Cognitive Skills among Children in Senegal: Disentangling the Roles of Schooling and Family Background

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Abstract

We use unique data to estimate the determinants of cognitive ability among 14 to 17 year olds in Senegal. Unlike standard school-based samples, tests were administered to current students as well as to children no longer—or never—enrolled. Years of schooling strongly affects cognitive skills, but conditional on years of school, parental education and household wealth, as well as local public school quality, have surprisingly modest effects on test performance. Instead, family background primarily affects skills indirectly through its impacts on years of schooling. Therefore closing the schooling gaps between poor and wealthy children will also close most of the gap in cognitive skills between these groups.

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1. Introduction

The benefits to economic development of investments in human capital, and particularly investments in schooling, are virtually universally accepted. This understanding has spawned an enormous empirical literature on the household and policy determinants of investments in schooling. Underlying most of this research, which usually employs standard household surveys, is the assumption that the quantity of schooling (or sometimes, simply enrollment status) is a reliable representation of the level of human capital, that is, the cognitive skills that education is assumed to impart. However, cognitive outcomes will in general be determined not just by years of schooling but also by the quality of schooling as well as by the level and quality of non-school 'home' inputs into human capital production. These factors are often correlated with schooling attainment, so that differences in actual human capital between individuals are likely to be wider than differences in their schooling. They are also likely to be correlated with household income or wealth, so that observed wealth-related disparities in schooling attainment may underestimate actual human capital investment gaps between poor and well-off children. For the same reason, econometric estimates of the effects of income or wealth on children's schooling, and potentially of other factors as well, will not measure the true impacts on children's human capital.

In this paper we use unique data on a cohort of 14-17 year olds in Senegal to estimate the relationship of schooling and human capital as measured by test scores. The analysis addresses the following questions: (1) What are the relative contributions of schooling attainment, local school quality, and household characteristics to human capital accumulation, and do these differ for girls and boys? (2) Do household and school characteristics affect human capital primarily through their effects on schooling duration or through learning conditional on the level of schooling? (3) How much will closing the gaps in schooling between poor and well-off children, and between girls and boys, contribute to closing gaps in human capital along these dimensions? Put another way, how much of the cognitive skill advantages of well-off children and boys is due to greater grade attainment and how much to differences in school and household environments or in the response to these environments?

The *Education et Bien-être des Ménages au Sénégal* (EBMS) survey was designed precisely to meet several difficult methodological challenges to answering these questions. The first is the need to have direct measures of human capital (test score data) for a representative sample of children. Academic tests of children in developing countries (e.g., Tan, Lane & Coustere, 1997; Michaelowa, 2001; Das, Dercon, Habyarimana & Krishnan, 2004), as in developed countries, almost always involve school-based samples, whereby tests are administered in class to a sample of children in a given grade. However, where non-enrollment or (for higher grades especially) dropout is prevalent, the use of in-school samples very likely leads to sample selection biases, because the sample of children tested will not be not representative of all children or even of those children who ever attended school. Further, such school-based surveys usually collect at best only rudimentary information on the households of the pupils tested. Beyond the fact that the impacts of household characteristics on learning are of intrinsic interest, their omission will bias even conditional estimates of the effects of school factors such as teacher quality on skills of currently enrolled children unless home inputs are uncorrelated with school resources.

The EBMS survey is unique in that it was designed to test a representative random sample of children, including children who are currently enrolled, formerly enrolled, and who never enrolled in school. Tests were given in written French and math, oral math, and 'life skills'. Further, the test information was matched both to detailed school surveys and to household surveys with information on the full range of factors potentially affecting schooling and cognitive outcomes.

In addition to this advantage, the EBMS was designed to deal with another key methodological problem, the potential endogeneity of schooling to human capital outcomes. It is plausible to expect unobserved ability or household preferences for human capital acquisition to be correlated positively both with test scores and the demand for, hence the level of, schooling. This implies an upward bias in the estimated effect of schooling on test performance. The survey collected a range of retrospective household information that can be used to instrument schooling in test regressions, which we use to explore the implications of endogeneity for the estimates of the impact of schooling on test performance. Finally, the regressions also deal with the possible selection bias implied by the fact that not all eligible children in the cohort participated in testing.[1]

Like a number of other countries in West Africa (primarily Francophone countries), Senegal has had difficulty meeting key education objectives such as universal primary enrollment. The gross primary enrollment ratio in 2002 was just 75%.[2] Repetition rates are very high while primary completion rates are very low both for boys (53%) and especially, girls (44%), leading to a very inadequate gross secondary enrollment rate of 19%. In addition to significant gender gaps in schooling investments, there are large gaps along wealth and location dimensions. It is of substantial policy interest to understand how these well these schooling attainment differences proxy difference in actual human capital, and conversely, how effective a policy of closing schooling attainment gaps will be in closing gaps in skills.

The remainder of this paper is organized as follows. The next section presents the conceptual framework and empirical strategy used in this paper. Section 3 discusses the EBMS data. The determinants of test scores are reported in Section 4. This section also presents results of several simulations: of the effects of reductions in wealth- and gender-related schooling disparities on gaps in cognitive skills; and of the comparative static effects of changes in various factors on cognitive skills, accounting for direct effects on test performance and indirect effects operating through changes in schooling. The last section summarizes and discusses policy implications of the results.

2. Conceptual framework and methodology

Cognitive ability as measured on tests is expected to be positively related to the level of schooling an individual attains, but not only to attainment. A key objective of this paper is to assess the relative importance for human capital accumulation of school attainment and other factors, specifically, local school quality and family characteristics such as wealth and parental education. The model underlying our analysis of the determinants of test scores is formally similar to frameworks for

[1] Glewwe and Jacoby (1994), like our study, is a rare example of an analysis that attempts to overcome the limitations of standard data. Their analysis of cognitive skills is limited to middle school students, but since the data are drawn from a representative household survey, they are able both to correct for selection into this group and to model the effects of household characteristics on test outcomes.

[2] This and other country level statistics were obtained from World Bank (http://www.worldbank.org, Data and Country Statistics).

analyzing the impact of education on earnings that adjust for variation in school quality (see Behrman & Birdsall, 1983; Card & Kruger, 1992). Using the terminology of Behrman and Birdsall, we assume that human capital is a function of 'effective schooling'. In the present case, effective schooling for the *i*th child is a function of actual years of schooling (S_i) as well as school characteristics ('quality' or Q_{ij}) and household characteristics (X_i) such as parental education and assets, and unobserved individual ability A_i, i.e., $f(S_i,Q_{ij},X_i,A_i)$. Using *j* to index the local school we can specify a production function for human capital H_i :

$$H_i = aX_i + bf(S_i,Q_{ij},X_i,A_i) + e_i \tag{1}$$

where e_i is a random disturbance term and H_i is measured by the standardized test score (individual score minus mean score over the standard deviation). X_i is assumed here to have effects on human capital independent of its impact through school effectiveness. In particular, knowledge of life skills and oral math can plausibly be transmitted from educated parents independently of whether or how much formal education occurs.[3]

A general specification for the effective schooling function $f(S_i,Q_{ij},X_i,A_i)$ would be one allowing for interactions among the arguments, yielding:

$$\begin{aligned} bf(S_i,Q_{ij},X_i,A_i) &= b_1S_i + b_2Q_{ij} + b_3X_i + b_4A_i + b_5S_iQ_{ij} \\ &\quad + b_6S_iX_i + b_7S_iA_i + b_8Q_{ij}X_i + b_9Q_{ij}A_i + b_{10}X_iA_i \end{aligned} \tag{2}$$

Higher school quality Q_{ij} raises effective schooling for a given level of schooling S_i ($b_2>0$) and presumably has larger total impacts the more years a child attends ($b_5>0$). We might expect similar patterns for parental education, since more educated parents will be better able to assist children with their schoolwork. Similarly, X_i also includes household-provided inputs such as notebooks, a desk at home, and electric lighting for evening study; these inputs (proxied by the overall asset wealth of the household in our models) also raise learning outcomes given S_i.[4] With respect to interactions of Q_{ij} and X_i, specific school inputs captured by Q_{ij} may be complements or substitutes with home inputs in the production of human capital, i.e., b_8 may be positive or negative.

Substituting into eq. 1 and rearranging yields:

$$H_i = a'X_i + b_1S_i + b_2Q_{ij} + b_5S_i Q_{ij} + b_6S_iX_i + b_8Q_{ij}X_i + e_i' \tag{3}$$

where $a' = a + b_3$ and $e_i' = e_i + \{b_4A_i + b_7S_iA_i + b_9Q_{ij}A_i + b_{10}X_iA_i\}$.

[3] One could make a similar argument for allowing unobserved ability Ai, or perhaps, interactions of ability and parental or other household characteristics, to have impacts other than through school effectiveness.

[4] Wealth and parental education may also increase effective schooling through associations with greater investments in early child health, which will increase the capacity of the child to learn.

Many of the variables in this equation reflect the choices of households or parents, who are assumed to have a utility function with arguments consisting of children's human capital as well as other consumption and leisure. Optimization subject to the production function of Eq. (1) and knowledge of child's ability A_i yields the desired level of human capital H_i and levels of inputs into its production: years of schooling, home-provided inputs (including parental time), and possibly also school inputs, to the extent that parents choose their children's schools or able to influence school quality. These inputs are therefore potentially endogenous, creating the potential for biases in OLS estimations of equations such as (3).

One potential source of bias is the presence of innate (and unobserved to the researcher) ability and its interactions in the error term. It is plausible to suppose that ability is correlated positively not only with test scores but also with the level of schooling, i.e., parents are more likely to keep high ability children in school longer. If so, the estimated effect of schooling attainment on test performance will be biased upward through the correlation of the bracketed terms in e_i' with S_i. Further, many home inputs into learning, such as parental time or provision of school supplies, are poorly captured, if at all, in surveys. Like A_i, these elements of X_i as well as their interactions with other test score determinants enter the error term. Since parents who provide more of these inputs—i.e., parents with strong preferences for education—will likely also want to keep their children in school longer, there again may be biases in the estimates of the effects of schooling on test scores.

There also may be important school and community-level heterogeneity associated with both human capital (test scores) and level of schooling. This would be a source of upward bias in the impacts of the level of schooling on skills since children in communities where parents have strong preferences for schooling are likely both to be in school longer and be provided with other, unmeasured school inputs that raise test scores. Since such communities are also likely to directly contribute more resources to schools or to pressure authorities to provide additional resources, estimates of school quality impacts may similarly be biased upward. Selective migration of high schooling preference households to areas with high quality schools is one way in which such heterogeneity can occur.[5] Measured household level covariates of interest such as wealth may also be associated with unobserved community level factors affecting human capital.

Addressing these sources of bias is difficult using standard surveys. To deal with the endogeneity of the quantity of schooling, instrumental variables approaches are called for, but variables such as household assets or school characteristics are not valid as instruments for the level of schooling because they presumably affect both schooling and (directly) cognitive ability. However, the EBMS survey was designed to collect information on a number of factors that may affect school enrollment or continuation but not test outcomes conditional on schooling. These consist primarily of economic or health shocks to the household that impinge on school enrollment. Retrospective information--usually for the last 10 years, starting slightly before most of the children in our sample become of school age--was collected on death and significant illness of adults and children, years of unexpectedly poor household enterprise performance or failure, and unusually good or bad harvest years. A number of these variables were found to have significant impacts in the expected directions

[5] On the other hand, if governments direct quality improvements to communities where enrollment (hence demand) is low, the bias on quality impacts will be negative (see Rosenzweig & Wolpin, 1986).

in first stage grade attainment regressions, presented below. We also utilize information on the education of the siblings of each parent, namely, the education level of the most educated siblings of the mother and the father. This is expected to influence a child's schooling via financial support to the household from high-earning uncles or aunts.

Another potential instrument is the distance to the nearest lower secondary school. While distance to primary schools is likely to directly impact learning by increasing the time required for travel and thus reducing time for schoolwork, distance to secondary school is expected to act primarily as an inducement to complete primary school and continue to the secondary level. Direct (negative) effects of secondary school distance on a child's skills are likely to be small for the 14 to 17 year olds in our sample because few have progressed beyond a year or two of lower secondary. Among test-takers, mean years of primary education is 5.4 while mean years of any secondary is just 0.8, meaning that the current burden of travel time for those attending secondary school probably does not have a large effect on the stock of human capital, which is formed over the child's entire schooling career.

While our economic or health shocks instruments are reasonably assumed to be exogenous events, their usefulness as instruments will be compromised if they affect not just school entry or continuation but also learning conditional on length of schooling. For example, a negative harvest shock conceivably could have a direct negative impact on learning via time use effects or reductions in other school inputs without forcing school withdrawal. This will be less of a concern if shocks that do not change school status have relatively transitory effects on learning, in which case cumulative effects on human capital (a stock) will be limited.[6] We report on tests of instrument validity below.

Another relevant issue in IV estimation is that finite sample bias in IV estimators has been shown to be an increasing function of the number of instruments, particularly if many of the instruments are individually weak (i.e., not strong predictors of the endogenous variable). Hence there is potentially a problem with including too many such instruments. Donald and Newey (2001) suggest choosing the number (and specific set) of instruments that minimizes a mean square error criterion. We employ their approach in choosing the instrument set for the model.[7]

Even with IV methods to predict an individual's schooling, measured household covariates X_i and/or school quality Q_{ij} may be correlated with unobservables at the community level that influence test outcomes. We include dummy variables for each of the 60 sample clusters into the model to control for these community fixed effects. Since all community level covariates, including school characteristics, difference out in this procedure, some key effects of interest, notably those of school quality (as well as distance to schools which are recorded at the cluster level) are not measured. Household level covariates, on the other hand, do vary within clusters and this variation will now be purged of associations with community unobservables. This is contingent on the assumption that the unobservables enter the model in linear fashion, which need not hold; non-linearities could enter

[6] Other analyses of education that use as instruments economic shocks or time-varying data on incomes and other factors implicitly make this assumption. In sequential probit analyses of school progression, Lillard and Willis (1994) and Sawada and Lokshin (2001) use such data to identify endogenous selection into a given school level when estimating the determinants of progression from that level to the next. This requires that the shocks or other time-varying factors that affect entry into the school level have no direct effects on progression through that level, hence on school performance.

[7] We thank Stephen Donald for sharing code and suggestions for this procedure.

through interactions of unobservables and Q_{ij} or X_i in the general formulation in Equation (3). Therefore community FE may merely reduce rather than eliminate this potential bias.[8]

The model in Eq. (3) also includes interactions of the level of schooling with both school quality and household characteristics. These terms should also be treated as endogenous since they contain schooling (its quantity and/or quality). This is very difficult to do with a limited roster of instruments since we include multiple measures of both school and household characteristics. However, initial (non-IV) estimations indicate that very few of these interactions were significant in the test score regressions. This suggests that a simpler linear model is appropriate, that is, Equation (3) without the interactions.[9]

A second important issue in the estimation of the test score regressions is selection into the test-taking sample. The survey interviewers were instructed to encourage all children in the 14 to 17 year age group to take the tests. The tests were usually held on a subsequent day in the local school or other public building. Inevitably, some children did not attend the test sessions, and this raises the possibility of sample selection bias. One might expect students with less education, or those who have greater work obligations in their households, to be less willing or able to be tested. Indeed, probits show test attendance to be positively related to own years of education as well as, for the written tests, parental education and assets. If selection occurs solely on these and other observables, coefficients will not be biased, but if it occurs on unobservables such as ability, they will be.

To deal with this we use a standard Heckman-type correction for selectivity in the test regressions, constructing the selection correction term from a probit model for taking the test estimated on the entire sample of children age 14 to 17. As an exclusion restriction to identify this term we use the identification code of the survey interviewer. It is expected that some interviewers, because of their training or demeanor, are better able to persuade families to send their children to take the tests. Indeed, many of the enumerator codes are statistically significant in the test taking probits and these dummies are always easily jointly significant at the 1% level. Conditional on the effect on test attendance, interviewer characteristics are not expected to influence outcomes on the tests, which were administered not by the interviewers but by specialists from a Senegalese education research institute.

The regressions provide estimates of the cognitive skill impacts of additional schooling (completed grade) controlling for school quality and household/individual characteristics, and of the impacts of school characteristics and household/individual characteristics controlling for level of schooling (and for household/individual or school characteristics). Unlike for schooling quantity,

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[8] *School*-level fixed effects would be a way to eliminate or reduce bias due to within-community heterogeneity that affects both choice of school and test outcomes. Recall, however, that our sample includes some children who did not attend formal primary school. School fixed effects would therefore entail a truncation of the sample, and lead to estimates not directly comparable to the other models, including those with community fixed effects.

[9] Focusing on the earnings impacts of school quantity and quality, Behrman and Birdsall (1983) derive a specification that essentially includes only interactions of years of schooling with a quality measure. They argue that this is preferable to including a linear quality term (our b_2Q_{ij}) since such a term implies that school quality affects outcomes even if the individual has no schooling. This is strictly true but we prefer to let the data determine the appropriate specification, and in any event the vast majority of children (close to 85%) have some schooling. The criticism does not apply to our treatment of X_i since as noted there are likely direct effects of household characteristics, not merely through interactions with level of schooling.

however, the test score regression estimates for school quality and household characteristics do not indicate the full impacts of changes in these covariates. This is because both also affect test scores indirectly through their effects on the level of schooling (or through effects on academic performance and thus promotion or withdrawal, hence grade attainment). Thus for a change, say, in some household characteristic X_k, the full effect on test outcomes is the sum of direct and indirect effects, i.e., $\partial H_i/\partial X_{ik} = \partial H_i/\partial X_{ik}|_s + \partial H_i/\partial S_i \cdot \partial S_i/\partial X_{ik}$. For the linear model $\partial H_i/\partial X_{ik}|_s = a'_k$ is the direct effect of X_k. The model also provides $\partial H_i/\partial S_i$, which is equal to b_1. We calculate $\partial S_i/\partial X_{ik}$, the effect of X_k on schooling, from the estimates of a two-limit tobit model for years of schooling. This procedure and the tobit results are discussed below in Section 4.

3. Data and sample characteristics

The EBMS was conducted in 2003 as a joint research project of Cornell University (USA), Centre de Recherche en Economie Appliquée (Senegal), and Institut National de la Recherche Agronomique (INRA, France).[10] The project was conceived in part as a follow-up survey to an earlier school-based survey known as PASEC (Programme d'Analyse des Systemes Educatifs de la CONFEMEN). That survey administered tests of written math and French to a cohort of students beginning in 2nd grade in 1996 and continuing through primary school (see Michaelowa, 2001; CONFEMEN, 1999). The 2003 survey attempted to relocate these children, who were now of middle school age (between 14 and 17). 28 rural and 32 urban communities of the original 120 PASEC clusters (defined by the catchment areas of the primary school in which the tests took place) were chosen for the new survey. Of the 20 PASEC children per cluster/school who were tested in 2nd grade in 1996, survey enumerators were able to find 15 on average in rural clusters and 17 in urban ones.

The original PASEC cohort itself was not a representative sample because it was a school-based (generally public school) sample. A small but non-trivial portion of children in Senegal never enter school, while others may attend private schools in areas where they are available. For these reasons the EBMS also interviewed randomly selected households in the same school catchment areas. Eligibility for inclusion in the sample required the presence of a child of the same age as the PASEC cohort. The number of such households was determined by the number of PASEC households relocated, with the objective of having 30 households in total per cluster. The total sample thus consists of 1820 households in 60 clusters. In terms of schooling, the sample is similar to national averages compiled by the World Bank. For example, net primary enrollment in our sample (primary enrollments of children 7-12) is 66 percent compared with 63 percent for the country as whole in 2000.

The EBMS consists of four linked surveys: the tests, the household survey, the school surveys, and the community survey. The household survey differs from standard surveys in its the level of detail on education as well as the inclusion of retrospective information on family economic and health shocks (discussed in Section 2) that may have influenced school enrollment and continuation and can potentially instrument grade attainment in test score regressions. The survey did not collect consumption or income data, so as a measure of household resources we use a wealth index based on information on assets. Specifically, following the methodology of Sahn and Stifel (2003), we use factor analysis to construct an asset index using information on ownership of durable goods such as a

[10] The project was funded by the US Agency for International Development, World Bank, Cornell University, UNICEF, and INRA.

radio, TV, refrigerator, and bicycle, motorcycle or car, as well as the source of drinking water (piped, surface water, or well water), and toilet facilities (flush, pit toilet, or latrine) of the domicile.

The school surveys were administered to directors of up to three schools (primary or lower secondary) used by local residents, including the original PASEC school. Information was collected on standard school quality measures such as textbook availability, teacher qualifications, and building condition, as well as aspects of school and classroom organization and management. We use information on the local PASEC school in the regressions as measures of local school quality. There are several reasons for using the data from this school. First, it is typically the main primary school of the area—in part because the cluster is defined as the catchment area around the PASEC school. Second, to model the effects of school covariates in a true production function framework for a sample of middle school age children, it would be necessary to specify these variables for the actual primary and (where relevant) lower secondary schools attended, and also, given that school choice cannot be assumed exogenous to test outcomes, to model these choices jointly with years of school as well as test scores. This would be a very demanding if not impossible task. Note, however, that the estimates on our school covariates can be given valid interpretations provided one is clear about what they are showing. Rather than as a human capital production function, it is appropriate to view the equation as a reduced form relation showing the net effect of local public school quality, incorporating substitution with private school (and with non-schooling).[11]

Each child age 14 to 17 was requested to take four relatively short tests: written French, written math, oral math, and 'life skills' (also oral). The tests were developed by the Institut National d'Etude et d'Action pour le Développement de l'Education in collaboration with the EBMS team (the test questionnaires are available from the authors). One reason for creating the two oral tests is that children with little or no schooling, or who have been long out of school, may be reluctant to even attempt a written test. The oral tests provided a more acceptable means to test their knowledge. Children also had the choice of having both oral tests administered in either French or the local language. The oral math test measures basic competence in mathematical calculations and solving simple problems. The life skills test was created so as to be able to measure basic practical knowledge rather than simply academic knowledge and included questions about nutrition and health practices, HIV/AIDS, government institutions and organization, and other topics. As anticipated, the number of children willing to take the oral tests alone was greater than the number willing to take all four tests. Among girls, for example, 931 took oral math and 970 took the life skills test but only 646 and 679 girls, respectively, agreed to take the written Math and French tests.

Our measure of schooling is the standard one, the highest completed grade. Given grade repetition this need not be the same as total years attending school, an issue we take up below in Section 4. Table 1 shows grade attainment and mean test scores by asset quartile and gender. Girls in the poorest quartile average 3.5 years of education compared with 6.6 for those in the richest. The

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[11] The distinction is important. Treated as a production function for human capital, Eq. (3) including just local public school quality would imply bias from likely non-random measurement error in school quality. Assume that all children enroll in either public or private school and let $Q_{pub\,j}$ and $Q_{pvt\,j}$ represent the level of quality of public and private schooling in community j, which is assumed to differ such that private quality is greater than public by some proportion ζ. Let $Public_i$ and $Private_i$ be indicators of public and private enrollment of child *i* and as in Eq. 3 let b_2 be the effect of the level of quality on human capital. Then the true impact of school quality on test scores of child *i* is equal to $b_2[Q_{pub\,j}*Public_i + (1+\zeta)Q_{pub\,j}*Private_i]$. Including only $Q_{pub\,j}$ thus implies measurement error in school quality equal to ζQ_{pub} for private enrolled children; note that this error is systematically negative if private school quality is higher than public school quality.

pattern is similar for boys though with higher schooling overall (5.5 vs. 5.0 years for girls). The wealth gap is schooling is thus quite large and, further, the differences in completed schooling are underestimated by these figures because of censoring: a larger share of wealthier children still attend school so will have a final grade attainment higher than their current grade. Similar patterns by wealth and gender are seen for primary completion (6 years or more) and non-enrollment.

The raw test scores are transformed for this analysis into standardized scores with zero mean and standard deviation equal to one, using the pooled girl and boy sample for each test. The higher cognitive skills of children in wealthier households are quite evident from Table 1. For both girls and boys, wealth gaps appear substantially larger for oral math and life skills than for the two written tests. The difference in mean scores of the top and bottom quartiles are always over 0.70 standard deviations for the oral tests but 0.50 s.d. or smaller for written French and math. A caveat is that these gaps are based on scores conditional on test participation. Given selection into the test-taking sample, which apparently operates more strongly for written tests, the wealth gaps among the population of children 14-17 as a whole may be different than among test participants. Our simulations using unconditional predicted scores in Section 4 addresses this problem, and considers the extent to which schooling disparities explain knowledge gaps between poor and well off children.

There are gender differences in test outcomes as well, though they are smaller than those between poorest and wealthiest quartiles. The largest gaps are for written and oral math, equal in both cases to 0.15 standard deviations (comparing the 'all' columns for girls and boys).

4. Results

We begin by considering the OLS and instrumental variables estimates of the impact of grade attainment on test scores. Table 2 shows the coefficients on years of schooling from OLS and IV regressions with and without controls for cluster fixed effects. The models include controls for selection into the sample of test-takers, using enumerator codes as instruments for the selection terms. The table also reports Sargan-Hansen tests of the overidentification restrictions. Conditional in the case of a single endogenous regressor on having at least one valid instrument (i.e., the structural equation is assumed to be at least exactly identified) these tests assess the joint hypothesis that the exclusion restrictions on the other instruments are valid, that is to say, the instruments are orthogonal to the disturbances in the test score regression. The p-values for the chi-square test statistics support the validity of the instruments: we are unable to reject the null in all cases except for boy's life skills where it can be rejected at 10%. The test does not require specifying which instrument(s) are valid; alternatively, if one or several instruments *a priori* can reasonably be regarded as excludable, one can test whether a specific subset of additional instruments are also valid. We do this using the 'difference-in-Sargan' or C-statistic (Hayashi, 2000). The considerations noted in Section 2 suggest to us that distance from the cluster to lower secondary school has the strongest claim to orthogonality (an assumption that cannot, of course, be tested). If distance is maintained exogenous in the non-fixed effects models (it necessarily drops out of the community fixed effects), then as shown, the overidentifying restrictions cannot be rejected for the subset of the other instruments used in these models, including enterprise shocks and highest education among each parents' siblings.

Turning to the schooling estimates themselves, for the regressions not including community fixed effects, for both boys and girls the point estimates in the IV models are usually similar in magnitude to – within 20% of – the OLS estimates (compare cols 1 and 2 for boys and 5 and 6 for girls). The one real exception to this out of the eight cases is written math for girls. For this test the girls' and boys' OLS coefficients are similar to one another, but for girls the IV estimate is basically zero and statistically insignificant, a clearly implausible result for the impact of schooling on a written test in an academic subject.

Columns 3 and 4 and 7 and 8 add cluster dummies to the OLS and IV regressions. Notably, in the models that do not instrument schooling, controlling for community level fixed effects essentially has no effect on the estimates of schooling impacts on test scores. Further, for boys, instrumented schooling impacts in the community fixed effects models are by and large similar to the non-IV fixed effects estimates (compare cols. 3 and 4).

For girls, on the other hand, the community fixed effects IV estimates for all four tests (last column) are implausible, that is, very small or even negative and not significantly different from zero. In part this is related to the power of the instruments, which for the two oral tests fixed effects models is substantially lower for girls than boys. For both oral tests, in fact, the F statistic for girls is near zero, meaning that our instruments are essentially unable to predict grade attainment. Although the tests discussed above provide support for the credibility of our instruments in terms of exclusion assumptions, research on instrumental variables also highlights the issue of 'weak' instruments, meaning an insufficiently strong correlation with the variable(s) they are supposed to predict. Among other problems this can lead to large finite sample bias in 2SLS estimates (Bound, Jaeger, & Baker, 1995). In the IV models without cluster fixed effects the impacts of the instruments in the first stage regressions are usually easily significantly different from zero at the 1 percent level, as indicated in the table. However, Staiger and Stock (1997), among others, suggest using values less than 10.0 for F-statistics for the instruments as a sign of potential problems. This threshold is closer to being achieved for boys than girls in our applications, but even for boys is only approached (and not reached) in two of the non-cluster fixed effects models. Therefore bias in several of the two stage estimates from weak instruments may be an issue here.[12]

Considering the results as a whole, in most cases where the instruments appear strong as indicated by relatively high F-statistics (hence where the two-stage estimates can be presumed to be reliable), the OLS and IV schooling estimates are consistent if not identical in magnitude. Wu-Hausman tests generally cannot reject the null of exogeneity of schooling.[13] In view of this and the

[12] Staiger and Stock (1997) (see also Bound et al., 1995) show that the finite sample bias in the 2SLS estimate will be toward the OLS estimate. This is clearly not occurring in the case of girl's written math, for which the OLS estimate is 0.22 while the 2SLS estimate is just .032. For the latter to be biased toward the former and thus away from the 'true' value, the true value would have to be even lower than .032 which is inconsistent with our strong priors that there should be a statistically significant and nontrivially positive effect of schooling on test performance. However, these authors show as well that with weak instruments, bias in 2SLS estimates (in any direction) may occur if there is even a weak correlation of the instruments with the second stage equation dependent variable, i.e., if the exclusion restriction is not perfectly valid.

[13] The Wu-Hausman test is implemented by including in the test score regression the residuals calculated from the first stage grade attainment regression. Out of 16 cases (4 tests x 2 regressions x 2 genders) the residual was significant at conventional levels only for the fixed effects specification for girls' written math. Therefore these standard tests provide support for the assumption of exogeneity, hence the validity of the OLS estimates, but it should be noted that the tests have

evident problems with the IV estimates in a number of other cases, we will discuss only the non-IV estimates in the remainder of the paper. Still, we are not making the claim that these estimates are completely free of endogeneity biases, and this should be kept in mind in the discussion of the results.

With respect to the magnitudes of the (non-IV) schooling coefficients, we will discuss specifically the community fixed effects results in what follows, but as noted these are essentially the same as without the community controls. For girls, an additional year of school has large impacts on test scores in both written French and written math, raising them by 0.21 and 0.22 standard deviations, respectively. The point estimates of the impacts for boys are slightly larger for both of these tests but not significantly so.[14] Boy and girl school effects diverge more sharply in the oral tests. For oral math, the effect of an additional year of school is only about 0.11 standard deviations for girls compared with over 0.16 for boys. For life skills we have a similar pattern, about 0.11 and 0.15. These differences are statistically significant. Both oral math ability and life skills may involve somewhat different sets of skills than the more academic written math and French; the gender differences in schooling effects on the former suggest that schooling is better at imparting these 'practical' skills to boys.[15]

Still, the results indicate that better educated children of either sex have an advantage over less educated children not just in standard academic subjects but also in practical ones. Since the coefficients are smaller for life skills (and oral math) than for written French and math, we might be tempted to conclude that schooling matters less for practical knowledge than for academic knowledge. However, while this may be plausible, the comparison is problematic. Since each score is normalized on the standard deviation of scores for that test, it uses not the full sample variance but the variance of the samples of test takers. Since the samples differ across tests, comparison of the effects of schooling and other covariates across tests are problematic even with the raw scores standardized in the usual way, unless the full sample variance is assumed to be the same as that for the test takers. As a practical matter this is not much of an issue for comparing just the two written tests, for which the samples largely overlapped. It also not an issue for comparing the impacts of covariates for boys and girls for a given test since the standardization is on the same pooled (boy and girl) test sample variance.

For results for the other covariates we turn to Tables 3 and 4 for the written tests and oral tests, respectively. Note that the coefficients on the selectivity terms in the written French and math test score regressions in Table 3 are positive and often significant, suggesting, as we might expect, positive selection on ability into the samples participating in the written exams. The evidence is weaker for the two oral tests, and there is one significant negative coefficient, for girls' life skills.[16]

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low power, as suggested by their inability to reject consistency of OLS and IV estimates in several cases where the two appear quite different.

[14] For each test, boy and girl coefficients are directly comparable because the dependent variable is standardized on the pooled (boy and girl) distribution, that is, the score is the difference from the mean of the pooled sample divided by its standard deviation.

[15] Somewhat speculatively, teachers (or parents) may be more concerned to insure that schooling has practical value for boys, who are expected to enter the labor market.

[16] Positive selection on cognitive ability is further supported by separate analysis on the subsample of children tested in 2nd grade for the original PASEC study. Residuals from regressions of normalized 2nd grade test scores using the same regressors as the 2003 test score models were calculated as measures of idiosyncratic ability. These residuals were positive

Conditional on the child's schooling attainment, parental education has rather limited effects on cognitive outcomes. In the regressions without cluster dummies there are several cases where parental schooling is significantly positively associated with test score, but the point estimates and significance levels often fall substantially when controlling for cluster fixed effects. In these regressions we see impacts for father's schooling on boys' test scores for written French and oral math, and for mother's schooling on boys' life skills knowledge. For girls, the only significant effects of parental education in the fixed effects models are for father's education in life skills. Where there are education impacts, they seem to be modest. For example, an additional year of paternal education raises boys' written French scores by 0.016 standard deviations; hence a boy with a primary educated father (six years of school) would score about 1.0 s.d. higher than one with an uneducated father. Alternate specifications were tried that allowed for non-linear effects of parental schooling but these provided poorer fits to the data, an unsurprising outcome given that few parents have gone beyond the primary level (Table 1).[17]

Household wealth as represented by the asset index also has fairly limited impacts on test scores. In the boys' cluster fixed effects regressions, significant positive effects of assets are seen for written French. For girls, the asset index only approaches statistical significance for life skills.

On the two oral tests but (surprisingly) not the written tests, rural children perform poorly compared with urban children. For example, the effect of rural location (estimated in the models without community fixed effects) is to lower oral math scores by 0.23 s.d. for girls and 16 s.d. for boys. Given the coefficient magnitudes for years of schooling in the same regressions, these effects are equivalent to a two and one year schooling disadvantage, respectively. It is possible that the rural dummy captures unmeasured aspects of school quality that are poorer in rural areas, but the results may instead reflect greater work demands on rural school children's time, leading to reduced time allocated to schoolwork hence lower scholastic achievement.

Age is entered as year dummies for age 14 to 16 (the base is age 17). For the two written tests younger children tend to do better, while for oral math and (more consistently) life skills, older children do better. It is likely that the practical knowledge represented by oral math and like skills tests deepens as children get older and gain experience at work and other practical activities. This may be less important for academic skills, which furthermore may depreciate with time out of school. Controlling for other factors (including schooling), such depreciation would be positively associated with age, hence lead to the negative impacts of age observed for written French and math.[18]

As described above, school characteristics (included in the non community fixed effects regressions) are drawn from the school survey for the main local (public) primary school. Several

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and significant in 2003 test-taking probits on this subsample, and were generally positive and significant in the 2003 test regressions as well, indicating that more able children are indeed more likely to take the tests, and to do well on them.

[17] An anonymous referee points out that for the mother the opportunity costs of time associated with better schooling (through a higher potential wage) could increase her labor supply and thus reduce the time she spends helping the child with schoolwork. This would tend *cet. par*. to reduce the estimated positive effects of education, though one might also expect positive effects from higher maternal earnings.

[18] To the extent that it is not captured by the age dummies, depreciation of human capital with time out of school would also imply an upward bias in the estimated impact of years of school: those with lower schooling are also likely to have been out of school longer at the time of testing, hence suffered greater loss of the skills acquired at school.

characteristics of the school director seem to matter for test outcomes. Male directors (over 90% of cases) are associated with better performance of girls on life skills. Years of experience as a director has no effects, but a dummy for the director having attained a baccalaureate degree or better is generally *negatively* associated with scores on the written tests for girls as well as boys. It is possible that this 'effect' of director education reflects compensatory efforts by the education authorities to allocate more qualified directors to poorly performing schools or areas. We do not see impacts of teacher education, except for girls' written math where the share of teachers with no better than a *college* (lower secondary) degree is negative, meaning that better educated teachers are associated with higher test scores.

A range of other variables from the school survey relating to pedagogy and facility condition, not reported, were generally not significantly associated with test scores controlling for level of schooling. The overall lack of significant school input effects, though a fairly common finding in non-experimental studies of cognitive skill determinants (see Glewwe and Kremer 2006), does not mean that they are not important in the production of human capital. As we have noted, these are not production function estimates but instead are essentially reduced form relations showing the effects on skills of the characteristics of local (usually public) primary schools. Not all of the sample children actually attended these schools, and further, these children are now of secondary school age, so our school data may not accurately reflect conditions extant when they were attending primary school.[19] Of course, both household and school variables may have indirect impacts on cognitive outcomes through their effects on school attainment. We explore these impacts below.

Given these likely data limitations for school quality, it is important to establish that our estimates of the impact of school *quantity* are not compromised. This would occur if the model excludes important aspects of quality that are correlated both with the level of schooling and test outcomes, whether because of parental education preferences that affect both or because higher quality improves test scores while also inducing parents to keep their children in school longer. We do not believe this to be happening with our estimates. First, the community fixed effects models remove any bias from the (linear) association of the level of schooling and unmeasured average local school quality. As seen, the fixed effects estimates of years of school impacts are almost the same as without these controls. Parents could still choose among high and low quality local schools, and this choice could be correlated with years of schooling (and test performance). However, the two stage estimates using predicted years of school remove the influence of household or individual-level unobservables that are correlated with test outcomes and level of schooling; these will include unmeasured school inputs (or variations in input quality) as long as these are not correlated with the instruments. As seen earlier, the IV estimates for schooling are for the most part consistent with the OLS results we are discussing here.[20] Finally, our expectation would be that any unmeasured quality would also be positively correlated with household education or wealth, leading to upward biases in the estimates for

[19] On the latter point, however, we note that the original PASEC survey collected school data, with a particular focus on pedagogy and the 2nd grade teachers' backgrounds, though there were some issues of data quality and missing responses. We similarly found very few significant associations of test scores with these variables.

[20] As discussed above, there are some exceptions to the overall consistency of the 2SLS and OLS estimates. When they are not consistent, however, this is quite unlikely to be due to unmeasured school quality information. This problem would presumably affect all the regressions to some extent, and should be similar for similar tests. Yet differences between OLS and 2SLS coefficients are seen for girls and not boys on the same test, of for girls on one written test and not the other.

the latter. However, the weak estimated impacts of parental schooling and assets argue against this process.

Note that our measure of schooling—grade attainment, or the highest grade completed—is not the same thing as total years in school. The two differ because of grade repetition, which is common in Francophone African education systems. Some 70% of children in our sample who ever attended school report at least one repeated grade (usually in primary), and mean number of repeats for this group is about 1.7 for both girls and boys. The question then arises whether and how to incorporate this in the estimation.[21] A logical approach is to enter the number of repeated grades and the highest grade attained separately in the test score regressions (note the two add up to total years in school). In such specifications (results available from the authors) the coefficient on the number of repeated grades is always negative and is large and highly significant for the two oral tests.

The fact that, controlling for completed grade level, repetition is negatively associated with achievement suggests that repetition captures unmeasured factors affecting test scores; if there were no such association, the effect of additional (repeated) years should be positive. Since grade repetition is a function of poor academic performance, this is not surprising. The number of repetitions conditional on completed grades could be viewed as a partial control for these unobservables, whether they represent individual ability or school (or teacher) differences in policies regarding grade promotion. Yet the addition of the repetition indicator to the test score regressions has very little impact on the estimates for the other covariates in the model, including highest grade as well as parental schooling and assets.[22] This reflects the fact that, perhaps contrary to expectations, repetitions are not associated with either parental education or assets, whether one conditions on grade attainment or not.[23] There are some associations with highest grade, depending on the test sample.[24] To the extent that the repetitions variable captures heterogeneity in factors affecting achievement, therefore, this seems to be largely random with respect to other covariates, hence not a serious problem for the estimates in Tables 3 and 4. However, it needs to be kept in mind that the grade attainment coefficients show the effects of successfully completing an additional grade, not of attending school for an additional year.

4.1. Implications for skill gaps of closing schooling gaps

The test score regression estimates allow us to address an important policy question: To what extent can policies that equalize schooling attainment between poor and well-off children, and between girls and boys, also close the existing gaps in human capital between these groups? The intervention

[21] We thank an anonymous referee for directing our attention to this issue.

[22] For example, for the boys community fixed effects regressions with the number of repetitions included, the coefficients on grade attainment are as follows (coefficients in original specification in parentheses): written French 0.238 (0.240); written math 0.226 (0.227); oral math 0.167 (0.162); life skills 0.149 (0.145).

[23] This was determined by regressing of the number of repetitions on highest grade and the other covariates. The results do show substantial effects of region, suggesting that the likelihood of repetition is a function, at least in part, of regional variation in promotion policy or school quality.

[24] The association of highest grade and number of repetitions tends to be positive for the oral math and life skills tests samples because these samples include more children who have no schooling (and thus no repetitions). It is negative for the written test samples, which include fewer such children hence in effect condition on having positive schooling.

we consider first is one that would make the level of schooling of 14-17 year old children in the poorest quartile (currently 3.5 years for girls and 4.4 years for boys) to be the same as that of the mean levels of children in the highest (6.6 for girls and 6.9 years for boys). For these simulations we want to consider gaps in expected human capital for the entire child sample, not just for those who actually took the tests. The appropriate unconditional predicted test score is obtained by applying the selectivity corrected regression coefficients (excluding the selection terms, since we are interested in the unconditional expectation) to the full sample data means for the group in question, for example, all girls in the lowest asset quartile.

For each test and gender, the first and second columns of Table 5 report the predicted standardized score[25] evaluated at the means of years of schooling and all other covariates for the fourth and first quartiles, respectively. The third column gives the predicted score for the first quartile under the assumption that years of schooling are the same as the mean for the richest quartile with all other covariates remaining at the first quartile values. As indicated, this implies increasing mean schooling of 14-17 year olds in the poorest quartile by about 2.5 years for girls and 3 years for boys.[26]

The differences across quartiles in expected test scores are large in all cases, often approaching one standard deviation and never lower than 0.69 s.d. The third columns for each gender indicate that raising the schooling of children in the lowest quartile to be the same as that of the highest substantially reduces the gaps in cognitive skills. For written French the expected test score gap is reduced by about 0.61 standard deviations for girls and 0.66 s.d. for boys, equivalent to proportional reductions of about 70 percent in each case. For written math the proportional reductions in the quartile gaps are 71 percent for boys and 81 percent for girls. The reductions are somewhat smaller for the oral math and life skills, reflecting the smaller schooling coefficients in these models. These simulations show that despite the existence of numerous wealth-related differences in family characteristics, school quality, and location that may have direct effects on learning outcomes, simply insuring that poorer children enter and stay in school to complete the same number of grades as wealthier children do will eliminate much or even most of the deficit in cognitive skills suffered by the former.[27]

This conclusion, moreover, is based on test scores and grade attainment for a school age cohort. Attainment for this group is censored, and this is a function of wealth: among children still attending school, wealthier children are more likely to stay in school longer. Completed schooling gaps between quartiles thus will be larger than gaps in current schooling, meaning that the ultimate cognitive skill

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[25] Standardized as before on the standard deviation of scores of the sample of test-takers.

[26] Note that for the two written tests, the unconditional predicted scores are lower than the scores for the sample of test-takers as reported in Table 1. Even in the top quartile, for boys the mean predicted standardized scores are no higher than the mean for children from all quartiles who took the tests and for girls in the highest quartile the predictions are non-trivially negative. This is because the mean characteristics of test participants for these two tests diverge substantially from the overall sample; this also applies to differences in unobservables (sample selectivity) the effects of which are also removed from the predictions. For oral math and life skills, selection was less operative in terms of both observables and unobservables, so mean predicted scores are not very different from the actual mean scores for test takers in each quartile.

[27] Note again that we are talking about equalizing *grade attainment*, not years in school, controlling for differences between poor and wealthy children in family and school characteristics. If poor students were more likely to repeat grades, this would mean more total years in school for poorer children to attain the same number of completed (passed) grades. As noted earlier, however, we do not find an association of wealth and grade repetition conditional on attainment.

gaps by wealth will be greater than calculated using current grade attainment. Equalizing final schooling would therefore reduce the cognitive gaps by more than indicated in the table.[28]

We also considered gender gaps in test scores (results available from the authors). Differences in girl and boy mean predicted test scores are smaller than those between bottom and top wealth quartiles, in large part because schooling gaps are smaller. If girls were to be given the same schooling as the mean for boys (an increase from 5.0 to 5.5 years), the gaps in written test scores would be reduced from 0.26 to 0.15 s.d. for French and from 0.35 to 0.24 s.d. for math. Therefore as with wealth related gaps, equalizing years of education would help close the gaps in human capital between girls and boys, but here both the gaps and the absolute magnitudes of the reductions are relatively small.

4.2. Determinants of school attainment: total impacts of covariates on test scores

The estimates and simulations above were concerned with the effects on test scores of years of schooling and wealth, parental education, and school characteristics conditioning on schooling. Although wealth and parental education have relatively small or inconsistently significant direct impacts on learning, they may still have important indirect effects on test outcomes by affecting the level of schooling. To calculate the total (direct and indirect) impacts of covariates on cognitive skills we use estimates obtained from a two-limit tobit model of grade attainment. The two-limit tobit allows us to account for the presence of both right and left hand censoring: left, because some children never attended school and thus are censored at zero years, and right, because many children—in fact, 60 percent—are still attending school so their ultimate attainment is not observed.

Appendix Table 1 shows marginal effects from the tobit estimations for girls and boys. We present changes in predicted *completed* schooling with respect to the variable rather than predicted current schooling, i.e., the level attained so far among 14 to 17 year olds. Effects on current schooling would be limited by the fact that most children of this age are (and are predicted by the model to be) still attending school. Changes in *x* for this group have no effect on current schooling but may affect ultimate attainment.[29]

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[28] Our preferred regression specification implies that the reduction in the cognitive gaps would be larger for completed schooling not just absolutely but proportionately as well, assuming that it is valid to project the estimated relationship of the *x*s to cognitive outcomes to higher levels of schooling. This derives from the equality of the direct schooling impacts across quartiles combined with lower base scores (equal to *xb* for covariates other than schooling) for the poorer group: at higher levels of schooling, schooling accounts for more of the absolute skill level and more of the difference in skills between quartiles, so closing the completed schooling gap leads to greater proportional reduction in the skills gap than would just equalizing current schooling.

[29] We calculate predicted completed schooling and its derivatives using the tobit estimates and data as follows. Define S_i as observed years of schooling, S_i^* as the latent function for years of schooling, and the upper censoring limit as UL_i, equal to current grade for a child still in school; the lower censoring limit is zero years (See Maddala (1983) for a general presentation of the two limit tobit model). We calculate predicted completed schooling for observation *i* as $E(S_i) = E(S_i|0<S_i^*<UL_i) \cdot Prob(0 < S_i^*< UL_i) + E(S_i| S_i^*> UL_i)\cdot Prob(S_i^*> UL_i)$. The last term shows desired final schooling conditional on still being in school. An alternative and somewhat more flexible model that accounts for uncompleted schooling, the ordered probit, estimates threshold parameters for each value of S_i. Given this, it cannot be used to predict final schooling for this cohort: for higher grades there are either no or too few observations to permit identification of the thresholds for these grades, which would be required for the predictions of completed years of school.

In contrast to their uneven impacts on test scores controlling for schooling, both parental education and household wealth have generally strongly significant positive impacts on children's grade attainment, with statistically similar effects for girls and boys. The parental education impacts are not very large. For girls, for example, the marginal effect of mother's schooling is 0.15, implying that having a mother with completed primary schooling (6 years) results in about a one year gain in a girl's completed education relative to having a mother with no schooling. Wealth effects on attainment, however, seem substantial. Given a standard deviation of the asset index close to unity (0.96), the table indicates that an approximately one s.d. increase in the wealth index raises expected completed schooling by 2.5 years for girls (1.9 years for boys), close to three times the effect of maternal primary schooling and twice the effect of paternal primary schooling. As found in many studies (Glick, 2006), distance to school is a greater barrier to girls' attainment than boys: distances to both the nearest primary and lower secondary schools (obtained from the community survey) have strongly significant negative effects on girls' grade attainment, while primary distance has no effect on boys and the boys' estimate for secondary distance effect is half that for girls.

Next we show the total contribution to human capital outcomes of selected covariates, both directly and indirectly through grade attainment. Table 6 presents these decompositions for written French and math tests only; the results for the two oral tests were qualitatively similar. Since the calculations of indirect effects use the marginal effect of the variable on predicted final schooling, the table shows the effects on cognitive skills once schooling is completed. To work through to predicted cognitive outcomes in this scenario, we assume that the effect of schooling on skills for subsequent grades is the same as that estimated using current schooling information, that is, b_1 in Eq. (3). We focus the discussion on the impacts of parental education and wealth.

The total impacts (first and fourth columns) of mother and father education are generally similar for the two tests and for girls and boys. For written French, an additional year of mother's schooling raises predicted scores by 0.06 and 0.05 standard deviations for girls and boys, respectively, while an additional year of father's schooling raises scores by 0.06 and 0.07 s.d. These changes come mostly through changes in grade attainment rather than through direct effects conditional on attainment. Note as well that the total impacts of an additional year of parental schooling are roughly one fourth that of a year of the child's own schooling (equal to 0.21 for girls and 0.24 for boys).

An approximately one standard deviation increase in the wealth index leads to a more substantial increase in expected scores of slightly over 0.6 s.d. for girls on each test, with somewhat lower gains for boys. Again the indirect effects contribute more to the total than the direct effects, reflecting the limited impacts of both parental education and wealth seen in the test score regressions. This reinforces the idea that policies to address cognitive skill gaps can effectively compensate for poor family background by raising schooling among disadvantaged children.[30]

[30] The very large apparent impacts of primary school distance in Table 6 reflects the large impact of primary distance on attainment in the tobits; this estimate in turn essentially captures the effect of not having a local primary school. The latter characterizes only a very small share of the sample, so the policy potential of this result is unclear.

6. Conclusions

This study has analyzed the relationship of cognitive skills to schooling attainment, family background, and school characteristics in a sample of 14 to 17 year olds in Senegal. Unlike most earlier work, our test score regressions are able to incorporate a range of household and school characteristics that are also likely to have impacts on human capital accumulation. In a further departure from standard school-based testing approaches, the tests—in written French, written math, and oral math and life skills—were administered to a cohort of 14 to 17 year olds that included current students, children who were no longer in school, and children with no formal education. The regressions controlled for selective participation in the taking of the tests, as well as, with more mixed results, the potential endogeneity of grade attainment to test outcomes.

The results confirm that the quantity of schooling matters strongly, both for standard academic knowledge and less academic 'life skills'. The results also suggest that, conditional on a child's level of schooling, having better educated parents or a higher level of household resources have only modest or statistically insignificant benefits for academic performance. This is also the case for the effects of school quality variables in our models, which enter essentially in a reduced form manner and capture the net effects of local public primary school characteristics.

These findings suggest that efforts to enroll and keep in school children from less advantaged backgrounds will contribute significantly to closing not just schooling gaps themselves but also the substantial skill gaps that exist between them and more affluent children. For written French and math, simulations imply that bringing current school attainment of 14 to17 year olds in the poorest wealth quartile up to the level of the wealthiest quartile would lead to proportional reductions of 70 to 80 percent in the current mean test score gaps between these quartiles. Gender gaps in cognitive skills are more modest, but in this case too, achieving (gender) parity in the level of schooling will serve to close a significant portion of these skill gaps.

While household characteristics have limited effects on test outcomes controlling for grade attainment, they have strongly significant impacts on attainment itself. Wealth in particular has large effects on schooling. Therefore to the extent that children from better-educated and (especially) well-off households gain more cognitive skills, this advantage comes mostly through indirect effects on the level of schooling rather than direct effects on learning. Again, this points to the need for, and large potential benefits of, policies to equalize school enrollment and continuation to compensate for differences in family backgrounds.

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Table 1
Children 14-17: Sample means

	Boys						Girls					
	Wealth quartile[a]						Wealth quartile[a]					
	1	2	3	4	Q4-Q1	All	1	2	3	4	Q4-Q1	All
Grade attainment[b]	4.41	4.95	5.91	6.94	2.53	5.49	3.52	4.36	5.27	6.59	3.07	5.00
Completed primary school	0.44	0.51	0.67	0.81	0.38	0.60	0.35	0.47	0.57	0.73	0.38	0.54
Never enrolled	0.19	0.14	0.07	0.04	-0.16	0.11	0.34	0.24	0.12	0.04	-0.29	0.18
Standardized test scores:												
Written french	-0.107	-0.151	-0.104	0.390	0.50	0.022	-0.171	-0.177	-0.156	0.329	0.50	-0.032
Written math	-0.043	-0.126	-0.054	0.381	0.42	0.057	-0.168	-0.189	-0.224	0.182	0.35	-0.094
Oral math	-0.252	-0.095	0.114	0.577	0.83	0.070	-0.407	-0.280	-0.009	0.313	0.72	-0.084
Life Skills	-0.294	-0.105	0.303	0.523	0.82	0.082	-0.458	-0.272	0.105	0.410	0.87	-0.040
Mother years of schooling						1.802						1.804
Father years of schooling						3.374						3.283
Asset index						0.020						0.145
School has separate toilets for girls, boys						0.432						0.416
Director years experience						11.141						11.020
Director male						0.937						0.934
Director has *bac* or higher						0.595						0.568
Share of teachers with *college* or less						0.198						0.221
Rural						0.534						0.485
Distance to primary (km)						0.257						0.282
Distance to lower secondary (km)						6.403						6.141
Mother died						0.039						0.048
Father/ household head died						0.146						0.129
Bad enterprise year						0.323						0.331
Highest mother sibling education						0.619						0.588
Mother sibling educ. missing						0.436						0.464
Highest mother sibling education						0.682						0.657
Father sibling educ. missing						0.335						0.347
No. of observations[c]						1,544						1,442

Notes:
[a]Quartiles of the asset index. Q4-Q1 is the difference in means of the 4th and 1st quartiles.
[b] Last sucessfully completed grade if still attending school, final grade attainment if no longer attending
[c] Refers to the full sample of children used to estimate grade attainment models. Sample sizes for test score regressions are given in Table 2.

Table 2
OLS and instrumental variables estimates of the effect of schooling on test scores

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Boys				Girls			
			With controls for community fixed effects				With controls for community fixed effects	
Test	OLS	IV[a]	OLS	IV[a]	OLS	IV[a]	OLS	IV[a]
Written French	0.242	0.255	0.229	0.270	0.214	0.196	0.223	-0.022
	(12.22)***	(2.87)***	(10.85)***	(3.14)***	(6.55)***	(1.25)	(6.28)***	(0.11)
F-test of instruments		3.71		3.22		5.94		3.29
p-value		0.00		0.01		0.00		0.01
Overidentification test:								
Sargan-Hansen (p-value)[b]		0.94		0.98		0.27		0.23
C-statistic (p-value)[c]		0.94				0.23		
Sample size	840		828		677		679	
Written math	0.231	0.171	0.227	0.258	0.222	0.030	0.219	-0.115
	(11.52)***	(1.86)*	(10.72)***	(2.79)***	(5.34)***	(0.21)	(4.88)***	(0.71)
F-test of instruments		4.22		3.31		7.66		6.35
p-value		0.00		0.01		0.00		0.00
Overidentification tests:								
Sargan-Hansen (p-value)[b]		0.32		0.46	0.69			0.82
C-statistic (p-value)[c]		0.40			0.58			
Sample size	794		797		645		646	
Oral math	0.165	0.144	0.163	0.193	0.114	0.091	0.124	-0.054
	(10.15)***	(2.11)**	(10.24)***	(2.80)***	(7.53)***	(1.42)	(7.66)***	(0.26)
F-test of instruments		8.28		6.63		4.19		0.71
p-value		0.000		0.00		0.03		0.62
Overidentification tests:								
Sargan-Hansen (p-value)[b]		0.41		0.86		0.89		0.77
C-statistic (p-value)[c]		0.66				0.83		
Sample size	1064		1042		924		931	
Life skills	0.149	0.205	0.145	0.223	0.111	0.121	0.118	0.026
	(9.80)***	(3.45)***	(9.03)***	(3.66)***	(7.95)***	(1.96)*	(7.55)***	(0.17)
F-test of instruments		8.59		6.31		4.52		0.85
p-value		0.00		0.00		0.00		0.52
Overidentification tests:								
Sargan-Hansen (p-value)[b]		0.09		0.68		0.91		0.85
C-statistic (p-value)[c]		0.10				0.84		
Sample size	1078		1125		943		970	

Notes:
t-statistics for regression coefficients in parentheses. All models include controls for selection and covariates described in the text. Samples sizes are smaller for non-cluster fixed effects due to dropped observations with missing values on school characteristics.
[a] Instruments: for non-fixed effects models, enterprise earnings shocks, distance to nearest lower secondary school and its square, education of most educated sibling of mother and of father. For fixed effects models, same but subtract lower secondary distance and add death of mother, death of father.
[b] Test of validity of all instruments conditional on structural equation being at least exactly identified. Test statistic is asymptotically chi-sq distributed under the null of orthogonality of instruments to errors in test regressions. Implemented in STATA using *ivreg2* routine.
[c] Tests validity of instruments other than distance to secondary school, for which exclusion restriction is assumed valid. Test statistic is asymptotically chi-sq distributed under the null of orthogonality of remaining instruments to errors in test regressions. Implemented in STATA using *ivreg2* routine.

Table 3
Children 14 to 17: determinants of test scores in Written French and math - OLS results (selected results)

	Written French				Written Math			
	Girls		Boys		Girls		Boys	
		Community fixed effects		Community fixed effects		Community fixed effects		Community fixed effects
Years of schooling	0.214	0.223	0.242	0.229	0.222	0.219	0.231	0.227
	(6.55)***	(6.28)***	(12.22)***	(10.85)***	(5.34)***	(4.88)***	(11.52)***	(10.72)***
Age=14	0.319	0.180	0.042	0.079	0.249	0.094	0.078	0.090
	(3.00)***	(1.43)	(0.52)	(0.95)	(1.81)*	(0.57)	(1.02)	(1.14)
Age=15	0.223	0.023	0.195	0.254	0.213	0.031	0.114	0.052
	(2.03)**	(0.18)	(1.60)	(2.11)**	(1.63)	(0.19)	(1.45)	(0.59)
Age=16	0.114	-0.120	0.187	0.168	0.271	0.086	0.084	-0.021
	(1.07)	(1.08)	(2.04)**	(1.74)*	(1.97)*	(0.55)	(0.96)	(0.26)
Mother schooling (years)	0.024	0.004	0.009	0.011	0.006	0.003	0.000	0.004
	(2.41)**	(0.50)	(0.95)	(1.11)	(0.64)	(0.29)	(0.00)	(0.51)
Father schooling (years)	0.010	0.006	0.017	0.016	0.014	0.009	0.013	0.010
	(1.32)	(0.85)	(1.99)*	(1.95)*	(1.87)*	(1.08)	(1.78)*	(1.27)
Asset index	0.091	0.054	0.079	0.176	0.050	0.040	0.048	0.090
	(1.43)	(0.89)	(1.12)	(2.34)**	(0.88)	(0.70)	(0.92)	(1.46)
Rural	-0.091		0.002		-0.136		0.055	
	(0.56)		(0.02)		(1.02)		(0.46)	
Public primary school characteristics:								
Separate toilets for girls, boys	-0.093		-0.084		0.043		-0.093	
	(0.85)		(0.85)		(0.45)		(0.99)	
Director years experience	0.002		0.004		0.002		0.000	
	(0.30)		(0.65)		(0.27)		(0.05)	
Director male	-0.010		-0.240		0.158		-0.172	
	(0.08)		(1.01)		(1.51)		(0.66)	
Director has *bac* or higher	-0.320		-0.161		-0.199		-0.127	
	(2.35)**		(1.64)		(1.82)*		(1.40)	
Share of teachers w/*college* or lower	-0.061		0.146		-0.286		0.069	
	(0.26)		(0.52)		(1.45)		(0.38)	
Lambda	0.549	0.100	0.342	0.508	0.505	0.114	0.382	0.132
	(2.96)***	(0.58)	(1.25)	(1.73)*	(4.22)***	(0.52)	(1.62)	(0.48)
No. of observations	677	679	828	838	645	646	794	797
R-squared	0.32	0.47	0.31	0.45	0.31	0.43	0.33	0.47

Notes:
Dependent variable is standardized test score. t-statistics in parentheses. Standard errors are adjusted for clustering.
Base category for age is 17. Models also include controls for region and ethnic group.
*** significant at 1%; **significant at 5%; *significant at 10%

Table 4
Children 14 to 17: determinants of test scores in oral math and life skills - OLS results (selected results)

	Oral Math				Life Skills			
	Girls		Boys		Girls		Boys	
		Community fixed effects		Community fixed effects		Community fixed effects		Community fixed effects
Years of schooling	0.114	0.124	0.165	0.163	0.111	0.118	0.149	0.145
	(7.53)***	(7.66)***	(10.15)***	(10.24)***	(7.95)***	(7.55)***	(9.80)***	(9.03)***
Age=14	-0.309	-0.270	-0.011	-0.001	-0.442	-0.353	-0.225	-0.168
	(3.73)***	(2.97)***	(0.14)	(0.01)	(5.35)***	(4.36)***	(3.09)***	(2.42)**
Age=15	-0.217	-0.182	0.118	0.150	-0.303	-0.206	-0.123	-0.057
	(2.04)**	(1.63)	(1.68)*	(2.06)**	(2.91)***	(2.02)**	(1.58)	(0.74)
Age=16	-0.147	-0.119	0.116	0.089	-0.220	-0.150	-0.029	-0.036
	(1.66)	(1.25)	(1.54)	(1.18)	(2.16)**	(1.72)*	(0.35)	(0.47)
Mother schooling (years)	0.020	0.010	0.024	0.018	0.010	-0.001	0.014	0.014
	(2.37)**	(1.09)	(2.36)**	(1.59)	(1.25)	(0.15)	(1.54)	(1.64)
Father schooling (years)	0.005	0.006	0.011	0.012	0.009	0.016	0.003	0.007
	(0.52)	(0.65)	(1.66)*	(1.85)*	(1.56)	(2.59)**	(0.46)	(1.00)
Asset index	0.063	0.057	-0.001	0.002	0.104	0.071	0.059	0.024
	(1.35)	(1.23)	(0.03)	(0.04)	(2.26)**	(1.56)	(1.13)	(0.47)
Rural	-0.232		-0.163		-0.291		-0.353	
	(2.03)**		(1.60)		(2.74)***		(3.70)***	
Public primary school characteristics:								
Separate toilets for girls, boys	0.011		0.165		-0.020		0.137	
	(0.11)		(1.94)*		(0.20)		(1.44)	
Director years experience	0.005		0.005		-0.003		0.000	
	(0.57)		(0.67)		(0.46)		(0.02)	
Director male	0.105		-0.009		0.334		-0.087	
	(0.86)		(0.03)		(2.29)**		(0.34)	
Director has *bac* or higher	-0.009		-0.030		0.011		0.032	
	(0.09)		(0.30)		(0.11)		(0.36)	
Share of teachers w/*college* or lower	-0.350		0.110		-0.014		-0.137	
	(1.92)*		(0.52)		(0.07)		(0.65)	
Lambda	0.075	0.067	0.399	0.228	-0.365	-0.099	-0.161	0.035
	(0.41)	(0.29)	(2.17)**	(1.16)	(1.84)*	(0.41)	(0.70)	(0.18)
No. of observations	924	931	1042	1064	943	970	1078	1125
R-squared	0.28	0.38	0.32	0.42	0.29	0.43	0.31	0.42

Notes:
Dependent variable is standardized test score. t-statistics in parentheses. Standard errors are adjusted for clustering.
Base category for age is 17. Models also include controls for region and ethnic group.
*** significant at 1%; **significant at 5%; *significant at 10%

Table 5
Wealth quartile gaps in predicted test scores

	Boys			Girls		
	Q4	Q1	Q1'	Q4	Q1	Q1'
Written French						
Mean score	0.011	-0.878	-0.267	-0.271	-1.210	-0.552
Test score gap (Q4-Q1)		0.889	0.277		0.939	0.281
Change in gap			-0.612			-0.658
Written Math						
Mean score	0.052	-0.762	-0.180	-0.334	-1.183	-0.499
Test score gap (Q4-Q1)		0.815	0.232		0.849	0.164
Change in gap			-0.583			-0.685
Oral Math						
Mean score	0.243	-0.488	-0.051	0.223	-0.468	-0.137
Test score gap (Q4-Q1)		0.732	0.294		0.691	0.360
Change in gap			-0.438			-0.331
Life Skills						
Mean score	0.553	-0.241	0.136	0.563	-0.337	0.004
Test score gap (Q4-Q1)		0.793	0.417		0.900	0.559
Change in gap			-0.376			-0.341

Notes:
Q4: predicted standardized test score at means of data for 4th quartile.
Q1': predicted standardized test score at means of data for 1st quartile but setting years of school equal to 4th quartile mean.
Q4-Q1: difference (in standard deviations) in scores of 4th and 1st quartile.

Table 6
Written French and math: decompositions of predicted effects of selected variables on standardized test scores

Variable	Girls			Boys		
	Total	Direct	Indirect	Total	Direct	Indirect
	$\partial H/\partial X_k$	$\partial H/\partial X_k \vert s$	$\partial H/\partial S \cdot \partial S/\partial X_k$	$\partial H/\partial X_k$	$\partial H/\partial X_k \vert s$	$\partial H/\partial S \cdot \partial S/\partial X_k$
			Written French			
Mother schooling	0.057	0.024	0.033	0.054	0.009	0.045
t-statistic		2.26	1.85		0.90	2.20
Father schooling	0.058	0.010	0.047	0.068	0.017	0.051
t-statistic		1.27	3.48		2.30	3.59
Assets	0.634	0.091	0.543	0.538	0.079	0.460
t-statistic		1.71	7.05		1.50	5.59
Primary distance (km)	-0.718		-0.718	0.056		0.056
t-statistic			-4.39			0.32
Lower secondary dist. (km)	-0.023		-0.023	-0.014		-0.014
t-statistic			-3.94			-2.36
			Written Math			
Mother schooling	0.040	0.006	0.034	0.043	0.000	0.043
t-statistic		0.57	1.85		0.00	2.20
Father schooling	0.064	0.014	0.049	0.062	0.013	0.048
t-statistic		1.75	3.48		2.05	3.59
Assets	0.614	0.050	0.564	0.486	0.048	0.438
t-statistic		0.91	7.05		1.03	5.59
Primary distance (km)	-0.747		-0.747	0.053		0.053
t-statistic			-4.39			0.32
Lower secondary distance (km)	-0.024		-0.024	-0.014		-0.014
t-statistic			-3.94			-2.36

Notes:
Calculations show effects in standard deviations of test scores.
t-statistics for indirect effects are for tobit marginal effects.

Appendix Table 1
Children 14-17: marginal effects from two-limit tobit models of grade attainment

	Girls		Boys		Boy-Girl Difference	
Variable	Marginal effect	t-statistic	Marginal effect	t-statistic	Marginal effect	t-statistic
Age=14	2.736	4.15	2.383	3.78	0.354	0.39
Age=15	1.198	1.88	1.879	3.16	-0.681	-0.78
Age=16	1.119	1.75	1.302	2.26	-0.183	-0.21
Mother schooling	0.153	1.85	0.184	2.20	-0.031	-0.26
Father schooling	0.222	3.48	0.209	3.59	0.013	0.16
Asset index	2.539	7.05	1.897	5.59	0.642	1.30
Separate toilets for girls, boys	-0.016	-0.03	0.898	1.87	-0.914	-1.34
Director years experience	-0.038	-1.12	0.030	0.91	-0.068	-1.44
Director male	-0.950	-0.90	-0.727	-0.69	-0.223	-0.15
Director has *bac* or higher	-1.084	-2.19	0.120	0.26	-1.205	-1.76
Share of teachers w/*college* or lower	1.186	1.10	2.029	1.84	-0.843	-0.55
Rural	1.399	2.11	0.654	1.01	0.745	0.80
Distance to primary (km)	-3.361	-4.39	0.230	0.32	-3.591	-3.43
Distance to lower secondary (km)	-0.108	-3.94	-0.060	-2.36	-0.048	-1.30
Mother died	-0.817	-0.81	-1.710	-1.70	0.893	0.63
Father/ household head died	0.380	0.57	0.453	0.76	-0.073	-0.08
Bad enterprise year	-0.107	-0.22	-1.000	-2.23	0.894	1.36
Highest mother sib education	0.240	0.94	0.459	1.86	-0.220	-0.62
Mother sib educ. missing	-0.284	-0.53	0.680	1.35	-0.963	-1.31
Highest father sib education	0.159	0.66	0.510	2.14	-0.351	-1.03
Father sib educ. missing	1.300	2.50	-0.159	-0.33	1.459	2.06
No. of observations	1,422		1,544			

Notes:
Shows effect of a unit change in the variable on predicted completed schooling. For discrete regressors, shows the change in the probability for a change in the variable from zero to 1. Standard errors are calculated using the delta method.
Base category for age is 17. Models also include controls for region and ethnicity.