THE WAGES OF FAILURE: NEW EVIDENCE ON SCHOOL RETENTION AND LONG-RUN OUTCOMES

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Abstract

By estimating differences in long-run education and labor market outcomes for cohorts of students exposed to differing state-level primary school retention rates, this article estimates the effects of retention on all students in a cohort, retained and promoted. We find that a 1 standard deviation increase in early grade retention is associated with a 0.7 percent increase in mean male hourly wages. Further, the observed positive wage effect is not limited to the lower tail of the wage distribution but appears to persist throughout the distribution. Though there is an extensive literature attempting to estimate the effect of retention on the retained, this analysis offers what may be the first estimates of average long-run impacts of retention on all students.

1. INTRODUCTION

Both the Goals 2000: Educate America Act, signed into law by Bill Clinton in 1994, and No Child Left Behind (NCLB), signed seven years later by George W. Bush, called for outcome-based reforms tying grade promotion to student performance. Opponents of retaining underperforming students argue that retention discourages students, destroys their confidence, delays exposure to the next year's curriculum, and leaves them further behind (Thompson and Cunningham 2000). Proponents of retention claim that the "social promotion" of underperforming students teaches students and teachers that effort and achievement are unimportant, forces teachers to deal with a much wider range of student preparedness, and denies students a second chance to learn what they missed (AFT 1997). Alleged costs or benefits of grade retention, then, have been used as a rationale for the large-scale, outcome-based education reforms of recent years and have just as often been used to criticize these policies. But there would seem to be little ground on which to make either judgment. Existing evidence is mixed, suffers from methodological problems, and is limited in scope to a narrow subset of students. In particular, most previous research on retention explores the impact of retention on the retained but abstracts from any possible impact on their promoted classmates. This is potentially a serious omission, as the vast majority of students are promoted. By estimating differences in long-run education and labor market outcomes for cohorts of students exposed to differing state-level primary school retention rates, this article attempts to fill that gap and shed some light on the potentially important effects of grade retention that may have been overlooked in previous work.

Our main finding is that a 1 standard deviation increase in retention through grade 2 is associated with an increase of 0.7 percent in male workers' average hourly wages. This finding is robust to controls for changes in school expenditures, early and delayed school entry, demographic composition of states, selective migration between states, and other potential confounding factors. Further, the observed positive wage effect is not limited to the lower tail of the wage distribution but appears to persist throughout the distribution. Though there is an extensive literature attempting to estimate the effect of retention on the retained, this analysis offers what may be the first estimates of average long-run impacts of retention on all students.

The remainder of the article is organized as follows. Section 2 reviews the previous literature and motivates the analysis; section 3 describes the data and empirical strategy; section 4 reports results and discusses possible implications of the findings; and section 5 summarizes and concludes.

2. THE IMPACT OF RETENTION

The Impact of Retention on the Retained

There is a vast literature examining the effect of retention on the retained. The following summary sketches the main findings but is by no means exhaustive. The main areas of comparison are academic achievement and high school completion. Several meta-surveys (Holmes and Matthews 1984; Holmes 1989; Jimerson 2001) find that in most early studies, retained students performed less well academically and were more apt to drop out of high school than were observationally similar promoted students.[1] However, drawing inferences from this body of research is difficult because selection on unobservables is a serious confounding factor. It is likely that decisions to retain one student and promote a superficially similar student hinge on student traits that the educator observes but the researcher does not (e.g., maturity, motivation, the ability to sit still). Retained students may therefore differ substantially from comparison groups of promoted students.

Three recent studies employ regression discontinuity strategies to address the selection problem. Jacob and Lefgren (2004) take advantage of an accountability policy in the Chicago public schools that tied summer school attendance and grade retention to test score cutoffs. They find that retained third graders scored higher on math achievement tests two years later than their promoted counterparts, while retained sixth graders scored lower on reading tests.[2] In a follow-up (Jacob and Lefgren 2007), the authors report a small increase in the probability of dropping out for students retained in eighth grade but no significant effect on the dropout rate for sixth-grade retention. Greene and Winters (2007), using a regression discontinuity design made possible by a similar Florida policy, report increased reading gains for students retained in third grade, relative to promoted students. These gains may have resulted from retention itself or from specific interventions for retained students that were also mandated by the Florida policy.

Potential Implications of Retention Policy for All Students

Is there a reason to believe that marginal increases in retention can have an impact beyond the marginally retained? Previous work in applied economic theory offers many. Economic theory predicts that retention policies influence the effort, expectations, and practices of students and their teachers, and

1. A few studies have found positive effects of retention in some settings (see Karweit 1991; Alexander, Entwisle, and Dauber 1995; Eide and Showalter 2001; Lorence et al. 2002).
2. The authors do not draw strong conclusions from the latter finding on retained sixth graders because promoted students faced high-stakes testing and retained students did not.

in so doing influence the outcomes of both promoted and retained students. Specifically, in the theoretical literature on education standards, altering a standard changes incentives for students and institutions. Neal and Schanzenbach (2007) emphasize the allocation of resources by institutions: the modification of a standard causes institutions to direct resources toward the subgroup of students nearest the cutoff and away from students at the top and bottom of the distribution, yielding potential effects on all students. Betts (1996) and Costrell (1994) report that when a standard rises, students throughout the distribution alter their effort choices, with relatively high-ability students increasing effort to meet it and others (those who had been marginally willing to meet the lower standard) giving up and reducing effort. To the extent that a change in retention policy is an implicit change in the standard for promotion, it may be expected to influence all students through these channels.[3]

A second strand of literature, the large body of theoretical and empirical work on peer effects, also leads one to expect an impact of retention policy on the promoted. Lazear's (2001) model of peer effects may be particularly relevant. In this model, a disrupting student prevents learning by all of his classmates for the duration of the disruption. Thus any intervention (such as a retention policy) that alters the behavior or allocation of disruptive students is expected to alter outcomes for all of that student's peers. More generally, a broad literature argues that interventions targeting a subgroup of students may alter outcomes of untreated classmates through imitation, mentoring, and other modes of social interaction (see, e.g., Akerlof 1997; Moffit 2001; Glaeser, Sacerdote, and Scheinkman 2003). There has been little previous work investigating peer effects associated with retention. Lavy, Paserman, and Schlosser (2008), in research that comes closest to this point, find that the proportion of delayed peers in a cohort (peers who are older than would be expected for their cohort) has a negative effect on student performance. The authors use the term *repeaters* to describe the older students but emphasize that they do so loosely: the vast majority of their repeaters delayed entry into first grade and were not retained. This research, then, yields results best interpreted as estimates of ability-related peer effects rather than retention effects.

In summary, economic theory offers many reasons to believe that retention policy has the potential to influence both the retained and the promoted. Further, large and far-reaching education reforms have been motivated in part

3. Because our data predate the broad standards-based reforms of recent years, we do not argue that changes in retention policies are due strictly to test-based standards. However, while the theoretical work cited above takes test-based standards as a point of departure, an identical logic holds for implicit modifications of informal standards of the sort that would yield changes in retention rates during the eras we study.

by alleged effects of retention on all students. But despite extensive research attempting to quantify the impact of retention on the subgroup of retained students, there is little or no existing evidence on the effect of retention policy on all students.[4] Below we describe the data and empirical strategy that we employ to investigate this broader effect.

3. EMPIRICAL STRATEGY

Specification

We model the response of adult labor market outcome, Y_{ibcy}, for individual i from state of birth b and year of birth cohort c, observed in the census or the American Community Survey (ACS) in year y, to the state of birth level retention rate, R_{bc}, as follows:

$$Y_{ibcy} = \alpha_0 + \alpha_1 R_{bc} + X_{ibcy}\alpha_2 + A_{ibcy}\alpha_3 + B_b\alpha_4 + T_{zc}\alpha_5 + \varepsilon_{ibcy}, \tag{1}$$

where X is a vector of personal characteristics, A_{ibcy} is a quadratic function of age,[5] B_b is a vector of state of birth indicators, T_{zc} is a vector of region z specific birth cohort indicators, and ε is the usual error term.[6] Equation 1 does not hold educational attainment constant because retention practices may alter labor market outcomes by changing highest grade attained or within-grade (cognitive or noncognitive) human capital accumulation.[7] Educational attainment and other intermediate outcomes are treated as endogenous, potential channels through which retention practices may affect labor market outcomes. Identification in this model comes from within-state changes in retention rates that predict within-state deviations from region-level trends in the dependent variable.[8] This model allows estimation of reduced form effects of grade retention for an entire cohort but does not separately identify the effect of retention on the subgroup of retained students. All regressions are population weighted, and the standard errors are clustered at the state of birth level.

Quantifying Retention Policies

One approach to studying the effects of retention policies is to look for within-state changes in relevant laws or other explicitly articulated polices.

4. There is also some research investigating the overall impact of school accountability on student outcomes (e.g., Carnoy and Loeb 2002; Hanushek and Raymond 2004). However, these articles do not specifically investigate grade retention.
5. All results are similar if a vector of age indicators is used instead. Under the specification with age indicators, one age indicator from each birth cohort is excluded.
6. We assume that individuals are educated in their state of birth. All retention measures are defined in the next section. Nonrandom migration is discussed in section 4.
7. In section 4 we also report results of a model that features educational attainment as the dependent variable.
8. Section 4 also presents results for models expanded to include current marital status, region and state of residence characteristics, and region of birth and region of residence interactions.

This approach is problematic because the content of retention-related laws varies greatly across states and over time, and the specific manner in which laws are translated into practice is often ambiguous. Beginning in the 1970s, many states passed laws that mandated minimum competency testing (MCT). Some of these laws may have influenced retention by tying grade promotion to test scores. However, early MCT laws were often simplistic one-page bills that left the crucial details of implementation to be worked out later (Pipho 2002). Beginning in 1985, and in several later periods, data on state-level MCT mandates were collected by the Education Commission of the States. States specified the grade levels tested and whether (and in which grades) the tests were to be used for grade promotion decisions. But most of the available data are too recent to allow a panel analysis of long-run outcomes, and the specific tests and test score cutoffs used in promotion decisions were not specified. Moreover, evidence suggests that retention rates often change in grade levels not specifically targeted by stated grade promotion policies. More specifically, educators appear to alter early promotion practices in anticipation of promotional gates on later grades (see Jacob 2002; Hauser, Frederick, and Andrew 2005). The diverse MCT laws, then, do not map to school retention practices in a coherent way. For these reasons, we argue that what states actually do is a better measure of their retention practices than their stated policies.

Because no national database of promotion and retention statistics exists, researchers often calculate retention rates from differences in age-grade retardation across consecutive years.[9] The rate of age-grade retardation, as used here, is the fraction of students below modal grade for a given age. For purposes of exposition, we focus on the complementary measure, the fraction of students who are "on time." More formally, we define the fraction of on-time students as the fraction of students in a birth cohort who have reached or exceeded the modal grade for that age. If O^a_{bc} is the fraction of students born in state b in cohort c observed to be on time when they reach the modal age a associated with a given grade, the inferred retention rate is:

$$R^a_{bc} = O^a_{bc} - O^{a+1}_{bc}. \tag{2}$$

This measure captures the decrease over a year in the fraction of students in a cohort who are on time. When fewer students are on time, it is assumed

9. Some researchers do not use differences in age-grade retardation across years to infer retention rates but take age-grade retardation itself as a proxy for retention. See Cascio 2005 for an analysis of the biases involved in such an approach. Also see Deming and Dynarski 2008 for evidence on changes over time in delayed entry (redshirting).

that retention accounts for the difference. We refine this measure in several ways.

We use the 1960, 1970, and 1980 Public Use Microdata to calculate retention rates. Thus we use three cohorts in the analysis, spaced at ten-year intervals. The censuses have three distinct advantages over other data sources. First, the samples are large enough to allow construction of precise state-year retention measures.[10] Second, the 1960–80 censuses also have the added advantage of including data on quarter of birth; the importance of this information will be clear shortly. Third, combining retention measures from the 1960–80 censuses and wage data from the 2000 census and the 2001–7 ACSs facilitates a panel analysis in which the effect of retention rates on long-run outcomes is estimated from within-state variation over time. But in order to infer retention rates, we must modify equation 2 to account for the fact that we do not observe a given birth cohort in consecutive grades and thus must compare on-time rates between consecutive cohorts in consecutive grades to estimate retention rates:

$$R^{a}_{bc} = O^{a}_{bc} - O^{a+1}_{bc-1}. \tag{3}$$

We label a student's cohort by the year he turned six, the modal age for first grade. In the 1980 census, age is reported as of 1 April 1980, so the majority of six-year-olds turned six in 1979. For example, the on-time measure $O^{6}_{MO\ 1979}$ is the fraction of six-year-olds in 1979 in Missouri who were in (or had completed) grade 1. Because we do not observe this cohort in grade 2, we calculate the grade 2 on-time rate for students who were seven years old in 1979 (and thus six years old in 1978.) The grade 1 retention rate is the difference between the 1979 cohort's grade 1 on-time rate and the 1978 cohort's grade 2 on-time rate. This approximation assumes smoothness of retention policy between these pairs of consecutive cohorts. For simplicity, we label this as the retention rate for 1979, although the retention rate applies to a two-year 1978–79 cohort.

An additional refinement of the on-time measure is warranted, given differing school age-entry laws in different states over time. The standard method for calculating retention rates from on-time status (e.g., Hauser 1999) is to define six-year-olds as on time if they are in first grade or higher, seven-year-olds if they are in second grade or higher, and so on. But this ignores age-entry laws and the census reporting date. For example, the age-entry cutoff for kindergarten in Missouri in 1979 was 1 October (i.e., state law required that students who turned five after 1 October defer entering kindergarten until the following year). By this rule, any student who was reported in the 1980 census to be

10. In contrast, the small state-year samples in the October supplement of the Current Population Survey yield extremely noisy state-year retention rates by grade.

seven years old in grade 1 would be classified as behind. But if a Missouri first grader's birthday fell between 1 October and 31 March and he had turned seven by the census reporting date (1 April 1980), he would have been six on 1 October 1979 and on time for grade 1. In this case, then, the standard rule incorrectly assigns about half of all students.[11] Using data on the student's quarter of birth and data on school age-entry laws (Bedard and Dhuey 2009), we are able to modify the on-time measure to account for state-level age-entry laws and the census reporting date.[12] Specifically, given the Missouri cutoff, we define seven-year-olds born in quarters 4 and 1 to be on time if they are in grade 1 or higher, those born in quarters 2 and 3 to be on time if they are in grade 2 or higher, and so on for other age cohorts. Of course, age-entry cutoffs do not always occur on the boundary between quarters (as was the case in this convenient example). When cutoffs are not in the boundary between quarters, we map students born in a specific quarter to the on-time status that most accurately characterizes the majority of students born in that quarter.

Changes in school entry cutoff dates also require that we exclude observations for which the age-entry law changes in the year we observe a cohort or during the previous or following year. This is important for at least two reasons. First, the difference in on-time rates between consecutive cohorts in consecutive grades is not a valid estimate of retention rates when the cutoffs change between the two cohorts, because the difference no longer nets out the misallocation of students in states with cutoffs that do line up exactly with reported quarter of birth information. Second, there may be some leading or lagging of cutoff changes in terms of early and late enrollment in the years immediately surrounding cutoff changes. This again invalidates differencing across consecutive years as a means of estimating retention.

A third refinement of the retention measure is warranted because some types of retention are not captured by equation 3. Students who moved from being on time to behind over consecutive grades would be accounted for in equation 3. However, some students move from being ahead to being (strictly) on time or from being behind to being further behind. To account for retention of these types, we let A denote the fraction of students in the cohort who are ahead and D the fraction of those who are two years behind, and modify equation 3 as follows:

$$R^{a}_{bc} = \left(O^{a}_{bc} - O^{a+1}_{bc-1}\right) + \left(A^{a}_{bc} - A^{a+1}_{bc-1}\right) + \left(D^{a+1}_{bc-1} - D^{a}_{bc}\right). \tag{4}$$

11. If the first-grade student's birthday fell between 1 April and 1 October, and he were seven at the time of the census, he would have been seven on 1 October of his grade 1 school year and behind. These students would be correctly assigned by the standard rule.

12. Appendix table A.1 reports school age-entry laws used to construct the retention rates.

Table 1. Retention Rates

	Cohort		
	1959	**1969**	**1979**
Grade 1 retention	0.083	0.072	0.055
	(0.041)	(0.036)	(0.023)
Grade 2 retention	0.073	0.050	0.045
	(0.050)	(0.022)	(0.017)
Grade 3 retention	0.054	0.039	0.054
	(0.039)	(0.026)	(0.016)

Note: Population weighted.

The retention measure we use, then, consists of the decrease in the fraction of students on time, the decrease in the fraction of students ahead, and the increase in the fraction of students who are two years behind.[13] Measures that consider age-grade retardation alone do not account for retention of the latter types.

Table 1 reports summary statistics for retention rates in grades 1, 2, and 3 calculated from 1960, 1970, and 1980 census samples, as described above. Note that the expression "grade g retention" is a simplification for brevity. The measure actually captures some retention in neighboring grades. To be precise, grade g retention refers to retention occurring over the age range associated with grade g, based on age-entry laws. In other words, this measure captures the treatment received by a legally defined age-entry cohort. Consistent with previous research, we find that the retention rate is higher in first grade. Previous research also suggests that policy interventions generate strong responses in early grades. Jacob (2002) notes that retention rates in grades 1 and 2 increased after the introduction of the Chicago public schools outcome-based policy initiative, even though the policy targeted later grades. And Hauser, Frederick, and Andrew (2005) observe that with the introduction of outcome-based reforms, early grade retention rates have responded more than later grades.[14] Early grade retention may be a preventative action, intended to address any observed problems prior to the time when performance becomes a binding determinant of promotion. Because early grade retention is most common and appears responsive to policy, we focus on retention over

13. Multigrade retention rates are calculated similarly: $R^{a}_{bc} = (O^{a}_{bc} - O^{a+s}_{bc-s}) + (A^{a}_{bc} - A^{g+s}_{bc-s}) + (D^{a+s}_{bc-s} - D^{a}_{bc})$, where s is the span of years over which retention is estimated.
14. Previous research suggests that retention is also common in kindergarten. However, estimates of retention in kindergarten are not easily inferred from on-time rates. This is because a subset of students does not attend kindergarten and thus appears on time in first grade without having been on time in kindergarten. This makes it impossible to estimate kindergarten retention using on-time rates.

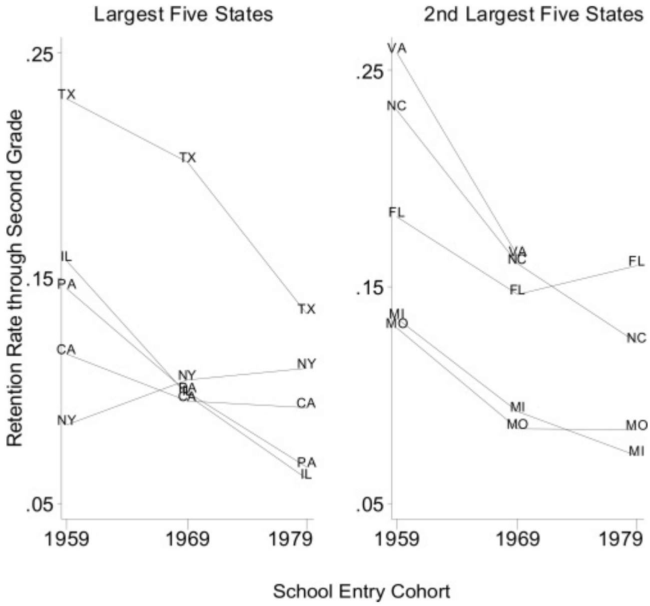

Figure 1. Retention Rates through Second Grade across School Entry Cohorts

grades 1 and 2 in the main regressions. Figure 1 displays retention rate time trends for the ten states with the largest student enrollments in 1959. A complete tabulation of estimated retention rates by state and year can be found in appendix table A.2.

Calculating on-time status from the census is an indirect way of estimating retention rates. Before moving to the main regressions, it is worth pausing to check our measure against a direct and independent measure to make sure that it is informative. Retention rates are reported directly by some states in some years (see Shepard and Smith 1989; Hauser, Frederick, and Andrew 2005). These data are sparse and do not go back far enough to allow for a full-scale panel analysis of long-run outcomes. However, the state-reported data on retention rates do have some overlap with the census sample we use. Specifically, there are grade 1 and grade 2 retention data from both sources for nine states in 1979. Figure 2 is a scatter plot of the two retention measures plotted against each other. The correlation between the census retention measure and the state-reported measure is 0.93, and sample means are 0.12 and 0.14, respectively. To the extent that we are able to compare, retention rates calculated from census on-time rates match very well with state-reported retention rates.

Long-Run Outcomes and Controls

For the dependent variables in the regressions that follow, we use log hourly wage and educational attainment outcomes for adult males in the 2000 5

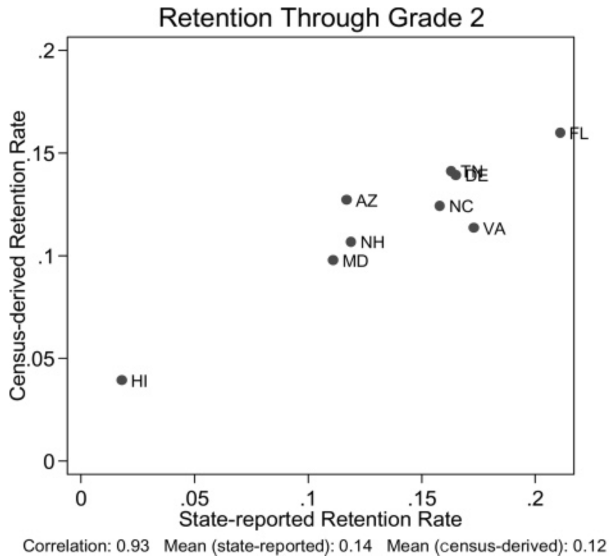

Figure 2. Retention Rates Derived from the Census versus Reported by States

Percent Public Use Microdata Sample and the 2001–7 ACS. The retention rate over grades 1 and 2 is calculated from the observed age and grade outcomes of birth cohorts covering three years. Thus adult respondents in three-year birth cohorts are mapped to the retention rate associated with the year (and birth state) in which they would have been in first or second grade.[15] Table 2 reports descriptive statistics for the sample of males between the ages of 25 and 54 whose labor market outcomes are observed in the census or ACS between 2000 and 2007. All models reported in section 4 include controls for the fraction of students who are on time in grade 1, ahead as of grade 1, the youngest age at which school entry is legally allowed, kindergarten subsidization, expenditure per student, pupil-teacher ratio, average daily attendance, relative salary of teachers, compulsory school-leaving age, a quadratic age function, and state of birth, race, and region of birth specific cohort indicators.[16]

15. Results are robust to other mappings. In all specifications, findings are similar whether two-, three-, or four-year cohorts are used. This speaks to concerns about our inability to perfectly map census and ACS wage data, as well as cohort-specific control variables, to retention cohorts. Results of these auxiliary regressions are available from the authors upon request.
16. Data on expenditures per student, pupil-teacher ratios, and average daily attendance are from the *Statistical Abstract of the United States*. Relative teacher salaries are defined as the average wage of teachers divided by the average wage of male 30–49-year-old bachelor of arts degree holders in the 1950–2000 U.S. censuses (intercensus years are linearly interpolated). The aforementioned variables are thirteen-year averages while the cohort is in school. The beginning of state-subsidized kindergarten is an indicator variable for whether publicly subsidized kindergarten existed (this information is from Dhuey 2009). School entry age laws (which enter linearly into all specifications) are measured at age five and are from Bedard and Dhuey 2009; see appendix table A.1 for more detail. School-leaving laws (specified linearly) are measured at age fourteen and are from the *Digest*

Table 2. Summary Statistics

	Employed Men		All Men	
	Mean	Standard Deviation	Mean	Standard Deviation
Ln hourly wage	3.054	0.708	–	–
Retention through grade 2	0.127	0.059	0.128	0.060
Percent on time or ahead as of grade 1	0.887	0.060	0.888	0.060
Percent ahead as of kindergarten	0.093	0.042	0.093	0.042
Expenditure per student (1,000s)	3.684	1.302	3.672	1.310
Average daily attendance (1,000s)	1,689	1,219	1,700	1,230
Pupil-teacher ratio	21.288	3.043	21.343	3.067
Relative teacher wage	0.619	0.075	0.619	0.075
Minimum school exit age (in years)	16.314	0.667	16.313	0.676
Publicly provided kindergarten	0.750	0.433	0.744	0.436
Youngest legal school entry age (in months)	57.542	1.517	57.542	1.512
Age	40.560	8.112	40.587	8.206
Black	0.091	0.288	0.105	0.307
Hispanic	0.064	0.246	0.067	0.251
Other race	0.028	0.165	0.030	0.172
Sample size	590,517		788,968	

Notes: Population weighted. "Employed men" sample is restricted to employed men who earn positive wages, are not in school, and are not in prison.

4. RESULTS

Wages

Table 3 reports the results from regressions of log hourly wages on retention through grade 2 (i.e., total fraction retained in grade 1 or 2). The first row reports the results for all men aged 25–54. Using the baseline specification from equation 1, column 1 indicates that a 0.1 change in the fraction of students retained through grade 2 in the respondent's state-year birth cohort is associated with a 2.3 percent increase in wages. This is a large effect, but it is associated with an extreme change in retention rates. The within-state standard deviation of retention rates is 0.03. This change, which we argue is a reasonably sized shock, is associated with an increase in wages of about 0.7 percent.

The model in column 1 allows that intermediate choices (e.g., education, state of residence, marital status) may be endogenous—potentially influenced

of Education Statistics (http://nces.ed.gov/Programs/digest/). Information not provided by the *Digest of Education Statistics* regarding the oldest age required by compulsory schooling comes from state statutes and corresponding historical session laws.

Table 3. The Impact of Retention Policy on ln Hourly Wages: Sampling and Specification Sensitivity

	Specification			Sample Size
	(1)	(2)	(3)	(4)
All men	0.2321** (0.0838)	0.2361** (0.0762)	0.2342** (0.0760)	590,517
White men	0.2743** (0.1000)	0.2881** (0.0929)	0.2894** (0.0919)	507,254
Excluding southern states (all men)	0.2974** (0.1084)	0.2840** (0.1041)	0.2833** (0.1028)	410,697
1957–59 and 1967–69 cohorts only	0.2674** (0.0885)	0.2321** (0.0878)	0.2188** (0.0881)	450,853
1967–69 and 1977–79 cohorts only	0.1843* (0.0949)	0.2246** (0.1024)	0.2311** (0.1046)	363,820
Including additional controls (all men)	0.2396** (0.0512)	0.2461** (0.0505)	0.2452** (0.0504)	590,517
Additional Controls:				
State of residence	No	Yes	Yes	
Region of residence ∗ Age	No	Yes	Yes	
State of residence GDP & unempl. rate	No	Yes	Yes	
Marital status	No	Yes	Yes	
Region of birth ∗ Region of residence	No	No	Yes	

Notes: All models are population weighted and the standard errors are heteroskedasticity-consistent and clustered at the state of birth level. All models also include controls for the fraction of students who are on time in grade 1, ahead as of grade 1, the youngest age at which school entry is legally allowed, kindergarten subsidization, pupil teacher ratio, relative salary of teachers, compulsory school leaving age, a quadratic age function, and state of birth, race, and region of birth specific cohort indicators. The models reported in the last row also control for the percent of children aged 5–9 who are black, Hispanic, and other races, family size, the number of children siblings (including the individual), the poverty rate, cohort size (measured by the size of the first grade cohort), and the fraction of men aged 25–29 who immigrated to the state from Central America (including Mexico), immigrated from somewhere other than Central America, and migrated from another state.
*significant at 10%; **significant at 5%.

by the state-level retention rate. This is the preferred specification, as it does not impose arbitrary limits on the mechanism by which retention rates may affect wages. However, it is worth investigating the robustness of the findings to other specifications. Specifically, there may be a concern that the results are driven by omitted variables correlated with state of residence economic conditions and state of birth retention rates. Column 2 reports estimates from a specification that includes controls for state of residence, state of residence GDP and unemployment rates, region of residence interacted with a quadratic function of age, and marital status. The coefficient on the retention rate changes only very slightly in this specification. Column 3 further adds region of birth by region of residence interaction terms to control for nonrandom migration of workers between regions.[17] Regressions from all models yield statistically and economically significant effects of the retention rate on hourly wages. All point estimates are similar, showing about a 0.7 percent wage increase in response to a standard deviation change in the retention rate.

A major concern, given the methodology used, is that changes in retention policy may be confounded with changes in other state policies that occurred simultaneously. One could imagine that state-level changes in retention policy are sometimes implemented in a package that includes other reforms. In all regressions, we have controlled for available and relevant education policy variables, including education expenditure per student, kindergarten subsidization, pupil-teacher ratio, relative salary of teachers, compulsory school-leaving age, and youngest legal school entry age. Changes in these education policies do not account for the observed association between retention rates and wages.

School age-entry laws merit close attention. They have been shown to influence student outcomes (Bedard and Dhuey 2009) and may also influence retention policy. We have controlled for this in several ways. First, as described in section 3, we use age-entry cutoffs in the construction of the retention measure and exclude observations for which the age-entry law changes in the year we observe a cohort or during the preceding or following year. This reduces the likelihood that age at entry is confounded with the retention measure and ensures that the measure is attached to a meaningful cohort of students, those grouped together by state law. Second, we control directly for youngest legal school entry age, as described above. Third, to capture any additional effects of within-state changes in delayed or accelerated entry into schooling, we also include control variables for the fraction of students on time and the fraction of students ahead when they enter grade 1. These controls are included in all specifications.[18]

Wage effects of retention are robust to the inclusion of controls for education policies, which would suggest that retention is not a proxy for other reforms. To go a step further, one could also investigate whether retention rates and other education policies do, in fact, tend to move in tandem. For example, do expenditures per pupil rise when the retention rate rises? State fixed effects regressions (with region-specific cohort indicators) of education policies on the retention rate reveal no significant correlation between the retention rate and expenditures per pupil, kindergarten subsidization, relative salary of teachers, compulsory school-leaving age, youngest legal school entry age, and the fractions of students ahead and on time when they enter grade 1.

17. See Heckman, Layne-Farrar, and Todd 1995.
18. The results are robust to the exclusion of these controls. For example, the retention coefficient reported in column 1 in table 3 changes from 0.2321 to 0.2327 when all education controls are excluded.

Only one policy appears to covary with the retention rate: increases in the retention rate are associated with a small, statistically significant *increase* in pupils per teacher (suggestive of larger class sizes and less teacher time per student). There would appear to be little or no evidence that increased retention rates proxy for increased investment in education or other human capital enhancing education reforms.[19]

One might further be concerned that other, noneducation system, policy and/or socioeconomic changes may be confounding factors. For example, institutional changes within states, concurrent with retention rate changes, may have altered labor market outcomes for nonwhite workers. Table 3, row 2, restricts the sample to white males. Findings are robust to this restriction, suggesting that main results are not driven by external policies that altered labor market outcomes for nonwhite workers.

Retention in the southern states was much higher than in the rest of the country in the 1940s and 1950s. *Brown v. Board of Education*, and the ensuing transformation of educational institutions in the South, may have ushered in pervasive changes in retention policy. To discern whether apparent effects of retention policy are driven by these broader policy changes, we drop southern states from the regressions in table 3, row 3. The results are robust to the exclusion of southern states.

Different external policy changes may have occurred in different subperiods covered by the data. In table 3, rows 4 and 5, point estimates appear similar for both of the ten-year subperiods across which we can observe changes in retention rates between cohorts, though smaller sample sizes lessen the precision of the estimates.

A remaining concern is that within-state changes in the demographic composition and characteristics of the school-age population may be confounded with retention. Row 6 in table 3 checks for this by including a broad range of additional controls. These include the percent of children aged 5–9 who are black, Hispanic, and other races; family size; number of children (including the individual); poverty rate; cohort size (measured by the size of the first-grade cohort); and fraction of men aged 25–29 who immigrated to the state from Central America (including Mexico), immigrated from somewhere other than Central America, and migrated from another state. The estimated effect of retention is largely unaffected by the inclusion of these variables.[20]

19. Results of these supporting regressions are available from the authors upon request.
20. Further, state fixed effects regressions of these variables on the retention rate reveal no significant correlation between changes in retention rates and demographic composition or characteristics for the school-age population. Supporting regressions are available upon request.

As is always the case, unobservables remain a possible confounding influence. For example, changes over time in average unobserved ability of cohorts, within a state, may influence results. However, more able cohorts would likely feature lower retention rates and higher wages, other things equal. Thus it is likely that effects of this type would work *against* finding a positive effect of retention on wages. Standard stories about unobserved changes in cohort composition would suggest, if anything, that the coefficients on retention in table 3 may be biased downward.

In summary, regressions from all models yield statistically and economically significant effects of the retention rate on hourly wages. All point estimates are similar, showing about a 0.7 percent wage increase in response to a standard deviation change in the retention rate.

Timing: Retention Impacts by Grade

The analysis so far has focused on overall retention through grade 2, but it may also be informative to examine retention by specific grades. Table 4, panel B, repeats the exercise of table 3, using grade 1 and grade 2 retention rates as separate regressors. The effects of grade 1 and 2 retention appear very similar. Panels C and D repeat the exercises of panels A and B but examine retention through grade 3. The wage effect of early grade retention appears to be driven by changes in grade 1 and 2 retention; panel D shows little or no effect of third-grade retention.[21] Panels E, F, and G, which contain results from separate regressions of log wages on grade 1, grade 2, and grade 3 retention, respectively, show the same pattern: positive wage effects for first-grade retention, imprecisely estimated positive effects for second-grade retention, and little or no effect of third-grade retention. Taken as a whole, the estimates reported in table 4 suggest that wage effects are strongest for retention in the earliest grades.

Educational Attainment

Educational attainment is one mechanism that could generate the observed association between grade retention rates and hourly wages later in the life cycle. Are changes in the retention rate associated with changes in educational attainment? Table 5 indicates that the answer is no. Under all specifications, the association between increased retention rates and educational attainment is

21. Kindergarten retention is not included as a covariate in table 4 because construction of kindergarten retention rates from the census is problematic (see note 14). If increased grade 1 retention were associated with *decreased* kindergarten retention, it would be difficult to infer whether stricter retention policy or weaker retention policy drove wage results above. However, state-reported retention data indicate that this is not the case and thus mitigate the concern: state-level changes over time in the kindergarten retention rate are positively associated with changes in the first-grade retention rate.

Table 4. The Impact of Retention Policy on ln Hourly Wages

	Specification		
	(1)	(2)	(3)
Panel A: Base Case			
Rentention through grade 2	0.2321**	0.2362**	0.2343**
	(0.0838)	(0.0762)	(0.0760)
Panel B			
Grade 1 retention	0.2895**	0.2913**	0.2930**
	(0.0769)	(0.0728)	(0.0734)
Grade 2 retention	0.1988*	0.2043**	0.2002**
	(0.1064)	(0.0987)	(0.0984)
Panel C			
Rentention through grade 3	0.1288**	0.1446**	0.1422**
	(0.0574)	(0.0549)	(0.0556)
Panel D			
Grade 1 retention	0.2541**	0.2483**	0.2531**
	(0.0797)	(0.0785)	(0.0788)
Grade 2 retention	0.1669	0.1840	0.1780
	(0.1229)	(0.1122)	(0.1136)
Grade 3 retention	0.0263	0.0569	0.0504
	(0.0868)	(0.0909)	(0.0912)
Panel E			
Grade 1 retention	0.2530**	0.2534**	0.2559**
	(0.0662)	(0.0687)	(0.0694)
Panel F			
Grade 2 retention	0.1653	0.1705*	0.1662*
	(0.1024)	(0.0948)	(0.0945)
Panel G			
Grade 3 retention	0.0080	0.0311	0.0279
	(0.0586)	(0.0624)	(0.0620)
Additional Controls:			
State of residence	No	Yes	Yes
Region of residence * Age	No	Yes	Yes
State of residence GDP & unempl. rate	No	Yes	Yes
Marital status	No	Yes	Yes
Region of birth * Region of residence	No	No	Yes

Notes: All models are population weighted, and the standard errors are heteroskedasticity-consistent and clustered at the state of birth level. All models also include controls for the fraction of students who are on time in grade 1, ahead as of grade 1, the youngest age at which school entry is legally allowed, kindergarten subsidization, pupil-teacher ratio, relative salary of teachers, compulsory school-leaving age, a quadratic age function, and state of birth, race, and region of birth–specific cohort indicators. The sample size is 590,517, except for models that include third-grade retention. The sample size for these models is 586,415 due to missing retention data for Nevada and Iowa in the late 1970s.
*significant at 10%; **significant at 5%.

statistically insignificant and economically small. In the baseline specification of column 1, a reasonably sized policy shock of 0.03 to the fraction of students retained is associated with a decrease of 0.1 percentage point in the high school graduation rate. The observed association between retention and some college or college completion is likewise small and statistically insignificant, but marginally positive. There would appear to be little or no clear evidence

Table 5. The Impact of Retention Policy on Educational Attainment

	Specification		
	(1)	(2)	(3)
Grade 10 or more	−0.0126 (0.0293)	−0.0108 (0.0306)	−0.0109 (0.0306)
Grade 11 or more	−0.0123 (0.0291)	−0.0086 (0.0319)	−0.0088 (0.0320)
High school graduation or more	−0.0345 (0.0342)	−0.0283 (0.0368)	−0.0292 (0.0368)
Some college or more	0.0454 (0.0749)	0.0594 (0.0761)	0.0533 (0.0776)
BA or more	0.0477 (0.0504)	0.0503 (0.0512)	0.0453 (0.0516)
Sample size	788,968	788,968	788,968
Additional Controls:			
State of residence	No	Yes	Yes
Region of residence ∗ Age	No	Yes	Yes
State of residence GDP & unempl. rate	No	Yes	Yes
Marital status	No	Yes	Yes
Region of birth ∗ Region of residence	No	No	Yes

Notes: All models are population weighted, and the standard errors are heteroskedasticity-consistent and clustered at the state of birth level. All models also include controls for the fraction of students who are on time in grade 1, ahead as of grade 1, the youngest age at which school entry is legally allowed, kindergarten subsidization, pupil-teacher ratio, relative salary of teachers, compulsory school-leaving age, a quadratic age function, and state of birth, race, and region of birth–specific cohort indicators.

that changes in the retention rate generate economically significant changes in the years of schooling a worker completes. Clearly, however, human capital acquisition depends on both the quantity and the quality of schooling received, and the dependent variables in table 5 capture only the former.

Potential Mechanisms and Distributional Effects

The analysis thus far has focused on the effect of early grade retention on all students. This leaves open the question of whether observed wage gains accrue primarily to retained or promoted students.[22] Because we do not observe retention histories for respondents in the 2000 census and the 2001–7 ACSs, we cannot sort students by early grade retention status and identify separately the wage effects on each subgroup. Even if this were possible, we would face

22. More precisely, when we discuss promoted students in the following analysis, we explore possible effects of retention policy on students who were not *marginally* retained. Most of these students were in fact promoted, so we use the term for simplicity of exposition.

Table 6. The Impact of Retention Policy on ln Hourly Wages across Quantiles

	Specification		
	(1)	(2)	(3)
10th quantile	0.2294*	0.2533**	0.2583**
	(0.1184)	(0.0968)	(0.1036)
25th quantile	0.1908**	0.1463**	0.1335**
	(0.0621)	(0.0747)	(0.0660)
50th quantile	0.1370**	0.1872**	0.1805**
	(0.0575)	(0.0600)	(0.0531)
75th quantile	0.1822**	0.2642**	0.2196**
	(0.0685)	(0.0713)	(0.0656)
90th quantile	0.2544**	0.1798*	0.1656
	(0.1105)	(0.1035)	(0.1128)
Additional Controls:			
State of residence	No	Yes	Yes
Region of residence * Age	No	Yes	Yes
State of residence GDP & unempl. rate	No	Yes	Yes
Marital status	No	Yes	Yes
Region of birth * Region of residence	No	No	Yes

Notes: All models are population weighted, and the standard errors are heteroskedasticity-consistent and clustered at the state of birth level. All models also include controls for the fraction of students who are on time in grade 1, ahead as of grade 1, the youngest age at which school entry is legally allowed, kindergarten subsidization, pupil-teacher ratio, relative salary of teachers, compulsory school-leaving age, a quadratic age function, and state of birth, race, and region of birth–specific cohort indicators. The sample size is 590,517.
*significant at 10%; **significant at 5%.

serious endogeneity issues. Similarly, because we do not observe socioeconomic status in childhood for adult respondents, we cannot parse out effects by socioeconomic status (SES) in early grades. However, we can examine some indirect evidence on mechanisms and distributional effects.

For example, one might expect that the long-run effects of retention policy would be concentrated in the lower part of the wage distribution, since it seems reasonable to think of retention as disproportionately affecting low-skill children. We employ quantile regressions to investigate this possibility and summarize the results in table 6. Though coefficients vary by specification and are not always precisely estimated, the retention coefficient is positive throughout the distribution. Moreover, in no specification is it possible to reject a uniform distribution of effects across the quantiles. The average wage effect reported in table 3, then, does not appear to be driven entirely by effects of early grade retention on low-wage earners. Interestingly, what it means to be in the top quartile of wage earners also appears to rise with the strictness of the retention policy to which a cohort was exposed.

Table 7. Retention Patterns across SES Groups

	Retention Rate for:				
	Overall	Lowest SES Quartile	Middle Two SES Quartiles	Highest SES Quartile	Sample Size
	(1)	(2)	(3)	(4)	(5)
Panel A					
Average	0.1295	0.2242	0.1107	0.0584	111
	(0.0596)	(0.0920)	(0.0421)	(0.0448)	
Panel B: First differences					
Overall change < -0.03	-0.0610	-0.0840	-0.0490	-0.0317	34
	(0.0318)	(0.0620)	(0.0384)	(0.0603)	
Overall change $> +0.03$	0.0567	0.0764	0.0401	0.1111	8
	(0.0249)	(0.1036)	(0.0479)	(0.0782)	

Notes: State-level census data from 1960–1980. Weighted by cohort size. Standard deviations are in parentheses.

The observed effects in the upper quantiles are suggestive of an effect on the promoted because it is hard to imagine that a great many students who failed grades 1 or 2 would end up high in the wage distribution. For such spillovers to exist, however, it must be the case that students in the upper quantiles are exposed to students who are retained in early grades. One might question whether this is true. Specifically, if high-wage workers attended schools for the advantaged that featured little or no retention—or, more to the point, little or no change in the retention rate over time—the upper quantile estimates reported in table 6 would be implausible. Below we investigate whether workers with high wages were likely to have been exposed to retained peers in early grades and whether there were changes over time in the intensity of that exposure.

Table 7 reports retention rates across childhood SES categories. Panel A reports state-level grade 1 retention rates by family income quartile for the 1960, 1970, and 1980 census samples used in the previous tables. Though there is more retention among children from the lowest family income quartile (0.22), retention is common among the middle 50 percent of children (0.11) and among the top quartile of the family income distribution (0.06).[23] Since the analysis in this article is based on within-state changes rather than levels, panel B reports changes in retention rates. Specifically, it indicates that when overall retention rates change between censuses by at least 1 standard deviation, the retention rates for children from families with low, middle, and high incomes all move in the same direction. In short, we observe similar changes in retention policy or practice among all SES subgroups and find no evidence

23. For our purposes, the family income distribution is the distribution of family income observed among our school entry cohorts, as defined in section 3.

Table 8. Distribution of Free Lunch Eligible Students across Schools

	Fraction of Schools Reporting Specified Percentage of Students Eligible for Free Lunch							
	0	**0–10**	**10–20**	**20–40**	**40–60**	**60–80**	**80–100**	**Percentage of Schools Reporting Free Lunch Status***
1988	0.034	0.181	0.213	0.273	0.122	0.072	0.106	31%
1992	0.029	0.147	0.166	0.277	0.178	0.114	0.089	70%
1996	0.019	0.126	0.148	0.276	0.191	0.130	0.111	81%
2000	0.021	0.167	0.156	0.264	0.182	0.126	0.083	86%
2004	0.041	0.129	0.133	0.261	0.199	0.143	0.094	89%

*Percentage of schools in NCES with complete data and at least five first graders reporting free lunch percentage.

that increased retention for one SES subgroup is associated with decreased retention for another.

Middle and high SES students are exposed to retention and to changes over time in retention rates, within their own SES quartiles. In addition, and perhaps just as important, middle and high SES students are exposed to low SES peers, and thus to changes in retention among this group. Table 8, based on the Common Core of Data from the National Center for Education Statistics (NCES), shows that in a majority of schools more than 20 percent of students are eligible for free lunches.[24] While the time period available from the NCES postdates our last wave of census data by several years, the fraction of students eligible for free lunch appears stable during the sixteen-year span for which we have data. The main conclusion we draw from table 8 is that the vast majority of children attend schools that include at least some children from low-income families.

So far we have shown that children from all SES groups are subject to retention and that almost all children attend schools that include low SES students, who are the most likely to be retained. One could still worry that perhaps retention was uncommon at middle and high SES *schools*, and thus students at these schools, though exposed to low SES students, were not exposed to failers. The final piece to the puzzle is to show that children from various SES groups are retained whether they attend low, medium, or high SES schools. Table 9 shows the distribution of retention across SES groups and school types using data from the 1998 kindergarten entry cohort from the Early Childhood Longitudinal Survey (ECLS). We use ECLS data here

24. The sample includes all schools with complete data that have at least five first-grade students and report the fraction of students eligible for free lunch.

Table 9. Distribution of Retention across SES Groups and School Types

	Lowest SES Quartile	Middle Two SES Quartiles	Highest SES Quartile	Overall
	(1)	(2)	(3)	(4)
Panel A				
All schools	0.1808	0.0945	0.0695	0.1094
	(0.3849)	(0.2926)	(0.2543)	(0.3122)
	[2398]	[5650]	[3248]	[11296]
Panel B				
Lowest school free lunch quartile	0.080	0.094	0.072	0.081
(High SES)	(0.272)	(0.292)	(0.259)	(0.274)
	[95]	[940]	[1221]	[2256]
Middle two school free lunch quartiles	0.162	0.090	0.044	0.097
(Middle SES)	(0.368)	(0.286)	(0.205)	(0.296)
	[659]	[2088]	[791]	[3538]
Highest school free lunch quartile	0.198	0.135	0.066	0.159
(Low SES)	(0.399)	(0.342)	(0.250)	(0.366)
	[724]	[751]	[127]	[1602]

Notes: Standard deviations are in parentheses, and sample sizes are in square brackets. ECLS data restricted to children who entered kindergarten for the first time in 1998. All estimates weighted using panel weights. A student is defined as retained if he or she repeated kindergarten, grade 1, or grade 2.

because it is the only source that reports both student-level and school-level SES (defined by the fraction of students eligible for free lunch). Although this is a much more recent school entry cohort, retention is directly measured for kindergarten through second grade, and ECLS-computed SES is used in place of family income, the distribution of retention across SES quartiles is similar to the corresponding rates shown for the census data used in the main analysis (compare table 9, panel A, with table 7, panel A). As one might expect, retention rates reported in table 9 are highest for low SES schools: 0.20, 0.14, and 0.07 for low, middle, and high SES students, respectively. More important for our purposes, while retention rates are lower at middle and high SES schools, there is still substantial retention—the highest SES quartile schools feature retention rates of 0.08, 0.09, and 0.07 for low, middle, and high SES students, respectively, and middle SES schools feature retention rates of 0.16, 0.09, and 0.04, respectively, for low, middle, and high SES students.

Taken as a whole, tables 7–9 indicate that advantaged students are exposed to retention both in their own SES strata and through the presence of low SES students in their school. This is particularly true for middle SES students who are likely to attend schools that draw a fairly large fraction of low-income students. It is not implausible, then, that long-run wages of advantaged or promoted students would respond to retention policy. As discussed in section 2, the effects of retention policy on advantaged or promoted students could be the result of associated changes in the targeting of institutional resources, the optimal effort choices of students, or the dynamics and composition of peer groups.[25]

5. SUMMARY AND CONCLUSION

Most previous research on retention explores the impact of retention on the retained but abstracts from any possible impact on promoted students. By estimating differences in long-run education and labor market outcomes for cohorts of students exposed to differing state-level primary school retention rates, this article estimates the effects of retention policy on all students in a cohort, retained and promoted. The main finding is that a 1 standard deviation increase in retention through grade 2 is associated with a 0.7 percent increase in average male hourly wages. Further, the observed positive wage effect is not limited to the lower tail of the wage distribution but appears to persist throughout the distribution.

Given the estimates above, we can approximate the costs and benefits associated with changes in retention rates. While the benefit-cost analysis here is very simplified, it demonstrates the importance of considering effects on all students. Similar to Cascio (2005) and Eide and Showalter (2001), we approximate large retention costs for the retained; however, our estimates also suggest that these are more than offset by the gains to those not retained. Assuming a 0.03 increase in retention and a rate of return to experience estimated from the 2000 census for men aged 25–64,[26] the cost for the average retained man is approximately $34,000.[27] However, marginally retained men are greatly outnumbered by promoted men, for whom the average return is approximately $5,700. With a 0.03 increase in retention, the average discounted lifetime gain per man is $4,500. Clearly gains on average do not imply positive effects for every worker. Given the assumptions in this exercise, some workers

25. Lazear's (2001) model, which emphasizes externalities associated with disruptive students, offers another interesting possibility. If increased retention allows for better handling of disruptive students, one could imagine that schools with only a few disruptive students might actually benefit more from increased retention (and an associated marginal reduction in the number of disruptive students) than schools with many disruptive students. In the latter case, there may be more instances in which multiple students disrupt simultaneously.
26. All calculations are for the average man who has fourteen years of education. The return to experience is estimated using $\ln y_i = \phi_0 + \phi_1 \exp_i + \phi_2 \exp_i^2 + u_i$.
27. This includes a $5,000 direct cost for an extra year of schooling (recall that our estimates in table 4 show no evidence of altered educational attainment), the loss of a year of income while still in school, and the loss of a year of experience in every working year. These losses are counterbalanced to some degree by the wage return associated with the retention policy. All values are discounted to age twenty.

bear very large costs, but small gains to the large majority swamp the losses to the marginally retained.

These results, we conclude, argue for a data-driven policy discussion that moves beyond a narrow focus on how retention influences the retained and explicitly takes into account effects of retention policy on both the promoted and the retained. To the extent that these effects may work in opposite directions, institutions will need to balance the gains and losses to the various groups. While we hesitate to offer specific policy recommendations on the optimal level of retention, we submit that the findings here, and continued research in this vein, will assist policy makers in striking the balance.

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Table A.1. School Entry Cutoff Dates (School Years 1955–80)

	1955	1956	1957	1958	1959	1960	1961	1962	1963	1964	1965	1966	1967	1968	1969	1970	1971	1972	1973	1974	1975	1976	1977	1978	1979	1980
AL	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1
AK	11.2	11.2	11.2	11.2	11.2	11.2	11.2	11.2	11.2	11.2	11.2	11.2	11.2	11.2	11.2	11.2	11.2	11.2	11.2	11.2	11.2	11.2	11.2	11.2	11.2	11.2
AZ	none	none	none	none	none	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	12.1	11.1
AR	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1
CA	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1
CO	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA
CT	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1
DE	9.1	9.1	9.1	9.1	9.1	9.1	9.1	9.1	9.1	9.1	9.1	9.1	9.1	9.1	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31
FL	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	12.1
GA	none	none	none	none	none	none	none	none	none	none	none	none	none	none	none	none	none	none	none	none	none	none	none	none	none	none
HI	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31
ID	none	none	none	none	none	none	none	none	10.16	10.16	10.16	10.16	10.16	10.16	10.16	10.16	10.16	10.16	10.16	10.16	10.16	10.16	10.16	10.16	10.16	10.16
IL	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1
IN	none	none	none	none	none	none	none	none	none	none	none	none	none	none	none	none	none	none	none	none	none	none	none	none	none	none
IA	11.15	11.15	11.15	11.15	11.15	11.15	11.15	10.15	10.15	10.15	10.15	10.15	10.15	10.15	10.15	10.15	10.15	10.15	10.15	10.15	9.15	9.15	9.15	9.15	9.15	9.15
KS	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	9.1	9.1	9.1	9.1	9.1	9.1	9.1	9.1	9.1	9.1	9.1	9.1	9.1	9.1	9.1	9.1
KY	12.30	12.30	12.30	12.30	12.30	12.30	12.30	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	10.1	10.1
LA	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31

Table A.1. Continued.

	1955	1956	1957	1958	1959	1960	1961	1962	1963	1964	1965	1966	1967	1968	1969	1970	1971	1972	1973	1974	1975	1976	1977	1978	1979	1980
ME	10.15	10.15	10.15	10.15	10.15	10.15	10.15	10.15	10.15	10.15	10.15	10.15	10.15	10.15	10.15	10.15	10.15	10.15	10.15	10.15	10.15	10.15	10.15	10.15	10.15	10.15
MD	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31
MA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA
MI	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1
MN	9.1	9.1	9.1	9.1	9.1	9.1	9.1	9.1	9.1	9.1	9.1	9.1	9.1	9.1	9.1	9.1	9.1	9.1	9.1	9.1	9.1	9.1	9.1	9.1	9.1	9.1
MS	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	12.1	11.1	10.1	9.1
MO	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1
MT	none	none	none	none	none	none	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	9.10	9.10
NE	10.15	10.15	10.15	10.15	10.15	10.15	10.15	10.15	10.15	10.15	10.15	10.15	10.15	10.15	10.15	10.15	10.15	10.15	10.15	10.15	10.15	10.15	10.15	10.15	10.15	10.15
NV	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	11.30	10.31	10.31	9.30	9.30	9.30	9.30	9.30	9.30
NH	9.30	9.30	9.30	9.30	9.30	9.30	9.30	9.30	9.30	9.30	9.30	9.30	9.30	9.30	9.30	9.30	9.30	9.30	9.30	9.30	9.30	9.30	9.30	9.30	9.30	9.30
NJ	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA
NM	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	9.1	9.1	9.1	9.1	9.1	9.1	9.1	9.1
NY	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1
NC	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.16	10.16	10.16	10.16	10.16	10.16	10.16	10.16	10.16	10.16	10.16
ND	12.31	12.31	12.31	12.31	12.31	11.30	10.31	10.31	10.31	10.31	10.31	10.31	10.31	10.31	10.31	10.31	10.31	10.31	10.31	10.31	9.30	8.31	8.31	8.31	8.31	8.31
OH	none	none	none	none	none	none	none	none	none	none	10.31	10.31	10.31	10.31	9.30	9.30	9.30	9.30	9.30	9.30	9.30	9.30	9.30	9.30	9.30	9.30
OK	11.1	11.1	11.1	11.1	11.1	11.1	11.1	11.1	11.1	11.1	11.1	11.1	11.1	11.1	11.1	11.1	11.1	11.1	11.1	11.1	11.1	11.1	11.1	11.1	11.1	9.1
OR	11.15	11.15	11.15	11.15	11.15	11.15	11.15	11.15	11.15	11.15	11.15	11.15	11.15	11.15	11.15	11.15	11.15	11.15	11.15	11.15	11.15	11.15	11.15	11.15	11.15	11.15

PA	2.1	2.1	2.1	2.1	2.1	2.1	2.1	2.1	2.1	2.1	2.1	2.1	2.1	2.1	2.1	2.1	2.1	2.1	2.1	2.1	2.1	2.1	2.1	2.1	2.1	2.1
RI	none	none	none	none	none	none	none	none	none	none	none	none	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31
SC	none	none	none	none	none	none	none	none	none	none	none	none	none	none	none	none	none	none	none	none	none	none	none	11.1	11.1	11.1
SD	11.1	11.1	11.1	11.1	11.1	11.1	11.1	11.1	11.1	11.1	11.1	11.1	11.1	11.1	11.1	11.1	11.1	11.1	11.1	11.1	11.1	11.1	11.1	11.1	9.1	9.1
TN	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	11.30	10.31	9.30	9.30	9.30	9.30	9.30	9.30	9.30	9.30	9.30	9.30	9.30	9.30	9.30
TX	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy
UT	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy
VT	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1
VA	9.30	9.30	9.30	9.30	9.30	9.30	9.30	9.30	9.30	9.30	9.30	9.30	9.30	9.30	9.30	9.30	9.30	9.30	9.30	10.31	11.30	12.31	12.31	12.31	LEA	LEA
WA	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	LEA	LEA	LEA	LEA
WV	none	none	none	none	none	none	none	none	none	none	none	none	none	none	none	none	none	11.1	11.1	11.1	11.1	11.1	11.1	11.1	11.1	11.1
WI	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	9.1	9.1
WY	9.15	9.15	9.15	9.15	9.15	9.15	9.15	9.15	9.15	9.15	9.15	9.15	9.15	9.15	9.15	9.15	9.15	9.15	9.15	9.15	9.15	9.15	9.15	9.15	9.15	9.15

Notes: The number before the decimal is the month of cutoff; the number after the decimal is the day of the cutoff. LEA indicates that the local education authority sets the cutoff date, ssy indicates that the cutoff date is the start of school year, none indicates that the cutoff date is not listed in the state statutes. All cutoff dates were collected from state statutes and corresponding historical state session laws. See Bedard and Dhuey 2009 for additional years as well as more detail regarding exact statutes and session laws, including changes.
Source: Bedard and Dhuey 2009.

Table A.2. Retention through Second Grade

	1959	1969	1979
AK	0.18	0.11	0.08
AL	0.24	0.17	0.15
AR	0.19	0.15	0.14
CA	0.12	0.10	0.09
CT	0.10	0.15	0.13
DE	0.11	–	0.14
FL	0.18	0.15	0.16
HI	0.02	0.09	0.04
IA	0.10	0.08	0.08
ID	–	0.12	0.09
IL	0.16	0.10	0.06
KS	0.10	0.02	0.07
KY	0.21	0.19	–
LA	0.25	0.18	0.16
MD	0.16	0.18	0.10
ME	0.14	0.26	0.08
MI	0.14	0.09	0.07
MN	0.00	0.04	0.05
MO	0.13	0.08	0.08
MS	0.43	0.22	–
MT	–	0.12	0.11
NC	0.23	0.16	0.12
ND	0.11	0.18	–
NE	0.10	0.05	0.04
NH	0.30	0.16	0.11
NM	0.31	0.20	0.08
NV	0.24	0.09	0.10
NY	0.08	0.10	0.11
OK	0.21	0.16	0.11
OR	0.11	0.11	0.09
PA	0.15	0.10	0.07
SD	0.15	0.18	–
TN	0.25	–	0.13
TX	0.23	0.20	0.13
UT	0.09	0.05	0.03
VA	0.26	0.16	–
VT	0.08	0.14	0.13
WA	0.11	0.09	–
WI	0.07	0.08	–
WY	0.06	0.01	0.05