# No Cohort Left Behind?

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Abstract

Much of the debate over the allocation of education resources focuses on the alleged benefits of smallness—of classroom or school—and is based on evidence from small-scale studies. This paper reframes the question in terms of cohort size. Using national data, we find that a 10-percent increase in kindergarten enrollment yields a 0.5 percent increase in cohort shrinkage across early grade transitions, which implies that larger cohorts feature higher rates of retention. Consistent with previous work on class and school size in more restricted settings, this cohort-tracking exercise provides robust evidence at the national level that smallness confers benefits.

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## I. Introduction

A large literature on the optimal allocation of education resources argues that small classes confer benefits. A narrower literature explores whether small schools confer benefits. But though much attention has been given to possible effects of "smallness" defined along these margins, there is no work exploring the effects of small grade cohorts on education outcomes. This paper attempts to fill that gap by investigating the link between entering cohort size and rates of subsequent grade retention.

Many arguments about smallness boil down to an argument about resource scarcity. Larger classes or schools may produce scarcity of time and personalized attention per student. Larger grade cohorts, as well, could generate similar scarcities, in terms or teacher time or other resources. One advantage of the focus on cohort size is that familiar ideas may be tested in a new setting—an unexplored dimension along which size might also matter. A second advantage of our approach is that it allows for an analysis of uncommon breadth and scope, as data on grade-specific enrollment counts by district for primary schools in the U.S. are available at the national level. This paper, then, offers a new and data-rich take on a long-debated theme: in a new way, we investigate whether smaller is better.

In Project STAR, source of the most widely cited evidence on class size and student achievement, students in larger classes had lower test scores and were more likely to repeat a grade.[1] Some other studies have also found adverse student outcomes in larger classes, though the evidence varies.[2] One motivation for a cohort-level analysis is that it offers a way to test whether effects implied by Project STAR and other small-scale studies on class size

[1] Project STAR findings are reported in Mosteller (1995), Finn and Achilles (1990), Hanushek (1999), Nye, Hedges, and Konstatopoulos (2000), Krueger (1999), Schanzenbach (2007), and Word et al (1990) .
[2] Hanushek (1999) and Krueger (2003) survey this extensive literature. See also Angrist and Lavy (1999), Ankerhielm (1995) and Hoxby (2000).

are visible at the national level. Evidence indicates that districts do not adjust teacher counts perfectly when they experience shocks to cohort size that deviate from trend. Changes in entering cohort size, then, are a source of variation in class size. If positive shocks to class size produce higher retention rates, then this should translate into declining enrollments from one grade to the next. Loosely speaking, large cohorts should produce more retention, and thus more "cohort shrinkage" between grades. We emphasize that there are other reasons one might see lower outcomes for large cohorts. There could be scarcity of other important resources not related to class size. But if class size truly matters, then cohort size should matter as well: To the extent that cohort size alters class size, we should expect to see effects of cohort size on education outcomes.

Hypothesized benefits of smallness are not limited to class size. Though more speculative than the class size literature, some previous research explores whether school size may influence student outcomes. A summary of the school size literature commissioned by the U.S. Department of Education concludes that smaller schools are associated with increased achievement, graduation rates, satisfaction, and behavior (Raywid, 1999).[3] Much attention has been paid to this research, and education policies promoting small schools have been motivated by it. The Bill and Melinda Gates Foundation, for example, embraced and funded these policies for several years in the 2000s, before revising their strategy.[4] In short, researchers have taken very seriously the idea that smallness may confer benefits. A natural extension is to explore a different but related definition of smallness.

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[3] Research on the effects of small schools is also reported in Wasley and Lear (2001) and Duke and Trautvetter (2009).

[4] Linda Shaw, The Seattle Times, November 5, 2006. See also Bill and Melinda Gates Foundation, Annual Report, 2003, www.gatesfoundation.org.

Do small cohorts confer benefits? This paper attempts to answer the question using district level counts of students from the National Center for Education Statistics (NCES) Common Core of Data (CCD). In the main analysis, we look within a district over time to discern whether fluctuations in cohort size predict cohort shrinkage. Our main finding is that an increase in kindergarten enrollment of 10 percent is associated with shrinkage of 0.5 percentage points in the size of a cohort between first and second grade. This finding is robust across multiple specifications that control for possible confounding influences (see Section II.C. for a detailed discussion about identification). A similar relationship between kindergarten cohort size and cohort shrinkage is also visible between second and third grade, and between third and fourth grade. County level specifications that control for changes in private school enrollment indicate that the main findings are not easily accounted for by between-district migration or the flow of students between public and private schools. We conclude that larger cohorts feature higher rates of cohort shrinkage, and that this is evidence of increased grade retention. If grade progression is an important outcome, then it is beneficial to be in a smaller cohort.

The remainder of the paper is organized as follows: Section II describes the data and empirical strategy; Section III presents results of the analysis; Section IV interprets the findings; Section V concludes.

## II. NCES Data and Empirical Strategy

### A. Data

All public school data are from the NCES CCD. The grade span and the number of full-time equivalent (FTE) kindergarten teachers for each school district are from the Local

Agency (School District) Universe for the 1992/1993 through 2005/2006 school years. Grade-specific student enrollments are from the Public Elementary/Secondary School Universe for 1992/1993 through 2007/2008. Combined, these sources form a panel of U.S. public school districts that tracks fourteen kindergarten cohorts (1992/1993 through 2005/2006) through second grade. Each kindergarten cohort is identified by the year of kindergarten entry. For example, the cohort that entered kindergarten in the fall of 1992, and who were potential first and second graders in the falls of 1993 and 1994 respectively, are referred to as the 1992 cohort. Each observation matches a kindergarten enrollment (cohort size) to the corresponding change in the size of the cohort between first and second grade.

Each observation is a district that spans at least kindergarten through grade five and has a minimum of five students enrolled in kindergarten in any given year. The grade span restriction is necessary to identify a cohort through the early elementary grades. However, as a cohort progresses, students may enter or leave a district. Since the CCD does not directly measure such transfers, cohort growth may be incorrectly measured; but, under the assumption that these transfers are uncorrelated with the natural variation in cohort size, this measurement error will not bias our estimates (see Section II.B for further discussion of this issue). There are 15,310 such districts in the CCD, summary statistics for which are reported in columns 1 and 2 in Table 1.[5] All summary statistics are weighted by average district size from 1992/93-2005/06, as measured by fifth through seventh grade enrollment. As this is a 14 year panel, districts are included multiple times. Sample sizes differ across variables due to non-reporting. The average years of inclusion in the sample is 12.5. Calculating the cohort

[5] We exclude 56 observations (district-year observations, not districts) with first, second, or third grade cohort growth rates exceeding positive or negative 10 (i.e., 10 times more second graders enrolled than first graders or vice versa). All results are similar if these observations (which are clear data errors) are included or if we use other trimming rules.

growth rate between grades one and two and linking this growth rate to kindergarten cohort size requires that a district have complete reporting for three consecutive years (see Section II.B for more detail). This reduces the sample to the 14,373 districts described by the statistics reported in columns 3 and 4. To discern changes within districts over time, we need more than one observation per district. When we drop districts with only one observation, we have 14,052 districts. This sample of 175,078 district-cohort observations, described in columns 5 and 6, will be used the main analysis. Notice that observable characteristics are similar across the columns of Table 1, alleviating concerns about non-random exclusion.

**B. Cohort Shrinkage**

The growth rate of a kindergarten cohort between first and second grade is defined as the difference between second and first grade enrollment divided by first grade enrollment:

$$(1) \qquad G_{dy} = \frac{e_{dy}^{2} - e_{dy}^{1}}{e_{dy}^{1}}$$

where $G_{dy}$ is the enrollment growth rate between grades one and two for the cohort that entered kindergarten in year $y$ in district $d$. Enrollment is denoted by $e$ and superscripts denote grade. [6] Equation (1) is written as function of enrollment because the data are available at this level. In an environment in which there is more first grade failure than second grade failure, we expect $G$ to be negative—hence the term “cohort shrinkage.” Interpreting changes in cohort shrinkage as stemming from changes in first grade failure involves several important subtleties that are easiest to see when Equation (1) is written as a

[6] It might seem natural to use cohort shrinkage between kindergarten and first grade as the dependent variable. However, this would create a situation in which kindergarten cohort size appeared on both the left and right side of the specifying equation. Measurement error in kindergarten enrollment would generate a spurious negative correlation with kindergarten cohort shrinkage. Thus, we did not estimate such a model.

function of kindergarten cohort size and retention (failure) levels. Assuming no inter-district transfers and no immigration or emigration (these will be discussed in detail later in this section), first grade enrollment for any given cohort *y* is:

(2) $$e_{dy}^{1} = e_{dy}^{k} - F_{dy}^{k} + F_{dy-1}^{1}$$

where $e_{dy}^{k}$ is the size of the kindergarten cohort in year *y* in district *d*, $F_{dy}^{k}$ is the number of cohort *y* children retained in kindergarten, and $F_{dy-1}^{1}$ is the number of children retained in grade one from the previous (*y*-1) cohort.[7] Second grade enrollment can similarly be written as:

(3) $$e_{dy}^{2} = e_{dy}^{k} - F_{dy}^{k} + F_{dy-1}^{1} - F_{dy}^{1} + F_{dy-1}^{2}$$

Generally speaking, enrollment in a given grade is kindergarten enrollment plus the stream of retention from the preceding cohort less retention from the cohort of interest. The difference in enrollment between grades one and two for a given cohort *y* therefore simplifies to:

(4) $$e_{dy}^{2} - e_{dy}^{1} = F_{dy-1}^{2} - F_{dy}^{1} .$$

Since our ultimate objective is to understand the effect of cohort size on the first grade failure rate we differentiate the growth rate with respect to kindergarten enrollment. This derivative helps to clarify the assumptions required to interpret changes in observed growth rates as changes in retention. If we assume that shocks to kindergarten enrollment are independent of failure rates for other cohorts[8], $\partial F_{dy-1}^{j} \big/ \partial e_{dy}^{k} = 0 \; \forall \; j = k,1,2$ :

[7] For simplicity we are assuming students can not fail twice. This could easily be incorporated. All equations are written in terms of kindergarten enrollment rather than first-time kindergarten size and the failure rate of the previous cohort because we can only observe kindergarten enrollment.
[8] We relax this assumption in Section III.

(5) $$\frac{\partial G_{dy}^{1}}{\partial e_{dy}^{k}} = -\frac{1}{e_{dy}^{1}}\frac{\partial F_{dy}^{1}}{\partial e_{dy}^{k}} - \frac{\partial e_{dy}^{1}}{\partial e_{dy}^{k}}\frac{[F_{dy-1}^{2} - F_{dy}^{1}]}{(e_{dy}^{1})^{2}}.$$

The change in the growth rate reflects the change in the first grade failure rate associated with an increase in kindergarten cohort size as well as the small change in the first grade reference cohort size (the denominator of *G*). Thus, the marginal change in failure rate captured by the first term of (5) is confounded by a second term which reflects the increase in the denominator. Assuming that the first grade failure rate exceeds the second grade failure rate, as is generally the case in retention data, the second term is positive. The existence of the second term implies, then, that cohort shrinkage slightly underestimates the response of first grade failure to a change in kindergarten cohort size.

Before proceeding to our main regressions, it is instructive to describe the cohort shrinkage measure used here to capture retention rates and compare it with those reported by Hauser, Frederick, and Andrew (2007), the only nationally representative comparable study we are aware of. The first row in column (5) of Table 1 reports that the average cohort growth rate between first and second grade for the sample is -0.013 (sd = 0.084). This says that first grade retention is 1.3 percent larger than second grade retention, given the reasoning above. In contrast, Hauser, Frederick, and Andrew (2007) estimate the difference between the first and second grade retention rates to be -4.5 percent in the CPS. There are several possible reasons for the difference between the two data sets. First, the time periods are slightly different. Second, the CCD enrollment data includes both immigrants and students who enter public school districts from private schools. Using published estimates on immigration from Homeland Security, we estimate the rate at which immigrant children flow into public schools to be about 0.4 to 0.6 percent per year.[9] Using data from the NCES Private School Universe Survey (PSS), we estimate that students from private schools enter into public schools at a rate about 0.6 percent between first and second grade. Adding these inflows to the CCD estimate, the difference between second and first grade retention is between -2.5 and -3 percent. Third, the CPS estimates may be larger in magnitude due to parental reporting errors in the CPS. The CPS estimates are too large if parents are less likely to report that their children repeat second grade compared to first grade. This could occur if there is more social stigma attached to later grade failure.

**C. Empirical strategy**

The identification strategy used here is similar in spirit to that used by Hoxby (2000) to estimate class size effects. We begin with the following simple model:

(6) $$G_{dy} = \beta_1 \ln e^k_{dy} + \beta_2 X_{dy} + \lambda_d + \theta_y + \varepsilon_{dy}$$

where districts are denoted by *d* and cohorts by kindergarten entry years *y* = 1992,...,2005. *G* is the rate of cohort growth between first and second grade as defined by equation (1), $e^k$ is kindergarten enrollment, *X* is the available set of district characteristics, λ is a vector of district fixed effects, θ is a vector of state by kindergarten entry year indicators, and ε is the usual error term. Equation (6) includes the natural log of kindergarten enrollment to take account of the fact that a one-student reduction is proportionately larger from a smaller base. The fixed effects control for unobservable differences between districts. Including a state-by-year fixed effect is necessary to remove bias created by changes in state educational policies which may be correlated with both student retention and enrollment size. These indicators

[9] This includes estimates on illegal immigrants.

also control for state and national-level cyclical and seasonal birth patterns (Dehejia and Lleras-Muney, 2004; Buckles and Hungerman, 2008). *X* includes the fraction of students in the district that are eligible for free lunch, the fraction of students that are Black, Hispanic, Asian, or Native American/Alaska Native (white is the omitted race category), district size measured by fifth-seventh grade enrollments, and the county-level unemployment rate. Summary statistics for these variables are reported in Table 1.

All regressions are weighted by average district size and the standard errors are clustered at the district level. The identifying assumption is that conditional on included control variables kindergarten cohort size within a given district varies randomly with fluctuations in the birth rate. More specifically, the coefficient of interest, $\beta_1$, captures within-district deviations from state trends in cohort growth associated with deviations in kindergarten cohort size. The inclusion of district size is particularly important because this allows us to purge the kindergarten cohort size regressor of effects due to changes over time in district size, leaving only kindergarten idiosyncratic effects. As identification comes from within-district deviations from state trends, we also probe the importance of the trend definition to ensure that the results are not sensitive to this specification. In particular, we also use linear district trends and flexible national trends. While we have tried to include all important available time trends and time-varying local factors that could confound the impact of cohort size fluctuations, unobservable factors that fluctuate at the local level and are correlated with cohort size will of course bias the estimate of interest. For this reason, we report the results for several specifications and perform a wide array of robustness checks.

As with other examinations into the effect of class size or school size on student outcomes, one must consider the endogenous responses of parents to enrollment changes.

Parents may wish to avoid large cohorts and may switch their students out of an overcrowded cohort within a school to achieve this purpose. However, the cost of switching districts is arguably much higher than the cost of switching schools within the same district. To switch school districts a family incurs the high cost of transporting a student some distance or moving the entire family into a neighborhood within the new school district. Conducting the analysis at the district level, as here, mitigates the problem of selective exit and entry into cohorts. County-level regressions, results of which we report as robustness checks, further limit the channel of migration. It would seem particularly costly to migrate to another county in response to being part of a large kindergarten cohort.

One might also be concerned about potentially endogenous decisions of parents that involve migration between public and private schools. Parents wishing to avoid large cohorts could switch their students out of the public school and into private school without having to migrate to another district or county. Using data on public and private school enrollments by grade and year, we explore whether migration of this type explains cohort shrinkage. We find little evidence that it does.

We will investigate cohort shrinkage between grades 1 and 2, grades 2 and 3, and grades 3 and 4. However, because of the potential for selective migration, we will focus on cohort shrinkage between grades 1 and 2 in the main analysis. We view these as the most reliable estimates for the purpose at hand: Less time has elapsed between kindergarten and first grade than between kindergarten and later grades, allowing less of an opportunity for selective migration between districts.

## III. Results

### A. Cohort Size and Cohort Shrinkage

The results of estimating equation (6) are presented in Table 2. Each column of the table represents a different regression. The first row of the table shows the estimated kindergarten cohort size effect on the first grade retention rate (as captured by cohort shrinkage between grades 1 and 2). Columns 1 and 2 show results from specifications that include national and state-level year indicators, respectively, and column 3 adds the full set of controls from Table 1. These estimates indicate that a cohort's growth rate is negatively affected by the cohort's kindergarten cohort size. When a kindergarten cohort in a given district is 10 percent larger, the cohort shrinks about 0.5 percentage points more between first and second grade, which would imply increased grade retention of 0.5 percentage points.[10] The focus on a 10 percent enrollment change is intended to give the results some context, and was chosen because it approximately represents the within district standard deviation of the state-year de-trended ln cohort size (see Appendix Table 1).[11] While an increase in retention of 0.5 percentage points may appear modest, the implied impact is large and economically meaningful relative to the first grade retention rate.

Results from column 4 are based on a model in which observations are not weighted by district size. Thus, the smaller districts play a greater role in the calculation of the coefficients. The coefficient on kindergarten enrollment in this column is larger in magnitude than in columns 1 through 3, suggesting that the negative association between cohort size and

[10] Comparison of this estimate to the relationship between class size and grade retention found in Project STAR reveals that the magnitudes are virtually the same: In STAR, first grade retention rises about 0.5 percentage points in response to a 10-percent increase in class size.

[11] It is also worth noting that many districts (9,832 out of 14,052) experience at least one relatively large deviation from during the sample period, where large is defined as the absolute value of 0.2 or more. See Appendix Table 2.

retention is stronger for small districts. Fluctuations in enrollment tend to generate larger changes in retention for these districts, as will be shown in more detail in Table 3.

Column 5 allows us to better interpret the coefficient on kindergarten enrollment in Columns 1 through 3. As described in equation (4), there are two potential sources of cohort shrinkage that are tied to retention rates. Firstly, when the first grade retention rate rises, a cohort becomes smaller between first and second grade because fewer students progress. But the cohort will become relatively larger between first and second grade if the second grade retention rate of the preceding cohort rises, i.e., if the cohort is augmented by failers from the grade ahead. It is worth distinguishing, then, whether the cohort shrinkage we observe in Table 1 is driven by increased first grade retention for the cohort in question or by a decreased flow of failers from the previous cohort. In column 5, we add kindergarten enrollment for the previous cohort to the full set of controls in column 3 in order to control for the possibility that cohort shrinkage is driven by this second form of retention.[12] The coefficient on lagged enrollment is very small, and does not significantly alter the cohort shrinkage estimate. Large cohorts, then, appear to be associated with higher rates of first grade retention, rather than with decreased retention in second grade for the cohort that precedes them.

### B. Robustness and Potential Mechanisms

Table 3 tests the robustness of this main finding and investigates some potential mechanisms. The regressions in Table 2 include national or state level time trends, but one

[12] Specifically, if cohort sizes are correlated across years, the kindergarten enrollment regressor could conceivably be capturing effects of kindergarten enrollment for the previous cohort that relate to second grade retention for that cohort. Controlling directly for the size of the previous cohort is a way to address this potential problem.

could go further. One might be concerned about district-specific trends over time that could be correlated with cohort sizes and retention rates. Because there are over 14,000 districts in the main sample, it is not computationally feasible to include time trends for every district. We rely on a random subsample to circumvent this difficulty. Column 3 of Table 3,[13] based on a model that includes district-level linear time trends along with the full set of controls from Table 1, displays results for a random subsample of 1,000 districts that report enrollment in all years. The effect of kindergarten enrollment on cohort shrinkage persists in this specification, and is larger in magnitude than the corresponding estimate in column 2. The larger coefficient estimate may reflect the fact that this specification better isolates the unanticipated component of cohort size changes, and that changes of this kind are more likely to yield changes in retention.[14]

One way that institutions may respond to fluctuations in enrollment is to add or remove kindergarten teachers. NCES data contain district-level counts of kindergarten teachers. In the model summarized in Table 3, columns 4 and 5, we include as a covariate the log of the number of kindergarten teachers. If class size influences grade progression in subsequent years, then the addition of teachers ought to reduce cohort shrinkage. Consistent with this intuition, the coefficients on teachers in columns 4 and 5 are positive and significant. However, they are quite small.

This might seem puzzling, as additional teachers should create smaller classes and mitigate the effects of large cohorts if large class sizes do in fact lead to higher retention. In our view, the most plausible explanation for the small coefficient on log teachers is classical

[13] For comparative ease, column 1 replicates the Table 2 column 3 results and column 2 runs this same specification restricting the sample to districts with complete enrollment reporting.
[14] We replicated this with 20 random samples and obtained similar results.

measurement error that attenuates the estimate. We find strong evidence that kindergarten teacher counts are badly measured. Part of this problem stems from the fact that we do not observe whether kindergarten was full-day or part-day. The survey question asks that respondents report teacher counts in terms of full time employment, making the counts difficult to interpret. For example, some districts with 40 kindergarten students report 1.0 teachers. If this is a district with "half-day" kindergarten, then the reported counts translate into two classes of 20. On the other hand, if this district has "full-day" kindergarten, class size is really 40 students. Either way, dividing enrollment by teacher count, yields a "class size" of 40 students, the estimate associated with a full-day class. Hence, we cannot tell which of these is the true class size. As there are a wide range of true teacher counts, enrollments, and class sizes, it is in general impossible to determine whether districts are reporting teachers as FTE or headcount. There is also evidence that districts change the way they report teachers, doing so in terms of FTE some years and in terms of simple headcounts in other years. Changes in reporting would make it appear that there were enormous changes in class size from year to year, even if class size stayed constant.

Consistent with a story of classical measurement error, we find that when we regress log kindergarten teachers on log kindergarten enrollment with the full set of controls used in Column 3 in Table 2, the coefficient on enrollment is 0.702 (standard error = 0.036). When class size rises one percent, within district, then the teacher count rises 0.7 percent. This would suggest that districts respond to changes in enrollment but not enough to keep class size constant. If we regress log kindergarten enrollment on log kindergarten teachers, however, the coefficient on log teachers is very small, only 0.127 (standard error = 0.016).

The coefficient estimate on log teachers may be attenuated, due to noise, perhaps a result of the reporting problems noted above.

There are several other reasons for the small coefficient on teachers in Table 3: 1) We do not observe teacher counts in first grade or whether first grade and kindergarten teacher counts are correlated. So it is not clear whether effects of teacher changes in kindergarten are masked by correlated teacher changes in first grade. 2) Adding or removing teachers is an endogenous response to enrollment changes. When trying to respond to short run fluctuations in enrollment, a district may have limited options. The data suggest that when districts add teachers to handle an idiosyncratically large kindergarten cohort, this action is not very effective in reducing first grade retention. This could occur simply because it is difficult to find quality teachers on short notice. 3) Resource scarcities other than teachers may be driving the increased retention rates. Class size is clearly an important resource constraint. Labor costs are the largest share of education expenses, and public expenditures on school dwarf public expenditures in early childhood. According to Lee, Tuljapurkar, and Edwards (2004) and Isaacs (2009), public education accounts for more than 60 percent of expenditures on children under the age of 19. So there is reason to believe that this is where resource crowding would be likely to occur and to generate large effects on education outcomes. Nevertheless, we cannot rule out that crowding out of other public resources, such as public programs for pre-school children, explain the observed effects of large cohorts. For these reasons, we do not characterize this result as a rigorous estimate of a “class size” effect. Arguably, if researchers were to collect less ambiguous data on teacher counts, it would be possible to study class size effects more directly, and perhaps to distinguish them from other forms of resource crowding.

Columns 6 through 9 of Table 3 report results of models with the full set of controls, disaggregated by kindergarten enrollment size. The relationship between kindergarten cohort size and cohort shrinkage is visible in districts of all sizes, but appears strongest in smaller districts. Retention rates for districts that average more than 400 kindergarten students appear to fluctuate less with increases in cohort size. These districts may be better able to shift resources to address year-to-year changes in enrollment. Alternatively, it may be that many of the relevant benefits of "smallness" kick in at a lower threshold.

Table 4 extends the analysis by examining the impact of entering cohort size on cohort shrinkage in later grades. Results of kindergarten cohort size are very similar for cohort shrinkage between first and second grade, between second and third grade, and between third and fourth grade: Increases in kindergarten enrollment of about 10 percent are associated with increased cohort shrinkage of about 0.5 percentage points in all the early grades. We have emphasized shrinkage between first and second grade because of concerns about selective migration between districts, but increased cohort shrinkage for larger cohorts is visible across the early grades, and does not appear to be an idiosyncratic property of the first to second grade transition. This finding also means that the cumulative retention effect across grades is substantially larger than first grade results reported in most of this paper. We note also that it is possible that benefits of small cohort size implied by the coefficients here may be due in part to lagged or contemporaneous effects of resource crowding prior to kindergarten.

Tables 2 through 4 show clear evidence of cohort shrinkage for larger cohorts. Whether this is due to retention may be less certain. One could still worry that selective migration out of larger cohorts may be causing the observed shrinkage. The results in Table 5 allow us to look closely at this possibility. As we have argued, it is costly for parents to switch districts in response to finding out that a child is part of a large district-level cohort. However, it is more costly to have to switch *counties*. In Table 5, column 1 reproduces the main result (of Table 2, column 3) while column 2 summarizes results of the same exercise using county-level counts of students instead of district-level counts. The finding of greater cohort shrinkage for larger cohorts is robust to aggregation to the county level, although the point estimates are somewhat smaller. Attenuation at the county-level is consistent with the notion some parents may respond to district-level cohort crowding by moving their kids to neighboring districts with smaller cohorts.[15]

One could still worry about migration from public schools to private schools as a response to being in large cohorts. Table 5, columns 3 through 6, address this possibility. Data on private school enrollments at the county level are available from the NCES Private School Universe Survey (PSS). This data is available in alternate years; thus we can observe changes in private school enrollment by cohort at the county level between grades 1 and 3 and include this in our county-level regression. As a first step toward this end, column 3 estimates cohort shrinkage between grades 1 and 3 as a function of kindergarten cohort size at the district level. As expected, given our earlier finding that cohort shrinkage between grades 1 and 2 is about the same size as shrinkage from grade 2 to grade 3, the coefficient on kindergarten cohort size is about twice as large as it was in column 1. (This is because the cohort shrinkage is calculated over two grades instead of one.) Column 4 summarizes results of an identical model aggregated to the county level. Column 5 restricts the sample to the alternating years for which we have data on private school enrollments. Estimated

[15] We thank an anonymous referee for highlighting this point.

coefficients in columns 4 and 5 are very similar and give no indication that the sample for which we have data on private schools is idiosyncratic. Column 6 summarizes results from the regression of primary interest: a model that includes the change in private school enrollment as a control to capture migration between private and public schools. When the change in private school enrollment between grades 1 and 3 is included as a regressor, the coefficient on kindergarten cohort size changes very little. We find no evidence, then, that kindergarten cohort size is proxying for flows between public and private school in these regressions. Evidence in Table 5 suggests that selective migration to private schools does not drive the observed relationship between kindergarten cohort-size and cohort shrinkage.

## IV. Discussion

Tables 2, 3, 4, and 5 provide robust evidence of higher rates of cohort shrinkage for larger cohorts, consistent with a story of increased rates of retention. One explanation for this finding is suggested by the literatures on class and school size: Smallness facilitates learning and engagement. Small cohorts may leverage the advantages of small group settings to improve student outcomes, just as small classes and small schools have been argued to do. An alternative explanation is that institutions engage in “cohort-smoothing,” retaining students in larger cohorts at higher rates in order to even out cohort sizes.[16] Though we cannot distinguish between these two potential mechanisms, both explanations imply that there are benefits associated with being in smaller cohorts, at least for those at the lower end

[16] We have also run models that include the log size of the preceding and following cohorts, along with linear district trends, to ensure that our results are not driven by administrators smoothing cohort sizes by retaining more students in large cohorts when small cohorts follow them. While the point estimate on the log size of the following cohort is generally positive (depending on the random draw of districts), its inclusion has little impact on the main cohort size coefficient. While these results suggest that cohort smoothing may exist, these effects do not drive the main estimate.

of the ability or performance distribution. For the student marginally retained–whether because he had not learned enough, or because an institution wanted to smooth an enrollment bulge even though the student had, in fact, learned enough–the outcome would seem a deleterious one.

## V. Summary and Conclusion

Evidence from district level counts of primary school students at the national level indicates that fluctuations in kindergarten cohort size within a district predict cohort shrinkage. Specifically, an increase in kindergarten enrollment of 10 percent is associated with increased shrinkage of 0.5 percent in cohort size across year-to-year transitions in the early grades. This would imply that larger cohorts feature higher rates of grade retention. Consistent with previous work on school size and class size in more restricted settings, the cohort-tracking exercise here provides robust evidence at the national level that smallness confers benefits.

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Table 1. Descriptive Statistics

| | All | | Reporting Cohort Shrinkage | | Reporting Cohort Shrinkage and 2+ Observations | |
|---|---|---|---|---|---|---|
| | Mean (Std Dev) (1) | Sample Size (2) | Mean (Std Dev) (3) | Sample Size (4) | Mean (Std Dev) (5) | Sample Size (6) |
| Cohort growth | -0.0133 | 175,399 | -0.0133 | 175,399 | -0.0132 | 175,078 |
| | (0.0840) | | (0.0840) | | (0.0839) | |
| Kindergarten enrollment | 4437 | 210,880 | 4376 | 175,399 | 4379 | 175,078 |
| | (11,821) | | (11,842) | | (11,847) | |
| ln Kindergarten enrollment | 6.76 | 210,880 | 6.74 | 175,399 | 6.74 | 175,078 |
| | (1.7595) | | (1.7506) | | (1.7507) | |
| Fraction free lunch eligible | 0.2911 | 181,814 | 0.2803 | 149,937 | 0.2799 | 149,649 |
| | (0.2175) | | (0.2159) | | (0.2155) | |
| Fraction black | 0.1625 | 206,819 | 0.1589 | 172,228 | 0.1588 | 171,909 |
| | (0.2118) | | (0.2090) | | (0.2088) | |
| Fraction Hispanic | 0.1581 | 206,819 | 0.1502 | 172,228 | 0.1501 | 171,909 |
| | (0.2202) | | (0.2150) | | (0.2149) | |
| Fraction Asian | 0.0398 | 206,819 | 0.0384 | 172,228 | 0.0383 | 171,909 |
| | (0.0736) | | (0.0723) | | (0.0722) | |
| Fraction Native American | 0.0117 | 206,819 | 0.0117 | 172,228 | 0.0117 | 171,909 |
| | (0.0568) | | (0.0567) | | (0.0568) | |
| District Size (grade 5-7 enrollment) | 13061 | 210,880 | 12732 | 175,399 | 12738 | 175,078 |
| | (34,487) | | (33,867) | | (33,881) | |
| County-Level Unemployement Rate | 5.61 | 183,703 | 5.66 | 162,286 | 5.66 | 162,114 |
| | (2.5426) | | (2.6105) | | (2.6104) | |
| Number of Districts | 15,310 | | 14,373 | | 14,052 | |

Weighted by average district-level 1992/93-2005/06 fifth-seventh grade enrollment.

Table 2. The Impact of Cohort Size on Cohort Growth

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| ln enrollment | -0.0552 | -0.0590 | -0.0591 | -0.0950 | -0.0559 |
| | (0.0084) | (0.0094) | (0.0094) | (0.0053) | (0.0081) |
| lagged ln enrollment | | | | | -0.0008 |
| | | | | | (0.0052) |
| Implied percentage point decrease in cohort growth associated with a 10% increase in enrollment | -0.0053 | -0.0056 | -0.0056 | -0.0091 | -0.0053 |
| Weighted | Yes | Yes | Yes | No | Yes |
| National-year FEs | Yes | No | No | No | No |
| State-year FEs | No | Yes | Yes | Yes | Yes |
| Additional controls | No | No | Yes | Yes | Yes |
| Lagged ln enrollment | No | No | No | No | Yes |
| Sample size | 175,078 | 175,078 | 175,078 | 175,078 | 159,454 |

Weighted by average district-level 1992/93-2005/06 fifth-seventh grade enrollment. All standard errors are clustered at the district level. Additional controls include fraction free lunch eligible, black, Hispanic, Asian, Native American, district size, and county-level unemployment as well as indicator variables for missing free lunch eligible , race, and unemployment information.

Table 3. Robustness and Potential Mechanisms

| | | | | | | By Kindergarten Enrollment Size | | | |
|---|---|---|---|---|---|---|---|---|---|
| | | | | | | <50 | 50-199 | 200-399 | 400+ |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| ln enrollment | -0.0591 | -0.0541 | -0.0838 | -0.0574 | -0.0926 | -0.0907 | -0.0834 | -0.0856 | -0.0305 |
| | (0.0094) | (0.0073) | (0.0179) | (0.0069) | (0.0173) | (0.0047) | (0.0158) | (0.0168) | (0.0131) |
| ln teachers | | | | 0.0032 | 0.0081 | | | | |
| | | | | (0.0011) | (0.0036) | | | | |
| Implied percentage point decrease in cohort growth associated with a 10% increase in enrollment | -0.0056 | -0.0052 | -0.0080 | -0.0055 | -0.0088 | -0.0086 | -0.0079 | -0.0082 | -0.0029 |
| Weighted | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| State-year FEs | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Additional controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Linear district trends | No | No | Yes | No | Yes | No | No | No | No |
| Reporting in all years | No | Yes | Yes | No | Yes | No | No | No | No |
| Sample size | 175,078 | 144,788 | 14,000 | 147,002 | 11,813 | 58,379 | 69,170 | 25,445 | 22,084 |

Weighted by average district-level 1992/93-2005/06 fifth-seventh grade enrollment. All standard errors are clustered at the district level. Additional controls include fraction free lunch eligible, black, Hispanic, Asian, Native American, district size, and county-level unemployment as well as indicator variables for missing free lunch eligible, race, and unemployment information. Column (1) includes 1000 randomly drawn districts.

Table 4. The Impact of Cohort Size on Cohort Growth across Primary Grades

| | (1) $G_{dy}^{1\,to\,2}$ | (2) $G_{dy}^{2\,to\,3}$ | (3) $G_{dy}^{3\,to\,4}$ |
|---|---|---|---|
| ln enrollment | -0.0591 | -0.0456 | -0.0479 |
| | (0.0094) | (0.0062) | (0.0079) |
| Implied percentage point decrease in cohort growth associated with a 10% increase in enrollment | -0.0056 | -0.0043 | -0.0046 |
| Weighted | Yes | Yes | Yes |
| State-year FEs | Yes | Yes | Yes |
| Additional controls | Yes | Yes | Yes |
| Sample size | 175,078 | 159,441 | 144,361 |

Weighted by average district-level 1992/93-2005/06 fifth-seventh grade enrollment. All standard errors are clustered at the district level. Additional controls include fraction free lunch eligible, black, Hispanic, Asian, Native American, district size, and county-level unemployment as well as indicator variables for missing free lunch eligible, race, and unemployment information.

Table 5. The Impact of Cohort Size on Cohort Growth: County Level Aggregation and Private Schools

| | (1) District-Level $G_{dy}^{1to2}$ | (2) County-Level $G_{dy}^{1to2}$ | (3) District-Level $G_{dy}^{1to3}$ | (4) County-Level $G_{dy}^{1to3}$ | (5)* County-Level $G_{dy}^{1to3}$ | (6)* County-Level $G_{dy}^{1to3}$ |
|---|---|---|---|---|---|---|
| ln enrollment | -0.0591 | -0.0441 | -0.1229 | -0.0836 | -0.0702 | -0.0674 |
| | (0.0094) | (0.0145) | (0.0212) | (0.0262) | (0.0341) | (0.0332) |
| Private school flow from grades 1 to 3 | | | | | | 0.00002 |
| | | | | | | (0.00002) |
| Implied percentage point decrease in cohort growth associated with a 10% increase in enrollment | -0.0056 | -0.0042 | -0.0117 | -0.0080 | -0.0067 | -0.0064 |
| Weighted | Yes | Yes | Yes | Yes | Yes | Yes |
| State-year FEs | Yes | Yes | Yes | Yes | Yes | Yes |
| Additional controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Sample size | 175,078 | 42,557 | 159,441 | 39,520 | 14,678 | 14,678 |

Weighted by average district or county-level 1992/93-2005/06 fifth-seventh grade enrollment. All standard errors are clustered at the district or county level. Additional controls include fraction free lunch eligible, black, Hispanic, Asian, Native American, district size, and county-level unemployment as well as indicator variables for missing free lunch eligible, race, and unemployment information. * Sample restricted to years for which private school enrollment data is available (alternating years).

Appendix Table 1. State-Year De-Trended ln Cohort Size

| | All | By Kindergarten Enrollment Size | | | |
|---|---|---|---|---|---|
| | | <50 | 50-199 | 200-399 | 400+ |
| | (1) | (2) | (3) | (4) | (5) |
| Standard deviation | 0.1291 | 0.2268 | 0.1515 | 0.1268 | 0.1148 |

Weighted by average district-level 1992/93-2005/06 fifth-seventh grade enrollment.

Appendix Table 2. Descriptive Statistics

| | Reporting Cohort Shrinkage and 2+ Observations | | District-Years Deviating from State-Year Trend by more than +/- 0.2 | |
|---|---|---|---|---|
| | Mean (Std Dev) (1) | Sample Size (2) | Mean (Std Dev) (3) | Sample Size (4) |
| Cohort growth | -0.0132 | 175,078 | 0.0050 | 35,263 |
| | (0.0839) | | (0.2011) | |
| Kindergarten enrollment | 4379 | 175,078 | 809 | 35,263 |
| | (11,847) | | (1,541) | |
| ln Kindergarten enrollment | 6.74 | 175,078 | 5.47 | 35,263 |
| | (1.7507) | | (1.6011) | |
| Fraction free lunch eligible | 0.2799 | 149,649 | 0.2601 | 30,948 |
| | (0.2155) | | (0.2296) | |
| Fraction black | 0.1588 | 171,909 | 0.1412 | 34,816 |
| | (0.2088) | | (0.2504) | |
| Fraction Hispanic | 0.1501 | 171,909 | 0.1143 | 34,816 |
| | (0.2149) | | (0.1955) | |
| Fraction Asian | 0.0383 | 171,909 | 0.0229 | 34,816 |
| | (0.0722) | | (0.0419) | |
| Fraction Native American | 0.0117 | 171,909 | 0.0232 | 34,816 |
| | (0.0568) | | (0.1031) | |
| District Size (grade 5-7 enrollment) | 12,738 | 175,078 | 2,350 | 35,263 |
| | (33,881) | | (4,347) | |
| County-Level Unemployment Rate | 5.66 | 162,114 | 6.00 | 31,440 |
| | (2.6104) | | (2.7989) | |
| Number of Districts | 14,052 | | 9,832 | |

Weighted by average district-level 1992/93-2005/06 fifth-seventh grade enrollment.

20 Random Draws of 1000 Districts with Complete Cohort Size Reporting

| | Without ln teachers | With ln teachers | | Without ln teachers | With ln teachers |
|---|---|---|---|---|---|
| Round 1 | -0.0838 | -0.0926 | Round 11 | -0.0751 | -0.0769 |
| | (0.0179) | (0.0173) | | (0.0156) | (0.0181) |
| Round 2 | -0.0705 | -0.0768 | Round 12 | -0.1129 | -0.1169 |
| | (0.0107) | (0.0137) | | (0.0213) | (0.0258) |
| Round 3 | -0.1060 | -0.0925 | Round 13 | -0.0484 | -0.0612 |
| | (0.0241) | (0.0102) | | (0.0209) | (0.0238) |
| Round 4 | -0.0901 | -0.1010 | Round 14 | -0.1100 | -0.1092 |
| | (0.0133) | (0.0171) | | (0.0170) | (0.0190) |
| Round 5 | -0.0659 | -0.0521 | Round 15 | -0.0942 | -0.0980 |
| | (0.0253) | (0.0212) | | (0.0107) | (0.0110) |
| Round 6 | -0.0765 | -0.0852 | Round 16 | -0.1177 | -0.1164 |
| | (0.0164) | (0.0168) | | (0.0237) | (0.0178) |
| Round 7 | -0.0856 | -0.1082 | Round 17 | -0.0759 | -0.0886 |
| | (0.0095) | (0.0124) | | (0.0101) | (0.0129) |
| Round 8 | -0.0970 | -0.1015 | Round 18 | -0.1130 | -0.1039 |
| | (0.0160) | (0.0187) | | (0.0248) | (0.0153) |
| Round 9 | -0.0953 | -0.1051 | Round 19 | -0.0885 | -0.1014 |
| | (0.0141) | (0.0172) | | (0.0103) | (0.0121) |
| Round 10 | -0.0974 | -0.1070 | Round 20 | -0.0877 | -0.0951 |
| | (0.0112) | (0.0135) | | (0.0111) | (0.0130) |
| Weighted | Yes | Yes | | Yes | Yes |
| State-year FEs | Yes | Yes | | Yes | Yes |
| Additional controls | Yes | Yes | | Yes | Yes |
| Linear district trends | Yes | Yes | | Yes | Yes |
| Reporting in all years | Yes | Yes | | Yes | Yes |

Weighted by average district-level 1992/93-2005/06 fifth-seventh grade enrollment. All standard errors are clustered at the district level. Additional controls include fraction free lunch eligible, black, Hispanic, Asian, Native American, district size, and county-level unemployment as well as indicator variables for missing free lunch eligible, race, and unemployment information.

20 Random Draws of 1000 Districts with Complete Cohort Size Reporting

| | Base Specification | Specification with "flanking" cohorts | | | | Base Specification | Specification with "flanking" cohorts | | |
|---|---|---|---|---|---|---|---|---|---|
| Independent Variable: | ln enrollment | ln enrollment | ln enrollment following cohort | ln enrollment preceding cohort | | ln enrollment | ln enrollment | ln enrollment following cohort | ln enrollment preceding cohort |
| Round 1 | -0.0838 | -0.0994 | 0.0311 | 0.0195 | Round 11 | -0.0751 | -0.0676 | 0.0223 | 0.0194 |
| | (0.0179) | (0.0145) | (0.0124) | (0.0072) | | (0.0156) | (0.0166) | (0.0089) | (0.0057) |
| Round 2 | -0.0705 | -0.0793 | 0.0455 | 0.0093 | Round 12 | -0.1129 | -0.1211 | 0.0170 | 0.0158 |
| | (0.0107) | (0.0106) | (0.0133) | (0.0077) | | (0.0213) | (0.0238) | (0.0110) | (0.0123) |
| Round 3 | -0.1060 | -0.1123 | -0.0209 | 0.0182 | Round 13 | -0.0484 | -0.0534 | -0.1332 | 0.0707 |
| | (0.0241) | (0.0261) | (0.0424) | (0.0137) | | (0.0209) | (0.0283) | (0.1204) | (0.0435) |
| Round 4 | -0.0901 | -0.0895 | 0.0363 | 0.0156 | Round 14 | -0.1100 | -0.0992 | -0.0059 | 0.0138 |
| | (0.0133) | (0.0133) | (0.0177) | (0.0088) | | (0.0170) | (0.0178) | (0.0221) | (0.0076) |
| Round 5 | -0.0659 | -0.0606 | -0.1735 | 0.0505 | Round 15 | -0.0942 | -0.0887 | 0.0102 | 0.0132 |
| | (0.0253) | (0.0267) | (0.1198) | (0.0389) | | (0.0107) | (0.0155) | (0.0116) | (0.0083) |
| Round 6 | -0.0765 | -0.0823 | 0.0154 | 0.0229 | Round 16 | -0.1177 | -0.1234 | -0.0079 | 0.0066 |
| | (0.0164) | (0.0159) | (0.0104) | (0.0086) | | (0.0237) | (0.0232) | (0.0221) | (0.0084) |
| Round 7 | -0.0856 | -0.0798 | 0.0398 | 0.0211 | Round 17 | -0.0759 | -0.0740 | -0.0044 | 0.0163 |
| | (0.0095) | (0.0083) | (0.0129) | (0.0085) | | (0.0101) | (0.0121) | (0.0182) | (0.0087) |
| Round 8 | -0.0970 | -0.0968 | 0.0432 | 0.0063 | Round 18 | -0.1130 | -0.1027 | -0.0397 | 0.0145 |
| | (0.0160) | (0.0189) | (0.0080) | (0.0067) | | (0.0248) | (0.0251) | (0.0409) | (0.0098) |
| Round 9 | -0.0953 | -0.0969 | 0.0079 | 0.0312 | Round 19 | -0.0885 | -0.0992 | 0.0132 | 0.0190 |
| | (0.0141) | (0.0167) | (0.0166) | (0.0094) | | (0.0103) | (0.0116) | (0.0152) | (0.0075) |
| Round 10 | -0.0974 | -0.0842 | 0.0104 | 0.0367 | Round 20 | -0.0877 | -0.0940 | 0.0303 | 0.0135 |
| | (0.0112) | (0.0090) | (0.0120) | (0.0081) | | (0.0111) | (0.0129) | (0.0101) | (0.0071) |
| Weighted | Yes | Yes | | | | Yes | Yes | | |
| State-year FEs | Yes | Yes | | | | Yes | Yes | | |
| Additional controls | Yes | Yes | | | | Yes | Yes | | |
| Linear district trends | Yes | Yes | | | | Yes | Yes | | |
| Reporting in all years | Yes | Yes | | | | Yes | Yes | | |

Weighted by average district-level 1992/93-2005/06 fifth-seventh grade enrollment. All standard errors are clustered at the district level. Additional controls include fraction free lunch eligible, black, Hispanic, Asian, Native American, district size, and county-level unemployment as well as indicator variables for missing free lunch eligible, race, and unemployment information.