# School Entry, Educational Attainment and Quarter of Birth: A Cautionary Tale of LATE<sup>\*</sup>

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### Abstract

Partly in response to increased testing and accountability, states and districts have been raising the minimum school entry age, but existing studies show mixed results regarding the effects of entry age. These studies may be severely biased because they violate the monotonicity assumption needed for LATE. We propose an instrument not subject to this bias and show no effect on the educational attainment of children born in the fourth quarter of moving from a December 31 to an earlier cutoff. We then estimate a model that reconciles the different IV estimates including ours. We find that one standard instrument is badly biased but that the other diverges from ours because it estimates a different LATE. We also find that an early entry age cutoff that is applied loosely (as in the 1950s) raises educational attainment but one that is strictly enforced lowers it.

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## 1 Introduction

Over the last four decades many states and school districts have increased the minimum age at which children may enter kindergarten. In the 1960s children frequently entered kindergarten when they were considerably less than five years old (or first grade when they were less than six years old). This was formally permitted in many states whereas, in other states, it was relatively easy to get around the rules. Today, thirty-eight states have cutoff dates requiring children entering kindergarten to be five years old before October 16 of the year in which they enter kindergarten, and some of the remaining states have districts that apply a stricter standard.

Whether delayed entry improves or worsens education outcomes is controversial. Many recent studies in both education and economics have been devoted to obtaining consistent estimates of the effects of school entry age on short run and long run outcomes. Angrist and Krueger (1992) address the potential endogeneity of entry age by using quarter of birth as an instrument for entry age. They (Angrist and Krueger, 1991) show that historically individuals born in the first quarter started school later than those born in the fourth quarter, completed less education and earned less than those born in the rest of the year.<sup>1</sup> Critics of this approach argue that quarter of birth may be directly related to student outcomes or parental socioeconomic status.<sup>2</sup> Buckles and Hungerman (2008) provide evidence that children born at different times in the year are conceived by women with different socioeconomic characteristics. To address this issue, several researchers have exploited the variation in state laws governing entry age (or, the "legal entry age") to identify its effect on test scores, wages, educational attainment and other outcomes.<sup>3</sup> However, since entry age depends on both state law and date of birth, the potential endogeneity of date of birth remains problematic for this approach.

The aim of this paper is threefold. *First*, we address certain under-appreciated issues in the instrumental variable literature. Imbens and Angrist (1994), Angrist and Imbens (1995) and Angrist, Imbens and Rubin (1996) show that with heterogeneous treatment effects, under certain conditions, IV identifies the Local Average Treatment Effect (LATE). One condition, termed "monotonicity," generally treated as an unimportant regularity condition, requires that while the instrument may have no effect on some individuals, all of those who are affected should be affected unidirectionally. We argue that both standard instruments, quarter of birth and legal entry age, may provide inconsistent estimates of LATE

<sup>&</sup>lt;sup>1</sup>See also Mayer and Knutson, 1999 and Cahan and Cohen, 1989.

<sup>&</sup>lt;sup>2</sup>Bound, Jaeger and Baker, 1995; Bound and Jaeger, 2000.

<sup>&</sup>lt;sup>3</sup>Allen and Barnsley, 1993; Bedard and Dhuey, 2006, 2008; Cascio and Lewis, 2006; Datar, 2005; Dobkin and Ferreira, 2010; Elder and Lubotsky, 2009; Fertig and Kluve, 2005; Fredriksson and Ockert, 2006; Puhani and Weber, 2007.

because they violate monotonicity.<sup>4</sup> Therefore, we propose an instrument that satisfies monotonicity and gives consistent estimates of the LATE of school entry age on educational attainment. Our two-sample two-stage least squares (TS2SLS) results, consistent with Angrist and Krueger, show a large negative effect of school entry age on educational attainment when the IV is quarter of birth. Using the "legal entry age" instrument yields a smaller but still substantial adverse effect though it falls short of statistical significance at conventional levels. Finally, when we use the consistent estimator that meets the monotonicity requirement, the effect of school entry age on educational attainment is very close to zero.

Comparing the different IV estimates does not tell us whether they diverge because the traditional estimators are inconsistent or because they are measuring different LATEs. A *second* aim of this paper is to address this issue and to reconcile the different IV estimates including ours. To achieve this, we develop a simple model of school entry and educational attainment and use indirect inference to estimate the parameters of the model. We then simulate our model to compare the IV estimates with their respective LATEs. "Quarter of birth" proves to be robust to the failure of monotonicity but legal entry age does not.

Third, we are interested in the broader question of the optimal age at which to start school and, in particular, optimal policy regarding school entry age. Legislation, such as the No Child Left Behind (NCLB) act of 2001, has put great pressure on schools to improve student performance on tests. Some states have responded by raising the school entry age (Deming and Dynarski, 2008; Stipek, 2006). Given the historical nature of our data, the instrumental variables approach captures the effect of delaying school entry as it was practiced in the 1950s. But school entry age laws are now enforced much more strictly. Thus, we conduct a policy experiment using simulated data to study the effect of having strict entry age rules (the current practice) on average educational attainment. We show that the entry age that maximizes a child's eventual educational attainment varies considerably from about age 4.5 years to well over seven. The policy experiment suggests that, in an environment where laws are strictly enforced, constraining fourth quarter children to enter late reduces average educational attainment. Taken together, the results imply that having a waiver policy that gives constrained children the choice to enter earlier than the legally established age could increase educational attainment, particularly among groups that have high dropout rates.

The next section explores the literature on school entry age. Section 3 outlines the TS2SLS methods that we use for our baseline model. Section 4 describes the data. We present the TS2SLS results in section 5. Section 6 builds and estimates a model of school entry age. We use this model to evaluate two standard IV estimators found in the literature.

<sup>&</sup>lt;sup>4</sup>As we discuss in some detail later, depending on the size of the band around the discontinuity, similar concerns may arise for analyses based on regression discontinuity.

In section 7, we use the model to conduct policy experiments to understand the effect of different policy regimes on educational attainment. Section 8 concludes.

## 2 School Entry Age: Background

### 2.1 Literature

There has been a recent explosion of interest in school entry age that makes it difficult to treat the literature with justice. Until the 1990s, studies that looked at the effect of school entry age on student outcomes largely ignored the potential endogeneity of entry age. However, affluent parents can afford child-care costs associated with delaying their child's school entry and are therefore more likely to do so. Thus, there is a positive association between parental socioeconomic conditions and entry age that can bias the OLS estimate towards a positive effect of entry age on academic outcomes. On the other hand, the OLS estimate could be downward biased if children who are less precocious intellectually and/or emotionally are redshirted<sup>5</sup> since these children are more likely to perform poorly on cognitive tests

Angrist and Krueger (1992), Cahan and Cohen (1989) and Mayer and Knutson (1999) address endogeneity by using quarter or month of birth as an instrument for entry age. More recent papers (Bedard and Dhuey, 2006; Datar, 2005; Elder and Lubotsky, 2009) have used legal entry age as an instrument. This approach instruments actual entry age with the age at which the child could first legally enter school. It thus relies on both variation in state (or country) laws and month of birth.

Although somewhat mixed, the evidence from this literature suggests that older entrants have higher test scores compared to early entrants in the *same grade* and are less likely to repeat grades. However, the test score differences fade by the time the child is in middle school. Black, Devereux and Salvanes (2008), using data from Norway, find a small beneficial effect of early entry on cognitive score at age 18. Comparing younger children of the *same age*, Barua and Lang (2008) find that early entrants perform better on achievement tests, presumably because they have completed more schooling relative to those who began school late.

Therefore it is important to determine whether entry age affects ultimate educational attainment. If late entry reduces grade retention, has no negative effect on performance within grade and has no adverse effect on ultimate grade completion, then later entry produces the same outcome at lower cost to the public (although parents pay more for child-care and their children enter the labor market later). However, if later entry is not

<sup>&</sup>lt;sup>5</sup>In this context, redshirting refers to the practice of postponing entrance into kindergarten of age-eligible children in order to allow extra time for socioemotional, intellectual, or physical growth.

offset by later exit, those who enter late leave school with less education and fewer skills than earlier entrants leaving at that age. In this case, delaying entry reduces human capital accumulation.

The literature on the effect of entry age on educational attainment provides mixed results. For the U.S., Angrist and Krueger (1992) and Dobkin and Ferreira (2007) find that older entrants attain slightly less education and Deming and Dynarski (2008) attribute much of the decline in educational attainment to the trend towards later school entry, but Bedard and Dhuey (2008) find no effect. Outside the US, some studies find a negative impact of early school entry on adult educational attainment and other outcomes (Allen and Barnsley, 1993; Fredriksson and Ockert, 2006) while others find positive or no effects (Fertig and Kluve, 2005; Black, Devereux and Salvanes, 2008).<sup>6</sup> In this paper, we argue that these findings are suspect because of important issues with the identification strategies used in the existing literature.<sup>7</sup>

### 2.2 Specification Issues

Historically, economists assumed that instrumental variables estimates captured a single coefficient, the common effect of the explanatory variable on the dependent variable. Lang (1993) criticized the use of quarter of birth as an instrument for education in Angrist and Krueger (1991). He argued that the relation between log wage and education was inherently nonlinear and that the standard log wage equation should be viewed as a linear approximation in which the coefficient on schooling is random. He further argued that the Angrist/Krueger estimate of the return to schooling could be a severely biased estimate of the average of this random coefficient (now termed the Average Treatment Effect) because the justification for their instrument implies that it estimates the returns to education only for those with relatively little education.

Imbens and Angrist (1994) show that in a random coefficients model (i.e. one with heterogeneous treatment effects), under certain conditions, instrumental variables can still be interpreted as a Local Average Treatment Effect. LATE is the average effect of a treatment on those individuals whose treatment status is changed by the instrument. With a binary treatment, these are the "compliers" who receive the treatment when the instrument applies but not otherwise.<sup>8</sup> With a multi-valued treatment, these are the individuals who increase

<sup>&</sup>lt;sup>6</sup>Consistent with finding no effect on educational attainment, Black, Devereux and Salvanes find a positive effect of early entry on earnings for younger workers, presumably because they have more experience. However Bedard and Dhuey find an adverse effect on earnings in the United States.

<sup>&</sup>lt;sup>7</sup>Our concerns do not apply to regression discontinuity designs applied to countries in which nearly all children enter at the prescribed age. It is not clear that the results of such studies can be applied to countries such as the United States where there is considerable redshirting and noncompliance with the laws.

<sup>&</sup>lt;sup>8</sup>Note that the terminology introduced by Imbens and Angrist (1994), although more familiar to empirical economists, is not helpful in this setting and should be replaced by monotonicity (Angrist and Imbens, 1995)

the intensity of their treatment, and the estimated LATE gives more weight to individuals with larger responses to the instrument (Angrist and Imbens, 1995).

One of the assumptions for the identification of LATE is monotonicity: while the instrument may have no effect on some individuals, all of those who are affected must be affected in the same direction. Both the quarter of birth instrument and the legal entry age instrument violate the monotonicity assumption. Many parents do not enroll their children at the earliest permissible entry age (and some find ways to enroll them earlier than is formally allowed). Such strategic behavior is more common among parents of children born in the latter half of the year (West, Meek and Hurst, 2000). Thus almost all students born in May enter kindergarten in September following their fifth birthday (or first grade following their sixth birthday). In contrast, some children born in October will enter before their fifth birthday, when they are younger than those born in May, while others will enter the following year when they are older than entrants born in May. Therefore quarter of birth is not monotonically related to school entry age. The monotonicity assumption is not directly verifiable since it involves counterfactuals. However, one can compare the cumulative distribution function (CDF) of entry age for those born in the first and fourth quarters to test for stochastic dominance. A necessary but not sufficient condition for monotonicity is that the CDF of entry age for those born in quarter of birth 1 and the CDF of entry age for those born in quarter of birth 4 should not cross.<sup>9</sup>

Figure 1 shows this for those born in the first and last quarters of 1952 using reported age and grade at the time of the 1960 Census.<sup>10</sup> We can see that neither distribution of entry age is greater than the other in the sense of first-order stochastic dominance. Being born in the first quarter rather than the fourth quarter raises entry age for some children and lowers it for others. Formal statistical tests confirm the visual result and we can reject the null hypothesis that either distribution stochastically dominates the other. The cdf of the first quarter distribution lies above that of the fourth quarter (9% compared with 6%) in the range of entry age [4.5, 4.75) and again at [5.5, 5.75) (i.e. 87% compared with 55%).

or uniformity (Heckman, Urzua and Vytlacil, 2006). The "treated" do not have the option of entering at the same age as the "controls." Thus the concepts of "always takers" and "never takers" do not apply. Moreover, in our example, neither those who enter at age five nor those who enter at age six are complying with or defying the intended treatment. Instead, we say that date of birth does not have a monotonic or uniform effect on entry age, and its failure to do so can make instrumental variables estimates that rely on birth date inconsistent.

<sup>&</sup>lt;sup>9</sup>See proof of this proposition in Angrist and Imbens (1995)

 $<sup>^{10}</sup>$ The ages refer to the year before first grade for those who do not attend kindergarten. Equivalently, we assume that students who enter school in first grade would have spent one year in kindergarten had they enrolled. The dating of kindergarten entry is imperfect because we do not have data on retention or acceleration. Very late entry is assumed to reflect retention and categorized as 5.75 - 6.5 depending on the quarter of entry. Similarly, very early entry is assumed to reflect acceleration and is categorized as 3.75 - 4.5.

The probability of either deviation happening by chance is extremely low.<sup>11</sup>



Figure 1: CDF of Entry Age: 1st and 4th Quarter Births: 1952

What is the nature of the bias due to the failure of the monotonicity assumption? Angrist, Imbens and Rubin (1996) show that the Wald coefficient in the presence of defiers is  $\lambda (Y_1 - Y_0 | complier) + (1 - \lambda) (Y_1 - Y_0 | defier)$  where  $\lambda = \frac{\% compliers}{(\% compliers - \% defiers)}$ . Note that if there are any defiers,  $\lambda > 1$  and the estimate never lies between  $(Y_1 - Y_0 | complier)$ and  $(Y_1 - Y_0 | defier)$  but it always more extreme.

It is straightforward to show that in our case, the corresponding expression is

$${}^{\Lambda}_{\beta}{}^{}_{wald} = \omega\beta_i + (1-\omega)\,\beta_l \tag{1}$$

where  $\omega$  is the proportion of the entry age change attributable to those increasing their entry age and  $\beta_i$  is defined by

$$\beta_i = \frac{(Y_1 - Y_0 | Increase\_entry\_age)}{(A_1 - A_0 | Increase\_entry\_age)}$$
(2)

where Y and A are mean educational attainment and entrance age respectively, and  $\beta_l$  is defined analogously for those lowering their entry age.

It is easy to develop examples where the IV estimate using QOB or legal entry age gives severely biased estimates because of the failure of the monotonicity assumption. For simplicity assume that children are born on one of two days during the year. This is shown in table 1. Those born on the earlier date (Type 1) always enter when they are exactly 5.5

<sup>&</sup>lt;sup>11</sup>We are grateful to Garry Barrett for confirming this assessment using McFadden's (1989) test of first order stochastic dominance as corrected by Barrett and Donald (2003).

	Early Birthday	Late	Birthday
Age at Entry	5.5	5	6
		(75%)	(25%)
Education	12	11.5	13.5
Coefficient on Entry Age		1	3
Average Entry Age	5.5	5.2	5
Average Education	12	12	

 Table 1: Example Illustrating Bias Due to Failure of Monotonicity

years old. Those born on the later date (Type 2) can enter either when they are exactly five years old or when they are exactly six years old. This is shown in table 1. On average, Type 1 children complete twelve years of education. Type 2 children would also complete an average of twelve years of education if they started at age 5.5, but this is not an option for them.

For type 2, most (75%) benefit somewhat from delaying school entry. Such children will on average complete an extra half year of education if they enter at age 6.0 instead of 5.5 and lose a half year of eventual completed education if they enter at age 5.0 instead of 5.5. Their coefficient on entry age or treatment effect is one. The remainder (25%) benefit a great deal from delaying their school entry. Such children gain a year and a half of education if they enter at age 6.0 instead of 5.5 and lose a year and a half if they enter at age 5.0. Their coefficient is three.

Note that all children benefit from entering school when they are older. However, there are also costs associated with later entry. The parents of children with a treatment effect of only one do not believe the additional attainment is worth the cost. Such children all enter when they are five years old. In contrast, the parents of children with a treatment effect of three believe the additional attainment exceeds the cost and delay school entry until their children are six years old.

The average entry age for Type 2 is 5.25 while the average (and universal) entry age for Type 1 is 5.5. Note that relative to entering at age 5.5, three-quarters of children born at the later date lose one half year of education and one-quarter gain one and a half years. The effect on average educational attainment is zero (i.e.  $\frac{3}{4} * (-0.5) + \frac{1}{4} * (1.5)$ ). Thus, both types of children have the same average educational attainment.

The instrumental variables estimate (in this case the Wald estimate) is the difference in educational attainment (0.0) divided by the difference in entry age (0.25) and is therefore zero. Even though every child benefits from entering school when older, the IV estimate is that entry age has no effect on the outcome. As this simple example illustrates, failure to

satisfy the monotonicity assumption can produce an estimate with the wrong sign.<sup>12</sup>

Although neither quarter of birth nor legal entry age satisfies monotonicity, it is possible to find an instrument that does. Figure 2 shows the distribution of entry age for children born in the fourth quarter in states that permit them to enter school in the year in which they turn five (unconstrained states) and in states that formally restrict them to enter only in the year in which they turn six (constrained states). We can see that first-order stochastic dominance is satisfied: children born in the fourth quarter in constrained states enter school later than those in unconstrained states. We note that first-order stochastic dominance is only a necessary, not a sufficient, condition for monotonicity and only one of several requirements for consistency.<sup>13</sup>



Figure 2: CDF of Entry Age: Constrained vs Unconstrained States 4th Quarter Births: 1952

In the next two sections we compare IV estimates of the effect of school entry on educational attainment for different choices of instrument. We use the argument above to propose an instrument that satisfies monotonicity and show that the quarter of birth instrument and the legal entry age instrument give biased estimates of the policy-relevant LATE.

<sup>&</sup>lt;sup>12</sup>Applying the formula in (1), the total change in entry age is -.25 of which -.375 is explained by those lowering their entry age. Thus the coefficient is 1.5\*1-.5\*3=0.

<sup>&</sup>lt;sup>13</sup>As discussed in detail in Heckman, Urzua and Vytlacil (2006), an additional variable influencing entry age but not included in the estimation could be correlated with the state law and lead to a violation of monotonicity. The IV estimate would provide an inconsistent estimate of LATE even though the usual requirements for IV are satisfied. For example, if states permitting early entry age also tended to be states with inexpensive childcare. Some parents may delay entry and take advantage of the inexpensive childcare in an early entry age state but they succeed in entering their child early in a state with a stricter cutoff. If this effect were modest, stochastic dominance could be satisfied even though monotonicity is not.

## 3 Methods: Two Sample Two Stage Least Squares

We estimate the following equation for educational attainment:

$$A_{i} = \alpha D_{i} + X_{i}^{\prime}\beta + \sum_{j=2}^{4} Q_{ij}\gamma_{j} + \delta S_{i} + \epsilon_{i}$$

$$\tag{3}$$

where,  $A_i$  is the educational attainment of individual *i*.  $D_i$  is the dummy endogenous variable that takes on the value of 1 if individual *i*'s school entry is delayed from the year in which he turns five to the year in which he turns six.  $Q_{ij}$  is a set of three dummy variables (j = 2, 3, 4) indicating the quarter of birth of the *ith* individual.  $X_i$  is a vector of observable individual characteristics and  $S_i$  denotes state dummy variables. Since OLS estimates of  $\alpha$ in the above model might be biased by the decision of some parents to accelerate or redshirt their children, we estimate a 2SLS model based on the following first stage equation:

$$D_i = \pi Z_i + X'_i \lambda + \sum_{j=2}^4 Q_{ij} \theta_j + \varphi S_i + \upsilon_i$$
(4)

The binary instrument  $Z_i$  equals one if the individual was required by state law to delay kindergarten entry. In other words if the child's month of birth is later than the state kindergarten entry age cutoff date,  $Z_i$  equals one and equals zero otherwise.

In this setting, LATE implies that we identify the policy relevant parameter, i.e. the effect on those individuals who delay enrollment only because they are constrained by the law. In contrast, it is unclear what the policy relevance of the LATE estimates using "quarter of birth" and "legal entry age" instruments would be even if they were consistent. If the law were uniform and strictly enforced and therefore monotonicity satisfied, the "born in first quarter" instrument could only hope to identify the effect of entering school when roughly six months older (on average) than those born in the other three quarters.<sup>14</sup> Unless we believe that the effect of entry age is linear, the effect of an average six-month difference in entry may be very uninformative about the effect of entering a full year earlier. For similar reasons, the entry age effect derived from regression discontinuity designs, even when consistent, is often of little policy interest.

Assessing the LATE measured when we use legal entry age as an instrument is more complex but similar. For example, suppose that we use legal entry age as an instrument in a country in which everyone enters exactly at the legally permitted age. In this case, monotonicity is satisfied. Moreover OLS and IV are identical, which simplifies the analysis. The LATE estimator in this case is a least squares approximation of the effect of entering

<sup>&</sup>lt;sup>14</sup>Literally, the weighted average of the effect of different entry age discrepancies with a mean discrepancy of about six months.

school when one day older. It is therefore a measure of the effect of moving the first day of school one day later if nothing else changed. However, for the most part, moving the first day of school from early September to early October in order to raise the school entry age is not part of the policy discussion. What is under discussion is whether to change the *minimum* entry age. The LATE estimate using legal entry age may be a very poor estimate of the effect of moving the entry age for a group of students from just under five years old to just under six years old.

To our knowledge, there is no large nationally representative data set with information on school entry age, educational attainment and quarter of birth. To circumvent the lack of data, we use the Two Sample Instrumental Variables (TSIV) procedure developed by Angrist and Krueger (1992,1995). TSIV requires that we have data on the endogenous variable  $(D_i)$  and the instrument,  $Z_i$ , for a cohort in one data set and the outcome of interest  $(A_i)$  and  $Z_i$  of the same cohort in another data set. We combine data from the 1960 and 1980 US Census for individuals born in the US between 1949 and 1953. We obtain first stage coefficients from the 1960 Census and use them to predict entry age of the contemporaneous 1980 Census respondents. Instrumental variable estimates are generated by regressing 1980 educational outcomes on the cross-sample fitted value of their entry age.<sup>15</sup> The standard errors are then adjusted to account for the use of a predicted value in the second stage. The appendix gives a detailed description of the method used to consistently estimater the correct asymptotic covariance matrix for TSIV with Moulton clustered standard errors.<sup>16</sup>

Since we control for quarter of birth (and state), the instrument has a monotonic effect on school entry age. The monotonicity assumption would be violated if there were "defiers". In other words, if some children born in the fourth quarter enter school early only when they are prohibited from doing so. Although we cannot directly test for such violations, we find them implausible.

Our identification strategy requires that the school entry cutoff date has no effect on either the entry age or the educational outcomes of children born in the first three quarters. This condition would be violated if parents do not want their child to be the youngest in class. Therefore, they may decide not to redshirt a child born in September in a state with a late cutoff (e.g. January 1), but decide to redshirt in a state with an early cutoff (e.g., October 1). In this scenario, school entry age laws would affect the entrance age of those children who are not directly constrained by the law (i.e. those born in the first three quarters). Such externality effects would be a threat to our identification strategy.

<sup>&</sup>lt;sup>15</sup>Inoue and Solon (2006) call this the two-sample two-stage least squares (TS2SLS) estimator. They note that in finite samples, the TSIV estimator originally proposed by Angrist and Krueger and the TS2SLS estimator typically used by practitioners are numerically distinct. In addition, they show that the TS2SLS estimator is asymptotically more efficient.

 $<sup>^{16}</sup>$ Note that the treatment varies at the state/quarter of birth level and that it is therefore important to cluster the standard errors at this level.

	1	0/1 or 9/30 cutoff	1/1 o	r 12/31 cutoff
Birth Quarter				
Born Quarter One	23.97		24.75	
4.5		11.4		10.99
5.5		73.44		72.99
6.5		14.38		14.99
7.5		0.78		1.03
Born Quarter Two	23.84		23.04	
4.25		7.42		7.45
5.25		75.94		73.02
6.25		15.56		18.3
7.25		1.08		1.23
Born Quarter Three	26.92		26.81	
4		7.28		6.76
5		72.97		73.57
6		18.79		18.59
7		0.96		1.08
Born Quarter Four	25.26		25.40	
3.75		4.76		7.64
4.75		40.57		67.21
5.75		47.77		23.54
6.75		6.90		1.62

 Table 2: Distribution of Entry Age (1960 census, 1949-1953 cohorts)

Table 2 provides some evidence that this "no externality" condition is satisfied and school entry age laws are not affecting entrance age of unconstrained children.<sup>17</sup> Using the 1960 Census, we show the average entry age by quarter of birth of individuals born between 1949-1953 in two types of states. As this table illustrates, the average school entry age of individuals born in the first three quarters does not vary much by whether they are in a fourth quarter constrained state or not. In other words, raising the minimum entry age does not affect redshirting among those not constrained by the law. In contrast, the distribution of entry age for the fourth quarter differs noticeably between the two types of states. Interestingly, increasing the minimum entry age, by constraining those born in the fourth quarter to enter late, appears not only to reduce the proportion of children entering at very young ages but also appears to increase the proportion entering even later than required by the law. This does not affect the consistency of the IV estimator but does suggest that the mechanism, and therefore the LATE, is more complex than simply

Note: Constrained to enter at the earliest reasonable age if actual entry age was either too young or too old

<sup>&</sup>lt;sup>17</sup>Angrist and Krueger (1992, table 2) shows mean differences in entry age by cutoff age. The pattern of differences implied by our table 2 are similar to theirs although we tend to find larger entry age differences across quarters of birth, possibly because we choose a narrower age range less subject to the effects of grade retention. They do not address whether the pattern of entry age in the first three quarters differs by cutoff date.

constraining some children who would not otherwise be constrained.<sup>18</sup>

Our instrument would be invalid if it affected the educational attainment of individuals born in the first three quarters. This would be the case if the age of the other children in the classroom affected the educational attainment of children not directly affected by the law. To test whether entry laws are independent of other factors affecting educational attainment, we regress educational attainment for children born in the first three quarters on state cutoff dates controlling for gender, race, age and age squared (Not including 3rd quarter births in states with September cutoffs). In essence this asks whether children unaffected by the cutoffs get more or less education in states with later cutoffs. Such a relation could arise even without an externality if the cutoff were endogenous to education levels, and it is for this reason that our principal estimates control for state. However, in fact, the coefficients on attainment are small and statistically insignificant. This suggests that the cutoff does not affect the outcomes of those it does not directly constrain and also that the cutoff is exogenous to education levels.<sup>19</sup>

Finally, it is possible that the cutoff date is endogenous to the relative performance of students born in different quarters. For example, one can argue that states in which children born in the fourth quarter do well in school adopt a December 31 cutoff. On the other hand, states where their performance is relatively weak adopt an earlier cutoff. If this is true, we would be subject to a critique similar to the Buckles and Hungerman (2008) critique of the quarter of birth instrument. They find that weather has a greater effect on births to disadvantaged mothers. Thus if the weather produced more (disproportionately disadvantaged) births in the fourth quarter in some states and those states had earlier cutoffs, our instrument would be likely to be invalid. However, table 2 also shows that the seasonality of births is similar in states with a third and fourth quarter cutoff. We cannot reject that seasonality of birth in this table is independent of state cutoffs ( $\chi^2_{(3)} = 0.10$ ).

## 4 Data

The data on school entry age comes from the one percent sample of the 1960 US Census Public Use Microdata Sample (PUMS). We use the 1980 U.S. PUMS five percent sample to measure educational attainment. Both samples have information on quarter of birth.

The main endogenous variable is a dummy variable indicating whether the individual delayed school enrollment from the year he turned 5 to the year he turned 6 or later. Age

 $<sup>^{18}</sup>$ It is important to note that in the 1960s there is significant noncompliance, especially among fourth quarter children, in both types of states. In states with a 10/1 or 9/30 cutoff, almost 45% of fourth quarter individuals enter school even before they are allowed to enter. On the other hand, in states which allow fourth quarter children to enter early, about 25% redshirt.

<sup>&</sup>lt;sup>19</sup>Not shown here, but regressions are available upon request.

in quarters was computed as of Census day (April 1, 1960) using information on quarter of birth. The census, however, does not collect school entry age information. School entry age can still be computed using highest grade completed if we assume that no one repeats or skips a grade. We do not know whether children attended kindergarten or entered first grade directly as was common during this period. We treat all individuals as having spent a year in kindergarten. Thus someone who first enrolled in school as a first-grader at exactly the age of 6 would be counted as having entered school at exactly age 5. Based on this assumption, we computed the school starting date for individuals born in the US between 1949 and 1953.

Our identification strategy requires knowledge of exact kindergarten entry cutoff dates for 1954 to 1958, the years in which the individuals in our sample were eligible to enroll in kindergarten. We collected data on state laws regarding kindergarten entry ages using historical state legal statutes. If the history of the statute indicated a change in the state law in any given year, we examined the state session law to determine the exact form of the change. Children who entered school in states that gave Local Education Authorities the power to set the entry age were deleted from the sample. Table 3 lists the kindergarten entry age cutoff dates for 1958 for the states used in our analysis.

For both samples, we use information on quarter of birth, age, state and cutoff date to determine whether each sample member was born before or after the state cutoff. We delete observations for whom we cannot determine whether the individual was born before or after the cutoff. For example, we drop individuals born in the third quarter in states with a September 1 cutoff. In both data sets, we restrict the sample to individuals whose state of birth and current residence were identical. The sample is restricted to blacks and whites including those of Hispanic origin. For the 1980 sample, we only include individuals who had completed at least one year of schooling. Our final sample includes 96676 observations in the 1960 Census and 373845 observations in the 1980 Census. All regressions include dummies for quarter of birth, sex, race and state and age in quarters and age squared.<sup>20</sup>

Table 3: School Entry Cutoff Dates in 1958							
1-Sep	10-Sept/15-Sept	30-Sept/1-Oct	15-Oct/16-Oct	31-Oct/1-Nov	1-Dec	31-Dec/1-Jan	1-Feb
Colorado	Iowa	Alabama	Idaho	DC	California	Connecticut	Pennsylvania
Delaware	Montana	Arkansas	Maine	North Dakota	Illinois	Florida	
Kansas	New Hampshire	Missouri	Nebraska	Oklahama	Louisiana	Kentucky	
Michigan	Ohio	New Jersey		South Dakota	New York	Maryland	
Minnesota	Wyoming	North Carolina		West Virginia	Wisconsin	Mississippi	
Oregon		Virginia				Nevada	
Texas						New Mexico	
Utah						Rhode Island	
						Tennessee	

Most of the identification in the data comes from (i) comparing the relative performance of fourth quarter and other births in states with December 31 cutoffs with those with

<sup>&</sup>lt;sup>20</sup>We experimented with also including year of birth, but the results were unchaged.

September 30 cutoffs and from (ii) comparing the fourth quarter relative to the first two quarters in states with cutoffs in the first half of September. As shown in table 3, there is considerable within region variation. Florida and Mississippi have late cutoffs while Alabama, Arkansas and Virginia have third quarter cutoffs. Pennsylvania and two New England states have late cutoffs while New Jersey has a third quarter cutoff and Ohio has an early cutoff.

## 5 Results

### 5.1 First Stage

Table 4 presents the first stage results from the 1960 Census for different choices of instrument. Column (1) reports results from the regression of entry age (in years) on one quarter of birth dummy (QOB 1 versus all others). Column (2) uses three quarter of birth dummies (QOB 4 is the omitted quarter). Column (3) shows first stage results using legal entry age as the instrument without quarter of birth controls and finally, column (4) reports estimates from our basic model, controlling for three birth quarters and a binary instrument (delayed by law).<sup>21</sup> Controlling for legally mandated delayed enrollment in column (4), the school entry age monotonically decreases with quarter of birth. Column (4) reveals that individuals born in the first quarter begin school when they are about one-half year older than are those born in the fourth quarter and who are not constrained by state laws. On the other hand, in column (2) the quarter of birth instrument shows a much smaller difference in entry age between the first and the fourth quarter since it fails to control for the more restrictive laws in some states. Note also that the effect of "delayed" is only .37. While some children born in the fourth quarter begin school when they are first allowed to enroll, others are held back an additional year until they are almost 6 years old, and some who are not legally entitled to enroll before age five are nevertheless able to do so.

One concern with the entry age variable is that since we assume there is no grade retention, we are overestimating entry age. This is especially problematic since past research has shown that the probability of repeating a grade is related to school entry age. Although we do not have information on grade retention in the Census, we can minimize the error in measuring the entry age variable by restricting the sample to the youngest cohort. The fifth column of table 4 restricts the sample to those born in 1953. If one assumes that entry patterns were constant from 1949 to 1953, then the difference between the baseline

<sup>&</sup>lt;sup>21</sup>Note that this specification is isomorphic to one in which legal age is used as the IV and quarter of birth is included in the structural equation. This specification can be found in the literature as a robustness check (Elder and Lubotsky, 2006). Angrist and Krueger (1992) also include specifications with state dummies but, since that paper pre-dated awareness of the weak instrument problem, used roughly 1400 interaction terms of quarter of birth, state and year of birth as instruments.

estimates in column (4) and those obtained using only the 1953 data reflect the effect of grade retention. In this case, estimates based on 1953 data would be preferred. Estimates using the 1953 only first-stage can be obtained by multiplying coefficient on "delayed" in the baseline model by .3664/.4273 or .8575.

Table 4:	First Stag	e Estimates	: 1960 cen	sus (1949-1	953 cohorts)
	(1)	(2)	(3)	(4)	Born in 1953*
	D	ependent V	ariable: S	chool Entra	nce Age
Born quarter 1	0.2447	0.2685		0.5585	0.5988
	(0.0193)	(0.0374)		(0.0132)	(0.0161)
Born quarter 2		0.1119		0.4024	0.4468
		(0.0377)		(0.0128)	(0.0161)
Born quarter 3		-0.0834		0.1942	0.1985
		(0.0392)		(0.0118)	(0.0161)
Delayed by Law				0.3664	0.4273
				(0.0397)	(0.0179)
Legal entry age			0.4141		
			(0.0551)		
Observations	96676	96676	96676	96676	19949
R-squared	0.09	0.11	0.09	0.11	0.18

Table 1. First Stag	Fetimatae 10	060 consus (1	040-1053 c	aborts)
Table 4: First Stag	e Estimates: 1	900 Census (1	949-1955 0	onorts)

Note: Robust standard errors clustered by state/quarter of birth.

Controls: state fixed effects, age in quarters, age square, race (white/black) and sex.

Sample restricted to individuals for whom state of birth is identical to birthplace.

It is also worth noting that, using the 1953 data, the difference in entry age between those born in the second and third quarter is almost exactly .25, suggesting that monotonicity would apply to a sample of individuals born in these quarters. This, in turn, would mean that it is possible to compute a LATE based on these samples. However, it is not clear that this LATE would be of any policy interest.

#### 5.2**Reduced-Form and TS2SLS Estimates**

Table 5 reports reduced-form estimates from the 1980 Census. In column (1), which gives the reduced form when the instrument is "born in first quarter," the instrument is associated with a large negative effect on educational attainment. In column (2), legal entry age instrument shows a somewhat smaller and statistically insignificant adverse effect. Finally, the last column indicates that controlling for quarter of birth, there is almost no effect of delayed school entry on educational attainment.

QOB	Legal Age	Delayed
dent Variable:	<b>Educational Attain</b>	ment
	-0.029	-0.0029
	(0.0211)	(0.0265)
-0.0444		-0.0689
(0.0081)		(0.0222)
		-0.0459
		(0.022)
		-0.0122
		(0.0234)
373845	373845	373845
0.05	0.05	0.05
	QOB dent Variable: -0.0444 (0.0081) 373845 0.05	QOB         Legal Age           dent Variable:         Educational Attainst -0.029 (0.0211)           -0.0444 (0.0081)         -0.0081)           373845         373845 0.05

Table 5: Reduced Form Estimates 1980 census (1949-1953 cohorts)

Note:

1. Robust standard errors clustered by state/quarter of birth

2. All estimates control for state fixed effects, age in quarters, age square, race (white/black) and gender

Table 6 combines estimates from the 1960 and 1980 Censuses. Using first stage coefficients reported in table 4, we predict entry age for the 1980 Census respondents. TS2SLS estimates are generated by a regression of 1980 educational outcomes on the predicted entry age. Using the method described in the appendix, we correct the standard errors to account for the fact that the predicted value of school entry age is used in the second stage. In addition, the standard errors are adjusted for clustering (at the level of state\*quarter of birth) using a parametric Moulton (1986) correction factor.

When we use "born in the first quarter" as our instrument, consistent with Angrist and Krueger, we find a large negative effect of school entry age on educational attainment. When we use legal entry age (not controlling for quarter of birth), we find a smaller but still substantial adverse effect that falls short of statistical significance at conventional levels and is therefore consistent with the zero effect in Bedard and Dhuey. The two estimates are significantly different at the .01 level. Finally, when we use the consistent estimator that meets the monotonicity requirement, our estimate is very close to zero.

	QOB	Legal Age	Delayed		
	Dependent Variable: Educational Attainment				
Predicted Entrance Age	-0.1815	-0.0700	-0.0078		
	(0.0422)	(0.0450)	(0.0727)		
Born in quarter 1			-0.0645		
<u>1</u>			(0.0229)		
Born in quarter 2			-0.0427		
			(0.0146)		
Born in quarter 3			-0.0107		
			(0.0152)		
Observations	373845	373845	373845		
R-squared	0.05	0.05	0.05		

Table 6: Two Sample Instrumental Variable Estimates 1960-1980 census

Note: Moulton-corrected standard errors in parentheses.

Additional controls for state, age in quarters, age squared, race (white/black) and sex.

We also study the effect of delayed enrollment on other measures of educational attainment namely, high school dropout/completion and college attendance. We have also looked at the differences in outcomes by sex, race and race and sex interacted, but do not find any statistically significant effect. We do not find any effect for whites or for either sex separately. However, as shown in table 7, we find a nontrivial and marginal statistically significant effect of delay on the dropout rate among blacks. For blacks delaying entry to school is associated with a decline in the dropout rate of about 8 percentage points. Our point estimates also suggest that delayed entry increases educational attainment among blacks by a nontrivial quarter of a year. However, the coefficient is not significant at conventional levels. We do not want to put too much weight on this finding. After all, we have looked for significant effects on several overlapping groups using multiple measures of educational attainment. Finding a t-statistic of just under 1.96 in one specification for one group is not all that unlikely. However, it is plausible that blacks were affected more positively by delay than were other groups. For blacks, it is particularly important to note the historical nature of the finding since we are looking at students starting school during a period that preceded the 1964 Civil Rights Act and when blacks were hugely disadvantaged both in terms of school quality and parental income. It is plausible that black children (and other children from disadvantaged backgrounds) who entered school early did so for financial reasons and were frequently pushed ahead before they were sufficiently mature.

	Entire Sample	Blacks Only
Attainment	-0.0078 (0.0727)	0.2759 (0.2250)
High School Dropout	-0.0136 (0.0107)	-0.0832 (0.0425)

Table 7: TS2SLS Estimates: Blacks Only

Note: Moulton-corrected standard errors in parentheses.

## 6 Different LATEs or Failure of Monotonicity?

There are a number of reasons that the three estimates in table 6 may differ. First, if quarter of birth is correlated with unobserved individual characteristics, both of the two traditional estimators are inconsistent. As discussed earlier, our approach requires the weaker assumption that the difference in unobserved characteristics across quarters of birth is uncorrelated with the state cutoff. We also require that the entry age law affects only the entry age and educational attainment of the formally constrained group (i.e. those born in the fourth quarter). Second, even if all three estimators are consistent when treatment effects are homogeneous (the traditional, non-LATE interpretation of IV), if treatment effects are heterogeneous, these instruments may not provide consistent estimates of the LATE. Finally, one or more of these estimators may be consistent, but the LATEs captured by the instruments may differ.

Our focus in this part of the paper is to distinguish between these last two explanations. We estimate a very simple model of the school entry age decision. We then use the estimated model to calculate the joint distribution of school entry age and educational attainment. Using these data, we measure the "effect" of entry age using each of the instruments. In each case, the estimate using the simulated data is similar to the estimate using the actual data. However, with the simulated data, we can calculate the true LATE that should be associated with that particular IV estimator. In this way, we can determine whether the departure from monotonicity is important.

### 6.1 Model

Our approach is very simple, consistent with the limited data available to estimate it. Every child has an entry age,  $E_i^*$ , which maximizes her educational attainment:

$$E_i^* = a_0 + a_1 \widetilde{E}_i.$$

We assume that the random component,  $\tilde{E}_i$  is distributed  $Beta(\alpha, \beta)$  with the two shape parameters  $\alpha$  and  $\beta$ . The parameters  $a_0$  and  $a_1$  determine the bounds of the attainmentmaximizing entry-age distribution,  $a_0$  gives the lower bound while  $a_0 + a_1$  sets the upper bound. We allow  $a_0$  to depend on the state entry age law. However, because we have data on quarter of birth (as opposed to month of birth), we restrict the analysis to two types of states. The unconstrained states (u) refers to states with a either a 1/1 cutoff or a 12/31 cutoff so that all children, including those born in the fourth quarter are permitted by the law to enter kindergarten in the year that they turn five. The second type of state, the fourth quarter constrained state (or "constrained state", for short) (c), is restricted to states with 9/30 or 10/1 cutoff.

We introduce a shift parameter for being in a constrained state:

$$a_o^c = a_o^u + \lambda$$

This implies that raising the minimum entry age for fourth quarter children may affect the attainment-maximizing entry age for everyone else. By allowing this age to be affected by school entry age laws, we are allowing for spillover effect of laws. Existence of such externalities would be a violation of the exclusion restriction required for identification using instrumental variables, including our own, based on entry age laws.

Let  $E_i$  be the actual age at which a child begins school.  $E_i$  would differ across children because of differences in quarter of birth and school cutoff. We assume that students suffer an education penalty if they enter at an age other than their attainment-maximizing entry age,  $E_i^*$ . For example, a student who is born on March 1 and whose attainment-maximizing entry age would be age 5 (if school started on March 1), is now forced to enter at age 5.5 because school begins on September 1. She suffers a loss associated with being six months away from her attainment-maximizing entry age. We assume that the education loss is linear in the absolute departure from the attainment-maximizing entry age. Thus, ultimate educational attainment is given by:

$$S_i = S_i^* + \mu * |E_i - E_i^*|$$

 $S_i^*$ , which is unobserved, is the educational attainment the individual would have attained if she had entered at exactly her attainment-maximizing age. We assume that  $S_i^*$  is independent of quarter of birth and state cutoff date. This assumption rules out season of birth effects.

Our choice of this particular form is driven by the paucity of data. As discussed below,

we use the data to identify six parameters.

### 6.2 Indirect Inference

We use indirect inference to estimate the six parameters of the model  $(a_0, a_1, \alpha, \beta, \lambda, \text{ and } \mu)$ so that the moments from the simulation match the moments from the data. We generate 10,000 draws from the beta distribution.

For simplicity, we assume that children born in quarter 1 are born on 2/15, quarter 2 on 5/15, quarter 3 on 8/15, and quarter 4 on 11/15. Further we assume that the first day of school each year is August 15th in every state. This implies that Quarter 1 students can enter school at age 4.5, 5.5, 6.5 or 7.5. Similarly, those born in quarter 2 can enter at 4.25, 5.25, 6.25 or 7.25 and so on for the third and the fourth quarter.

We do not impose that individuals enter school at the date that is closest to their attainment-maximizing age. Instead we assume that individuals with the lowest attainment-maximizing age are the ones, among those born in a given quarter, who enter when youngest. In other words, if we observe in the data that 10% of first quarter children enter at age 4.5, we assume that these are the 10% of the first quarter children with the lowest attainment-maximizing age. If we think that parents act optimally, this is equivalent to saying that any benefits (or costs, if negative) other than the effect on attainment are non-decreasing in attainment-maximizing entry age.<sup>22</sup>

Based on these assumptions, we use the distribution of entry age (1949-1953 cohorts) from the 1960 Census to generate simulated data. Thus, we allocate individuals to their entry age in the simulated data consistent with their quarter of birth and whether they live in a 4th quarter constrained state or not.

Next, we regress educational attainment from the 1980 census on three quarter of birth dummies, age in quarters and its square and state dummies, separately for the two types of states to get the vector of coefficients  $\hat{\beta}_{data}$  (i.e. a total of six moments, coefficients on three quarter of birth dummies in each type of state). These coefficients are the difference in average education between those born in each of the first three quarters and those born in the fourth quarter in each type of state. Identification in this model depends only on within state-type education differences since we are not using the difference in average educational attainment between the two types of states.

Finally, we characterize the loss function as the sum of the squared deviations between the regression coefficients from the simulated data and the actual regression coefficients weighted by the inverse of the variance-covariance matrix of the estimates,  $\hat{\Sigma}$ .

More formally, the objective of our indirect inference simulations is to choose parameters

 $<sup>^{22}</sup>$ It allows these costs to differ by quarter of birth and by state law so that the cost of delaying from age 4.5 to 5.5 may differ from the cost of delaying from 4.25 to 5.25 at the same point in the cdf.

of attainment-maximizing entry age distribution  $(\alpha, \beta, a_0, a_1)$  plus the shift parameter for constrained states,  $\lambda$ ) and of the education loss function  $(\mu)$  to minimize the following loss function:

$$(\widehat{\beta}_{data} - \widehat{\beta}_{sim})'\widehat{\Sigma}^{-1}(\widehat{\beta}_{data} - \widehat{\beta}_{sim})$$

### 6.3 Simulation Results

We begin by estimating the model without  $\lambda$ , the parameter measuring the externality on those born in the first three quarters that results from constraining the entry age of those born in the fourth quarter. Table 8 shows results from regressions of educational attainment on three quarter of birth dummies using the actual and simulated data. It shows the average difference in educational attainment between the fourth quarter and the three other quarters. The model fits quite well. No parameter is off by more than about one half of a standard error.

	<b>Unconstrained States</b>		<b>Constrained States</b>	
	Estimated	Simulated	Estimated	Simulated
Quarter 1	-0.076	-0.078	-0.093	-0.095
	(0.013)		(0.010)	
Quarter 2	-0.019	-0.018	-0.046	-0.039
Quarter 2	(0.016)	0.010	(0.014)	0.007
Quarter 3	0.023 (0.012)	0.018	-0.004 (0.007)	-0.004

Table 8: Educational Attainment and Quarter of Birth by State Type

Note: Robust SE clustered by state/quarter of birth. Controls include state dummies, age in quarters and its square. N=292771

In fact, the loss function is 0.60 which is distributed as a  $\chi^2$  with one degree of freedom. Therefore any parameter that we add to the model will not be statistically significant since the best it can do is to reduce the loss function to  $0.^{23}$  In particular, when we add the externality term to the model, it has a point estimate of 0.002 and improves the loss

<sup>&</sup>lt;sup>23</sup>Although the literature on indirect inference assumes that if the number of model parameters equals the number of empirical parameters, the fit must be perfect, it is easy to show that this need not be the case even when the underlying model is correctly specified.

function only trivially. Therefore in the remainder of this section, we use the model in which  $\lambda$  is constrained to equal 0.

The model parameters appear to us to be quite plausible. The lowest attainmentmaximizing age is 4.50 while the maximum is 7.63. The parameters of the beta-distribution imply that optimal entry age is skewed. The mean is 5.18 but the median is only 5.01. Most children would benefit from entering when relatively young, but some would be better off being significantly older than the norm. Children lose about two-thirds of a year of educational attainment if they enter a full year away from their attainment-maximizing age. But such large differences are rare, occurring only among a small percentage of those who would be best off entering when substantially older than the norm.

### 6.4 Reconsidering the Instruments

We now ask whether the failure of monotonicity produces estimates that are notably different from the LATE the (first) quarter of birth and legal entry age IV estimators are intended to measure.

Table 9 shows the results of applying each of the IV estimators to the data generated by our model. The first column reproduces the results from table 6. The corresponding rows in the second column show the estimates applied to our data. Although our parameters were not chosen to match the three IV estimates, the model fits the broad pattern found in the data. The "born in first quarter" instrument shows the most adverse effect of delaying entry while the "delayed by law" instrument finds the least adverse and possibly positive effect. In each case, the estimate derived from the model lies within the confidence interval of the actual estimate.

Next we ask how well each IV estimator would capture its intended LATE if the true world were generated by our model. What LATE should each estimator capture? In the absence of monotonicity the concept is not well-defined, but a reasonable interpretation is that it should be a weighted average of the treatment effects where all the weights are positive. For the quarter-of-birth instrument, it is straightforward to implement this definition. We can calculate the treatment effect for each individual born in the first quarter of being born in the second, third and fourth quarters. We then weight each of these changes by the absolute value of the change in entry age.<sup>24</sup>

<sup>&</sup>lt;sup>24</sup>Of course, we cannot calculate a treatment effect for those who do not change their entry age, but such individuals get zero weight in the calculation in any event.

0.19	
-0.18	-0.23
(0.04)	-0.24
0.07	0.20
-0.07 (0.05)	-0.09
	-0.25
-0.01	0.06
	(0.04) -0.07 (0.05) -0.01 (0.07)

Table 9: Effects of Entry Age on Education

The true LATE defined in this way is given in the second row of the last column of table 9. At least in the world represented by our model, the IV estimator is somewhat biased but only trivially. It is off by about .01.

Although the legal entry LATE relies on variation in both birth date and state laws, it seems to us that the goal is to estimate the effect of a small increase in entry age (from being born on, for example, February 1 rather than February 2) rather than some strange combination of small increases due to birth dates and large increases due to state law. We therefore calculate the (numeric) derivative of educational attainment with respect to an increase in entry age for all individuals in our sample and take the average. The result of this exercise is shown in the fourth row of table 9. It is evident that if this is the LATE that "legal age" is intended to capture, then it badly fails to do so. The estimated LATE is quite far from the true LATE.

By construction, using our approach, we get a consistent LATE estimate of the effect of the policy change of moving from a December 31 to an earlier cutoff on the educational attainment of children born in the fourth quarter (i.e. those children whose behavior is affected by the law). However, it is important to recognize that our estimates assume that there are no externalities from this change. We find no evidence of the existence of such externalities, but this is quite different from finding strong evidence of their absence. Conditional on this caveat, those children whose entry is delayed, on average, are not harmed and may benefit slightly from the delay.

Table 10: Effect of Raising Entry Age by Type of Law Enforcement: QOB 4 Only					
	Weak Enforcement	Strong Enforcement			
Change in Educational Attainment	0.02	-0.242			
Percent Increasing Educational Attainment*	57.54	19.08			
*Decults are only for these changing their educational levels					

\*Results are only for those changing their educational levels

## 7 Policy Experiments

An important policy question that arises from our analysis is whether our results would hold in the current school system where school entry laws are relatively strictly enforced. The weakly enforced cutoff dates in the 1950's may not be applicable to the debates involving school entry age today. Schools today are under great pressure to adhere to strict standards. As discussed in the introduction, a variety of factors have pushed states and districts to increase entry age requirements and enforce them more strictly, but it is very uncertain as to whether such policies are beneficial.

To study the effect of delaying school entry on attainment in recent years, we use the simulated data to perform some policy experiments. First, we look at the effect of moving from a January 1 cutoff to an October 1 cutoff around the 1950's, a period when such cutoffs were very loosely enforced. Second, we consider what would have happened had there been a strict October 1 cutoff.

Table 10 reports the results from these two experiments. In the first column, we explore the effect of the policy with weak enforcement. Consistent with the LATE estimates, the effect on average educational attainment is small but positive, and the majority of those whose entry age is changed by the law increase their attainment. The second column shows the results from the policy experiment with strict enforcement. We find that moving from a January 1 cutoff to an October 1 cutoff lowers average educational attainment of those born in the fourth quarter by about one-fourth year, and the great majority of those who are compelled to change their behavior are hurt by it.

When laws were weakly enforced, the constrained children (those born in the fourth quarter) had the option to enter school earlier than officially permissible. We see ample evidence of this happening in our data. In this environment, overall, children benefited, in terms of higher educational attainment, by moving to an October 1st cutoff. However, the policy experiment suggests that, in an environment where laws are strictly enforced, constraining fourth quarter children to enter late hurts these children and reduces average educational attainment.

## 8 Conclusion

In this paper, we argue that previous studies that have used IV to deal with the endogeneity of school entry age have focused on a LATE of no real policy or practical interest. Our instrument measures the effect on children who would otherwise enter kindergarten in the year they turn five of being required by law to delay entry until the year in which they turn six. Moreover, previous studies have failed to provide consistent estimates of the LATE because of the failure of the monotonicity assumption. As a practical matter, this turns out to be a serious problem for the "legal age" instrument but not for the "first-quarter birth" instrument.

The born in first quarter instrument, consistent with Angrist and Krueger, gives a large negative effect of school entry age on educational attainment. When we use legal entry age (not controlling for quarter of birth), we find a smaller adverse effect but one that falls short of statistical significance at conventional levels (consistent with the zero effect in Bedard and Dhuey). We propose an instrument that satisfies the monotonicity assumption and gives a consistent estimates of the policy-relevant LATE: the effect of requiring a child to enter school in the year she turns six when she would otherwise have entered a year earlier. The results are consistent with no important policy effect as the policy was practiced in the 1950s.

However, over the last fifty years, school entry age laws have become noticeably stricter both in requiring children to be older before entering school and through stricter enforcement of the laws limiting entry although they generally continue to permit redshirting. We find that stricter enforcement of the laws in the 1950s would have had adverse effects on educational attainment. While we do not know whether the results continue to apply today, they do provide evidence of considerable variation in optimal entry age and therefore suggest that having a waiver policy that gives constrained children the choice to enter earlier than the legally established age could increase educational attainment, particularly among groups that have high dropout rates.

Some of the concerns raised in this paper are well known. In particular, a number of the papers we cite question existing instrumental variables estimates on the grounds that birth date is likely to be correlated with unmeasured characteristics. Partially in response to these concerns, it has become increasingly popular to rely on a regression discontinuity design (see for example, Black, Devereux and Salvanes, 2008). This seems to us very defensible in the Norwegian context where almost all children enter school exactly when first permitted by law. However, it remains problematic in the U.S. context.

A typical regression discontinuity band is one month before and after the cutoff date. Therefore, the regression discontinuity looks at the combined effects of being a month younger on those most committed to entering young and those most committed to entering when old and being eleven months older on those in the middle. In the U.S. context, where there is considerable redshirting and many children get around the law, the first two groups can be quite large. Consequently monotonicity is violated, and it is unclear exactly what the regression discontinuity is estimating. Moreover, there is no reason to believe that it provides a consistent estimate of the parameter of policy interest, the effect on children whose entry age is raised by the entry age regulation. Thus many of the concerns raised in this paper apply to regression discontinuity.

### **Appendix: Standard Error Derivation**

Let the first stage be

$$Y_{ic} = X_{ic}B_1 + cD_{ic} + \alpha_c + \varepsilon_{ic} \tag{A1}$$

where observations are indexed by i and grouped in clusters indexed by c. D is the excluded instrument. Within each cluster c, the  $(Y_i, X_i)'s$  are correlated, but  $(Y_i, X_i)$  from different clusters are independent. Let  $\alpha_c$  be the random component specific to cluster c and  $\varepsilon_{ic}$  is the individual specific error term.

For convenience we can write the first stage as

$$Y_{ic} = Z_{ic}\Gamma + \alpha_c + \varepsilon_{ic}$$

Let the structural equation be

$$y_{ic} = X_{ic}B_2 + \gamma Y_{ic} + \mu_c + \nu_{ic}$$

$$= X_{ic}B_2 + \gamma (Z_{ic}\Gamma + \alpha_c + \varepsilon_{ic}) + \mu_c + \nu_{ic}$$

$$= X_{ic}B_2 + \gamma Z_{ic}\Gamma + \delta_c + \zeta_{ic}$$

$$= X_{ic}B_2 + \gamma Z_{ic}\widehat{\Gamma} + \delta_c + \zeta_{ic} + \gamma (Z_{ic}(\Gamma - \widehat{\Gamma}))$$
(A2)

Let  $X^* = [X \ Z\widehat{\Gamma}]$  and  $B = [B_2 \ \gamma]$ . Then

$$V(\widehat{B}) = E(X^{*'}X^{*})^{-1}X^{*'}\varpi \,\varpi' X^{*}(X^{*'}X^{*})^{-1}$$

where  $\varpi$  is the error term defined above i.e.

$$\varpi = \delta_c + \zeta_{ic} + \left[ \gamma Z_{ic} (\Gamma - \widehat{\Gamma}) \right]$$

Each of the error terms is orthogonal to Z. Therefore the TS2SLS covariance matrix in the presence of clustering is given by:

$$V(\hat{B}^{TS2SLS,Moulton}) = (X^{*\prime}X^{*})^{-1}X^{*\prime}\Omega X^{*}(X^{*\prime}X^{*})^{-1} + \gamma^{2}(X^{*}X^{*})^{-1}X^{*\prime}ZV(\hat{\Gamma})Z^{\prime}X^{*}(X^{*\prime}X^{*})^{-1}$$
(A3)

where  $\Omega$  is a block diagonal matrix with diagonal elements  $\omega_c$  (the intra-cluster correlation

matrix for each cluster c)

$$\omega = \begin{bmatrix} \sigma_{\delta}^2 + \sigma_{\zeta}^2 & \sigma_{\delta}^2 & \dots & \sigma_{\delta}^2 \\ \sigma_{\delta}^2 & \sigma_{\delta}^2 + \sigma_{\zeta}^2 & \dots \\ \vdots & \vdots & \ddots & \vdots \\ \sigma_{\delta}^2 & \dots & \sigma_{\delta}^2 + \sigma_{\zeta}^2 \end{bmatrix}$$
(A4)

and

$$V(\widehat{\Gamma}) = (Z'Z)^{-1} Z' \Omega_2 Z (Z'Z)^{-1}.$$
 (A5)

It is easy to show that this formula reduces to the asymptotic covariance matrix formula for TS2SLS estimator derived by Inoue and Solon (2010). However, we also correct for the possibility of Moulton clustering in each stage.

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