Institutional Births and Early Child Health: Evidence from Ghana's Free Delivery Policy

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January 24, 2020

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Many developing countries have implemented policies to encourage deliveries in formal health facilities to combat maternal and infant mortality. Evidence about their effectiveness remains limited and mixed, raising questions of whether health systems can improve child survival. This paper evaluates a Ghanaian policy that made facility births free using a regression-discontinuity-in-time design, along with difference-in-difference estimates, to measure effects on delivery decisions, health-service take-up, and mortality and health. We find large effects on facility births, particularly for the poorest mothers, which lead to substantial reductions in longer-run child mortality (but no change in neonatal mortality) and improvements in child health.

Keywords: child mortality, institutional delivery, health-service provision

JEL Codes: I15, I18, O15

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Abstract

Many developing countries have implemented policies to encourage deliveries in formal health facilities to combat maternal and infant mortality. Evidence about their effectiveness remains limited and mixed, raising questions of whether health systems can improve child survival. This paper evaluates a Ghanaian policy that made facility births free using a regression-discontinuity-in-time design, along with difference-in-difference estimates, to measure effects on delivery decisions, health-service take-up, and mortality and health. We find large effects on facility births and some effects on mortality and health, particularly among the poorest. We find the strongest effects on long-term outcomes, possibly explaining previously mixed findings.

1 Introduction

Despite substantial reductions over recent decades, rates of early child mortality in many developing countries remain roughly ten times higher than in more developed nations (UN IGME, 2018). Nearly half of all deaths under the age of 5 occur in the first 28 days of life (the neonatal period), and of these, the majority occur at birth or in the days following birth (Lawn et al., 2005). In regions where child mortality tends to be highest (including Sub Saharan Africa and South Asia), only about half of births occur in a health facility. As a result, many developing country governments and global health organizations are pursuing policies aimed at raising the number of institutional births. However, there is surprisingly little evidence on the efficacy of facility births to reduce early child mortality, and this evidence is quite mixed.

Studies that examine the impacts of policies that shift women who otherwise do not use facilities into facilities generally find no effects. Powell-Jackson et al. (2015) show that a large conditional cash transfer program in India that encouraged facility deliveries had no effect on neonatal mortality. Godlonton and Okeke (2016) also find no overall effect on early mortality following a ban on traditional birth attendants that increased institutional deliveries in Nigeria. However, they do find large reductions in neonatal mortality for women whose closest facility was "high" quality.[1]

[1] Godlonton and Okeke (2016) define "high" quality facilities as those meeting at least four of the following criteria: has operating theater, has intensive care unit, has pharmacy, has trained staff available 24 hours per day, offers blood transfusions, offers ambulance services, offers laboratory services, is open 7 days a week.

Indeed, health care quality is likely an important mitigating factor on the impact of expanding access to institutional births. A large literature documents the low quality of care available in many health facilities in developing countries. Absenteeism rates are high (Chaudhury et al., 2006), and effort and knowledge of best practices among practitioners can be limited (Das and Hammer, 2005; Das et al., 2008; Das and Hammer, 2014). In addition, surveys of health facilities often find inadequate supplies of drugs, equipment, and infrastructure.[2] Thus, the current lack of facility use may simply reflect a revealed preference for home births given the available options. Alternatively, programs that effectively get more women to deliver in facilities may trigger supply side constraints that limit resulting improvements in health. For example, Adam et al. (2018) show that over-crowding in a private health facility in Kenya, due to a public health sector strike, increased neonatal mortality.

On the other hand, a pair of studies from developing countries have also found that, for women who typically use facilities, institutional births can confer large child health benefits.[3] Okeke and Chari (2018) compare outcomes between children born during the day and during the night in areas with and without 24-hour care in Nigeria and find that neonatal mortality doubles when women are unable to deliver in facilities. Friedman and Keats (2019) show that for Kenyan women who would normally deliver in hospitals, disruptions to care at birth caused by health worker strikes increase both neonatal and infant mortality (deaths within the first year of life). This evidence suggests that removing barriers to institutional deliveries could have positive benefits even under standard levels of care.

This paper contributes new evidence on the effects of expanding access to facility births by examining a reform in Ghana that made institutional deliveries free beginning in 2004. We construct a retrospective panel of births using data from the 1993, 1998, 2003, and 2008 rounds of the Ghana Demographic and Health Survey (GDHS). We employ two identification strategies that each take advantage of different features of the available data. First, for outcomes that are reported retrospectively, and for which the timing of measurement should not change reporting (e.g., delivery location and neonatal mortality), we use a regression-discontinuity-in-time design, looking

[2] see, e.g., Demographic and Health Surveys Service Provision Assessment, `https://dhsprogram.com/What-We-Do/Survey-Types/SPA.cfm`; World Bank Service Delivery Indicators, `https://www.sdindicators.org/`

[3] Positive effects have also been found in more developed countries. In the Netherlands, Daysal et al. (2015) use distance to hospitals as an instrument for hospital births and find that hospital births decrease neonatal mortality relative to home deliveries.

for discontinuous changes in outcomes of births reported just before and after the policy change. Second, for outcomes for which the timing of measurement matters (e.g., longer-run mortality, health status, and take-up of vaccines, especially those not typically administered at birth), we use a difference-in-difference specification.

Looking at births around the implementation of the policy we find a sharp increase in institutional deliveries following the reform. Although the policy provided payments for services directly from the government to both public and private facilities, meaning women could deliver in either type of facility free of charge, all of the gains were seen in public facilities. Moreover, we find that the change in location of birth was driven predominantly by those most likely to alter their behavior as a result of the policy, namely, those who are most financially constrained. Among women in the bottom three quintiles of the household wealth distribution (the "poor") we see a 9 percentage point increase in institutional births, representing a 30 percent increase relative to the mean for this group prior to the reform, and no change for women in the top two wealth quintiles (the "non-poor").[4]

Similar to other studies that examine impacts following policies that encourage women to deliver in facilities, we do not observe any statistically significant changes in neonatal mortality. However, we do find evidence of longer-run improvements. Point estimates for both 7-day and 1-month mortality are negative, but are imprecisely measured, and show no systematic differences between women at different points of the wealth distribution. In contrast, there is evidence that infant mortality decreases for children born to relatively poorer women following the reform. Although these estimates are not statistically significant in all specifications, and therefore should be treated with some caution, the reform appears to have reduced infant mortality by about 28 percent (20 fewer deaths per 1000 live births), effectively closing the mortality gap that existed between children born to poor and non-poor women prior to the policy change.

Results from the difference-in-difference estimation show the same patterns of mortality effects. We use the timing of the GDHS surveys, and the differential responses to the policy by household wealth, to measure the impacts of the program. Children under 5 in the 1993, 1998, and 2003 survey rounds were unaffected by the program, while children under 5 in the 2008 survey were

[4]Results are similar if we 1) exclude the middle quintile and designate only the bottom two quintiles as poor, and 2) replace poor and non-poor with rural and urban.

born under a regime of free delivery care. As in the regression-discontinuity-in-time framework, we find no change in neonatal mortality and a large decrease in infant mortality among children born to poor women. We see even larger decreases in mortality by age 3; age-3 mortality declines by 37 per 1000 live births, again eliminating the gap in mortality that existed prior to the reform across children born to poor and non-poor women. Reassuringly, there are no differential impacts on the poor across survey rounds prior to the reform, suggesting that parallel trends assumptions are justified.

Among children who survive, we also find improved health outcomes following the reform. We use child height- and weight-for-age z-scores as summary measures of health. Relative to children born to non-poor mothers, children born to poor women increase height by an additional .154 standard deviations following the reform and are 5 percentage points less likely to be stunted (defined as having a height-for-age z-score less than 2 standard deviations below the reference population mean). In addition, although we do not see a statistically significant differential change in weight-for-age after the policy change, children born to poor women are 4 percentage points less likely to be wasted (having a weight-for-age z-score less than 2 standard deviations below the reference population mean) as well. To the extent that the deaths averted by the reform were from among children who are smaller than average (e.g. premature babies), these results are potentially underestimates of the true effect of institutional births on child health.

Finally, there is evidence that children who gain access to institutional births also receive more health inputs in their first year of life. In particular, children born to poor mothers exposed to the free delivery policy are disproportionately, and statistically significantly, more likely to be vaccinated than children born to non-poor mothers following the reform. Depending on the vaccine, effect sizes range from an increase of 5 percent (for tuberculosis) to 25 percent (for measles). These results are especially striking given that the free delivery policy made no explicit change to the cost of obtaining vaccines, and that most vaccines are administered after children reach at least 6 weeks of age, that is, well after birth. Although we lack data to shed light on what mechanisms connect facility births to these longer-run health seeking behaviors, our results are consistent with Okeke and Chari (2018), who find that plausibly random non-facility births decrease postnatal check-ups, and Friedman and Keats (2019), who find that disruptions to care at birth reduce vaccination rates in the first year of a child's life.

One issue of interpretation is whether these results are driven by income effects rather than location of birth effects. For women who would give birth in a facility even in the absence of the reform, the free delivery policy was effectively an income transfer. This was likely the case for the majority of non-poor women, 82 percent of whom delivered in facilities prior to the reform. However, if income effects lead to improved child health outcomes among the non-poor, then this biases our difference-in-difference estimates of the effects of institutional births downward. On the other hand, similar income effects among the poor would bias these effects upward. We test for this and find evidence that wealth effects are not responsible for the gains we observe in child health outcomes among the poor. In addition, we also find no evidence of changes in the characteristics of mothers who give birth before and after the reform or in the number of children they have.

Taken together, our results help clarify some of the prior mixed evidence on the efficacy of institutional births. First, our finding of little or no effect on neonatal mortality mirrors the results from other studies that also examine policies designed to shift women into facility births (Powell-Jackson et al., 2015; Godlonton and Okeke, 2016). One possibility for these null effects is that women with low-risk pregnancies, or who otherwise have low returns to institutional deliveries, form the compliant population under such policies. However, in both this paper and Godlonton and Okeke (2016), the evidence suggests, if anything, compliers are somewhat higher-risk. Another possibility is that health facilities do not adequately provide the necessary care to address neonatal mortality risks, consistent with the literature documenting the low quality of care in developing countries. Indeed, surveys of facilities in Ghana find few are equipped to manage obstetric and neonatal emergencies (Saleh, 2013; Nesbitt et al., 2013). The patterns of facility use and mortality prior to the free delivery policy provide further support for this possibility: neonatal mortality rates among children born to the non-poor, who overwhelmingly delivered in facilities prior to the reform, were almost identical to those of children born to the poor, who mainly delivered at home.

At the same time, we do find impacts on longer-run child mortality and health. These long-term effects were not addressed in previous studies of these policies, but they are in line with studies showing that access to facility births can have large mortality impacts even in these settings (Okeke and Chari, 2018; Friedman and Keats, 2019). One possible link between institutional births and longer-run child health may be through increased preventative health care or greater attachment to the health care system early in life. The evidence of increased vaccinations following the free delivery policy found in this paper, as well as evidence from Okeke and Chari (2018) and Friedman and Keats (2019), supports this potential channel. In addition, to the extent that there are spillovers (through, for example, herd immunity), the vaccination results may also help explain why child mortality gaps between the poor and non-poor close despite evidence that the policy did not fully close the gap in institutional births (Ward, 2014; Luca et al., 2017; Carpenter and Lawler, 2017).

Our study also contributes to a large literature on the collection of user fees for health services, particularly in developing countries. A key focus of the debate around user fees is the effect on demand for services and whether small user fees decrease demand for a health input among those who need it. Our results show that user fees constrain women from seeking institutional deliveries and that this contributes to excess mortality and reduced health, particularly for the poor. These results are consistent with previous studies showing that policies that removed user fees increased demand for health services in Uganda (Deininger and Mpuga, 2005) and Zambia (Hangoma et al., 2018). Our findings are also in line with research on cost-sharing for particular inputs using randomized evaluations (Cohen et al., 2010; Kremer and Holla, 2009). Besides screening for those with high demand, user fees are also used to decrease government spending. In this case, the optimal policy may still be unclear as the policy of free institutional births ended when it ran out of funding.

The remainder of this paper proceeds as follows. Section 2 provides information on health care in Ghana, with a focus on available maternity care and the free delivery policy. Section 3 describes the sample characteristics in our data, while Section 4 presents the analysis and results. Finally, we conclude with a discussion of our findings in Section 5.

2 Background on health care in Ghana and the free delivery policy

In Ghana child delivery services are offered in public health clinics, district and regional hospitals, and national referral hospitals. Private facilities, including mission clinics and private maternity homes, also offer basic obstetric services, but are less frequently used. The distribution of health facilities is uneven, with most located in more urban areas. Quality of care varies as well, with hospitals generally offering better services. However, no clinics, health centers, or maternity wards, and less than 30 percent of hospitals, offer comprehensive emergency obstetric or neonatal care

(Saleh, 2013). A survey of all facilities that provide delivery services in one region of Ghana similarly found that few provided emergency obstetric or neonatal care, and that this was mainly driven by a lack of infrastructure, supplies, or essential medicines (Nesbitt et al., 2013). Nationally, there are 10 doctors, nurses, and midwives per 10,000 people, which is comparable to the regional average (13), and well below the 23 high-level health professionals recommended by the World Health Organization (WHO, 2018). Public health care spending as a fraction of total government spending is 7.1 percent, which is also similar to the average spending of other Sub-Saharan African countries.

In late September 2003 the Government of Ghana announced the free delivery care policy. Prior to this point, health care financing in Ghana largely followed a system of "cash and carry" in which all health facility services required full upfront payment before the delivery of care. In surveys conducted before the reform, women cited the cost of user fees as one of the most important barriers to accessing institutional births (Witter et al., 2007). In 1998 antenatal care in public facilities was exempted from charging user fees through the Safe Motherhood Initiative launched by the Ghana Health Service and the Ghana Ministry of Health.

Under the free delivery policy, expectant mothers could deliver at either public or private facilities at no cost. In turn, health facilities were reimbursed for deliveries and other related services by the central government. Initial financing for the scheme included support from the Highly Indebted Poor Countries (HIPC) initiative. The government announced the reimbursement schedule in early 2004, and payments to facilities for services provided followed thereafter. The removal of user fees was first implemented in the four most deprived regions of the country (Northern, Upper East, Upper West, and Central). In 2003, just 21 percent of children in these regions were born in a health facility, compared to 52 percent elsewhere. The policy was officially expanded to cover the remaining 6 regions of Ghana in 2005.

A handful of studies from reproductive health and medical journals used survey data from select regions of the country to examine the effects of the reform around the time of its implementation. These studies find that out-of-pocket expenses for women delivering in facilities declined by more than 25 percent (Asante et al., 2007); that institutional births increased, particularly for the poor (Penfold et al., 2007); and little evidence of any decline in health care quality as a consequence of the reform (Witter et al., 2007).

However, by 2007 funds had run out and many facilities began to reintroduce user fees (Witter and Adjei, 2007). In mid-2008, after noting a decline in the use of skilled maternity care, the government moved to automatically register (at no cost) all pregnant women who had attended at least one antenatal visit at an accredited facility into the National Health Insurance Scheme (NHIS).[5] We therefore consider 2004-2008 as the treatment period in this study.

3 Sample characteristics

Data come from the 1993, 1998, 2003, and 2008 Ghana Demographic and Health Surveys (GDHS). In each cross section, data are collected from a nationally representative sample of women aged 15-49 and contain information on basic demographic characteristics of women (e.g. age, education, asset ownership) as well as completed birth histories, including information on the month and year of all births and all child deaths that have occurred prior to the survey date. Additional information on location of birth, medical personnel present at birth, vaccinations, and child anthropometric measurements (height and weight) are collected for all children under the age of 5 at the time of the survey.

Sample characteristics are presented in Table 3. The first column shows overall means, while columns 2 and 3 presents means for children born to poor and non-poor women separately. Less than half of all children are born in any health facility, with large differences across the poor and non-poor. Just 30 percent of poor women delivered in a facility, while almost 80 percent of non-poor women did so. While a handful of women used private facilities (predominantly the non-poor), the overwhelming majority of institutional births for all women were in government-run facilities. Among facility births, more than 70 percent occurred in a hospital rather than a clinic or health center. There are also large gaps across household wealth with respect to early infant care as proxied by vaccination rates. Children born to poor women are roughly 10 percentage points less likely to be vaccinated against tuberculosis (BCG), diphtheria, pertussis, and tetanus (DPT), polio, and the measles.

Given the differences in early life health inputs across wealth categories, it is not altogether

[5] The NHIS was also first introduced in 2004, with the goal of achieving universal coverage within 5 years. However, initial take up was low (less than 20 percent were covered by 2007, (Witter et al., 2007)) and a study examining usage between 2004 and 2007 found it had no impact on institutional deliveries (Chankova et al., 2009).

surprising that there are also disparities in child health outcomes. However, these differences take time to fully materialize. Infant mortality rates – measured as either deaths in the first 7 days or first 1 month of life – are approximately 35 and 46 per 1000 births, respectively. Children born to poor mothers experience 2 additional deaths per 1000 births in the first week, and 7 additional deaths in the first month, compared to children of more well-off mothers. This mortality gap increases significantly by the end of a child's first year; children born to the poor now experience 23 additional deaths per 1000 births compared to the non-poor. Among children who survive, indicators of overall health also reveal differences by mother's wealth. While all children in the sample have height- and weight-for-age z-scores below the reference population mean, children born to the poor are about a half a standard deviation smaller across both measures. Similarly, these differences are relatively small to begin with and appear to widen as children age.

4 Analysis and Results

4.1 Regression Discontinuity in Time Results

For the first set of analyses, we estimate the effects of removing user fees for institutional deliveries with a regression-discontinuity-in-time approach, looking at delivery characteristics and child mortality for those born before and after the policy reform. For each outcome we estimate:

$$Y = \beta after + \gamma \textit{Birth month-year} + \delta(\textit{Birth month-year} \times after) + \varepsilon_1 \tag{1}$$

where *after*=1 if a child was born in any month following the implementation of the reform (January 2004), and the variable *Birth month-year* has been re-centered to this date as well so that the coefficient β can be interpreted directly. This design with linear splines in either side of the discontinuity allows us to control for changes over time, with different trends before and after the policy change. Standard errors are clustered at the birth-month-year level. For this analysis, we use all four waves of the Ghana DHS (1993, 1998, 2003, and 2008). For birth location and delivery outcomes, data are limited to children born in the 5 years preceding each survey; for mortality outcomes we include all children born between 1989 and 2008.

We present our findings both as figures and with regression coefficients in tables. In the main figures, we collapse the outcome data to the year level, for ease of visual presentation, as the month-averages are much noisier. However, the lines plotted reflect best-fit lines using monthly data, and the figures with the monthly data are included in the Online Appendix. The regression coefficients presented are those derived from estimating the equation above.

We find that the policy had large and immediate effects on the location of births (Figures 2a and 2b and Panel A of Table 4). Prior to the policy change the fraction of children delivered at a health facility was approximately 42 percent, and had largely remained unchanged in the preceding 14 years. After implementation of the policy began in early 2004, however, the rate of facility births jumped by 8.1 percentage points and remained higher with a trend upward, for the subsequent 5 years. Similarly, immediately following the reform, the fraction of births attended by a doctor, nurse, or midwife increased discontinuously by 6.3 percentage points. Government facility births jumped by a statistically significant 9.2 percentage points, while births at private facilities slipped by about 1 percentage point, although this difference is not statistically significantly different from zero. This suggests that the gains in facility births almost entirely represent women switching from home births to births at government facilities.

Remarkably, both the timing and the size of the discontinuity in institutional births was identical in regions that did not technically adopt the policy until 2005 and those regions that first adopted the policy in 2004. Appendix Figure A1 shows these results, separately plotting the estimates over time in early and late adopting regions. Although it is not entirely clear why this occurred, there is some descriptive evidence that women may have crossed regional borders to seek out facilities offering free deliveries in 2004 and early 2005, and that some late-adopting regions were providing services free to patients before the policy was made national (Witter et al., 2007).

There were, however, heterogeneous treatment effects. In particular, the effects of the reform appear to have been concentrated entirely among relatively poorer women. Figures 2c and 2d and Panel B of Table 4 show the response to the reform separately for women in the bottom 3 quintiles of the wealth index distribution (the "poor") and the top 2 quintiles of the wealth distribution (the "non-poor"). In this case we estimate:

$$\begin{aligned} Y = \; & \beta_1 after + \beta_2 (after \times poor) + \gamma_1 Birth\ month\text{-}year + \gamma_2 (Birth\ month\text{-}year \times poor) \\ & + \delta_1 (Birth\ month\text{-}year \times after) + \delta_2 (Birth\ month\text{-}year \times after \times poor) + \zeta poor + \varepsilon_2 \end{aligned} \tag{2}$$

where β_1 measures the discontinuity in outcomes for non-poor women and β_2 measures the difference in the discontinuity in outcomes for poor women relative to non-poor women. The discontinuity for outcomes among children born to the poor, $\beta_1 + \beta_2$, is presented at the bottom of the table along with its standard error and a test of the joint significance of this sum. Finally, the coefficients γ_1, γ_2, δ_1, and δ_2 allow for different trends in outcomes across poor and non-poor women, both before and after the reform.

On the eve of the policy change, just 28 percent of children born to poor mothers were delivered in any health facility, compared to approximately 84 percent among the non-poor. For the poor, the incidence of facility births and government facility births, and the probability a trained health professional attended a birth, all increased discontinuously in 2004 (by 8.6, 10.2, and 6.3 percentage points, respectively). In contrast, among the non-poor, there was no apparent discontinuity around the start of 2004 in any of the outcomes associated with location of delivery, suggesting that user fees were not a binding constraint for this group prior to the reform.

We next investigate the effects of expanding access to institutional deliveries on early child mortality. Results from estimating equations 1 and 2 are presented in Table 5. Similar to other studies that examine the effects of increasing facility births, we find no statistically significant effects on newborn mortality measured as either deaths in the first 7 days, or first 30 days, following birth. Prior to the reform, the rates of 7-day and 1-month mortality were approximately 34 and 41 per 1000 births. In the full sample, the estimated discontinuities for both measures are negative (-6 and -5), but they are measured imprecisely and we cannot reject that there was no change in either outcome. Looking at children born to poor and non-poor women separately in Panel B, we again find reductions in neonatal mortality but no systematic differences in the pattern of results.

However, we do find evidence of differential effects for 12-month mortality. In particular, for children born to poor mothers we find that infant mortality drops at the discontinuity by approximately 21 fewer deaths per 1000 births. This effect is large and statistically significant beyond the 95 percent confidence level – eliminating the mortality gap that existed between children of the poor and non-poor prior to the policy change – and it is sustained in subsequent years (see Figure 3d). For children born to the non-poor, there appears to be an increase in 12-month mortality at the discontinuity, but this effect is not statistically significant and is limited to only the cohorts born in the first few months after the reform – otherwise, mortality for this group is unchanged.

For each outcome in Tables 4 and 5 we test the extent to which the linear specification is sensitive to the size of the bandwidth around the start date of the policy. Figures A2 and A3 plot the estimated discontinuity at each bandwidth for the location of birth and infant mortality outcomes. We find that the estimated discontinuity is quite stable once the data window is opened to include at least 2 years of births on either side of the cutoff. Moreover, the graphical evidence in Figure 2 and 3 supports the choice of the linear specification, and adding quadratic terms to equation 1 or 2 has minimal impact on the size or significance of the estimated discontinuities at the preferred bandwidth.[6]

4.2 Difference-in-Difference Results

We continue our investigation of the effects of the reform using a difference-in-difference strategy that takes advantage of the differential effects of the policy across poor and non-poor women. This approach allows us to examine impacts on longer-run child health (proxied by height- and weight-for age z-scores) and preventative health care (vaccines) that cannot be analyzed using the regression discontinuity design due to the timing of the survey rounds and the fact that these outcomes vary widely depending on the age at which children are surveyed. In particular, for children born on the eve of the reform data were collected when they were only a few months old (in the 2003 GDHS), whereas for children born just after the reform data were collected when they were nearly 5 years old (in the 2008 GDHS). The difference-in-difference strategy also provides another robustness check for the regression-discontinuity-in-time results by allowing for nonlinear, and possibly differential, trends in periods prior to the reform.

In this application, the difference-in-differences design compares children under the age of 5, born to poor and non-poor women, across the GDHS cross sections from 1993, 1998, 2003, and 2008. Specifically, we estimate:

$$\begin{aligned} Y = \; & \beta_1(SY_{1998} \times \mathit{poor}) + \beta_2(SY_{2003} \times \mathit{poor}) + \beta_3(SY_{2008} \times \mathit{poor}) \\ & + \gamma_1(SY_{1998}) + \gamma_2(SY_{2003}) + \gamma_3(SY_{2008}) + \delta \mathit{poor} + X\theta + \varepsilon_3 \end{aligned} \tag{3}$$

where $SY_j = 1$ if the survey was conducted in year j or afterward, and thus the coefficients γ_j

[6] Results not shown but available upon request.

measure changes in outcome Y across survey rounds. The coefficients β_1 and β_2 measure any differential trends prior to the implementation of the reform, and therefore serve as a falsification test of the difference-in-difference strategy. The coefficient of interest is β_3, which measures the differential effect among children born to poor women relative to children born to non-poor women following the start of the free delivery policy in 2004. Finally, X is a vector of controls and includes age of child at the time of the survey. Standard errors are clustered at the birth-month-year level.

We first revisit the effect of the reform on delivery outcomes. Unsurprisingly, the difference-in-difference estimates (presented in Table 6) closely match the regression discontinuity results. The coefficient on the interaction term $SY_{2008} \times poor$ shows that poor women disproportionately increased institutional births following the reform relative to non-poor women. As before, these gains appear to be largely driven by increases in government facility usage, and smaller (and statistically insignificant) decreases in use of private facilities. There also appear to be differential effects between the 1998 and 2003 surveys – non-poor women increase facility use while poor women do not – but this is likely due to the differential trends in these outcomes in the years leading up to the reform. Indeed, as seen previously in Figure 2c and Table 4 Panel B column 1, the non-poor were gradually (and smoothly) increasing usage of facilities in the 14 years prior to the reform while the poor had essentially no change in usage until 2004.

The difference-in-difference estimates of the effect of the free delivery policy on mortality (shown in Table 7) also closely track the regression discontinuity results. There are no differential effects among the poor in either 7-day or 1-month mortality as a consequence of the reform. For 12-month mortality, we again find large effects among children born to poor women after the policy change. The coefficient on the interaction term $SY_{2008} \times poor$ in column 3 shows a reduction of nearly 25 deaths per 1000 births before a child has reached 1 year of age. This effect closes the mortality gap that existed between children born to poor and non-poor women prior to the reform. Mortality reductions are even larger when measured as deaths within the first 3 years of life (column 4), and also effectively close the mortality gap between poor and non-poor children.[7] For both outcomes we observe nearly identical trends between children born to the poor and non-poor in the GDHS surveys conducted before 2004 (see also Figure 4).

Among children who survive, there is evidence that both overall health and access to early-life

[7] The regression discontinuity results for 36-month mortality are similar and available upon request.

preventative health care improved as well. Table 8 presents results on child height- and weight-for-age, and Table 9 presents results on vaccinations. In these regressions we do not include the 1993 GDHS because outcomes in this survey were measured only for children born in the previous 3 years (as opposed to the last 5 years in the other samples) and, since vaccination rates and child anthropometric measures are age dependent, this creates artificial differences in average outcomes across surveys.[8]

Following the removal of fees for facility births, children born to poor mothers experienced disproportional gains in both stature and weight relative to children born to non-poor mothers during the same period. Column 1 of Table 8 shows height-for-age among poor children increased by an additional .154 standard deviations, reducing the height gap with children born to the non-poor by 20 percent. Moreover, these gains lowered the probability a child is stunted (defined as having a height-for-age z-score less than 2 standard deviations below the reference mean) by an additional 5 percentage points relative to the non-poor. Weight-for-age also increased following the reform, but not differentially across groups of children. However, the probability a child is wasted (weight-for-age z-score less than 2 standard deviations below the reference mean) declined among poor children by an additional 4 percentage points relative to children born to the non-poor, who saw no change in this outcome. These effects may underestimate the true impact of the policy to the extent that the children who survived as a consequence of the reform are negatively selected in terms of either height or weight.

We also find large impacts on vaccination rates among children born to the poor after the implementation of the reform. Vaccination rates for BCG (which targets tuberculosis), DPT (diphtheria, pertussis, and tetanus), polio, and measles increased by an additional 4-8 percent. In contrast, there was little change in vaccination rates among the non-poor born during the same period. Looking at the coefficients on the interaction term $SY_{2003} \times poor$, there is no evidence of differential trends in these outcomes in the two survey rounds prior to the reform These improvements are particularly striking given that, except for BCG, these vaccines are not typically given at birth (the Ghana Ministry of Health recommends the first dose of DPT at 6 weeks and the measles vaccine at 9 months). In addition, while the policy reform made deliveries at facilities free to mothers it did not affect the cost or availability of vaccines.

[8]Including the 1993 GDHS has no impact on the difference-in-difference estimates presented in Table 8.

Finally, we use the difference-in-difference framework to address potential competing explanations for our results. First, we check whether characteristics of mothers changed following the reform. Of particular concern is whether there were other contemporaneous programs or policies that had out sized effects on poor women (or on the types of poor women who became pregnant after the policy change) that could have contributed to the gains we observe in their children's outcomes. In Table A1 we present results of estimating equation 3 on a set of mother characteristics and find little change in these characteristics around the start of the free delivery policy (columns 1-4). If anything, poor women who gave birth following the reform are somewhat worse off in terms of household assets. Importantly, we see no change in the number of births per woman in the 5 years after the removal of fees (column 5), or in the risk profile of mothers as measured either by parity or total number of children a woman has borne who have died (columns 6 and 7).[9]

Another possibility is that income effects, rather than location of birth effects, are driving the results. For women who would have delivered in facilities in the absence of the reform, the policy of removing user fees acted as an income transfer. Such income effects were particularly likely for the non-poor, who overwhelmingly used facilities prior to the reform, but there were also segments of the poor who regularly delivered in facilities prior to 2004 as well. To the extent that child health outcomes are increasing with household wealth, the presence of income effects among the non-poor would tend to bias the effects estimated in this paper downward. At the same time, any income effects accruing to the poor would bias effects upward. Because we are more concerned with upward bias, we test the sensitivity of our results by removing women from the middle wealth quintile (the most well-off of the poor), who were twice as likely (41 percent) to deliver in facilities prior to the reform compared to women in the bottom two wealth quintiles. these are also the ones likely to have the largest response to income effects, if there are any Online Appendix tables A2-A5 replicate the main results tables with this sample restriction. Reassuringly, we find nearly identical results suggesting that, indeed, our results are driven more by an institutional birth effect rather than an income effect.

inline

[9]There is also no change in the total number of births following the reform. Results available upon request.

5 Discussion

This paper contributes to a still small but growing body of evidence regarding the efficacy of policies that encourage institutional births as a means to improve child health and early mortality outcomes. We find that the policy to remove user fees for facility deliveries had large and immediate effects on the rate of institutional births, particularly for the poorest mothers, suggesting that costs are a significant barrier to access to health care. In terms of child mortality outcomes, we find both that neonatal mortality is largely unaffected by location of birth *and* that location of birth has large effects on infant and 3-year mortality. We also find that children born in facilities are taller and heavier, suggesting gains in overall health as well, although these effects are more modest.

The lack of any measurable effect of institutional births on neonatal mortality could be due to a lack of statistical power, but it could also indicate that facilities are unable to adequately address common mortality risks at, or near, the point of delivery. This is consistent with survey evidence showing that most facilities in Ghana lack the capacity to offer emergency obstetric or newborn care. This interpretation is further supported by the fact that, prior to the reform, there was no discernible gap in newborn mortality between children of poor and non-poor women despite a sizable gap in the usage of facilities during the same period and a gap in mortality at older ages. It is also in line with the pattern of effects found in previous studies that examine the impacts of policies that encourage women to deliver in facilities.

At the same time, our results also suggest that facility deliveries do increase long-run health, likely through greater attachment to and use of public health services in the first months of life. The 12-month and 36-month mortality reductions we observe are quite large relative to the increase in facility usage, implying either perfect targeting of at-risk pregnancies or a massive effect of institutional births. Given that prenatal care was nearly universal at the time of the reform, it is possible that women were aware of these mortality risks and that those with high-risk pregnancies disproportionately took up free institutional deliveries. However, this would not necessarily explain why we see long run effects when there are no impacts on neonatal deaths. According to estimates from the World Health Organization, in Ghana the majority of child deaths in the post-neonatal period (i.e. after 1 month of life) were due to diarrhea, measles, malaria, and acute respiratory infections.[10] One possible explanation is that the increase in facility births creates additional benefits – beyond those services provided at birth – that help increase children's defenses against these illnesses. Indeed, the vaccine results found in this paper suggest a potential channel for the mortality and child health results may be greater use of preventative health care or increased contact with postnatal health service providers. Moreover, the increase in vaccination rates themselves may also help reconcile the large mortality effects. Studies of immunization policies in the U.S. and Canada have found that impacts on the incidence of targeted illnesses often exceed changes in the fraction of the population who receive vaccines due to spillovers and herd immunity effects (Ward, 2014; Luca et al., 2017; Carpenter and Lawler, 2017).[11] This, along with estimates of an improvement in health status among survivors, also suggests that the full health benefits of facility births are not necessarily immediately observed, but rather show up throughout childhood.

[10] http://www.who.int/healthinfo/global_burden_disease/estimates/en/index2.html

[11] The non-poor likely did not benefit from such spillovers given the high level of geographic segregation in Ghana by household wealth - 90 percent of the poor live in rural areas whil 73 percent of the non-poor live in urban areas.

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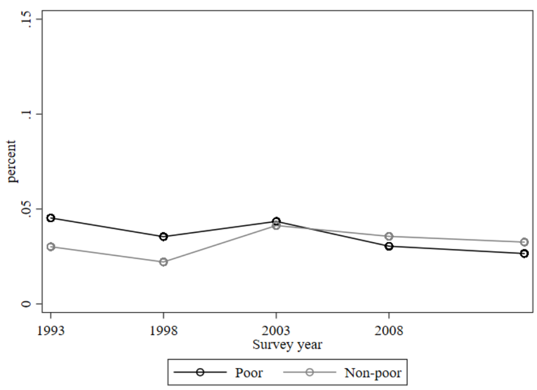
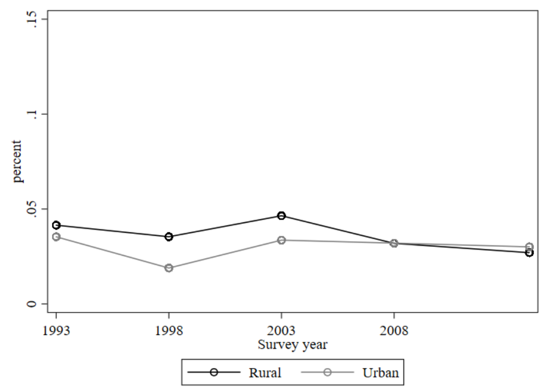
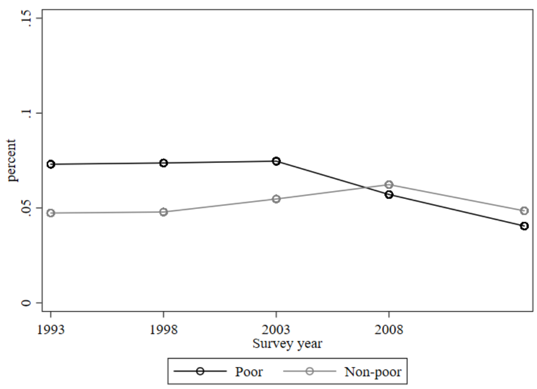
6 Figures and Tables



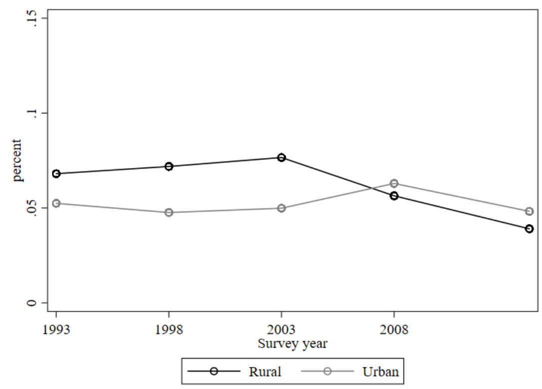
(a) 1 month mortality



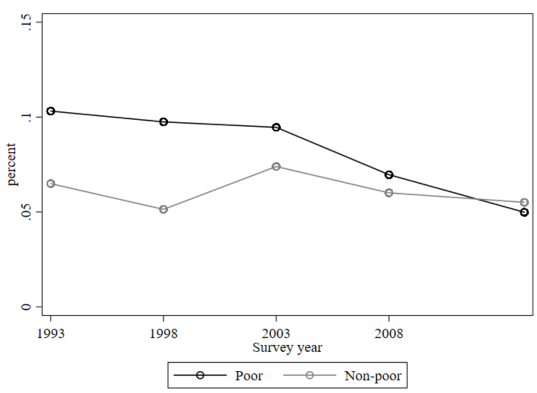
(b) 1 month mortality



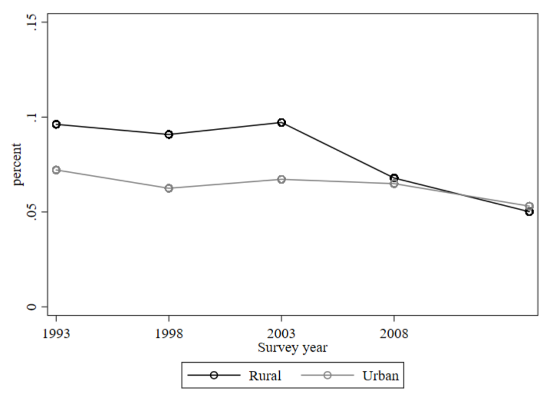
(c) 12 month mortality



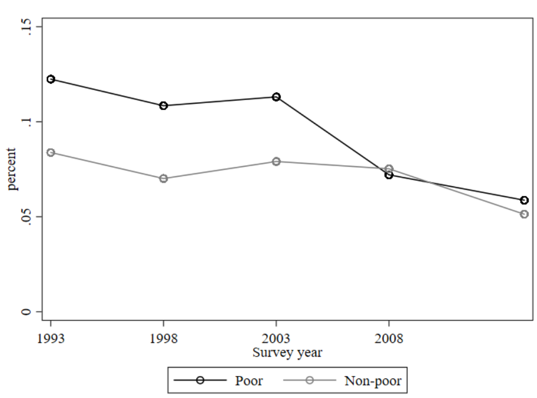
(d) 12 month mortality



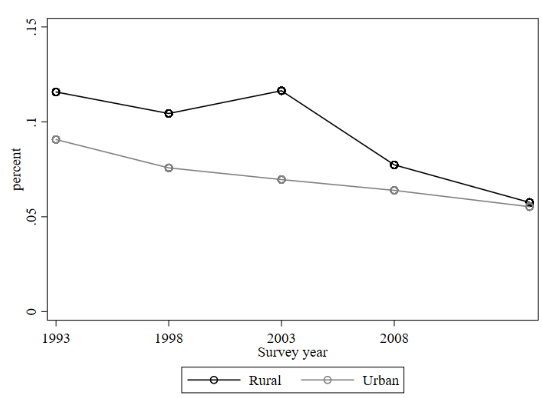
(e) 24 month mortality



(f) 24 month mortality



(g) 36 month mortality



(h) 36 month mortality

Figure 1 – Effect of free delivery policy on child mortality (by survey year). Data are from the 1993, 1998, 2003, 2008, and 2014 GDHS and include all births recorded in the 5 years preceding each survey.

Table 1 – Difference in Differences: Effect of free delivery policy on child mortality (per 1000 births)

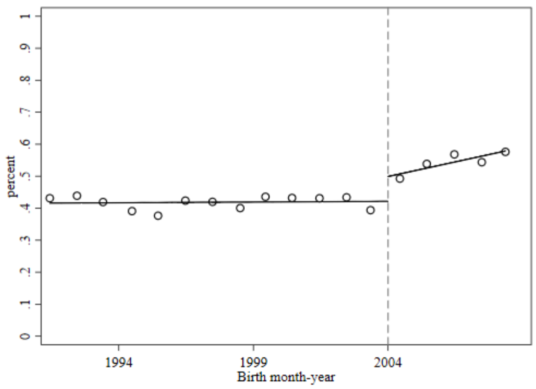
	7-day mortality (1)	1-month mortality (2)	12-month mortality (3)	36-month mortality (4)
$SY_{1998} \times$ poor	0.001	-0.002	0.000	-0.001
	(0.009)	(0.009)	(0.012)	(0.021)
$SY_{2003} \times$ poor	-0.010	-0.011	-0.006	-0.006
	(0.009)	(0.010)	(0.013)	(0.023)
$SY_{2008} \times$ poor	-0.002	-0.008	-0.025 *	-0.036 *
	(0.010)	(0.010)	(0.015)	(0.021)
$SY_{2014} \times$ poor	-0.003	0.001	-0.001	0.015
	(0.008)	(0.009)	(0.013)	(0.019)
SY_{1998}	-0.005	-0.006	0.005	-0.009
	(0.007)	(0.007)	(0.010)	(0.016)
SY_{2003}	0.018 **	0.019 **	0.006	0.011
	(0.008)	(0.008)	(0.011)	(0.018)
SY_{2008}	-0.009	-0.005	0.006	-0.005
	(0.009)	(0.010)	(0.012)	(0.018)
SY_{2014}	0.001	-0.003	-0.013	-0.025 *
	(0.007)	(0.008)	(0.011)	(0.015)
Poor	0.004	0.010 *	0.016 **	0.026 **
	(0.005)	(0.005)	(0.008)	(0.013)
Constant	0.020 ***	0.025 ***	0.030 ***	0.048 ***
	(0.005)	(0.006)	(0.008)	(0.013)
Observations	18777	18777	14893	7461

Note: Data are from the 1993, 1998, 2003, 2008, and 2014 GDHS and include all births recorded in the 5 years preceding each survey. Standard errors, clustered at the birth-month-year level, are in parentheses.

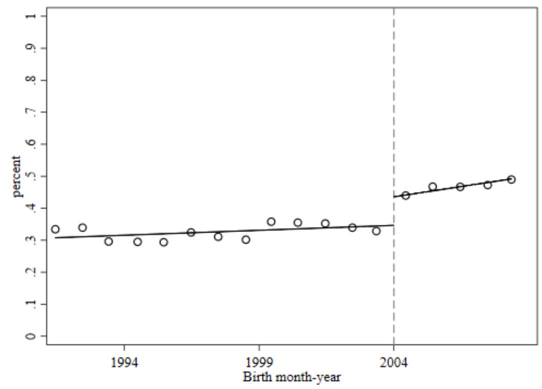
Table 2 – Difference in Differences: Effect of free delivery policy on child mortality (per 1000 births)

	7-day mortality (1)	1-month mortality (2)	12-month mortality (3)	36-month mortality (4)
SY_{1998} × rural	0.010	0.012	0.011	0.007
	(0.010)	(0.010)	(0.013)	(0.023)
SY_{2003} × rural	-0.001	-0.004	0.001	0.016
	(0.009)	(0.009)	(0.014)	(0.023)
SY_{2008} × rural	-0.011	-0.013	-0.032 **	-0.033
	(0.008)	(0.009)	(0.014)	(0.021)
SY_{2014} × rural	0.001	-0.001	0.000	-0.005
	(0.007)	(0.008)	(0.012)	(0.018)
SY_{1998}	-0.012	-0.015 *	-0.002	-0.013
	(0.008)	(0.009)	(0.012)	(0.019)
SY_{2003}	0.012	0.014 *	0.002	-0.005
	(0.008)	(0.008)	(0.012)	(0.020)
SY_{2008}	-0.003	-0.002	0.011	-0.007
	(0.007)	(0.007)	(0.012)	(0.018)
SY_{2014}	-0.002	-0.003	-0.015	-0.012
	(0.006)	(0.006)	(0.010)	(0.014)
Rural	-0.004	0.000	0.005	0.010
	(0.006)	(0.006)	(0.009)	(0.013)
Constant	0.026 ***	0.032 ***	0.036 ***	0.058 ***
	(0.006)	(0.007)	(0.009)	(0.014)
Observations	18777	18777	14893	7461

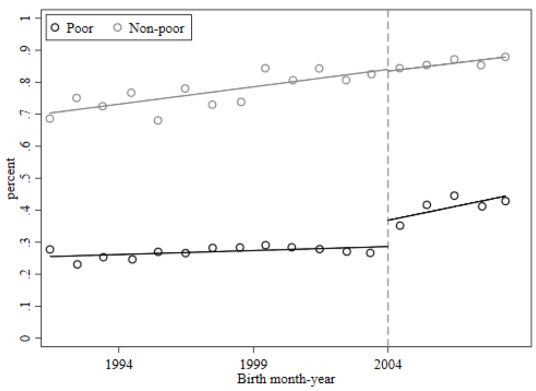
Note: Data are from the 1993, 1998, 2003, 2008, and 2014 GDHS and include all births recorded in the 5 years preceding each survey. Standard errors, clustered at the birth-month-year level, are in parentheses.



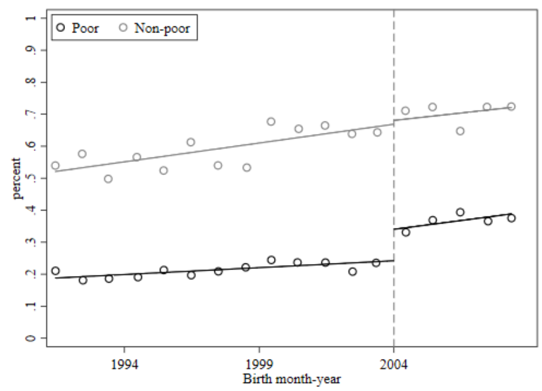
(a) Deliver at facility



(b) Deliver at government facility

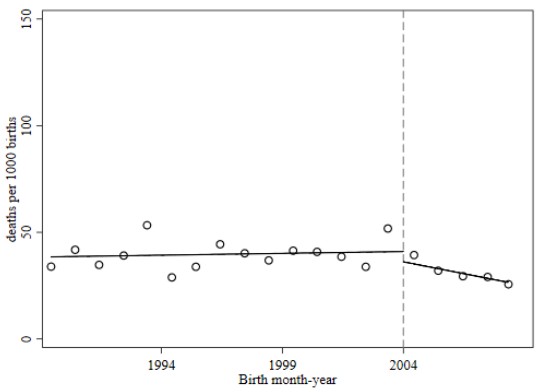


(c) Deliver at facility

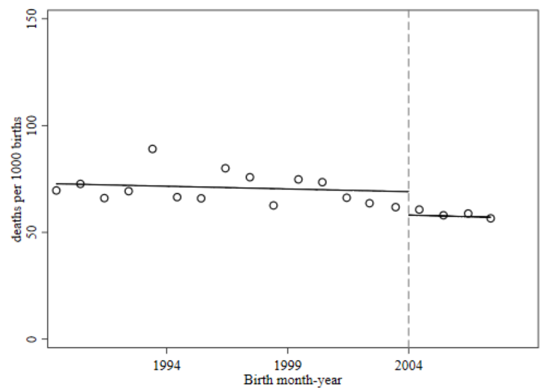


(d) Deliver at government facility

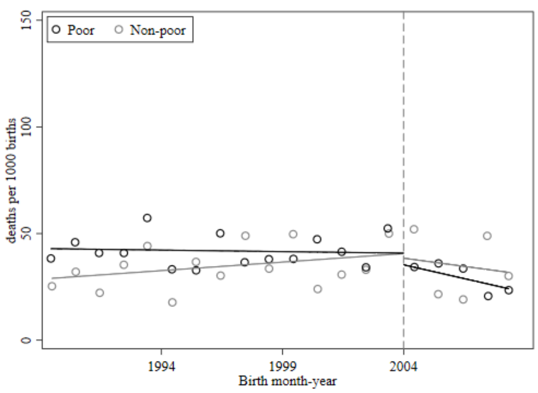
Figure 2 – Effect of free delivery policy on location of birth (by birth month). Data are from the 1993, 1998, 2003, and 2008 GDHS and include all births recorded in the 5 years preceding each survey.



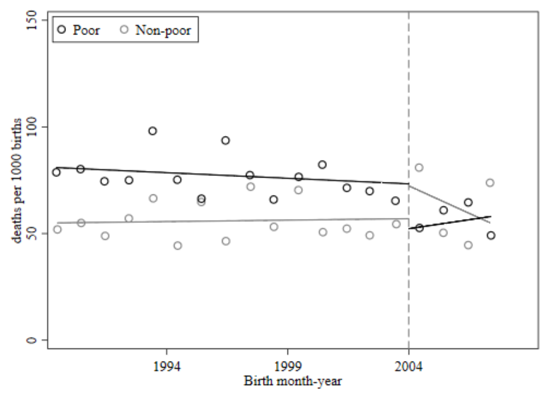
(a) 1-month mortality



(b) 12-month mortality

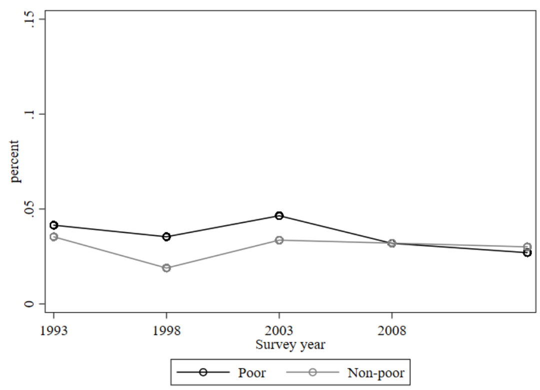


(c) 1-month mortality

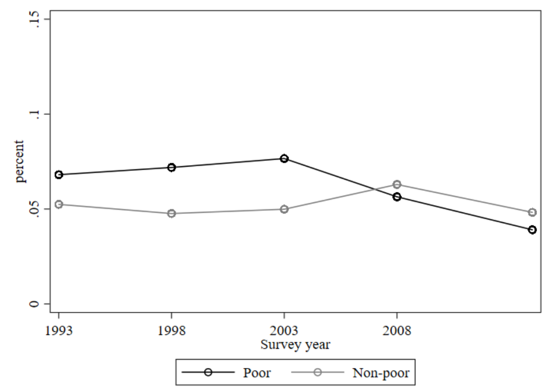


(d) 12-month mortality

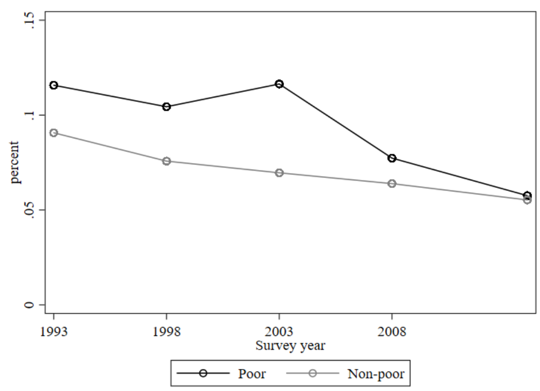
Figure 3 – Effect of free delivery policy on child mortality (by birth month). Data are from the 1993, 1998, 2003, and 2008 GDHS and include all births recorded between 1989 and 2008.



(a) 1-month mortality



(b) 12-month mortality



(c) 36-month mortality

Figure 4 – Effect of free delivery policy on child mortality (by survey year). Data are from the 1993, 1998, 2003, and 2008 GDHS and include all births recorded in the 5 years preceding each survey.

Table 3 – Child Characteristics

	All	Poor	Non-Poor
Facility birth	0.44	0.30	0.78
	(0.50)	(0.46)	(0.41)
Government facility birth	0.36	0.25	0.61
	(0.48)	(0.43)	(0.49)
Private facility birth	0.09	0.05	0.17
	(0.28)	(0.22)	(0.38)
Doctor, nurse, or midwife present	0.45	0.31	0.79
	(0.50)	(0.46)	(0.40)
Any prenatal care	0.91	0.87	0.98
	(0.29)	(0.33)	(0.15)
Has a health card	0.90	0.87	0.96
	(0.30)	(0.33)	(0.20)
BCG vaccine	0.84	0.81	0.92
	(0.36)	(0.39)	(0.27)
Complete DPT vaccine	0.65	0.61	0.75
	(0.48)	(0.49)	(0.43)
Complete polio vaccine	0.63	0.60	0.72
	(0.48)	(0.49)	(0.45)
Measles vaccine	0.64	0.61	0.70
	(0.48)	(0.49)	(0.46)
Height for age: z-score	-1.15	-1.31	-0.76
	(1.50)	(1.51)	(1.40)
Weight for age: z-score	-1.12	-1.25	-0.82
	(1.27)	(1.26)	(1.22)
Number of observations	12224	8678	3546
Child died, first 7 days	35.26	36.00	33.59
	(184.44)	(186.29)	(180.17)
Child died, first month	45.80	48.02	40.76
	(209.05)	(213.81)	(197.74)
Child died, first year	85.62	92.61	69.85
	(279.80)	(289.89)	(254.90)
Child died, first 3 years	126.08	138.06	98.97
	(331.94)	(344.97)	(298.64)
Number of observations	53343	37028	16315

Note: Standard deviations in parentheses. Computed based on one observation per child. Mortality information is collected for all children born to surveyed women. Other variables are only asked about children born in the last 5 years.

Table 4 – Regression Discontinuity: Effect of free delivery policy on delivery outcomes

	Facility birth (1)	Doc./Nurse present (2)	Govt fac. birth (3)	Priv fac. birth (4)
Panel A.				
Born after reform	0.081 ***	0.063 ***	0.092 ***	-0.011
	(0.020)	(0.020)	(0.020)	(0.015)
Birth month-year	0.000	0.000	0.000 *	0.000 ***
	(0.000)	(0.000)	(0.000)	(0.000)
Birth month-year × born after reform	0.002 ***	0.002 ***	0.001 *	0.001 *
	(0.000)	(0.001)	(0.000)	(0.000)
Constant	0.420 ***	0.430 ***	0.345 ***	0.075 ***
	(0.013)	(0.013)	(0.013)	(0.007)
Panel B.				
Born after reform	-0.008	-0.009	0.010	-0.018
	(0.026)	(0.029)	(0.043)	(0.041)
Born after reform × Poor	0.094 ***	0.072 *	0.091 *	0.002
	(0.036)	(0.040)	(0.048)	(0.042)
Birth month-year	0.001 ***	0.001 ***	0.001 ***	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Birth month-year × Poor	-0.001 ***	-0.001 ***	-0.001 ***	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Birth month-year × born after reform	0.000	0.000	0.000	0.000
	(0.001)	(0.001)	(0.001)	(0.001)
Birth month-year × born after × Poor	0.001	0.001	0.001	0.001
	(0.001)	(0.001)	(0.001)	(0.001)
Poor	-0.559 ***	-0.547 ***	-0.432 ***	-0.128 ***
	(0.018)	(0.019)	(0.024)	(0.018)
Constant	0.843 ***	0.843 ***	0.671 ***	0.172 ***
	(0.016)	(0.016)	(0.023)	(0.017)
Observations	8187	8226	8187	8187
Discontinuity for poor	0.086	0.063	0.102	-0.016
s.e.	(0.022)	(0.023)	(0.020)	(0.010)
p-value	0.000	0.006	0.000	0.122

Note: Data are from the 1993, 1998, 2003, and 2008 GDHS and include all births recorded in the 5 years preceding each survey. Standard errors, clustered at the birth-month-year level, are in parentheses.

Table 5 – Regression Discontinuity: Effect of free delivery policy on child mortality (per 1000 births)

	7-day mortality (1)	1-month mortality (2)	12-month mortality (3)
Panel A.			
Born after reform	-6.348	-5.032	-10.981
	(6.607)	(6.203)	(8.659)
Birth month-year	0.024	0.014	-0.021
	(0.026)	(0.028)	(0.033)
Birth month-year × born after reform	-0.162	-0.200	-0.004
	(0.190)	(0.184)	(0.334)
Constant	34.376 ***	40.980 ***	69.013 ***
	(2.986)	(3.260)	(3.876)
Panel B.			
Born after reform	-10.498	-2.357	14.921
	(11.849)	(12.932)	(16.962)
Born after reform × Poor	6.103	-3.339	-35.783 *
	(13.471)	(15.642)	(20.066)
Birth month-year	0.074 *	0.067	0.011
	(0.042)	(0.045)	(0.055)
Birth month-year × Poor	-0.072	-0.078	-0.055
	(0.047)	(0.053)	(0.065)
Birth month-year × born after reform	-0.072	-0.194	-0.441
	(0.349)	(0.378)	(0.711)
Birth month-year × born after × Poor	-0.130	-0.009	0.624
	(0.367)	(0.423)	(0.804)
Poor	-3.025	0.138	16.273 **
	(5.745)	(6.446)	(8.197)
Constant	36.405 ***	40.657 ***	56.975 ***
	(5.052)	(5.459)	(7.004)
Observations	20048	20048	18113
Discontinuity for poor	-4.395	-5.696	-20.861
s.e.	(7.530)	(7.527)	(10.173)
p-value	0.560	0.450	0.041

Note: Data are from the 1993, 1998, 2003, and 2008 GDHS and include all births recorded in between 1989 and 2008. Standard errors, clustered at the birth-month-year level, are in parentheses.

Table 6 – Difference in Differences: Effect of free delivery policy on delivery outcomes

	Facility birth (1)	Doc./Nurse present (2)	Govt fac. birth (3)	Priv fac. birth (4)
SY_{1998} × poor	-0.013	-0.009	-0.018	0.005
	(0.031)	(0.032)	(0.030)	(0.021)
SY_{2003} × poor	-0.074 ***	-0.064 ***	-0.059 **	-0.014
	(0.024)	(0.024)	(0.027)	(0.020)
SY_{2008} × poor	0.095 ***	0.065 ***	0.074 ***	0.021
	(0.020)	(0.020)	(0.025)	(0.020)
SY_{1998}	0.022	0.011	0.021	0.000
	(0.023)	(0.024)	(0.025)	(0.020)
SY_{2003}	0.080 ***	0.079 ***	0.083 ***	-0.003
	(0.021)	(0.021)	(0.025)	(0.018)
SY_{2008}	0.045 ***	0.049 ***	0.069 ***	-0.024
	(0.017)	(0.017)	(0.023)	(0.020)
Poor	-0.457 ***	-0.462 ***	-0.336 ***	-0.120 ***
	(0.024)	(0.025)	(0.024)	(0.016)
Constant	0.718 ***	0.734 ***	0.533 ***	0.185 ***
	(0.017)	(0.018)	(0.019)	(0.017)
Observations	11674	11725	11674	11674

Note: Data are from the 1993, 1998, 2003, and 2008 GDHS and include all births recorded in the 5 years preceding each survey. Standard errors, clustered at the birth-month-year level, are in parentheses.

Table 7 – Difference in Differences: Effect of free delivery policy on child mortality (per 1000 births)

	7-day mortality (1)	1-month mortality (2)	12-month mortality (3)	36-month mortality (4)
SY_{1998} × poor	0.877	-1.840	0.299	-0.438
	(8.732)	(8.585)	(11.857)	(21.571)
SY_{2003} × poor	-10.523	-11.171	-5.864	-4.288
	(9.436)	(9.798)	(13.313)	(23.297)
SY_{2008} × poor	-1.084	-7.324	-24.840 *	-37.150 *
	(9.465)	(10.393)	(14.800)	(21.615)
SY_{1998}	-6.852	-8.031	1.157	-13.896
	(6.482)	(6.824)	(9.873)	(15.392)
SY_{2003}	18.645 ***	19.200 ***	6.851	8.799
	(7.707)	(8.087)	(10.656)	(17.756)
SY_{2008}	-9.900	-5.727	7.232	-3.627
	(8.807)	(9.612)	(12.113)	(18.414)
Poor	8.329	15.170 ***	25.188 ***	38.641 ***
	(5.582)	(5.366)	(7.806)	(12.755)
Constant	23.812 ***	30.667 ***	32.437 ***	95.098 ***
	(5.634)	(6.156)	(8.869)	(35.745)
Observations	13148	13148	10431	5290

Note: Data are from the 1993, 1998, 2003, and 2008 GDHS and include all births recorded in the 5 years preceding each survey. Standard errors, clustered at the birth-month-year level, are in parentheses.

Table 8 – Difference in Differences: Effect of free delivery policy on child health

	Height for age (1)	Stunted (2)	Weight for age (3)	Wasted (4)
SY_{2003} × poor	-0.014	-0.014	0.067	-0.001
	(0.099)	(0.030)	(0.078)	(0.027)
SY_{2008} × poor	0.154 *	-0.049 **	-0.062	-0.039 *
	(0.081)	(0.024)	(0.077)	(0.022)
SY_{2003}	-0.105	0.041	0.057	-0.025
	(0.107)	(0.025)	(0.094)	(0.024)
SY_{2008}	0.280 ***	-0.044 **	0.223 ***	-0.020
	(0.104)	(0.022)	(0.089)	(0.019)
Poor	-0.640 ***	0.185 ***	-0.482 ***	0.128 ***
	(0.079)	(0.024)	(0.060)	(0.021)
Constant	-0.023	0.008	-0.577 ***	0.152 ***
	(0.106)	(0.022)	(0.117)	(0.028)
Observations	7734	7734	7734	7734

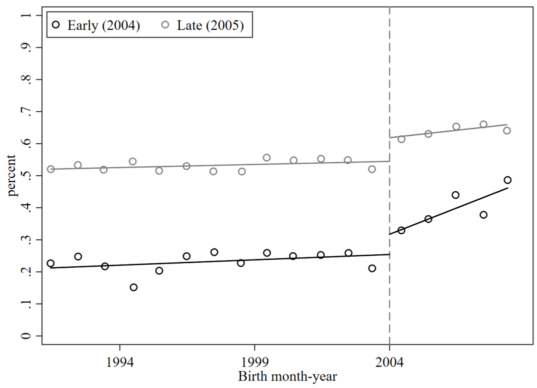
Note: Data are from the 1998, 2003, and 2008 GDHS and include all births recorded in the 5 years preceding each survey. Standard errors, clustered at the birth-month-year level, are in parentheses.

Table 9 – Difference in Differences: Effect of free delivery policy on vaccines

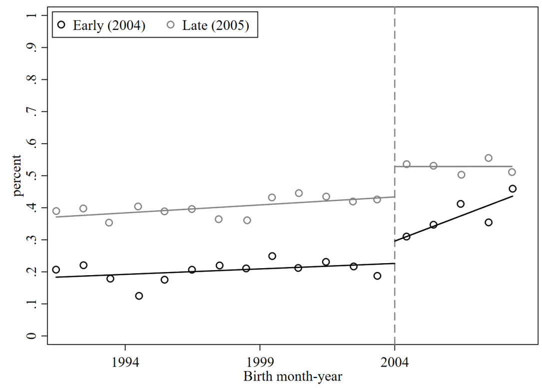
	Health card (1)	BCG (2)	DPT (3)	Polio (4)	Measles (5)
$SY_{2003} \times$ poor	0.010	0.001	-0.009	0.020	0.009
	(0.016)	(0.019)	(0.024)	(0.025)	(0.023)
$SY_{2008} \times$ poor	0.043 ***	0.037 *	0.078 ***	0.034	0.067 ***
	(0.014)	(0.019)	(0.026)	(0.028)	(0.025)
SY_{2003}	0.012	-0.005	0.049	0.029	0.049
	(0.011)	(0.013)	(0.034)	(0.035)	(0.043)
SY_{2008}	0.015	0.034 ***	0.013	0.019	-0.007
	(0.010)	(0.013)	(0.039)	(0.041)	(0.051)
Poor	-0.088 ***	-0.109 ***	-0.151 ***	-0.124 ***	-0.121 ***
	(0.011)	(0.011)	(0.017)	(0.017)	(0.018)
Constant	0.908 ***	0.889 ***	0.595 ***	0.581 ***	0.374 ***
	(0.017)	(0.016)	(0.047)	(0.047)	(0.049)
Observations	8918	8932	8839	8908	8922

Note: Data are from the 1998, 2003, and 2008 GDHS and include all births recorded in the 5 years preceding each survey. Standard errors, clustered at the birth-month-year level, are in parentheses.

7 Online Appendix

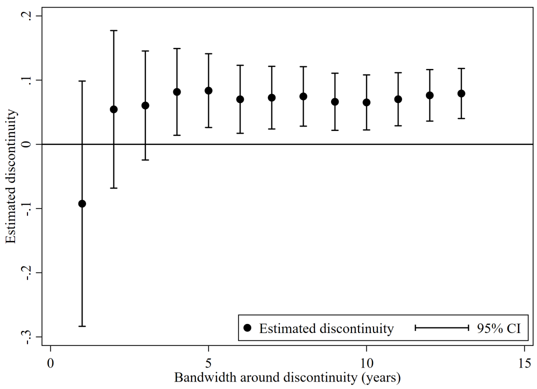


(a) Deliver at facility

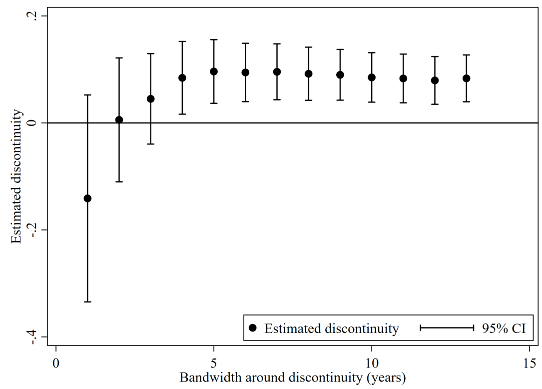


(b) Deliver at government facility

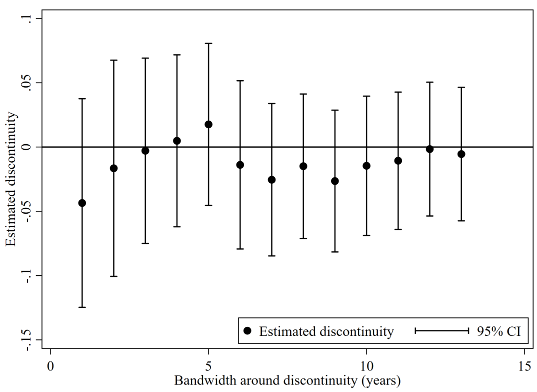
Figure A1 – Effect of free delivery policy on location of birth (by birth month) estimated separately for regions that adopted early (January 2004) and late (April 2005). Data are from the 1993, 1998, 2003, and 2008 GDHS and include all births recorded in the 5 years preceding each survey.



(a) Full sample

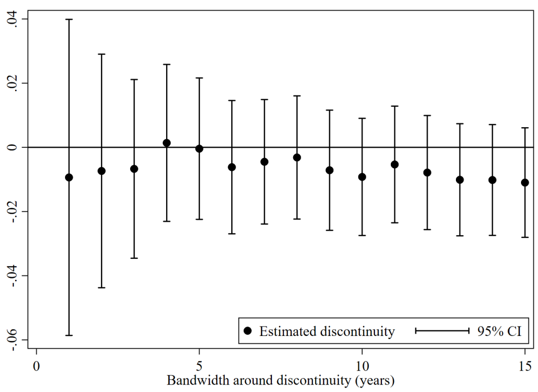


(b) Poor

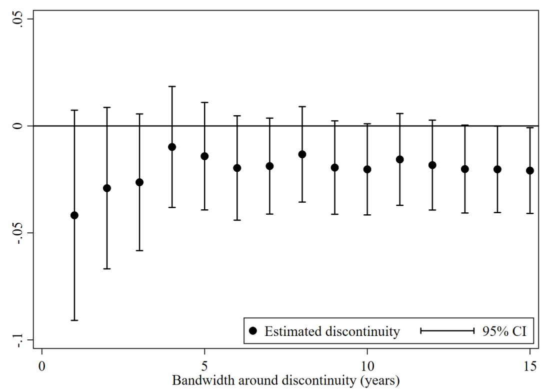


(c) Non-poor

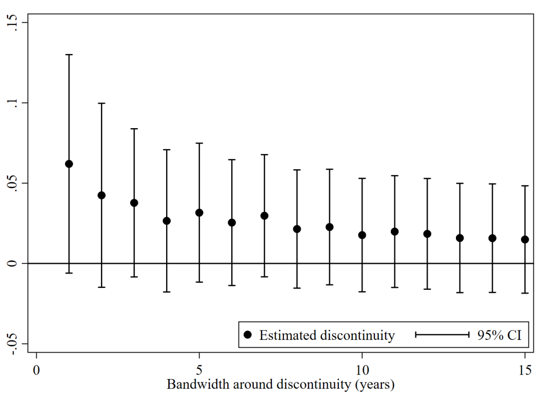
Figure A2 – Discontinuity in facility births as a function of bandwidth. Data are from the 1993, 1998, 2003, and 2008 GDHS and include all births recorded in the 5 years preceding each survey.



(a) Full sample

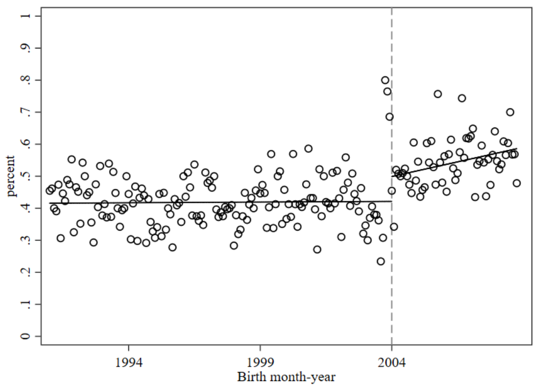


(b) Poor

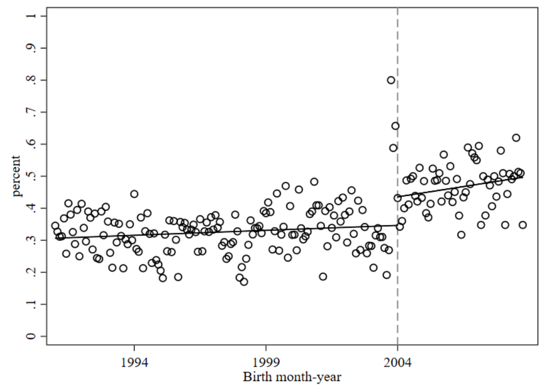


(c) Non-poor

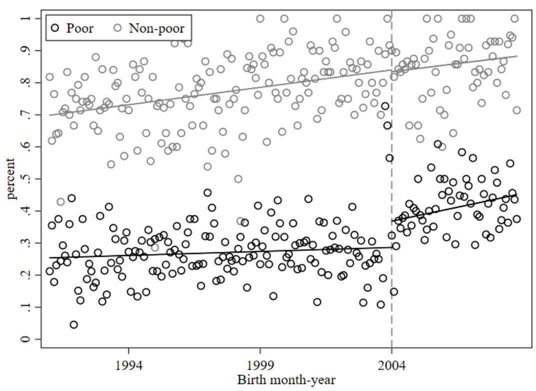
Figure A3 – Discontinuity in 12-month mortality as a function of bandwidth. Data are from the 1993, 1998, 2003, and 2008 GDHS and include all births recorded in the 5 years preceding each survey.



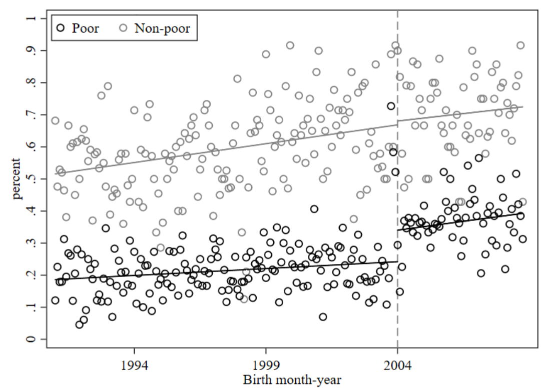
(a) Deliver at facility



(b) Deliver at government facility

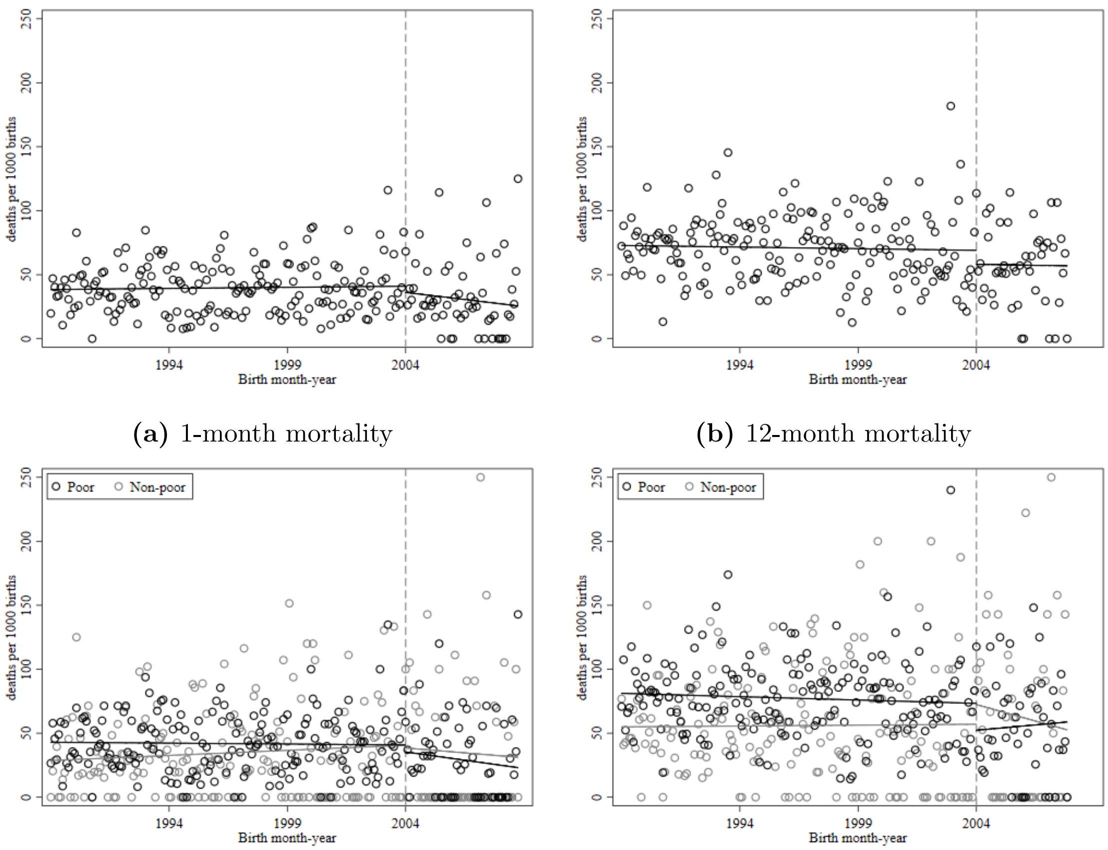


(c) Deliver at facility



(d) Deliver at government facility

Figure A4 – Effect of free delivery policy on location of birth (by birth month). Data are from the 1993, 1998, 2003, and 2008 GDHS and include all births recorded in the 5 years preceding each survey.



(a) 1-month mortality

(b) 12-month mortality

(c) 1-month mortality

(d) 12-month mortality

Figure A5 – Effect of free delivery policy on child mortality (by birth month). Data are from the 1993, 1998, 2003, and 2008 GDHS and include all births recorded between 1989 and 2008.

Table A1 – Difference in Differences: Mother characteristics

	Age (1)	Education (2)	Married (3)	Wealth index (4)	Births last 5 yrs (5)	Total kids born (6)	Total kids died (7)
$SY_{1998} \times$ poor	0.760	-0.409	-0.001	-273.522 ***	-0.082 **	0.227 *	0.011
	(0.467)	(0.290)	(0.022)	(37.484)	(0.038)	(0.136)	(0.050)
$SY_{2003} \times$ poor	-0.182	0.176	0.010	37.018	-0.018	0.146	-0.033
	(0.414)	(0.257)	(0.020)	(33.228)	(0.033)	(0.120)	(0.044)
$SY_{2008} \times$ poor	-0.885 **	0.013	-0.027	-91.911 ***	0.031	-0.018	-0.001
	(0.421)	(0.262)	(0.020)	(33.743)	(0.034)	(0.122)	(0.045)
SY_{1998}	3.422 ***	0.044	0.014	123.464 ***	-0.119 ***	0.747 ***	0.104 ***
	(0.374)	(0.233)	(0.018)	(30.047)	(0.030)	(0.109)	(0.040)
SY_{2003}	0.655 *	-0.397 *	0.009	24.238	0.014	-0.023	0.004
	(0.347)	(0.216)	(0.017)	(27.879)	(0.028)	(0.101)	(0.037)
SY_{2008}	0.513	0.689 ***	0.001	22.941	-0.014	-0.133	-0.082 **
	(0.350)	(0.218)	(0.017)	(28.113)	(0.028)	(0.102)	(0.037)
Poor	0.122	-3.928 ***	0.010	-1384.713 ***	0.196 ***	0.526 ***	0.214 ***
	(0.355)	(0.221)	(0.017)	(28.473)	(0.029)	(0.103)	(0.038)
Constant	22.823 ***	7.386 ***	0.903 ***	775.450 ***	1.676 ***	1.813 ***	0.101 ***
	(0.300)	(0.187)	(0.014)	(24.101)	(0.024)	(0.087)	(0.032)
Observations	8461	8455	8461	8461	8461	8461	8461

Note: Data are from the 1993, 1998, 2003, and 2008 GDHS and include one observation for each woman who gave birth in the 5 years preceding each survey. Standard errors are in parentheses.

Table A2 – Difference in Differences: Effect of free delivery policy on delivery outcomes – excluding the middle wealth quintile

	Facility birth (1)	Doc./Nurse present (2)	Govt fac. birth (3)	Priv fac. birth (4)
$SY_{1998} \times$ poor	-0.058 *	-0.060 *	-0.037	-0.021
	(0.032)	(0.032)	(0.031)	(0.021)
$SY_{2003} \times$ poor	-0.048 **	-0.040 *	-0.047 *	-0.001
	(0.024)	(0.023)	(0.027)	(0.019)
$SY_{2008} \times$ poor	0.068 ***	0.036 *	0.050 *	0.018
	(0.020)	(0.020)	(0.026)	(0.019)
SY_{1998}	0.019	0.009	0.019	0.000
	(0.024)	(0.024)	(0.025)	(0.020)
SY_{2003}	0.080 ***	0.079 ***	0.083 ***	-0.003
	(0.021)	(0.021)	(0.025)	(0.018)
SY_{2008}	0.045 ***	0.050 ***	0.070 ***	-0.024
	(0.017)	(0.017)	(0.023)	(0.020)
Poor	-0.481 ***	-0.484 ***	-0.364 ***	-0.117 ***
	(0.025)	(0.026)	(0.025)	(0.016)
Constant	0.714 ***	0.731 ***	0.530 ***	0.185 ***
	(0.017)	(0.018)	(0.019)	(0.017)
Observations	9565	9608	9565	9565

Note: Data are from the 1993, 1998, 2003, and 2008 GDHS and include all births recorded in the 5 years preceding each survey. Standard errors, clustered at the birth-month-year level, are in parentheses.

Table A3 – Difference in Differences: Effect of free delivery policy on child mortality (per 1000 births) – excluding the middle wealth quintile

	7-day mortality (1)	1-month mortality (2)	12-month mortality (3)	36-month mortality (4)
SY_{1998} × poor	-2.037	-5.093	-3.006	-10.402
	(9.283)	(9.069)	(12.320)	(21.921)
SY_{2003} × poor	-13.676	-14.435	-6.711	3.485
	(9.548)	(9.607)	(13.987)	(24.841)
SY_{2008} × poor	-0.807	-5.940	-26.933 *	-39.223 *
	(9.462)	(10.410)	(15.699)	(23.816)
SY_{1998}	-6.827	-8.003	1.373	-13.332
	(6.506)	(6.849)	(9.939)	(15.659)
SY_{2003}	18.634 ***	19.188 ***	6.848	9.259
	(7.715)	(8.089)	(10.672)	(17.735)
SY_{2008}	-9.823	-5.643	7.116	-4.254
	(8.806)	(9.595)	(12.083)	(18.178)
Poor	10.090 *	17.360 ***	28.504 ***	48.921 ***
	(5.929)	(5.964)	(8.322)	(12.032)
Constant	22.141 ***	28.836 ***	27.183 ***	62.878
	(5.749)	(6.340)	(9.468)	(39.397)
Observations	10716	10716	8497	4345

Note: Data are from the 1993, 1998, 2003, and 2008 GDHS and include all births recorded in the 5 years preceding each survey. Standard errors, clustered at the birth-month-year level, are in parentheses.

Table A4 – Difference in Differences: Effect of free delivery policy on child health – excluding the middle wealth quintile

	Height for age (1)	Stunted (2)	Weight for age (3)	Wasted (4)
$SY_{2003} \times$ poor	-0.012	-0.016	0.082	-0.004
	(0.104)	(0.032)	(0.084)	(0.029)
$SY_{2008} \times$ poor	0.160 *	-0.054 **	-0.067	-0.038
	(0.089)	(0.027)	(0.082)	(0.024)
SY_{2003}	-0.105	0.041 *	0.057	-0.025
	(0.107)	(0.025)	(0.093)	(0.024)
SY_{2008}	0.280 ***	-0.044 **	0.223 ***	-0.020
	(0.104)	(0.022)	(0.089)	(0.019)
Poor	-0.682 ***	0.205 ***	-0.536 ***	0.146 ***
	(0.082)	(0.025)	(0.065)	(0.024)
Constant	-0.027	0.009	-0.590 ***	0.154 ***
	(0.108)	(0.022)	(0.119)	(0.029)
Observations	6383	6383	6383	6383

Note: Data are from the 1998, 2003, and 2008 GDHS and include all births recorded in the 5 years preceding each survey. Standard errors, clustered at the birth-month-year level, are in parentheses.

Table A5 – Difference in Differences: Effect of free delivery policy on vaccines – excluding the middle wealth quintile

	Health card (1)	BCG (2)	DPT (3)	Polio (4)	Measles (5)
SY_{2003} × poor	0.013	0.005	-0.009	0.029	0.015
	(0.019)	(0.023)	(0.025)	(0.026)	(0.026)
SY_{2008} × poor	0.054 ***	0.038 *	0.088 ***	0.042	0.071 ***
	(0.017)	(0.023)	(0.027)	(0.030)	(0.027)
SY_{2003}	0.012	-0.005	0.049	0.029	0.049
	(0.011)	(0.013)	(0.034)	(0.035)	(0.043)
SY_{2008}	0.015	0.034 ***	0.012	0.019	-0.007
	(0.010)	(0.014)	(0.039)	(0.041)	(0.051)
Poor	-0.106 ***	-0.129 ***	-0.174 ***	-0.152 ***	-0.144 ***
	(0.012)	(0.013)	(0.018)	(0.017)	(0.020)
Constant	0.908 ***	0.888 ***	0.598 ***	0.586 ***	0.374 ***
	(0.018)	(0.018)	(0.047)	(0.047)	(0.049)
Observations	7367	7380	7299	7363	7371

Note: Data are from the 1998, 2003, and 2008 GDHS and include all births recorded in the 5 years preceding each survey. Standard errors, clustered at the birth-month-year level, are in parentheses.