Demography (2018) 55:2371–2394
https://doi.org/10.1007/s13524-018-0722-3

From Privilege to Prevalence: Contextual Effects of Women's Schooling on African Marital Timing

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Published online: 17 October 2018

Abstract

In Africa and elsewhere, educated women tend to marry later than their less-educated peers. Beyond being an attribute of individual women, education is also an aggregate phenomenon: the social meaning of a woman's educational attainment depends on the educational attainments of her age-mates. Using data from 30 countries and 246 birth cohorts across sub-Saharan Africa, we investigate the impact of educational context (the percentage of women in a country cohort who ever attended school) on the relationship between a woman's educational attainment and her marital timing. In contexts where access to education is prevalent, the marital timing of uneducated and highly educated women is more similar than in contexts where attending school is limited to a privileged minority. This *across-country convergence* is driven by uneducated women marrying later in high-education contexts, especially through lower rates of very early marriages. However, within countries over time, the marital ages of women from different educational groups tend to diverge as educational access expands. This *within-country divergence* is most often driven by later marriage among highly educated women, although divergence in some countries is driven by earlier marriage among women who never attended school.

Keywords Education · Marriage · Africa · Cohort · Hazard models

Electronic supplementary material The online version of this article (https://doi.org/10.1007/s13524-018-0722-3) contains supplementary material, which is available to authorized users.

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Introduction

Across sub-Saharan Africa, women who have gone further in school tend to marry at older ages than their less-educated peers (Arnaldo 2004; Bongaarts et al. 2017; Gyimah 2009; Ikamari 2005; Kaufmann and Meekers 1998; Manda and Meyer 2005). In addition to being an attribute of individual women, educational attainment is also an aggregate phenomenon: the *social meaning* of a woman's educational attainment depends on the distribution of education within her birth cohort (Kravdal 2002; Schofer and Meyer 2005; Stevens et al. 2008). For populations in which few have attended school, five years of schooling might convey elite status; in settings with more widespread access to education, though, five years of schooling might indicate extreme disadvantage. Although the relationship between a woman's educational attainment and her marital timing is well documented, no research has investigated how this relationship varies systematically with the prevalence of education in her birth cohort. We use data from 246 birth cohorts, spanning four decades and representing 30 sub-Saharan African countries, to address this gap.

Contemporary sub-Saharan Africa is characterized by a unique degree of heterogeneity in educational attainment *across space*. For example, the percentage of women aged 20–24 who had never attended school was only 0.2 % in Lesotho in 2014 but more than 75 % in Niger in 2012 (ICF 2016). We even see dramatic differences between neighboring countries: for instance, in 2014, the percentage of women aged 20–24 who reported no formal education was 11 % in Ghana but more than 60 % in Burkina Faso (ICF 2016).

In addition to heterogeneity across space, many sub-Saharan African countries have experienced transformations in access to education *over time*. In virtually all countries in the region, young women have progressed further in school than their mothers. The pace of this transformation has been stunning in some countries: in Malawi, for example, more than one-half of women born between 1956 and 1960 never attended school, whereas the majority of women born 35 years later completed at least eight years of school (ICF 2016). In other countries, such as Mali and Sierra Leone, educational gains have been more modest (ICF 2016). Previous research in Latin America, Asia, and North Africa has found evidence that the association between educational attainment and age at first marriage has changed over time (Bongaarts et al. 2017; Esteve et al. 2013; Kroeger et al. 2015; Raymo 2003; Yu and Xie 2015). However, in sub-Saharan Africa, only a single-country investigation of Ghana (Gyimah 2009) and an analysis of pooled regional data (Bongaarts et al. 2017) have studied temporal changes in this association.

We exploit these two sources of educational heterogeneity (across space and over time) to address a single question: in sub-Saharan Africa, how does the relationship between education and the timing of first marriage vary with the educational composition of one's cohort? The literature supports three unique scenarios, each of which suggests a different answer to this question.

In what we define as a *convergent* scenario, the gap in age at first marriage by educational status will be at its narrowest in cohorts with widespread educational access.[1] We identify two possible routes to convergence, which differ in whether the pattern is driven by the behavior of uneducated or highly educated women. First,

[1] Throughout this article, "convergence" and "divergence" refer exclusively to a narrowing or widening of the gap in across-group means, rather than to a reduction or increase in variance.

convergence might be driven by a *bandwagon effect* among uneducated women (*Scenario Conv-1*). Mass education speeds up cultural change, creates new cultures, and propagates Western values, leading to fertility decline (Caldwell 1980) and, conceivably, later entry into marriage. If education spreads behaviors from the educational elite to the non-elite, the externalities of education will affect uneducated women as well-educated women's behaviors diffuse through the population and shape the cultural context within which all women operate. Convergence may therefore occur if the age at marriage is higher—among educated and uneducated women alike—in contexts with high educational access. Colleran et al. (2014) observed this pattern in Poland, where less-educated women adopted the low-fertility behavior of educated peers when the average level of education in their communities increased. This scenario has also been observed in studies of contraceptive use in India: the behavior of uneducated women is more sensitive to differences in community-level educational attainment than the behavior of more-educated women (McNay et al. 2003; Moursund and Kravdal 2003).

Second, convergence will likely emerge if the timing of life events is shaped by education's conveyance of *elite status* to those who attend school (*Scenario Conv-2*; Johnson-Hanks 2006; Mann 1985). In contexts with widespread access to education, a woman needs more years of education to be considered elite relative to others in her cohort (Bourdieu and Passeron 1990; Burke 2015; Foster 1980; Hendi 2015, 2017). Therefore, if elite status influences age at marriage, a high level of educational attainment (e.g., completing secondary school) will be more strongly associated with later marriage in contexts where that level is remarkable than in contexts where it is commonplace. In this scenario, as access to education expands, the formerly excluded populations who enter school will continue to marry at younger ages because they will still be of lower status, despite their education. Through this process, the gap in marital age between uneducated and well-educated women will diminish as well-educated women begin to exhibit marital behaviors similar to those of the less-educated.

In a *divergent* scenario, the gaps in age at first marriage by educational status will be broadest when penetration of schooling is high. This pattern has been observed over time in Ghana (Gyimah 2009), Latin America (Esteve et al. 2013; Kroeger et al. 2015), and East Asia (Raymo 2003; Yu and Xie 2015). Again, this pattern could be driven by two separate forces. First, divergence may occur as "no education" moves from being a catch-all category to an indicator of *relative disadvantage* (*Scenario Div-1*; Hendi 2015, 2017). In cohorts with very low access to education, "no education" is the modal status, and educational attainment differentiates only the most elite women from all others. The large "no education" group is likely heterogeneous, containing women who conform to a variety of social norms and cultural conventions that shape the timing of marriage. In contrast, in cohorts in which education is prevalent, women of average social status will have higher educational attainment than those at the bottom of the status hierarchy. Education will be a more meaningful indication of social status in these contexts because the "no education" category will be a stronger marker of relative disadvantage, such as living in more isolated areas or more extreme poverty (Cleland 2002; Kritz and Gurak 1989). In turn, women with no formal education in high-education contexts might behave more distinctly, leading to larger gaps in age at marriage between women of different educational statuses.

Second, differences in age at first marriage by educational attainment may be divergent if, in contexts with wider access to education, educated women conform to *distinct social norms* that lead to delayed marriage (*Scenario Div-2*; Gyimah 2009; Kroeger et al. 2015). For example, as more women earn advanced credentials, educated young women may be more likely to enter the formal labor market or to choose their own partners (Gyimah 2009; Rindfuss et al. 1996; Yu and Xie 2015).

Finally, we foresee the possibility of a *stable* scenario if the relationship between age at first marriage and educational attainment is shaped by the *cumulative experience* of attending school (Baker 2014). Consistent with a "dose-response" mechanism—through which individual-level exposure to schooling affects behaviors—students with improved literacy, numeracy, and cognitive skills wait longer to have sex (Grant and Hallman 2008; Marteleto et al. 2008); entry into first marriage could operate through a similar process. In this scenario, the relationship between educational attainment and age at marriage does not depend on the broader context of educational opportunity. Rather, women with the same educational status will display similar marriage behavior across all cohorts. The gaps in age at first marriage by educational status will therefore be of comparable size across varying levels of educational access. As a given level of schooling becomes more widespread, the delayed age at first marriage associated with that level of schooling will itself become more widespread within the population.

Methods

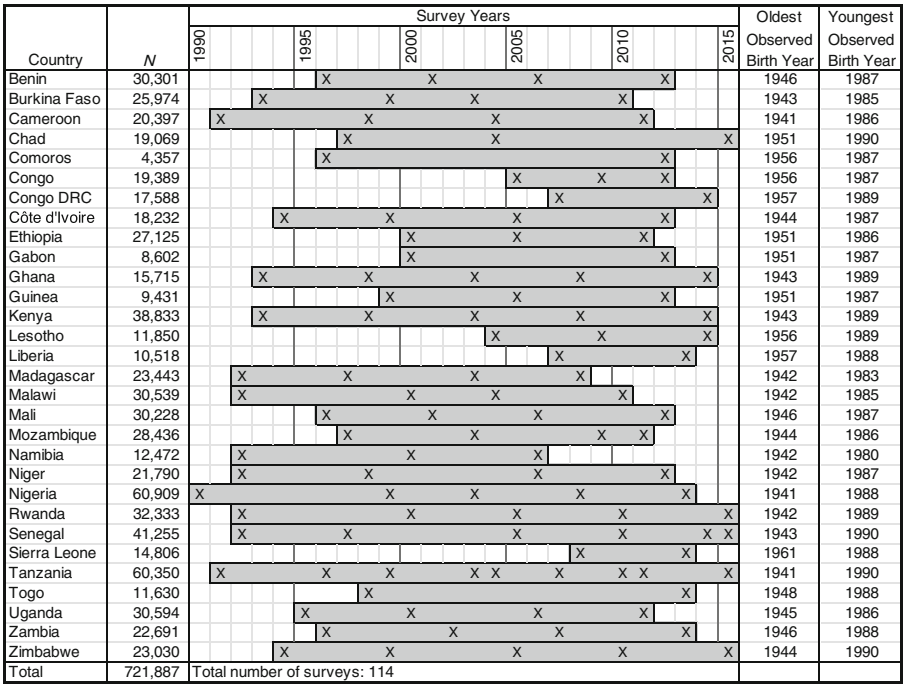
Data

We use 114 Demographic and Health Surveys (DHS) collected between 1990 and 2015, representing women from 30 sub-Saharan African countries (Fig. 1). The DHS are nationally representative household-based surveys focused on sexual and reproductive health, child health, and fertility (ICF 2016). Questionnaires and survey implementation protocols are standardized to allow for comparison across countries and over time.

We pool all available surveys for each country (see Fig. 1) to generate five-year cohorts of women aged 25–49 born between 1941 and 1990. For the oldest cohort, we include a 10-year age span (1941–1950) because of small sample sizes in earlier surveys. Countries vary in the timing and frequency of surveys, so some countries have fewer birth cohorts available than other countries. We exclude four countries (Central African Republic, Gambia, São Tomé and Principe, and Swaziland) because only one DHS survey was conducted there, providing insufficient power to analyze trends when the data are organized into five-year birth cohorts. We also exclude 10 country-cohorts with fewer than 300 observations. Our full sample contains 721,887 women representing 246 country-specific birth cohorts.

Measures

In sub-Saharan Africa, marriage takes various forms, including traditional marriages, civil unions, religious marriages, and de facto marriages with no ceremony (Arnaldo



Country	N	Oldest Observed Birth Year	Youngest Observed Birth Year
Benin	30,301	1946	1987
Burkina Faso	25,974	1943	1985
Cameroon	20,397	1941	1986
Chad	19,069	1951	1990
Comoros	4,357	1956	1987
Congo	19,389	1956	1987
Congo DRC	17,588	1957	1989
Côte d'Ivoire	18,232	1944	1987
Ethiopia	27,125	1951	1986
Gabon	8,602	1951	1987
Ghana	15,715	1943	1989
Guinea	9,431	1951	1987
Kenya	38,833	1943	1989
Lesotho	11,850	1956	1989
Liberia	10,518	1957	1988
Madagascar	23,443	1942	1983
Malawi	30,539	1942	1985
Mali	30,228	1946	1987
Mozambique	28,436	1944	1986
Namibia	12,472	1942	1980
Niger	21,790	1942	1987
Nigeria	60,909	1941	1988
Rwanda	32,333	1942	1989
Senegal	41,255	1943	1990
Sierra Leone	14,806	1961	1988
Tanzania	60,350	1941	1990
Togo	11,630	1948	1988
Uganda	30,594	1945	1986
Zambia	22,691	1946	1988
Zimbabwe	23,030	1944	1990
Total	721,887		

Total number of surveys: 114

Fig. 1 Summary of the timing and location of DHS data sets included in the analysis. Country-cohorts with fewer than 300 individuals are dropped from analysis due to potentially small sample sizes within a given educational category; oldest and youngest observed birth year columns reflect the range of birth years included in analyses. Countries with only a single DHS survey (Central African Republic, Gambia, and São Tomé and Principe) are excluded from analysis due to concerns regarding sufficient power to analyze trends over time

2004; Meekers 1992). The measure of age at marriage in the DHS does not distinguish between cohabitation and marriage; rather, this variable represents the age at which a woman began cohabitating or got married (whichever occurred first). With no alternative, we adopt this definition of marriage, recognizing that it conceals heterogeneity in lived experience that is likely associated with educational attainment. In Sierra Leone, for example, Bledsoe (1990) determined that educated women progress quickly through the conjugal process and are particularly likely to have their marriages officially recognized by family, religious organizations, and the state.

It might seem that in order to examine the effects of contextual differences in educational access, one should measure educational attainment using quantiles that identify where a woman with a given level of education fits within the educational distribution of her birth cohort—for example, the 25th percentile might correspond to three years of schooling in one cohort and seven years in another (Hendi 2017; Rotman et al. 2016). However, measuring educational attainment in this way conflates the two dimensions that we seek to disentangle: the amount of education an individual woman has achieved, and the prevalence of formal schooling in her birth cohort. Instead, we measure individual educational attainment and cohort-level educational composition separately and use the interaction between the two measures to examine how the effect of individual-level educational attainment depends on the educational distribution of one's birth cohort.

Most of the scenarios described earlier hinge on the status or sociocultural norms associated with a given level of educational attainment. Because educational credentials convey more meaningful differences in status than do years of education, we model individual-level educational attainment using a credential-based categorical measure corresponding to four attainment categories that are socially and economically salient across sub-Saharan Africa: *no formal education* (did not attend school), *some primary* (attended but did not complete primary school), *completed primary* (completed primary school and may have attended secondary school), and *completed secondary* (completed at least secondary school). An additional benefit of this measure—as opposed to a numeric measure of years of education—is that it will not be affected by countries' differing years-to-credential requirements.[2]

To examine differences in educational opportunities across birth cohorts, we calculate the percentage ever educated: the percentage of women within each country-cohort who received any formal education. We identify three principal advantages to using percentage ever educated to represent differences in cohort-level educational composition. First, in its most basic form, the degree to which education is widespread within a population is represented by the proportion of women who have received any education at all. If the act of attending school—and not the knowledge therein attained—prompts changes in societal norms (Caldwell 1980), differences in educational attainment at the lowest end of the spectrum (no formal education vs. some) will be a more meaningful reflection of educational expansion than changes at the higher end (primary vs. secondary). Second, although our measure is highly correlated with mean years of education (R = .933), the mean years measure could be biased by heterogeneity in countries' years-to-credential requirements; such heterogeneity will not affect the percentage ever educated measure. Third, our measure is easier to interpret than more complex representations of the distribution of education within a population, such as the Gini coefficient for education (Qian and Smyth 2007; Sahn and Stifel 2003; Thomas et al. 2001). In practice, the Gini coefficient for educational attainment in this setting is nearly perfectly negatively correlated with the percentage ever educated (R = –.987).

Analysis

Decomposition analysis of temporal changes in the mean age at marriage may appear to be the best approach to examining how the relationship between marital behavior and individual-level educational attainment varies across the observed range of cohort-level educational access.[3] However, this approach obscures rather than clarifies the main question driving our analysis. Decomposition seeks independent contributions from composition and effects: it estimates the change over time in mean age at marriage that would be observed if only composition varied (holding the behavior of women in each

[2] Two countries in our sample implemented policy changes during the observation period that altered their years-to-credential requirements. In Kenya, an eighth year of primary school was added; in Ethiopia, years seven and eight, previously part of junior secondary school, were reclassified as primary school. We allow the relationship between years of schooling and educational attainment categories to change across cohorts in order to reflect these policy changes, while acknowledging that these changes may themselves influence our outcome.

[3] See Bongaarts et al. (2017) for a recent application of decomposition to examine marital age trends.

group constant) versus the change that would be observed if only behavior varied (holding the proportion in each group constant). In contrast, we are examining how changes in behavior are *contingent on* changes in composition. That is, we ask how the relative timing of marriage within each educational group varies systematically, according to the shifting cohort-level distribution of education.

To answer this research question, we instead use multilevel discrete-time hazard models (also known as Cox models or proportional hazard models) predicting age at first marriage, and we interact individual-level educational attainment with contextual-level educational access. Proportional hazard models assume that the risk of marriage for any individual depends on two factors: (1) an underlying, duration-dependent risk, and (2) the individual's values on a set of covariates (Allison 2014; Cox and Oakes 1984). These models account for censoring of first marriage, so we can include cohorts that have not yet completed their transition to marriage and draw comparisons, regardless of the proportion of never-married women. One trade-off for the flexibility offered by this framework is that the models assume proportional hazards; that is, effects of covariates estimated by these models are assumed to be constant across age.[4]

We first examine how the association between individual-level educational attainment and marital timing differs across the full spectrum of educational access. Because the model includes women from 30 countries born between 1941 and 1990 (Fig. 1), it measures the changing relationship both across space and over time. The model includes the cohort-level measure of percentage ever educated, the individual-level measure of educational attainment, and a cross-level interaction between these two variables, along with random effects for each five-year country-specific birth cohort (Level 2) and country (Level 3). We also present a version of this model with individual-level controls for birth year and rural residence.

Given the variability of levels and trends in educational access within sub-Saharan Africa, there is no reason to assume that a single scenario (convergent, divergent, or stable) will apply equally well to all countries or periods. Therefore, we perform additional analyses that isolate variation across space and over time. To isolate differences across space, we limit our investigation to the oldest and youngest five-year birth cohorts for which we have data for almost all countries (the 1956–1960 and 1981–1985 birth cohorts, respectively). By limiting our analyses to women born within a short timeframe, we exclude differences that may be due to the changes that a country has undergone over time. These models are identical to the full model described earlier except that because each country contributes only one birth cohort, they have only country-level random effects.

Finally, we isolate differences across time by comparing cohorts within each country and holding space constant. We do this by estimating separate models for each of the 30 countries in our sample. We employ multilevel discrete-time hazard models (estimated separately for each country), with random effects for each five-year birth cohort, our

[4] We tested this assumption separately for each country using Schoenfield residuals for individual-level educational attainment. Of 90 country-specific education categories, this assumption was violated nine times. In these nine cases, graphs revealed that the education-level residual trend lines did not cross one another and were sloped only at extreme ends of the marital age distribution (less than 14 and greater than 35). Thus, even in these cases, the estimated parameters still yield a weighted average of the effect over most of the observed age range but will conceal how the effect differs for very early or delayed marriage ages (Lundborg et al. 2016).

cohort-level measure of educational composition, individual-level educational attainment, and an interaction term between cohort-level educational composition and individual-level educational attainment.

We display the results of our models using graphs of hazard ratios for each educational attainment category across the range of educational distributions. To ensure meaningful comparisons across models with different ranges of educational access, we estimate hazard ratios relative to the lowest observed level of educational access for each analytic sample. All models are estimated using the *coxme* package in R (Therneau 2012).

Robustness Checks

Important spatial heterogeneity likely exists within countries. In particular, educational experiences often differ widely for urban versus rural residents of the same country. To examine how educational context may operate differently between urban and rural areas, we estimate a version of the model described earlier, with our educational access measure and cohort-level random effects estimated separately for urban and rural women in each cohort. In other words, rather than pooling women within country-cohorts, we calculate the percentage ever educated for urban versus rural women within each cohort and include random effects at the country-urban/rural cohort level (see online appendix 1).

Age at first marriage is reported retrospectively in the DHS, which can cause accuracy to decline with respondent age (Neal and Hosegood 2015). However, researchers can limit associated bias by constraining the age range of women analyzed (Koski et al. 2017). To examine whether the broad age range of women in our models (25–49 years) influences our results, we estimate a version of the full-sample model in which women's ages are constrained to 25–34 years (see online appendix 2).

In our models examining differences across space (i.e., the within-birth cohort models), the cohorts represent slightly different sets of countries. To ensure that the differences that we detect between these two cohorts are not due to differences in sample representation, we run a version of the models limiting the analysis to women from countries that have data for both the earlier and later cohorts (see online appendix 3).

Results

Educational Distributions Across Countries and Over Time

Figure 2 shows each country's trajectory over time in the percentage of women ever educated, from the oldest to the youngest birth cohorts available. The fact that our sample encompasses the full range of this variable, from a low of 1.9 % (Niger's 1941–1950 birth cohort) to a high of 99.3 % (Zimbabwe's 1986–1990 birth cohort), underscores the astounding heterogeneity in educational access that sub-Saharan Africa has experienced in recent decades. Educational access increased over time in all countries (i.e., net positive slopes, Fig. 2). However, we do not find evidence that the *gap* in educational access across countries has closed over time; in fact, the standard deviation of countries' percentage ever educated is slightly larger in more recent cohorts than in

earlier cohorts (e.g., 24.8 for women born 1955–1960 vs. 26.0 for women born 1981–1985).

The degree of educational progress has been regionally heterogeneous, and many of the greatest improvements in educational outcomes (i.e., steepest slopes in Fig. 2) occurred in East Africa. Meanwhile, the educational outcomes for many West African women today lag behind those of Central and Southern African women a generation ago. Within regions, countries differ widely in the magnitude of change over time. For example, in West Africa, some countries, such as Mali and Chad, have experienced relatively little change in the percentage ever educated across cohorts; others, such as Liberia and Togo, have experienced more substantial increases in educational access. Among countries that experienced dramatic increases, we also observe differences in the timing of these changes. In Tanzania, Zimbabwe, and Nigeria, the greatest improvements occurred for cohorts born in the 1950s and 1960s, and the trend lines subsequently flatten out. On the other hand, Sierra Leone, Malawi, and Uganda have all experienced rapid change in recent cohorts.

Educational Context and Age at First Marriage

Table 1 presents the results of our multilevel hazard model for the full 30-country sample, with and without adjusting for birth year and rural residence. To illustrate how differences in marital hazards by educational attainment vary systematically according to the educational contexts in which women are embedded, Fig. 3 displays education-specific hazard ratios across the full observed range of percentage ever educated. When access to educational opportunities is limited to a privileged minority, less-educated women tend to marry much earlier than more-educated women (i.e., less-educated women have a higher marital hazard; Table 1 and Fig. 3). In contrast, consistent with a

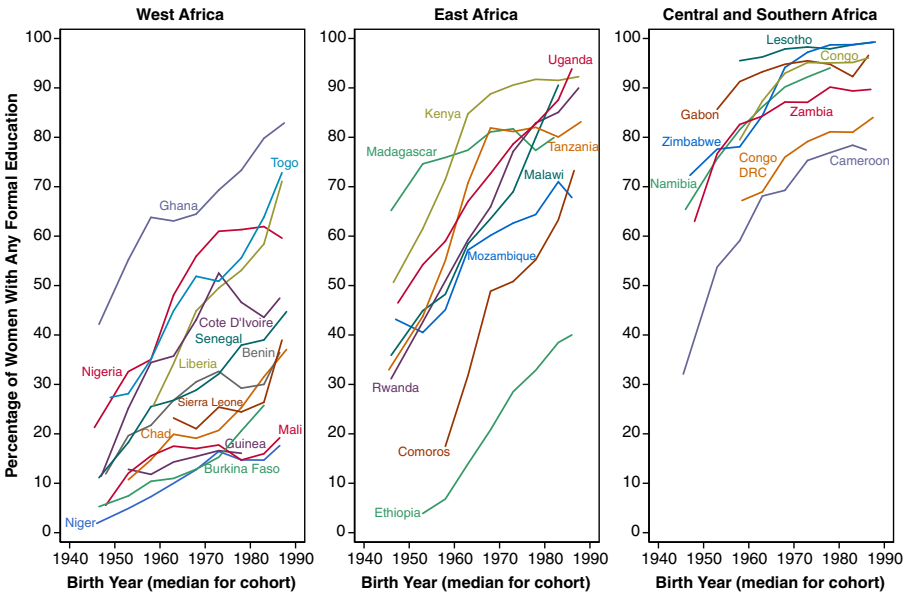


Fig. 2 Changes in the percentage ever educated within 30 sub-Saharan African countries over time

convergent scenario, in contexts where access to education is more prevalent—moving right along the *x*-axis of Fig. 3—less-educated women and better-educated women behave more similarly, on average. In other words, in high-education contexts, an individual's own educational attainment is a less salient predictor of her marital timing.

The most notable differences in hazard ratios are among women with no formal education: marital patterns among the uneducated converge on those of well-educated women where educational opportunities are more widely available (see Table 1 and Fig. 3), supporting the hypothesis that mass education is associated with social conditions that impact even the uneducated through a bandwagon effect (*Scenario Conv-1*). On the other hand, the marital hazard ratios for women with completed secondary and completed primary education—the bottom two lines in Fig. 3—are much less sensitive to variation in cohort-level educational distributions. The results of the full model are virtually identical when run with controls for rural residency and birth year; this iteration of the model additionally shows that rural status is associated with significantly higher marital hazards,[5] and later birth year is associated with significantly lower marital hazards (Table 1).

To aid interpretation, hazard ratios are often translated into more substantively meaningful terms through survival estimates (for example, the age at which 50 % of women with a particular set of covariates are predicted to have married). Because of the multilevel structure of our analysis, we cannot calculate survival estimates for the results presented in Table 1. However, to render these results more intuitive and to connect them to marriage ages, we present a similar model that does allow for the presentation of survival estimates.[6] According to this model, in the lowest-education cohort, one-half of women with no formal education are predicted to marry by age 16, and one-half of women with completed secondary education are predicted to marry by age 24, resulting in a gap of eight years between the two educational categories. In the highest-education cohort, these numbers are 19 and 23, respectively, summing to a gap of only four years.

As access to education increases, convergence of uneducated women's marital timing upon that of highly educated women could be driven by various social processes. This pattern could possibly be due to declines in very early marriage—defined here as marrying before age 15—among uneducated women (Dixon-Mueller 2008; Singh and Samara 1996). Alternatively, convergence could be due to postponed marriages among somewhat older uneducated women (e.g., marrying in their early 20s rather than their late teens). Finally, it could be due to the exclusion of uneducated women from the marriage market altogether, perhaps because having never attended school is an increasingly salient indicator of relative disadvantage in contexts where education is more widely available.

To investigate this question, we isolate women with no formal education living in contexts that fall within the two extremes of educational access: those country-cohorts with less than 10 % ever educated (lowest-education contexts, $N = 4{,}445$ uneducated women) and those country-cohorts with greater than 90 % ever educated (highest-education contexts, $N = 3{,}392$ uneducated women). First, we examine the proportion of

[5] For a more extensive investigation of differences by urban/rural status, see online appendix 1.

[6] This model, which estimates clustered standard errors at the country-cohort level but does not include frailty terms for countries or cohorts, is presented in online appendix 4. The mean absolute difference in hazard ratios between our main model and this alternate specification is very small (0.01), so survival estimates from this model are instructive for understanding our main results.

Table 1 Results for full-country hazard model (with and without controls) and two cohort-specific models

	Full Model (all birth cohorts)			Full Model, Controls (all birth cohorts)			Earlier Cohort (1956–1960 birth cohort)			Later Cohort (1981–1985 birth cohort)		
	Coef. [$e^{Coef.}$]	SE (Coef.)	Sig.	Coef. [$e^{Coef.}$]	SE (Coef.)	Sig.	Coef. [$e^{Coef.}$]	SE (Coef.)	Sig.	Coef. [$e^{Coef.}$]	SE (Coef.)	Sig.
% Women Ever Educated	–0.006	0.001	***	–0.004	0.001	***	–0.007	0.002	**	–0.004	0.002	
	[0.994]			[0.997]			[0.993]			[0.996]		
Education (ref. = none)												
Some primary	–0.393	0.009	***	–0.329	0.009	***	–0.227	0.025	***	–0.499	0.021	***
	[0.675]			[0.720]			[0.797]			[0.607]		
Completed primary	–0.894	0.009	***	-0.795	0.010	***	–0.708	0.026	***	–1.003	0.023	***
	[0.409]			[0.452]			[0.493]			[0.367]		
Completed secondary	–1.676	0.017	***	–1.561	0.017	***	–1.187	0.047	***	–2.048	0.040	***
	[0.187]			[0.210]			[0.305]			[0.129]		
Rural (ref. = urban)				0.169	0.003	***						
				[1.184]								
Birth Year (ref. = 1970)				-0.003	0.001	***						
				[0.997]								
Interaction Terms												
% ever educated × some primary	0.003	0.000	***	0.003	0.000	***	0.003	0.001	***	0.005	0.000	***
	[1.003]			[1.003]			[1.003]			[1.005]		
% ever educated × completed primary	0.006	0.000	***	0.005	0.000	***	0.005	0.001	***	0.007	0.000	***
	[1.006]			[1.005]			[1.005]			[1.007]		
% ever educated × completed secondary	0.007	0.000	***	0.007	0.000	***	0.005	0.001	***	0.011	0.001	***
	[1.007]			[1.007]			[1.006]			[1.011]		

Table 1 (continued)

	Full Model (all birth cohorts)			Full Model, Controls (all birth cohorts)			Earlier Cohort (1956–1960 birth cohort)			Later Cohort (1981–1985 birth cohort)		
	Coef. [$e^{Coef.}$]	SE (Coef.)	Sig.	Coef. [$e^{Coef.}$]	SE (Coef.)	Sig.	Coef. [$e^{Coef.}$]	SE (Coef.)	Sig.	Coef. [$e^{Coef.}$]	SE (Coef.)	Sig.
Variance Components												
Country-cohorts	0.0080			0.0100								
Country	0.0789			0.0640			0.0908			0.0758		
Number of Women	721,887			721,887			58,511			98,617		
Number of Country-Cohorts	246			246			—			—		
Number of Countries	30			30			29			28		
Chi-Squared (integrated)	148,958			152,509			10,580			23,235		
Degrees of Freedom	9			11			8			8		

**$p < .01$; ***$p < .001$

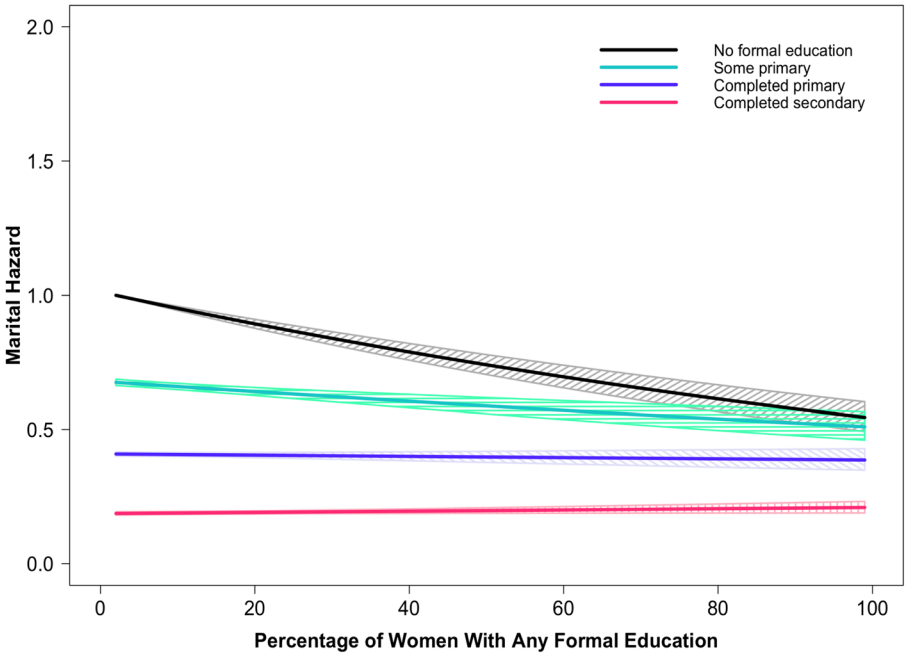


Fig. 3 Convergence of marital hazard ratios in the full model. Clouds represent 95 % confidence intervals. Confidence is 100 % at the baseline ratio (no formal education, lowest observed coefficient for percentage ever educated) for each model. The range of coefficients for percentage ever educated reflects the observed country-cohort range for our sample. The full model (plotted here without controls for birth year or urban/rural status) depicts convergence that operates across space and/or over time. Because the trend is primarily driven by the behaviors of uneducated women, we see this convergence as consistent with the *bandwagon effect*, in which uneducated women adopt the behaviors of well-educated women in cohorts with high proportions of educated women

women who married before age 15 and find dramatic differences between these two sets of country-cohorts: 35 % of uneducated women in the lowest-education cohorts report very early marriage, compared with only 18 % of uneducated women in the highest-education cohorts. Second, we examine the difference in the average age at marriage among women who marry at age 17 or higher, and we find that this number is quite similar across the two groups: 20.1 years among uneducated women in the lowest-education cohorts and 20.7 among uneducated women in the highest-education cohorts. Finally, we examine the difference in the proportion of uneducated women who report having never married by age 30, and we find that this proportion is very low in both groups: 0.1 % for the lowest-education cohorts and 1.2 % for the highest-education cohorts. Together, these results suggest that the observed bandwagon effect among uneducated women in higher-education contexts is largely due to a decline in very early marriages.

There may also exist an age beyond which marriage is not a viable option for women, presumably because childbearing is a principal goal of marriage. More-educated women tend to marry later than less-educated women, but a ceiling effect might limit their ability to delay marriage indefinitely, thereby closing the gap between marital ages of the best- and least-educated women in a cohort. Such an effect could lead to convergence (if the age ceiling were strongest in high-education contexts) or divergence (if the opposite were

true). Among highly educated women aged 30 or older, the percentage who married before age 30 was 85 % in those country-cohorts with less than 20 % ever educated (low-education contexts, $N = 1{,}206$) and 83 % for country-cohorts with greater than 90 % ever educated (highest-education contexts, $N = 7{,}070$). We also compared the 90th and 95th percentiles of marital age among all highly educated married women in high-education versus low-education contexts; both pairs of percentiles were within one year of each other. Because the distribution of delayed marriage among highly educated women is so similar in both educational contexts, we conclude that any sort of ceiling effect operates to a comparable degree at both extremes of educational access and is not driving the convergence documented in Fig. 3.

To investigate how the relationship between marital age and educational attainment varies *across space,* we estimate our multilevel hazard model separately for an earlier and later cohort of women, born from 1956–1960 and from 1981–1985, respectively (Table 1 and Fig. 4). As in the full model, we identify strong convergence within both the earlier and later cohorts. Removing time trends from the model—and, therefore, drawing our comparisons strictly across space—these models demonstrate that educational attainment plays a less important role in predicting marital timing in countries where educational opportunities are more widely available. The similarities between Fig. 3 and Fig. 4 (panels a and b) suggest that the full-model convergence is driven in large part by differences across space.[7]

Despite these broad similarities, we also note differences between the two birth cohorts that suggest macro-level changes *over time.* First, although the convergence among women born 1956–1960 is largely driven by the lower marital hazards of women with no formal education in countries where education is prevalent (bandwagon effect, *Conv-1*), among women born 25 years later, the convergence in marital timing is driven by behaviors of women at both ends of the educational spectrum. This later cohort model provides support for both a bandwagon effect (*Conv-1*) and a pattern in which primary and secondary school credentials are less salient markers of elite status (*Conv-2*) in contexts with more widespread educational access. Second, Fig. 4 illustrates a more prominent bandwagon effect (i.e., steeper convergence among uneducated women) in the earlier cohort than the later cohort. Third, the gap between educational categories is narrower for the earlier cohort across the full range of percentage educated.

Finally, we investigate within-country trends across educational access (and, roughly, over time) by estimating hazard models separately for each country. In contrast with our identification of strong convergence across space, considerable heterogeneity exists within countries over time, with divergence being the most frequently observed pattern. To provide a visual depiction of differing within-country patterns, Fig. 5 displays the hazard ratios over time for uneducated and highly educated women in four countries exhibiting contrasting trends in marital timing across educational context. Panel a shows that as educational access has increased in Malawi, the marital hazard ratio of uneducated women has increased, but the marital hazard for women with completed secondary education has remained constant. This is consistent with the hypothesis that as educational opportunities become more widely available, having never attended school becomes a better marker of relative disadvantage (*Div-1*). Panel b shows that quite a different pattern of divergence has unfolded in Liberia: the marital hazard ratios

[7] This conclusion can also be drawn from the fact that in the full model, between-country variance is substantially larger than within-country, between-cohort variance (Table 1).

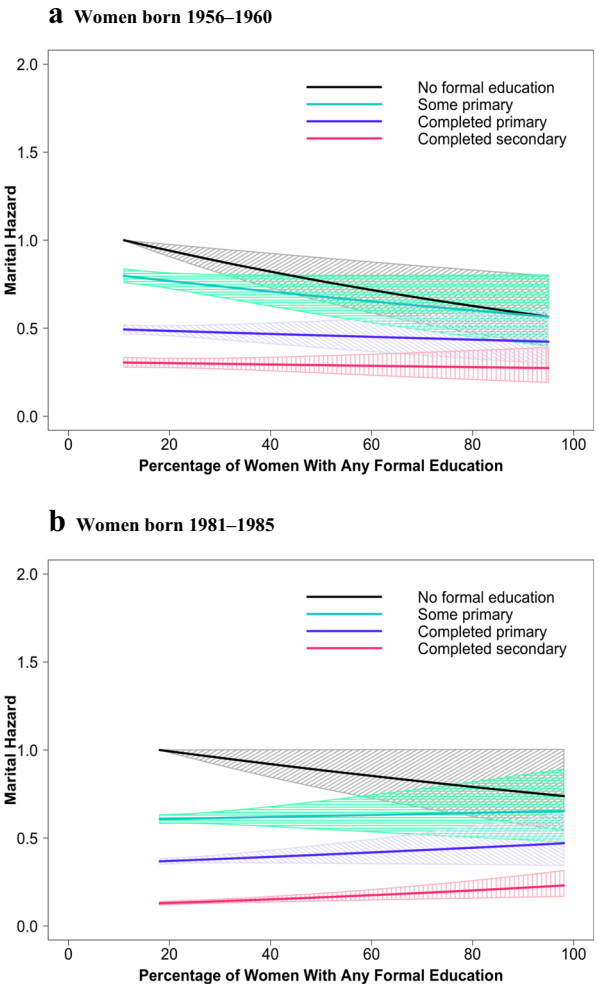


Fig. 4 Convergence of marital hazard ratios across space, for women born 1956–1960 and 1981–1985. Clouds represent 95 % confidence intervals. Confidence is 100 % at the baseline ratio (no formal education, lowest observed coefficient for percentage ever educated) for each model. Each figure's range of coefficients for percentage ever educated reflects the observed country-cohort range for women in that birth cohort. Both of these time-constrained plots (without controls for birth year or urban/rural status) depict convergence that operates across space. Convergence among the earlier cohort is primarily driven by the behaviors of uneducated women, consistent with the *bandwagon effect* (i.e., uneducated women's adoption of highly educated women's behaviors in countries with high levels of educational expansion). Among the later cohort, convergence is driven by the behaviors of the uneducated (consistent with a *bandwagon effect*) and the highly educated (consistent with our theory of education as a less effective marker of *elite status* in countries where high education is the norm)

of uneducated women have remained steady over time, but those of women with completed secondary education have decreased. This suggests a pattern whereby as more women have achieved higher educational credentials, a system of distinct social norms has developed, with highly educated women delaying marriage further into adulthood than they did when they were a narrower slice of the overall population (*Div-2*). Panel c shows that marital behavior in Nigeria has changed among both groups,

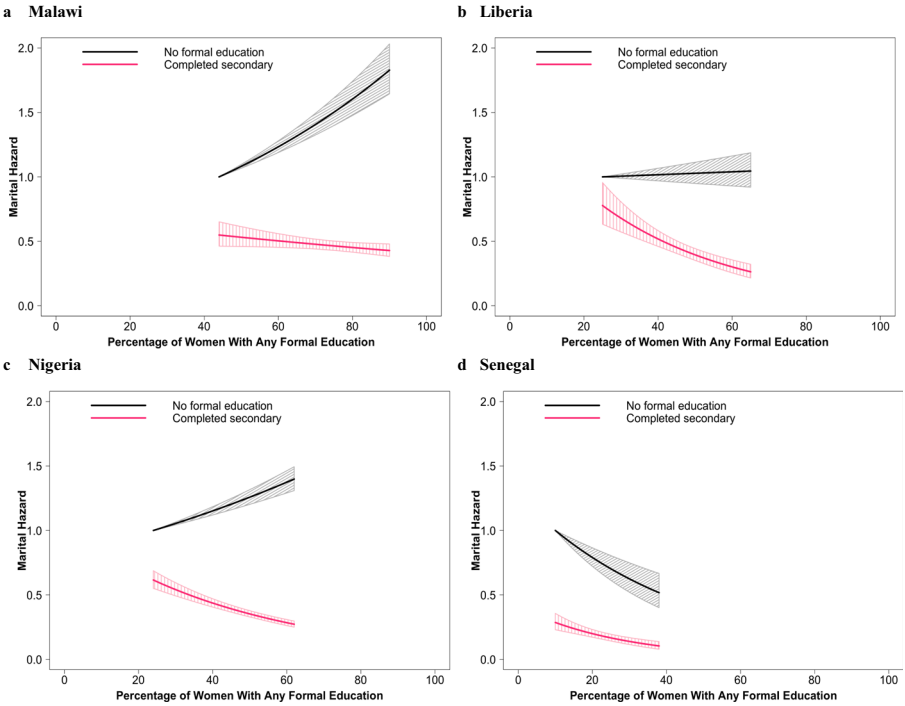


Fig. 5 Four countries' contrasting trends in marital hazard ratios over time. Clouds represent 95 % confidence intervals. Confidence is 100 % at the baseline ratio (no formal education, lowest observed coefficient for percentage ever educated) for each model. Each figure's range of coefficients for percentage ever educated reflects the observed range for women from that country. These within-country plots represent the iteration of the model without controls for birth year or urban/rural status. Divergence in Malawi is driven largely by women with no formal education, which we see as an indication that lack of education has become a more salient marker of *relative disadvantage* over time. In Liberia, divergence is largely driven by women who completed secondary school, consistent with the emergence of *distinct social norms* among the highly educated. In Nigeria, the divergence is driven by both groups, indicating a combination of these two effects. Convergence in Senegal is largely driven by women with no formal education, consistent with a *bandwagon effect*

suggesting a combination of the two divergence scenarios (*Div-1* and *Div-2*). And finally, panel d shows that Senegal has experienced convergence driven largely by the changing behavior of uneducated women—a bandwagon effect similar to what is observed in Figs. 3 and 4 when comparing across national contexts (*Conv-1*).

In Fig. 6, we provide a summary of the results of the full set of country-specific models. Of the 30 countries in our sample, 21 experienced divergence in the gap between marital ages for the highest and lowest educational groups (significant in 11 countries), whereas 9 countries experienced convergence (significant in only 2 countries). Countries in Fig. 6 are sorted by the magnitude of the change in the gap in marital hazard ratios between the least- and most-educated women and by the educational group primarily responsible for the change: countries that experience the most extreme divergence or convergence are located at the figure's poles, and countries that experience comparatively large changes in the group with no formal education are located at the top portion of the figure. The closed circles represent hazard ratios for the cohort with the lowest percentage ever educated, and the open circles represent the hazard ratios for the cohort with the highest percentage ever

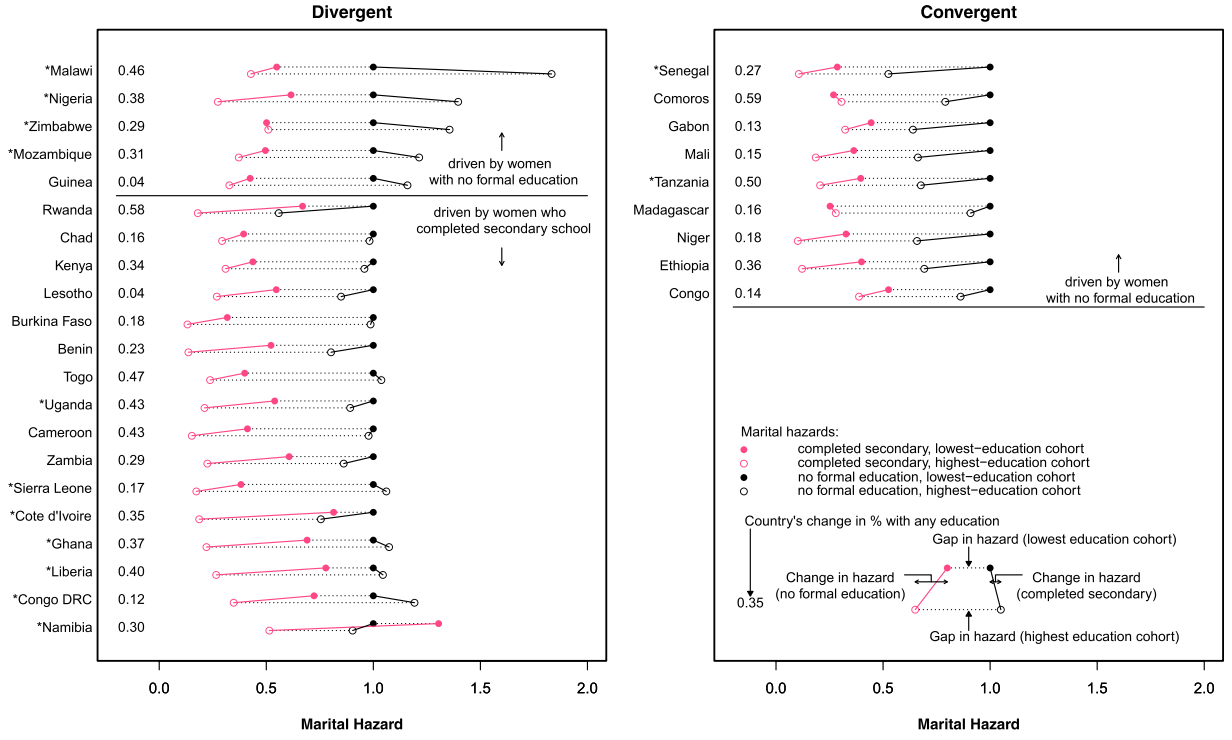


Fig. 6 Differing within-country patterns of marital hazard ratios over time. These values represent the iteration of the model without controls for birth year or urban/rural status. Countries are sorted by their degree of divergence or convergence over time, such that the countries with the greatest changes are positioned at the figure's poles. Asterisks before country names represent significant divergence or convergence, calculated by comparing the gaps between educational groups—represented by dotted lines in the figure—for the highest-education and lowest-education cohorts ($p < .05$, two-sided tests)

educated. With few minor exceptions, the lowest percentage ever educated corresponds to the earliest observed cohort, and the highest percentage ever educated corresponds to the most recent observed cohort (Fig. 2), so these trends can be interpreted as unfolding from top row to bottom row over time.

Comparing the within-cohort distance between the dots in each country's top and bottom rows provides information about patterns of divergence or convergence. For example, in Malawi, Zimbabwe, and Mozambique, we see strong divergence, with the gap in marital hazard ratios substantially larger in cohorts with more widespread educational opportunities (i.e., the closed circles are closer together than the open circles in Fig. 6). In these countries, this change is primarily driven by the marital behavior of the least educated becoming more distinct over time, likely indicating that lack of education has become a more salient marker of relative disadvantage as educational opportunities increase within the region (*Div-1*). In other divergent countries, on the other hand (e.g., Liberia, Ghana, and Côte d'Ivoire), the divergence is primarily driven by the behavior of the most-educated women, likely because the expansion of education comes with changing societal norms for the behaviors of well-educated women (*Div-2*). In Nigeria and the Democratic Republic of Congo, strong divergence is driven by both the most- and least-educated women in the population. Finally, in the model for Namibia, the line describing the marital behavior of highly educated women crosses that for uneducated women, indicating a comparatively high marital hazard among the highest-educated women in the earliest observed cohort.[8]

On the right side of Fig. 6, in countries demonstrating convergence, the pattern is primarily driven by the behaviors of women with no formal education. Thus, as in the multi-country models, convergence appears to be largely driven by the *bandwagon effect*, with increasing educational prevalence leading to later marital timing among women with no formal education (*Conv-1*). With the exception of Zimbabwe, Madagascar, and Comoros, highly educated women on both sides of the figure delay marriage more in higher-education settings, a likely reflection of the changing societal norms for the behaviors of well-educated women, as described in *Div-2* earlier.

Finally, some countries display virtually no divergent or convergent trends, despite substantial increases in educational opportunities over time (e.g., Rwanda, Chad, Niger, Ethiopia, and Congo; Fig. 6). In these countries, marital hazard ratios have decreased to varying degrees among both educated and uneducated women as educational opportunities have become more widely available, as evidenced by a leftward shift of both sets of dots; however, the gap between these different levels of educational attainment has changed little. This pattern supports the hypothesis that the difference in marital timing between highly educated and uneducated women does not depend on the overall distribution of educational opportunities.

Not surprisingly, a positive association exists between countries' level of educational access and the degree of convergence or divergence. Of the eight countries remaining in the lowest range of the *y*-axis in Fig. 2 (e.g., Chad, Guinea, Mali, and Niger), only Sierra Leone exhibits significant convergence or divergence. On the other hand, among those countries that have reached higher levels of educational access, substantial changes in

[8] We find this result puzzling, but we suggest two possible explanations. First, our data reach back further in time for Namibia (to women born in 1942) than for most countries, and educational differences may have been less distinguishing in this era. Second, this earliest cohort of women contains only 34 women with completed secondary school, so the estimate is based on an unusually small cell size.

hazard ratios are observed more frequently, with significant convergence or divergence in 12 of the 22 countries (e.g., Liberia, Malawi, and Congo DRC). These findings suggest that dramatic changes in the relationship between educational attainment and marital patterns will not occur in the absence of widespread educational access.

How does one reconcile the strong convergence in the full model and the largely divergent results of the country-specific models? Convergence tends to exist across space (i.e., the marital timing of uneducated women is particularly early in low-education countries), whereas divergence has unfolded over time. Indeed, a comparison across the panels of Fig. 4 reveals that convergence is steeper in the earlier cohort, and the gap between educational categories is wider across the full range of percentage educated for the later cohort. Because many countries have undergone divergence over time, the association between timing of marriage and educational attainment is more of a distinguishing factor for the younger generation than it was for the prior generation.

Discussion and Conclusions

In our full model and cohort-specific models, we find strong empirical support for the bandwagon effect: in contexts with widespread access to educational opportunities, the marital timing of women with no formal education converges on that of their more-educated peers. These results suggest that in contexts where education is prevalent, the social correlates of education spill over into the segments of populations that schools don't reach, thereby shaping life course transitions. Caldwell made a similar case for the impact of mass education on fertility behavior: as education became a widespread phenomenon, he argued that the adoption of a new familial culture was not proportional to one's own level of educational attainment. Rather, Caldwell wrote, "Once the change has taken place, it tends to become universal, and educational differentials are of little importance" (1980:231).

Does this bandwagon effect suggest that even uneducated women are better off in contexts where more women go to school? In other words, is delayed marriage a social good for women who have never attended school? We hesitate to make normative claims based on observed trends in the timing of marriage across diverse social contexts. For instance, a pattern of accelerated marriage for uneducated women might be an optimal outcome if they are socially isolated, have limited opportunities for employment, and would otherwise add to already strained resources in their natal households. However, our finding that this shift is primarily driven by a decline in very early marriages leads us to conclude that uneducated women benefit from living in contexts with expanded access to schooling, at least in terms of marital timing. Very early marriage is widely recognized as having negative consequences for physical and emotional health and social well-being, regardless of context (Dixon-Mueller 2008).

Looking ahead, can we expect this pattern of convergence in marital patterns across countries to persist? It is difficult to predict, especially in light of the spread of universal primary education policies across Africa over the past two decades. Beginning with Malawi in 1994, 40 countries in sub-Saharan Africa have eliminated school fees at the primary level, causing school enrollment rates to surge (Langsten 2017; Nishimura and Byamugisha 2011; World Bank 2009). Although researchers have used the early adopters of this policy change (Malawi, Uganda, and Ethiopia) to show that increased access to schooling has reduced women's ideal family size (Behrman 2015) but has not

increased age at first birth (Grant 2015), no study has yet examined the effect of these policies on marital timing. We point to this as a promising area for future research, particularly as the first cohorts exposed to free primary schooling enter adulthood over the next decade.

Because our study ends with cohorts born in the late 1980s, it excludes most of the women and girls who were affected by the continent's changes in educational policy. Our results may therefore capture trends specific to a particular era of African educational expansion and may not be reflective of more enduring social processes; however, we venture a prediction. In the short run, we can expect these policies to increase the regional contrasts in educational access that we document in Fig. 2, given that most Eastern and Southern African countries eliminated school fees before most West African countries (Nishimura and Byamugisha 2011). Longer term, these policies might lead to convergence via the bandwagon effect: as schools penetrate farther into remote areas, uneducated women might be exposed to educational spillover effects (Nishimura et al. 2008; Theunynck 2009). However, if the overcrowding and poor-quality instruction associated with these policies (Langsten 2017) decrease the impact of attending only some primary school on girls' behaviors later in life, more pronounced status distinctions—and consequently divergent marital behaviors—might emerge for women with intermediate categories of educational attainment.

Our country-level findings appear to be largely consistent with Bongaarts et al.'s aggregated identification (2017) of divergence over time in Central/Western Africa and stability in Eastern/Southern Africa. However, our analyses also reveal remarkable heterogeneity within African regions, which is otherwise masked by an aggregated analysis. This heterogeneity exists both in terms of trajectories in access to education over time (Fig. 2) and in associations between marital timing and educational context (Fig. 6).

Despite the varied trajectories for marital timing amid educational expansion, we note some regional similarities in the results of our within-country models (Fig. 6). First, countries in the Great Lakes region of East Africa (including Kenya, Tanzania, Rwanda, Ethiopia, and Uganda) show parallel downward-sloping trends in marital hazard among both uneducated and highly educated women, suggesting *stability* over time in the association between relative marital timing and educational attainment. Second, in the South-Eastern African countries of Malawi, Mozambique, and Zimbabwe, we see divergence driven by the behavior of uneducated women, suggesting that lack of education has become an increasingly salient marker of relative disadvantage in these countries (*Div-1*). On the other hand, countries that experienced divergence driven by the behavior of highly educated women (*Div-2*) are primarily located in West Africa (e.g., Benin, Burkina Faso, Chad, Cameroon, Côte d'Ivoire, Ghana, Liberia, Sierra Leone, and Togo), with Zambia and Lesotho standing out as clear exceptions to this pattern. Finally, the only two countries that experienced convergence driven by both educated and uneducated women are the island nations of Madagascar and Comoros. Although identifying the mechanisms driving these regional differences is beyond the scope of this study, we suggest this as a fruitful area for future research.

Our results are descriptive, and we have focused on mapping out broad macro-level patterns in the associations between individual-level educational attainment, aggregate-level educational conditions, and the timing of marriage. In contexts where education is prevalent, we do not know what specifically leads uneducated women to exhibit a delayed pattern of marriage compared with contexts where attending school is limited

to a privileged minority. Although considerable evidence supports Caldwell's (1980) assertion that increasing opportunities to attend school are a key driver of family transitions (for a recent summary of this literature, see Bongaarts et al. 2017), the trends we observe might also be driven by infrastructural, economic, or cultural transformations that occurred concurrently with increased educational access. Recognizing that these effects operate in tandem—and that the correlation between these other factors and education may vary over time and space—it is not our goal to isolate or quantify the predictive power of education in particular, absent the broader economic and sociocultural context. Instead, this project serves to quantify marital differences across educational groups in relation to cohort differences in educational opportunities. However, if a future investigation were to use a different measure of development (e.g., percentage urbanized, per capita GDP) as its contextual-level variable, a comparison of that study's findings with our own could elucidate what happens when education expands in the absence of economic change, and vice versa.

In all scenarios, low marital hazards do not necessarily represent a concerted decision or preference to delay marriage. Instead, later marriage could represent women's inability to locate suitable partners or families' inability to pay dowries. Although a compelling area for future research, analysis of women's failure in the marriage market is beyond the scope of this study.

Likewise, it is beyond the scope of our study to adjudicate how education shapes marital behavior—whether directly through the skills or knowledge imparted through school, or indirectly through the status that a particular attainment level conveys. Our finding that educational prevalence shapes relative differences in marital timing between educational groups demonstrates that schooling operates at least in part by drawing symbolic distinctions between different socioeconomic strata. Indeed, it would be difficult to explain this finding if schools merely imparted human capital. Nonetheless, education likely operates simultaneously as a marker of status and a stock of skills and knowledge.

The models presented herein represent the best fit across the full data set, but they may not be equally accurate representations of differences in marital behavior by educational attainment for all cohorts. This could be due to nonlinear relationships between education and marriage or to heteroskedasticity in educational attainment and marital timing across level of educational access. Moreover, despite the best efforts of the DHS to collect data that are comparable across time and space and that accurately represent the underlying population, variations in measurement may have influenced our results as a result of the repeated cross-sectional nature of the data.

Our focus on country-level birth cohorts limits our ability to assess finer-scale dimensions of educational distributions that could differentially affect the social meaning of education. National contexts, or even urban-rural contexts (see online appendix 1), are coarse assessments of the educational composition of women's peer communities, and the broad changes likely mask considerable heterogeneity within countries. Historical patterns in educational investment and current political dynamics have led to differences across subnational regions in access to education and school quality. Although sample size restrictions limited our ability to assess regional differences within country-cohorts, future research should examine these dynamics at the subnational level.

Marriage markets are complex, and the suitability of a given pairing is strongly affected by the educational attainment of each partner and social norms regarding

assortative mating (Blossfeld 2009; Gage and Bledsoe 1994; Schwartz and Mare 2005). Furthermore, broad educational conditions, such as the ratio of men to women with complementary levels of educational attainment, will affect a woman's ease of finding a good match (Mu and Xie 2014; Raymo and Iwasawa 2005). Our focus on women's schooling, without accounting for men's education, limits our ability to understand the contextual effects of educational access on marriage market dynamics. Although it is outside the scope of this article, we recognize the importance of investigating African assortative marriage markets amid changing educational distributions for both sexes, and we identify this as a valuable area for future research.

To our knowledge, this study is the first to examine how the substantial heterogeneity in educational expansion in sub-Saharan Africa is associated with differences in the relative timing of marriage between women with different levels of educational attainment. Conceiving of exposure to schooling as both an individual characteristic and an aggregate social phenomenon, we show that the distribution of educational opportunities within birth cohorts shapes the relationship between an individual's educational attainment and her other life events.

Acknowledgments We are grateful for helpful feedback from Sarah K. Cowan, Amal Harrati, Jennifer Johnson-Hanks, Jayanti Owens, Hyunjoon Park, Will Lowe, and Kenneth Wachter. Previous versions of this research were presented at the Population Association of America Annual Meeting (April 2017, Chicago, IL), the International Union of the Scientific Study of Population Annual Meeting (October 2017, Cape Town, South Africa), and the African Studies Works in Progress Series at Princeton University (April 2017, Princeton, NJ); and benefitted greatly from suggestions from members of the audience. Research reported in this publication was supported by the Eunice Kennedy Shriver National Institute of Child Health & Human Development of the National Institutes of Health under Award Number P2CHD047879. The content is solely the responsibility of the authors and does not necessarily represent the official views of the National Institutes of Health.

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