Childhood Immunization, Mortality and Human Capital Accumulation: Micro-Evidence from India

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Abstract

In the mid-1980s, the Indian government embarked on one of the largest childhood immunization programs-called "Universal Immunization Program" (UIP)-in order to reduce the high mortality and morbidity among children. I examine the effect of this immunization program on child mortality and educational attainment by exploiting district-by-cohort variation in exposure to the program. Results indicate that exposure to the program reduced infant mortality by 0.4 percentage points and under-five child mortality by 0.5 percentage points. These effects on mortality are sizable–they account for approximately one-fifth of the decline in infant and under-five child mortality rates between 1985-1990. The effects are more pronounced in rural areas, for poor people, and for members of historically disadvantaged groups. While the program clearly reduced mortality, it had mixed effects on children's educational outcomes. I find it had a negative impact on primary school completion, but a positive impact on secondary school completion. The negative effect at low levels of schooling may be due to lower average health among marginal surviving children or a quantity-quality trade-off where the unanticipated survival of children induces families to under-invest in each child. The greater propensity to complete secondary school on the other hand may be due to improved health among those farther away from the margin of survival.

JEL classification: I1, I2, J13, J18, O15.
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1 Introduction

Scientific evidence has shown that vaccines are effective in reducing childhood mortality and morbidity (Levine et al., 2005; United Nations, 2006). Therefore, childhood immunization has become a critical component of public health policies in many countries. Recent evidence suggests, however, that in many countries child mortality is either increasing or stagnating despite these countries having an active childhood immunization program in place (Adjuik et al., 2006; Aaby and Jenson, 2005). In addition, the level of child mortality still remains unacceptably high in many parts of the world. For example, the under-five mortality in 2003 was 9.2 percent in South Asia, 17.5 percent in Sub-Saharan Africa, and only 0.06 percent in industrialized countries. (Glewwe and Miguel, 2008). In light of these numbers, the foremost question to ask is: are childhood immunization programs effective in terms of reducing mortality, especially in poorer developing countries? Answering this is of tremendous policy relevance considering the large number of countries that have childhood immunization programs, or are considering adopting one.

In addition, we may care about the general question of long-run effects of child health on educational outcomes. Although school enrollment and access to schools have grown considerably in developing countries, 113 million primary-school-aged children were not enrolled in 2001, most children never reach secondary school, and academic achievement as measured by standardized tests is dismally low (Glewwe and Kremer, 2006). It is natural to ask the extent to which poor child health is responsible for these poor educational outcomes.

This paper takes a step toward addressing these questions by evaluating a large-scale government-sponsored childhood immunization program in India called "Universal Immunization Program" (hereafter, UIP). In 1985-86 the Government of India launched UIP in 31 districts. Each year additional districts were phased into the program and by 1990 all 443 districts of India were covered by UIP. The program delivered free immunization shots for children under one year of age to protect them from six Vaccine Preventable Diseases (hereafter, VPDs).[1]

UIP has features that facilitate evaluation. On the one hand, there is district variation in UIP exposure: UIP was implemented gradually across districts in India, with the timing apparently determined by fixed district characteristics. On the other hand, there is cohort variation in UIP exposure: only children who were twelve months old or younger at the time the program began would have been eligible to receive the immunizations. This enables me to use a difference-in-differences-type estimation strategy to identify the effect of UIP. The identifying assumption is that without UIP, the cohort difference in mortality and educational outcomes would have been the same between the districts that implemented UIP sooner and the districts that implemented UIP later. I apply this identification strategy using the "Reproductive and Child Health Survey" (hereafter, RCH), a large nationally representative individual-level data-set.

The main finding of this paper is that UIP reduced infant mortality by 0.4 percentage points and under-five mortality by 0.5 percentage points. The effects on mortality outcomes are substantial given that the infant mortality in India was 9.7% and under-five mortality estimate was 15% before the launch of the program. Indeed they account for approximately one-fifth of the decline in infant and under-five child mortality rates between 1985-1990. There is no differential effect by gender but effects are more pronounced in rural areas, for poor people, and low caste people. I verify that these results are not due to differential trends in child mortality between early-UIP districts and later-UIP districts by doing control experiments using older cohorts and using a health outcome unrelated to the immunizations.

Next, I examine the effects of UIP on the educational outcomes of surviving children and find mixed results. The program had a negative impact on primary school completion, but a positive impact on secondary school completion. The results on education outcomes

[1] The six VPDs are Diphtheria, Pertussis, Tetanus, Poliomyelitis, Measles and Tuberculosis.

can be explained in terms of change in the composition of the surviving children due to the immunization program. The negative effect on education may be due to lower quality of the "marginal child" similar to the argument made by Donohue and Levitt (2001) and Gruber, Levine and Staiger (1999); UIP induced some children to survive who otherwise would have died, and these children may be less healthy. The negative results are also consistent with the quantity-quality trade-off where an unanticipated increase in household size due to the immunization program induces the households to under-invest in each child. On the other hand, the result that UIP increased the education of some children is similar to the findings of Miguel and Kremer (2004), Lucas (2005), Bobonis et al. (2006), Bleakley (2007) and Kremer et al. (2007). The greater propensity to complete secondary school may be due to improved health among those children who are not at the margin of survival.

This paper contributes to the existing literature in several ways. First, I am not aware of any previous studies that rigorously quantify the effects of immunization programs on children's outcomes. On the one hand, there have been process evaluations which describe the implementation of UIP and vaccination coverage. On the other hand, there have been medical evaluations which examine the effects of immunizations on health outcomes but these studies are conducted in laboratory-like settings rather than in actual developing-country contexts. There is widespread skepticism about the public health service delivery system in developing countries. Chaudhury et al. (2006) find, for instance, that 39% of doctors and 31% of other health care workers were absent from work in nationally representative surveys of primary health centers in Bangladesh, Ecuador, India, Indonesia, Peru and Uganda. In these environments, can a mass immunization program successfully reduce child mortality? What are the consequences for children's educational outcomes?

Second, this paper adds to the literature on the effects of child health on schooling. Most studies linking health and education are unable to distinguish causality from mere correlation, though there are a few recent exceptions (including Miguel and Kremer, 2004;

Lucas, 2005; Bobonis et al., 2006; Bleakley, 2007; Kremer et al., 2007; see Glewwe and Miguel, 2008 for a review). None of the studies estimating the causal effect of health on education have used variation in health provided by an immunization program. Immunization programs provide a new and different source of variation in child health. In particular, whereas health interventions used by other studies primarily reduce the morbidity of children, immunization programs reduce both the mortality and morbidity of children. Since immunization programs operate on different margins, the consequences for education could be different from those other health interventions.

The rest of the paper is structured as follows: in Section 2 I discuss the related literature and provide an overview of UIP. Section 3 presents the empirical framework and Section 4 describes the data. Section 5 presents the results on mortality outcomes and Section 6 presents the results on educational outcomes. Finally, Section 7 concludes.

2 Background

2.1 The Universal Immunization Program

Approximately 3 million children die each year of vaccine preventable diseases (VPDs) with a disproportionate number of these children residing in developing countries (Kane and Lasher, 2002). Vaccines remain one of the most cost-effective public health initiatives, yet the cover against VPDs remains far from complete; recent estimates suggest that approximately 34 million children are not completely immunized with almost 98 percent of them residing in developing countries (Frenkel and Nielsen, 2003). Reducing child mortality by two-thirds between 1990 and 2015 is the fourth of eight Millennium Development Goals endorsed by world leaders in the Millennium Declaration in 2000.

In India, immunization of children against VPDs has been a central goal of the health care system from the 1970s. The Expanded Program on Immunization (EPI) was initiated in 1978 to make six childhood vaccines (BCG, DPT, TT, DT, Polio and typhoid) available to all eligible children. The main objective of EPI was to reduce mortality and morbidity by controlling six target diseases- Tuberculosis, Diphtheria, Tetanus, Pertussis, Polio and Typhoid. EPI failed to achieve the objective of immunizing children; because the program was limited primarily to major hospitals in urban areas and coverage levels were very low. In 1985, the Government of India made childhood immunization a Technology Mission and launched Universal Immunization Program (UIP) with much dynamism to attain the goal of achieving 85 percent coverage for tuberculosis, diphtheria, tetanus, pertussis, polio and measles for all children by 1990.

Under UIP, each child had to be vaccinated before he or she turned one year of age with three doses of DPT vaccine, three doses of polio vaccine and one dose each of measles and BCG vaccine. Table 1 in the appendix lists some symptoms associated with the diseases that these shots protect against. The symptoms range from mild to severe, with serious sickness and death more likely among infants (whose immune systems are not yet mature) and poor children (whose immune systems are weakened due to malnutrition). It is worth noting that immunization protects individuals not only from illness per se, but also from the long-term effects of that illness on their physical, emotional, and cognitive development (Bloom et al., 2005). Additionally these diseases are communicable, so there are significant positive externalities from being vaccinated. That is, the vaccines reduces the risk of disease not only for the children vaccinated but also people around them by reducing the transmission rate of the diseases.

There were not sufficient resources to implement the program all over the country at the same time. Thus, UIP had a phased roll-out, beginning with 31 districts in 1985-86 and covering all districts by 1990. The program was implemented through the existing network of primary health care infrastructure which consists of a referral center called "community health center" for every 80 to 120 thousand people, a primary health center for 20 to 30 thousand people, and a sub-center for every 3 to 5 thousand people. The program made provision for additional inputs in the form of additional staff, vaccines, and equipment for storage and transportation of vaccines such as walk-in-coolers, refrigerators and vaccine carriers.

Below I take advantage of the staggered implementation of UIP across districts to help identify UIP's effect, therefore it is essential to understand what determined the timing. Toward this end, I had numerous conversations with officials in the UIP division of the Ministry of Health and Family Welfare. The timing was not completely random. It seems that the capacity of the district to achieve the immunization coverage rates targeted by UIP and to maintain this level in subsequent years was a major factor in the selection of the district. In addition, infrastructure and other health facilities to deliver the UIP services were also taken into account while selecting the districts. In other words, selection of districts was based on fixed characteristics of the districts. For example, early-adopting districts may have more primary health centers, more nurses, or have better health care infrastructure. Selection on fixed district characteristics does not cause problems for the interpretation of my estimated treatment effects because they rely on within-district variation in exposure to UIP only; that is, I always control for district fixed effects. A more serious problem would be if the timing of implementation depended on underlying district-specific trends in the outcome variables. It must be emphasized that UIP officials never indicated that district trends in mortality or education were part of the criteria for earlier implementation. However, to address this potential concern, I perform control experiments using older cohorts who are not exposed to UIP and using a health outcome unrelated to the immunizations; I discuss these control experiment in detail in section 5.

UIP is one of the largest in the world in terms of quantities of vaccines used, number of intended beneficiaries, number of immunization sessions organized, the geographical spread and the diversity of areas covered. Surprisingly, there have not been any studies estimating UIP's effect on mortality, much less education. Previous evaluations of UIP were mostly sanctioned by Ministry of Health and Family Welfare and international donor agencies like WHO, UNICEF and were basically process evaluations that look at the coverage of vaccines.[2] They show that UIP was able to substantially increase the coverage of immunization shots (*Figure 1 in Appendix*). Vaccine coverage by antigen shows substantial increase during the UIP period. The vaccine coverage increased from a low 30-40% at the start of the program to approximately 80-100% by 1990-91.

The extent to which UIP reduced child mortality and its effects on the schooling of surviving children remains an open questions. Answering these questions is of great interest for India. There is widespread debate about the proper implementation of public health programs in India. Many claim that the public health service delivery system in India is inefficient and that government-sponsored programs exist only on paper and real take-off of public health programs is either doubtful or slow. Moreover, it should have relevance for policymakers and public health activists outside of India. Many countries have mass immunization programs or are considering adopting them. Immunization programs compete for limited funding for public health initiatives and other welfare programs, with budget constraints especially tight in poor developing countries. Do immunization programs help the intended beneficiaries? I present my strategy for evaluating UIP after briefly reviewing the related literature.

2.2 Related Literature

I am not aware of any studies that rigorously quantify the effect of a childhood immunization program on mortality and educational outcomes in a developing country setting. However, this paper is related to two literatures which I discuss briefly below. First is the medical literature on the impact of immunization vaccines on mortality and other health outcomes. Second is the economics literature on the causal effect of child health on educational outcomes.

In the medical literature, there have been several types of studies. First are clinical trials of vaccines. These have firmly established that the DPT vaccine effectively pro-

[2] Gupta and Murali (1989); Sathyamala (1989); Annual Report (1987-88), MoHFW; UNICEF Coverage Survey 2002; WHO Review (2004).

tects against diphtheria, pertussis and tetanus; the BCG vaccine against typhoid and the measles and polio vaccines against measles and polio, respectively (Levine et al., 2005; United Nations, 2006). There have also been a few epidemiological studies in developing countries which examine the impact of specific vaccines on child mortality (Breiman et al., 2004 for Matlab, Bangaladesh; Koeing et al., 1990 for Senegal). These studies reconfirms the laboratory evidence and find decreased risk of death for vaccinated children. The sample sizes tend to be small in these studies, unfortunately. Finally, there are numerous studies that examine the cost-effectiveness and cost-benefit of vaccines (Navas, 2005; Ekwueme, 2000; see Bloom, Canning and Weston, 2005 for a review). These studies look at outcomes like averted illnesses, hospitalizations and deaths, disability-adjusted life years (DALYs) gains, and medical costs. These studies suggest immunization is a highly cost-effective intervention. This medical research underlies the public health policy of many countries to require vaccinations for all children.

This paper differs from the medical studies in several respects. First, it is evaluating the effect of a program that provided vaccinations, not the effect of the vaccinations as the medical studies have done. Although we know scientifically that vaccinations reduce mortality, we do not know whether a mass childhood immunization program can be effective in reducing mortality in a poor developing country such as India. The success of the program depends not only on the efficacy of the vaccinations but also the public health delivery system. Second, given the low baseline vaccination coverage and average health status in poor developing countries, it may well be that the marginal impact of the vaccinations may be greater than in developed countries where the medical studies were done. Third, the medical literature has ignored non-health outcomes such as education.

There is a large body of literature in economics that shows positive correlations between health and education. Glewwe and Miguel (2008) provide a review. It is difficult to infer the causal effect of health on education from these studies since it is easy to imagine that it may be education that is affecting health, or some omitted variable that is affecting both health and education. Behrman (1996) argues that existing evidence on child health and education (at least up to the time of his paper) is inconclusive because of the difficulty in separating causality from correlations.

More recently, researchers have used randomized experiments and natural experiments to identify the causal effect of health on education. In randomized experiments, the researcher randomly assigns similar units to different health treatments (a treatment group that receives a medical treatment and a control group that doesn't), generating an exogenous source of variation in health. Randomized experiments examining the effect of child health on education include the following. Miguel and Kremer (2004) find that providing children with deworming medication significantly reduced serious worm infections and increased school attendance. Bobonis, Miguel and Sharma (2006) find that a iron supplementation program significantly reduced anemia and school absenteeism. Vermeersch and Kremer (2004) find that a school breakfast program significantly raised preschool attendance and cognitive test scores.

Researchers have also used natural experiments to identify the effect of child health on educational outcomes. Along these lines, Bleakley (2007) exploits region-by-time variation in exposure to the hookworm eradication program sponsored by the Rockefeller Sanitary Commission in the 1910s in the United States to identify the effects of reducing hookworm infections on educational outcomes. He finds that regions that experienced greater reductions in hookworm infections had larger increases in school attendance and literacy. Few working papers also use a difference-in-differences strategy to estimate the effect of malaria on human capital accumulation (Bleakley, 2007 for the U.S., Brazil, Colombia and Mexico; Kremer et al., 2007 for India; Lucas, 2005 for Sri Lanka, Paraguay and Trinidad).

This paper takes the tactic in this latter group of studies by taking advantage of a natural experiment to identify the effect of health on education. As described in the next section, I use district-by-cohort variation in exposure to UIP to obtain estimates of the effect of health on education. It is one of only a handful of studies that addresses the issue of endogeneity in health when estimating the effect of health on education. Moreover, it uses a new and different source of variation in child health than has never been used before. In particular, none of the previous studies have looked at the impact of an immunization program. Immunization programs affect both mortality (of the youngest children) and morbidity (of older individuals). In contrast, the aforementioned studies that estimate the causal effect of health on education use health interventions-deworming treatments, malaria treatments and school meals for school-aged children-that operate primarily on the morbidity margin.

3 Empirical Framework

The objective of this study is to estimate causal impact of a developing-country immunization program on child mortality and education outcomes. Ideally to identify the effect, we would conduct a randomized experiment where some children are placed into a treatment group that receives immunizations and others are placed in a control group. We would follow these children over time and compare their mortality and educational outcomes. The control group describes the counterfactual of what the treatment group's outcomes would have been had the medical intervention not occurred. This is a simple and convincing approach since at the outset of the experiment, the children were similar.

In the absence of a randomized experiment, I rely on a natural experiment. I use variation provided by India's implementation of UIP in the 1980s. In particular, I estimate the program effect by utilizing the following two sources of variation in exposure to UIP: variation across districts and variation across cohorts. First, variation across districts comes from the fact that districts got the program in different years. Figure 2 in the Appendix shows the number of districts added on to UIP each year. UIP was implemented in 48 districts (31 according to old district definitions) in the first year, 92 additional ones in the second year and so on until all 563 districts (443 according to old district definitions) were covered in 1990.[3] Second, variation across cohorts comes from the fact that only children who are twelve months or younger when UIP was implemented would have been eligible to receive the shots. Table 2 in the appendix shows the schedule for the vaccines that UIP provided; the shots are administered on a strict schedule in the first year of a child's life for maximal efficacy. Children older than one year were not treated by UIP. Table 3 of the Appendix shows the birth cohorts that were eligible for UIP by district's year of UIP inception. For example, a child born in 1985 would have been exposed to UIP if he lived in one of the 48 districts that implemented UIP first (in 1986), but not if he lived in a district that implemented UIP later.

The difference-in-differences approach uses only the within-district cross-cohort variation in exposure to UIP to identify the effect of UIP. This is elaborated next.

3.1 Difference-in-Differences Strategy

Consider the following equation:

$$Y_{idt} = \beta_0 + \beta_1 UIP_{idt} + \delta X_{idt} + e_{idt} \tag{1}$$

where Y_{idt} is the outcome variables for an individual i, residing in district d, at time t. UIP_{idt} indicates exposure to UIP and X is a vector of individual and household characteristics (e.g., sex, caste, religion, age, birth order, mother's education, mother's age, rural/urban). e_{idt} is the error term.

We wish to estimate the effect of UIP so the parameter of interest in this equation is β_1. But β_1 in equation (1) may not be consistently estimated due to omitted variable bias. Districts may be different from each other on many unobserved dimensions that can affect the outcome variables. For example, UIP officials indicated that the timing of UIP implementation across districts was not random and instead apparently determined

[3] The number of districts increased from 443 to 593 between the UIP period (1985-1990) and the RCH survey year (2002-04). The data section has more details.

by some features of districts that could be considered constant over time such as health infrastructure. Similarly, each year of birth can also be systematically different in ways that affect the outcome variables. For example, there is progress in health and education over time, or some country-wide economic shocks in a particular year may affect that year's newborns differently than the another year's. I address these concerns by adding district fixed effects and year of birth fixed effects to equation (1):

$$Y_{idt} = \beta_0 + \beta_1 UIP_{idt} + \gamma_d + \phi_t + \delta X_{idt} + e_{idt} \tag{2}$$

where Y_{idt} is the outcome variables for an individual i, residing in district d, at time t. UIP_{idt} indicates exposure to UIP. γ and ϕ are district and year of birth fixed effects, respectively. X is individual and household controls.

The parameter β_1 in equation (2) can be interpreted as the causal effect of UIP under the assumption that the difference in outcomes between the younger and older cohorts would have been the same between earlier-implementing districts and later-implementing districts in the absence of UIP. [4] In other words, the parallel trend assumption should be satisfied. While it is not possible to directly test this assumption-it is a counterfactual-I can assess its validity in a couple of ways. Below, I do this by: (1) estimating equation (2) using only older cohorts that have never been exposed to UIP but where I falsify their treatment status; and (2) estimating equation (2) using an outcome that is unlikely to be affected by the program.

The identifying assumption would also be violated if there were some other contemporaneous interventions that had the exact same district-by-cohort variation as UIP and which also affect the outcome variables I examine. To the best of my knowledge, I am not aware of any child health or education intervention that can contaminate the identification of the effect of the UIP.

[4]If UIP exposure were a simple interaction between two binary variables, say being in an earlier-implementing district and being in a younger birth cohort, then β_1 would be a differences-in-differences estimate, i.e., the cohort difference in outcome in earlier-implementing states that is in excess of the cohort difference in later-implementing states. In fact I use more variation in UIP exposure but the intuition is similar to the simple binary case and so I term my approach a difference-in-differences-type strategy.

The parameter β_1 in equation (2) may underestimate the true effect of UIP for a couple of reasons. First, because consumption of vaccines has a positive externality, it is possible that the control group benefits indirectly from UIP. That is, although the control group is not eligible for UIP vaccinations, they may benefit because the diseases spread more slowly when more people are vaccinated. Second, recall that the EPI program preceded the UIP program in India. The EPI had very low vaccination coverage rates and operated only out of major hospitals so it is unlikely to pose a significant problem. But it is quite likely that the control (i.e., older) cohorts in urban areas got vaccinated under EPI, which means the treatment group is partially treated already. This would cause me to underestimate the program effect in urban areas. Third, inter-district movement of households across districts may confound the estimated effects, but it is not much of a concern here, because of limited inter-district migration in India (Munshi and Rosenzweig, 2006; Chatttopadhyay and Duflo, 2004).

3.2 Allowing for Heterogeneity in Program Effects

UIP may not have uniform impacts. The impact may differ based on child sex, socioeconomic status, rural/urban, caste, etc. For example, perhaps the rich would have immunized their children even in the absence of the program and it is the poor who would benefit more from the program. On the other hand, there are also stories of "elite capture" and it is possible that rich and elite class capture most of the benefits of the program. As another example, the program may have different impacts in rural areas from urban areas due to differences in the availability of health care infrastructure to deliver the services. Also, the program effect may vary by gender. Oster (2007) shows that girls in India are discriminated against in access to vaccination.

To test whether the program effect varies by sex, I estimate the following equation:

$$Y_{idt} = \beta_0 + \beta_1 UIP_{idt} + \beta_2 UIP_{idt} * Female_{idt} + \beta_3 Female_{idt} + \gamma_d + \phi_t + \delta X_{idt} + e_{idt} \quad (3)$$

where the omitted category is male. β_1 measures the average program effect for male children and β_2 captures the additional program effect for females.

Similarly, to examine whether there is a differential program impact by rural residence, I estimate the following equation:

$$Y_{idt} = \beta_0 + \beta_1 UIP_{idt} + \beta_2 UIP_{idt} * Rural_{idt} + \beta_3 Rural_{idt} + \gamma_d + \phi_t + \delta X_{idt} + e_{idt} \tag{4}$$

where the omitted category is urban residence. β_1 captures the program effect for children residing in urban areas and β_2 captures the additional program effect of residing in rural areas.

Next, I allow the program effect to vary by socio-economic status of the household.[5] To capture the heterogeneity in treatment effect by household socio-economic status, I estimate the following equation:

$$\begin{aligned} Y_{idt} = \beta_0 + \beta_1 UIP_{idt} + \beta_2 UIP_{idt} * Low_{idt} + \beta_3 UIP_{idt} * Middle_{idt} \\ + \beta_4 Low_{idt} + \beta_5 Middle_{idt} + \gamma_d + \phi_t + \delta X_{idt} + e_{idt} \end{aligned} \tag{5}$$

where the omitted category is households with high socio-economic status. β_1 captures the program effect for children residing in household with high socio-economic status, β_2 captures the differential program effect for children residing in household with low socio-economic status (relative to the high category) and β_3 captures the differential program effect for children residing in household with middle socio-economic status.

To estimate how the program effects differ by social group, I estimate the following

[5] The survey asks whether the household owns the following consumer durables: radio, television set, refrigerator, bicycle, motorcycle and car. Based on ownership of these consumer durables, the RCH survey categorizes the household into three different categories in terms of socio-economic status: Low, Middle, and High. Though this is not the perfect measure of household wealth status, this is the best we can do given the fact that the health survey does not collect direct information on the income and wealth of the households.

equation:

$$\begin{aligned} Y_{idt} &= \beta_0 + \beta_1 UIP_{idt} + \beta_2 UIP_{idt} * SC_{idt} + \beta_3 UIP_{idt} * ST_{idt} + \beta_4 UIP_{idt} * OBC_{idt} \\ &+ \beta_4 SC_{idt} + \beta_5 ST_{idt} + +\beta_6 OBC_{idt} + \gamma_d + \phi_t + \delta X_{idt} + e_{idt} \end{aligned} \tag{6}$$

where the omitted category is household from high caste groups. β_1 captures the program effect for children belonging to high caste groups, β_2 captures the differential program effect for children belonging to Schedule Castes (SC) (relative to the high castes), β_3 captures the differential program effect for children belonging to Schedule Tribes (ST) and β_4 captures the differential program effect for children belonging to Other Backward Castes (OBC). SCs and STs are historically disadvantaged minority groups in India. OBCs are not as poorly off as SCs, but also have faced discrimination historically.

4 Data

My empirical analysis uses data from two sources: individual-level data from the Reproductive and Child Health (RCH) Survey and administrative data about UIP from the Ministry of Health and Family Welfare, Government of India.

The RCH survey is a large, nationally representative survey. Due to the timing of UIP, it is appropriate to use the second wave of the RCH survey, which was conducted during 2002-2004. First, I use the "fertility file" to construct the sample for my child mortality analysis. Fertility history is collected for one woman who is aged 15 to 44 from each surveyed household. The fertility history includes information on all children ever born to a woman even if the child has died by the time of the survey. This enables me to collect for each child born to a woman in the fertility file the year of birth, whether he/she has died and if so, when he/she died. Second, I use the "household file" to construct the sample for analyzing the educational outcomes of surviving children. Both the fertility and household files contain information on such control variables as district, rural/urban, child sex, age, and birth order, and household social group, religion and socio-economic condition.

The Ministry of Health and Family Welfare provided administrative information about UIP. First, I talk to several UIP officials to find out the details of how UIP was implemented. It was these conversations that led me to believe that the timing of UIP could be considered conditional on district fixed effects, leading me to the difference-in-differences strategy. Second, I obtained from them a list of new districts that implemented UIP each year, from year 1 (1985-86) to year 5 when all districts were covered (1989-90).

I mapped the year of UIP implementation from the district-level administrative data back to the individual-level RCH survey data using the district codes. One complication was that the number of districts increased from 443 to 593 between the UIP period (1985-1990) and the survey period (2002-04). Either an existing district split into two or more new districts or a new district was formed by taking areas from two or more districts. I successfully match 563 districts by looking at district census handbooks, district websites and other government sources (a success rate of 95 percent).

The main outcome variables for my mortality analysis are the probability of dying within the first twelve months (which I label Pr(Infant Mortality) and the probability of dying within the first five years (Pr(Under-five Mortality)). Both infant mortality and under-five mortality are common health indicators used by governments and international agencies. Under-five mortality is an indicator of the cumulative exposure to mortality risk during the most vulnerable years of childhood; it includes infant mortality as well as mortality from age 1 to 5.

For my analysis of the educational outcomes of surviving children, the main outcome variables are Pr(Literate), Pr(Primary School Completion), Pr(Middle School Completion), Pr(Secondary School Completion) and Years of Schooling. All the education outcomes variables are dichotomous variables except Years of Schooling and are defined as follows. Pr(Primary School Completion) is defined as Pr(Years of Schooling $\geq$5), Pr(Middle

School Completion) is defined as Pr(Years of Schooling $\geq$8), Pr(Secondary School Completion) is defined as Pr(Years of Schooling $\geq$10). The education outcomes variables are conditional on being literate.[6]

Table 1 shows the descriptive statistics of the variables used in the mortality and education analyses. The paper uses children born between 1983 and 1992 for the mortality outcomes and children born between 1983 to 1997 for the educational outcomes. The number of observations for child mortality analysis is 297,385 and for education analysis there are 898,789 observations.[7] In the child mortality sample, 69 percent of the children lives in rural areas and 47 percent belongs to a poor household. Majority of the children are Hindu (76 percent) and disadvantaged minority group ST and SC forms 33 percent of the sample. The mean mother's age is 37.1 years and only 39 percent of the mothers are literate. The mean mother's age is higher than the mother's age of the average child because the survey was done in 2002-04 and the paper uses the children born between 1983 and 1992. The demographic and family characteristics of sample used in education analysis are similar to the sample used in child mortality analysis.

In the child mortality sample, the mean infant mortality rate is 9% and the mean under-five mortality rate is 11%. In the education sample, 82 percent of the children are literate and conditional on being literate the average years of schooling is 6.2 years. Conditional on being literate, about 65 percent children have completed primary schooling, 36 percent have completed middle school and only 18 percent of children have completed secondary school. The mean age of the children is 13.71 years; some of the children in the sample are still in school.

[6] The survey asks Years of Schooling question only to the literate individuals.

[7] Different samples are used for the child mortality analysis and education analysis because as explained earlier in this section, the data for the mortality analysis are from the "fertility file" of the RCH and the data for the education analysis are from the "household file". Only one woman aged 15-44 from each household answers the supplemental fertility questions, hence the smaller sample size.

5 Effect of UIP on Child Mortality

5.1 Basic Results

Table 2 reports the results of estimating equation (1) and equation (2) for infant mortality and for under-five mortality.[8] The main coefficient of interest is the coefficient for the variable "Exposed to UIP", which gives the average program effect. Column (1) and Column (3) show the naive estimates of the program effect by estimating equation (1). Estimates from Column (1) and Column (3) suggest a significant negative impact of the program on Pr(Infant Mortality) and Pr(Under-Five Mortality). Results from column (1) and Column (3) suggest that the program decreases the probability of infant mortality by 2.2 percentage points and the probability of under-five mortality by 2.8 percentage points. On comparing the naive estimates with difference-in-differences estimates in Column (2) and Column (4), it turns out that naive estimates grossly overestimate the program effect. Estimating equation (1) is not the correct approach because districts differ in their fixed characteristics and ignoring these fixed differences would bias the program effect. In particular, UIP officials stated that the timing of UIP was not random, and instead districts that had better health care infrastructure received it sooner. Given this statement, it is not surprising that the estimates in columns (1) and (3) are so large-they encapsulate not only the true effect of UIP but also the effect of being in a district with better health care infrastructure (which not surprisingly has a large negative effect on mortality!). Similarly, there could be cohort-specific characteristics and without taking in account of these characteristics, it is not possible to get the true effect of the program. For example, there is improvement in health conditions and care in India over time, so younger cohorts would have lower mortality even without UIP and the estimates in columns (1) and (3) erroneously attribute these secular improvements over time to UIP.

The naive estimates in columns (1) and (3) highlight the dangers of giving causal

[8] I estimate these models using OLS, i.e., using the linear probability model with standard errors clustered at the district level. I also estimated these models using logit and find qualitatively similar results; these results are available upon request.

interpretations to parameter estimates when the sources of variation are not plausibly exogenous, and motivate my difference-in-differences approach. The preferred estimates are in Columns (2) and (4)-these are results from estimating equation (2), which includes district fixed effects and year of birth fixed effects. Results from Column (2) and Column (4) suggest that the program significantly reduces infant mortality and under-five mortality. UIP decreases the probability of infant mortality by 0.4 percentage points, and the estimate is statistically significant at 10 percent level of significance (column 2). The official estimate of infant mortality was 9.7 percentage points in 1985 and 8 percentage points in 1990. The 0.4 percentage point effect of UIP is 4.1% of the baseline infant mortality rate and a fifth of the decline in infant mortality between 1985-1990. Thus, over a short period, UIP caused a meaningfully sized reduction in infant mortality; it took India thirty-four years to bring down the infant mortality from 14.6% in 1951 to 9.7% in 1985.

Column(4) reports the results for under-five mortality. Results show that the program has a negative and significant impact on under-five mortality. The program reduced under-five mortality by 0.5 percentage points. The under-five mortality was 15 percentage points in 1985 and reduced to 12.3 percentage points by 1990. The 0.5 percentage point effect of UIP is 3.3% of the baseline under-five mortality rate and almost a fifth of the decline in under-five mortality between 1985-1990. It should be noted that the program has a larger impact on under-five mortality compared to infant mortality. This is to be expected because the under-five mortality rate includes infant mortality as well as mortality of children aged 1 up to 5. Thus there is a tenth of a percentage point decline in the mortality of children aged 1 up to 5 (fourth tenths of a percentage point is the decline in mortality of children up to age 1). It makes sense that the mortality declines are greatest for infants. Vaccinations under UIP begin at birth, and though maximal protection is not gained until all the doses are administered according to schedule, protection begins right away. This early protection makes a big difference for infant survival since infants do not have well developed immune systems yet.

In all the regression models in Table 2, the signs of the control variables are as expected. Mother's age and mother's education have negative and significant effect on infant and under-five mortality. Poor and disadvantaged minority children(ST and SC) are more likely to die. For Other Backward Caste and Hindu children, the estimates are positive and significant, meaning that children belonging to these categories have higher probability of dying.

5.2 Heterogeneity in Program Effects

Table 3 and Table 4 show the results from estimating equations (3)-(6) where the effect of the program on mortality outcomes vary by gender (column 1), by rural (column 2), by caste (column 3) and by socio-economic status (column 4). Table 3 reports the results on infant mortality and Table 4 reports the results on under-five mortality. Column (1) of Table 3 suggests that UIP did not have a different effect for boys and girls, i.e., infant mortality decreased by about the same amount for both girls and boys due to UIP (though the point estimate is negative for girls, suggesting that girls might have benefited a little more). Column (2) suggests that the program had a zero effect in urban areas (the point estimate is 0.05 percentage points and is insignificant) and rural areas had a negative effect that is significantly different both from the urban effect and from zero at 5 percent level of significance. That a reduction in child mortality is found only in rural areas may be due to urban people already having access to vaccinations even before UIP or just that health conditions and care tend to be better in urban areas (as indicated by the much lower infant mortality rates at the outset). Thus there is more room for improvement in the rural areas, and that is where I find mortality effects. In Column (3), the program effects are negative for SC and OBC children; these effects are both significantly different from zero and from the effect for higher caste groups. The effect for ST children is negative but not significant.[9] Finally, the program also has negative and significant impact on

[9]Similar to the SC and the OBC, the ST are also a disadvantaged group with higher-than-average child mortality so perhaps it is surprising that UIP did not reduce this group's child mortality more. This weak effect is probably due to the fact though UIP

infant mortality for children from poor households (column 4). There was no reduction in mortality among children from middle- and high-income households. Columns (3) and (4) both inform on whether the effects of UIP vary by household socioeconomic status (there is much overlap between being in a low social group and being poor), and they both clearly show that it is the children from disadvantaged households who experience the declines in mortality; there is no effect of UIP on higher caste groups or the middle-income and rich. Children from higher caste and non-poor households have lower mortality rates at the outset, and may have been getting vaccinated to some extent already.

I show the results for under-five mortality in Table 4. They are similar to the infant mortality results in Table 3 except the estimated reduction in mortality is higher for reasons stated earlier (the under-five mortality measure includes mortality of infants and older children). As before, it appears to be the children from rural, lower caste groups and poor households who are benefiting from UIP in terms of increased survival rate.

5.3 Testing for Differential Trends in Child Mortality

The identifying assumption for my empirical strategy is that in absence of the program, the difference in outcomes of younger and older cohorts would be the same between earlier-implementing districts and later-implementing districts. We would not be able to interpret the coefficients for "Exposed to UIP" in Table 2-4 as the causal effect of the immunization program if the aforementioned were not true. In this subsection, I assess the validity of this assumption by performing two "control experiments": (1) estimating equation (2) using only older cohorts that have never been exposed to UIP but where I falsify their treatment status; and (2) estimating equation (2) using an outcome that is unlikely to be affected by the program.

To test the possibility of a differential trend in infant and child mortality, I do a control experiment in which I only use data on older cohorts who are not affected by the

was far-reaching, it may not have reached many of the places where the ST reside; the ST mostly live in remote, sparsely settled rural areas.

program (Duflo, 2004 and Angrist, Chin and Godoy, 2008 have control experiments of this type). I take advantage of older cohorts born between 1977 and 1982 who are not treated.[10] In this control experiment, I assign pseudo treatment to cohorts born during 1977-1982 as if the program were implemented during 1977-1982 (instead of 1985-1990). For example, districts that got the program in 1985-1986, now get the pseudo program in 1977-78. For these cohorts, I run the same basic specification outlined in equation (2). Columns (1)-(2) of Table 5 report the results of this control experiment. The outcome variables are Pr(Infant Mortality) and Pr(Under-Five Mortality). The coefficient for the "Exposed to UIP" variable should be zero if the identifying assumption is correct, i.e., the cohort change in mortality would have been the same in earlier-implementing districts and later-implementing districts in the absence of UIP. This is because nobody in the sample actually got exposed to UIP. In Columns (1)-(2), I find statistically insignificant coefficients for the pseudo treatment variable and point estimates are actually close to zero and positive. This suggests that district-specific cohort trends do not appear to be confounding the estimates of the effect of UIP using my difference-in-differences approach, and that the difference-in-differences estimates shown in Tables (2)-(4) can be interpreted as the causal of effect of UIP.

Another control experiment that can be performed is to check outcomes that are unrelated to UIP itself but are susceptible to the same unobserved district-cohort changes that are putatively confounding the difference-in-differences estimates. For example, one might be concerned that earlier-implementing districts had more improvements in the health care which in turn is causing the greater decline in infant and child mortality compared to later-implementing districts. Of course the previous control experiment using older cohorts does not support this claim, but arguably the differential trends estimated using those cohort do not apply to the cohorts in my main analysis because of some regime

[10] I am unable to use older cohorts born before 1977 because of data limitation. There are very few observations for period before 1977 because of the sampling nature of the RCH survey. The survey has information on women who are 15-44 years old at the time of the survey (2002-04). Given the age restriction on the mothers, there are very few children born before 1977.

shift. The RCH survey does not collect many health outcomes across people of different age groups, but one such health outcome is "blindness". I perform a control experiment in which I examine the impact of the program on blindness. I measure blindness with a dichotomous variable.[11] The medical literature suggests that immunization shots given under UIP are unlikely to protect the children from blindness. If this is true, I should not observe any correlation between exposure to UIP and blindness outcome. Column (3) of Table 6 reports the results of estimating equation (2) with blindness as the outcome. The coefficient for "Exposed to UIP" is not significantly different from zero. Thus, it does not appear that earlier-implementing districts have significantly more progress in health care compared to later-implementing districts. If anything, it is the other way around since the point estimate is positive (and for this outcome as for mortality, more positive number means worse health). The result of this second control experiment provides more support for the validity of the identifying assumption for the difference-in-differences approach that I use to estimate the causal effect of UIP.

5.4 Discussion of Child Mortality Results

I find that on average, UIP reduced infant mortality by 0.4 percentage points and under-five child mortality by 0.5 percentage points. The effects are more pronounced in rural areas, for poor people, and for members of historically disadvantaged groups. For example, children born into poor households were 0.9 percentage points less likely to die within the first twelve months and 1.3 percentage points less likely to die within the first five years. Thus, there are huge benefits of a mass immunization program in terms of reducing child mortality.

As mentioned in section 3, these estimates may underestimate the true effect of UIP for a couple of reasons. First, because consumption of vaccines has a positive externality,

[11] The survey asks the blindness question for each members of the household in the household roster. The answers are categorized as partially blind, completely blind, night blind and not blind. I construct an outcome variable "blindnes" by combining partially blind, completely blind and night blind together.

it is possible that the control group benefits indirectly from UIP. Second, an immunization program may provide more benefits in urban areas than has been estimated because prior to UIP, some people in urban areas might have been vaccinated under the EPI program.

The dramatic effects on child mortality are consistent with children's immune systems being strengthened. This would suggest that there would be significant decreases in child morbidity, too, due to UIP. Immunization not only protects against the specific diseases (whose symptoms may be deadly to infants but only illness among older children) but also improves the overall immune system of the body (which protects the children from other illnesses). It is likely that the benefits of UIP on child health conditional on surviving would not be confined to children from rural, low caste and poor households; children from urban, higher caste and non-poor households may be far from the margin of survival but there is still room for improvement in terms of health status. Unfortunately I do not have the data to directly assess the effects of UIP on child health outcomes besides the extreme outcome of mortality.[12]

It is worth highlighting that UIP has been a successful program in reducing child mortality in India despite the fact that India is characterized by poor service delivery mechanism and high absenteeism of health staffs. Banerjee, Deaton and Duflo (2004) portray a very bleak picture of public and private health care provision in Udaipur district of India and find that 45% of medical personnel are absent in health subcenters. A similarly high level of absenteeism has also been found in a nationally representative survey of primary health centers in India by Chaudhury et al. (2006). Despite these impediments, the immunization program did achieve its intended objective of reducing mortality among Indian children.

[12]The RCH survey has extensive health measures for children under age 5. Given that the RCH survey is collected in 2002-04, all these children would have been exposed to UIP, leaving no variation in treatment. Thus these rich child health measures are not usable for the purposes of estimating the impact of UIP.

6 Effect of UIP on the Educational Outcomes of Surviving Children

Some recent studies using plausibly exogenous variation in child health from interventions to reduce worm diseases and malaria and from school nutrition programs have found a causal relationship running from child health to education (these were discussed in subsection 2.2). In particular, improving child health improves educational outcomes. The ability to attend school more regularly and often and to concentrate on studies better when one is healthier is thought to be responsible at least in part. In the previous section, I found that UIP reduced child mortality and speculate that it reduced child morbidity too. Given these beneficial effects of UIP on child health, it is natural to ask what are the consequences for the educational outcomes of the surviving children.

6.1 Estimation Results

Education results are based on children who survived beyond age five. Table 6 presents the results of estimating equation (2) using each of the educational outcomes in turn-Pr(Literate), Pr(Primary School Completion), Pr(Middle School Completion), Pr(Secondary School Completion), and Years of Schooling.[13] Column (1) suggests that the program has no effect on the the probability of being literate. The educational outcomes are conditional on child being literate.[14] Column (2) suggests there is no significant effect of UIP on years of schooling completed. This masks a nonlinear effect of UIP on schooling. UIP significantly decreased the probability of primary school completion (by 4.6 percentage points), had no impact on middle school completion and significantly increased the probability of secondary school (by 1.9 percentage points).

[13] I estimate these models using OLS, so in the case of a dichotomous outcome I am using the linear probability model. For the dichotomous outcomes, I have also used logit and find qualitatively similar results; these results are available upon request.

[14] The RCH survey asks for years of schooling completed only for people responding affirmatively to being literate. In theory, this could lead to selection bias when I examine the impact of UIP on years of schooling and the primary, middle and secondary school indicator variables since the sample is conditional on being literate. Therefore, I use the Heckman two-step correction method to correct for the sample selection bias. In practice, the bias on the estimated effect of UIP is negligible given the insignificant result in Column (1). In additional analysis, I have coded being illiterate as having zero years of schooling or at least less than primary school completion and all the results I am about to discuss remain.

Results in Table 6 suggest that though UIP did not raise years of schooling on average, it reduced schooling at low levels of education and increased it at higher levels of education. To get more detail on the effect of UIP at different points in the education distribution, I estimate equation (2) for each level of schooling k, where the dependent variable is Pr(Years of Schooling $\geq$ k), where k = 1 to 15. The estimated coefficients with the 95-percent confidence interval are plotted in Figure 1. Each point on the graph is from a different regression. The "S" shape of Figure 1 suggests that the program decreased the number of years of primary schooling for some children, but increased the number of secondary schooling for others. At the low end, there is a shift away from completing 5 years of schooling toward completing 2-4 years of schooling. At the high end, there is a shift away from completing fewer than ten years of schooling toward completing 10-12 years of schooling. Apparently the decrease at the lower end of the education distribution offsets the gains at the upper end, leading to a zero average effect on years of schooling.

In Figure 2, I perform the same analysis as in Figure 1 but separately for children in rural and urban areas. Comparing Panels A and B, we see that all the negative impact of UIP at the low end of the education distribution is coming from rural areas-the graph in Panel B is flat at zero for the first nine years of schooling. This is especially interesting since it was only in rural areas where UIP had an impact on child mortality. UIP increased schooling at the high end of the education distribution for children in both the urban and rural areas, however.

In Figure 3, I perform the same analysis as in Figure 1 but separately for children from low, middle and high socio-economic status households. We see that all the negative impact of UIP at the low end of the education distribution is coming from poor households-the graph in Panels B and C does not have the trough at the lower levels of schooling. This is also interesting because it was only in poor households where UIP significantly reduced child mortality. The effects have an inverse U-shape at the high end of the education distribution in all three wealth category though results are only significant for the poor category.[15]

6.2 Channels for the Effect of UIP on Educational Outcomes

UIP had mixed impacts on children's educational outcomes. It appears to have decreased the number of primary grades completed for some children but increased the number of secondary grades completed for other children. This nonlinear effect in which some children have worse educational outcomes and others have better ones is unusual vis-a-vis the existing literature which has tended to find positive effects on education for health interventions that improve child health. There are various explanations for the results that I find in Table 6 and Figures 1-3. Though I cannot conclusively pin down the pathways, here I describe several hypotheses consistent with the results. These hypotheses are not mutually exclusive, and may each have a part in the overall results.

6.2.1 Composition Effect

Change in the composition of the surviving pool of the children can be one factor that is driving the nonlinear effects on education. First, UIP reduced child mortality. I will call the children who UIP saved from dying marginal children. Though they are alive (and in this sense, have better health than without UIP), these marginal children are probably less healthy than the average child. Because they have worse health among surviving children, they have worse educational outcomes. This may be because they attend school less frequently, have less capacity to focus and learn, or take longer time to complete normal tasks.

As argued earlier, UIP also likely reduced child morbidity among inframarginal children (i.e., the children far from the margin of survival). Since their health is better in the traditional sense (i.e., as in the other papers estimating the causal impact of child health on education such as Miguel and Kremer, 2004 and Bleakley, 2007), we might expect their

[15] The lack of significance is likely because the number of observations is much less when I perform the analysis separately for each SES category; results remain significant in Panel A because 45% of the sample is in the poor SES category.

educational outcomes to improve as in these other papers.

That UIP improved health on two margins, from dying to survival for the children on the margin of survival, from less healthy to more healthy for inframarginal children, provides a cohesive story for the nonlinear effects on education. On the one hand, the marginal children are likely to be concentrated on the lowest parts of the education distribution, causing there an estimated reduction in primary school completion. Corroborating this assertion is that in Figures 2 and 3, we observe a negative impact at the low end of the education distribution only among those children who experienced a decline in mortality-the rural and the poor. On the other hand, the inframarginal children are on higher parts of the distribution and their improvement in health causes them to attain more years of schooling.

6.2.2 Quantity-Quality Tradeoff

These results on education can be analyzed in the framework of the quantity-quality tradeoff model that was developed by Gary Becker and his associates (Becker, 1960; Becker and Lewis, 1973; Becker and Tomes, 1976). In these models, an increasing marginal cost of quality (child outcome) with respect to quantity (number of children) leads to a tradeoff between quantity and quality[16]. In this paper, I find that UIP reduces child mortality. This may increase the number of surviving children, though I do not test this here.[17] Empirical studies generally conclude that child mortality reduction modestly decreases the number of births, increases the number of surviving children, and stimulates population growth.[18] If this is true, parents have less resources to spend on each child, leading to less investment in the quality (education) of the children. Thus, the quantity-quality tradeoff provides an explanation for the negative effect on primary school completion.

The empirical evidence on the quantity-quality tradeoff is mixed (e.g., Black et al.,

[16] Li et al., (2005)

[17] In a companion paper, I examine the impact of UIP on the fertility of the women and on the number of surviving children.

[18] Azarnert (2006); see Preston (1978) for a collection of demographic essays that come to such conclusion and Palloni and Rafalimanana (1999) for a broad survey of literature; see also Rutstein (1974), Chowdhary et al. (1976), Balakrishnan (1978), Olsen (1980), and Olsen and Wolpin (1983).

2005; Angrist et al., 2005). It is possible that this tradeoff does not exist in developed countries where there exists a well structured public education system and welfare programs targeting childbearing and child care. In a developing country like India, the cost of child quality is mostly borne by the parents because these countries lack a well-functioning public education system and they do not have any support for childbearing and childcare. Thus, the quantity-quality tradeoff is a more likely to be relevant to developing country like India.[19]

6.2.3 Other Explanations

School quality is another channel that can be driving the education results in the paper. Decline in child mortality may have increased the number of surviving children making the classrooms crowded and thereby putting downward pressure on school quality. This is possible if the government is hard pressed for resources and decides to put more emphasis on child health programs but does not simultaneously improve the existing school infrastructure and quality. Decrease in school quality may result in either less school participation or reduced learning in the schools.

Another explanation for the negative effect on primary school completion is that improving child health may actually increase children's labor force participation. It is quite likely that when returns to schooling are low or if the family is credit constrained, children join the agricultural field with their parents to support the family or they enter the child labor market. If a child is somewhat healthy but not so healthy that he can earn much working, he may be sent to school. When his health improves say due to UIP, he may become healthy enough to work and therefore drop out of school.

Besides the story where the health of the inframarginal children improves, another story for why there may be positive effects on education is that UIP has increased the

[19] There is some evidence on the quantity-quality trade-off from developing countries in studies of epidemiology and public health also, although the methods of these studies are usually different from those of economists. See, for example, the survey by Karmaus and Botezan (2002).

life expectancy of children, causing parents to invest more in children's education because there is a longer period to collect the returns. Jayachandran and Lleras-Muney (forthcoming) find empirical evidence in support of this hypothesis. This hypothesis is unlikely to apply in the present case, though, because UIP primarily affects the mortality of very young children. Conditional on surviving to age 5, children's life expectancy does not differ much with or without UIP. Yet, parents are likely not making educational investment decisions before age 5 in most of India and given this, UIP should not impact education through this mechanism. However, it is true that vaccinations reduce morbidity at later ages even if mortality is largely unaffected, and this can add up to meaningful differences in productive days between UIP-exposed children and other children.

7 Conclusion and Policy Implications

By using the phase-in feature of India's Universal Immunization Program immunization program and eligibility rules that only granted vaccinations to children up to twelve months, I estimate the causal effect of UIP on children's health and education outcomes. I find that the program significantly reduces infant mortality and under-five mortality in India. Contrary to the popular belief that India is plagued with inefficient program implementation capacity and poor public health service delivery system, this paper establishes that UIP was successful in achieving its objective of reducing mortality.

Among surviving children, UIP had a negative impact on primary school completion for some, but a positive impact on secondary school completion for others. The results on education outcomes can be explained in terms of change in the composition of the surviving children due to the immunization program. The negative effect on education may be due to lower quality of the "marginal child" similar to the argument made by Donohue and Levitt (2001) and Gruber, Levine and Staiger (1999); UIP induced some children to survive who otherwise would have died, and these children may be less healthy. The negative results are also consistent with the quantity-quality trade-off where an unanticipated increase in household size due to the immunization program induces the households to under-invest in each child. On the other hand, the result that UIP increased the education of some children is likely due to improved health among those children who are not at the margin of survival.

The results of this paper have important policy implications for the design of optimal health and education policy in developing countries. While the program had the intended benefit of increasing the survival probability of young children, there are mixed results for educational outcomes with more children less likely to complete primary school. It may be that the resources of both families and schools were too severely constrained to meet the needs of the marginal children. A lesson may be that child health and education policies have to be considered jointly so that children not only survive but are also given adequate resources and opportunities to receive a decent education. Policymakers should provide additional resources to educate the marginal child and help them perform better. Provision of extra teacher similar to "balasakhi" would be a step forward in this direction (Banerjee et al., 2007).

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Table 1: Descriptive Statistics

	Sample used in Child Mortality Analysis		Sample used in Education Analysis	
Variables	Mean	S.D.	Mean	S.D.
Treated	0.75	(0.43)	0.67	(0.47)
Rural	0.69	(0.46)	0.68	(0.46)
Female	0.48	(0.50)	0.50	(0.50)
Low SES	0.47	(0.50)	0.45	(0.50)
Middle SES	0.31	(0.46)	0.32	(0.47)
High SES	0.22	(0.41)	0.22	(0.42)
ST	0.16	(0.37)	0.17	(0.37)
SC	0.17	(0.38)	0.17	(0.37)
OBC	0.38	(0.48)	0.37	(0.48)
Hindu	0.76	(0.43)	0.74	(0.44)
Muslim	0.13	(0.33)	0.14	(0.35)
Christian	0.07	(0.25)	0.07	(0.25)
Birth Order	2.37	(1.38)		
Mother Age	37.06	(3.95)		
Mother Literate	0.39	(0.49)		
Infant Mortality	0.09	(0.28)		
Under-five Mortality	0.11	(0.31)		
Literate			0.82	(0.39)
Years of Schooling			6.15	(3.39)
Primary School Completion			0.65	(0.48)
Middle School Completion			0.36	(0.48)
Secondary School Completion			0.18	(0.38)
Age			13.71	(4.17)
Number of States and UTs	35		35	
Number of Districts	561		561	
Number of Observations	297,385		898,789	

Notes: ST, SC and OBC are Scheduled Tribe, Scheduled Caste and Other Backward Caste respectively. ST and SC are historically disadvantaged group. SES is socio-economic status of the households. Different samples are used for the child mortality analysis and education analysis because the data are from different file of the RCH survey. The paper uses information from the household file for education analysis and information from fertility file for child mortality analysis. Household file has information on all the individuals who live in the house, and from the household file one women is selected (15-44 years) to be asked about her complete fertility history.

Table 2: Effect of UIP on Child Mortality

Independent variables	Pr(Infant Mortality) (1)	(2)	Pr(Under-five Mortality) (3)	(4)
Exposed to UIP	**-0.022*** **	**-0.004***	**-0.028*** **	**-0.005** **
	(0.002)	(0.002)	(0.002)	(0.002)
Female	-0.008***	−0.008***	-0.001	-0.0009
	(0.001)	(0.001)	(0.001)	(0.001)
Poor	0.032***	0.027***	0.048***	0.039***
	(0.002)	(0.002)	(0.002)	(0.002)
ST	−0.009***	0.007**	0.001	0.020***
	(0.003)	(0.002)	(0.004)	(0.003)
SC	0.006***	0.006***	0.016***	0.015***
	(0.002)	(0.002)	(0.003)	(0.002)
OBC	0.008***	0.004**	0.010***	0.005**
	(0.002)	(0.002)	(0.002)	(0.002)
Hindu	0.021***	0.010***	0.023***	0.010***
	(0.005)	(0.003)	(0.006)	(0.004)
Muslim	-0.003	-0.005	-0.002	-0.005
	(0.005)	(0.004)	(0.006)	(0.004)
Christian	-0.022***	0.008**	-0.032***	0.008
	(0.006)	(0.004)	(0.007)	(0.005)
Birth Order	0.011***	0.011***	0.015***	0.015***
	(0.0007)	(0.0007)	(0.0008)	(0.0008)
Mother's Age	−0.007***	−0.008***	−0.008***	−0.009***
	(0.0002)	(0.0003)	(0.0003)	(0.0003)
Mother's Education	−0.027***	−0.019***	-0.035***	-0.025***
	(0.002)	(0.002)	(0.002)	(0.002)
Rural	0.005***	0.010***	0.006***	0.013***
	(0.002)	(0.002)	(0.001)	(0.002)
District Fixed Effects	N	Y	N	Y
Year of Birth Fixed Effects	N	Y	N	Y
N	297,385	297,385	297,385	297,385
R Square	0.03	0.04	0.05	0.05

Notes: Each column is from estimating a separate linear probability model. Robust standard errors clustered at district level are in parentheses. Poor is a dummy indicating household with low socio-economic condition. Survey year dummy used. Scheduled Caste(SC) and Scheduled Tribe(ST)are traditionally disadvantaged minority group. OBC is Other Backward Caste. RCH district sample weights applied . * shows significance at 10-percent level, ** at 5-percent level and *** at 1-percent level.

Table 3: Heterogeneous Program Effects

Independent Variables	Pr(Infant Mortality) (1)	(2)	(3)	(4)
Exposed to UIP	**-0.003**	**0.0005**	**0.003**	**0.003**
	(0.002)	(0.003)	(0.003)	(0.003)
Exposed to UIP*Female	-0.0004			
	(0.003)			
Exposed to UIP*Rural		-0.006**		
		(0.003)		
Exposed to UIP*ST			-0.005	
			(0.004)	
Exposed to UIP*SC			-0.015***	
			(0.005)	
Exposed to UIP*OBC			-0.009***	
			(0.003)	
Exposed to UIP*Poor				-0.012***
				(0.003)
Exposed to UIP*Middle				-0.001
				(0.003)
Female	-0.008***	-0.008***	-0.008***	0.008***
	(0.002)	(0.001)	(0.001)	(0.001)
Poor	0.027***	0.027***	0.027***	0.046***
	(0.002)	(0.002)	(0.002)	(0.003)
Middle				0.013***
				(0.003)
ST	0.009***	0.009***	0.013***	0.008***
	(0.003)	(0.003)	(0.004)	(0.002)
SC	0.009***	0.009***	0.020***	0.007***
	(0.002)	(0.002)	(0.004)	(0.002)
OBC	0.004***	0.004**	0.011***	0.003*
	(0.002)	(0.002)	(0.003)	(0.002)
Rural	0.010***	0.014***	0.010***	0.007***
	(0.001)	(0.003)	0.001)	(0.001)
N	297,385	297,385	297,385	297,385
R Square	0.04	0.04	0.04	0.04

Notes: Each column is from estimating a separate linear probability model. Robust standard errors clustered at district level are in parentheses. Poor and middle are a dummy indicating household with low and medium socio economic status (SES) respectively. Each column includes Hindu, Muslim, Christian, birth order, mother's age, mother's education and survey year as controls. All regressions include district and year of birth fixed effects. Scheduled Caste(SC) and Scheduled Tribe(ST) are traditionally disadvantaged minority group. OBC is Other Backward Caste.
RCH district sample weights applied .
* shows significance at 10-percent level, ** at 5-percent level and *** at 1-percent level.

Table 4: Heterogeneous Program Effects

Independent Variables	Pr(Under-five Mortality) (1)	(2)	(3)	(4)
Exposed to UIP	**-0.003**	**0.003**	**0.005**	**0.004**
	(0.003)	(0.003)	(0.003)	(0.003)
Exposed to UIP*Female	-0.002			
	(0.003)			
Exposed to UIP*Rural		-0.011***		
		(0.003)		
Exposed to UIP*ST			-0.008*	
			(0.005)	
Exposed to UIP*SC			-0.016***	
			(0.005)	
Exposed to UIP*OBC			-0.013***	
			(0.004)	
Exposed to UIP*Poor				-0.017***
				(0.004)
Exposed to UIP*Middle				-.002
				(.003)
Female	0.0008	-0.0005	-0.0005	-0.0004
	(0.003)	(0.001)	(0.001)	(0.001)
Poor	0.039***	-0.40	-0.041***	0.064
	(0.002)	(0.002)	(0.002)	(0.004)
Middle				.015***
				(.003)
ST	0.020***	0.021***	0.027***	0.020***
	(0.003)	(0.003)	(0.005)	(0.003)
SC	0.015***	0.016***	0.028***	0.014***
	(0.002)	(0.003)	0.004)	(0.002)
OBC	0.005***	0.005**	0.015***	0.004**
	(0.002)	(0.002)	(0.004)	(0.002)
Rural	0.013***	0.023***	0.014***	0.011***
	(0.002)	(0.003)	0.002)	(0.002)
N	297,385	297,385	297,385	297,385
R Square	0.05	0.05	0.05	0.05

Notes: Each column is from estimating a separate linear probability model. Robust standard errors clustered at district level are in parentheses. Poor and middle are a dummy indicating household with low and medium socio economic status (SES) respectively. Each column includes Hindu, Muslim, Christian, birth order, mother's age, mother's education and survey year as controls. All regressions include district and year of birth fixed effects. Scheduled Caste(SC) and Scheduled Tribe(ST) are traditionally disadvantaged minority group. OBC is Other Backward Caste.
RCH district sample weights applied .
* shows significance at 10-percent level, ** at 5-percent level and *** at 1-percent level.

Table 5: Control Experiments

Independent variables	Using Older Cohorts: Pr(Infant mortality) (1)	Using Older Cohorts: Pr(Under-five mortality) (2)	Using Another Health Measure: Pr(Blindness) (3)
Exposed to UIP	**0.0003**	**0.0014**	**0.0009**
	(0.006)	(0.006)	(0.0007)
Female	-0.010***	-0.003	-0.0004
	(0.002)	(0.003)	(0.0004)
Poor	0.034***	0.054***	-0.0007
	(0.003)	(0.004)	(0.0005)
ST	0.002	0.008	-0.0028**
	(0.006)	(0.007)	(0.0009)
SC	0.009**	0.024***	-0.0008
	(0.004)	(0.005)	(0.0007)
OBC	0.005	0.004	-0.0024***
	(0.004)	(0.004)	(0.0005)
Hindu	0.008	0.015*	-0.0011
	(0.007)	(0.007)	(0.0013)
Muslim	-0.008	0.0005	-0.0023
	(0.008)	(0.009)	(0.0014)
Christian	-0.001	0.006	-0.0008
	(0.009)	(0.012)	(0.0018)
Birth Order	0.018***	0.026***	
	(0.002)	(0.002)	
Mother's Age	-0.012***	-0.013***	
	(0.0007)	(0.0008)	
Mother's Education	-0.026***	-0.037***	
	(0.003)	(0.003)	
Age			0.0051**
			(0.0017)
Rural	0.012***	0.015***	-0.0022***
	(0.003)	(0.003)	(0.0005)
Observation	88,879	88,879	432,740
R Square	0.05	0.07	0.007

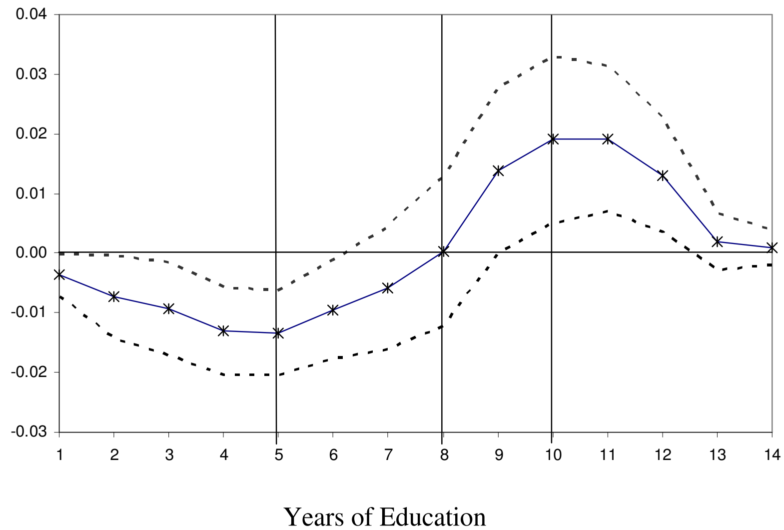
Notes: Each column is from estimating a separate linear probability model. Robust standard errors clustered at district level are in parentheses. Poor is a dummy indicating household with low socio economic status. Survey year dummy used. Scheduled Caste(SC) and Scheduled Tribe(ST) are traditionally disadvantaged minority group. OBC is Other Backward Caste.
All regressions include district and year of birth fixed effects. RCH district sample weights applied .
* shows significance at 10-percent level, ** at 5-percent level and *** at 1-percent level.

Table 6: Effects of UIP on Education Outcomes

Independent Variables	Pr(Literate) (1)	Years of Schooling (2)	Pr(Primary school Completion) (3)	Pr(Middle School Completion) (4)	Pr(Secondary School Completion) (5)
Exposed to UIP	**0.0011**	**0.0059**	**-0.0468*** **	**0.0002**	**0.019*** **
	(0.0033)	(0.0283)	(0.0036)	(0.0063)	(0.0071)
Female	-0.0923***	0.0377**	-0.0198***	0.0012	.0188***
	(0.0031)	(0.0137)	(0.0018)	(0.0022)	(.0019)
Poor	-0.1732***	-0.5835***	-0.1137***	-0.0763***	-.0266***
	(0.0041)	(0.0247)	(0.0038)	(0.0036)	(.003521)
ST	-0.1344***	-0.3277***	-0.0678***	-0.0370***	-.0179***
	(0.0071)	(0.0308)	(0.0040)	(0.0042)	(.0036)
SC	-0.0803***	-0.4399***	-0.0499***	-0.0538***	-.049***
	(0.0041)	(0.0208)	(0.0025)	(0.0031)	(.0026)
OBC	-0.0444***	-0.3072***	-0.0299***	-0.0345***	-.0337***
	(0.0031)	(0.0164)	(0.0020)	(0.0022)	(.0019)
Hindu	-0.0276***	-0.0180	-0.0057	-0.0053	-.0027
	(0.0061)	(0.0381)	(0.0040)	(0.0049)	(.0044)
Muslim	-0.1263***	-0.6178***	-0.0889***	-0.0749***	-.0375***
	(0.0077)	(0.0474)	(0.0056)	(0.0064)	(.0057)
Christian	0.0109	-0.0888	-0.0111	-0.0119	-.0037
	(0.0116)	(0.0530)	(0.0071)	(0.0063)	(.0056)
Age	0.0927***	1.0983***	0.2942***	0.0362***	-.071***
	(0.0052)	(0.0455)	(0.0061)	(0.0076)	(.0086)
Rural	-0.0252***	-0.3350***	-0.0235***	-0.0332***	-.0422***
	(0.0027)	(0.0154)	(0.0017)	(0.0021)	(.0019)
N	1099940	898789	898789	898789	898789
R-squared	0.18	0.65	0.55	0.47	0.33

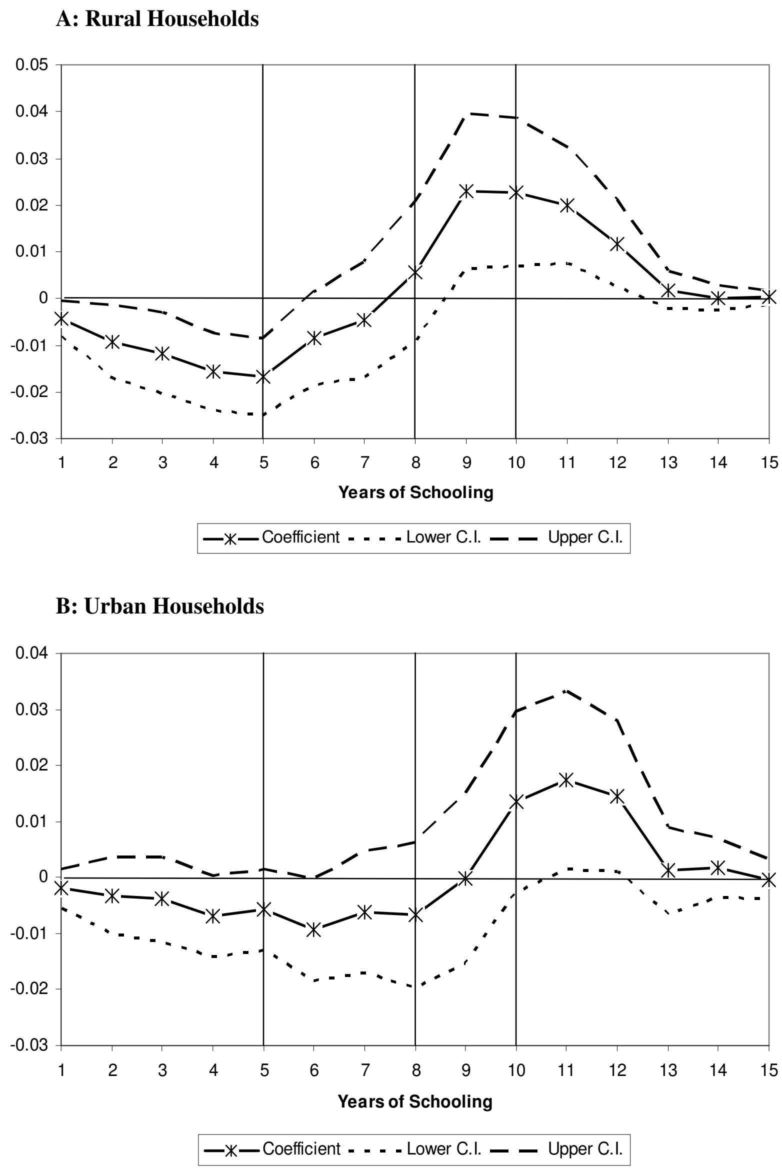
Notes: Columns (1), (3), (4), and (5) are from estimating a linear probability model. Columns (2), (3), (4) and (5) are from using Heckman Two-Step Method to correct for selectivity-bias. Robust standard errors clustered at district level are in parentheses.
Poor is a dummy indicating household with low socio economic status. Survey year dummy used. Scheduled Caste(SC) and Scheduled Tribe(ST)are traditionally disadvantaged minority group. OBC is Other Backward Caste. RCH district sample weights applied.
Primary School Completion is years of schooling $\geq$5, Middle School Completion is $\geq$8 and Secondary School Completion is $\geq$10
All regressions include district and year of birth fixed effects. * shows significance at 10-percent level, ** at 5-percent level and *** at 1-percent level.

Figure 1: Effect of UIP at Each Years of Schooling



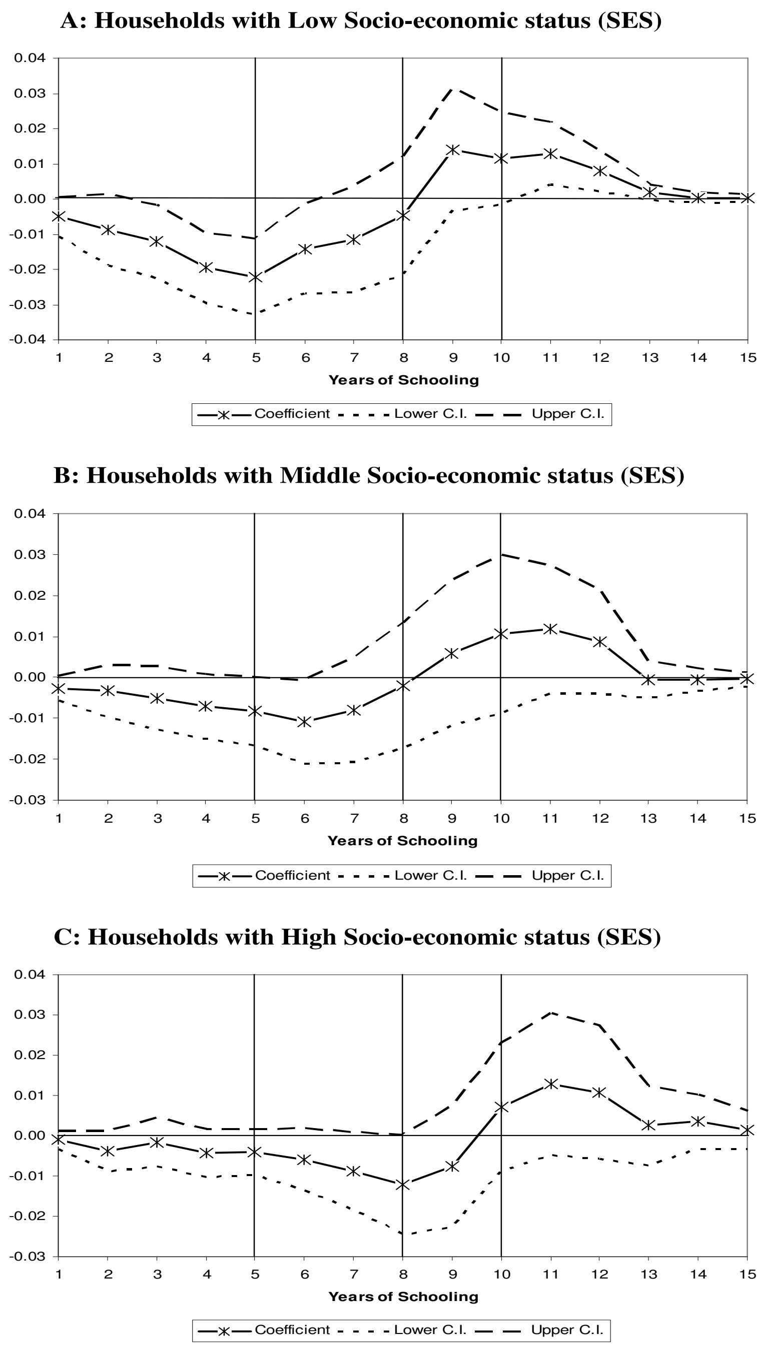
Notes: The figure shows difference-in-differences Pr(Years of Schooling $\geq$k) where k is labeled level of schooling. The broken line shows the 95-percent confidence interval. Each point is the estimated coefficient from a separate regression at each level of education. Primary school is completing grade five, middle school is completing grade eight and secondary school is completing grade 10.

Figure 2: Heterogenous Effects of UIP at Each Years of Schooling



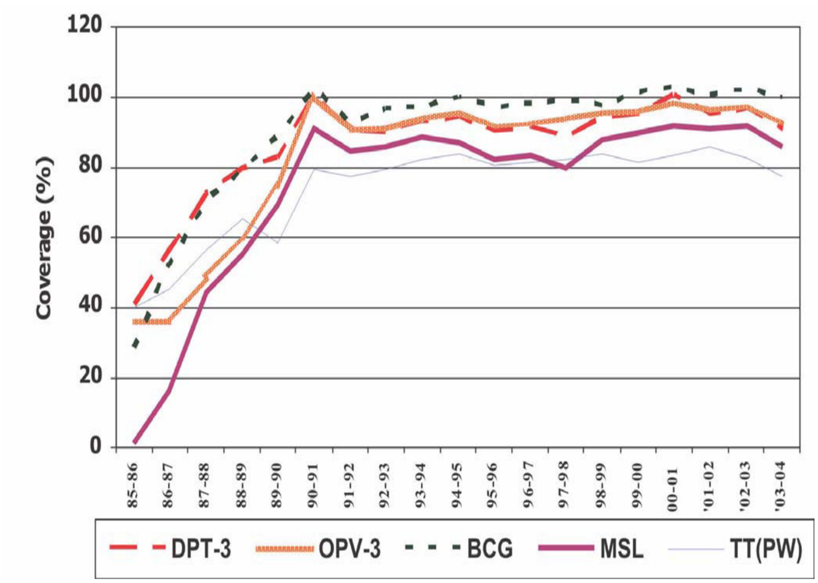
Notes: The figure shows difference-in-differences Pr(Years of Schooling $\geq$k) where k is labeled level of schooling. The broken line shows the 95-percent confidence interval. Each point is the estimated coefficient from a separate regression at each level of education. Primary school is completing grade five, middle school is completing grade eight and secondary school is completing grade 10.

Figure 3: Heterogenous Effects of UIP at Each Years of Schooling



Notes: The figure shows difference-in-differences Pr(Years of Schooling $\geq$k) where k is labeled level of schooling. The broken line shows the 95-percent confidence interval. Each point is the estimated coefficient from a separate regression at each level of education. Primary school is completing grade five, middle school is completing grade eight and secondary school is completing grade 10.

Figure 4: Reported national vaccine coverage, by antigen from 1985 to 2004



Source: Evaluation and Intelligence Division;Ministry of Health and Family Welfare (MoHFW)

Figure 5: Phase-in of Districts over Years