Car Seats as Contraception

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Abstract: Since 1977, U.S. states have passed laws steadily raising the age for which a child must ride in a car safety seat. These laws significantly raise the cost of having a third child, as many regular-sized cars cannot fit three child seats in the back. Using census data and state-year variation in laws, we estimate that when women have two children of ages requiring mandated car seats, they have a lower annual probability of giving birth by 0.73 percentage points. Consistent with a causal channel, this effect is limited to third child births, is concentrated in households with access to a car, and is larger when a male is present (when both front seats are likely to be occupied). We estimate that these laws prevented only 57 car crash fatalities of children nationwide in 2017. Simultaneously, they led to a permanent reduction of approximately 8,000 births in the same year, and 145,000 fewer births since 1980, with 90% of this decline being since 2000.

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The U.S. has experienced a series of dramatic shifts in fertility over the last century. After the huge secular decline in the mid-20th century, birth rates began to rebound in the 1970s, such that the U.S. had an above-replacement total fertility rate (TFR) of 2.12 children per woman as recently as 2007. Since then, TFRs have declined in ten out of the eleven subsequent years, reaching an all-time low of 1.73 in 2018.[1] These recent changes are puzzling, because they do not obviously coincide with shifts in factors used to explain 20th century changes in birth rates, such as increased female labor force participation (Adsera 2005), declining value of children for agricultural purposes (Becker 1960), increased female education (Black, Devereux and Salvanes 2008, McCrary and Royer 2011), access to contraception (Goldin and Katz 2002, Bailey 2010), and greater returns to human capital investments (Becker, Murphy and Tamura (1990)).

The most recent decline is also puzzling under standard economic theories of fertility. A recovering economy ought to have increased the budget set for available children, and led to a subjective sense of them being cheaper, unless the costs of having children are rising faster than incomes.[2] Becker (1960) noted this puzzle - unless children are an inferior good, which he viewed as unlikely, when people become richer they should both have more children and invest more in each one. One periodically hears the complaint that someone "cannot afford" to have more children. If these complaints are taken at face value, it raises the question of which costs are driving the decline in fertility during a period of economic growth.

We consider one unexpected cost – child car seat laws. Since 1977, U.S. states have passed laws mandating that children be restrained in child safety seats. Initially these laws typically applied to children ages one to three. However, since the mid-1990s state-mandated age limits

[1] See St Louis Fed data at https://fred.stlouisfed.org/series/SPDYNTFRTINUSA
[2] The NBER dates the end of the 2007-2008 recession to June 2009.

have seen a huge ratcheting upwards. The median age for seat belts in U.S. states is now eight, and every single law change has been to increase the age, not reduce it. Enthusiasm for these laws has not been curbed by studies showing that child car seats are generally no more effective than seat belts in preventing death or serious injury for children above age two (e.g. Levitt 2008, Doyle and Levitt 2010). Part of this may be due to the perception that such mandates are virtually costless, beyond that of the car seats themselves (though for many families, even these may be burdensome).

These laws can significantly raise the cost of having children for women who already have two young children, and are considering a third. While the particular type of mandated restraining device varies, many cars cannot easily accommodate three child seats in the back row of seats, as would be needed if both front seats are occupied by adults. This especially increases the cost of a third child for many families, by necessitating the purchase of a larger car. The most practical options, like minivans, have additional problems of being expensive and unfashionable. We exploit the specific nature of costs associated with a third child, along with considerable variation in mandated ages over states and years, to identify the impact of state car seat laws on fertility.

Using census data on the number and age of children for each woman surveyed, we estimate the effect of these laws on birthrates from 1973 to 2017. We find that when a woman has two children below the car seat age, her chances of giving birth that year decline by 0.73 percentage points. This represents a large decline, as the probability of giving birth for a woman age 18-35 with two children already is 9.36% in our sample. The baseline estimate includes fixed effects at the county-year-number of children level, so the effect is not driven by changing birth rates in that county in general, nor changing birth rates in that county for third children in particular. It also includes controls for all combinations of children's ages, so comparisons are between, for instance, families with a six-year-old and a four-year-old, where in some state-years both are required to be in car seats, and in others they are not. We also include controls for the woman's age and demographics by both county and year. The effect of car seat laws on third child births survives all these additional drivers of birth rates.

By contrast, we do not find significant effects of car seat laws at other birth margins where car-seat-related crowding is unlikely to be an issue. For instance, there are no significant differences in birth rates when the woman has only one child total of car seat age, or two children total where only one child is required to be in a car seat. This is consistent with car seat laws mattering specifically through a channel of crowding at the third child, and is difficult to reconcile with other mechanisms where car seat laws are proxying for other demographic or social trends.

We find that the estimated effects are driven entirely by households with access to a car, consistent with car usage mattering directly. The effect is also concentrated in households where there is an adult male in the household, increasing the likelihood that both front seats are occupied by adults. Somewhat surprisingly, the effects are larger among households with higher income levels. This suggests that the pressures leading to reduced birthrates may not be entirely financial, or that these groups bear a greater burden through higher compliance rates.

The results up to this point measure the contemporaneous effect of car seat laws – the reduction in birth rates while a third child would require a third car seat. However, this effect is comprised of two separate components. The first represents an intertemporal shift in birth rates, whereby child birth is delayed until existing children age out of car seats, offsetting some or all of the contemporaneous effect. The second component represents a permanent effect, whereby the additional cost dissuades some women from ever having a third child or more.

We estimate the permanent component of the car seat laws with two distinct approaches. First, when conditioning on women that give birth to a second child, we examine the lifetime probability of having a third child. Each additional year that the two eldest children are required to be in car seats results in a 0.60 percentage point reduction in the lifetime probability of having a third child. Second, we embed our panel data estimations into dynamic simulations which incorporate potential offsetting effects. We estimate that switching from an eight-year-old mandate to a four-year-old mandate would result in the average woman having 0.0076 more children. In 2017, we estimate that car seat laws lead to a permanent reduction of approximately 8,000 births, and have prevented 145,000 births over our sample period, with 60% of this effect being since 2008. By contrast, if *current* laws had applied over the whole sample, we estimate there would have been a further 350,000 fewer births. When comparing simulated birth decreases with predicted changes in births from our base regressions (which only measure contemporaneous effects), we estimate that approximately 71% of the contemporaneous effect is permanent.

Finally, using Federal data on fatal car crashes dating back to 1975, we estimate the effect of car seat laws on the number of child fatalities in car crashes. Existing work such as Levitt (2008) and Levitt and Doyle (2010) shows no significant effects of the *use* of car seats on death or serious injury rates for children over age 2, conditional on getting in a crash.[3] This still leaves open the question of the impact of mandates themselves, whose effect may not map cleanly to actual usage.

[3] As Levitt (2008) notes, most of the prior work studying the effect of child car seats (e.g. Kahane 1986, Partyka 1988, Hertz 1996) has severe methodological problems, such as comparing child car seat use with riding unrestrained, thereby ignoring the other option of using a seat belt. Much of the research looking at the effect of car seat laws is also not particularly well identified. Farmer, Howard, Rothman and MacPherson (2009) estimate the effect of booster seat laws on death rates among only the set of accidents with a fatality, but do not control for either state or time fixed effects. Pressley, Trieu, Barlow and Kendig (2009) compares states with and without booster seat laws in a single year. Sun, Bauer and Hardman (2010) compare time series changes within a single state. Eichelberger, Chouinard, Jermakian (2012) examine death rates before and after law changes for five U.S. states, taking only data two years either side of the law change, and find an insignificant change in fatalities.

Using similar fixed effects specifications as our birth rate tests, we find that the estimated impact of car seat laws on deaths of children below age eight is miniscule. Our best estimates are that existing car seat mandates prevented 57 fatalities nationwide in 2017, with the most favorable estimates being 140 fatalities prevented. In the vast majority of specifications, we are unable to reject a null hypothesis of zero lives saved. Ignoring the financial cost of purchasing safety seats, these estimates allow one to calculate the implied ratio of the value of a child's life *saved* (conditional on them being born) versus the value of a child's life *prevented* (children who might have been born, but were not). We estimate this ratio to be between 57 and 141.

This enormous ratio presents a considerable puzzle, and makes it difficult to justify car seat laws by placing a huge value on children's lives. Children's lives are now on both sides of the ledger, yet the cost side is two orders of magnitude greater. If current policies reflect representative preferences, at a societal level the ratio is analogous to the Willingness to Accept / Willingness to Pay (WTA/WTP) gap.[4] People's acceptable price to acquire a good they don't yet own is generally lower than their price to part with a good already in their possession, a phenomenon known as the endowment effect. Kahneman, Knetsch and Thaler (1990) survey the literature and find a range of WTA/WTP ratios from 1.4 to 16.5, making our ratio puzzling even in this context. The divergence is likely related to the fact that children are highly non-substitutable for their parents (Hanemann 1990). But even here, the simplest prediction of a very high WTA is that parents should use car seats of their own accord, without a need for government mandates.[5]

[4] See Plott and Zeiler (2005), Shogren, Shin Hayes and Kliebenstein (1994), Hanemann (1991), Kahneman, Knetsch and Thaler (1990), and Coursey, Hovis and Schulze (1987), among many others.

[5] In other settings, workers in some contexts display an extraordinarily *low* willingness to pay to reduce death risk, at least for themselves. Thaler and Rosen (1976) document that the average worker in risky occupations appears willing to sacrifice only $176 per year to reduce the risk of death by 0.001.

An alternative interpretation is that policymakers do not understand the magnitude of the tradeoffs involved, and either overestimate the importance of safety seats on car crash fatalities, and/or underestimate the effects on fertility. The latter seems quite likely, given how infrequently the issue is discussed. There is also a large difference in salience between the dramatic event of a small child dying in a car crash, versus the largely unseen effect of a family who wanted another child deciding that the cost is too high. From a public choice perspective, regulatory agencies like the National Highway Traffic Safety Administration (NHTSA) are tasked with reducing the number of car crash fatalities, but no equivalent agencies exist to increase birth rates.

This paper fits into two strands of literature. First, we contribute to the literature on the determinants of fertility, particularly the large secular decline in fertility rates (Becker 1960, Adsera 2005, Goldin and Katz 2002, Bailey 2010, Black, Devereux and Salvanes 2008, McCrary and Royer 2011). Closest to the current paper is Hacamo (2020), who documents how mortgage market deregulation lead to more births via greater access to housing. Our paper documents a new driver of fertility decisions, and provides well-identified evidence on its magnitude. Second, we contribute to the literature showing the unintended effects of government mandates, in areas as varied as employment of disabled people (Acemoglu and Angrist 2001), wildlife protection (Lueck and Michael 2003), traffic safety (Peltzman 1975), pollution (Davis 2008), healthcare (Jacobson, Chang, Newhouse and Earle 2017), education (Peltzman 1973) and others. The current tradeoff is particularly perverse, given the sheer magnitude by which the unintended consequences exceed the intended consequences.

2. Data and Sample Selection

Our empirical tests rely on the union of two data sources: state-level legislative actions mandating the use of child seats, and granular data on fertility rates and family formation through time and across geographic areas. For subsequent analysis, we also use data on car crash fatalities.

2.1. Child Seat Laws

We obtain state car seat laws using Lexis Nexis StateCapital and NexisUni.[6] We take the current version of the law, and trace the history of the amendments, augmenting these databases when necessary with a combination of the HeinOnline State Sessions Laws database and manual searches of online state legislative archives. We read each amendment, and record the age at which children can ride unrestricted regardless of any other criteria. We also collect the age at which a child may ride in a seat belt if he or she meets certain height and/or weight requirements (if applicable). We combine these with data on children's height and weight distributions by age and year from the Center for Disease Control. We then identify the age at which point 50% of children can ride unrestrained using a seatbelt.[7]

2.2. Car Crash Data

Data on car crashes come from the Fatality Analysis Reporting System, from 1975 to 2018. This database contains information from vehicle crashes in which there is at least one fatality, and includes information on vehicle and passenger characteristics.

2.3 Fertility

[6] Following Bae et al. (2014), who conduct a similar literature search, we search for the phrase "(child! OR infant! OR baby OR youth) w/20 (restrain! OR seat! OR belt! OR booster OR passenger)"

[7] In practice, height and weight exceptions do not end up affecting large fractions of children, and our results are largely unchanged if we just use the explicit age restrictions

Our data on birth rates come from a compilation of assorted U.S. Census Bureau data products, aggregated and standardized by IPUMS USA, a service of the Minnesota Population Center. More specifically, our primary analysis is based on yearly vintages of the American Community Survey (ACS) conducted from 2000 through 2017, and 5% random samples from the 1990 and 2000 decennial census.[8]

The result is a dataset comprised of repeated cross-sectional snapshots of U.S. households taken at different points in time. For each cross-section, we are able to observe key characteristics of all surveyed households, including the age and sex of the household head, all other adults (including spouses), and all children present in the household. Moreover, each survey includes other relevant characteristics, such as race/ethnicity, household income, car ownership, and geographic location (generally at the county level, though at the state level in earlier vintages).

2.4. Sample Selection

Given the research questions we study, the ideal dataset would be a longitudinal panel following a large cross-section of women in the U.S. throughout their range of fertile years and as they move locations, adding new cohorts of women as old cohorts exit the sample. While we do not have access to such a dataset, we can construct a panel with many similar features from our cross-sectional snapshots. Our basic strategy is to use cross-sectional information at the time of the survey on numbers and ages of children to infer the ages and birth decisions made in prior years. We describe this process briefly here, and give full details in the Internet Appendix.

We are interested in the decision of each woman to give birth, rather than each household. We exclude group homes, households with inmates or children-in-law, and households with no

[8] We are unable to use the 2010 decennial census, which omits key fields needed to infer family structure (e.g., number of children for each woman).

adult woman present (as households with only an adult male and children will lack information on the mother's age). If a household has multiple women over 18 years of age, we split them into separate observations, assigning children to the corresponding mother, and noting the presence of any adult males. This unit, which we term a "household", thus differs from the census definition.

We then take information from the survey year *t* for the age of the woman and any children, and infer ages and birth events for all prior years from *t*-1 back to the year in which the adult-age woman was 18 years of age.[9] In doing so, we are only able to infer birth events for children that remain in the household until the time the snapshot is recorded. This method would fail to accurately count the number of children, for instance, of a woman surveyed at age 40 who had given birth to a child when she was 21, if the child (who would be 19 in the survey year) has since left the household. To ensure that we have a complete snapshot, in our main tests we limit the sample to women who are 35 years or younger in the survey year. For this group, we will accurately count their number of children as long as they gave birth at 18 or older, and their children do not leave home before age 18. Because this sample fails to include birth decisions of older women, in some tests we extend the sample to include women aged 40 and below at the time of the survey. As birth rates have been going up over time for women aged 36 and above, extending the sample allows us to capture a wider range of women who end up having three or more children, rather than just those who started at younger ages.

We merge this panel of yearly household ages and birth events to the state-year level car seat laws at the time the decision to conceive would have been made, yielding our final sample. We implicitly assume that a household has not moved across states from when the woman was

[9] Note, we generically refer to each ACS survey (e.g., 2001 ACS) as being taken at the end of the vintage year (e.g., 2001) for simplicity, whereas the survey is conducted throughout the year.

adult-age until the year of the snapshot. If households move states, this assumption will introduce measurement error and likely attenuate our results, biasing against finding a statistically significant effect. It is unclear how this assumption would bias us towards finding a result unless households strategically move across state lines in response to changing car seat laws, which seems unlikely. One factor militating against selective migration driving our results is that the overall cost of moving states (in both money and hassle) seems from casual observation to be higher than the cost of upgrading to a bigger car, so families who are unable to afford the latter are not obviously likely to be able to afford the former either.

2.5. Final Sample

We construct our final panel from the ACS and decennial census cross-sections. Because we restrict the sample to women 35 years of age or younger on the snapshot date, prior to the earliest ACS snapshot year (2000) the maximum observable female age in the panel decreases as the panel extends backwards in time. Intuitively, the sample will exclude a woman who is 33 years old in 1997, as she will be 36 years old in the earliest snapshot in 2000. In this sense, when examining birth years before 2000, there is a tradeoff between using variation over a longer time period, versus having the whole cross section of all female ages in the year in question. In our main estimates, we examine the results for the full sample, but in robustness checks we also truncate our final panel to begin in 2000, so each panel year contains a full cross-section of female ages.

Extending each snapshot backwards in time results in overlapping samples, with more overlap in earlier panel years. For example, while only one snapshot (2017) is used to create the 2017 panel year, the 2014 panel year is generated from four snapshots (2014-2017). To ensure that we do not underweight fertility decisions in more recent years, and to better estimate the effect of car seat laws on the overall population of 18- to 35-year-old women, we re-weight observations in our empirical tests by the inverse of the number of observations for that combination of panel year and female age. This produces equal weights for all female ages and panel years, offsetting the unevenness in the original data. More details are provided in the Internet Appendix.

Table 1 describes our final sample, reflecting this re-weighting. Panel A presents weighted summary statistics for the survey year itself, and Panel B presents summary statistics for the resulting panel of household-year observations. The average woman in our sample is slightly over 29 years of age at the time of the survey. 77% of households surveyed have access to a car and 87% have at least one employed individual, when jointly considering the response of both the woman and any listed male spouse/partner. The average household has 0.96 children in the survey year. Finally, the average state-mandated children's age at which car seat laws apply to households in our sample is 6.8 years.

In Panel B, the final panel has approximately 69.7M household-year observations with an average adult woman age of 25.3. The average woman in our sample has an annual probability of giving birth of 8.4%. In comparison, a back of the envelope estimate of publicly reported fertility rates for women aged 18 through 35 in 2010 is 9.1%.[10] One contributing factor to our lower estimated birth rate is the inability to observe births in which the child was subsequently separated from the mother (e.g., through adoption, divorce with fraternal custody, etc.). However, it is unlikely that this lower estimated birth rate poses a significant challenge to our empirical strategy, which is largely based on both state and year variation in car seat laws, and variation in the number and ages of children across households in a given state.

[10] To arrive at this estimate, we begin with estimated birth rates published in Table 3 of CDC REPORT, available at https://bit.ly/2UPbXMu. We then equal-weight across all female ages from 18 to 35.

Figure 1 reports the average estimated age at which a child in our sample is no longer required to use a car seat by year, along with the 10^{th} and 90^{th} percentiles. The figure depicts the introduction of the first laws in the early 1980s, a set of original laws introduced in lagging states between 1995 and 1998, before a steady increase in the stringency of laws from 2002 to 2013.

3. Results

3.1 Methodology

We use a panel regression setting where the main dependent variable is a dummy variable equal to one if the woman in question gave birth that year. Our main independent variable of interest is *Two Children, Both Bound*. This takes a value of one if the woman already has exactly two children whose age and state-year mandate requires they use child safety seats. Our main prediction is that physical limitations of many cars precludes the use of three car seats in the back seat, and thus women will be particularly less likely to give birth in years when they already have exactly two children, both mandated to be in a car seat, unless willing to purchase a larger vehicle.

To distinguish the effect of car seat laws from other factors that will affect birth rates, our specifications employ numerous controls, fixed effects and interaction terms. We include *County-Year-Number of Children* fixed effects to control for the overall fertility in a county and year, allowing it to vary by the number of children a woman has. This variation is at the state-year level in the 2000-2004 ACS surveys (which lack county information). This controls for variation in overall birthrates due to many economic and demographic factors, and allows for changes in overall desired family size in that county and year.[11] This is important for distinguishing the effect of car seat laws from the impact of other secular changes that might affect birth rates in general,

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[11] Year in this case, and all others, refers to the time index of the panel. In contrast, *Vintage Year* refers to the year the data was collected.

or third child birth probabilities specifically. Next, we include dummy variables for every combination of children's ages. In other words, we also estimate a general probability of giving birth for each combination of children's ages, such that the effect of car seat laws is measured relative to other families with the same number and ages of children. This allows us to measure the effect of car seat laws relative to the overall patterns in the spacing of children's births, while putting very few structural assumptions on the shape of such birth patterns. These form the minimum set of controls against which we distinguish car seat laws.

We then include a range of demographic variables, such as a vector of yearly values of a woman's age, and dummies for each combination of race interacted with a dummy for Hispanic ethnicity (e.g. Hispanic white, non-Hispanic white, etc.), abbreviated as *Race* for simplicity. Finally, in some specifications we include time-varying variables which may affect birth rates, but which we only observe on the survey date. As such variables may potentially be impacted by the history of birth decisions, we include them separately. These are quintiles of household income within a survey vintage, the highest education level among household members, and the presence of a male in the household. In later specifications, we allow each demographic control to vary by county or year, by interacting each variable with a vector of dummies for each county (*County*[Variable]*), panel year (*Year*[Variable]*), or both.

The ability to distinguish the effects of car seats from a wide variety of potential drivers of birth rates stems from both the specific predictions of car seat laws on third child births at particular ages, and state- and year-level variation in the minimum age at which children can wear seat belts. While there is a large secular increase in minimum seatbelt age, the specific changes in states and years around this trend do not show any clear patterns. Concerns that passage of these law changes may be related to economic or demographic events is also unlikely to drive our results. Any event that affects birth rates in general for that county and year, or even birth rates for third children for that county and year, is absorbed by granular *County-Year-Number of Children* fixed effects.

3.2 Baseline Effect of Car Seat Laws on Fertility

The baseline results are presented in Table 2, which considers the effect of *Two Children, Both Bound* on birth decisions. Standard errors are double clustered by state and year. Panel A uses our main sample. Column 1 includes only variables related to family size: county-year-number of children fixed effects, as well as fixed effects for each combination of children's ages. Women with exactly two children both under the state-year car seat mandate age have a probability of giving birth that is lower by 0.422 percentage points (*t*-statistic of -2.79). In terms of economic magnitude, by way of comparison over the whole sample (years from 1973 to 2017), the likelihood of giving birth in a given year for women that have exactly two children is 12.14% for 25 year olds, 8.55% for 30 year olds, and 5.20% for 35 year olds. Column 2 adds controls for the woman's age and race, and the effect increases to -0.554, with a *t*-statistic of -4.09. Column 3 adds ex-post demographic controls measured in the survey year, namely household income, education, and male type (i.e. husband, permanent partner / other adult male, or none). The effect increases slightly to -0.620, with a *t*-statistic of -4.80. Columns 4 through 6 allow all demographic controls to vary by year, county, and both county and year respectively. The effect increases in the full controls specification to -0.732, with a *t*-statistic of -5.69.

Panel B examines the effect of different samples along several dimension. Firstly, we extend the sample to include women of older ages at the time of the survey: 36 and below (column 1), 38 and below (column 2) and 40 and below (column 3). In each case, we are able to examine birthing decisions for a wider range of women, and ensure that our effects are not somehow being driven by truncating the sample at 35 years and below. The tradeoff is that for women between ages 36 and 40, there will be more measurement error in the total number of children they have, due to the possibility that they gave birth at younger ages (e.g. 18-20) but their now-adult children are no longer living at home. In any case, the results (analogous to Panel A column 6) are similar with or without the inclusion of women older than 35. The coefficients on *Two Children, Both Bound* for samples of women 36 and under, 38 and under, and 40 and under are, respectively -0.756, -0.699 and -0.688, with all *t*-statistics greater than -5.9.

Next, we consider the effect of different time periods. In the base specification in Panel A, we exclude decennial census observations from the year of the census itself, since the April survey date will only capture birth decisions for a minority of the months of that year. In column 4, we include such partial year observations, and find that the results are similar, with a coefficient of -0.688 and a *t*-statistic of -5.29. Finally, column 5 includes only observations from the 2000 ACS onwards. This sample ensures that at every date we have the full sample of female ages from 18-35, albeit at the cost of examining a considering shorter time period. The effect of *Two Children, Both Bound* is -0.476, with a *t*-statistic of -3.42.

3.4 Other Birth Margins

The estimates in our base specification in Table 2 control for a large number of other potential drivers of birth rates. Nonetheless, to ensure that we are not simply proxying for other changing demographic trends that are somehow not being controlled for, we compare our base effects to other numbers and ages of children where we would not expect car seat laws to necessarily have an impact. Specifically, we introduce *Two Children, One Bound*, which captures cases where only one child out of two is required to be in a car seat. In this instance, the birth of a third child would result in two children in car seats and an older third child who would only need a seatbelt. We also examine *One Child, Bound*, which equals one when the woman has exactly one child total who is also required to be in a car seat. In both cases, if the particular constraint is fitting three car seats in the back, then we would not expect the laws to significantly alter birth rates in such cases.

We examine these effects in Table 3. Column 1 contrasts having one versus two children in car seats, including base controls at the state and year level. The coefficient on *Two Children, One Bound* is 0.184 (*t*-statistic of 1.27). The coefficient on *Two Children, Both Bound* is slightly smaller at -0.552 (*t*-statistic of -2.58), reflecting the change in base case, which no longer includes cases where one child is bound. In column 2, we examine the effect of having only one child total required to be in a car seat. The coefficient is -0.283, and statistically insignificant from zero with a *t*-statistic of -1.23. The coefficient on *Two Children, Both Bound* in this specification is very close to that in Table 2 Panel A.

Finally, we include another variable, *Two Bound*, which equals one when there are two children required to be in car seats, regardless of the total number of children. When our main variable *Two Children, Both Bound* is included in the same regression, *Two Bound* measures the marginal impact on birth probabilities among "three-plus" child households of two children being in car seats. Meanwhile, *Two Children, Both Bound* now measures the impact of two children being in car seats specifically at the two child total margin, over and above the base effect of all two-car-seat-age families. Econometrically, this ensures that our main result is not proxying for a broader effect of *Two Bound*, which might occur if there were some reason that two children being of car seat age reduced birth probabilities regardless of the total number of children in the family.

Columns 3 includes the *Two Bound* variable. The addition slightly increases the coefficient on *Two Children, Both Bound* to -0.986 (with a *t*-statistic of -7.71), which is now measuring the difference relative to the base *Two Bound* case. Moreover, the coefficient for *Two Bound* is positive at 0.254 with a *t*-statistic of 2.76. Adding together the two coefficients to get the total effect in for having two children that both bound gives a total of -0.732, very similar to before. This indicates that the particular reduction in births comes from exactly two children who are of car seat age, rather than being some general property of having two car seat aged children.

In order to evaluate the robustness of these other margins, in columns 4-6 we run the same specifications on the sample of all women aged 40 and below at the time of the survey. Particularly for the *Two Bound* variable, it is important to see how these effects vary if women of older ages are included. Such observations from women ages 36-40 are more likely to be important for understanding births of later children, as these necessarily occur at older female ages for any given woman than births of her earlier children.

Including women up to age 40 reduces both the magnitude and significance of all other car seat related coefficients, whereas the *Two Children, Both Bound* effect remains similarly large and significant. The coefficient on *Two Children, One Bound* is now 0.030 (with a *t*-statistic of 0.30), the coefficient on *One Child, Bound* is -0.142 (with a *t*-statistic of -0.61), and the coefficient on *Two Bound* is now 0.079 (with a *t*-statistic of 1.28). This reduction in statistical significance occurs notwithstanding the fact that there are almost twice as many observations. Overall, these results reinforce the conclusion that the margins where birth rates vary the most from similar family situations is precisely the case where back seat crowding predicts the largest impacts. Car seat laws have small and inconsistent effects at other birth margins.

3.4. Car Ownership, Adult Male Presence, Household Income, Urban Density

Next, we examine how the effect of these laws on fertility varies with demographic factors. The first of these is that the effects of car seat laws should be stronger for households that have access to a car. This prediction is relatively unambiguous compared with other demographic factors – regardless of wealth, households that do not own a car ought to be less affected by laws relating to car seats. It is also worth noting that car ownership is recorded at the year of the survey, so may not correctly measure ownership at the time of the birth decision.

We examine this prediction via an interaction term in Table 4. The specifications mirror Columns 6 from Panel A of Table 2, which allow demographic characteristics to vary by year and county. In Column 1, the coefficient on *No Car * Two Children, Both Bound* is 1.820 (*t*-statistic of 7.03), indicating that birth rate is significantly higher for women with two children of car seat age if they do not own a car, relative to similar women who have two car-seat-aged children and who own cars. The base effect of *Two Children, Both Bound* now measures the effect for the omitted group of women with a car in the household, and is somewhat larger at -1.005 (*t*-statistic of -7.60). In other words, the effect among car-owning households is greater than the effect for all households in Table 2, and more than 100% of the effect is offset for households that do not own a car. Because these regressions also include *No Car* on its own, as well as *No Car * Two Children,* these effects are distinct from the impact of car ownership on birth probabilities, or on birth rates conditional on having two children.

Next, we predict that the effect of having two children in car seats will be stronger when there is an adult male / parent present in the household. Households with only a single mother are more likely to be able to accommodate a third car-seat-aged child, by shifting an older child into the front seat.[12] We test this prediction in column 2 with the interaction *No Male Present * Two*

[12] Car seats for younger children, which tend to be larger, in general cannot be placed in the front seat of cars that have airbags, due to the risk of injury (although in modern cars, the airbag can often be turned off for this purpose). However, smaller booster seats for older children, which simply raise the child's height, do not pose the same danger. Like most questions in this sphere, internet searches on this question produce maximally dire warnings about not letting children ride in the front seat until age 13, and less guidance on lower bounds of acceptably safe

Children, Both Bound. The coefficient on the interaction term is 2.066 (*t*-statistic of 11.15), when including base controls (and the *No Male Present* dummy itself, as well as interacted with *Two Children)*. In this specification, the base effect of *Two Children, Both Bound* is -0.950, with a *t*-statistic of -6.99, again larger than the effect in Table 2. The absence of an adult male more than completely offsets the base effect. Like with car ownership, because the univariate *No Male Present* and the interaction *No Male Present * Two Children* are included, these estimates do not reflect differential births rates for single mothers, nor a different propensity to give birth conditional on having two children.

Next, we turn to other cross-sectional demographic effects. In particular, we examine how car seat laws interact with household income and urban density, for which our predictions are less straightforward. For these tests, we condition only on families that own a car, as this is the first order prediction of our channel, and cross-sectional demographic variables (e.g., income) may give distorted measures if they are correlated with car ownership. To capture cross-sectional variation in both wealth and urban density relative to other characteristics plausibly correlated with fertility decisions, we form deciles for each characteristic of interest within panel year, vintage, car ownership, and age of the woman.

In column 3, we find that the effect increases with household income. The coefficient on *Household Income * Two Children, Both Bound* is -0.162 (*t*-statistic of -5.43), where the coefficient represents the increase in the *Two Children, Both Bound* effect for each additional decile of household income. This result is perhaps surprising. One interpretation is that buying a larger car is not only a financial consideration. Rather, large cars like minivans also have certain

behavior. At a minimum, the lack of an additional adult gives the woman the *option* of accommodating the oldest child (who will be the binding restriction) in the front seat.

class and aesthetic connotations that may make people reluctant to switch, even when they can afford to. The result may also reflect an increased willingness among richer families to adhere to state mandates. In column 4, we find weak evidence that the effect of car seats laws increases with the county-level urban density of an area. This is seen in the coefficient on *Urban Density * Two Children, Both Bound* coefficient of -0.061 (*t*-statistic of -1.86). Because there is more ambiguity on the appropriate measure of both household income and urban density, in untabulated results we explore other ways of constructing percentiles, including across all years, within a state, excluding the conditioning on car ownership etc. Household income shows significantly increased effects when using a range of alternative measures, whereas urban density is insignificant in many of them. This may reflect the fact that urban density involves competing effects in different directions – for instance, rural counties have more space that makes parking and storing large cars easier, but also have fewer public transport alternatives to car ownership.

4. Estimated Number of Lives Prevented and Saved

4.1. Temporary versus Permanent Effects on Fertility

The results so far measure the contemporaneous effect of car seat mandates – that is, the reduction in birth rates when a woman's children are below a particular mandated age. However, children will eventually age out of the restrictions, at which point the threshold will cease to bind. Because we estimate only the effects of current year restrictions, it is plausible that some of the reduction in birth rates represents a shift in the *timing* of births, rather than a shift in the total number of births. In other words, some of the initial reduction may be offset by women simply delaying the birth of a third child until one of their children is not required to be in a car seat. Testing this idea is somewhat more complicated than it initially seems, as there are multiple ways a restriction can cease to be binding – the children could age out, but the woman could also purchase a larger car and have a third child anyway, at which point the marginal cost of the fourth child is actually lower than the third. We adopt two different strategies to incorporate any offsetting effect that the laws have on the intertemporal shifting of births. We first present results from a reduced-form approach, which we follow with results from simulations in the next subsection.

To estimate the long term effects in reduced-form, Table 5 switches from a panel setting to one which instead considers the future history of births for each woman in the year in which she gives birth to her second child. We take as a dependent variable a dummy variable for whether she gives birth to a third child by the end of the sample. In constructing our variable of interest, we use the ages of the eldest child in the year of the second birth, and the evolution of laws over that and subsequent years, to measure ex-post how many years both children would be required to be in car seats.[13] We consider both a continuous version of the number of years bound (Panel A), and dummy variables for each number of years bound (Panel B).

Switching from a panel to this framework forces us to reconsider the set of fixed effects used, as we no longer have as rich a state-space over which to identify fertility decisions. For instance, *County-Year-Number of Children* fixed effects are now equivalent to just *County-Year* fixed effects, since all observations are for women who just gave birth to their second child. Second, the set of all age combinations now reduces just to dummy variables for the age of the oldest child. Both of these are now also measured only at the point of the birth of the second child, rather than varying across subsequent years. On the other hand, other demographic variables are

[13] We have also run these regressions using only the state of laws at the time of the birth of the second child, thereby ignoring any later changes, and the results are similar. Because birth choices are likely revised at each point in time, with knowledge of law changes between the year of the second birth and that point, but not the path of future law changes, the information from law changes incorporated into birth decisions likely lies somewhere between these two cases.

now more stringent, as year and county interactions with demographic variables are implicitly year (or county) by two children by demographic variables.

We present these results in Table 5. In Panel A, we explain lifetime probabilities of having a third child (conditional on having at least two children) as a function of the remaining number of years the eldest child will be required to be in car seats. Column 1 includes *County-Year*, *Oldest Child Age*, and demographic controls. Each additional year of being bound reduces the lifetime probability of having a third child by 0.644 percentage points (*t*-statistic of -4.00). Column 2 adds ex-post demographic controls, and the effect increases somewhat to -0.723, with a *t*-statistic of -4.34. Columns 3-5 allow characteristics to vary by year, county, and both year and county respectively. The effect on the lifetime probability is reduced somewhat to -0.599 percentage points (t-statistic of -4.67) in the full specification.

The previous panel imposes a linear effect of each year of being bound with two children, which may not be appropriate if women have non-linear preferences over years spent raising children or abilities to delay child-birth. In Panel B, we replace the continuous variable with dummy variables for the remaining number of years the eldest child is bound at the birth of the second child. Consistent with our predictions, each additional year being bound monotonically reduces the lifetime probability of giving birth to a third child. The final specification indicates that being bound for six years, which is equivalent to having a two-year-old and a newborn in a state where the unrestricted age is eight years old (as is the case for most of the U.S. at present) reduces the lifetime probability of a third child by 3.612 percentage points (*t*-statistic of -4.28). To put this into perspective, the annual birth probability for a female with two children is 9.3%.

These results indicate a permanent effect of car seat laws on lifetime probabilities of giving birth. However, comparing these magnitudes to those in previous tables is somewhat complicated.

The main obstacle is that the change from a panel setting to a cross-section of one observation per second-child woman means that the fixed effects have somewhat different impacts and interpretations. In addition, while the numbers in earlier tables represent per-year changes in birth rates, Table 5 considers the effect on cumulative birth probabilities, but over a horizon that is not uniform across all observations (as women will be interviewed in the surveys at different ages). In light of this, and to consider a richer set of possible impacts of the law, we next turn to simulations based on panel data.

4.2 Counterfactual Simulations

Next we characterize the potential effect of counterfactual seat belt laws on overall fertility rates. We do this in a dynamic simulation approach which incorporates an adaption of our reduced-form panel data regressions that allows for intertemporal shifts in birth decisions. A chief advantage of this approach is the ability to characterize the effect of the state mandates on overall birth rates among *all* women, rather than just the effect on women for whom the laws currently bind. An import consideration taken into account by the simulations is the impact of a change in car seat laws on the distribution of other covariates, which in turn have follow-on effects.

For instance, intuition suggests that if mandates were counterfactually rolled back to age four for our whole sample, birth rates would increase relative to existing laws, with a larger effect in recent years which have more stringent laws. However, a consequence of this would be an increase in three-child families relative to two-child families, which may have additional effects on births. Suppose birth rates among women with three children have generally decreased over time relative to two child households for unspecified reasons beyond car seat laws. Then the increase in births due to the counterfactual law will be partially offset by the compositional shift in the share of two-child and three-child households. By using simulations, we are able to retain these other sources of variation in birth rates from the panel regression and their interaction with a counterfactual law change, thus estimating a richer set of total consequences of these laws. In addition, with only a mild assumption regarding the subset of unobservable fixed effects, we can also estimate the effect on births for a full cross-section of 18 to 35 year old women from 1980 to 2017, not just the age-year combinations that periodic census snapshots are able to capture.[14]

We begin by modifying our baseline model estimated in Table 2 to consider the two possible avenues through which state mandates may no longer impact a woman with two children, both bound by the law. The first way in which a woman who was previously in the *Two Children, Both Bound* category may drop out of this category is for her to give birth while both existing children are still mandated to be in car seats. In other words, she may pay the higher cost of having a third child while complying with the laws, such as by purchasing a larger vehicle, thereby reducing the marginal cost of subsequent children. The second way in which she drops out of the *Two Children, Both Bound* category is that at least one child "ages out" of the law. That is, the household still has two children, but the eldest child transitions from a car seat to a normal seatbelt. If a woman merely delays birth decisions due to car seat laws, the resulting increase in birth rates associated with a child aging out would partially offset the main effect in Table 2.

We modify our original panel regression specifications in two ways to account for these effects. First, to account for the effects of women who have additional children while being bound, when constructing our key variable *Two Children, Both Bound*, we now do not condition on the number of children in a household at time *t*. Instead, from the point in which the second child is born in a household, we assign *Two Children, Both Bound* to take on a value of one for each year

[14] Details regarding this assumption which involves the interpolation of some fixed effects, along with remaining simulation details, can be found in the Internet Appendix.

in which the eldest child is below the mandated age to ride without a safety seat. Prior to this change, having a third child while the first two children are in car seats would remove a woman from the *Two Children, Both Bound* category. Now, the explanatory variable is constructed as if the household continues to have two children. Thus, the estimated coefficient captures the joint effect of state mandates for households that continue to have two children, *and* any offsetting effect from households that have a third child while the first two are in car seats (for whom the marginal cost of additional children may now be lower). Next, we consider the case of children aging out of the laws. We introduce a second covariate, *wasBound*, equal to one in the panel year in which the new construction of *Two Children, Both Bound* transitions back to a value of zero, denoting the year in which state-mandated laws no longer apply to the eldest child. We allow for the effect to materialize over multiple years by including four additional lags in the panel regression.

With this, we re-estimate the final specification in Table 2 after introducing these two modifications. To gauge the impact of child safety seat laws across time and the lifecycle of a woman, we embed the point estimates (including fixed effects) into a dynamic simulation framework which generates a counter-factual panel under an alternate set of state-level laws.[15] We begin by considering each woman at the point in which she is 18 years old. Importantly, as the simulations do not incorporate actual birth outcomes, we are no longer constrained to women that are 35 years of age or younger at the point of being surveyed. Instead, we simply need the count of women that turn 18 in a given year for each characteristic combination (e.g., county, race, income, etc.). This allows us to consider a full cross-section of women from age 18 to 35 in each

[15] Note, we use the STATA package REGHDFE to estimate a model with many high-density fixed effects. To make the estimation tractable, the routine imposes regularization constraints on the fixed effects. Thus, caution should be taken when using point estimates of these fixed effects (which our simulations do). In light of this concern, in the Internet Appendix we perform a validation exercise for the use of fixed effect point estimates. The exercise suggests no bias is introduced in the context of our simulation approach.

year of our sample beginning in 1980. Next, using a counter-factual set of state-mandated car seat ages (e.g., a uniform two-year-old requirement across all states) we re-calculate all affected covariates. We then compute the probability of each woman giving birth using these covariates and the coefficient point estimates, and simulate one realization of births. Next, we advance one period and update all path-dependent covariates for each woman. Thus, all variables of interest (e.g. *Two Children, Both Bound*) will depend on simulated birth realizations which vary across iterations.[16] Moreover, the effect of simulated births is also reflected in fixed effects based on the number and age of children in a household at a point in time. We then simulate another set of births for the current period, roll forward one period, and repeat the previous steps for each woman until she reaches 35 years of age or the panel year reaches 2017, whichever occurs first. We repeat this exercise 500 times, yielding a counterfactual panel in each, from which we compute means.

The first exercise we undertake is aimed at quantifying the effect of the increase in mandated car seat age on average fertility rates across all women in our sample. To do this, we simulate the panel under a counter-factual where laws are uniformly set to different constant age limits across all states and years. Panel A of Figure 2 contrasts the average annual birth rate of all women under counterfactual uniform laws with equivalent birth rates from the true set of state-year laws (which became more stringent over the sample). As a result, a counterfactual uniform four-year-old mandate would be a decrease for most states in the latter half of our sample, whereas a counterfactual uniform eight-year-old mandate would be an increase for most states and years.

Figure 2 Panel A shows that the average woman in the sample would have a higher probability of giving birth of 0.012% per year for uniform mandates at age two, and 0.0096% for

[16] In order to consider the effects of counterfactual laws on a 35-year-old woman in 1980, we must simulate her birth decisions in all previous years. As we rely on some fixed effect estimates that vary by year, and our panel begins in 1980, we fix all prior years at their corresponding values as of 1980.

a four-year uniform mandate. The effect on birth rates is fairly similar within this range. This likely reflects the low likelihood that women immediately become pregnant after the birth of a child, resulting in relatively few women for which a two- or three-year-old car seat law would bind. Moreover, as the simulations incorporate child age combination fixed effects, it may also reflect differential follow-on effects for women with different permutations of children ages. As a consequence, increasing mandates from age two to four is relatively low cost in terms of births prevented. Moreover, the relatively modest increase compared to true births partially reflects the early part of our sample in which car seat laws were either absent or relatively lax. However, as mandates increase to age five and upwards, the estimated average birth probability decreases considerably. A uniform eight-year-old mandate would result in a lower annual birth probability per woman of -0.033%, and a uniform nine-year-old mandate would result in lower annual birth probabilities of -0.046%.

These estimates also allow one to compare the difference between various uniform age mandates. This is taken from the difference between two points, each which represents an effect relative to true laws. For instance, the difference between a uniform four-year-old mandate (0.0096% relative to existing laws) and a uniform eight-year-old mandate (-0.0325% relative effect) is a 0.042% per year difference in average birth rates.

These estimated annual effects can be turned into an approximate effect on total fertility rates by multiplying them by 18 (the number of years each woman is affected between ages 18 and 35). Reducing the mandate from eight years to four years corresponds to an increase in births of 0.757 percentage points, or 0.00757 more children per household. To put this into perspective, the standard deviation of year-to-year TFR changes in the U.S. is 0.033 from 2000 to 2017, suggesting an economically significant effect of the car seat laws on overall birth rates.

To give more economic content to these numbers, we also multiply the annual birth probabilities by the number of women affected, to estimate the change in the total number of births. We estimate that uniform age mandates of two, three and four years old if applied throughout the sample would have led the U.S. to have had 145,000, 143,000 and 117,000 more births, respectively. By contrast, seven- and eight-year-old mandates would have resulted in 235,000 fewer and 392,000 fewer births respectively. In other words, a switch from a uniform three year mandates to uniform eight year mandates would have been associated with 536,000 more births.

Another useful counterfactual is to estimate how many fewer births would have occurred if the heterogeneous laws in 2019 had been enacted across states in 1980 and not changed since. This would have resulted in 350,000 fewer births. This highlights the extent to which current laws (which are likely to proxy for ongoing effects) are considerably more detrimental than the historical average. Put differently, of the 145,000 total births estimated to have been prevented by the path of car seat laws since 1980, over 60% of these occurred in the ten years from 2008-2017, and over 90% occurred since 2000.

Panel B performs a similar exercise, but breaks out the difference in birth rates by year. This illustrates how each uniform age-limit would have compared in terms of annual birthrates relative to true laws. A uniform age mandate of four years old would have had no effect, if not negative, until 1999, as it would represent a small but uneven change from the law that existed at the time. However, by 2017, switching to a uniform four-year-old mandate would have had a large positive effect (0.029%). Conversely, an eight-year-old mandate would have had a large negative effect in 1980 (-0.048%), but a small effect in 2017 (-0.004%). Such estimates can similarly be transformed into numbers of births. For instance, a uniform four-year-old mandate in 2017 would have resulted in 7,700 more births in 2017, and a three-year-old mandate would have resulted in 8,000 more births.

We next consider potential heterogeneity in the effect across a woman's lifecycle. In general, probabilities of giving birth tend to exhibit a humped-shape pattern, whereas the probability of being able to conceive declines with age (see Balasch 2010 for a review), meaning that the effect of delaying childbirth by several years will likely vary by female age. To this end, we also examine the effect of different age mandates on women across different ages. Panel C of Figure 2 reports the difference in birth rates for each year of a woman's life, averaged across women of that age in all sample years. All estimates are reported relative to a baseline of uniform eight-year-old laws. First, consistent with Panel A, we see a monotonic increase in birth rates as laws are rolled back to younger age mandates. Second, the estimated difference in birth rates steadily rises with female age to approximately age 26 or 27 before tapering off. We see no difference in effects prior to age 22. This can be attributed to our simulation design, which does not accommodate multiple birth events (e.g., twins). Thus, age 22 is the first year in which a woman would not be bound by an age-four law but would be by a more restrictive mandate.

Finally, we contrast the simulation estimates against our reduced-form panel regressions to gauge the relative share of the effect that is permanent. This ratio will depend to a certain extent on which sample and specification is being considered, and does not easily lend itself to calculating standard errors. Moreover, in the spirit of the critique in Lucas (1976), we cannot rule out the possibility that a counterfactual policy might alter the implied effects of other covariates (such as the included fixed effects), yielding a different overall effect. Nonetheless, with these caveats in mind, the coefficient *Two Children, Both Bound* from the final analogous specification of Table 2 Panel A (-0.732) represents the contemporaneous effect of child seat laws. To estimate the implied difference in nominal births of a policy change, we compute the difference in weighted number of woman-year observations bound under true laws with the weighted woman-year observation count under a counterfactual policy, such as a uniform two-year-old mandate. Taking the product of this difference and the coefficient yields an estimated reduction of 209,000 births from 1980 to 2017, based on the contemporaneous effect of the laws. Next, we compare this with the total birth reduction of a two-year mandate estimated from the simulations above, modified slightly to include only those female age-year observations included in the Table 2 sample (e.g. excluding 34-year-olds in 1984). This gives an estimated permanent reduction of 147,000 births. Taking the ratio of these values suggests an estimated 71% permanent effect.

4.3 Effect of Car Seat Laws on Car Crash Fatalities

We now consider the other half of the car seat law tradeoff – how much do these laws reduce the number of children's car crash fatalities? Existing work on the intensive margin of using a car seat (e.g. Levitt 2008) finds minimal effects. However, these papers largely examine the impact of *using* a car seat, whereas the impact of *mandating* safety seats is plausibly even lower. Some fraction of people will use car seats for their children even without a government mandate – it seems unlikely that many people in 1978 were attempting to strap their newborn infant into a seatbelt, for instance. Secondly, without perfect law enforcement, these laws are unlikely to be universally followed. To make matters worse, if risk tolerance or poor judgment is positively correlated across decisions, a willingness to flout child car seat laws may be positively associated with breaking other laws that materially affect the chances of serious accidents, such as speeding, driving while intoxicated etc. In other words, the people for whom the use of a car seat may be more likely to matter at the margin (because their behavior puts them at a higher risk of a crash) are potentially the same people that are less likely to adhere to car seat laws.

To test this, we take the U.S. government's FARS database of car crash fatalities since 1975. Our dependent variable is the fatality rate in a state-year for children of that age. This is done by combining the number of fatalities in a state-year-child age triplet, with population estimates for the triplet from the CDC. We define the dependent variable as the death rate per 100,000 children. Our key independent variable a dummy takes a value of one if the state-year-child age is required to be in a car seat. The specifications include similar fixed effects as previous tests, when possible, to ensure an equivalence between how stringent our tests are for identifying the effect of car seats on births, versus identifying the effect on fatalities. While we do not have demographic controls, we are able to control for different combinations of state, year and age fixed effects. In columns 1-4, the sample includes death rates for all children 14 years and under (where they are very likely to be passengers, rather than drivers). Columns 5-8 restricts the sample to children 8 years and under, who are the ones primarily affected by the range of current laws.

Column 1 includes fixed effects for *State*, *Year* and *Age*. Column 2 includes *State-Year* and *Age*. Column 3 includes *State-Year* and *Age-Year*. Column 4 includes *State-Year*, *Age-Year*, and *State-Age*. We find that effects are generally small and statistically insignificant. The largest effect is with *State-Year* and *Age-Year* fixed effects, of a reduction of -0.350 deaths per 100,000 children (*t*-statistic of -1.90). However, controlling for *State-Age* fixed effects reduces this to -0.079 (*t*-statistic of -0.350). Limiting the sample to children 8 years of age or younger in columns 5-8 produces similar effects. The maximum decrease is now -0.426 deaths per 100,000 children in column 7 (*t*-statistic of -2.60), and adding *State-Age* fixed effects again reduces this considerably.

Panel B repeats the same exercise, but splits out the effect according to the child age in question. Overall, the most reliable effects are observed for restrictions on four-year-olds and five-year-olds, in terms of being consistently negative point estimates across specifications, and showing some marginally significant effects with the full set of controls. These estimates indicate a reduction of -0.695 deaths per 100,000 for four-year olds (*t*-statistic of -1.79), and a reduction of -0.762 deaths for five year olds (*t*-statistic of -1.84) (-0.598 deaths and -0.871 deaths respectively in column 8 with only 8 year olds and below included). Meanwhile, there are some ages (such as one year olds) that show significantly *positive* effects on death rates from car seat restrictions, an effect for which we do not have a clear explanation.

While the evidence for car seat laws reducing death rates is weak and inconsistent, our main interest is in the economic magnitude implied by the point estimates. We multiply the coefficients in Panel B by the total number of children who are restricted in all U.S. states in 2017 (the year with the most stringent laws in our sample). In the full specification of fixed effects (column 4 and 8), the number of children's lives saved nationwide is estimated to be 40 in column 4 and 57 in column 8. Under the most charitable interpretation where we take the maximum effects (i.e. column 3 and column 7) and treat all negative point estimates as being genuine but all positive point estimates as being zero, the total number of lives saved is estimated to be 122 and 140 respectively. To give context, it is worth noting how many children's car crash fatalities there are in the first place in the U.S. In 1978, the number of fatalities for children age 0-8 was 2,392. By 2017, this had declined 73% to 654 total. However, this is almost exactly the same as the 72% decline in fatalities for children ages 9-12 over the same period from 975 to 273 (who were largely unaffected by these laws). In other words, back of the envelope approximations are consistent with the small numbers we document.

Conclusion

We document a large and perverse effect whereby child car seat mandates have the unintended consequence of large reductions in birth rates. This effect is identified by combining state and year variation in mandates with the outsized cost of having a third child in a car seat. We estimate that these laws are currently preventing approximately 8,000 annual births, around 141 times greater than plausible estimates of the number of lives saved in car crashes.

A question left unanswered is why such policies have been adopted almost universally across states and grown steadily more stringent, even without Federal mandates (Bae et al. 2014). The answer that seems most compelling, but perhaps surprising under public choice economics, is that regulators are simply unaware of the magnitude (or maybe even the existence) of costs being imposed on family formation. At a minimum, the relative lack of public discussion of this tradeoff suggests that it may not be foremost in the minds of policymakers. It is nonetheless difficult to imagine the compelling social interest in existing policy arrangements.

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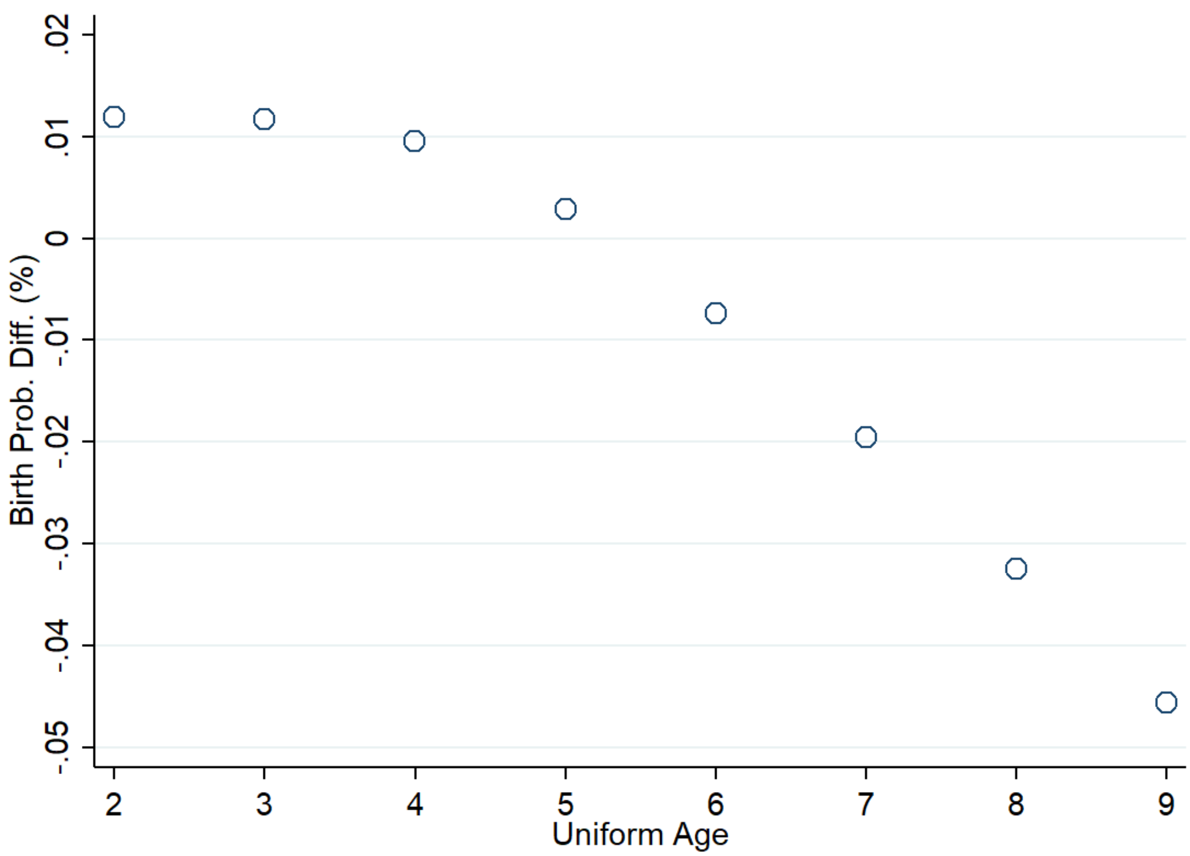
Figure 1 – Distribution of Child Car Seat Laws Over Time

This figure shows the distribution of child car safety seat laws in the U.S. since 1980. It presents the sample-weighted average minimum age at which a child can ride in just a seat belt, along with the 10th percentile and 90th percentile ages.

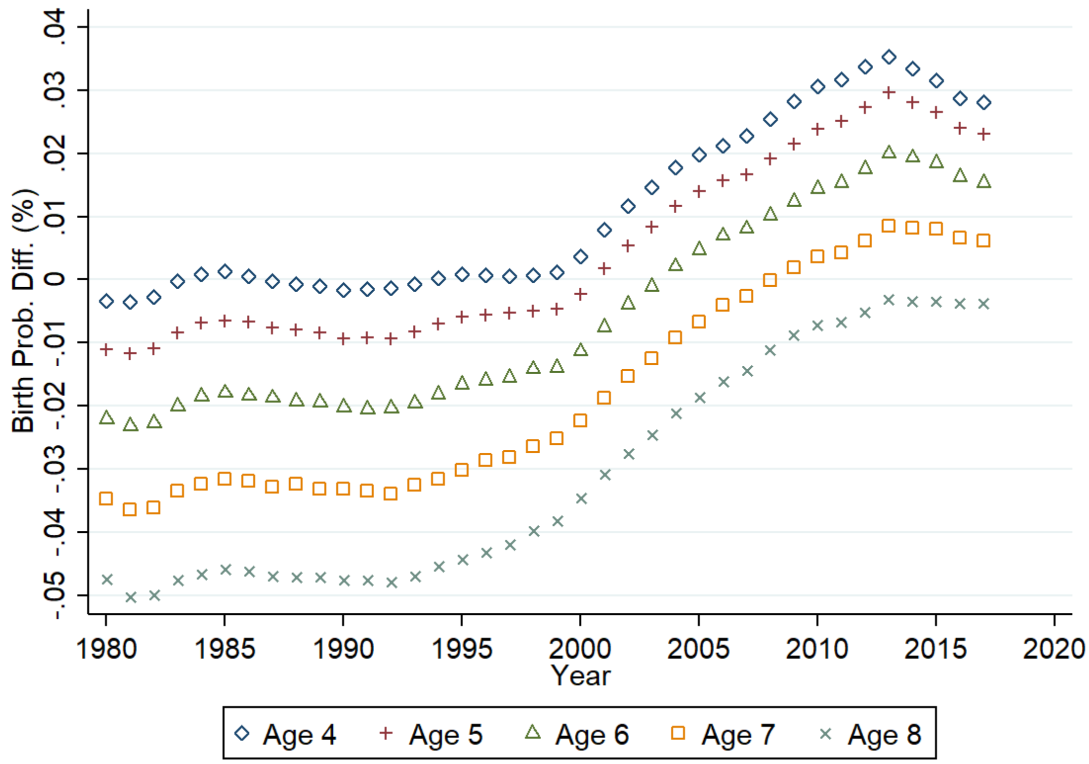
Figure 2 – Counterfactual Simulations of Birth Rates Under Lower Car Seat Age Mandates

This figure shows the change in annual birth rates for women ages 18-35 under simulations of counterfactual car seat law mandates. Baseline regressions are described in section 4.2, where a woman's probability of giving birth each year is regressed on variables relating to car seat laws (the number of children in total, and the number required to be in car seats), as well as county-year-number of children fixed effects, and demographic variables interacted with county and year fixed effects. Coefficients are used to simulate 500 draws of the data, under counterfactual legal mandates. In Panel A, we plot the average difference in birthrates for all women in all years relative to the distribution of births under actual historical laws. We construct different counterfactual legal mandates were from ages two to nine, if such mandates applied in all states and years. Panel B plots the effect on annual birth rates for the same levels of age mandate, split out by different sample years. Panel C plots the annual effect across all sample years for women of different ages.

Panel A – Effect of Different Age Mandates on Annual Birth Rates, All Ages and Years



Panel B – Annual Effect on Average Birth Rates by Year



Panel C - Annual Effect on Average Birth Rates by Female Age

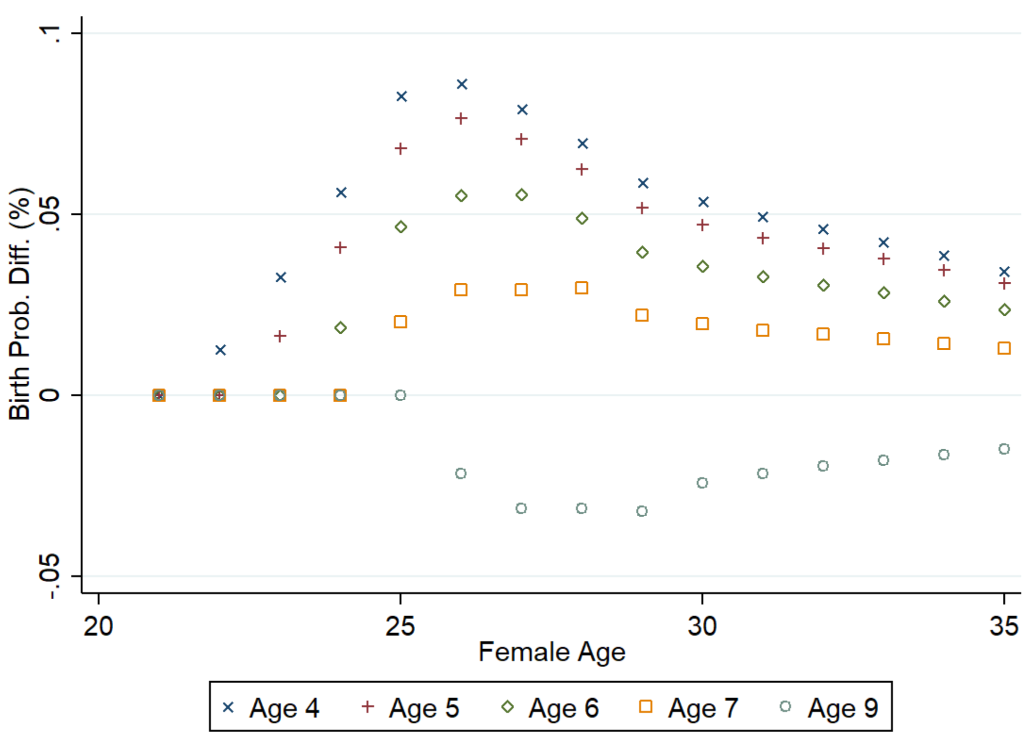


Table 1 – Summary Statistics

This table presents summary statistics for the main variables used in the paper. Panel A presents statistics for the underlying survey cross-sections, and Panel B presents statistics for the panel of annual observations constructed from the survey snapshots. "Age Threshold" refers to the minimum state-mandated age at which 50% of children can ride in a car with only a seat belt (based either on blanket age limitations, or combinations with height and/or weight). "White", "Black" and "Asian" refer only to non-Hispanic populations within each group. All other variables are from the ACS surveys.

Panel A - Cross-Sectional Observations

	N	Mean	S.D.	p10	p25	p75	p90
Female Age	3,959,887	29.64	5.065	22	26	34	35
Year	3,959,887	2,011.2529	5.149	2,003	2,007	2,013	2,016
Age Threshold	3,959,887	6.830	1.579	4	6	8	8
No. of Children	3,959,887	0.961	1.219	0	0	2	3
1(Car)	3,959,887	0.766	0.423	0	1	1	1
HH Income	3,959,887	78,737.562	74,416.325	16,200	34,000	62,000	100,000
1(Employed)	3,959,887	0.865	0.342	0	1	1	1
1(White)	3,959,887	0.646	0.478	0	0	1	1
1(Black)	3,959,887	0.102	0.303	0	0	0	0
1(Hispanic)	3,959,887	0.160	0.366	0	0	0	0
1(Asian)	3,959,887	0.0618	0.241	0	0	0	0
1(College)	3,959,887	0.409	0.492	0	0	0	1
1(High School)	3,959,887	0.951	0.216	1	1	1	1

Panel B - Final Panel

	N	Mean	S.D.	p10	p25	p75	p90
Female Age	69,726,094	25.30	4.91	19	21	29	32
Year	69,726,094	1998.47	11.75	1982	1988	2009	2014
1(Birth Year)	69,726,094	8.407	27.75	0	0	0	0
Age Threshold	69,726,094	4.253	2.72	0	2	6	8

Table 2 – Base Effect of Child Safety Seat Laws on Third Child Births

This Table examines the impact of child safety seat laws on the probability that a woman gives birth. Panel A presents results of OLS regressions on the primary sample, which considers annual observations for women aged 18-35 between 1973 and 2017, constructed using the 1990 and 2000 U.S. Census and the 2001-2017 ACS surveys. The dependent variable is a dummy equal to one if the woman gave birth in that year. The main independent variable, *Two Children, Both Bound*, is a dummy equal to one if the woman currently has two children, both of whom are required to use a child safety seat according to state laws in that year. *County-Year-#Children* are fixed effects for each combination of county, year and number of children. *Child Ages* are fixed effects for every combination of number and ages of each child. *Base Char. F.E* indicates fixed effects controls for the woman's age, race and Hispanic identity. *Ex-Post Char. F.E.* refers to education level, quintiles of household income, and the presence and marital status of an adult male in the household, all measured in the survey year. In each column for the fixed effects, "County" denotes the interaction of state fixed effects with each control, while "Year" denotes an interaction with year fixed effects. Panel B considers alternate samples: a) including women aged 36 and below, 38 and below, and 40 and below at the time of the survey (columns 1 -3), b) including the vintage year of the census surveys, and c) including only sample years from 2000 onwards, drawing on the 2000 census and 2001-2017 ACS. Reported *t*-statistic in parentheses are heteroscedasticity-robust and double-clustered by state and year. *, ** and *** indicate statistical significance at the 10%, 5% and 1% level, respectively.

Panel A - Baseline Effect of Car Seat Mandates on Third Child Births						
Dep. Variable: 1(Birth Year)	(1)	(2)	(3)	(4)	(5)	(6)
Two Children, Both Bound	-0.422***	-0.554***	-0.620***	-0.737***	-0.587***	-0.732***
	(-2.79)	(-4.09)	(-4.80)	(-5.44)	(-4.76)	(-5.69)
County-Year-#Children, Child Ages	Y	Y	Y	Y	Y	Y
Base Char. F.E.	N	Y	Y	Year	County	County, Year
Ex-Post Char. F.E.	N	N	Y	Year	County	County, Year
Observations	69,691,299	69,691,299	69,691,299	69,691,299	69,691,279	69,691,279
R-squared	0.045	0.048	0.063	0.066	0.065	0.068

Panel B - Different Samples					
Dep. Variable: 1(Birth Year)	<37 at Survey Time	<39 at Survey Time	<41 at Survey Time	Including Vintage Year	2000 ACS onwards
Two Children, Both Bound	-0.756***	-0.699***	-0.688***	-0.688***	-0.476***
	(-5.94)	(-5.99)	(-7.14)	(-5.29)	(-3.42)
County-Year-#Children, Child Ages	Y	Y	Y	Y	Y
Characteristics F.E.	County, Year	County, Year	County, Year	County, Year	County, Year
Observations	78,400,158	97,298,222	115,283,113	72,660,268	27,487,142
R-squared	0.066	0.063	0.061	0.068	0.076

Table 3 –Car Seat Restrictions at Other Birth Margins

This Table examines how the effect of child car seat laws on birth rates varies with the total number of children in the family, and the number affected by the laws. The dependent variable is a dummy equal to one if the woman gave birth in that year. The main independent variable, *Two Children, Both Bound*, is a dummy equal to one if the woman currently has two children, both of whom are required to use a child safety seat according to state laws in that year. *Two Children, One Bound* is similarly defined for two children total but only one required to be in a car seat, and *One Child, Bound* is for one child total who is also required to be in a car seat. *Two Bound* is a dummy equal to one when the woman has two children required to be in car seats (regardless of her total number of children). Details for all remaining covariates are listed in Table 2. Reported *t*-statistic in parentheses are heteroscedasticity-robust and double-clustered by state and year. *, ** and *** indicate statistical significance at the 10%, 5% and 1% level, respectively.

Dep. Variable: 1(Birth Year)	<36 Years Old	<36 Years Old	<36 Years Old	<41 Years Old	<41 Years Old	<41 Years Old
Two Children, Both Bound	-0.552**	-0.734***	-0.986***	-0.663***	-0.690***	-0.767***
	(-2.58)	(-5.67)	(-7.71)	(-4.20)	(-7.08)	(-8.19)
Two Children, One Bound	0.184			0.028		
	(1.27)			(0.27)		
One Child, Bound		-0.283			-0.142	
		(-1.23)			(-0.61)	
Two Bound			0.254***			0.079
			(2.76)			(1.28)
County-Year-#Children, Child Ages	Y	Y	Y	Y	Y	Y
Characteristics F.E.	County, Year	County, Year	County, Year	County, Year	County, Year	County, Year
Observations	69,691,279	69,691,279	69,691,279	115,283,113	115,283,113	115,283,113
R-squared	0.068	0.068	0.068	0.061	0.061	0.061

Table 4 – Cases Where Two Child Car Seat Restrictions Bind Stronger

This Table examines how the effect of child car seat laws on birth rates varies with car ownership, the presence of an adult male in the household, household income, and urban density. The dependent variable is a dummy equal to one if the woman gave birth in that year. The main independent variable, *Two Children, Both Bound*, is a dummy equal to one if the woman currently has two children, both of whom are required to use a child safety seat according to state laws in that year. *No Car* is a dummy equal to one if the household has no access to a car in the year the survey was conducted. *No Male in Household* is a dummy equal to one if there is no adult male present in the house in the year the survey was conducted. *Household Income* and *Urban Density* are deciles of household income and urban density within vintage, year and female age. Details for all remaining covariates are listed in Table 2. Reported *t*-statistic in parentheses are heteroscedasticity-robust and double-clustered by state and year. *, ** and *** indicate statistical significance at the 10%, 5% and 1% level, respectively.

Dep. Variable: 1(Birth Year)	(1)	(2)	(3)	(4)
Two Children, Both Bound	-1.005***	-0.950***	0.802***	-0.430**
	(-7.60)	(-6.99)	(3.41)	(-2.02)
No Car * Two Children, Both Bound	1.820***			
	(7.03)			
No Car	-0.124*			
	(-1.80)			
No Car * Two Children	3.038***			
	(20.17)			
No Male in Household * Two Children, Both Bound		2.066***		
		(11.15)		
No Male in Household * Two Children		4.450***		
		(24.78)		
Household Income * Two Children, Both Bound			-0.289***	
			(-7.64)	
Household Income			-0.162***	
			(-5.43)	
Household Income * Two Children			-0.114***	
			(-3.35)	
Urban Density * Two Children, Both Bound				-0.061*
				(-1.87)
Urban Density				-0.016
				(-0.16)
Urban Density * Two Children				-0.337
				(-1.63)
County-Year-#Children, Child Ages	Y	Y	Y	Y
Characteristics F.E.	County, Year	County, Year	County, Year	County, Year
Observations	69,691,279	69,691,279	55,049,475	55,049,475
R-squared	0.068	0.069	0.067	0.067

Table 5 – Lifetime Effects of Car Seat Laws on Child Birth

This Table examines how child car seat laws effect the lifetime probability of a woman giving birth to three or more children, when conditioning on giving birth to two children. The dependent variable is a dummy variable equal to one if the woman later gave birth to a third child, and zero otherwise. In Panel A the independent variable is Years Bound, the total number of years starting from the second birth year that the woman will have two children required to be in car seats. This variable incorporates subsequent law changes after the birth year. In Panel B the independent variable is a vector of dummy variables corresponding to the ordinal values of Years Bound (rounded down when the number is less than a whole year). Lifetime Base Char. F.E indicates fixed effects controls for female age, race and Hispanic identity. Details for all remaining covariates are listed in Table 2. Reported *t*-statistic in parentheses are heteroscedasticity-robust and double-clustered by state and year. *, ** and *** indicate statistical significance at the 10%, 5% and 1% level, respectively.

Dep. Variable: 1(3+ Lifetime Births)	(1)	(2)	(3)	(4)	(5)
Number of Years Bound at Second Birth	-0.644***	-0.723***	-0.750***	-0.595***	-0.599***
	(-4.00)	(-4.34)	(-4.85)	(-4.50)	(-4.67)
County-Year F.E., 2nd Child Age F.E.	Y	Y	Y	Y	Y
Lifetime Base Char. F.E.	Y	Y	Year	County	County, Year
Ex-Post Char. F.E.	N	Y	Year	County	County, Year
Observations	1,671,971	1,671,971	1,670,317	1,671,930	1,670,261
R-squared	0.177	0.184	0.198	0.195	0.209

Panel B - Probability of Ever Having Third Child and Discrete Restriction Measure					
Dep. Variable: 1(3+ Lifetime Births)	(1)	(2)	(3)	(4)	(5)
One Year Bound At Second Birth	-0.279	-0.338	-0.351	-0.685**	-0.657**
	(-1.00)	(-1.23)	(-1.40)	(-2.61)	(-2.56)
Two Years Bound At Second Birth	-0.322	-0.448	-0.465	-0.826**	-0.778**
	(-0.91)	(-1.27)	(-1.43)	(-2.53)	(-2.37)
Three Years Bound At Second Birth	-1.390***	-1.552***	-1.481***	-1.844***	-1.700***
	(-2.99)	(-3.34)	(-3.47)	(-4.36)	(-4.18)
Four Years Bound At Second Birth	-2.218***	-2.479***	-2.499***	-2.472***	-2.405***
	(-3.63)	(-4.01)	(-4.45)	(-4.47)	(-4.68)
Five Years Bound At Second Birth	-2.562***	-3.083***	-3.273***	-2.765***	-2.839***
	(-3.22)	(-3.72)	(-4.32)	(-3.88)	(-4.13)
Six Years Bound At Second Birth	-4.480***	-4.830***	-5.024***	-3.558***	-3.612***
	(-3.92)	(-4.20)	(-4.77)	(-4.16)	(-4.28)
Seven Years Bound At Second Birth	-5.734***	-6.254***	-6.282***	-5.609***	-5.546***
	(-4.78)	(-5.11)	(-5.40)	(-5.64)	(-5.59)
Eight Years Bound At Second Birth	-13.860***	-15.134***	-15.903***	-14.399***	-14.834***
	(-4.40)	(-4.83)	(-4.99)	(-4.96)	(-5.00)
County-Year F.E., 2nd Child Age F.E.	Y	Y	Y	Y	Y
Lifetime Base Char. F.E.	Y	Y	Year	County	County, Year
Ex-Post Char. F.E.	N	Y	Year	County	County, Year
Observations	1,671,971	1,671,971	1,670,317	1,671,930	1,670,261
R-squared	0.177	0.184	0.198	0.195	0.209

Table 6 –Car Seat Laws and Children's Car Crash Fatality Rates

This Table examines how child car seat laws effect the number of children's fatalities in car accidents each year from 1975 to 2018. The dependent variable is the number of deaths per 100,000 children, and the unit of observation is child age-state-year. In Panel A, the primary independent variable is *Restricted*, a dummy equal to one state laws mandate that child age-state-year be restrained in a car safety seat. Panel B interacts *Restricted* with a vector of child ages. Reported *t*-statistic in parentheses are heteroscedasticity-robust and double-clustered by state and year. *, ** and *** indicate statistical significance at the 10%, 5% and 1% level, respectively.

Panel A - Continuous Restriction Measure								
Dep. Var.: Yearly Car Fataility (per 100k)	Children 14 and Under				Children 8 and Under			
Children of that Age Bound	-0.041	-0.070	-0.350*	-0.079	-0.047	-0.116	-0.426**	-0.144
	(-0.30)	(-0.49)	(-1.90)	(-0.35)	(-0.31)	(-0.82)	(-2.60)	(-0.69)
Age F.E.	Y	Y	Year	Year, State	Y	Y	Year	Year, State
State F.E.	Y	N	N	N	Y	N	N	N
Year F.E.	Y	N	N	N	Y	N	N	N
State-Year F.E.	N	Y	Y	Y	N	Y	Y	Y
Observations	32,895	32,895	32,895	32,895	19,737	19,737	19,737	19,737
R-squared	0.283	0.345	0.366	0.395	0.268	0.360	0.377	0.401

Panel B - Discrete Restriction Measure								
Dep. Var.: Yearly Car Fatality (per 100k)	Children 14 and Under				Children 8 and Under			
Zero Year Olds Bound	0.625	0.571	-0.342	0.148	0.857**	0.785*	-0.429	0.108
	(1.64)	(1.42)	(-0.79)	(0.32)	(2.19)	(2.00)	(-1.22)	(0.31)
One Year Olds Bound	0.902***	0.900***	-0.104	0.306	1.059***	1.012***	-0.292	0.169
	(3.19)	(3.12)	(-0.20)	(0.59)	(3.16)	(2.97)	(-0.53)	(0.30)
Two Year Olds Bound	-0.776**	-0.791**	-0.671*	0.146	-0.639**	-0.695**	-0.776**	0.056
	(-2.49)	(-2.44)	(-1.89)	(0.33)	(-2.10)	(-2.21)	(-2.29)	(0.13)
Three Year Olds Bound	-0.432**	-0.475**	-0.480	-0.116	-0.330*	-0.398***	-0.429	-0.136
	(-2.20)	(-2.30)	(-1.53)	(-0.32)	(-2.01)	(-2.79)	(-1.67)	(-0.45)
Four Year Olds Bound	-0.145	-0.104	-1.015**	-0.695*	-0.263	-0.230	-0.944***	-0.598
	(-0.53)	(-0.38)	(-2.51)	(-1.79)	(-1.05)	(-1.01)	(-2.94)	(-1.67)
Five Year Olds Bound	-0.208	-0.239	-0.430**	-0.762*	-0.415**	-0.501***	-0.611**	-0.871*
	(-1.04)	(-1.24)	(-2.02)	(-1.84)	(-2.33)	(-2.92)	(-2.04)	(-1.86)
Six Year Olds Bound	-0.225	-0.375**	0.115	0.067	-0.456***	-0.695***	-0.029	-0.047
	(-1.56)	(-2.28)	(0.89)	(0.21)	(-2.99)	(-4.23)	(-0.27)	(-0.15)
Seven Year Olds Bound	0.223	0.095	0.129	-0.107	-0.032	-0.234	0.001	-0.158
	(1.48)	(0.81)	(0.45)	(-0.37)	(-0.17)	(-1.53)	(0.00)	(-0.50)
Eight Year Olds Bound	-0.972	0.202	0.169	1.088	-1.064	-0.174	0.022	0.776
	(-0.68)	(0.15)	(0.12)	(0.68)	(-0.73)	(-0.12)	(0.01)	(0.46)
Age F.E.	Y	Y	Year	Year, State	Y	Y	Year	Year, State
State F.E.	Y	N	N	N	Y	N	N	N
Year F.E.	Y	N	N	N	Y	N	N	N
State-Year F.E.	N	Y	Y	Y	N	Y	Y	Y
Observations	32,895	32,895	32,895	32,895	19,737	19,737	19,737	19,737
R-squared	0.284	0.346	0.366	0.395	0.271	0.362	0.378	0.401