a child involved in a crash was unrestrained after a child safety seat law was passed, there is a substantial decrease in the proportion of children who were restrained in traditional seatbelts. This is intuitively plausible: Marginally complying parents were those who previously used traditional seat belts—and not (irresponsible) parents not restraining their children at all—before it became mandatory to restrain their children in safety seats. Table 4 clearly shows that traditional seat belt use decreased by almost exactly the same amount than child safety seat use increased post-reform. This finding adds further credibility to our identification strategy.

4.3 Who are the Non-Complying and Over-Complying Parents?

In light of our results indicating that the laws were highly effective in promoting use of child safety seats, two important questions arise: First, which types of parents used child safety seats *before* they were required to do so? And second, which types of parents chose to ignore the laws *after* they were enacted? Table 5 reports results of analyses intended to answer these questions. Column (1) shows predictors of over-compliance—the decision to use a child safety seat when not required to do so. Column (2) shows predictors of non-compliance. We estimate the model for over-compliance after limiting the sample to children who are not covered by a law; the analysis of non-compliance is conducted only among children who by law, ought to have been in a child safety seat. Models include controls for year and state, along with the observable demographics reported in the table.

[Insert Table 5 about here]

The results in column (1) show that male drivers, younger drivers and older drivers, as well as drivers with previous serious traffic violations are all less likely to use a safety seats when not required to do so. We additionally see that drivers who are in a passenger car—as opposed to SUVs, vans or trucks—are less likely to over-comply, whereas drivers with newer cars are more likely to over-comply. Child age also appears to be an important predictor of over-compliance, with older children being less likely to be restrained in a child safety seat when not required to be. The analysis suggests that more safety-conscious drivers—those without any major violations and those vans or SUVs—were those who used safety seats even in the absence of a law.

Column (2) shows the results for non-compliant behavior. Along with the driver, child, car and crash-specific characteristics, we additionally test for whether the legal fine for non-compliance deters non-compliant behavior. The fine for non-compliance varies significantly by state and year. Specifically, for recent years, they vary between $20 in West Virginia up to $500 in Nevada for first time offenders (IIHS 2013).

Again, the results indicate that male drivers, younger and older drivers, and drivers with previous major violations are more likely to be non-compliant with the law. Our one quasi-measure of wealth also appears related to compliance behavior: drivers of newer cars are more likely to comply. Older children appear more likely to be unrestrained or restrained with a traditional seat belt when, by law, they should have been in a child safety seat.

An interesting finding is that the fines for non-compliance do not appear to be an effective deterrent. In term of the cost vs. signaling hypothesis outlined above, this suggests that parents may be insensitive to the direct legal costs of restraint, and instead may view the law as an information signal that child safety seats are effective.

5. CONCLUSION

This paper had two main objectives. First, using real-world data on all fatal accidents in the US from 1975 to 2011, we test whether stricter state requirements are effective in increasing the actual age of children in child safety seats. We additionally investigate whether the laws were effective in reducing children's fatality risk when involved in a fatal accident. Second, we investigate what factors are important determinants of compliance behavior.

With respect to the first paper objective: We find that tighter state-level requirements governing the use of child safety seats are highly effective in increasing the actual age of children in child safety seats in the US. When states pass laws making it mandatory that older children are restrained in safety seats, the likelihood for those children to be restrained increases by about 25ppt in the short- to medium-run, and by almost 30ppt in the long-run. This estimate is of importance in many policy-relevant contexts because it identifies the share of marginal children whose guardians change their behavior when policymakers pass new laws. The parents of an estimated 8.6 million American children responded to these legislative changes intend to improve children's welfare.

With 95% statistical certainty, we can exclude the possibility that passing stricter legal age requirements for child safety seats saved more than two hundred young lives per year. One significant point estimate suggest that more stringent laws only saved 26 lives per year, albeit among a highly selective sample of children involved in fatal accidents. When applying common values to assess the value of statistical lives, for example $10M, the monetized benefits in terms of lives saved falls between $260M and $1.97B per year. At a median price of $200 per safety seat, the cost of buying one for each American child under 8 living in a household with a car is $5.96B. However, in assessing the safety impact of the laws, it is important to consider that our results do not bear on other potential safety impacts of child safety seat laws, such as a reduced injury rate.

From an economic standpoint, parents who believe in the effectiveness of safety seats (and for whom expected benefits outweigh costs) will restrain their children even in the absence of laws and "over-comply." We find that this share of voluntary safety seat users has decreased from 48 percent among children of age one in the 1980s, to 12 percent among children of age five and a mere 3 percent among children of age seven. Despite this decrease in pre-reform over-compliance behavior, the effectiveness of the laws in changing behaviors has remained relatively constant over time. Across the entire child age distribution up to age 8, we find that safety seat use increases by up to 30ppt when laws become more stringent. Furthermore, we provide evidence suggesting that the cost of breaking the law—i.e., how high the fine is—is not a significant deterrent for non-compliance. Rather, compliance is related to socio-demographics, such as driver age and gender, tastes for risk, such as previous traffic violations and vehicle type, and our quasi-measure of wealth—having a newer car. In sum, these results suggest that in a child safety context, compliant behavior is generated through the signaling effect of the law, rather than through its punitive force.

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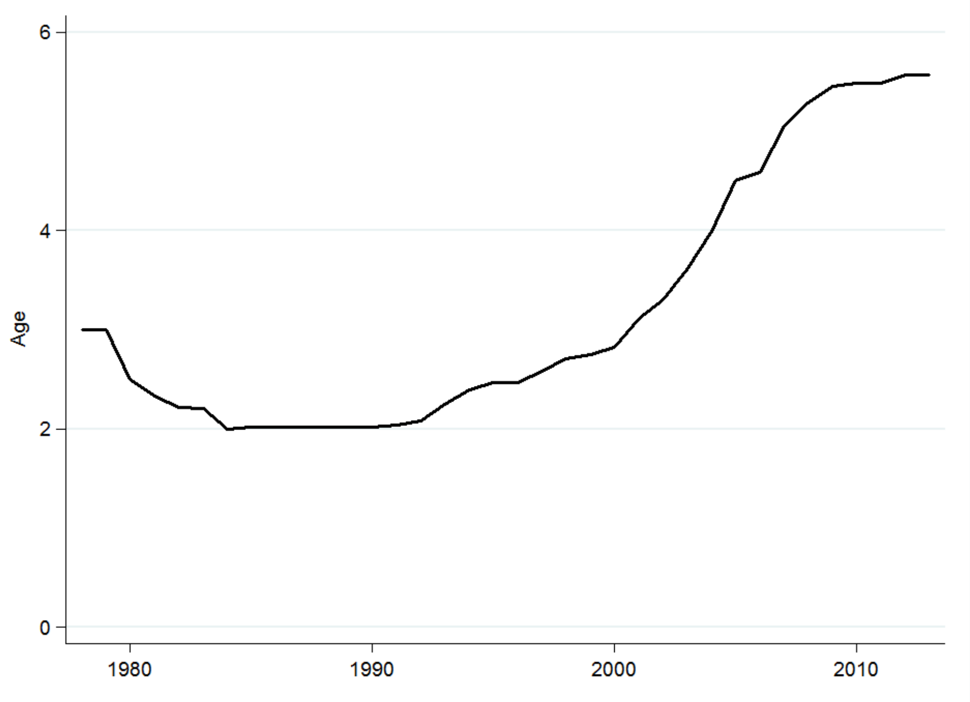
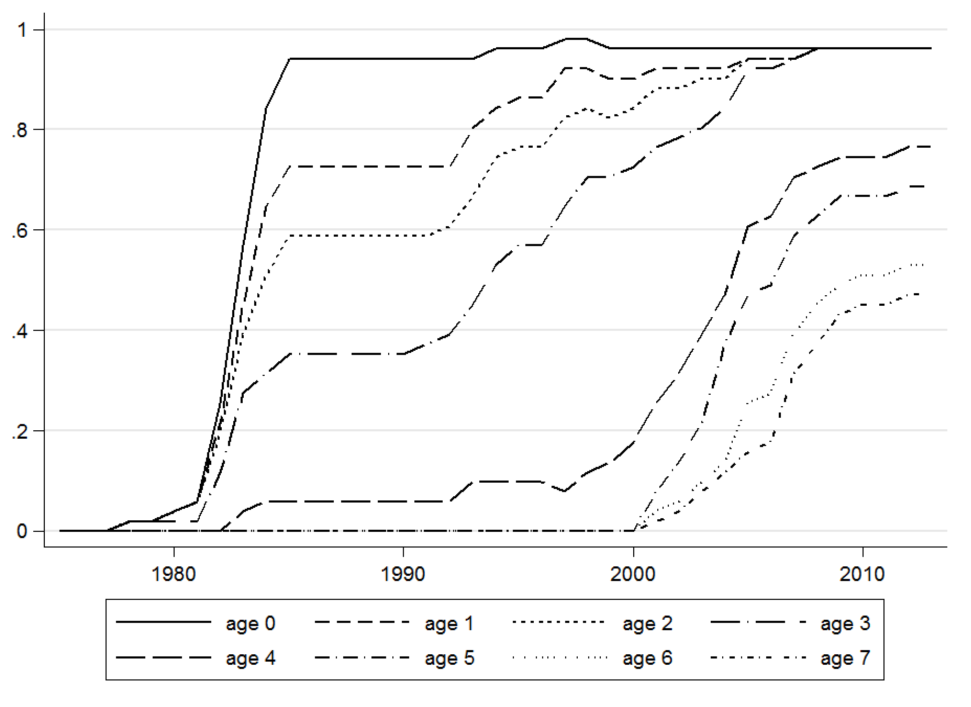
FIGURES AND TABLES

FIGURE 1. Average Age of Required Child Safety Seat Use



Sources: Own calculation using the IIHS law data

FIGURE 2: Share of States where Children are Required to be in Safety Seats, by Year



Sources: Own calculation using the IIHS law data

FIGURE 3: Event Study Graph of Probability that Child Was Restrained after Passage of Law (Selection Corrected Sample)

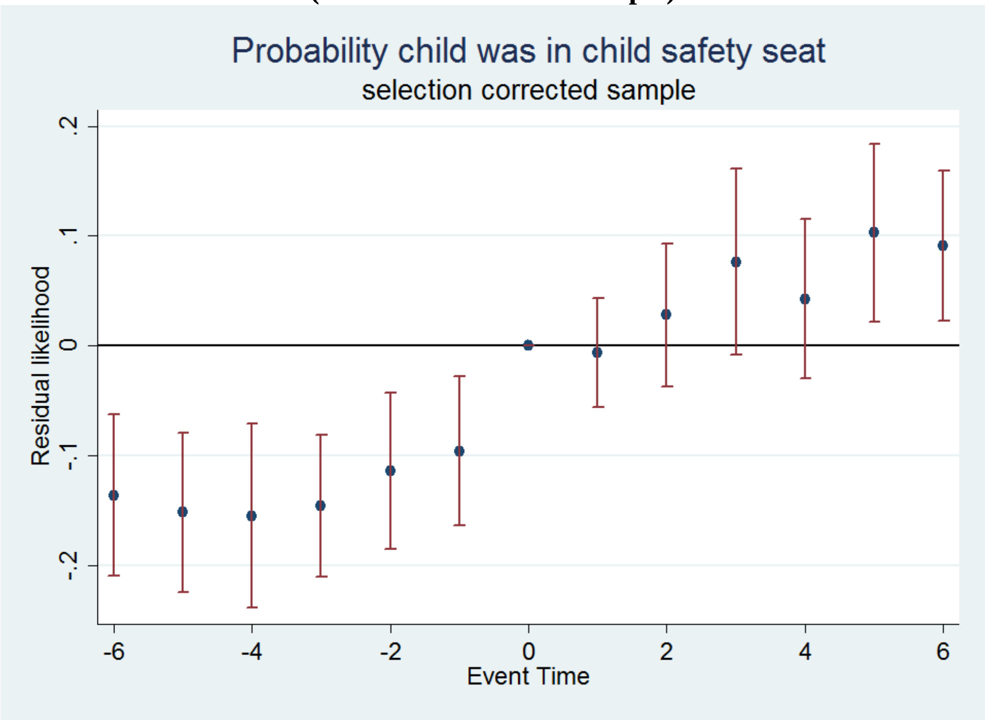


TABLE 1. Characteristics of Children Ages 0 to 7 Involved in a Fatal Crash

	Full Sample		Selection Corrected Sample	
	Mean	Std. Dev.	Mean	Std. Dev.
Child age	3.4	(2.2)	3.5	(2.2)
Child is male	0.51	(0.50)	0.52	(0.50)
Child was covered by a safety seat law	0.41	(0.49)	0.43	(0.50)
Child was restrained in a child safety seat	0.33	(0.47)	0.41	(0.50)
Child was restrained in a seatbelt	0.24	(0.43)	0.32	(0.47)
Child died in crash	0.22	(0.42)	0.04	(0.20)
Child sat in the front	0.29	(0.45)	0.26	(0.44)
Driver's age	32.3	(11.1)	32.9	(10.2)
Driver was male	0.44	(0.50)	0.47	(0.50)
Driver had previous low level traffic violations	0.29	(0.46)	0.29	(0.46)
Driver had previous high level traffic violations	0.10	(0.30)	0.08	(0.28)
Vehicle was a passenger car	0.69	(0.46)	0.57	(0.50)
Vehicle weight (1000lbs)	2.52	(1.45)	2.53	(1.64)
Vehicle year	1986	(9.8)	1991	(9.8)
2-vehicle accident	0.58	(0.49)	0.69	(0.46)
Weekend crash	0.38	(0.49)	0.40	(0.49)
Rural road crash	0.10	(0.30)	0.07	(0.26)
Front-impact crash	0.44	(0.50)	0.68	(0.47)
N	77,837		22,018	

Note: data from the Fatality Analysis Reporting System (FARS) for the years 1975-2011. Values in columns (1) and (2) are obtained from the full sample of all 0 to 7 year-olds involved in a fatal crash; results in columns (3) and (4) are obtained using a sub-sample of all 0 to 7 year-olds involved in 2-car fatal crash where someone died in the other car. See main text for further explanations.

TABLE 2. Baseline Results for Being Restrained in Child Safety Seats and the Likelihood of Dying

	Selection Corrected Sample				Full sample	
	(1)	(2)	(3)	(4)	(5)	(6)
		Panel A: Likelihood of child safety seat use				
Required by Law	0.184***	0.087**	0.310***	0.294***	0.159***	0.253***
	(0.020)	(0.027)	(0.023)	(0.023)	(0.016)	(0.016)
Child Age, State, Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State*Year FE; State*Age FE	No	Yes	Yes	Yes	No	Yes
Year*Age FE	No	Yes	No	No	No	No
Additional Demographics	No	No	No	Yes	No	Yes
Pre-law Mean	0.17	0.17	0.17	0.17	0.13	0.13
Adj. R^2	0.461	0.515	0.489	0.510	0.388	0.452
N	22,018	22,018	22,018	22,018	77,837	77,837
		Panel B: Likelihood of death in the accident				
Required by Law	-0.001	-0.000	-0.005	-0.004	-0.011*	-0.016
	(0.004)	(0.008)	(0.008)	(0.007)	(0.005)	(0.008)
Child Age, State, Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State*Year FE; State*Age FE	No	Yes	Yes	Yes	No	Yes
Year*Age FE	No	Yes	No	No	No	No
Additional Demographics	No	No	No	Yes	No	Yes
Pre-law Mean	0.04	0.04	0.04	0.04	0.22	0.22
Adj. R^2	0.004	0.035	0.034	0.052	0.018	0.087
N	22,018	22,018	22,018	22,018	77,837	77,837

Note: * p<0.05, ** p<0.01, *** p<0.001; data from the Fatality Analysis Reporting System (FARS) for the years 1975-2011. Each column in each panel represents one regression model as in equation (1). The binary dependent variable in Panel A is one if the child was restrained in a child safety seat. The binary dependent variable in Panel B is one if the child died in the accident. All reported regressions are linear probability models but the results are robust to running probit models and calculating marginal effects. Standard errors are clustered at the state level and in parentheses. Results in columns (5) and (6) are obtained from analyses using the sample of all 0 to 7 year-olds involved in a fatal crash; results in columns (1) to (4) are obtained from analyses using the sub-sample of all 0 to 7 year-olds involved in 2-car fatal crash where someone died in the other car. "Addition demographic controls" include where the child was seated in the car (front row, back left, middle or right), and indicators for whether the car was a passenger car, the vehicle weight, indicators for whether it was a 2 or 3-car crash, whether the crash happened on a weekend, whether it happened on a rural road, whether it was a front, rear, right or left-side impact, whether it was an indirect crash, the number of people involved in the crash, indicators for whether the driver was male, had a previous high or low-level traffic infractions, and the driver's age.

TABLE 3. Effects on Restraint Use by Child Age

	Age of Children							
	0	1	2	3	4	5	6	7
	Panel A: State and Year FE							
Required by Law	0.025	0.075*	0.044	0.090*	0.036	0.120*	0.185**	0.117*
	(0.092)	(0.034)	(0.029)	(0.041)	(0.036)	(0.050)	(0.060)	(0.045)
Pre-law Mean	0.33	0.48	0.36	0.28	0.24	0.12	0.06	0.03
Adj. R^2	0.414	0.349	0.353	0.335	0.289	0.276	0.222	0.129
N	2,177	3,012	3,132	3,047	2,944	2,685	2,480	2,541
	Panel B: State FE and State-specific Linear Time Trend							
Required by Law	0.388***	0.260***	0.082*	0.110*	0.212***	0.304***	0.291***	0.175**
	(0.054)	(0.058)	(0.037)	(0.046)	(0.039)	(0.052)	(0.069)	(0.051)
Pre-law Mean	0.33	0.48	0.36	0.28	0.24	0.12	0.06	0.03
Adj. R^2	0.397	0.323	0.344	0.330	0.277	0.261	0.206	0.122
N	2,177	3,012	3,132	3,047	2,944	2,685	2,480	2,541

Note: * p<0.05, ** p<0.01, *** p<0.001; data from the Fatality Analysis Reporting System (FARS) for the years 1975-2011. Each column in each panel is one difference-in-differences (DD) model run for different age groups as indicated in the column header. The dependent variable in all columns and panels indicates whether the child was restrained in a child safety seat, or not. The main variable of interest is law_s*$Effective_t$. All reported regressions are linear probability models but the results are robust to running probit models and calculating marginal effects. Standard errors are clustered at the state level and are reported in parentheses. Panel A shows a standard DD model with state and year fixed effects. Panel B replaces year fixed effects with state-specific linear time trends. All models also include the controls listen in the notes to Table 2. All regressions use the selection-corrected sample, i.e., the sub-sample of all 0 to 7 year-olds involved in 2-car fatal crash where someone died in the other car.

TABLE 4. Effects of the Law on Alternative Restraint Options

	Child Safety Seat (1)	Seatbelt (2)	No Restraint (3)
Required by Law	0.292***	-0.277***	-0.030*
	(0.024)	(0.021)	(0.012)
Age, Year, State Fixed Effects	Yes	Yes	Yes
State*Year FE; State*Age FE	Yes	Yes	Yes
Additional Demographic Controls	Yes	Yes	Yes
Pre-law Mean	0.17	0.46	0.37
Adj. R^2	0.510	0.343	0.438
N	22,018	22,018	22,018

Note: * p<0.05, ** p<0.01, *** p<0.001; data from the Fatality Analysis Reporting System (FARS) for the years 1975-2011. Each column represents one model as in equation (1). The binary dependent variable in column (1) is one if the child was restrained in a child safety seat. The binary dependent variable in column (2) is one if the child was restrained with a regular seatbelt, and the binary dependent variable in column (3) is one if the child was unrestrained. All models are linear probability models but the results are robust to running probit models and calculating marginal effects. Standard errors are clustered at the state level and are reported in parentheses. All three regressions use the selection-corrected sample, i.e., the sub-sample of all 0 to 7 year-olds involved in 2-car fatal crash where someone died in the other car. "Addition demographic controls" include where the child was seated in the car (front row, back left, middle or right), and indicators for whether the car was a passenger car, the vehicle weight, indicators for whether it was a 2 or 3-car crash, whether the crash happened on a weekend, whether it happened on a rural road, whether it was a front, rear, right or left-side impact, whether it was an indirect crash, the number of people involved in the crash, indicators for whether the driver was male, had a previous high or low-level traffic infractions, and the driver's age.

TABLE 5: Characteristics Associated with Over- and Non-Compliance

	Over-compliance (1)	Non-compliance (2)
Ln(Fine)	N/A	-0.027
		(0.016)
Driver Male	-0.014*	0.034***
	(0.007)	(0.009)
Driver age <25	-0.038***	0.037*
	(0.010)	(0.015)
Driver age >44	-0.027**	0.040**
	(0.009)	(0.014)
Driver has previous minor violation	-0.007	0.001
	(0.006)	(0.009)
Driver has previous major violation	-0.039**	0.052***
	(0.013)	(0.015)
Passenger car	-0.037**	0.026
	(0.012)	(0.014)
Car age <5yrs	0.031***	-0.070***
	(0.008)	(0.008)
Car weight (1000lbs)	-0.007	0.006
	(0.005)	(0.007)
Crash on weekend	-0.009	0.026**
	(0.006)	(0.009)
Crash on rural road	-0.018	0.028
	(0.011)	(0.019)
Child male	-0.004	-0.011
	(0.007)	(0.008)
Child Age 3 -5	-0.196***	0.213***
	(0.035)	(0.014)
Child Age 6-7	-0.413***	0.524***
	(0.040)	(0.029)
Variable Mean	0.169	0.267
Adj. R^2	0.264	0.184
N	12572	9724

Note: * p<0.05, ** p<0.01, *** p<0.001; data from the Fatality Analysis Reporting System (FARS) for the years 1975-2011. Each column is one simple regression model with the dependent variable displayed in the column header. Over-compliance is a dummy that is one if the child was restrained in a safety seat even though not legally obliged. Non-compliance is a dummy that is one of the child was no restrained in a safety seat even though legally obliged. Both models also include state and year fixed effects.

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