An Unintended Effect of School Entrance Age: Pushing Children Ahead through Private School

Elisa Taveras*
Binghamton University

November 10, 2021
Current draft available here

Abstract

Does a child's birth date affect his or her probability of attending a private school? In the United States, most children must be five years old by September to start public kindergarten. An alternative option is to attend private schools, which are not obliged to comply with states' cutoffs. To explore this, I look at the effect of children's quarter of birth on their probability of attending private school by grade (pre-kindergarten through 12th grade). Using the American Community Survey, I find that children born in July–September and October–December are more likely to attend private kindergarten than children born between April and June. The effect does not persist at higher grades. These findings indicate that, when limited by the entrance age cutoff, parents use private schools to bypass the restriction, giving their children a head start on schooling, and later transfer them to public school as they progress through K–12.

Keywords: Private School, K–12, Date of Birth, School Starting Age.

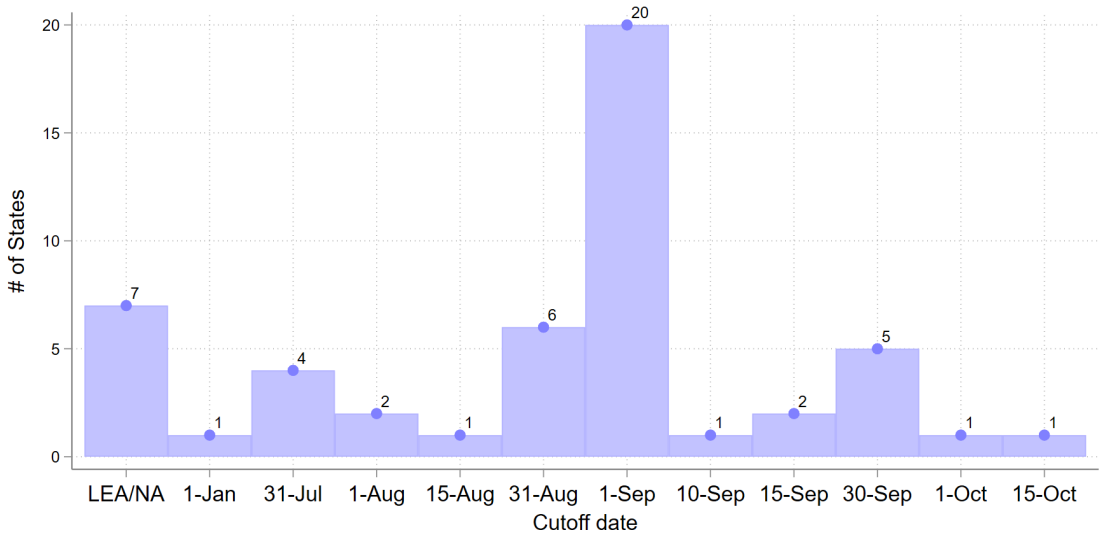
JEL Classification Codes: I20, I21, J01, J13.

*Email: etavera2@binghamton.edu. I am grateful for comments from Solomon W. Polachek, David Slichter, Alfonso Flores-Lagunes, and participants in the following conferences: Graduate Students in Economics of Education Zoom (GEEZ) Seminars, Association for Education Finance and Policy (AEFP) 46th Annual Conference, and Western Economic Association International (WEAI) 96th Annual Conference. All errors are my own.

1 Introduction

This paper asks whether children's date of birth affects their probability of attending a private school. For kindergarten-age children, the entrance age cutoff determines whether they can attend kindergarten in a given year. In the US,[1] kindergarten entrance age is five years by the cutoff date, which varies by state (Figure 1). The majority of the cutoffs are by September, and the most common cutoff date is September 1.[2] Given this deadline, children born after September must wait until the following year to attend kindergarten.

Figure 1: Distribution by Kindergarten Entrance Age Cutoff Date



Note. This graph presents the number of states and District of Columbia (DC) by cutoff date based on the information on National Center of Education Statistics (NCES), State requirements for Kindergarten: https://nces.ed.gov/programs/statereform/tab1_3-2020.asp; LEA/NA are those states that Local Education Agencies (5) can decide school entrance age rules or states with not applicable cutoff (2)

Because private schools are not required to comply with state cutoffs, parents who feel that their "late-born" children are ready to start school might choose to take this route instead:

> *"... My daughter was born a few days after our state's August 1st kindergarten cutoff date. If we followed state regulations, she would be 6-years-old when she started kindergarten... I knew she was ready for kindergarten. I wanted to push her ahead... we sent her to a private school that accepted young kindergarteners... "*[3]

In this paper, I study whether children born later in the year are more likely to attend private school. Using the American Community Survey (ACS) from 2008 to 2019,

[1] Most countries also have similar rules.

[2] Historical information about cutoff dates can be found in Bedard & Dhuey (2012) and Colasanti (2007). The general trend is that the school cutoffs are being set earlier in the year over time.

[3] https://childrensmd.org/browse-by-age-group/kindergarten-cut-dates-red-shirt-child-push-ahead/

I analyze the effect of children's quarter of birth (season of birth) on the probability of attending a private school by grade, from pre-kindergarten (pre-K) through 12th grade. I compare children born in April–June ($AprMayJun$) to those born the rest of the year: July–September ($JulAugSep$), October–December ($OctNovDec$), and January–March ($JanFebMar$).

The findings are as follows. First, there is a salient effect at kindergarten, with children born from July through December being more likely to attend kindergarten at a private school than those born in $AprMayJun$. The strongest effect is for those born after most states' cutoff, $OctNovDec$. Second, the effect does not persist at higher grades, meaning that after kindergarten, the probability of attending private school is not different for children born at different times of the year. Thus, parents use private schools to circumvent the entrance age cutoff, and when the cutoff is no longer a constraint, they transfer their children to a public school. Third, the effect at kindergarten is stronger for girls and increases with parents' educational attainment and income. Finally, access to public alternatives that closely substitute kindergarten decreases children's quarter of birth impact on private kindergarten attendance.

The analysis consists of three parts as follows. I first present the estimation of the probability of attending private school, with the quarter of birth as controls. I find that at kindergarten, children born in $JulAugSep$ and $OctNovDec$ are 0.37 and 1.09 percentage points more likely to attend private kindergarten than children born in $AprMayJun$. This effect is due to five-year-olds (the age when children typically start kindergarten) and is thus because of the cutoff. The effect does not persist at higher grades, which suggests children then transfer to public school. In Section 4, I present anecdotal evidence that parents of children born after the cutoff follow the mechanism suggested by the pattern. Furthermore, I estimate the impact of the quarter of birth on private spending and find that parents of children born from July – December spend more than $100 million in private kindergarten. Finally, the section concludes by exploring the heterogeneity of the analysis by gender and parents' characteristics.

I then explore the robustness of the results. I first show that state characteristics— cutoffs, access to public kindergarten, compulsory kindergarten laws— do not qualitatively affect the main findings. Moreover, I demonstrate that the correlation between private school attendance and the season of birth is not due to seasonal differences in parental characteristics. I also replicate the results using 1960–1980 and find qualitatively similar results. Finally, I explore different model specifications and again find qualitatively similar results.

The second part of the analysis uses the Early Childhood Longitudinal Studies of the kindergarten class of 2010–2011 (ECLSK2011) to supplement previous findings. Estimating the effect of being born from September to December and age at kindergarten entrance on the probability of attending a private school gives qualitatively similar results

to the ACS. Moreover, since the ECLSK2011 allows me to see children's trajectories over time due to the panel characteristic, I find suggestive evidence that children who go to a private kindergarten move to public school at higher grades.

The final part of the analysis explores how access to pre-K and transitional kindergarten (TK), also called "young fives" or developmental kindergarten, might affect the salient effect of private schools at kindergarten. I estimate the main analysis for kindergarten for states with mostly universal state-funded pre-K and find that the effect on private kindergarten is qualitatively the same. On the contrary, for states with state-funded TK, the private school effect at kindergarten significantly decreases. Thus, the results suggest that parents consider the type of school when choosing what to do with their "late-born" children and choose private kindergarten when they lack another comparable option.

To my knowledge, this is the first paper that explores how the season of birth/school entrance age laws can affect private school attendance. Moreover, it is the first to present evidence of how parents use private schools to help advance children into K–12 education. This paper therefore adds to the slim literature exploring characteristics that explain the probability of attending private K–12 schools. In this literature, Conley & Glauber (2006) explore the effect of the number of siblings (sibship size) on boys' probability of private school attendance and find that second-born boys are less likely to attend private schools when family size increases. Dynarski *et al.* (2009) use Catholic school vouchers to explore the elasticity of prices on the probability of sending children to private schools.

This paper also adds to the literature that explores the effect of children being born in a certain season/month on outcomes both in the short (test scores during K–12) and long term. In the short term, research both in the US and other countries finds older children tend to perform better in tests and that the effect decreases over time (Davis *et al.*, 1980; Datar, 2006; Bedard & Dhuey, 2006; Puhani & Weber, 2007; McEwan & Shapiro, 2008; Sprietsma, 2010; Dhuey *et al.*, 2019).[4] Regarding these differences, Elder & Lubotsky (2009) find evidence that the test gap is due to prior human capital accumulation since older children already perform better in test scores upon entering kindergarten.

In terms of longer-term effects, there are mixed findings. First, various papers find that those who are relatively young/born before the school cutoff tend to have higher educational attainment (e.g., Angrist & Krueger, 1991; Cascio & Lewis, 2006; Dobkin & Ferreira, 2010; McCrary & Royer, 2011), which perhaps is due to younger students having a larger retention rate (Dobkin & Ferreira, 2010 in the US and McEwan & Shapiro, 2008 in Chile). On the contrary, some papers find that children who are relatively young when entering school tend to have lower education and prepare, attend, and finish college less than children who are relatively old/born after the cutoff (Bedard & Dhuey, 2006; Dhuey *et al.*, 2019; Fredriksson & Öckert, 2014 in Sweden). Black *et al.* (2011) use Norwegian

[4]See Stipek (2002) for a summary of various other papers.

data and find little effect of school starting age on educational attainment. In terms of the labor market, some papers find that younger students entering the labor market earlier, earn more earlier in their career (Black *et al.*, 2011; Fredriksson & Öckert, 2014; Oosterbeek *et al.*, 2020), but this effect does not persist later in life. On the other hand, Dobkin & Ferreira (2010) use US census data of Texans and Californians and find no effect of school entrance age on labor market outcomes and house ownership.[5]

This paper contributes in several ways. First, this paper is the first to document and estimate the role that the date of birth plays in private school attendance and the behavior of using the private school as a way around the entrance age cutoff. Second, the findings suggest that entrance age laws negatively affect the welfare of those parents constrained by the cutoff. To avoid this welfare loss, parents spend more than $100 million on sending their "late-born" children to a private kindergarten. Third, compared with children of similar age, children that are pushed ahead through a private kindergarten start accumulating knowledge earlier on and in a different setting than otherwise. In addition, if these children steadily progress through K–12, they will enter the labor market earlier than otherwise. Consequently, in the long run, children might perceive a benefit from receiving this private kindergarten treatment. Finally, because parents who are more educated and have higher-income advance their children at a higher rate, the use of private schools to bypass the entrance age cutoff creates inequalities on school pathways for children from different socioeconomic backgrounds.

The rest of the paper continues as follows. Section 2 briefly discuss the conceptual framework. Section 3 presents the data, and Section 4 discusses the main empirical application. In Section 5, I explore robustness checks and alternative specifications. Section 6 presents the empirical application using the ECLSK2011 data, and Section 7 explores how access to state-funded pre-K and TK affects private school attendance. Section 8 concludes.

2 Conceptual Framework

How does entrance age cutoff affect K–12 school choices? To answer this question, let's consider the path into schooling that children take. We start with the kindergarten-age children for which the cutoff is relevant, five-year-old children. Taking these children as the starting point, we can explore what happens at kindergarten and how school choices

[5]Other outcomes in terms of health also suggest that starting school relatively young has negative health effects (Elder & Lubotsky, 2009; Black *et al.*, 2011; Bahrs & Schumann, 2020). Moreover, children's relative age at school entry affects crime. Dhuey *et al.*, 2019 and Peña, 2019 for Black men find that those who were relatively young at school entry are more prone to being incarcerated (juvenile incarceration in Dhuey *et al.*, 2019) in the US. According to Danish data, Landersø *et al.*, 2017 finds that those who were relatively young at school entry are more likely to commit juvenile crimes. Cook & Kang (2016) find that although children relatively young at school entry are more likely to commit juvenile crimes, they are less likely to commit felonies by age 19.

at higher grades depend on this initial choice.

Five-year-old children. Children are either born before or after the entrance age cutoff. This cutoff then determines parents' choice set at kindergarten. For those born before the cutoff, the entrance age law is not binding. Thus they can start public kindergarten.[6] For children born after the cutoff, complying with it would mean that they start kindergarten the following year. Alternatively, parents can send their children to a private kindergarten that allows younger children, starting them earlier than they would otherwise. For this *"earlier starters"*—children born after the cutoff that attend a private kindergarten— what happens in grades 1–12? How does the initial choice affect choices in the following school years?

Subsequent years. For *"earlier starters,"* at first grade and following grades, parents can then choose between keeping their children in private school and switching them to public school. The school choice will then depend on two mechanisms: switching costs and information acquisition.

The switching costs mechanism arises due to the choice at higher grades depending on how costly it is for parents to switch their children from a private to a public school. These switching costs include components such as the opportunity cost of the required time and energy to find a new school and the psychological cost on children's well-being due to changes in the school environment.

Information acquisition can also affect higher grade choices. By choosing a private kindergarten, parents learn about private schools' characteristics. This acquired knowledge then updates their prior views regarding schools and affects school choice in two ways. First, as parents gain information about private schools, and to the extent that they are risk-averse, they might perceive the public school choice as riskier than the private one, thus being more likely to keep their children in a private school. Second, if parents positively (negatively) update their views regarding private schools, they might lean toward keeping (switching) their children in private schools (to public schools). Hereafter, I refer to both mechanisms as *"switching costs"* for brevity.

In this framework, the effect of children's date of birth on private school attendance through K–12 would depend on if switching costs are either *"high"* or *"low"*. Precisely, if switching costs are *"high,"* the initial school type will generate inertia at higher grades, and *"earlier starters"* will stay in a private school. Hence, the effect of date of birth on private school attendance should be somewhat persistent through K–12. On the contrary, if switching costs are *"low,"* initial school choice would not be strongly related to choices at higher grades. In this case, because entrance age laws would only determine choices

[6]Parents can also choose to hold back their children, but this choice is not due to the cutoff.

at kindergarten, the effect of the date of birth on private school attendance should fade right after it.

In Section 4, I empirically study the effect of season of birth on private school attendance through K–12. There I discuss the qualitative evidence regarding the "switching costs" mechanism discussed above.

3 Data

To implement my analysis, I use the 2008–2019 ACS.[7] I limit my sample to 3- to 18-year-old household children who are born in one of the 50 states and the District of Columbia (DC) and who are currently attending school from pre-K through 12th grade.[8] I use the type of school currently attending to classify children between those attending a public or private school. Finally, to measure when children are born, I use the children's quarter of birth.[9]

Table 1 presents the unweighted mean and standard deviation of the sample's variables. The information for the complete sample is in column (1), while the following columns present the summary statistics by grade. For the complete sample, the average age is 10.88 years, half of the sample is male, and most of the sample is non-Hispanic white. Regarding the quarter of birth, children are born at similar rates across quarters, with just slightly more children (51% of the sample) born in April–September. Thus, I have a balanced sample across characteristics. Finally, 15% of the children currently in pre-K through 12th grade are attending a private school.

Across grades, in columns (2)–(7), the average age by grade (5.42 for kindergarten) shows the effect of the school cutoff, with some children being almost a year older than other children in the same grade. Moreover, children are similarly distributed by gender, race, and quarters of birth composition. In terms of private school attendance by grade, even though almost half of the children attend private schools in pre-K, the percentage significantly decreases after that. From pre-K to kindergarten, private school attendance decreases by 33 percentage points. In part, this reflects that public pre-Ks are not widely available and children in pre-Ks would be in private ones at a higher rate. From kindergarten to the first grade, private school attendance decreases by 1 percentage point, stabilizing around 12%. Figure 2 presents private school attendance by each grade and shows that the percentage of private school attendees is stable after the first grade, with a slight decrease after the eighth grade, which is likely due to children transferring schools when starting high school.

[7] Ruggles *et al.* (2020)

[8] The precise question is "At any time IN THE LAST 3 MONTHS, has this person attended school or college? Include only nursery or preschool, kindergarten, elementary school, home school, and schooling which leads to a high school diploma or a college degree."

[9] The ACS public use data do not have more detail on date of birth.

Figure A.1 shows private school attendance by grade and quarter of birth the same graph, limited to K–12.[10] Figure A.1 shows variations of the percentage of private school attendance by the quarter of birth at the earliest grades, especially for $JulAugSep$ and $OctNovDec$ at kindergarten. At kindergarten, the percentage of private school attendance increases with the quarter of birth. Compared to $JanFebMar$, the difference is 0.21, 0.48, and 0.47 percentage points for $AprMayJun$, $JulAugSep$, and $OctNovDec$, respectively. Thus, children born later in the year are more likely to go to private school for kindergarten. The distribution by the quarter of birth becomes more stable after that.

Table 1: Sample Description

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
				Grade			
	All	Pre-K	K	1st	2nd	3rd	>3rd
Age	10.88	4.00	5.42	6.50	7.49	8.49	13.46
	(4.17)	(0.75)	(0.63)	(0.61)	(0.63)	(0.66)	(2.62)
Male	0.51	0.52	0.52	0.51	0.51	0.51	0.51
	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)
Non-Hispanic White	0.66	0.68	0.64	0.65	0.65	0.65	0.67
	(0.47)	(0.46)	(0.48)	(0.48)	(0.48)	(0.48)	(0.47)
Non-Hispanic Black	0.10	0.09	0.10	0.09	0.10	0.10	0.10
	(0.30)	(0.29)	(0.30)	(0.29)	(0.30)	(0.30)	(0.30)
Hispanic	0.18	0.16	0.20	0.20	0.20	0.20	0.18
	(0.39)	(0.37)	(0.40)	(0.40)	(0.40)	(0.40)	(0.38)
Other Race/Ethnicity	0.05	0.06	0.06	0.06	0.06	0.06	0.05
	(0.22)	(0.24)	(0.24)	(0.23)	(0.23)	(0.23)	(0.22)
JanFebMar	0.24	0.24	0.24	0.24	0.24	0.24	0.24
	(0.43)	(0.43)	(0.43)	(0.43)	(0.43)	(0.43)	(0.43)
AprMayJun	0.25	0.24	0.25	0.25	0.25	0.25	0.25
	(0.43)	(0.43)	(0.43)	(0.43)	(0.43)	(0.43)	(0.43)
JulAugSep	0.26	0.26	0.26	0.26	0.26	0.26	0.26
	(0.44)	(0.44)	(0.44)	(0.44)	(0.44)	(0.44)	(0.44)
OctNovDec	0.25	0.26	0.25	0.24	0.24	0.25	0.25
	(0.43)	(0.44)	(0.43)	(0.43)	(0.43)	(0.43)	(0.43)
PrivateSchool	0.15	0.47	0.14	0.13	0.12	0.12	0.12
	(0.36)	(0.50)	(0.35)	(0.33)	(0.33)	(0.33)	(0.32)
N	5,426,062	461,778	374,473	363,550	371,629	382,896	3,471,736

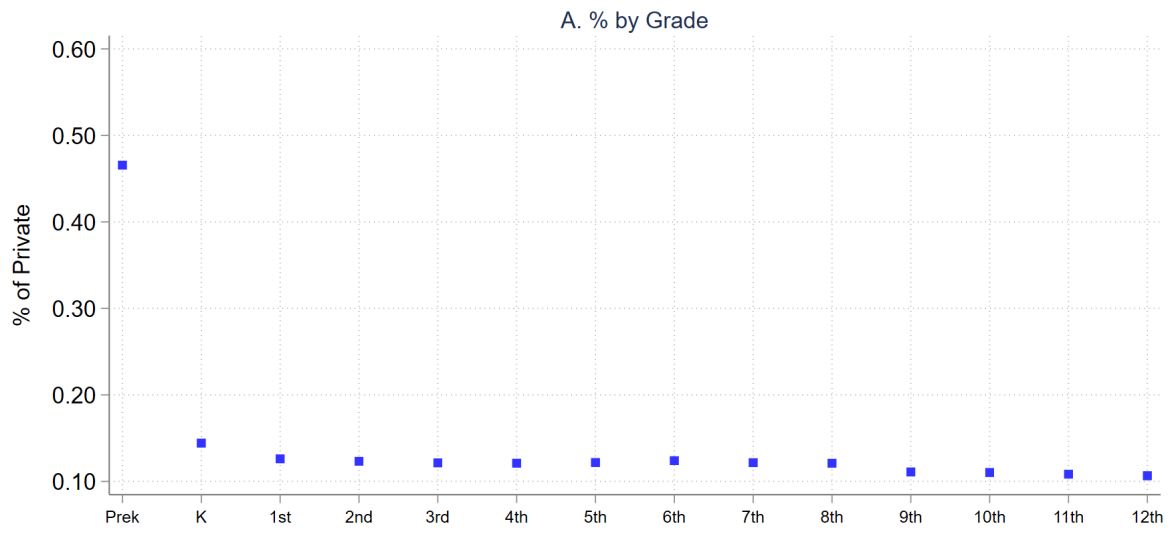
Note. This table presents the mean and standard deviation (in parenthesis) of the main variables from the ACS 2008–2019 sample by grade. N represents the number of observation.

From the sample characteristics, three observations can be made regarding private school attendance. First, attendance varies across grades, with children going to private school at a higher rate at earlier grades. Second, private school varies somewhat by the quarter of birth. Third, the private school variation by the quarter of birth variation is

[10]For pre-K, the percentage of children attending private school are for 46.8% $JanFebMar$, 47.1% $AprMayJun$,46.3% for $JulAugSep$, and 46.1% $OctNovDec$. Thus, the percentage of private school attendance decreases over the year, with those born in January–June being more likely to attend private school. I exclude Pre-K from the graph given it is at a different level than the rest of the grades.

more relevant at earlier grades. In the following section, I formally look at the reduced-form effect of the quarter of birth on the probability of attending a private school.

Figure 2: Percentage of Children Attending Private School by Grade and Quarter of Birth



Note. This figure shows the percentage of children attending private school by grade. Observations by grade: N= (461,778; 374,473; 363,550; 371,629; 382,896; 383,378; 389,006; 391,971; 391,174; 396,180; 396,315; 394,537; 384,222; 344,953)

4 Results

For each grade, I estimate

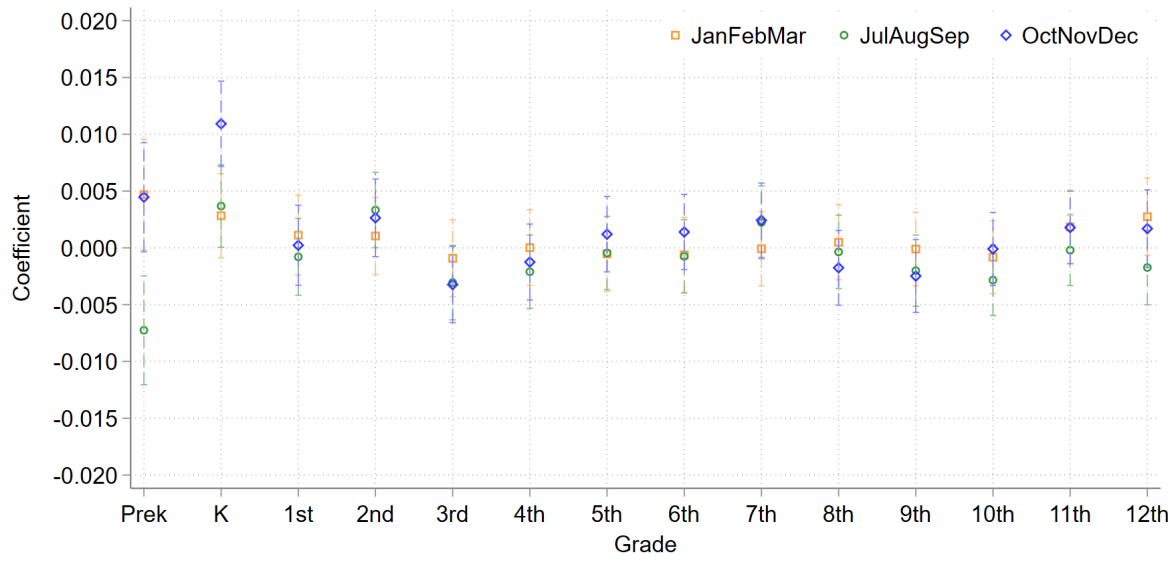
$$PrivateSchool_{ist} = \gamma_0 JulAugSep_i + \gamma_1 OctNovDec_i + \gamma_2 JanFebMar_i + X_i'\beta + \alpha_t + \zeta_s + \epsilon_{ist}, \quad (1)$$

where X_i includes a male dummy, age, and dummies for race/ethnicity: non-Hispanic white, non-Hispanic black, and Hispanic (the reference group is other races). I also include year (α_t) and state of residence (ζ_s) fixed effects. The variables of interest are the quarter of birth dummy variables: $JulAugSep$, $OctNovDec$, and $JanFebMar$ (the reference quarter is $AprMayJun$). I estimate the model using a linear probability model (LPM).

The model in equation (1) explores the hypothesis that the kindergarten entrance age cutoff plays a role in private school attendance. If the public kindergarten entrance age cutoff increases private school attendance, those born after the cutoff should be more likely to be in a private school. As we saw in Figure 1, most states set their cutoff by September. Thus, the $OctNovDec$ coefficient should be positive and quantitatively larger.

Figure 3 shows the estimated coefficients and 95% confidence intervals. First, the results show a pattern with a salient effect at kindergarten, where the strongest effect is for those born in $OctNovDec$. Precisely, children born in $JulAugSep$ and $OctNovDec$ are 0.37 percentage points (hereafter, *p.p.*) and 1.09 *p.p.* more likely to attend a private kindergarten than those born in the $AprMayJun$ quarter. As seen in Table 1 – column (3), since 14% of children attending kindergarten are in a private school, the $JulAugSep$ and $OctNovDec$ effect represent a 2.64% and 7.79% of this average, respectively. Second, the effect does not persist at later grades. The effect of the quarter of birth on private school attendance largely disappears, and most coefficients are not significant.[11] After the effect at kindergarten, the largest values are for $OctNovDec$ and $JanFebMar$ at pre-K (0.45% and 0.47%, significant at the 10% level), which likely means that some children born after the cutoff would go to pre-K as they wait to attend kindergarten the following year. Because most pre-K are private (47% of pre-K children attending a private one) is thus expected that children born after the cutoff and complying with it by going to pre-K would be more likely to attend a private pre-K. Nevertheless, the $OctNovDec$ coefficient at kindergarten is 2.33 times the second largest coefficient.

Figure 3: Effect of Quarter of Birth on Private School Attendance by Grade



Note. Coefficient and 95% confidence interval of the quarter of birth variables in regression (1). The following covariates are included in the regression but excluded from the graph for brevity: age, dummies for male, Non-Hispanic white, Non-Hispanic black, and Hispanic. I also include year and state fixed effects. *Observations:* N= (461,778; 374,473; 363,550; 371,629; 382,896; 383,378; 389,006; 391,971; 391,174; 396,180; 396,315; 394,537; 384,222; 344,953)

[11] For the $JulAugSep$ quarter, the effect is –0.72 *p.p.* for pre-K and 0.33 *p.p.* for 2nd grade (significant at the 5% level). At the 10% significance level, the coefficients are negative for $JulAugSep$ (3rd and 12th grade: –0.31 *p.p.* and –0.28 *p.p.*) and $OctNovDec$ (3rd grade: –0.32 *p.p.*).

Estimation by Age. The hypothesis is that the entrance age cutoff increases private school attendance for children born later in the year. Combined with the finding of a salient effect at kindergarten, it suggests that the effect must be due to five-year-old children, the typical kindergarten entrance age. To show if this is true, I can estimate equation (1) by age. It is important to note that the analysis by age is not easily interpretable since access to public school varies by grade (especially public pre-K). Thus, the grade that the child is in will affect private school attendance and, due to the cutoff, children of the same age might be in different grades. Nonetheless, since the analysis is informative, Figure A.2 shows the results of estimating (1) by age. The figure shows that the strongest coefficient is at age five for those born in $OctNovDec$, with the other quarters being also significant at age five. The rest of the coefficients are either not significant or are significantly smaller. Therefore, the salient effect at age five aligns with the effect being due to the role of kindergarten entrance age laws.

A final point worth mentioning is that the results by age suggest that using private schools as childcare or limited public options do not seem to be alternative explanations for the effect at kindergarten observed in Figure 3. If the quarter of birth effect were due to childcare preferences or limited public options, we would see a similar effect at ages three and four (when children are in pre-K) to the effect we see at age five. Nevertheless, this is not the pattern we see, suggesting these forces are not driving the differences in private school attendance by the quarter of birth at kindergarten.

Effect at grades 1–12. As discussed in Section 2, switching costs will determine how persistent the season of birth's effect on private school attendance is through K–12. If switching costs are high, this effect should persist at higher grades, while no persistent arise from switching costs being low. The findings in Figure 3 that the effect of the season of birth on private school attendance is strong at kindergarten but not after that qualitatively suggests that switching costs must be low.

The key takeaway from the main results in Figure 3 is that the quarter of birth affects private school attendance, but this effect is mainly for children in kindergarten. After this, the effect of the quarter of birth dissipates. This pattern suggests that children's quarter of birth can affect the type of school they attend at the start of formal schooling.

An explanation for this pattern is that to get around the school entrance age, parents of children born after the cutoff might choose to send their children to private kindergarten. Since most states set their cutoff by September, the effect is the strongest for children born in $OctNovDec$. Moreover, since the effect dissipates after that, it must be that many of those children initially enrolled in private schools due to the cutoff end up transferring to public schools at later grades. If those children stay in private school through K–12, the effect should be more consistent across grades. However, this is not what the results

suggest.[12]

The anecdotal evidence, quoted from parenting websites/blogs, is consistent with the pattern shown in the data:

> *"We sent our daughter to private school for kindergarten with the intent of transferring her into our public school for first grade. Our public schools will accept your child into first grade as long as they have completed an accredited kindergarten."*[13]

> *"...I have an early January birthday and my parents sent me to private school for a couple years to get around the cutoff for public school..."*[14]

> *"Back in the 70s my mother did this too, as I also had a December birthday. Once the kid successfully finishes kindergarten, next year they will let them in first."*§

> *"I was in a similar situation as a kid, but the school district wouldn't allow it. My parents instead sent me to a private school until they finally let me into the public schools in 3rd grade ..."*§[15,16]

Importantly, some states allow children that completed an accredited private kindergarten to advance to grade one.[17] Moreover, even though most states have an entrance age cutoff in the first grade, states do not define an age requirement from second grade and on. Hence, children with late birth dates could transfer to public school when the school cutoff is no longer relevant.

A consequence of this "private school detour" choice is that these children start school a year earlier than they would otherwise, which affects them in different ways. Being relatively younger than other children in the classroom makes them more likely to have lower test scores in the short term. At the same time, they benefit by learning from the older children in the classroom, which increases their test scores (e.g., Elder & Lubotsky, 2009; Bedard & Dhuey, 2012; Leuven & Rønning, 2016; Cascio & Schanzenbach, 2016). Additionally, compared with those born in the same quarter and year, those in a private kindergarten build human capital earlier and are exposed to a different quality school. If private schools are of better quality, there can be an education gap between these children who would otherwise enter school at the same time. Finally, if these children steadily progress through K–12, they will enter the labor market earlier than otherwise. Thus, these children might benefit in the long run due to a head start through private school.

[12] Note that the number of children per grade is fairly constant and the sample is representative.

[13] https://childrensmd.org/browse-by-age-group/kindergarten-cut-dates-red-shirt-child-push-ahead/

[14] Comment by Robin on 02.04.2019 in https://happilyevaafter.com/our-preschool-plan-for-major/

[15] §Comments on 06.06.2018 on https://www.reddit.com/r/Parenting/comments

[16] Additional examples: https://www.dcurbanmom.com/jforum/posts/list/499987.page https://www.cultofpedagogy.com/academic-redshirting/ http://www.city-data.com/forum/parenting/1853076-moving-daughter-will-miss-kindergarten-cutoff.html

[17] For example, Missouri: https://dese.mo.gov/governmental-affairs/kindergarten-first-grade-entry; California: https://www.cde.ca.gov/ci/gs/em/kindergartenfaq.asp; North Carolina: https://stateboard.ncpublicschools.gov/legal-affairs/resources-1/school-entry-age; Hawaii: http://www.hawaiipublicschools.org/ParentsAndStudents/GradeLevelOverview/Kindergarten

4.1 Private School Spending

The results thus far show that children born July–December are more likely to attend private school at kindergarten. Given that private schools are not free, how much do parents spend to do so? To answer this question, I then create the variable *Spending*, which equals the state's average private school tuition at kindergarten if the child is attending a private school and zero otherwise.[18] Table A.1 presents this variable mean and standard deviation in column (1). On average, parents paid just above $1,000 on school tuition at kindergarten. Using this variable, I estimate:

$$Spending_{ist} = \gamma_0 JulAugSep_i + \gamma_1 OctNovDec_i + \gamma_2 JanFebMar_i + X_i'\beta + \alpha_t + \zeta_s + \epsilon_{ist},$$

where controls variables are as in equation (1). This equation estimates the tuition cost of using private kindergarten to give "late-born" children a head start into schooling.

Table A.1 shows the estimation results in column (2). The results show that parents of children born in $OctNovDec$ ($JulAugSep$) are spending in school tuition just above $100 ($40) more at kindergarten than the reference group. This value represents 8.06% (3.19%) of the average school tuition at that grade. Moreover, using the US population, I calculate that around 0.95 million kindergarten children are born in $JulAugSep$ and $OctNovDec$, respectively.[19] Combining this figure with results in Table A.1 – column (2), I estimate that parents privately spend an aggregated amount of almost $140 million to push their late-born children ahead through private kindergarten.

The results suggest that parents of children born after the cutoff are negatively affected by this constraint. The aggregated private expenditure shows that to reduce such welfare loss, they spend a sizable amount on school tuition to bypass the entrance age cutoff. It is important to note that this calculation only represents the effect of entrance age laws on school tuition expenditure through an increase in private school attendance. A more comprehensive estimation of the impact of this mechanism should also include its effect on children's well-being and longer-term outcomes. Exploring these avenues is left for future research.

4.2 Heterogeneity Analyses

In this section I discuss two heterogeneous analyses concerning children and parents' characteristics, which help me understand who uses private schools in the way described in the main results. In Section 4.2.1, I look at the effect of the quarter of birth on private

[18] I calculate the average private school tuition per state and grade using the tuition cost per school available here: https://www.privateschoolreview.com/.

[19] Starting from the 2019 US population of 328.2 million people (https://www.census.gov/quickfacts/fact/table/US/HSG650219) and keeping the 5–18 years old population (16.3%). Because around 7.08% of them should be in kindergarten (based on the ACS), about 3.79 million children are in kindergarten.

school attendance by children's gender. I then explore how the effect varies by parents' education and labor income in Section 4.2.2.

4.2.1 Heterogeneity by Children's Gender

There are several reasons to expect that the effect of the quarter of birth on private school attendance varies by children's gender. Girls are emotionally prepared for school earlier than boys, and parents concerned with children's readiness tend to delay school entrance (academic redshirting) more for boys than girls (DiPrete & Jennings, 2012; Bassok & Reardon, 2013; Cook & Kang, 2018). Thus, since the results suggest that children born late in the year are pushed into school earlier via private schools, the salient effect at kindergarten should be quantitatively larger for girls than for boys.

Figure 4 presents the results estimating equation (1) for boys, in Figure A and girls in Figure B. Qualitative results by gender are similar to the main findings, with a salient effect at kindergarten for children born later in the year and dissipating after that. As expected, the effect at kindergarten is larger for girls than for boys. Precisely, the $OctNovDec$ coefficient is 0.46 percentage points larger for girls than boys, or 1.5 times the value for boys at kindergarten. Other quarters of birth are not significant for boys but are significant for girls, at 0.69 *p.p.* for $JanFebMar$ and 0.51 *p.p.* for $JulAugSep$ (significant at the 10% level). In pre-K, girls are also more likely to attend private school if born in $OctNovDec$ (0.95 *p.p.*) and $JanFebMar$ (0.73 *p.p.*). The effect at pre-K reflects those children born after the cutoff who, instead of being pushed ahead, attend pre-K. The fact that the effect at pre-K is stronger for girls also points to school readiness, with parents likely choosing to keep boys out of school more due to school readiness concerns.

Girls getting a head start through private school at a higher rate than boys are in line with parents' consideration of how ready their children are to start school. When constrained by the entrance age cutoff, they use private schools to push their children into school earlier. The results align with the redshirting literature that finds parents hold boys back more than girls.

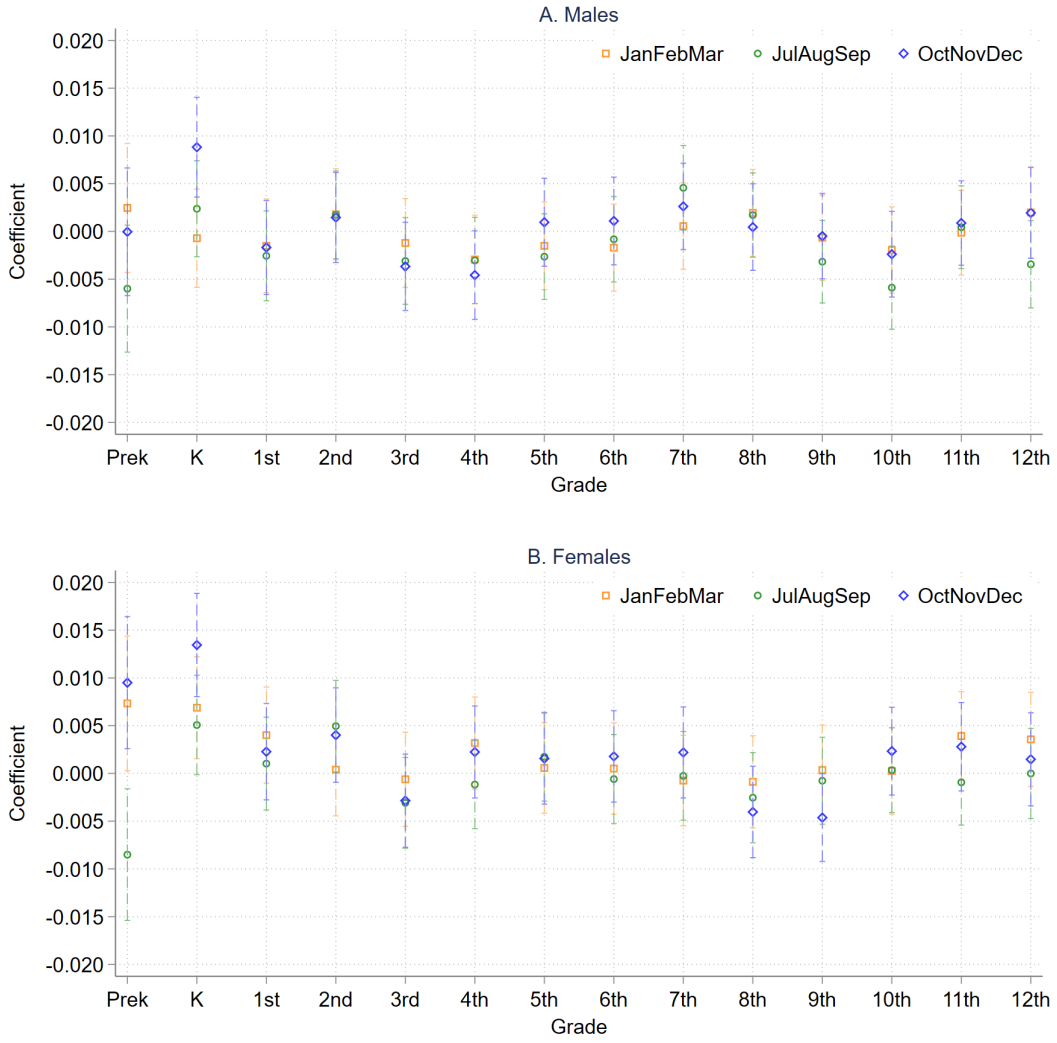
4.2.2 Heterogeneity by Parent Characteristics

Because parents determine early school choices, it is relevant to understand how private school attendance varies by parents' characteristics. To do so, I concentrate on parents' education and labor income. Parents educational attainment is not only a key determinant of their income but might also affect how much weight they give to their children's formal schooling. Regarding parents' labor earnings, it accounts for their availability to invest in human capital by paying for private schools.

I divide the sample as follows. For educational attainment, I take the parents' highest level of education and classify children into three groups: (1) high school or less (less

than a year of college to 12 years of education), (2) more than high school (1–3 years of college), and (3) college or plus (4 years of college or more). For parents' income, I create the *family income* by aggregating parents' labor income (Consumer Price Index corrected to December 2019) and split the sample based on the family's income quartile.

Figure 4: Effect of Quarter of Birth on Private School Attendance By Grade and Children's Gender



Note. Coefficient and 95% confidence interval of the quarter of birth variables in regression (1). The following covariates are included in the regression but excluded from the graph for brevity: age, dummies for male, Non-Hispanic white, Non-Hispanic black, and Hispanic. I also include year and state fixed effects. *Observations:* N_{Males}= (239,648; 192,929; 187,166; 190,378; 196,027; 196,438; 199,586; 201,840; 200,891; 204,074; 204,249; 203,035; 197,268; 175,298); $N_{Females}$= (222,130; 181,544; 176,384; 181,251; 186,869; 186,940; 189,420; 190,131; 190,283; 192,106; 192,066; 191,502; 186,954; 169,655)

Figure A.3 shows the percentage of children attending private school by parent education and income characteristics, where Figure A.3-A shows the percentage by parents' educational attainment and Figure A.3-B the percentage by parents' income. The graphs show an increasing relationship between the probability of private school attendance and

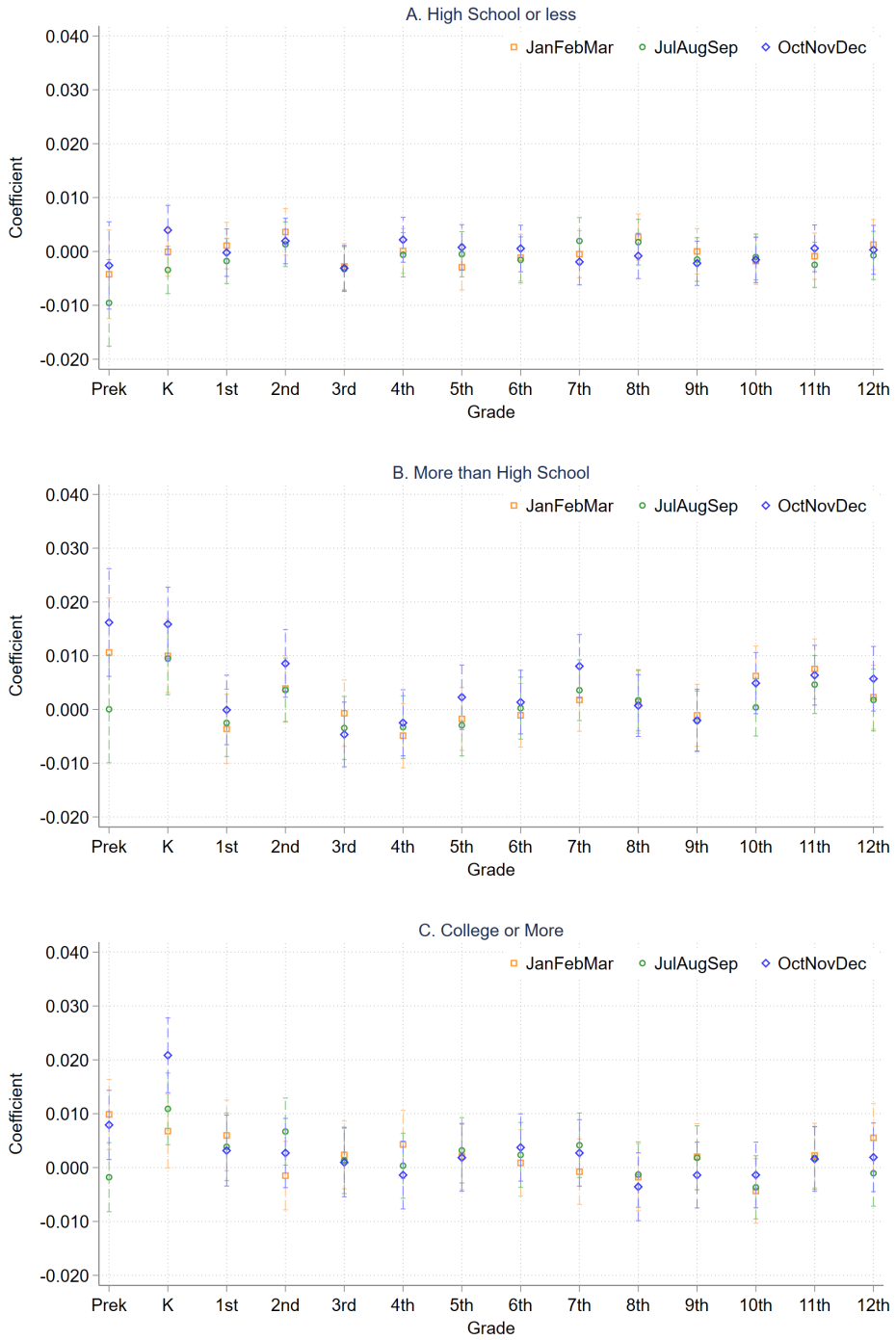
family level of education/income. Children with parents with a college education or higher (fourth income quartile) are 16.01 (14.19) percentage points more likely to go to private kindergarten than children of parents with a high school education or less (first income quartile). However, the increasing relationship does not mean that the effect of the quarter of birth on private school increases on parents' education and income. Parents with high education/income could send their children to private school at a higher rate for reasons unrelated to children's date of birth, such as school quality. Nevertheless, I expect the quarter of birth effect at kindergarten to increase with parents' education and income.

Figure 5 presents the results of equation (1) by grade and parents' educational attainment. First, the salient effect at kindergarten is increasing with parents' education. The figure shows that children born in $JulAugSep$ are more likely to go to private school if their parent has more than a high school education (0.95 *p.p.*) and a college education or higher (1.09 *p.p.*). For children born in $OctNovDec$, the effect is 0.40 *p.p.* for a parent with a high school education or less (significant at the 10% level), 1.58 *p.p.* if more than high school and 2.08 *p.p.* if college and higher. The $JanFebMar$ quarter of birth also has an effect of 1.00 *p.p.* if the parent has more than a high school education and 0.68 *p.p.* (significant at the 10% level) if they have a college education or higher. Moreover, in pre-K, children born in $OctNovDec$ ($JanFebMar$) are more likely to go to private school than the reference group by 1.62 *p.p.* (1.06 *p.p.*) for more than high school and 0.79 *p.p.* (0.99 *p.p.*) for more than college. After kindergarten, the effect dissipates, following the pattern of the main results.

Figure 6 shows the results by family income, and they are in line with the main results where we have a salient effect at kindergarten, with the effect increasing by income level. There is a quantitatively similar effect at pre-K: $OctNovDec$ for the first and fourth quartile, and an effect for $JanFebMar$ for the fourth quartile. At kindergarten, $OctNovDec$ is increasing in quartiles, with effects from the first to fourth quartile at 0.83 *p.p.*, 1.26 *p.p.*, 1.14 *p.p.*, and 2.44 *p.p.*, respectively. For $JulAugSep$, the effect is only significant for the fourth quartile, at 1.54%. The effect at $JanFebMar$ is also significant for the first quartile by 0.68 *p.p.*.

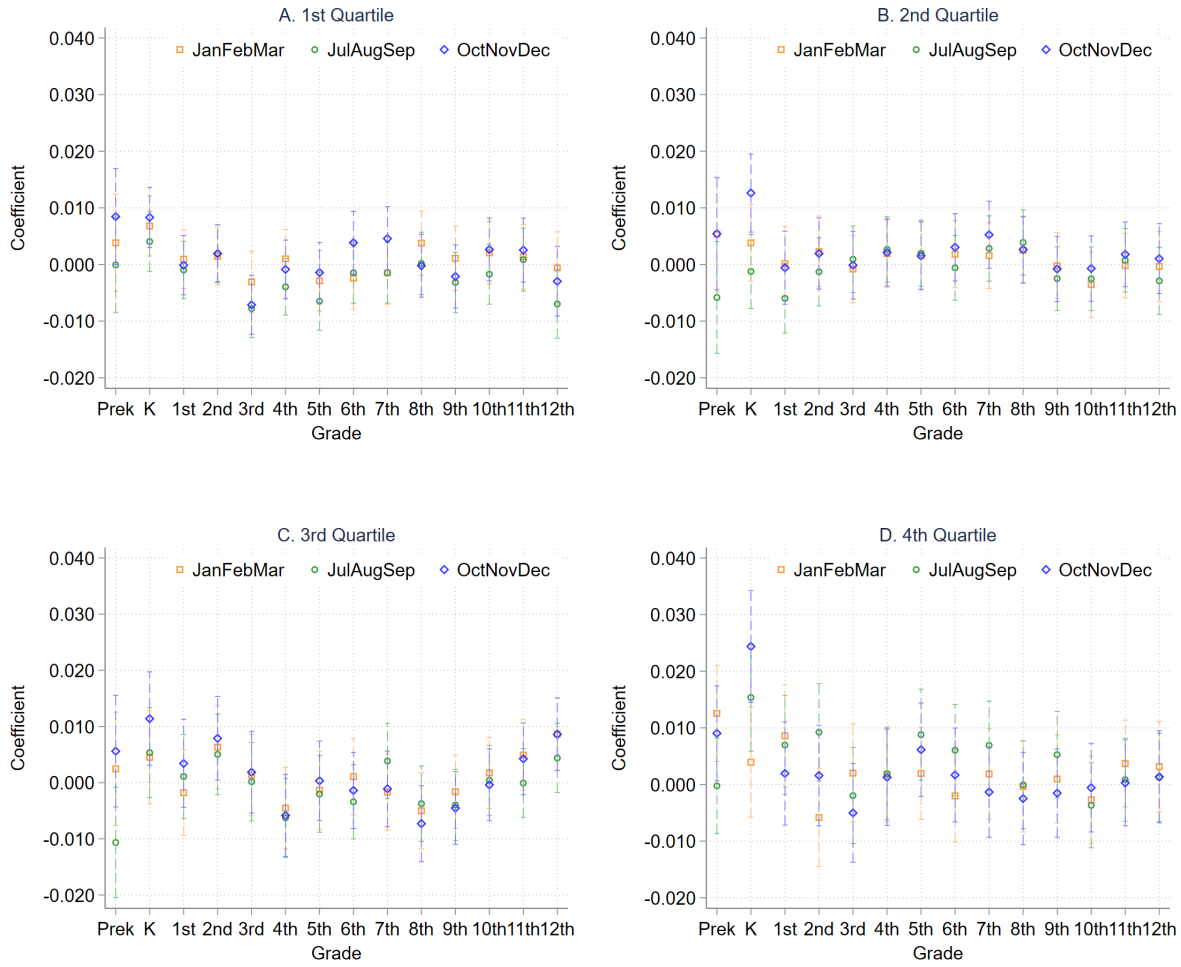
The results show that only the salient effect at kindergarten increases with parents' educational attainment and family income. The effect at pre-K, on the contrary, do not. These results are in line with parents reacting differently regarding school choices when limited by the cutoff. The increase in choosing private kindergarten with parents' educational attainment and family income suggests that more educated/higher-income parents seek to advance their children through K–12 at a higher rate than less education/lower-income parents.

Figure 5: Effect of Quarter of Birth on Private School Attendance by Grade and Parents' Educational Attainment



Note. Coefficient and 95% confidence interval of the quarter of birth variables in regression (1). The following covariates are included in the regression but excluded from the graph for brevity: age, dummies for male, Non-Hispanic white, Non-Hispanic black, and Hispanic. I also include year and state fixed effects. *Observations:* N_{HS}=(102,725; 112,683; 110,482; 113,404; 117,116; 117,777; 119,210; 121,473; 121,726; 124,103; 126,814; 125,848; 120,629; 108,157); N_{MoreHS}= (105,454; 94,901; 92,641; 95,939; 99,445; 100,165; 102,090; 103,756; 103,638; 105,359; 105,673; 105,696; 103,657; 92,499) $N_{Collegeplus}$= (253,599; 166,889; 160,427; 162,286; 166,335; 165,436; 167,706; 166,742; 165,810; 166,718; 163,828; 162,993; 159,936; 144,297)

Figure 6: Effect of Quarter of Birth on Private School Attendance by Grade and Family Income



Note. Coefficient and 95% confidence interval of the quarter of birth variables in regression (1). The following covariates are included in the regression but excluded from the graph for brevity: age, dummies for male, Non-Hispanic white, Non-Hispanic black, and Hispanic. I also include year and state fixed effects. *Observations:* N_{1st}=(102,557; 100,284; 96,970; 97,571; 99,429; 98,501; 98,465; 99,134; 97,664; 98,640; 99,573; 97,174; 90,816; 79,761); N_{2nd}=(104,312; 97,205; 93,744; 95,729; 98,562; 97,693; 98,419; 98,700; 98,041; 99,078; 98,508; 97,522; 95,176; 84,654); N_{3rd}=(117,827; 91,125; 88,575; 91,012; 94,237; 95,002; 96,806; 98,017; 98,473; 99,241; 99,226; 99,960; 98,738; 89,424); N_{4th}= (137,082; 85,859; 84,261; 87,317; 90,668; 92,182; 95,316; 96,120; 96,996; 99,221; 99,008; 99,881; 99,492; 91,114)

My findings are in line with the redshirting literature, where families with higher education and income hold back their children more (e.g., Bassok & Reardon, 2013; Dhuey *et al.*, 2019; Schanzenbach & Howard, 2017). Put together, one implication of these findings is that while lower-income children follow a more traditional path, families with higher education and income tend to deviate children more from regular school progression. Thus, children from different socioeconomic backgrounds would have different paths through formal schooling.

5 Robustness Check and Specification

In this section, I further explore the main findings. First, I look at the robustness of the results through a series of analyses, exploring sample restrictions and variations in states' policies. I then explore if the results are not sensitive to an alternative specification.

5.1 Robustness Check

This section explores the robustness of the main results. I first look at a different estimation strategy in Section 5.1.1, and in Section 5.1.2 I analyze the sample of children born between July and December. In Section 5.1.3 I analyze if access to public kindergarten can explain the results, and in Section 5.1.4, I explore the sensitivity to compulsory laws by limiting the sample to states where kindergarten is compulsory.

Furthermore, a critique of using the quarter of birth is that the children's season of birth is associated with parents' characteristics (e.g., Buckles & Hungerman, 2013). To explore this, in Section 5.1.5 I analyze how stable the results are across a set of parents' observable characteristics. Then in Section 5.1.6 I look at the results' sensitivity of a change in the survey question after 2016 by limiting the sample to 2008–2016. Finally, I present the results using data from 1960 to 1980 in Section 5.1.7.

5.1.1 Estimation Strategy

In the main analysis, I use an LPM to estimate the quarter of birth effect on private school attendance. To explore if the results are sensitive to the estimation strategy, I estimate equation (1) using a probit model instead. Figure A.4 shows the margin effects and 95% confidence intervals. The results are quantitatively and qualitatively the same as the main findings and are thus not sensitive to the estimation strategy.

5.1.2 Entrance Age July–September Cutoffs

Entering a public kindergarten in a given year depends on the interaction between the date of birth and the entrance age cutoff date. To explore this, I limit the sample to children born in July–December living in states with $JulAugSep$ cutoffs. Thus, the sample has a "control" ($JulAugSep$) and a "treated" ($OctNovDec$) group by the cutoff. Using this sample, I estimate equation (1) including only the $OctNovDec$ dummy, with $JulAugSep$ being the reference group.

Given the cutoff, I expect that a portion of the children born in $OctNovDec$ would attend a private pre-K (wait for next year), while others would advance through private kindergarten, and the $OctNovDec$ coefficient would be larger for these grades. After that, the effect should dissipate. Figure A.5 shows the estimation results. As expected, $OctNovDec$ increases the probability of attending a private school in pre-K and decreasing

after that. I next estimate equation (1) using only children born in July–December in states with September school cutoffs. The results, in Figure A.6, are unchanged. Thus, the results are in line with the main results.

5.1.3 Effect of Public Kindergarten Availability

An alternative explanation for choosing private kindergarten is the availability of public ones. To explore if this could explain the results, I estimate equation (1) for the sample of states and DC that require school districts to offer a kindergarten program by 2020.[20] If the results are due to a lack of available public schools, the salient effect at kindergarten should disappear or significantly decrease.

Figure A.7 shows the results, which are qualitatively similar to the main findings in Figure 3 with the salient effect at kindergarten. Consequently, the availability of public kindergarten cannot explain the pattern I observe.

5.1.4 Effect of Kindergarten Attendance Requirement

An additional question is how the results relate to compulsory kindergarten laws since not all states and DC require children to attend kindergarten. Using states' laws, I limit the sample to children living in states and DC that require kindergarten attendance by 2020. Figure A.8 presents the results of estimating equation (1) for this sample, with qualitatively similar results to the main findings.

5.1.5 Effect of Parent Characteristics

One of the main critiques of using the quarter of birth is that parents of different backgrounds might have children at different times of the year, correlating parents' characteristics with the quarter of birth (e.g., Buckles & Hungerman, 2013). Regarding this concern, it is worth noticing that I expect observable and unobservable parent characteristics that explain children's private school attendance to persist across all grades. It would mean that the quarter of birth effect on private schools should be more consistent across grades. There is no reason to believe that these characteristics will affect children in a particular grade (kindergarten) differently. However, the results do not show this pattern. Nonetheless, I address this concern in this section by exploring the impact of observable parent characteristics on the main results.

I use parents' characteristics to see if including these in the estimation of equation (1) changes the results. I include the variables shown in Table 2. The first regression is the main results in Figure 3, followed by the regression for children with two parents in the household (regression (2)). For regressions (3)–(8), only for those with both parents,

[20] A summary of states' requirements are available at https://nces.ed.gov/programs/statereform/tab1_3-2020.asp.

I progressively include parents' characteristics. I control for parents' demographics as parents' age (regression (3)) and parents' quarter of birth (regression (4)). In regression (5), I also include *Born in US* dummies equal to one if born in one of the 50 states and DC. In regression (6), I add *education* controls measured as parents' years of education. *Employed* dummies equal to one if employed and zero otherwise are the added control in regression (7). Finally, *Earnings* control for both parents' labor income in regression (8).

Table 2: Additional Control Variables

	Regression							
Characteristics	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Both Parents	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Birth Quarter	No	No	No	Yes	Yes	Yes	Yes	Yes
Born in the US	No	No	No	No	Yes	Yes	Yes	Yes
Education	No	No	No	No	No	Yes	Yes	Yes
Employed	No	No	No	No	No	No	Yes	Yes
Earnings	No	No	No	No	No	No	No	Yes

Importantly, if the results are stable across regressions (1)–(8), then parent characteristics are not driving the results found above. Figure A.9 to Figure A.11 show the coefficients and 95% confidence intervals of all eight regressions. Overall, the results are robust and, if anything, are larger when including all the parents' characteristics. When limiting the analysis to children with both parents in regression (2), the coefficients quantitatively increase but stay within the confidence interval of the main findings. The controls that increase the value much more than other controls are parents' education (regression (6)), but even there, the coefficients stay within the confidence intervals of regression (2). Finally, the pattern observed in the main results persist. Therefore, the results are consistent across various parent characteristics controls.

5.1.6 Effect of Homeschooling

In the ACS, homeschooling counts as attending a private school as long as it *would lead to a degree*. However, there are nuances in the way that homeschooling counts depending on the survey year. Up to 2016, the questionnaire precisely stated that homeschooling counted as attending school only for grades 1–12. In 2016–2019, the questionnaire did not limit the grades for which homeschooling counts.[21] Thus, the question excludes homeschooling for pre-K and kindergarten for the 2008–2016 subsample. To explore if the results are sensitive to this variation, I estimate equation (1) for 2008–2016, counting homeschooling only for grades 1 through 12. Since the main results are for kindergarten,

[21] See details of the question at https://usa.ipums.org/usa-action/variables/SCHOOL.

finding the same pattern for this subsample would suggest that the exclusion of homeschooling in pre-K and kindergarten does not affect the main conclusions.

As shown in Figure A.12, the subsample results are quantitatively similar to the main results, thus easing the concern that variation in the inclusion of homeschooling in kindergarten could affect the results.

5.1.7 How Has This Relationship Changed Over Time?

The quarter of birth effect on private school attendance should be lower nowadays than in the past. One of the reasons is that a higher percentage of parents choose to "redshirt" their school-eligible children nowadays, making the school cutoff less relevant.[22] Moreover, changes in school structure would affect private school attendance. Some of these changes are the increase in pre-K attendance over time, the decrease in private school enrollment, the decreasing role of religious-affiliated private schools, and an increase in state-funded pre-K and kindergarten availability.

To explore the effect over time, I use information from the 1960–1980 census (the 1960 1% and 5% state sample, the 1970 1% state sample, and the 1980 5% state sample).[23] Figure A.16 shows the results using these samples. The findings align with the main results: the effect is larger in earlier grades and is strongest for the quarter after the most common cutoffs, which in these samples is $JanFebMar$.

Contrary to the main results, the effect is more persistent across grades. Using the availability of the type of private school for the 1970–1980 samples, I explore the role of religious-affiliated private schools in this persistence. The results by type of private school (Figure A.18 to Figure A.19) show that the persistence at higher grades is driven by religious-affiliated schools, with these children transferring to public schools at a lower rate. The pattern of the nonreligious private school follows the pattern of the main results in Figure 3. Thus, variations on the importance of religious-affiliated private schools explain the persistence at later grades in the 1960–1980 sample and its absence in the main results. For more details on the sample and results, see Section A.1 in the Appendix.

5.2 Alternative Specification: Entrance Age Cutoff

Up to this point, I use the quarter of birth as the variable of interest to explain its effects on private school attendance. Alternatively, in this section, I exploit the relationship between the state cutoff date and children's quarter of birth to measure treatment. As Figure 1 shows, the cutoff varies by state, and whether a child is affected by it depends

[22] Deming & Dynarski (2008) show that the percentage of six-year-old children in first grade has decreased over time since 96% in 1968.

[23] Unfortunately, the information for 1990 does not have the quarter of birth variable available.

on the relationship of this cutoff with their birth date.

A few features regarding this analysis must be kept in mind. First, the analysis excludes children living in states that do not have a cutoff or states in which local education agencies (LEAs) choose their own entrance age rules.[24] Second, the school cutoff can vary over time.[25] To account for these variations, for each year, I recreate the year-state cutoffs using various yearly reports[26] as well as the detail of the enacted laws.[27,28]

Using the school cutoff by state and year, I create $AvgMonthsTreat$, a variable measuring the average months that a child born in a given quarter needs to wait to enter kindergarten. For example, if the cutoff is September 1, those born in September need to wait 12 months to start kindergarten, while those born in August wait for 1 month, and so on. Then, I can calculate $AvgMonthsTreat$ as $AvgMonthsTreat = 7$ for a child born in $JanFebMar$ quarter, $AvgMonthsTreat = 4$ if born in $AprMayJune$, $AvgMonthsTreat = 5$ if born in $JulAugSep$, and $AvgMonthsTreat = 10$ if born in $OctNovDec$.

Using this variable, by grade, I estimate the following model:

$$PrivateSchool_{ist} = \gamma_0 AvgMonthsTreat_i + X_i^{'}\beta + \alpha_t + \zeta_s + \epsilon_{ist}, \tag{2}$$

where the controls are the same as in (1) and $AvgMonthTreat$ is the variable of interest. The prior is that if the cutoff increases private school attendance for those away from the cutoff, then as the $AvgMonthTreat$ increases, the probability of being in private school should increase. Moreover, the results should be at entrance of school, with the effect dissipating for higher grades.

Figure 7 shows the $AvgMonthTreat$ coefficients and 95% confidence intervals. The results are as expected and qualitatively in the same direction as the main results in Figure 3. The largest quantitative effect is at pre-K and kindergarten, with the impact decreasing after that. Children who are born one additional month after the cutoff are more likely to attend private school by 0.17 *p.p.* at pre-K and 0.13 *p.p.* at kindergarten. It is expected that those in pre-K would be in private school because most pre-Ks are private, and if the child is born after the cutoff they would be in pre-K at a higher rate. The effect is slightly smaller at kindergarten but qualitatively similar.

[24] LEAs choosing the cutoff means that different school districts in New York, for example, have agency on their entry age rules.

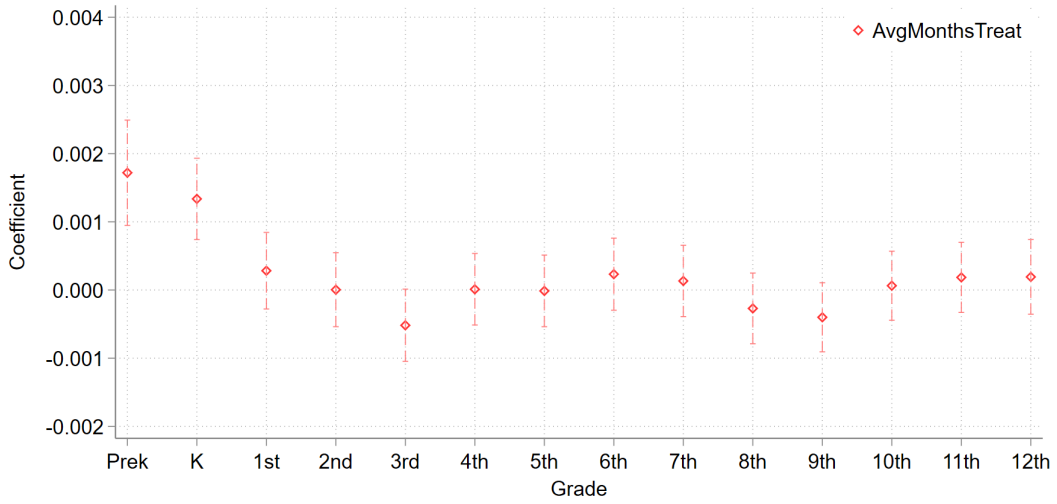
[25] See Colasanti (2007) for a 35-year comparison.

[26] Yearly reports in the Education Commission of the States (ECS): 2008–2009:http://www.ecs.org/clearinghouse/79/58/7958.pdf; 2010:http://www.ecs.org/clearinghouse/73/67/7367.pdf; 2011–2012:http://www.ecs.org/clearinghouse/82/58/8258.pdf. For 2018, I use the 2018 table in NCES https://nces.ed.gov/programs/statereform/tab5_3.asp. For 2019, I use the most recent table in NCES https://nces.ed.gov/programs/statereform/tab1_3-2020.as.

[27] State legislation: https://b5.caspio.com/dp.asp

[28] When the law says, for example, that started in the school year 2009–2010, I assign that school cutoff year 2009.

Figure 7: Effect of Average Months Away from Entrance Age Cutoff on Private School Attendance



Note. Coefficient and 95% confidence interval of the $AvgMonthsTreat$ variable in regression (2). The following covariates are included in the regression but excluded from the graph for brevity: age, dummies for male, Non-Hispanic white, Non-Hispanic black, and Hispanic. I also include year and state fixed effects. The analysis excludes children living in states that do not have a cutoff or that the Local Education Agency (LEA) decides their own rules regarding of entrance age. *Observations:* N= (361,787; 300,012; 290,479; 296,635; 305,591; 305,388; 310,046; 312,388; 311,129; 314,363; 314,274; 312,351; 303,325; 271,840)

5.2.1 Alternative Measures

The construction of $AvgMonthTreat$ might not fully account for the effect of being born after the cutoff. Using the September 1 cutoff as an example, the quarter of birth $JulAugSep$ includes August (which is only 1 month away from the cutoff) and September (12 months away from the cutoff), including both the children who need to wait for the least and the most months to start kindergarten. Thus, it does not account for the cutoff non-linearity effect due to more directly affecting the children born just after it.

I create three alternative measures. First, I use the maximum number of months away from the cutoff in a given quarter of birth to define the $MaxMonthsTreat$ variable.[29] The second variable uses a piecewise average approach by weighting the four months after the cutoff, assigning a value of four to the month just after the cutoff and a value of one to the fourth month away from the cutoff. For example, for the September 1 cutoff, September has a value of four, October is three, November is two, and December is one, while the rest of the months have a value of zero. I define $AvgDMonthTreat$ as the average of this variable for each quarter of birth.

The final variable uses the probability of being in kindergarten by month of birth based on the ECLSK2011 information to create $AvgPercMonthsTreat$, measuring how

[29]Children born in $JulAugSep$, with a September 1 cutoff, have a value of $MaxMonthsTreat = 12$.

likely a child will be in kindergarten if born after the cutoff. For the September 1 cutoff, $JanFebMar$ and $AprMayJun$ are equal to zero, while $JulAugSept$ and $OctNovDec$ are the average number of children (out of 100) in kindergarten born in a given quarter.

Figure A.13 shows the results of these three alternative variables, and they are qualitatively similar to those in Figure 7. Contrary to $AvgMonthsTreat$, the coefficients are not significant at pre-K while intensifying at kindergarten. Thus, considering the nonlinearity of the school cutoff effect highlights that the salient effect is at kindergarten. Although the results for grades 1 through 12 are more volatile with these measures, estimating equation (2) for grades 1 through 12 combined provides nonsignificant results.

6 Additional Evidence: Early Childhood Longitudinal Studies (ECLSK2011)

I supplement the main analysis using the ECLSK2011, a longitudinal study that follows students enrolled in kindergarten in fall 2010 to spring 2011 until spring 2016, which corresponds to fifth grade with a constant grade progression.[30] The survey is representative of children in kindergarten that year and includes both those in public and private schools.

This sample has various advantages that would add to the main findings. First, the month of birth is available, which allows me to separate those born in August and September. It also has information on the age at kindergarten entrance ($AgeKEntry$).[31] Even though this variable is endogenous, the correlation is informative since, under the mechanism that explains the main results, children who enter a private kindergarten should be younger than otherwise, which would mean a negative correlation. Finally, the panel feature of the data allows me to explore children's transitions over time. A couple of limitations is that the sample is somewhat small and attrition decreases the sample at higher grades due to children transferring to other schools.

For each child, I limit the sample to the first "grade" observation.[32] Additionally, I exclude children who did do not have detail regarding the grade they are in.[33] The final sample keeps those with information of the type of school, the month of birth, year of

[30]The follow-up data are collected by semester in the first two years and the spring semester after that: fall 2011, spring 2012, fall 2012, spring 2013, spring 2014, spring 2015, and spring 2016.

[31]This variable measures the age in months on September 1, 2010 if it was the child's first year in kindergarten. The reference year (in that case, 2010) varies based on parents' reports of their child's first kindergarten year.

[32]The raw database can have more than one observation by grade due to data collection, with the first two years collecting information by semester, and grade retention.

[33]After spring of 2012, children in a different grade from the one that corresponds with a standard grade progression have their grade information as *other*. Excluding these eliminates 1.5% of the total sample.

birth, race, and gender.[34]

Table 3 presents the unweighted summary statistics per grade, kindergarten through 5. The sample is half male, and most of them are born between May and August. In terms of *AgeKEntry*, children enter kindergarten at around 66 months of age, or 5.5 years. Finally, in line with the ACS sample, around 12% of the children in kindergarten are in private school, decreasing 2 percentage points at the first grade. Figure A.14 presents the percentage of private school enrollment by months of birth. Although there is some variation, the probability of being in a private school increases with the month of birth, being the largest for September and November. The correlation between the probability of being in private school and the month of birth is 0.59.

Table 3: Sample Description: ECLSK2011

	(1)	(2)	(3)	(4)	(5)	(6)
			Grade			
	K	1st	2nd	3rd	4rd	5th
Male	0.51	0.51	0.50	0.51	0.50	0.51
	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)
AgeKEntry	66.08	66.25	66.29	66.28	66.31	66.31
	(4.64)	(4.57)	(4.59)	(4.58)	(4.60)	(4.60)
EneFebMarApr	0.32	0.32	0.33	0.33	0.32	0.32
	(0.47)	(0.47)	(0.47)	(0.47)	(0.47)	(0.47)
MayJunJulAug	0.35	0.34	0.34	0.34	0.34	0.34
	(0.48)	(0.47)	(0.47)	(0.47)	(0.47)	(0.47)
SepOctNovDec	0.33	0.33	0.33	0.33	0.33	0.33
	(0.47)	(0.47)	(0.47)	(0.47)	(0.47)	(0.47)
Private	0.12	0.10	0.09	0.09	0.09	0.09
	(0.33)	(0.30)	(0.29)	(0.29)	(0.29)	(0.29)
N	18,084	14,857	13,194	12,187	11,317	10,699

Note. This table presents the mean and standard deviations (in parenthesis) of the main variables from the ECLSK2011 sample. N represents the number of observation.

I estimate equation (1) by grade with three different variables as my explanatory variable of interest instead of the quarter of birth dummies.[35] First, I use *SepDec*, a dummy equal to one if the month of birth is from September to December and zero otherwise. Second, based on their month of birth, I construct the months away from September 1, $MonthToCutoff$.[36] Finally, I use the age at kindergarten entry, $AgeKEntry$, as the variable of interest.

Table 4 shows the results. Columns (1)–(6) present the information for each grade available. Panel A presents the information when using *SepDec*, followed by

[34]Excluding those without this information eliminates an additional 0.3% of the total sample.
[35]In this estimation, the control variables are: male dummy, year of birth dummies, race dummies.
[36]For example, this variable is equal to 1 and 12 for those born in August and September, respectively.

$MonthToCutoff$ in Panel B and $AgeKEntry$ in Panel C. Looking at Panel A, the results for kindergarten are that those born from September to December are 3.06 *p.p.* more likely to be in a private kindergarten. This effect does not persist at later grades, and the coefficients even become negative. In Panel B, although $MonthToCutoff$ is not significant across grades, the sign points toward the expected direction, with those away from the cutoff being more likely to be in a private kindergarten. Finally, in Panel C, as expected, there is a negative correlation between $AgeKEntry$ and private school attendance. This negative correlation is quantitatively stronger at kindergarten and decreases after that, which is in line with some children transferring to public school as they progress through grades.

Table 4: Effects of Treatment Measures on Private School Attendance

	(1)	(2)	(3)	(4)	(5)	(6)
			Grade			
	K	1st	2nd	3rd	4rd	5th
Panel A: Month of Birth						
SepDec	0.0306**	-0.00951	-0.0134	-0.0122	-0.0154	-0.0151
	(0.0133)	(0.0102)	(0.0102)	(0.0105)	(0.0108)	(0.0111)
N	18,084	14,857	13,194	12,187	11,317	10,699
Panel B: Away from Sept 1st						
$MonthToCutoff$	0.00160	-0.00163*	-0.00145	-0.00144	-0.00164	-0.00131
	(0.00116)	(0.000980)	(0.00101)	(0.00105)	(0.00108)	(0.00111)
N	18,084	14,857	13,194	12,187	11,317	10,699
Panel C: Age at Entry						
$AgeKEntry$	-0.00806***	-0.00418**	-0.00351*	-0.00326*	-0.00284	-0.00273
	(0.00189)	(0.00178)	(0.00181)	(0.00176)	(0.00178)	(0.00173)
N	15,805	12,942	11,525	10,661	9,893	9,354

Note. Effect of treatment variables on Private School Attendance. *Columns:* Grade that the child is in, from kindergarten to 5th grade. *Panels:* Panel A presents the results using $SepDec$, a variable equal to one if the child is born in the months September–December; panel B presents the results using $MonthtoCutoff$, a variable equal to the number of months away from September 1 that the child is born; panel C presents the results using $AgeKEntry$, a variable equal to the age at kindergarten entry measured in months. The following covariates are included in the regression but excluded from the table for brevity: male dummy, year of birth dummies, and race dummies. Standard errors cluster at the school level in parentheses; *p<0.10, **p<0.05, ***p<0.010.

An important cautionary note is that when looking at the higher grades, attrition is not random. The survey did not follow all the children who moved to another school in higher grades. Because of this, many children who might have started in a private school and moved to a public school, for example, might not be present in the sample at a higher grade.

Robustness Checks. As robustness checks, I first look at the heterogeneous effect by year of birth, 1) 2003–2004 and 2) 2005–2006, to corroborate that the effect should be

stronger for the youngest kids. The results in Table A.2 are in line with the expected results, with the results for children born in 2005–2006 having the expected signs and driving the main results. The second robustness check determines if there is an effect of including sample weights. I analyze using the *unadjusted weights*, which are the weights assigned to the children in the base sample (at kindergarten), as well as the weights that adjust for nonresponse in the following years. Table A.3 shows the results for *unadjusted weights* in Panel A and *adjusted weights* in Panel B. The results are qualitatively similar to the main results in Table 4.

Transition to Public School. Taking advantage of the data's panel characteristics, I further explore if those in a private kindergarten are more likely to transition to public school. The mechanism explaining the main results suggests that children who attend a private kindergarten due to being born after the cutoff move to public schools in higher grades. In that case, I should find a positive relationship between the variable of interest and the probability of being in public schools in grades one through five. To analyze this, for the first through fifth grades, I estimate

$$PublicS_i = \gamma_0 DateBirth_i + X_i^{'}\beta + \epsilon_i \tag{3}$$

for those who were in a private kindergarten. The variable $DateBirth_i$ is measured as in Table 4.

Table A.4 shows the results. Although I do not have a statistically significant effect, the sign of the coefficients points toward those born after the cutoff being more likely to be in public schools. Additionally, the coefficients, especially for $SepDec$ and $AgeKEntry$, quantitatively increase as grades increase. This result is in line with an increasing number of children who went to private kindergarten and transferred to public school as they progressed through grades. The results in Table A.4 likely underestimate the real effect. Due to the attrition created by children moving to different schools, those who transfer from a private to a public school are less likely to be in the sample at higher grades.[37]

The findings using the ECLSK2011 are qualitatively similar to the main results in Figure 3, giving more confidence to the main findings due to the more precise date of birth and additional information such as the age at kindergarten entrance. Additionally, the transition to public school points toward these children transferring to public school at higher grades. Thus, ECLSK2011 results pointing toward the mechanism discussed in the main findings, with children born late in the year being more likely to be in a private school at kindergarten and transferring to public school as they progress through grades.

[37] At kindergarten, running a regression of *attrition* (= 1 if I observe the child less than six times) on private school dummy gives a significant coefficient of 0.0794.

7 Early Childhood Education Policies and Mechanisms

The salient effect of the quarter of birth on private schooling at kindergarten suggests that parents choose a private school to give their children a head start into formal schooling. Consequently, it is relevant to think about how this choice is affected by alternative early childhood education options that parents of kindergarten-age children might face. In this section, I exploit how the availability of state-funded pre-K and TK interacts with private school attendance in kindergarten.

7.1 State-Funded Pre-K

For children turning five years old after the cutoff, going to pre-K is an alternative to being pushed ahead. Consequently, access to public pre-K could play an important role in private kindergarten attendance. I exploit the variation in access to state-funded pre-K to classify states between those with *mostly universal* and *limited* pre-K. Based on the 2018 ECS report on pre-K funding, nine states and DC have mostly universal pre-K.[38] The rest of the states have either no state-funded pre-K or a pre-K that does not broadly serve children.

Pre-K interacts with private school attendance in the following way. Children born too late to enter a public kindergarten could instead attend pre-K for a year while they wait to attend kindergarten the following year. With public pre-K widely available, parents might choose that option instead of sending their children to private kindergarten. Thus, the main results for kindergarten should be smaller or disappear for those children in states with mostly universal state-funded pre-K.

At kindergarten, I estimate equations (1) and (2) separately by type of public pre-K access; Table 5 shows the estimation results. The first two columns present the main results for kindergarten, while columns (3) and (4) present the results for states with mostly universal pre-K. The final two columns, columns (5) and (6), present the results for states with limited pre-K. For each sample, the first column presents the estimation results using the quarter of birth, while the second column presents the results using $AvgMonthTreat$ as the variable of interest.

The results are qualitatively similar to the main findings, no matter the level of access to public pre-K. When estimating using the quarter of birth, there is a slight variation in the significance of $JulAugSep$, but the results are not quantitatively different from each other. Similarly, when estimating using the $AvgMonthTreat$ (columns (2), (4), and (6)), the effect of being born one additional month after the cutoff on the probability of

[38]The states are Oklahoma, West Virginia, Vermont, Florida, Georgia, Illinois, Iowa, New York, and Wisconsin. See https://www.ecs.org/wp-content/uploads/How-States-Fund-Pre-K_A-Primer-for-Policymakers.pdf.

attending a private kindergarten is similar across types of state-funded pre-K programs. The results suggest that access to mostly universal public pre-K does not affect the use of private kindergarten as a way to push "late-born" children into formal schooling.

Alternative Measure. I also measure pre-K "universality" by the percentage of four-year-olds served per year. Using the National Institute of Early Education Research (NIEER) 2019 report[39] of the percentage of four-year-olds enrolled in a state-funded pre-K, I define mostly universality states as those serving more than 50% of the four-year-olds.[40] The results in Table A.5 are qualitatively similar to the results using the ECS reports in Table 5.

Table 5: Effect of Date of Birth on Private Kindergarten Attendance by Type of Pre-K Access

	(1)	(2)	(3)	(4)	(5)	(6)
	All		**Mostly Universal Pre-K**		**Limited Pre-K**	
JulAugSep	0.00368**		0.00263		0.00393*	
	(0.00185)		(0.00407)		(0.00207)	
OctNovDec	0.0109***		0.0116***		0.0104***	
	(0.00192)		(0.00417)		(0.00215)	
JanFebMar	0.00283		0.00479		0.00219	
	(0.00189)		(0.00414)		(0.00212)	
AvgMonthTreat		0.00134***		0.00170**		0.00123***
		(0.000305)		(0.000736)		(0.000334)
N	374,473	300,012	88,532	66,785	285,941	233,227

Note. Effect of the quarter of birth and *AvgMonthTreat* on private school attendance at kindergarten estimated by regression (1) and (2), respectively. *Columns:* columns (1) and (2) present the results for the complete sample; columns (3) and (4) present the results for states with mostly-universal publicly funded pre-K; columns (5) and (6) present the results for the rest of states. The following covariates are included in the regression but excluded from the table for brevity: age, dummies for male, Non-Hispanic white, Non-Hispanic black, and Hispanic. I also include year and state fixed effects. N represents the number of observations. The analysis using *AvgMonthTreat* excludes children living in states that do not have a cutoff or that the Local Education Agency (LEA) decides their own rules regarding of entrance age. Standard errors cluster in parentheses; *p<0.10, **p<0.05, ***p<0.010.

7.2 Transitional Kindergarten

California and Michigan offer a public alternative for those born at the end of the year.[41] When California moved the cutoff to September 1 in 2012, it implemented the

[39]Source: https://nieer.org/state-preschool-yearbooks.

[40]States included Vermont, New York, Wisconsin, Iowa, Florida, Georgia, Oklahoma, West Virginia. It also includes DC.

[41]There are other exceptions. For example, many states allow children who were in kindergarten in other states to transfer over. Also, various states permit school districts to allow "gifted" younger children to enter kindergarten. I do not consider these options because they are either not general enough or are not at the state level. See the list of waivers by states: https://reports.ecs.org/comparisons/state-

first public and widely available TK. The TK program bridges pre-K to kindergarten and serves children born between September 1 and December 2. Similarly, Michigan set its cutoff to September 1 in 2015 but allows alternative options for children born by December 1. These late-born children can either start kindergarten[42] or can enter Michigan's version of TK.

Table 6: Effect of Date of Birth on Private Kindergarten Attendance by TK

	(1)	(2)	(3)	(4)	(5)	(6)
	All		**No Access to TK**		**Access to TK**	
$JulAugSep$	0.00368**		0.00512**		-0.00248	
	(0.00185)		(0.00203)		(0.00439)	
$OctNovDec$	0.0109***		0.0150***		-0.00611	
	(0.00192)		(0.00213)		(0.00431)	
$JanFebMar$	0.00283		0.00308		0.00145	
	(0.00189)		(0.00207)		(0.00457)	
$AvgMonthTreat$		0.00134***		0.00176***		-0.000325
		(0.000305)		(0.000345)		(0.000636)
N	374,473	300,012	318,173	243,712	56,300	56,300

Note. Effect of the quarter of birth and $AvgMonthTreat$ on private school attendance at kindergarten estimated by regression (1) and (2), respectively. *Columns:* columns (1) and (2) present the results for the complete sample; columns (3) and (4) present the results for states with no access to publicly funded TK; columns (5) and (6) present the results for states with publicly funded TK (California and Michigan). The following covariates are included in the regression but excluded from the table for brevity: age, dummies for male, Non-Hispanic white, Non-Hispanic black, and Hispanic. I also include year and state fixed effects. N represents the number of observations. The analysis using $AvgMonthTreat$ excludes children living in states that do not have a cutoff or that the Local Education Agency (LEA) decides their own rules regarding of entrance age. Standard errors cluster in parentheses; *p<0.10, **p<0.05, ***p<0.010.

The expected role is that with TK as an alternative option for children born at the end of the year, the effect of being born in $JulAugSep$ and $OctNovDec$ on private school at kindergarten should decrease compared with the main results. To explore this effect, I estimate equations (1) and (2) by type of access to publicly available TK. Table 6 shows the results. The first two columns show the main results for kindergarten, using the quarter of birth (column (1)) and the $AvgMonthTreat$ (column (2)). Columns (3) and (4) show the results for the states with no publicly available TK, and columns (5) and (6) present the results for the TK states.

When looking at the quarter of birth regressions for TK states in column (5), the

k-3-policies-09

[42] If parents sign a waiver. Michigan state legislation from 2010 to 2016: https://b5.caspio.com/dp.asp?AppKey=b7f93000dc11a73b394a43d5b019&st=MI

results are not statistically significant and are negative for the quarters affected by the TK policies ($JulAugSep$ and $OctNovDec$). The results for $JanFebMar$ are not significant but both positive and qualitatively similar, aligning with this quarter not being affected by access to TK. The results when using $AvgMonthTreat$, in column (6), shows that in the presence of TK, being born an additional month after the cutoff does not increase the likelihood of attending a private kindergarten.

The results are in line with parents not choosing private school at kindergarten as much in states with access to public TK. Combining these results with the pre-K results, in Section 7.4 I discuss the mechanisms suggested by these findings.

7.3 Alternative Analysis

An alternative way to analyze the effect of mostly universal state-funded pre-K and TK is to think of states with these alternative policies as "treated" while keeping the rest of the states as a "control." At kindergarten, I estimate the following model:

$$\begin{aligned} PrivateSchool_{ist} = \gamma' DateBirth_i + \theta_0 Treat_s + \delta' Treat_s \times DateBirth_i \\ + \beta' X_i + \alpha_t + \epsilon_{ist}, \end{aligned} \tag{4}$$

where $Treat$ depends on the policies analyzed, pre-K or TK. $DateBirth$ includes the variables measuring when children are born, which varies by policy type and the model estimated. The coefficients of interest, δ', measure the effect of living in a treated state and being treated by the cutoff, compared with the control group.

When analyzing the effect of state-funded pre-K, $Treat_s$ is equal to one if the child lives in a state with mostly universal pre-K, defined as in Table 5. The variables included in $DateBirth_i$ are $JulAugSep_i$, $OctNovDec_i$ and $FebMar_i$. For TK, the variable $Treat_s$ is equal to one if the child lives in California/Michigan.[43] Since TK targets children born in September through December, $DateBirth_i$ is $JulAugSep_i$ and $OctNovDec_i$. For both policies, I also estimate the model using $AvgMonthTreat_i$ as the alternative measure of $DateBirth_i$.

The expected sign of the coefficients of interest, δ, should be negative but with differences in impact by policy based on the findings before. When pre-K is the treatment, the coefficients should not be significant and quantitatively small. On the contrary, when TK is the treatment, the coefficients should be negative, significant, and quantitatively large.

Table 7 shows the results. Columns (1) and (2) present the results when pre-K is the treatment, while columns (3) and (4) present the results using TK. The results align with the findings before. Mostly universal pre-K results are not significant, with results

[43] Before the policy change (2012/2015), the cutoff was in December, and the use of private schools in these states might already be lower for late-born children. Thus, the treatment can be at the state level.

being negative for the quarter of birth and positive for the $AvgMonthTreat$, but both are qualitatively equal to zero. In the case of TK, in columns (3) and (4), the effects are negative and, for columns (3), strongly significant. Quantitatively, the largest coefficient is for $Treat \times OctNovDec$. These results suggest that while access to mostly universal pre-K does not affect private kindergarten attendance, the contrary is true for TK, which decreases private kindergarten attendance compared with states that do not have it.

Table 7: Effect of Access to Pre-K/TK on the Impact of Date of Birth on Private Kindergarten Attendance

	(1)	(2)	(3)	(4)
	Mostly Universal Pre-K		**Transitional Kindergarten**	
$Treat \times JulAugSep_i$	-0.00158		-0.0107***	
	(0.00473)		(0.00273)	
$Treat \times OctNovDec_i$	-0.00156		-0.0204***	
	(0.00539)		(0.00336)	
$Treat \times JanFebMar_i$	-0.000372			
	(0.00401)			
$Treat \times AvgMonthTreat_i$		0.000189		-0.00121
		(0.000511)		(0.000931)
N	374,473	300,012	374,473	300,012

Note. Effect of access to mostly-universal pre-K and transitional kindergarten on private school attendance at kindergarten estimated by regression (4). *Columns:* columns (1) and (2) present the results for access to mostly-universal publicly funded pre-K; columns (3) and (4) present the results for access to publicly funded TK (California and Michigan). The following covariates are included in the regression but excluded from the graph for brevity: age, dummies for male, Non-Hispanic white, Non-Hispanic black, and Hispanic. I also include year and state fixed effects. N represents the number of observations. The analysis using $AvgMonthTreat$ excludes children living in states that do not have a cutoff or that the Local Education Agency (LEA) decides their own rules regarding of entrance age. Standard errors clustered at the state level in parentheses; *p<0.10, **p<0.05, ***p<0.010.

7.4 Early Childhood Education Policies and Mechanisms

From the results above, we have that while the parents in states with public TK use private school at kindergarten significantly less, access to mostly universal pre-K does not meaningfully affect the likelihood to attend private school. Although there might be many reasons behind school choices, these alternative early childhood education policies help us to shed light on the mechanisms behind why parents choose to use private kindergarten.

The muted effect of state-funded pre-K on private kindergarten attendance, combined with TK decreasing the use of private school at kindergarten, points toward parents choosing private kindergarten while keeping in mind the type of education their child would receive. Note that TK programs cover kindergarten material and are taught by

a teacher with the same qualifications as a kindergarten teacher. Therefore, TK allows children to access a more advanced education than pre-K would offer.

Thus, the differences between TK's and pre-K's effect on private kindergarten attendance align with parents considering the education's characteristics when choosing what to do with their late-born children. If parents were not worried about the type of education, state-funded universal pre-K would have a more meaningful effect on private school attendance than what we see. It must be that parents of late-born children limited by the school cutoff think they are ready for kindergarten. Anecdotally, some parents do seem to feel this way:

> *"...My daughter turned 5 in December so she was not allowed to start kindergarten in public school. But I, as her mother who knows her best, felt she was ready so I took her to a private school where she was tested and deemed ready for kindergarten. She has not had academic or behavioral problems.... "* [44]

> *"...I just registered my son today in private kindergarten. His birthday is the 7th and the cut-off is the 1st. He's also very anxious to start school and is definitely ready. ... "* [45]

Parents who choose private kindergarten to advance their late-born children do so because other public alternatives do not fit what they want their children to access. However, publicly funded TK provides an alternative that closely corresponds to a private kindergarten for late-born children through public school instead. Thus publicly funded TK (and having more flexible cutoffs or state-funded alternative options like TK) grants children born after the cutoff, from all backgrounds, access a more advanced curriculum earlier childhood education than a pre-K would.

8 Conclusion

This paper explores the effect of the quarter of birth on the probability of attending a private school. Since state laws impose an age cutoff on kindergarten entrance, children's date of birth determines when they can start school. Because private schools do not need to comply with entrance age cutoffs, I explore if parents use private schooling as a substitute when their children are born late in the year.

Using the 2008–2019 ACS database, I analyze children who are 3–18 years old and are attending pre-K through 12 grades. I find that children's quarter of birth affects the probability of attending a private school, with a salient effect at kindergarten. Compared with those born in April–June, the probability of attending a private school increases for

[44] Comment by Ruth (April 26, 2016): https://www.cultofpedagogy.com/academic-redshirting/

[45] Comment by TheGoodLife, posted July 30, 2009: https://community.babycenter.com/post/a12730095/miss_the_kindergarten_cut-off_but_more_than_ready

those born in July–December at kindergarten. The effect does not persist in later grades, and therefore after children enter K–12, their date of birth does not affect how likely they are to attend a private school. The results are robust across a large number of analyses.

The pattern I find suggests that for parents of children born after the kindergarten entrance age cutoff, private schools function to circumvent the cutoff constraint and to allow their children to be enrolled earlier. Furthermore, these parents transfer their children to public schools at higher grades when the cutoff is not limited.

I further explore how the effect of date of birth on private kindergarten attendance is affected by access to alternative early childhood education choices: state-funded pre-K and TK. Mostly universal public pre-K does not affect private kindergarten, while TK does suggest that parents' choice of private kindergarten is due to the level of education the children would have access to.

This paper explores factors affecting private school attendance and influencing parents' decisions regarding early schooling choices. In this case, I find an unintended effect of school cutoff laws that incentivize parents to use private schooling as an option for earlier enrollment. Additionally, when comparing children's education outcomes based on their birth date, it must be considered that some of the older children (born in the fall) might benefit from private schooling, which could affect their educational outcomes. If private kindergarten provides some skill advantages to children, there will be differential human capital accumulation.

A theoretical framework of skill accumulation that includes empirical evidence on early childhood education points toward earlier investment in education, boosting the productivity of educational investment later in life.[46] Moreover, there is evidence that early childhood intervention, including kindergarten, can affect test-score and long-term outcomes.[47] Keeping this in mind, to the extent that private kindergartens are of high quality and offer differentiated characteristics, and that an earlier boost on skill formation have a larger return, the school cutoff would have an unintended effect. Those children who started kindergarten early would disproportionately benefit from being *"late-born"* by acquiring distinctive skills and being better prepared for later education. This effect would create disparity with those same-age children who waited to start in a public kindergarten, especially those from disadvantaged backgrounds. Future research would aim to explore the welfare effect of private schooling on children's outcomes.

[46] See Cunha & Heckman (2007) for the framework and Elango *et al.* (2016) for a summary of the early intervention literature, especially in preschool.

[47] For early childhood intervention that includes kindergarten, see Schanzenbach (2006) and Chetty *et al.* (2011).

References

Angrist, Joshua D., & Krueger, Alan B. 1991. Does Compulsory School Attendance Affect Schooling and Earnings? *The Quarterly Journal of Economics*, **106**(4), 979–1014.

Bahrs, Michael, & Schumann, Mathias. 2020. Unlucky to be Young? The Long-Term Effects of School Starting Sge on Smoking Behavior and Health. *Journal of Population Economics*, **33**(2), 555–600.

Bassok, Daphna, & Reardon, Sean F. 2013. Academic Redshirting in Kindergarten: Prevalence, Pat- terns, and Implications. *Educational Evaluation and Policy Analysis*, **35**(3), 283–297.

Bedard, Kelly, & Dhuey, Elizabeth. 2006. The Persistence of Early Childhood Maturity: International Evidence of Long-Run Age Effects. *Quarterly Journal of Economics*, **121**(4), 1437–1472.

Bedard, Kelly, & Dhuey, Elizabeth. 2012. School-Entry Policies and Skill Accumulation Across Directly and Indirectly Affected Individuals. *Journal of Human Resources*, **47**(3), 643–683.

Black, Sandra E, Devereux, Paul J, & Salvanes, Kjell G. 2011. Too Young to Leave The Nest? The Effects of School Starting Age. *The Review of Economics and Statistics*, **93**(2), 455–467.

Bound, J., & Jaeger, D. A. 1996. On the Validity of Season of Birth as an Instrument in Wage Equations: A Comment on Angrist & Krueger's Does Compulsory School Attendance Affect Schooling and Earnings?. *NBER Working Paper*, Bound, J., & Jaeger, D. A. (1996). On the Validity.

Buckles, Kasey S., & Hungerman, Daniel M. 2013. Season of Birth and Later Outcomes: Old Questions, New Answers. *Review of Economics and Statistics*, **95**(3), 711–724.

Cascio, Elizabeth U, & Lewis, Ethan G. 2006. Schooling and the AFQT: Evidence from School Entry Laws. *The Journal of Human Resources*, **41**(2), 294–318.

Cascio, Elizabeth U., & Schanzenbach, Diane Whitmore. 2016. First in The Class? Age and The Education Production Function. *Education Finance and Policy*, **11**(3), 225–250.

Chetty, Raj, Friedman, John N., Hilger, Nathaniel, Saez, Emmanuel, Schanzenbach, Diane Whitmore, & Yagan, Danny. 2011. How Does Your Kindergarten Classroom Affect Your Earnings? Evidence from Project Star. *Quarterly Journal of Economics*, **126**(4), 1593–1660.

Colasanti, Michael. 2007. *Kindergarten Entrance Ages: A 30 Year Trend Analysis*. Tech. rept. March.

Conley, Dalton, & Glauber, Rebecca. 2006. Parental Educational Investment and Children's Academic Risk: Estimates of the Impact of Sibship Size and Birth Order from Exogenous Variation in Fertility. *Journal of Human Resources*, **41**(4), 722–737.

Cook, Philip, & Kang, Songman. 2018. The School-Entry-Age Rule Affects Redshirting Patterns and Resulting Disparities in Achievement. *NBER Working Paper*, 1–42.

Cook, Philip J., & Kang, Songman. 2016. Birthdays, Schooling, and Crime: Regression-Discontinuity Analysis of School Performance, Delinquency, Dropout, and Crime Initiation. *American Economic Journal: Applied Economics*, **8**(1), 33–57.

Cunha, Flavio, & Heckman, James. 2007. The Technology of Skill Formation. *American Economic Review*, **97**(2), 31–47.

Datar, Ashlesha. 2006. Does Delaying Kindergarten Entrance Give Children a Head Start? *Economics of Education Review*, **25**(1), 43–62.

Davis, B. Glen, Trimble, C. Scott, & Vincent, Denny R. 1980. Does Age of Entrance Affect School Achievement? *The Elementary School Journal*, **80**(3), 133–143.

Deming, David, & Dynarski, Susan. 2008. The Lengthening of Childhood. *Journal of Economic Perspectives*, **22**(3), 71–92.

Dhuey, Elizabeth, Figlio, David, Karbownik, Krzysztof, & Roth, Jeffrey. 2019. School Starting Age and Cognitive Development. *Journal of Policy Analysis and Management*, **38**(3), 538–578.

DiPrete, Thomas A., & Jennings, Jennifer L. 2012. Social and Behavioral Skills and The Gender Gap in Early Educational Achievement. *Social Science Research*, **41**(1), 1–15.

Dobkin, Carlos, & Ferreira, Fernando. 2010. Do School Entry Laws Affect Educational Attainment and Labor Market Outcomes? *Economics of Education Review*, **29**(1), 40–54.

Dynarski, Susan M., Gruber, Jonathan, & Li, Danielle. 2009. Cheaper by the Dozen: Using Sibling Discounts at Catholic Schools to Estimate the Price Elasticity of Private School Attendance. *NBER Working Paper*, 1–30.

(ECS), Education Commission of the States. *State Policy Database, retrieved Jan 26 2021*.

Elango, S., J. L. García, J., Heckman, J., & Hojman, A. 2016. Early Childhood Education In R. A. Moffitt (Ed.). *Economics of Means-Tested Transfer Programs in the United States*, **Volume 2**(Chapter 4), 235–297.

Elder, Todd E., & Lubotsky, Darren H. 2009. Kindergarten Entrance Age and Children's Achievement: Impacts of State Policies, Family Background, and Peers. *Journal of Human Resources*, **44**(3), 641–683.

Fredriksson, Peter, & Öckert, Björn. 2014. Life-Cycle Effects of Age at School Start. *The Economic Journal*, **124**(579), 977–1004.

Landersø, Rasmus, Nielsen, Helena Skyt, & Simonsen, Marianne. 2017. School Starting Age and the Crime-age Profile. *Economic Journal*, **127**(602), 1096–1118.

Leigh, Andrew, & Ryan, Chris. 2008. Estimating Returns to Education Using Different Natural Experiment Techniques. *Economics of Education Review*, **27**(2), 149–160.

Leuven, Edwin, & Rønning, Marte. 2016. Classroom Grade Composition and Pupil Achievement. *Economic Journal*, **126**(593), 1164–1192.

McCrary, Justin, & Royer, Heather. 2011. The Effect of Female Education on Fertility and Infant Health: Evidence from School Entry Policies Using Exact Date of Birth. *American Economic Review*, **101**(1), 158–195.

McEwan, Patrick J., & Shapiro, Joseph S. 2008. The Benefits of Delayed Primary School Enrollment: Discontinuity Estimates Using Exact Birth Dates. *Journal of Human Resources*, **43**(1), 1–29.

Oosterbeek, Hessel, ter Meulen, Simon, & van Der Klaauw, Bas. 2020. Long-Term Effects of School-Starting-Age Rules. *IZA Discussion Papers*.

Peña, Pablo A. 2019. Relative Age and Incarceration: Born on The Wrong Side of the Calendar. *Education Economics*, **27**(6), 588–607.

Puhani, Patrick, & Weber, Andrea. 2007. Does the Early Bird Catch the Worm? Instrumental Variable Estimates of Educational Effects of Age of School Entry in Germany. *Empirical Economics*, **32**(2-3), 359–386.

Ruggles, Steven, Flood, Sarah, Goeken, Ronald, Grover, Josiah, Meyer, Erin, Pacas, Jose, & Sobek, Matthew. 2020. *IPUMS USA: Version 10.0 [ACS]. Minneapolis, MN: IPUMS, 2020. https://doi.org/10.18128/D010.V10.0.*

Schanzenbach, Diane Whitmore. 2006. What Have Researchers Learned from Project STAR? *Brookings Papers on Education Policy*, **9**, 205–228.

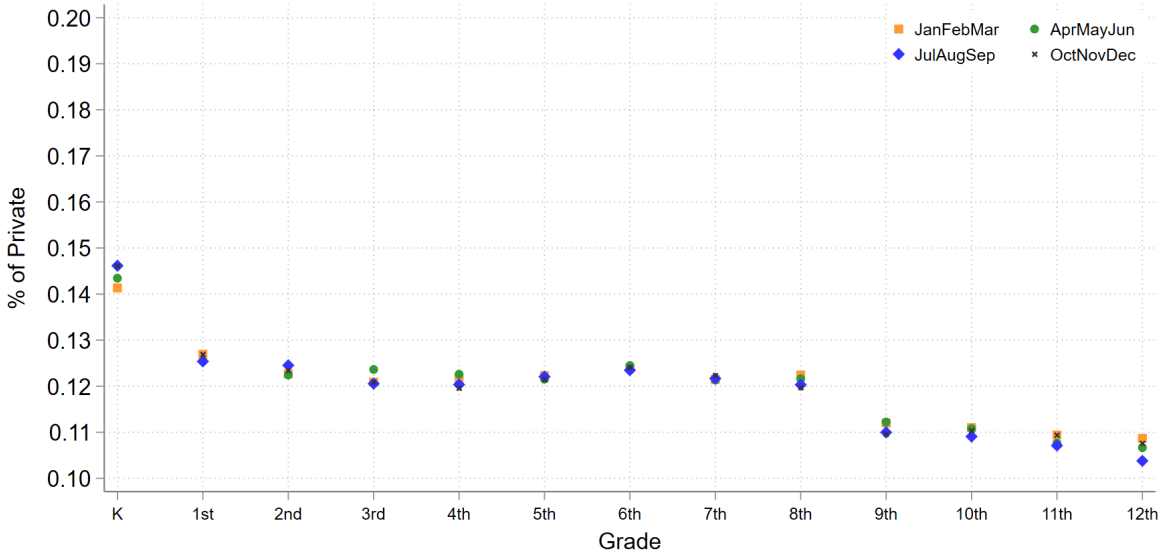
Schanzenbach, Diane Whitmore;, & Howard, Stephanie Larson. 2017. Is Your Child Ready for Kindergarten? *Education Next*, **17**(3), 18–24.

Sprietsma, Maresa. 2010. The Effect of Relative Age in the First Grade of Primary School on Long-Term Scholastic Results: International Comparative Evidence using PISA 2003. *Education Economics*, **18**(1), 1–32.

Stipek, Deborah. 2002. At What Age Should Children Enter Kindergarten? A Question for Policy Makers and Parents. *Social Policy Report*, **16**(2), 1–20.

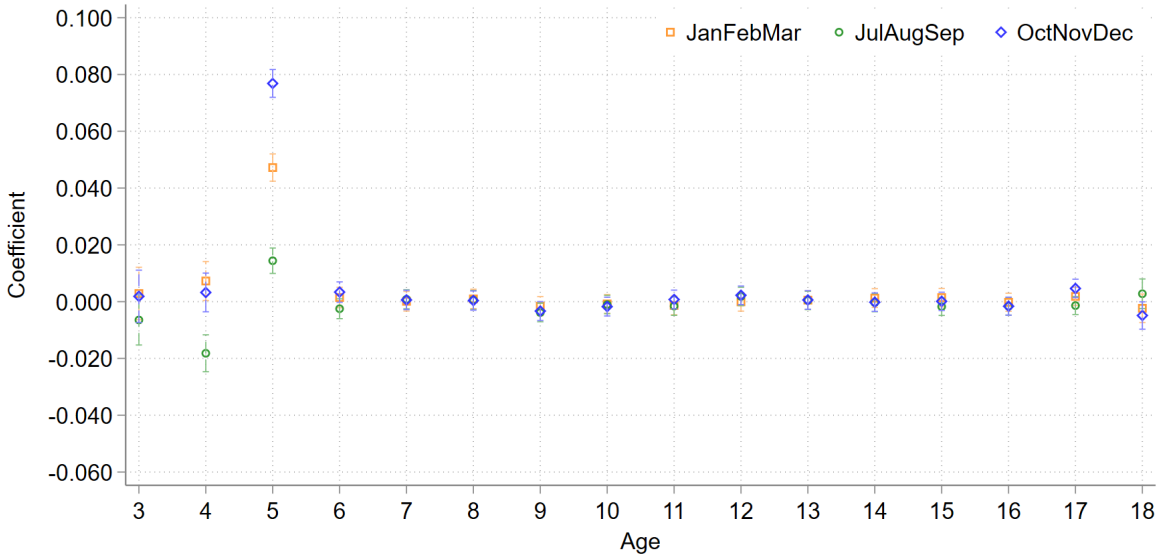
Appendix

Figure A.1: Percentage of Children Attending Private School by Grade $K - -12$ and Quarter of Birth



Note. This presents percentage of children attending private school from kindergarten to grade 12th.

Figure A.2: Effect of Quarter of Birth on Private School Attendance by Age



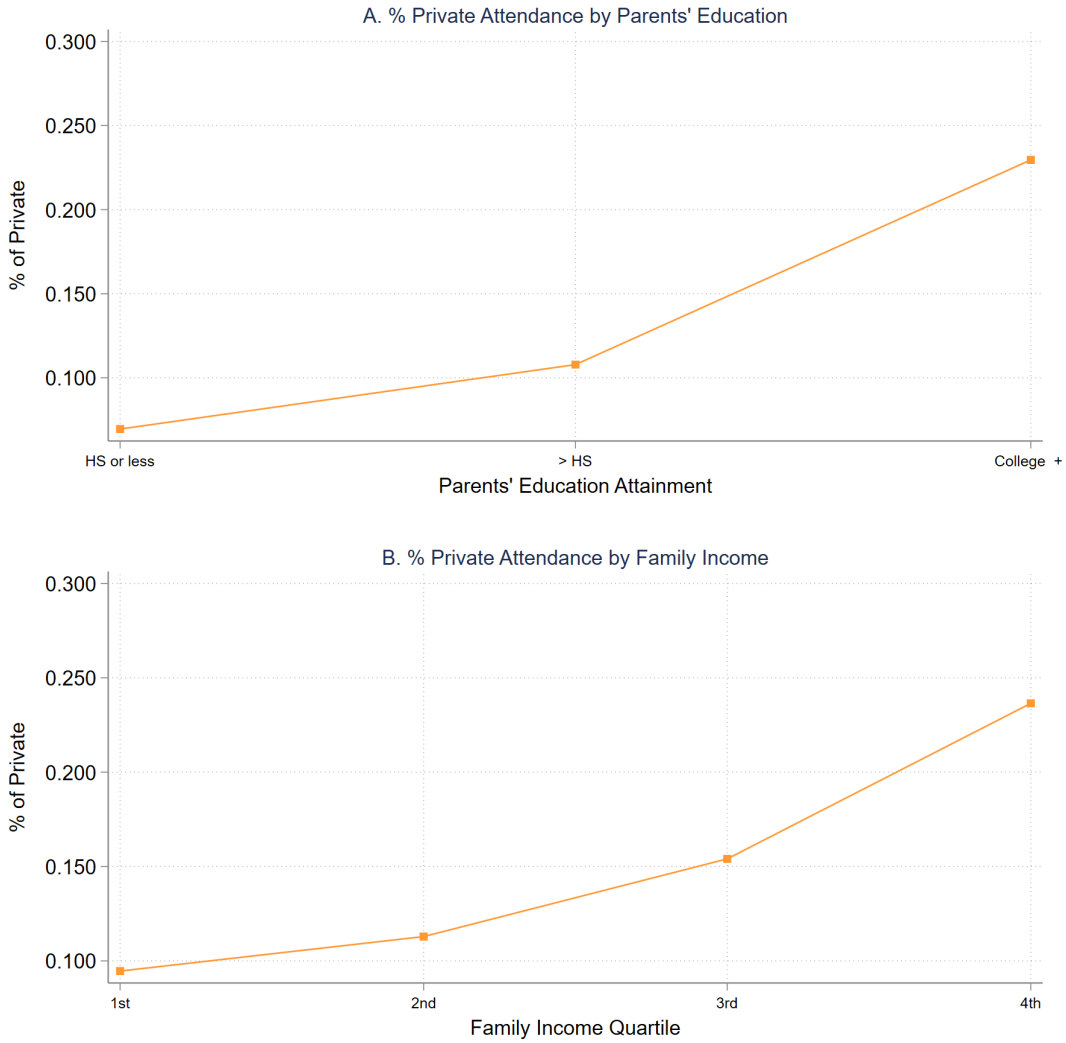
Note. Coefficient and 95% confidence interval of the quarter of birth variables in regression (1). The following covariates are included in the regression but excluded from the graph for brevity: age, dummies for male, Non-Hispanic white, Non-Hispanic black, and Hispanic. I also include year and state fixed effects. *Observations:* N= (129,622; 226,074; 316,076; 356,603; 366,753; 373,170; 378,528; 385,212; 385,820; 389,871; 391,780; 392,554; 392,448; 390,103; 373,600; 177,848)

Table A.1: Effect of Quarter of Birth on School Expenditure at Kindergarten

	Sample Mean (Standard Deviation) (1)	**Coefficient (Standard Error)** (2)
Spending	1,278.28 (3,352.27)	
$JulAugSep$		40.73** (16.92)
$OctNovDec$		103.0*** (17.65)
$JanFebMar$		30.42* (17.37)
N	374,473	374,473

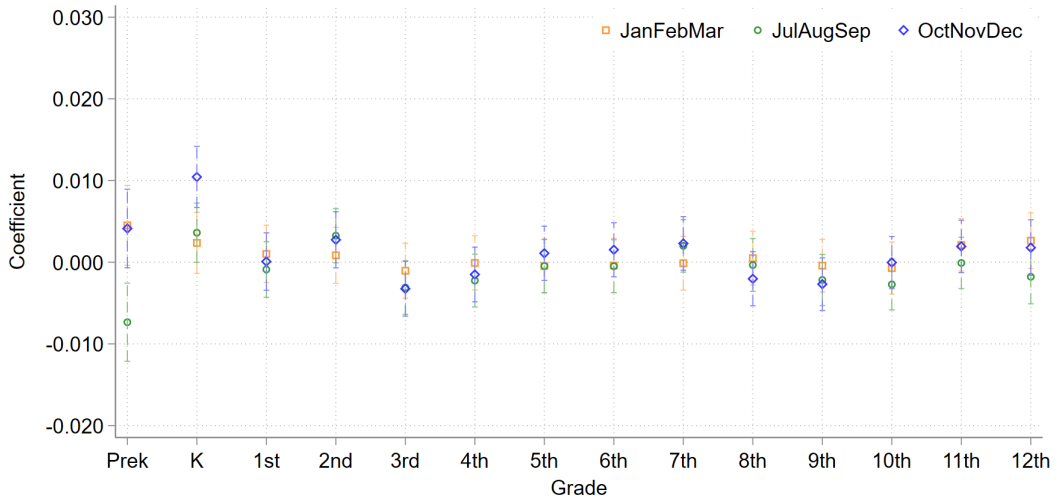
Note. The table shows the mean and standard deviation in parenthesis of price in column (1) and the estimation of the effect of quarter of birth on price of schooling in column (2). The following covariates are included in the regression in column (2) but excluded from the graph for brevity: age, dummies for male, Non-Hispanic white, Non-Hispanic black, and Hispanic. I also include year and state fixed effects. N represents the number of observations. The analysis using $AvgMonthTreat$ excludes children living in states that do not have a cutoff or that the Local Education Agency (LEA) decides their own rules regarding of entrance age. Standard errors cluster in parentheses; *p<0.10, **p<0.05, ***p<0.010.

Figure A.3: Percentage of Children Attending Private School by Parents' Education and Family's Income



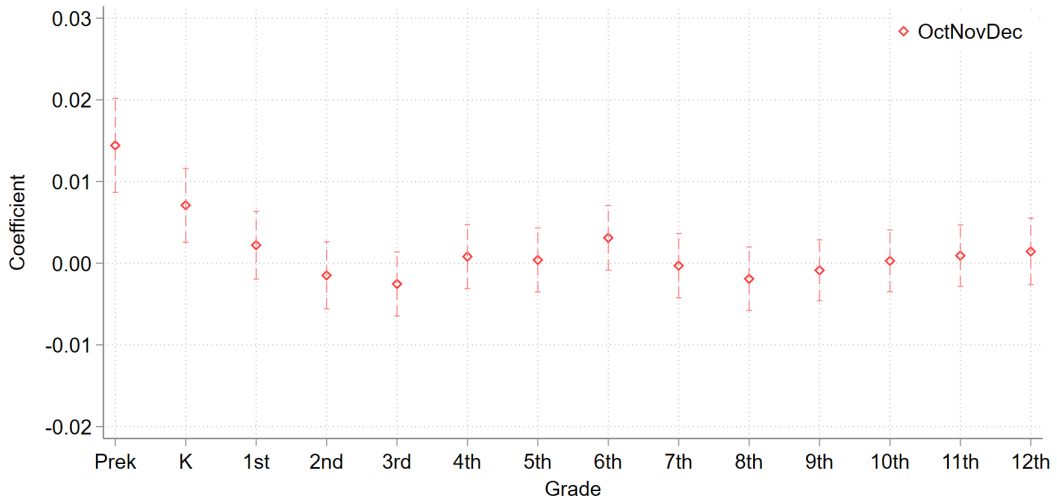
Note. Percentage of private school attendance by parents highest educational attainment and parents' income. *Observations:* N_{Educ}= (1,642,147; 1,410,913; 2,373,002); N_{Inc}= (1,362,855; 1,405,657; 1,301,064; 1,356,486)

Figure A.4: Effect of Quarter of Birth on Private School Attendance: Probit estimation



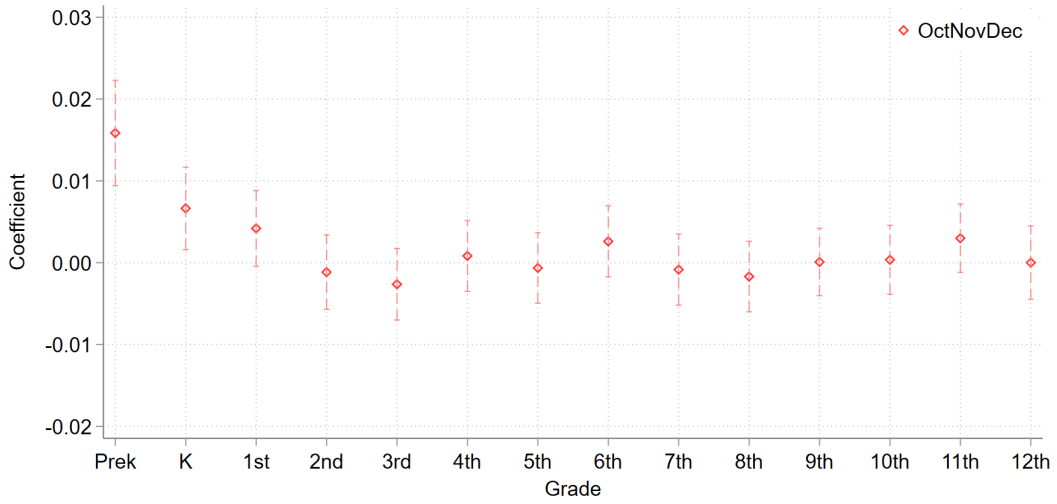
Note. Coefficient and 95% confidence interval of the quarter of birth variables in regression (1). The following covariates are included in the regression but excluded from the graph for brevity: age, dummies for male, Non-Hispanic white, Non-Hispanic black, and Hispanic. I also include year and state fixed effects. *Observations:* N_{grade}=(461,778; 374,473; 363,550; 371,629; 382,896; 383,378; 389,006; 391,971; 391,174; 396,180; 396,315; 394,537; 384,222; 344,953)

Figure A.5: Effect of Quarter of Birth on Private School Attendance by Grade: sample of children born from July to December that live in states with July to September cutoff



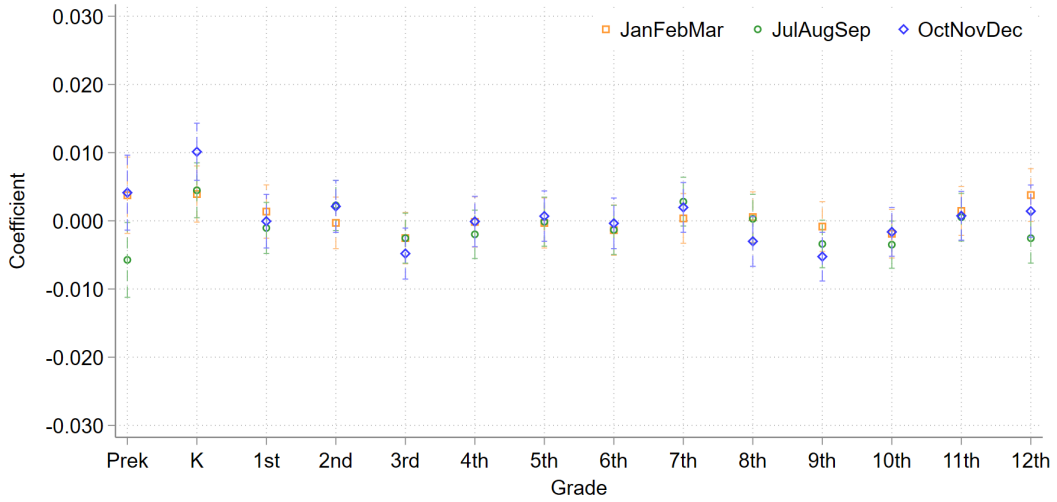
Note. Coefficient and 95% confidence interval of the quarter of birth variables in regression (1). The following covariates are included in the regression but excluded from the graph for brevity: age, dummies for male, Non-Hispanic white, Non-Hispanic black, and Hispanic. I also include year and state fixed effects. *Observations:* N=(145,893; 119,762; 115,349; 117,517; 121,272; 120,750; 122,335; 123,425; 123,006; 123,903; 123,346; 121,968; 118,467; 106,011)

Figure A.6: Effect of Quarter of Birth on Private School Attendance by Grade: sample of children born from July to December that live in states with a September cutoff



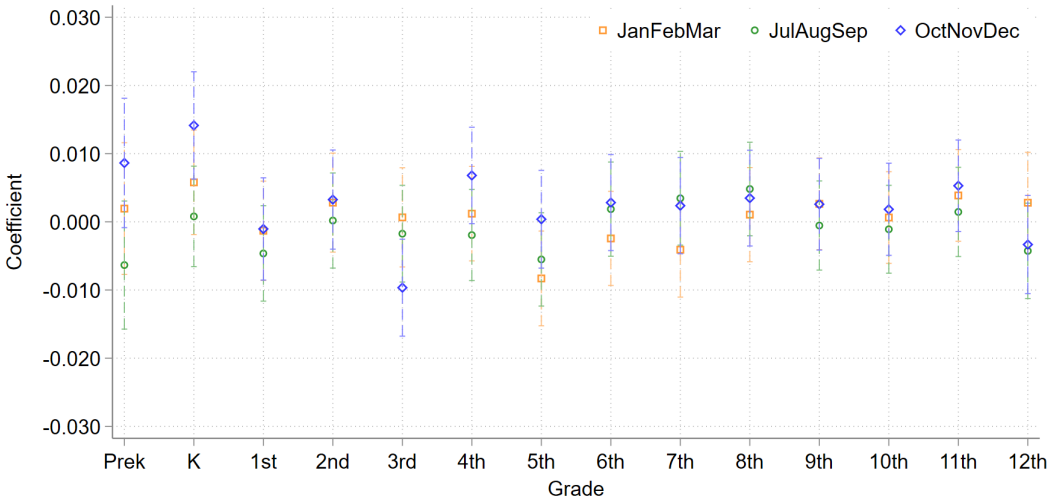
Note. Coefficient and 95% confidence interval of the quarter of birth variables in regression (1). The following covariates are included in the regression but excluded from the graph for brevity: age, dummies for male, Non-Hispanic white, Non-Hispanic black, and Hispanic. I also include year and state fixed effects. *Observations:* N=(115,523; 95,565; 91,476; 93,041; 96,138; 95,720; 96,928; 97,968; 97,453; 98,078; 97,989; 96,952; 94,155; 84,401)

Figure A.7: Effect of Quarter of Birth on Private School Attendance by Grade: sample of states that requires the districts to offer kindergarten



Note. Coefficient and 95% confidence interval of the quarter of birth variables in regression (1). The following covariates are included in the regression but excluded from the graph for brevity: age, dummies for male, Non-Hispanic white, Non-Hispanic black, and Hispanic. I also include year and state fixed effects. *Observations:* N=(371,675; 305,346; 295,622; 301,808; 310,683; 310,787; 315,428; 317,441; 316,232; 319,871; 319,276; 317,122; 308,323; 276,304)

Figure A.8: Effect of Quarter of Birth on Private School Attendance by Grade: sample of states that requires children to attend kindergarten



Note. Coefficient and 95% confidence interval of the quarter of birth variables in regression (1). The following covariates are included in the regression but excluded from the graph for brevity: age, dummies for male, Non-Hispanic white, Non-Hispanic black, and Hispanic. I also include year and state fixed effects. *Observations:* N= (99,001; 81,678; 79,313; 80,640; 82,695; 83,375; 84,259; 85,118; 85,420; 85,937; 86,655; 85,868; 83,412; 74,352)

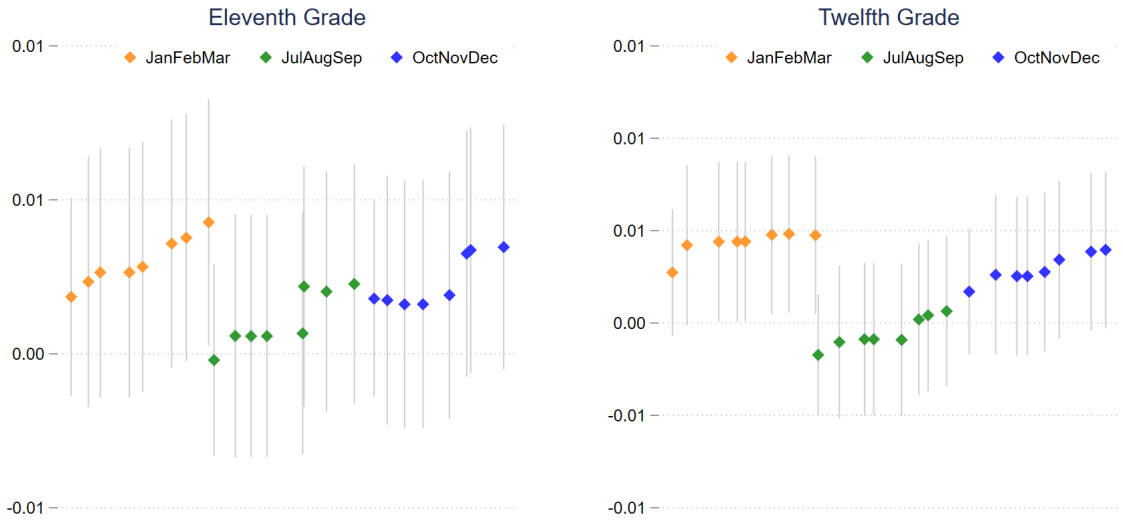
Figure A.9: Effect of Quarter of Birth on Private School Attendance by Grade: Pre-K–4th

Note. Figures show the main results coefficients and the robustness subsamples that includes the additional controls as presented in Table 2. The base covariates included in the regression but excluded from the graphs for brevity: age, dummies for male, Non-Hispanic white, Non-Hispanic black, and Hispanic. I also include year and state fixed effects. *Observations:* N_{PK}= (461,778; 357,923); N_{K}= (374,473; 276,772); N_{1st}=(363,550; 268,105); N_{2nd}=(371,629; 272,602); N_{3rd}=(382,896; 280,267); N_{4th}=(383,378; 279,607)

Figure A.10: Effect of Quarter of Birth on Private School Attendance by Grade: 5th–10th

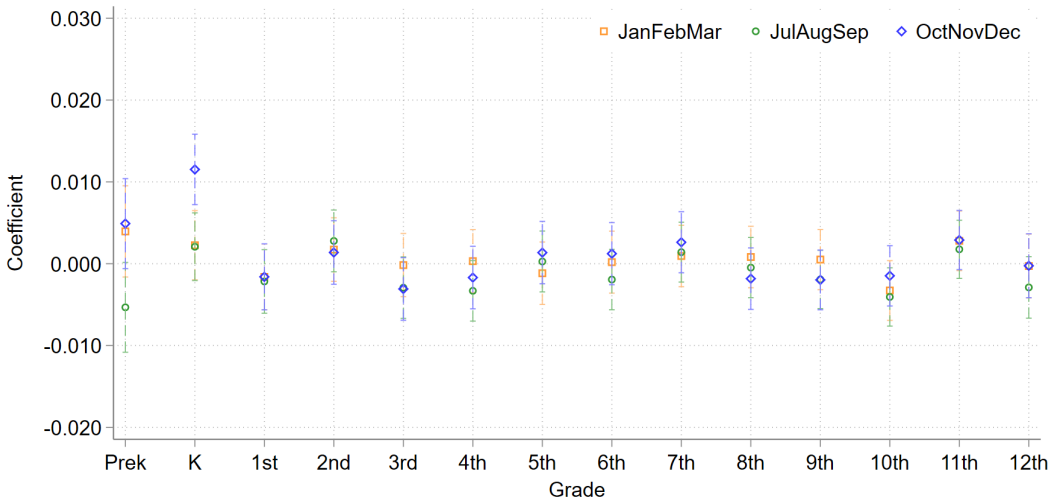
Note. Figures show the main results coefficients and the robustness subsamples that includes the additional controls as presented in Table 2. The base covariates included in the regression but excluded from the graphs for brevity: age, dummies for male, Non-Hispanic white, Non-Hispanic black, and Hispanic. I also include year and state fixed effects. *Observations:* N_{5th}= (389,006; 283,312); N_{6th}= (391,971; 284,446); N_{7th}=(391,174; 282,544); N_{8th}= (396,180; 284,857) ; N_{9th}=(396,315; 282,166); N_{10th}=(394,537; 280,720)

Figure A.11: Effect of Quarter of Birth on Private School Attendance by Grade: 11th–12th



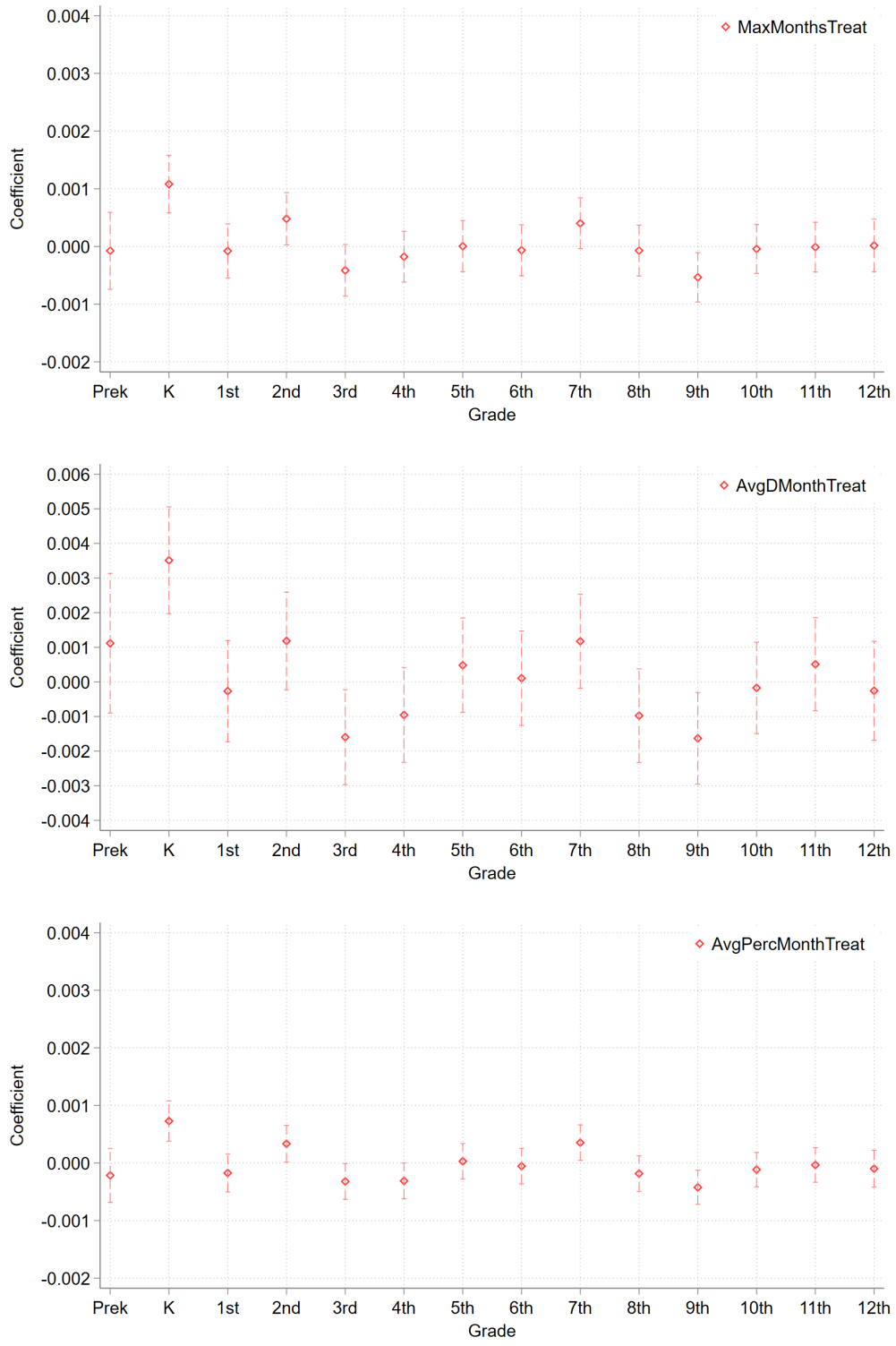
Note. Figures show the main results coefficients and the robustness subsamples that includes the additional controls as presented in Table 2. The base covariates included in the regression but excluded from the graphs for brevity: age, dummies for male, Non-Hispanic white, Non-Hispanic black, and Hispanic. I also include year and state fixed effects. *Observations:* N_{11th}=(384,222; 274,527); N_{12th}=(344,953; 247,827)

Figure A.12: Effect of Quarter of Birth on Private School Attendance: ACS 2008-2016 Sample



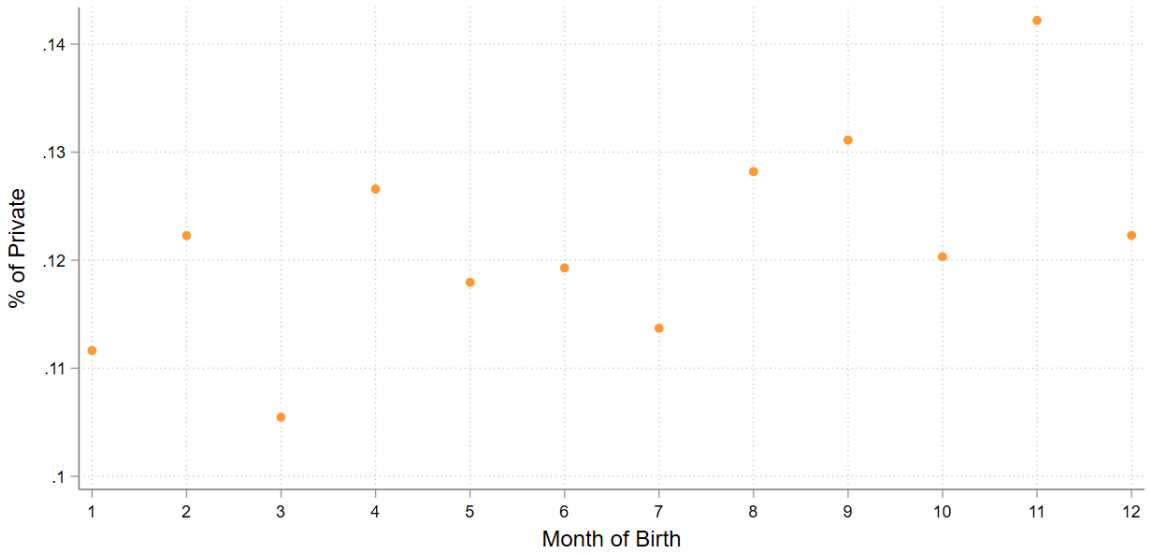
Note. Coefficient and 95% confidence interval of the quarter of birth variables in regression (1). The following covariates are included in the regression but excluded from the graph for brevity: age, dummies for male, Non-Hispanic white, Non-Hispanic black, and Hispanic. I also include year and state fixed effects. *Observations:* N_{grade}=(349,634; 285,493; 278,039; 283,890; 291,048; 290,389; 293,052; 294,896; 295,726; 299,272; 299,768; 299,057; 290,196; 261,360)

Figure A.13: Effect of Months Away from Entrance Age Cutoff on Private School Attendance: alternative variables



Note. Coefficient and 95% confidence interval of the "average month away from the cutoff" variable in regression (2). Panel A uses $MaxMonthsTreat$ as the variable of interest. Panel B and Panel C present the results using $AvgDMonthTreat$ and $AvgPercMonthsTreat$, respectively. The following covariates are included in the regression but excluded from the graph for brevity: age, dummies for male, Non-Hispanic white, Non-Hispanic black, and Hispanic. The analysis excludes children living in states that do not have a cutoff or that the Local Education Agency (LEA) decides their own rules regarding of entrance age. I also include year and state fixed effects. *Observations:* N= (361,787; 300,012; 290,479; 296,635; 305,591; 305,388; 310,046; 312,388; 311,129; 314,363; 314,274; 312,351; 303,325; 271,840)

Figure A.14: Percentage of Children Attending Private School at Kindergarten by Month of Birth



Note. Percentage of children attending private school at kindergarten by month of birth. *Observations:* N= (1,675; 1,292; 1,479; 1,343; 1,509; 1,509; 1,583; 1,638; 1,533; 1,496; 1,498; 1,529)

Table A.2: Effect of Treatment Measures on Private School Attendance: By Year of Birth

	(1)	(2)	(3)	(4)	(5)	(6)
				Grade		
	K	1st	2nd	3rd	4rd	5th
			Panel I. Those Born 2003/2004			
A: Using Month of Birth						
SepDec	-0.0365**	-0.0470***	-0.0528***	-0.0542***	-0.0537***	-0.0540***
	(0.0150)	(0.0159)	(0.0164)	(0.0169)	(0.0177)	(0.0186)
N	5,605	4,713	4,240	3,899	3,648	3,455
B: Month Away from Sept 1st						
$MonthtoCutoff$	-0.00575***	-0.00657***	-0.00695***	-0.00703***	-0.00712***	-0.00684***
	(0.00194)	(0.00207)	(0.00213)	(0.00218)	(0.00226)	(0.00238)
N	5,605	4,713	4,240	3,899	3,648	3,455
C: Using Age at Entry						
AgeKEntry	-0.00199	-0.00115	-0.000674	-0.0000550	0.000369	0.000463
	(0.00172)	(0.00183)	(0.00196)	(0.00192)	(0.00198)	(0.00206)
N	4,966	4,169	3,753	3,458	3,238	3,065
			Panel II. Those Born 2005/2006			
A: Using Month of Birth						
SepDec	0.0747***	0.0185	0.0170	0.0207	0.0151	0.0158
	(0.0189)	(0.0129)	(0.0125)	(0.0129)	(0.0127)	(0.0126)
N	12,479	10,144	8,954	8,288	7,669	7,244
B: Month Away from Sept 1st						
$MonthToCutoff$	0.00495***	0.000855	0.00138	0.00151	0.00131	0.00165
	(0.00141)	(0.00108)	(0.00109)	(0.00112)	(0.00111)	(0.00110)
N	12,479	10,144	8,954	8,288	7,669	7,244
C: Using Age at Entry						
AgeKEntry	-0.0114***	-0.00597***	-0.00522**	-0.00509**	-0.00471**	-0.00460**
	(0.00235)	(0.00215)	(0.00214)	(0.00210)	(0.00215)	(0.00211)
N	10,839	8,773	7,772	7,203	6,655	6,289

Note. Effect of treatment variables on Private School Attendance. *Columns:* Grade that the child is in, from kindergarten to 5th grade. *Panels:* Panel I presents the results for children born 2003–2004; Panel II presents the results for children born 2005–2006. In each panel, the subpanels are as follows: *A* presents the results using *SepDec*, a variable equal to one if the child is born in the months September–December; *B* presents the results using $MonthtoCutoff$, a variable equal to the number of months away from September 1 that the child is born; *C* presents the results using *AgeKEntry*, a variable equal to the age at kindergarten entry measured in months. The following covariates are included in the regression but excluded from the table for brevity: male dummy, year of birth dummies, and race dummies. N represents the number of observations. Standard errors cluster at the school level in parentheses; *p<0.10, **p<0.05, ***p<0.010.

Table A.3: Effects of Treatment Measures on Private School Attendance: Using Sample Weights

	(1)	(2)	(3)	(4)	(5)	(6)
			Grade			
	K	1st	2nd	3rd	4rd	5th
			Panel I. Weighted Unadjusted			
A: Month of Birth						
SepDec	0.0302**	-0.00816	-0.00436	-0.00609	-0.0121	-0.0113
	(0.0152)	(0.00947)	(0.00956)	(0.00950)	(0.00984)	(0.0103)
N	16,084	13,376	11,944	11,071	10,316	9,767
B: Month Away from Sept 1st						
$MonthToCutoff$	0.00165	-0.00133	-0.000567	-0.000708	-0.00111	-0.000784
	(0.00134)	(0.000925)	(0.000901)	(0.000931)	(0.000956)	(0.00103)
N	16,084	13,376	11,944	11,071	10,316	9,767
C: Age at Entry						
AgeKEntry	-0.00692***	-0.00371**	-0.00326**	-0.00245	-0.00207	-0.00213
	(0.00183)	(0.00157)	(0.00156)	(0.00149)	(0.00153)	(0.00150)
N	14,491	11,990	10,716	9,944	9,256	8,759
			Panel II. Nonresponse-adjusted weights			
A: Month of Birth						
SepDec	0.0302**	-0.000377	-0.000232	-0.00588	-0.0213*	-0.0163
	(0.0152)	(0.0116)	(0.0118)	(0.0112)	(0.0112)	(0.0135)
N	16,084	11,135	10,040	9,080	8,452	7,966
B: Month Away from Sept 1st						
$MonthToCutoff$	0.00165	-0.000548	0.000247	0.000212	-0.00120	-0.000329
	(0.00134)	(0.00118)	(0.00113)	(0.00109)	(0.00108)	(0.00131)
N	16,084	11,135	10,040	9,080	8,452	7,966
C: Age at Entry						
AgeKEntry	-0.00692***	-0.00428**	-0.00400**	-0.00232	-0.00193	-0.00232
	(0.00183)	(0.00176)	(0.00185)	(0.00181)	(0.00186)	(0.00193)
N	14,491	10034	9,079	8,224	7,664	7,215

Note. Effect of treatment variables on Private School Attendance. *Columns:* Grade that the child is in, from kindergarten to 5th grade. *Panels:* Panel I presents the weighted unadjusted results; Panel II presents the weighted nonresponse-adjusted results. In each panel, the subpanels are as follows: *A* presents the results using *SepDec*, a variable equal to one if the child is born in the months September–December; *B* presents the results using $MonthtoCutoff$, a variable equal to the number of months away from September 1 that the child is born; *C* presents the results using $AgeKEntry$, a variable equal to the age at kindergarten entry measured in months. The following covariates are included in the regression but excluded from the table for brevity: male dummy, year of birth dummies, and race dummies. N represents the number of observations. Standard errors cluster at the school level in parentheses; *p<0.10, **p<0.05, ***p<0.010.

Table A.4: Effects of Treatment Measures on Public School Attendance for Grades 1st–5th: Conditional on Private Kindergarten

	(1)	(2)	(3)	(4)	(5)
			Grade		
	1st	2nd	3rd	4rd	5th
A: Month of Birth					
SepDec	0.0136	0.0466	0.0469	0.0479	0.0423
	(0.0234)	(0.0287)	(0.0306)	(0.0332)	(0.0339)
N	1,657	1,439	1,318	1,228	1,149
B: Month Away from Sept 1st					
$MonthToCutoff$	-0.000637	0.00134	0.00235	0.00187	0.000277
	(0.00262)	(0.00315)	(0.00335)	(0.00368)	(0.00376)
N	1,657	1,439	1,318	1,228	1,149
C: Age at Entry					
AgeKEntry	-0.000509	-0.00188	-0.00172	-0.00158	-0.00188
	(0.00234)	(0.00309)	(0.00373)	(0.00402)	(0.00400)
N	1,571	1,366	1,254	1,169	1,098

Note. Effect of treatment variables on Private School Attendance. *Columns:* Grade that the child is in, from 1st to 5th grade. *Panels:* Panel A presents the results using *SepDec*, a variable equal to one if the child is born in the months September–December; panel B presents the results using $MonthtoCutoff$, a variable equal to the number of months away from September 1 that the child is born; panel C presents the results using *AgeKEntry*, a variable equal to the age at kindergarten entry measured in months. The following covariates are included in the regression but excluded from the table for brevity: male dummy, year of birth dummies, and race dummies. N represents the number of observations. Standard errors cluster at the school level in parentheses; $^{*}p<0.10$, $^{**}p<0.05$, $^{***}p<0.010$.

Table A.5: Effect of when Children are Born on Private Kindergarten Attendance by Type of Pre-K Access: Alternative Pre-K Measure

	(1)	(2)	(3)	(4)	(5)	(6)
	All		**Mostly Universal Pre-K**		**Limited Pre-K**	
JulAugSep	0.00368**		0.000468		0.00435**	
	(0.00185)		(0.00447)		(0.00202)	
OctNovDec	0.0109***		0.0110**		0.0105***	
	(0.00192)		(0.00459)		(0.00211)	
JanFebMar	0.00283		0.00289		0.00275	
	(0.00189)		(0.00455)		(0.00207)	
AvgMonthTreat		0.00134***		0.00175**		0.00124***
		(0.000305)		(0.000831)		(0.000327)
N	374,473	300,012	73,311	51,564	301,162	248,448

Note. Effect of the quarter of birth and *AvgMonthTreat* on private school attendance at kindergarten estimated by regression (1) and (2), respectively. *Columns:* columns (1) and (2) present the results for the complete sample; columns (3) and (4) present the results for states with mostly-universal publicly funded pre-K; columns (5) and (6) present the results for the rest of states. The following covariates are included in the regression but excluded from the table for brevity: age, dummies for male, Non-Hispanic white, Non-Hispanic black, and Hispanic. I also include year and state fixed effects. N represents the number of observations. The analysis using *AvgMonthTreat* excludes children living in states that do not have a cutoff or that the Local Education Agency (LEA) decides their own rules regarding of entrance age. Standard errors cluster in parentheses; *p<0.10, **p<0.05, ***p<0.010.

A.1 Effect of Quarter of Birth on Private School Attendance: 1960–1980

Table A.6 shows the unweighted sample characteristics, which mimic that of the main results in Table 1. The only difference is that for the 1960 database, individuals in the school are only ages 5–18 (K–12 grade). There are some aspects worth noting. First, the children's ages at each grade increases over time, reflecting the combination of children entering school later due to the entrance age cutoff starting earlier and the increasing of redshirting. Given that the school cutoff was later for those years, the quarter of birth distribution in pre-K (kindergarten) is skewed in 1970–1980 (1960) toward the *JanFebMar* birth quarter compared to the primary data set.

In terms of private schooling, note that the percentage of private pre-K is much higher in 1970–1980, more than 60%, compared with around 47% nowadays, which is likely due to the rise of publicly offered preschool options. For kindergarten, the percentage of private school attendance was 14% for 1960 and 16% for 1970 and 1980, which is two percentage points larger than nowadays. In grades 1–12, the rate of private schools' attendance is slightly higher in 1960 than in 1970 and 1980. This change could be due to religious reasons or just because private school is relatively cheaper in the past.[48]

[48]Although this report does not go that far in the past, it shows that from 1999–2000 to 2007–

Figure A.15 shows the distribution in detail of the percentage of those going to private school by grade in 1960–1980. As with the data in Section 3, we can see a similar pattern of private schools' relevancy at the beginning and then staying somewhat constant, with a slight decrease at the start of high school.

A.1.1 Results

The estimated coefficients and 95% confidence intervals of model (1) are in Figure A.16. In terms of the general pattern, we see a similar patter over grades but with differences in persistence. It is clear that for 1960–1980, the effect persists more than in 2008–2019. Although I expected the effect to be higher, the results for 1980 are much larger for kindergarten than the effect found for the other samples. An important question is the explanation behind these differences. One possible explanation could be that since private schools' fees were lower than nowadays, parents might have kept their children in private school through K–12 at a higher rate. Moreover, for 1980, comparing the variation from the base group ($AprMayJun$) to the $OctNovDec$ quarter of birth, the difference of the percentage of private school attendance for 2008–2019 is 0.16 percentage points (15.07% versus 14.91%), and for 1980 it is 0.40 percentage points (13.86% versus 13.46%). Therefore, the base value of private school attendees was much higher.[49]

The Role of Religious-Affiliated Private Schools. Another possible explanation is the importance of religious-affiliated private schools, which there were more of in the past than it is nowadays. If religious-affiliated private schools were of more importance in 1960–1980, then perhaps this is why we see a larger effect at kindergarten and a more persistent effect than the main results. Nowadays that effect would not be that relevant due to a decrease in enrollment in religious schools.[50] To analyze religious schools, I use the the 1970 and 1980 samples that separate private schools between those that are religious and those that are not.

Figure A.17 shows the distribution of private school grades for religious and nonreligious schools in 1970 and 1980. Children are more likely to be in a nonreligious private pre-K for both periods, with a smaller gap in 1980. At kindergarten, as before, the percentage of private schooling decreases for both types but more so for nonreligious. After that, a significant difference is that the percentage of those in religious-affiliated private school stabilizes at a larger percentage than for nonreligious. In 1980, the variation from kindergarten to first grade between the nonreligious and religious private schools is par-

2008, the average tuition cost of private elementary and secondary school increased by 37%; see https://nces.ed.gov/programs/digest/d19/tables/dt19_205.50.asp.

[49] In the 80s, there was a rise of private school students; see https://www.nytimes.com/1984/12/21/us/us-sees-private-school-enrollment-surge-in-80-s.

[50] Although the report does not go that far in the past, the NCES reports the decrease on private school enrollment since 1999–2000 in https://nces.ed.gov/programs/coe/private-school-enrolled.

ticularly striking, falling 16.65 percentage points for religious schools and 31.34 for non-religious. Therefore, the patterns suggest that children who start in a religious-affiliated private school stay there at a higher rate as they move through K–12.

I estimate equation (1) for three different dependent variables. First, I use $PrivNonReligious$, a dummy equal to one if the child is attending a private nonreligious school and zero otherwise. $PrivReligious$ is a dummy equal to one if the child is in a private religious school and zero otherwise. Finally, I limit the sample to private school attendees and estimate $NonReligious$, equal to one if in a nonreligious school and zero otherwise.

Figure A.18 to Figure A.19 show the results: the persistence in these samples is due to religious schools. The nonreligious schools' pattern seems to follow more closely Figure 3. This comparison suggests that, as the number of religious-affiliated private schools and enrollment there decreases,[51] the nonreligious pattern with an effect only at kindergarten persists.

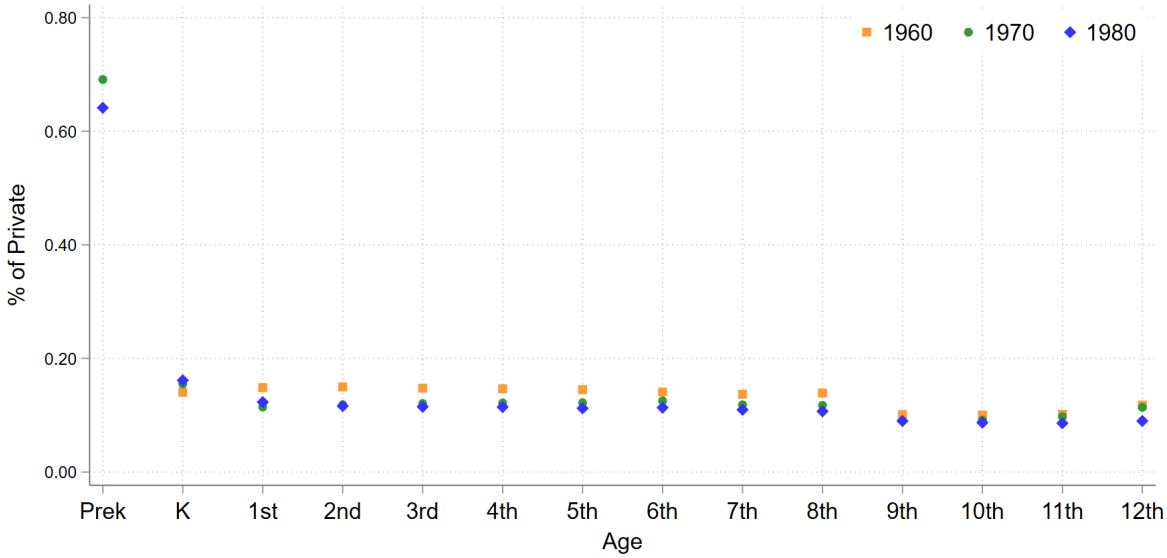
[51] As mentioned before, enrollment has decreased. Additionally, Catholic private schools have been continuously closing, decreasing their supply over time; see https://www.ncea.org.

Table A.6: Sample Description 1960-1980

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: 1960							
	All	K	1st	2nd	3rd	4th	>4th
Age	10.88	5.30	6.42	7.48	8.51	9.53	13.45
	(3.55)	(0.46)	(0.96)	(1.08)	(1.09)	(0.98)	(2.28)
JanFebMar	0.24	0.27	0.24	0.24	0.24	0.24	0.24
	(0.43)	(0.44)	(0.43)	(0.43)	(0.43)	(0.43)	(0.43)
AprMayJun	0.24	0.23	0.23	0.24	0.25	0.24	0.24
	(0.43)	(0.42)	(0.42)	(0.43)	(0.43)	(0.42)	(0.43)
JulAugSep	0.27	0.26	0.27	0.27	0.27	0.27	0.27
	(0.44)	(0.44)	(0.44)	(0.44)	(0.44)	(0.44)	(0.44)
OctNovDec	0.25	0.24	0.25	0.25	0.25	0.25	0.26
	(0.43)	(0.43)	(0.43)	(0.43)	(0.43)	(0.43)	(0.44)
PrivateSchool	0.14	0.14	0.15	0.15	0.15	0.15	0.13
	(0.34)	(0.35)	(0.36)	(0.36)	(0.35)	(0.35)	(0.33)
N	2,179,791	118,532	222,153	215,616	207,357	195,209	1,220,924
Panel B: 1970							
	All	Pre-K	K	1st	2nd	3rd	>3rd
Age	11.16	4.07	5.33	6.46	7.49	8.51	13.23
	(3.75)	(0.78)	(0.61)	(0.87)	(0.84)	(0.84)	(2.58)
JanFebMar	0.24	0.26	0.24	0.24	0.24	0.24	0.24
	(0.43)	(0.44)	(0.43)	(0.43)	(0.43)	(0.43)	(0.43)
AprMayJun	0.24	0.24	0.24	0.24	0.24	0.24	0.24
	(0.43)	(0.43)	(0.43)	(0.43)	(0.43)	(0.43)	(0.43)
JulAugSep	0.27	0.24	0.26	0.27	0.27	0.26	0.27
	(0.44)	(0.43)	(0.44)	(0.44)	(0.44)	(0.44)	(0.44)
OctNovDec	0.25	0.26	0.25	0.25	0.25	0.25	0.25
	(0.43)	(0.44)	(0.43)	(0.43)	(0.43)	(0.43)	(0.43)
PrivateSchool	0.13	0.69	0.16	0.11	0.12	0.12	0.11
	(0.33)	(0.46)	(0.36)	(0.32)	(0.32)	(0.33)	(0.31)
N	457,227	8,441	27,916	37,249	37,172	38,650	307,799
Panel B: 1980							
	All	Pre-K	K	1st	2nd	3rd	>3rd
Age	11.18	3.96	5.35	6.51	7.55	8.56	13.46
	(4.00)	(0.78)	(0.65)	(0.81)	(0.79)	(0.75)	(2.61)
JanFebMar	0.24	0.27	0.24	0.24	0.25	0.25	0.24
	(0.43)	(0.44)	(0.43)	(0.43)	(0.43)	(0.43)	(0.43)
AprMayJun	0.24	0.23	0.24	0.24	0.24	0.24	0.24
	(0.43)	(0.42)	(0.43)	(0.43)	(0.43)	(0.43)	(0.43)
JulAugSep	0.26	0.24	0.27	0.26	0.26	0.26	0.27
	(0.44)	(0.43)	(0.44)	(0.44)	(0.44)	(0.44)	(0.44)
OctNovDec	0.25	0.26	0.25	0.25	0.25	0.25	0.25
	(0.43)	(0.44)	(0.43)	(0.43)	(0.43)	(0.43)	(0.43)
PrivateSchool	0.14	0.64	0.16	0.12	0.12	0.12	0.10
	(0.34)	(0.48)	(0.37)	(0.33)	(0.32)	(0.32)	(0.30)
N	2,219,913	111,741	146,909	152,460	155,517	169,611	1,483,675

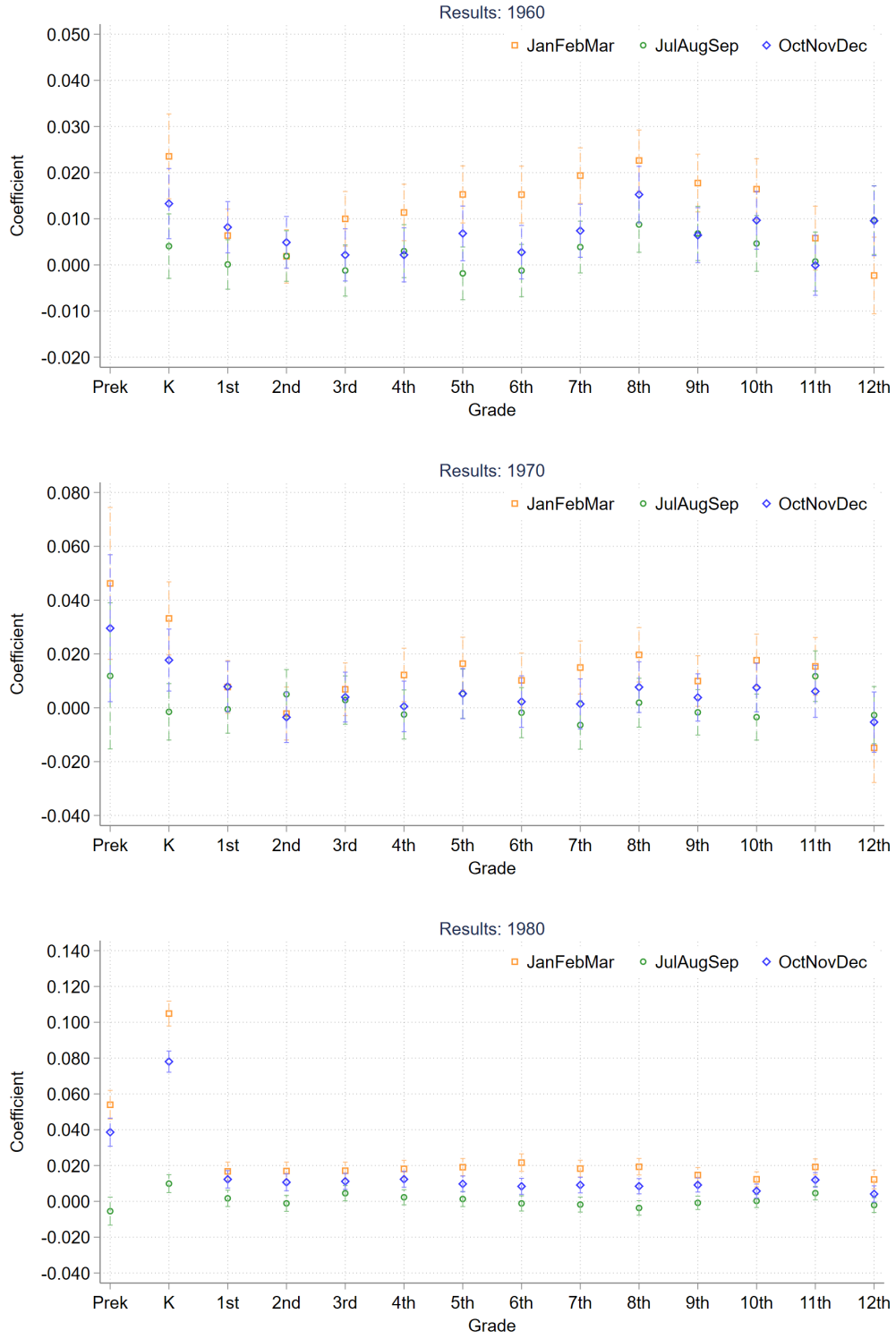
Note. This table presents the mean and standard deviations (in parenthesis) of the main variables. *Panel A:* information for the 1% and 5% 1960 Cesus; *Panel B:* information for the 1% State Census; *Panel C:* information for the 5% 1980 Census. N represents the number of observations.

Figure A.15: Percentage of Individuals that are in Private School by Age and Grade from 1960-1980



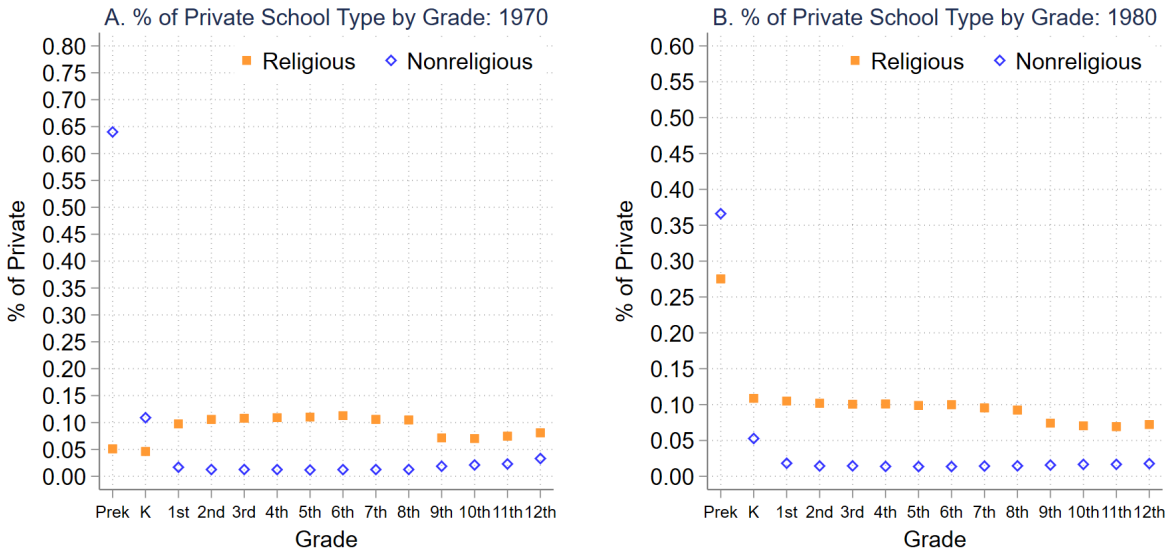
Note. % of private school attendees by grade for 1960-1980.

Figure A.16: Effect of Quarter of Birth on Private School Attendance by Grade in 1960-1980



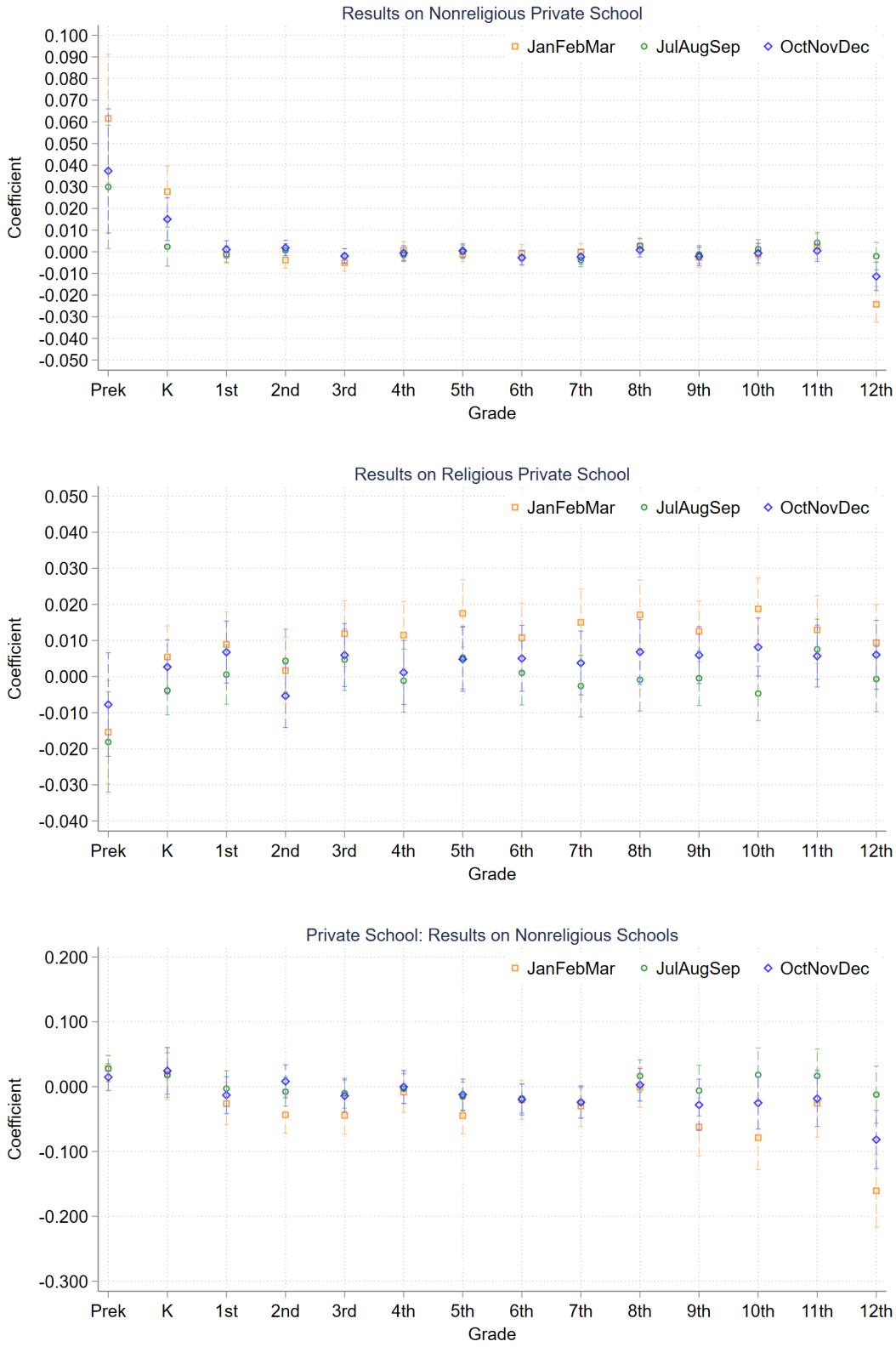
Note. Coefficient and 95% confidence interval of the quarter of birth variables in regression (1). The following covariates are included in the regression but excluded from the graph for brevity: age, dummies for male, Non-Hispanic white, Non-Hispanic black, and Hispanic. I also include year and state fixed effects. *Observations:* N_{60}=(118,532; 222,153; 215,616; 207,357; 195,209; 192,976; 191,104; 193,165; 165,662; 137,129; 127,061; 117,869; 95,958); N_{70}= (8,441; 27,916; 37,249; 37,172; 38,650; 37,863; 37,578; 37,232; 37,296; 35,936; 34,448; 32,838; 29,142; 25,466); N_{80}= (146,909; 111,741; 152,460; 155,517; 169,611; 169,998; 166,675; 163,426; 165,919; 170,412; 176,137; 176,165; 159,391; 135,552)

Figure A.17: Percentage of Individuals in Private School by Type of Private School in 1970-1980



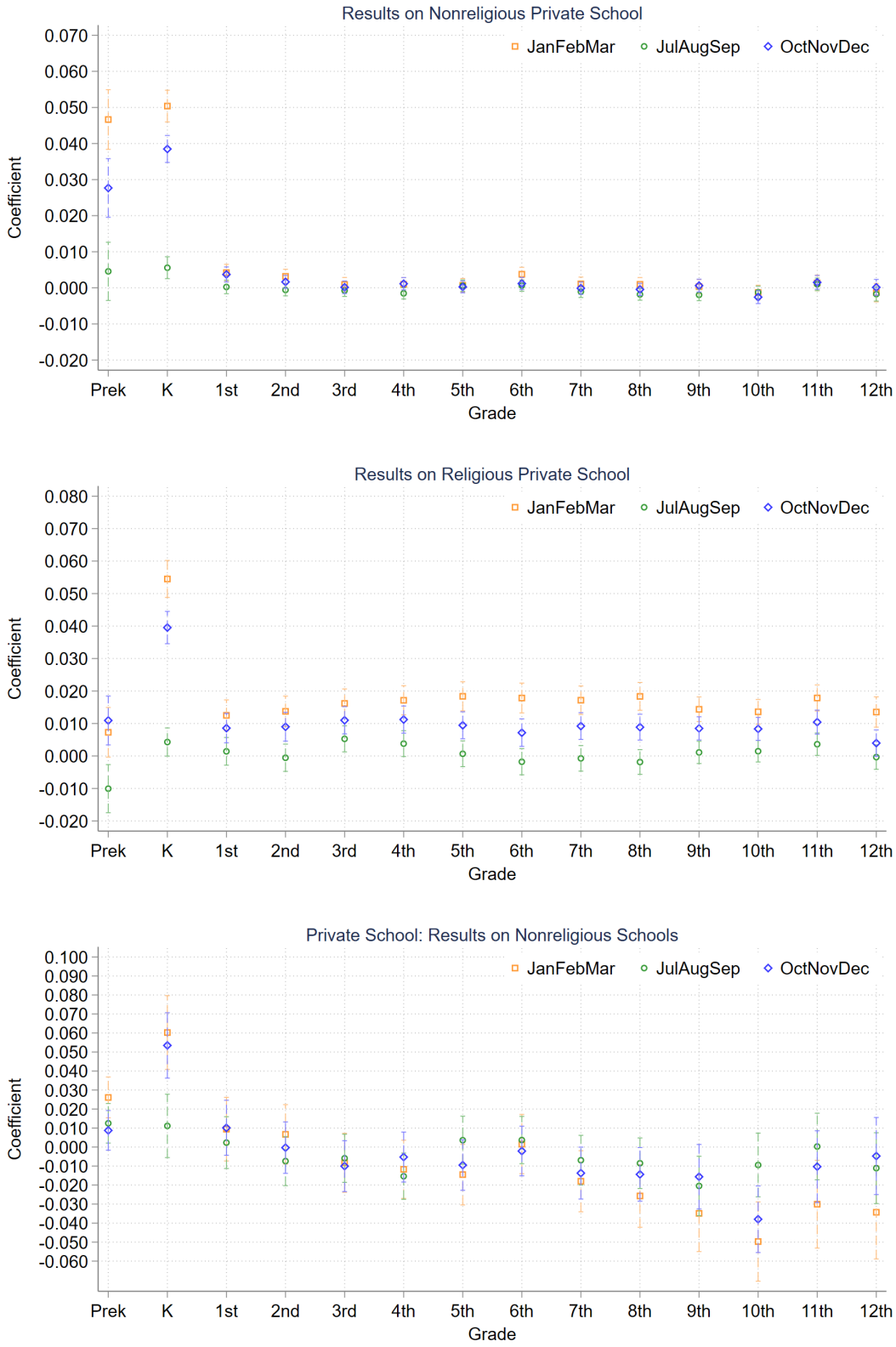
Note. *Figure A* shows the % of religious and nonreligious private school attendees by grade in 1970; *Figure B* shows % of religious and nonreligious private school attendees by grade in 1980

Figure A.18: Effect of Quarter of Birth on Private School Attendance in 1970 by Type and Grade



Note. Coefficient and 95% confidence interval of the quarter of birth variables in regression (1). The following covariates are included in the regression but excluded from the graph for brevity: age, dummies for male, Non-Hispanic white, Non-Hispanic black, and Hispanic. I also include year and state fixed effects. *Observations:* N_{all}= (8,441; 27,916; 37,249; 37,172; 38,650; 37,863; 37,578; 37,232; 37,296; 35,936; 34,448; 32,838; 29,142; 25,466); $N_{private}$= (5,834; 4,331; 4,263; 4,401; 4,664; 4,603; 4,583; 4,665; 4,419; 4,225; 3,107; 3,003; 2,846; 2,903)

Figure A.19: Effect of Quarter of Birth on Private School Attendance in 1980 by Type and Grade



Note. Coefficient and 95% confidence interval of the quarter of birth variables in regression (1). The following covariates are included in the regression but excluded from the graph for brevity: age, dummies for male, Non-Hispanic white, Non-Hispanic black, and Hispanic. I also include year and state fixed effects. *Observations:* N_{all}= (111,741; 146,909; 152,460; 155,517; 169,611; 169,998; 166,675; 163,426; 165,919; 170,412; 176,137; 176,165; 159,391; 135,552); $N_{private}$= (71,666; 23,722; 18,776; 18,069; 19,510; 19,491; 18,717; 18,536; 18,206; 18,225; 15,835; 15,352; 13,704; 12,196)