# Do teacher aides help or hurt student achievement? the role of teacher experience

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23 August 2017

**Do Teacher Aides Help or Hurt Student Achievement?**

The Role of Teacher Experience

ABSTRACT

Employment of teacher aides in U. S. public schools increased roughly six-fold since 1969. Yet randomized studies of aides find predominantly negative effects on student achievement. This study of public elementary schools in Oregon explores the role of teacher experience in the effectiveness of aides and finds a strong complementarity between aides and teacher experience.

The complementarity explains two results of prior studies: negative effects for aides and positive effects primarily for early years of teacher experience. Without complementarity, the effect of aides is negative; with it, the effect is positive for schools with experienced teachers and negative only for schools with inexperienced teachers. Similarly, without complementarity, the effect of experience is negative; with it, the effect is positive for schools that use aides intensively and negative only for schools that do not.

The study exploits the longitudinal, hierarchical structure of the Oregon data to estimate a hierarchical linear model with controls for both observed and unobserved influences on individual student achievement. A series of alternative specifications, including a nullification test of causality based on prior test scores suggest robustness for the estimates.

Results of the study suggest that prior evidence for the effectiveness of aides is too pessimistic in the context of experienced teachers and that prior evidence for the effectiveness of teacher experience is too pessimistic in the context of schools that use aides intensively. The results also suggest that experienced teachers have expertise important to effective supervision of aides, particularly in schools where teachers are relatively inexperienced and aides are prevalent.

Hence, attempts to address problems of large class sizes by adding aides are more likely to be effective in schools with experienced teachers.

## INTRODUCTION

A half century ago, most employees in public schools across the United States were classroom teachers, despite the crush of baby-boom students at the time. Today, classroom teachers are in the minority in half the states, displaced by an array of administrative and other support staff (NCES, 2015). Among these groups, teacher aides have grown most rapidly—six-fold since 1969—from two to twelve percent of staff (NCES, 2015, 2016). Richmond (2014) and others attribute the rapid growth in the use of aides to increased pressure on budgets, the expansion of special education, and the federal provision of Title 1 funds for compensatory education in schools with a disproportionate number of disadvantaged students.

Despite the rapid growth in the use of aides, the issue of whether they improve student learning remains controversial, at least outside special education. The most definitive evidence to date comes from two large-scale, randomized studies of elementary students, one for Indiana by Gerber and others (2001), and one for Tennessee by Lapsley and others (2002). Both find predominantly negative effects for aides and only isolated positive effects: the Indiana study only in high SES schools, and the Tennessee study only in reading and only for students with an aide for at least two or three years. An earlier study of the Tennessee elementary schools by Krueger (1999) also found little effect for aides.

These two studies pose two puzzles that motivate ours: why does making a full-time aide available to classroom teachers impede student learning (even if the aides are costless to the schools, as they were for these studies); and why are positive effects for aides found in *high* SES

schools? Research in special education, where aides are most commonly used, suggests the same answer to both questions—complementarity between teacher experience and aides is important. French and Pickett (1997), Grangreco and others (2001), and Wallace and others (2001) all emphasize the skills and experience required for effective supervision of aides. Moreover, the Indiana study notes that both teachers and principals report that teachers assigned an aide made extensive changes to their instructional strategies. Less experienced teachers (either individually, or collectively within a school) may lack the expertise to make such extensive changes effectively, especially since qualifications for aides are typically rudimentary; almost all schools require a high school diploma, but fewer than half require any college or qualifying exams (NCES, 2007).

Complementarity means that aides are less effective for inexperienced teachers and inexperienced teachers are less effective with an aide than without. Hence, the effects of complementarity could resolve the two puzzles. The negative effects for aides in prior studies could result from neglecting the role of teacher experience in the effectiveness of aides, and the positive effect for aides in high SES schools could reflect the greater experience typical of teachers in those schools.

We pursue these issues by accounting for complementarity in our estimates. We rely on dense administrative data for fourth- and fifth-grade students enrolled in public elementary schools in the state of Oregon for the years 2009 through 2015. The data represent a census of students, teachers, aides, and districts each year.

The data link individual students from year to year and to particular schools and grades as long as they remain in public schools, but not to individual teachers or aides. The structure of the data enables us to identify estimates of a longitudinal hierarchical linear model (HLM) similar to

HLM specifications used by Hanushek and Rivkin (2009) and Duckworth and others (2010). As the latter note, the HLM approach aids in controlling for confounding factors by 'differencing out' unobserved factors at various levels in the hierarchy, while still permitting the inclusion of controls for observed time-varying factors. In addition, the longitudinal nature of the approach permits a nullification test for causality by reversing the temporal order of the dependent variable. If current aides affect past student achievement, then a contemporaneous link between aides and achievement is likely spurious, and the nullification test rejects a causal link. Our empirical strategy yields estimates of the direct effects of experience and teacher aides, as well as the indirect effects from complementarity between them.

Foreshadowing our results, we find strong complementarity between teacher experience and aides. Without the effect of complementarity included, as in prior studies, the effect of aides is negative, but with it included, the effect of aides is positive in schools with experienced teachers and negative only in schools with inexperienced teachers. Analogously, the effect of teacher experience is negative without complementarity, but with it, the effect is positive in schools that use aides intensively and negative only in schools that do not.

Our findings suggest that unless estimates account for the complementarity, estimated effects of aides are too low in the context of experienced teachers and too high in the context of inexperienced teachers. Similarly, estimated effects of experience are too low when aides are widely used and too high when they are not. The Oregon data we use do not link individual students to their teachers, or aides, so we are unable to distinguish between the complementary effects of teacher experience due to within-school spillover effects of the average level of experience in the school from those directly due to the individual experience of particular supervising teachers. In any event, the results imply that in schools where aides are widely used,

experienced teachers have valuable expertise in supervising aides that may be useful to others as well, particularly in schools with relatively experienced teachers. Hence, the advantages of teacher experience are larger in schools where aides are widely used than in schools where they are not. Moreover, attempts to address issues of large class sizes by adding teacher aides are more likely to be effective in the context of experienced teachers.

Estimates of the effect sizes for both aides and teacher experience are modest, but the large dimensions of the data yield substantial power and statistical significance.

We begin with a more detailed discussion of prior studies of the effects of aides and follow that with a discussion of our data and empirical strategy before presenting results and robustness checks. We conclude with a discussion of implications and limitations.

## PRIOR STUDIES

We focus on the Tennessee and Indiana studies for two reasons: each reviews other prior studies of aides, something we will not repeat here, and they are the studies most relevant to ours. They also both rely on data from large-scale randomized experiments, in which the data link each student to individual classes taught by teachers with or without the assistance of an aide, so their results provide a useful reference point for ours.

### The Tennessee study.

The study of Tennessee schools by Gerber and others (2001) relies on data from the familiar Project STAR to address three major questions for students in grades K through three. We paraphrase these as 1) does a full-time aide increase student performance, 2) If so, does the effect depend on the number of years the student attends classes with a full-time aide; and 3) are some duties aides perform more important than other duties? While the Tennessee study is expansive,

it does not pursue the issue of whether or not aides are more effective for more experienced teachers (either for individual teachers or for the school as a whole).

On the first question, the study reports a 'plethora of negative findings'. On the second, the study reports that indeed, the sole positive effect for aides is in reading achievement for students who had classes with an aide for at least 2 or 3 years. On the final question. the study reports that the types of duties performed by aides made no difference, but this result does not imply either that the distribution of duties is actually irrelevant for individual teachers and classrooms or that teacher expertise in assigning and supervising the duties of aides is unimportant. The irrelevance of the time spent across types of duties is precisely the outcome one would expect to observe, given the heterogeneity of students, teachers and aides and given limitations on what aides can and cannot do (Kern, 1968). Presumably, supervising teachers seek to assign the distribution of duties they think works best for their students and the relative strengths and weaknesses of their aide. No single distribution of duties is likely to work best for all teachers and aides. Only a systematic difference among teachers in their effectiveness in assigning duties, as possibly between experienced and inexperienced teachers, would yield evidence that the distribution of duties makes a difference. Hence, a more relevant question is whether experienced teachers assign duties more effectively, but our data do not include details of duties that aides perform.

**The Indiana Study.**

The study of Indiana schools by Lapsley and others (2002) relies on data collected for students in grade three as part of the Indiana Prime Time project, which like Project STAR, aimed primarily at identifying the effect of smaller class sizes, or more loosely, pupil-teacher ratios. The study reports a significantly positive effect only for full-time aides is in high socio economic (SES)

schools.[1] Otherwise, effects of aides were negative or insignificant. Average teacher experience is higher in high SES schools, in part because the share of teachers with fewer than three years of experience is almost twice as high in low SES schools (Adamson and Darling-Hammond, 2011). The greater experience of teachers in high SES schools means that aides should be more effective in these schools if teacher experience and aides are complementary, as we find.

## DATA AND EMPIRICAL STRATEGY

### Data.

We rely on administrative data for fourth- and fifth-grade students in Oregon public elementary schools for the years 2009 through 2015. The data link individual students from year to year and to particular schools and grades, as long as they remain in public schools, but not to individual teachers or aides. There are about 100,000 students, 928 schools and 199 districts represented each year in the data. Our analysis focuses exclusively on students, teachers and aides involved in regular instruction. The strengths of the data include their longitudinal, hierarchical structure and density, which lend substantial statistical power to the estimates; an important limitation is that they do not link students, teachers, or aides to each classroom, which prevents identification of the ability to identify classroom-level effects.

We focus on elementary students in the fourth and fifth grades for two reasons. Most prior studies of aides also focus on primary or elementary grades, and our empirical strategy relies on each student's prior year score to control for unobserved student-level factors, and in our data, the third grade is the first in which the state administered the Oregon Assessment of Knowledge and Skills (OAKS).

[1] The Indiana study measures SES as we do, as eligibility for free or reduced lunch.

Table (1) summarizes student test scores, ethnicity, and socio economic status (SES), along with the variables related to aides and teacher experience. SCORE is a composite score for reading and math from the OAKS. We focus on the composite score to reduce confounding school-level heterogeneity in the relative strengths of reading and math programs across schools and time. The ethnicity variables are self-reported and highlight the disproportionately few African Americans in Oregon. Low SES indicates the student is economically disadvantaged, as reported by the school based on participation in the free or reduced lunch program. TEXP represents the school-level average years of teacher experience, and AIDES represents the school-level ratio of aides to teachers in full-time equivalent terms. The mean for AIDES is lower than one might expect because it is in full-time equivalent terms, and part-time aides are common. Even so, AIDES is near unity in over two dozen schools.

**Empirical Strategy.**

The Oregon data are administrative, not randomized, so we pursue a multi-stage strategy to address issues of possible bias from omitted factors or reverse causality.

Our strategy begins with a baseline model that includes variables for teacher experience and aides, the interaction between them, and an extensive set of controls for both observed and unobserved student- and school-level factors. We then subject the baseline model to a series of robustness checks, including a nullification test of causality and conclude with estimates of an expanded model. Results are robust at each stage.

*Baseline Model.*

Our baseline model for the SCORE of student i in grade g of school s in year t is:

$$[SCORE_{igst} = a_s + a_t + a_{gt} + b_1 SCORE_{igst-1} + b_2 D_{igst} + b_3\ TEXP_{st} + b_4 AIDES_{st} + b_5 TEXP_{st}\ x AIDES_{st} + e_{igst}]$$

The (a) terms represent unobserved fixed effects respectively, for each school, year, and grade by year. ($b_1$) is the coefficient for SCORE from the prior year. ($b_2$) is a vector of coefficients for the student demographic variables. ($b_3$) is the coefficient for the direct effect of TEXP. ($b_4$) is the coefficient for the direct effect of AIDES, and ($b_5$) is the coefficient for the interaction between TEXP and AIDES. The final term (*e*) is the error. The effect size we derive from ($b_3$), the direct effect of AIDES, provides a useful reference point to the corresponding effect sizes reported in the Tennessee and Indiana studies.

The sign of ($b_5$) is our primary focus—a positive sign indicates complementarity between teacher experience and aides. The total effect for each is the sum of this coefficient (weighted by an appropriate value for the other factor) and the direct coefficient. We expect the direct coefficient to be negative or insignificant for both AIDES and TEXP, so their total effects rise from negative (or insignificant) to positive as the level of the other rises if complementarity is significant.

A more detailed explanation of the terms for unobserved effects may be helpful. The school-specific term accounts for all those factors unique to a particular school that persist over time in our data. The year-specific term accounts for all those unobserved factors unique to each year that are common across schools in our data. For example, Oregon's school funding equalization formula equalizes the level of funding per student across similar schools, but that level of funding varies from year to year for all schools. Similarly, the grade-by-year term accounts for all those unobserved grade-level factors unique to each year that tend to be common to all schools, such as state-mandated changes in curriculum and changes in the OAKS exam.

Note that we are not able to include either grade-by-school or school-by-year effects because we only observe school-by-year variations in AIDES and TEXP. Hanushek and Rivkin (2009) note that their results for teacher experience are not sensitive to whether school-by-year effects are included, as long as grade and year effects are also included. We address this and other issues of bias by subjecting estimates of the baseline model to a series of robustness checks, including a nullification test of causality.

*Expanded Model.*

We conclude our estimation strategy by presenting estimates of an expanded model. We cannot include school-by-year effects because we only observe school-by-year variation for teacher experience and aides, but we are able to add district-by-year effects in an expanded model because districts typically have multiple elementary schools. Thus, we are able to estimate an expanded model, extended to include district-by-year effects, as well as a school-by-year term for school-average SES. The latter captures potentially important school-wide effects of changes in the SES composition of students. The former captures effects of unobserved factors unique to each district in each year but common to schools in the same district. Both the former and the latter are significant in the expanded model, but results are otherwise similar.

RESULTS

**Baseline Model.**

The first stage of our multi-stage empirical strategy is to estimate the baseline model. Table (2) presents regression estimates of this model in the first column of results (we turn to the second column of results for the extended model after several robustness checks of the baseline model). With an R-square of (0.591), the explanatory power of the baseline model is relatively high, with

much of the explanatory power arising from the prior test score. School, grade-by-year and year effects are all significant at the 0.05 level, as are all the coefficients related to AIDE and TEXP. As expected, the direct effects of AIDES and TEXP are both negative, and the indirect effect of the interaction between them is positive, consistent with complementarity. We defer a discussion of total effects, which require summing the direct and indirect effects for particular values for the variables until we present calculations of effect sizes in Tables (3) and (4) below. Results for the prior test score and student demographics are routine and not of direct interest, so we omit them for brevity in Table (2).

**Robustness.**

We probe the robustness of the results from the baseline model in several ways. Adding the school-by-year ratio of students to teachers yields similar results, as does adding interactions between AIDES and either Low SES or the ratio of students to teachers. (The individual interactions are also insignificant.

In addition, we perform a nullification test of causality by testing if current values of AIDES or TEXP affect the prior test score. If they do, then the test rejects a causal link, since a causal link between instruction and achievement before students received the instruction is implausible. We perform the test by swapping the places for the current score and prior score in the baseline model, so that the prior score is the dependent variable, and the current score an independent variable, with all other variables the same. Note that the critique in Chetty and others (2014) does not apply here because the prior score does not appear in the construction of any independent variables.

After estimating this nullification specification, we find no significant link between the prior score and current values of AIDES, TEXP, or their interaction, so the nullification test fails to reject a causal link.

**Expanded Model.**

As a final robustness check, we estimate an expanded model that adds both district-by-year effects and school-by-year average SES to the baseline model. The failure of the nullification test to reject causality suggests that the expanded model should yield qualitatively similar results.

If we turn again to Table (2), we see that results from the baseline and expanded models are indeed, qualitatively similar in signs and significance, even though the additional variables are both significant and the R-square rises to (0.603). Both the negative direct coefficient for AIDES and the positive interaction coefficient are larger in absolute value in the expanded model, but these changes tend to be offsetting and are easier to assess in terms of the total effect sizes discussed below.

**Effect Sizes.**

Consistent with the Tennessee and Indiana studies, we calculate effect sizes for a full-time aide (AIDES=1). Although an aide ratio of one is very high among the schools in our data, it is within the range we observe and applies to roughly two dozen schools.

Table (3) presents effect sizes for AIDES. Effect sizes depend on the level of teacher experience in this case, so we calculate the effect size for three levels of TEXP. One is the direct effect with no complementarity (for TEXP=0), a second is the total (direct plus complementary) effect for average TEXP (TEXP=9.7), and a third the total effect for TEXP one standard deviation above the average (TEXP=11.4).

The general pattern of significance and magnitude of the effects in Table (3) is similar for the two models and consistent with our expectations: the effect of a full-time aide is negative for schools with inexperienced teachers, but increasingly positive (less negative) as average teacher experience rises.

The largest and only significant difference in the effect sizes for the base and expanded models is for the direct effect (when there is no complementarity from a positive level of teacher experience). In this case, the effect size is significantly negative for both models, but more than twice as large in the expanded model.

It is worth noting that this effect size (-0.18) falls near the center of the range of negative effect sizes found in the fully randomized Tennessee study (-0.13 to -0.20), which adds weight to the robustness of the estimates here.

How large are these effects in terms of educational relevance? Lipsey and others (2012) suggest measuring relevance relative to effect sizes for economic disadvantage and other student demographic factors. Measured in these terms, the effect size for aides in the context of highly experienced teachers is substantial, roughly the same as the effect size for African American ethnicity and about a quarter of the effect size for economic disadvantage. Similar comparisons in the context of less experienced teachers are proportional and easily inferred.

Table (4) presents analogous effect sizes for teacher experience, one for the direct effect without complementarity (AIDES= 0), a second with complementarity and an average aide ratio (AIDES= 0.4), and a third for with complementarity and an aide ratio of unity (AIDES=1.0). Again, the effect sizes do not differ significantly for the two models.

As expected, the effect of TEXP is negative at low ratios of aides to teachers but increasingly positive (less negative) as the ratio of aides rises. This pattern provides important context for interpreting the negative or insignificant effects found for teacher experience in prior studies: the returns to teacher experience are significantly positive in schools that use aides intensively, and negative or insignificant only in schools that do not.

Measured in terms of educational relevance, the effect for experience with full-time aides is about a quarter of the effect size for African American ethnicity and about a tenth the effect size for economic disadvantage—small, but nontrivial effects. Similar comparisons are proportional and easily inferred for the other levels of aides in Table (4).

## CONCLUDING REMARKS

Briefly put, our results indicate that aides are more effective in schools with more experienced teachers and experienced teachers are more effective in schools where aides are more prevalent. The results imply that prior negative results for the effectiveness of aides are too pessimistic in the context of experienced teachers and that experienced teachers have expertise important to effectively supervising aides, expertise that may be useful to other teachers, particularly in schools where teachers are relatively inexperienced and aides are prevalent. They also imply that attempts to address problems of large class sizes by adding aides will be more effective in schools with experienced teachers.

Our data are administrative, not experimental, which raises serious issues for validity. Fortunately, a series of robustness checks, including a nullification test for causality lends support to validity. Moreover, our estimate for the direct effect of full-time aides (that is, the

effect without complementarity) is strikingly similar to the direct effect found in the Gerber and others (2001) study of Tennessee schools based on randomized data.

Even so, the complementarity we find raises issues our data cannot resolve. One is how much of the complementarity is due directly to the benefits to the experience of individual teachers with an aide and how much to within-school spillovers of those benefits among teachers. Another is whether duty assignments for aides made by experienced teachers are more effective than those made by inexperienced teachers.

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**Table 1. Summary Statistics**

(grades 3 and 4 Oregon public schools 2009-2015)

| *student demographics* | mean | std dev |
|---|---|---|
| amer ind | 0.019 | 0.135 |
| Asian pacific | 0.041 | 0.198 |
| African am | 0.026 | 0.158 |
| Hispanic | 0.21 | 0.407 |
| white | 0.658 | 0.474 |
| multiethnic | 0.04 | 0.196 |
| decline to respond | 0.007 | 0.082 |
| low SES | 0.546 | 0.498 |
| | | |
| *focus variables* | | |
| SCORE | 440 | 14 |
| AIDES | 0.36 | 0.29 |
| TEXP | 9.7 | 1.7 |

***Notes***

See text for details of the data and variables.

Abbreviated ethnicity identifiers are from the data.

Low SES indicates the student is from an economically disadvantaged family, as reported by the school.

SCORE is a composite score for reading and math on the Oregon Assessment of Knowledge and skills (OAKS).

AIDES is the school ratio of aides to teachers by FTE.

TEXP is the school average years of teacher experience.

**Table 2. Estimates**

(grades 4 and 5, Oregon public schools, 2009-2015)

*robust standard errors below coefficients*

| VARIABLES | BASELINE MODEL | EXTENDED MODEL |
|---|---|---|
| AIDES | -1.047* | -2.538* |
| | 0.220 | 0.364 |
| TEXP | -0.150* | -0.138* |
| | 0.016 | 0.024 |
| AIDESxTEXP | 0.173* | 0.294* |
| | 0.028 | 0.053 |
| PRIOR SCORE | yes* | yes* |
| STUDENT DEMOGRAPHICS | yes* | yes* |
| SCHOOL EFFECTS | yes* | yes* |
| YEAR EFFECTS | yes* | yes* |
| GRADE BY YEAR | yes* | yes* |
| SCHOOL SES | no | yes* |
| DISTRICT BY YEAR | no | yes* |
| RSQ | 0.591 | 0.603 |
| NOBS | 635,633 | 635,633 |

**significant at 0.05*

***Notes***

See Table (1) and text for details of the data and variables.

The Dependent variable is a composite score for reading and math for fourth- and fifth-graders on the OAKS test.

AIDES is the school ratio of aides to teachers by FTE.

TEXP is the school average years of teacher experience.

Student demographic variables include gender, ethnicity, SES and NEW, whether the student is new to the school.

**Table 3. Effect Sizes, Full-Time Aide**

(4th and 5th grades, Oregon public schools)

| *Mean Teacher Experience (TEXP)* | Base model | Extended model |
|---|---|---|
| high TEXP=11.4 | 0.07* | 0.06* |
| mid TEXP=9.7 | 0.05* | 0.02* |
| low TEXP=8.0 | -0.02* | -0.02* |
| no interaction/ TEXP=0 | -0.07* | -0.18* |

*significant at 0.05

Effect sizes calculated from Table (2) and

Expressed in standard deviations of SCORE,

A composite test score for reading and math.

See text and Tables (1) and (2) for details.

**Table 4. Effect Sizes, Teacher Experience**

($4^{th}$ and $5^{th}$ grades, Oregon public schools)

| Aide ratio | base model | extended model |
|---|---|---|
| 1.0 | 0.00 | 0.02* |
| avg 0.4 | -0.01 | -0.00 |
| 0.0 | -0.02* | -0.02* |

*significant at 0.05

Effect sizes are calculated from Table (2) for one year of TEXP (school avg. teacher experience) and expressed in standard deviations of SCORE, a composite score for reading and math.

See text and Tables (1) and (2) for details.