EDUCATION AND FERTILITY IN SUB-SAHARAN AFRICA: INDIVIDUAL AND COMMUNITY EFFECTS*

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Using data from Demographic and Health Surveys for 22 countries in sub-Saharan Africa, I show that the average educational level in a village or a community of a similar size has a significant depressing effect on a woman's birth rates, net of urbanization and her own education. According to simulations, average fertility for these countries would be 1.00 lower if education were expanded from the current level in the region to the relatively high level in Kenya. The exclusion of aggregate education from the model leaves a response of only 0.52. A considerable aggregate contribution is estimated even when several potential determinants of education are included. This finding illustrates the need to consider aggregate education in future assessments of the total impact of education.

Although women's education has been one of the most thoroughly studied determinants of fertility, with the perspective now often extended to include the closely related topic "women's position," important questions are still poorly answered. One of these questions is whether the educational level of people in a neighborhood or a larger surrounding area has an effect on a woman's fertility above and beyond that of her own education. For example, do uneducated women who live in communities where the average educational level is high have a lower fertility than uneducated women elsewhere? This is an important question to answer because if aggregate education has such an effect, fertility will decline more steeply in response to an increase in women's education than is suggested by a model that includes only individual-level effects. Caldwell (1980) discussed the possible importance of "mass education" more than 20 years ago, and the mechanisms he addressed can be classified largely as aggregate-level effects. In the two decades that have elapsed, however, little empirical research has been devoted to this issue.

It would be particularly valuable to gain more knowledge about the impact of schooling in sub-Saharan Africa, where many countries still display a total fertility above six, some of them without a sign of decline (e.g., Kirk and Pillet 1998). Parents may suffer from having many children, and growing up with many siblings may be detrimental to the children's well-being. In addition, large families may contribute to burdens on other families because of the possibly harmful societal effects of population growth (for a discussion of such micro- and macro-mechanisms, see, for example, Ahlburg, Kelley, and Mason 1996; Cassen 1994; Kravdal 2001a). A reduction of fertility therefore is widely considered to be an important goal, and it would be helpful to know whether investments in education are an even more promising pathway than has usually been believed (in addition to being beneficial for other reasons). There is plenty of room for improvement in the region because the average level of education is still low.

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The objective of the study reported here was to estimate how education influenced birth rates in sub-Saharan Africa in the 1990s through both individual- and aggregate-level effects and to illustrate the consequences of omitting the aggregate variable from the regression model when assessing the total impact of education. The study included all countries in the region where a Demographic and Health Survey (DHS) was undertaken between 1992 and 1999 and was available with a general permit when the analysis started. I focused on births two years before the surveys, and estimated models separately for first- and higher-order births because of the fundamentally different behavioral mechanisms involved. In this article, I show by simulation how hypothetical changes in the educational distribution would affect total fertility through both individual and aggregate effects.

THEORETICAL CONSIDERATIONS

Suggested Individual-Level Effects of Education

There are many possible reasons why a woman's fertility is usually inversely related to the length of her education, at least above a certain level. For example, childbearing desires have been thought to be weaker among the educated because they tend to (1) face higher opportunity costs of childbearing; (2) encourage children's schooling, which entails cash expenses and makes children less available for domestic and agricultural work; (3) be less dependent on children as old-age security or as support if women become widows at a relatively young age; (4) want to dispose more time and investments in each child, not only in terms of education; (5) live in nucleated families, which may reduce fertility partly because childbearing costs must be covered to a larger extent by parents; (6) have a stronger preference for other sources of satisfaction; and (7) experience lower infant and child mortality rates, which influence desires through replacement and insurance effects. These fertility-inhibiting effects may be set off against the possibly stimulating impact of a higher purchasing power resulting from educated women's own work or their marriage into relatively rich families.

The lower mortality among the better educated has a bearing not only on the "demand" side, but on the "supply" side. In addition, "supply" and "regulation costs" are likely to be influenced by education one way or the other through, for example, age at marriage, knowledge about and acceptance of modern contraception, norms about postpartum sexual abstinence and breast-feeding, and fecundity (which may be higher among women with generally better health or who have been treated for sexually transmitted diseases). As is widely known, the fertility-stimulating effects seem to have been dominant at a moderate educational level in many countries, particularly in Africa during the 1970s and early 1980s.

One reason why education may operate through these channels is that women learn to read and write at school and gain various other skills that enhance their productivity. In addition, the position of women relative to men may be involved. Although women's "physical autonomy" (much more constrained in South Asia than in Africa), "economic autonomy," and "decision-making autonomy" (to use Jejeebhoy's 1995 terms) are likely to depend, to a large extent, on community norms and institutional structures, there may also be individual variations that are determined by individual factors, such as education (e.g., Niraula and Morgan 1996). If a woman is well educated, she may, for example, be allowed by the family to work outside the house or may be heard more often in discussions with her husband or in-laws. These opportunities will add to the effect of her literacy and skills and may reduce fertility desires through factors such as opportunity costs, old-age security concerns, and child mortality. (However, education does not *always* contribute to an improvement in women's status; see Balk 1994 for an example from Asia.)

Women's status also may operate through channels other than those just mentioned. For example, if the husband wants more children than the wife (which has been indicated in some studies; see, for example, Bankole and Sing 1998), a strengthening of women's decision-making autonomy is likely to reduce fertility (see also Mason and Smith 2000). Moreover, women who have an inferior position relative to men and consider it unlikely that their daughters' lives will be different from theirs may not only consider the childbearing costs to be generally low and the rewards high; they also may expect boys to be even more useful than girls. Such preferences for boys (which are much less pronounced in Africa than in parts of Asia) will enhance fertility desires in settings where fertility is not extremely high (e.g., Cain 1993). Other examples can be found in Mason (1997).

The Possible Importance of Others' Education

Many of the causally intermediate factors mentioned earlier may depend not only on a woman's own education but also on the education of other women because of social learning, social influence, or more indirect effects (e.g., Bongaarts and Watkins 1996; Kohler, Behrman, and Watkins 2001; Montgomery and Casterline 1996). *Social learning* means that knowledge and attitudes are transmitted directly from others by communication and observation. *Social influence* refers to a more passive imitation of behavior, driven by a desire to gain other people's approval or to avoid sanctions. The more indirect mechanism is that others' ideas, resources, or behavior can influence society and social institutions and therefore behavior more generally.

In principle, these influential others may be close neighbors, other men or women in a village or city, or even people in other parts of the country, and they may be the same or a different age. In this study, the focus was on the education of other women of reproductive age in the same village or in an area of a similar size. Although a woman certainly does not interact directly with all other women in the area, she may interact with a subgroup that can be considered a random selection. Besides, those with whom she interacts directly may themselves be part of interaction chains that, in total, include the entire female population. Because there are also indirect mechanisms, it is theoretically reasonable to include indicators of the overall situation, such as the average length of education among all women in the area, as community variables in statistical models.

What is it, more precisely, that can be transmitted from other people through these more or less direct links? One possibility is that uneducated women have more knowledge of contraception if they live in a society where the average educational level is high than if they live elsewhere. These women also may simply tend to imitate the more widespread use of contraception among the better educated. Another possibility is that their preference structure and their practice of breast-feeding and postpartum abstinence is influenced by aggregate education.

Other people's education also may be important to those who have more education. A diffusion of factual knowledge of contraception, for example, is less relevant, but there may be a more efficient interpretation of the ideas and attitudes that the better educated have been exposed to at school or through reading if there are more women with whom these experiences may be shared.

One of Caldwell's (1980) main arguments was that the introduction of compulsory education in a country, which will lead to generally high enrollment rates ("mass education"), is likely to reduce fertility by increasing parents' costs of childbearing and by undermining the possibilities for making use of children's work potential. A similar argument may be relevant at a lower geographic level: seeing many children in the locality who are enrolled in school may make everyone more conscious about the need to educate their offspring, regardless of legislation. Another important point raised by Caldwell was that a generally high educational level among adults and children alike may strengthen Western middle-class values, with more emphasis on individual rights than on the duty to a larger family network. Caldwell thought that an increase in the proportion with a few years of schooling (breadth of education) would be more important than an increase in the average length of education within this group (depth of education).

A woman's chance of finding a relatively well-paying job (where she cannot bring her children with her), in which she would have high opportunity costs of childbearing, also may depend on other people's education. When many other women in the community are educated, the attitude toward women's work in the modern sector is likely to become more liberal, and more jobs that are attractive to and suitable for women may be created. On the other hand, there may be an opposite and more immediate effect: with a high proportion of well-educated women in the community, there are more women with whom to compete in the job market.

Admittedly, the relevance of these opportunity-cost arguments can be questioned. Child care is probably often freely available from relatives, who, in the African fostering tradition, are thought to be almost as close to the children as are their mothers. Alternatively, child care may be purchased (at a price that depends on the educational level in the community).

Of course, if a general rise in women's education (with or without an accompanying rise for men) contributes to undermine old ideas about women's rights and obligations compared with men, it is not only the opportunity costs of childbearing that will be enhanced. An improvement of women's status as a contextual phenomenon, and the concomitant changes in women's individual position, may influence fertility for many other reasons, a few of which were reviewed earlier. Better-educated women, as well as those who have little education, may be affected.

Another possible mechanism is that broader economic transformations may take place as a result of a better-educated workforce (in the long run): agriculture may be generally modernized, which will reduce children's relative importance in the fields. Moreover, a higher productivity in agriculture and a higher general level of knowledge may facilitate the establishment of manufacturing industries and lead to a greater concentration of people in urban areas. Furthermore, the generally increased wealth that is likely to be part of these transformations may have an impact on fertility.

Some researchers have estimated net effects of aggregate education. For example, Tienda, Diaz, and Smith (1985) found that the average length of education had an impact on cumulated fertility in Peru, net of the woman's own education, and Lesthaeghe et al. (1985) showed net effects of aggregate education both on cumulated fertility and on some proximate determinants in Kenya. Hirschman and Guest (1990) estimated a significant fertility-inhibiting effect of the proportion of women with postprimary education in four Southeast Asian countries. Unfortunately, none of these studies included a control for urbanization. A recent analysis from Zimbabwe showed that the effects of community education disappeared completely when such a variable was entered into the models (Kravdal 2000). In fact, an effect of the educational level in the community on actual fertility, net of urbanization and the woman's own education, still has not been documented statistically. Thomas (1999) indicated that he had found such an effect for South African women, but did not show its size. The significant effect that appeared in a study from Bangladesh, with a control for urbanization, was for contraceptive use, not actual fertility (Amin, Diamond, and Steele 1996). A study by Axinn and Barber (2001) addressed a closely related issue. It showed that a woman's use of modern contraception depended on the distance to school in both childhood and adulthood, partly because of its influence on her own children's schooling. Some earlier efforts to uncover the effects of distance to school were made with community data collected in the World Fertility Surveys in a few countries (reviewed in Casterline 1985).

Macro-Micro Interactions

Some of the effects of aggregate education that were suggested earlier are most likely to be felt among women who have little education, whereas others may be just as strong or even stronger among those who have some education. The current empirical evidence about such macro-micro interactions is mixed. In a review of a number of studies that used different (not always relevant) control variables, Jejeebhoy (1995) reported that individual effects of education on fertility were the most negative in countries where women's literacy was high. In other words, community education has the strongest fertility-reducing effect among the more educated. On the other hand, Jejeebhoy found, for example, that fertility *desires* responded most sharply to secondary education in countries with generally *low* educational levels. The few original multilevel studies addressing such interactions have pointed in different directions (Kravdal 2000; Lesthaeghe et al. 1985; Tienda et al. 1985).

DATA AND METHODS

Data

DHS data for the following countries and years were included: Benin, 1996; Burkina Faso, 1999; Cameroon, 1998; Central African Republic, 1994; Chad, 1996; Cote d'Ivoire, 1994; Comoros, 1996; Ghana, 1998; Kenya, 1998; Madagascar, 1997; Malawi, 1992; Mali, 1996; Mozambique, 1997; Namibia, 1992; Niger, 1998; Rwanda, 1992; Senegal, 1997; Tanzania, 1996; Togo, 1998; Uganda, 1995; Zambia, 1996; and Zimbabwe 1994. Only the data for women were used.

The DHS surveys in these and other countries were conducted primarily to provide researchers and policy makers with comprehensive and comparable data on fertility and child health and their determinants. In addition to a core module used throughout, many countries included several additional questions. My study was based on the information about a woman's educational level at the time of interview; whether her place of residence at that time had a rural or urban character; her birth history; and various indicators of, for example, the wealth of her household, her religion, and her status compared with men's.[1]

The surveys used a clustered sample. Each province or region of a country was divided into small census enumeration areas, which spanned one or a few villages or settlements, a small town, or part of a larger town or city. They typically included about 1,000 people, of whom roughly a quarter were women of reproductive age. Some of these areas were then selected randomly, to be representative of the region or province or their urban and rural parts. There was a total of 5,986 such areas in the 22 countries (ranging from 100 to 521 in any country). Within each selected area, about 25 households were randomly selected, and all women of reproductive age in the households were interviewed.

Averages of educational level and some other variables were calculated for such groups of approximately 25 women (referred to later as DHS clusters) and taken as prox-

1. Birth histories are affected by the respondents' misreporting (with omissions being more problematic than incorrect dating, which may go in both directions), as well as interviewers' displacement of recent births to avoid asking a long sequence of questions on the health of children younger than three or five years (see, for example, Kirk and Pillet 1998 for a more detailed account). This misreporting should not be critical for this study, whose focus was on fertility *differentials* (by education) and behavior during a two-year period before the interview. For example, displacement of the birth of a child older than two would affect only the variable "time since last previous birth" in the hazard models. Moreover, if the tendency to omit births (for example, because the child died) is particularly strong when education is low, which is more likely than the opposite, it would strengthen, rather than weaken, the conclusion of the study.

ies for the corresponding enumeration-area averages, these being theoretically more satisfactory community variables to consider. Of course, averages of clusters and enumeration areas are not exactly equal, but a simulation experiment showed that this measurement error was completely unimportant.[2] If the hypothesis that the average education among the selected 25 women has no effect is rejected (as in this study), one can also reject the hypothesis that the average of the enumeration area has no effect.

Hazard Regression Models

Discrete-time hazard regression models for the two years before the survey were estimated. This limit is not critical. For example, a four-year period of analysis yielded almost exactly the same estimates.

Each woman contributed a series of three-month observation intervals, which tests showed to be a sufficiently short interval. In the first-birth models, the women were followed from two years before the survey unless they were younger than 14 at that time, in which case they were followed from age 14. Similarly, in the multiepisode models that were estimated for the higher-parity transitions, the follow-up was from two years before the survey or the time of first birth, whichever was the latest. The "clock" was reset each time a birth occurred within this period of a maximum of two years: "Duration since last previous birth" was set to zero in that month, and parity was increased by one (or more if multiple births). Parity, duration since previous birth, and mother's age were updated every three months and entered as categorical variables (with, for example, the levels for parity being 1, 2, 3, 4, 5, and 6 or more). This procedure yielded a total of 415,986 three-month observation intervals to be used for the modeling of first births and 935,277 for the modeling of higher-order births. The numbers of births (events) in these models were 12,461 and 46,398, respectively.[3]

Generally, one should be careful when including characteristics at the time of interviews in such models because they may be different from those earlier in the two-year period. Even more critical, they may be partly a response to births. Fortunately, education is not problematic in this respect. The birth rate at any age, in principle, may be influenced by the educational level at that age, as well as by the educational activity (whose possible effects were not discussed earlier); a birth, in turn, may have consequences for subsequent enrollment and thus educational attainment. Such a simultaneity bias, however, is restricted to the effects of secondary education among the youngest women and only for first births (for a brief discussion, see Kravdal 2001b).

I used weights defined in the DHS data to obtain estimates that are representative of the 22 countries on the whole. For further explanation of this weighting, see Kravdal (2001b), where the inclusion of a random term at the cluster level also is shown to be unnecessary.

2. First, a sample of 1,000 areas, each including 250 women, was set up. Different realistic educational distributions were chosen for the areas, and an age at first birth was simulated for each of the 250 women on the basis of their education and the effect estimates from first-birth models shown later. Subsequently, 25 women were drawn randomly from each area, and two models were estimated from the resulting sample, one including the area average and one including the average among the 25 women. This process was completed 100 times to assess the distributional characteristics of the difference between the two corresponding aggregate-effect estimates. The mean difference was 0.003 (about 5% of the effect), whereas the standard deviation was 0.001 (and would have been even smaller with a larger number of areas, as in the study). As expected, the effect of the area average was the strongest (and, of course, virtually identical to that assumed in the simulation). The magnitude of the bias accords well with Greene's (1993:281) formula for a linear univariate model.

3. Such a "piecemeal approach" has been criticized by, for example, Heckman and Walker (1990), but more advanced simultaneous modeling that would include unobserved variables correlated over parity transitions is left to future studies.

When models for individual fertility are estimated (from surveys or other sources) with the intention of assessing the impact of education, aggregate education is rarely considered. This can be a serious misspecification. When only individual education is included in the model, part of the aggregate effect is captured, but not the whole; later I provide an empirical example of this failure to pick up the entire aggregate effect. It is also easy to show mathematically for a simple linear regression model (Kravdal 2001b) but not for a hazard regression model.

Definition of Education

Five categories were defined for women's individual education, according to the number of years completed: (1) no education or an incomplete primary education lasting fewer than 3 years, (2) an incomplete primary education of a longer duration (3–6 years), (3) a complete primary education or an incomplete secondary education of fewer than 2 years (i.e., 7–8 years in total), (4) 2–3 years of secondary education (i.e., 9–10 years in total), and (5) a secondary education of 4 or more years (i.e., 11 or more years in total).

The main measure of aggregate education was the average number of years at school among the women in the cluster. (When the woman in focus was excluded, the effect estimates were not perceptibly different.) In addition, some models were estimated for women in clusters in which at least one woman had three or more years of education (about 90% of the total sample). These models included both the proportion of women with at least three years of education and the average level within this group.

Controls for Determinants of Education

A number of factors are likely to influence both education and fertility and should therefore be included as control variables. Variables with a causal position between education and fertility, however, should not be included when the intention is to estimate total effects.

The urban-rural character of the place of residence is a particularly important control variable. A woman who has grown up in an urban area is, for obvious reasons, more likely than other women to have some education, and her behavior as an adult also may be influenced by her early experience in an educated neighborhood. In addition, the degree of urbanization in the community where she lived at the time of interview must have had a bearing on the general educational level there, which may also be of importance. The Zimbabwean study (Kravdal 2000) already referred to showed not only that it was important to control for urbanization but also that the results were sensitive to the choice of urban-rural variable. In the current study, the urban-rural character of the place where the woman lived at the time of the interview was used. This is the variable that had the most pronounced impact in the Zimbabwean study.

One cannot completely rule out the possibility of a reverse relationship. As I pointed out earlier, the aggregate level of schooling may stimulate urbanization in the long run. Similarly, a woman's own education may make migration to a city more attractive to her. If these effects are of any quantitative importance, it means that the total effects of investments in education are more negative than indicated by the estimates shown here (given the signs of the effects of urbanization and education and the positive education-urbanization correlation).

The causal position of some other variables available in the data is even more diffuse. For example, the wealth in the woman's household and her autonomy, as reported at the time of the interview, may be among the consequences of her education. Conversely, these factors may be linked with factors that have determined her education, such as the economic resources of her family of origin or their attitudes toward women. In addition, they may have been influenced by fertility during the two previous years. These variables were therefore not included at the individual level. Neither was religion, although it is probably quite stable over a person's life and more likely to affect than to be affected by education. Religion was not included because model experimentation showed it to be unimportant as a control variable.

The inclusion of corresponding aggregate-level variables is less problematic. The causal position vis-à-vis aggregate education is, of course, just as ambiguous. These aggregate factors, however, are at least not influenced by a woman's own education and, more important, by her fertility (unless there is selective migration, as Kravdal 2001b discussed).

Two of the aggregate-level variables included in the models are crude indicators of women's status in the community: the proportion of women who reported a preference for boys (the ideal number of sons larger than the ideal number of daughters) and the proportion of working women who did not participate in decisions about how to spend the money they earned. The following were indicators of wealth and economic modernization: the proportion of households that have electricity, the average score on an indicator of wealth (constructed as a variable with levels 0, 1, or 2, depending on the household's possession of some consumer items), and the proportion of married women with husbands who worked in the agricultural sector. In addition, there were three indicators of religiosity: the proportion who were Muslim; the proportion with a traditional religion; and the proportion who were neither Christian, Muslim, nor traditionalists.

In addition, I included country to capture various characteristics at the national level that may have a bearing on both fertility and education.

Simulations

I performed Monte Carlo simulations to examine how changes in the educational distribution affect total fertility. Birth histories were generated for 50,000 women in a given educational category. This was experimentally proved to be a sufficiently large simulation sample. Starting at age 14, a three-month birth log-odd, easily transformed into a probability, was predicted for each woman every third month on the basis of characteristics at the beginning of the three-month interval and model estimates. A birth was ascribed to the woman within the interval if a random number with a uniform distribution over [0,1] was less than the calculated probability. The average number of births before age 45 in this sample was the simulated education-specific total fertility. When this procedure was repeated for all five educational categories, an average simulated total fertility could be calculated.

RESULTS

Main Effects of Individual and Aggregate Education

According to a model that includes only age, individual education, and country, a woman's education strongly reduces her first-birth rate (Model 1, Panel A, Table 1). This effect is considerably weaker when urbanization is included (Model 2).

Increases in the average educational level are found to push fertility down, net of individual education and urbanization: an additional year reduces the log-odds of a first birth by 0.064 (Model 4). The inclusion of this variable also reduces the effect of individual education. Stated differently, the effect of individual education in a simpler model captures part of the aggregate effect (but, as shown later, not the entire effect). Without a control for urbanization, the aggregate effect is, of course, stronger (Model 3).

Similar patterns, but generally weaker educational effects, are seen for higher-order births (Panel B). The exception is that a few years of education does not reduce fertility when aggregate education is included.

Such negative effects of community education do not appear consistently when models are estimated for each country separately, however. No significant effects were

Table 1. Estimates From Discrete-Time Hazard Models for First- and Higher-Order Births, Based on DHS Data for 22 Countries in Sub-Saharan Africa for the 1990s

Independent Variable	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Panel A: First Births[a]						
Woman's education						
0–2 years[b]	0	0	0	0		0
3–6 years	–0.22**	–0.16**	–0.10**	–0.09**		–0.09**
7–8 years	–0.38**	–0.29**	–0.18**	–0.18**		–0.17**
9–10 years	–0.78**	–0.65**	–0.51**	–0.50**		–0.49**
11+ years	–1.14**	–0.96**	–0.81**	–0.79**		–0.77**
Urban vs. Rural		–0.32**		–0.18**	–0.21**	–0.01
Average length of education (years)			–0.085**	–0.064**	–0.107**	–0.024**
Proportion with a preference for boys						–0.04
Proportion of working women not deciding over money they earn						0.04
Average score on wealth indicator (0–2)						0.08**
Proportion who have electricity						–0.44**
Proportion of married women with husbands in agriculture						0.23**
Proportion Muslim						0.10*
Proportion traditional religion						0.01
Proportion other religion or no religion						0.34**
Panel B: Higher-Order Births[c]						
Woman's education						
0–2 years[b]	0	0	0	0		0
3–6 years	–0.05**	–0.01	0.03†	0.02		0.02
7–8 years	–0.16**	–0.10**	–0.05**	–0.05**		–0.05**
9–10 years	–0.33**	–0.23**	–0.17**	–0.16**		–0.15**
11+ years	–0.50**	–0.37**	–0.31**	–0.29**		–0.28**
Urban vs. rural		–0.27**		–0.21**	–0.22**	–0.14**
Average length of education (years)			–0.053**	–0.030**	–0.041**	–0.024**
Proportion with a preference for boys						0.13**
Proportion of working women not deciding over money they earn						0.18**
Average score on wealth indicator (0–2)						0.09**
Proportion who have electricity						–0.21**

(continued)

(Table 1, continued)

Independent Variable	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Panel B: Higher-Order Births[c] (cont.)						
Proportion of married women with husbands in agriculture						0.09**
Proportion Muslim						–0.13**
Proportion other religion or no religion						0.03
Proportion traditional religion						–0.08†

[a]Age (7 categories) and country (22 categories) were also included in the models. Indicators for missing information were included in Model 6, without being important for other estimates.

[b]Arbitrarily chosen reference category.

[c]Age (7 categories), duration since last previous birth (6 categories), parity (6 categories), and country (22 categories) were also included in the models. Indicators for missing information were included in Model 6, but without being important for other estimates.

†$p < .10$; *$p < .05$; **$p < .01$

found for Zimbabwe, as was also reported by Kravdal (2000) on the basis of census data on education at a higher aggregation level (which gives weaker effects). The other countries with nonsignificant educational effects for both first- and higher-order births were Burkina Faso, Central African Republic, Uganda, Ghana, and Malawi. This finding has no obvious explanation. For example, these countries are not known to have less efficient school systems than the others in the region. Besides, if one additional year of education actually does have relatively little impact on skills and attitudes in these societies, individual effects also should be weaker, which was not the case (results not shown). Moreover, the sample sizes are not particularly small, and the enumeration areas are of the same size as those in other countries. For Zimbabwe (but not the five other countries), one may suspect a relatively weak effect of an additional year of aggregate education because the level is already high. However, the data did not suggest such a nonlinear pattern. Whereas the first-birth log-odds declined sharply with increasing aggregate education at low levels and display an irregular pattern at high levels, the higher-order birth rates were more negatively influenced by average education at high than at low levels (Panel A, Table 2).

In light of Caldwell's (1980) hypothesis, it is interesting to see significant effects of both breadth and depth of education, for first- as well as higher-order births (Panel B, Table 2).

Macro-Micro Interactions

Significant macro-micro interaction effects were found (Panel C, Table 2). For first births, a higher general level of education has the sharpest depressing effect among women who have less than three years of schooling. The weakest impact of average education is estimated for those with at least four years of secondary education. However, the effect is significant even at that level (results not shown). The opposite is found for higher-order births: the effect of aggregate education is the sharpest for women with two to three years of secondary education and the weakest for those with fewer than three years of primary education.

It can be shown by simulation that the interaction effects for higher-order births are dominant. As a conclusion, an additional year of aggregate education reduces total fertil-

Table 2. Estimates From Discrete-Time Hazard Models for First- and Higher-Order Births, Based on DHS Data for 22 Countries in Sub-Saharan Africa for the 1990s

Independent Variable	First Births[a]	Higher-Order Births[b]
Panel A[c]		
Woman's education		
0–2 years[d]	0	0
3–6 years	–0.08**	0.00
7–8 years	–0.19**	–0.06**
9–10 years	–0.52**	–0.17**
11+ years	–0.81**	–0.27**
Average length of education (integer part)		
0 year	0.26**	–0.00
1 year	0.16**	0.02
2 years	0.01	0.04†
3 years[d]	0	0
4 years	–0.05	–0.04*
5 years	–0.08*	–0.07**
6 years	–0.08†	–0.11**
7 years	–0.25**	–0.18**
8 years	–0.19**	–0.24**
9+ years	–0.38**	–0.31**
Panel B[e]		
Woman's education		
0–2 years[d]	0.00	0.00
3–6 years	–0.09**	0.01
7–8 years	–0.16**	–0.06**
9–10 years	–0.49**	–0.17**
11+ years	–0.78**	–0.29**
Proportion of women with three or more years of education	–0.27**	–0.12**
Average length of education among women with three or more years of education (years)	–0.065**	–0.025**
Panel C[f]		
Woman's education		
0–2 years[d]	0	0
3–6 years	–0.06†	–0.01
7–8 years	–0.15**	–0.06**
9–10 years	–0.48**	–0.12**
11+ years	–0.83**	–0.27**
Average length of education (years)	–0.082**	–0.013**
Woman's education × (average length of education (years) – 4)		
0–2 years × (average – 4)	0	0
3–6 years × (average – 4)	0.026*	–0.023**
7–8 years × (average – 4)	0.021	–0.034**

(continued)

(Table 2, continued)

Independent Variable	First Births[a]	Higher-Order Births[b]
Woman's education × average length of education (years) – 4 (cont.)		
9–10 years × (average – 4)	0.027†	–0.049**
11+ years × (average – 4)	0.039*	–0.032**

[a]Age (7 categories), urbanization, and country (22 categories) were also included in the models.

[b]Age (7 categories), duration since last previous birth (6 categories), parity (6 categories), urbanization, and country (22 categories) were also included in the models.

[c]Estimates are from models in which aggregate education is categorized.

[d]Arbitrarily chosen reference category.

[e]Estimates are from models including measures of both the depth and the breadth of education (only for women in clusters in which at least one woman had three or more years of education).

[f]Estimates are from models including an interaction between individual and aggregate education.

$^{\dagger}p < .10$; $^{*}p < .05$; $^{**}p < .01$

ity, regardless of the woman's own education, but less markedly for the poorly educated. Conversely, a woman's own education generally exerts a monotonic negative effect, except at the lowest level of aggregate education. For example, when the average is less than one year, the higher-order birth rate is higher for women with three to six years of education than for the less educated, and this effect is not fully offset by the lower first-birth rate.

Inclusion of Other Aggregate Variables

When aggregate indicators of wealth, economic modernization, religion, and women's status are included, the effect of average education on first-birth rates is reduced to less than half its size in the simpler models (see Table 1, Model 6). The effect on higher-order birth rates is reduced by one-fifth. As I pointed out earlier, these variables, to some extent, may be considered causally before education, which would mean that the true impact of investments in education (through the aggregate-level effect) is weaker than indicated in the simpler model. If additional indicators of these factors or indicators of other potential sources of spuriousness had been available, even weaker effects of aggregate education might have been estimated.[4] On the other hand, the opposite causality is also possible, in which case part of the total effect of aggregate education will be tapped out by the inclusion of such variables.

Implications for Total Fertility

I calculated the impact on total fertility of a hypothetical change in the educational distribution, according to different models, by comparing simulated total fertility for one population with that for another in which the educational distribution is different. For each population, simulated total fertility is the weighted average of the simulated education-specific total fertility for the five educational categories, with weights equal to the proportions of women in these five categories.

4. This study was based on only one set of measurements for each enumeration area. If similar data were available for these areas for an earlier period, it would be easy to remove at least the persistent unobserved heterogeneity effects. Such an analysis can be performed for the African countries that used the same enumeration areas in previous DHS rounds.

The simulations were based on predicted birth log-odds. Such predictions (for any level of the other variables) can be made, for example, for urban areas in a particular country by setting only the corresponding country dummy variable and the urbanization dummy variable to 1. Because fertility measures that are reasonably representative of the entire region are preferred, however, the country dummy variables were substituted with the proportions living in the respective countries (according to population statistics), and the urbanization dummy variable was substituted with the proportion living in urban areas. (If other values were chosen, all log-odds simply would have been higher or lower by an additive constant. The patterns of educational differences would have been the same.)

As a first example, fertility was simulated, for two populations that have the educational distribution calculated for the 22 countries as a whole and for Kenya. Kenya has the highest average educational level among the 22 countries (6.9 years, as opposed to the 22-country average of 3.8 years).

I present this comparison only as an illustration of the magnitude of the aggregate-level contribution, without any claim to realism. An increase in the average educational level in sub-Saharan Africa up to the level currently observed in Kenya will not necessarily occur soon. Whereas some countries in the region indeed made great progress in the 1960s and 1970s, others lagged far behind. Further, setbacks were experienced in the 1980s as a result of the economic crisis and the structural adjustment programs (e.g., Lloyd, Kaufman, and Hewett 2000). Contrary to other main regions of the world, the primary enrollment rate fell in sub-Saharan Africa during the 1980s (see also UNESCO 2001), and the quality of the schools deteriorated. A recovery has been witnessed in more recent years, but there are high barriers to the expansion of education, such as a heavily constrained economy, high population growth, and parents' skeptical attitudes (see also Lloyd and Blanc 1996).

When aggregate education is left out of the models (Model 2, Table 1), the simulated education-specific total fertility is 6.04, 5.79, 5.23, 4.22, and 3.36 for the five categories, which gives an average of 5.55 when the 22-country distribution is used. When the educational distribution for Kenya is used instead, fertility is 0.52 lower (i.e., 5.03).

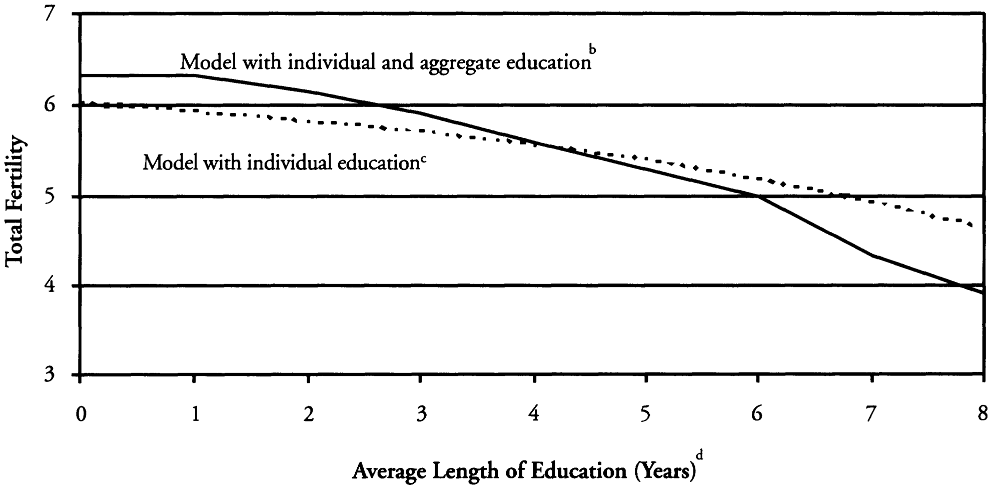
When aggregate education is included (Model 4, Table 1), almost the same fertility (5.53), of course, is simulated as a 22-country average. However, the levels simulated for the different educational groups (5.85, 5.83, 5.37, 4.50, and 3.69) differ less. When a new average is calculated from these education-specific levels with the Kenyan educational distribution as weights, total fertility is 0.38 lower. When average education is set to 6.9 years instead of the 22-country average of 3.8, however, a lower fertility is simulated for all educational categories (5.21, 5.18, 4.75, 3.90, and 3.15). Thus, average fertility for the Kenyan distribution is 1.00 lower (i.e., 4.53). This finding clearly illustrates that a relatively small part of the aggregate effect is picked up by the individual-level variable in the simplest model.

A model including aggregate education and not individual education captures the entire impact of education (Model 5, Table 1; see Kravdal 2001b for a mathematical proof for a simpler model) but, of course, without providing any information about the individual and aggregate contributions. According to simulations based on such a model, expansion to the level in Kenya would reduce fertility by 1.05.

I obtained a more conservative assessment by using estimates from a model in which various other structural variables are also included (Model 6, Table 1). The result of this simulation is that expansion of education up to the Kenyan level would lead to a drop of 0.75 in total fertility.

As a second illustration of the differences between the models, I simulated fertility for nine different educational distributions that correspond to average education of approximately 0, 1, 2, 3, 4, 5, 6, 7, and 8 years (see a detailed account in the notes to Figure

Figure 1. Simulated Total Fertility,[a] According to Two Different Models



[a]Average number of births during reproductive ages was simulated for 50,000 women in each of the five educational categories. In the predictions of birth log-odds, average education was not set to 0,1,...,8, but 0.5,1.5,...,8.5. To obtain figures representative of the 22 countries, I set the country dummy variables to the proportions living in the respective countries, and set the urbanization dummy variable to the proportion living in urban areas. "Simulated total fertility" is the weighted average of these five education-specific averages, with the educational distributions shown in Appendix Table A1 as weights.

[b]The model included the same variables as described in note c, plus aggregate education and an interaction between individual and aggregate education as described in Table 2.

[c]The model included age, duration since last previous birth (not for first birth), parity (not for first birth), urbanization, country, and individual education, with the same categories as described in Table 1.

[d]The value 0 corresponds to an average education in the range [0,1), 1 corresponds to [1,2), and so forth.

1 and in Appendix Table A1). The simulations were done as described earlier, except that aggregate education, when included, was entered as a 10-category variable along with a macro-micro interaction (specified as in Table 2).

Total fertility simulated from a purely individual model declines monotonically from 6.02 to 4.63 across the nine settings. The change is most pronounced at the highest educational levels. Almost the same pattern is seen when aggregate education and the macro-micro interaction are included, except that the differences are generally larger. (If the interaction had been taken out and the aggregate effect specified as linear, total fertility also would have declined at low levels of education, but the pattern otherwise would have been similar.)

If total fertility is plotted versus the proportion with at least three years of schooling, rather than versus the average length of education (as in Figure 1), the increasing steepness of the education-fertility curve appears slightly more clearly. There is no marked turning point, however. This pattern sheds some doubt on the idea suggested by Caldwell (1980) and recently supported by Lloyd et al. (2000), on the basis of African country-level data, that there is almost a threshold, at a high level of primary schooling, for the onset of a decline in fertility. Admittedly, a cross-sectional study such as this is not the perfect background for discussing possible triggers of fertility transition. However, when fertility is markedly lower in populations where, say, 60% have more than three years of education than in populations where the corresponding percentage is 10%, it is not unlikely that the former have experienced a decline, which may even have started at levels considerably below the moderate level of 60%.[5]

CONCLUSION

Univariate tabulations of the total fertility rate by individual women's educational levels, such as those routinely produced in DHS country reports, give an incorrect impression of the impact that investments in education would have on fertility. One reason is that the urban background of the better educated is not taken into account. On the other hand, the average educational level among other women in the enumeration area, which typically is the size of a village, has a strongly significant depressing effect on a woman's birth rate, above and beyond that of her own education. This spillover from others' education, which has not been seen in previous studies, more than compensates for the attenuation of effects that results from a control for urbanization (according to simulations not reported).

Community education influences fertility both among the uneducated and the better educated, although slightly more markedly for the latter, and both the proportion with at least three years of education and the average level within this group are of importance. Unfortunately, nothing was learned about the underlying mechanisms. The data cannot reveal the contents of the social interactions or whether a woman is more influenced by close neighbors and social equals than by others in the enumeration area, for example.

A simulation based on a model in which aggregate education was included showed that the average fertility for the 22 countries would have been 1.00 lower if education had been expanded from the level currently observed in the region to that in Kenya. Only 0.38 of this difference is an individual effect. If only individual education was included in the model, some of the aggregate effect was captured, but only a small part. Simulations showed a drop in fertility of no more than 0.52. (In reality, such an expansion of education, of course, may be hard to achieve, and should it take place, it would not necessarily have the same influence on fertility as suggested by the model estimates. The effect would depend partly on whether the content and quality of the education is the same as in the past.)

The inclusion of aggregate education exclusively would have yielded the same estimates of the total impact of expansion. This result does not imply, of course, that generally anything would be gained by aggregating available individual data to perform a purely ecological analysis; it merely illustrates that a researcher with access only to aggregate data, for example from censuses, is well equipped to trace such total effects in certain situations (especially if fertility measures are less crude than they usually are in aggregate data and the number of potential control variables is less limited).

According to the most complex model, which includes finely categorized individual and aggregate education, as well as an interaction (of modest importance) between them, fertility will decline as more people are educated. The decline is steeper at high than at low levels, but no threshold can be discerned.

These model estimates and simulation results illustrate that aggregate education indeed must be taken into account. Even the results from the most complex models, however, must not be considered accurate assessments of the total impact of education,

5. Lloyd et al. focused on teenage education and found in a univariate analysis that increases in the proportion with four or more years of schooling had a much more inhibiting effect on fertility when a level of about 75% was already reached than at lower levels. In the present study, such a marked turning point also failed to appear when nine settings were defined according to the proportion of teenagers with more than four years of education, with category limits in steps of 0.1. When teenage aggregate education, instead of the education among women in all reproductive ages, was included in the models, the effects were generally weaker and not particularly sharp at high levels. Thus, it does not seem that the use of adult, rather than teenage, education has masked a threshold effect.

primarily because the study was based entirely on the village or an area of a similar size as the aggregation level, while the education of people outside this area may have an impact as well. The greater distance would weaken the effects stemming from direct social interaction, but, as a larger subpopulation, these people may be more influential in other ways. Conveniently, further modeling revealed that the average educational level in the "province" (the largest regional unit defined in the surveys, of which there are nine in each country on average) had a completely negligible additional effect on fertility (results not shown). I also found that the inclusion of this variable as the only aggregate-education variable would be a poor specification: the effect would be only about half the effect estimated with the village-level variable (results not shown). Unfortunately, units between the province and the village level generally were not identified in the data, and it was not possible to go below the village level. It is hoped that it will be possible to address the importance of the choice of level in future studies on the basis of other data.

One also should bear in mind that structural factors other than urbanization may be partly responsible for the aggregate effects. In an attempt to check this possibility, I included some indicators of economic modernization, religion, and women's general status in the community in the models. Doing so reduced the aggregate effects to about the size of the individual effects. Because some of these indicators (and, in fact, urbanization), to some extent, may be consequences, rather than determinants, of education, one may argue that this is a conservative estimate of the total effect of education. On the other hand, there may be variations in modernization and women's status or other important factors that are not captured by the included variables. In that case, the aggregate effects may be smaller than was suggested.

In addition, the estimates of individual-level effects of education may be biased because of unobserved factors. Therefore, even though the true effects of aggregate education may be weaker than was indicated here, they are not necessarily less important relative to the individual effects. The fact that the controls in this study are not perfect does not undermine the conclusion that community education deserves attention in future assessments of the importance of schooling for a decline in fertility.

Appendix Table A1. Educational Distribution Among All Women in DHS Clusters Where the Average Length of Education Is in the Intervals [0,1), [1,2), [2,3), [3,4), [4,5), [5,6), [6,7), [7,8), or [8,9) (Symbolized by the Integer Parts 0 Through 8)

Proportion of Women With Education	Average Length of Education (Years)								
	0	1	2	3	4	5	6	7	8
0–2 years	0.943	0.748	0.586	0.442	0.319	0.217	0.142	0.084	0.053
3–6 years	0.051	0.205	0.297	0.352	0.368	0.335	0.276	0.224	0.167
7–8 years	0.004	0.031	0.079	0.140	0.207	0.291	0.330	0.332	0.271
9–10 years	0.002	0.014	0.027	0.047	0.072	0.099	0.152	0.200	0.246
11+ years	0.000	0.003	0.011	0.020	0.035	0.058	0.099	0.160	0.262

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