Early Academic Performance, Grade Repetition, and School Attainment in Senegal: A Panel Data Analysis

Peter Glick and David E. Sahn

Little is known in developing country environments about how a child's cognitive skills manifested in the first years of schooling are related to later educational success, because the panel data needed to analyze this question have been lacking. This study takes advantage of a unique data set from Senegal that combines test score data for children from the second grade with information on their subsequent school progression from a follow-up survey conducted seven years later. Measures of skills from early primary school, corrected for measurement error using multiple test observations per child, are strongly positively associated with later school progression. A plausible interpretation is that parents invest more in a child's education when the returns to doing so are higher. The results point to the need for remedial policies to target lagging students early on to reduce early dropout. Grade repetition policies target poorly performing students and are pervasive in Francophone Africa. Using variation across schools in test score thresholds for promotion to identify the effects of second-grade repetition, the analysis shows that repeating students are more likely to leave school before completing primary school than students with similar ability who are not held back, pointing to the need for alternative measures to improve the skills of lagging children. JEL codes: I21, O15

Reflecting widespread recognition of the importance of human capital for individual welfare and economic growth, an enormous amount of empirical research in developing countries has examined the determinants of household investments in children's schooling.[1] However, little is known about the dynamics of schooling, in particular, the relationship of early cognitive skills to later education outcomes. In developing countries, where households are likely to be resource and credit constrained, decisions on how long to continue a child's education may depend strongly on the perceived returns to schooling

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1. See Glewwe and Kremer (2006) and Glick (2008) for reviews of the literature.

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and, in turn, on a child's apparent ability. The effect of ability may be particularly large among poorer families in such environments, for whom resource or credit constraints are likely to be more binding. Knowledge of the nature and extent of these patterns would be highly relevant for policy. In particular, a strong connection between early achievement as manifested by test scores and subsequent school attainment would point to the need for policies directed at children who fall behind academically early in their school careers.

Knowledge of this relationship in developing country contexts is very sparse, primarily because the data needed to analyze this question have been lacking. The vast majority of studies of schooling in developing countries rely on cross-sectional data. Dynamic analyses of school attainment, however, require panel data containing both early measures of skills and later schooling outcomes. Surveys with this information are rare in developing countries, especially for Africa.

This study takes advantage of unusual panel data from Senegal that combines test score data for children from the second grade with information on their school progression from a follow-up survey conducted seven years later. With information on school and household characteristics, the analysis is able to control for confounding influences on school attainment in estimates of the effects of early academic performance on later education outcomes as well as determine whether wealth and school differences exacerbate or mitigate the impacts of early achievement gaps among children.

The study also exploits the panel to assess the impacts of early grade repetition—a pervasive policy targeting poor achievers—on later school attainment. While repetition normally must be considered as jointly determined by factors that also influence attainment, this study is able to take advantage of the fact that, conditional on observed academic ability at the end of second grade, having to repeat second grade is reasonably considered exogenous to student ability or effort.

Indicators of early cognitive ability, corrected for measurement error using multiple test observations per child, are strongly positively associated with later school progression. If interpreted causally, this result is consistent with a model of human capital investments under certain assumptions about returns to schooling in the labor market. Household wealth and parental (father's) schooling also have (conditional on early measured ability) direct positive effects on attainment, and there is some evidence that the impact of academic ability on later school outcomes differs for children in poor and well-off households, as well as between girls and boys. The large impact of early test scores on later attainment, controlling for family and school characteristics, points to the importance of school policies that direct early remedial attention to low-achieving children. Finally, conditional on academic ability, repeating a grade has a negative impact on school progression, implying that the private costs associated with stringent repetition policies exacerbate the negative effects on attainment of poor early academic outcomes.

I. Conceptual Framework

A simple model of child ability and household schooling investments helps frame the empirical analysis. The framework shows that making predictions about the relationship of ability to schooling attainment (or about interactions of household wealth and ability in schooling demand) is not straightforward without specific assumptions about the shape of the schooling returns functions. Consider a two-period model of parental investment in the education of two children of differing ability, A^i (= 1,2), as measured, for example, by tests early in school, and assume $A^1 > A^2$. For the exposition, assume that A^i represents the ability endowment of the child given exogenously to the parents (as discussed below, this cannot be assumed for the test score measures used in this study). For a pure investment model in which parents view schooling only as a means to future consumption through transfers from children's period 2 wealth (W^i), the lifetime utility of the parents is written as:

$$(1) \qquad U = U_1\left(C_1, C_2\left[W^1(S^1, A^1), W_2(S^2, A^2)\right]\right)$$

where C_t is consumption in period t, and W^1 and W^2 are functions of individual child ability and the level of schooling S^i. The parents maximize equation (1) subject to the intertemporal budget constraint

$$(2) \qquad C_1 + \frac{C_2}{1+r} = Y_1 + \frac{W^1(S^1, A^1)}{1+r} + \frac{W^2(S^2, A^2)}{1+r} - P^1S^1 - P^2S^2$$

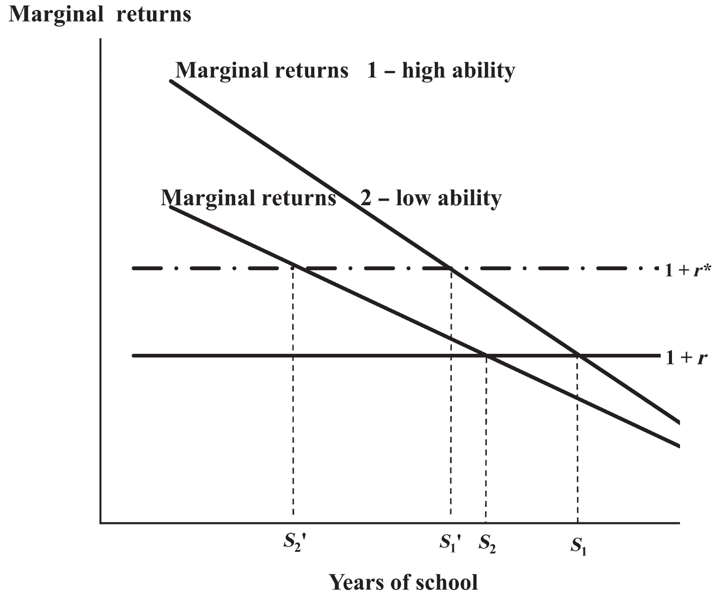
where P^i is the unit cost of schooling for child i. The first-order conditions imply (in the absence of credit constraints) that parents invest in the education of each child until the marginal return equals the market rate of interest:[2]

$$(3) \qquad \frac{1}{P^1}\frac{\partial W^1(S^1, A^1)}{\partial S^1} = \frac{1}{P^2}\frac{\partial W^2(S^2, A^2)}{\partial S^2} = 1 + r.$$

If costs are the same, the difference in the optimal levels of S^1 and S^2 will depend on how ability affects the schooling return functions. A standard assumption in models of human capital investment is that returns diminish with additional schooling, which can be captured using the quadratic form $W = \alpha_1 S - \alpha_2 S^2$ ($\alpha_1 > 0$, $\alpha_2 > 0$). A simple way to allow returns to be influenced by ability is to assume that returns to the low-ability child, relative to those to the high-ability child, are reduced proportionately by some factor β ($0 < \beta < 1$), so returns for the low-ability child are $\beta(\alpha_1 S - \alpha_2 S^2)$. This yields marginal return functions $\alpha_1 - 2\alpha_2 S$ for the high-ability child and $\beta\alpha_1 - 2\beta\alpha_2 S$ for the low-ability child (figure 1). At any level of schooling, marginal returns are higher for the high-ability child, so for any interest rate, investments

2. For the exposition, direct schooling impacts on utility are ignored so this becomes a pure investment model.

FIGURE 1. Higher Marginal Returns for Higher Ability Child and Larger Income Effects for Low-Ability Child



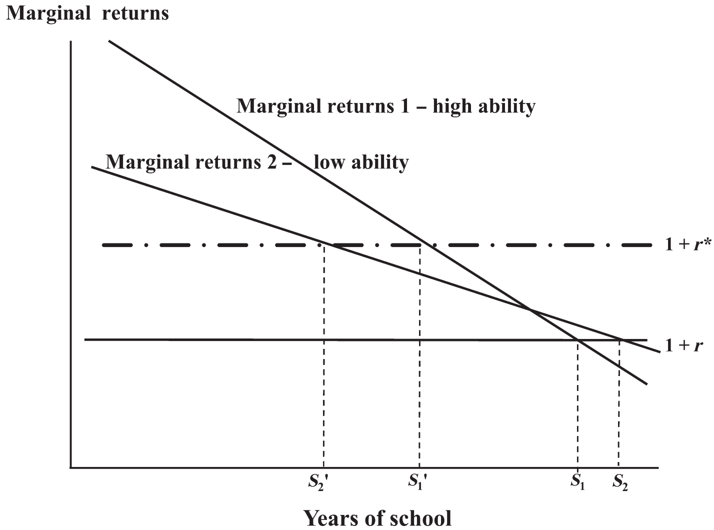
Source: Authors' analysis based on data from the PASEC surveys and the 2003 EBMS; see text for details.

in that child's schooling are greater. If households are credit constrained, a similar result would be obtained, but marginal returns would be equated to the parents' marginal rate of substitution of future for current consumption, the shadow interest rate (r^*).

While this outcome seems intuitive, it depends on the form of the returns functions, as pointed out by Behrman (1997). Alternative and no less arbitrary assumptions about how ability influences human capital production could yield a different outcome. One could flexibly represent quadratic returns to the low-ability child as $\gamma\alpha_1 S - \delta\alpha_2 S^2$, with $0 < \gamma < 1$, $0 < \delta < 1$, implying marginal returns $\gamma\alpha_1 - 2\delta\alpha_2 S$. Specific values of these parameters would produce marginal returns that are larger for the high-ability child over most of the schooling range but eventually become larger for the low-ability child, as depicted in figure 2. Thus, at a low enough interest rate (such as r in the figure), schooling investments will be greater for this child. Therefore, the relationship of total schooling and child ability is conceptually ambiguous.[3] Consumption-based considerations would add more ambiguity. For example, parental preferences for

3. The likelihood that the marginal return functions cross as in figure 2 increases if the high-ability child is also more productive in home or work activities. This raises the opportunity cost of schooling of that child relative to the low-ability child and, through the increase in cost P in equation (3), shifts down the marginal returns for the more able child.

FIGURE 2. Switch in Relative Marginal Returns for High- and Low-Ability Children, and Larger Income Effects for Low-Ability Child



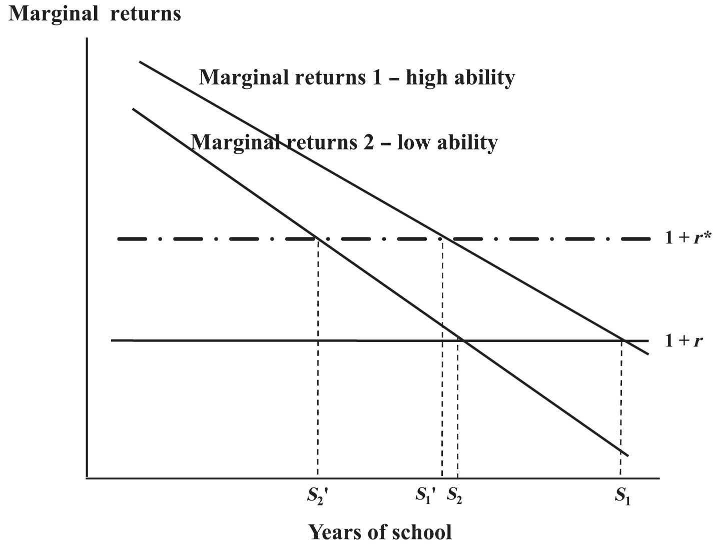
Source: Authors' analysis based on data from the PASEC surveys and the 2003 EBMS; see text for details.

equity might lead to investing more resources in low-ability children to compensate for their otherwise lower future income and welfare.

Next, consider another question of interest for the empirical analysis: how the response to ability may be affected by the level of household income—essentially, the sign on an interaction term of an ability measure and household income or assets in a model estimating schooling attainment.[4] In a pure investment framework with no borrowing constraints, there should be no interaction, since investments occur until marginal returns equal the interest rate. But credit constraints are likely, and these should impinge more on households with low current resources. For a credit-constrained (poor) household, the rate of interest is determined as indicated by the marginal rate of substitution of future for current consumption. If the constraint binds, this shadow interest rate r^* will be higher than the market rate r. This is shown in figure 1, with schooling of the two children under the binding constraint equal to $S^{1'}$ and $S^{2'}$. The effect of a sufficiently large increase in income is to relax the credit constraint so that marginal returns are equated to r rather than r^*. While in both the poor (constrained) and better-off household, the higher ability child receives more schooling, this gap gets smaller as income increases, because (given the nature of the quadratic returns functions) marginal returns are falling less steeply for the low-ability child (see figure 1). Therefore, it takes more additional investment to reach equilibrium for that child at the new lower interest rate.

4. The analysis here is formally similar to Garg and Morduch's (1998) analysis of the effects of changes in income on girl–boy gaps in health investments.

FIGURE 3. Higher Marginal Returns and Larger Income Effects for High-Ability Child



Source: Authors' analysis based on data from the PASEC surveys and the 2003 EBMS; see text for details.

The returns functions depicted in figure 2 yields this result as well. Once again, however, different assumptions about how ability affects the returns to schooling imply different outcomes. If, as in figure 2, returns to the low-ability child are represented by the quadratic $\gamma\alpha_1 S - \delta\alpha_2 S^2$ but with a sufficiently smaller absolute value of γ (or larger value of δ), returns could appear as in figure 3. In this case, the low-ability child receives less schooling both at low and high incomes, but the gap widens with income, because the marginal returns fall more steeply for this child. Again, this ambiguity occurs in a pure investment framework without resort to assumptions about parental preferences, though these would add further uncertainty to predictions. For example, if inequality aversion were a normal good, increases in income could lead to greater additions to investments in the low-ability child even if credit constraints were not operative.

Therefore, the relationship of early manifested ability and subsequent school investments, and the role of household resources in modifying the effects of ability on investments, are empirical questions. They can be addressed here because of the availability of both test score measures of early ability and information on subsequent schooling.[5]

5. However, a disadvantage of the data used for this study is that they do not have this information for multiple children in the same family, so household fixed effects cannot be used to eliminate unmeasured household factors associated with both ability and later outcomes. The heterogeneity issue is discussed in the next section.

II. Data and Empirical Approach

The 2003 Senegal Household Education and Welfare (EBMS) survey[6] was conceived as a follow-up to an earlier, school-based, study, the Program on the Analysis of Education Systems of the Conference of Francophone Ministers of Education (PASEC). The PASEC study administered tests of written math and French to a cohort of students beginning in second grade in 1995 and continuing through subsequent years of primary school (see CONFEMEN 1999; Michaelowa 2003). The 2003 survey attempted to re-interview these children, who were now of middle school age (ages 14–17). Of the original 120 PASEC clusters, 60 (28 urban and 32 rural communities) were randomly selected for the new survey. Of the 20 PASEC children per cluster/school who were tested in second grade in 1995–96, survey enumerators were able to find, on average, 15 children in rural clusters and 17 in urban ones. It should be noted that the sample, by construction, selected on children who made it into second grade, which eliminates a nontrivial share of children who do not make it that far (primarily because they never entered school).[7]

The present analysis combines the test score information and detailed school and teacher information from the PASEC survey with information from the 2003 EBMS survey, which contains detailed information on household characteristics and child schooling. Information was also collected on the schooling of children who no longer reside at home. This is important since 16 percent of the PASEC cohort was no longer living at home in 2003, and this behavior is likely to be endogenous to schooling outcomes (for example, a young person may live with relatives to attend secondary school, or leave school and home to marry or find work).[8]

In simplest form, the models estimated can be represented as follows:

$$S_i = a_0 + a_1 A_{95i} + a_2 \mathbf{X}_i + a_3 \mathbf{Q}_i + a_4 A^*_{95i} \mathbf{X}_i + a_5 A^*_{95i} \mathbf{Q}_i + e_i, \quad (4)$$

where S_i is the later schooling measure (grade attainment reported in 2003) of child i, A_{95i} is ability measured in 1995–96 (test score in second grade), $\mathbf{X}_i$ is a vector of individual and household characteristics, and $\mathbf{Q}_i$ is a vector of school inputs such as teacher background and supplies.

6. EBMS was a joint research project of Cornell University (United States), Centre de Recherche en Economie Appliquée (Senegal), and Institut National de la Recherche Agronomique (France).

7. Randomly selected households with children of similar age in the same school catchment areas were also interviewed in the EBMS survey to achieve greater representivity and a larger sample. Of this group, 24 percent did not enter second grade, almost all of whom never enrolled. Since this analysis focuses on the links of early academic success and later school outcomes, only the PASEC cohort, for which early test score information is available, is used.

8. For children not living at home, information was collected on highest completed grade and current enrollment but not on school entry and exit ages, so total years in school cannot be calculated for these children. Therefore, the highest completed grade is used as the dependent variable in the analysis. It will differ from total years of school because of grade repetition. The implications of repetition are discussed in section IV.

The coefficients must be interpreted carefully and will depend in part on how A_{95i} is represented. One option is to use actual test scores from the 1995 to 1996 school year. Using the second-grade pretest (given at the start of the school year) provides a measure of ability at the end of the child's first year of school, thus coming close to measuring ability at the time of school entry. However, in addition to measuring a child's inherent ability 'endowment,' these pretest scores also reflect school and home inputs into learning during first grade and home inputs during the preschool period that lead to better cognitive development; nutrition is likely to be particularly important (Glewwe and Miguel 2008). If, further, these unmeasured inputs also directly affect later attainment or are correlated with factors that do (such as parental preferences), this will upwardly bias the estimated effect of early achievement. Regarded as associational rather than as strictly causal, the relation of early performance to school attainment is still relevant for policy. Test score information is relatively easy to obtain and could potentially be used to direct remedial action to academically lagging children.

An alternative to test scores is the 'residual method', in which the unexplained portion in a production function regression for human capital (with the test score as the dependent variable) is taken as the individual's genetic ability endowment. In this approach an equation such as the following is estimated:

$$TS_i = b_0 + b_1\mathbf{X}_i + b_2\mathbf{Q}_{95i} + v_i. \qquad (5)$$

where TS_i is the 1995 pretest score and the residual v_i is the endowment—ability purged (in contrast to the test score itself) of the effects of measured household characteristics $\mathbf{X}_i$ and grade-specific school factors $\mathbf{Q}_{95i}$such as teacher background and class variables (note that $\mathbf{Q}_{95i}$differs from Q_i in equation (4), which would incorporate all years and teachers of the child at the school). Conceptual models of parental decision-making would generally assume that parents are able to assess the child's ability as represented by v_i and make investment decisions based on it. For modeling behavior, then, this may be the preferred measure. Estimation of equation (5) requires detailed school, teacher, and household data, which the Senegal surveys have.

However, there are shortcomings with using this measure of A_{95i} in equation (4) as well. Since only measured school and household inputs are controlled, the residual will pick up the effects of unmeasured household or school factors. Moreover, these may be correlated with S_i, leading to endogenity problems in estimating the effect of "pure" ability, just as with early test scores. A second problem, relevant to either ability measure, is measurement error. It is easy to see how a single test can be a noisy measure of a student's knowledge. In particular, performance could be highly dependent on the students' disposition on the day of the test. If this source of error is random, it will, all things equal, bias toward zero the estimated impacts of early test score (or similarly, endowment as measured by the residual).

Fortunately, the PASEC data permit the use of an instrumental variables procedure to deal with this problem and correct for (nonsystematic) measurement error. For each second-grade student there are multiple—potentially four—test scores: French and math pretests, and French and math posttests (conducted at the end of the school year). Say that test score TS_1 is being used as a measure of cognitive ability and that the relationship between them takes the form $A_{95i} = \alpha_0 + \alpha_1 TS_{1i} + \mu_{1i}$, where μ_i captures the noise in the relationship. An additional test TS_2 provides another proxy for ability: $A_{95i} = \beta_0 + \beta_1 TS_{2i} + \mu_{2i}$. If measurement error is uncorrelated across these indicators ($\mathrm{Cov}(\mu_{1i}, \mu_{2i}) = 0$), their correlation is due only to their common dependence on A_{95i}. A consistent estimate of a_1 in equation (1) can then be obtained by instrumenting for one test score using the value of the other.[9] The method is applicable as well if the residual approach is used (Pitt, Rosenzweig, and Hassan 1990); in that case, one test score residual is used as an instrument for the other.

The data provide several options: use the French second-grade pretest to instrument the math pretest score (and the reverse), or use the second-grade posttests to predict pretest scores. The first approach appears less desirable. Since the two pretests were given at the same time, measurement errors are likely to be correlated (perhaps because of the child's disposition on that day), violating the key assumption for the instrumental variable strategy. A second issue is that a nontrivial number of students are missing test scores for one of the pretests. For these reasons, the posttest average of the math and French scores (or test score regression residual) are used to predict the average of the pretest math and French scores (or residuals); averaging the two permits the inclusion of cases where only one test was taken in either or both periods. The less attractive aspect of this choice is that the posttest score instrument will incorporate the effects of school inputs in second grade (and the posttest residual will capture impacts of unmeasured inputs during this period). Finally, some 6 percent of children in the sample took the posttests but not the pretests. Estimates from the predicting equation are used to impute the pretest score for these cases.

It should be stressed that this instrumental variables strategy deals with measurement error but not with any potential endogeneity-related bias in the estimate of a_1, which would come about if unmeasured school or home inputs are correlated with both initial achievement and subsequent attainment.[10]

9. This is the multiple indicators approach (see Wooldridge 2002, for a general presentation). It is noteworthy that even with a correlation between the error terms of the indicators (systematic measurement error), Bound, Brown, and Mathiowetz (2001) show that the multiple indicator instrumental variables solution will still generally be a more consistent estimator of the true parameter of interest than will ordinary least squares estimation using the indicator with measurement error.

10. Potential instruments for early academic achievement in the data, at least for the second-grade posttest score, include second-grade teacher variables related to qualifications and pedagogical practices, provided these are truly idiosyncratic and vary across teachers/grades within a school. While some of these factors are indeed associated with the posttest outcomes, these instruments had low power and resulted in implausible or highly nonrobust second stage estimates of the effect of early test score on grade attainment.

The fact that the measured relation of later success to early achievement may be associational rather than strictly causal is not necessarily a hindrance from a policy perspective. As already noted, knowledge of this association would make it possible to target early on children who are otherwise likely to do poorly later. For a behavioral interpretation, however, heterogeneity bias is clearly a greater concern. A partial correction is to estimate school fixed effects, which purge estimates of bias from linear associations of school-level unobservables with the included covariates. It is plausible that unmeasured household preferences and child ability operate in significant part through the choice of school (and implicitly through location since school and cluster are interchangeable in the PASEC survey). The fixed effects estimates will eliminate bias from this source. They will not, obviously, account for heterogeneity among children *within* schools. Therefore, it cannot be claimed that the estimates of the effects of early skills represent purely causal effects.[11]

With respect to the functional form of equation (4), ordered probit is used to model the determinants of years of completed schooling. The ordered probit model treats grade attainment as the outcome of a series of ordered discrete choices. The model is easily extended to allow for right censoring of the dependent variable, an important concern since some 60 percent of the cohort is still attending school.[12]

Two alternate specifications are estimated, reflecting different assumptions about unobserved (school-level) heterogeneity: random effects and (as just described) fixed effects. The school random effects model directly parameterizes the within-school correlation of errors but assumes that these are not correlated with included regressors. Estimation of the random effects model involves specifying the likelihoods conditional on the school-level random effect, then integrating over all possible values of the random effect to obtain the unconditional likelihood. This is done using Hermite integration, following the approach suggested by Butler and Moffit (1982) (for further details, see Glick and Sahn 2000). The fixed effects specification, in contrast, does not require the orthagonality assumption on the school-level errors; it eliminates any linear associations of the errors with included covariates by including dummy variables for each school. If the correlation of school-level covariates

11. It is useful to compare the approach of this study with several other analyses (Glewwe and Jacoby 1994; Alderman and others 1996; Kingdon 1996) on cross-sectional data that estimate school attainment or skills using contemporaneously measured ability as captured by Raven's abstract thinking test. This is usually presented as a measure of innate (hence preschool) ability, but as Glewwe and Kremer (2006) note that interpretation is implausible because the test is likely to also reflect an individual's schooling. Therefore, these studies do not provide strictly causal estimates of the impact of innate ability (as the present study does not), nor do they permit an understanding of how later education success is related to ability measured at or near the start of school (which, in contrast, the panel data in this study do permit).

12. As almost no children report a grade higher than 9 by the 2003 survey, the top category is aggregated as grade 9 and above.

and the error term is in fact zero, however, the fixed effects model is less efficient than the random effects model.[13]

Since estimation in the fixed effects model is based only on within-school variation, the effects of school factors that are constant across children within a school are not estimated. Nevertheless, the impacts of teacher characteristics and classroom supplies can be estimated because they retain some variation across children within the same school. Specifically, the 1995–96 school surveys included detailed information on teacher background and classroom characteristics and supplies for the original second-grade PASEC class and for subsequent primary grades attended by the cohort. Basic teacher and class information was also collected on at least one class containing grade repeaters in subsequent years (for example, a second-grade class in 1996–97, when non-repeaters would be in the third grade). The ordered probit regressions include the averages for these variables for each student enrolled in classes for the school years for which complete information is available: 1995–96,1996–97, and 1997–98 (there will be fewer than three years of data for early dropouts or absent children).[14] The paths of individual children diverge, because some children repeat one or more grades while others progress smoothly to the next grade (while some drop out), so there is some within-school (across child) variation in teacher characteristics and classroom supplies measures.

Information on director background and school facilities was collected only in 1995. Because of the large number of potential covariates for classroom supplies and facilities, principal components analysis was used for data reduction. For classroom supplies, the variables used were presence of a dictionary, ruler, compass, and eraser; for school facilities, the variables were presence of a library, toilet, drinking water source, and canteen. The first principal component in each case was included in the school attainment models. Finally, as the household survey did not collect consumption or income data, factor analysis was used to construct a wealth index from information on ownership of durable goods (see Sahn and Stifel 2003 for details of the method). Table 1 presents descriptive statistics on the variables used in the analysis.

13. A potentially serious concern with fixed effects in nonlinear models is bias due to the incidental parameters problem: the inclusion of cluster dummy variables means that as total sample size increases, so does the number of parameters to be estimated (with a fixed number of observations T per cluster, increasing the sample size means increasing the number of cluster dummy variables). This renders all the estimates inconsistent asymptotically. However, Monte Carlo evidence provided by Greene (2002) indicates that for the values of T in the data set used here (about 15), the sample bias is small for an ordered probit model—slightly under 10 percent—with a small downward bias in variance as well. In view of these results, it is reasonable to use fixed effects in this study, while recognizing the likelihood of a small degree of bias.

14. In addition, for children who repeat second grade twice and hence are still in second grade in the third year of the PASEC study (1997–98), information on their teacher and class will be lacking for that year, because data in 1997–98 were collected only for fourth and third grade classes. For these cases, information on the child's second-grade class in 1996–97 was used.

TABLE 1. PASEC Cohort: Means and Standard Deviations of Selected Variables

	All		First pretest score quartile		Fourth pretest score quartile	
Variable	Mean	Standard deviation	Mean	Standard deviation	Mean	Standard deviation
Highest grade completed	6.03	1.65	5.28	1.45	7.07	1.51
Still in school in 2003	0.63	0.48	0.58	0.49	0.78	0.42
Standardized second-grade pretest score[a]	0.003	0.73	−0.895	0.20	0.971	0.33
Age (years)	15.62	0.95	15.48	0.97	15.56	0.92
Girl	0.41	0.49	0.42	0.49	0.42	0.49
Asset index[b]	−0.022	0.91	−0.293	0.81	0.204	1.00
Mother's years of schooling	1.53	3.06	1.07	2.46	2.30	3.79
Father's years of schooling	2.70	4.27	2.23	3.82	3.82	5.07
Missing mother's schooling (percent)	0.10	0.30	0.07	0.26	0.13	0.34
Missing father's schooling (percent)	0.17	0.37	0.13	0.34	0.18	0.38
Rural (percent)	0.56	0.50	0.63	0.48	0.49	0.50
School director's experience (years)	12.67	8.92	9.95	8.54	14.43	9.22
Director has baccalauréat or higher (percent)	0.54	0.49	0.58	0.50	0.50	0.50
Teachers, average years of experience	11.53	7.16	9.96	7.64	12.48	6.67
Teachers, share with a baccalauréat or higher (percent)	0.43	0.39	0.40	0.42	0.45	0.35
Teachers, share female (percent)	0.24	0.36	0.20	0.34	0.32	0.38
Classroom supplies first principal component[c]	0.01	1.38	−0.09	1.61	0.07	1.15
School facilities first principal component[c]	−0.07	1.42	−0.29	1.48	0.13	1.36
Number of observations	834		208		209	

Note: Teacher and classroom supplies variables are the child-specific averages for three years of data from the PASEC survey.

a. Average of French and math scores administered at start of second grade (1996–96 school year). Predicted using second-grade posttest.

b. Constructed employing factor analysis using information on ownership of durable goods (TV, refrigerator, bicycle, and others) and housing characteristics (including drinking water source and toilet facilities).

c. Based on principal components analysis using classroom and school attributes described in the text.

Source: Authors' analysis based on data from the PASEC surveys and the 2003 Senegal Household Education and Welfare Survey (EBMS); see text for details.

III. Early Performance and Grade Attainment: Estimation Results

This section briefly discusses the first-stage regressions used to generate the test score residuals as these results are of intrinsic interest. The full regression results are shown in appendix table A-1. The dependent variables are standardized scores on second-grade post- and pretests. Test scores are lower if the student is a girl, and higher for wealthier children. For the posttest score, there is a positive impact of mother's but not father's education. Pretest scores are positively associated with the experience and education of the school director and teachers, as well as the share of teachers that are women. The teacher variables used in the pretest model are averages over different classes for the school, since the relevant class-specific information (for most children, first grade in 1994–95) is not available. Thus, these variables capture the overall quality of teachers. The posttest regression uses actual values for teachers and classroom supplies for the child's second-grade class. The teacher variables are largely not significant in this regression, with the striking exception of a large positive interaction of student gender (being a girl) and having a female teacher, suggesting that girls respond better to female teachers or that female teachers make more of an effort to encourage girls. There is also a positive impact on the posttest score of having better equipped classrooms.

In the grade attainment-ordered probits, the estimated effects of early academic ability using either predicted test score or the predicted residual were very similar, reflecting the high correlation between these two indicators. Despite the presence of a number of highly significant covariates, the explained variation in the test score regressions is low. As a result, the unexplained portion (the regression residuals) is highly correlated with the dependent variable (test score). This carries over, of course, to the measurement error-corrected predictions—the pretest score predicted from the posttest score is highly correlated with the pretest residual predicted from the posttest residual. Given the similarity of results, only the results for the ordered probit attainment models using the predicted test score are presented (table 2).[15]

Four models are presented in table 2: base models using school-level random and fixed effects (regressions 1 and 3 in table 2) and these models with interactions added (regressions 2 and 4). There is a very strong, highly significant association of grade attainment with early academic success as measured by test score at the start of second grade. The ordered probit model estimates the parameters of the linear index function underlying the model; to get comparative static effects, it is necessary to calculate changes in probabilities using the estimates. Calculations using the random effects estimates

15. One interpretation of the low R^2s in the test score regression is that most of the variation in scores is due to variation in unmeasured ability. However, as suggested above, it is more likely a combination of ability and the influence of unmeasured household or school factors.

TABLE 2. Ordered Probit Models of Grade Attainment

Variable	School-level random effects (1)	(2)	School-level fixed effects (3)	(4)
Intercept	3.785 (3.26)***	4.012 (3.16)***	3.432 (4.43)***	3.502 (4.36)***
Second-grade pretest score[a]	0.602 (5.15)***	0.448 (2.27)**	0.689 (8.59)***	0.587 (7.01)***
Girl	−0.173 (0.92)	−0.081 (0.42)	−0.278 (1.89)*	−0.199 (1.43)
Asset index	0.360 (2.24)**	0.371 (2.15)**	0.407 (5.24)***	0.414 (5.35)***
Mother's schooling	−0.009 (0.28)	−0.012 (0.36)	−0.006 (0.36)	−0.009 (0.49)
Missing mother's schooling	−0.142 (0.36)	−0.112 (0.29)	−0.128 (0.69)	−0.071 (0.37)
Father's schooling	0.035 (1.38)	0.035 (1.26)	0.034 (2.34)**	0.033 (2.40)**
Missing father's schooling	−0.357 (1.49)	−0.357 (1.44)	−0.343 (1.86)*	−0.338 (1.76)*
Pretest score × asset index		0.120 (0.98)		0.131 (1.96)**
Pretest score × girl		0.481 (1.98)**		0.364 (2.51)**
Pretest score × father schooling		0.004 (0.10)		0.002 (0.07)
Director's experience	0.000 (0.01)	−0.004 (0.18)		
Director has baccalauréat or higher	−0.145 (0.45)	−0.206 (0.67)		
Teachers, average experience	−0.016 (0.66)	0.018 (0.68)	−0.050 (2.09)**	−0.052 (2.22)**
Teachers, share with baccalauréat or higher	0.217 (0.65)	0.182 (0.54)	0.671 (1.45)	0.624 (1.38)
Teachers, share female	−0.640 (1.44)	−0.652 (1.40)	−1.485 (4.57)***	−1.383 (4.56)***
Girl × share teachers female	0.321 (0.90)	0.179 (0.44)	0.536 (2.32)**	0.397 (1.72)*
Classroom supplies first principal component	0.041 (0.36)	0.040 (0.35)	−0.015 (0.19)	−0.022 (0.29)
School facilities first principal component	0.002 (0.01)	0.002 (0.01)	−0.097 (0.72)	0.000 (0.00)
Rural	−0.256 (0.48)	−0.314 (0.53)		
Rho	0.068 (1.41)	0.070 (1.36)		
Number of observations	834	834	834	834

*Significant at the 10 percent level; **significant at the 5 percent level; ***significant at the 1 percent level.

Note: Numbers in parentheses are aysmptotic t-statistics. Standard errors in the fixed effects model are adjusted for clustering at the school level. See table 1 and text for variable definitions. Models also include ethnic group dummy variables, ordered probit threshold parameters, and province dummy variables (random effects model) or school dummy variables (fixed effects model).

a. Predicted using second-grade posttest score.

Source: Authors' analysis based on data from the PASEC surveys and the 2003 EBMS; see text for details.

indicate that at the mean of the regressors, a one standard deviation improvement in the second-grade pretest score is associated with a 22 percentage point increase in the probability of completing the sixth grade (relative to a mean estimated probability of 56 percent). The result for the fixed effects model with controls for community/school is virtually the same. These are clearly large impacts. Although, as noted, it cannot be established how much of this association is causal, the robustness of the estimates to controls for school and community demonstrates that they are not simply due to an association of high achievement and attainment with better schools or other locality-specific factors.

This very large and robust association of early academic performance with later educational success has implications for policy. It argues strongly for policies to target children who display early cognitive disadvantages. A limitation of the analysis, however, is that it cannot say what policies should be implemented or at what point in the child's life they would be most effective. For example, if poor cognitive skills early in school reflect largely prior nutritional deficiencies, the best time to intervene is probably well before school age, in which case it would not be very useful to rely on early school measures. One approach to dealing with poor primary performers—grade repetition—is assessed later in the paper.

While there is little comparable evidence from other developing countries, the findings here accord with panel-based research in industrialized countries that considers how early (at school entry) disparities in children's skills are related to later achievement gaps (for example, Phillips, Crouse, and Ralph 1998; Duncan and others 2007), and how academic performance is linked to educational attainment, dropout, and labor market success (Robertson and Symons, 1990; Currie and Thomas 2001; Maani and Kalb 2007).

Conditional on early test score, household wealth is a strongly significant determinant of attainment. A one standard deviation increase in the wealth index implies an approximately 12 percentage point increase in the probability of completing the sixth grade. As with test score impacts, this effect is essentially the same with and without controls for school fixed effects. These wealth impacts may reflect a number of factors, including an income effect on the demand for education and the association of wealth with inputs such as early childhood nutrition or school supplies that improve school performance and progression. Among other household covariates, there is a positive impact of father's schooling (significant only in the fixed effects case). This is in contrast to the tendency in the test score regressions for mother's schooling and not father's to improve test score outcomes. It is possible that educated mothers spend more time helping children with schoolwork than educated fathers do (leading to larger maternal education effects on scores) but that schooling duration decisions largely reflect father's preferences, which are tied to father's education. In the fixed effects model but not in the random effects model, girls go less far in school than do boys with similar early achievement.

There are few significant coefficients on school covariates.[16] However, it is striking that, at least in the fixed effects case, the share of classes taught by a female teacher has a negative effect on attainment that is larger for boys (recall that there is variation within schools in the share of each child's teachers who are women). A smaller negative effect for girls is understandable considering the apparent positive effect of female teachers on girl's learning, but the reason for an overall negative effect of female teachers on attainment seems puzzling at first glance. One possibility is that (male) household decision-makers believe incorrectly that female teachers are of lower quality.[17] Perhaps more probable in view of results discussed below is that female teachers are more likely to fail students (especially boys), which impedes school progression.

The ordered probit models in columns 2 and 4 in table 2 add interactions of early score with assets, gender, and paternal schooling.[18] There is a positive interaction of assets and pretest score in the fixed effects model. As noted in section I, even if the test score represented a measure of ability that was exogenous to parental behavior, theoretical considerations do not lead to firm predictions about the existence or direction of this interaction. In both the random effects and fixed effects models there is a strongly significant positive interaction of test score and being a girl, meaning that the positive effect of test score on subsequent school investments is larger for girls. Caution is necessary here, however, since a higher score for girls, even as early as the end of first grade, could reflect household preferences for girls' human capital that also lead to higher attainment. Still, the results may indicate that the returns to schooling are more sensitive to observed ability for girls than they are for boys. This could happen, for example, if parents expect that a daughter with strong academic skills will be more likely (because of higher potential pay or greater ambition) to go on to participate in the labor market or gain full-time employment. This would increase their incentive to invest further in the girl's schooling, since any earnings benefit from an additional year of education would be realized over a greater number of future years of work. Since male full-time participation is more or less universal, this effect would not occur for boys (see Glick 2008 for discussion).

16. Also included in earlier specifications was distance to the nearest lower secondary school (collected in 2003). This was also insignificant. Characteristics other than distance of lower secondary schools may affect school continuation decisions, but information on these characteristics was not available.

17. The share of female teachers is unlikely to be acting merely as a proxy for poor school quality: there is no consistent pattern of correlations of female share with observed quality indicators or teacher and director background measures.

18. In initial specifications, nonlinearities in the effect of test score itself were found to be insignificant.

TABLE 3. Predicted Probabilities of Completing Sixth Grade for First and Fourth Quartiles of Second-Grade Pretest Scores

		First pretest score quartile		
Model and probability	Fourth pretest score quartile (1)	(2)	Eliminate wealth gaps[a] (3)	Eliminate wealth and paternal education gaps (4)
School random effects model				
Probability (grade ≥ 6)	0.787	0.326	0.393	0.414
Difference in probabilities for fourth and first quartile		0.461	0.395	0.374
School fixed effects model				
Probability (grade ≥ 6)	0.799	0.386	0.464	0.486
Difference in probabilities for fourth and first quartile		0.413	0.334	0.313

Note: Table shows predicted probabilities at test score quartile means for all variables, based on ordered probit model estimates.

a. Column 3 shows the probability for first quartile after setting wealth equal to fourth quartile means. Column 4 shows the probability for first quartile after setting both wealth and paternal education equal to fourth quartile means.

Source: Authors' analysis based on data from the PASEC surveys and the 2003 EBMS; see text for details.

To put the importance of early skills in perspective, the estimates were used to predict the attainment of children in different quartiles of the pretest score distribution (table 3). Columns 1 and 2 show the predicted probabilities of completing at least the sixth grade, evaluated at the means of the regressors for the fourth and first quartiles. The probabilities differ substantially for the two groups, though this also reflects differences in other factors such as wealth and parental education. Column 3 shows the probabilities for the bottom scoring quartile after "eliminating" wealth differences by setting the asset index for this group to the mean of children in the highest achievement quartile. As expected from the association of early test performance and wealth, if household resources were the same for the two groups, the gap in probabilities would be smaller. However, the gap remains very large, closing only from 0.46 to 0.40 in the random effects case; results are similar for the fixed effects case. Column 4 shows that also eliminating gaps in father's schooling further narrows the attainment gap, but only slightly. Controlling for differences in school factors (in the random effects models) or cluster location (fixed effects models) did not substantially reduce the large differences in expected attainment. Thus, the large gap in attainment between early low- and high-achieving children reflects largely differences in early academic ability rather than associations of ability with other factors influencing attainment. This should not be taken to mean that wealth and parental education do not contribute strongly to attainment gaps, but rather that they appear to do so primarily through impacts early on, as captured by achievement measures in second grade.

IV. How Does Targeting Poor Performers for Grade Repetition Affect Attainment?

Interpreted as a causal relationship, the link between early measures of cognitive ability and grade attainment indicates that school investments by parents are strongly reinforcing with respect to ability: high-achieving children are kept in school longer. Since the indicators of early cognitive skills may incorporate unmeasured factors that also affect later schooling, the results should be considered suggestive of such behavior rather than conclusive evidence of it. Still, even interpreted as an association, the findings indicate clearly that students who lag behind their peers early in primary school are at substantially higher risk of early withdrawal from school. Thus, if education authorities are committed to ensuring greater equity in educational attainment, targeting poor early performers for remedial help would be a logical step.

Poor performers are currently targeted for one kind of remedial treatment: grade repetition. Repetition is particularly pervasive in Francophone Africa, where it averages 20 percent for primary students as compared with 10 percent in Anglophone Africa and 2 percent in Organisation for Economic Co-operation and Development countries (Michaelowa 2003). In the Senegalese sample, 76 percent of PASEC students report having repeated at least one primary grade, and 40 percent report two or more repetitions. While repetition is thought to help lagging students catch up with their peers, the impacts of repetition on either attainment (specifically primary completion) or learning have not been established. The costs of repetition, however, are clearly large, both for the education system (in demands on teachers and classroom resources) and for families, who have to finance additional time in school for children to reach a given grade. If labor markets rewarded grade attainment—or achievement of specific levels such as primary completion—rather than solely (less easily observed) human capital, the returns to families of additional schooling would be lower than where there is less repetition. In terms of the model discussed in section I, the marginal returns for the low-ability (repeating) child in figures 1–3 would effectively shift further downward relative to those for a high-ability (nonrepeating) child—possibly explaining in part the large negative association of low measured ability and grade attainment.

It is of interest then to see how this "targeted" policy affects school attainment in the sample. Of course, even if there are negative impacts on attainment, they may be offset by improvements in learning from repeating a grade, so information on both outcomes is needed to assess repetition policies.

Attempts to measure the impacts of repetition must deal with the fact that repetition is not exogenous to school attainment or dropout. Repeaters would be expected to be relatively poor academic performers who may well have lower attainment than nonrepeaters, aside from any positive or negative effects of repetition itself. In a few, mostly U.S. studies, researchers have been able to handle endogeneity by taking advantage of natural policy experiments, such as changes in retention policy over time or across states, or regression discontinuity designs made possible by the presence of an official test score threshold for promotion (Jacob and Lefgren 2004; Allensworth 2005; Greene and Winters 2007). While this is not the case for this study, the panel data make it possible to control for academic achievement at the time the decision is made to repeat a student (measured by the second-grade posttest score). Conditional on a student's academic performance to that date, variation in the requirement that a student repeat the grade should reflect variation in school or teacher repetition rules or attitudes rather than parental (or child) education preferences or effort.[19] This approach is similar to that of several analyses for developing countries of the effects of repetition on student learning (Gomes-Neto and Hanushek 1994; Michaelowa 2003), though unlike those studies, the present study also estimates impacts on attainment or dropout. This is made possible by the fact that the 2003 survey, rather than being school based, interviewed members of the PASEC cohort whether they were still in school or not and gathered information on the last year and grade enrolled of those who had left school.

Because the analysis is considering the effect of a repetition occurring near the start of a child's education, it focuses on a measure of early attainment rather than ultimate attainment, as in the ordered probits. It estimates the effect of second-grade test score and second-grade repetition on the probability that the child completes grade 4, or equivalently, drops out before grade 5. This choice of dependent variable avoids censoring problems, as very few children (under 4 percent) who are still enrolled as of 2003 have not already progressed beyond this level. As indicated, this analysis uses the posttest rather than the pretest, as the posttest measures skills at the time the repetition determination is made. As before, this is corrected for nonsystematic measurement error, now using the pretest score as an instrument.

A problem with the data is attrition from the PASEC sample. Some 20 percent of the PASEC second-grade cohort of 1995–96 is not in the testing sample in the next year—that is, in either the third or second-grade classes visited by the survey at the end of the 1996–97 school year. There are two possible reasons for this: the children had already dropped out, or they were

19. It is not being assumed that a student's academic ability (perhaps relative to that of peers, as discussed below) is the only factor in a teacher's decision on whether to hold the student back; in particular, the student's social or emotional maturity may also be considered. However, this will not bias the estimate of the effect of repetition unless, controlling for academic achievement, this factor would independently lead to early dropout. There is no reason to expect a (presumably temporarily) immature child not to continue in school if this immaturity is not causing the student to lag behind academically.

still enrolled but were not in attendance on the day the PASEC team returned to the school. The 2003 data indicate that the large majority of these missing children attained third grade or higher and thus belong in the second group.

Being missing is not random with respect to academic ability: children who were absent for the 1997 follow-up scored significantly lower on the prior year tests than those present, and this is the case even for children who were still in school but merely not in attendance. Since they had lower achievement at the end of second grade, children who were absent (for either reason) would be more likely to be repeaters. Further, for children who were enrolled but absent during the 1997 visit, attendance was probably poorer on average than for nonabsent children, and factors that lead to poor attendance such as higher opportunity costs or lower motivation (hence negatively affect selection into the 1996–97 sample) might also independently result in earlier dropout. Therefore, both early (before third grade) school leaving and simple absence may lead to selection bias when estimating the impacts of repetition.

It is very difficult to come up with valid instruments for this selection.[20] Even without this, however, the data allow a test for selectivity. For children missing in 1996–97, information is unavailable on second-grade repetition, but the dependent variable of interest, dropout before grade five, is observed. If there is selection on unobservables, dropout should be associated with selection (the presence of the child in 1996–97 re-survey), conditional on test score and other measured characteristics. In probits for early dropout using the full 1995–96 sample, there is a significant negative effect of a dummy variable for being in the 1996–97 sample, controlling for posttest score and other covariates (results available from the authors). However, this effect vanishes after dropping the small number of children who reported leaving school right after second grade. This finding indicates that selection into the 1996–97 sample is related positively to the propensity for continuing in school, but this selection arises from children who drop out immediately rather than those who are simply not in attendance for the 1997 test. Of course, whatever the source, the possibility of bias must be considered. It bears noting that selection issues in modeling school outcomes in panels with high withdrawal or absenteeism are hardly unique to these study data, though the ability to test for selection is unusual.

20. The EBMS collected information on health and economic shocks to the household (parental illness, unemployment, negative and positive harvest shocks, and other events) that can predict ultimate school attainment (see Glick and Sahn 2009). However, these shocks are unconvincing as instruments in the present context: they would have to predict attendance on a given day in 1996–97 but have no independent effects on dropout in that year or the next several years. Shock-related factors that affect attendance (such as loss of income or demands on the child's time) will likely also influence the decision on whether and how long to continue schooling.

Probit regressions (presented in appendix table A-2) on the sample of children present in 1996–97 confirm that the probability of repeating second grade is negatively and very strongly affected by the posttest score (*t*-statistics greater than 5.0), whether controlling for school fixed effects or not. A 1 standard deviation reduction in the posttest score increases the repetition probability by about 11 percent—a large impact given the mean repetition of 14 percent.[21] In the fixed effects model, most of the school dummy variables are highly significant and there is substantial variation in the magnitudes of the school effects. The variation across schools in repetition probabilities conditional on test performance appears to be the case at least partly because teachers or schools assess students relative to their peers, as many have suggested (Bernard, Simon, and Vianou 2005). In a specification of the non-fixed effects model including both individual score and mean class score, the mean score has a positive and significant impact, meaning that a student with a given level of ability is more likely to be held back when ranked lower within his or her class. Repetition probabilities are also higher when the teacher is a woman. Perhaps consistent with the beneficial impact of female teachers on girls' test scores, the positive female teacher effect on repetition (controlling for test score) is significantly smaller if the student is a girl.

Among household variables, parental education has no impact on repetition controlling for score, but wealth is negatively associated with repetition. Since the wealth effect persists in the within-school model, it is evident that not all of the variation in repetition conditional on ability is due to variation across teachers or schools. It is possible that wealthier parents are able to bribe teachers to avoid repetition or, possibly, are more willing to appeal the decisions of teachers (or are more effective at it). This could pose some problems for the identification of causal repetition effects. However, wealth is an included covariate in the dropout models, and this controls for contamination from wealth-related behavior affecting both repetition and dropout (to effectively capture these effects, polynomials in wealth were added to the dropout models but were not significant). It is necessary to assume that unobserved factors not captured by wealth are not significantly affecting both the teacher repetition decision and later attainment outcomes conditional on skills.

The probit results for early dropout for this sample, including the indicator for being a repeater, are presented without school fixed effects (table 4). The fixed effects estimation is not feasible for this analysis, because the identification of repetition impacts conditional on academic performance relies on variation across classes in teacher/school repetition practices—essentially, variation in test score thresholds for passing second grade—such that children with similar scores but in different schools will have different probabilities of being

21. This is all the more noteworthy because teacher promotion decisions are not based on the results of the PASEC survey tests (which were graded by survey personnel for use by the PASEC project only) but on internal tests and other assessments of the children's skills.

TABLE 4. Determinants of Dropout before Fifth Grade and 1997 Test Score

	Dropout[a]		Test score[b]	
Variable	Coefficient	t-statistic	Coefficient	t-statistic
Intercept	−1.973	−5.961***	0.000	−0.001
Second-grade posttest score[c]	−0.237	−1.837*	0.804	14.618***
Repeated second grade	0.492	2.306**	−0.204	−1.601
Girl	0.008	0.043	0.010	0.116
Asset index	−0.240	−2.475**	0.066	1.221
Mother's schooling	−0.032	−1.036	0.011	1.001
Missing mother's schooling	0.040	0.161	0.018	0.132
Father 's schooling	−0.001	−0.051	0.014	1.691*
Missing father's schooling	0.609	3.233***	0.059	0.510
Rural	0.153	0.889	−0.045	−0.297
Director's experience	−0.001	−0.180	0.004	0.632
Director has baccalauréat or higher	−0.067	−0.466	0.190	1.322
Teachers, average experience	0.021	2.033**	−0.016	−1.833*
Teacher has baccalauréat or higher	0.338	1.837*	0.097	0.553*
Teachers, share female	0.387	1.556	0.156	0.768
Girl × share teachers female	−0.382	−0.826	−0.011	−0.067
Classroom supplies principal component	−0.023	−0.590	0.025	0.571
School facilities principal component	−0.031	−0.644	0.071	1.424
Number of observations[d]	664		556	

*Significant at the 10 percent level; **significant at the 5 percent level; ***significant at the 1 percent level.

Note: See table 1 and text for variable definitions. Model also includes ethnic group dummy variables and province dummy variables. Standard errors are adjusted for clustering at the school level.

a. Probit model estimates. Dependent variable is leaving school before fifth grade.

b. Ordinary least squares estimates. Dependent variable is the standardized score from the end of the 1996–97school year (see text for details).

c. Predicted using second-grade pretest score.

d. Sample size is lower for test score model because of missing test data.

Source: Authors' analysis based on data from the PASEC surveys and the 2003 EBMS; see text for details.

held back. Considering only differences among students within a class obviously eliminates this variation, so the repeat dummy variable would do no more than capture a possible nonlinear impact of test score on dropout.[22]

The results indicate that conditional on skills as captured by posttest score (which has the expected negative effect), repeating the second grade has a large

22. The non-fixed effects model does not control for unobserved school or community factors that affect both repetition and dropout. It is important to remember, however, that the regressions include the test score, which is an ideal control to capture factors affecting schooling outcomes; conditional on test scores and the other covariates, it seems likely that the repetition-dropout relation will not be substantially affected by unmeasured factors. It is worth recalling as well from the attainment models that the effects of test score itself are extraordinarily robust to adding school fixed effects.

and significant positive impact on the probability of early dropout. The implied increase in the probability is about 11 percent, a large impact given the rate of dropout before fifth grade of 15 percent. The other estimates for the probit model are qualitatively similar to the earlier ordered probit results, though perhaps reflecting the relatively limited variation in the dependent variable, there are fewer significant coefficients. Most notably, greater wealth reduces the probability of leaving school before grade 5.

It can be concluded, therefore, that grade repetition as a policy for poor achievers has detrimental impacts on attainment among children in Senegal. The study results, subject to the caveat about selection noted earlier, suggest that children who are made to repeat a grade are less likely to complete primary school than children with similar academic ability who are not held back. This is not surprising, given that repetition effectively raises the private costs of achieving a given grade level. It may be inferred that part of the large negative effect of poor early achievement on school attainment operates through this pathway. It is also possible that poorly educated parents find it difficult to discern by other means how much their child is learning; such parents may take a failure to be promoted as a signal that the returns to schooling are particularly low for their child. While there appear to be no similar studies in other African contexts that plausibly attempt to control for repetition endogeneity, similar negative impacts on attainment were found for Uruguay by Manacorda (2007), who used both regression discontinuity and a natural experiment approach.[23] The findings on dropout impacts from the broader U.S. literature are mixed (Grissom and Shepard 1989; Eide and Showalter 2001; Allensworth 2005), but the differences in contexts between the United States and Africa are in any case very large.

As noted, there still may be a benefit to repetition if children who repeat a grade learn more by staying behind than children with similar skills who are promoted to the next, more difficult grade. The PASEC test data make it possible to address this question in a straightforward way. Students who repeated second grade in 1996–97 were tested at the end of the school year using the same second-grade math and French posttests taken in the previous year. Students who were in the third grade took a different test that nevertheless had a substantial number of questions in common with the second-grade tests. There were 38 such anchor items on the math and French tests combined, from

23. King, Orazem, and Paterno's (2008) study using panel data from Pakistan addresses the dropout implications of repetition using a different methodology. Decomposing the promotion probability into the part determined by student merit or ability and the part due to other factors, they find that only student merit has a (positive) effect on school continuation. If the merit-based part is considered a control for student ability, the nonmerit portion essentially captures exogenous sources of variation in repetition probabilities much in the way the repetition indicator controlling for ability does in the present analysis. Thus, the finding that nonmerit factors have no effect on continuation conditional on academic merit is in contrast to the negative effect of repetition found in the this study.

which standardized 1997 scores were calculated for each child. The determinants of this score were estimated in a linear regression including the posttest second-grade score and the repeat dummy variable as well as the other controls used in the dropout probit.

The results show, not surprisingly, a very large and highly significant positive association of scores in 1997 with previous year scores (see table 4). Having repeated second grade is associated with a small (0.2 standard deviation) reduction in test score relative to not repeating, though this effect is not quite significant at the 10 percent level A negative effect of repetition is not implausible, as children left behind may feel stigmatized or get less encouragement to do well in school (note that the negative coefficient does not mean that repeating children do not increase their skills over the subsequent year, only that they gain less than those with similar initial skills who are promoted).[24]

In sum, for a given level of skill (test score) at the end of second grade, having a student repeat that grade does not lead to greater test score gains relative to being promoted and may even lead to slightly reduced improvement, though this effect is imprecisely estimated. Similar results (showing no significant benefit) reported in Michaelowa (2003) were obtained in an analysis for all the Francophone countries in the PASEC study employing a broadly similar methodology. These findings contrast with the widespread perception among teachers documented in CONFEMEN (2003) that repetition leads to greater improvement. The findings suggest that there is little academic benefit to repetition to justify the costs to schools and households.

V. Conclusion

The estimates and simulations in this study indicate that early academic performance is an important predictor of school attainment in a cohort of children in Senegal. Conditional on measured skills at the start of second grade, gender, father's education, and especially household wealth also have impacts on attainment, in expected directions. There is some evidence as well that the positive effects of early achievement are greater for wealthy children and for girls. The strong association of early achievement and later educational attainment points to the benefits of measures directed to lagging children to avoid later disadvantages in skills and labor market outcomes.[25] However, the one (quite costly) policy that is currently directed at such children, grade repetition, tends

24. A negative association might also suggest that unobservable factors such as ability or motivation are affecting both repetition and subsequent test performance. Recall, however, that as with the dropout model, the estimation conditions on test outcomes immediately prior to the repetition decision (second-grade posttest score), which should control for these factors.

25. Though as noted in section III, the appropriate measures and their timing will depend on the source of the early disadvantage.

to exacerbate the negative impacts of poor early school performance on attainment.

Since evidence from a number of African countries indicates that there is a premium to primary (and secondary) completion in terms of earnings or access to formal sector employment, the finding that having lagging learners repeat grades increases premature primary dropout is important for policy. Given this finding as well as the evidence that being held back does not improve learning outcomes relative to being promoted, it would likely be significantly more effective to devise other measures to improve the skills of lagging children rather than simply having them repeat grades. One measure might be to hire local secondary school graduates to tutor such children, along the lines of a successful intervention evaluated in India by Banerjee and others (2007).

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Appendix

Table A-1. Determinants of Second-grade Pre- and Posttest Scores

	Pretest		Posttest	
Variable	Coefficient	*t*-statistic	Coefficient	*t*-statistic
Intercept	−1.302	−3.735***	−0.314	−1.020
Girl	−0.296	−3.180***	−0.361	−3.909***
Asset index	0.152	2.928***	0.209	2.947***
Mother's schooling	0.024	1.459	0.032	2.187**
Missing mother's schooling	0.143	1.013	0.100	0.645
Father's schooling	0.005	0.487	0.007	0.664
Missing father's schooling	0.118	0.867	0.142	1.151
Rural	0.344	2.174**	0.098	0.710
Director's experience	0.021	2.340**	0.014	1.388
Director has baccalauréat or higher	0.386	3.135***	−0.043	−0.309
Teacher experience[a]	0.053	4.642***	0.011	1.546
Teacher has baccalauréat or higher[a]	0.301	2.072**	0.237	1.485
Teacher female[a]	0.398	1.947*	−0.182	−1.045
Missing teacher information in 1995–96			1.056	3.311***
Girl × teachers share female	0.059	0.351	0.541	3.764***
Classroom supplies first principal component[a]	0.009	0.215	0.094	1.996**
School facilities first principal component	−0.005	−0.106	0.071	1.213
Adjusted R^2	0.214		0.177	

(Continued)

TABLE A-1 Continued

	Pretest		Posttest	
Variable	Coefficient	*t*-statistic	Coefficient	*t*-statistic
Number of observations	787		756	

*Significant at the 10 percent level; **significant at the 5 percent level; ***significant at the 1 percent level.

Note: Pretest and posttest scores are the standardized average scores on French and math tests administered at the beginning and end of second grade, respectively. The models also include ethnic group dummy variables and province dummy variables. Standard errors are adjusted for clustering at the school level. The varying sample sizes reflect the number of students in the PASEC cohort present when the tests were given. See table 1 and text for variable definitions.

a. For the posttest regression, the teacher and classroom supplies variables are the values for the child's second-grade class (the year ending with the posttest). For the pretest regression, they are simple averages over grades 2–4 in the school.

Source: Authors' analysis based on data from the PASEC surveys and the 2003 EBMS; see text for details.

TABLE A-2. Determinants of Second-Grade Repetition: Probit Results

			With school fixed effects	
Variable	Coefficient	*t*-statistic	Coefficient	*t*-statistic
Intercept	−0.766	−1.808*	−2.094	−4.799***
Second-grade posttest score[a]	−1.295	−6.718***	−2.186	−7.323***
Girl	0.370	2.178**	0.409	1.707*
Asset index	−0.212	−1.679*	−0.469	−2.239**
Missing mother's schooling	−0.209	−0.570	0.209	0.352
Mother's schooling	−0.005	−0.147	0.026	0.590
Father's schooling	−0.014	−0.660	−0.025	−0.701
Missing father's schooling	−0.147	−0.636	−0.206	−0.535
Rural	−0.799	−2.370**		
Director's experience	−0.004	−0.251		
Director has baccalauréat or higher	−0.316	−1.296		
Teacher has baccalauréat or higher	−0.249	−0.997		
Teacher female	1.026	2.968***		
Girl × female teacher	−1.203	−3.990***	−1.548	−3.731***
Classroom supplies principal component	−0.446	−3.352***		
School facilities principal component	−0.146	−1.179		
Number of observations	664		664	

*Significant at the 10 percent level; **significant at the 5 percent level; ***significant at the 1 percent level.

Note: For fixed effects model, school dummy variables and ethnicity dummy variables not shown. For non-fixed effects model, ethnicity and province dummy variables not shown. Standard errors are adjusted for clustering at the school level. Teacher and classroom supplies variables refer to the second-grade class.

a. Predicted using second-grade pretest score.

Source: Authors' analysis based on data from the PASEC surveys and the 2003 EBMS; see text for details.

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