Schooling Attainment and the Introduction of Kindergartens into Public Schools[†]

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Abstract

During the late 1960s and 1970s, many states in the South and West began funding kindergarten for the first time, contributing to sizable and rapid increases in public school kindergarten enrollment over the period. In this paper, I use differences across states in the timing of these funding initiatives to examine the relationship between public preschool attendance and eventual schooling attainment. Using data from the Decennial Census, the October Current Population Survey, and the High School and Beyond Survey, I find that white children aged five near the time of the reforms were 20 percent less likely to have been retained in grade. For racial minorities, kindergarten attendance is estimated to reduce the probability of grade retention by between 30 and 40 percent. I find little evidence of an association between kindergarten attendance and high school dropout. Although effects on retention alone may not be large enough to justify funding of universal prekindergarten, the findings suggest some benefits to attending state-sponsored preschools, particularly for more disadvantaged students.

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I. Introduction

State commitment to preschool programs has grown considerably over the last several decades. Since the late 1970s, many states have begun sponsoring "prekindergarten" (pre-K) programs for four-year-olds, operated either through public schools or through public-private partnerships. Today, forty-three states have pre-K programs in operation, and state programs serve at least 900,000 children—as many as are served by the federal Head Start program.[1] While these programs generally target low-income children, four states to date—Georgia, New York, Oklahoma, and Florida—have passed legislation to make publicly funded pre-K more widely accessible.

The impact of preschool on children's cognitive and social development has long been a matter of debate. A variety of early childhood education programs, both experimental (e.g., the Perry Preschool Project) and non-experimental (e.g., Head Start), have consistently been shown to improve school achievement and even social and economic status during early adulthood (Barnett, 1998; Karoly, *et al.*, 1998; Currie, 2001). Though promising, the results of these studies do not generalize readily to state-sponsored preschool programs, which are primarily operated through public schools and funded at significantly lower levels.[2] With few exceptions (e.g., Gormley and Gayer, 2003), direct evaluation of these programs has not been very convincing, with studies suffering from small sample sizes, high attrition rates, and lack of adequate control groups (Gilliam and Zigler, 2000).

In this paper, I use an episode of rapid, policy-induced kindergarten construction thirty years ago to estimate the effect of attending a public school preschool on schooling

[1] This figure is based only on public school pre-K enrollment, as observed in the Common Core of Data. Since state-funded prekindergartens are sometimes operated outside of the public schools, it is a lower bound on the number of affected children.

[2] Average spending per student in state-sponsored pre-K programs was $2547 over FY 2001 and FY 2002 (Doherty, 2002). By contrast, Head Start cost the federal government approximately $6934 per participant in FY 2002 (U.S. Department of Health and Human Services, 2003).

attainment during adolescence and early adulthood. Between 1965 and 1975, many states—particularly in the South and West—expanded funding of kindergartens, either incorporating kindergarten into state school funding programs or making large appropriations to encourage kindergarten operation at the district level. Over the same decade, the percent of school districts operating kindergartens in these regions grew from approximately 30 to 84 percent, and the estimated public school kindergarten enrollment rate rose from 33 to nearly 78 percent, as shown in Figure 1. By the mid 1980s, the probability that a first grader had attended a public school kindergarten was about the same in all parts of the United States.

This historical episode offers a unique opportunity to examine the effects of attending a preschool operating within the public school system. First, in terms of funding levels and curriculum, the kindergartens of the 1960s and 1970s appear to have been quite similar to the prekindergartens of today, though targeted toward slightly older children. Further, while more four-year-olds today receive center-based care or education before school entry, today's prospective pre-K attendee is arguably similar to the "marginal" kindergarten attendee during the period of kindergarten expansion.

Second, the state funding initiatives generate credible identifying variation in public preschool attendance. Although seats in free kindergartens were not randomly assigned by the initiatives, increases in state aid generated abrupt and unexpectedly large across-cohort differences in the availability of seats—and consequently in the probability of having attended public school kindergarten—among children residing within the even same community. To the extent that schooling attainment of the average child would have otherwise been trending smoothly over time, the effect of kindergarten can be isolated by comparing cohorts aged five around the time that funding was introduced. Given differences across states in the exact date of funding adoption, I can also control for

unrestricted regional trends.[3] The funding events appear to provide for credible comparisons: most other correlates of schooling attainment, including measures of family background, school quality, and other large programs, appear to have been trending similarly across all states during the period of expansion.

To construct inter-cohort comparisons, I use data from the Common Core of Data, the Decennial Census, and the October Current Population Survey. For cohorts defined by state and year of school entry, I match measures of schooling attainment during adolescence and early adulthood to estimated kindergarten enrollment rates at age five. I consider two measures of schooling attainment explicitly targeted by policymakers during the period and often considered in evaluations of early childhood interventions: grade retention—approximated here with the proportion of a cohort "below" grade for age—and high school dropout.

Focusing on individuals born in the South or West and aged five between 1959 and 1978, I find that the expansion of kindergarten programs contributed to large reductions over time in grade retention. Two-stage least squares estimates imply that kindergarten attendance lowered grade retention rates of white children by at least 20 percent. Effects for racial minorities are larger—on the order of 30 to 40 percent—arguably due to their relatively high probability of receiving only informal care in the absence of kindergarten. Most reductions in retention appear to have been realized at young ages. Estimated effects on grade retention are similar across surveys, and are qualitatively similar to those obtained in across-state comparisons for the sophomore cohort from the 1980 High School and Beyond Survey, one of the few surveys during the relevant period to ask retrospective

[3] Berlinski and Sanguinetti (2002) use a similar research design, using a policy-induced change in the availability of kindergarten slots in Argentina to explore the effects of kindergarten attendance on school achievement.

questions on kindergarten attendance. However, similar comparisons across all three data sets reveal little impact of kindergarten on high school dropout behavior.

To the extent that these findings can be generalized, they suggest that foundation of prekindergartens in public schools might yield at least short-term benefits, particularly if targeted toward the most disadvantaged students. In fact, effects on retention are similar to those found for more intensive, targeted early childhood interventions (see Barnett, 1998). Though effects on grade retention may not necessarily be large enough to justify funding universal pre-K, reductions in grade repetition do represent a cost savings to state governments, which would otherwise have to allocate substantial resources toward re-instruction and remedial education programs.

II. The Kindergarten Program

A. The Curriculum

By the mid twentieth century, kindergartens in the United States were largely operated through public school systems and attended almost exclusively by five-year-old children in half-day programs. Although kindergartens were not effectively universal until the 1980s, public school kindergartens have generally been accessible to all children eligible by age and place of residence (National Education Association, 1962; Dombkowski, 2001). These characteristics make the kindergarten distinct from other well-known preschool programs, like Head Start, which limit enrollments to "at-risk" children and are often operated outside of a school district's jurisdiction.

Like these targeted programs, however, public school kindergartens have historically maintained a curriculum distinct from that of other primary school grades, focused more on

children's social development and less on academic training.[4] In the early 1960s, for example, promotion of social development was viewed by most teachers as the main value of kindergarten, and the typical schedule for a kindergarten class included "play time" with "color, paint, blocks, or other toys," songs, snacks, naps, outdoor play, and story time, but included little structured time learning to count and read (National Education Association, 1962). This view of kindergarten was shared by policymakers during the period of kindergarten expansion.[5]

To provide a further benchmark for interpretation, Table 1 draws out the similarities between this "traditional" kindergarten, as observed in the 1960s, and today's pre-kindergarten programs, as observed in the 1990s and early 2000s.[6] The typical kindergarten appears to have been of slightly lower quality. For example, kindergartens were much more likely to be half-day programs (94 percent versus 68 percent of prekindergartens), and the average kindergarten class had more students (24 versus 17 in the average pre-K class). However, estimated state funding per half-day student—approximately $1300 to $1400 (in constant 1996 dollars) is remarkably similar across programs.

If reflective of the pedagogical practices in the typical class, classroom layouts also suggest considerable similarities in curriculum across programs. Classroom arrangements in the kindergarten were fluid, with 97 percent of kindergartens having "movable tables and chairs"; similarly, 95 percent of prekindergartens have "equipment/materials [that] can be easily moved when necessary" (National Education Association, 1962; Georgia Applied

[4] This is much less the case today. The kindergarten curriculum has become increasingly academic, focused on acquisition of reading and arithmetic skills (West, Denton, and Germino-Hausken, 2000).
[5] This is evidenced both in the text of legislation and in state promotional materials about kindergarten. For example, the Kentucky Task Force on Kindergarten in 1975 wrote that the child who "accepts and adjusts to success and lack of success" was fulfilling the first "learning need" of the kindergarten (Jones, 1977, p. 35).
[6] The data come from a variety of sources, including surveys of teachers and schools conducted by the National Education Association (1962, 1969) and by the Council for School Performance (1998) for Georgia, the state with the largest and most accessible prekindergarten program in operation today.

Research Center, 1998). The activity centers historically most prevalent in the kindergarten classroom—a library, and places for blocks, art, home play, science, music, and dramatic play (see Figure 2)—are also present, if not more prevalent, in most prekindergarten classrooms.

The available evidence thus suggests that the expansion of public school kindergarten programs in the 1960s and 1970s is best interpreted as an investment in early childhood development and school readiness, rather than as an advancement of regular schooling to a lower starting age. Furthermore, though perhaps of slightly lower quality, the typical kindergarten in the 1960s appears similar to the average prekindergarten today. The rapid expansion of kindergarten programs during this period might therefore provide some insight into the potential effects of wider access to pre-K programs.

B. State Funding for Kindergarten

It is only in recent history that state governments have begun funding kindergarten programs. Kindergartens began as institutions separate from the public school system, funded largely through philanthropic organizations or private tuition. Through the persistent efforts of advocacy groups, kindergarten programs slowly became incorporated into urban school systems, at the same time gaining partial funding through local tax revenues (Beatty, 1995). By the late 1950s, kindergartens were operated in 70 percent of urban school systems, and over 80 percent of programs were publicly funded (Dean, 1960). Much of this public funding came through local tax revenues: by the mid 1960s, only 27 states in the U.S. made contributions for kindergarten (U.S. Department of Health, Education, and Welfare, 1963a, 1963b, 1967; Tanner and Tanner, 1973).

There were, however, remarkable changes over the next decade: between 1966 and 1975, 19 states—18 of which were in the southern or western parts of the country—began

funding kindergartens for the first time. Table 2 gives more detail on the timing of these and later changes.[7] The year of adoption gives the earliest reform that in principle made funds for kindergarten available to all districts in the state, whether through application or entitlement. In only a few instances did a mandate for local kindergarten programs accompany or follow closely upon establishment of state aid: response was at local discretion.[8] By the late 1970s, only U.S. two states—Mississippi and North Dakota—did not fund kindergarten programs.

State funding was provided through two types of regimes. In some cases, kindergarten was incorporated into a state's school foundation program, the vehicle through which the state has historically fulfilled the basic funding needs of school districts. Foundation program funding promised financial support for kindergartens on an equal basis with support for all other grades in a state's public school system: for each kindergarten class operated, a district was entitled to funding to offset instructional expenses like teachers' salaries. The remaining states channeled funds to districts through appropriations outside of the foundation program. This method of finance made state funding more vulnerable to budget cuts, though all states eventually made kindergarten a part of the basic state school program.

State funding should have loosened fiscal constraints on school districts, making it feasible for public school kindergartens to be operated where they would have otherwise been unaffordable. This is suggested by a strong cross-sectional relationship between funding and kindergarten enrollment before the period of expansion: of the 25 states estimated kindergarten enrollment rates of over 50 percent in the mid-1960s, only one

[7] Measures of state support for kindergarten were culled from a variety of sources. See the Data Appendix.
[8] These states include Maryland, Virginia, Oklahoma, West Virginia, and Georgia.

(Missouri) did not have state funding for kindergarten programs. The state contribution was particularly important in the South, where districts relied relatively heavily on state funding.[9]

A key role for state funding in the operation of kindergarten programs is suggested by Table 3, which gives kindergarten enrollment rates of five-year-olds in the South and West and the rest of the country, as observed in the 1960, 1970, and 1980 Decennial Censuses. In 1960, the average five-year-old in the South or West was about half as likely to be enrolled in kindergarten (public or otherwise) as the average five-year-old elsewhere in the country. By 1980, the gap in kindergarten enrollment across regions appears to have closed almost completely. Consistent with the timing of the funding initiatives, most of this convergence appears to have occurred during the 1970s and to have been driven by large increases in public kindergarten enrollment.

III. The State Funding Initiatives as a Quasi-Experiment

A. Motivations

The goal of this paper is to estimate the relationship between public school kindergarten attendance and schooling attainment. This relationship is not necessarily straightforward to identify. Seats in kindergarten programs were not randomly assigned by the initiatives, so kindergarten enrollment status will not be uncorrelated with eventual schooling outcomes. For example, when public school kindergartens were not widespread, kindergarten programs may have only been offered in relatively wealthy districts.[10] Simple comparisons of attendees to non-attendees should therefore yield upward-biased estimates.

[9] In the 1960s, over 50 percent of non-federal revenue for school operation in the average southern state was derived from state sources.

[10] Such positive selection of public kindergarten enrollees appears to be present. For example, in both the 1960 and 1970 Censuses, the average five year-old public kindergarten attendee in the South or West had a more highly educated mother and came from a smaller family than the average non-attendee, regardless of race.

A similar problem arises in evaluations of early childhood education programs more generally. Researchers have approached this problem in two ways. The first is direct experimentation: seats in some preschool programs, such as the Perry Preschool Project or the Carolina Abecedarian Program, have been randomly assigned, so that children with and without the program experience are on average identical (for summaries, see Barnett, 1998; Kaloly, *et al.*, 1998, Currie, 2001). Credible inferences about the effects of program participation can then be drawn from by comparing the experimental treatment and control groups.

The second methodological approach involves constructing comparisons from observational data for non-experimental programs. For example, in a series of papers, Currie and Thomas (1995, 1999) and Garces, *et al.* (2002) estimate the effect of Head Start on a variety of outcomes by comparing siblings, or by controlling for family fixed effects. Using a different quasi-experimental approach, Gormley and Gayer (2003) estimate the effect of pre-K attendance on test scores at age five by comparing children turning age four near the cutoff date for school entry: children born right after the cutoff were not eligible to attend pre-K, but were otherwise similar to children turning age four before the cutoff date.

The analytical approach taken in this paper falls in the second category. The observations in Section II provide the basis for the identification strategy. In particular, the funding initiatives generate variation in kindergarten attendance that potentially mirrors that of a controlled experiment. Given that kindergarten is generally attended for one year at age five, funding should have induced an abrupt across-cohort difference in kindergarten attendance within any given state. To the extent that families with children in subsequent academic cohorts were on average identical along dimensions that would bear of future schooling attainment, the outcomes of the older individuals can be used to estimate what

would have happened for the younger individuals in the absence of kindergarten. Even if this assumption is not satisfied, differences in the timing of the funding initiatives can also be used to control for underlying inter-cohort trends in schooling attainment. Similar comparisons have recently been used to estimate the return to education (see Card, 1999).

B. Empirical Framework

Like most evaluations of early childhood interventions, I will take a reduced-form estimation approach, starting with the simple linear regression model

(1) $$y_{ics} = \theta k_{ics} + f(x_{ics}) + \varepsilon_{ics},$$

where y_{ics} is an outcome for an individual *i* in birth cohort *c* in state *s*, k_{ics} is an indicator for whether the individual ever attended a public kindergarten program, and $f(x_{ics})$ is a function of observable characteristics, such as measures of family background. In this panel data context, the error term, ε_{ics}, is posited to have the structure $\varepsilon_{ics} = \alpha_c + \alpha_s + v_{ics}$, where α_c and α_s are cohort and state fixed effects, respectively, and v_{ics} is a mean zero error term that might be correlated with kindergarten attendance, k_{ics}. Since all outcomes in this paper will be expressed as indicator variables at the individual level, the parameter θ can be interpreted as the change in the probability with which an outcome occurs (i.e., a change in the probability of repeating a grade) associated with having attended kindergarten.

Because data on outcomes and kindergarten attendance are not available from the same data source, the model will be collapsed to cohort-state cell averages, the common unit of observation. In particular, if the effects of observables can be modeled linearly (i.e., $f(x_{ics}) = \beta' x_{ics}$), and random samples are available at different points in the life cycle of a given cohort, then θ is still identified in the model

(1') $$\bar{y}_{cs} = \theta \bar{k}_{cs} + \beta' \bar{x}_{cs} + \alpha_c + \alpha_s + v_{cs},$$

where $\bar{y}_{cs}$ is an average outcome (e.g., grade retention rate) and $\bar{x}_{cs}$ the average observable characteristics for cohort *c* in state *s*, and $\bar{k}_{cs}$ is the fraction of the cohort that attended kindergarten, estimated as of age five.[11] The error term v_{cs} now represents unobservable correlates of *average* outcomes, which may be related to the kindergarten attendance rate.

As discussed above, there is good reason to believe that k_{ics}, and thereby $\bar{k}_{cs}$, is indeed correlated with these unobservable determinants of schooling attainment. Formally, we should be concerned that $E\left[\bar{k}_{cs} v_{cs} \mid \bar{x}_{cs}, \alpha_c, \alpha_s\right] \neq 0$, in which case ordinary least squares (OLS) estimates of (1') will yield biased and inconsistent estimates of the effect of kindergarten attendance. Instead of estimating equation (1') via OLS, I therefore construct an instrument for $\bar{k}_{cs}$ using the state funding initiatives, and estimate the model using two-stage least squares (2SLS).

Taking the cohort-state cell average as the unit of observation, the first-stage relationship is given by the simple differences-in-differences (DD) model

(2) $$\bar{k}_{cs} = \delta Z_{cs} + \lambda' \bar{x}_{cs} + \gamma_c + \gamma_s + \omega_{cs},$$

where the instrument is given by $Z_{cs} = 1\left[c + 5 \geq t_s^*\right]$, with t_s^* representing the year of adoption. The variable Z_{cs} is thus equal to one if cohort *c* would have been aged five after the funding initiative had been passed. The parameter δ is the effect of state funding availability on the kindergarten attendance rate, holding constant observable characteristics

[11] By averaging across all individual-level variation within group (e.g., the state-cohort cell), standard errors of regression estimates are less likely to be understated (Moulton, 1986). However, the error terms might still be positively correlated across cohorts, tending toward overstatement of standard errors (Bertrand, Duflo, and Mullainathan, 2002). In all regressions presented in the paper, I therefore estimate standard errors allowing for correlation across all cells within the same state, separately in the pre-funding and post-funding periods. When I "cluster" on state alone (allowing for correlation in error terms across pre and post cohorts within the same state), the standard errors are slightly larger, though not so large as to alter the conclusions of the paper.

($\bar{x}_{cs}$), and what would have otherwise been expected in the state during the period of interest (captured in the state fixed effect, γ_s) and given trends common to all states under consideration (captured in the cohort fixed effect, γ_c). By the above reasoning, I expect to find that $\delta > 0$: state funding should have raised kindergarten attendance rates above and beyond what would have otherwise been expected.

The model imposes a straightforward exclusion restriction: the introduction of state funding for kindergartens cannot otherwise have a direct effect on schooling outcomes, or $E\left[Z_{cs} v_{cs} \mid \bar{x}_{cs}, \alpha_c, \alpha_s\right] = 0$. Although I can never directly test whether program enactment is independent of unobserved shocks to average schooling attainment, I am able to test whether program enactment is correlated with inter-cohort changes in observable characteristics—an exercise that I perform in Section VIII. If the exclusion restriction holds, it should be the case that observable characteristics are not strong predictors of Z_{cs}, or vice versa. This exercise shows that the introduction of state funding is not highly correlated with measures of family background and school quality that would be expected to exert a direct influence on schooling attainment when multiple cohorts can be observed.

IV. Data Sources

A. Sample Selection

The data requirements for a study of this kind are considerable. Ideally, I would have a data set where I could observe the state in which an individual entered school and his exact date of birth (together used to define the year an individual would have entered kindergarten), whether the individual in fact attended public school kindergarten, measures of schooling attainment, and measures of background, such as family circumstances and

school resources. Further, since the research design rests on across-cohort and across-state comparisons, such data would needed for individuals in *different* cohorts aged five around the time of the funding initiatives.

Unfortunately, the ideal data for this analysis do not exist. Instead, I merge measures of average schooling attainment during adolescence and early adulthood to estimated kindergarten attendance rates for cohorts defined by state and year of school entry. I restrict attention to individuals born between 1954 and 1973, who would have been aged five—the kindergarten entrance age—between the fall of 1959 and 1978. In part, this particular choice of cohorts derives from restrictions imposed by the Census, my primary source of data on schooling outcomes (see below). However, it has the benefit that estimates will be based on the cohorts directly affected by funding initiatives, not those entering school long before or long after they were passed: as was shown in Table 2, the vast majority of states (16 of 24) began state funding for kindergarten between 1966 and 1973. This should help to make the estimates more believable, given the simple first-stage specification, and the fact that the full effect of the state funding initiatives is realized within three years of passage, as shown below.

There are several other aspects of the analysis worth mentioning. First, so that cohort effects will be identified using comparable states, I restrict the analysis to states in the southern and western parts of the United States, where the majority of the funding initiatives for kindergarten were passed, as shown in Table 2.[12] I will also conduct the analysis separately by race (white and nonwhite)—a fixed, exogenous characteristic that has

[12] The South is defined as states in the South Atlantic, East South Central and West South Central census divisions, plus Missouri, historically considered a border state. The West is defined as states in the Mountain and Pacific census divisions. Together, these regions identify 31 states, 21 of which passed funding initiatives affecting the 1954 to 1973 birth cohorts. Restricting attention to this region effectively eliminates only New Hampshire from the analysis. Since no states passed funding initiatives between 1960 and 1965 (Tanner and Tanner, 1973), the 1959 to 1965 school entry cohorts in "control" states were unlikely to have been affected by similar kindergarten expansions.

historically been highly correlated with socio-economic status.[13] In practice, this distinction is important for interpretation of the finding, as there were differences by race in the activities for which public school kindergarten may have substituted. Finally, the data are structured such that I observe different cohorts at different points in their school careers, and sometimes more than once, as discussed below. Because the measures of attainment have strong gradients in age, the model in (1') is estimated with age fixed effects.

B. Data on Schooling Attainment and Kindergarten Attendance

I consider two measures of a cohort's schooling attainment: rates of grade retention and high school dropout, calculated from the Decennial Census and the October Current Population Survey (CPS).[14] For several reasons, I view the Census as my primary data source. First, the Census has relatively large sample sizes, so dropout and retention rates for narrowly defined cohorts can be more precisely estimated. Second, state of birth is observed in the Census, but not in the CPS. State of birth is more likely to be randomly assigned than current state of residence. By observing state of birth, I am also able to draw samples from the same underlying population at different points in the lifecycle. This is impossible when only current state of residence can be observed, as in the CPS, and migration rates are high. Finally, although the CPS is annual, all states are not individually identified in the survey until 1977.[15] Despite its drawbacks, the CPS is nonetheless included in the analysis to provide a

[13] In practice, it would be difficult to stratify the data in any other way for a cohort-level analysis, as none of the data sets contain retrospective information on family income or poverty status when individuals were young.

[14] I use the 1969 to 1997 October Current Population School Enrollment Supplements, and the following Decennial Census Public-Use Microdata Samples: 1% samples from the Form 2 (15%) state, metro, and neighborhood files in 1970; and 5% samples in 1980, 1990, and 2000. See Data Appendix.

[15] From 1968 to 1976, state of residence is released in the October CPS for the following states in the South and West: California, Florida, Texas, and Washington, D.C. (1968-1976); North Carolina (1973-1976); and Georgia, Kentucky, Louisiana, Maryland, Missouri, Oregon, Tennessee, and West Virginia (1968-1972).

second set of estimates, and to provide more information on affected cohorts at young ages, when the effect of kindergarten on retention should have been realized.

To measure grade retention, I construct a proxy—the fraction of a cohort that is "below grade" given its age—using information on age, school enrollment status, and highest grade attended.[16] Provided that they entered school in the academic year when first eligible, children forced to repeat a grade or held back will be old relative to their classmates, or enrolled in a grade below other members of their school entry cohort (Shepard and Smith, 1989). This fraction is estimated for 6 to 15-year-olds in the 1970 and 1980 Censuses and the 1968 through 1988 October CPS School Enrollment Supplements.[17] As discussed above, the resulting series represent cohorts that would have been aged five between the fall of 1959 and the fall of 1978.[18] Because of a change in coding of the education variable in the Census in 1990, this series cannot be extended to more recent cohorts.[19]

I then define the high school dropout rate as the fraction of a cohort not currently enrolled and school and without a high school degree. I estimate the dropout rate for 16 to 35-year-olds in the 1980 through 2000 Decennial Censuses, and for 16 to 25-year-olds in the 1970 through 1998 October CPS files. Because quarter of birth is no longer recorded in the 1990 and 2000 Censuses, age is measured as of April 1 in this survey. Once again, the resulting series is based on individuals who would have been aged five between 1959 and 1978.

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[16] Administrative data on grade retention are not widely available during the period of interest. In a validation study of this proxy using the 1992, 1995, and 1999 October Current Population Surveys, Cascio (2003) shows that it is highly correlated with actual retention experiences.

[17] Age is measured as of October in both surveys, and an individual is coded as a repeater if age-highest grade attended>5. To calculate age in the Census, I use information on age of April 1 and quarter of birth.

[18] In the Census, school enrollment information is collected during the spring of the academic year.

[19] The schooling measure in the 1990 Census groups together grades 1 to 4 and 5 to 8. It also pertains to educational attainment, not highest grade attended.

To construct kindergarten enrollment rates for these cohorts, I use enrollment aggregates collected by the U.S. Office of Education (OE) in *Fall Statistics of Public Schools* and the *Biennial Survey of Education*. I estimate a cohort's kindergarten attendance rate with the ratio of public school kindergarten enrollment to first grade enrollment in the academic year when the cohort is age five.[20] Because the OE does not break out enrollment by age group or race, this series might misrepresent the true public kindergarten attendance rate of a given cohort. However, the OE data permit me to construct kindergarten enrollment rates on annual basis and for the earliest cohorts in this study, which would be impossible using the Census or October CPS. The OE series appears to be fairly representative: for a subset of cohorts where both measures can be observed, it generates first stage estimates quite similar to those derived from public kindergarten enrollment rates of five-year-olds in the October CPS, as shown below.

V. The First-Stage: State Funding and Kindergarten Attendance

A. DD Models

Table 4 presents estimates of the first-stage DD model given in equation (2), using the OE series on public kindergarten enrollment and data on public kindergarten enrollment of five-year-olds from the October CPS.[21] Column (1) gives a first-stage estimate for the cohorts of interest. Consistent with expectations, state funding had a large impact on kindergarten enrollment. The estimate implies that children aged five after kindergartens were funded were about 39.5 percentage points more likely to have attended kindergarten.

[20] Where missing (e.g., in the even years for the *Biennial Survey of Education*), the public kindergarten enrollment ratios are linearly interpolated. A nearly identical series results when kindergarten enrollment aggregates are normalized with the number of live births in the state five years prior, taken from *Vital Statistics*. It is also similar to a series constructed by normalizing kindergarten enrollment by first grade enrollment in the state the next year (a more "cohort-specific" enrollment rate).

[21] These estimates drop years in which enrollment rates are linearly interpolated.

As mentioned above, OE kindergarten enrollment series might mismeasure the fraction of a cohort attending kindergarten. The remaining columns of Table 4 therefore present a comparison of first stage estimates from the October CPS and from the OE series, when restricted to cover the same cohorts. The October CPS public enrollment series gives the fraction of the cohort enrolled in public kindergarten at age five, estimated separated by race (white and nonwhite). By matching children to their mothers in the CPS, I have also constructed measures of average family background (as of age five) by cohort to investigate the robustness of the first-stage relationship to observable characteristics.[22] Like the OE series, the October CPS enrollment series represents cohorts born between 1963 and 1982, or aged five between 1968 and 1987.[23]

Despite a slightly different definition of public kindergartens, results from the October CPS are quite comparable to those from the OE enrollment series.[24] For both samples, the CPS-based estimate of the first stage (column (3)) is very close in magnitude to the OE-based estimate for the same cohorts (column (2)). The estimate is little changed by the inclusion of family background controls in the model (panel B). State funding adoption therefore appears largely uncorrelated with the family background characteristics of prospective school entrants, suggesting the plausibility of the instrument.

Although there are similarities in these overall results, the OE series appears to be more representative of the enrollment experiences of white children. In fact, in the baseline specification (Panel A), results from the CPS are identical to those OE series when restricted to cover the same cohorts (column (2)). The first-stage estimate for nonwhites is about 25%

[22] These measures include the fraction of children in female-headed households, average maternal education, average maternal employment, and average maternal age.
[23] The first year of the October CPS is 1968. Five year-olds in October 1968 would have been born either in during the first three quarters of 1963 or the last quarter of 1962.
[24] Public kindergartens in the CPS encompass both public school kindergartens and other publicly-funded kindergartens that might be operated outside of schools.

smaller (0.322 versus 0.448). Lacking a superior alternative, I nonetheless merge the OE enrollment series to outcomes for both races. Effects of public school kindergarten on outcomes estimated for racial minorities are therefore likely to be slightly understated.

B. Event-Study Models

While suggestive, the DD models for the first stage might not fully convince the reader of the credibility of the identification strategy. Consider the extension of model (2) to an event-study framework:

$$(2') \qquad \bar{k}_{cs} = \sum_{j=-m}^{M} \delta^j Z_{cs}^j + \lambda' \bar{x}_{cs} + \gamma_c + \gamma_s + \omega_{cs} ,$$

where $Z_{cs}^j = 1\left[c + 5 = t_s^* + j\right]$, and all other terms are as previously defined. Estimates of δ^j, where $j = -m, \ldots, M$, give the effect of state aid on kindergarten attendance (provision) j periods after it is first available, holding constant what would have otherwise been expected in that state during the period of interest and given trends common to all states under observation. If funding availability were truly the force behind observed trends, it should be the case that $\delta^j = 0 \; \forall \; j < 0$, or that kindergarten enrollment did not diverge from trend in the years prior to funding enactment.[25]

Figure 2 shows estimates of δ^j, $j = -7, \ldots, 7$, from model (2') based on the OE series on public school kindergarten enrollment and provision.[26] Dashed lines connect the 95 percent confidence intervals for the point estimates. Consistent with the estimates presented in Table 4, the public school kindergarten enrollment rate was significantly higher than

[25] The estimation strategy is similar to that employed in Jacobson, LaLonde, and Sullivan (1993). Model (2) is essentially a restricted version of model (2').

[26] I estimate the model using observations from southern and border states only and setting $m = 7$ and $M = 7$ For the purposes of identification, I further define $Z_{cs}^{-7} = 1\left[c + 5 \leq t_s^* - 7\right]$ and $Z_{cs}^{7} = 1\left[c + 5 \geq t_s^* + 7\right]$; since the relative year indicators would otherwise be exhaustive, I also omit the indicator for the period immediately preceding the initiative, Z_{cs}^{-1}, or set $\delta^{-1} = 0$.

otherwise would have been expected in the years immediately following passage of the funding initiative. Most of the effect of funding occurs within several years of its availability. During the first year of the initiative ($j = 0$), the kindergarten enrollment rate is 16 percentage points higher than would have been expected. By the second year the initiative has been in place ($j = 1$), the effect of funding on kindergarten enrollment has been near fully realized; point estimates range between 0.3 and 0.4 for $j \geq 2$. Using the above test, funding appears to be the true force behind observed trends: all coefficients for $j \geq 0$ are significantly different from zero, while nearly all coefficients for $j < 0$ are close to zero and magnitude and not statistically significant.

To further investigate whether the OE data are representative, Figure 3 does the same exercise public kindergarten enrollment in the October CPS and for the same cohorts in the OE.[27] Consistent with results in Table 4, the figure shows a remarkable similarity between CPS and OE series in the time path of public kindergarten enrollment rates entering school around the time of the reforms. Also consistent with results discussed above, the administrative series appears to overstate the effect of state funding on kindergarten enrollment for minorities, though a first-stage effect for minorities is still present.

VI. The Reduced-Form: State Funding and Schooling Attainment

A. Grade Retention

Table 5 gives estimates of reduced form relationship between grade retention and state funding availability in the Census and October CPS. The baseline model (Panel A) controls for state and cohort fixed effects, age fixed effects, and fixed effects for month of

[27] All models are estimated with controls for family background, though they have little impact on the results.

the state school entry cutoff date when the cohort is age five. In related work (Cascio, 2003), I show that measurement error in the grade retention proxy is highly correlated with the timing of school entry laws, which give the date by which entering kindergartners must reach age five.[28] To the extent that states changed their school entry laws while kindergartens were being introduced (e.g., to smooth the transition), effects of school entry laws will not be absorbed by state fixed effects. The cutoff month fixed effects are thus included in the model to reduce the chances of attributing to kindergarten attendance what are in fact the effects of changes in school entry legislation.[29] Coefficients on these fixed effects are identified from states that changed their entry laws during the period.

Estimates of these baseline specifications show that state funding was associated with large and statistically significant reductions in the fraction of children below grade. For the pooled sample (ages 6 to 15), state funding was associated with 2 percentage point reduction in grade retention for whites in both the Census and the October CPS. For the nonwhite subsample, state funding was associated with a 2 percentage point reduction in grade retention in the Census, and a larger, statistically significant 4 percentage point reduction in retention in the October CPS. Given average proportions of children below grade (mean of dependent variable, Panel D), these effects imply that white children aged five after state funding was introduced were 10 percent less likely to have been retained. For minorities, the effect ranges between 7 and 14 percent.

[28] In particular, in states with relatively early school entry laws, such as those in August or September, the proportion of students classified as below grade given age in October is relatively high, or "false positives" are relatively more common. By contrast, in states with relatively late school entry dates, such as those in December or January, the proportion of students classified as below grade is relatively low, or "false negatives" are relatively more common.

[29] This would happen if states passing funding initiatives simultaneously moved the school entry cutoff date to later in the academic year. This appears to have happened in a few instances (see Cascio, 2003). Consistent with this, results are slightly larger in magnitude if the school entry cutoff fixed effects are not included in the model.

If kindergarten expansion is ultimately responsible for these effects, one might argue that they should have occurred early in the school career.[30] Unfortunately, the below grade measure is cumulative, providing no direct evidence on *when* retention took place: first grade repeaters are observationally equivalent to later repeaters. To investigate whether declines in grade retention were realized at young ages, columns (5) and (6) therefore present estimates of the model when the sample is restricted to 6 to 10 year olds in the October CPS.[31] Estimates are slightly smaller in magnitude for whites, but slightly larger and still significant for the nonwhite sample.

Most of these results are upheld, if not strengthened, by the inclusion of additional controls in the model. Panel B adds several controls for the cohort's family background, including average maternal educational attainment, average maternal age at birth, and fraction of households headed by females. For both the CPS and Census analyses, these average family background characteristics are estimated for cohorts when observed between the ages of zero and nine in the 1960 and 1970 Census.[32] Panel C then adds log cohort size as a proxy measure of school quality: larger cohorts may strain existing school resources, yielding fewer effective resources per student (Card and Lemieux, 2001). A comparison of results in Panel A to those in Panel C show that these controls largely serve to absorb residual variation: point estimates in the fully saturated specification are quite similar to the fixed effects only model, though in general more precisely estimated.

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[30] Available data on grade-specific retention suggest that most repetition during the period took place in first grade and, to a somewhat lesser extent, in seventh, eighth, or ninth grade (Rose, *et al.*, 1983; Shepard and Smith, 1989).

[31] Limiting the analysis to this sample has the additional benefit of circumventing some of the problems presented by migration in the October CPS: children are arguably more likely to be in the same state in which they entered school when relatively young. However, due to the unbalanced nature of the CPS panel, estimates are more representative of later cohorts and states which are individually identified in the CPS prior to 1977.

[32] Additional controls, like family size or income, can be constructed, but they are more likely to be contaminated by differences across cohorts in the point in the lifecycle in which they are measured.

An event study model, like that estimated above for public kindergarten enrollment, provides another way of viewing this reduced form relationship. Figure 5 plots the coefficients on the relative year indicators $\{Z_{cs}^{j} \; j = -7, \ldots, 7\}$ from model (2′), where the dependent variable is now fraction below grade (measured in the Census).[33] Coefficient estimates from this exercise are imprecise, suggesting the advantages of pooling across cohorts in the pre-funding and post-funding periods. However, the point estimates themselves are somewhat reassuring. For the white subsample (Panel A), all coefficient estimates in the post-funding period $j \geq 0$ are negative, while most pre-reform are closer to zero and slightly positive. Estimates for the nonwhite sample are less precise, but appear to be broadly characterized by similar observations.

B. High School Dropout

Table 6 presents reduced-form estimates for the effect of state funding on high school dropout rates, with specifications similar to those in Table 6. Unlike the models above, however, school entry cutoff month fixed effects are not included as controls. More important for understanding trends in high school dropout—particularly at the youngest ages—are state compulsory schooling laws, which give the earliest age at which an individual can leave school. If states raised compulsory schooling ages from 16 to 17 over the period of interest, I will be more likely to observe a 17 year-old enrolled in school, though there may be no effect on the dropout rate of the cohort when observed at a later age. However, an increase in the compulsory schooling age to 18 might have a more permanent impact.[34]

[33] All covariates (e.g., those included in Panel C of Table 5) are included in the model. As with all other models in the paper, regressions are weighted by cell size of the dependent variable and standard errors are estimated allowing for correlation in error terms within state-reform period (pre, post) groups.

[34] Most individuals will have not completed high school by age 17; some may have completed high school by age 18.

As was the case above, to the extent that compulsory schooling laws change over time, state fixed effects will not be sufficient to capture their effects on high school dropout behavior, and reductions in high school dropout may be spuriously attributed to increases in kindergarten attendance. While I would ideally include fixed effects for compulsory schooling ages in the model, I have unfortunately not yet been able to date all changes in compulsory schooling regimes for the cohorts of interest. However, the available evidence suggests that were few changes over the relevant period, and most were confined to small states, which receive less weight in the analysis.[35]

Even if slightly overstated, the reduced-form effects of state kindergarten funding on high school dropout are small and for the most part not distinguishable from zero, as shown in Table 6. Columns (1) through (4) show results from the Census. For the sample of white 16 to 35 year-olds (column (1)), cohorts entering school after kindergartens were funded were a significant 0.8 percentage points (6 percent) less likely to be high school dropouts (Panel A). When the model is estimated with family background controls, this effect falls to 0.4 percentage points, or 3 percent, and can no longer be statistically distinguished from zero. Reduced-form results for nonwhite 16 to 35 year-olds (column (2)) are not significant in any of the specifications, and are even smaller in magnitude: state funding is associated with reductions in high school dropout of between 0.2 and 0.4 percentage points (1 to 2 percent). Similar results arise when the model is estimated using only 16 to 25 year-olds in the Census sample (columns (3) and (4)).

[35] Between 1970 and 1980—when the earliest cohorts under investigation would have been age 16—only two Arkansas and Tennessee changed their compulsory schooling ages (Angrist and Krueger, 1991). In both cases, ages were reduced, not increased. Between 1970 and 1997—a period encompassing the remaining cohorts—only five more states in the sample (Montana, New Mexico, South Carolina, Tennessee, and Virginia) raised their compulsory schooling ages. These changes may have occurred after 1990, and therefore not be relevant. With the exception of Virginia, states adopting compulsory schooling ages of 18 (Montana, New Mexico) are small.

Though possibly contaminated by migration, estimates from the October CPS for the same age group, presented in columns (5) and (6) of Table 6, seems to echo results in the Census. For white 16-25 year-olds in the CPS, state funding is estimated to reduce the probability of high school dropout by between 7 and 10 percent (1.3 and 1.8 percentage points, respectively). For nonwhites in this age group (column (6)), state funding was associated with declines in high school dropout between 0.5 and 3 percent (0.1 and 0.6 percentage points, respectively).

All in all, these results suggest that kindergarten expansion may have had a very modest impact on the high school dropout rates of whites, but a negligible impact on the high school dropout behavior of minorities. Graphical evidence on this point is provided by Figure 6, which shows estimates from the event study specification (2′) for high school dropout rates. For whites, all coefficient estimates in the post-funding period ($j \geq 0$) are negative, though none are significant. Most pre-reform estimates are close to zero or slightly positive. For nonwhites, the coefficients on the relative year indicators are very noisy, suggesting no relationship between state funding and high school dropout behavior. Both observations seem quite consistent with the reduced-form DD results for dropout presented above.

VII. The Effect of Kindergarten Attendance on Schooling Attainment

A. Estimates from the Census and October CPS

These reduced-form results suggest that state expansions in kindergarten programs may have had fairly large impacts on grade retention rates, particularly for minorities. To quantify the magnitudes of these effects, Table 7 gives the 2SLS estimates of the effect of kindergarten attendance on grade retention. I use the simple DD specification for the first

stage. Although observable family background characteristics and cohort size appear to exert little influence on either the first stage or reduced-form regression results, all specifications include these controls, in addition to the relevant fixed effects. (Relevant reduced-form estimates are found in Panel C of Tables 5, and are presented again for reference in Panel C.)

The 2SLS estimates suggest that public kindergarten attendance did in fact have large impacts on retention. Regardless of race, estimates from the Census (columns (1) and (2)) imply that attending a public school kindergarten reduced the probability of grade retention by about 21 percent (-0.034/0.165 for whites and -0.052/0.239 for nonwhites). Estimates from the October CPS for the same age group (columns (3) and (4)) are of about the same order of magnitude for whites (a 19 percent reduction in reduction, or -0.037/0.199) and larger for the nonwhite sample (a 34 percent reduction in retention, or -0.098/0.281). Estimates for nonwhites are even stronger for cohorts when observed at younger ages in the CPS, as predicted above.

The results for high school dropout, shown in Table 8, are also broadly consistent with expectations. For minorities, 2SLS uncovers no evidence of a relationship: in each specification (columns (2), (4), and (6)), kindergarten is estimated to reduce the probability of high school dropout by less than 1 percentage point (less than 5 percent), although estimates are not very precise. For whites, point estimates are suggestive of larger effects—particularly in the CPS—but I am hesitant to draw any strong conclusions due to potential non-random migration. This conclusion is also not supported by estimates from another survey available for the period of interest, discussed below.

B. Estimates from the High School and Beyond Survey

There have been some drawbacks to the analysis so far. In particular, I have not been able to follow the same individuals over time, and I have only controlled for a limited set of potential confounding variables. I therefore present an alternative set of estimates from the High School and Beyond (HSB) Survey, a longitudinal NCES survey of sophomores in 1980. The HSB is one of the few available surveys to represent individuals entering school during the period of interest, and to collect retrospective information on kindergarten attendance and grade retention, as well as an extensive list of variables related to family background and school quality.[36] If "on grade," HSB respondents would have entered kindergarten in the fall of 1969, by which time less than half of the funding initiatives had been passed (see Table 2).

The HSB has several major drawbacks, which is why it has not been central to the analysis thus far. First, the survey represents essentially only one cohort—sophomores in spring of 1980. This "cohort" is furthermore not defined in the same way as I have defined cohort in this paper: because of grade retention, or delayed or accelerated school entry, individuals who were sophomores in 1980 may have been aged five in different academic years. Different entry cohorts observed as sophomores are therefore likely to have very different backgrounds and preferences, precluding an across-cohort comparison. Furthermore, the sample is selected on individuals who remained in school long enough to attend tenth grade, and state of birth is also not observed. The latter is not as critical of a

[36] School quality related variables include measures of the racial composition of schools attended at different points in the school career (first, sixth, and ninth grades). This is useful since school desegregation is occurring in the South when the cohorts of interest were in school. Controlling for the racial composition of schools attended reduces the magnitude of the estimates, particularly for minorities, and makes them slightly less precise. Desegregation is discussed further in the next section.

problem as in the CPS, however, because information on kindergarten attendance and schooling outcomes on the same individual can be observed.

Bearing all of this in mind, the HSB can still be used to explore the relationship between kindergarten attendance and schooling attainment. To do this, I make an across-state comparison, stratifying southern and western states into two groups: those where funding initiatives were passed by the fall of 1969 ("early adoption" states), and those where funding initiatives were passed later. Using the early adoption indicator as an instrument, I then compare the kindergarten attendance rates and schooling outcomes for individuals currently residing in these two groups of states. Because "on grade" sophomores—the majority of the sample—would have entered kindergarten in the fall of 1969, as mentioned above, I choose 1969 as the threshold for determination of early adoption status.

The results of this estimation strategy for grade repetition are shown in the last two columns of Table 7.[37] As background, Panel D gives the first-stage estimates from the early adoption instrument: white sophomores residing in early adoption states in 1980 were about 20 percentage points more likely to have attended kindergarten than their counterparts living elsewhere in the South or West. For the nonwhite subsample, the effect of funding in this across-state comparison is smaller (15.4 percentage points), which appears consist with results from the October CPS presented in Table 4. When normalized by race-specific enrollment rates in the region (not shown in the table), these effects imply that state funding was associated with 20 to 25 percent increases in kindergarten enrollment.[38]

The 2SLS estimates for retention from the HSB, presented in Panel B, are larger in magnitude than those from the Census and CPS, though precisely estimated only for the

[37] All regressions control for a number of family background and school characteristics, listed in the notes to Table 7. All regressions are also weighted by HSB panel weights, since the grade repetition questions were asked in the First-Year Follow-Up Survey (in 1982). Standard errors are clustered on state.
[38] The retrospective kindergarten enrollment rate for whites is 78.8 percent and for non-whites is 78.6 percent.

white sample. Results are in fact implausibly large (reductions in retention are on the order 80 percent), suggesting that the comparisons are contaminated by differences in unobservable characteristics across states. (This is confirmed in the next section.) They are, however, broadly consistent with those found in the Census and CPS, and thereby suggestive of an effect of kindergarten attendance on retention.

The last two columns of Table 8 repeat this exercise for high school dropout, here measured with an indicator for whether a respondent was recorded as a dropout in the first-year follow-up survey in 1982. This measure of dropout is therefore not completely comparable to that used in the CPS and Census, and might misrepresent an individual's eventual schooling attainment. This measure does yield results quite different from those above: for the white sample, 2SLS estimates suggest that kindergarten attendance is positively associated with high school dropout rates, while the point estimate for nonwhites is large and negative. In both cases, estimates are very imprecise. This makes it difficult to draw any strong conclusions, except to say that the HSB does not provide support for hypothesis that kindergarten reduces high school dropout among whites, as suggested by the CPS-based analysis.

C. Comparison of 2SLS and OLS Estimates

The 2SLS estimates of the effect of kindergarten attendance, particularly for grade retention, are in general as large in magnitude, if not larger than their OLS counterparts (Table 7, comparison of Panels A and B). This finding is surprising given my earlier contention that public kindergarten attendees were likely to have been positively selected in the absence of funding, leading OLS coefficients to be upward biased in magnitude.

There are two potential explanations for this regularity. First, OLS estimates might in fact be attenuated on account of measurement error in the grade retention proxy. Measurement error in a binary dependent variable will attenuate regression coefficients, a result which carries over when the model presented in this paper aggregated into cell means.[39] Cascio (2003) estimates that OLS estimates could be attenuated by more than 30 percent when below grade is used as a dependent variable. Although this validation study corresponds to later cohorts, the attenuation factor appears to be the similar across races.

While attenuation bias can potentially explain the similarity in OLS and 2SLS estimates for whites, it cannot fully explain the large differences in estimates observed for nonwhites. A second potential explanation for the difference is that children drawn into public school kindergartens, particularly within the minority population, were likely to benefit disproportionately from the program. This "local average treatment effect" interpretation of the findings (Angrist, Imbens, and Rubin, 1996) seems reasonable, particularly in light of the activities for which kindergarten would have substituted, as discussed below.

VIII. Robustness of the findings

Collectively, the Census, CPS, and HSB have helped to build a fairly consistent story about the effect of kindergarten on schooling attainment. Results from all three data sources indicate that attending kindergarten reduced the probability of being retained in grade. Results from the CPS suggest that effects are larger for minorities. This is also suggested by the first-stage estimates for nonwhites in Table 4: while still strong in a statistical sense, the

[39] The attenuation factor is by $1 - p_0 - p_1$, where p_0 is the "false negative" rate and p_1 is the "false positive" rate (Hausman, 2001).

estimated impact of state funding on public kindergarten enrollment for nonwhites appears to be smaller in magnitude than that implied by the OE series.

The credibility of these conclusions rests on the assumption that unobservable determinants of schooling attainment were trending similarly across all states under investigation around the time that the funding initiatives were passed. If policymakers decided to fund kindergartens because of some perceived change in cohort quality, or because to an unusually politically active generation of parents, we should be concerned that cohorts entering school after the initiatives were somehow different than their immediate predecessors in ways that might matter for their educational attainment. Although there is evidence of that the exact year in which kindergartens were funded was somewhat idiosyncratic (Forgione, 1977), the remainder of this section examines the relationship of state funding for kindergartens to family background, school quality, and other programs to which children might have been exposed.

A. State Funding, Family Background, and School Quality

As earlier discussed, the correlation between state funding and *observable* cohort characteristics provides an indirect test of the exclusion restriction. If state funding appears to have little predictive power with respect to observables, we might be inclined to believe that it also has little predictive power with respect to *unobservable* characteristics. Table 9 therefore presents estimates of model (2) for the control variables used in the Census and CPS analyses, and estimates of the association between "early adoption" and background characteristics in the HSB (columns (5) and (6)). For comparison, columns (3) and (4) give the coefficient from a regression on of the variable listed on kindergarten attendance.

For the controls used in the Census and CPS analysis (Panel A), state funding appears to have little predictive power. Regardless of race, there are no statistically significant shifts in the values of family background characteristics for cohorts entering school in the post-funding period. This is not particularly surprising, given that these covariates did not exert much influence on the estimates. By contrast, there are statistically significant relationships between kindergarten attendance and maternal education, and kindergarten attendance and the probability of living in a female-headed household, at least for white children (column (3)). Consistent with earlier statements, these correlations suggest that public kindergarten attendees tended to be positively selected before kindergartens were widespread.

The results for the HSB (Panel B) are not as convincing, perhaps because the HSB permits only across state comparisons for essentially one cohort. For nonwhites in early adoption states (column (6)), mothers were significantly less likely to be high school dropouts, and sophomores where significantly less likely to be living on their own. However, selection appears to be slightly stronger when individuals are stratified on kindergarten attendance (column (4)). Similar results hold for the white HSB sample. While all of these covariates (and more) are controlled for in the analysis above, these findings make the HSB comparisons less believable. In some ways, however, this is reassuring, since the estimates implied effects of kindergarten on retention that were implausibly large.

In Cascio (2003), I use a similar differences-in-differences specification to estimate the relationship between state funding in the South and measures of school resources and state demographic and economic characteristics.[40] Among the state characteristics

[40] School quality variables, taken from the 1962, 1972, and 1982 *Digest of Education Statistics* include average annual teacher salary, the pupil/teacher ratio, and average term length, and the federal and state shares of overall school funding. Variables taken from the *State Data Book* include state percent rural, percent non-

considered, I find only that inter-decadal trends in the birth rate are correlated with the timing of kindergarten adoption, and marginally so.[41] In addition, school resource measures that are correlated with program enactment—the pupil/teacher ratio, the federal contribution to total state school funding, and term length—suggest that kindergarten adoption may have in fact led to a relative deterioration in school quality. While some of these variables might simply be mechanically related to kindergarten expansion, the funding of public school kindergartens required a large commitment of funds that may have diverted resources from other school programs.[42] If observed trends represent true cutbacks in other school programs, my estimates of the effect of kindergarten attendance on schooling attainment should be biased downward.

B. State Funding and Other Programs

The late 1960s and 1970s were a particularly active era in education policy-making. Fortunately for this analysis, which relies on inter-cohort comparisons, some of the largest new programs were federal programs (e.g., Title I) which arguably would have impacted all states at the same time. However, other programs, like school finance equalization and court-order school desegregation, were state-specific and potentially introduced around the time that kindergartens were first funded.

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white, total population, the birth rate, and the infant mortality rate. Since the Bureau of Labor Statistics state-level series on unemployment does not go back to 1960, estimate state unemployment rates using the *Unemployment Insurance Financial Handbook*, which gives the ratio of U.I. claims to potential claimants.

[41] In 1960, the average birth rate (per 1000 resident population) in the South is 24.13. It falls to 18.92 in 1970 and 15.66 in 1980. The inter-decadal reduction in the birth rate is approximately 0.661 births/1000 resident population lower if kindergarten funding begins that decade. The estimate is significant at the 10 percent level.

[42] School resource measures that are significantly related to state funding might be mechanically associated with kindergarten expansion. For example, state funding of kindergartens may have displaced some federal funding (particularly through Title I), and kindergarten programs may have also been operated for fewer days of the school year, particularly if not subject to the restrictions associated with foundation program funding.

Since the analysis here is dominated by the South, school desegregation appears to be of particular concern.[43] Though ruled inherently unequal by the 1954 Supreme Court case *Brown v. Board of Education*, school segregation persisted into the 1960s (Orfield, 1983; Boozer, Krueger, and Wolkon, 1992). School integration effectively began with passage of the 1964 Civil Rights Act, though most large urban districts were desegregated under court order beginning in 1968. Although no evidence to date has been presented to suggest that desegregation is causally linked to grade repetition, Guryan (2001) shows that high school dropout among blacks declined more quickly from 1970 to 1980 in school districts under court order to desegregate.

I cannot completely rule out that school desegregation is confounding my estimates. However, three pieces of evidence suggest that trends in the racial composition of schools were smooth around the time in which public school kindergartens were first funded, in which case the across-cohort comparisons remain credible. First, I take two "snapshots" of school integration from Orfield (1983): the percent of enrolled black students attending majority white schools, by state, in 1968 and 1980. Trends in this crude measure of racial composition are virtually across all states regardless of decade of adoption.[44] I estimate that the percent of blacks attending majority white schools rose an insignificant 1.82 percentage points more than would have otherwise been expected during the decade in which kindergarten was first funded.

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[43] Card and Payne (2002) document court-ordered changes to school finance regimes in the following states in the sample of adopters: Arkansas (1983), Kentucky (1989), Montana (1989), Texas (1989, 1991), and West Virginia (1978, 1988). With the exception of the rulings in Arkansas and West Virginia, these changes occurred too late to have an affect of on the grade retention rates of the 1959-1978 school entry cohorts. Dropping these states from the analysis has essentially no effect on the results.

[44] I assume that no blacks were in majority white schools in the early 1960s. Among southern-born black students born in the 1950s, 79.2 percent report having attended at least one all black school; 77.8 percent report having attended an all black grade school (Card and Krueger, 1992, p. 166). Using the same survey, Boozer, Krueger, and Wolkon (1992) show that the proportion of blacks attending all (majority) black schools in southern and "border" states in 1959 was approximately 90 (95) percent.

Second, I estimate the correlation between the timing of major changes in school desegregation and the timing of kindergarten adoption, using the most comprehensive available database on court-ordered desegregation plans implemented during the 1960s, 1970s, and early 1980s (Welch and Light, 1987).[45] Focusing on southern states, Figure 7 plots the year of each district-level court-ordered desegregation plan against the year of first state aid for kindergarten. The size of each point represents the reduction in the district dissimilarity index, a measure of the degree of school integration within any given district, in the year following plan implementation.[46] If desegregation plans were fully confounded with the timing of kindergarten adoption, points on this graph would lie on the 45° line. Instead of having a slope of one, however, the least-squares regression lines that characterize these plots have slopes that cannot be statistically distinguished from zero, implying that the timing of desegregation was essentially orthogonal to the year of first state funding.

As a final test, I conduct the Census and CPS analyses separately for the South and for the West, which was little affected by court-ordered desegregation. When I stratify the analysis in this way, it strengthens the findings for retention for minorities. For nonwhite children in western states, the Census yields a 2SLS estimate for grade retention of -0.093 (or 52%, with robust standard error of 0.056), and the CPS yields a 2SLS estimate of -0.098 (or 49%, with robust standard error of 0.07). For the same cohorts, I also find evidence of

[45] The database gives the year of court-ordered desegregation for all districts enrolling more than 50,000 students and having 20 to 90 percent minority representation, as well as for a sub-sample of districts enrolling 15,000 to 50,000 students and having 10 to 90 percent minority representation. The plans represent about 20 percent of the school-aged population and approximately 45 percent of all minority enrollment in the U.S. in 1968 (Welch and Light, 1987; p. 32). (Figures are not given separately for the South.)

[46] The dissimilarity index is roughly the fraction of students that would have to change schools to achieve racial balance. The reduction in the dissimilarity index (roughly the fraction of students that would have to change schools to achieve is given alongside the year of implementation in Appendix B of Welch and Light (1987). I drop observations for which Welch and Light do not give the change in the dissimilarity index. Further, plans/districts with increases in the dissimilarity index in the year(s) immediately following plan implementation are assigned a weight of zero. Including all plans and giving them all equal weight yields roughly the same conclusion as stated above.

statistically significant declines in high school dropout (on the order of 46%) for nonwhites in the Census, though not in the CPS. Similar estimates suggest that whites in western states did not benefit from kindergarten enrollment. In general, these results suggest that, if anything, school desegregation in the South is working against finding an effect, particularly for the minority subsample.

IX. Discussion and Policy Implications

A. Reconciling the Findings

On balance, results from the Census, CPS, and HSB suggest that the spread of kindergarten programs through the South and West had fairly large impacts on grade retention, particularly for minorities, but no effect on high school dropout. These findings are surprising for several reasons.

First, numerous researchers have documented a strong link between grade retention and high school dropout (e.g., Roderick, 1994; Jimerson, 1999), and many policymakers in the 1960s and 1970s believed that such a link existed when funding legislation was passed. With the possible exception of the results for the West discussed above, this paper suggests that this correlation may not in fact represent a causal relationship: to the extent that the spread of kindergartens generated a true exogenous "shock" to grade retention rates, we would expect the program to indirectly uncover the true relationship between retention and dropout. Instead, repeaters may in general be negatively selected, or more likely to drop out of school even in the absence of retention.

Second, kindergarten attendance appears to have had effects on retention commensurate with those of other preschool programs (see Barnett, 1998), despite the fact that kindergartens are a fairly weak form of early childhood intervention. This result might

also arise from differences in key program characteristics. Unlike Head Start or other more, intensive early-childhood programs, kindergartens are operated within public schools. As a result, curriculums might be better integrated across grades, and the gains from preschool programs more likely to be "captured" over the longer-term. Currie and Thomas (2000) have indirectly argued the same point: for blacks, gains from Head Start participation may be eroded by subsequent attendance at relatively low quality schools.

On the other hand, the finding of surprisingly large effects for kindergarten on retention might arise from differences across studies in the "counterfactual" to preschool enrollment. To illuminate the counterfactual, Table 10 presents estimates of "first-stage" regressions (model (2)) for several alternative activities to public kindergarten for five-year-olds in the 1968 to 1987 October CPS files. The effect of state funding on enrollment in private kindergarten and first grade shows how much public school kindergarten substituted for other formal programs in which five-year-olds might have been enrolled.[47] By contrast, the effect of state funding on overall school enrollment provides insight into how much public school kindergartens substituted for home care.[48]

The table shows that most five-year-olds in the 1960s and 1970s would have been at home, or educated informally, in the absence of public school kindergarten programs. When normalized by the effect of funding on public kindergarten enrollment reported for similar specifications in Table 4 (Panel B, columns (4) and (5)), these coefficients imply that

[47] Although school entry age restrictions appear to have been fairly well-enforced during the period of interest (Cascio, 2003), some families may have enrolled their five-year-olds in first grade (or higher grades).

[48] The coefficients are in fact linked by an identity. Let ENR, PUBK, PRIVK, FGPLUS, and NS represent indicator variables for enrollment in school overall, public kindergarten, private kindergarten, first grade or higher, and nursery school. The latter four categories exhaust the possibilities for school enrollment. Thus, it must be the case that ENR=PUBK+PRIVK+FGPLUS+NS, and ΔENR= ΔPUBK+ ΔPRIVK+ ΔFGPLUS+ ΔNS. Thus, given that nursery school enrollment of five-year-olds is negligible during this period, it should be the case that any estimated change in public kindergarten enrollment can be decomposed into three parts: ΔPUBK$\approx$ ΔENR- ΔPRIVK- ΔFGPLUS. Because of data limitations, this decomposition cannot document the movement of children from other publicly-funded preschool programs like Head Start (which might be reported as "public kindergarten") into true public school kindergarten programs.

between 56 percent of white five-year-olds, and 65 percent of nonwhite five-year-olds would have been schooled informally (at home) in the absence of kindergartens. The remaining public kindergarten attendees appear to have been drawn primarily from private programs. Thus, to the extent that home-based care is truly inferior (see Magnuson, *et al.* (2004a) for a review of this literature), one might expect public kindergarten expansion to have had relatively large effects.

B. Who is the Marginal Pre-K Attendee?

This counterfactual to public school kindergarten enrollment—what five-year-old children would have been doing in the absence of kindergarten—is critical to understanding the relevance of this study to the current policy environment. Although kindergartens and prekindergartens look similar along many superficial dimensions, we should be hesitant to draw any strong conclusions about the potential effects of state-sponsored pre-K to the extent that the "marginal" kindergartner—the child for whom effects are identified—looks significantly different from the prospective pre-kindergarten enrollee.

Does the prospective four-year-old pre-K attendee in fact bear any resemblance to this child? This question is unfortunately not an easy one to answer. To date, prekindergarten has become truly widespread in only two states (Georgia and Oklahoma), and only in Oklahoma are the state-funded pre-K programs, like kindergartens, operated primarily through the public schools. The universalization of prekindergarten in practice therefore affords little opportunity to investigate accompanying changes in activities and care of four-year-olds.[49]

[49] I do, however, plan to use the available variation to estimate the pre-K counterfactual directly in future work.

However, there are several reasons to believe that marginal prekindergartner is highly likely to be from an informal educational setting, like the public school kindergartners in this study. Existing pre-kindergarten programs generally target disadvantaged students, helping to close the gap between Head Start eligibility and attendance.[50] Children attending state-funded pre-K programs in public schools are therefore likely to be fairly close to the margin of attending Head Start—from relatively poor families lacking the resources to enroll them in other formal preschool programs. Such children would arguably be the first to take-up on further expansions of prekindergarten programs and, as suggested by this study, the most likely to benefit.

X. Conclusion

This paper has explored the relationship between schooling attainment and state expansions in public school kindergarten programs during the 1960s and 1970s, arguably the largest and most rapidly executed state investments ever undertaken in early childhood education. These expansions appear to have provided a year of preschool to a generation of schoolchildren, many of whom would have otherwise began formal schooling in first grade.

Exploiting differences in the timing of state funding initiatives for kindergarten, and using data from numerous sources, I find evidence of large effect of public kindergarten attendance on grade retention. For white children aged five after state funding for kindergarten had begun, the probability of being retained in grade was about 20 percent less than it would have otherwise been. For racial minorities, this effect could arguably be as high as 30 to 40 percent. Most of the gains appear to have been realized at young ages. Despite the strong evidence of an effect on retention, I have uncovered no evidence of an

[50] Head Start has never been funded so as to meet its mandate to serve all poor children. Currie and Neidell (2003) estimate that only 65 percent of eligible 3 and 4 year-olds were served by Head Start in 2000.

effect of kindergarten on high school dropout using similar comparisons. These effects seem reasonable in light of the alternative forms of care available for five-year-olds at the time, and the quality of kindergarten as an early childhood program.

This paper provides some insight into the potential effects of extending public preschools to four-year-olds. Because prekindergartens have only recently been introduced into public schools (since the late 1970s), their longer-run impacts have thus far been difficult to predict. It is also difficult to generalize to state-sponsored preschools from existing evaluations centered on higher quality programs. Further, direct estimates of the short-term impacts of prekindergarten have generally not been based on credible comparisons (Gilliam and Zigler, 2000). Although caution should of course be exercised in generalization from this history episode, broad similarities across programs in curriculum, funding, and the probable counterfactual suggest that public schooling for four-year-olds could have benefits early in the school career, particularly for relatively disadvantaged children.

XI. Data Appendix

A. Coding of Public Kindergarten Adoption Dates

Adoption of kindergarten into a state's public school system is defined by large, sharp increases in the availability of state support for public school kindergarten. Since a single document bringing together the necessary dates was not available, I drew them from a variety of sources on a state-by-state basis. Seven types of documents were used to determine these dates (listed in Table 2):

1. State session laws, including appropriations acts;
2. Dissertations (Chapman, 1975; Forgione, 1977; Harris, 1987; Lightfoot, 1982; Murray, 1973);
3. A periodic publication of the U.S. Department of Health, Education and Welfare Office of Education giving details on state funding of education, including funding formulas (1963a, 1963b, 1967, 1969, 1972, 1976, 1980).
4. Compilations of legislation by the Education Commission of the States (1971a, 1971b, 1972, 1973, 1974a, 1974b, 1974c, 1974d, 1975a, 1975b, 1976);
5. Compilations of legislation by the National Education Association (1966, 1967, 1968, 1969, 1971, 1972, 1973);
6. State reports (Alabama Department of Education, 1977; Jones, 1977; Texas State Department of Community Affairs, 1972; Watson and Lanham, 1971);
7. Newspaper and journal articles (Tanner and Tanner, 1973; Kanensiger, 1983).

B. Coding of School Entry Ages

Estimation of models for grade retention required knowledge of changes in cutoff dates for school entry affecting the 1953-78 birth cohorts. I derived dates from archival work based on the histories of current age at school entry statutes and knowledge of the first grade entry ages in 1955 (Angrist and Krueger, 1992), and 1965, 1972, and 1978 (*Digest of Education Statistics*).The complete time series of legislation used in Appendix Table A1 of Cascio (2003)

C. Data on Kindergarten Enrollment

The annual, state-level series of kindergarten enrollment rates is constructed by normalizing state enrollment aggregates (described below) with an estimate of the age-eligible population. To create the longest possible series, I used the number of public school first graders in the state *the next year* as a baseline denominator. The resulting kindergarten enrollment rates are highly correlated those constructed using data on the population of live births during the calendar year five years prior, taken from published aggregates from *Vital Statistics* for 1958-75 (computer-coded data provided by Kenneth Chay) and from the author's tabulations from the *Natality Detail Data* for 1976-85. The high degree of correlation is not surprising given that kindergarten repetition rates were relatively low during this period (see Chapter 3).

The state-level kindergarten enrollment aggregates used in this paper were taken from several administrative sources. Data from 1957, 1963, and annually from 1964 to 1982 was hand-entered by the author from annual publications of the Office of Education. Although part of the same series, the publication goes by different names over the period.[51] The earliest dates are taken from the *Biennial Survey of Education*, and where missing, data are linearly interpolated. This practice should smooth over breaks in enrollment rates. The underlying enrollment data were collected through federal survey of the Department of Education in each state, the survey form largely consistent from year to year. Most states are reported to have gathered relevant information on enrollment from local school districts by way of a similar survey. Enrollment figures are taken as of October of the year. Annual data from 1983 through 2000 (used in Figure 1) were drawn from the *Common Core of Data: State Nonfiscal Survey*, available from ICPSR and for more recent years directly from the NCES website (http://nces.ed.gov/ccd/stNfis.asp).

Until 1980, the "pre-primary" enrollment category in *Fall Statistics* is inclusive of attendance at both kindergartens and preschools operated as part of a state's public schools. In the late 1970s, public preschool provision was rare in the South: as of 1980, only four southern states—Florida, Maryland, Texas, and Virginia—reported non-zero pre-kindergarten enrollment figures.[52] As a result, using pre-primary enrollment aggregates from 1964-79 and kindergarten enrollment aggregates thereafter presents little loss in enrollment series consistency over time.

D. Census Data

To construct outcome measures, I use data from 1970 through 2000 Decennial Census PUMS. The 1970 samples are the "15%" (Form 2) State, County Group, and Neighborhood samples, which collectively yield a 3 percent sample of the population. The 1980 sample is the State (A) 5% PUMS, and the 1990 and 2000 samples are the 5 percent PUMS.

Kindergarten funding availability is assigned on the basis of state of birth. State of birth is, however, missing for about five percent of all individuals in the 1970 PUMS. Individuals for whom state of birth is missing are dropped from the analysis.

To calculate family background characteristics for cohorts, children are matched to their mothers, or to their assumed primary female caregivers in the household when between the ages of 0 and 9 and observed in the 1960 Decennial Census (1%), or the 1970 and 1980 Census files listed above. An individual is classified as either (1) the child of the

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[51] Including *Fall Statistics of Public Elementary and Secondary Day Schools: Pupils, Teachers, Instruction Rooms, and Expenditures* (1964-68), *Statistics of Public Elementary and Secondary Day Schools* (1969-78), *Statistics of Public Elementary and Secondary School Systems: Schools, Pupils, and Staff* (1979-80), and *Public School Enrollment, United States* (1981-82).

[52] Proportions of pre-primary enrollees in pre-kindergarten in these states were 3.6, 10.4, 7.9, and 1.8 percent, respectively. In Washington, D.C., enrollment in pre-kindergarten programs was substantially higher: in 1980, pre-kindergarten represented 32 percent of total reported public pre-primary enrollment

householder; (2) a child in a subfamily; or (3) some other relative of the household. Type (1) children are matched to either the spouse of the householder, or the household herself. Type (2) children are matched to either the wife or primary individual within the same subfamily. Type (3) children are matched to the woman who is either the wife of the household head or the householder; such women could be grandmothers, aunts, older sisters, etc.

If a match to a woman could not be made, family background characteristics—including maternal education and labor force participation status and number of siblings—are imputed using race, "child type," and state-of-birth-specific means. There are effectively four "child types," when the above-specified types (4) and (5) are combined. A mother is assumed to be working if she worked in the year prior to the Census; number of siblings is computed as the woman's fertility less one. Calculation of other family background variables is self-explanatory.

E. High School and Beyond Sample

The sample drawn from the High School and Beyond Sophomore Cohort consists of individuals likely to have attended kindergarten in the United States and attending school in in the South or West in the spring of 1980. The sample is further limited to individuals with non-missing answers to the kindergarten attendance question (in the 1980 base year survey) and the grade retention questions (in the 1982 first follow-up survey). All calculations are weighted using the panel weights from the first follow-up survey. Although kindergarten attendance rates appear to be exceptionally high for the period, my tabulations are consistent with those given in the original base year codebook for the survey.

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TABLE 1 - KINDERGARTENS AND PREKINDERGARTENS: A COMPARISON OF PROGRAM CHARACTERISTICS

	Program and Time Period	
	Kindergarten 1960's	Pre-Kindergarten 1990's
A. State Funding per Half-day Student (in 1996 dollars)		
Total	$2,435	n/a
State contribution	$1,293	$1,386
B. Program Characteristics (percent):		
Children per class in half-day program (average)	24	17
Half day program	94	68
Funded through state or local education funds	91	80
Movable equipment and materials	97	95
C. Activity/Learning Centers in the Classroom (percent):		
Library or Reading/Quiet Area	95	94
Blocks	94	98
Art	85	100
Home play	82	100
Science	82	76
Music	74	96
Dramatic play	55	91

Sources:

Funding: 1960s: Dollars per student are calculated first in nominal dollars from available archival sources (see Data Appendix for more details), then converted to 1996 dollars using the GDP deflator. Total per-student expenditure adds the expected local contribution to the state contribution, where local effort is (contemporaneous) percent of non-federal school revenue in the state derived from local sources. Figures pertain to half-day students, and to southern states only in the first year of kindergarten availablity. 1990s/2000s: Doherty (2002), for FY 2001 and 2002, as available. Figures converted to 1996 dollars using the GDP deflator.

Program Characteristics: National Education Association (1969) and U.S. Department of Education, National Center for Education Statistics (2003). Data for academic years beginning in 1967 and 2000, respectively.

Activity/Learning Centers in the Classroom: National Education Association (1962) and Council for School Performance (1998). Data for academic years beginning in 1961 and 1997, respectively. More recent data are for prekindergarten programs in Georgia.

TABLE 2 - STATE FUNDING INITIATIVES FOR KINDERGARTEN AFTER 1960

	Region		
Year of Adoption	South	West	Other
1966		AK	
1967	MD, MO		
1968	DE, FL, VA		NH
1969	OK		
1971	WV	AZ	
1973	AR, NC, SC, TN, TX	OR	
1974		MT	
1975		NM, ID	
1977	AL, KY		
1978	GA		
1980			ND
1986	MS		

Notes: Year of adoption corresponds to the first year in which state funds for kindergarten were available to all districts in the state, either through application or entitlement. The South includes all states in the southern census divisions, plus Missouri. The West consists of all states in the western census divisions.

Sources: See Data Appendix.

TABLE 3 - TRENDS IN KINDERGARTEN ENROLLMENT RATES OF FIVE-YEAR-OLDS IN THE DECENNIAL CENSUS, BY REGION

	Census Year			Inter-Decadal Changes	
	1960	1970	1980	1960 to 1970	1970 to 1980
	(1)	(2)	(3)	(4)	(5)
A. South and West					
Kindergarten (Overall)	0.285	0.492	0.756	0.207	0.264
Public Kindergarten	0.235	0.384	0.650	0.149	0.266
N	19099	56336	84079	-	-
B. Rest of the Country					
Kindergarten (Overall)	0.512	0.678	0.776	0.166	0.098
Public Kindergarten	0.455	0.620	0.679	0.165	0.059
N	19218	54586	69297	-	-
C. South and West / Rest of the Country					
Kindergarten (Overall)	0.56	0.73	0.97	0.17	0.25
Public Kindergarten	0.52	0.62	0.96	0.10	0.34

Notes: Author's tabulations from the 1960 (1%), 1970 (1% Form 2 state, metro, and neighborhood files, making for a 3% sample overall) and 1980 Census PUMS (5% sample). The South includes all states in the southern census divisions, plus Missouri. The West consists of all states in the western census divisions. State is state of birth. Enrollment rates are expressed as fractions of five year olds enrolled. Age is measured as of October 1 using information on age as of April 1 and quarter of birth.

TABLE 4. THE EFFECT OF STATE FUNDING ON PUBLIC KINDERGARTEN ENROLLMENT: COMPARISON OF TWO SURVEYS

Data Source:	Office of Education		October CPS		
Dependent Variable:	Public School K/First Grade Enrollment Ratio		Public K Enrollment Rate (5 year-olds)		
Race:	All		All	White	Non-white
	(1)	(2)	(3)	(4)	(5)
A. Fixed Effects Only					
Coefficient on Zcs	0.395	0.448	0.424	0.448	0.322
	(0.067)	(0.064)	(0.075)	(0.076)	(0.067)
R^2	0.93	0.89	0.71	0.76	0.49
B. + Family Background Controls					
Coefficient on Zcs			0.414	0.436	0.313
			(0.067)	(0.066)	(0.060)
R^2			0.71	0.77	0.50
C. Sample					
Mean of Dependent Variable					
N	461	591	802	411	391
Years	1959-78	1968-87		1968-87	
Birth Cohorts	1954-73	1963-82		1963-82	

Notes: In all regressions, sample is limited to Missouri, plus states in southern and western census divisions. Models include only those years for which kindergarten enrollment figures are not missing. All models include fixed effects for state and cohort. Models in Panel B add controls for family background as observed at age 5 in the October CPS. Family background variables include average maternal educational attainment, average age at birth, fraction of households female-headed, and the fraction of observations for which children could not be matched to mothers. Regressions in columns (1) and (2) are weighted by cohort size (estimted from the Census), and regressions in columns (3)-(5) are weighted by cell size for the dependent variable. In all regressions, standard errors (in parentheses) are clustered to allow for residual correlation within state in the pre and post periods.

TABLE 5 - REDUCED FORM ESTIMATES: THE EFFECT OF STATE FUNDING ON GRADE RETENTION

	Dependent Variable: Fraction of Cohort Below Grade for Age					
	Decennial Census		October CPS			
	Ages 6-15		Ages 6-15		Ages 6-10	
	White	Non-white	White	Non-white	White	Non-white
	(1)	(2)	(3)	(4)	(5)	(6)
A. Fixed Effects						
Coefficient on Zcs	-0.019	-0.017	-0.021	-0.040	-0.015	-0.043
	(0.009)	(0.020)	(0.011)	(0.016)	(0.011)	(0.014)
R^2	0.87	0.86	0.55	0.40	0.61	0.45
B. + Family Background Controls						
Coefficient on Zcs	-0.012	-0.015	-0.020	-0.039	-0.013	-0.042
	(0.005)	(0.014)	(0.010)	(0.015)	(0.009)	(0.013)
R^2	0.89	0.88	0.55	0.40	0.61	0.45
C. + Log Cohort Size						
Coefficient on Zcs	-0.013	-0.020	-0.015	-0.035	-0.013	-0.043
	(0.005)	(0.012)	(0.009)	(0.014)	(0.009)	(0.012)
R^2	0.90	0.89	0.56	0.40	0.61	0.45
D. Sample						
Mean of Dep. Variable	0.165	0.239	0.199	0.281	0.156	0.209
N	610	607	3041	2881	1145	1090
Years	1970, 1980		1968-88			
Birth Cohorts	1954-73		1954-73			

Notes: In all regressions, sample is limited to Missouri, plus states in southern and western census divisions. Census estimates are based on native-born samples only. Age is measured as of October in both data sets. All models include fixed effects for state, cohort, month of school entry cutoff date, and age. Models in Panels B and C add controls for family background, including average maternal educational attainment, average age at birth, fraction of households female-headed, and the fraction of observations for which children could not be matched to mothers. Models in Panel C add the log of cohort size, which cohort size is estimated from the Census and corresponds to all races. All regressions are weighted by cell size for the dependent variable, and standard errors in all regressions (in parentheses) are clustered to allow for residual correlation within state in the pre and post periods.

TABLE 6 - REDUCED FORM ESTIMATES: THE EFFECT OF STATE FUNDING ON HIGH SCHOOL

	Dependent Variable: Fraction of Cohort Without HS Degree and Not Currently Enrolled					
	Decennial Census				October CPS	
	Ages 16-35		Ages 16-25		Ages 16-25	
	White	Non-white	White	Non-white	White	Non-white
	(1)	(2)	(3)	(4)	(5)	(6)
A. Fixed Effects						
State Funding Indicator	-0.008	-0.004	-0.008	-0.005	-0.018	-0.006
	(0.004)	(0.007)	(0.004)	(0.006)	(0.008)	(0.009)
R^2	0.90	0.86	0.92	0.91	0.27	0.19
B. + Family Background						
State Funding Indicator	-0.004	-0.002	-0.002	-0.004	-0.018	-0.004
	(0.003)	(0.005)	(0.003)	(0.006)	(0.008)	(0.008)
R^2	0.90	0.86	0.93	0.91	0.27	0.19
C. + Log Cohort Size						
State Funding Indicator	-0.004	-0.003	-0.002	-0.004	-0.013	-0.001
	(0.003)	(0.005)	(0.003)	(0.006)	(0.007)	(0.008)
R^2	0.91	0.86	0.93	0.91	0.27	0.20
D. Sample						
Mean of Dep. Variable	0.13	0.191	0.137	0.183	0.182	0.194
N	1222	1214	611	607	5427	5028
Years	1980, 1990, 2000				1970-98	
Birth Cohorts	1954-73				1954-73	

Notes: Age in the Census is measured as of April. Otherwise, see notes to Table 5.

TABLE 7 - OLS AND 2SLS ESTIMATES OF THE EFFECT OF PUBLIC SCHOOL KINDERGARTEN ATTENDANCE ON GRADE RETENTION

Dependent Variable:	*Fraction of Cohort Below Grade for Age*						*Repeated a Grade*	
	Decennial Census		October CPS				High School and Beyond	
	Ages 6-15		Ages 6-15		Ages 6-10		Sophomores	
	White	Non-white	White	Non-white	White	Non-white	White	Non-white
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. OLS								
Public K Attendance	-0.024	-0.029	-0.040	-0.014	-0.044	-0.056	-0.031	-0.054
	(0.007)	(0.027)	(0.014)	(0.032)	(0.013)	(0.029)	(0.013)	(0.028)
R^2	0.90	0.89	0.56	0.40	0.62	0.45	0.03	0.03
B. 2SLS								
Public K Attendance	-0.034	-0.052	-0.037	-0.098	-0.034	-0.136	-0.121	-0.185
	(0.013)	(0.030)	(0.017)	(0.047)	(0.021)	(0.049)	(0.067)	(0.167)
R^2	0.9	0.88	0.56	0.4	0.62	0.44	0.02	0.01
C. Reduced Form								
State Funding Indicator	-0.013	-0.020	-0.015	-0.035	-0.013	-0.043	-0.024	-0.028
	(0.005)	(0.012)	(0.009)	(0.014)	(0.009)	(0.012)	(0.012)	(0.024)
R^2	0.90	0.89	0.56	0.40	0.61	0.45	0.03	0.03
D. First Stage (Dep. Var.: Public K Attendance)								
State Funding Indicator	0.366	0.375	0.405	0.361	0.380	0.316	0.199	0.154
	(0.048)	(0.059)	(0.079)	(0.074)	(0.084)	(0.075)	(0.034)	(0.041)
R^2	0.94	0.94	0.91	0.92	0.91	0.92	0.10	0.08
E. Sample								
Mean of Dep. Variable	0.165	0.239	0.199	0.281	0.156	0.209	0.147	0.222
N	610	607	3041	2881	1145	1090	8507	3480
Years	1970, 1980		1968-88				1980	
Birth Cohorts	1954-73		1954-73				1965	

Notes: In all regressions, sample is limited to Missouri, plus states in southern and western census divisions. Census and HSB models are based on native-born samples only. Columns (1)-(6): The unit of observation is the cohort-state cell. Models include fixed effects for state, cohort, month of school entry cutoff date, age; family background controls (see Table 5) and the log of cohort size. Age is measured as of October in both data sets. All regressions are weighted by cell size for the dependent variable, and standard errors in all regressions (in parentheses) are also clustered to allow for residual correlation within state in the pre and post periods. In the two-stage least squares (2SLS) models, the instrument is an indicator for school entry after passage of the state funding initiative. Columns (7)-(8): The unit of observation is the individual. Models include as controls measures of family background (indicators for female-headed household, self-headed household maternal education less than high school, and maternal employment status before and during elementary school) and measures of school quality/racial integration (indicators for predominantly black schools and predominantly hispanic schools in 1st, 6th, and 9th grades, percent of teachers in school (10th grade) with Ph.D. or MA, percent of teachers absent on the average day, percent of teachers leaving not due to death or retirement, and percent of students in school that drop out. Where missing, control variables are estimated with state/race-specific means. All regressions include indicators for missing data. All regressions are weighted by HSB panel weights, and standard errors (in parentheses) are clustered to allow for residual correlation within state. In the 2SLS models, the instrument is an indicator for currently residing in a state that funded kindergarten by 1969 or earlier.

TABLE 8 - OLS AND 2SLS ESTIMATES OF THE EFFECT OF PUBLIC SCHOOL KINDERGARTEN ATTENDANCE ON HIGH SCHOOL DROPOUT

Dependent Variable:	*Frac. of Cohort Without HS Degree, Not Currently Enrolled*						*Not Currently Enrolled*	
	Decennial Census				October CPS		High School and Beyond	
	Ages 16-35		Ages 16-25		Ages 16-25		Seniors	
	White	Non-white	White	Non-white	White	Non-white	White	Non-white
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. OLS								
Public K Attendance	-0.006	-0.010	-0.002	-0.005	-0.029	-0.034	-0.073	-0.018
	(0.005)	(0.012)	(0.006)	(0.013)	(0.014)	(0.018)	(0.016)	(0.026)
R^2	0.91	0.86	0.93	0.91	0.27	0.20	0.07	0.04
B. 2SLS								
Public K Attendance	-0.010	-0.006	-0.005	-0.009	-0.037	-0.003	0.036	-0.101
	(0.007)	(0.013)	(0.008)	(0.014)	(0.016)	(0.022)	(0.053)	(0.136)
R^2	0.91	0.86	0.93	0.91	0.27	0.20	0.06	0.04
C. Reduced Form								
State Funding Indicator	-0.004	-0.003	-0.002	-0.004	-0.013	-0.001	0.007	-0.015
	(0.003)	(0.005)	(0.003)	(0.006)	(0.007)	(0.008)	(0.010)	(0.021)
R^2	0.91	0.86	0.93	0.91	0.27	0.20	0.07	0.04
D. First Stage (Dep. Var.: Public K Attendance)								
State Funding Indicator	0.371	0.388	0.371	0.385	0.354	0.353	0.199	0.154
	(0.049)	(0.061)	(0.050)	(0.061)	(0.067)	(0.063)	(0.034)	(0.041)
R^2	0.93	0.93	0.93	0.93	0.92	0.92	0.10	0.08
E. Sample								
Mean of Dep. Variable	0.13	0.191	0.137	0.183	0.182	0.194	0.154	0.176
N	1222	1214	611	607	5427	5028	8507	3480
Years	1980, 1990, 2000				1970-98		1982	
Birth Cohorts	1954-73				1954-73		1965	

Notes: See notes to Table 7.

TABLE 9 - HOW ROBUST ARE THE ESTIMATES? THE RELATIONSHIP BETWEEN OF STATE FUNDING AND OBSERVABLE CHARACTERISTICS

	Average		Coefficient on: K Attendance		Coefficient on: State Funding Indicator	
	White (1)	Non-white (2)	White (3)	Non-white (4)	White (5)	Non-white (6)
A. Census/CPS Analyses						
Average Maternal Education	11.4	10.0	0.264	0.626	0.120	0.089
			(0.127)	(0.696)	(0.075)	(0.295)
Average Maternal Age at Birth	26.3	27.6	0.108	-0.473	0.017	-0.232
			(0.121)	(0.300)	(0.056)	(0.156)
Female Headed Household	0.10	0.32	-0.016	0.037	-0.004	0.001
			(0.008)	-0.038	(0.004)	(0.017)
Log Cohort Size	11.4	11.4	0.050	0.050	-0.013	0.020
			(0.047)	(0.047)	(0.027)	(0.024)
N (cells)	612	609	612	609	612	609
B. HSB Analysis						
Mother High School Dropout	0.311	0.48	-0.135	-0.132	-0.046	-0.087
			(0.015)	(0.025)	(0.019)	(0.040)
Mother Worked During Elementary School	0.616	0.766	-0.006	0.056	-0.010	-0.012
			(0.022)	(0.028)	(0.022)	(0.035)
Mother Worked Before Elementary School	0.443	0.674	-0.039	0.040	-0.067	-0.033
			(0.018)	(0.033)	(0.016)	(0.054)
Female Headed Household	0.192	0.357	-0.017	0.024	0.020	0.003
			(0.016)	(0.027)	(0.021)	(0.031)
Live On Own	0.031	0.09	-0.011	-0.043	-0.005	-0.044
			(0.007)	(0.017)	(0.006)	(0.018)
Almost All/All Black 1st Grade	0.021	0.317	-0.015	0.015	-0.022	-0.040
			(0.007)	(0.043)	(0.008)	(0.088)
Almost All/All Hispanic 1st Grade	0.027	0.07	-0.001	-0.004	-0.010	-0.003
			(0.005)	(0.011)	(0.016)	(0.043)
N (individuals)	8507	3480	8507	3480	8507	3480

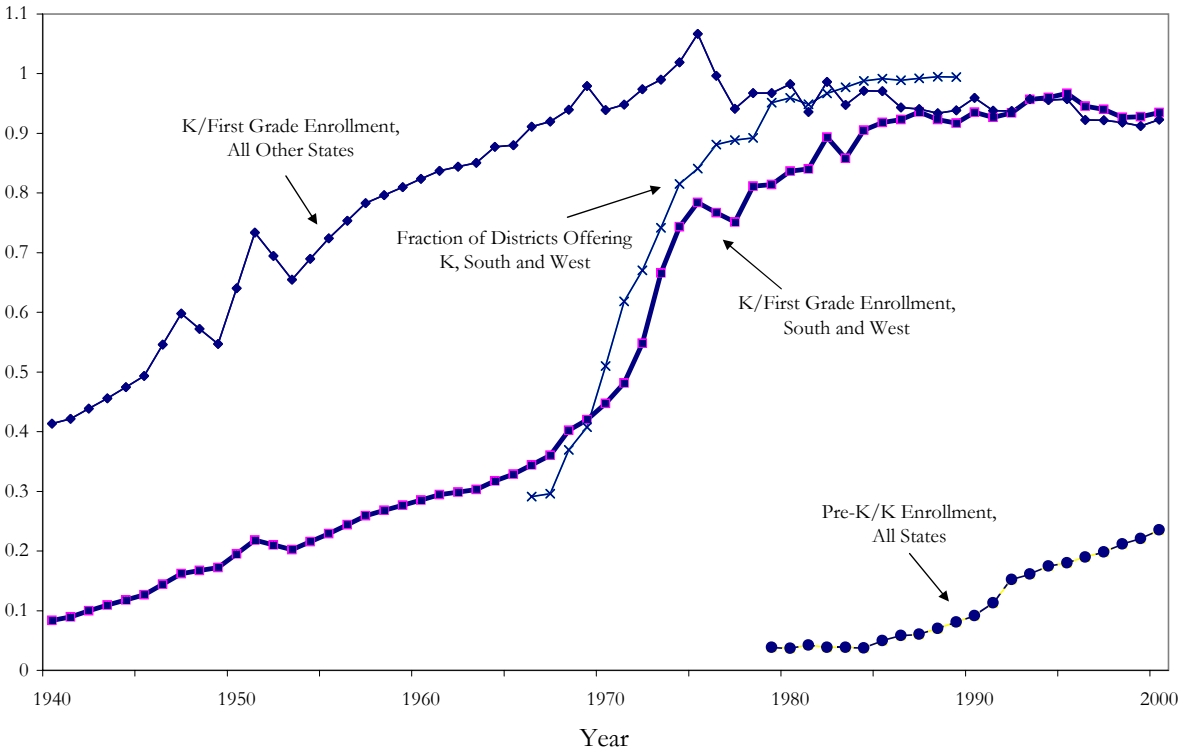
Notes: In all regressions, sample is limited to Missouri, plus states in southern and western census divisions. Census and HSB models are based on native-born samples only. Panel A: All models include fixed effects for state and cohort. The unit of observation is the cell mean. All regressions are weighted by cell size for the dependent variable, and standard errors in all regressions (in parentheses) are also clustered to allow for residual correlation within state in the pre and post periods. In columns (5) and (6), the state funding indicator is an indicator for school entry after passage of the state funding initiative. Panel B: The unit of observation is the individual. All regressions are weighted by HSB panel weights, and standard errors (in parentheses) are clustered to allow for residual correlation within state. In columns (5) and (6), the state funding indicator is an indicator for currently residing in a state that funded kindergarten by 1969 or earlier.

TABLE 10 - THE COUNTERFACTUAL: THE EFFECT OF STATE FUNDING ON ENROLLMENT OF 5 YEAR-OLDS IN PRIVATE KINDERGARTENS, FIRST GRADE, AND SCHOOL OVERALL
(Data Source: October CPS)

Dependent Variable:	Overall School Enrollment		Private K Enrollment		Enrollment in Grades 1 & Above	
	White	Non-white	White	Non-white	White	Non-white
	(1)	(2)	(3)	(4)	(5)	(6)
State Funding Indicator	0.242	0.205	-0.17	-0.068	-0.025	-0.011
	(0.040)	(0.044)	(0.023)	(0.020)	(0.020)	(0.037)
R^2	0.77	0.58	0.68	0.28	0.62	0.42
Percent of Total Change in Public K Enrollment	56%	65%	39%	22%	6%	4%
Mean of Dependent Variable	0.876	0.881	0.124	0.066	0.074	0.111
N	411	391	411	391	411	391
Years	1968-87					
Birth Cohorts	1963-82					

Notes: In all regressions, sample is limited to Missouri, plus states in southern and western census divisions. All models include fixed effects for state and cohort. All regressions include controls for cohort and state fixed effects, plus controls for family background as observed for cohorts at age five in the October CPS (see Table 4). Regressions are weighted by cell size for the dependent variable. In all regressions, standard errors (in parentheses) are clustered to allow for residual correlation within state in the pre and post periods. "Percent of Total Change in Public K Enrollment" calculated by normalizing the reported coefficient by the coefficient from the corresponding specification for public kindergarten attendance, reported in Table 4, Panel B.

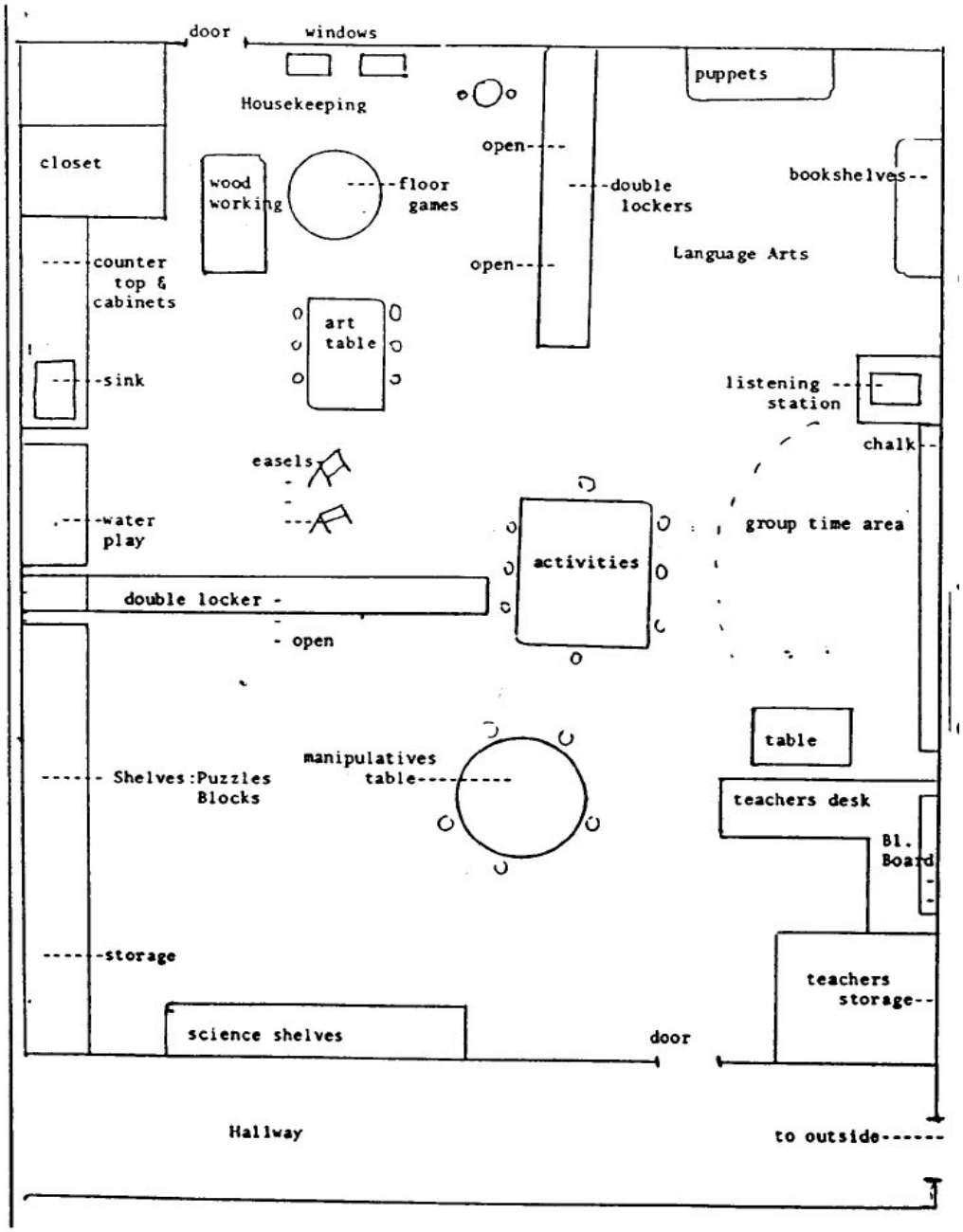
FIGURE 1 - PUBLIC SCHOOL KINDERGARTEN ENROLLMENT RATIOS IN THE U.S.: 1940-2000



Notes: The South includes states in the three southern census divisions and Missouri. The West includes states in the two western census divisions. Enrollment data is available only evary second year from 1939 to 1957. Ratios are linearly interpolated when years are missing. From 1939 to 1978, kindergarten enrollment aggregates include all pre-primary enrollments. The percent of (primary school) districts offering kindergarten is first calculated at the state/year level, then weighted by first grade enrollment to create a region-specific average. Regional averages are based on linearly interpolated figures for 1970, 1973, and 1983.

Sources: See Data Appendix.

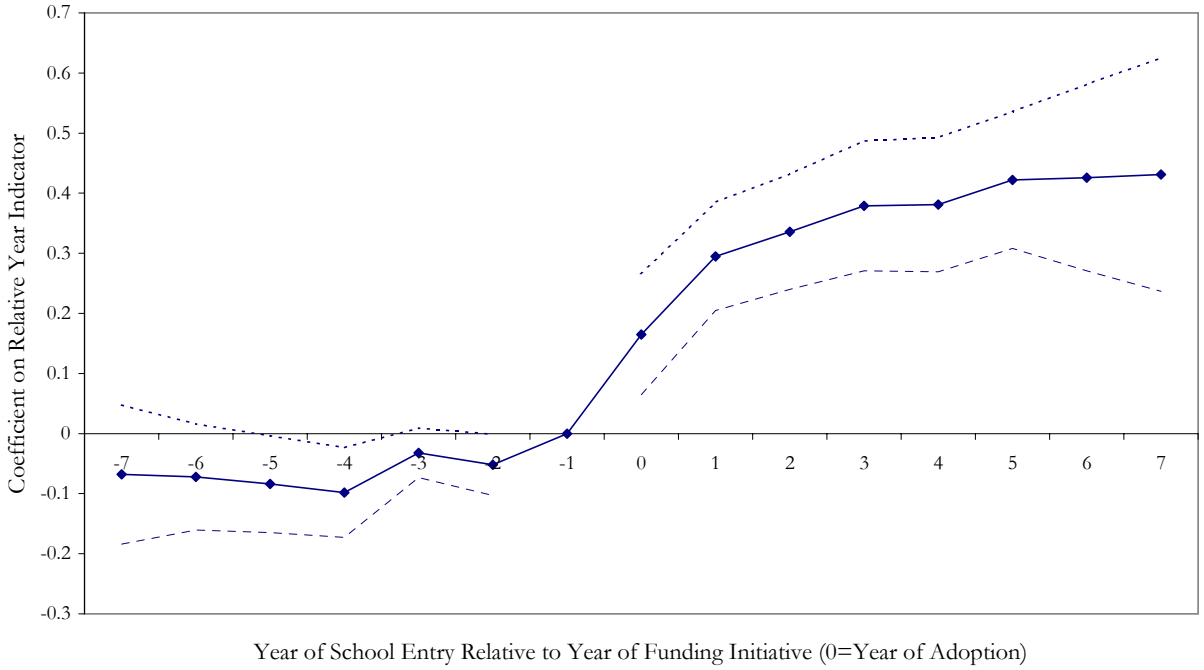
FIGURE 2 - FLOOR PLAN OF AN ALABAMA KINDERGARTEN IN THE MID-1970S



Notes: This figure depicts the floor plan of a pilot public school kindergarten at Wright's Mill Road School in Auburn, Alabama during the 1973-74 academic year.

Source: Alabama State Department of Education (1977), p. 7.

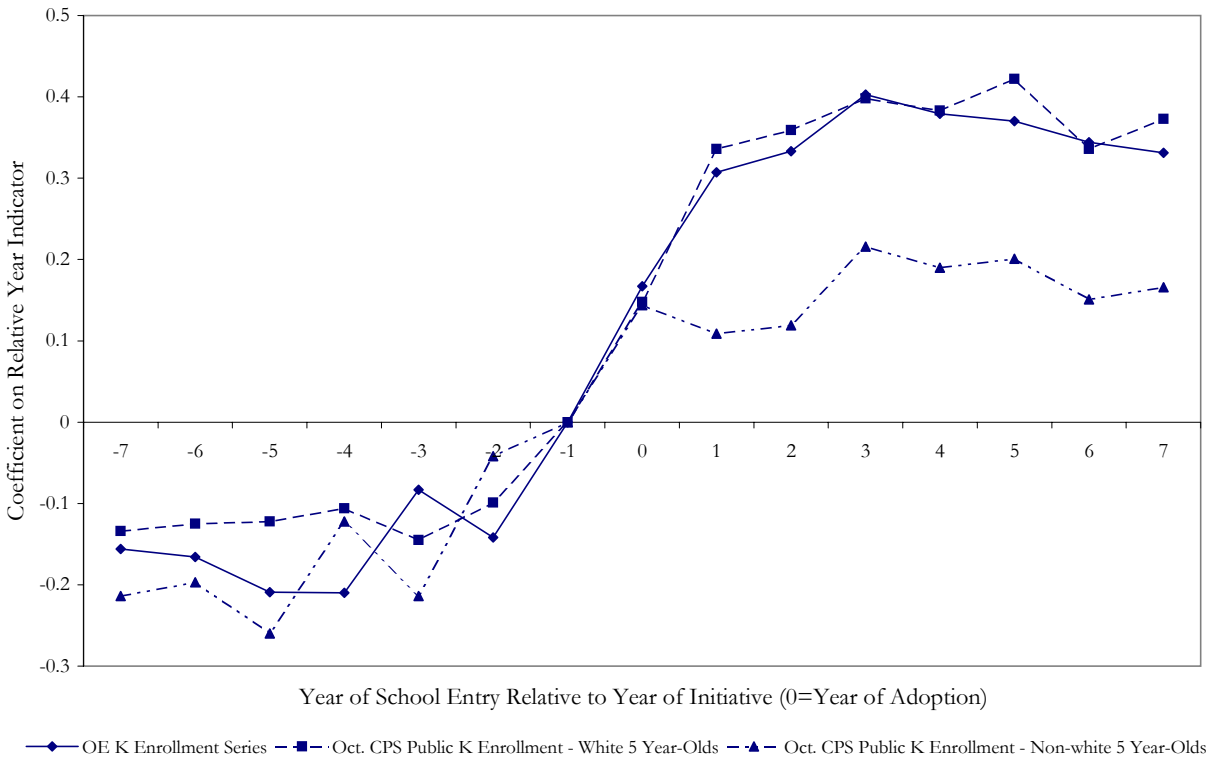
FIGURE 3 - TRENDS IN PUBLIC SCHOOL KINDERGARTEN ENROLLMENT RATES AROUND THE FUNDING INITIATVE
(OE kindergarten enrollment series, 1959-78 entry cohorts)



Notes: The figures plot coefficients on $Z^j cs$ (the normalized year indicators from model (2')) from a regression of the estimated public kindergarten enrollment rates on $Z^j cs$ (j=-7,…,7, with j=-1 the omitted category), cohort and state of birth fixed effects, and family background controls (from th CPS, see notes to Table 4).

Source: Biennial Survey of Education (1957-63), *Fall Statistics of Public Schools* (1964-78).

FIGURE 4 - COMPARISON OF THE OE AND OCTOBER CPS KINDERGARTEN ENROLLMENT SERIES
(1968-87 School Entry Cohorts)



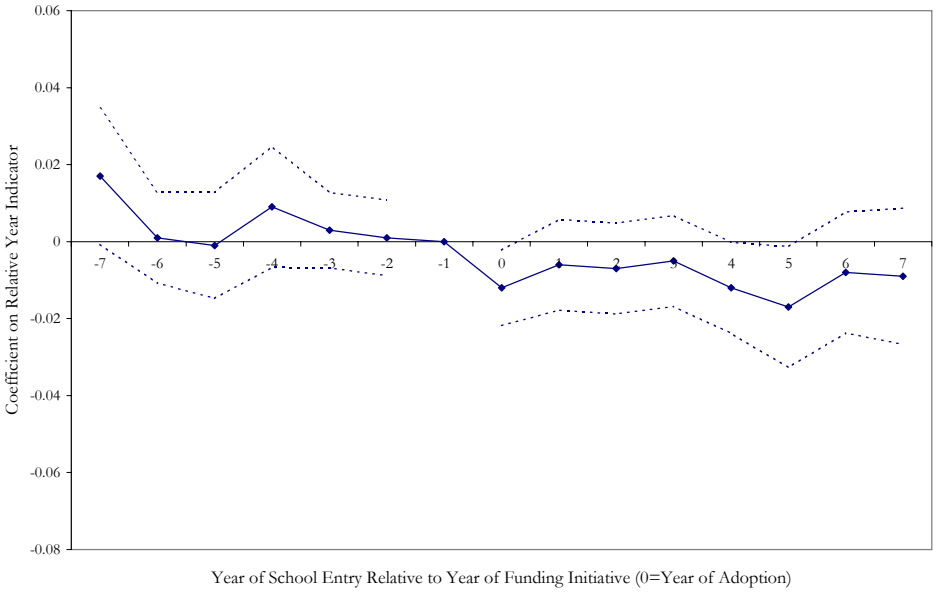
Notes: The figures plot coefficients on $Z^j cs$ (the normalized year indicators from model (2')) from a regression of the estimated public kindergarten enrollment rates on $Z^j cs$ (j=-7,…,7, with j=-1 the omitted category), cohort and state of birth fixed effects, and family background controls (from the CPS, see notes to Table 4)..

Sample: Five-year-olds.

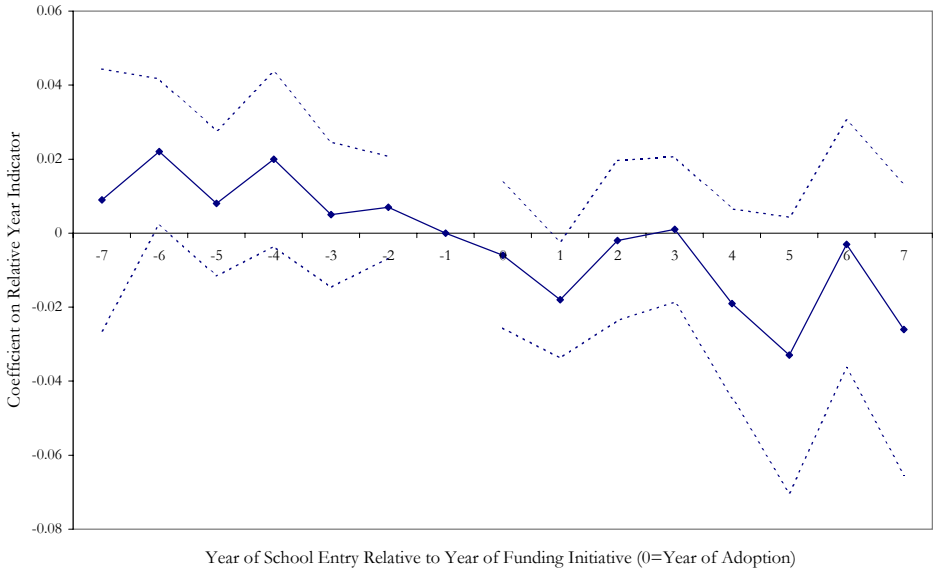
Sources: *Fall Statistics of Public Schools* (OE enrollment series) and the 1968-1987 October CPS School Enrollment Supplements.

FIGURE 5 - TRENDS IN GRADE RETENTION RATES, BY RACE, FOR COHORTS AGED FIVE AROUND TIME OF THE FUNDING INITIATIVE
(Census series, 1959-78 entry cohorts)

A. White 6-15 Year Olds



B. Non-white 6-15 Year Olds



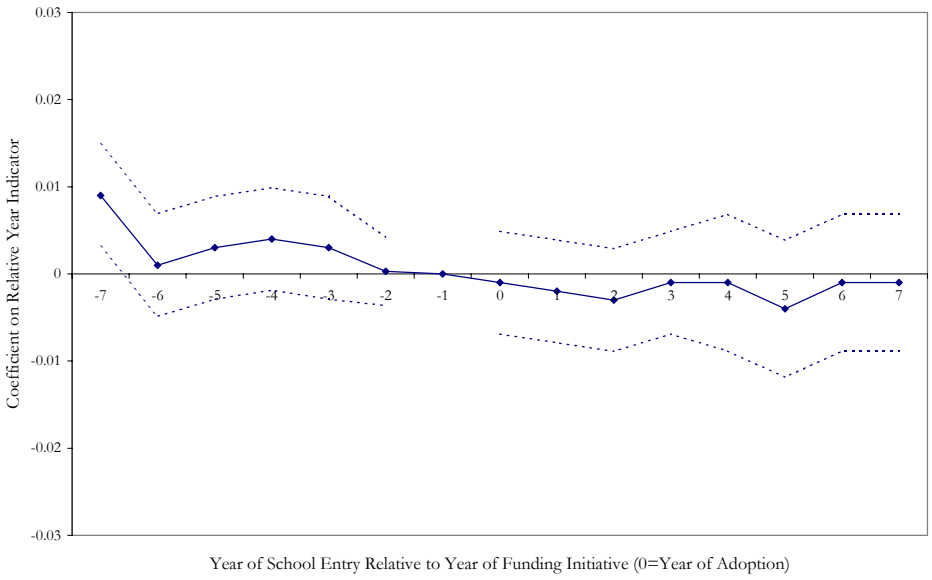
Notes: The figures plot coefficients on Z^{j}_{cs} (the normalized year indicators from model (2)) from a regression of the estimated grade retention rate on Z^{j}_{cs} (j=-7,…,7, with j=-1 the omitted category), cohort fixed effects, state of birth fixed effects, age fixed effects, school entry cutoff month fixed effects, family background controls (see notes to Table 5), and the log of cohort size. The dashed lines give the 95% confidence interval for the point estimates, where standard errors have been clustered on state of birth by Z_{cs}.

Sample: 6-15 year olds born in the South and West.

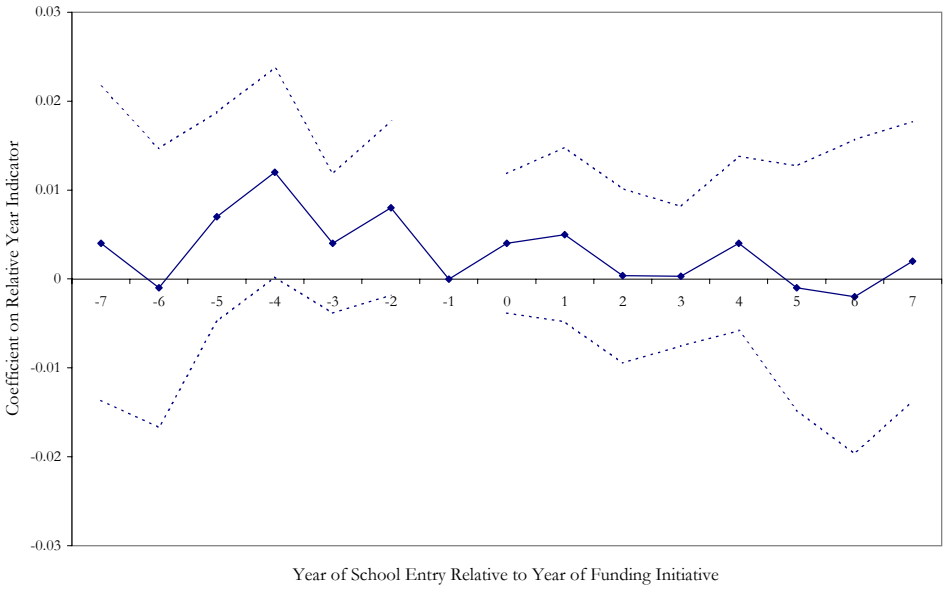
Sources: 1970 and 1980 Decennial Census PUMS. (1970: 3% sample constructed from Form 2 (15%) state, neighborhood, and metro area files; 1980: 5% sample)

FIGURE 6 - TRENDS IN HIGH SCHOOL DROPOUT RATES, BY RACE, FOR COHORTS AGED FIVE AROUND TIME OF THE FUNDING INITIATIVE
(Census series, 1959-78 entry cohorts)

A. White 16-35 Year-Olds



B. Non-white 16-35 Year-Olds



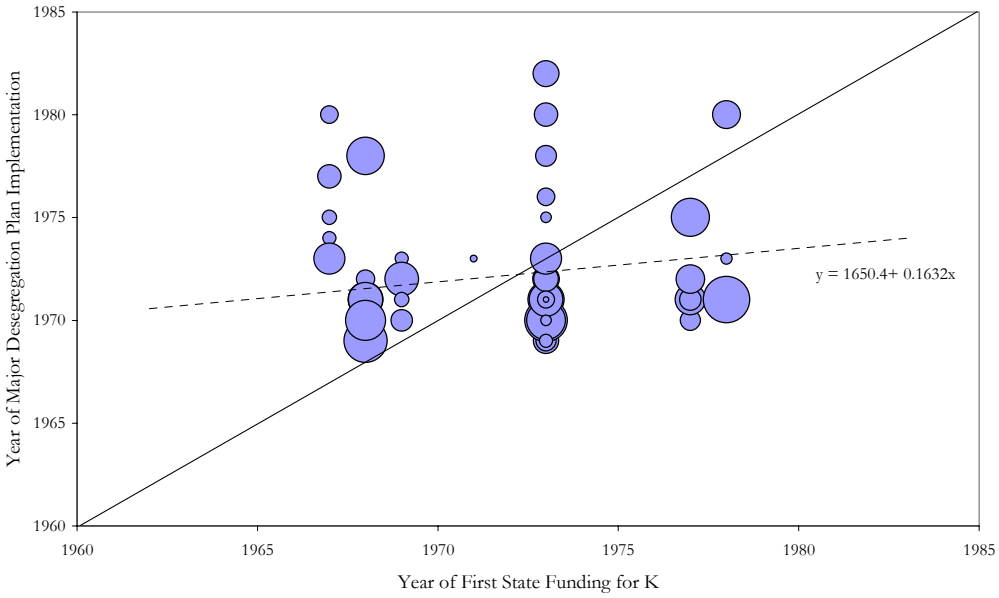
Notes: The figures plot coefficients on Z^j_{cs} (the normalized year indicators from model (2)) from a regression of the high school dropout rate on Z^j_{cs} (j=-7,…,7, with j=-1 the omitted category), cohort fixed effects, state of birth fixed effects, age fixed effects, family background controls (see notes to Table 5) and the log of cohort size. The dashed lines give the 95% confidence interval for the point estimates, where standard errors have been clustered on state of birth by Z_{cs}.

Sample: 16-35 year olds born in the South and West.

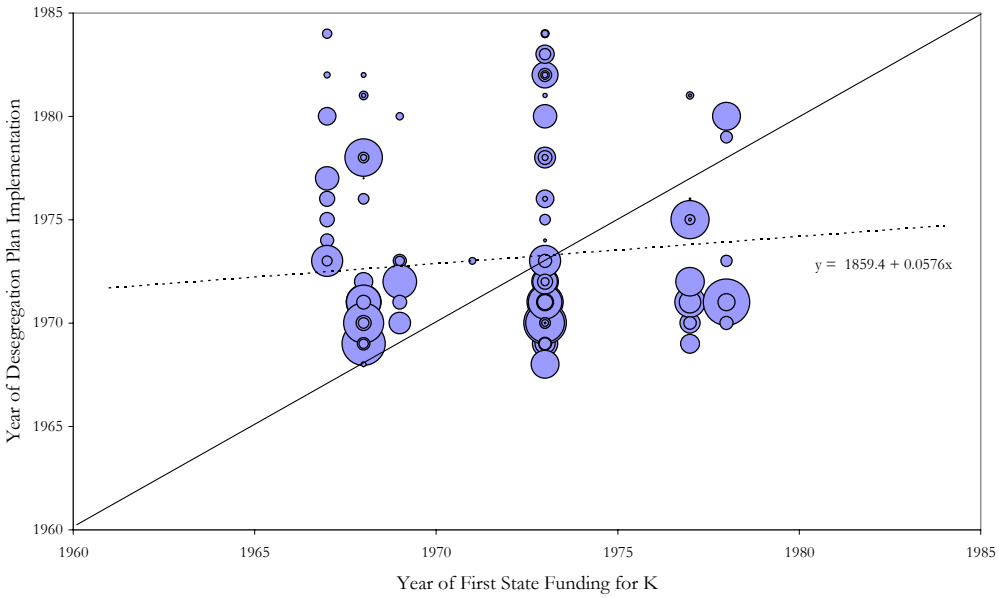
Sources: 1980, 1990, and 2000 Decennial Census PUMS (5% samples)

FIGURE 7 - YEAR OF FIRST STATE AID FOR PUBLIC SCHOOL KINDERGARTEN VERSUS YEAR OF DISTRICT-LEVEL COURT-ORDERED DESEGREGATION PLAN: SOUTHERN STATES

A. Welch and Light (1987) Major Plans



B. All Plans in Welch and Light (1987)



Notes: Size of points represents the reduction in the district racial dissimilarity index in the year following the desegregation plan. The dashed lines are weighted least squares fits, where the weights are the magnitude of this reduction.

Sources: Year of court-ordered desegregation plans and changes in the dissimilarity index are from Appendix Table A3 of Welch and Light (1987). Year of first state aid for kindergarten is as listed in Table 2.