Wage Gains From Failure:
New Evidence on School Retention Policies 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 paper estimates the effects of retention policy on all students in a cohort, retained and promoted. We find that a one standard deviation increase in early grade retention is associated with a 0.6 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.

I. Introduction

Both the "Goals 2000: Educate America Act," signed into law by Bill Clinton in 1994, and "No Child Left Behind," 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 farther behind.[1] Proponents of retention claim "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.[2] 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 a potentially 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 paper 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.

[1] Thompson and Cunningham (2000)
[2] American Federation of Teachers (1997)

Our main finding is that a one standard deviation increase in retention through grade two is associated with an increase of 0.6 percent in male workers' average hourly wages. This finding is robust to controls for changing demographic composition of states, selective migration between states, school age entry laws, 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 paper is organized as follows. Section II reviews the previous literature and motivates the analysis; Section III describes the data and empirical strategy; Section IV reports results and discusses possible implications of the findings, and Section V summarizes and concludes.

II. The Impact of Retention

A. 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 1989, Holmes and Mathews 1984, and 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 observationally similar promoted students.[3] However,

[3] A few studies have found positive effects of retention in some settings (see Karweit 1991, Alexander, Entwisle et al. 1994, Eide and Showalter 2001, Dworkin, Lorence et al. 1999).

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 cut-offs. 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.[4] 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 drop-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.

B. 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.

[4] 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

Economic theory predicts that retention policies influence the effort, expectations, and practices of students and their teachers, and 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 Schatzenbach (2008) emphasize the allocation of resources by institutions: The modification of a standard causes institutions to direct resources toward the subgroup of students nearest the cut-off and away from students at the top and bottom of the distribution, yielding potential effects on all students. In Betts (1996) and Costrell (1994), 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.[5]

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

[5] 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.

other modes of social interaction (e.g. Akerlof 1997, Glaeser, Sacerdote, and Scheinkman 2003, and Moffit 2001).

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 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.[6] Below, we describe the data and empirical strategy that we employ to investigate this broader effect.

III. Empirical Strategy

A. 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 Census or American Community Survey (ACS) in year *y*, to the state of birth level retention rate, R_{bc}, as follows:

(1) $Y_{ibcy} = \alpha_0 + \alpha_1 R_{bc} + X_{ibcy}\alpha_2 + A_{ibcy}\alpha_3 + B_b\alpha_4 + T_c\alpha_5 + \varepsilon_{ibcy}$,

where *X* is a vector of personal characteristics, A_{ibcy} is a quadratic function of age,[7] B_b is a vector of state of birth indicators, T_{zc} is a vector of birth-region-specific birth cohort indicators, and ε is the usual error term.[8] Equation (1) does not hold educational attainment constant because retention practices may alter labor market outcomes by

[6] There is also some research investigating the overall impact of school accountability on student outcomes (e.g., Hanushek and Raynond, 2004, Carnoy and Loeb, 2004). However, these papers do not specifically investigate grade retention.

[7] All results are similar if a vector of age indicators is used instead. Under specification, one age indicator from each birth cohort is excluded.

[8] We assume that individuals are educated in their state of birth. All retention measures are defined in Section III.B. Non-random migration is discussed in Section IV.A.

changing highest grade attained or by changing within-grade (cognitive or non-cognitive) human capital accumulation.[9] 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.[10] 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.

B. 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. 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

[9] In Section IV, we also report results of a model that features educational attainment as the dependent variable.
[10] Section IV 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.

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 cut-offs used in promotion decisions were not specified. Moreover, evidence suggests that retention rates often change most 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 and Lefgren 2002 and Hauser et al 2006). 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 actual 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.[11] 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_{bc}^{a} 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:

(2) $R_{bc}^{a} = O_{bc}^{a} - O_{bc}^{a+1}$

[11] 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 Demming and Dynarski (2008) for evidence on changes over time in delayed entry (redshirting).

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

We use the 1960, 1970, and 1980 Public Use Micro Census Data to calculate retention rates. 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.[12] Second, the 1960-1980 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 1960-1980 Censuses and wage data from the 2000 Census and the 2001-2007 ACSs facilitates a panel analysis in which the effect of retention policy on long-run outcomes is estimated from within-state variation over time. But in order to infer retention rates, we must modify equation (2) slightly 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:

(3) $R_{bc}^{a} = O_{bc}^{a} - O_{bc-1}^{a+1}$

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 April 1, 1980, so the majority of six-year olds turned six in 1979. For example, the on-time measure $O_{MO\,1979}^{6}$ is the fraction of six-year-olds in 1979 in Missouri, who were in (or had completed) grade one. Because we do not observe this cohort in grade two, we calculate the grade two on-time rate for students who were seven years old in 1979 (and thus six years old in 1978.) The grade one retention rate is

[12] 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.

the difference between the 1979 cohort's grade one on-time rate and the 1978 cohort's grade two on-time rate. For simplicity, we label this as the retention rate for 1979, although the retention rate applies to a two-year 1978-1979 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 on-time status is to define six year-olds to be on time if they are in first grade or higher, seven-year-olds as on time 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 cut-off for kindergarten in Missouri in 1979 was October 1 (i.e., state law required that students who turned 5 after October 1 defer entering kindergarten until the following year.) By the standard rule, any student who was reported in the 1980 Census to be seven years old in grade 1 would be classified as behind. But if a Missouri first grader's birthday fell between October 1 and March 31, and he had turned seven by the Census reporting date (April 1, 1980), he would have been six on October 1, 1979 and "on time" for grade one. In this case, then, the standard rule *incorrectly assigns about half of all students*.[13] Using data on the student's quarter of birth, and data on school age entry laws (Bedard and Dhuey 2008), we are able to modify the on-time measure to account for state-level age entry laws and the Census reporting date. Specifically, given the Missouri cut-off, we define seven-year-olds born in quarters 4 and 1 to be on-time if they are in grade one or higher, and those born in quarters 2 and 3 to be on time if they are in grade two or higher, and so on for other age cohorts. Of course, age entry cut-offs do not always occur on the boundary between quarters (as was the case in this convenient example). When cut-offs

[13] If the first grade student's birthday fell between April 1 and October 1, and he was 7 at the time of the Census, he would have been 7 on October 1 of his grade 1 school year and behind. These students would be correctly assigned by the standard rule.

are not on 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 retention experienced by a birth cohort occurs in important ways not captured by equation (3). Students who moved from being on-time to behind over consecutive grades would be accounted for in (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 students who are two years behind, and modify (3) as follows:

$$(4) \qquad R_{bc}^{a} = (O_{bc}^{a} - O_{bc-1}^{a+1}) + (A_{bc}^{a} - A_{bc-1}^{a+1}) + (D_{bc-1}^{a+1} - D_{bc}^{a}),$$

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.[14] 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 one, two, and three 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 their strongest responses in early grades. Jacob and Lefgren (2002) show that retention rates increased more in first grade than in any other grade, after the introduction of the Chicago Public Schools outcome-based policy initiative, even though the policy targeted later grades. Hauser et al (2006) draw similar conclusions: With the introduction of outcome-based reforms, early grade retention rates appear to have responded more than later grades.[15] 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 more common and appears to respond more to policy, we focus on retention over grades one and two in the main regressions.

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[14] Multi-grade 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.

[15] Previous research suggests 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 appear on-time in first grade without having been on-time in kindergarten. This makes it impossible to estimate kindergarten retention using on-time rates.

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 Shepherd and Smith 1989, Hauser et al 2006). 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 exist grade one and grade two retention data from both sources for 9 states in 1979. Figure 1 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.

C. 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 U.S. 5 percent Public Use Micro Census and the 2001-2007 ACS. The retention rate over grades one and two 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) they would have been in first or second grade. Table 1 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 IV include controls for the fraction of students who are on time in grade one, ahead as of grade one, 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.

IV. Results

A. Wages

Table 2 reports the results from regressions of log hourly wages on retention through grade two (i.e., total fraction retained in grade one or two). Using the baseline specification from equation (1) on the sample of men ages 25 to 54, column 1 indicates that a 0.1 change in the fraction of students retained through grade two 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 0.6 percent.

The model in column 1 allows that intermediate choices (e.g. education, state of residence, marital status) may be endogenous—potentially influenced 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 impact 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-level trends in wages and 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 non-random migration of workers between regions.[16] Results from this model are nearly identical to results in columns 1 and 2.

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 0.6 percent wage increase in response to a standard-deviation change in the retention rate.

B. Educational Attainment

Educational attainment is one mechanism that could generate the observed association between first grade retention rates and hourly wages later in the lifecycle. Are changes in the retention rate associated with changes in educational attainment? As we report in Table 3, the association between increased retention rates and educational attainment is 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 0.15 percentage points 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 that changes in the retention rate generate economically significant changes in the years of schooling a worker

[16] See Heckman et al, 1995.

completes. Clearly, however, human capital acquisition depends on both the quantity and quality of schooling received, and the dependent variables in Table 3 capture only the former.

C. Timing - Retention Impacts by Grade

The analysis so far has focused on overall retention though grade two. But it may also be informative to examine retention by specific grades. Table 4, Panel B repeats the exercise of Table 2, using grade one and grade two retention rates as separate regressors. The effects of grade one and grade two retention appear very similar, though the latter is imprecisely estimated in some specifications. Panels C and D repeat the exercises of Panels A and B, but examine retention through grade three. The wage effect of early grade retention appears to be driven by changes in grade one and two retention; Panel D shows little or no effect of third grade retention.[17] Panels E, F and G, which contain results from separate regressions of log wages on grade one retention, grade two retention, and grade three retention, respectively, show the same pattern: Positive wage effects for first grade retention and marginally positive effects for second grade retention, but little or no effect of third grade retention.

D. Robustness

[17] Kindergarten retention is not included as a covariate in Table 4 because construction of kindergarten retention rates from the Census is problematic (see footnote 15). If increased grade 1 retention were associated with *decreased* kindergarten retention, then 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.

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. School age entry laws, for example, have been shown to influence student outcomes (Bedard and Dhuey, 2008) and may also influence retention policy. We have controlled for this in several ways. First, as described in Section III.B, we use age-entry cut-offs 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, then, 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 for a wide variety of education policy variables in all specifications. These include: average pupil-teacher ratios, relative teacher salaries, the minimum school leaving age, the availability of publically funded kindergarten, and youngest legal school entry age to account for changes in school age entry laws. Third, to capture any additional effects of within-state changes in age at entry, we also include control variables for the fraction of students on time and the fraction of students ahead when they enter grade one. These controls are included in all specifications, including those reported in Table 2.

One might further be concerned that other, non-education 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 non-white workers. Table 5, row 1 shows the main regressions results from Table 2, while 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 non-white workers.

Retention in the southern states was much higher than in the rest of the country in the 1940s and 1950s. Brown versus 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 row 3. The results are robust to the exclusion of southern states.

One might also imagine that there were different external policy changes in different sub-periods covered by the data. In Table 5, rows 4 and 5, point estimates appear similar for both of the ten-year sub-periods 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 composition and characteristics of the school-age population may be confounded with retention. Row 6 in Table 5 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, 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. The estimated effect of retention is largely unaffected by the inclusion of these variables.

As is always the case, unobservables remain a possible confounding influence. For example, changes over time in average unobserved ability of cohorts, within 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, and suggest that the coefficients on retention in Table 2 are downward biased.

E. Discussion

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.[18] Because we do not observe retention histories for respondents in the 2000 Census and 2001-2007 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 serious endogeneity issues. Similarly, because we do not observe socioeconomic status in childhood for adult respondents, we cannot parse out effects by 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 results in Table 6. Though coefficients vary by specification and are not always precisely estimated, the retention

[18] 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, and so we use the term for simplicity of exposition.

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 2, 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.

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 one or two 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—then the upper quantile estimates reported in Table 6 are 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 one 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 fifty percent of children (0.11) and among the top quartile of the family income distribution (0.06).[19] As the analysis in this paper is based on within-state changes rather

[19] For our purposes, the family income distribution is the distribution of family income observed among our school entry cohorts, as defined in Section IIIB.

than levels, Panel B reports changes in retention rates. Specifically, Panel B indicates that when overall retention rates change between Censuses by at least one 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 that increased retention for one SES subgroup is associated with decreased retention for another.

Middle and high SES students, then, are exposed to retention, and to changes over time in retention rates, within their own SES quartiles. In addition, and perhaps just as importantly, middle and high SES students are exposed to low SES peers, and thus to changes in retention among this group. Table 8, based on data from the Common Core of National Center for Educations Statistics (NCES), shows that in a majority of schools, more than 20 percent of students are eligible for free lunches.[20] 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 16 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 that 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

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

high SES schools. Table 9 shows the distribution of retention across SES groups and school types using data from the 1998 kindergarten entry cohort form the Early Childhood Longitudinal Survey (ECLS). We use ECLS data here because it is the only source that reports both student-level SES and school-level SES (defined by the fraction of students eligible for free lunches). 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 importantly 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 through 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 II.B, 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.[21]

V. 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 paper estimates the effects of retention policy on all students in a cohort, retained and promoted. The main finding is that a one standard deviation increase in retention through grade two is associated with 0.6 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. 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.

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, and stands in contrast to the large cost estimates of retention on the retained reported by Cascio (2005) and Eide and Showalter (2001). Assuming a 0.03 increase in retention and a rate of return to

[21] Lazear's (1999) model, which emphasizes externalities associated with disruptive students, offers another interesting possibility. If increased retention allows for better handling of disruptive students, then 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 maybe more instances in which multiple students disrupt simultaneously.

experience estimated from the 2000 Census for men aged 25-64,[22] the cost for the average retained man is approximately $34,500.[23] However, marginally retained men are greatly outnumbered by promoted men, for whom the average return is approximately $4750. With a 0.03 increase in retention, then, the average gain per man is $3600. Clearly, gains on average do not imply positive effects for every worker. Given the assumptions in this exercise, some workers bear very large costs, but small gains to the large majority swamp the losses to the marginally retained.

[22] All calculations are for the average man who has 14 years of education. The return to experience is estimated using $\ln y_i = \phi_0 + \phi_1 \exp_i + \phi_2 \exp_i^2 + u_i$.

[23] This includes a $5000 direct cost for an extra year of schooling (recall that our estimates in Table 3 show no evidence of altered educational attainment), the loss of a year of income while still in school, and the lost 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 20.

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Figure 1

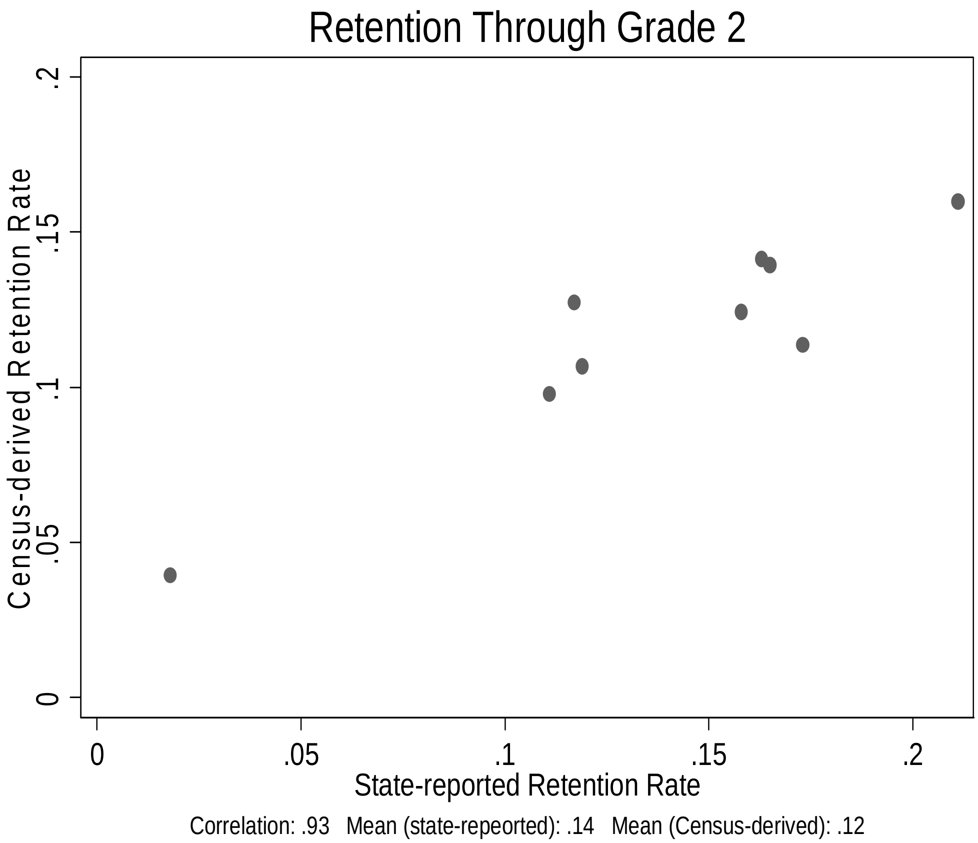
Retention Through Grade 2
Census-derived Retention Rate
0
.05
.1
.15
.2
0
.05
.1
.15
.2
State-reported Retention Rate
Correlation: .93   Mean (state-repeorted): .14   Mean (Census-derived): .12

Table 1a. Retention Rates

	Cohort		
	1959	1969	1979
Grade one retention	0.083	0.073	0.056
	(0.041)	(0.037)	(0.022)
Grade two retention	0.073	0.050	0.046
	(0.050)	(0.022)	(0.017)
Grade three retention	0.054	0.040	0.056
	(0.039)	(0.026)	(0.018)

Population weighted.

Table 1b. Summary Statistics

	Employed Men		All Men	
	Mean	Standard Deviation	Mean	Standard Deviation
Ln hourly wage	3.05	0.71	--	--
Retention through Grade 2	0.13	0.06	0.13	0.06
Percent ontime or ahead as of grade 1	0.89	0.06	0.89	0.06
Percent ahead as of kindergarten	0.09	0.04	0.09	0.04
Pupil-teacher ratio	21.27	3.03	21.33	3.05
Relative teacher wage	0.62	0.07	0.62	0.07
Minimum school exit age	16.32	0.67	16.32	0.67
Pubilically provided kindergarten	0.75	0.43	0.74	0.44
Youngest legal school entry age	57.49	1.55	57.49	1.55
Age	40.47	8.12	40.50	8.22
Black	0.09	0.29	0.11	0.31
Hispanic	0.07	0.25	0.07	0.25
Other race	0.03	0.17	0.03	0.17
Sample Size	596,233		797,080	

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

Table 2. The Impact of Retention Policy on ln Hourly Wages

	Specification		
	(1)	(2)	(3)
Retention	**0.1935**	**0.1925**	**0.1911**
	(0.0847)	(0.0791)	(0.0785)
Implied impact of a one standard deviation increase in retention (the within state standard deviation is 0.03)	0.6%	0.6%	0.6%
Sample Size	596,233	596,233	596,233
<u>Additional Controls:</u>			
State of Residence	No	Yes	Yes
Region of Residence*Age	No	Yes	Yes
State of Residence GDP & UER	No	Yes	Yes
Marital Status	No	Yes	Yes
Region of Birth*Region of Residence	No	No	Yes

All models are population weighted and clustered at the state of birth level. All models also include controls for the fraction of students who are on time in grade one, ahead as of grade one, 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. Bold coefficients are statistically significant at the 5% level and bold italics are statistically significant at the 10% level.

Table 3. The Impact of Retention Policy on Educational Attainment

	Specification		
	(1)	(2)	(3)
Grade 10 or more	-0.0161	-0.0140	-0.0139
	-(0.0161)	(0.0339)	(0.0340)
Grade 11 or more	-0.0204	-0.0155	-0.0156
	(0.0305)	(0.0340)	(0.0341)
High school graduation or more	-0.0508	-0.0436	-0.0443
	(0.0346)	(0.0372)	(0.0373)
Some college or more	0.0264	0.0389	0.0333
	(0.0755)	(0.0770)	(0.0791)
BA or more	0.0402	0.0391	0.0340
	(0.0535)	(0.0523)	(0.0527)
Sample Size	797,080	797,080	797,080
Additional Controls:			
State of Residence	No	Yes	Yes
Region of Residence*Age	No	Yes	Yes
State of Residence GDP & UER	No	Yes	Yes
Marital Status	No	Yes	Yes
Region of Birth*Region of Residence	No	No	Yes

All models are population weighted and clustered at the state of birth level. All models also include controls for the fraction of students who are on time in grade one, ahead as of grade one, 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. Bold coefficients are statistically significant at the 5% level and bold italics are statistically significant at the 10% level.

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

	Specification		
	(1)	(2)	(3)
Panel A: Base Case			
Rentention through grade 2	**0.1935**	**0.1925**	**0.1911**
	(0.0847)	(0.0791)	(0.0785)
Panel B			
Grade 1 retention	**0.2183**	**0.2166**	**0.2207**
	(0.0749)	(0.0695)	(0.0699)
Grade 2 retention	0.1797	***0.1792***	***0.1747***
	(0.1069)	(0.1026)	(0.1021)
Panel C			
Rentention through grade 3	***0.1200***	**0.1315**	**0.1293**
	(0.0606)	(0.0592)	(0.0593)
Panel D			
Grade 1 retention	**0.2108**	**0.2032**	**0.2087**
	(0.0751)	(0.0717)	(0.0719)
Grade 2 retention	0.1965	***0.2077***	0.2003
	(0.1235)	(0.1193)	(0.1196)
Grade 3 retention	0.0359	0.0611	0.0548
	(0.0824)	(0.0894)	(0.0893)
Panel E			
Grade 1 retention	**0.1569**	**0.1546**	**0.1603**
	(0.0626)	(0.0648)	(0.0658)
Panel F			
Grade 2 retention	0.1306	0.1303	0.1249
	(0.1016)	(0.0975)	(0.0974)
Panel G			
Grade 3 retention	-0.0045	0.0151	0.0130
	(0.0588)	(0.0630)	(0.0626)
Additional Controls:			
State of Residence	No	Yes	Yes
Region of Residence*Age	No	Yes	Yes
State of Residence GDP & UER	No	Yes	Yes
Marital Status	No	Yes	Yes
Region of Birth*Region of Residence	No	No	Yes

All models are population weighted and clustered at the state of birth level. All models also include controls for the fraction of students who are on time in grade one, ahead as of grade one, 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. Bold coefficients are statistically significant at the 5% level and bold italics are statistically significant at the 10% level. The sample size is 569,233.

Table 5. The Impact of Retention Policy on ln Hourly Wages

	Specification			Sample Size
	(1)	(2)	(3)	(4)
All men	**0.1935**	**0.1925**	**0.1911**	596,233
	(0.0847)	(0.0791)	(0.0785)	
White men	**0.2534**	**0.2570**	**0.2588**	510,829
	(0.1041)	(0.0949)	(0.0935)	
Exlcuding southern states (all men)	**0.2508**	**0.2214**	**0.2208**	411,805
	(0.1264)	(0.1168)	(0.1149)	
1957-59 and 1967-69 cohorts only	**0.2012**	0.1667	0.1551	453,129
	(0.0985)	(0.1016)	(0.1021)	
1967-69 and 1977-79 cohorts only	0.1434	***0.1906***	***0.1983***	369,536
	(0.0968)	(0.0998)	(0.1026)	
Including additional controls (all men)	**0.1823**	**0.1863**	**0.1857**	596,233
	(0.0622)	(0.0635)	(0.0632)	
Additional Controls:				
State of Residence	No	Yes	Yes	
Region of Residence*Age	No	Yes	Yes	
State of Residence GDP & UER	No	Yes	Yes	
Marital Status	No	Yes	Yes	
Region of Birth*Region of Residence	No	No	Yes	

All models are population weighted and clustered at the state of birth level. All models also include controls for the fraction of students who are on time in grade one, ahead as of grade one, 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. Bold coefficients are statistically significant at the 5% level and bold italics are statistically significant at the 10% level.

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

	Specification		
	(1)	(2)	(3)
10th Quantile	0.1455	**0.2110**	**0.2209**
	(0.0991)	(0.1020)	(0.1033)
25th Quantile	**0.1454**	0.0893	0.0814
	(0.0687)	(0.0729)	(0.0666)
50th Quantile	***0.0942***	**0.1210**	**0.1302**
	(0.0555)	(0.0556)	(0.0541)
75th Quantile	**0.1726**	**0.2211**	**0.1882**
	(0.0753)	(0.0683)	(0.0682)
90th Quantile	**0.277**	0.1784	0.1405
	(0.1132)	(0.1126)	(0.1111)
Additional Controls:			
State of Residence	No	Yes	Yes
Region of Residence*Age	No	Yes	Yes
State of Residence GDP & UER	No	Yes	Yes
Marital Status	No	Yes	Yes
Region of Birth*Region of Residence	No	No	Yes

All models are population weighted and clustered at the state of birth level. All models also include controls for the fraction of students who are on time in grade one, ahead as of grade one, 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. Bold coefficients are statistically significant at the 5% level and bold italics are statistically significant at the 10% level.

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)	(4)	(5)	(6)
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)	

State-level Census data from 1960-1980. Weighted by cohort size. Standard deviations in parentheses.

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.

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

Standard deviations in parentheses and sample sizes 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 they repeated kindergarten, grade one, or grade two.