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# A SHOT AT ECONOMIC PROSPERITY: LONG-TERM EFFECTS OF INDIA'S CHILDHOOD IMMUNIZATION PROGRAM ON EARNINGS AND CONSUMPTION EXPENDITURE

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A Shot at Economic Prosperity: Long-term Effects of India's Childhood Immunization Program on Earnings and Consumption Expenditure
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**ABSTRACT**

Routine childhood vaccinations are among the most cost-effective child health interventions. In recent years, the broader benefits of vaccines, which include improved cognitive and schooling outcomes, have also been established. This paper evaluates the long-term economic benefits of India's national program of childhood vaccinations, known as the Universal Immunization Programme (UIP). We combine individual-level data from the 68th round of the National Sample Survey of India (2011–2012) with district-wise data on the rollout of UIP from 1985 to1990. We employ age-district fixed effects regression models to compare the earnings and per capita household consumer spending of 21- to 26-year-old adults who were born in UIP-covered districts vis-à-vis non-UIP districts between 1985 and 1990. We find that exposure to UIP in infancy increases weekly wages by 13.8% (95% CI: 7.6% to 20.3%, p<0.01) and monthly per capita household consumption expenditure by 2.9% (95% CI: 0.7% to 5.0%, p<0.01). Program exposure also reduces the probability that an individual's household relies on agriculture as the main source of income by 1.9% (95% CI: 0.0% to 3.5%, p<0.01). The findings are robust to several specifications including varying study duration and accounting for potential migration. The effects vary by sex, location, and caste group.

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A data appendix is available at http://www.nber.org/data-appendix/w30173

## 1. Introduction

Vaccination is one of the most cost-effective interventions for preventing childhood deaths, yet in 2020, 23 million children under the age of one year did not receive basic vaccines (World Health Organisation, 2021a). Vaccine-preventable diseases continue to kill approximately 700,000 children globally every year (Frenkel, 2021). Vaccination rates decreased substantially during the COVID-19 pandemic, with poorer regions of the world, which already lagged in immunization before the pandemic bearing the brunt of the burden: diphtheria, pertussis (whooping cough), and tetanus third dose (DPT-3) vaccination fell to 84% from 90% in South Asia (UNICEF, 2021a), for example. In 2020, India had the highest number of unvaccinated children, 3.5 million, up 1.4 million from the previous year (UNICEF, 2021b). The economic and health shocks from the COVID-19 pandemic and relative low priority given to health spending necessitate immediate and sustained increases in government funding for immunization.

In addition to protecting against the specific disease for which a vaccine is administered, routine childhood immunization may also have nonspecific effects such as providing broader immunity and reducing all cause-mortality (Higgins et al., 2016; Mina et al., 2019). Other benefits of vaccination include reduced out-of-pocket medical expenses; reduced antimicrobial resistance; and improved long-term health, cognition, and schooling outcomes (Bärnighausen et al., 2014; Benn et al., 2013; Bloom et al., 2018; Nandi and Shet, 2020; Sevilla et al., 2018). The life-long benefits of vaccines are consistent with the fetal origins hypothesis, which posits that infectious disease episodes in the first two to three years of life can damage the long-term growth and cognitive development of children (Almond et al., 2018; Almond and Currie, 2011; Currie and Vogl, 2013). Interventions that prevent disease transmission or cure infections—vaccines, clean water, sanitation, drugs—may therefore also improve long-term health, learning, and economic outcomes.

Although the immediate health benefits and cost-effectiveness of vaccines are well established, the potential long-term human capital development benefits, such as cognitive gains or improved schooling, have been studied only to a limited extent. Such analysis requires longitudinal or long-term data that until recently were available mainly in high-income countries. At the same time, high-income countries have had near universal coverage of routine childhood vaccines for many years, and thus the control group for long-term vaccine benefit studies may not be large enough for analysis. In low- and middle-income countries (LMICs), a few studies have linked the receipt of measles and *Haemophilus influenzae* type B (Hib) vaccines with 0.1 to 0.2 points higher anthropometric z-scores, 1.7 to 4.5 percentage points higher standardized test scores, and an additional 0.2 to 0.3 years of schooling in Ethiopia, India, South Africa, and Vietnam, and a 7% increase in the male school enrollment rate in Bangladesh (Anekwe et al., 2015; Driessen et al., 2015; Nandi et al., 2019b, 2019a). Full vaccination status or exposure to national immunization programs in early childhood has similarly been linked with higher schooling attainment or improved cognitive test scores in China, India, and the Philippines (Arsenault et al., 2020; Bloom et al., 2012; Joe and Kumar Verma, 2021; Nandi et al., 2020b; Oskorouchi et al., 2020). Although the benefits of childhood vaccination on cognitive outcomes and schooling attainment have recently been quantified, estimates of the long-term economic effects of immunization in LMICs are not available.

This paper examines the long-term effects of India's routine childhood vaccination program, known as the Universal Immunization Programme (UIP), on wages, consumption expenditure, and primary source of income. We combine data on the district-wise implementation of UIP from 1985 to 1990 with household and individual socioeconomic characteristics, wages, and consumption data drawn from the National Sample Survey of India 2011. We employ age-district fixed effects regression models to estimate the effect of exposure to UIP in the first year of life on the wages and consumption expenditure of 21- to 26-year-old adults who were born during the five-year implementation phase. We find that those born after their district had UIP coverage have 14% higher wages and 3% higher consumption expenditure than those who were born before the program was implemented. Households of individuals who were treated under the program are also 2% less likely to rely on agriculture as their primary source of income (based on household head's occupation). Treatment effects vary substantially by sex and socioeconomic groups, and the findings are robust to several alternative specifications and variations in study periods.

## 2. Potential pathways of long-term effects of vaccines

Vaccination can improve long-term health and economic outcomes via multiple pathways. The *fetal origins* hypothesis, first proposed by David Barker in 1990 and widely researched and accepted later, is the main theoretical pathway (Almond and Currie, 2011; Barker, 1990; Calkins and Devaskar, 2011). Following this theory, stimuli in the first two to three years of life—disease or another stressor, for example—elicit a biological response that can alter "the structure and function of various organs" (Calkins and Devaskar, 2011). Repeated episodes of infections can hinder physical growth and reduce adult height, which in turn has been linked with worse economic productivity (Currie and Vogl, 2013). Children attain 80–90% of their adult brain size within the first three years of life, and disease exposure during this phase can permanently alter brain development and reduce cognitive functioning (Fox et al., 2010; Grayson and Fair, 2017). Furthermore, seasonal influenza and other influenza-like illnesses, pneumonia, and diarrheal infections can result in school absenteeism, affecting longer-term learning and educational outcomes (Allison et al., 2019; Bhalotra and Venkataramani, 2011; Farha and Thomson, 2005).

Childhood vaccines can mitigate adverse long-term outcomes by preventing episodes of the specific disease that a vaccine targets. In addition, recent evidence shows that the measles vaccine can provide broad immunity against all infections. An episode of measles can reduce the innate immunity of children for a period of up to two years, making them vulnerable to other diseases (Mina et al., 2019). A live attenuated measles vaccine can induce immune memory and reduce other infections (Pollard et al., 2017; Sørup et al., 2014). The protection provided against measles infection and the broader immunity gain could translate into improved brain development, schooling, and economic outcomes (Anekwe et al., 2015; Atwood, 2021; Driessen et al., 2015; Nandi et al., 2019b).

A large body of literature finds improved schooling outcomes from decreased disease exposure. Analysis of in utero exposure to the 1918 influenza pandemic in Taiwan finds that a 0.1% increase in the maternal mortality rate[2] reduces years of school completed by 2.5% (Lin and Liu,

[2] Maternal mortality rate at the region-year level was employed as a proxy for exposure to the pandemic.

2014). Nelson (2010) also looks at in utero exposure to the influenza pandemic and finds that those born in 1919 were 13% less likely to graduate from college and had 0.04 fewer years of schooling than those born in other years between 1912 and 1922 in Brazil. An unpublished study finds that those exposed to the influenza pandemic in utero were 1% to 1.5%[3] less likely to graduate from high school in the United States (Beach et al., 2018). Analysis of a deworming program for school-aged children in Kenya, where drugs were randomly phased into schools, estimates that the direct effect (excluding externalities created by the program) increased overall school participation by 0.14 years per pupil treated, but no effect is found on standardized test scores (Miguel and Kremer, 2004). Ozier (2018) also looks at a school deworming program in Kenya and finds that children under the age of one who lived in communities where the deworming program was implemented have 0.5 to 0.8 additional years of schooling. Bleakley (2007) analyzes the hookworm eradication campaign implemented in the American South in the 1910s and finds that a child who was infected with hookworm disease attended on average 2.1 fewer years of school than an uninfected child. A study analyzes the effect of malaria exposure in Paraguay and Sri Lanka where malaria eradication programs were implemented and finds that a 10% decrease in malaria incidence results in a 0.1-year increase in completed schooling and increases the probability of being literate by 1% (Lucas, 2010). Finally, in an unpublished study, Bhalotra and Venkataramani (2013) estimate that a one standard deviation decrease in childhood diarrhea mortality leads to a 0.1 standard deviation increase in test scores of girls.

In addition to schooling outcomes, a few studies examine the link between diseases and labor market outcomes, consumption, or economic growth. A study on early-life malaria exposure finds that boys covered by a malaria eradication program in the most malarious states of India have increases in household per capita expenditure of approximately 2% in adulthood (Cutler et al., 2010). Baird et al. (2016) find that exposure to a deworming program in school-aged children in Kenya leads to log wage increases of 19.7 points 10 years after the intervention. Analysis of the effects of malaria eradication programs in Brazil, Colombia, Mexico, and the United States finds that childhood infection with malaria reduces adult income by 50% (Bleakley, 2010). Beach et al. (2016) find that eliminating early life exposure to typhoid fever increased income by 1% in later life in the United States. Nelson (2010) finds that in utero exposure to the 1918 influenza pandemic in Brazil led to an 8.6% lower likelihood that individuals born in 1919 would have formal employment than those born in other years between 1912 and 1922.

Our paper makes several important contributions to the literature on the long-term economic effects of health interventions. First, studies estimating the economic benefits of vaccines address short-term forgone medical expenses (including cost savings to health care providers), financial risk protection (e.g., value of insurance), and the monetized value of health gains (e.g., value of statistical life years) due to reduced disease incidence (Megiddo et al., 2014; Ozawa et al., 2017, 2016; Riumallo-Herl et al., 2018). To the best of our knowledge, this is the first study of the long-term economic benefits of vaccines in LMICs, and one that evaluates the human capital development aspect of vaccines (Nandi and Shet, 2020). Previous studies of human capital development analyze only the link between vaccines and standardized test scores or schooling attainment, not earnings or consumption. To the best of our knowledge, Atwood (2021) is the only study to estimate a 1.1% increase in future income due to measles vaccinations, but in the context of the United States and not LMICs. A large volume of published work links nutrition or

[3] This is the effect found for the average level of pandemic intensity.

undernutrition, famine, air pollution, diseases, and war in early childhood with later-life economic outcomes (Currie and Vogl, 2013), and the literature on diseases focuses heavily on malaria eradication and deworming; little is known about the long-term effects of vaccines or other low-cost preventive health interventions.

Second, we contribute substantially to the understanding of how large-scale public health programs can aid long-term human capital development and economic growth in India and potentially other LMICs. Although previous studies have examined the educational benefits of India's national nutrition and early childhood development program (the Integrated Child Development Services) (Nandi et al., 2020a, 2017); economic outcomes remain inadequately studied.

Last, our study informs the policy discussion surrounding universal routine childhood immunization coverage in India and other LMICs. In the past two decades, routine childhood vaccination coverage increased from 50% to 80% in LMICs, followed by a sharp reduction due to the ongoing COVID-19 pandemic (World Bank, 2021). Additional challenges such as vaccine hesitancy have also become significant in the past few years. Our findings can help reinforce and revitalize the drive for universal immunization and ensure sustained efforts in future years, even when major childhood diseases are on the verge of eradication.

## 3. Background on the Universal Immunization Programme

The Universal Immunization Programme was launched in 1985 and implemented in phases; all districts were covered by 1990 (Pradhan, 2010). Prior to launch, some routine childhood vaccines were administered before 1985 but mainly in urban areas and with negligible coverage rates (Lahariya, 2014). The measles vaccine—a key vaccine with potential long-term benefits—was not provided before 1985 because it was not yet available in India.

Figure 1 shows the rollout of UIP by district. Initially, the program administered vaccines for six diseases: diphtheria, pertussis, and tetanus (DPT); measles; polio; and Bacillus Calmette-Guérin (BCG) for tuberculosis (Pradhan, 2010). UIP aimed for full coverage of 85% of infants by March 1990 (Lahariya, 2014). By the time the program was fully implemented, vaccination rates had risen considerably although they were not universal. Historical data starting from the 1980s are available for the DPT-3 vaccine which is commonly used as a performance measure of national immunization programs, and the measles vaccine which is known to indirectly protect children against other childhood illnesses (Mina et al., 2019, 2015; World Bank, 2021). Figure 2 presents the trends in coverage rates during 1980 to 2000 in India. Aided by the UIP, national coverage of the DPT3 vaccine increased from 6% in 1980 to 70% in 1990, with a clear break in trend starting in 1986, which was the first year of UIP implementation (1985-1986). Similarly, coverage of the measles vaccine increased from 1% in 1985 (first year of available data) to 56% in 1990. Since the mid-1990s, vaccine coverage rates have varied by year but have consistently remained above 50%.

UIP is among the largest immunization programs in the world, attempting to vaccinate an annual cohort of 26 million children with a budget of $2 billion (Chatterjee et al., 2016). A major success of the program was the 2014 elimination of polio through a special campaign that immunized 170 million under-five children (Deutsch et al., 2017). Despite UIP's achievements, coverage has yet to be universal. DPT-3 coverage among 12-23-month-old Indian children was 85% in 2020 (International Institute for Population Sciences, 2021). To address delayed and missed vaccination, the Indian government recently implemented additional vaccination campaigns known as Mission Indradhanush and Intensified Mission Indradhanush, which together raised immunization rates (Clarke-Deelder et al., 2021; Summan et al., 2021). Currently, UIP provides vaccination for polio (oral polio vaccine); DPT; BCG; measles; hepatitis B; Hib containing pentavalent (DPT, hepatitis B, and Hib); inactivated polio vaccine; tetanus toxoid; and, in endemic areas, Japanese encephalitis.

## 4. Data and descriptive statistics

### *4.1. Data sources*

Data primarily come from two datasets. National Sample Surveys (NSS) are routine, nationally representative surveys that collect data on an exhaustive set of socioeconomic characteristics. NSS round 68 (NSS-68) was conducted between July 2011 and June 2012 and contains the following outcome variables: wages, monthly per capita expenditure, and income source (agriculture versus nonagriculture) of household. NSS-68 collected data on 456,999 individuals from 101,724 households in 626 districts. The data for the year of UIP implementation—our treatment variable—were taken from our previous work (Nandi et al., 2020b). We reviewed district bifurcations and creation of new districts and states and carefully matched the districts in NSS-68 data retrospectively with the phased district-wise rollout of UIP from 1985 to 1990. All control variables come from NSS-68 data except for the probability of being a migrant, which is predicted using the National Family Health Survey 4 (International Institute for Population Sciences, 2017), discussed further in a later section.

### *4.2. Outcome variables and sample selection*

Our main outcome variable from NSS-68 is log weekly wages, measured in Indian rupees. Our sample includes individuals who were in the job market and had wage information. Because UIP implementation started in 1985 and ended by 1990, we include people born between 1985 and 1990 in our main analysis sample. Those born in this period would be between the ages of 21 and 26 when surveyed in 2011–2012. We select this study sample because in a sample with a wider age range, factors other than the immunization program could affect wages. The likelihood of major generational economic and educational reforms increases with time and could affect wage growth among individuals. Therefore, someone born in 1995 and in the job market (at age 16) at the time of the survey may not have had access to the same education system and labor market as someone born in 1980 (age 31 at the time of the survey). Individuals who were attending school or enrolled in institutes of higher education in 2011–2012 are excluded from the analysis; this situation applies to 9% of our 21- to 26-year-old sample. Out of employed individuals, we include those who had salaried employment (33% of the sample) and exclude those who were self-employed or employed in a household enterprise because the survey did not collect wage data for them.

We also examine the log of monthly per capita expenditure (MPCE) as an outcome variable measuring standard of living. A special NSS module collects data on various goods and services consumed by the household. For common monthly expenditures such as food, personal care items, entertainment, and rent, the household is asked what it spent over the previous 30 days. For expenditures on durable goods, furniture, household items, and school fees, the household is asked for its spending estimate over the previous 365 days. This amount is divided by 12 and added to the monthly estimate to yield the total monthly household consumption expenditure. This number is then divided by the number of members in the household to arrive at the monthly per capita expenditure. We use MPCE data for the full sample, irrespective of employment status.

Finally, we examine household income source as an outcome variable. Specifically, we are interested in knowing whether the household receives income from agricultural or nonagricultural sources. Employment in agriculture could be less secure and less desirable than formal employment in other sectors. This variable was assigned a value of 1 if the head of the household depended primarily on agriculture for income and 0 if nonagricultural income was the primary source. Therefore, even those who did not have salaried work were included in this sample, unlike the analysis of wages.

### *4.3. Assignment of treatment status*

UIP focuses on vaccination of infants (under the age of one year), following World Health Organization recommendations during the 1980s (World Health Organisation, 2021b, 2020, p. 3). For each district, residents born either after or during the year of UIP implementation in that district are included in the treatment group: they were potentially vaccinated by UIP during the first year of life. People living in districts without UIP by their birth year are in the control group. These data are not available as exact dates or months, so we assign treatment status based on the year of birth and the year of UIP implementation. UIP implementation proceeded in five one-year phases, starting in 1985–1986 and ending in 1989–1990 (see Figure 1). We used the endpoint of this data range as the year of program implementation. For example, 1985–1986 districts were coded as having the program implemented in 1986, and 1989–1990 districts were coded as having the program implemented in 1990.

### *4.4. Effect of migration*

NSS-68 has data on individuals' current district of residence but not their district of birth. Cutler et al. (2010) suggest that current residence status often indicates birth location because of the low migration patterns in rural India. In 2011, at the time of NSS-68, 73% of the Indian population was rural, and out-of-district migration was only 15% (Office of the Registrar General and Census Commissioner, 2011). However, we approach incorporating the potential effect of migration more thoroughly by conducting additional analysis using data from the National Family Health Survey, round 4 (NFHS-4).

NFHS-4 is a nationally representative cross-sectional survey conducted between 2015 and 2016. It surveyed 2.87 million individuals in 601,509 households in all states and union territories of India. Unlike NSS-68, NFHS-4 collected information on migration status (whether someone had lived in the same location since birth) of a subsample of adult men and women. First, we conduct a probit regression of migrant status in NFHS-4 (a person had not lived in the same location since birth) on a vector of background characteristics of individuals (born in 1985–1990), including sex, age, relationship to household head, age of household head, marital status, caste, religion, household size, and wealth quintiles. Then, using the estimated coefficients from this regression and the same set of explanatory variables from NSS-68,[4] we predict the probability of migration for NSS-68 individuals. Our model uses the predicted probability as an explanatory variable, as discussed in Section 5. Additional robustness checks that exclude potential migrants are also conducted, as discussed in Section 5.6.

### *4.5. Background characteristics of the treatment and control groups*

Table 1 shows the major socioeconomic and demographic characteristics, by control and treatment groups, for individuals whose birth year is between 1985 and 1990 (21- to 26-year-olds). Wages are significantly higher in the control group (INR 1,557 vs. INR 1,279 mean value, p<0.01), primarily due to outliers. Median wages were not statistically different between the two groups. UIP-covered households are more likely to have agriculture as the primary income source (23% vs. 25%, p<0.01). The control group is on average 2.23 years older than the treatment group (25.02  vs. 22.79 years, p<0.01). The age difference may also explain the higher rate of marriage (53% vs. 34%, p<0.01) in the control group. Another significant difference between the treatment and control groups is the mean probability of being a migrant (96% vs. 95%, p<0.01). The control group has a larger proportion of graduates (4% vs. 2%, p<0.01) and a smaller proportion of those who completed secondary education (14% vs. 20%, p<0.01). More people in the control group reside in the western and southern regions and fewer are from the central region. This would mean that the UIP program rolled out earlier in the central region (see Figure 1). Also, the control group included significantly more Hindu and Christian households and fewer Muslim and Sikh households.

## 5. Empirical strategy

### *5.1. Testing for selective program placement*

No administrative data are available on the selection criteria for districts in each UIP phase. An earlier study posited that UIP rollout was prioritized in districts with higher levels of health infrastructure and capacity to vaccinate but did not provide any evidence (Kumar, 2009). We systematically test for selective placement of UIP using additional village and district level data. The 1991 Census of India was the first national census to publish data on demographic indicators (e.g., age and sex distribution) and infrastructure (e.g., availability of a primary health center or electricity) for all 634,000 Indian villages. We matched the UIP rollout data with the district indicators in Census 1991 data to determine the phase of UIP rollout for each village (assuming that a village was covered when its parent district was covered). Then, we examine if the village characteristics can predict early rollout sufficiently. We estimate the following probit model:

[4] NFHS-4 collected data on assets such as televisions, radios, and cars, and housing condition indicators such as quality of roof and number of rooms in the house. Using these indicators, we create a wealth index in the spirit of Filmer and Pritchett (2001). We divide the wealth index into five quintiles. In NSS-68, we consider MPCE quintiles (no data on assets were available) as equivalent to NFHS wealth quintiles.

$$PhaseY_k = \alpha_0 + \alpha_1 x_k + \theta_s + u_k \tag{1}$$

where $PhaseY_k$ is the binary indicator of whether the district containing village $k$ was selected in the *Y-th* phase of UIP. Analysis is conducted separately for the first two phases of UIP (Y=1 or Y=2). In the first model, phase 1 villages are compared with the remaining villages of India. In the second model, phase 2 villages are compared with the remaining non-UIP villages (excluding phase 1 villages). The covariate set *x* includes a series of village-level indicators such as log of population by age and sex, male and female literacy rates, share of socioeconomically disadvantaged groups (known as scheduled caste and scheduled tribe), and availability of different types of health care facilities (e.g., primary health center or sub-center), community health worker, private doctor, drinking water, paved road, and electricity. State fixed-effects (denoted by $\theta_s$ for the *s-th* state) are also included.

We also repeat this analysis at the district level using the following probit model:

$$PhaseY_d = \beta_0 + \beta_1 \bar{x}_d + \theta_s + e_d \tag{1A}$$

where $PhaseY_d$ is the binary indicator of whether the district *d* was selected in the *Y-th* phase of UIP (Y=1 or Y=2). $\bar{x}_d$ indicates the district level mean values of the covariate set *x*. Analysis is done for 20 major states of India using Census 1991 data obtained from Nandi and Deolalikar (2013). Standard errors of all models are clustered at the district level.

Appendix Table A1 presents the results from these regression models. We find that village health infrastructure is generally not associated with selection into UIP phases 1 or 2. Out of nine health indicators, the availability of a private doctor is positively linked with selection in phase 1 in the village level analysis but negatively linked in the district level model. Similarly, availability of a maternity home or family welfare center show some statistically significant associations with selection in phases 1 or 2, but no clear pattern emerges across regression models. Except for one case, the estimated coefficients of other demographics and infrastructure indicators are also statistically insignificant. Considering that improving health and other physical infrastructure is an expensive and slow process, we argue that the phase-wise UIP rollout was not determined by the underlying health system's capability of districts. Additionally, UIP rollout was also not associated with schooling infrastructure, reducing the possibility of unobserved biases (e.g., districts that are prioritized for UIP also receive greater schooling inputs that can affect future labor market outcomes).

### *5.2. Main model specification*

Characteristics of districts unobserved in the 1991 Census data, e.g., disease prevalence, population density, transportation infrastructure, or political factors, may still be associated with UIP rollout. Systematic differences between the treatment and control districts could bias ordinary least squared estimates of the effect of UIP coverage on labor market outcomes. Differences between the groups could also evolve over time. To account for such potential biases, we employ an age-district fixed effects model that incorporates household and individual characteristics and district-and-time-varying factors. Our fixed effects log wage regression model takes the following form:

$$log(w_{i,j}) = \beta_0 + \beta_1 UIP_{i,j} + \beta_2 X_{i,j} + \partial Age_i \times District_j + \epsilon_{i,j} \tag{2}$$

where $w_{ij}$ are wages observed in 2011-2012 for individual $i$ in district $j$, $UIP_{i,j}$ is a binary variable equal to 1 if UIP was implemented before or during the birth year of individual $i$ and 0 otherwise, $Age_i$ is age of individual $i$ and $District_i$ is the current district of individual $i$, and $Age_i \times District_i$ is the vector of dummy variables for age and district fixed effects. The source of variation at the individual level is from the year of birth, controlling for district and age (people of same age but born in different years).

Similarly, the regression model for MPCE is as follows:

$$log(MPCE_{i,j}) = \beta_0 + \beta_1 UIP_{i,j} + \beta_2 X_{i,j} + \partial Age_i \times District_j + \epsilon_{i,j} \tag{3}$$

We estimate the probability that a household relies on agriculture using a fixed effects linear probability model:

$$\Pr(Agri_{i,j}) = \beta_0 + \beta_1 UIP_{i,j} + \beta_2 X_{i,j} + \partial Age_i \times District_j + \epsilon_{i,j} \tag{4}$$

Where $\Pr(Agri_{i,j})$ indicates the probability that individual *i* is in an agriculture-supported household. In all three models, the vector $\boldsymbol{X_{i,j}}$ consists of control variables commonly found to affect wages: locality (urban vs. rural), caste, sex, religion, household size, and education level. We also include the following: whether household head is female, education of household head, relationship to household head, and the predicted probability that the individual is a migrant.

We include education of household head to account for intergenerational transfer of resources. Because education is highly associated with income and wealth, higher education of the household head may mean greater transfer of resources to dependents and offspring, equivalent to greater investments in education and health. Alternatively, for poorly educated and low-income households, children may need to enter the job market earlier to support their families rather than invest in their own human capital.

Relationship to the household head is included as a measure of intrahousehold resource allocation. The relative position of a child in a household may affect resource allocation in early and later life, which can in turn affect wages. For example, with limited resources, some households in India invest more in the education and health of boys (Barcellos et al., 2014; Oster, 2009). We also include an indicator of whether the household head is female. Female-headed households often differ socioeconomically from male-headed households, including being poorer (Meenakshi et al., 2000; Meenakshi and Ray, 2002). Finally, the probability of being a migrant, as discussed in Section 4.4, is included in *X*. We cluster all standard errors at the district level.

*5.3. Consideration of benefits of vaccination for older cohorts*

In 2016, 24%, 29%, and 23% of Indian children between the ages of 10 and 23 months had delayed vaccination of measles, DPT-1, and BCG, respectively; where delay is defined as receiving the vaccine at least 28 days after the recommended eligibility age (Choudhary et al., 2019). Delayed vaccination can occur for many reasons: extreme weather may prevent families from reaching vaccination sites, for example, or logistical issues may interrupt the vaccine supply chain. Implementation of UIP may not have been perfect during the early years—as evidenced by the less-than-universal coverage after full program rollout—and delays may have been common. However, even if delayed, vaccination may still have long-term benefits. Although the focus of UIP is vaccination during the first year of life, per the recommended schedule, infants living in districts where UIP was implemented later may have received delayed vaccinations. Although vaccines should be administered close to the recommended schedule, most vaccines do not have an upper age limit (World Health Organisation, 2021b), and the program could have administered "catch-up" vaccinations.

UIP may also have benefited nontarget cohorts through a second pathway in which reduced disease transmission from other vaccinated children in the household or the neighborhood protected unvaccinated children (Basta et al., 2009; Loeb et al., 2010; Longini and Halloran, 2005). Vaccination of younger siblings has been previously shown to provide protective effects to unvaccinated older siblings and other older members of the household (Diaz et al., 1991; King et al., 2006; Zielinski et al., 2003). To test these pathways, we consider late or partial exposure of children to UIP by using three alternative definitions of treatment status. We repeat our analysis with treatment status variables based on whether UIP was implemented one, two or three years, respectively, after the birth year, and code 0 otherwise.

### *5.4. Parallel trends analysis*

The validity of our empirical strategy depends on the parallel trends assumption—that is, time trends in the outcome variable should be similar between UIP and non-UIP districts in years leading up to UIP implementation, even though the levels could be different. To test for parallel trends, we first divide our sample into four pairs of treatment and control group combinations based on the year of implementation: (i) individuals from districts in which UIP was introduced by 1986 (treatment) versus all other districts (control); (ii) individuals from districts in which UIP was introduced by 1987 (treatment) versus all other districts, excluding the 1986 UIP districts (control); (iii) individuals from districts in which UIP was introduced in 1988 (treatment) versus all other districts, excluding the 1986 and 1987 UIP districts (control); and (iv) individuals from districts in which UIP was introduced in 1989 (treatment) versus all other districts, excluding the 1986, 1987, and 1988 UIP districts (control). By the end of 1990, all districts had UIP, and no control group remained.

Then, we test for parallel trends for each treatment-control pair in two ways. First, we estimate the average annual residual log wages of those born between 1975 and the year before UIP introduction (e.g., born in 1975–1984 during the first subset before UIP was implemented in 1985–1986, born in 1975–1985 during the second subset, and so on), controlling for district fixed effects. Appendix Figures A1–A4 present the trends in residual log wages, separately for future treatment and control groups (e.g., those born in 1975–1984 separately in districts that will have UIP in 1985–1986 versus the remaining districts). We find that leading up to the introduction of UIP, the trends were similar across treatment and control groups in each of the four analysis subsamples, satisfying the parallel trends assumption.

Second, separately in each of the four data subsets, we regress log wages on the covariate set $X_{i,j}$, an indicator for the future treatment group (e.g., 1986 UIP districts in case of the first subset of 1975–1984 data), identifiers for year of birth, and interaction terms between year and the treatment indicator. The estimated coefficient of the future treatment and birth year interaction indicates whether wages were statistically different between the treatment and control groups year by year leading up to the introduction of UIP, controlling for observable characteristics of individuals. Appendix Figures A5–A8 present the estimated coefficients along with their 95% confidence intervals. Generally, no statistical differences appear in the year-by-year trends in wages between treatment and control groups in all four analysis subsets before UIP implementation in those districts, validating the parallel trends assumption. Parallel trend test results for MPCE and occupational choice are similar and therefore not presented separately.

### *5.5. Cohort size variations: selective mortality and fertility*

Following Araujo et al. (2019), who evaluate the long-term benefits of an iodine supplementation program in Tanzania, we test for selection biases in the study sample by examining cohort sizes. First, UIP may affect parental fertility choice: some parents may have chosen to delay childbearing until after UIP was implemented in their home district. These parents could be richer and more knowledgeable about public health programs and in turn may invest more in the human capital development of their children. This potentially creates an upward bias in the future wages of their children. A second issue is that death rates, especially from vaccine-preventable diseases, may be higher among children in the control group. As a result, the treatment group may have more “weaker” children who survive but have lower health and human capital than the control group children who went through “survival of the fittest,” resulting in a possible downward bias in wages for the treatment group. We evaluate these issues by comparing the cohort sizes in NSS-68 in UIP 1985–1986 districts vis-à-vis other districts during 1975–1988. We exclude other treatment groups—that is, those born in UIP districts post 1985–1986—to keep the control group uncontaminated. The results, presented in Appendix Figure A9, show that the relative cohort sizes between the treatment and control group follow a parallel trend, and no divergence occurs after the introduction of UIP in 1985–1986. This implies that bias due to selective fertility or mortality in the study sample is unlikely.

### *5.6. Placebo tests*

We conduct placebo tests to gauge the validity of our findings. Children born several years (e.g., more than 5 years) prior to the 1985-1990 implementation of the UIP would not have received any vaccines since the target group of UIP was children under the age of one year. As a result, the long-term labor market outcomes of such older children should not be different between UIP and non-UIP districts. To examine this, we consider two additional subsamples of adults who were born during 1975-1980 and 1970-1975. We evaluate the effect of UIP on these cohorts using the age and district fixed-effects models described previously.

### *5.7. Robustness checks and treatment heterogeneity*

We conduct additional robustness checks. First, we exclude from our sample individuals who were most likely to be out-of-district migrants. We do this only for those born between 1986 and 1990, because migrants born before 1986 would all be in the control group and individuals born after 1990 would all be part of the treatment group. The 2011 Census of India estimates that among urban male, urban female, rural male, and rural female populations, 24%, 30%, 4%, and 15%, respectively, were out-of-district migrants (Office of the Registrar General and Census Commissioner, 2011). We divide our data into these population subgroups and exclude the corresponding top part of the predicted probability distribution of being a migrant. For example, for urban males, we exclude those with values in the top 24% of the predicted probability distribution, and for rural females, we drop the top 15%. The exclusion of these observations is imperfect because being a migrant does not necessarily mean that treatment status is inaccurately assigned. For example, if individuals born in 1988 in a district where UIP was implemented in 1988 migrated to another district where implementation occurred between 1985 and 1988, their assigned treatment status would be correct despite their status as migrants and we would lose valid observations. However, conducting the analysis based on a sample of individuals least likely to be migrants was the best method to test for the robustness of our results.

Second, we examine whether our choice of study period matters. We consider two additional groups—those born between 1985 and 1995 (21- to 31-year-olds) and those born between 1980 and 1995 (16- to 31-year-olds)—and repeat our analysis. Third, we exclude education control variables. With education controls, our coefficient of interest measures differences in vaccination between UIP-exposed and non-exposed individuals with the same level of education. However, schooling attainment itself may be a function of UIP exposure (Nandi et al., 2020b).

Finally, in addition to the robustness checks, we conduct heterogeneity analyses by gender, location (rural vs. urban and high-focus states vs. low-focus states), caste (scheduled caste, scheduled tribe, and other backward classes), occupation (salaried workers only), and religion (Hindu and non-Hindu). The Indian government designates the states of Assam, Bihar, Chhattisgarh, Jharkand, Madhya Pradesh, Orissa, Rajasthan, Uttar Pradesh, and Uttaranchal as high-focus states (HFS) due to high levels of fertility and child mortality, while the remaining states are considered to be low-focus (LFS).

## 6. Results

### *6.1. Effect of UIP exposure on economic outcomes*

Tables 2–4, models 1A–1C, present the coefficient of interest—the effect of UIP coverage on wages, MPCE, and income source, respectively. Appendix Table A2 provides the full model results for wage outcome, Appendix Table A3 for MPCE outcome, and Appendix Table A4 for household income source outcome. We find that exposure to UIP in infancy increases wages by 14% (95% CI: 8%–20%, p<0.01) in the principal model for those born between 1985 and 1990. This result is insensitive to changing the sample's age group, with models 1B (born 1980–1995) and model 1C (born 1985–1995) having identical coefficients. We find that UIP exposure in infancy increases MPCE by 3% (95% CI: 1%–5% , p <0.01) in model 1A for those born between

1985 and 1990, and this result is consistent in models 1B and 1C. Finally, we find that individuals exposed to UIP have a 2% (95% CI: 0%–4% , p<0.05) lower probability of being in households primarily supported by agriculture. These results are consistent in models 1B and 1C.

*6.2. Benefits of vaccination for older cohorts*

In Tables 2 and 3, models 2A–2C, we redefine the treatment variable, where those born one year before UIP implementation are considered to have received (weak) treatment as well. We find individuals who are partially exposed to UIP have wages only 7% higher (95% CI: 0%–15% , p <0.01) than those in the control group in model 2B (born 1985–1995). For MPCE outcomes the results are similar to those with exposure only at birth. In models 2A and 2C, expenditure increases by 4% (95% CI: 1%–6%, p>0.01). Appendix Tables A2 and A3 show the full model results. In Appendix Table A5, we present longer delays in exposure to UIP for only five-year birth cohorts. Specifically, we consider children exposed to UIP two and three years after birth as receiving treatment in these two sets of models. We find positive effects on wages for those born two years before UIP of 8% but not for those born three years prior to UIP. Those with delayed exposure two and three years after birth have a 5% and a 4% increase in MPCE, respectively. No significant effects are found on the household income source outcome variable.

*6.3. Exclusion of migrant populations*

As a robustness check, we excluded the proportion of the sample within each sex-locality group that was most likely to be migrant, using predicted probabilities of being a migrant based on 2011 census rates of out-of-district migration. We find that the coefficients in all models remain the same, and the results are insensitive to exclusion of migrant populations for samples born in 1980–1995 and 1985–1995. Appendix Tables A2 and A3 provide the full results.

*6.4. Heterogeneity across population groups*

Models 4A–13A in Tables 2 and 3 show the coefficient of exposure on treatment status with age-district fixed effects for those born between 1985 and 1990, by subsample group. Appendix Tables A6–A13 provide the full model results. We see that the effect of UIP exposure during infancy differs for various subsamples. For rural, male, scheduled caste or scheduled tribe, and Hindu households, UIP exposure during infancy has a significant and positive effect on wages. Individuals residing in rural areas and males who were exposed to UIP at birth have 14% (95% CI: 5%–23% , p<0.01) and 16% (95% CI: 9%–23%, p<0.01) higher wages than the control group, respectively. However, program exposure has no effect on urban, female, other backward caste, and non-Hindu individuals. Individuals exposed to UIP in high-focus states had 21% higher wages (95% CI: 7%–38%, p<0.01) and in low-focus states had 12% higher wages (95% CI: 5%–19%, p<0.01) than individuals in control groups. For the MPCE outcomes, we find that the coefficients are similar to the complete sample models for rural, female, and Hindu households, but insignificant for other household groups. UIP exposed adults in LFS had significantly higher MPCE, while no effect was found in HFS. Results were similar in samples across different time periods. In model 15A, which includes salaried workers, the treatment effect on wages is 13% (95% CI: 3%–23%, p<0.01).

*6.5. Additional robustness checks, placebo test, event study*

We perform additional robustness checks and present the results in Tables 2 and 3. In models 14A, for those born between 1985 and 1990 we show the results excluding education controls. We find that the coefficient of exposure decreases 2 percentage points from the main model, showing a 12% increase in wages (95% CI: 6%–19%, p<0.01) for the treatment group relative to the control group. For MPCE, model results without education controls are identical to the main model.

Table A5, models 7 and 8 present the placebo tests results. We find no statistically significant wage effect of UIP exposure among those born during 1975-1980 and 1970-1975. Finally, we conduct an event study analysis of the effect of UIP on wages. We regress log wages on binary indicators of birth years before and after the 1986 introduction of UIP. Indicators of lagging years 1981-1985 and leading years 1987-1991 are included. Also included on the right-hand side are the covariate set $X_{i,j}$ and indicators of the year of UIP implementation in an individual's home district. Appendix Figure A10 shows the estimated coefficients of lagging and leading years along with the corresponding 95% confidence intervals. While there was a secular trend in wages during 1981-1991 even after controlling for covariates, the growth rate of wages was much higher during the post-UIP period (1987-1991) as compared with the pre-UIP (1981-1985) period which is consistent with our main findings.

## 7. Discussion and conclusion

An estimated 400,000 Indian children under age five die yearly from vaccine-preventable diseases such as pneumonia, diarrheal diseases, measles, and meningitis (Wahl et al., 2019). Vaccines can not only save these lives but also improve cognitive outcomes and educational attainment (Nandi et al., 2020b, 2019b). Our study adds to the growing body of literature showing the substantial long-term economic benefits of immunization in LMICs. We find that adults aged 21 to 26 in districts where the Universal Immunization Programme was implemented at the time of birth have 14% higher weekly wages. We also examine changes in monthly per capita household consumption expenditure and find a 3% higher MPCE for adults with UIP exposure. Finally, vaccination also influences livelihoods: treatment individuals' households are 2% less likely to rely on agriculture as their principal source of income. These results are robust to changing the sample size to include both 16- to 31-year-olds and 21- to 31-year-olds.

While there is a substantial and well-established literature on the long-term economic benefits of disease reduction in LMICs (Almond et al., 2018; Currie and Vogl, 2013), the only published study of labor market effects of childhood vaccination is from the United States, which linked measles vaccinations with a 1.1% rise in future wages (Atwood, 2021). However, these estimates may not be generalizable to LMICs such as India that have a higher burden of vaccine preventable diseases and lower coverage of clean water and sanitation. In a yet unpublished study, Atwood and Pearlman (2022) show that log wages were 2%-12% higher – depending upon the data and methods used – among adults in Mexico who had received the measles vaccine in childhood as compared with those who did not. These estimates are up to 10 times larger than the effects Atwood (2021) found in the United States — and the authors attribute these differences to the higher disease burden and lower access to healthcare in Mexico. Similarly, Hamory et al. (2021) find that those with an additional two to three years of exposure to childhood deworming treatment in Kenya had 13% higher hourly earnings 20 years later. In addition to this recent evidence, there are a number of studies including studies on deworming programs, malaria eradication, and the influenza pandemic which show exposure effects on wages ranging from 8.6% to 50% in LMICs (Baird et al., 2016; Bleakley, 2010; Nelson, 2010).

The effects of exposure to UIP differ by population subgroup. Rural, male, scheduled caste and scheduled tribe, and Hindu adults experienced a positive effect of the program on wages, but urban, female, non-Hindu, and other backward caste adults did not. Similarly, for MPCE, we found that only rural, Hindu, and females experienced a rise in their consumption expenditure. Both high- and low-focus states saw an increase in wages, with a higher increase in HFS, but no effect was found on MPCE for HFS. These differential effects have many plausible reasons.

First, we do not observe actual receipt of vaccines but conduct an intent-to-treat analysis. Underlying socioeconomic characteristics of individuals and supply-side factors may be associated with vaccination (Summan et al., 2022a). The first-ever population-based national vaccination estimates in India are available from the National Family Health Survey conducted in 1992–1993 (International Institute for Population Sciences and ICF, 1995). Coverage of DPT-3 vaccination was only 46.9% and measles vaccination was only 32.7% at that time. In rural areas, these rates were 41.8% and 28.7%, respectively; in urban areas, they were 64.2% and 32.7%, respectively. There was a difference by sex as well: 49.8% of females versus 53.8% of males received the DPT-3 dose. Among socioeconomic groups, the lowest vaccination rates were observed among Muslim and scheduled caste and tribe households. These groups have historically had lower vaccination rates, and contemporary estimates suggest these vaccination gaps, though narrower, persist even today (International Institute for Population Sciences, 2017). Therefore, the lower rates of vaccination among some population subgroups may explain part of the difference in labor market outcomes.

Second, known statistical discrimination exists against socioeconomically disadvantaged and minority groups in the Indian job market. Women and individuals from lower-caste groups may lack access to the same job opportunities conditional on their level of education and productivity (Agrawal, 2014; Sengupta and Das, 2014). This would reduce the potential benefits of UIP among these groups. High-focus states may have experienced greater increase in wages than low-focus states due to the overall higher level of disease, and therefore benefits of vaccination, relative to states with overall better health outcomes.

The primary mechanism for these effects is reduced disease exposure in childhood, which has long-lasting health effects. Although outside the scope of this paper, other work has confirmed the health effects of UIP. A study exploited the temporal rollout in UIP and found that the program led to higher child height-for-age and weight-for-age metrics, both common measures of overall health status for children (Anekwe et al., 2015). These health outcomes are related to education outcomes. Studying UIP exposure, Nandi et al. (2020b) show that children in UIP-exposed districts completed 0.18 more years of school compared with control groups. Stunted child development, reduced human capital accumulation, and poorer health and productivity of workers result in lower wages.

Our findings have important policy implications. Higher investment in UIP can pay very large returns in terms of increased per capita income, with vaccinated populations earning 14% higher wages. A simple back-of-the-envelope calculation with the most recent Indian data—a 471 million labor force with 27% salaried workers,[5] 15% of them unvaccinated,[6] and a gross domestic product per capita of $1,900 (World Bank, 2021)—would mean overall economic output could increase by 0.11% to 0.28%. This is a lower bound of the potential effect because we lack earnings data for all workers. If this rate were applied to all workers in the labor force, the effect of UIP could increase gross domestic product by 1.2%. Although the country's ministries of health are typically responsible for funding health programs, it is widely recognized that a multisectoral approach is required for effective change, and the support of ministries of finance is needed (UNICEF, 2020). Our estimates show a direct link between vaccination and labor market outcomes and make a strong economic case for adequate funding for routine vaccines.

Globally, an additional 8.5 million and 8.9 million children did not receive their DTP-3 and meningococcal conjugate vaccine dose-1vaccine in 2020 relative to the number of missed doses projected (Causey et al., 2021). The highest reductions in vaccination rates were in March and April 2020, during the beginning of the COVID-19 pandemic, and the regions hit the hardest were North Africa, the Middle East, South Asia, and Latin America and the Caribbean (Causey et al., 2021)—the same regions with the lowest overall vaccination rates prior to the pandemic (UNICEF, 2021a). New evidence from India suggests vaccination rates decreased by 9% for DPT3 and 10% for polio third dose, while timely vaccination decreased between 3-5% due to the pandemic (Summan et al., 2022b). A 2015 study estimated a global funding gap of $7.6 billion in 2016–2020 for delivery of full vaccination programs in 94 LMICs, which corresponds to 0.2% of general government expenditures (Ozawa et al., 2012). For India's UIP an annual funding gap of $560 million was recently estimated to reach a 90% vaccination target (Schueller et al., 2021). The disruptions to immunization and the persistent funding gaps not only lead to higher levels of preventable deaths but can also substantially lower standards of living and even compromise poverty reduction efforts in LMICs in the long term. The increased expenditure of 0.2% of the government budget is many times smaller than the future increase in economic output that an immunization program could deliver.

India has the largest population of unvaccinated children in the world: the rate of full immunization (BCG, measles, and three doses each of DPT and polio) was only 62% in 2016 (IIPS, 2016). Although Mission Indradhanush and other programs have increased vaccination rates substantially (Clarke-Deelder et al., 2021; Summan et al., 2021), their long-term sustainability is uncertain. A recent analysis found that the per dose cost of vaccination under Intensified Mission Indradhanush was substantially higher than the per cost dose of routine immunization: $4.73 versus $1.31 in Bihar and $3.45 versus $1.43 in Uttar Pradesh (Chatterjee et al., 2021). This higher cost was attributed to the time required to identify children missed by UIP and the additional cost of vaccination in hard-to-reach areas. Routine immunization budgets must incorporate the full costs of catch-up vaccination and have adequate funding to reach new birth cohorts.

[5] Labor force includes salaried workers, self-employed, domestic worker/working in household enterprise, and unemployed individuals.
[6] This is based on current DPT-3 vaccination rate in infants.

To vaccinate children who missed vaccines because of COVID-19 or delays in immunization campaigns, countries will have to engage in catch-up vaccination campaigns. The World Health Organization (2020a) states that a catch-up vaccination strategy is an integral part of any national immunization program to ensure protection for individuals who may have missed doses. Ideally, vaccines should be administered on the recommended schedule, but most vaccines do not have an upper age limit (World Health Organisation, 2021b). Early identification and vaccination of children who missed doses is the most practical approach, because identifying them at later ages is more challenging (World Health Organisation, 2021b). World Health Organization guidelines for interrupted or delayed routine immunization do not set a maximum age limit for vaccines but rather recommend the time between vaccines and sometimes a different number of doses, depending on the age of the child (World Health Organisation, 2020). Indian immunization guidelines give an upper age limit for certain vaccines—five years of age for *Haemophilus influenzae*, pneumococcal vaccine, and BCG, and eight months for rotavirus (Indian Academy of Pediatrics, 2020). However, they allow for delayed vaccination several years after the recommended age.

Catch-up vaccination campaigns have become common in LMICs. For example, although the first dose of measles vaccine should typically be administered at eight months of age, in China, children up to age seven are targeted for catch-up (Zhang et al., 2017). Hutton et al. (2010) estimate that catch-up vaccination of children aged one to 19 years would be cost-effective at a cost of $2,500 per quality-adjusted life year gained and would remain cost-effective even if catch-up vaccination targets only children under two. Catch-up vaccination continues to be important as rates of newborn immunization increase, depending on the level of coverage needed to achieve herd immunity, and as transmission decreases with the population of susceptible individuals (Hutton and Brandeau, 2013).

Our analysis has important limitations. First, because we lack data on place of birth and have only current residence data, individuals may be wrongly assigned to the treatment or control group if their current district had a different UIP implementation date than their birth district. However, as discussed earlier, the current rate of out-of-district migration is approximately 15%, according to the 2011 Census. We predict the probability of an individual being a migrant and dropped 15% of observations that were most likely to be migrants; our estimates were robust to this specification. Moreover, the treatment status of some migrants would not change, depending on the timing of UIP implementation in their birth and current districts even if birth district was known.

Second, vaccination benefits may be underestimated because we do not consider spillover effects to other household members. By reducing secondary transmission of disease, vaccination of infants may protect unvaccinated older siblings or neighborhood children. Our results may also be underestimates if residents of control districts traveled to treatment districts to receive the vaccine. In this case, some members of the treatment group were wrongly assigned to the control group and biased our coefficients downward.

A third limitation is that wage data, by definition, exist only for hired workers. If any systematic difference exists in the treatment or control group individuals who choose work outside the home rather than work for a household enterprise, are self-employed, or stay out of the job market, and their income differs from the current control and treatment groups, our results may be biased. For example, those who did not receive vaccines may have had more illness during childhood and been unable to find jobs as easily as the treatment group. In this example, the effect on wages would be biased downward. To account for individuals without wage data, we used monthly per capita household expenditure as an additional outcome variable. Although this is not a perfect variable to observe outcomes for the treatment group, it was the best proxy we could identify in our data set, and we find positive effects on MPCE from UIP coverage.

Immunization is the most cost-effective tool for decreasing mortality and morbidity in children. In addition to the well-established health and cognitive benefits, vaccination has substantial economic benefits. The recent pandemic shock to immunization rates combined with already low vaccination rates in many LMICs will not only increase mortality and morbidity for these cohorts but also portend long-term harms in the form of lower incomes and standards of living. From an economic and a health perspective, it is critical that funding to immunization programs increases.

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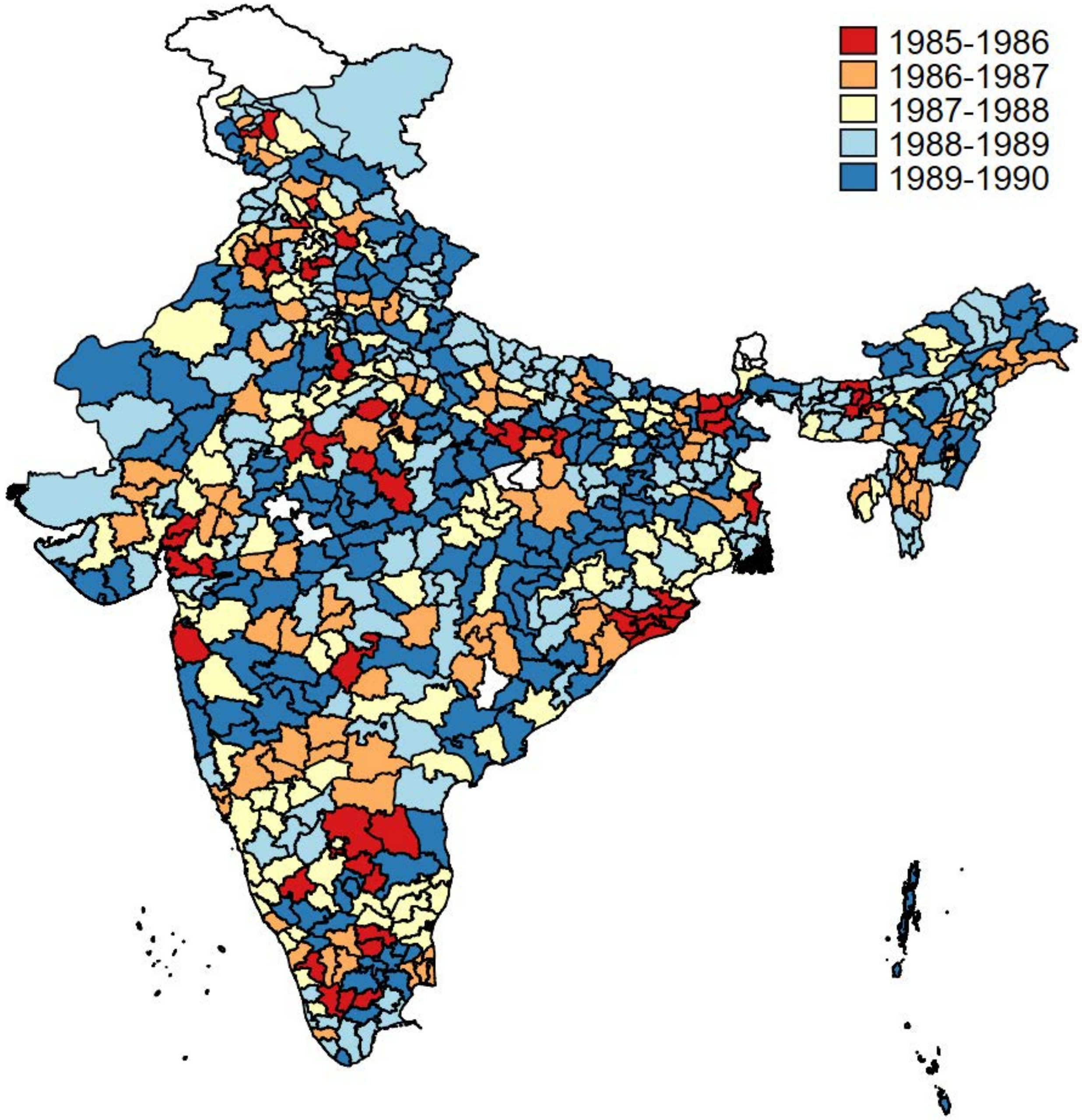
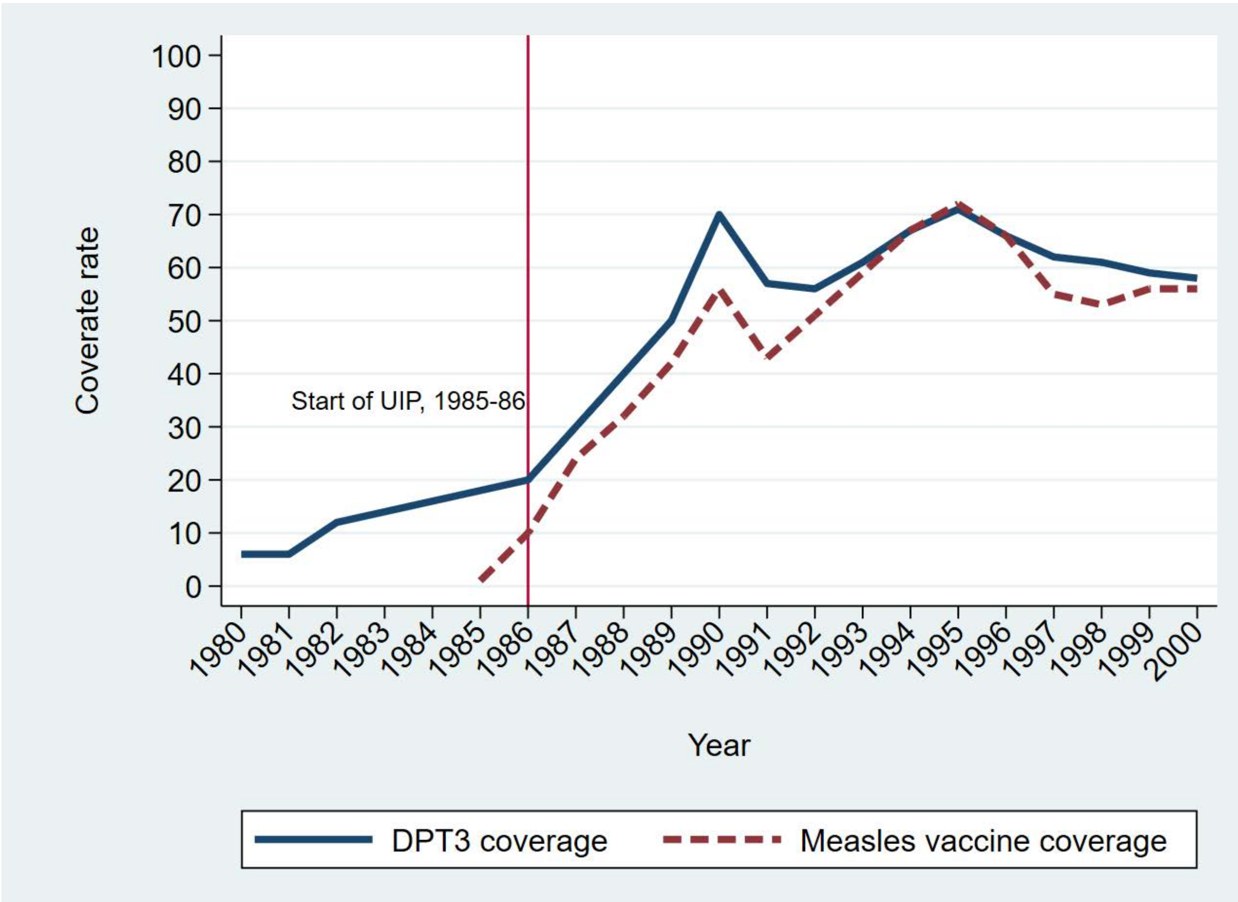
**Figure 1: Rollout of the Universal Immunization Programme, by year and district**




Note: Color codes denote the year of UIP implementation in a district. Districts with no data appear in white.

**Figure 2: Coverage of DPT3 and measles vaccines in India, 1980-2000**




Note: Data are from the World Bank. Coverage rate is calculated among 12-23 month old children.

**Table 1: Socioeconomic characteristics, by control and treatment groups**

| | UIP-covered district | | Control | | |
|---|---|---|---|---|---|
| | Mean | Standard deviation | Mean | Standard deviation | Difference in means |
| Wages | 1,278.88 | 1,145.20 | 1,556.96 | 1,605.97 | -278.08** |
| MPCE (INR) | 1,714.88 | 1,485.58 | 1,949.89 | 2,053.55 | -235.00** |
| Agricultural occupation | 0.15 | 0.36 | 0.13 | 0.34 | 0.02** |
| Age | 22.79 | 1.29 | 25.02 | 1.30 | -2.23** |
| Rural | 0.55 | 0.50 | 0.52 | 0.50 | 0.03** |
| Female | 0.19 | 0.39 | 0.21 | 0.41 | -0.02* |
| Female head | 0.14 | 0.35 | 0.12 | 0.32 | 0.02** |
| Married | 0.34 | 0.48 | 0.53 | 0.50 | -0.18** |
| Probability of being migrant | 0.96 | 0.09 | 0.95 | 0.11 | 0.01** |
| *Region* | | | | | |
| Northeast | 0.09 | 0.28 | 0.09 | 0.29 | 0.00 |
| North | 0.22 | 0.42 | 0.23 | 0.42 | -0.01 |
| West | 0.24 | 0.42 | 0.19 | 0.40 | 0.04** |
| South | 0.28 | 0.45 | 0.26 | 0.44 | 0.02* |
| Central | 0.05 | 0.21 | 0.07 | 0.26 | -0.03** |
| East | 0.13 | 0.34 | 0.14 | 0.34 | 0.00 |
| *Caste* | | | | | |
| General | 0.26 | 0.44 | 0.28 | 0.45 | -0.01 |
| Scheduled caste | 0.12 | 0.32 | 0.13 | 0.34 | -0.01* |
| Scheduled tribe | 0.23 | 0.42 | 0.21 | 0.41 | 0.01+ |
| Other backward caste | 0.40 | 0.49 | 0.38 | 0.49 | 0.01 |
| *Religion* | | | | | |
| Hindu | 0.75 | 0.43 | 0.77 | 0.42 | -0.02* |
| Muslim | 0.15 | 0.36 | 0.13 | 0.33 | 0.02** |
| Christian | 0.05 | 0.22 | 0.06 | 0.24 | -0.01* |
| Sikh | 0.03 | 0.18 | 0.02 | 0.14 | 0.02** |
| *Relationship to head of household* | | | | | |
| Head of household | 0.17 | 0.37 | 0.27 | 0.44 | -0.1** |
| Spouse | 0.05 | 0.21 | 0.08 | 0.27 | -0.03** |
| Child | 0.66 | 0.47 | 0.54 | 0.50 | 0.13** |
| Grandchild | 0.02 | 0.14 | 0.01 | 0.11 | 0.01** |
| Parent | 0.00 | 0.00 | 0.00 | 0.00 | 0** |
| *Education* | | | | | |
| Middle or lower | 0.55 | 0.50 | 0.51 | 0.50 | 0.04** |
| Secondary | 0.16 | 0.36 | 0.13 | 0.34 | 0.02** |
| Higher secondary | 0.12 | 0.32 | 0.10 | 0.31 | 0.01* |

| | | | | | |
|---|---|---|---|---|---|
| Graduate | 0.11 | 0.31 | 0.15 | 0.35 | -0.04** |
| Postgraduate | 0.02 | 0.15 | 0.06 | 0.24 | -0.04** |
| *Education of head of household* | | | | | |
| Middle or lower | 0.77 | 0.42 | 0.70 | 0.46 | 0.07** |
| Secondary | 0.12 | 0.32 | 0.11 | 0.32 | 0.00 |
| Higher secondary | 0.05 | 0.21 | 0.06 | 0.23 | -0.01** |
| Graduate | 0.05 | 0.21 | 0.08 | 0.27 | -0.03** |
| Postgraduate | 0.01 | 0.11 | 0.03 | 0.17 | -0.02** |
| Sample size | 3,941 | | 6,840 | | |

Note: Data are from National Sample Survey, 68th round. The sample consists of 21- to 26-year-olds. Treatment group comprises individuals living in districts where the Universal Immunization Programme was implemented by the year of their birth or earlier. MPCE=monthly per capita expenditure. INR = Indian rupees. +p<0.1, *p<0.05, **p<0.01.

**Table 2: Summary results of effect of UIP exposure on wages**

| Model | Model description | Time period | | | | | |
|---|---|---|---|---|---|---|---|
| | | A) 1985-90 | | B) 1980-95 | | C) 1985-95 | |
| | | Coeffcient | Sample size | Coeffcient | Sample size | Coeffcient | Sample size |
| 1 | Main model | 0.14** (0.03) | 10,781 | 0.14** (0.03) | 15,750 | 0.14** (0.03) | 26,562 |
| 2 | With partial effects | 0.07+ (0.03) | 10,781 | 0.07* (0.03) | 15,750 | 0.06+ (0.03) | 26,562 |
| 3 | Without predicted migrants | 0.16** (0.03) | 8,963 | 0.16** (0.03) | 13,932 | 0.16** (0.03) | 24,744 |
| 4 | Rural | 0.14** (0.04) | 5,716 | 0.14** (0.04) | 8,582 | 0.14** (0.04) | 14,284 |
| 5 | Urban | 0.08+ (0.05) | 5,065 | 0.08+ (0.05) | 7,168 | 0.09+ (0.05) | 12,278 |
| 6 | Male | 0.16** (0.03) | 8,618 | 0.16** (0.03) | 12,660 | 0.15** (0.03) | 21,140 |
| 7 | Female | -0.05 (0.12) | 2,163 | -0.07 (0.12) | 3,090 | -0.07 (0.12) | 5,422 |
| 8 | SC/ST | 0.2** (0.06) | 15,798 | 0.2** (0.06) | 16,385 | 0.2** (0.06) | 17,855 |
| 9 | OBC | 0.05 (0.05) | 4,178 | 0.05 (0.05) | 6,169 | 0.06 (0.05) | 10,252 |
| 10 | Hindu | 0.14** (0.03) | 8,261 | 0.14** (0.03) | 11,920 | 0.15** (0.04) | 20,342 |
| 11 | Non-Hindu | 0.09 (0.08) | 2,520 | 0.08 (0.08) | 3,830 | 0.09 (0.08) | 6,220 |
| 12 | High focus states | 0.21** (0.06) | 3,119 | 0.23** (0.07) | 7,941 | 0.22** (0.07) | 4,884 |
| 13 | Low focus states | 0.12** (0.03) | 7,662 | 0.12** (0.03) | 18,621 | 0.12** (0.03) | 10,866 |
| 14 | No education control | 0.12** (0.03) | 10,781 | 0.12** (0.03) | 26,562 | 0.12** (0.03) | 15,750 |
| 15 | Only salaried workers | 0.13** (0.04) | 5,699 | 0.14** (0.07) | 13,644 | 0.13** (0.04) | 7,529 |

Notes: Data are from National Sample Survey (68th round). The sample consists of 21- to 26-year-olds. The treatment group comprises individuals living in districts where the Universal Immunization Programme was implemented in the year of their birth or earlier. Standard errors clustered at district level. Includes age and district-level fixed effects. *OBC*=other backward caste; *ST*=scheduled tribe; *SC*=scheduled caste. Standard errors are clustered at the district level. +p<0.1, *p<0.05, **p<0.01.

**Table 3: Summary results of effect of UIP exposure on consumption expenditure**

| Model | Model description | Time period | | | | | |
|---|---|---|---|---|---|---|---|
| | | A) 1985-90 | | B) 1985-95 | | C) 1980-95 | |
| | | Coefficient | Sample size | Coefficient | Sample size | Coefficient | Sample size |
| 1 | Main model | 0.03** (0.01) | 46,557 | 0.03** (0.01) | 91,191 | 0.03** (0.01) | 129,980 |
| 2 | With partial effects | 0.04** (0.01) | 46,557 | 0.03** (0.01) | 91,191 | 0.04** (0.01) | 129,980 |
| 3 | Without predicted migrants | 0.03** (0.01) | 38,346 | 0.03* (0.01) | 82,980 | 0.03* (0.01) | 121,769 |
| 4 | Rural | 0.04** (0.01) | 27,854 | 0.04** (0.01) | 55,158 | 0.04** (0.01) | 78,263 |
| 5 | Urban | 0.01 (0.02) | 18,703 | 0.01 (0.02) | 36,033 | 0.01 (0.02) | 51,717 |
| 6 | Male | 0.01 (0.02) | 22,813 | 0.01 (0.02) | 46,297 | 0.01 (0.01) | 65,008 |
| 7 | Female | 0.03* (0.01) | 23,744 | 0.04* (0.01) | 44,894 | 0.03* (0.01) | 64,972 |
| 8 | SC/ST | 0.04+ (0.02) | 76,076 | 0.04+ (0.02) | 82,469 | 0.04+ (0.02) | 87,720 |
| 9 | OBC | 0.03 (0.02) | 18,058 | 0.03+ (0.02) | 35,607 | 0.03+ (0.02) | 50,827 |
| 10 | Hindu | 0.04** (0.01) | 34,268 | 0.04** (0.01) | 66,082 | 0.03** (0.01) | 95,291 |
| 11 | Non-Hindu | 0.02 (0.02) | 12,289 | 0.01 (0.02) | 25,109 | 0.01 (0.02) | 34,689 |
| 12 | High focus states | 0 (0.02) | 17,278 | 0 (0.02) | 49,887 | 0 (0.02) | 35,379 |
| 13 | Low focus states | 0.04** (0.01) | 29,279 | 0.04** (0.01) | 80,093 | 0.04** (0.01) | 55,812 |
| 14 | No education control | 0.03** (0.01) | 46,564 | 0.03** (0.01) | 129,988 | 0.03** (0.01) | 91,198 |
| 15 | Only salaried workers | 0.02 (0.04) | 5,802 | 0.02 (0.02) | 13,883 | 0.02 (0.04) | 7,665 |

Notes: Data are from National Sample Survey (68th round). The sample consists of 21- to 26-year-olds. The treatment group comprises individuals living in districts where the Universal Immunization Programme was implemented in the year of their birth or earlier. Includes age and district-level fixed effects. *OBC*=other backward caste; *ST*=scheduled tribe; *SC*=scheduled caste. Standard errors are clustered at the district level. +p<0.1, *p<0.05, **p<0.01.

**Table 4: Summary results of effect of UIP exposure on agriculture as household income source**

| Model | Model description | Time period: A) 1985-90 Coefficient | A) 1985-90 Sample size | B) 1985-95 Coefficient | B) 1985-95 Sample size | C) 1980-95 Coefficient | C) 1980-95 Sample size |
|---|---|---|---|---|---|---|---|
| 1 | Main model | -0.02* (0.01) | 46,557 | -0.02* (0.01) | 91,191 | -0.02* (0.01) | 129,980 |
| 2 | With partial effects | -0.02+ (0.01) | 46,714 | -0.01+ (0.01) | 90,025 | -0.01+ (0.01) | 127,752 |
| 3 | Without predicted migrants | -0.02* (0.01) | 38,069 | -0.02* (0.01) | 82,703 | -0.02* (0.01) | 121,492 |

Notes: Data are from National Sample Survey (68th round). The sample consists of 21- to 26-year-olds. The treatment group comprises individuals living in districts where the Universal Immunization Programme was implemented in the year of their birth or earlier. Includes age and district-level fixed effects. +p<0.1, *p<0.05, **p<0.01.