Differential Fertility, Human Capital, and Development

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Abstract

Discussions of cross-sectional fertility heterogeneity and its interaction with economic growth typically assume that the poor have more children than the rich. Micro-data from 48 developing countries suggest that this phenomenon is very recent. Over the second half of the twentieth century, these countries saw the association of economic status with fertility and the association of the number of siblings with their education flip from generally positive to generally negative. Because large families switched from investing in more education to investing in less, heterogeneity in fertility across families initially increased but now largely decreases average educational attainment. While changes in GDP *per capita*, women's work, sectoral composition, urbanization, and population health do not explain the reversal, roughly half of it can be attributed to the rising aggregate education levels of the parent generation. The results are most consistent with theories of the fertility transition based on changing preferences over the quality and quantity of children, and somewhat less so with theories that incorporate subsistence consumption constraints.

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1 Introduction

From the moment the early greats of statistics defined the concept of correlation, they expressed concern that the negative correlations between various desirable attributes and fertility spelled the doom of the human race. Francis Galton, Karl Pearson, and Ronald Fisher, not to mention their many peers in the field of eugenics, all argued that the higher fertility rates of the poor implied the genetic deterioration of humankind.[1] Over the next century, the pattern of 'differential fertility' between the rich and poor, between the literate and illiterate, and between the intelligent and feeble-minded caused much concern about the evolution of average traits among humans.

While the genetic theory has largely gone out of vogue, economists and other social scientists have continued to study how differential fertility affects aggregate outcomes.[2] Among modern economists, this interest dates back to Kuznets (1973), who suggested that differential fertility adversely affects both the distribution and the growth rate of income. A long line of research since then has formalized and further developed these theories.[3] At the core of this literature is the observation that, in most present-day settings, wealthy parents have fewer children than poor parents, and they educate their children more. Compared to a population without heterogeneity in fertility rates, a population with greater fertility among the poor has a higher share of children from poor families, which lowers its average skill level in both the transition and the steady state. Some models also demonstrate how these fertility differences can give rise to poverty traps, thus widening inequality.[4] Much of this work posits that the excess fertility of the poor can help explain the growth experiences of developing countries over the 20^{th} century.

Drawing on extensive micro-data from 48 developing countries, this paper documents a history of differential fertility that contradicts this account of the growth process. In the not-too-distant past, the data exhibited a widespread *positive* correlation between economic status and fertility. This new evidence, which challenges conventional wisdom on heterogeneity in childbearing decisions,

[1]For histories of the eugenic perspective on differential fertility, see Kevles (1985) and Chapter 1 of Lynn (1996).

[2]Preston and Campbell (1993) note that in the presence of intergenerational mobility, the distribution of traits will eventually reach a steady state. While this steady-state distribution may depend on the extent of differential fertility, it also invalidates earlier arguments that differential fertility leads to the perpetual deterioration of average traits. Nevertheless, some authors (e.g., Lynn 1996) continue to argue that differential fertility has perpetual dysgenic effects.

[3]Key references include Althaus (1980), Dahan and Tsiddon (1998), Morand (1999), Galor and Moav (2002), Kremer and Chen (2002), de la Croix and Doepke (2003), and Moav (2005).

[4]Empirically, Lam (1986) documents that the effect of differential fertility on inequality is sensitive to the choice of inequality metric. His result does not necessarily overturn the general equilibrium reasoning of the more recent theories, but it does challenge much of the literature that preceded his paper.

has an important implication: until quite recently, cross-sectional heterogeneity in fertility rates promoted the growth of human capital instead of slowing it.

The notion that fertility once increased in income is not new, but the literatures on the theory of fertility and its interaction with the macroeconomy have focused disproportionately on the current regime in which it decreases in income.[5] A few gaps in the existing results may account for this oversight. To begin, the most systematic existing evidence for a positive fertility-income elasticity has emerged only recently and deals with England several centuries ago (Clark and Hamilton 2006; Clark and Cummins 2010). The extent to which the European patterns of the past apply to poor countries in the 20^{th} and 21^{st} centuries is not known. What little evidence exists on currently poor countries is scattered, mostly relying on small datasets from rural corners of the world, especially in Africa (Schultz 1986; Skirrbekk 2008).[6] More broadly, current research provides little guidance as to where, why, and how recently the relationship flipped from positive to negative.

We have similarly little information on how heterogeneity in fertility interacts with heterogeneity in skill investment during the fertility transition. This interaction lies at the crux of the effect of differential fertility on the *per capita* stock of human capital and is thus crucial to the predominant reasoning in the macroeconomic literature on differential fertility. A few fragments of data do suggest that the number of siblings may not have always been negatively associated with their education, as is widely observed today (Blake 1981; Steelman et al. 2002). The association flipped from positive to negative over the twentieth century in urban Indonesia (Maralani 2008), and it alternated between zero and less than zero in China since the Communist Revolution (Lu and Treiman 2008). A number of small, cross-sectional studies have found the association to be unstable in developing countries, particularly in Africa (Buchmann and Hannum 2001). Yet this work has not identified generalizable patterns in the evolution of the association, nor has it investigated the consequences for the human capital stock.[7]

This paper thus studies two closely-related associations: that between household economic status and fertility and that between sibship size and education. In the main empirical work, I use

[5]Nevertheless, in his early work on fertility, Becker (1960) expressed keen awareness that fertility may have once increased in income; he saw the current negative elasticity as puzzling. For more recent theoretical work seeking to explain the current fertility regime, see Jones et al. (2011).

[6]At the aggregate level, Strulik and Sikandar (2002) analyze a panel of countries and find no relationship between GDP *per capita* and fertility below a threshold level of income and a negative relationship above it.

[7]An important exception is Mare and Maralani (2006), who incorporate changes in the education-sibship size relationship into their analysis of the interaction of demography and social stratification in Indonesia. Also see Mare (2011).

data from the Demographic and Health Surveys (DHS) to form two generations of sibships: viewing survey respondents (who are women of childbearing age) as both parents and siblings. I first treat them as parents, using fertility history data to construct two cross-sections of families from 20 countries in the 1986-1994 and 2006-2011 periods. Between these periods, the relationship between parental economic status (measured by durable goods ownership) and the number of surviving children flipped from positive to negative in the African countries in my sample, as well as in the rural parts of Asia in my sample. The relationship was negative throughout in Latin America, leading one to wonder whether the fertility history data capture the tail end of a global transition in fertility regimes. Fortunately, the DHS sibling history data allow me to retrospectively construct a longer panel of families from 42 countries, and the results suggest exactly that sort of global transition. Among the earliest observed birth cohorts (mostly of the 1940s and 1950s), both the number of ever-born siblings and the number of surviving siblings are positively associated with years of education 21 countries but negatively associated in just two. In contrast, among the latest observed birth cohorts (mostly of the 1980s), 18 countries exhibit negative associations between both measures of sibship size and educational attainment, while just six show the opposite. Although the DHS does not offer much data on childhood economic circumstance, three supplementary datasets (from Bangladesh, Indonesia, and Mexico) suggest that one can attribute much of the reversal in the education-sibship size relationship to the reversal of the fertility-income relationship.[8]

After documenting these facts, the paper explores explanations for the reversal of differential fertility—both theoretically and empirically—and quantifies its effects on the skill distribution. A simple model of child quantity and quality investments suggests a few likely hypotheses while clarifying which ostensibly reasonable explanations do not work. Theory suggests that the most likely explanations include the rise of women's work, changes in the sectoral composition of the economy, the elimination of subsistence constraints, health improvements, and the evolution of preferences regarding the quantity and quality of children. In the data, however, changes in women's labor force participation, sectoral composition, and child mortality do not predict changes in the education-sibship size association. Instead, one variable stands out as having an especially important role in the reversal of the education-sibship size association: the average educational attainment of the previous generation. As the average education of the parents' generation increases,

[8]These supplementary data also show similar patterns for men, whom one cannot study in the DHS sibling histories.

the education-sibship size becomes more negative; the former can account for more than half of the latter.[9] This result dovetails with recent work by Murtin (forthcoming) showing that the level of schooling among adults is the most robust determinant of fertility decline in 70 countries over a 130-year period.

Together, these results point most strongly to preference-based explanations for the observed reversal. Several authors, most notably Caldwell (1980, 1982, see also Axinn and Barber 2001), argue that mass education induces widespread changes in fertility norms. If mass education (or, more generally, Western influence) increases the importance of child quality relative to quantity in the utility function, and if the preferences of the most educated couples are most sensitive, then the relationship between economic status and fertility can flip from positive to negative. An alternative explanation, which receives very mixed evidence in the data, involves subsistence consumption constraints. In the presence of these constraints, the relationship between income and fertility can follow a hump shape, so that a rightward shift in the income distribution flips the estimated slope of the relationship between economic status and fertility.[10] Two patterns in the data contravene the subsistence constraint hypothesis, although they do not entirely rule it out. First, although early data display a hump-shaped relationship between measures of economic status and the number of children, this hump disappears in more recent data. The theory would predict a time-invariant hump. Second, changes in log GDP *per capita* are uncorrelated with changes in the education-sibship size association, which is difficult to reconcile with a theory based on subsistence constraints. Even so, GDP *per capita* may be too transitory a measure of long-term family income; in this sense, the pronounced role of aggregate education trends may be consistent with subsistence constraints.

The paper concludes with a reweighting exercise to quantify how the evolution of differential fertility has influenced trends in average educational attainment. Treating all families of the same actual sibship size as a 'type,' I reweight the sample to ask what would transpire if all types of families had the same number of children, with no change to their educational decisions. This counterfactual is somewhat unnatural because it ignores parents' reoptimization as a consequence of changing fertility. But fertility is a choice variable, not an exogenous parameter, so any counterfactual simulation that directly manipulates fertility must involve a departure from theory. In

[9]Male and female education do not have significantly different effects, shedding further doubt on theories rooted in the empowerment of women.

[10]See Galor (2011) for a discussion of subsistence constraints and fertility.

fact, existing reweighting techniques, such as those in the labor economics literature (Blinder 1973; Oaxaca 1973; and DiNardo, Fortin, and Lemiux 1996), share this inattention to the endogenous responses of agents in the economy. Because the counterfactual deals solely with changes in the composition of a birth cohort, I call the difference between the observed and reweighted mean education levels the 'composition effect' of differential fertility.

As one might expect, the results of the reweighting exercise do not adhere to the theory that differential fertility between the rich and the poor depresses average skill. Only in two countries did differential fertility depress average education levels throughout the entire sample period. The remaining countries are split fairly evenly in two groups. In one, differential fertility elevated average education throughout the sample period, due to a consistently positive relationship between surviving sibship size and education. In the other, the influence of differential fertility changed over the sample period, typically starting positive and ending negative. The effects are usually less than half a year: moderate in comparison to the nearly four-year increase in average educational attainment over the sample period. But the effects are meaningfully large relative to the level of average education, especially among the early cohorts. For example, for women born during 1950-54, the reweighted counterfactual average differs from the actual average by 15 percent. Large or small, however, the results do not support claims that differential fertility is always bad for aggregate human capital and economic growth.[11]

At least since Becker (1960, 1981), economists have recognized that fertility may have once been positively correlated with income, but systematic evidence on the reversal of this relationship has emerged only recently, primarily for Western Europe.[12] The evidence in this paper suggests that a positive fertility-income gradient was prevalent in much of the developing world until fairly recently. This finding has implications for theories of fertility and the demographic transition, as well as for understanding the role of differential fertility in the process of growth. The basic time-series facts about long-run fertility decline are somewhat overdetermined, so a more thorough treatment of changing cross-sectional fertility patterns will help narrow the field of candidate theories of the fertility transition.

[11] The same conclusion arises in Galor and Moav's (2002) model of differential fertility and the evolution of preferences.

[12] Interestingly, eugenicists of the 19^{th} and early 20^{th} centuries also assumed (without much evidence) a past fertility regime in which fertility increased in social status. Galton (1869) believed that "civilization" tended to diminish the fertility among the better-off. Fisher (1930) even went so far as to characterize the excess fertility of the poor as an intrinsic feature of civilization that ultimately leads to its demise.

2 Two Generations of Sibships

I construct two generations of sibships by viewing respondents as both mothers and daughters. Most of the data offer two counts of fertility or sibship size: surviving children and all children ever born. Section 3, which presents basic facts, reports results for counts of both measures. Thereafter, the paper focuses on counts of surviving children for two reasons. First, surviving fertility is easier to interpret in most economic theories of the demand for children. Second, only surviving fertility is relevant for the distribution of skills among adults, which is the main concern of the literature on the aggregate effects of differential fertility.

2.1 Demographic and Health Surveys

Carried out in over 90 countries over the past three decades, the Demographic and Health Surveys interview nationally-representative samples of women of childbearing age (generally 15-49). All surveys include questions about the respondent's educational attainment and children; some also include questions about household durable goods ownership or the respondent's siblings.

2.1.1 Fertility Histories

The first set of analyses draws on the fertility histories, in which respondents list all of their children ever born, with information on survival. To avoid the complicated task of disentangling cohort effects from changes in the timing of childbearing, I focus on women at least 45 years old and interpret their numbers of children as completed fertility. The focus on older women also has the advantage of capturing cohorts of mothers more likely to be in the early regime in which fertility is increasing in economic status. To ensure similar measurement of household economic status across surveys, I also restrict attention to surveys containing questions about household ownership of five durable goods: radio, television, refrigerator, motorcycle, and car.[13] I compare results from two time periods, pre-1995 and post-2005, and only include countries with survey data from both periods, leaving me with 62,146 women from 46 surveys in 20 countries.

The use of consumer durables to measure wealth or economic status deserves further discussion. Annual income or consumption might seem to provide better proxies for long-run economic

[13] Many surveys also ask about bicycle ownership, but I omit this durable good because the presence of children may strongly influence the household's demand for it.

status, but unfortunately, these variables are not available in the DHS.[14] Existing work on the DHS has drawn extensively on durable goods ownership to measure economic status, much of it using the method proposed by Filmer and Pritchett (2001), which takes the first principal component of a vector of variables measuring housing conditions and ownership of several durable goods.[15] I modify this approach in two ways. First, I only use data on ownership of the five durable goods listed above. By not incorporating measures of housing conditions, I avoid the tasks of determining whether certain conditions (e.g., access to piped water) are individually or communally determined and whether these conditions directly influence fertility. Second, rather than using principal components analysis, I simply take the sum of a vector of ownership indicators. This index of economic status is comparable across countries and time periods, notwithstanding concerns about changes in relative prices.

2.1.2 Sibling Histories

The DHS began administering a sibling history module in the late 1980s for the purpose of estimating maternal mortality rates in settings with poor or absent vital registration systems (Stanton et al. 2000). The module asks respondents to list all children ever born to their biological mothers, with information on sex, year of birth, and year of death if no longer alive. Analyses of maternal mortality and all-cause adult mortality have since then drawn extensively on DHS data (e.g., Timæus and Jasseh 2004; Obermeyer et al. 2010). However, in addition to their intended value for mortality estimation, the sibling history data also offer a window into the sibling structure that adult women experienced as children.

As of December 2012, data from 89 DHS's with full sibling histories were in the public domain. Of these, seven (from Bangladesh, Indonesia, Jordan, and Nepal) included only ever-married women, introducing concerns about selection bias. From these surveys, I only include age groups in which the rate of ever marriage is at least 95 percent. Therefore, I include women over 30 from the relevant surveys in Bangladesh and Nepal, but I discard the 5 surveys from Indonesia and Jordan, where female marriage rates are lower.[16] I also discard data from the 1989 Bolivia DHS and the

[14]Montgomery et al. (2000) also point out that, due to transitory income shocks, income and consumption are not *a priori* superior to asset (or durable goods) indices.

[15]Young (2012) also proposes a method to use the growth of durables ownership to estimate consumption growth.

[16]Nepal has two surveys with sibling histories, one of ever-married women in 1996 and one of all women in 2006. I restrict the 1996 sample to women over 30, but I include all respondents to the 2006 survey.

1999 Nigeria DHS due to irregularities in the sibling history data, leaving 82 surveys for analysis.[17] Africa is overrepresented, a consequence of the near absence of systematic data on adult mortality in the continent prior to the entrance of the DHS. To limit the number of respondents who have not finished schooling or whose mothers have not completed childbearing, I drop data on women less than 20 years old, leaving a final sample of 803,527 women born between 1942 and 1989.

2.2 Supplementary Sibling History Datasets

The DHS data are useful in their breadth, allowing me to track the evolution of the education-sibship size relationship across roughly half a century of female birth cohorts in 40 countries. Yet they suffer from two major shortcomings. The most obvious is their omission of men, for whom the relationship of interest may be different. Additionally, they offer little information on aspects of the respondent's childhood environment, such as the economic status of her parents.

To supplement the DHS on these two fronts, I draw on three closely related Family Life Surveys: the Indonesia Family Life Survey (IFLS), the Matlab Health and Socioeconomic Survey (MHSS), and the Mexico Family Life Survey (MxFLS). All three surveys contain data on education and parental characteristics, and all three also include a sibling history module, although for two of the surveys, it only covers siblings who survived to adulthood. The IFLS is a panel study of a sample of households representing 83 percent of the Indonesian population; I use the 1993 and 1997 waves.[18] The MHSS, a representative sample of Matlab thana, a rural area in Bangladesh, fielded in 1996. And the MxFLS is a nationally-representative panel study, of which I use the 2002 wave. From each of the three surveys, I assemble a cross-section of adults born between 1940 and 1982.[19] The resulting samples are limited in size and lack information on deceased siblings, but they allow an exploration of gender heterogeneity and the role of parental covariates.

3 Changing Cross-Sectional Fertility Patterns

This section provides basic facts about the evolution of differential fertility patterns in developing countries over the second half of the twentieth century. In all of the analyses, I first separate the sam-

[17]In their analysis of adult mortality in the developing world, Obermeyer et al. (2010) also omit these two surveys.

[18]Most of the IFLS data come from the 1993 wave, but for individuals below age 20 in 1993, I use data from 1997 to maximize the likelihood that that they have completed their schooling.

[19]For the IFLS and MHSS, I relax the age lower bound to 18 to increase the number of individuals born in the 1970s.

ple into country-by-period cells and then estimate a simple mean or ordinary least squares (OLS) regression within each cell.[20] For any cross-country results, I then compute unweighted aggregates of the cell-level statistics.

3.1 Household Economic Status and Fertility: Evidence from Fertility Histories

To assess the evolution of the relationship between household economic status and fertility, I estimate separate country-level regressions for survey respondents aged 45-49 in the early (1986-1994) and late (2006-2011) DHS periods. For woman i in county c and period t (early or late), I run the following ordinary least squares (OLS) regression:

$$fertility_{ict} = \alpha_{ct} + \beta_{ct} index_{ict} + X'_{ict}\lambda_{ct} + \varepsilon_{ict} \tag{1}$$

where $fertility_{ict}$ denotes the woman's number of children (ever born or surviving), $index_{ict}$ denotes the durable goods ownership index (which varies between 0 and 5), and the vector X_{ict} contains age indicators and survey year indicators.

The main results for both ever-born fertility and surviving fertility appear in Table 1, which shows averages of the country-specific coefficients at the continent level. Panel A pools urban and rural areas, showing results both with and without controlling for an urban residence indicator. Panels B and C report results for solely urban and solely rural areas, respectively. A cross signifies that the late-period coefficient differs significantly from the early-period average coefficient. For each of the continent-by-period cells, Appendix Table 1 shows averages of the country-by-period means and standard deviations of the relevant variables.

The data reveal a reversal in the relationship of household economic status and surviving fertility: certainly for Africa and to some extent for Asia, but not for Latin America. In Africa, controlling for urban residence (Panel A2), each additional durable good is associated with one-fifth *more* surviving children in the early period but one-fifth *fewer* children in the late period. This flip is especially pronounced in rural areas (Panel C). Indeed, the same patterns hold in rural areas of the Asian countries in the sample, although not in urban areas of these countries. In the full

[20] Because the analyses aim to describe a (heterogeneous) equilibrium relationship, not to estimate a correctly-specified econometric model, I use sampling weights throughout the data work. Most DHS samples are self-weighting, so the results are not sensitive to the use of sampling weights.

Asian sample, controlling for urban residence, the durables index is uncorrelated with surviving fertility during the early period, but the association turns negative by the late period. All of these inter-period changes in coefficients are statistically significant at the 5 percent level. Note that the same patterns do not generally hold in Latin America, where the durable goods index negatively predicts surviving fertility in both the early and late periods. Nevertheless, in rural areas within Latin America, the relationship becomes significantly more negative over time. These results may suggest a shared process that operates at different times across and within countries: visiting urban areas before rural, and visiting Latin America before Asia and Africa.

When one counts all children ever born instead of only those that survived, the picture changes considerably. Survival rates are positively related to economic status throughout the sample period, which makes the ever-born coefficients more negative than the surviving coefficients. Indeed, the durables index is negatively correlated with ever-born fertility in all regions and time periods, although the relationship is small and statistically insignificant for rural Africa in the early period. Still, throughout Africa and Asia, the relationship becomes more negative between the early and late periods. Again, the sibling history results will help resolve whether the association of economic and ever-born fertility was positive at some time before the fertility history sample period.

The literature on the determinants of fertility has emphasized the fertility-limiting effects of parental education (Caldwell 1982), which bears a positive relationship with durable goods ownership. Appendix Table 2 explores the role of parental education, by adding husband's and wife's education to the covariates in the specification estimated in Table 1, Panel A1 (for the full sample). In Africa and Asia, the coefficient on the durables index still starts positive and becomes significantly more negative between the early and late periods. Interestingly, in Africa, it is *not* always the case that the education levels of a couple decrease their fertility. In the early period, the wife's education is not significantly associated with fertility, and the husband's education if anything bears a positively relationship with fertility.

3.2 Sibship Size and Educational Attainment: Evidence from Sibling Histories

The fertility history results provide fairly compelling evidence of a reversal in the relationship between economic status and surviving fertility in Africa and rural Asia, but they leaves several longer-run questions unanswered. Did the same reversal occur for counts of ever-born children at some earlier date? Did it occur in Latin America? The sibling histories offer a window onto the answers to these questions for birth cohorts going back to the 1940s. Unfortunately, the DHS's collect very little data on economic conditions in childhood. However, if we are willing to assume that educational attainment has always increased childhood household economic status, then we can infer the evolution of the income-fertility association from changes in the relationship between sibship size and education. In fact, just as important, the relationship between sibship size and education is directly relevant for assessing the effect of differential fertility on the skill distribution.

3.2.1 Main Evidence

To assess the evolution of the relationship between sibship size and education, I estimate separate country-level regressions for women born in 1940-1959, 1960-1969, 1970-1979, and 1980-1989. For woman i born in county c and time period t, I run the following OLS regression:

$$highest\ grade_{ict} = \alpha_{ct} + \beta_{ct} sibsize_{ict} + X'_{ict}\lambda_{ct} + \varepsilon_{ict} \tag{2}$$

where $highest\ grade_{ict}$ denotes the woman's highest grade completed, $sibsize_{ict}$ denotes the woman's sibship size, and X_{ict} is a vector of year-of-birth dummies. The organization of the data by birth cohort may seem unnatural because standard theories of fertility and educational attainment involve parents, not children, as decision-makers. Nonetheless, it is natural for the sibling data I employ; alternative ways of organizing the data lead to similar results.[21] The resulting estimates of β_{ct} can be interpreted as period, rather than cohort, measures of the equilibrium education-sibship size relationship. From this perspective, respondents born during period t are treated as representative of all children from a hypothetical group of families.

Figure 1 displays estimates of Equation (2) for up to four birth cohort categories from each country. The figure represents each coefficient as a circle, with the associated 95 percent confidence interval drawn around it. Estimates based on counts of ever-born siblings appear in blue, while those based on counts of surviving siblings appear in red.

Both the ever-born sibling and the surviving sibling coefficients tend to decrease across successive birth cohorts. For the earliest birth cohorts, most coefficients are significantly positive, while

[21] For instance, one can separate the sample by the years of the respondent's mother's first birth. This alternative separation of the sample produces qualitatively similar results.

for the latest birth cohorts, few coefficients are significantly positive, and many are significantly negative. Consistent with the fertility history results, this reversal in the education-sibship size relationship occurs earliest in Latin America, followed soon thereafter by several countries in Asia. In Africa, the reversal has been quite recent, and several countries remain in the pre-reversal regime. As the figure makes clear, attempts to characterize the education-sibship size relationship as generally negative miss a pervasive feature of recent demographic history.

Many theories of the demographic transition predict that parents choose their fertility to target the number of surviving children. A comparison of the ever-born sibling and the surviving sibling coefficients sheds light on this issue. Overall, the two sets of coefficients are similar, but the figure reveals some divergences. In particular, many of the surviving sibling coefficients exceed the ever-born coefficients for the same country and birth cohort. Figure 3 explores this pattern more carefully by graphing the surviving sibling coefficient against the ever-born sibling coefficient. The coefficients are the same as those in Figure 2, now plotted in x, y space. For ease of interpretation, the figure includes three reference lines: at $x = 0$, at $y = 0$, and on the 45° line.

The scatterplot in Figure 2 confirms the impression that the surviving sibling coefficients are on average larger than the ever-born sibling coefficients. But it also shows that the difference between the two coefficients is largest when the the coefficient on ever-born siblings is positive. This finding has two natural interpretations in the context of 20^{th}-century demographic history. First, since the child mortality rate was high in early cohorts, the variances of ever-born and surviving sibship size were most different during this period, possibly leading to a larger gap in coefficients. Second, surviving sibship size may have been a disproportionately strong proxy for parental economic status in the early cohorts. When rich parents have more children and healthier children, the relationship between parental wealth and surviving children will be larger than the that between parental wealth and ever-born children. Conversely, when wealthy parents have fewer children than poor parents, their children's lower mortality risk will make the surviving sibling coefficient less negative than the ever-born sibling coefficient. If mortality differences between rich and poor are greatest in the positive regime, then the difference between surviving and ever-born offspring will also be greatest.

3.2.2 Accounting for Birth Order

A large body of research posits that birth order affects educational attainment (Steelman et al. 2002; Black et al. 2005). Because birth order and family size are mechanically correlated, birth order effects may bias cross-sectional estimates of the effect of family size on education.[22] As a result, researchers are often careful to control for birth order in estimating the association of family size and educational attainment. However, the present paper is concerned not with causal effects but with equilibrium differences between large and small families, making such regression adjustment unnecessary. Birth order effects are one of many reasons for the different outcomes of children from large and small families. If all birth orders within a given family size were sampled with equal probability, then the estimates of β_{ct} in Section 4.2.1 are unbiased. Nevertheless, as described in Appendix 1, women of early birth orders are overrepresented in the DHS. In other words, for a given sibship size, more first-born women are observed than last-born women. Appendix 1 offers some explanations for this surprising pattern and proposes a procedure to estimate what β_{ct} would have been if birth orders were uniformly distributed within each sibship size.[23] The adjusted estimates are nearly identical to the baseline estimates of the Section 4.2.1.

3.2.3 Are Patterns for Men Similar to Those for Women?

Because the DHS only collects sibling history data from female respondents, it leaves a major gap: men. Fortunately, the supplementary surveys interview both men and women, so an analysis of gender heterogeneity is possible in Bangladesh, Indonesia, and Mexico. Recall that the supplementary surveys only include information on surviving siblings. For each survey, I estimate Equation (2) for men and women born in the 1940s, 1950s, 1960s, and 1970s.[24] Table 2 presents the results.

For both genders, all three surveys show similar patterns of declining education-sibship size relationships over the sample period. In Bangladesh and Indonesia, the relationship begins strongly positive and declines to a level that is closer to zero (albeit still positive) and statistically insignificant. For Indonesian men and women born in the 1940s, each additional sibling is associated with

[22] Birth order and family size are correlated because children of high birth orders necessarily come from large families.

[23] The potential reasons include (1) differential mortality by birth order; (2) son-biased fertility stopping; (3) the timing of fertility cycles within a fixed window birth cohorts; and (4) recall bias. The procedure to estimate a hypothetical β_{ct} under a uniform distribution of birth orders involves regressing education on sibship size and birth order, and then taking a linear combination of the coefficients on the two regressors. See Appendix 1 for more information.

[24] Because the Mexico Family Life Survey took place in the 2002, data on completed education are available for cohorts into the 1980s. For this survey, I define the 1970s as running from 1970 to 1982.

an additional 0.4 years of schooling. That quantity declines to 0.3 years of schooling for the 1960s cohorts and to less than 0.2 years of schooling for the 1970s cohorts.[25] In Bangladesh, the association begins at 0.3 for men and 0.1 for women born in the 1940s, declining to roughly half those quantities for men and women born in the 1970s.[26] Meanwhile, the Mexican data show an education-sibship size relationship of zero for the cohorts of the 1940s but -0.3 for the cohorts of the 1970s, irrespective of gender.

3.3 Linking Parental Economic Status, Sibship Size, and Educational Attainment

The fertility history results seem to contain the last phases of the global transition to a negative relationship between economic status and fertility, while the sibling history results point to a widespread shift of the education-sibship size link from positive to negative. The two phenomena seem connected, but unfortunately, the DHS does not include questions on respondents' childhood background characteristics, preventing a longer-term look at the evolution of the relationship between economic status and fertility.

However, the Family Life Surveys do include such questions. Because childhood family wealth or income are not available, I use parental education to measure parental economic status. In Table 2, I estimate Equation (2) using pooled data on men and women from the supplementary surveys.[27] For each country and period of birth, I run three regressions: one with no parental covariates, one with father's education, and one with both parents' education.[28] Father's education is likely a better proxy for overall household economic status during the sample period (due to consistently higher rates of male labor force participation), but I include the specification with mother's education for completeness.

Consistent with an important role for parental economic status, Table 2 reveals large changes in the education-sibship size coefficients after adjustment for parental education. Both sets of adjusted coefficients are more stable than the unadjusted coefficients across successive birth cohorts.

[25]The coefficients for the 1970s cohorts are statistically insignificant, but the sample sizes for these cohorts are small and the standard errors large.

[26]In the Bangladeshi data, the female education-sibship size association first increases from the 1940s to the 1950s and then begins to decline. This pattern may be due to the near absence of female education among the oldest cohorts.

[27]All regressions in Table 2 include gender as a covariate.

[28]Parental education is measured in years. The MxFLS only contains data on broad education categories, but for ease of comparison across settings, I convert them to a measure of years of education. Using data from the 2000 Mexico census, I determine the mean years of education among adults in each education level, and I then assign that mean to each individual in the Mexico Family Life Survey.

For Indonesia, the unadjusted coefficients transition from 0.34 in the 1940s to 0.07 in the 1970s, while the both sets of adjusted coefficients go from roughly 0.2 to 0. Matlab, Bangladesh is similar, with the adjusted coefficients declining from 0.2 to 0.1 while the adjusted coefficients remain stable at 0.1. Finally, for Mexico, the unadjusted coefficients fall from 0 to -0.3; the adjusted coefficients also begin at 0 but decrease to between -0.1 to -0.15. The differences between the time paths of the unadjusted and adjusted coefficients suggest that the evolution of the education-sibship size relationship is largely due to a changing relationship between parental education and sibship size.[29]

Is such a change in the relationship between parental education and sibship size evident in the data? Appendix Table 3 investigates this issue by regressing sibship size on parental education for each country and period of birth.[30] The most interpretable results come from univariate regressions of sibship size on father's educational attainment, which reveal declining coefficients in all three countries. Between the 1940s and the 1970s, the coefficient on paternal education declines from 0.11 to 0.05 in Indonesia; from 0.06 to 0 in Bangladesh; and from -0.03 to -0.11 in Mexico. Together with Table 2, these results imply that the changing relationship between parental economic status and fertility can account for between half and all of the changing relationship between sibship size and education.[31]

4 Theoretical Considerations

The existing theoretical literature offers several possible explanations for observed change in fertility regimes.[32] This section uses a simple quality-quantity framework to illuminate these various theories and identify their testable predictions. Much of the discussion relates to Jones et al.'s (2011) catalogue of economic theories of fertility, although Jones et al. are concerned with explaining a negative fertility-wage relationship, rather than a transition from a positive relationship to a neg-

[29]The results in Table 2 are also consistent with a changing relationship between parental education and own education, rather than a changing relationship between parental education and sibship size. In unreported results, however, the relationship between parental education and own education was large, positive, and stable throughout the sample period.

[30]Because the appropriate unit of observation is the family, not the offspring, I divide each respondent's sampling weight by his or her sibship size in Appendix Table 3. The reweighted sample is representative of the parents of survivors.

[31]With the inclusion of maternal education as a covariate, the results become less clear. Maternal education might be expected to decrease the number of surviving offspring through its effect on the mother's opportunity cost of time, as well as her knowledge and beliefs about family size limitation. On the other hand, it may also increase children's survival probabilities, thus increasing the number of surviving offspring. The results for maternal education are fairly noisy and do not confirm any one theory.

[32]See Clark (2005, 2007) for a useful summary of many of these theories, motivated by his pioneering work on the British demographic transition.

ative relationship. In this sense, the discussion bears a closer connection with the "unified growth theory" literature (Galor 2005, 2011), which attempts to simultaneously explain the demographic transition and the emergence of modern economic growth.

In the framework, parents maximize a utility function over their own consumption, the number of children, and their children's human capital, which is assumed to be constant within a family. Consider a separable, logarithmic utility function, as is common in the literature on the interaction of long-run economic growth with heterogeneity in income and childbearing decisions (e.g., Galor and Moav 2002; de la Croix and Doepke 2003; Moav 2005):

$$U(c,n,h) = \alpha \log(c) + (1-\alpha)\left(\log(n) + \beta \log(h)\right) \tag{3}$$

where c is parental consumption, n is the number of (surviving) children, and h is human capital per child. The parameter $\alpha \in (0,1)$ indexes the weight the parents place on their own consumption relative to the combined quantity and quality of chidden, or nh^{β}. The parameter $\beta > 0$, in turn, corresponds to the importance of quality relative to quantity. Child quality, or human capital, is determined by the production function $h(e) = \theta_0 + \theta_1 e$, where e denotes education spending, and θ_0 and θ_1 are strictly positive. One can view θ_0 as a human capital endowment or as compulsory public school.[33]

The remaining ingredient for the framework is a budget constraint. For reasons that will become apparent, most existing theoretical analyses restrict whether the quantity and quality of children have goods costs or time costs, but to keep the setup as general as possible, I allow both types of costs for both dimensions of investment in children. Each child costs τ^n units of time and κ^n goods, while each additional unit of education bears a per-child time cost of τ^e and a per-child goods cost of κ^e, which I normalize to $\kappa^e = 1$ (without loss of generality). Assuming that the parents have an overall time endowment of 1, the budget constraint is:

$$c + \kappa^n n + ne \leq w\left(1 - \tau^n n - \tau^e ne\right) \tag{4}$$

[33] The choice of functional form for the human capital production function is not innocuous. A human capital endowment is necessary to generate a negative fertility-wage relationship in a model with exogenous wage heterogeneity and log utility. Based on this insight, Jones and Tertilt (2011) suggest the functional form I use, inspired by Becker and Tomes (1976). De la Croix and Doepke (2003) and Murtin (forthcoming) use slightly more complicated specifications, to which the results below are robust.

where w is the parents' wage rate. Several papers by Galor and coauthors (e.g., Galor and Weil 2000; Galor and Moav 2002) also impose a subsistence consumption constraint $c \geq \underline{c}$.

The setup leads to interpretable closed-form solutions for optimal fertility and child investment. For fertility, we have:

$$n^* = \frac{(1-\alpha)(1-\beta)}{\tau^n - \frac{\theta_0}{\theta_1}\tau^e + \frac{\kappa^n - \theta_0/\theta_1}{w}} \tag{5}$$

which is positive if the time cost of education (τ^e) is small and if the human capital endowment (θ_0) is not too large. For child investment, we have:

$$e^* = \left(\frac{1-\alpha}{\alpha + \alpha\beta - \beta}\right)\left(\beta\frac{\kappa^n + \tau^n w}{1 + \tau^e w} - \frac{\theta_0}{\theta_1}\right) \tag{6}$$

which is positive if the human capital endowment is not too large and if $\alpha > \frac{\beta}{1+\beta}$, so that the parents place sufficient weight on their own consumption. I assume that the parameter restrictions for a positive solution hold.[34]

The relationship between wages and optimal fertility depends on the structure of child costs. If children have only goods costs or only time costs, then fertility increases in the wage.[35] Thus, the interaction of goods costs with time costs plays an important role. De la Croix and Doepke (2003) and Moav (2005) suggest the convenient and reasonable assumption that children bear only a time cost ($\tau^n > 0, \kappa^n = 0$) while education bears only a goods cost ($\tau^e = 0, \kappa^e > 0$), which guarantees that $\frac{\partial n^*}{\partial w} < 0$, as in the new fertility regime identified in this paper.[36] In addition, optimal education is increasing in wages only if $\tau^n > \kappa^n \tau^e$, so their assumptions also rationalize that empirical regularity. To simplify the discussion below, henceforth, I will allow for a goods cost of children ($\kappa^n \geq 0$) but maintain the assumption that education does not require parental time ($\tau^e = 0$).[37] The remainder of this section uses the framework to explore possible explanations for the change in fertility regimes, and to draw out other predictions of these explanations.

[34]Positive solutions for both n^* and e^* are guaranteed if $\min(1,\beta)\,\frac{\kappa^n + \tau^n w}{1+\tau^e w} > \frac{\theta_0}{\theta_1}$ and $\alpha > \frac{\beta}{1+\beta}$.

[35]This result stems from the assumption of log utility, which implies an elasticity of substitution of 1 between children and parental consumption. An elasticity of substitution of greater than 1 is necessary to obtain a negative fertility-wage relationship with time costs only (Jones and Schoonbroodt 2010; and Jones et al. 2011). With goods costs only, the fertility-wage relationship is always positive.

[36]Technically, Moav (2005) assumes that parents' productivity as teachers increases with their human capital. However, this assumption to the same budget constraint as the assumption that education bears only a goods cost.

[37]A small time cost of education would not substantively change the results. However, because schools look after children during the day, education is at least as likely to *reduce* the time cost of children as to increase it.

Women's Work One natural explanation for the regime shift is the rise in women's work outside the home. The reasoning is similar to that of Galor and Weil (1996), who argue that long-run technological progress increased the return to mental skills.[38] Since women have a comparative advantage in mental tasks, the gender gap in wages shrunk over time, eventually inducing greater women's labor force participation and lowering fertility due to the increased opportunity cost of childbearing. Galor and Weil consider neither quality investments nor cross-sectional heterogeneity, but such extensions would be natural. In the framework here, one cannot generate a negative relationship between wages and fertility without assuming a positive opportunity cost of childcare time ($\tau^n > 0$).

This explanation runs up against the empirical reality, originally documented by Goldin (1995), that women's labor force participation follows a u-shape over the course of economic development.[39] Rates of women's labor force participation were high in Africa throughout the sample period, but $\frac{\partial n^*}{\partial w}$ was positive. But a closer reading of Goldin (1995) suggests that in the early stages of development, when labor is mostly agricultural, women's work is compatible with childrearing. Women's labor force participation then decreases when manufacturing predominates and increases with the rise of the service sector, but service jobs compete with childrearing. If women's opportunity cost of time explains the fertility regime shift, then structural transformation (i.e., the emergence of the service sector) must also play a key role.

The Rise of Human Capital Another attractive explanation is the rise of the demand and supply of schooling, which plays a key role in many models of the transition from Malthusian stagnation to growth (e.g., Becker et al. 1990; Galor and Weil 2000). The expression for n^* above clarifies that such an explanation needs to be nuanced. On the one hand, an increase in the return to education (θ_1) makes $\frac{\partial n^*}{\partial w}$ more positive rather than more negative.[40] On the other, an increase in the human capital endowment (θ_0), perhaps from an expansion in compulsory public schooling, makes $\frac{\partial n^*}{\partial w}$ more negative. But an increase in θ_0 also induces greater fertility for all households, which does not seem to match the time-series facts. If the data show that changes in cross-sectional fertility patterns are associated with declining fertility, then a rising endowment cannot explain the changes.

[38] For a related theory, see Lagerlof (2003).

[39] Also see Mammen and Paxson (1998), and Olivetti (2012).

[40] An increase in θ_1 leads all parents to increase education per child by the same amount, which in turn leads them to reduce fertility. Because the poor have a higher marginal utility of consumption, their fertility response is larger.

Decreasing Child Mortality Theories of the fertility transition often incorporate reductions in child mortality. Because the bulk of mortality decline has occurred for children younger than school-starting age, one can think of a it as a reduction in the quantity costs of surviving children, τ^n and κ^n. Equation (5) makes clear that a decline in κ^n can flip $\frac{\partial n^*}{\partial w}$ from positive to negative. Again, however, the explanation runs up against basic time-series facts. As in the original Barro-Becker model (1989), reductions in τ^n and κ^n increase optimal fertility and decrease optimal schooling investment, which appears counterfactual.[41] A separate complication is the mortality gap between rich and poor, which has likely shrunk in absolute terms over the sample period. Then the fertility of the rich would have increased, rather than decreased, relative to that of the poor.

Subsistence Constraints In the context of the escape from the Malthusian trap, it is natural to consider the effect of a subsistence consumption constraint: c must be larger than some threshold $\underline{c}$. Suppose we choose parameter values to guarantee that $\frac{\partial n^*}{\partial w} < 0$ when the subsistence constraint does not bind. Then when the subsistence constraint *does* bind—i.e., $\alpha w < \underline{c}$—the family consumes $\underline{c}$ and spends $w - \underline{c}$ on the combined quality and quantity of children. Increases in the wage lead to increases in both child quality and child quantity. Once the family escapes the subsistence constraint, however, further increases in the wage decrease optimal fertility.[42] This theory generates a hump-shaped relationship between w and n^*. If we fit a linear regression of n^* on w, however, the slope coefficient could flip as the average wage increases.

Changing Preferences In interpreting the patterns in Section 3, many non-economists would think first of preferences, rather than the budget constraint. Many non-economic theories of the transition from high to low fertility (Caldwell 1982, Casterline 2001) posit changes in beliefs and norms regarding child-rearing. Consider the introduction of new 'Western' norms that increase β, thus raising optimal education and lowering optimal fertility. If these new norms affect the highest-wage (or highest-education) families most strongly, then $\frac{\partial n^*}{\partial w}$ could flip from positive to negative. The same basic reasoning would hold in arguments based on the empowerment of women rather than the diffusion of norms (Duflo 2012). If women have lower β's than men, and if women of

[41] Child mortality has different effects in the old-age security model of Boldrin and Jones (2002). I discuss the predictions of this model below.

[42] The models of Morand (1999) and Mookherjee et al. (2012), while not explicitly studying subsistence constraints, generate similar dynamics by considering multiple sectors, within which fertility may rise with the wage.

higher economic status make the earliest gains in household bargaining power, then richer households will be the first to transition to low fertility.

Following the tradition of theories concerned with the evolutionary effects of differential fertility, Galor and Moav (2002) develop a model that combines heterogeneity in β with a subsistence constraint.[43] The evolutionary dynamics in their model generate an endogenous reversal of the fertility-wage elasticity from positive to negative. Family dynasties with high β's accumulate more human capital and therefore become richer than their low-β counterparts. Early in the process of development, the subsistence constraint binds for the poorer, low-β types, so that the high-β types choose higher fertility in addition to higher investment per child. This differential fertility pushes up the average skill level in the population, generating technological progress that gradually pushes low-β families over the subsistence constraint. At that point, the poorer, low-β types transition to high fertility, leading $\frac{\partial n^*}{\partial w}$ to become negative. As a result, Galor and Moav's model has the testable prediction that increases in the average skill level should make $\frac{\partial n^*}{\partial w}$ more negative.

Intergenerational Wealth Transfers A separate class of theories, which does not fit into the framework above, emphasizes upward intergenerational transfers from children to parents, in the form of child labor or old-age support.[44] Caldwell emphasizes how the expansion of schools alters child-rearing norms, so that parents come to view children as net recipients of, rather than net contributors to, household resources. This model bears similarities with other theories of changing preferences. Following a different thread in Caldwell's work, Boldrin and Jones study parental behavior when old-age security is the primary motive for childbearing. Within their framework, financial deepening could flip $\frac{\partial n^*}{\partial w}$ if wealthy families substituted other savings vehicles for children. But this reasoning gives no account for why the decreases in quantity investment would be accompanied by increases in quality investment. Additionally, as stressed by Galor (2005, 2011), wealthier couples typically have access to a wider variety of savings vehicles before the fertility transition. Finally, Lee (2000) argues that data from no society suggest a net upward flow of resources across generations, unless one counts pension systems.[45]

[43]Galor and Moav (2002) point out that such heterogeneity could arise because of genetic diversity or culture. Fernandez and Fogli (2010) document significant cultural persistence in fertility rates among immigrants to the United States.

[44]Other prominent references include Cain (1983), Nugent (1985), Ehrlich and Lui (1991), and Morand (1999).

[45]Galor (2005, 2011) also points out that net transfers from offspring to parents are extremely rare in non-human species, so that such transfers are unreasonable to incorporate into a theory of human behavior in the very long run. It is unclear whether this argument applies to the time frame in my data.

Contraception Advocates of family planning might instead emphasize the uneven adoption of effective contraceptive technology (Potts 1997). From this perspective, the currently negative relationship between economic status and fertility is due to an unmet need for contraception among the poor. But a non-demand-based theory of this type fails to account for the early regime during which fertility increases in economic status. One possibility is that women from richer households have a higher biological capacity to bear children (fecundity) due to their better health (and lower maternal mortality rates). If true, then one would expect population health improvements to decrease $\frac{\partial n^*}{\partial w}$.

5 Subsistence Constraints and Functional Forms

In both the fertility history results and the sibling history results, differences in β_{ct} across countries or over time may have one of two causes. First, the functional form linking the independent and dependent variables may have actually changed. Second, the functional form may be stable but non-monotonic, and a shift in the distribution of the independent variable may have flipped the OLS coefficient.

This issue has especially important implications for the interpretation of the fertility history results. A stable, hump-shaped relationship between economic status and fertility would be consistent with a subsistence constraint model, whereas a rotation in functional form would point to other factors. Consistent with increasing income (and consistent with the results of Young 2012), Appendix Table 1 shows large increases in average durable goods ownership (as well as decreases in average ever-born and surviving fertility) between the early and late DHS periods.

To explore possible non-monotonicities, Figure 3 shows average fertility for discrete counts of durable goods in the early and late periods.[46] The plots show evidence of a hump shape in the 1986-94 period, but the hump disappears by the 2006-11 period. The proper interpretation not entirely clear. Changes in the relative prices of consumer durable goods may have obscured a time-invariant hump in the late period, although it seems likely that the durable goods included in the index *decreased* in relative price over the relevant period rather than increasing. If true, then one would expect the increasing portion of relationship to expand rather than disappear. In this sense, the data do seem to suggest a change in functional form; within the urban and rural sectors, fertility

[46]For consistency with other results, these plots were first estimated at the country level and were then averaged across countries.

is generally flat or declining with respect to durable goods ownership in the late period. Note that the higher categories sometimes represent very few observations, especially in Africa. In Appendix Figure 5, which bins households only by being above or below the median of the durables index, the patterns are more obviously consistent with the regression estimates in Table 1.

The supplementary sibling history data point to the same phenomenon. No direct measure of childhood household economic status exists in the Family Life Surveys, but patterns by paternal education are illuminating. To this end, Appendix Figure 6 plots average sibship size by categories of paternal education.[47] The earliest cohorts display a hump-shaped relationship between paternal education and sibship size, which flattens or reverses among the latest cohorts. This pattern is consistent with the subsistence constraint theory if broad-based income gains eventually lifted the least-educated out of poverty.

In the case of the education-sibship size relationship, the details of functional form are not as essential for connecting the results with theory. Movement over a time-invariant, hump-shaped relationship between income and fertility could generate a rotation in the education-sibship size link. Nevertheless, it is interesting to consider whether the education-sibship size relationship has rotated. To investigate this issue, I re-run Equation (2) with a vector of indicators for each sibship size from 2 to 13:

$$highest\,grade_{ict} = \alpha_{ct} + \sum_{k=2}^{13} \beta_{ct}^{k} 1\left[sibsize_{ict} = k\right] + X_{ict}'\lambda_{ct} + \varepsilon_{ict} \tag{7}$$

where $1\left[sibsize_{ict} = k\right]$ is an indicator for being a member of a sibship of size k. Women who were only children are the reference group, and women with more than 12 siblings (less than 1 percent of the sample) are omitted. To simplify the results, I separate women into just two birth cohort categories for this exercise: pre- and post-1970.[48] I first estimate Equation (7) separately for each country-cohort cell and then average the coefficients for each cohort across countries.

The results, which appear in the top two panels of Figure 4, provide clear evidence of a slope reversal. For both measures of sibship size, the pre-1970 plots slope upward, while the post-1970 plots slope generally downward. The only noteworthy anomaly concerns women from one-child

[47] As in Appendix Table 3, I divide each respondent's sampling weight by his or her surviving sibship size in Appendix Figure 6. The reweighted sample is representative of the parents of survivors.

[48] I assign women born in 1970 to the post-1970 cohort.

families in the post-1970 birth cohorts, who attain less education than would be predicted by a regression estimated using only the other sibship sizes. The finding that only children are uniquely disadvantaged matches much of the existing literature on industrialized countries (e.g., Butcher and Case 1994; Black et al. 2005).

In the remainder of Figure 4, the panels display probability mass functions of ever-born and surviving sibship size for the pre- and post-1970 cohorts. For consistency with previous pooled results, I first estimate the probability mass functions at the country level and then average their values across countries. The middle row of Figure 4 exhibits the individual-level probability mass functions, based on standard histogram calculations. While the distribution of sibship sizes across siblings may be of interest, the distribution across families may be more relevant. As such, the bottom row of Figure 7 presents family-level mass functions, obtained by dividing the sampling weights by surviving sibship size.

Several noteworthy patterns arise in the estimated mass functions. First, the ever-born sibling mass functions are quite similar for pre- and post-1970 cohorts. This result may strike some as surprising, given that fertility declined in many parts of the world during the sample period. However, fertility rates in Africa, which constitutes more than 75 percent of the sample, stagnated or even slightly increased during the sample period (Garenne and Joseph 2002). Second, the surviving sibling mass functions for the post-1970 cohorts are rightward shifted relative to those for pre-1970 cohorts. On average, then, women born after 1970 had more surviving siblings than those born before 1970. This too accords with post-World War II demographic history, which included widespread declines in child mortality (Hill and Pebley 1989). Third, the family-level mass functions show a somewhat implausible uptick at 1, especially for the pre-1970 cohorts. This uptick may be due to the underreporting of older siblings, as discussed in 3.2.2 and Appendix 1.

6 Macroeconomic and Demographic Determinants of β_{ct}

The reversals of the fertility-economic status relationship and the education-sibship size relationship in the developing world occurred during a half-century that included much economic and demographic change. During this period, sample countries had varied experiences in economic growth, structural transformation, urbanization, educational expansion, and mortality and fertility decline. Because the data on the education-sibship size link provide the longest time horizon, this section assesses how that link relates to economic and demographic aggregates. This analysis seeks to shed additional light on which of the mechanisms outlined in Section 4 is the most likely mediator of the observed changes in fertility regimes. I first rerun Equation (2) for five-year birth cohorts from 1945-9 to 1985-9.[49] I then recover each country-cohort's estimated coefficient on surviving sibship size ($\hat{\beta}_{ct}$) and estimate its relationship with economic and demographic aggregates in the period of birth.[50]

The economic and demographic aggregates come from a variety of sources. One set consists of cohort averages in the DHS. Others include log GDP *per capita* (from the Penn World Table [Heston et al. 2012]), average adult (25+) educational attainment (from Barro and Lee 2010 and Cohen and Soto 2007), urbanization (from UNPD 2011), female labor force participation (from ILO 2012), the sectoral composition of both value added (from the Penn World Table), and an indicator for the presence of polygamy (from Tertilt 2005).[51] To obtain estimates for the 5-year birth periods, I average across the five years of each birth period. For variables that are not available annually, I first linearly interpolate between observations within each country.[52]

6.1 Cross-Sectional Patterns

Although the main analysis of economic and demographic aggregates takes advantage of the panel structure of the data by controlling for country and birth period fixed effects, cross-sectional analyses serve as a useful starting point. Figure 5 documents the evolution of cross-sectional relationships between several aggregate variables and β_{ct}. Three of the four panels—for GDP *per capita*, average education, and urbanization—display a series of local linear regressions, one per period of birth. Data on women's labor force participation are too sparse to estimate cohort-level local linear regressions, so the fourth panel shows a scatter plot, maintaining the same color scheme used in the other three panels.

[49] For precision, I omit cells with less than 200 observations, representing 2.5 percent of all cells.

[50] I focus on the surviving sibship size coefficients because it bears a closer link to the theories proposed in Section 4, and because it is directly relevant to the effect of differential fertility on average education. Unreported results for the ever-born sibship size coefficients are qualitatively similar but somewhat smaller in magnitude.

[51] Unlike the other aggregates, the polygyny indicator does not change over time.

[52] The education data are available every five years in Barro and Lee (2010) and every ten years in Cohen and Soto (2007), so I linearly interpolate both series. I use the Barro-Lee estimates when available; for countries that only have Cohen-Soto estimates, I use the Cohen-Soto estimates to generate predicted Barro-Lee estimates, based on a regression of Barro-Lee on Cohen-Soto in the sample of countries with both measures.

Throughout the sample period, more educated and more urban places have more negative education-sibship size associations. Although the intercepts appear to shift downward over time, the slopes on these cross-sectional curves are fairly stable. These patterns suggest that structural transformation or mass education may be intimately linked to the reversal of β_{ct}. Section 6.2 will tell whether they are robust to the inclusion of country fixed effects.

Meanwhile, β_{ct} does not show a consistent relationship with GDP *per capita* or women's labor force participation. The relationship between and log GDP *per capita* in the birth period goes from flat to significantly negative, at least if one ignores the extreme outlier of Gabon.[53] Meanwhile, no discernible pattern emerges in the scatter plot of β_{ct} and women's labor force participation. These cross-sectional analyses do not suggest a direct link between the evolution of the education-sibship size relationship and either aggregate income or women's labor force participation.

Another noteworthy cross-sectional result, not reported in Figure 5, is that β_{ct} in polygamous countries exceeds that in monogamous countries by 0.1 to 0.2.[54] This result is consistent with the argument by Tertilt (2005, 2006) that men in polygamous societies have an incentive to invest their wealth in a large number of children. In such societies, a groom typically 'buys' a bride from her father, so that men benefit from having many daughters but do not lose from having many sons. By this reasoning, wealthy men demand many wives and many children per wife.[55] The correlation between economic status and fertility thus tends to be more positive in polygynous countries.

6.2 Panel Analysis

The cross-sectional patterns in Figure 5 lead one to ask whether changes in aggregate socioeconomic and demographic variables can account for the observed changes in the education-sibship size relationship.[56] One can address this question by including cohort and country fixed effects, as in the following regression specification:

$$\hat{\beta}_{ct} = Z'_{ct}\psi + \tau_t + \mu_c + \varepsilon_{ct} \tag{8}$$

[53] According to data from the CIA World Factbook, Gabon's oil production *per capita* is more than twice that of any other country in the sample, so its GDP *per capita* proxies for a different bundle of country characteristics.

[54] This result holds within Africa as well. It is highly statistically significant, both within Africa and across the world.

[55] Note that the patterns in this paper cannot be driven by the number of wives per husband (the extensive margin). The DHS sibling history roster asks for all siblings with the same biological mother.

[56] One might also be interested in the effects of changes in polygyny, but consistent panel data on polygyny are not available.

where Z_{ct} is a vector of independent variables, and τ_t and μ_c are cohort and country fixed effects, respectively. This specification nets out sample-wide time trends and time-invariant country characteristics.

As a first step, it is instructive to leave Z_{ct} out of Equation (8) and to recover the cohort effects, τ_t. Figure 6 plots the evolution of these cohort effects over time, with associated 95 percent confidence intervals for tests of differences from the omitted cohort (1945-9). The cohort effects, drawn in black, begin trending downward in the 1960s and become significantly negative in the 1970s. The last cohort effect, for 1985-9, implies that net of country fixed effects, the education-sibship size association is 0.28 lower in 1985-9 than in 1945-9. The panel is unbalanced, so these cohort effects are not necessarily capturing a representative time trend for all countries in the sample, but the pattern is striking nonetheless. For three birth cohorts in the 1960s and 1970s, however, the panel is balanced, allowing me to estimate the average education-sibship size association for the all countries. The figure plots these three cohort-level averages in red, with the magnitudes given on the right-hand axis. If one uses this right-hand axis to center the cohort effects estimated in the unbalanced sample, then the plot shows a clear flip from a positive average education-sibship size association to a negative average association.

6.2.1 Using Cohort Average Outcomes as Covariates

Table 4 presents estimations of Equation (8) in which the covariates Z_{ct} are cohort average outcomes from the DHS: average completed education, average surviving sibship size, and the average fraction of siblings dying before they reach age 5.[57] Because these average outcomes are codetermined with the education-sibship size relationship, one should not think of Table 4's estimates of ψ as representing causal effects. Even so, the estimates can shed light on the mechanism driving the change in fertility regimes. In the theoretical framework, an increase in the human capital endowment (θ_0) made $\frac{\partial n^*}{\partial w}$ more negative, but it also increased n^* directly. In effect, that theory predicts a negative correlation between β_{ct} and average surviving sibship size.

[57]The average fraction of the respondent's own siblings who died before age 5 is very highly correlated with the fraction of the entire cohort's siblings who died before age 5. Generally, however, neither measure is an unbiased measure of the overall under-5 mortality rate—see King and Gakidou (2008). They nonetheless serve as a transparent proxy for the mortality environment in childhood. Furthermore, the bias corrections in the literature seek to account for the families omitted from the calculation because no siblings survive to adulthood, and the mortality conditions these omitted families faced are not necessarily relevant to the respondents' families.

Before reporting the results, I note two more estimation details. First, because the regressions in Table 4 measure changing equilibrium associations, I include only one covariate in each regression (in addition to the cohort and country fixed effects). If multiple covariates entered into the same regression, the coefficients would become uninterpretable. Second, the estimates of β_{ct} and the cohort average outcomes are based on the same data, which introduces regressor measurement error that is correlated with regressand measurement error. In addition to the OLS results, the table thus reports estimations that correct for correlated measurement errors using Fuller's (1987) method-of-moments technique.

The results in Table 4 give three conclusions: (1) as the education-sibship size association declines, average educational investment increases, (2) as the education-sibship size association declines, average family size also declines, and (3) the education-sibship size association has no relation to child mortality rates. These results reject the hypothesis that β_{ct} flipped because of rising human capital endowments. Instead, the reversal of the education-sibship size association appears to accompany the broader fertility decline. Recall, however, that surviving sibship size *increased* on average during the sample period, due to fertility and mortality trends in Africa. As a result, fertility decline cannot itself explain the reversal of β_{ct}. Indeed, in the last row of the table, I ask what fraction of the 1985-9 time effect, as estimated in Figure 6, can be explained by changes in each cohort average outcome. Because average surviving sibship size moved in the 'wrong' direction, it accounts for -40 percent of the reversal of β_{ct}. Meanwhile, rising average education can account for a striking (positive) two-thirds of the reversal of β_{ct}. From the perspective of understanding the reversal's causes, however, the cohort's own average education is of considerably less interest than the average education of the parent generation. The next section considers the previous generation's average educational attainment, along with several other socioeconomic aggregates.

6.2.2 Using Socioeconomic Aggregates in Early Life as Covariates

Data on many socioeconomic aggregates are not available for the full set of countries. This data limitation makes multiple regression somewhat cumbersome because the sample composition changes with each covariate. As such, Table 5 presents estimations of two regression specifications. The first—which includes log GPD *per capita*, average adult educational attainment, and urbanization in the birth period—allows a close to complete sample of country-cohorts, while the second—which adds sectoral shares of value added—has a much reduced sample. All coefficient estimates are small and statistically insignificant, except for that of average adult educational attainment. The lack of a role for GDP *per capita* makes the subsistence constraint hypothesis seem unlikely, although GDP *per capita* may be too noisy a measure of long-term income.[58] Similarly, the lack of a role for sectoral composition suggests that the reversal of β_{ct} is not due to structural transformation or changes in the productivity of women's skills (which, as discussed in Section 4, are related to the rise of the service economy).

At the same time, the coefficient on education in column (1) suggests that the rising educational attainment of the parent generation can account for 57% of 1985-9 cohort effect effect for β_{ct}, as reported in Figure 6. Thus, rising education of the parent generation can account for more than half of the reversal of the education-sibship size relationship among offspring. The coefficient on education shrinks somewhat in column (2), but this reduction is entirely due to the fact that column (2) discards roughly half of the sample for a lack of data on sectoral composition. In this selected sample, the coefficient on average adult education is invariant to the inclusion of measures of sectoral composition. Given the absent roles of all other socioeconomic covariates, the role of education is most consistent with theories of preference change. If education is a better measure of long-term parental income than GDP, however, the results also support the subsistence constraint hypothesis.

Several theories in Section 4 deal specifically with the position of women, so Table 6 explores the role of two female-specific covariates: the female labor force participation rate and the average educational attainment of women. Data on labor force participation are unfortunately rare, leaving only 70 observations from 36 countries for the regression presented in column (1).[59] Among these observations, changes in female labor force participation bear no relation with changes in β_{ct}.[60] Switching the focus to education, Column (2) asks whether the role of average education is due to women or men. While the coefficients on average female education and average male education are jointly significantly different from zero, they are not significantly different from each other, and the coefficient on average male education is larger and individually more significant. The results in

[58] In fact, in their model with subsistence consumption constraints, Galor and Moav (2002) predict that rising education flips the cross-sectional relationship between parental skill and fertility. However, although they do not explicitly consider the role of GDP *per capita*, their model would predict that rising production would also flip the relationship.

[59] In this small sample, the coefficient on average adult education remains large and statistically significant, with and without the inclusion of female labor force participation.

[60] The sample omits the 1950 observation for Bolivia, in which both female and male labor force participation are unreasonably high. If one includes this observation, the coefficient on female labor force participation becomes significantly *positive*, opposite the prediction of the theory.

Table 6 thus suggest that the causes of the reversal are not specific to the empowerment of women.

7 Differential Fertility and the Evolution of the Human Capital Stock

Social scientists of many stripes have argued that the higher fertility of the poor (and less-educated) is a drag on average skill because a lower-skill group grows faster than the rest of the population in transition and has a larger population share in steady state. But if high-fertility families once educated their children *more* than low-fertility families, then differential fertility may have once promoted economic growth rather than hindering it. Relative to the case in which sibship size and education are uncorrelated, a positive education-sibship size relationship increases the share of educated children in the population, while a negative relationship decreases that share. As a consequence, the preceding results suggest that heterogeneity in fertility rates across families may have once increased the human capital stock, even if in more recent times it decreased the human capital stock.

More precisely, the effect of differential fertility on average educational attainment operates through two channels. The channel described above involves a mechanical change in the socioeconomic composition of each birth cohort, so one might appropriately call it a *composition effect*. But adjustments in fertility rates could also alter educational investments through the household budget constraint. This second channel, related to the quality-quantity tradeoff, might be called the *adjustment effect*. Although the composition effect is straightforward to estimate, the adjustment effect is not; one would need either natural experimental evidence on the quality-quantity tradeoff faced by different types of families or detailed information on households' budget sets, neither of which are available here. As such, this section quantifies only the composition effect and its changes over time in the forty sample countries.

To investigate changes in the composition effect of differential fertility, I generate counterfactual averages through a simple reweighting exercise. I define a family's *type* to be its actual number of surviving siblings, and I then ask what average education would have been if all family types had had the same number of surviving siblings (on average). For a given family type, I assume that educational attainment is independent of the number of siblings, allowing me to generate this counterfactual average by dividing each individual's sampling weight by her surviving sibship size.

This conditional independence assumption shuts down the adjustment effect. While the assumption may be implausible, it offers a transparent way to quantify the effect of differential fertility on the (female) *per capita* human capital stock. Really, any question about an "effect" of differential fertility requires some departure from theory because differential fertility is generated by equilibrium behavior; one cannot directly manipulate it. The reweighting approach has similarities to the techniques of Blinder (1973), Oaxaca (1973), and DiNardo, Fortin, and Lemiux (1996), and it shares with these techniques its inattention to equilibrium responses to the simulated change.

For each 5-year birth cohort within a country, I reweight the sample to estimate the counterfactual average years of education that would have arisen if all family types had the same mean family size. I then subtract the reweighted average from the observed average to quantify the compositional effect of differential fertility on average education. The difference of the observed and reweighted averages can be interpreted as the composition effect of differential fertility relative to the case of equal fertility for all families. Standard errors are computed with the delta method.[61]

The results, presented in Figure 7, indicate much cross-country heterogeneity, which is entirely consistent with the findings reported earlier in the paper.[62] For 5-year birth cohorts from 1945-9 to 1985-9, the figure displays trends in the difference between the observed and reweighted averages. In some countries, predominantly African, differential fertility increased average educational attainment throughout the sample period. These countries have not transitioned to the regime in which sibship size and education are negatively correlated. Opposite these countries are the Dominican Republic and South Africa, where the effect of differential fertility was negative (although not always statistically significant) throughout the sample period. Recall that only these two countries exhibit persistently negative associations between surviving sibship size and education in Figure 1. Finally, several countries have undergone a transition from a regime in which differential fertility promoted the growth of human capital to a regime in which differential fertility depressed it. For two compelling examples, consider the Andean nations of Bolivia and Peru. For the 1945-9

[61] The reweighting technique for generating the counterfactual average does not naturally lead to a covariance matrix. However, the technique is equivalent to estimating average education for each family type, multiplying each type-specific average by an estimated weight corresponding to the counterfactual share of individuals from that type, and then summing across types. The intermediate quantities in this alternative procedure have a standard covariance matrix, so one can use the delta method to compute the standard error for the reweighted average. Appendix 2 describes the application of the delta method in detail.

[62] For a decomposition of these averages, Appendix Figure 8 shows composition effects on the share of the cohort with 0, 1-5, 6-8, and at least 9 years of education. The results show that Figure 7's composition effects on average educational attainment are not driven by any one schooling level.

cohort, differential fertility increased average education by 0.3 to 0.5 years in both countries. In contrast, for the 1985-9 cohort, differential fertility reduced average education by 0.5 years.

These magnitudes are meaningful but small relative to the overwhelming increase in female education during the sample period. On average, the 1985-9 cohorts have 3.7 more years of education than the 1945-9 cohorts.[63] The largest differences between the observed and reweighted averages are ± 0.6, and the average within-country change in these differences between 1945-9 and 1985-9 is -0.17. Therefore, the shift from a positive to a negative education-sibship size relationship did not have a large effect on the evolution of average female educational attainment across the 40 countries in the sample. Nevertheless, relative to the level of average educational attainment, the effects of differential fertility are reasonably large for early cohorts. For the 1950-4 cohort, the effect of differential fertility on mean education was on average 15 percent of the cohort's mean education (in absolute value). As mean education rose, the relative magnitude of the differential fertility effect shrank: for the 1985-9 cohort, the effect of differential fertility on mean education was on average 4 percent of the cohort's mean education.

8 Conclusion

Prior to the results of this paper, little evidence existed on positive associations between economic status and fertility, and between sibship size and education, in the 20^{th} century. The lack of solid evidence led many researchers, especially those studying the aggregate consequences of differential fertility, to focus instead on the negative associations widely observed today.

A wide range of data from 48 developing countries reveals that both associations were indeed positive well into the 20^{th} century. They became negative only recently: first in Latin America, then in Asia, and finally in Africa. Although the data do not paint a completely definitive picture of the causes of this reversal, they are most consistent with explanations based on changing preferences, and also to some extent with explanations based on subsistence consumption constraints. Increases in the parents' generation's education were by far the most important predictor of the reversal; the data show little role for child mortality rates, GDP *per capita*, sectoral composition, urbanization,

[63]Not all countries have data available on all cohorts, so this finding is based on a regression of cohort average education on country and cohort indicators. The coefficient on the 1985-9 cohort indicator is 3.7, indicating that the 1985-9 cohorts have 3.7 more years of education than the omitted category, the 1945-9 cohorts.

and women's labor force participation. The results for GDP *per capita* cast some doubt on the subsistence constraints theory, but GDP *per capita* may be too transitory a measure of income (relative to average educational attainment, for example). Also in contrast to the subsistence constraints, although the data show a hump-shaped relationship between household economic status and fertility in the past, they do not in the present, even over apparently the same support of measures of household economic status.

Apart from adding an interesting twist to recent demographic history, the flip of these associations provides discipline to existing theories of long-run growth and the demographic transition. For instance, in the baseline model in Section 4, an increase in the return to education *decreases* the fertility of the poor relative to that of the rich. In this sense, the reversal of differential fertility is difficult to reconcile with theories of fertility decline based solely on rising returns to human capital. Broadly, then, the results suggest much reward from incorporating cross-sectional heterogeneity into models at the intersection of macroeconomics and demography.

More practically, because the reversal has gone largely unrecognized in the literature on the effects of differential fertility on the human capital stock, that literature has missed an important aspect of the interaction between demography and economic growth. In the mid-20^{th} century, fertility differences between families of higher and lower economic status increased average education in most of the countries under study. These fertility differences eventually flipped in many countries, so the effects of differential fertility on the *per capita* stock of human capital also reversed later in the century. A fruitful direction for future research would investigate the general equilibrium implications of these changes for the evolution of income inequality.

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Appendix 1: Birth Order Adjustment

This appendix gives further details on the birth order adjustment described in Section 4.2.2. The problem stems from the fact that women of different birth orders may have different probabilities of appearing in the sample, even holding sibship size constant. Basic theory does not predict this pattern; if one sampled daughters with equal probabality from a population of families, then birth orders would be uniformly distributed within each sibship size. But several mechanisms could lead to non-random sampling of birth orders in the DHS sibling history sample. First, childhood mortality may vary with birth order. Second, if parents follow son-biased fertility-stopping rules, such that they continue childbearing until the birth of a boy, the probability of a later-born girl in a sibship size of n is less than $\frac{1}{n}$. Third, because the sampling frame is defined by women's ages, booms and busts in fertility rates across successive cohorts of their mothers may also lead to nonrandom sampling of birth orders. For example, if the cohort of mothers who initiated childbearing in the 1980 subsequently bore an unusually large number of children, then a sample of women born in 1980 would disproportionately consist of first-born women from large families. Fourth, the distribution of *reported* birth orders may be nonuniform because women may be more likely to remember deceased younger siblings than deceased older siblings. This recall bias would lead to a larger number of early-born (e.g., first- or second-born) women than would be implied by a uniform distribution within each sibship size.

If birth order has an independent effect on education, then a nonuniform birth order distribution within each sibship size may bias the estimated relationship between education and sibship size. Notably, the existence of bias depends on the estimand of interest. If one wished to assess the effect of differential fertility on average adult education, and if the birth-order distribution were due to mortality differences by birth order, then one would want results that take into account survival differences by birth order. In this case, the estimates for surviving sibships in Section 4.2.1 are appropriate. But one might also be interested in what the relationship between education and sibship size would be if birth orders were uniformly distributed within each sibship size. In that case, the baseline estimates would require adjustment.[64]

Appendix Figure 1 suggests that the distribution of birth orders within each sibship size is

[64]Irregularities in the birth order distribution that result from recall error do not have a clear remedy. The recall error described in the previous paragraph would also generate non-classical measurement error in sibship size.

highly nonuniform. For ever-born siblings and surviving siblings separately, the figure draws the frequency distribution of birth orders within each sibship size from 2 to 12.[65] If birth order had a uniform distribution within each sibship size, each curve would be flat. But for both ever-born siblings and surviving siblings, all of the curves slope downward, implying that early-born children are overrepresented in the sample. This result suggests that adjustment for birth order may refine the estimates shown in Figure 1.

To adjust the education-sibship size relationship estimates for the nonuniform birth order distribution, I first run a regression that allows for separate effects of family size and birth order:

$$highest\,grade_{ict} = \alpha_{ct} + \gamma_{ct} sibsize_{ict} + \delta_{ct} order_{ict} + \varepsilon_{ict} \tag{9}$$

This regression specification simply adds birth order, $order_{ict}$, to Equation (2). If the distribution of birth orders within each sibship size were uniform, then $\beta_{ct} = \gamma_{ct} + \frac{1}{2}\delta_{ct}$, where β_{ct} is the coefficient on sibship size from Equation (2). The estimates of Equation (9) are thus useful for generating the counterfactual education-sibship size relationships that would arise under a uniform distribution of birth orders within each sibship size. On the other hand, estimates of γ_{ct} and δ_{ct} are also of separate interest because they contribute to the literature that disentangles family size and birth order effects. This paper does not focus on causal effects, but the estimates are nonetheless relevant.

Appendix Figures 2 and 3 display the adjusted sibship size and birth order coefficients, respectively. The representation of the estimates is the same as in Figure 1, with ever-born sibling estimates in blue and surviving sibling estimates in red. Comparing Appendix Figure 2 to Figure 1, observe that the change from positive to negative coefficients becomes more pronounced when one controls for birth order. In other words, the regime shift from a positive to a negative education-sibship size relationship was strongest for early-born children. This finding implies a countervailing shift in the relationship between education and birth order, which Appendix Figure 3 confirms. Estimates of δ_{ct} move from weakly negative to weakly positive, implying that early-born children used to hold an advantage over their later-born siblings, while now, the opposite is true. Note that many of the confidence intervals contain zero, so the inferences we can draw from the data are limited. Notwithstanding this imprecision, the shift in the relationship between education and birth order

[65] Figure 3 does not draw a curve for sibship size 1 because that sibship size trivially consists only of first-born children.

raises interesting questions about changes in intra-household resource allocation. Although these questions are beyond the scope of this paper, the shift suggests that adjusting estimates of β_{ct} to (counterfactually) simulate a uniform birth order distribution may lead to new conclusions.

However, the counterfactual estimates do not differ much from the estimates based on the observed birth order distribution. Appendix Figure 4 represents the two sets of coefficients in a scatterplot. The estimates assuming a uniform birth order distribution within each sibship size, or $\hat{\gamma}_{ct} + \frac{1}{2}\hat{\delta}_{ct}$, appear on the vertical axis. The baseline estimates, $\hat{\beta}_{ct}$, appear on the horizontal axis. For both ever-born siblings and surviving siblings, the scatterplots are clustered around the 45° line, indicating that adjustment for the nonuniform birth order distribution does not substantively alter estimates of the education-sibship size relationship. Ultimately, because many of the changes in the education-birth order relationship are small, and because the adjustment formula multiplies that relationship by one-half, the adjustment turns out to be unimportant.

Appendix 2: Variance of the Composition Effect Estimator

This appendix describes the use of the delta method to calculate the variance of the estimator of the composition effect of differential fertility. Consider a population consisting of women from sibship sizes $1, 2, \cdots, K$. Let μ_k be the mean education level among women from sibship size k, and let η_k be the proportion of women from that sibship size. Define $\mathbf{I}$ to be the identity matrix of dimension K, and define the following $K \times 1$ vectors: $\mu = [\mu_1, \mu_2, \cdots, \mu_K]'$, $\eta = [\eta_1, \eta_2, \cdots, \eta_K]'$, $\mathbf{1} = [1, 1, \cdots, 1]'$, and $\iota = \left[1, \frac{1}{2}, \cdots, \frac{1}{K}\right]'$. Then the composition effect of differential fertility is:

$$g(\mu, \eta) = \left[\eta - \left\{\left(\iota'\mathbf{I}\eta\right)'\mathbf{1}\right\}^{-1}\left(\iota'\mathbf{I}\eta\right)\right]'\mu$$

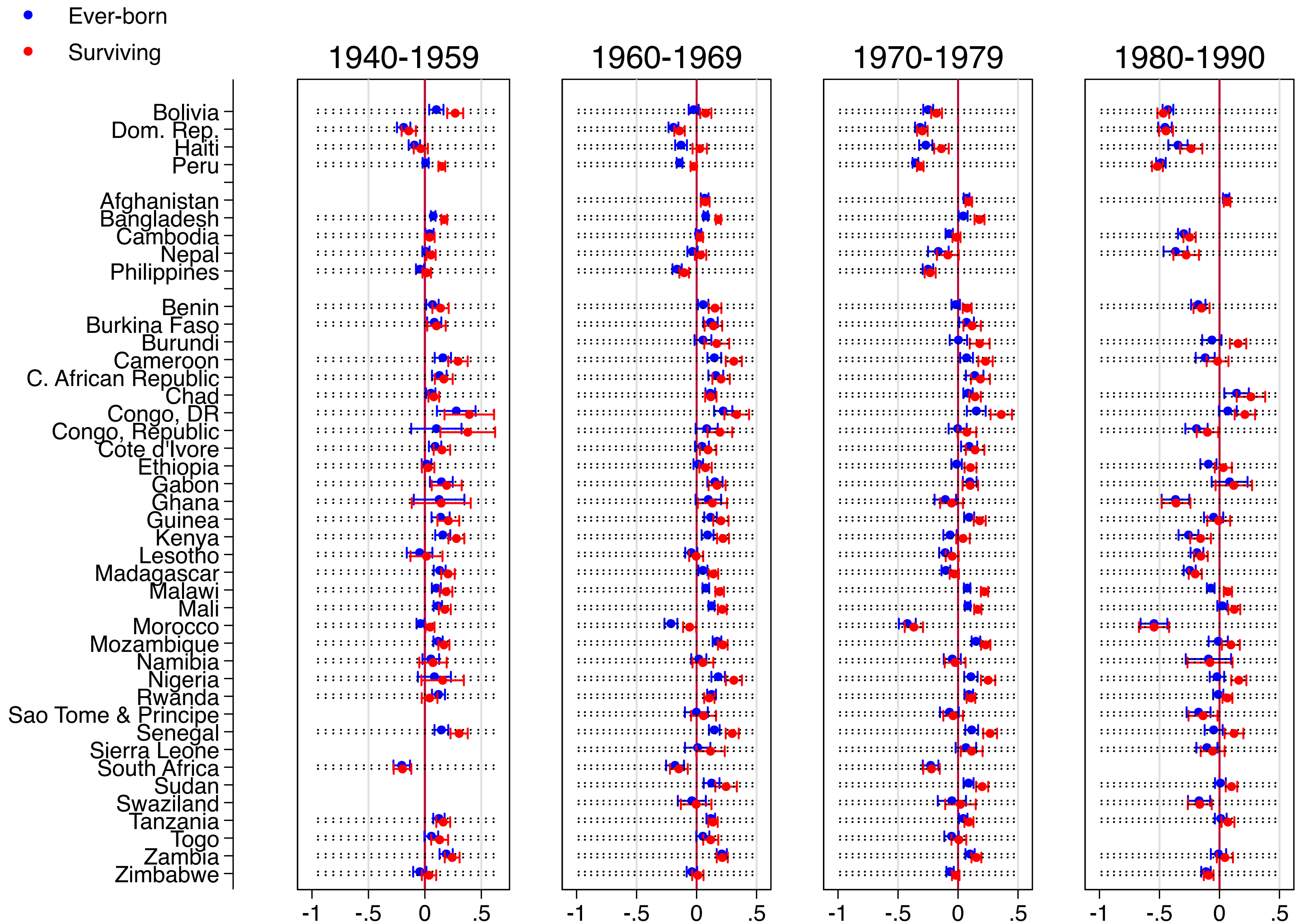
Let $\hat{\mu}$ and $\hat{\eta}$ be estimators of μ and η, respectively, and define the covariance matrix $\Sigma = V\left[\begin{pmatrix}\hat{\mu}\\ \hat{\eta}\end{pmatrix}\right]$. Then the estimator for the composition effect of differential fertility is:

$$\hat{g}(\mu, \eta) = g(\hat{\mu}, \hat{\eta})$$

And the delta method estimator of the variance is:

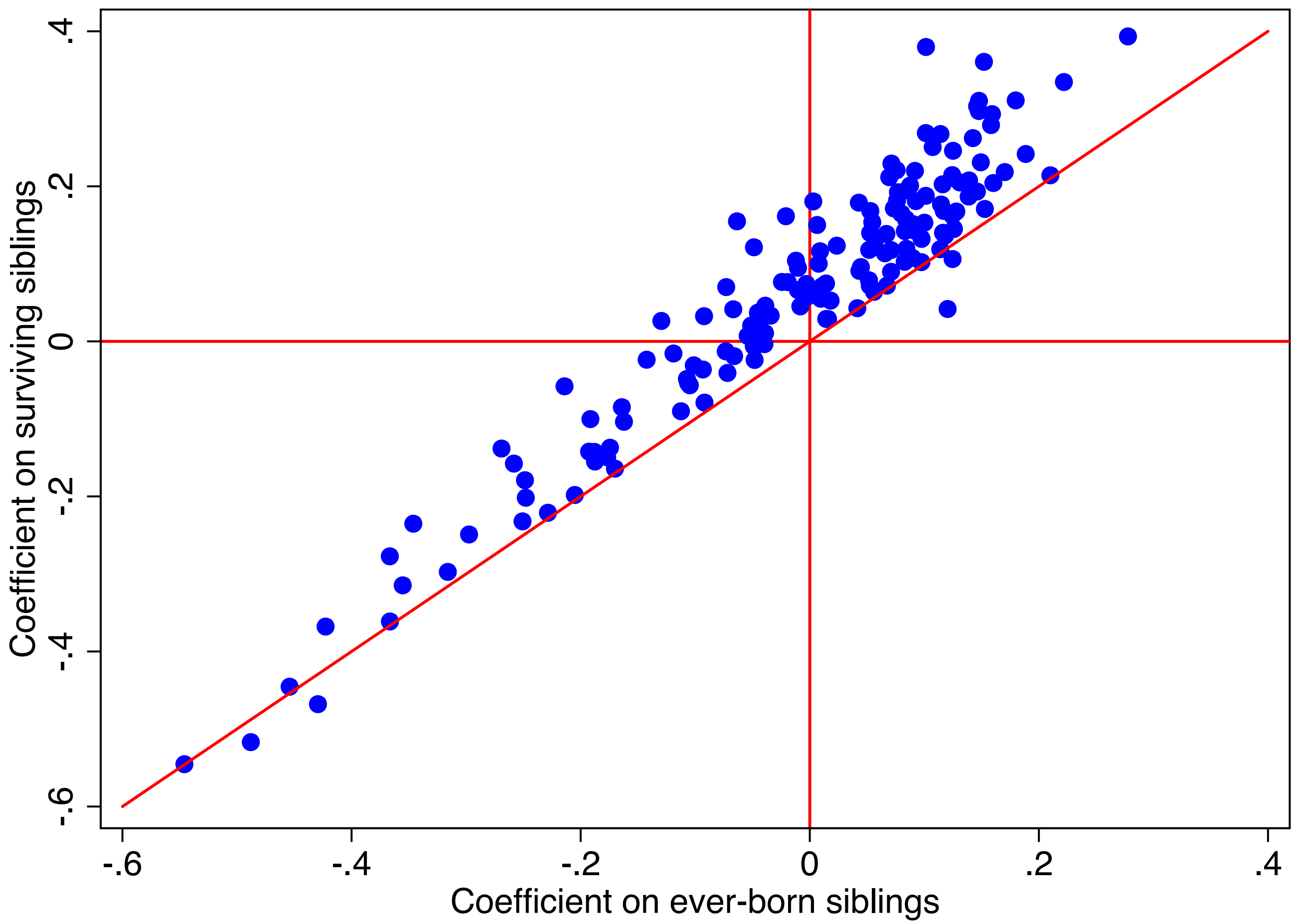
$$\hat{V}\left[\hat{g}(\mu, \eta)\right] = \nabla g(\hat{\mu}, \hat{\eta})' \cdot \hat{\Sigma} \cdot \nabla g(\hat{\mu}, \hat{\eta})$$

Figure 1: Education-Sibship Size Coefficients by Period of Birth



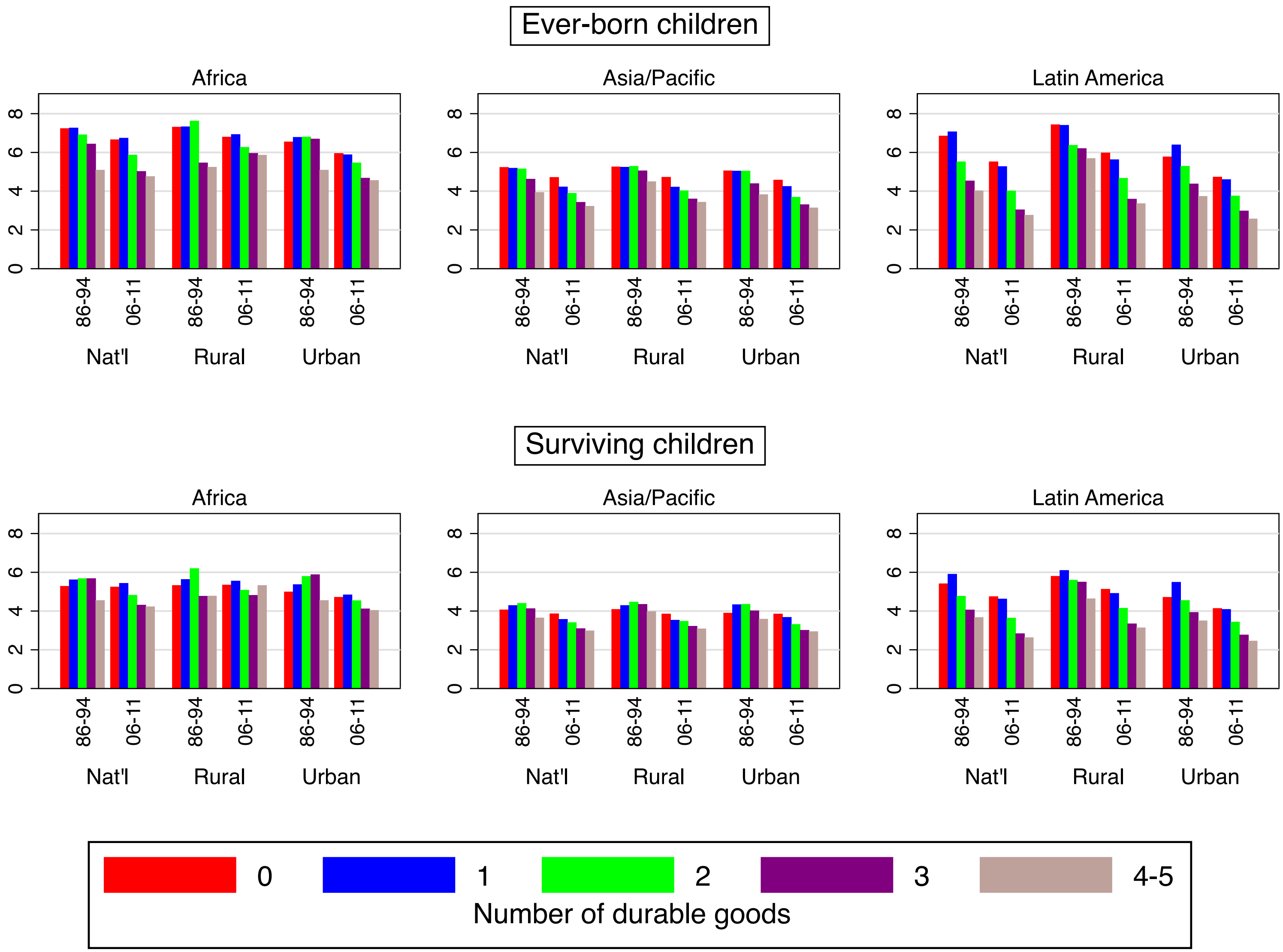
Note: From regressions of years of education on sibship size and birth year indicators. Bands represent 95% CIs. Data source: DHS Sibling Histories.

Figure 2: Education-Sibship Size Coefficients using Surviving vs. Ever-born Sibship Size



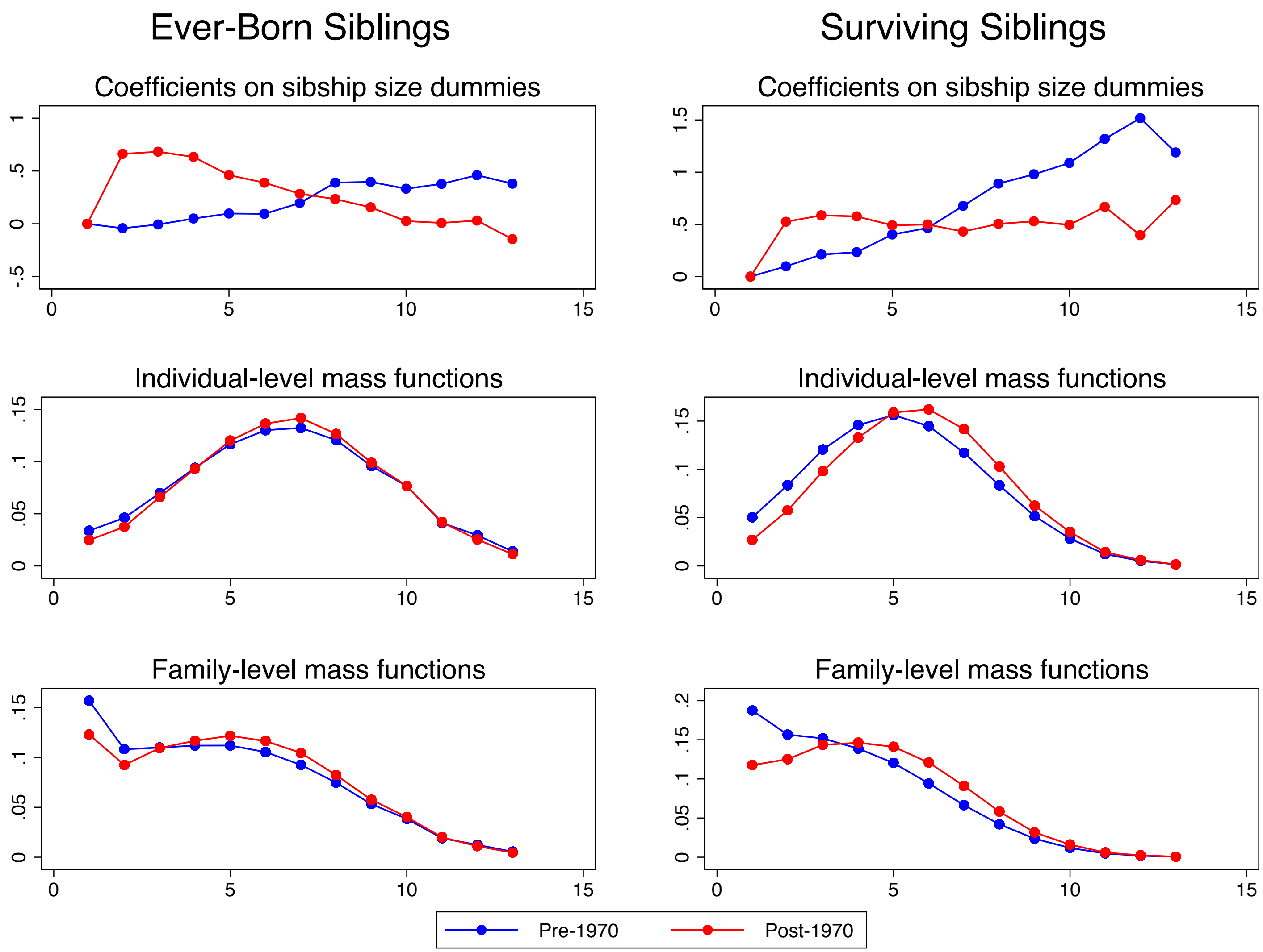
Note: Coefficients are as reported in Figure 1. Data source: DHS Sibling Histories.

Figure 3: Completed Fertility by Household Durable Goods Ownership



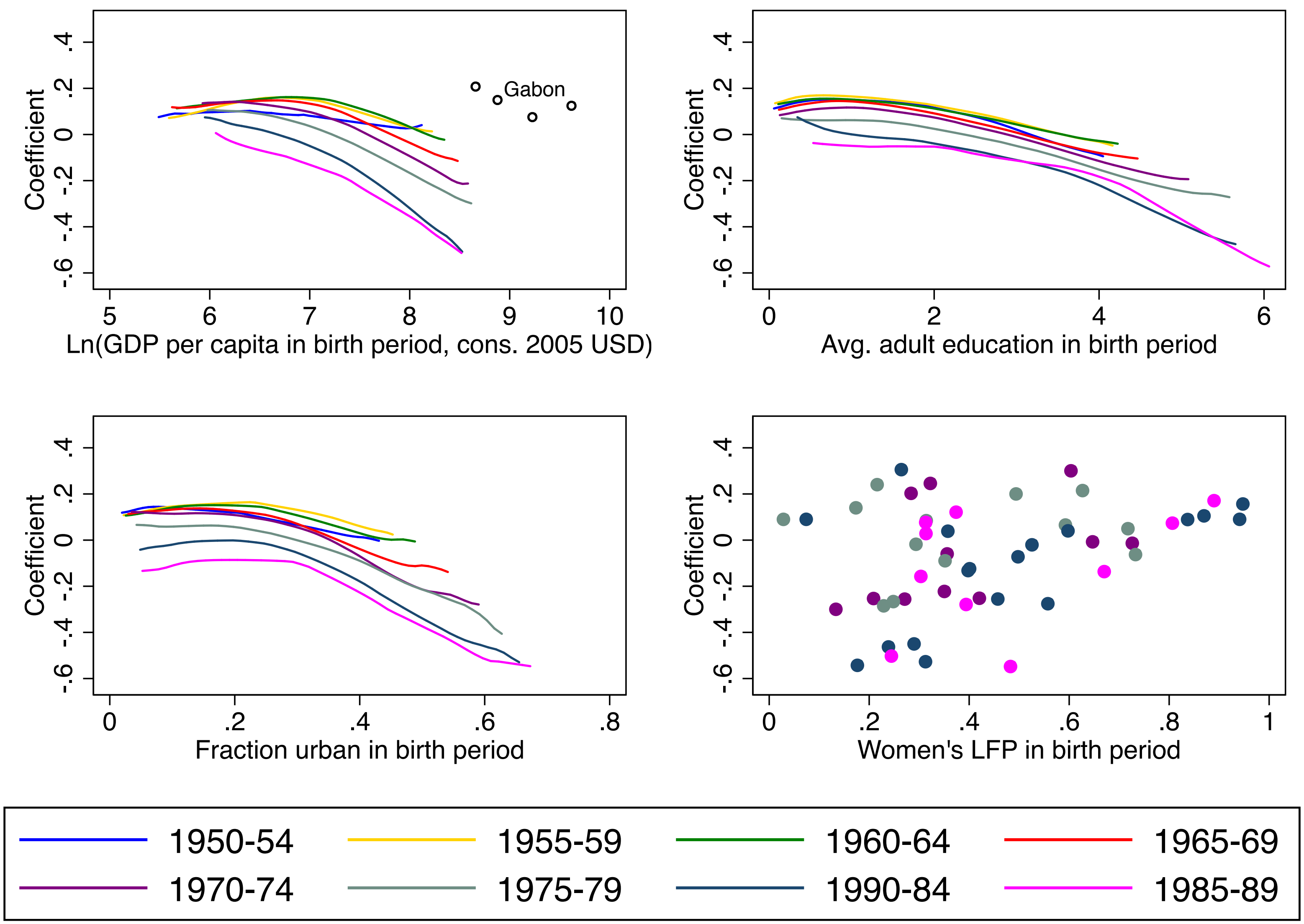
Note: Continental averages of country-specific averages. The durables index is the sum of ownership dummies for radio, television, refrigerator, motorcycle, and car. Data source: women age 45-49 in the DHS Fertility Histories.

Figure 4: Non-Parametric Relationships between Sibship Size and Educational Attainment



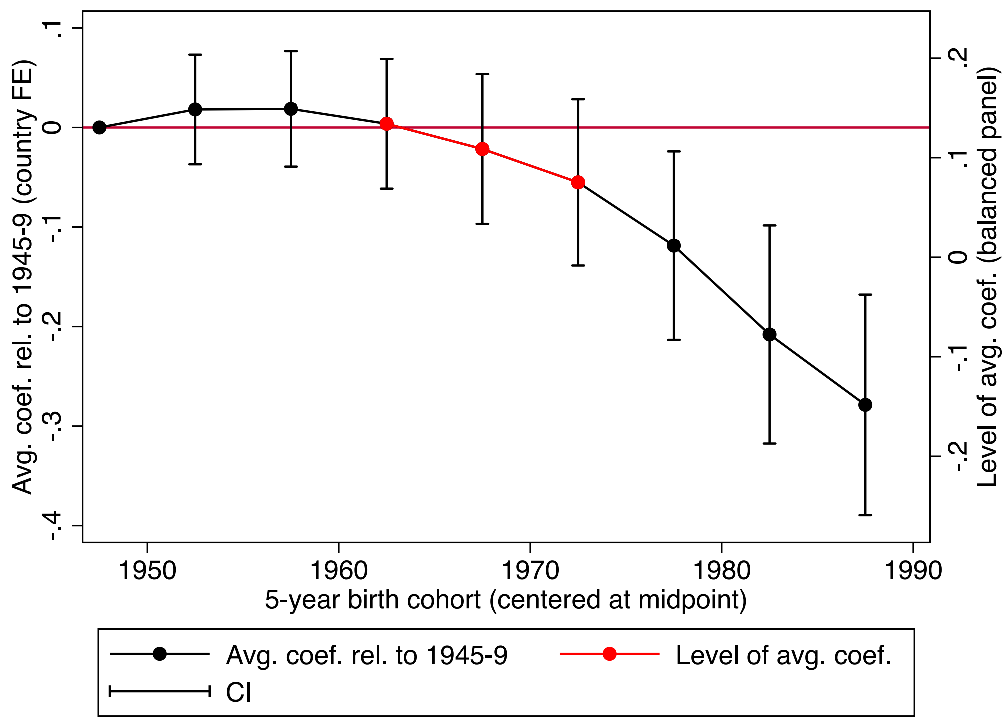
Note: For each country and birth year category, I regressed educational attainment on a vector of sibship size dummies and a vector of birth year dummies. I then averaged the pre- and post-1970 coefficients across countries, which I plot in the top panel. The middle panel shows the individual-level probability mass function for sibship size in the surviving adult population. The bottom panel reweights the individual-level mass function by the inverse of family size, in order to estimate the distribution of sibship sizes across families rather than individuals. Data source: DHS Sibling Histories.

Figure 5: Cross-Sectional Determinants of the Education-Sibship Size Relationship



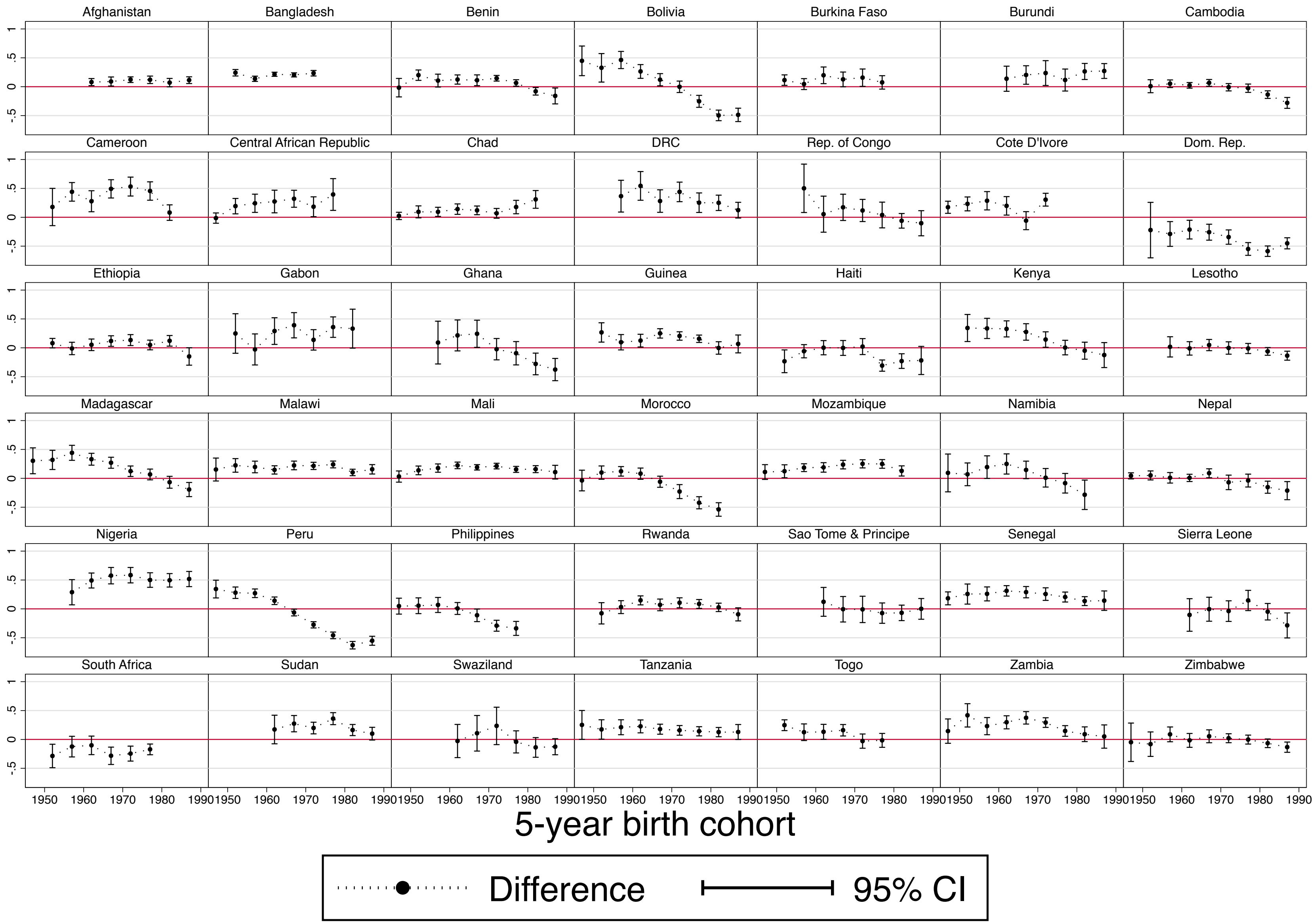
Note: 307 observations from 42 countries. The dependent variable is the coefficient from a regression of education on surviving sibship size. Data source: DHS Sibling Histories.

Figure 6: Time-Series of Education-Sibship Size Coefficients



Note: 307 observations from 42 countries. The dependent variable is the coefficient from a regression of education on surviving sibship size. Brackets contain standard errors clustered at the country level. Data source: DHS Sibling Histories.

Figure 7: Difference Between Observed and Reweighted Cohort Average Education



Note: The figure plots the observed mean minus the reweighted mean. The reweighted means were computed by dividing each woman's sampling weight by her surviving sibship size. Confidence intervals were calculated with the delta method. Data source: DHS Sibling Histories.

Table 1: Household Durable Goods Ownership and Completed Fertility

	Africa (Burkina Faso, Burundi, Cameroon, Ghana, Kenya, Madagascar, Malawi, Namibia, Niger, Nigeria, Senegal, Tanzania, Zambia, Zimbabwe)				Asia/Pacific (India, Indonesia)				Latin America/Caribbean (Colombia, Dominican Republic, Haiti, Peru)			
	Ever-born		Surviving		Ever-born		Surviving		Ever-born		Surviving	
	'86-'94	'06-'11	'86-'94	'06-'11	'86-'94	'06-'11	'86-'94	'06-'11	'86-'94	'06-'11	'86-'94	'06-'11
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
A. Urban and rural areas												
A1. Without urban residence indicator												
Durables index	-0.316*	-0.565*†	0.095	-0.314*†	-0.236*	-0.368*†	-0.023	-0.212*†	-0.883*	-0.780*	-0.582*	-0.599*
	[0.066]	[0.037]	[0.056]	[0.031]	[0.033]	[0.022]	[0.028]	[0.019]	[0.058]	[0.030]	[0.048]	[0.028]
N	6,269	13,860	6,269	13,860	11,721	14,007	11,721	14,007	3,929	12,313	3,929	12,313
A2. With urban residence indicator												
Durables index	-0.176*	-0.353*†	0.158*	-0.165*†	-0.188*	-0.332*†	-0.002	-0.196*†	-0.637*	-0.611*	-0.403*	-0.466*
	[0.071]	[0.040]	[0.059]	[0.034]	[0.037]	[0.024]	[0.031]	[0.020]	[0.061]	[0.035]	[0.052]	[0.031]
Urban	-0.852*	-1.253*†	-0.433*	-0.878*†	-0.270*	-0.238*	-0.115	-0.092	-1.356*	-1.037*	-1.004*	-0.843*
	[0.140]	[0.093]	[0.114]	[0.080]	[0.109]	[0.084]	[0.092]	[0.071]	[0.181]	[0.087]	[0.152]	[0.079]
N	6,269	13,860	6,269	13,860	11,721	14,007	11,721	14,007	3,929	12,313	3,929	12,313
B. Urban Areas												
Durables index	-0.218*	-0.507*†	0.081	-0.278*†	-0.328*	-0.342*	-0.124*	-0.223*	-0.703*	-0.609*	-0.477*	-0.480*
	[0.084]	[0.047]	[0.073]	[0.040]	[0.045]	[0.035]	[0.042]	[0.029]	[0.068]	[0.042]	[0.060]	[0.037]
N	1,704	4,212	1,704	4,212	3,654	6,242	3,654	6,242	2,513	8,123	2,513	8,123
C. Rural Areas												
Durables index	-0.069	-0.229*	0.280*	-0.078†	-0.057	-0.324*†	0.112*	-0.175*†	-0.454*	-0.620*	-0.212*	-0.452*†
	[0.108]	[0.058]	[0.089]	[0.051]	[0.056]	[0.033]	[0.044]	[0.028]	[0.124]	[0.058]	[0.105]	[0.053]
N	4,565	9,648	4,565	9,648	8,067	7,765	8,067	7,765	1,416	4,190	1,416	4,190

Note: Each entry is a simple average of country-specific coefficients; standard errors are in brackets. The durables index is the sum of ownership dummies for the following durable goods: radio, television, refrigerator, motorcycle, and car. Each regression controls for single-year age indicators and survey year indicators, and clusters standard errors at the PSU level. Sample sizes are the sum of the country-specific sample sizes. The sample includes a country if and only if it had at least one standard DHS survey with a full durable goods module in both the early and late periods. * sig. diff. from zero at 5% level; † sig. diff. from the early-period coefficient at 5% level. Data source: women age 45-49 in the DHS Fertility Histories.

Table 2: Education-Sibship Size Coefficients by Gender and Period of Birth

	1940-1949	1950-1959	1960-1969	1970-1982
	(1)	(2)	(3)	(4)
Indonesia				
Men	0.399	0.427	0.303	0.179
	[0.074]**	[0.063]**	[0.070]**	[0.156]
N	949	1,450	1,133	132
Women	0.418	0.383	0.295	0.085
	[0.065]**	[0.046]**	[0.056]**	[0.098]
N	1,076	1,614	1,762	479
Matlab, Bangladesh				
Men	0.309	0.274	0.172	0.143
	[0.086]**	[0.070]**	[0.076]*	[0.077]
N	751	920	894	780
Women	0.123	0.249	0.141	0.062
	[0.028]**	[0.039]**	[0.039]**	[0.067]
N	968	1,130	1,481	967
Mexico				
Men	0.05	-0.023	-0.186	-0.29
	[0.088]	[0.086]	[0.067]**	[0.045]**
N	845	1,256	1,644	2,154
Women	0.017	-0.038	-0.127	-0.29
	[0.066]	[0.068]	[0.052]*	[0.044]**
N	966	1,574	2,222	3,053

Note: OLS coefficients. Brackets contain standard errors clustered at the PSU level. Each coefficient is from a separate regression. * different from zero at 5% level; ** different from zero at 1% level. Data source: Family Life Surveys.

Table 3: Education-Sibship Size Coefficients with and without Parental SES Covariates

	1940-1949	1950-1959	1960-1969	1970-1982
	(1)	(2)	(3)	(4)
Indonesia				
Unadjusted	0.344	0.415	0.328	0.074
	[0.061]**	[0.051]**	[0.058]**	[0.103]
Adjusted for dad's ed.	0.189	0.225	0.119	-0.012
	[0.056]**	[0.040]**	[0.043]**	[0.088]
Adjusted for dad's and	0.183	0.211	0.099	-0.009
mom's ed.	[0.055]**	[0.039]**	[0.042]*	[0.089]
N	1,430	2,049	2,009	460
Matlab, Bangladesh				
Unadjusted	0.191	0.264	0.160	0.093
	[0.040]**	[0.038]**	[0.037]**	[0.052]
Adjusted for dad's ed.	0.102	0.138	0.071	0.119
	[0.037]**	[0.036]**	[0.034]*	[0.046]**
Adjusted for dad's and	0.087	0.130	0.064	0.131
mom's ed.	[0.037]*	[0.035]**	[0.033]*	[0.046]**
N	1,678	2,007	2,317	1,705
Mexico				
Unadjusted	0.032	-0.037	-0.162	-0.301
	[0.080]	[0.067]	[0.057]**	[0.037]**
Adjusted for dad's ed.	0.071	0.0002	-0.045	-0.154
	[0.070]	[0.057]	[0.048]	[0.034]**
Adjusted for dad's and	0.054	0.012	-0.006	-0.108
mom's ed.	[0.066]	[0.046]	[0.044]	[0.033]**
N	1,376	2,261	3,166	4,393

Note: OLS coefficients. Brackets contain standard errors clustered at the PSU level. Each coefficient is from a separate regression. The samples include both men and women, and all regressions control for a gender indicator. * different from zero at 5% level; ** different from zero at 1% level. Data source: Family Life Surveys.

Table 4: Demographic Correlates of the Education-Sibship Size Relationship

	Mean (SD)	OLS	Fuller	OLS	Fuller	OLS	Fuller
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Cohort average education	4.2	-0.043	-0.047				
	(2.7)	[0.023]*	[0.027]*				
Cohort average surviving	4.4			0.098	0.103		
sibship size	(0.7)			[0.036]**	[0.048]**		
Cohort average fraction of	0.10					0.40	0.58
siblings dying under 5	(0.04)					[0.83]	[1.30]
Birth Cohort FE		X	X	X	X	X	X
Country FE		X	X	X	X	X	X

Note: 307 observations from 42 countries. The dependent variable is the coefficient from a regression of education on surviving sibship size. Brackets contain standard errors clustered at the country level. The Fuller estimates are block-bootstrapped. * sig. at the 10% level; ** sig. at the 5% level. Data source: DHS Sibling Histories.

Table 5: Macroeconomic Determinants of the Education-Sibship Size Relationship

	OLS	OLS
	(1)	(2)
Ln(GDP per capita in birth period)	0.045	0.025
	[0.088]	[0.065]
Avg. adult yrs. ed. in birth period	-0.107	-0.060
	[0.028]**	[0.032]*
Fraction urban in birth period	-0.50	-0.48
	[0.42]	[0.49]
Fraction of value added in birth period, manufacturing		0.21
		[0.21]
Fraction of value added in birth period, services		0.01
		[0.22]
Number of observations	214	121
Number of countries	38	37
Birth Cohort FE	X	X
Country FE	X	X

Note: Brackets contain standard errors clustered at the country level. * sig. at the 10% level; ** sig. at the 5% level.

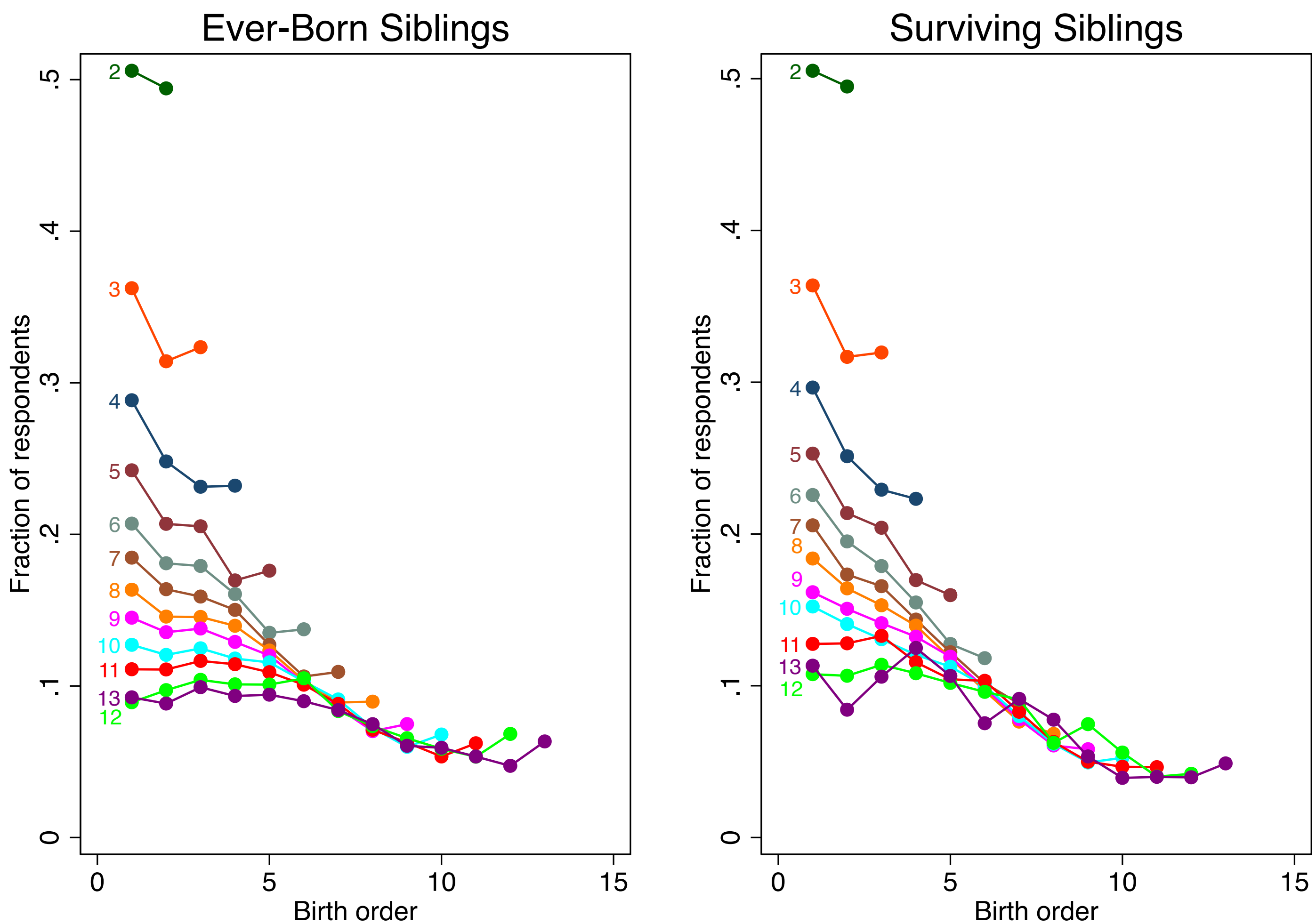
Table 6: Gender Determinants of the Education-Sibship Size Association

	OLS	OLS
	(1)	(2)
Women's labor force participation rate	0.072	
	[0.067]	
Avg. adult male yrs. ed. in birth period		-0.071
		[0.023]**
Avg. adult female yrs. ed. in birth period		-0.057
		[0.040]
p-value: joint test of education coefficients		0.001
p-value: difference of education coefficients		0.797
Number of observations	70	234
Number of countries	36	34
Birth Cohort FE	X	X
Country FE	X	X

Note: Brackets contain standard errors clustered at the country level. The sample for women's labor force participation omits the 1950 observation for Bolivia, in which both female and male labor force participation are unreasonably high. * sig. at the 10% level; ** sig. at the 5% level.

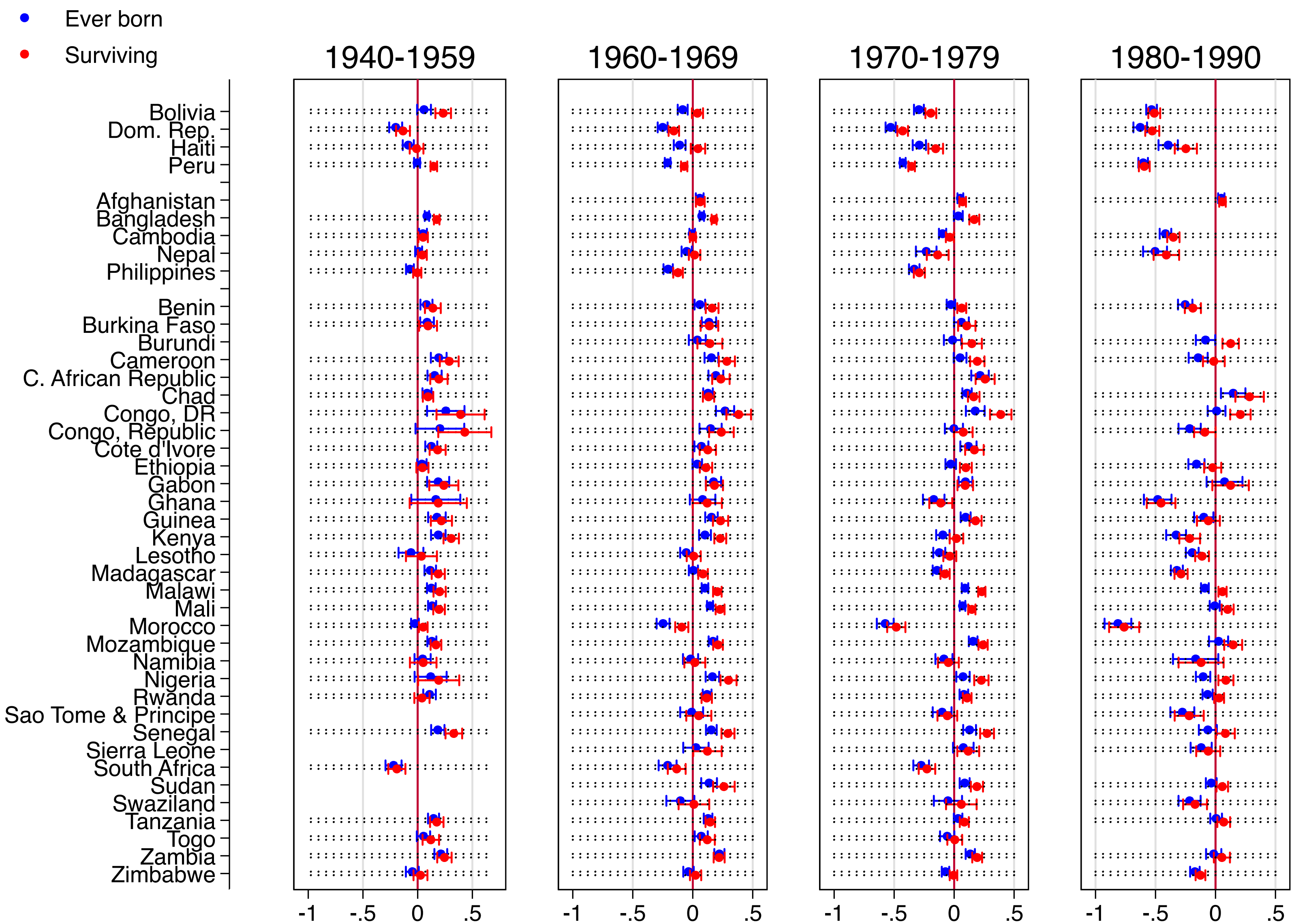
Appendix Tables and Figures

Appendix Figure 1: Distribution of Birth Orders by Sibship Size



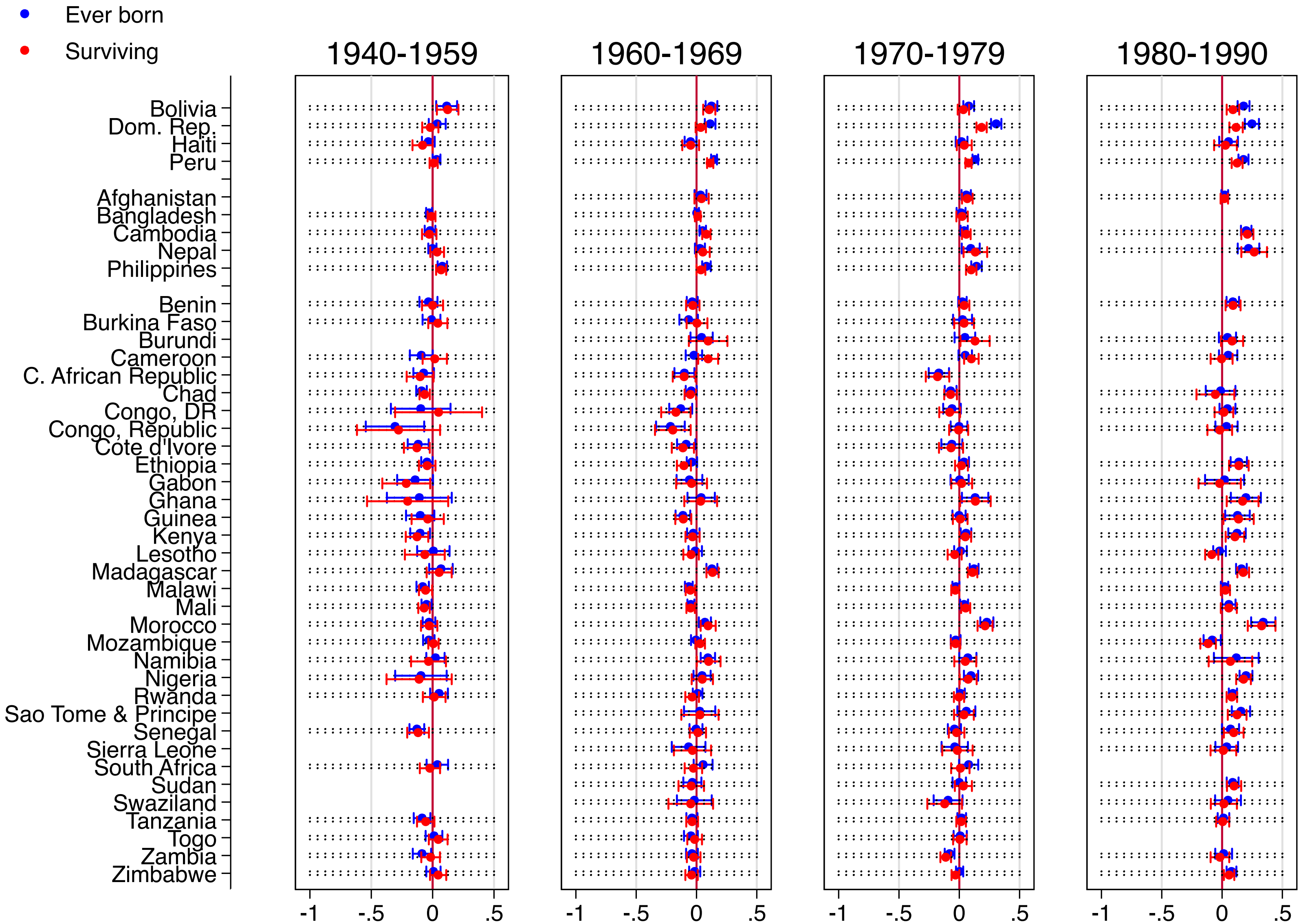
Note: The relative frequencies are first calculated within each country and then averaged across countries. Data source: DHS Sibling Histories.

Appendix Figure 2: Education-Sibship Size Coefficients by Period of Birth, Controlling for Birth Order



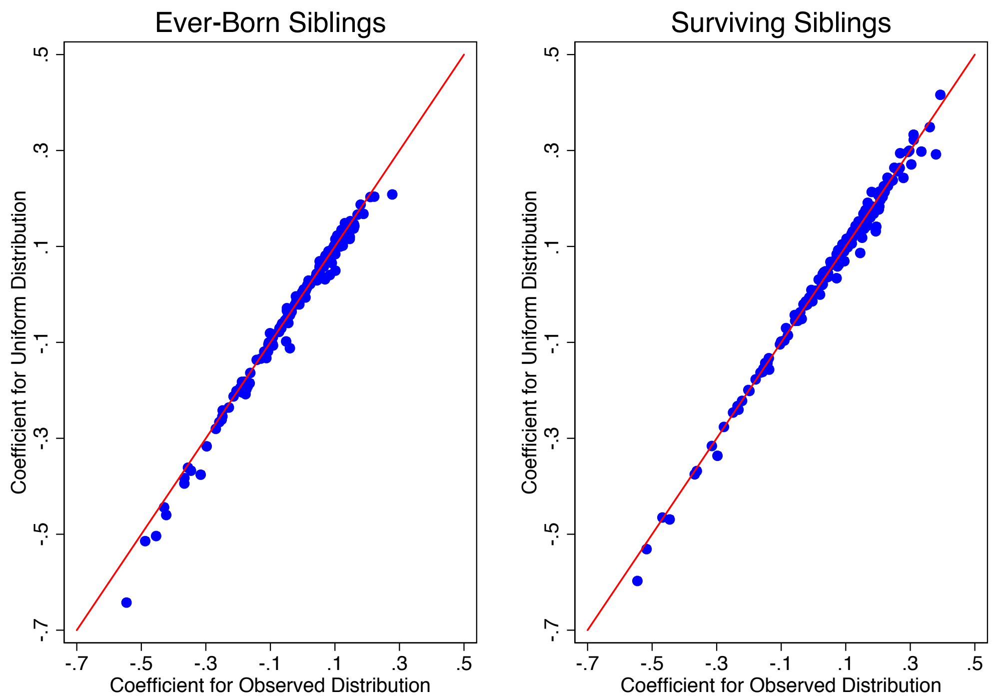
Note: From regressions of years of education on sibship size, birth order, and birth year indicators. Bands represent 95% CIs. Data source: DHS Sibling Histories.

Appendix Figure 3: Education-Birth Order Coefficients by Period of Birth, Controlling for Sibship Size



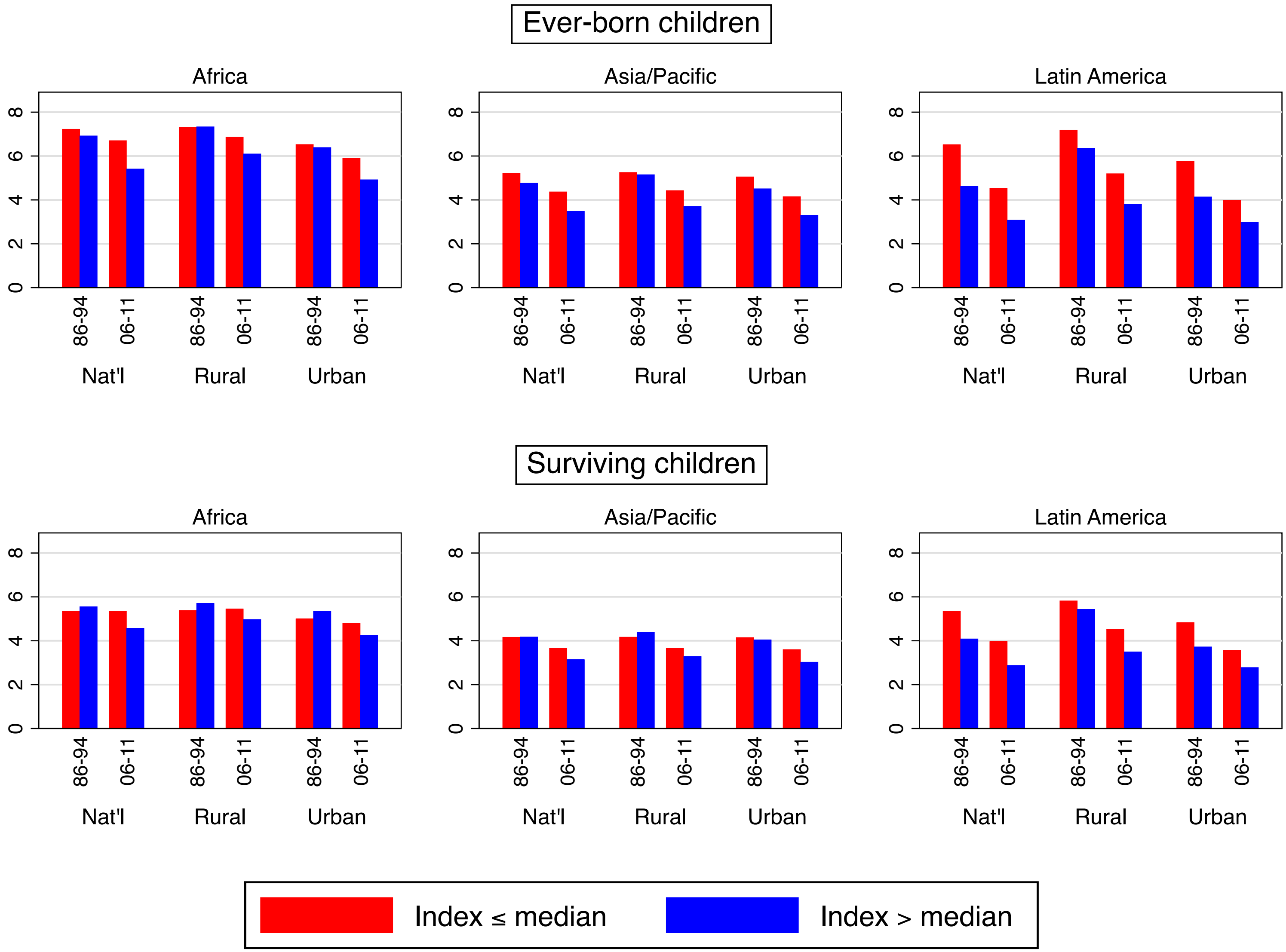
Note: From regressions of years of education on sibship size, birth order, and birth year indicators. Bands represent 95% CIs. Data source: DHS Sibling Histories.

Appendix Figure 4: Education-Sibship Size Coefficients under Observed and Uniform Birth Order Distributions



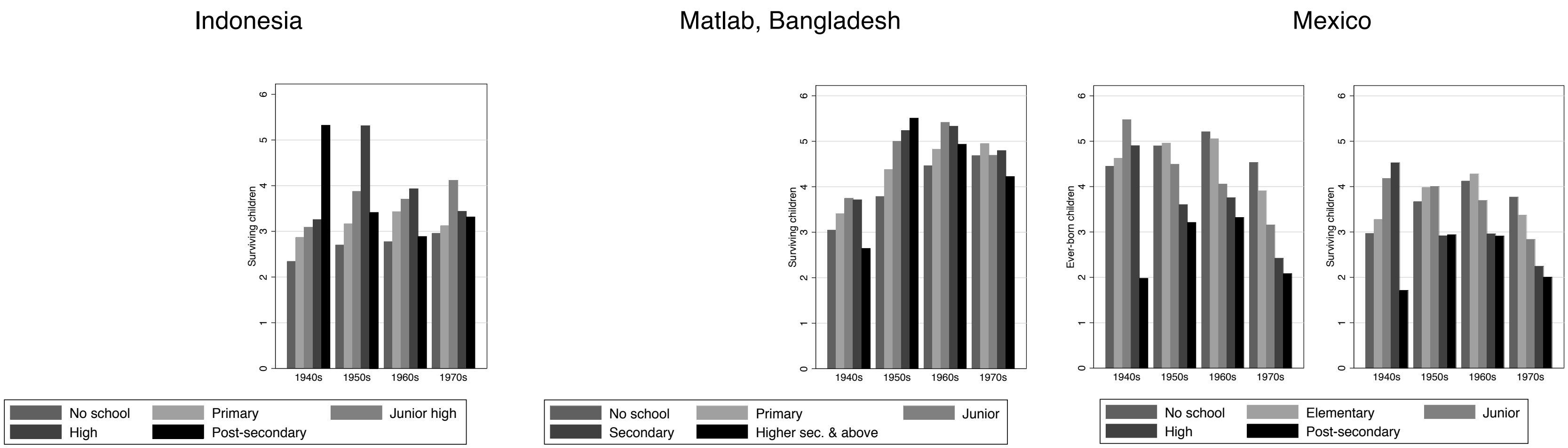
Note: The coefficient on the x-axis is the same as that plotted in Figure 2, from a regression of educational attainment on sibship size and birth year indicators. The coefficient on the y-axis is equal to the coefficient from Appendix Figure 3 plus one-half the coefficient from Appendix Figure 4, to simulate the univariate coefficient on sibship size if a uniform distribution of birth orders were observed for each sibship size. Data source: DHS Sibling Histories.

Appendix Figure 5: Completed Fertility by Household Durable Goods Ownership, Relative to the Median



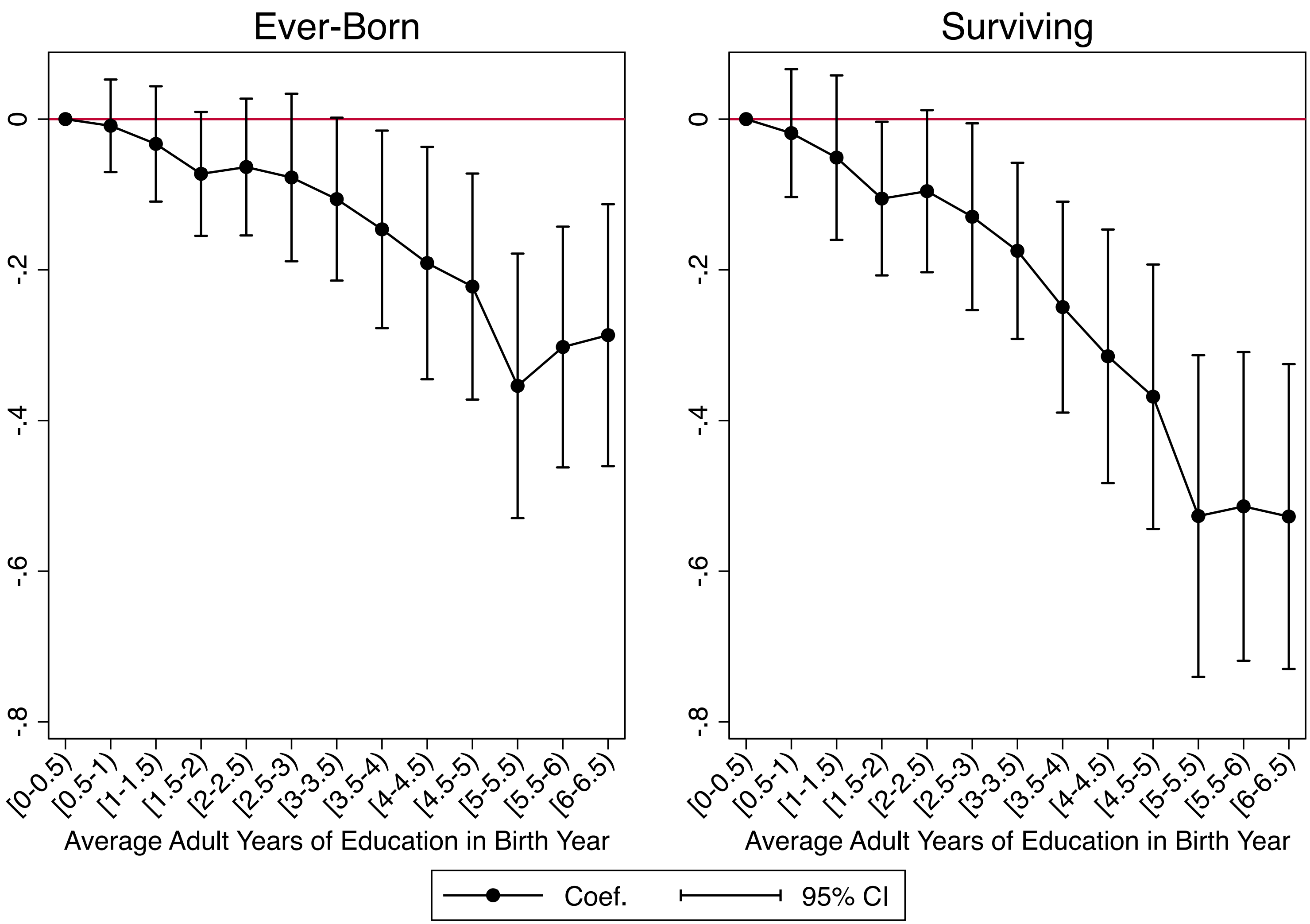
Note: Continental averages of country-specific averages. The durables index is the sum of ownership dummies for radio, television, refrigerator, motorcycle, and car. Data source: women age 45-49 in the DHS Fertility Histories.

Appendix Figure 6: Father's Education and Sibship Size



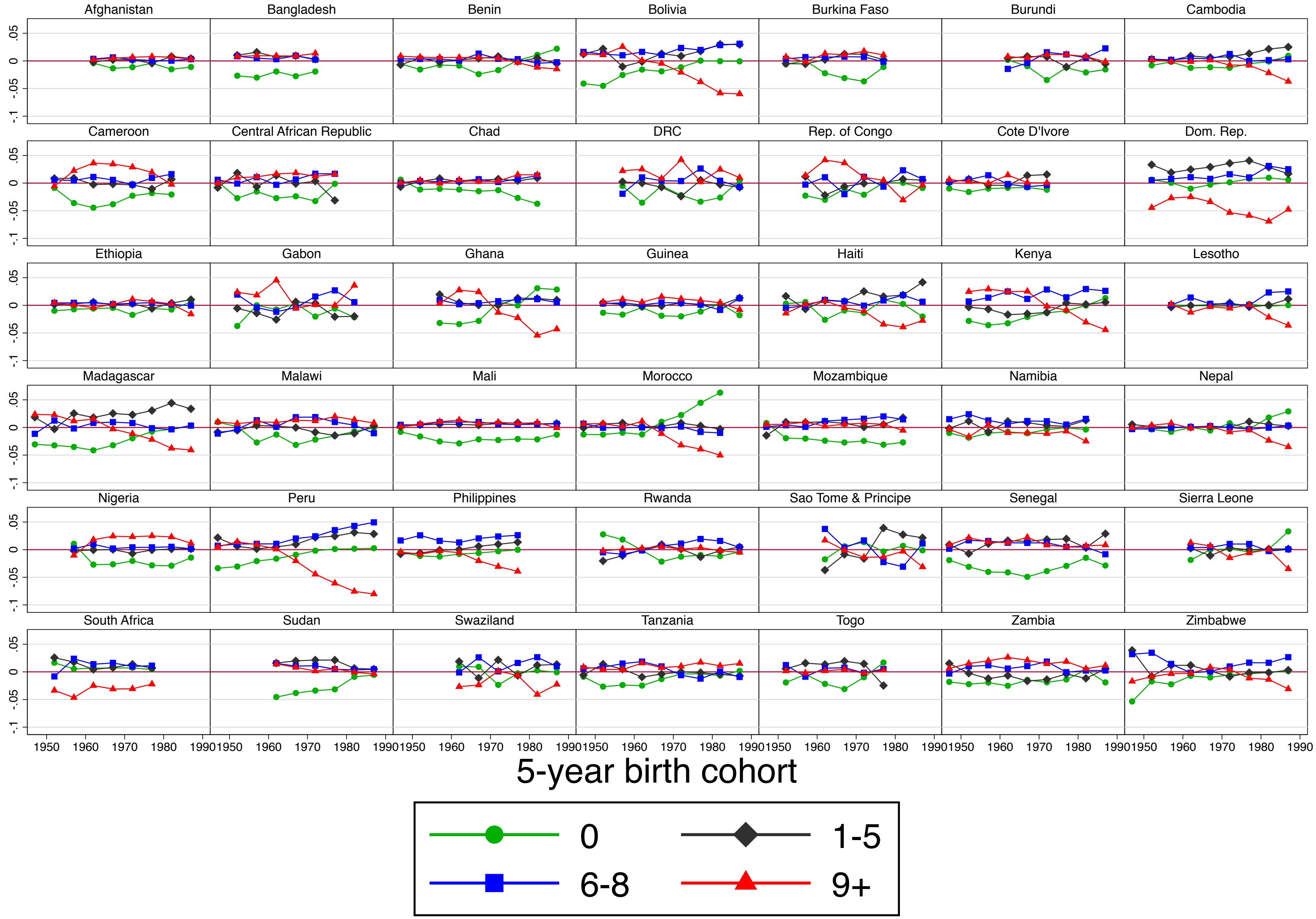
Note: Means are weighted by the survey weight divided by the surviving sibship size. Only the Mexico sample contains data on siblings who died in childhood, so the plot for ever-born sibship size is only possible for Mexico. Data source: Family Life Surveys.

Appendix Figure 7: Average Adult Education in Birth Period and the Education-Sibship Size Relationship



Note: From a regression of $\hat{\beta}$ on indicators for each average education category, with birth year and country fixed effects. Data source: DHS Sibling Histories.

Appendix Figure 8: Difference Between Observed and Reweighted Cohort Education Shares



Note: The figure plots the observed share in each education category minus the reweighted share. The reweighted shares were computed by dividing each woman's sampling weight by her surviving sibship size. Data source: DHS Sibling Histories.

Appendix Table 1: Avgs. of Country-Specific Means and Standard Deviations in the Fertility Histories

	Africa (Burkina Faso, Burundi, Cameroon, Ghana, Kenya, Madagascar, Malawi, Namibia, Niger, Nigeria, Senegal, Tanzania, Zambia, Zimbabwe)		**Asia/Pacific** (India, Indonesia)		**Latin America/Caribbean** (Colombia, Dominican Republic, Haiti, Peru)	
	'86-'94	'06-'11	'86-'94	'06-'11	'86-'94	'06-'11
	(1)	(2)	(3)	(4)	(5)	(6)
Ever-born fertility	7.14	6.32	5.12	4.02	5.79	4.04
	[3.22]	[2.88]	[2.67]	[2.29]	[3.43]	[2.44]
Surviving fertility	5.39	5.12	4.19	3.45	4.89	3.59
	[2.69]	[2.49]	[2.22]	[1.90]	[2.89]	[2.16]
Durables index	0.68	1.26	1.12	1.77	1.79	2.24
	[0.86]	[1.10]	[1.22]	[1.38]	[1.22]	[1.02]
Woman's years of education	1.39	3.60	2.88	4.21	3.63	6.31
	[2.27]	[3.76]	[3.75]	[4.41]	[3.64]	[4.59]
Husband's years of education	2.52	4.57	4.88	6.26	4.65	7.31
	[3.03]	[4.21]	[4.68]	[4.78]	[4.25]	[4.88]
Urban	0.21	0.30	0.28	0.38	0.57	0.65
	[0.39]	[0.43]	[0.45]	[0.48]	[0.47]	[0.44]
N	6,269	13,860	11,721	14,007	3,929	12,313

Note: Average means, with average standard deviations in brackets. Each entry represents a simple average of country-specific statistics. The mean for husband's years of education is for the subsample with non-missing values on that variable (roughly 94% of the overall sample). Sample sizes refer to the sum of the country-specific sample sizes. The sample includes a country if and only if it was the site of at least one standard DHS survey with a full durable goods module in both the early and late periods. The durables index is the sum of ownership dummies for the following durable goods: radio, television, refrigerator, motorcycle, and car. Data source: women age 45-49 in the DHS Fertility Histories.

Appendix Table 2: Socioeconomic Characteristics and Completed Fertility

	Africa (Burkina Faso, Burundi, Cameroon, Ghana, Kenya, Madagascar, Malawi, Namibia, Niger, Nigeria, Senegal, Tanzania, Zambia, Zimbabwe)				Asia/Pacific (India, Indonesia)				Latin America/Caribbean (Colombia, Dominican Republic, Haiti, Peru)			
	Ever-born		Surviving		Ever-born		Surviving		Ever-born		Surviving	
	‘86-'94	‘06-'11	‘86-'94	‘06-'11	‘86-'94	‘06-'11	‘86-'94	‘06-'11	‘86-'94	‘06-'11	‘86-'94	‘06-'11
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Durables index	-0.120*	-0.070	0.152*	-.0002†	0.007	-0.145*†	0.079*	-0.078*†	-0.391*	-0.293*	-0.234*	-0.212*
	[0.075]	[0.041]	[0.064]	[0.036]	[0.044]	[0.025]	[0.037]	[0.022]	[0.064]	[0.038]	[0.057]	[0.035]
Woman’s years of education	-0.067	-0.156*†	-0.044	-0.102*	-0.098*	-0.109*	-0.063*	-0.079*	-0.140*	-0.147*	-0.100*	-0.118*
	[0.038]	[0.013]	[0.036]	[0.012]	[0.015]	[0.010]	[0.013]	[0.008]	[0.021]	[0.010]	[0.018]	[0.009]
Husband’s years of education	0.026	-0.061*†	0.059*	-0.025*†	-0.025	-0.017	0.008	-0.002	-0.102*	-0.059*	-0.075*	-0.047*
	[0.029]	[0.011]	[0.025]	[0.010]	[0.014]	[0.010]	[0.012]	[0.008]	[0.020]	[0.011]	[0.018]	[0.010]
Urban	-0.795*	-0.815*	-0.431*	-0.603*	-0.064	-0.012	-0.029	0.063	-0.852*	-0.489*	-0.621*	-0.392
	[0.143]	[0.090]	[0.118]	[0.079]	[0.111]	[0.08]	[0.096]	[0.071]	[0.178]	[0.079]	[0.147]	[0.072]
N	6,269	13,860	6,269	13,860	11,721	14,007	11,721	14,007	3,929	12,313	3,929	12,313

Note: Each entry represents a simple average of country-specific coefficients, with the associated standard error in brackets. The durables index is the sum of ownership dummies for the following durable goods: radio, television, refrigerator, motorcycle, and car. Each country-specific regression controls for single-year age indicators and survey year indicators, and clusters standard errors at the PSU level. Sample sizes refer to the sum of the country-specific sample sizes. The sample includes a country if and only if it was the site of at least one standard DHS survey with a full durable goods module in both the early and late periods. * sig. diff. from zero at the 5% level; † sig. diff. from the early-period coefficient at the 5% level. Data source: women age 45-49 in the DHS Fertility Histories.

Appendix Table 3: Association between Parental SES and Surviving Sibship Size

	1940-1949		1950-1959		1960-1969		1970-1982	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Indonesia								
Mother's Yrs. of Ed.		-0.006		0.037		0.056		-0.022
		[0.029]		[0.030]		[0.025]*		[0.036]
Father's Yrs. of Ed.	0.105	0.108	0.111	0.090	0.101	0.069	0.049	0.062
	[0.020]**	[0.026]**	[0.019]**	[0.023]**	[0.019]**	[0.025]**	[0.025]*	[0.032]*
N	1,430	1,430	2,049	2,049	2,009	2,009	460	460
Matlab, Bangladesh								
Mother's Yrs. of Ed.		0.142		0.017		0.005		-0.052
		[0.071]*		[0.046]		[0.040]		[0.034]
Father's Yrs. of Ed.	0.066	0.044	0.145	0.141	0.087	0.086	-0.004	0.016
	[0.023]**	[0.025]*	[0.018]**	[0.020]**	[0.017]**	[0.019]**	[0.017]	[0.019]
N	1,678	1,678	2,007	2,007	2,317	2,317	1,705	1,705
Mexico								
Mother's Yrs. of Ed.		0.110		-0.078		-0.116		-0.085
		[0.046]**		[0.052]		[0.029]**		[0.017]**
Father's Yrs. of Ed.	-0.028	-0.073	-0.031	0.010	-0.072	-0.002	-0.112	-0.065
	[0.031]	[0.037]*	[0.039]	[0.041]	[0.016]**	[0.022]	[0.011]**	[0.016]**
N	1,376	1,376	2,261	2,261	3,166	3,166	4,393	4,393

Note: OLS coefficients. Brackets contain standard errors clustered at the PSU level. Each coefficient is from a separate regression. The samples include both men and women. Observations are weighted by the sampling weight divided by the sibship size. * different from zero at 5% level; ** different from zero at 1% level. Data source: Family Life Surveys.