Does work impede child's learning? The case of Senegal *

Christelle Dumas†

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Abstract

This paper assesses the impact of labor performed during childhood on cognitive achievement of teenagers, measured by tests. Introduction of community fixed effects and use of multiple tests taken at the entry of primary school allows to control for unobserved heterogeneity and measurement error in the entry tests. We find no detrimental impact of participation of children to economic activities on their subsequent learning once controlling for the number of years of education but rather a positive, though small, impact. This could come from increased monetary resources. Working more than 4 hours a week or as an employee though prevents the child to learn as much as the other children.

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†University Cergy-Pontoise-THEMA. 33, bd du Port. 95011 Cergy-Pontoise. France. Tel: (33-0) 1 34 25 72 76. christelle.dumas@u-cergy.fr

1 Introduction

In Senegal, one child out of three is working[1] while only half of the children receive a full primary education. Children are often portrayed as being either enrolled in school or economically active but not both in the same time. This representation is actually fallacious in most parts of the world and especially in Africa. In our Senegalese dataset, we find that 58% of working children are enrolled in school.[2] The picture is thus not as clear-cut as it may seem at first glance. Given the very low levels of education, understanding how they translate into human capital and how work may impede learning is valuable.

An important body of literature discusses the causes of child labor in developing countries; there is in comparison relatively little on the consequences of children's work on their future trajectories. While some people argue that every child should be entitled to live a work-free childhood, others consider child labor acceptable as long as it does not harm their development. The International Labor Organization's position is a mix of these two options since the Minimum Age Convention (C138) stipulates that:[3]

> National laws or regulations may permit the employment or work of persons 13 to 15 years of age on light work which is: (a) not likely to be harmful to their health or development; and (b) not such as to prejudice their attendance at school [...] or their capacity to benefit from the instruction received.

The aim of this paper is thus to assess the impact of children's participation to economic activities on their human capital accumulation. Research on this topic has so far mainly focused on the question of the impact of child labor on school attendance (Ravallion and Wodon, 2000; Boozer and Suri, 2001; Assaad et al., 2002; Canals-Cerda and Ridao-Cano, 2004; Beegle et al., 2004; Assaad et al., 2005). Most of these papers conclude to a fairly small effect of participating to economic activities on a range of educational outcomes related to enrollment.

However, in an environment where school quality is low, number of years spent in school may not be a very good indicator for future wages or productivity. Insofar as an important number of children are able to combine both schooling and work, the last aspect mentioned in the convention, namely the impact of participation to work on learning achievement, is particularly important and has seldom been studied. It could well be that almost no effect of labor can be identified, while labor has a detrimental impact on learning of children enrolled in school. We thus prefer to analyze cognitive achievement of children rather than their education level.

Labor is generally expected to be detrimental to learning since it reduces time available for resting, attending school or studying at home. Two points need to be made readily: first, we will not address the question of how work

[1] Source: Unicef, wwww.unicef.org/infobycountry, following ILO definition of child labor.

[2] 15% of children combine both activities while 14% have none

[3] Children below 12 should not work, even if it is not harmful, while children older than 13 may be allowed to under the conditions stipulated in the text.

disrupts schooling, meaning we will only measure the impact of work on learning for a given number of years of education. The only disruption that will be taken into account is the one on attendance for a child enrolled in school since it may well affect how much s/he gains from school. Second, this effect is potentially fairly small: in most cases, classes in primary school only last for half a day, leaving room for working during the other half. In addition, high agricultural season takes place during school holidays. It seems to be quite rare that children miss school in order to work. Still, exhaustion and impossibility to do homework could significantly slow down learning. On the other hand, working increases the available income and allows to pay for some expenses associated to education: transportation, lunches outside home, books or even tutorials. Moreover, working along with parents (since it is the prevalent working environment for children in Africa) may be an opportunity to learn from them. The sign of the effect of work on learning is thus ambiguous and needs to be empirically estimated.

To my knowledge, the only paper that estimates the impact of child labor on learning achievement measured by tests is Heady (2003) on Ghana.[4] He finds that work has a substantial negative effect on learning achievement in reading and mathematics if it is performed outside the home (even when controlling for education level of the children). Several drawbacks need to be considered though in this paper: first, it only uses current information of children's time allocation while the process of human capital accumulation is a long-term one. Current status of the child regarding school and work is probably a poor proxy of the past time allocation decisions especially since these choices depend highly on age. Second, human capital investments are likely to be affected by characteristics such as cognitive ability, preferences, school quality, working environment which may also determine learning achievement. While Heady attempts to control for these characteristics with a Raven's matrix, the strategy has some limitations: Raven's matrices may not only capture innate cognitive ability but also be affected by education as other tests scores have demonstrated to be (see, for example, Hansen et al. (2004)). This is even the more so that these tests results have been collected at the same time than the other outcomes. In addition, using this variable in a control strategy assumes there is no measurement error in abilities. Lastly, while it is argued to capture heterogeneity in innate abilities, other environmental factors such as school quality or labor market organisations are unlikely to be captured by such a variable and, as such, is unobserved heterogeneity that could affect both investments decisions and results.

As a consequence, our paper tries to investigate a bit further the relationship between participation to economic activities and learning achievement. To address the previously mentioned caveats, we use the EBMS data which provides tests results for a sample of Senegalese teenagers along with retrospective

[4]Other papers that incidentally address the question by using other indicators of achievement include Patrinos and Psacharopoulos (1995) (grade repetition) and Akabayashi and Psacharopoulos (1999) (parental assessment of the ability of the child to read and do written calculations). Both of them find virtually no effect of labor on cognitive achievement.

information on work and schooling. The number of years spent working and at school should be more relevant for explaining children's learning achievement than their current status. In order to take into account the endogeneity issue for time allocation decisions, we combine the following strategies: we introduce communities' fixed effects to control for heterogeneity such as differences in school quality and working opportunities on the one hand and we control for results obtained to tests taken at the entry of primary school on the other hand. These tests can be argued to capture true innate ability given the early age at which they are taken. Nevertheless, they still could be spoilt by measurement error. Availability of other tests taken by the same pupils later in the year allows us to deal with this issue and our strategy provides reliable estimates of causal impact of work on learning achievement.

We find that work is not detrimental to learning achievement per se: once controlling for education level, the effect of one additional year of working is significantly positive for 3 tests out of 4. On a more methodological aspect, controlling for selection bias through measures of ability and communities fixed effects proves important and indicates that the naive estimate of the effect of work is downwardly biased, as expected.

Section 2 describes data and provides some statistics on the relationships between working and schooling decisions. Section 3 presents the empirical model and section 4 offers the results.

2 Data description and time allocation of children in Senegal

2.1 Ebms and Pasec datasets

The data used in this paper comes from an original survey entitled Éducation et Bien-Être des Ménages au Sénégal (Ebms)[5] conducted between April and June 2003 and to which the author of this paper took part. This survey covers a national sample of 1800 households. It provides information on household composition, household asset ownership and housing characteristics. At the individual level, information was collected on education, health, employment status and activities of every household member. We thus know the detail of children's schooling trajectories and their working starting date. For the purpose of this study, it is noteworthy that such an information is not available for the participation of children in housekeeping; this prevents us from studying the impact of domestic chores on learning but it could be expected to be lower than the effect of economic activities. Additionally, asking for retrospective data on housekeeping would have been very prone to measurement error and arguably with little relevance. As a result, this study focuses on the impact of

[5]This survey was designed by a team composed of Peter Glick, David Sahn, and Léopold Sarr (Cornell University, USA), and Christelle Dumas and Sylvie Lambert (LEA-INRA, France), and implemented in association with the Centre de Recherche en Économie Appliquée (Dakar, Senegal).

participation to economic activities, which is consistent with the ILO definition of child labor.

The sample includes households of children who participated in an earlier survey conducted by the PASEC.[6] These children, who were roughly aged between 7 and 10 in 1995, were tested every year as long as they were still enrolled. This paper only uses the first tests in mathematics and French, which took place at the beginning (in 95) and at the end (in 96) of their second grade. The Ebms survey was designed so as to complement the PASEC survey, in order to obtain more information on the households.[7] The cluster structure of the original PASEC survey was therefore maintained. We recovered on average 13 children per cluster (out of the 20 who participated) for 60 clusters from the original PASEC sample.

Moreover, a sub-sample of teenagers took several tests designed by the Ineade:[8] two of these tests (life skills and easy mathematics) are oral ones, while the two more advanced (French and advanced mathematics) are written. In addition, the oral ones could be taken in local language rather than French if the child wished so.[9] In total, 2366 children have taken at least one out of the four tests in 2003, 1597 have taken the four and among the Pasec children, they are respectively 594 and 468. The core sample for this paper is constituted of Pasec children tested in 2003.

2.2 Variables

As previously mentioned, we aim to assess the impact of child work on learning achievement. Learning achievement will be measured by tests taken in 2003 by teenagers while their initial abilities are assessed by tests they have taken in 1995. In order to take into account the full work trajectory, we use the number of years where the individual has performed an economic activity during childhood. Controlling for time spent at school allows us to evaluate the pure effect of work on learning. To do so, we compute the number of years spent studying. This section gives more details on how we define these variables and briefly discusses the control variables introduced in the estimations.

2.2.1 Education and work

The number of years of education is computed by adding the number of repetitions to the last grade completed by the pupil. Given that the survey took place just before the end of the school year, children attending school at that time had nearly completed one more year than the last passed grade. For those children, we add one to the previous calculation. The resulting variable is thus

[6] Programme d'analyse des systèmes éducatifs des pays membres de la CONFEMEN.

[7] Another goal of the Ebms survey was to increase the sample with households not sending their children to school in each cluster. This aspect is not used in this study.

[8] Institut National d'Étude et d'Action pour le Développement de l'Éducation. The Ineade had already been in charge of designing the tests for the Pasec.

[9] French is very rarely spoken in the families and is learnt at school.

the number of years of education at the time of the survey, independently of the final grade attained. If child labor increases repetition then it will show up in the estimates since for a given number of years of education, working children have attained lower grades than the ones who do not work and as such have lower outcomes measured by tests in 2003.

In the data set, we do know the age when the child started working but we do not know if s/he has been working continually since. What we call "number of years of work" is actually the number of years since the child started working. As mentioned, it only takes into account participation to economic activities. The rationale for focusing on work duration is that even if the child interrupted his/her activity, it should still be a good proxy for past time allocations: children who already have performed an activity are more likely to do it again. Actually, only 3.24% of children who have ever worked have not been performing any economic activity during the year of the survey. In addition, working very early in life may be particularly detrimental and this should be captured when considering age when started working. In addition of age, we have some information on the working conditions when the child started, namely hours of work and employment status (employed, independent or familial worker). Combining hours of work with the time spent working at the time of the survey will provide us with a very rough (and unperfect) proxy for number of hours worked in childhood. In the last section, we enquire into whether these various measures of work intensity matter for learning.

2.2.2 Tests

Tests in 1995 (and subsequent years) and then in 2003 have all been designed by the Ineade. In 1995, there were only two written tests, one of mathematics, the second of French; in 2003, four tests are available: easy and advanced mathematics, French and life skills. They aim to assess cognitive acquisition due to school. As such, they are focused on evaluation of schooling outcomes such as reading, vocabulary, conjugation for French tests and additions, subtractions, reading time, measures assessments, small problems for mathematics. Life skills test is somewhat different: it aims to assess knowledge of the child on different common topics, with a focus on hygiene (water purification, diarrhea, malaria, etc.). For most parts, tests take a multiple-choice questions format. Children who do not answer to some questions but take the test are granted the points corresponding to a random answer (e.g., if they have to choose between three answers, they get one-third of the points). This is equivalent to the standard procedure of giving negative points to the ones who provided a wrong answer. Tests are marked from 0 to 100. For results measured in 2003, we normalize scores in order to interpret and compare coefficients: we divide the result by the standard-error of the score on the population. Descriptive statistics for the scores on different sub-samples are provided in Table 12 in appendix and moments for the normalized test scores are reported in Table 13.

2.2.3 Control variables

All the Pasec children were attending the second grade of primary school in 1995-96. Since school is compulsory from age 7, they all should have been aged 8 or 9 at that time. There was in fact considerable variance. First, a child could have started school before (after) the legal age and thus be younger (resp. older). Second, he may already have repeated the first or second grade and thus be older. Children may perform differently at tests in 1995-96 depending on their age or, to put it differently, assessment of their innate abilities must take into account this heterogeneity. For convenience, we use age in 2003 and given that it is a linear function of age in 95, the estimate captures the age differences when in second grade. Older children in 1995 have in fact lower abilities for a given test level and as such are likely to perform worse at tests taken when teenagers. We thus do expect a negative correlation between age and outcomes in 2003.

Parental education, wealth, family composition and possibly gender are variables that may affect speed of learning. Given the potential endogeneity of some of them, we introduce them only in a second stage, once heterogeneity has been controlled for. Parental education is defined as the level of education (none, uncomplete primary, complete primary, and so on) attained by each of the parent. Wealth is an indicator of permanent income, built on information on durable goods and housing. Household ranking according to this indicator is stable when alternative subsets of variables are used.

2.3 Sample and attrition

As previously mentioned, we wish to use observations for which both tests taken in 2003 and tests taken in 1995 are available. This raises the question of attrition since not all Pasec children were tested again. Possible reasons for not having been reinterviewed are the following:

- child's household has not been found, generally because it has moved;
- the household has been found back but the child has not been tested. This could either be due to the fact that the child does not live anymore in the household or that he was not available for the tests.[10]

Among the 1203 Pasec children living in the 60 areas surveyed in the Ebms,

- 981 (82%) belonged to a reinterviewed household;
- among which 687 (70% of the 981) have been tested while 294 have not (11% registered as living in the household, 19% away).

[10]Given that we have adopted a fairly loose definition for the household and specifically on whether a child belongs to it in order to increase the registered information on children away from the household a large part of the year, we do not address this distinction.

In the end, only 57.4% of the original sample is tested again. This high attrition rate needs to be addressed. The Pasec survey includes a small questionnaire which helps for describing the different attrition mechanisms since it is available for the full sample.[11] We first explain whether a household has been found back and then, for children whose household has been identified, whether they have taken at least one test.

The main results are the following: living in a rural area, with one's mother or grand-parents and in a wealthy household increase the likelihood of finding the household. No determinants of household attrition can be found for urban zones. Scores in second grade, repetition in the first two grades and children's activities have no effect on the household attrition. For the matter of individual attrition in a reinterviewed household, results are the following: again, no determinants of attrition can be found for urban zones. In rural ones, children from wealthier households or those who have not repeated their second grade are less likely to be successfully tracked: this must be due to the fact that households choose to send their succeeding children away to attend lower secondary school when they can afford it. Scores in second grade and children's activities though have no effect on attrition.

2.4 Children's education and labor in Ebms data

This section briefly describes the distribution of the previous variables for the sample of interest. Among Pasec children, aged 13 to 18, half of them have more than 9 years of education at the time of the survey (recall that it is time spent enrolled at school: half of children have only completed grade 6) and the average number of years of education is 8. The average number of worked years is 2.43, while the median is 0. Consistently with the possible combination of both activities, the correlation between the two variables is -0.13 and is statistically significant at the 5% level.

Table 1: Hours of work

		Whole sample	Working Children
Hours at beginning	Average	6.6	14.8
	Median	0	9.9
Average hours	Average	7.2	15.7
	Median	0	9.7

Two measures of work intensity are available: number of hours when the child started working and the average between this and the work intensity at the time of the survey. It turns out that the correlation between the two is quite high

[11]We are reluctant to make an intensive use of these variables since they are reported by young children and are thus very prone to measurement error.

(0.9) and that their means reflect the fact that work intensity tend to increase with age (see Table 1). The average number of hours at the beginning (0 if the child has never worked) is 6.6 hours per week but a working child performs on average 14.8 hours per week. Half of working children were working less than 10 hours per week though when they started their activity. These figures probably hide considerable discrepancies between different times of the year due to the agricultural cycle.

Table 2: Employment status of children when they started their activity

	Urban	Rural	Total
Employed	17.11%	3.04%	6.54%
Independant	5.26%	5.22%	5.23%
Familial worker	39.47%	86.09%	74.51%
Apprentice	38.16%	5.65%	13.73%

On the matter of the type of work, Table 2 reports the employment status of children (again when they started working), by type of area. Family worker is by far the most important category and is more prevalent in rural areas than in urban ones. Employed children and apprentices are on the contrary mainly found in urban zones.

Table 3: Past activities and mean test scores in 2003

Test	No work	Some work	p-value	Less than 8 years of edu	8 years or more	p-value
Life skills	66.82 (315)	60.48 (262)	0.00	58.62 (200)	66.39 (386)	0.00
Oral maths	62.60 (313)	59.83 (252)	0.12	54.97 (194)	64.41 (379)	0.00
Written maths	77.73 (282)	73.72 (190)	0.03	64.12 (110)	79.30 (366)	0.00
Written French	80.40 (280)	76.94 (191)	0.05	69.57 (112)	81.61 (363)	0.00

Note: Sample constituted of Pasec children between 13 and 18 years old and who have taken the test; the number of observations for each mean is in parenthesis below the figure. "No work" means that the child declares no work during his/her childhood. The p-values give the statistical significance when testing the difference between no work and some work on the one hand and between children who have attended school for more than 8 years and those who have not on the other hand.

Let us now turn onto the association between work and low results. We compare means between children who have worked and those who have not. Results are consigned in Table 3.[12] The results are consistent throughout the tests offered to the children. Those who have worked during their childhood have systematically lower results when they are teenagers; the difference is broadly of 5 points out of 100 and is always significant at the 1% level. This conforms to the general view that work is detrimental to human capital accumulation. On the right panel of the Table, we run the same exercise by comparing children who have attended school for 5 years or more and children who failed to do so; as expected, more educated children get better scores and the difference is quite important (around 10 points). The next section explains why this raw correlation may not be very informative on the real impact of child work on his/her cognitive achievement and how we can proceed to exhibit such an effect.

3 Estimation of a human capital production function

The aim of the paper is to assess the impact of work on human capital accumulation. This can be formalized in the following way:

$$T_{ic} = F(W_{ic}, S_{ic}, \delta_c, X_{ic}, u_{ic}) \tag{1}$$

where T_{ic} is a measure of cognitive achievement for child i living in community c, W_{ic} and S_{ic} respectively number of years spent working and enrolled in school, δ_c quality of education and working opportunities in community c, X_{ic} personal characteristics such as gender, age and socioeconomic background of child i and u that may reflect any unobservable determinant of results (preferences, innate abilities) or measurement error. S_{ic} is expected to have a positive sign, while sign for W_{ic} has to be determined empirically as discussed in introduction.

3.1 Issues to tackle in the estimation

The crucial point of our paper is to note that time allocation decisions (between work, schooling and leisure) are likely to be endogenous in equation (1). This could either come from the presence in u_{ic} of specific abilities of the child or from failure to control for all the relevant characteristics of the environment.

As for specific capacities of the child, more able pupils may spend more time in school/devote more energy and thus get better scores; the opposite may happen if schooling time and innate abilities are somewhat substitutes rather than complements in the human capital production function. In the same vein, children with lower endowments for education may also devote more time to work in order to loosen budget constraints and permit siblings' schooling. The outcome

[12] For consistency purposes, we provide the results for the same sample. With the larger sample of children who have taken the tests in 2003, we get very similar results but the mean differences are systematically significant at the 1% level.

of the allocation decision depends on human capital production function and on parents' preferences for equity as discussed by Becker and Tomes (1976) but in any case, we do expect to see a correlation between time allocation decisions and unobservable heterogeneity such as innate abilities.

Moreover, living environment of the children affect both their choices and their cognitive achievements. Indeed, one unobserved factor in the human capital production function is the effort made by the pupil to learn. This effort is surely correlated with what s/he expects to gain from learning. Good schools, high returns to education in the labor market will affect both how much education they demand and how much they strive to benefit from it. On labor's side, places with working opportunities for the children might be places with low returns to education. This is notably the case of agricultural areas, where most of child labor is found and where skilled jobs are rare. If this were to be true, we would expect to find a downward bias when estimating the impact of labor on learning.

For all these reasons, W_{ic} and S_{ic} are likely to be correlated with u_{ic}. The aim of the paper is thus to provide an estimation of the effect of one additional year of work on cognitive achievement of children, net of spurious correlations.

3.2 Empirical strategy for identification

The empirical strategy we propose is a control one. This is motivated by the fact that standard options such as finding appropriate instruments or relying on natural experiments are very difficult to implement in this setting. A convincing instrument could be a ban of child labor or a change in compulsory schooling laws but none of it exists in Senegal nor could be enforced in most developing countries. Moreover, using a control strategy will allow us to try diverse specifications, which would not be the case if only a reduced set of instruments was to be found.

Let us now detail our identification strategy. It is quite obvious from the previous discussion that two sources of endogeneity coexist. The first one lies in unobserved environmental variables and common to all children of the area while the second lies in choices specific to children, either due to their abilities or to their (or their parents') preferences towards education.

3.2.1 Communities fixed effect

Since households are clustered in 60 communities, we are in the position to control for environment differences by introducing communities fixed effects. These fixed effects will capture school supply and its quality, but also working opportunities and social norms prevailing in the area. Fixed effects are particularly useful for the last two aspects since social norms and expectations for returns to education and work are very difficult to observe.

3.2.2 Results to tests taken at the entry of primary school

Controlling for heterogeneity in abilities of the individuals is a much more difficult task. Since it is a recurrent issue in the returns to education literature, there is already a fair amount of contributions to the question. Results to tests taken at the entry of primary school can be considered as proxies for innate abilities. They are measured after only one year of schooling and probably reflect intrinsic capacities of the children as well as the learning environment in which they have lived when infant. Controlling for innate abilities of the children this way should leave in the residual term only measurement error and other "safe" unobserved determinants. However, failure to capture the whole heterogeneity (also called "measurement error" in the indicator) would lead to biased estimates, as shown by Hansen et al. (2004). We thus chose to exploit the repetition of tests within the same year in order to implement the multiple indicator solution offered in Wooldridge (2002). If two indicators of the same unobserved variable (here, innate ability) are available, we can combine both of them to get an estimate of the true innate ability indicator, even if both are measured with error. This is done by instrumenting one of the indicators by the other and allows us to consider that we control for the true value of innate abilities. More specifically, if *abil* stands for the true innate abilities, we assume that:

$$
\begin{aligned}
T^{early\_1} &= \delta_0 + \delta_1 abil + a_1 \\
T^{early\_2} &= \rho_0 + \rho_1 abil + a_2
\end{aligned}
$$

where T^{early_1} and T^{early_2} are two proxies for *abil* and a_1 uncorrelated with explanative variables of the interest regression, $\delta_1 \neq 0$, $Cov(abil, a_1) = 0$ and the same for the second equation. The crucial assumption is that there is no common measurement error in the two indicators or, to put it differently, that the correlation between the two indicators arises only from their common dependence on the true indicator ($Cov(a_1, a_2) = 0$).

To sum up, we wish to use tests taken at the entry of primary school to control for innate abilities of the children when explaining cognitive level in 2003, but also exploit the fact that they are several to take into account the measurement error imbedded in each of them. We actually have two options for implementing this approach. Two tests are available for each session when the child was enrolled in second grade of primary school: French and mathematics. We can thus either use:

- only tests taken in the same discipline but both measured at the beginning and at the end of second grade ($T^{95\text{-}F}$ and $T^{96\text{-}F}$ when trying to describe outcomes in French in 2003 for instance) or
- only tests taken at the entry ($T^{95\text{-}F}$ and $T^{95\text{-}m}$) whatever the outcome.

Both strategies have their pros and cons that we detail later on.

Let us start with some common considerations. All of these early tests are very likely to capture the same information on schooling abilities at a very young age, which can be seen as the latent variable. These tests were designed so as to evaluate the progression of children through school. Cognitive achievement is then measured by similar tests, designed by the same institute, which had received instructions to produce comparable tests except that they were for a higher level. As a consequence, the initial score as well as the final ones are focused on the same specific abilities of the children. It is thus expected that children who are faring well with the tests when they are in second grade are also the ones who do better later on. For this reason, the indicators can reasonably be argued as capturing the relevant information: they may not do justice to all the dimensions of human capital but it would not be the case neither of the final cognitive achievement indicator. Note however that the life skills test differs from the other tests and that this argument may be less accurate in this case.

The first option consists in using only results obtained in French (both at entry and end of second grade) when we explain French outcomes in 2003 and results obtained in maths when we explain math results in 2003. This strategy is legitimate if the relevant latent variable is specific to the discipline and well predicted by two successive tests in the same discipline[13] (for life skills, given that it is not obvious to guess which latent variable is the most likely to affect final outcomes, we try both). The main caveat of this approach is that the way children have progressed along the year may be affected by their learning effort. Another way to put it is that results measured at the end of the second grade are more likely to be spoilt with other factors than true innate ability, as for instance capacity to learn and make progress, and that we do not fully control.

For these reasons, it may be worth focusing on abilities measured at the entry of second grade. In this case, our last point does not apply; unfortunately, this also implies that the tests may not capture information on abilities of the children to progress. This being said, the main caveat is that if we consider that abilities for the children in terms of French and of mathematics are very different and do not proxy for the same latent variable of capacities, then the approach is not valid.

To conclude on the comparison between the methods, it is worth noting that the data could give us some hints about what the relevant latent variables are through correlations between indicators (a strong correlation between two indicators reflect the fact that they tend to describe the same latent variable); unfortunately, Table 10 in appendix shows it is not the case and confirms that these indicators are prone to measurement error and that the multiple-indicator solution needs to be implemented.

Most of the criticisms (in the returns to education literature) of this technique lied in the fact that tests that were used were collected at the end of the education span and thus could have been affected by it; in our case, tests used as controls cannot demonstrate a reverse causality issue since they are collected

[13]We also tried a specification in which both results in math and French were introduced as control variables. It turns out that the full estimation with fixed effects and instrumentation of these scores fail to identify precisely the coefficients due to the low number of observations. This estimation is thus omitted in the paper.

when the child has hardly started its education. This strategy provides estimates of the impact of one additional schooling or working year on cognitive achievement for a given level of endowments. However, the latent variable of ability that we try to measure indirectly via tests is made of different components: true innate ability but also parental preferences for school versus work that may have affected child's cognitive achievement even with an early measure in second grade (Dumas and Lambert, 2007). We thus have to assume that the mix of these ingredients does not change greatly over the school life course of the child and affects in the same way his/her outcomes when teenager.

3.2.3 In practice

The estimation method implied by these considerations is actually quite simple: it consists in running an IV estimation on

$$T_{ic} = K_0 + \alpha W_{ic} + \beta S_{ic} + \gamma X_{ic} + \sum_{c=1}^{60} \delta_c + \eta T_{ic}^{early\_1} + u_{ic}$$

where T_{ic} stands for the various outcome variables (life skills, oral maths, written maths and written French tests) and $T_{ic}^{early\_1}$ is instrumented by $T_{ic}^{early\_2}$.

The previous discussion suggests different options for the choice of T^{early_1} and T^{early_2}, which can be freely exchanged; we sum up these options in Table 4.

Table 4: Assumptions and control tests in the regressions

	Assumptions	Outcome	T^{early_1}	T^{early_2}
Option 1	latent variable may be specific to discipline; ability to progress taken into account	LS, WF LS, OM, WM	T^{French_95} T^{maths_95}	T^{French_96} T^{maths_96}
Option 2	maths and French tests reflect the same ability; ability to progress not taken into account	LS, OM, WM, WF	T^{maths_95}	T^{French_95}

Depending on the implemented strategy, we need for each observation the tests results obtained in 2003 and some scores in mathematics and French obtained at the beginning and at the end of school year 1995-1996. In appendix, the reader will find the number of available observations for each test (Table 11) and descriptive statistics of the outcomes (tests results in 2003) for various subsamples (Table 12).

The lower number of observations for written tests is due to the fact that children needed to be able to write in order to take that test. Given that we control for unobserved abilities, selection bias should not be an issue. Moreover, Table 12 clearly shows that restriction to children having taken the tests at school entry is unlikely to bias the sample since they have fairly similar outcomes.

4 Results

4.1 Impact of time devoted to work and education on cognitive achievement

We discuss in detail the results based on a range of specifications, going from OLS to the full model (communities fixed effects and early scores instrumented). Tables 5 and 6 only report estimates of interest, namely effect of one additional year of work/school respectively. For sake of simplicity, option 1 has been retained for discussing the detail and results for option 2 are only delivered in the last columns for comparison.

Table 5: Estimated impact of child labor

Scores in 2003	1	2	3	4	5	6	7
Life skills	-0.035**	-0.034**	-0.012	-0.011	-0.008	0.001	-0.008
	(0.013)	(0.013)	(0.016)	(0.016)	(0.017)	(0.018)	(0.017)
Oral maths	-0.001	0.000	0.033+	0.034*	0.032+	0.044*	0.035+
	(0.013)	(0.013)	(0.017)	(0.017)	(0.017)	(0.019)	(0.018)
Written maths	0.009	0.011	0.034*	0.032+	0.029	0.033+	0.033+
	(0.014)	(0.014)	(0.017)	(0.017)	(0.018)	(0.019)	(0.018)
Written French	0.011	0.014	0.036+	0.034+	0.026	0.032	0.032+
	(0.015)	(0.015)	(0.019)	(0.019)	(0.017)	(0.021)	(0.019)
Fixed effects	no	no	yes	yes	no	yes	yes
Scores in 95	no	yes	no	yes			
Scores in 95 instrumented by 96					yes	yes	
Math scores in 95 instrumented by French score in 95							yes

Note: Scores are normalized by their standard errors thus estimates are the impact of one additional year of labor on scores in 2003 measured in standard errors. See Table 11 for the number of observations. Control tests for specifications (2) and (4) to (6) are French in 95 for written French and mathematics for all the other ones. **, * and + mean respectively that the coefficient is significantly different from 0 at the 1%, 5% and 10% level.

We first discuss the estimated impact of child labor on cognitive achievement (Table 5). It appears at first glance that coefficients are only negative for life skills, but never statistically significant in the final specification. Estimates of the impact of child labor are positive for mathematics results, be they basic or advanced. We thus have little empirical support for a detrimental impact of child labor on learning, controlling for time spent enrolled in school. Second, if we compare specifications (1) and (3), we find that controlling for village fixed

Table 6: Estimated impact of school attendance

Scores in 2003	1	2	3	4	5	6	7
Life skills	0.124**	0.118**	0.114**	0.106**	0.077**	0.071**	0.101**
	(0.020)	(0.020)	(0.019)	(0.019)	(0.028)	(0.024)	(0.020)
Oral maths	0.122**	0.119**	0.138**	0.131**	0.075**	0.107**	0.122**
	(0.020)	(0.020)	(0.020)	(0.020)	(0.028)	(0.024)	(0.022)
Written maths	0.200**	0.196**	0.219**	0.216**	0.180**	0.207**	0.212**
	(0.025)	(0.025)	(0.024)	(0.024)	(0.033)	(0.028)	(0.025)
Written French	0.180**	0.173**	0.177**	0.171**	0.140**	0.137**	0.162**
	(0.027)	(0.027)	(0.026)	(0.026)	(0.032)	(0.032)	(0.027)
Fixed effects	no	no	yes	yes	no	yes	yes
Scores in 95	no	yes	no	yes			
Scores in 95 instrumented by 96					yes	yes	
Math scores in 95 instrumented by French score in 95							yes

Note: Scores are normalized by their standard errors thus estimates are the impact of one additional year of labor on scores in 2003 measured in standard errors. See Table 11 for the number of observations. Control tests for specifications (2) and (4) to (6) are French in 95 for written French and mathematics for all the other ones. **, * and + mean respectively that the coefficient is significantly different from 0 at the 1%, 5% and 10% level.

effects leads to a higher positive impact of child labor on knowledge, or, to put it differently, that controlling for village fixed effects corrects for a negative bias in the estimates of child labor. This is consistent with the argument that areas with child labor are also the ones where individuals invest less in learning. Third, when comparing specification (1) and (2), we observe that controlling for abilities has no effect on the estimates. Fourth, introducing abilities when we are controlling for fixed effects (switching from estimation (3) to (4)) has also little effect. However, for reasons detailed before, this estimation may still be biased. Instrumentation of scores (from estimation (2) to (5) or (4) to (6)) tend to increase the estimated effect of child labor on tests, showing that in most of the cases not taking into account the measurement error imbedded in tests will bias the results. This is consistent with the fact that less skilled children are more often put to economic participation than the others. This being said, the change in the estimates is rarely significant at the 5% level. The most convincing estimation is thus the one provided in column (6) where we control for communities fixed effects and instrument the score: we find that child labor has no impact on life skills test and a positive impact on maths, larger on basic skills than the advanced ones. The estimated effect on written French is of the same order of magnitude than for the written maths but fails to be significant, even at the 10% level. Specification (7), where the second option is implemented, confirms the results and identifies a positive effect of time devoted to labor on cognitive achievement that is significant at the 10% level for all tests except the one on life skills.

Table 6 indicates that an additional year of schooling leads to a significant increase in cognitive achievement: the estimated impact varies roughly between one-fifteenth and one-fifth of a standard error and is thus higher than the impact of one year spent working. Controlling for scores leads to small variations in the estimated effect for most of the tests. If we use the instrumented scores, we then get lower estimates. The biases generated by individual heterogeneity were thus positive: this seems to show that parents do not compensate for unequal capacities of their offspring. Controlling for village fixed effects does not correct for biases of the same sign depending on the tests. Again, specification (7) is in keeping with that has been found with the first option. The estimated effect of education is much smaller on life skills than on the other topics, while the written and thus advanced tests are more improved by increased education than is oral maths, as could be expected.

Comparison of the estimated impacts between the different tests draws the following picture: knowledge on life skills is likely to be gained out of school but is not improved through work, knowledge on oral maths is gained through school but can be acquired through participation to economic activities (one year spent working allows the children to learn as much as half what they would have learned from school on that topic), while the discrepancy between what can be learnt at school and from work is higher for advanced (and written) tests. Both tests in mathematics being quite comparable, except for their modalities (oral vs. written), it is quite likely that the main difference between the estimates comes from the fact that less educated children are not able to read and write.

4.2 Adding social background variables

If the scores are insufficient to control for heterogeneity in abilities or other factors determining both human capital investments and results, then adding some social background variables may alter the results if these variables are able to pick up some heterogeneity. We now try to introduce such additional controls. It is also interesting *per se* since it will provide us with some estimations of the effect of social background on learning.

We thus run the first specification with the following controls: age (as previously), gender, father's and mother's education, wealth and whether the child has an older brother and an older sister. Results are given in Table 7.

Estimates for the effect of the number of years of schooling are very similar to what we found before without control variables; results change a bit for the effect of labor but not systematically in the same way. The control variables are almost never significant, even at the 10% level, attesting that control for abilities also capture differences in social background. Father's educational level

Table 7: Impact of child labor and schooling with controls, option 2

	Life skills	Oral maths	Written maths	Written French
Years of schooling	0.102**	0.119**	0.202**	0.154**
	(0.021)	(0.022)	(0.025)	(0.027)
Years of labor	0.000	0.028	0.031+	0.046*
	(0.017)	(0.019)	(0.018)	(0.020)
Boy	-0.109	0.183+	0.090	-0.128
	(0.089)	(0.094)	(0.090)	(0.097)
Father's education	-0.018	0.053+	0.050+	0.043
	(0.026)	(0.028)	(0.026)	(0.029)
Mother's education	0.014	-0.005	-0.029	0.000
	(0.037)	(0.039)	(0.037)	(0.040)
Wealth	0.129+	0.105	0.149*	0.119
	(0.069)	(0.074)	(0.073)	(0.079)
Household size	0.006	0.005	0.001	0.002
	(0.007)	(0.008)	(0.008)	(0.008)
No older brother	0.007	0.116	0.053	0.109
	(0.079)	(0.085)	(0.084)	(0.091)
No older sister	-0.230**	-0.145	-0.104	0.008
	(0.083)	(0.089)	(0.088)	(0.096)
Age	-0.067+	-0.036	-0.147**	-0.085+
	(0.041)	(0.044)	(0.045)	(0.048)
Score	0.022*	0.016+	0.019*	0.022*
	(0.009)	(0.009)	(0.010)	(0.010)
# Obs.	526	514	431	430
R^2	0.41	0.34	0.45	0.37

Note: Scores are normalized by their standard errors thus estimates are the impact of one additional year of labor on scores in 2003 measured in standard errors. Scores are measured in 1995 and instrumented by their value in 1996. **, * and + mean respectively that the coefficient is significantly different from 0 at the 1%, 5% and 10% level.

and wealth only improve learning in maths, leaving acquisition in the other disciplines unchanged. The Table also shows that, as expected, the point estimate for age is negative.

4.3 Summary

The two strategies find very similar results. Controlling for the time devoted to education, having worked when child does not hamper learning but the benefit it provides is quite small. This small benefit could arise from increased resources due to the participation of the child to economic activities or from learning due to a higher parents' frequentation. By comparison, the effect of work is at best 1/3 the one for schooling and it is clear that if time allocated to work is taken on schooling time (meaning fewer years of education), work implies a lower cognitive achievement at the end of the day.

4.4 Including intensity at work and type of occupation

So far, we have implicitly assumed that all years of work were comparable; it is indeed not the case if some children perform economic activities only in evening or in the week-end while others do it more extensively. To put it differently, it is quite likely that the small benefit of work we have identified could vanish if work is performed during long spells. As already said, we consider the time spent working when the child started as a proxy for the intensity of work during childhood. The intensity measure is given in number of hours per week in order to be interpretable. The control strategy is particularly useful at this stage since we just have to introduce this new variable in one of the agreed specifications. This being said, this measure is likely to be spoilt with measurement errors and its estimate will probably be biased towards zero. For the sake of simplicity, we just provide the results when using the second strategy (as in column 7 of Table 5), given that the previous results were almost the same and that this estimation uses the largest number of observations.

Table 8: Impact of work's intensity on results

Scores in 2003	Life skills	Oral maths	Written maths	Written French
Years of schooling	0.098**	0.119**	0.186**	0.147**
	(0.021)	(0.023)	(0.026)	(0.028)
Years of labor	-0.012	0.034+	0.040*	0.041*
	(0.017)	(0.019)	(0.019)	(0.020)
Intensity	0.002	-0.001	-0.011*	-0.008+
	(0.004)	(0.004)	(0.004)	(0.005)
# obs.	530	518	434	433
R^2	0.38	0.31	0.43	0.37

Note: Estimations include communities fixed effects and control for maths score in 95, which is instrumented by French score in 95.

Table 8 shows that, despite the imperfection of the measure, we do find a negative impact of long hours spent working for advanced tests; this is not the case for the easier test of oral mathematics. Namely, while participating to economic activities increases by 0.04 standard error the results when teenagers, each hour per week spent working decreases it by roughly 0.01 standard error. The positive impact of working is then outset for advanced tests when the child worked more than 4 hours per week on average.

Table 9: Impact of occupation in childhood on results

Scores in 2003	Life skills	Oral maths	Written maths	Written French
Years of schooling	0.094**	0.124**	0.197**	0.145**
	(0.022)	(0.023)	(0.026)	(0.028)
Years of labor	0.020	0.020	0.027	0.033
	(0.026)	(0.028)	(0.029)	(0.031)
Employee	-0.344	-0.106	-0.472+	-0.577*
	(0.236)	(0.248)	(0.272)	(0.287)
Self-employed	0.089	-0.108	0.070	0.392
	(0.277)	(0.290)	(0.347)	(0.367)
Familial worker	-0.254	0.123	0.037	-0.035
	(0.172)	(0.181)	(0.186)	(0.197)
Apprentice	-0.098	0.156	-0.343	-0.361
	(0.199)	(0.214)	(0.250)	(0.278)
No occupation	Ref	Ref	Ref	Ref
# obs.	542	530	443	442
R^2	0.38	0.31	0.42	0.36

Note: Estimations include communities fixed effects and control for maths score in 95, which is instrumented by French score in 95.

Table 9 reports the results when we take into consideration the occupation status of the child when he used to work. The estimates for the number of years of labor are not significant anymore and this is due to a slight decrease of the coefficient when adding these new variables but also to the induced loss of precision. None of the occupational statuses show up to be significant except for the fact of having been an employee, which is highly detrimental to the learning process: it decreases by half a standard error results to tests. Table 2 showed that less than 7% of children are employed outside of their family and this is much more prevalent in urban areas than in the rural ones. These children learn less than the others for a given time spent in school.

5 Conclusion

This paper offers estimate of the impact of working during childhood on cognitive achievement. For a given amount of schooling and once heterogeneity in environment and abilities has been control for, children who have been working do not perform worse but even slightly better than the others. This could be either due to an increase in resources that allow the child to learn better if the resources are allocated to schooling inputs, or to knowledge gained from working with the parents. The positive impact of work vanishes though if the child worked more than 4 hours a week on average when he started his activity or even strongly depletes accumulation of human capital if he was employed outside of the household.

6 Appendix

Table 10: Correlation between scores

	math in 95	math in 96	French in 95	French in 96
math in 95	1			
math in 96	0.40	1		
French in 95	0.39	0.39	1	
French in 96	0.32	0.61	0.45	1

Note: Correlations computed on the sample of children aged 13 to 18 and who have taken the tests. All the correlations are statistically significant at the 1% level.

Table 11: Number of observations

Test	Life skills	Oral maths	Written maths	Written French
Score in 2003 + Scores in 95	542	530	443	442
Score in 2003 + Scores in French (95-96)	492	-	-	405
Score in 2003 + Scores in Maths (95-96)	520	506	425	-

Table 12: Descriptive statistics for the scores using different criteria

Criteria	# obs.	Mean	Std Dev	Min	Max
Life skills score					
All	2354	61.89	18.62	0	100
$13 \leq$ age ≤ 18	2321	61.97	18.56	0	100
+ has been to school	2040	63.44	18.21	0	100
+ both tests avail. in 95	578	63.98	17.19	15	100
+ both tests avail. in 96	517	64.36	16.95	15	100
Oral maths score					
All	2200	59.18	22.20	0	100
$13 \leq$ age ≤ 18	2177	59.19	22.19	0	100
+ has been to school	1923	60.84	21.68	0	100
+ both tests avail. in 95	565	61.51	20.82	0	100
+ both tests avail. in 96	506	61.51	20.59	0	100
Written maths score					
All	1641	76.32	20.42	0	100
$13 \leq$ age ≤ 18	1624	76.33	20.45	0	100
+ has been to school	1595	76.74	20.17	0	100
+ both tests avail. in 95	470	76.01	20.11	0	100
+ both tests avail. in 96	426	76.20	19.94	0	100
Written French score					
All	1703	78.52	19.89	0	100
$13 \leq$ age ≤ 18	1680	78.63	19.82	0	100
+ has been to school	1647	79.01	19.53	0	100
+ both tests avail. in 95	469	79.04	18.95	0	100
+ both tests avail. in 96	424	79.83	18.25	0	100

Table 13: Descriptive statistics for normalized tests scores

Test	# obs	Mean	Std Dev	Min	Max
Life skills	578	3.72	1	.87	5.81
Oral maths	565	2.95	1	0	4.80
Written maths	470	3.78	1	0	4.97
Written French	469	4.17	1	0	5.27

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