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Maternal Education and Maternal Mortality: Evidence from a Large Panel and Various Natural Experiments

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Abstract

We examine whether a causal relationship exists between maternal education and maternal mortality. Despite considerable evidence in favour of a causal relationship between education and a range of other health behaviours and outcomes, and a significant gradient between maternal mortality and education across time and countries, no comprehensive study exists to examine whether this relationship is—at least in part—causal. By forming a large panel of data consisting of 108 countries over 20 years, and by examining three natural experiments resulting in plausibly exogenous expansions in education, we present considerable evidence that increases in maternal education causally reduce the likelihood of dying in child birth. The size of this relationship is considerable. Our preferred estimates suggest that a country moving from 0 to 1 years of education will reduce maternal mortality by 174 deaths per 100,000 births, while moving from 7 to 8 years results in a smaller, but still significant, 15 deaths per 100,000 births.

1 Introduction

Life expectancy exhibits a consistently positive gradient in education that, unlike income gradients, tends not to diminish as education levels rise and is evident across and within rich and poor countries (Richards and Barry, 1998; Kunst and Mackenbach, 1994; Elo and Preston, 1996;

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Lleras-Muney, 2005; Meara et al., 2008). Maternal mortality is an important determinant of life expectancy for women in developing countries but there is not much evidence of the relationship between maternal mortality and education. This paper attempts to fill that gap.

Every day, approximately 800 women die from preventable causes related to pregnancy and childbirth, 99% of all maternal deaths occur in developing countries and they are largely preventable (WHO, 2012). The maternal mortality ratio in developing countries is 240 per 100,000 births versus 16 per 100,000 in developed countries. There are large disparities between and within countries, with a few countries having extremely high maternal mortality ratios of 1,000 or more per 100,000 live births; more than half of all maternal deaths occur in sub-Saharan Africa and almost one third occur in South Asia (WHO, 2012). The WHO identifies poverty, limited access to public services, cultural practices and lack of information as the main causes or sources of variation in maternal mortality ratios.

Between 1990 and 2010, maternal mortality worldwide dropped by almost 50%. While this is impressive, certainly relative to trends in the preceding twenty years, the average rate of decline in the global maternal mortality ratio (the number of maternal deaths per 100,000 live births) of 3.1% per year over this period falls well below the annual decline of 5.5% required to achieve the Millennium Development Goals (MDG) for maternal health adopted by the international community in 2000. There are signs of increasing policy commitment to addressing maternal mortality: official development assistance for maternal and new-born health has risen relative, for instance, to funding for child health (Greco et al., 2008).[1]

However policy documents on maternal mortality seldom indicate education as a cause and the academic literature has very little to say about the relationship between maternal mortality and education. This said, there is a lively literature in economics that documents a positive correlation between education and other indicators of health. One possibility, relatively uninteresting for policy, is that this correlation is driven by time preference, people with lower discount rates tending to invest more in both forms of human capital. While this is likely so, the crop of studies that identify causal effects of education on health most often tend to associate this with educated people engaging in healthier behaviours, for instance, smoking and drinking less and being more likely to seek prenatal care, adopt new drugs or adhere to treatment for disease (Goldman and Smith, 2010; Goldman and Lakdawalla, 2001; Currie and Moretti, 2003; Lichtenberg and Lleras-Muney, 2005; Cutler and Lleras-Muney, 2010; Glied and Lleras-Muney, 2008;

[1] Overall donor disbursements increased from US$2,119 million in 2003 to $3,482 million in 2006; funding for child health increased by 63% and that for maternal and new-born health increased by 66%. In the 68 priority countries, child-related disbursements increased from a mean of $4 per child in 2003 to $7 per child in 2006; disbursements for maternal and neonatal health increased from $7 per live birth in 2003 to $12 per live birth in 2006.

Jensen and Lleras-Muney, 2012). This is consistent with the efficacy that education confers in acquiring and processing relevant information (Rosenzweig, 1995; Rosenzweig and Schultz, 1989; Cutler and Lleras-Muney, 2010). Education may also influence health through its influence on income and, thereby, the facility to purchase health inputs, but the impacts of education on health tend to be larger than direct impacts of income, to hold conditional upon income, and to be similar for men and women, at least in richer countries (Lleras-Muney, 2005; Cutler and Lleras-Muney, 2010).

The proximate causes of maternal mortality are pregnancy complications such as pre-eclampsia, bleeding, infections and unsafe abortion.[2] To the extent that educated women are more likely to adopt simple and low-cost practices to maintain hygiene, are more able to react to symptoms such as bleeding or high blood pressure, more likely to assess the information on abortion and place of abortion, and more willing to accept treatment and birth attendance, education may plausibly cause declines in maternal mortality. Moreover, if these are the mechanisms by which it acts then education may have an impact on maternal mortality conditional upon the state of health services. The payoff to education tends to be higher in circumstances where learning is important, for instance, circumstances of structural and technological change (Foster and Rosenzweig, 2004). Such change is characteristic of developing countries which, in the post-war period, have experienced massive technological change in the public health sphere (Cutler et al., 2006) as well as changes in the economic (and occupational) structure.

Using panel data for 108 developing countries and new estimates of maternal mortality ratios for 1990-2010, we estimate the relationship between maternal mortality and educational attainment under the standard panel data identifying assumptions, unconditional and conditional upon indicators of income, public health provision and fertility. We find large and consistently significant impacts of education. Indeed, depending upon the specification, the (standardized) impacts of education on maternal mortality are equal to or larger than the impacts of skilled birth attendance, an instrument that the WHO currently focuses upon in devising programs to address maternal mortality. In contrast, within-country variation in GDP has no significant impact on maternal mortality conditional upon education. We supplement the cross-country analysis with quasi-experimental case studies from Nigeria, Zimbabwe and Kenya. These are three countries that implemented substantial education reforms. We use cohort and region variation in education generated by the reform, which we argue is plausibly exogenous. To match this to maternal mortality rates, we construct cohort mortality rates for regions within these countries using the Demographic and Health Surveys. In two of the three case we identify a sizeable and negative impact of improvements in education on maternal mortality. In both Nigeria and Zimbabwe

[2] We provide a more extensive discussion of the medical literature related to maternal mortality and death in and surrounding childbirth in appendix B.

we find that the educational expansions due to policy experiments significantly reduce maternal death in affected cohorts, and find that these results are surprisingly close to those estimated in cross-country regressions. In Kenya—which experienced a schooling extension at the lower high school level—we do not find evidence of such a reduction, estimating that the effect on maternal mortality is not significantly different to zero.

All in all, this paper generates new data on maternal mortality that is available for individuals within regions and states of countries, and it provides what would appear to be the first systematic consideration of the relationship between maternal mortality ratios and education. It opens up several avenues for further research.

2 Methodology

2.1 Cross-Country Analysis

In order to examine the effect of educational expansions on maternal mortality, we run country-level regressions of the following form:

$$MMR_{it} = \alpha_i + \mathbf{educ}_{it}\beta + \mathbf{W}_{it}\gamma + \delta_t + \varepsilon_{it}, \tag{1}$$

where MMR_{it} refers to the maternal mortality rate in country i and period t, $\mathbf{educ}_{it}$ to a series of variables measuring the percent of the population with a given level of education, and $\mathbf{W}_{it}$ refers to a vector of relevant country- and time-varying covariates. Given that we have both temporal and regional variation in the variables of interest, we employ a fixed-effects methodology which allows for us to control for all time-fixed country-specific factors which may confound the analysis in (1). The ability to eliminate these time invariant factors is particularly important in this analysis, given that many unobserved factors, such as the institutional and political aspects of a given country, are likely to be correlated with both maternal mortality and educational attainment. Whilst some of these variables are not strictly "time-invariant", they evolve sufficiently slowly that our analysis over a comparatively short time horizon will mean that a fixed-effect estimator will largely control for them. We cluster standard errors at the level of the country (allowing for the stochastic term u_{it} to be correlated over time for each i.)[3]

[3] In certain cases when we restrict to only certain sub-groups of countries, the number of countries in each regression is less than 50. Rule of thumb-type considerations suggest that clustering standard errors using less than around 50 clusters is not optimal (Angrist and Pischke, 2009; Nichols and Schaffer, 2007). In these cases we also estimate using heteroscedasticity robust standar errors, and wild bootstrapped standard errors (MacKinnon and Webb, 2014; Cameron and Miller, 2015). To be conservative, we always report the set of estimates with the

We are able to estimate (1) for a number of different subgroups. Whilst we primarily focus on women of reproductive age, our data allows us to run a similar analysis for educational attainment of both gender groups and for various age subgroups created within the fertile age range. We also segregate countries by region- and income-level, and present group-specific estimates of the above specification. A full list of the educational variables which we use and a description of the time varying covariates $\mathbf{W}_{it}$ is provided in the data section of this document.

2.2 Natural Experiments

In addition to (1) which we estimate with panel data, we focus on a number of country-specific cases in countries which have experienced isolated, policy-defined expansions in schooling achievement. Such expansions allow for us to causally identify the effect of education on maternal mortality if we believe that we can identify similar subgroups of individuals who are differentially affected by these isolated shocks to education. In these country case studies, we construct comparable subgroups of women who entered school on either side of an important educational reform. We exploit variation in the intensity of treatment between regions and cohorts of birth to estimate the effect of educational expansions on maternal mortality.

Prominent educational reforms discussed in the economics literature are listed below in table 1. Of the reforms listed here, we identify three which are suitable to use as natural experiments to identify the effect of education on maternal mortality. These are Nigeria, Zimbabwe, and Kenya. Of the other three cases listed, they were either not examined as they occurred too recently to observe the outcome of interest (maternal mortality) or because microdata for maternal mortality outcomes was not available for the country in question. For each of the three natural experiments examined, we briefly describe the nature of the reform, and the identification strategy used in estimation.

2.2.1 Nigeria 1976

The Universal Primary Education (UPE) program was initiated in Nigeria in September 1976 as a national program directed at primary school enrolment. This program involved the provision of free primary school education, along with significant classroom construction (Osili and Long, 2008). The gross primary enrolment rate doubled between 1974 and 1981 for male students, and nearly tripled for females. Osili and Long (2008) suggest using a double-differences identification

largest standard errors (on our variables of interest).

strategy in which women from cohorts born between 1970 and 1975 in high-intensity states[4] act as the treatment group. As per Osili and Long, rather than opt for a control group immediately older than the treatment group, we examine women born between 1956-1961 in order for our analysis to not be confounded by over- and under-age enrolments in primary school. We also examine the effect on cohorts born between 1965 and 1970, which may be partially affected by the reform given over-age enrolment in primary school.

Two measures of treatment intensity are defined: a dummy for those living in high-intensity states, and a measure of federal capital funds dispersed per capita in each state in 1976 (the year of the reform)[5]. The effect of UPE on schooling and maternal mortality is then estimated using a difference-in-difference framework:

$$y_{ijk} = \alpha + \beta \text{ UPE Cohort}_{jk} + \gamma \text{UPE Input}_k + \delta(\text{UPE Input}_k \times \text{UPE Cohort}_{jk}) + \mathbf{X}'_{ijk}\theta + \varepsilon_{ijk}. \quad (2)$$

The schooling and maternal mortality outcomes (y) for individual i in cohort j and state k are regressed on controls for year of birth, religion, ethnicity, and additional controls for groups differentially affected by the 1967-'70 civil war (see Akresh et al. (2012) for further discussion.) The UPE Cohort dummy captures whether an individual was of primary school age at the time of the reform (year of birth between 1970 and 1975 inclusive), and UPE Input is a dummy for whether an invididual resides in a high-intensity treatment state, or the investment per capita of a given individual's state as discussed above. For educational outcomes we expect that δ—the reduced form estimate of the effect of UPE—should be positive, and, if education reduces maternal mortality, that $\delta < 0$ for maternal mortality outcomes. Further details and summary statistics for outcome and treatment variables are provided in section 3.

2.2.2 Zimbabwe 1980

Agüero and Bharadwaj (2014) use extensions of schooling availability to black citizens in 1980 after independence in Zimbabwe to isolate the effect of education on health investments. This extension in years of education was observed in high-school age students, with the authors demonstrating a discontinuity between the ages of 14 and 15. The reform in question occurred in April 1980, and has been used in the literature in earlier papers (see for example Edwards (1995)).

[4]Prior to the UPE program the non-Western states of Nigeria had low primary enrolment rates and low levels of investment per capita. With the introduction of the UPE, these states experienced a considerable expansion in educational investments (Osili and Long, 2008), and so are defined as 'high-intensity' states.

[5]Two additional treatment measures are examined by interacting funds per capita with treatment state, and funds per capita with number of years affected by the treatment. Further details are available as notes to Table 7a.

In estimating the effect of the reform we follow Agüero and Bharadwaj (2014) in estimating around the discontinuity in educational attainment between the cohorts aged 14 and 15 in 1980:

$$y_{ij} = \beta_1 \text{DumAge}_{ij} + \beta_2 \text{DumAge}_{ij} \times (\text{Age80}_{ij} - 14) + \beta_3 (1 - \text{DumAge}_{ij}) \times (\text{Age80}_{ij} - 14) + \mathbf{X}'_{ij}\theta + \varepsilon_{ij}. \tag{3}$$

Here *DumAge* takes the value of 1 for those aged 14 or less in 1980, and 0 for all women aged over 14 in 1980, and linear trends are fitted on each side of this cutoff.[6] The $\mathbf{X}_{ij}$ vector includes controls for age at 1980, survey fixed-effects, region fixed effects, and a rural dummy variable. In this context β_1 is the reduced-form estimate of the effect of the expansion on education and maternal mortality outcome variables.

2.2.3 Kenya 1985

A primary school reform in Kenya in 1985 resulted in a change in the structure of education from a "7-4-2-3" system (7 years primary education, 4 years of lower secondary, 2 years of upper secondary, and 3 years of tertiary) to an "8-4-4" system. Significantly, this means an increase by one year in required primary schooling to receive the Kenyan Certificate of Primary Education (KCPE). This reform was sharp, taking place nationally in January of 1985 by allowing no students to progress from primary to secondary education in this school year. Chicoine (2012) shows that this has a significant impact on years of completed schooling, both for those women who complete only primary school, as for those women who progress to secondary education. Chicoine (2012) demonstrates that for those individuals born prior to or in 1963, there is null (or very low) probability that they will be treated by the 1985 reform in Kenya. For those born between 1963 and 1972, the probability of treatment is an increasing function of birth year, and for those born post-1972 they are treated with a probability of 1 (where 'treatment' implies matriculating to secondary school after 1985, and hence completing an additional year of primary schooling).

A treatment variable is then defined in line with the probability that an individual in each cohort is affected by the reform. This variable is approximately identical to that defined by Chicoine (2012). We use this treatment in a reduced form equation of the following form:

$$y_{ijq} = \alpha + \beta \text{Treat}_{jk} + \mathbf{age}'_{ijq}\gamma + \mathbf{qob\ trend}'_{jq}\delta + \mathbf{X}'_{ijq}\theta + \varepsilon_{ijq}, \tag{4}$$

where as in the prior subsections we estimate the effect of the plausibly exogenous treatment

[6] This is in line with that suggested by Agüero and Bharadwaj (2014). We also present results estimated with a quadratic and cubic trend.

on individual i, from cohort j, and, in this case, as per Chicoine (2012), control for quarter of birth q. The vectors **age** (in years) and **quarter of birth trend** include cubic and quadratic terms respectively, while $\mathbf{X}_{ijq}$ includes a quarter of birth fixed effects, ethnicity, and rural/urban controls.

3 Data

3.1 Cross-Country Regressions

Measures of educational attainment come from the cross–country dataset compiled by Barro and Lee (2013, 2010) which provides data on total years of schooling, years of schooling by education level (primary, secondary and tertiary), plus a measure of the proportion of the population with each level of education (primary, secondary and tertiary) or with no formal education. The Barro-Lee data allows us to observe the evolution of national attainment figures over time in five-year age groups, both for the entire population, and for the subgroup of females. The age-specific nature of this dataset allows for us to limit our analysis to individuals of fertile age, which for simplicity we define as between the ages of 15 and 39.[7] We also construct educational measures based upon individual data collected in the Demographic and Health Surveys (DHS). This data allows for us to observe the education outcomes reported by a representative sample of individuals in a subsample of countries, and allow us to test the robustness of the results using the Barro-Lee data. The DHS education data can be constructed at the regional (sub-country) level and we take advantage of this for our country-specific estimates.

The education data are merged by country and year with information on maternal mortality ratios (hereafter MMR) provided by the World Health Organization (WHO, 2012). The MMR indicator is calculated by the World Health Organization (WHO) using figures collected in household surveys and administrative records for maternal mortality and live births. Maternal deaths are classified (ICD 10) as those mothers who die from any cause "related to or aggravated by pregnancy or its management (excluding accidental or incidental causes) during pregnancy and childbirth or within 42 days of termination of pregnancy…".[8] Given concerns of underreporting by member states and the difficulty in compiling accurate measures of maternal mortality, point estimates are reported along with confidence intervals in the MMR dataset used.

[7] Similar estimates are run for the age group 15-49. The qualitative implications of the estimates do not change.

[8] A full description is provided by The World Health Organization at http://apps.who.int/gho/indicatorregistry/App_Main/view_indicator.aspx?iid=26

Merging Barro-Lee and MMR data results in a panel of 146 countries with observations over a period of 20 years (1990-2010). Both sets of data provide observations on a 5-yearly cycle; 1990, 1995, 2000, 2005 and 2010. To this main dataset we add measures for the log of GDP per capita, immunization rates—which we consider an imperfect proxy for national health expenditures—and fertility which are accessed from The World Bank Data Bank. We construct two further variables for the analysis by combining results from household surveys with available, but sporadic, cross-country measures. In order to control for births attended by skilled health staff and for the adolescent fertility rate, we use household data from the Demographic and Health Surveys (DHS) for that subset of countries and time periods for which a DHS survey is available. Where DHS data is not available, we access more complete cross-country data provided by The World Bank (2013) Data Bank. A precise description of what these variables represent and how they are constructed is provided in appendix A. The full merged dataset is available for download from the authors' webpage.

The estimation sample with non-missing data for all relevant variables contains 108 countries observed quinquennially through 1990-2010. The panel is unbalanced containing gaps in years in which attended births or adolescent fertility observations are not available. A full list of countries and years of availability is presented in table 8 in appendix C, and summary statistics are presented in table 2a below.

Trends in maternal mortality and education in the cross-country sample are displayed in figures 1-2. There is considerable variation both across time and continents, but overall a consistently positive trend in years of schooling, and a negative trend in regional maternal mortality figures. Exceptionally, in sub-Saharan Africa, maternal mortality stagnated between 1990-1995, beginning to fall in line with other regions during the late 1990s and early 2000s.

Despite reductions in maternal mortality in sub-Saharan Africa, this region still had overwhelmingly the highest rates of maternal death associated with childbirth in 2010 (Figure 5). Whilst certain countries in South, Central and East Asia, along with Middle East and North Africa, and Latin America have achieved ratios of less than 100 deaths per 100,000 births, all but a small hand-full of sub-Saharan countries still have ratios which exceed 300 per 100,000 births.

3.2 Natural Experiments

In order to focus on country-specific case studies, we use micro-level survey data from the Demographic and Health Surveys (DHS). These surveys focus on maternal and child health, and have a module focusing on maternal mortality. This maternal mortality module of the DHS is based upon the sisterhood method, where surveyed women are asked about sisters who have died, and whether death was during or related to child-birth (Rutstein and Rojas, 2006). These surveys allow us to calculate cohort specific maternal mortality rates, which can be combined with similarly calculated cohort specific educational attainment figures. As we do not have educational data on women who have died during childbirth and are not available to be surveyed, we use the microdata from the DHS respondents themselves. Effectively we work with two individual-level databases: a maternal mortality database of all sisters of respondents and their survival status, and an education database with years of completed education of all survey respondents.

Given that we observe data on each respondent's sisters and their survival status, our maternal mortality measure in this case is at the level of the woman, *not* at the level of the birth. While in the cross-country specification (1) we examine deaths per 100,000 births, in each country case we examine deaths per woman (or the probability that a woman dies in child birth) over her fertile life. This latter measure is referred to as maternal mortality rate, and reflects both the risk of maternal death per birth and the frequency of births in a population. The maternal mortality rate and the maternal mortality ratio are not directly comparable without some measure of the fertility in a given country. In the results section of this paper we use fertilty data described in section 3.1 to provide a rough comparison of the results from cross-country and country-specific estimates.

The DHS interviews women aged 15-49 at the time of the survey. To correct for the fact that many women are interviewed before they have completed their fertility (younger cohorts in the sample), as a robustness check we use the correction method discussed by Rutstein and Rojas (2006) and Stanton et al. (1997). This involves weighting each age cohort by an adjustment factor, which is calculated based upon its exposure to maternal mortality.[9] We note however that in the empirical strategies outlined in section 2.2, all identification is either based upon a double-difference estimator, or includes appropriate cohort-specific trends, so we expect that variation in exposure to maternal mortality should be accounted for even in the absence of this correction.

Table 2b presents summary statistics from the DHS data for each of these country-specific experiments. In each case we present descriptive statistics for years of education, maternal mortality rate, and treatment intensity, along with the time period examined in the natural experiment of interest. In Zimbabwe and Kenya, the two countries in which the natural experiments occurred at the secondary or upper-primary level, average educational attainment is approximately 7-8

[9]These adjustment factors have been calculated as 0.107, 0.206, 0.343, 0.503, 0.664, 0.882, and 0.900 for five age groups from 15-19 to 45-49 (respectively).

years for all women in the sample, while in Nigeria, where the natural experiment of interest involves primary school construction, average education in the cohorts of interest is approximately 5 years. In each case, over the entire range of cohorts examined, betwee 1 and 2 in every 100 women dies during child birth.

4 Results

4.1 Global

4.1.1 The Sensitivity of Maternal Mortality to Education (and Health Interventions)

Table 3 provides results for our principal cross-country specification (1), estimated on cross-country panel data for 108 developing countries observed at five yearly intervals in 1990-2010, and using the percentage of people in a country and year who have attained specified levels of education. We present similar results disaggregated by country income group and regional classifications as tables 4a and 4b respectively. In order to test the robustness of our results to alternative specifications of measures of education, we run regressions using years of education and years of education squared, and these results are included as appendix tables 9a and 9b. We start with a simple panel data specification in which the identifying variation is within countries over time, with country fixed effects absorbing all country-specific time-invariant components of, for instance, political institutions, climate, culture and deep-set attitudes towards women that may produce a correlation between education and maternal mortality. We then add flexible controls for aggregate trended unobservables and for country and time-varying variables that we expect are correlated with education and maternal mortality.

The pooled within-country correlation of maternal mortality rates with women's education is significantly negative for the percent of the population attaining each education level. The estimates suggest that the MMR-returns to investment in women's education are positive and weakly diminishing in level of education (this is confirmed by fitting a quadratic in total years of education: the quadratic term is positive and significant but the turning point lies outside the sample range). The point estimates are consistently largest for primary education but they are not statistically significantly different from the estimates for higher levels of education.

The estimates are robust to the introduction of year dummies, and further time-varying controls which reflect changes in the health and development status of a country. When examining coefficients by region- and income-specific groups (tables 4a and 4b), we find that the relationship between educational improvements and MMR reduction is larger in lower income environments (consistent with their higher baseline rates of maternal mortality and their lower baseline levels of education).

Column (2) is the favoured specification. It shows that, conditional upon aggregate trends in maternal mortality, if an additional one percent of the population enrolled in primary education this would lower the number of maternal deaths by 10 per 100,000 live births (The omitted group is the percent of the population with no formal education). This coefficient is estimated with precision (a p-value less than 0.01). It denotes a large impact, equivalent to 0.04 of a standard deviation and 5% of the mean maternal mortality rate in the sample. It is illustrative to consider the impact of moving the entire population of individuals with no education—on average 17.5% of women—into enrolment in primary education. Our estimates suggest this would lower MMR by 175.5 per 100,000 births, which is 0.58 of a s.d. of total MMR. The addition of controls in the remaining columns of the table lowers the coefficient on primary education but in no case is it significantly different from the coefficient in column 2. In the richest model, conditional upon controls for economic and health variables (column 7), the point estimate on primary education is reduced to almost two thirds of its initial size (column 2). For policy purposes it can be useful to look at the estimates conditional and unconditional upon alternative sets of controls since cohort changes in primary education will sometimes evolve similarly to changes in the controls that we introduce. We examine this in the following section.

Conditional on the controls introduced, the coefficients on secondary and tertiary education also remain significant. An additional percent of the population moving into secondary education lowers maternal mortality by 10 in 100,000 births. The effect of moving the sample population of uneducated women of fertile age into secondary education is estimated to create a mean reduction in maternal mortality of 172 (0.77 s.d.) in addition to the reduction of 196 associated with their passage through primary education. So the total impact of moving all uneducated women into secondary education is a reduction in MMR of 368 deaths per 100,000 births.

In columns 3-7 we introduce a succession of controls for the state of the economy, public health care, and fertility. We control for the logarithm of per capita GDP so as to assess whether education is simply proxying income. We find that it is not; the coefficients on education are unchanged and the coefficient on GDP is insignificantly different from zero (column 3). This is important, given that the most recent WHO factsheet on maternal mortality highlights differences in maternal mortality ratios by income and not education level (WHO, 2012).

Another widely recognised stylized fact highlighted in the WHO factsheet is that maternal mortality is a function of health services. We investigate this, introducing into the equation, DPT immunization rates for children as an indicator of the quality of public health provision.[10] The coefficient on primary education falls by about 10% but this drop is not statistically significant. Immunization has a direct impact on maternal mortality. A one standard deviation increase in immunization rates (which is 15.9% points) lowers maternal mortality by 45 per 100,000 birth, which is 0.14 s.d. or 20% of the mean (column 5).

We also include in the equation an index of health provision that is more directly related to maternal mortality. Skilled care before, during and after childbirth is recognised in the public health community to save the lives of both women and new-born babies. Maternal deaths tend to occur because of bleeding and infection and timely management and treatment can make the difference between life and death (WHO, 2012). India and Nepal for instance have, in the last decade, introduced financial incentives to encourage women to give birth in facilities with birth attendants rather than at home (Bauhoff et al., 2012; Powell-Jackson and Hanson, 2012). We find that the percentage of births attended by a skilled professional is associated with lower maternal mortality conditional upon immunization (column 8). A one s.d. increase in the percentage of births attended (which is 27.6% points) results in a drop in MMR of 35 which is 0.11 s.d. or 16% of the mean. Conditioning upon birth attendance does not alter the immunization coefficient.

The measure of maternal mortality that we use here is per live birth and so it is mechanically a function of the number of live births in the country and, plausibly, a function of fertility (the number of births per woman). Women in developing countries have on average many more pregnancies than women in developed countries, and their lifetime risk of death due to pregnancy is higher. A woman's lifetime risk of maternal death—the probability that a 15 year old woman will eventually die from a maternal cause—is 1 in 3,800 in developed countries, versus 1 in 150 in developing countries. Introducing the fertility rate in column 7, we see that an additional birth per mother is associated with an increase in maternal mortality of 28 per 100,000 live births or that a one s.d. increase in fertility (which is 1.7 births) leads to 48 additional maternal deaths, which is 0.16 of a s.d. or 22% of mean fertility. In the main specification however it should be noted that the effect of total fertility is weakly estimated, and not significant at typical levels of confidence.

Teenage fertility is highly prevalent in developing countries, and this is closely associated

[10]UNICEF for instance regards immunization rates as indicating the quality of overall public health provision in developing countries. Most immunization is directed at children under the age of 12 (and sometimes up to 24) months and our measure of immunization rates pertains to children but it includes tetanus and tetanus injections for pregnant mothers are part of routine prenatal care and in this way immunization rates may also directly influence maternal mortality.

with women dropping out of school early. Adolescent women face relatively high risks of complications and death as a result of pregnancy than older women (Conde-Agudelo et al., 2005; Patton et al., 2009). Whether this is because such women are negatively selected or because they are biologically more at risk is unclear and we know of no causal evidence on this. Introducing into the model the percentage of women who give birth at age 15-19, we find a significantly positive association. A one s.d. increase in the percent of teen births (which is 46 percentage points) results in 91 more maternal deaths per 100,000 births, which is 0.31 s.d. and 42% of the mean. Overall, in standardised terms, the association of maternal mortality with fertility and the prevalence of teen births is stronger than its association with either health interventions or education. It is well established that education tends to lower fertility and raise age at birth. In standardized terms, the impacts of primary and secondary education, DPT immunization rates and attended delivery on the maternal mortality ratio are almost identical, each conditional upon the others. To the extent that standardized estimates are useful, this suggests that the payoff to basic education in terms of a health sector outcome is equal to or larger than the payoff to a common index of provision in the health sector. This contributes to a literature documenting synergies between investments in education and health. It is of particular import for policy given that WHO policy documents on maternal mortality have under-emphasised education.

Alternative specification of education The specification in appendix Table 9a (column 2) suggests that an additional year of education for all women lowers the number of maternal deaths by 54 per 100,000 live births. A one standard deviation increase in years of education, of 3.21 years, would lower MMR by 173 per 100,000 births, which is 0.58 of a s.d. of MMR. The introduction of controls—particularly the addition of immunization—renders the linear years of education variable insignificant, although the coefficient is still negative. In appendix Table 9b we show estimates using a quadratic in years of education, the functional form suggested by a non-parametric plot of the relationship of interest (see Figure 3). The education coefficient is now robust to the introduction of immunization and other controls. With the exception of fertility which is now significant and of larger magnitude, other controls behave similarly in terms of their effects on MMR as the estimates in Table 3 discussed earlier. The linear and quadratic terms in education are significant and they indicate continuously positive but diminishing marginal returns (in terms of MMR reductions) to years of education. The estimates in column 2 show that moving from 0 to 1 year of education is associated with a reduction of 210 deaths per 100,000 live births, while moving from 5 to 6 years of education is associated with a smaller, but still very significant, reduction of 94 maternal deaths per 100,000 births. In the model with all controls (column 8) these estimates are attenuated slightly but still suggest significant effects: reductions of 175 deaths and 65 deaths respectively when moving from 0 to 1 or from 5 to 6 years of education.

While we have only a limited number of observations for maternal mortality and education for each country in our cross-country data, in figure 4 we examine correlations between maternal mortality and education both across and within countries. The clear negative relationship is once again seen when plotting country averages of log MMR against years of schooling (the solid points in figure 4). However, regression results are not only driven by this across-country result. Intra-country correlations appear to also be negative in the large majority of cases. Arrows overlayed on top of country averages show that, in a given country, increases in education are associated with reductions in log MMR in 116 of 146 cases, and is unrelated or positively related in only 30 cases.

Finally, when working with the subset of countries for which we are able to calculate maternal mortality ratios using DHS microdata, we run identical regressions, however replace the dependent variable (MMR measures from cross-country data provided by the WHO) with their DHS counterpart which we calculate from all DHS surveys. These results are displayed in appendix table 11. In general, results based on maternal mortality estimates from microdata agree reasonably well with WHO measures. However, given that microdata results are based on a smaller number of events (maternal deaths), DHS results are, unsurprisingly, somewhat less precise. Despite this, panel A shows that when using either DHS or WHO data, (quadratic) years of education are significantly related to MMR. In column (8) we see that conditional on full controls, DHS and WHO estimates result in similar conclusions, and reasonably similar point estimates (tests of equality of the coefficients on the linear and quadratic term cannot be rejected). We return to using DHS microdata in the country case studies considered in section 4.2.

4.1.2 Education, Female Education, or Male Education?

Thus far, all regression results present the conditional effect of female education on maternal mortality. However, fundamentally, we have *not* controlled for the educational attainment of other members of the population, precluding us from being able to conclude that these effects are only due to *women's* education. The inclusion of women's education in a regression without controlling for male education may be problematic, especially when considering that levels of female and male education in a country, on average, are highly correlated.[11]

In this section we turn to results which allow us to test whether increases in male or in female education drive reductions in maternal mortality. We run a series of regressions as per (1), where now the vector of educational variables $\mathbf{educ}_{it}$ contains measures of both male and

[11] For example, in the Barro-Lee sample used in this paper the R^2 of a regression of female to male education and an intercept is 0.918, while the coefficient on male education is 1.190.

female education in the population. We can thus test whether *conditional* on male education, increases in female education cause reductions in rates of maternal mortality. And vice versa, conditioning on female education, we can test whether higher levels of male education reduce rates of maternal mortality.

In table 5 we present results where education for each group of the population is measured by the percent with each level of education (primary, secondary or tertiary). This is the analogue of table 3, however now for both genders. Overwhelmingly, it is apparent that the results we have discussed in the previous section are driven *only* by increases in education of the cohort of fertile women. When controlling for female education, the effect of the level of men's education in the population of interest is never significantly different from zero. This is in line with recent empirical findings which suggest that women are more likely to seek family planning advice and contraceptives when males were not involved in consultations Ashraf et al. (2014). Whilst this is consistent with more educated and more empowered women being more likely to seek family planning services, the same logic does not seem to hold for males, potentially reflecting different childbearing and contraceptive preferences, which are reflected in maternal mortality rates.

Column 2 of table 5 suggests that a 1% increase in the rate of completion of primary education among women is (unconditionally) associated with a reduction in the rate of maternal mortality of 11.33 per 100,000 live births. Similarly higher rates of secondary and tertiary completion among women reduce maternal mortality. However, when turning to rates of male education, no effect is seen (with t-statistics nearly always well below 1). This result remains even when conditioning on our full set of controls. Column 7 shows that a 1% increase in female rates of primary education reduces MMR by 8.9 in 100,000 births, while a 1% increase in male primary education has a small positive (insignificant) relationship.

These results are stable regardless of the set of controls used, and also the way in which education is mesured. Appendix table 10 presents the same set of regressions with education measured as a quadratic in years for males and females. While the effect of female education on MMR—after controlling for male education—is a significant negative linear and positive quadratic effect (suggesting that education reduces maternal mortality, and that returns to education are diminishing)[12], the conditional effect of male education on maternal mortality rates is once again insignificant at all levels in the sample range. Similar effects are seen when education is measured simply as years of schooling, providing considerable evidence that it is actually the level of education of the women in the cohort that is important, not total educational attainment,

[12]Quadratic figures have a turning point from a negative to a positive effect on MMR at between 15.5 to 18.5 years of education (depending upon the set of controls included). These values of considerably higher than the maximum sample value displayed in table 2a.

or the attainment of men in the same cohort.

4.1.3 Potential Pathways and Correlated Effects

The discussion in the preceding sections establishes that declines in maternal mortality in developing countries have been associated with gains in education, improvements in public health services and declines in fertility. Since our primary interest in this paper is in the benefits flowing from education, in this section we consider the extent to which improvements in public health services and declines in fertility are themselves driven by improvements in education. We take each of the controls that were used in Table 3 (including GDP which had no significant impact on maternal mortality) and regress them on education conditional upon country fixed effects (Table 6):

$$fert_{it} = \alpha + \mathbf{educ}'_{it}\beta + \mathbf{W}'_{i}\gamma + u_{it}. \tag{5}$$

We see that increases in education are significant predictors of within-country variation in fertility, immunization, the percentage of attended births and log per capita GDP. The dependent variables in Table 6 have been cast as z-scores (see notes to Table 6) so the coefficients on the education variables indicate the standard deviation change in the outcome flowing from a one year increase in education at the specified level. For example, moving an additional percent of non-school attendees into primary education results in a 0.018 s.d. decline in fertility; an increase of approximately 0.014 s.d. in both immunization rates and attended births; and a 0.008 s.d. increase in log GDP p.c. So education not only directly influences maternal mortality, it also is strongly correlated with other determinants of maternal mortality.

We can plug the relationships estimated in Table 6 into the specifications in Table 3. For instance, using column 7 in Table 3, we found that a one s.d. reduction in fertility led to a 0.21 s.d. decline in MMR. Since Table 6 column 1 tells us that a single percentage increase in primary education years leads to a 0.0177 s.d. decline in fertility, it follows that there is a fertility-mediated impact of primary education on MMR of 0.0177×0.21 or 0.004 s.d. This is in addition to the direct impact of a year of primary education on MMR in column 7, which is 0.025 s.d. When considering that the total percent of women without formal education is much larger than the one percent which the preceding calculations assume, these effects can become quite large. Similar calculations can be made for education-led impacts of immunization and attended delivery on maternal mortality, and we present these calculations in the final row of table 3. Rather than present these calculations as the impact of moving 1% of women into primary education, we present the impact of moving all uneducated women into schooling. We consider this to be a more relevant indicator for policy. Overall, these results show that education is strongly positively correlated with health service provision conditional upon country fixed effects and aggregate trends. While this association may not be entirely causal, causal components such as that educated people demand public health services merit testing.

4.2 Country-Specific Results

We take advantage of the reforms described in sections 2.2.1-2.2.3 to estimate the effect that plausibly exogenous within-country variation in education expansion has on maternal mortality rates. Regression results from these country-specific expansions in schooling are presented below in Tables 7a-7c. Each table displays reduced form results for the effect of the reform on educational attainment and on maternal mortality. For each case we also present a descriptive (graphical) summary of the evolution of education, maternal mortality, and the date of the educational reform (Figures 6a-8b). In those graphs where two vertical cut-off points are plotted, the first signifies the tail end of the "treatment" group, while the second represents the beginning of the "control" group (with intermediate cohorts considered as transitionary, or partially affected). Whilst these figures are each plotted from data at the national level, the regression analysis presented in Tables 7a-7c, relies on sub-national variation resulting from variation in reform intensity by region.

In each case as a robustness check we also examine the effect of some placebo reform. This placebo reform is defined by comparing birth cohorts who had completely finished the relevant level of school at the time of the reform, and hence who were both unaffected by the expansion in education. We expect in this case that the effect of the false reform—on both education and maternal mortality—should not be significantly different to zero, while we expect that the true reform should increase educational attainment and reduce maternal mortality. These placebo results are presented in appendix tables 12a-12c

With the exception of one of twenty different outcome treatment-outcome pairs, as expected we see that the placebo reform has no significant effect on either schooling or on maternal mortality. This provides some evidence in favour of the functional form of equations (2), (3) and (4). Turning to the true reforms, we find that in all cases these significantly increased educational attainment. In Nigeria the effect of residing in a high-intensity state (column 2 of Table 7a) is approximately 2.1 additional years of education. The effect of extensions in schooling availability due to independence in Zimbabwe is approximately 1.2 additional years,[13] and the effect of the

[13]This is very similar (and not significantly different) to the 1.252 years estimated by Agüero and Bharadwaj

changes in the structure of the KCPE was an additional 0.95 years of education on average for female students.

Turning to the reduced form effect of these reforms on maternal mortality, we see that in two of the three cases: Nigeria and Zimbabwe, the reforms significantly reduce maternal mortality in the treated cohorts. The exception here is Kenya, where we find a weakly positive (but not significantly different from zero) effect. The lack of a result in this country is perhaps unsurprising for two reasons. Firstly the educational expansion occurred at a relatively high level (between seven and eight years), while our earlier results suggest that the effect of education on maternal mortality is greatest when the educational expansion occurs at low levels. Secondly, the DHS data on maternal mortality from Kenya is by far the noisiest, with large fluctuations by birth cohort over the range of the treatment period.

In the case of Nigeria we find statistically and economically significant reductions in maternal mortality by the age of 25 in response to the reform. Depending on the treatment examined, the effect is between a 1 and 2% reduction in total mortality (or between 1 and 2 less women in 100 dying in child birth). In the case of Zimbabwe we find a slightly smaller effect of a 0.4% reduction in total mortality. Once again, it is perhaps not surprising that the effect of the Zimbabwe reform is smaller than the effect of the Nigeria reform, given that the Nigeria reform occured at the primary level, while the Zimbabwe reform largely increased educational attainment at the high school level. We can calculate the approximate effect of an additional year of education by dividing the "second stage" effect of the reform on maternal mortality by the "first stage" effect of the reform on education in years. Using the preferred estimates from Tables 7a and 7b, this gives an estimate of $-0.0192/2.147 = -0.0089$ and $-0.00413/1.148 = -0.0033$ respectively.

How do these estimates compare with the cross-country estimates reported earlier? It turns out that they match up rather unexpectedly well with cross-country estimates for the effect of additional years of education. Recall that the cross-country estimates use WHO maternal mortality ratios which are maternal deaths per live birth while the country specific estimates use DHS maternal mortality rates which are maternal deaths per woman. In order to provide a rough comparison, we use recent data (Central Intelligence Agency, 2012) on fertility per woman for Zimbabwe and Nigeria to convert the maternal deaths per woman estimates calculated in the paragraph above, to deaths per birth. These figures of 3.61 and 5.38 births per woman (respectively), result in per-birth estimates of $0.0088/5.38 = 0.00166$, and $0.0036/3.61 = 0.00091$. These correspond to an effect of 166 deaths per 100,000 live births (for an expansion at the lower primary level), and 9.1 deaths per 100,000 live births for an expansion in high school. When

(2014) using only two waves of the Zimbabwean DHS.

we use the years of schooling results from table 9b (column 8), we find that an expansion in schooling from 1 to 2 years results in a reduction in MMR of 153.0 deaths per 100,000 births, and an expansion from 7 to 8 years results in a reduction of 21.5 deaths per 100,000 births: quite close to the country-specific results from Nigeria and Zimbabwe.

5 Conclusion

In the last two decades, maternal mortality has declined sharply (by 50%) and education has risen sharply. The analysis in this paper suggests that some part of the decline in maternal mortality may be attributed to the rise in education. Using the cross-country data sample, we use the changes in maternal mortality, education and the control variables in the analysis between 1990 and 2010 to crudely simulate the contribution of education. In this twenty year period, the average percent of women with no formal education has fallen from 22 to 13%. Applying this reduction of 9% to the estimates in Table 3 suggests that education will have led to a decline in the maternal mortality ratio of 105 deaths per 100,000 births (unconditional) or about 71 (conditional upon health services and fertility which, we noted, are correlated with education). The actual decline in the maternal mortality ratio in the same twenty year period in our sample is 119. This suggests that education gains in this period can explain roughly half or possibly more of the observed decline in maternal mortality. Compare this with the role of attended births. The percentage of attended births has risen from 72% to 82%. Our estimates suggest that this will have led to a decline in MMR of 15 per 100,000 births. These simulations are only suggestive but the evidence overall does suggest that the rapid growth in education in developing countries may have made a substantial and largely unrecognised contribution to the dramatic declines in maternal mortality witnessed in recent years.

Figures

Figure 1: Educational Attainment by Region: 1990-2010

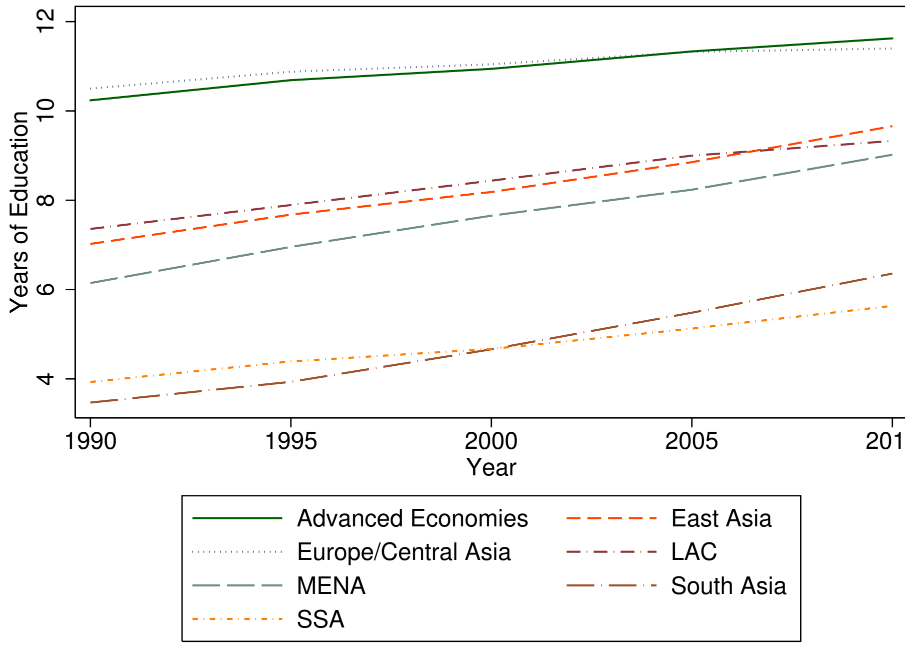


Figure 2: Maternal Mortality Ratio by Region: 1990-2010

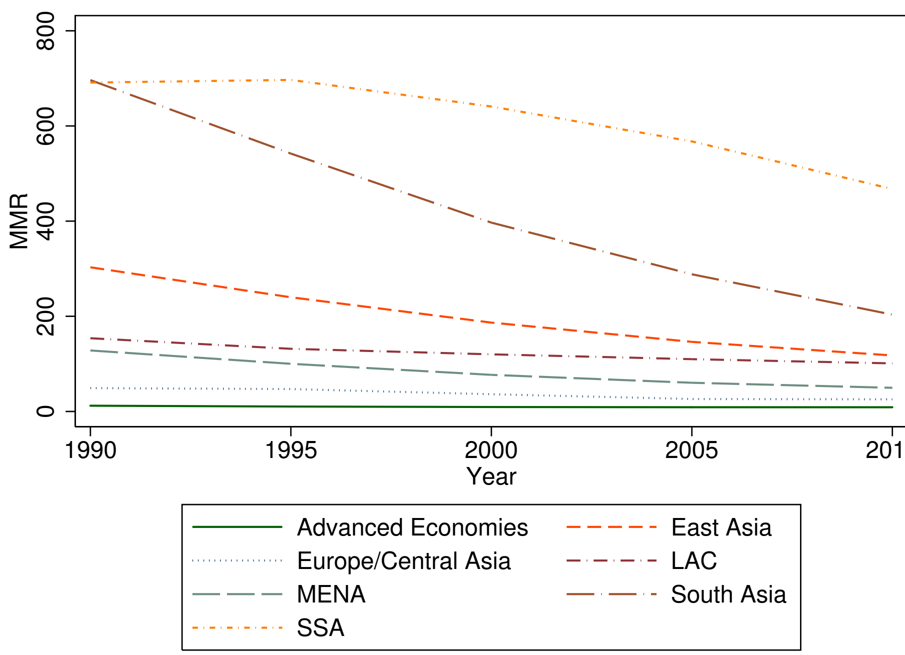
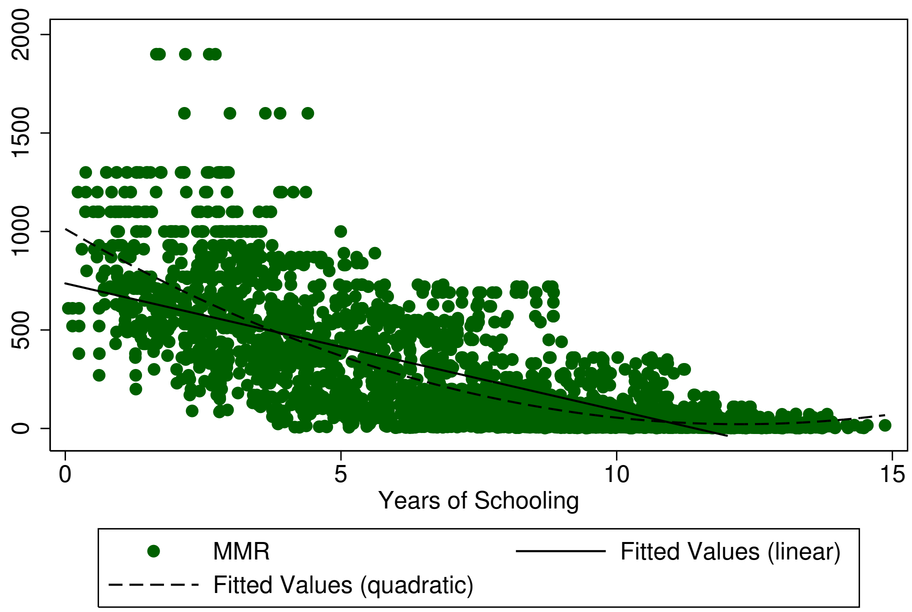
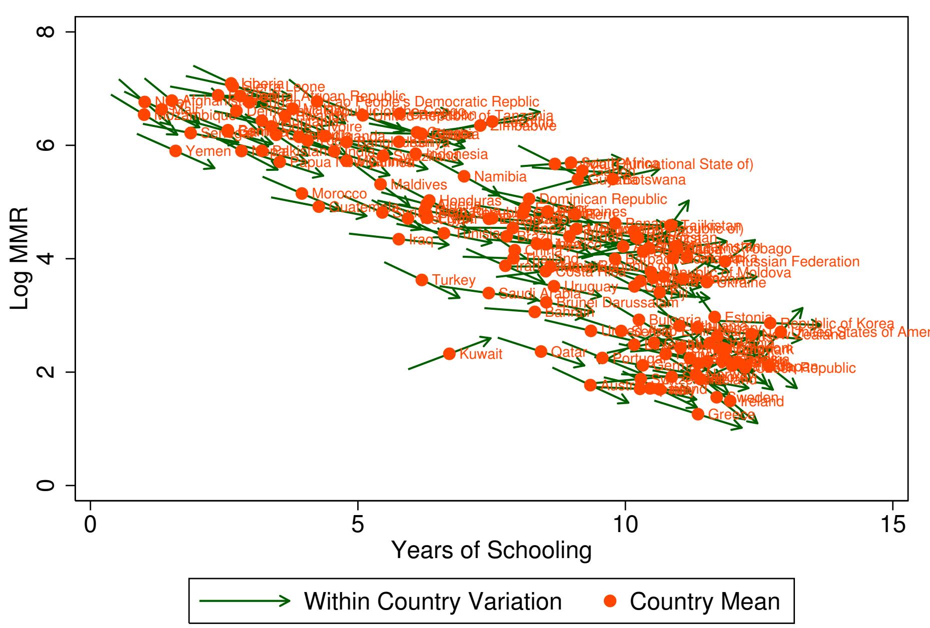


Figure 3: Maternal Mortality and Education: Functional Form



Notes to figure: Each point represents a country average of maternal deaths per 100,000 live births.
Education data is for women aged 15–39.

Figure 4: Between and Within Country Correlations: Education and MMR



116 countries have a negative trend, 30 have a positive trend.

Figure 5: Maternal Mortality Ratio by Country

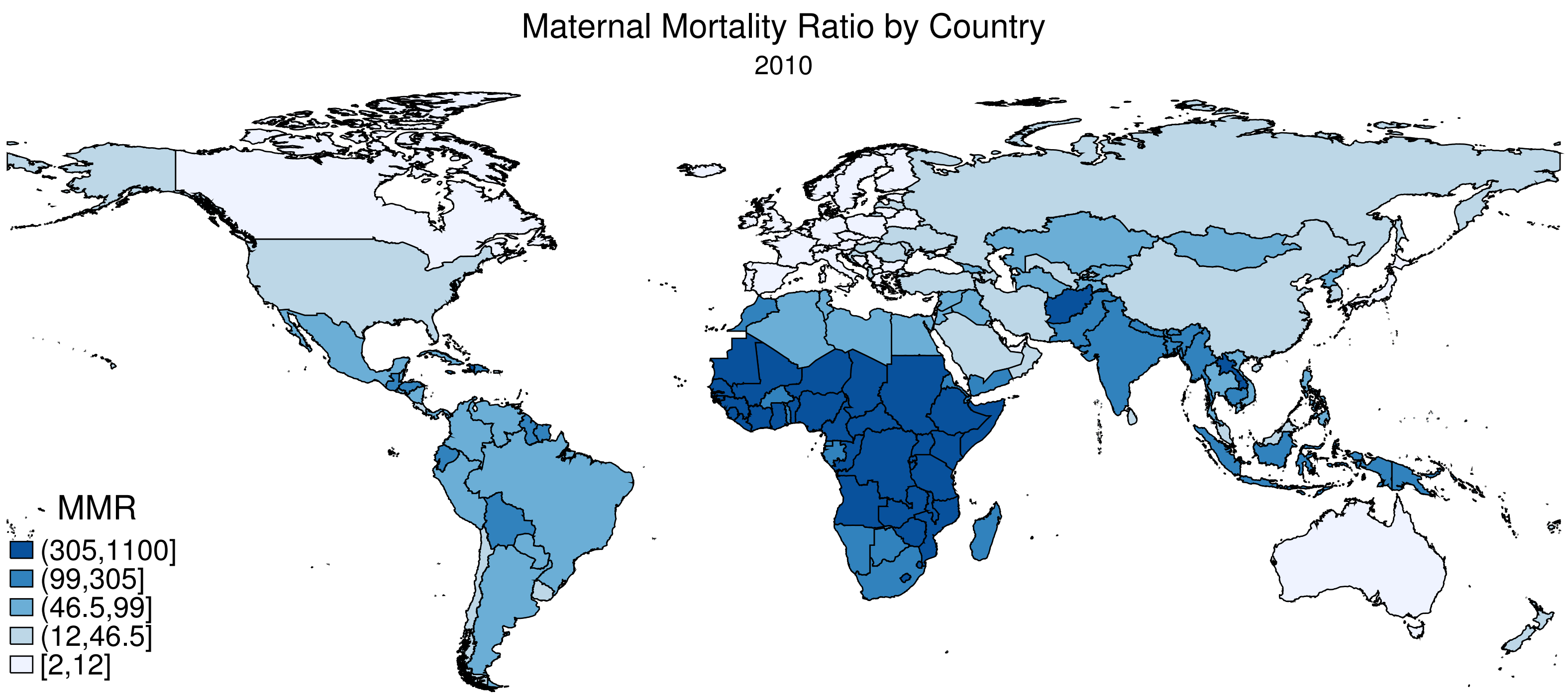


Figure 6a: Educational Attainment by Year – Nigeria

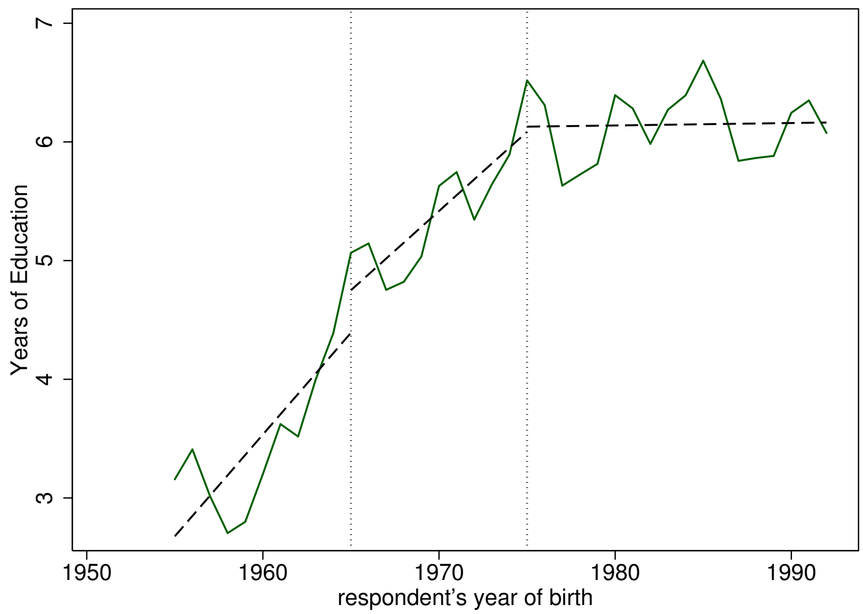


Figure 6b: Maternal Mortality by Year – Nigeria

Note to figures 6a-6b: 3 year moving averages are displayed for maternal mortality and education. The first vertical dotted line represents the end of the control group, and the second vertical dotted line represents the beginning of fully treated cohorts. Cohorts in between are partially treated. Further details in the body of the text, and table 1.

Figure 7a: Educational Attainment by Year – Kenya

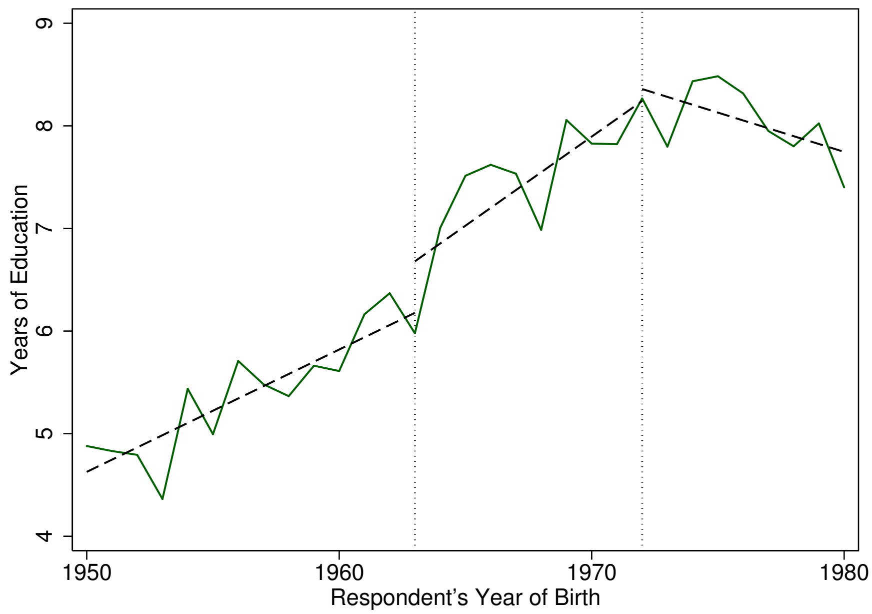
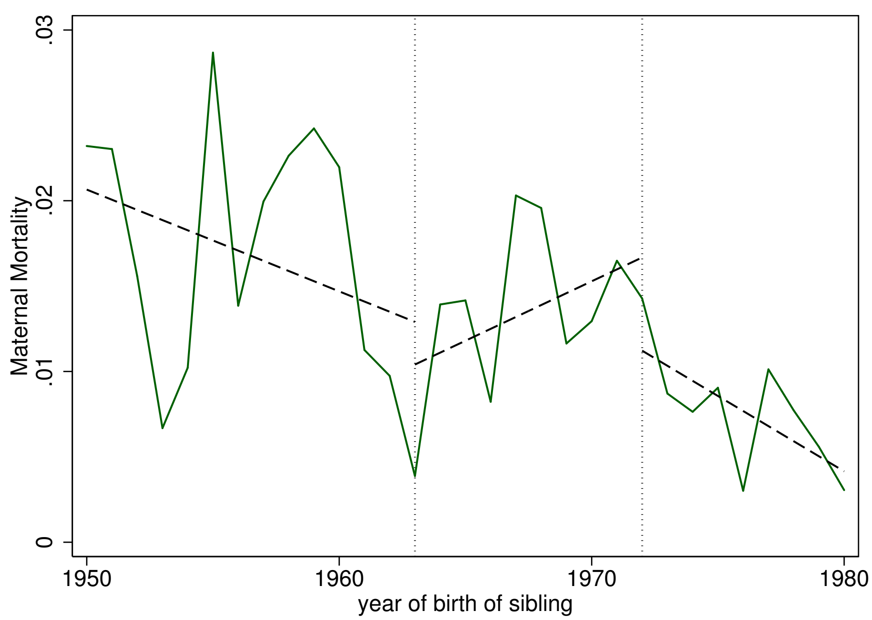


Figure 7b: Maternal Mortality by Year – Kenya



Note to figures 7a-7b: The first vertical dotted line represents the end of the control group, and the second vertical dotted line represents the beginning of fully treated cohorts. Cohorts in between are partially treated. Further details are provided in the body of the text, and in table 1.

Figure 8a: Educational Attainment by Year – Zimbabwe

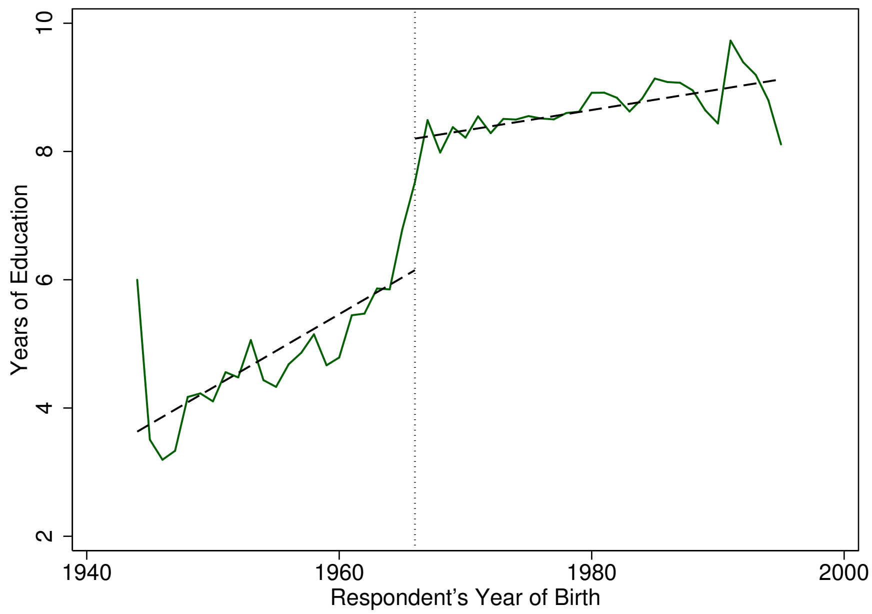


Figure 8b: Maternal Mortality by Year – Zimbabwe

Note to figures 8a-8b: Treatment was a one year expansion in schooling, differentially affecting *only* the 1965/1966 cohorts. All cohorts to the left of the vertical dotted line (1966 and after) are considered treated, and all cohorts to the right of the line (1965 and before) are not treated. Further details are provided in the body of the text, and in table 1.

Tables

Table 1: Educational Experiments: treatment and control groups

Country	Year	Expansion	Treatment Years	Control Years
Indonesia	1973-78	Primary school construction	1968-72	1957-62
Nigeria	1976	Universal Primary Education	1970-75	1956-61
Zimbabwe	1980	High school education expansion	1966	1965
Kenya	1985	Additional year of primary school	post-1972	pre-1963
Botswana	1986	High school year rearrangement	pre-1970, post 1982	1974
Sierra Leone	2001	Free Primary Education	1990-93	1980-86

Table 2a: Summary Statistics - Cross Country

Variable	Obs	Mean	Std. Dev.	Min	Max
Maternal Mortality	710.0	220.6	300.9	2.0	1900.0
ln(Maternal Mortality)	710.0	4.302	1.649	0.6931	7.55
GDP per capita	702.0	9190.0	13870.0	64.36	104500.0
ln(GDP per capita)	702.0	7.966	1.649	4.164	11.56
Immunization	690.0	84.75	15.9	18.0	99.0
Fertility	718.0	3.163	1.676	0.887	8.659
Percent Attended Births	450.0	77.29	27.59	-2.6e-06	100.0
Population (Millions)	670.0	40.49	144.0	0.09515	1338.0
Teen Births	670.0	55.68	46.12	2.796	220.6
EDUCATION - FEMALE					
Total Years of Education	730.0	8.07	3.319	0.4692	13.99
Years of Primary Education	730.0	4.714	1.693	0.3421	8.907
Years of Secondary Education	730.0	2.963	1.754	0.04875	7.459
Years of Tertiary Education	730.0	0.3932	0.3744	7.15e-08	2.048
Percent Primary	730.0	23.83	17.44	0.02	77.85
Percent Secondary	730.0	45.97	23.88	1.203	95.65
Percent Tertiary	730.0	12.58	12.05	0.0	62.86
Percent No Education	730.0	17.61	23.4	0.0	93.59

NOTES: Maternal mortality is expressed in terms of deaths per 100,000 live births. Immunization is expressed as the percent of children of ages 12-23 months who are immunized against diphtheria, pertussis and tetanus (DPT). Fertility represents births per woman, and teen births are expressed as the number of births per 1000 women between the ages of 15–19.

Table 2b: Summary Statistics – Natural Experiments

Variable	Obs	Mean	Std. Dev.	Min	Max
Panel A – Nigeria					
Years of Education	13221	4.822	5.349	0	22
Investment per Capita	12748	0.881	0.545	0.014	2.195
Non-West State	13235	0.828	0.377	0	1
Year of Birth (education)	13235	1968.329	5.119	1956	1975
Maternal Mortality	25354	0.019	0.137	0	1
Under 25 Maternal Mortality	29676	0.006	0.074	0	1
Year of Birth (MM)	29967	1968.472	5.381	1956	1975
Panel B – Zimbabwe					
Years of Education	10195	7.023	3.788	0	21
High School Enrollment	10195	0.439	0.496	0	1
Treated	10201	0.622	0.485	0	1
Year of Birth (education)	10201	1966.128	4.786	1956	1974
Maternal Mortality	23699	0.013	0.115	0	1
Under 25 Maternal Mortality	28631	0.003	0.055	0	1
Year of Birth (MM)	28842	1966.023	4.736	1957	1974
Panel C – Kenya					
Years of Education	13712	7.168	4.149	0	23
Treated	13712	0.575	0.443	0	1
Year of Birth (education)	13712	1968.389	8.147	1950	1980
Maternal Mortality	22738	0.014	0.116	0	1
Under 25 Maternal Mortality	25616	0.006	0.076	0	1
Year of Birth (MM)	25686	1967.686	7.770	1950	1980

Notes: Year of birth (education) refers to the birth cohorts of respondents to the DHS surveys for whom we observe educational attainment. Year of birth (MM) refers to the birth cohorts in the maternal mortality data, who are sisters of DHS respondents. In panel A investment per capita refers to federal funds dispersed for school construction in 1976 (in naira). We do not observe this for the state Abuja which has existed since 1991 only. In all panels maternal mortality and under 25 maternal mortality refers deaths due to pregnancy divided by the total number of women.

Table 3: Cross-Country Results of MMR and Female Educational Attainment

VARIABLES	(1) MMR	(2) MMR	(3) MMR	(4) MMR	(5) MMR	(6) MMR	(7) MMR	(8) MMR
Primary Education (% Population)	-9.926***	-10.17***	-8.451***	-8.449***	-7.656***	-7.229***	-6.746***	-6.083***
	(1.727)	(2.044)	(2.007)	(2.003)	(2.121)	(2.251)	(2.534)	(2.304)
Secondary Education (% Population)	-9.918***	-9.804***	-6.799***	-6.816***	-6.117***	-5.488***	-4.930**	-4.203**
	(1.446)	(1.640)	(1.801)	(1.788)	(1.824)	(2.024)	(2.262)	(1.931)
Tertiary Education (% Population)	-9.381***	-10.47***	-4.931*	-5.027**	-4.709*	-4.250*	-4.141*	-3.608
	(1.527)	(1.791)	(2.605)	(2.507)	(2.377)	(2.484)	(2.469)	(2.181)
year==1995			-9.926	-10.37	-3.020	-0.826	5.491	0.0350
			(15.88)	(16.23)	(16.15)	(15.47)	(17.15)	(18.01)
year==2000			-31.66	-32.06	-21.92	-17.45	-7.005	-10.75
			(20.97)	(21.57)	(21.65)	(20.03)	(22.81)	(22.91)
year==2005			-56.39*	-59.10*	-38.11	-36.70	-21.34	-19.09
			(28.89)	(33.84)	(33.81)	(30.96)	(32.94)	(32.31)
year==2010			-75.43**	-80.06*	-56.89	-52.99	-34.22	-20.16
			(34.20)	(45.65)	(45.47)	(41.64)	(43.40)	(42.66)
log GDP per capita				5.443	3.344	6.517	1.795	-0.740
				(25.37)	(26.27)	(25.42)	(23.20)	(23.53)
Immunization (DPT)					-2.147*	-1.888	-1.781	-1.788
					(1.204)	(1.246)	(1.279)	(1.280)
Attended Births						-1.348	-1.038	-1.422
						(1.010)	(0.943)	(0.968)
Fertility							23.89	5.942
							(27.74)	(30.24)
Teen births								1.964
								(1.275)
Constant	1,031***	1,045***	831.3***	792.3***	922.9***	931.2***	808.6**	745.1**
	(120.2)	(143.7)	(149.1)	(259.0)	(274.9)	(257.4)	(340.9)	(352.5)
Observations	710	428	428	428	428	428	428	428
R-squared	0.499	0.652	0.677	0.677	0.696	0.705	0.707	0.717
Number of countries	142	108	108	108	108	108	108	108

NOTES: All regressions include fixed-effects by country. For the full list of countries by year see table 8. Results are for the percent of the female population between the ages of 15 and 39 with each level of education in each country. A full description of control variables is available in section 3, and as the note to table 2a. Standard errors clustered at the level of the country are diplayed. *p<0.1; **p<0.05; ***p<0.01

Table 4a: Cross-Country Results of MMR and Female Educational Attainment By Region

VARIABLES	(1) Advanced Economies	(2) East Asia and the Pacific	(3) Europe and Central Asia	(4) Latin America and the Caribbean	(5) Middle East and North Africa	(6) South Asia	(7) Sub-Saharan Africa
Primary Education (% Population)	-1.117*	-8.910**	3.508*	-5.732**	0.0997	-8.119*	-10.43**
	(0.537)	(2.890)	(1.771)	(2.706)	(0.902)	(3.909)	(4.206)
Secondary Education (% Population)	-1.077*	-7.771**	4.355*	-4.173	-0.487	-12.20	-2.061
	(0.525)	(2.847)	(2.226)	(2.456)	(1.337)	(7.065)	(5.725)
Tertiary Education (% Population)	-0.704	-2.574	5.997**	-5.795**	-2.735**	14.49	1.311
	(0.498)	(5.957)	(2.785)	(2.701)	(1.188)	(24.04)	(19.45)
Constant	110.9**	901.1***	-364.6*	614.4**	160.1**	945.6***	1,087***
	(50.11)	(231.4)	(199.5)	(227.1)	(54.94)	(179.4)	(235.2)
Observations	78	42	67	83	34	23	101
R-squared	0.554	0.682	0.540	0.503	0.590	0.935	0.607
Number of countries	18	11	16	20	12	6	25

NOTES: All regressions include fixed-effects by country. Results are for average years of education of females between the ages of 15 and 39 in each country. Results are reported for specification (2) from table 3 which includes country and year fixed effects. Standard errors are clustered by country. *p<0.1; **p<0.05; ***p<0.01

Table 4b: Cross-Country Results of MMR and Female Educational Attainment By Income Group

VARIABLES	(1) Low	(2) Lower Middle	(3) Upper Middle	(4) High
Primary Education (% Population)	-9.972*	-5.157**	-3.567	0.0519
	(4.759)	(2.210)	(2.306)	(0.0779)
Secondary Education (% Population)	-0.877	-5.469**	-2.743	-0.0237
	(5.349)	(2.350)	(2.330)	(0.104)
Tertiary Education (% Population)	-8.746	-10.18**	-2.362	0.0860
	(36.03)	(4.340)	(1.978)	(0.147)
Constant	1,097***	781.4***	373.3*	12.43
	(178.4)	(167.3)	(200.3)	(9.365)
Observations	79	114	122	113
R-squared	0.769	0.540	0.349	0.124
Number of countries	19	28	31	30

NOTES: All regressions include fixed-effects by country. Results are for average years of education of females between the ages of 15 and 39 in each country. Results are reported for specification (2) from table 3 which includes country and year fixed effects. Countries are classified according to World Bank income groups, and standard errors are clustered by country. *p<0.1; **p<0.05; ***p<0.01

Table 5: Cross-Country Results: MMR and Female versus Male Education

VARIABLES	(1) MMR	(2) MMR	(3) MMR	(4) MMR	(5) MMR	(6) MMR	(7) MMR	(8) MMR
Primary Education (% Females)	-15.22***	-17.21***	-14.76***	-14.84***	-14.08***	-13.49***	-13.27***	-12.45***
	(3.545)	(4.132)	(3.564)	(3.575)	(3.783)	(3.853)	(3.959)	(3.920)
Secondary Education (% Females)	-9.257***	-12.91***	-8.656***	-8.684***	-8.054***	-7.745***	-7.266**	-6.731**
	(2.805)	(3.408)	(3.122)	(3.121)	(2.932)	(2.945)	(3.039)	(2.831)
Tertiary Education (% Females)	-9.002***	-8.958**	3.165	3.163	3.649	3.684	3.522	3.655
	(3.443)	(3.759)	(5.006)	(5.026)	(4.819)	(4.909)	(4.921)	(4.828)
Primary Education (% Males)	5.970	9.105*	8.742**	8.844**	8.965**	8.621**	8.629**	8.178**
	(4.188)	(4.814)	(4.040)	(4.036)	(4.010)	(3.911)	(3.899)	(3.962)
Secondary Education (% Males)	-0.414	4.578	3.878	3.937	4.099	4.210	3.985	3.899
	(3.404)	(4.152)	(3.385)	(3.362)	(2.997)	(2.981)	(2.963)	(2.924)
Tertiary Education (% Males)	-0.430	-1.944	-9.030*	-8.918*	-9.089*	-8.788*	-8.514*	-8.273
	(4.534)	(5.045)	(4.985)	(5.061)	(4.895)	(5.023)	(5.034)	(5.076)
Observations	710	428	428	428	428	428	428	428
R-squared	0.522	0.692	0.733	0.733	0.754	0.758	0.759	0.764
Number of countries	142	108	108	108	108	108	108	108

NOTES: All regressions include fixed-effects by country. For the full list of countries by year see table 8. Educational variables are the same as those in table 3 however include both female and male figures for each variable (ages 15-39). A full description of control variables is available in section 3, and as the note to table 2a. Standard errors clustered at the level of the country are diplayed. *p<0.1; **p<0.05; ***p<0.01

Table 6: Correlations between Education and Health/Development Outcomes

VARIABLES	(1) Fertility	(2) Immunization	(3) Percent Attend	(4) ln(GDP pc)	(5) Teen Births
Primary Education (% Population)	-0.0177***	0.0147***	0.0138***	0.00793***	-0.00829***
	(0.00165)	(0.00281)	(0.00234)	(0.00144)	(0.00285)
Secondary Education (% Population)	-0.0315***	0.0261***	0.0315***	0.0181***	-0.0243***
	(0.00105)	(0.00193)	(0.00152)	(0.00119)	(0.00187)
Tertiary Education (% Population)	-0.0377***	0.0241***	0.0320***	0.0483***	-0.0293***
	(0.00161)	(0.00237)	(0.00296)	(0.00273)	(0.00223)
Constant	2.331***	-1.836***	-2.193***	-1.629***	1.688***
	(0.0798)	(0.180)	(0.146)	(0.0639)	(0.174)
Observations	718	690	450	702	670
R-squared	0.723	0.417	0.692	0.581	0.482
Effect Size	0.066 s.d.	0.049 s.d.	0.065 s.d.	0.022 s.d.	0.060 s.d.

NOTES: Each regression includes fixed effects by country, and heteroscedasticity robust standard errors. Each dependent variable has been transformed to a z-score by subtracting its global mean and dividing by its standard deviation. Education measures are for the female population between 15 and 39. For discussion of the effect size, see section 4.1.3. *p<0.1; **p<0.05; ***p<0.01

Table 7a: Effect of 1976 Educational Expansion: Nigeria

	(1)	(2)	(3)	(4)
PANEL A: OUTCOME – EDUCATION				
Intensity 70-75	0.983*	2.147***	0.0574	0.175***
	(0.555)	(0.790)	(0.0545)	(0.0451)
Intensity 65-69	0.671*	1.040**	0.0925	0.195***
	(0.382)	(0.445)	(0.0641)	(0.0540)
Intensity	0.360			
	(0.513)			
Observations	12,735	12,735	12,735	12,735
R-squared	0.425	0.424	0.426	0.427
PANEL B: OUTCOME – MMR				
Intensity 70-75	-0.000214	-0.0135	-2.67e-05	2.95e-05
	(0.00867)	(0.0116)	(0.000877)	(0.000753)
Intensity 65-69	-0.000119	-0.00839	0.00257*	0.00161
	(0.00637)	(0.00752)	(0.00153)	(0.00121)
Intensity	0.000119			
	(0.00812)			
Observations	28,694	28,694	28,694	28,694
R-squared	0.008	0.008	0.013	0.013
PANEL C: OUTCOME – MMR (UNDER 25)				
Intensity 70-75	-0.000323	-0.0192**	-0.00128*	-0.00120***
	(0.00546)	(0.00949)	(0.000650)	(0.000336)
Intensity 65-69	-0.00162	-0.0118**	-0.000768	-0.00151***
	(0.00353)	(0.00517)	(0.000712)	(0.000500)
Intensity	-0.00419			
	(0.00513)			
Observations	28,694	28,694	28,694	28,694
R-squared	0.005	0.005	0.007	0.007

NOTES: Columns (1)-(4) represent different measures of treatment intensity. Column (1) uses capital expenditure on school construction in 1976 in each individual's state as their treatment intensity, column (2) uses a dummy for residence in non-West (high-intensity) states, column (3) uses the number of years exposed to the reform interacted with the high-intensity state dummy as the intensity measure, and column (4) uses capital expenditure interacted with the high-intensity state dummy. The effect of the reform is identified for 1970-1975 birth cohorts (who are fully affected), and 1965-1969 cohorts, who are affected partially via over-age enrollments. Panel A shows the effect of the education reforms on educational attainment, panel B shows the effect on life-time maternal mortality, and panel C the effect on maternal mortality under the age of 25. All regressions are double-differences, however in columns (2)-(4) the intensity dummy is captured by state of residence fixed effects. Additional controls include religion, ethnicity and year of birth fixed effects, plus time trends by state, and controls for the length of exposure to the civil war in Biafra (Akresh et al., 2012). Standard errors are clustered by state and birth cohort. *p<0.1; **p<0.05; ***p<0.01

Table 7b: Effect of 1980 Educational Expansion: Zimbabwe

	Years of Education			Maternal Mortality		
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
DumAge	1.244***	0.446**	0.509	-0.00413**	-0.000283	0.00265
	(0.196)	(0.182)	(0.327)	(0.00143)	(0.00451)	(0.00701)
DumAge×(Age1980-14)	-0.186***	-0.575***	-0.934***	0.000155	0.000186	0.000582
	(0.0257)	(0.0682)	(0.189)	(0.000311)	(0.00107)	(0.00227)
(1-DumAge)×(Age1980-14)	-0.135***	-0.324**	-0.0588	-0.000249	0.00219	0.00516
	(0.0313)	(0.113)	(0.299)	(0.000432)	(0.00235)	(0.00676)
Observations	10,198	10,198	10,198	28,631	28,631	28,631
R-squared	0.223	0.226	0.227	0.002	0.002	0.002

NOTES: Columns (1) and (4) include a linear trend for age, columns (2) and (5) a quadratic, and columns (3) and (6) a cubic trend. DumAge (treatment) refers to the birth cohort which was 14 years old at the time of the reform (1980). Additional controls included are survey, region and birth cohort fixed effects, along with a rural dummy variable. Standard errors are clustered at region of residence. *p<0.1; **p<0.05; ***p<0.01

Table 7c: Effect of 1985 Educational Expansion: Kenya

VARIABLES	(1) Years of Education	(2) Maternal Mortality
Treatment	0.953***	0.00689
	(0.265)	(0.00553)
Observations	13,703	25,602
R-squared	0.203	0.031

NOTES: Each regression includes a cubic term for age at time of reform, a cuadratic trend for quarter of birth, fixed effects by quarter of birth and ethnicity, and a dummy for rural or urban residence. The nature of the treatment variable is defined in section 2.2.3. *p<0.1; **p<0.05; ***p<0.01

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A Data Appendix

A.1 Data for Panel Analysis

The variable 'Percent Attended Births' represents the number of births attended by a skilled physician. This variable has been constructed from World Bank data relating to births attended by skilled health staff (http://data.worldbank.org/indicator/SH.STA.BRTC.ZS) and DHS data constructed from the cross-country micro data. The DHS data comes from a question regarding place of birth, where those mothers who report births in a public or private facility are assumed to have been attended by a skilled health professional.

Given the sporadic nature of the data produced, the following procedure was followed: 'Percent Attended Births' was set equal to the DHS microdata average for each year/country group pair for which this observation was available. In the case that DHS data was not available and World Bank data was, 'Percent Attended Births' was set equal to the rate from the World Bank data. Given that MMR and Education data is available in 5 year periods (1990, 1995, 2000, 2005, 2010), for each of these years where the fifth year was not available but there was data in the preceeding 5 year period, the average of the preceeding 5 year period was taken and used as the data point in the fifth year.

A.2 Data for Quasi-Experiments

Micro-level data for education and maternal mortality is used in each country. Both come from DHS. For each country we pool all available DHS waves in which the maternal mortality module was collected. These are, for Zimbabwe: 1994, 1999, 2005 and 2010; for Kenya: 1998, 2003, and 2008; and for Nigeria: 1999 and 2008.

From each respondent in the DHS Individual Recode file (ie all index women), we use their education, along with their region of birth and residence, data of birth, and various characteristics such as ethnicity and religion. To create a maternal mortality database, we take information on all of the sisters of each woman in the DHS data. For these sisters, the index woman reports if they are alive, their data of birth, if they died during child birth, and if so, in what the maternal death occurred. From this, we generate a sisters file recording maternal mortality rates for each woman, (and hence birth cohorts). Full details regarding all data, and data itself is available on the web (https://github.com/damiancclarke/MMR).

B Maternal Mortality in the Medical Literature

The medical literature suggests that maternal mortality is principally associated with labour, delivery and the early postpartum period; largely occuring between the third trimester of pregnancy and the first post-partum week. The principal medical cause of these deaths is obstetric haemorrhage, a largely preventable cause provided "access to timely and competent obstetric care" is available (Ronsmans and Graham, 2006). Along with haemorrhage, a number of other causes are reported, including sepsis, abortion, hypertensive disorders, obstructed labour, embolism and ectopic pregnancy[14]. Despite the fact that these complications account for the majority of deaths during labour, birth and the post-partum period, the prevalence of each varies by region (Khan et al., 2006). Whilst haemorrhage is the principal cause in developing regions, complications from caesarian sections and anaesthesia are more common in developed countries, and other region-specific disease burdens are noted (abortion in Latin America and the Caribbean, sepsis and HIV in Africa and anaemia in Asia).

Much focus in the existing literature is on the difficulty of obtaining accurate statistics of maternal mortality, particularly in those areas where maternal mortality is highest(Ronsmans and Graham, 2006; McCarthy and Maine, 1992; McAlister and Baskett, 2006). The ability to compile credible data is confounded by the lack of consistent classification prior to the introduction of ICD-9 coding, the need to collect data from an array of data sources, misclassification of maternal death, significant under-reporting of maternal mortality, and missing and incomplete data(Yazbeck, 2012; Hogan et al., 2010).

Early work[15] in the medical literature highlighted the importance of addressing the 'proximal causes' or inputs of maternal mortality; namely the likelihood that a women becomes pregnant, or the likelihood of complications (and the treatment of these complications) conditional upon her becoming pregnant (McCarthy and Maine, 1992; Goodburn and Campbell, 2001; Trussell and Pebley, 1984). Along with attendence and medical care for mothers during pregnancy and child birth, these studies suggest that altering maternal age, quantity of births, and spacing of births could have a considerable effect on rates of maternal mortality.

More recent medical studies also suggest that socioeconomic factors are fundamental in the reduction of maternal mortality. Costello et al. (2004), for example, argue that the focus on

[14]Highly cited figures (AbouZahr et al., 1991) suggest figures of 25% due to haemorrhage, 20% due to indirect causes, 15% due to infection, 13% due to abortions, 12% due to eclampsia, 8% to obstructed labour and 8% to other direct causes. Khan et al. (2006) provide evidence broadly in agreement with these values however note that these estimates suffer from large confidence intervals.

[15]The Safe Motherhood Initiative of 1987 was put in place in response to the increasing recognition of this topic in developing countries.

primary care and skilled attendance is not a sufficient strategy, citing considerable reductions in maternal mortality following community-based interventions[16]. McAlister and Baskett (2006) suggest that social—and specifically gender-specific factors such as female education—should predict maternal mortality, demonstrating cross-country correlation between these variables. Recent work from Ahmed et al. (2012) suggests that family planning inputs would allow for considerable reductions in maternal deaths world-wide, with the authors estimating that the equivalent of 44% of maternal deaths in 2008 were averted due to contraceptive use, and that meeting unmet demand for family planning could prevent a further 29%. Finally, the importance of socio-economic factors is also highlighted by Ahmed et al. (2010) (and earlier by Shen and Williamson (1999)) who suggest that more educated and empowered women are more likely to utilise health services related to maternal mortality such as antenatal care, and skilled birth attendants, and by Bhutta and Black (2013) who higlight the social determinants of maternal health, and the link of poor health to poorly served urban slum environments where social and educational support networks are weak.

[16] Also see Manandhar et al. (2004) for the results of a randomized trial of community-based meetings.

C Appendix Tables

Table 8: Full Country Data

Country	1990	1995	2000	2005	2010	Country	1990	1995	2000	2005	2010
Afghanistan				x	x	Lesotho		x	x	x	x
Albania	x	x	x	x	x	Liberia			x		x
Algeria		x	x	x	x	Libya		x	x		
Argentina		x	x	x	x	Lithuania		x	x	x	x
Armenia		x	x	x	x	Luxembourg	x			x	
Australia	x	x	x	x	x	Malawi	x	x	x	x	x
Austria	x	x	x	x	x	Malaysia	x	x	x	x	x
Bahrain		x		x	x	Maldives		x		x	x
Bangladesh	x	x	x	x	x	Mali	x	x	x	x	x
Barbados		x	x	x	x	Malta		x			x
Belgium	x	x	x	x	x	Mauritania		x		x	x
Belize		x	x	x	x	Mauritius	x	x	x	x	
Benin		x	x	x	x	Mexico	x		x	x	x
Bolivia	x	x	x	x	x	Mongolia			x	x	x
Botswana	x		x		x	Morocco	x	x	x	x	
Brazil	x	x	x	x	x	Mozambique		x	x	x	x
Bulgaria	x	x	x	x	x	Namibia		x	x		x
Burundi	x		x	x	x	Nepal		x	x	x	x
Cambodia		x	x	x	x	Netherlands	x	x	x	x	x
Cameroon	x	x	x	x	x	Nicaragua		x	x	x	x
Canada	x	x	x	x	x	Niger	x	x	x		x
Chile		x	x	x	x	Norway	x	x	x	x	x
China	x	x	x	x	x	Pakistan	x	x	x	x	x
Colombia	x	x	x	x	x	Panama		x	x	x	x
Croatia		x	x	x	x	Paraguay	x		x	x	x
Cuba		x	x	x		Peru	x	x	x	x	x
Cyprus				x		Philippines	x	x	x	x	x
Denmark	x	x	x	x	x	Poland	x	x	x	x	
Ecuador	x	x	x	x		Portugal	x		x	x	
Estonia		x	x	x	x	Qatar			x	x	x
Fiji			x	x	x	Moldova		x	x	x	x
Finland		x		x		Romania	x	x	x	x	x
France	x	x	x	x	x	Rwanda	x	x	x	x	x
Gabon		x	x			Senegal	x	x	x	x	
Germany	x	x	x	x	x	Serbia			x	x	x
Ghana	x	x	x	x	x	Singapore				x	
Guatemala	x	x	x	x	x	Slovenia		x	x	x	x
Guyana		x	x	x	x	Sudan	x				x
Haiti		x	x		x	Swaziland		x	x	x	x
Honduras	x	x	x	x	x	Sweden	x	x	x	x	x
Hungary	x	x	x	x	x	Switzerland	x	x	x	x	x
India	x	x	x		x	Tajikistan		x	x	x	x
Indonesia	x	x	x	x	x	Thailand	x		x		x
Iraq			x		x	Togo	x	x	x	x	x
Ireland	x	x	x	x	x	Tonga		x	x	x	x
Israel	x					Tunisia	x	x	x		x
Italy				x		Turkey	x	x	x	x	x
Jamaica	x		x	x	x	Uganda	x	x	x	x	x
Japan	x	x	x	x	x	Ukraine		x	x	x	x
Jordan	x		x	x	x	Tanzania	x	x	x	x	x
Kazakhstan		x	x	x	x	Uruguay			x	x	x
Kenya	x	x	x	x	x	Viet Nam		x	x	x	x
Kuwait	x		x	x	x	Zambia	x	x	x	x	x
Latvia		x	x	x	x	Zimbabwe	x	x	x		x

Table 9a: Cross-Country Results of MMR and Female Educational Attainment (years)

VARIABLES	(1) MMR	(2) MMR	(3) MMR	(4) MMR	(5) MMR	(6) MMR	(7) MMR	(8) MMR
Years of Education	-49.72***	-54.75***	-32.80*	-32.39*	-29.08*	-21.99	-15.50	-11.41
	(8.946)	(12.44)	(16.88)	(16.85)	(15.51)	(15.08)	(13.00)	(10.57)
year==1995			-2.585	-5.196	6.666	7.935	22.52	12.14
			(15.47)	(16.80)	(16.68)	(15.56)	(14.90)	(17.42)
year==2000			-27.73	-30.76	-13.22	-9.447	15.97	7.254
			(18.31)	(20.44)	(20.50)	(18.17)	(16.94)	(18.92)
year==2005			-46.03*	-58.12*	-22.38	-24.43	12.78	11.16
			(23.80)	(33.54)	(32.85)	(29.40)	(27.37)	(26.10)
year==2010			-56.77*	-76.58	-36.59	-36.56	9.074	23.74
			(30.02)	(48.08)	(46.26)	(41.51)	(39.35)	(36.60)
log GDP per capita				18.85	12.63	17.21	1.281	-1.671
				(26.57)	(27.08)	(24.99)	(23.86)	(24.12)
Immunization (DPT)					-3.439***	-2.994**	-2.455*	-2.374*
					(1.162)	(1.241)	(1.299)	(1.319)
Attended Births						-1.851	-0.861	-1.447
						(1.141)	(1.160)	(1.128)
Fertility							64.36***	31.19
							(21.83)	(28.03)
Teen births								2.885*
								(1.725)
Constant	625.4***	649.9***	501.2***	358.3	651.5***	661.8***	379.8	350.0
	(72.03)	(99.20)	(126.3)	(216.6)	(230.8)	(208.4)	(241.8)	(269.2)
Observations	710	428	428	428	428	428	428	428
R-squared	0.375	0.525	0.545	0.547	0.601	0.617	0.641	0.664
Number of countries	142	108	108	108	108	108	108	108

NOTES: All regressions include fixed-effects by country. For the full list of countries by year see table 8. Results are for average years of education of females between the ages of 15 and 39 in each country. A full description of control variables is available in section 3, and as the note to table 2a. Standar errors clustered at the level of the country are shown. *p<0.1; **p<0.05; ***p<0.01

Table 9b: Cross-Country Results of MMR and Female Educational Attainment (years squared)

VARIABLES	(1) MMR	(2) MMR	(3) MMR	(4) MMR	(5) MMR	(6) MMR	(7) MMR	(8) MMR
Years of Education	-220.0***	-215.9***	-195.3***	-195.7***	-182.7***	-181.9***	-179.7***	-172.2***
	(24.50)	(28.08)	(27.68)	(27.56)	(30.81)	(35.15)	(40.62)	(41.20)
Years of Education Squared	11.33***	11.01***	11.05***	11.07***	10.31***	10.27***	10.16***	9.770***
	(1.338)	(1.562)	(1.574)	(1.559)	(1.699)	(1.893)	(2.164)	(2.216)
year==1995			0.362	0.797	6.537	6.589	7.619	5.143
			(14.80)	(16.28)	(15.49)	(15.64)	(15.07)	(16.01)
year==2000			-15.24	-14.72	-6.723	-6.596	-4.856	-6.616
			(18.45)	(20.53)	(19.78)	(20.14)	(19.69)	(19.98)
year==2005			-41.94*	-39.95	-22.66	-22.74	-20.16	-19.37
			(22.96)	(31.13)	(29.47)	(29.24)	(28.29)	(28.14)
year==2010			-51.79*	-48.53	-29.71	-29.74	-26.63	-20.96
			(26.22)	(41.15)	(38.86)	(38.69)	(38.19)	(37.94)
log GDP per capita				-3.094	-4.807	-4.561	-5.439	-6.047
				(20.58)	(20.10)	(19.82)	(19.03)	(19.43)
Immunization (DPT)					-1.784*	-1.772*	-1.747*	-1.751*
					(1.037)	(1.041)	(1.047)	(1.055)
Attended Births						-0.0741	-0.0241	-0.228
						(0.948)	(0.933)	(0.936)
Fertility							4.483	-2.955
							(23.86)	(25.67)
Teen births								0.847
								(1.205)
Constant	1,132***	1,117***	969.6***	993.9***	1,102***	1,101***	1,077***	1,041***
	(99.11)	(113.3)	(115.3)	(186.7)	(178.1)	(178.7)	(258.9)	(275.9)
Observations	710	428	428	428	428	428	428	428
R-squared	0.587	0.735	0.756	0.756	0.769	0.769	0.769	0.771
Number of countries	142	108	108	108	108	108	108	108

NOTES: All regressions include fixed-effects by country. For the full list of countries by year see table 8. Results are for average years of education of females between the ages of 15 and 39 in each country. A full description of control variables is available in section 3, and as the note to table 2a. Standard errors clustered at the level of the country are shown. *p<0.1; **p<0.05; ***p<0.01

Table 10: Cross-Country Results: MMR and Female versus Male Education (years squared)

VARIABLES	(1) MMR	(2) MMR	(3) MMR	(4) MMR	(5) MMR	(6) MMR	(7) MMR	(8) MMR
Years of Education (Female)	-263.5***	-217.2***	-203.4***	-204.6***	-188.5***	-188.9***	-186.5***	-176.9***
	(62.52)	(66.51)	(57.05)	(56.86)	(56.02)	(55.92)	(54.59)	(55.48)
Years of Education Squared (Female)	14.71***	11.52***	13.45***	13.53***	12.38***	12.41***	12.27***	11.59***
	(3.358)	(3.993)	(3.925)	(3.917)	(3.797)	(3.784)	(3.658)	(3.788)
Years of Education (Male)	76.66	1.727	19.36	20.80	13.86	13.35	12.42	6.150
	(81.88)	(125.4)	(112.9)	(113.8)	(110.7)	(113.2)	(110.8)	(112.9)
Years of Education Squared (Male)	-5.387	-0.642	-3.215	-3.302	-2.686	-2.676	-2.617	-2.090
	(4.339)	(7.176)	(6.751)	(6.808)	(6.631)	(6.697)	(6.549)	(6.701)
Observations	710	428	428	428	428	428	428	428
R-squared	0.593	0.736	0.760	0.760	0.773	0.773	0.773	0.774
Number of countries	142	108	108	108	108	108	108	108

NOTES: All regressions include fixed-effects by country. For the full list of countries by year see table 8. Educational variables are the same as those in table 3 however include both female and male figures for each variable (ages 15-39). A full description of control variables is available in section 3, and as the note to table 2a. Standard errors clustered at the level of the country are diplayed. *p<0.1; **p<0.05; ***p<0.01

Table 11: Comparison of Results: DHS microdata versus WHO data

VARIABLES	(1) MMR	(2) MMR	(3) MMR	(4) MMR	(5) MMR	(6) MMR	(7) MMR	(8) MMR
PANEL A: QUADRATIC (YEARS)								
DHS data								
Years of Education	-69.87	-103.2*	-182.2**	-184.5**	-184.4**	-180.7**	-181.3**	-197.5**
	(57.11)	(50.62)	(86.27)	(89.60)	(89.73)	(87.42)	(87.26)	(90.39)
Years of Education Squared	5.793	7.834*	8.426	9.268	9.130	9.101	9.156	9.832*
	(4.670)	(4.228)	(5.603)	(5.589)	(5.554)	(5.405)	(5.617)	(5.734)
WHO data								
Years of Education	-317.8***	-299.1***	-210.3***	-212.7***	-212.4***	-206.0***	-207.1***	-195.5***
	(33.98)	(45.28)	(52.38)	(55.00)	(53.31)	(53.72)	(51.42)	(53.74)
Years of Education Squared	19.34***	17.77***	15.59***	16.42***	16.03***	15.98***	16.09***	15.60***
	(2.888)	(3.601)	(3.415)	(3.358)	(3.358)	(3.355)	(3.598)	(3.548)
PANEL B: LEVELS (ATTAINMENT)								
DHS data								
Primary Education (% Population)	-2.678	-1.951	-7.172*	-6.740	-6.725	-6.583	-6.530	-7.514
	(2.816)	(2.871)	(4.222)	(4.611)	(4.599)	(4.405)	(4.428)	(5.051)
Secondary Education (% Population)	-0.376	-1.213	-6.225	-5.790	-6.002	-5.642	-5.755	-6.817
	(3.327)	(3.068)	(4.984)	(5.279)	(5.581)	(5.707)	(5.953)	(6.552)
Tertiary Education (% Population)	-2.277	-3.771	-17.92	-15.71	-15.44	-14.18	-13.30	-13.94
	(8.245)	(8.111)	(13.37)	(14.99)	(14.84)	(14.79)	(14.10)	(14.42)
WHO data								
Primary Education (% Population)	-15.09***	-14.66***	-10.20***	-9.814***	-9.757***	-9.505***	-9.390***	-8.964***
	(1.837)	(2.284)	(3.135)	(3.125)	(3.123)	(3.161)	(2.789)	(3.131)
Secondary Education (% Population)	-10.83***	-9.436***	-2.823	-2.436	-3.247	-2.608	-2.853	-2.393
	(1.948)	(1.916)	(3.333)	(3.548)	(3.412)	(3.716)	(3.473)	(4.053)
Tertiary Education (% Population)	-18.31***	-18.01***	-6.394	-4.430	-3.413	-1.171	0.740	1.017
	(3.735)	(3.084)	(7.481)	(7.385)	(6.971)	(7.501)	(6.904)	(7.252)
Observations	159	115	115	115	115	115	115	115
Number of Countries	37	31	31	31	31	31	31	31

NOTES: All regressions include fixed-effects by country. Results are for average years of education of females between the ages of 15 and 39 in each country. Controls in each column are identical to those in table 3. Panel A compares estimates of the effect of maternal mortality on years of education, when maternal mortality data either comes from DHS (microdata) or WHO. Panel B presents similar results, however using levels of education rather than years. *p<0.1; **p<0.05; ***p<0.01

Table 12a: 1976 Educational Expansion Placebo: Nigeria

	(1)	(2)	(3)	(4)
PANEL A: OUTCOME – EDUCATION				
Intensity 56-60	1.534*	1.449	-0.348	0.213
	(0.818)	(1.177)	(0.336)	(0.168)
Intensity	-0.242			
	(0.872)			
Observations	2,617	2,617	2,617	2,617
R-squared	0.299	0.297	0.300	0.301
PANEL B: OUTCOME – MMR				
Intensity 56-60	-0.0118	-0.00631	-0.00315	-0.00257
	(0.0107)	(0.0117)	(0.00389)	(0.00204)
Intensity	0.00135			
	(0.0119)			
Observations	6,508	6,508	6,508	6,508
R-squared	0.014	0.014	0.015	0.015
PANEL C: OUTCOME – MMR (UNDER 25)				
Intensity 56-60	0.00356	0.00177	-0.00196	-0.00156
	(0.00616)	(0.00589)	(0.00227)	(0.00148)
Intensity	-0.00886			
	(0.00992)			
Observations	6,508	6,508	6,508	6,508
R-squared	0.011	0.011	0.011	0.011

NOTES: For a full description of outcomes and treatments see Table 7a. A placebo treatment here is defined by comparing two groups who had already left primary school by the time of the reform. The birth cohorts from 1956-1961 were defined as the placebo 'treatment' and the cohorts from 1950-1955 were defined as controls. *p<0.1; **p<0.05; ***p<0.01

Table 12b: 1980 Educational Expansion Placebo: Zimbabwe

	Years of Education			Maternal Mortality		
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
DumAge	-0.668*	-0.372	-0.824	0.00439	-0.00142	-0.000423
	(0.344)	(0.319)	(0.833)	(0.00307)	(0.00605)	(0.00684)
DumAge×age1980less20	-0.372***	0.0627	-0.171	0.000489	-0.000297	-0.00266
	(0.0345)	(0.109)	(0.215)	(0.000268)	(0.00133)	(0.00338)
(1-DumAge)×age1980less20	-0.0727	-0.256*	-0.624	0.000598	-0.00253	-0.000121
	(0.0622)	(0.118)	(0.698)	(0.000542)	(0.00253)	(0.00678)
Observations	6,773	6,773	6,773	20,836	20,836	20,836
R-squared	0.184	0.188	0.188	0.001	0.001	0.001

NOTES: For a full description of outcomes and treatments see Table 7b. A placebo treatment here is defined by comparing two groups who had already left primary school by the time of the reform in 1980. The placebo 'treatment' was defined between the cohorts born in 1960 and 1961 (20 years old at the time of the reform), and the same window is used (16 years) as the real treatment in Table 7b. *p<0.1; **p<0.05; ***p<0.01

Table 12c: 1985 Educational Expansion Placebo: Kenya

VARIABLES	(1) Years of Education	(2) Maternal Mortality
Treatment	-0.0994	-0.0152
	(0.531)	(0.00978)
Observations	13,703	25,602
R-squared	0.203	0.031

NOTES: For a full description of outcomes and treatments see Table 7c. A placebo treatment here is defined by comparing two groups who had already left primary school by the time of the reform in 1985. The placebo 'treatment' was defined as occurring in 1977, and hence affecting (at least partially) birth cohorts from 1955 to 1963, rather than the true affected cohorts of 1964 to 1972. *p<0.1; **p<0.05; ***p<0.01