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Schooling and basic aspects of intelligence: A natural quasi-experiment in Malawi☆

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ABSTRACT

The relationship between educational age and chronological age and measures of information processing and intelligence was studied in a group of children of 7 to 14 years of age ($N=268$) in a rural area in the Ntcheu district (Malawi). There was a relatively weak relationship between chronological and educational age in this area, and the impact of factors often threatening the validity of the comparisons of schooled and unschooled children, such as socioeconomic status and sex, was small. Reaction time measures of different levels of cognitive complexity and Raven test scores showed significant relations with chronological age, educational age, and their interaction. The strength of the relations was similar for these three variables. These findings are in line with a view that in middle childhood and early adolescence basic information processing and intelligence are not enhanced by schooling and develop along similar lines in schooled and unschooled children. Our results suggest that the schooling is more efficacious by addressing skill transfer and the development of metacognitive knowledge than by addressing basic information processing.

1. Introduction

Schooled individuals consistently outperform unschooled individuals in basic intelligence testing (Ceci, 1990, 1991; Christian, Bachman, & Morrison, 2001; Van de Vijver, 1997). The implication seems obvious: Unschooled persons are less intelligent than schooled individuals. Decontexualization of thinking through schooling is generally accepted as a main origin of the difference in measured intelligence between schooled and unschooled persons (Vygotsky, 1978; cf. Kozulin, 2003). The necessity of extracting information from written sources introduces a way of thinking that fosters the acquisition of abstract principles: "literacy will give a new set of tools to the child" (Egan & Gajdamaschko, 2003, p. 87). Also, school instruction provides children with cognitive challenges not often found in the everyday context and fosters the transfer of their skills (Bruner, 1966). Schools present a mix of important effects.

A critical concern in understanding the effects of schooling is whether the effect is confined to measured intelligence or whether it extends to information processing (Brouwers, Mishra, & Van de Vijver, 2006; Cliffordson & Gustafsson, 2008). In an influential review, Ceci (1990) reports that school effects go beyond measured intelligence and extend to memory, perceptual abilities and concept formation. For example, he found that schooled children were more able to use depth perception cues than unschooled children in two-dimensional pictures, such as the Children's Embedded Figures Test.

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The strong effect of schooling has been challenged from various perspectives (e.g., Poortinga & Van de Vijver, 2004). First, there is impressive evidence of highly skilled abstract reasoning by unschooled or poorly schooled people, such as the navigation skills among the Trukese who make voyages over hundreds of miles of open ocean without a compass, sextant, or star tables (Gladwin, 1964) and reasoning during lawsuits about land assignment among the Tobrianders (Hutchins, 1980). Such studies indicate that schooling is not necessary for the emergence of complex problem solving. Second, a closer look at the design of studies on the cognitive effects of schooling shows a set of common shortcomings of these studies (Brouwers et al., 2006; Rogoff, 1981). In countries with a strict observance of age rules about compulsory schooling (i.e., all medium- and high-affluence countries), chronological and educational age are strongly related, making it virtually impossible to disentangle their influence. Furthermore, in countries in which relatively few children attend schools or drop out early, there tends to be a relation between school attendance and parental socioeconomic status, with lower-status children showing a higher dropout rate (Rogoff, 1981). Third, there is an assessment issue to consider in comparing schooled and unschooled children. School tasks and psychological tests often use similar formats and contents, and the resulting bias against unschooled children can easily lead to an overestimation of the performance differences of unschooled and schooled children (Rogoff, 1981). Thus, the question of whether schooling influences basic cognitive functions lacks a solid empirical basis.

From a methodological perspective, there are three kinds of approaches that can address the design problems of studies comparing schooled and unschooled individuals. The first is to use *natural experiments* (Scheier, 1959), which amounts to doing research in locations unaffected by the usual confounds that complicate studies of schooling effects (such as the close relationship between chronological and educational age or the positive relationship between socioeconomic status and school attendance). A good example is Scribner and Cole's (1981) study of "unschooled literacy" among the Vai in Liberia. The Vai have developed an indigenous script that is transmitted in informal settings, enabling a study design in which the effects of literacy and the effects of schooling can be separated. These authors did not find support for the view that literacy leads to a major shift in reasoning. A major disadvantage of natural experiments is their limited availability. Moreover, a methodological problem of natural experiments is that unintended factors such as interest or motivation may influence the results (e.g., participants may show very low or high motivation to participate or school quality may be very low).

Whereas natural experiments can only be carried out in non-Western settings, a second approach, which could be called *common-curriculum designs*, attempts to determine how Western school settings (or any other common school setting) can be used to examine the influence of schooling on cognitive development, taking into account the problems of the typical designs of such studies. Three broad kinds of studies have been identified (Ceci, 1990, 1991): birth-date cutoff studies, summer holiday studies (Allinder, Fuchs, Fuchs, & Hamlett, 1992; Crone & Whitehurst, 1999) and between-grades regression discontinuity designs (Cahan & Cohen, 1989; Cliffordson & Gustafsson, 2008). Birth-date cutoff studies compare children born just before the birth-date cutoff of school enrollment with children born just after, and have found that after one year in school, the children born just before the birth-date cutoff perform significantly higher on IQ tests than their peers born just after this date. Summer holiday studies have found that sustained absence of schooling is linked to a significant decline in performance on IQ tests. Between-grades regression discontinuity designs found that the regression slope of chronological age on performance is discontinuous from one grade to the next. Compared to natural experiments, it is difficult to generalize the findings from birth-date cutoff, summer holiday, and between-grades regression discontinuity studies to a school career of several years because of the limited time span (and hence the grade-specific schooling effects) covered by these studies.

A third kind of study, *natural quasi-experiments*, attempts to combine the opportunity to draw strong inferences from natural experiments with the wide availability of natural contexts to carry out common-curriculum studies (Brouwers et al., 2006). Natural settings are identified in which typical problems of common curriculum designs, such as the very high correlation of educational and chronological age, are less salient and additional measures can be taken to "unconfound" the relevant variables, such as an adapted test design or statistical post hoc control. Many countries of the majority world provide settings for such studies. As studies in these countries cannot rule out the impact of confounds, it is important to measure potentially confounding variables, such as pupils' age in various grades as well as the sex ratio in various grades, often needed as girls tend to drop out earlier. Measures of these variables as well as a careful analysis of the cultural context of the study make it possible to determine to what extent the study location is closer to the natural experiment (ideally, without any confound) or to the common-curriculum design with its potentially strong confounding. If the confounding is weak or moderate, appropriate statistical analysis can help to control for it, thereby mimicking natural experiments as much as possible.

In the present paper data on the effects of chronological and educational age and schooling on information processing speed are reported. The study was carried out in Malawi, which ranks among the poorest nations. The country has a low GDP (Power Purchasing Parity: US$ 670; Index Mundi, 2004), a high annual population growth rate, and a high birth rate (2003: 2.21%). The illiteracy rate is 58%, the classroom/pupil ratio is 1:120 and the textbook/pupil ratio is 1:4 (Improving Education Quality Project, n.d.). Preschool playgroups are available for fewer than 1% of the children (World Education Forum, n.d.). The quality of the primary schools, which are typically understaffed and underfunded, cannot be compared with that found in Western countries. Fewer topics are covered in the curriculum in Malawi than in western countries. As a consequence, Malawian pupils were expected to have less scholastic knowledge than their Western counterparts after completing primary education.

Enrollment and school attendance are also very different from the situation in Western countries. Often children do not enroll at all and thus remain unschooled. Enrolled children do not necessarily attend school full time. Due to family responsibilities children may occasionally miss a day at school. Dropout and repetition rates are high. According to government estimates, repetition rates (all standards) are 15%, dropout (all standards) 17%, and the survival rate to Grade Eight 20% (Ministry of Education, Science & Technology, n.d.). Furthermore, dropout rates are 10% higher for girls than for boys, 25% higher for children of the lowest

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socioeconomic status than for children of the highest socioeconomic status, and 20% higher for children in rural schools than for children in urban schools (Worldbank Group, n.d.). It is not common for children to attend school, drop out for a year, and then start attending again.

As dropout rates are nonrandom with regard to child sex, socioeconomic status, and school location (urban vs. rural), Malawi is unlikely to provide a context for an ideal natural experiment in which these factors are completely independent. However, as described in the next section, the correlation of calendar age and educational age is much lower than typically found in Western countries (Van de Vijver, 1997).

The present study is a natural quasi-experiment that attempts to examine the influence of schooling on basic intellectual functioning among children of primary school age in Malawi. The focus of the study is less on the usual distinction between literates and illiterates (as dichotomous variables) and more on the role of educational and chronological age (as continuous variables). We were specifically interested in the role of both ages in basic features of information processing. In order to overcome the assessment problems mentioned previously, we used "culture-reduced" tests (Jensen, 1980), which can be expected to show a limited susceptibility to differences in educational background.

2. Method

2.1. Participants

Participants were recruited from five villages in the Ntcheu district, a rural area in Malawi, approximately 200 km south of the capital Lilongwe and bordering on Mozambique. Ntcheu District has a population of 370,757 (Government of Malawi, n.d.). About 76.1% of the population lives below the poverty line (defined as living below $2 a day; Rural Poverty Portal, 2007). The main source of income is subsistence farming. Children in this area spend much time carrying out various family survival tasks. In particular unschooled children and dropouts usually help their parents in farming and the household.

Children were recruited with the help of village chiefs. The chiefs had to give their approval to have the study in their villages and they pointed out the children who were eligible for participation in the study sample. There was no reason to suspect that the village chiefs applied a specific mechanism for selecting children. Chiefs selected schoolchildren as well as unschooled children. Additional schooled and unschooled children were recruited through a snowball procedure, in which children already included in the study or their parents gave us access to other children eligible for inclusion. The sample comprised children who had never attended school ($n = 20$; 7.5%), school-going children ($n = 209$; 78.0%), and dropouts ($n = 39$; 14.6%). Table 1 displays the numbers of children at each chronological and educational age in each school year. The dispersion of chronological age was large in each grade, which might be due to a combination of three factors. First, the age of enrollment in primary school is often older than prescribed by national law; second, the repetition rate is high; and third, each dropout provided their age at the time of testing, but their highest grade at the time of dropping out of school, making them old relative to their educational age.

As Table 1 shows, the 39 dropouts were aged nine, ten or eleven years at the time of testing and had attended school for one, two or three years before they dropped out. This dropout does not seem to be subject to sex bias in our this sample, which is in line with the non-significant correlations between sex and the three school-related variables, presented in Table 2. However, it should be noted that although there was not a strong sex bias in dropout, there was an age bias: children of the ages of 12, 13, and 14 years who dropped out of school are not present in our sample. This bias may point to their unavailability at the time testing because to responsibilities to their families, such as working on the field, that kept them occupied elsewhere.

Correlations between age and educational status are also given in Table 2. The correlation between chronological and educational age was significant ($r = .78$, $p < .01$; 95% confidence interval from .72 to .82). The value of .78 is significantly below the values found in Western research, which are often in the high .90's (Van de Vijver, 1997). Furthermore, chronological age was

Table 1
Frequencies of combinations of chronological and educational age for girls and boys ($N = 268$)

	Age								
Grade	7	8	9	10	11	12	13	14	Total
0 (unschooled)			2/3	2/6	2/5				6/14
1	2/3	1/2	19/11	6/5	4/3				32/24
			(8/6)	(3/4)	(4/2)				(15/12)
2		5/3	12/13	2/9	7/7				26/32
			(1/1)	(0/1)	(5/1)				(6/3)
3		3/0	0/4	11/7	2/7				16/18
				(0/1)	(0/2)				(0/3)
4				2/5	6/6	1/0			9/11
5				1/2	3/5	2/1			6/8
6					4/3	5/5	2/7	4/3	15/18
7						4/4	8/3	7/5	19/12
8								0/2	0/2
Total	2/3	9/5	33/31	24/34	28/36	12/10	10/10	11/10	139/129
			(9/7)	(3/6)	(9/5)				(21/18)

Note. Cells refer to number of girls/boys, respectively. The numbers in brackets are the numbers of dropouts (girls/boys) for the respective cells.

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Table 2
Correlations between age and school-related variables ($N=268$)

	Educational age	School going [a]	Schooled [a]	Sex [b]
Chronological age	.78**	.17**	.08	.00
Educational age [a]	1.00	.53**	.40**	−.03
School going [a]		1.00	.53**	−.03
Schooled [a]			1.00	−.10

**$p<.01$.
[a] 0=no, 1=yes.
[b] 0=female, 1=male.

unrelated to schooling (unschooled vs. schooled; $r=.08$, *ns*) and showed a significant though weak correlation of .17 ($p<.01$) with current school status (not attending vs. attending school).

2.2. Instruments

2.2.1. Computer-assisted cognitive tasks

A computer-assisted cognitive ability test battery was administered to participants (cf. Helms-Lorenz, Van de Vijver, & Poortinga, 2003; Van de Vijver & Willemse, 1991). The battery was developed to assess simple cognitive processes and to avoid a strong interference of cultural and linguistic knowledge. The battery consisted of three subtests with a common format. For each, children were shown a display with seven white, empty frames on a white background. In all tasks the response was to identify the "odd one out". Each subtest included two series of ten items with a short break in between.

In the first subtest (Computer-Assisted Reaction Time Task 1, CRT1) one of the frames turned black after several seconds. Fig. 1a depicts an example of a test screen for this task as viewed by the pupils. Children had to press the corresponding button on the response board as fast as possible.

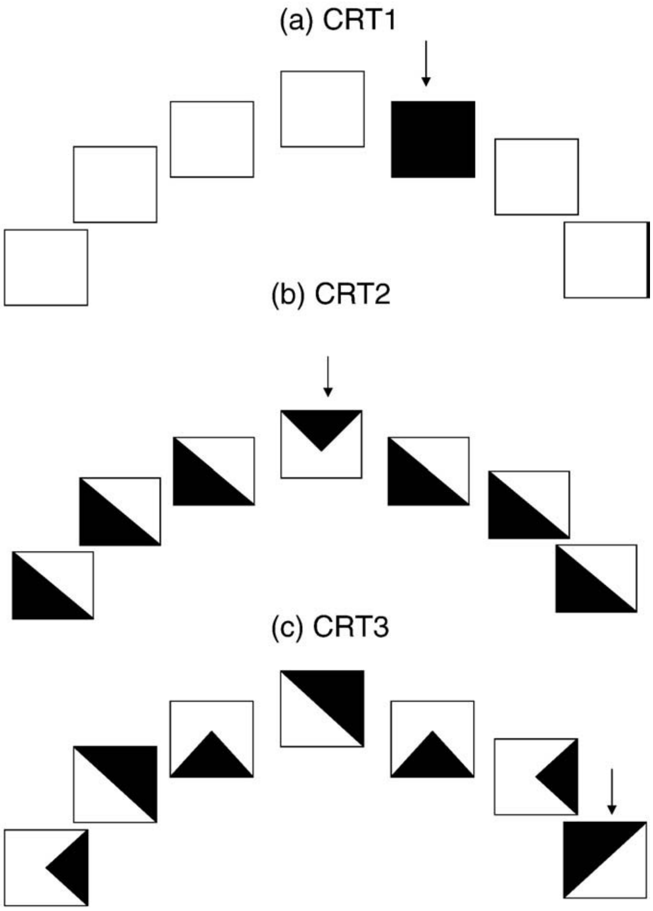

Fig. 1. Examples of test screens used in the elementary cognitive tasks.

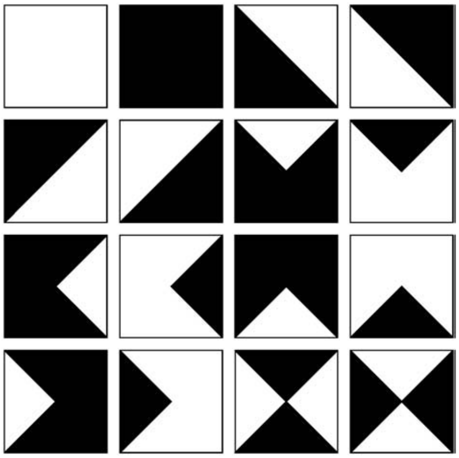

Fig. 2. The 16 geometric figures used in the study.

A set of 16 geometric figures used in the other two subtests tasks is shown Fig. 2. In the second subtest (CRT2) six of the white frames were filled with identical figures and the seventh frame with a different figure. The different figure remained the same across all items in the subtest (see Fig. 1b). Children had to press the button on the response board that corresponded to the different figure (odd one out).

In the third subtest (CRT3) six of the white frames were filled with three pairs of identical figures and the seventh one with another (see Fig. 1c). The child had to press the button on the response board that corresponded to the odd one out.

In all tasks the position of the target figures varied randomly across items. In an analysis that is not further documented here, the proportion of errors was found to be less than 5%. The internal consistencies of the three tasks (based on RTs) were .97, .93 and .89, respectively.

2.2.2. Ravens Colored Progressive Matrices

The Ravens Colored Progressive Matrices was also administered to all children. The test is a measure of fluid reasoning that has been frequently used in cross-cultural studies. The test has shown good psychometric properties in resource-limited settings (see, e.g., Irvine, 1969, and Brouwers, Van de Vijver, & Van Hemert, in press, for overviews). The Raven CPM test consists of three series of twelve items (series A, Ab, and B). Because a pilot study showed that completing the entire test was too exhausting for the children, only series Ab was administered (12 items). The children were tested individually. Administration was on paper and untimed. The score was the number of correctly solved items (Cronbach's alpha = .71).

2.3. Procedure

Testing was done in roofed buildings, where it was not dark. Village chiefs had specifically appointed these settings for the benefit of our study. Children were seated at a table. When a child was too small, testing was done with the child standing at the table. A single experimenter, who had been trained to explain our computerized tasks to children, tested all children. The language used for testing was Chichewa, the main language of Malawi. Each session lasted about 1 h per child. With the exception of CRT3, which turned out to be very difficult for the seven-year-olds, all participants received all tasks.

Each session started with training on the task procedures. During training another child was also present, to prepare that child for his or her own session. Training started with the monitor only. It was explained to children what they should attend to and which figures were important. Explanations were repeated until children seemed to have fully understood the meaning. The

Table 3
Mean reaction times (ms) and Raven scores per chronological and educational age (N = 268)

Chronological age	CRT1	CRT2	CRT3	Raven	Educational age	CRT1	CRT2	CRT3	Raven
7	1006	1926		7.00	0	1094	2070	8843	5.36
8	879	2056	8695	6.17	1	918	2020	9039	6.10
9	1099	2064	8722	4.84	2	999	2104	8474	4.82
10	994	1997	7969	4.99	3	883	1923	7635	5.48
11	799	1875	7472	5.86	4	917	1969	7523	5.23
12	886	1875	6798	4.25	5	821	1768	5249	4.56
13	823	1949	7231	7.44	6	806	1834	7169	6.43
14	748	1634	6485	6.73	7	873	1866	7447	5.18
					8	572	1199	5746	9.50

CRT = Computer-Assisted Reaction Time Task.

Table 4
Correlations and summary of regression analysis with chronological age, educational age, and chronological × educational age interaction as predictors of cognitive test performance (Correlations (*r*), regression coefficient (*b*), and adjusted squared multiple correlations (R^2)

	Chronological age [a]			Educational age [a]			Chron. × educ. age interaction [a, b]		
Task	*r*	*b*	R^2	*r*	*B*	R^2	*r*	*b*	R^2
CRT1	−.20 (−.22)	−48 (−51)	.05 (.06)	−.21 (−.26)	−33 (−40)	.04 (.07)	−.20 (−.25)	−2.39 (−2.85)	.04 (.06)
CRT2	−.17 (−.19)	−51 (−55)	.03 (.04)	−.22 (−.24)	−41 (−45)	.05 (.06)	−.21 (−.23)	−2.96 (−3.18)	.04 (.05)
CRT3	−.34 (−.34)	−399 (−368)	.12 (.12)	−.39 (−.39)	−318 (−305)	.16 (.16)	−.38 (−.38)	−22.88 (−21.61)	.15 (.15)
Raven	.13 (.11)*	.27 (.24)	.04 (.03)	.13 (.13)*	.16 (.17)	.03 (.03)	.14 (.13) [b]	.01 (.01)	.03 (.03)

CRT = Computer-Assisted Reaction Time Task.
*$p<.05$. All other cells are significant at $p<.01$.
[a] The values in brackets are the values obtained by excluding all dropouts from the analysis.
[b] Defined as the product of both ages.

response box was explained next. Children were told which buttons to press and when. They were told to respond fast, and without making any errors. These instructions were followed by four practice items. All practice items were repeated if the child gave one or more incorrect responses. The actual testing started when all practice items had been solved correctly. When children made more than three errors across ten trials they were asked to repeat the task. This sometimes happened with the youngest children. Incorrect responses were treated as missing values in the data. Sessions ended with the paper- and-pencil version of the Ab scale from the Raven CPM, which took about 10 min.

3. Results

The mean reaction times and Raven scores are presented in Table 3. As might be expected, reaction times tended to decrease with both chronological and educational age, while the Raven scores tended to increase with both ages. A central question to the study is a comparison of the relative importance of chronological and educational age to the changes in scores. At first glance it might seem appropriate to apply a set of regression analyses using each kind of age and their interaction (the latter to account for multiplicative effects) as predictors and the reaction times and Raven test scores as dependent variables. However, the high correlation of .78 between chronological and educational age (Table 2) create problems of multicollinearity in a regression analysis. Instead, we adopted a more indirect approach by carrying out separate analyses for each predictor and comparing the outcomes.

As can be seen in Table 4, chronological age was a significant predictor of all tasks; squared multiple correlations were modest for the simplest reaction time tasks, and larger for the most complex reaction time measure. An increase of one calendar year of age led to an average reaction time decrease of 48, 51, and 399 ms on the three reaction time tasks respectively, and an increase of .27 on the Raven test. Educational age was also a significant predictor for all tasks. The squared multiple correlations were also modest for the simpler reaction time tasks and higher for the most complex reaction time measure. An increase of one school year led to an average reaction time decrease of 33, 41, and 318 ms, respectively and an increase of .16 on the Raven test. The interaction of chronological and educational age (calculated as their product term) revealed squared multiple correlations that were similar to the values found for each separate age. The exclusion of dropouts from the analysis did not change the effects of chronological, educational age or their interaction (see Table 4). It seems fair to conclude that educational and chronological age produced similar effects and did not show any interactive effect.

Insight into the relative importance of the two kinds of age and their interaction can also be derived from their correlations with the cognitive measures. As can be seen in Table 4, the correlations were remarkably similar. This similarity in effect is further supported by results after the elimination of chronological age from educational age and vice versa. Table 5 shows that the effects of each age disappear after one has been corrected for the other. The significances of the differences in the dependent correlation coefficients of Table 4 were tested by using a procedure described by Dunn and Clark (1969; cf. Hittner, May, & Silver, 2003). No difference reached significance.

Another way of estimating the relative influence of chronological and educational age is to examine the score differences in the groups that are relatively young or old for their grade (which is the same as examining the differences in the groups that have comparatively little or much schooling given their age). We computed a single age for grade score from the two separate ages (with

Table 5
Correlations between cognitive test performance and chronological age and educational age ($N=268$)

Task	Chronological age [a]	Educational age [b]
CRT1	−.06	−.06
CRT2	−.00	−.08
CRT3	−.09**	−.12**
Raven	−.00	.01

CRT = Computer-Assisted Reaction Time Task.
**$p<.01$.
[a] Educational age was eliminated by standardizing all scores per grade.
[b] Chronological age was eliminated by standardizing all scores per age.

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values −1 and 0 for young and typical age for grade and 1, 2, 3 or 4 for old age for grade). This score yielded a correlation of −.20 with Raven performance ($p<.01$). The negative sign signifies a reduction in the effect of age the longer children are longer enrolled in school. It can be concluded that chronological and educational age show similar relations with the cognitive test scores across the age range of our study.

4. Discussion

The study set out to examine the influence of education and chronological age on basic measures of information processing (simple and complex reaction time measures and Raven's Colored Matrices). These tests were administered to children of 7 to 14 years of age in a rural area in the Ntcheu district in Malawi. This research site was chosen because the correlation between chronological and educational age is weaker here than what would be expected in a Western setting. It was found that both age indicators showed the same relations with cognitive tests and that their interaction did not show a higher correlation. This means that that within this age range and the nature of schooling found here, schooling does not have a stronger impact on basic features of information processing and intelligence than the everyday environment.

Past studies that have compared schooled and unschooled individuals have consistently reported stronger effects of schooling than reported in the present study. How can these seemingly conflicting results be reconciled? First of all, it could well be that the culture-reduced nature of the test materials used in the present study decreased the size of the performance differences of schooled and unschooled children. Studies that have used more specific test materials closer to school experience, such as math ability tests, have demonstrated the specificity of schooling effects on cognition in comparison to skills learned in other everyday settings, such as tailoring (Lave, 1997) and trade (Posner, 1982). According to this line of reasoning, performance differences of schooled and unschooled individuals are mainly a function of the "school saturation" of the tasks used. Second, the differences in years of schooling between schooled and unschooled participants in most studies reported in the literature were much larger than in the present study. It is not clear to what extent our data can be generalized to much longer periods of schooling or to cultural contexts in which the quality of the schooling is higher than found in the current study. Yet, the variation in each kind of age studied was appreciable (the range for each age was eight years) and there is no reason to expect that the observed similarity in the effects would disappear by extending the age range. The literature on skill learning provides examples of increased stability through various mathematical functions. Most of those that have shown a reasonable fit to the data involve some kind of diminishing return: Score increases tend to be strongest in the beginning of the learning process, after which they gradually decrease. A longitudinal study design would be better suited to identify such a learning curves than the cross-sectional design used in the current study; yet, the current study suggests that longer periods of schooling of a preferably higher quality would be required to produce major differences (if any) between schooled and unschooled children in basic cognitive functioning.

The results of our study are complementary rather than contradictory to other studies showing strong school effects. Our study shows that basic information processing performance is not differentially affected by educational age compared to chronological age; studies showing effects of schooling show them for more crystallized aspects of intelligence.

The present results are difficult to combine with a view that either educational or chronological age is the main motor of information processing development. Our data are in line with the view, expressed by Piaget (1966), that cognitive stimulation from the regular everyday environment is sufficient for the development of basic cognitive skills, and by Vygotsky (1978), that the tools of intellectual cognitive adaptation are available everywhere. Cole, Gay, Click, and Sharp (1971) expressed a similar view when they wrote that "cultural differences in cognition reside more in the situations to which particular cognitive processes are applied than in the existence of a process in one cultural group and its absence in another" (p. 233).

In a similar light, we see our data as complementary to Scribner and Cole (1981), who found that schooled literacy had a broader influence on cognitive test scores than unschooled literacy. In our view, schooling influences test-related factors. Although basic information processing skills are typically not acquired in school, schooling does affect various performance-related aspects (Cole, 1996; Rogoff, 1981; Serpell, 1993; Serpell & Haynes, 2004) that may affect the measurement of basic cognitive skills, which are performance based and may involve school-related knowledge or skills. In addition there are schooling differences in metacognitive knowledge. Skill transfer and the training of metacognitive knowledge are part of regular school curricula, but are much less trained in everyday life.

The discussion on the influence of schooling on cognitive functioning has focused largely on the issue of whether schooling is necessary or sufficient for developing thinking skills. The findings of the present study should not be taken to indicate that cognitive training in school is ineffective, as it does not create new cognitive structures. Even if schooling does not play a formative role in information processing, schooling can still be important or even essential in training the execution of available processes. Schooling has been associated with the development of numerous abilities and skills, such as the enhancement of verbal-linguistic skills and understanding decontextualized thought (Christian et al., 2001; Rogoff, 1981). The finding that schooling is not necessary for the emergence of such abilities and skills is entirely compatible with a view that schooling can play an important role in fostering and furthering them.

We do not know of studies that examine whether or how these abilities and skills are taught in Malawian schools, or even whether these are taught. Yet, like many governments of developing countries, the Malawian Ministry of Education emphasizes that these skills should be trained within the many application domains that are relevant for pupils after leaving school to participate in society: "The primary and secondary school curriculum of the future should strive to impact essential skills and knowledge on a broad range of issues including new basic skills; critical thinking and analytical skills, civic and democratic values, computer skills, entrepreneurial skills, life skills and environmental education" (Ministry of Education, n.d.).

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The generalization of the findings reported here needs to be tested. A disadvantage of natural experiments and quasi-experiments is that variations in relevant factors, such as the relation between educational and chronological age or the quality of the schooling contributing to educational age cannot be varied. The schools in our sample were characterized by massive class sizes, scarcity of learning materials, and poor teacher education. It remains to be determined whether a larger effect of schooling would have been found if school quality had been higher.

Understanding the multiple and complex effects of schooling is crucial for educational policy. Many studies in the past have focused on the dichotomy between schooled and unschooled individuals and worked from an implicit agenda of identifying the "core differences" between schooled and unschooled. The focus on the endpoints of an ontogenetic development could easily lead to a reification of these differences. There is a tremendous variability in school quality across the globe. There are thousands of research locations that would qualify as sites for natural quasi-experiments to examine culture–cognition relationships. Our insight in this relation may can be enhanced by focusing less on the schooled–unschooled dichotomy and more on naturally occurring factors, such as variability in school quality and the often weak relationship between educational and chronological age as a research tool opportunity.

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