US Child Safety Seat Laws: Are they Effective, and Who Complies?

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Abstract

This paper assesses the effectiveness of child safety seat laws in the US. Over the past 35 years, 181 of these laws have steadily increased mandatory child safety seat restraint ages. We exploit state-year level variation in these 181 laws and estimate Difference-in-Differences and well as triple difference models using FARS data from 1978 to 2011. To be able to generalize from this US census of all accidents with at least one fatality, we apply the Levitt-Porter Sample Selection Correction Method. Our findings show that increasing age thresholds is effective in increasing the actual age of children in safety seats. Across the child age distribution, restraint rates increase by between 9ppt and 30ppt, or by between 50 and 170 percent, in the long-run. The second part of the paper estimates the impact of these child safety seat laws on the likelihood of a child to die in a fatal accident. We obtain imprecise, but mostly negative effects. Finally, we find that the laws primarily induce compliant parents to switch from traditional seatbelt use to child safety seat use, with only small effects among parents who do not restrain their children.

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

JEL Codes: I18, I31, Z13, Q54

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1. INTRODUCTION

In all US states, 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). In the US, since the passage of the first child safety seat legislation in Tennessee in 1978, such laws have become progressively more restrictive: 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 age three. Since then, the average upper age requirement has almost doubled from 3 years in 2000 to almost 6 years in 2012. Today, there exists substantial cross-state variation in the age until which children have to ride in child safety seats: for example, Illinois requires that all children under 8 years ride in a child safety seat, while in South Dakota, children as young as 5 may ride in an adult seat belt. These differences across US states reflect political and ideological disagreement about whether law enforcement and punishment, or parental education are most effective in ensuring children's safety. For example, former Governor Schwarzenegger (R-CAL) vetoed an increase in the age threshold twice, saying he would favor parental education over more regulation (Seipel 2011). In many cases, however, the law makers have favored increasingly strict laws over parental education.

[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 and their impact on safety 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.[1] Identifying the impact of the laws on parental behavior is important since the laws can only be effective if parents comply.

The main objective of this paper is therefore to assess whether parents have changed their behavior in response to the evolving child safety regulations. The empirical approach exploits state variation in the timing of the introduction and expansion of child safety seat legislation to children of different ages, using standard difference-in-difference-in-difference (DDD) models. Our analysis compares safety seat use among children required by law to use them to use among three control groups: (i) similarly aged children in different states but in the same year, (ii) similarly aged children in the same state but in different years, and (iii) older children in the same state and same year. Because the state-level laws vary by child age, our approach allows us to control for confounding factors that vary by state and year, but also for state-year-specific road safety conditions that affect all children in the state. Our estimated effects therefore capture differences in child safety seat use that are arguably driven by the law changes alone rather than by other confounding factors, such as changing road conditions or other state-level traffic laws.

We use the US Fatality Analysis Reporting System (FARS) data covering the years 1975 to 2011. The FARS database includes information on the universe of children involved in fatal accidents (approximately 77,000 children), including their restraint use at the time of the accident as well as whether they died in the crash; it also provides rich information on the driver, vehicle and crash for each accident. It is the only available database that is suitable to study the impact of

[1] Several empirical studies in and outside of economics empirically study the impact of seatbelt laws for adults on adults' seatbelt usage, injuries and deaths (Loeb 1995; Cohen and Einav 2003; Hakes and Viscusi 2007; Sen and Mizzen, 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 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.

the nearly two hundred state-level child safety seat laws evaluated in this paper on safety seat use and child mortality in car accidents.[2]

There is one main methodological concern when using the FARS database, however: namely, whether results produced using the select sample of cars and drivers in the FARS—those involved in fatal accidents—can be generalized to the population at large. It is likely that worse drivers and drivers with older cars are more likely to be involved in fatal accidents. Moreover, if the use of safety seats prevents deaths, then progressively restrictive safety seat laws may make it less likely that complying parents are included in the FARS data. These issues may cause biased estimates of how the law impact use of child safety seats or their effectiveness.

To address this problem, we use a sample selection correction method, the *Levitt-Porter FARS Sample Selection Correction Method*, which has become standard in the economics of road safety research (cf. Levitt and Porter 2001a, b; Levitt 2008, Islam and Goetzke 2009). We focus on the subset of vehicles involved in two-car crashes wherein someone died in the second car. By focusing on this set of vehicles and occupants, we are able to investigate restraint behavior and restraint effectiveness among people included in the sample because of a death in the *other* car, rather than because of a death in their own car. To the extent that safety seat usage and the associated fatality outcomes in one car are independent of the fatality probability in the second car, the correction produces a sample that is unaffected by the bias that may result from focusing on fatal crashes. Levitt and Porter (2001a) provide a formal proof that this assumption holds conditional on a rich set of passenger control variables as well as spatial and time fixed effects, which we include in our analysis. They show that, after using the selection correction method, the

[2] For example, Levitt and Porter (2010) and Carpenter and Stehr (2008) use injury data but these datasets either focus on select states or years. The one national dataset, the General Estimates Survey, is a sample of crashes having occurred in the US that includes some crashes that are included in the FARS, along with less serious crashes that resulted in injury or property damage. These data do not suit our purpose because state indicators are not available.

FARS data can be used to produce reliable, causal estimates of the effectiveness of safety technologies in a representative sample of crashes where a fatality could have occurred.

In addition, Islam and Goetzke (2009) show empirically that *Levitt-Porter FARS Sample Selection Correction Method* can also be used to produce unbiased estimates of seatbelt use relative to a representative sample of the universe of US drivers. In our context, we show that using the *Levitt-Porter FARS Sample Selection Correction Method* on the FARS data produces a sample of children whose restraint use is a very good approximation of average restraint use among a random sample of vehicles on American roads. We therefore use the selection corrected sample for the majority of our estimations, and argue that our estimated results reflect the impacts of the law on child seat use in the general population.

The second part of the paper examines whether child safety seat laws reduce children's fatality risk when involved in a fatal accident. The existing literature mostly relied on observed behavior in crashes, comparing outcomes among children in safety seats to those among other children. Studies comparing children in safety seats to *unrestrained* children conducted throughout the 1980s found that the seats reduce fatality risk by around 50 percent (Kahane 1986; Partyka 1988; Hertz 1996; Zaza 2001; Starnes 2005; Rice et al. 2009; Jones and Ziebarth 2016a). However, 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 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 protective effects—at least for fatality rates among children aged two to six (Levitt 2008; Levitt and Doyle 2010). In a replication and extension of Levitt (2008), Jones and Ziebarth (2016a) confirm that there is no evidence that child safety seats are more effective than traditional seatbelts in preventing the death of children aged 2 to 6 who are involved in a fatal crash—even under driving and auto conditions in the new millennium.

One concern with the papers cited above is that observed differences in safety outcomes may be due to unobserved differences between children in safety seats and others, such as socioeconomic status or parental driving habits. Our paper overcomes this source of endogeneity by assessing the effectiveness of child safety seats when their use has been prompted by an exogenous change in legislation. We estimate how the risk of dying in a fatal accident differs for children who are required by law to use a child safety seat and those who are not. After using the *Levitt-Porter FARS Sample Selection Correction Method,* we are able to generalize our estimates to the population of crashes with a potential fatality. This analysis allows us to estimate the causal impact of the laws on the probability of dying in a potentially fatal crash.

Using data from between 1980 and 2000, we additionally explore whether the count of fatalities at the state-year-child age level is lower in years when the state level laws required use of child seats. One existing paper in the economics literature uses a similar approach. Evans and Graham (1990) use state-level fatality counts obtained from the FARS to examine the safety impact of child safety seat legislation between 1976 and 1986. The authors find a significant, 30 percent decrease in the number of child fatalities in state-years where a newborn child safety seat law exists. One potential drawback of this paper is that the analysis does not control for secular changes in fatality rates that are unrelated to the laws. Our work expands on this by updating the analysis to include more recent years, and by estimating models that control for secular differences in traffic safety across states and years, as well as age groups.

Our findings in the first part of the paper show that child safety seat legislation increases the probability that children ride in safety seats when involved in a fatal accident. Relative to the baseline restraint use rate, our most conservative estimates suggest that the laws increase child safety seat use by about 50 percent. Extrapolating to the entire US population, our estimates suggest that at least 2.7 million children under the age of eight ride in safety seats each year as a result of the more stringent laws. Our estimates from the full and selection-corrected samples are generally very similar in percentage terms, and highly robust to the inclusion of covariates, suggesting that the impact of the laws is not correlated with demographics, nor is it largely impacted by the sample of children we consider. The finding is in line with the existing literature. For example, seat belt laws have been shown to increase seat belt use among the general population by about 20 to 30 percent (Cohen and Einav 2003), and among youth between by between 45 and 80 percent (Carpenter and Stehr 2008).

We also find suggestive evidence that the increasingly strict legislation reduced the risk of dying in a fatal crash, although our coefficient estimates are generally small in magnitude and imprecisely estimated. After using the selection correction method, we obtain estimated impacts of the laws on the probability of dying in a fatal crash of between -0.0008 and -0.005 percentage points, although our coefficients are imprecisely estimated. On a base fatality rate of only 4 percent, the 95 percent confidence interval of the larger coefficient only allows us to rule out decreases in the fatality risk of more than 47 percent among children covered by the law; the smaller estimate – from a model with more controls – allows us to rule out fatality risk reductions of more than 20 percent. The results of our analysis using the count of fatalities among the full population of children of each age in each state and year confirms the main result: our estimates are small and negative, yet imprecise. Our upper bound estimate, significant at the 10 percent level, implies a reduction in child fatalities of 6 percent after laws are passed. Using the estimate's confidence interval allows us to conclude with 95 percent statistical certainty that child safety seat laws saved no more than 39 young lives per year in the US.

Next, we show that child safety seat laws are primarily effective in shifting law-abiding parents away from use of traditional seatbelts to use of child safety seats. However, the laws do little to decrease the share of unrestrained children involved in fatal accidents. Given existing evidence in the economics of road safety that child safety seats for older children are unlikely to lead to substantial fatality reductions relative to traditional seatbelts (Levitt 2008; Levitt et al. 2010; Jones and Ziebarth, 2016), it is not surprising that we do not show substantial reductions in fatalities associated with the laws: parents may simply switch between equally effective safety measures when laws are expanded.

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 uncover two pieces of evidence to suggest that the laws do not shift restraint behavior through their direct costs—the legal penalties. First, our one quasi-measure of wealth—possession of a new car—is positively related to 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 practices, rather than by increasing the costs associated with failing to use a safety seat.

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 information on 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, as well as whether the person died in the crash. Following the contemporaneous literature on this topic (cf. Levitt 2008; Levitt et al. 2010; Jones and Ziebarth, 2016a), 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. In addition, 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-accident observations, obtained from 62,657 crashes where at least one person died – about 1,693 fatal crashes per year. The first two columns of Table 1 list the characteristics of the children, along with characteristics of their vehicles, drivers and crashes. The average child age is 3.4 years. 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 eventually died, implying to a total of 20,144 child fatalities on American roads over the sample period, or about 544 per year.

[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. Over the same time period, the share of SUVs has tripled from about 10 to 30 percent (Jones and Ziebarth, 2016a). The mean vehicle year in our dataset is 1986. About 37 percent of vehicles are five years old or newer. 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).

Correcting for Sample Selection

The FARS data represent a subset of children traveling in vehicles—namely, those involved in fatal traffic accidents. As discussed in the Introduction, this creates two potential issues: first, that the characteristics of these children (and their drivers) differ significantly from those of the general population; and second, that the sample of children may change if the policy is effective in reducing fatalities, a fact that will cause us to underestimate the life-saving impact of the laws.

To correct for this source of bias and render our estimates generalizable to the population, we use the *Levitt-Porter FARS Sample Selection Correction Method* (Porter and Levitt 2001a, b; Islam and Goetzke 2009). Accordingly, we limit the sample to children involved in two-car accidents where someone in the *other* car was fatally wounded. This leaves us with a group of children who are included in the sample because someone died in the other car, regardless of whether someone died in their own car. This method corrects for sample selection because it is reasonable to assume that a fatality in car B is uncorrelated with the restraint choices and fatality outcomes car A, the child's car: the child in car A is only included in FARS because of the fatality in car B—the one her own vehicle happened to collide with. Following the literature, this correction allows us to generalize the results from our analyses to the full population (Levitt and Porter 2001a; Islam and Goetzke 2009). It also allows us to accurately estimate the causal impact of the laws in reducing the fatality risk in a potentially fatal accident.

Whereas our full sample includes 77,837 child-accident observations, the sample size in the selection-corrected sample decreases to 22,018 child-accident observations, obtained from 17,387 crashes. Comparing columns (3) and (4) with columns (1) and (2) in Table 1 shows how the characteristics of the selection correction sample compare to the full sample. Children in the full sample are 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 in the full sample are older and more likely to be passenger cars. Compared to the full sample, a larger share of children in the selection corrected sample were restrained in safety seats (41 percent vs. 33 percent) or restrained in traditional seatbelts (32 percent vs. 24 percent).

To verify that the selection corrected sample is comparable with a nationally representative sample, we compare the restraint use in our sample to nationally representative restraint use characteristics obtained from the National Survey of Use of Booster Seats (NSUBS).[3] The NSUBS reports provide restraint use rates for children aged 4 to 7, for safety seats, booster seats and traditional seatbelts, and unrestrained children. We are not able to measure booster seat use in our

[3] The NSUBS survey has been conducted biannually since 2006, and is therefore inappropriate for our main analysis.

data until 2008, a fact that complicates comparison between the two data sources. However, we can compare the prevalence of unrestrained children across data sources and samples. For example, in 2006 (the first available year of NSUBS data) 9 percent of children in the NSUBS data were unrestrained (NHTSA 2007). In our data, among 4 to 7 year olds in 2006, we find a 24 percent rate of unrestrained children in the full sample, and a 10 percent rate in the selection corrected sample. In 2009, the NSUBS reports a 13 percent rate of unrestrained 4 to 7-year old children; we find a 21 percent rate in the full sample and a 7 percent rate in the selection corrected sample. Repeating this exercise for all available years, we find that restraint use in the selection corrected sample is a close match for the nationally representative survey results (available upon request).

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

The law data on mandatory child safety seat restraint ages come primarily 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 relevant states and years.

Many state laws are complex (cf. Bae et al. 2014). For one, the wording of some laws makes the age threshold conditional (or substitutive) on children's height and/or weight. The state of Louisiana, for example, 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 ignore 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.[4] Because height and weight change rapidly when children are young, it is very plausible that parents mainly follow the age thresholds. Second, our data do not include height or weight measures for children, only their age. Ignoring the height-weight conditions of laws will cause us to mismeasure compliance with the laws, in either direction. For example, in cases where a child's age does not require that she rides in a child safety seat, but her height and weight do, we will erroneously characterize her guardian as over-complying with the law if she is in a safety seat. In the opposite case—where she should be in a child safety seat by age, but she is not because of a height and weight caveat – we will identify her guardian as under-complying with the law, when they are compliant. The converse of these are also true. To the extent that these situations are equally likely, our mismeasurement will be classical in nature and will cause our estimates of the true effect to exhibit less precisions, but without bias in one direction or the other.

An additional complication arises from the fact that some states allow parents to substitute booster seats in lieu of child safety seats for children, often beyond a certain height or weight threshold; other states (12 states in 2010) have explicit and separate laws governing booster seat use for older children, along with child safety seat laws for younger ones. In the first case, we use the age in the one law that governs both child safety seat and booster seats as the relevant age

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[4] However, properly verifying the child's age has also been cited as one major law enforcement challenge. NHTSA (2010b) reports that officers would ask older children directly if they were not happy with the answer the parents provided.

threshold. In the second, we use the age that is explicitly provided for child safety seats. The FARS data do not distinguish between booster seats and child safety seats until 2008. Graphical inspection suggests that pre-2008, booster seats were coded as child safety seats and we therefore code the very few (<150) cases where booster seats are indicated as child safety seats in years 2008 through 2011. This approach will not bias our results for laws that allow parents to substitute booster seats for child safety seats. However, in the cases that separate laws exist requiring booster seats for older children, using the child safety seat age will cause us to erroneously characterize parents who are following the booster seat laws as over-complying with the child safety seat laws. This should cause us to underestimate the impact of the laws.

Figure 2 illustrates the evolution of child safety seat laws over time. Each line indicates for every year the share of states that required children of the indicated 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 requiring 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.

[Insert Figure 2 about here]

3. EMPIRICAL APPROACH AND IDENTIFICATION

3.1 Main Specification: Impacts of the laws on child safety seat use

Our empirical approach exploits rich variation in child safety seat laws across the 50 US states and the District of Columbia, over 37 years. Our main model is 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 (i) children in states without laws in a given year and (ii) children in the same state in years prior to the law change, but also of (iii) older children in the same state who are not required to use safety seats in the given year. We estimate the following model:

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

$$+\gamma_a + \theta_t + \lambda_s + \theta_t * \lambda_s + \gamma_a * \lambda_s + \gamma_a * \theta_t + \varepsilon_i \quad (1)$$

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. In our main specification, both unrestrained children and children restrained with traditional seatbelts are coded as zero.

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 main binary treatment indicator varies at the state *(s)*, year *(t)*, and child age level *(a)*. The coefficient estimate δ represents the DDD estimate of the impact of changes in age requirements 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.

To accurately estimate the DDD coefficient, we include state, year and child age fixed effects. They net out potential differences in restraint behavior by state, time or child age that are independent of the laws. In addition, the "fully saturated" model in equation (1) includes controls for all first-order interactions of the state, year, and child age fixed effects. These capture differences in restraint behavior that vary at the state-year, state-child age or year-child age level, but are unrelated to the laws. For example, if a national recommendation that 4-year olds should use child safety seats is passed concurrently with the expansion of a state law to cover 4-year olds, our identification strategy will capture the impact of the law change net of the impact of the national recommendation. Similarly, we will be able to isolate impacts of individual laws even in cases when state-level policies impacting road safety behavior change coincidentally with federal safety seat legislation or unrelated trends.

In addition to this fully saturated model, we also estimate variants of model (1) that exclude some of the interaction controls. It is possible that including such a stringent set of controls may cause us to under-estimate the true impact of the laws. For example, 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. In addition, many states increased their laws in jumps—for example, Connecticut increased its upper age requirement from 3 to 6 in 2005. The interaction controls will absorb much of the variation in restraint use for changes that occurred concurrently across states or for many ages at once. We therefore also estimate (1) after excluding all first-order interaction controls—we call this the "parsimonious" model specification—as well as after excluding the age-year controls only.

In some specifications we also control for the vector of individual-level characteristics $\boldsymbol{X_{ist}}$. These include the gender of the child, the position of the child in the car, car- and crash-level characteristics, and a set of driver characteristics such as the age, gender, and previous traffic violations of the driver.[5] Standard errors are routinely clustered at the state level where the laws vary over time (Bertrand et al. 2004, Cameron and Miller 2015).

3.2 Event Study

We additionally undertake an event study graph to evaluate the validity of the assumption that observed restraint use changes do not predate the policy changes. We estimate the following variant of the main model in equation (1):

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

Using the year before the passage of a new law as the baseline, we create indicators Law_{ase} to denote years before and after children of age a, from state s are covered by a law. Focusing on 6 years before and after the introduction of new laws, we estimate (2) controlling for state, year and child age fixed effects and the covariates of our parsimonious specification.[6] We then plot the coefficients by event time, e, relative to the baseline year.

3.3 Impacts on Fatalities

In the second part of the paper, we 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) on an indicator of whether the child i of age a in state s and year t died in the fatal accident or not (*Child died in crash* in Table 1). Then, δ yields the change in the child death probability after laws have become more stringent.

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[5] We estimate (1) as a linear probability model but the results are robust to estimating probit models and calculating marginal effects (available upon request).

[6] We choose to show the event study graph using the parsimonious specification because the more demanding statistical requirements of the event study approach does not allow us to precisely estimate coefficients using the fully saturated model.

As previously discussed, using the FARS data to estimate the effectiveness of child safety seat laws in preventing fatalities may produce biased results if the laws are effective and, as a result, reduce the probability of being included in the sample. Using the *Levitt-Porter Sample Selection Method* will help with this issue. However, we additionally conduct a robustness check whereby we estimate the impact of the laws on *counts of child fatalities* at the state-year-child age level. Since the FARS is a census of road safety fatalities, we are able to produce complete counts of the number of child fatalities in each year-state-child-age cell. Following Evans and Graham (1990), we use the count measure as an outcomes and estimate the following log Poisson model:

$$\log(\mu_{ast}) = \alpha + \delta Law_s * Age_a * Effective_t + \beta \ln(Pop_{ast})$$

$$+\gamma_a + \theta_t + \lambda_s + \theta_t * \lambda_s + \gamma_a * \lambda_s + \gamma_a * \theta_t + \varepsilon_{ast} \qquad (3)$$

where μ_{ast} is the expected fatality count among children of age *a*, in state *s* in year *t*. This model controls for child age, state, and year fixed effects, their interactions, and the total number of children of each age, in each year and state, which we obtained from the Census. Age-specific population data are not available before 1980 and our analysis therefore excludes 1976-1979, the earliest years of the laws. The model therefore estimates the impact of the laws on the count of child fatalities relative to the full population of children in each state, and does not suffer from the potential bias by focusing on fatalities within the sample of fatal accidents. As with our main approach, we also estimate (3) after excluding some of the interaction terms to avoid underestimating the impact of the laws.

3.3 Effect Heterogeneity by Age

To test for heterogeneity in effects of the laws, we estimate models separately 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. By estimating the models by child age, our DDD model in equation (1) reduces to a series of difference-in-difference (DD) estimates with state and year fixed effects. These models test for each child age group separately whether safety seat use is greater in state-years where it is required. We expect results from this approach to reflect the results from the fully saturated DDD model, since it relies on variation in laws over time, within states and age groups. The DD model results are therefore a robustness check of the validity of our approach, in addition to providing insight into the effects by age. We also estimate how the laws impact fatality rates for children of each age.

3.4 Effect on Alternative Restraint Options

We also estimate (1) on 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 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 prior research suggests that 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 2016a). The results of this exercise give us information of how the laws impacted use of alternative restraint types, and also act as a robustness check to ensure that our child safety seat measure is correctly identified.

3.5 Investigating Over- and Under-Compliance

Finally, we investigate the characteristics associated with child safety seat use, both before and after the passage of laws. In particular, we define two behaviors: *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.

We estimate (1) on these two outcomes, providing evidence as to which child, crash and driver characteristics predict use of child safety seats both when it is not required by law, and when it is. The non-compliance analysis also 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 by law to be in a child safety seat (*over-compliance*) and, *(b)* children who are required by law to be in a child safety seat (*non-compliance*).

4. RESULTS

4.1 Main Specification: DDD Models

Table 2, Panel A shows results that address the first research question: namely, do drivers restrain their child passengers in safety seats when laws change and require them to do so? Each column shows results of 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 involved in fatal crashes, while the first four columns show results for the selection-corrected sample. This is our preferred sample that corrects for the fact that drivers involved in fatal accident are worse drivers via the *Levitt-Porter Sample Selection Correction Method*. The results from these first four columns better reflect the impact of the laws in a general population of drivers.

[Insert Table 2 about here]

Column (1) reports results from the fully saturated model that includes all first and second order interactions of child age, state and year covariates. The fully saturated DDD specification produces an effect estimate of 9.7 percentage points, which represents an increase in safety seat use of 57 percent relative to the baseline of 17 percent. Because the expansions of many state laws occurred in close succession for children of similar ages (see Section 3), column (1) likely underestimates the true compliance effect. As such, we interpret the 9.7 percentage points increase in column (1) as a lower bound estimate. The remaining estimates reported in Panel A of Table 2 show how the coefficient estimate changes as we selectively exclude controls from the fully saturated DDD model.

Column (2) shows the 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). As we discussed in Section 3, this approach allows for the lumpy nature of the law changes but does not control for general increases in age-year specific restraint use from the main estimate. As such, this specification produces a much larger coefficient estimate of 19.8 percentage points, or about a doubling of the baseline restraint rate. For the full sample, we obtain an estimate of 16.9 percentage points—a 130 percent effect relative to the pre-law safety seat use rate in the full sample. As one can see, the estimates from the selection corrected and full samples are relatively similar and not statistically different from one another.

We also report estimates from models that exclude the age-year ($\gamma_a * \theta_t$) fixed effects, but include the state-year and state-age controls. This model produces an estimate of law effectiveness that is net of concurrent state-specific changes in restraint behavior, as well as differences in restraint usage across states among similarly aged children; it does not control for age-specific changes over time in restraint use common across states. Column (3) shows an estimated law effect of 32 percentage points while column (4)—where we add covariates—reveals an almost identical estimate of 31 percentage points. In percentage terms, we estimate a 170 percent increase in child safety seat use among the selection corrected sample. We also report a very similar estimate using this specification for the full sample in column (6). Note that this estimate can be interpreted as the long-run effect because the model estimates 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.

Finally, note the following: Because controlling for observable driver and crash characteristics barely alters the coefficient estimate in column (3) vs. column (4), observable features of drivers and accidents are not systematically and simultaneously correlated with the impacts of the laws on use. This reinforces the validity that the changes in the laws exogenously impact behavior.

4.2 Event Study

Our identification strategy only produces unbiased estimates of the impact of the laws if legislated changes are not related to other state-specific factors that may affect restraint use. All our models include at least state, year and child age fixed effects, which should eliminate many potential concerns. Following equation (2) we produce an event study graph in Figure 3, to test the assumption of no pre-trends in restrain use.

[Insert Figure 3 about here]

Figure 3 shows that in the years before the passage of new laws, average child seat use was significantly lower as compared to post-reform years. Additionally, there is no evidence of pre-reform trends in child safety seat use. We observe a discrete and clear increase of about 10 percentage points in the probability that children are restrained in safety seats in the first post-reform year. The effect continues to increase by an additional 5 to 8 percentage points in the subsequent five years. 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).

In sum, the event study in Figure 3 provides little evidence of endogenous law changes, but clear evidence for a causal effect of the laws on child safety seat use.

4.3 Effects on Likelihood of Child to Die in a Fatal Accident

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. The six regression results reported in Panel B show the estimated impact of the laws on the outcome variable *Child died in crash* and are otherwise identically specified as the models in Panel A following equation (1).

As seen, except for one significant coefficient in column (5), all other coefficient estimates are small in size and not statistically significant. However, the effects are imprecisely estimated: relative to the mean fatality rate of just 4 percent in the selection corrected sample, our largest point estimate in column (4) only let us exclude with 95% statistical probability a reduction of 1.9 percentage points or more in the likelihood of death—a potential effect that would translate to a 48 percent decrease of the mean. The estimated effect in the parsimonious specification in column (1) allows us to rule out decreases larger than 0.7 percentage points – 18 percent of the mean – and the estimate in the fully saturated model in column (2) only allows us to rule out reductions in the fatality risk of more than 0.8 percentage points – 20 percent of the mean.

In column (5), the precisely estimated coefficient suggests that in the full sample of children involved in fatal crashes, children covered by child safety seat laws are about 5 percent less likely to die than uncovered children.

Next, we run another specification to test for the impact of child safety seat legislation on child mortality using a count data model as in equation (3). Table 3 reports estimation results from the Poisson models using state-year-child age specific fatality counts.

[Insert Table 3 about here]

Column (1) reports the coefficient estimate from the fully saturated model. Here, again, despite a negative coefficient, we find no evidence that among children covered by law, the total number of fatalities decreased significantly. When we repeat the analysis excluding all the interaction controls (column (2)) and excluding the age-year controls only (column (3)), we obtain slightly larger coefficient estimates that are marginally significant at the 10% level. In column (3), for instance, we estimate a 6 percent reduction (p-value=0.052) in the number of child fatalities among covered children. Given the estimate's large standard error, however, we can only rule out effect sizes larger than -12.4 percent (or 39 children per year) with 95% certainty.

4.4 Effect Heterogeneity by Age

Next, we take a closer look at the operating mechanisms of the laws and investigate heterogeneity in their effects. We repeat our analyses separately for children of each age, which reduces the DDD models to DD model for each age group. All results are produced using the selection corrected sample and control for state and year fixed effects. Panel A of Table 4 shows the results for child safety seat use, where each column shows a result from one of the eight separate DD models by child age.

[Insert Table 4 about here]

In Panel A, the coefficient estimates vary in size from 2.5 percentage points (for age 0) to 23.8 percentage points (for age 6). Further, the average pre-reform restraint use rate varies highly by child age, meaning that the estimated impacts vary highly in percentage terms. We find significant point estimates for laws affecting children aged 1 (7.3 percentage point or 15 percent), aged 3 (8.9 percentage points or 33 percent), aged 5 (16.6 percentage points or 138 percent), aged 6 (23.8 percentage points or 396 percent) and aged 7 (15.7 percentage points or 523 percent). The estimated effect for children aged below 1, aged 2 and 4 are smaller, and not significantly different than 0. In general, the laws seem to have impacted older children more strongly.

In Panel B, the child death effects separately by child age confirm our previous findings in Panel B of Table 2 and Table 3: we are unable to obtain enough statistical precision to reject the null hypothesis that child safety seat laws have been ineffective in reducing the likelihood that a child dies when involved in a fatal accident.

4.5 Effect on Alternative Restraint Use

Tables 5 and 6 show how the laws affected the use of traditional seatbelts and the probability of children being unrestrained. Table 5 shows our standard DDD model in equation (1) using two new outcomes variables as described in Section 3.4: traditional *seatbelt* use (column (2)), and *no restraint* use (column (3)). The estimate in column (1) is the estimate from the model in column (4) of Panel A in Table 2 using state, year and age fixed effects in addition to state-year and state-age fixed effects as well as socio-demographic controls.

[Insert Tables 5 and 6 about here]

The empirical evidence in Table 5 is very clear. While there is a moderate 3 percentage point (8.1 percent) 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 28 percentage point (60.2 percent) decrease in the probability that a child was restrained in a traditional seatbelt.

Further, Table 6 estimates the model separately by child age. The findings by age show that, while the effect is smaller for younger children, the laws appear to have caused parents to substitute away from traditional seatbelts among children of all ages. We estimate negligible decreases in the probability of being unrestrained for children of most ages: our largest negative impacts are among newborns (4 percentage point decrease or 20 percent) and three year olds (5 percentage point decrease or 20 percent), but these estimates are insignificant. This is intuitively plausible: Law-abiding parents are those who previously used traditional seatbelts—and not parents who chose to leave their children unrestrained—before it became mandatory to restrain their children in safety seats.

4.6 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 characteristics predict use of child safety seats *before* they were required? And second, which characteristics are associated with violating the laws *after* they were enacted? Table 7 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 7 about here]

Column (1) shows that male drivers, younger drivers and older drivers, as well as drivers with previous serious traffic violations are all less likely to use 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 with 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 seatbelt when, by law, they should have been in a child safety seat. Further, time of day of the crash is an important predictor, where non-compliance was more likely in crashes that occurred very early in the morning or later at night as opposed to during the day. We do not find evidence that the fine predicts compliance with the law, and the result is robust to alterative specification to the fine amount (categorical specification, and quadratic in the fine amount; results available upon request).

5. CONCLUSION

This paper evaluates the effectiveness of a government child safety regulation that exists in the majority of countries worldwide and has dramatically tightened in the last 40 years: the mandated use of child safety seats. Average age thresholds for mandated use have increased from two years old in the 1980s to almost six years old in recent years in the US.

Using real-world data on all fatal accidents in the US from 1975 to 2011, we first evaluate whether parents comply when child safety seat laws are introduced or tightened. We also examine whether parents respond by reducing use of traditional seatbelts, or by buckling their children in when they would otherwise had ridden unrestrained. Second, this paper investigates whether the child safety seat laws reduce children's fatality risk when involved in a fatal accident. In addition, we identify characteristics that are predictors of child safety seat use compliance behavior.

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, safety seat use among children involved in fatal accidents increases by between 10 and 30 percentage points—or by between 57 and 170 percent. Our use of the Levitt Porter Sample Selection method – and our finding that restrain use among this sample is a good match for the population – lets us conclude that our estimates are a good approximation of the impact of the laws in the general public.

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 and that an average parent responded to the laws to the same extent as those involved in fatal accidents, an increase in child safety seat use of 10 percentage points would translate into about 3 million young children whose parents altered their parenting behavior when laws changed.

Our findings primarily show a substitution effect—from the use of traditional seatbelts to child safety seats—rather than a substantial decrease in the share of unrestrained children. This finding highlights the importance of comparing the safety benefits of safety seats to traditional seatbelts, rather than to riding unrestrained. The finding may also be an explanation for why we do not find a large, statistically significant impact of the laws on the likelihood that children die in fatal accidents: we cannot say with high statistical certainty whether the probability of dying in a fatal accident has decreased due to the laws. However, our estimates are imprecise. The results from the count data model allow us to rule out with 95% statistical certainty that passing stricter legal age requirements for child safety seats saved more than 39 young lives per year in the US.

This estimated fatality impact also captures any unintended consequences of the laws. For example, if parents change their driving behavior because they believe that their children are safer when riding in safety seats, our estimated impacts will also capture the safety impact of the changes in driving style (Peltzman 1975). However, we do not find changes in non-child fatalities associated with the laws (results available upon request), which suggests that driving behavior may not be affected by the laws. More importantly, if parents do not use child safety seats correctly—a fact that is well-documented in the literature—our estimated impacts will also include the effects of using a safety seat incorrectly (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).

When applying values common in the health economics literature to assess the value of statistical lives, for example $10M (Viscusi and Aldy 2003), the monetized benefits of 39 lives saved per year is $390M per year. In contrast, at a median price of $200 per safety seat, the cost of buying a new seat for every child born in America each year is about $767M. Because these seats can be used for several years—they typically expire after six years—and potentially passed on to younger siblings, every newborn in America may not require a seat and our cost number may be an overestimate. Further, our calculated benefit does not include other potential safety benefits of child safety seat laws, such as a reduced injury rate but or other nonpecuniary costs of requiring children to ride in safety seats (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). However, using these number to conduct a back-of-the envelope calculation yields a net annual cost of the seats of $377M.

Our analysis of the determinants of compliance suggests that the cost of breaking the law—i.e., how high the fine is—is not a significant deterrent. 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 a child safety context, compliant behavior is generated through the signaling effect of the law, rather than through its punitive force. This suggests that other regulatory tools, such as information campaigns, may be effective in producing the intended behavioral changes among parents.

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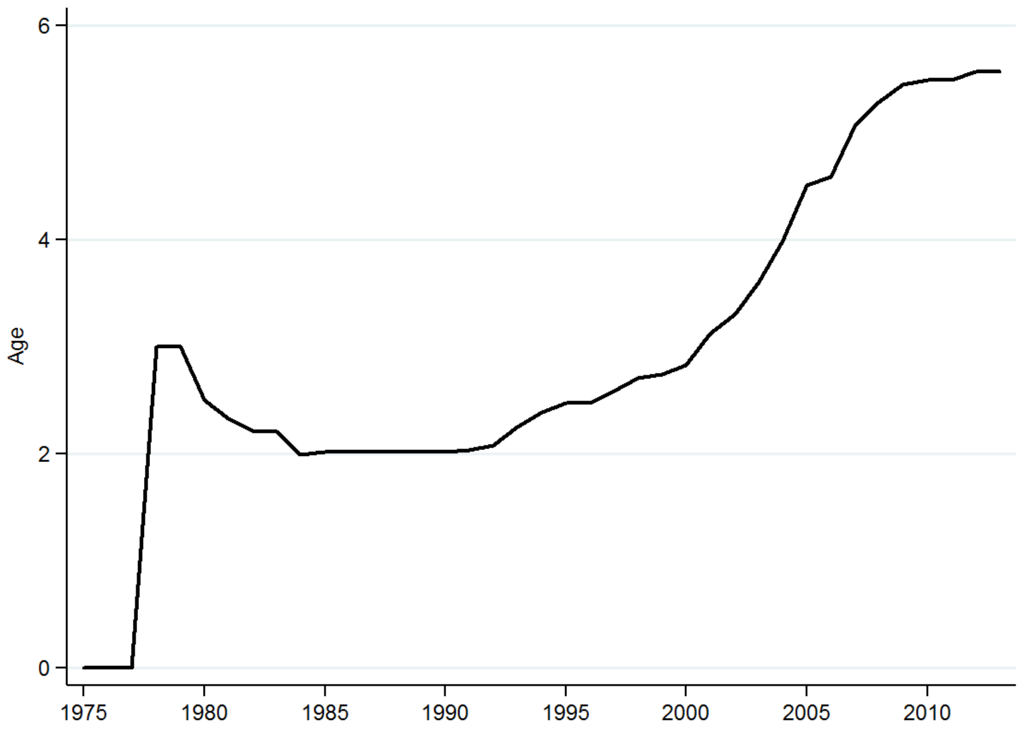
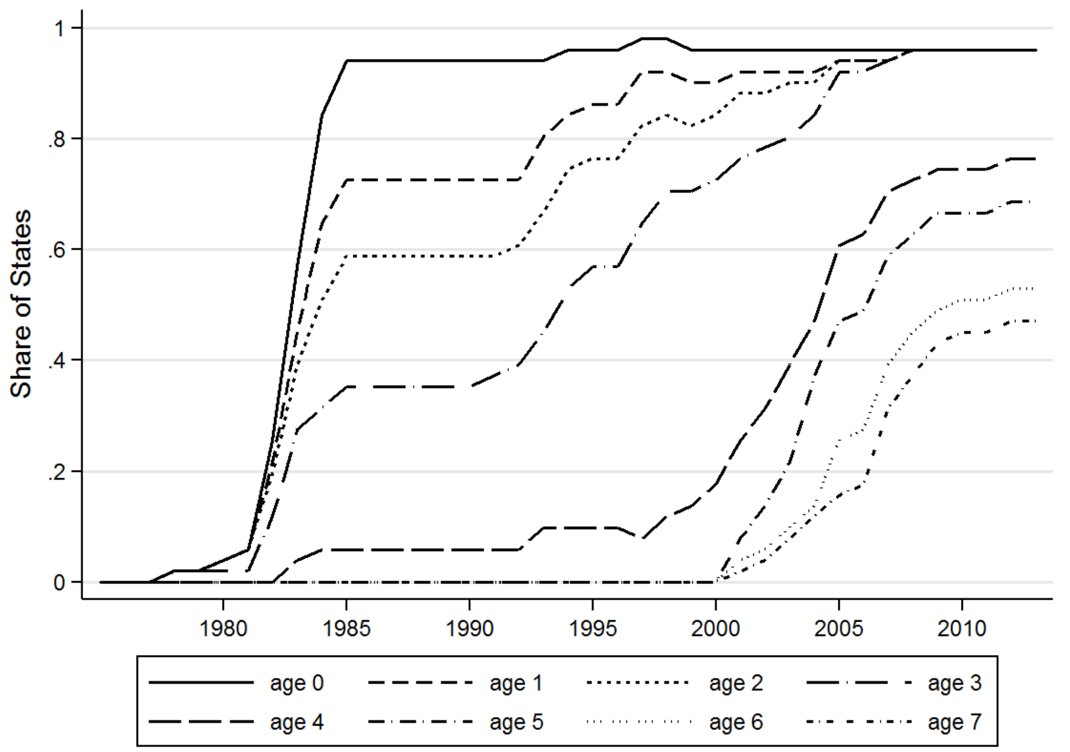
FIGURES AND TABLES

FIGURE 1. Average Age of Required Child Safety Seat Use



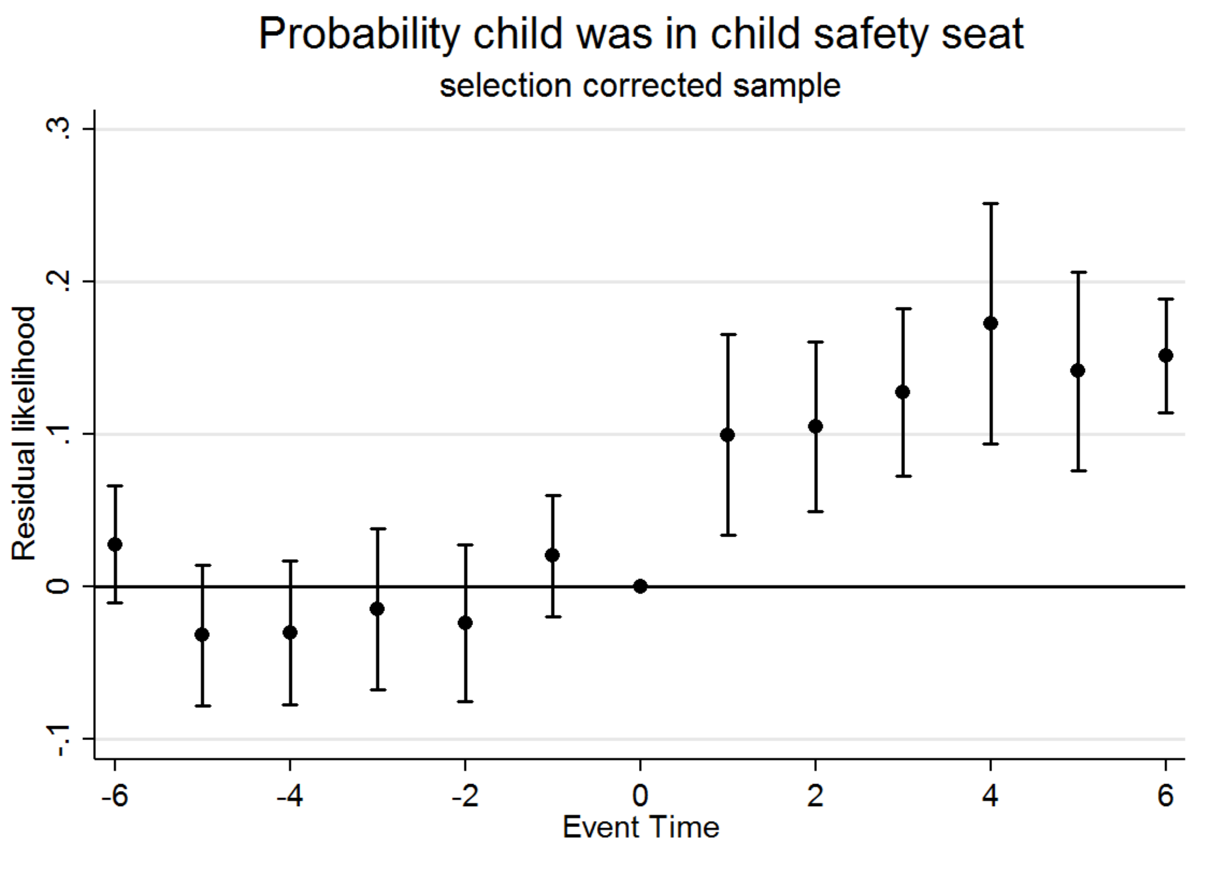
Sources: Own calculation using the IIHS law data

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)



Note: Event time '0' equals the year before the law was passed, i.e., t-1.

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)	0.42	(0.49)
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)
Total Number of Accidents	62,657		17,387	
Total Number of Child Deaths	20,144		974	
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	0.097***	0.198***	0.324***	0.308***	0.169***	0.263***
	(0.027)	(0.022)	(0.023)	(0.023)	(0.017)	(0.015)
Child Age, State, Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State*Year FE; State*Age FE	Yes	No	Yes	Yes	No	Yes
Year*Age FE	Yes	No	No	No	No	No
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	0.518	0.466	0.492	0.513	0.392	0.455
N	22,018	22,018	22,018	22,018	77,837	77,837
			Panel B: Likelihood of death			
Required by Law	-0.000	-0.001	-0.005	-0.004	-0.011*	-0.016
	(0.008)	(0.004)	(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	Yes	No	Yes	Yes	No	Yes
Year*Age FE	Yes	No	No	No	No	No
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	0.035	0.004	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. 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. "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. Count Models of Effects of Laws on Fatalities

	(1)	(2)	(3)
Required by Law	-0.021	-0.052	-0.061
	(0.040)	(0.028)	(0.031)
Child Age, State, Year FE	Yes	Yes	Yes
State*Year FE; State*Age FE	Yes	No	Yes
Year*Age FE	Yes	No	No
Pre-law Number of Deaths	9,639	9,639	9,639
Psuedo. R^2	0.226	0.152	0.218
N	10,829	10,829	10,829

Note: * p<0.05, ** p<0.01, *** p<0.001; data from the Fatality Analysis Reporting System (FARS) for the years 1980-2010 collapsed at the year-state-child age level. Each column represents one model as in equation (3). All models are log Poisson regressions. Standard errors are clustered at the state level and are reported in parentheses. All models include controls for the natural log of the age-specific population of children in each state and year.

TABLE 4. Effects on Restraint Use by Child Age

	Age of Children							
	0	1	2	3	4	5	6	7
Panel A: Likelihood of child safety seat use								
Required by Law	0.025	0.073*	0.053	0.089*	0.058	0.166**	0.238***	0.157*
	(0.092)	(0.034)	(0.032)	(0.042)	(0.035)	(0.052)	(0.047)	(0.034)
Pre-law Mean	0.33	0.48	0.36	0.27	0.24	0.12	0.06	0.03
Adj. R^2	0.414	0.351	0.357	0.345	0.316	0.317	0.262	0.158
Panel B: Likelihood of death								
Required by Law	0.060	0.006	0.023	0.011	-0.004	-0.018	0.002	-0.032
	(0.043)	(0.024)	(0.014)	(0.014)	(0.015)	(0.021)	(0.025)	(0.022)
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 Panel A indicates whether the child was restrained in a child safety seat, or not. The dependent variables in Panel B indicates whether the child was restrained with a regular seatbelt, or unrestrained. The main variable of interest is $law_s*Effective_t$. Standard errors are clustered at the state level and are reported in parentheses. All models also include the controls listed in the notes to Table 2 as well as state and year fixed effects. All regressions use the selection-corrected sample.

TABLE 5. Effects of the Law on Alternative Restraint Use

	Child Safety Seat	Seatbelt	No Restraint
	(1)	(2)	(3)
Required by Law	0.308***	-0.277***	-0.030*
	(0.023)	(0.021)	(0.012)
Age, Year, State Fixed Effects	Yes	Yes	Yes
State*Year FE; State*Age FE	Yes	Yes	Yes
Demographic Controls	Yes	Yes	Yes
Pre-law Mean	0.17	0.46	0.37
Adj. R^2	0.513	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. See Table 2.

TABLE 6. Effects on of the Law on Alternative Restraint Use by Child Age

	Age of Children							
	0	1	2	3	4	5	6	7
	Panel A: Seatbelt							
Required by Law	0.015	-0.058*	-0.055	-0.039	-0.051	-0.185*	-0.260**	-0.155*
	(0.033)	(0.029)	(0.033)	(0.039)	(0.041)	(0.072)	(0.077)	(0.069)
	Panel B: Unrestrained							
Required by Law	-0.040	-0.015	0.003	-0.050	-0.007	0.020	0.022	-0.002
	(0.105)	(0.028)	(0.030)	(0.033)	(0.034)	(0.039)	(0.043)	(0.053)
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 Panel A indicates whether the child was restrained with a traditional seat belt, or not. The dependent variables in Panel B indicates whether the child was unrestrained. The main variable of interest is law_s*$Effective_t$. Standard errors are clustered at the state level and are reported in parentheses. All models also include the controls listed in the notes to Table 2 as well as state and year fixed effects. All regressions use the selection-corrected sample.

TABLE 7: Characteristics Associated with Over- and Non-Compliance

	Over-compliance (1)	Non-compliance (2)
Ln(Fine)	N/A	-0.015
		(0.017)
Driver Male	-0.015*	0.030**
	(0.007)	(0.010)
Driver age <25	-0.039***	0.036*
	(0.010)	(0.016)
Driver age >44	-0.026**	0.049***
	(0.009)	(0.013)
Driver has previous minor violation	-0.007	-0.003
	(0.006)	(0.009)
Driver has previous major violation	-0.039**	0.057***
	(0.013)	(0.015)
Passenger car	-0.037**	0.025
	(0.012)	(0.014)
Car age <5yrs	0.031***	-0.068***
	(0.008)	(0.008)
Car weight (1000lbs)	-0.008	0.007
	(0.005)	(0.007)
Crash on weekend	-0.010	0.022*
	(0.006)	(0.009)
Crash on rural road	-0.019	0.030
	(0.011)	(0.018)
Crash in morning	0.013	0.097**
	(0.018)	(0.030)
Crash in evening	-0.015	0.052***
	(0.010)	(0.014)
Child male	-0.004	-0.009
	(0.007)	(0.007)
Child Age 3 -5	-0.195***	0.203***
	(0.034)	(0.014)
Child Age 6-7	-0.411***	0.475***
	(0.040)	(0.021)
Variable Mean	0.171	0.260
Adj. R^2	0.270	0.180
N	12,572	9,724

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.