The Impact of Early-Life Access to Oral Polio Vaccines on Disability: Evidence from India*

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Abstract

We evaluate the impact of oral polio vaccines on the incidence of all disabilities (locomotor, hearing, visual, speech and mental) in India, focusing on polio-related disability which constitutes the largest fraction of locomotor disabilities. Polio was hyperendemic in India even as recently as the early 1990s, but the country was declared wild polio virus-free in 2014. Intent-to-treat effects from difference-in-differences with multiple time period models that condition on demographic and socio-economic characteristics reveal that with access to oral polio vaccines in the year of birth, the incidence of any disability, locomotor disability and polio-related disability declined by 20.5%, 11.6% and 7.2%, respectively, signaling substantial gains. Impacts on any disability underline that polio vaccines brought positive spillover effects on other disability categories as well. We test for absence of pre-trends, and implement falsification tests and estimate alternate specifications to present robustness checks that offer support for these results. The eradication of polio in India, while relatively late, brought significant health benefits and is a notable health economics success story in a developing context.

Key Words: Polio, locomotor, disability, acute flaccid paralysis, oral polio vaccines, early-life, childhood, difference-in-differences with multiple time period models

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1. Introduction

Wild polio virus was hyperendemic in India. Estimates note that even as late as the beginning years of the 1990s, on average 500 to 1000 Indian children developed polio-related paralysis each day (John and Vashishtha 2013). Evidence from 1980, which is the earliest year that reliable data is available, indicates that the cost to the national economy per paralyzed child was INR 150,000 (2022 USD 64,503.8), which translates into an annual loss of INR 450 million (2022 USD 193.5 million) or 0.1% of India's 1980 Gross National Product in current prices (John 1981; World Bank 2013). This percentage constitutes about 7% of the government's relative allocation to healthcare in the 1980s (Government of India 2016), a sizeable fraction for a developing country. In contrast, polio had been eradicated in the United States by 1979. This study evaluates the efficacy of oral polio vaccines (OPVs) in reducing the incidence of acute flaccid paralysis – the largest disease burden associated with polio, which is caused by damage to lower motor neurons – which we term polio-related disability, thus directly tying access to vaccines in early childhood with reductions in disability in India.

Our research answers the question: what was the impact of gaining access to OPVs at the district-level on the incidence of any disabilities in India, especially polio-related disability? This is important given the magnitude of costs that polio entailed, because the proportion vaccinated with OPVs is an incomplete measure of protection afforded in terms of disabilities averted (the relationship between percent vaccinated and cases of disability averted need not be one-to-one as other indicators may matter as well), and because there is relatively little research on people with disabilities in developing contexts. We focus on any disability (which includes locomotor, hearing, visual, speech and mental), locomotor disability - the largest component of any disability (50.6% of those with any disability have a locomotor disability in our sample), and

polio-related disability - the largest component of locomotor disability (21.5% of those with locomotor disability identify polio as the cause). A reason for considering any disability is because of evidence that the physical disabilities caused by polio (and embodied in locomotor challenges) may manifest in other dimensions such as depression, a significant component of mental health disability (Shiri et al. 2015, Bagcchi 2019). Hence, we want a catch-all measure of disability to evaluate whether OPVs had positive spillover effects on alternate disability categories, and to understand comprehensively what vaccine access in early childhood achieved.

Despite scattered evidence that vaccines decreased the prevalence of polio in India (John and Vashishtha 2013; Nandi et al. 2016), to the best of our knowledge, there are no studies in health economics that have used empirical tools to undertake a comprehensive analysis of the effectiveness of OPVs in reducing the incidence of disabilities in developing environments. This is important because given widespread resource constraints, it is possible that the efficacy of OPVs differs in these contexts. Overall lack of education and awareness, the relatively high opportunity cost of time, administrative inefficiencies resulting from lack of a well-developed health infrastructure, insufficient number of trained health personnel, and/or mis-targeting, which are all likely to be relatively more prevalent in developing countries, are other reasons for why OPV efficacy may differ in these contexts as compared to others. This is our primary contribution. Further, our study newly documents what the spillover impacts of OPVs on disabilities more broadly (all types of disabilities including locomotor, hearing, visual, speech, mental) has been in a resource-constrained context. Our study finds unintended positive externalities of OPVs in that while OPVs reduced polio-related disability as expected, the impact on our overall measure for other disability types is larger in magnitude and significance.

Although there is relatively little research on the efficacy of OPVs in the developing world, there is acknowledgement that the impact of vaccines in less-developed environments can differ for a variety of reasons. Ozer et al. (2018) finds that compulsory schooling laws and the resultant increase in maternal education in Turkey raises the likelihood that their children receive the complete course of important vaccines like those for diphtheria, pertussis, and tetanus (DPT) and Hepatitis B. Focusing on early childhood vaccines including OPVs in India, Haaren and Klonner (2021) find evidence that recent cash transfer programs (2011's *Indira Gandhi Matritva Sahyog Yojana*) have had beneficial impacts on increasing immunization rates, have raised the frequency of visits to public health systems in early-life, and have lengthened birth intervals between the first two parities (which the cash transfer program targeted). This is in contrast to first generation cash transfer schemes in India such as 2005's *Janani Suraksha Yojana* (JSY) – the world's largest health program at the time – which had, at best, an incomplete record since coverage of some key vaccines improved but others did not (De and Timilsina 2020). Huang et al. (2023) finds that Hepatitis B vaccination campaigns in China had beneficial unintended consequences by reducing male alcohol use in adulthood. However, as far as we know, there is no research evaluating efficacy of OPVs in reducing disability rates in India or any other developing country.

There is growing evidence from the developed world that eradicating infectious diseases and focusing on related inputs such as sanitation and the provision of clean water brings measurable benefits to human capital, especially if these interventions occur in early childhood, with benefits realized both contemporaneously and in later life in terms of cognitive markers,

adult health, labor force participation, earnings, and reduced mortality.[1] Meyer (2021), Gensowski et al. (2019), and Serratos-Sotelo et al. (2019) are three economic studies that focus on polio, where the first evaluates the Salk Polio vaccine trial of 1954 in the United States. Using comparison groups of trial counties that were provided access to the polio vaccine versus counties that were targeted but not included in the trial, Meyer (2021) documents a decline in all-cause mortality between 1958 to 1962, that amplified once federal funding also became available for childhood vaccines in 1963. However, Meyer (2021) considers circumstances from the early days of the vaccine almost seventy years ago, and evaluates impacts on mortality (not disability). Gensowski et al. (2019) leverages the 1952 polio epidemic in Denmark to document that although childhood disability was associated with early retirement and receipt of disability pensions by age 50, polio survivors were more likely to earn university degrees and to work in white-collar occupations, thus implying that they made education and labor market choices that valued cognitive skills over physical ones. Serratos-Sotelo et al. (2019) considers the long-term economic effects of the introduction of the polio vaccine in Sweden in 1957 and finds that there were no measurable benefits from vaccine exposure in terms of adult income, education or hospitalizations. There is no evaluation of effects on disability.

Our study contributes by using more recent data to understand how access to OPVs reduced disability in a country where economic hardship and socio-economic disadvantage characterize large shares of the population. Importantly, we document that OPVs had positive spillover effects on other categories of disabilities beyond polio-related ones alone. We use multiple sources of representative data including those from the National Sample Survey

[1] Notable research in this area includes Daramola et al. (2022), Meyer (2021), Butikofer and Salvanes (2020), Egedeso et al. (2020), Alsan and Goldin (2019), Mauricio and Noymer (2019), Gensowski et al. (2019), Serratos-Sotelo et al. (2019), Beach et al. (2016), Barreca (2010), Bleakley and Lange (2009), Currie et al. (2008), Cutler and Miller (2007), Bleakley (2007), and Almond (2006).

Organization, the National Family Health Surveys, and Family Planning Statistics of India. Our preferred empirical approach leverages newly developed difference-in-differences with multiple time period methods that exploit the staggered diffusion of OPVs across Indian districts, but avoids the "forbidden" comparisons. John and Vashishtha (2013) notes that the spread of OPVs was dictated by financing and administrative stipulations decided by the World Health Organization (WHO), UNICEF, CDC - United States, and Rotary International, which set up the National Polio Surveillance System, a partnership between the WHO and the Government of India. Conditional on a saturated set of controls, the timing of when districts receive OPVs was thus mostly exogenous to any individual's direction (as good as random from an individual's perspective).

Using information on the dissemination of OPVs across the districts of India and focusing on all individuals who were born from 1988 onwards (the immunization program extended beyond urban centers just before then), we document significant negative impacts of access in the year of birth on disabilities. More specifically, with controls for individual and household demographic and socio-economic characteristics, and state-level spending on polio campaigns, we estimate intent-to-treat effects (ITTs) which show that the incidence of any disability declined by 20.5%, of locomotor disability declined by 11.6%, and of polio-related disability decreased by 7.2%. These are sizeable gains relative to 1988. In particular, these results denote about a six-fold decline in the incidence of any disabilities relative to the baseline mean in 1988. Focusing on polio disability, the 7.2% on average decrease signifies a ten-fold decline relative to the mean at baseline. The fact that the ITT effect of OPVs on any disability is larger in magnitude than the impact of OPVs on polio-related disability signals that these vaccines had positive spillovers effects on other types of disabilities including locomotor,

hearing, visual, speech, and mental-related challenges. This is thus the unintended beneficial health externality that resulted from the introduction of these vaccines.

We present several robustness and specification checks of our estimates. Our pre-trends analysis cannot rule out the absence of trends in the seven years (1988 through 1994) preceding treatment. Next, we undertake a falsification check where in the data, we move the (true) year in which the district gained access to OPVs to 5 years later. Given that 0 – 5 years is the crucial time period in which the oral polio vaccine needs to be received, in this artificial scenario, many individuals would now be too old to have received the vaccines and thus should not benefit. We demonstrate that this is indeed the case (ITT is not significantly different from zero in all cases). Next, in order to confirm the results of the pre-trend tests, we restrict the analysis to 1988 through 1994 which are the years preceding the start of treatment (we discuss why 1995 is the benchmark year below). If there is no anticipation, ITT should not be significantly different from zero for all outcomes in this pre-treatment time period, and the results confirm this to be the case. Further, tests demonstrate that in the pre-treatment time periods (pre-OPV access), health and socio-economic status (SES) variables did not differ in districts that received OPVs relatively early versus those that received them relatively late.

Our study contributes to the literature by evaluating the efficacy of early-life OPV access in reducing disabilities in a large developing country where the polio burden was consistently among the highest in the world. We provide results from multiple specifications to show that the benefits of OPV access in the year of birth have been significant. Further, we document positive spillovers of OPV access on other disability types beyond polio-related disability alone. Given the high burden of this condition, a lesson learned is that India may have been too slow in

targeting the control of this disease. However, once attention and resources were mobilized, the relative speed and efficacy with which the disease was brought under control are remarkable.

2. Background of Polio in India

Poliomyelitis is an infectious disease that has existed since ancient times and is caused by a virus which mainly infects children (polio is also known as "infant paralysis"). This virus is only found in humans, and is spread through the fecal-oral route mostly by exposure to contaminated drinking water and/or because of unsanitary conditions. In a proportion of cases, it can lead to irreversible paralysis in limbs that waste away causing deformities in arms and legs and often resulting in death by affecting breathing muscles (Stafford and Gurney 1951). No cure exists, but effective (injectable) vaccines were developed in 1955, while the oral vaccine, the focus of this study and the most widely distributed version across the developing world given its ease of administration, was developed in 1962 (Ochmann and Roser 2017).

Despite the availability of vaccines, even as late as 1980, 125 countries reported paralytic polio cases among which the largest proportion (38.2%) was reported in South and South East Asia; in this region, India had the dubious distinction of having 94.5% of this region's total (Ochmann and Roser 2017). While the United States reported 0.03 paralytic polio cases per one million in 1980, India reported 27.2 paralytic polio cases per one million that year (Ochmann and Roser 2017).[2] Starting from this level, and thanks to a re-focus in international and national priorities that increased funding for programs such as 1995's Pulse Polio vaccination campaigns that were conducted as often as ten times annually to track and vaccinate every eligible child across all locations including state and district border transit points, India was declared wild polio virus-free by the WHO in 2014 (John and Vashishtha 2013; Bahl et al. 2014).

[2] A reason for this was that at independence in 1947, India invested in control of tuberculosis, malaria and leprosy, ignoring polio as a low priority disease with a high cost of eradication (John and Vashishtha 2013).

Panel A in Figure 1 shows that from 1988 onwards, the incidence of any disability has declined from 16.3% in the late 1980s to 10.8% in 2000-2011. The proportion of those with locomotor disability among those with any disability declined from 58.6% in the 1980s to 45.6% in the first decade of 2000. Polio-related disability constituted over 31.0% of all locomotor disability in the 1980s. This proportion declined to 26.3% in the 1990s with significant gains from then on (reflecting a renewed immunization effort including 1995's Pulse Polio campaign) such that this disease constituted 15.8% of locomotor disabilities in the later years of our sample. The renewed immunization effort in the 1990s is also reflected in Panel B of Figure 1 which plots the proportion of children 0-5 years who had access to OPVs in their year of birth. As shown, there is an increase in access from the late 1980s to early 1990s onwards, an evident jump after 1995 when Pulse Polio begins, and then essentially plateauing in this proportion from 2000 to 2010. From around 2011 onwards, almost every child has access. This year thus marks the endpoint of our analysis.[3]

3. Data and Summary Statistics

Our research uses three sources of data. The first is the National Sample Survey Organization (NSSO) data's disability modules from 2002 and 2018. NSSO had an earlier disability module in 1990, but these data do not include information on districts, our preferred unit of analysis for the dissemination of OPV vaccines, and thus cannot be used. The NSSO disability modules are the source of our outcome variables and the individual and household level controls. We consider three types of disability outcomes – any, locomotor, and polio-related disability, in their indicator form. Any disability includes locomotor (paralysis, deformity/loss of limb or dysfunction of joints/limbs), visual (no light perception, has perception

[3] Appendix Figure 1 shows the counterpart of Figure 1 when we focus on individuals below 17 years of age. Proportions remain about the same.

but cannot count fingers even with spectacles up to a distance of one to three meters, and normally uses spectacles), hearing (profound, severe, and moderate), speech (cannot speak, speaks in single words, speaks unintelligibly, stammers, speaks with abnormal voice, any other speech defect), and mental (unnecessary and/or excessive worry and depression, repetitive behavior, changes of mood/mood swings, talking/laughing to self, and seeing visions).[4] Causes of locomotor disability include cerebral palsy, polio, leprosy (cured/not cured), stroke, arthritis, cardio-respiratory disease, cancer, tuberculosis, burns/injuries, medical/surgical intervention, and old age. As noted above, polio is the largest cause of locomotor disability with 21.5% of those with the latter identifying this disease as the cause.

We note two qualifications here. First, up to 70% of individuals with polio may not exhibit symptoms (Ochmann and Roser 2017). Thus our estimates may be an undercount. This is however a conservative bias for us since if we did have an accurate (higher) count, our results would be even larger. Second, general development of a district may reduce the incidence of polio by improving sanitation, the nutritional environment, and access to health care and infrastructure. We control for this in our preferred specification as timing of access to OPVs is a district specific variable; further, we include regressors for demographic, socio-economic and state-related characteristics in all models.

As far as we know, there is no reliable administrative data available on when districts gained access to OPVs. Hence we use the National Family Health Surveys (NFHS) data which are the Demographic and Health Surveys (DHS) for India to evaluate the spread of OPVs across districts over time. There are five rounds that are currently publicly available (1992-1993, 1998-1999, 2005-2006, 2015-2016 and 2019-2020) but we focus on the first, second and fourth rounds

[4] The majority of individuals (99%) report only one disability, either locomotor, visual, hearing, speech or mental, in our sample.

since the third round does not reveal district identities, and there is little new information in the last round given India's polio virus-free status by 2014. The NFHS data are the source of information for the earliest year in which a district had access to the first dose of the OPV vaccine.[5] The OPV vaccine targets children in the 0 – 5 years age-group (Sokhey et al. 1996). The NFHS are particularly appropriate for gathering these data in these early years as they track responses to questions asked of mothers on the polio vaccination status of their last three children (the questionnaires ask information only for three children) when they were 0 – 5 years old.

Across the first, second and fourth NFHS surveys, the questionnaire asks mothers "Please tell me if (NAME) (has) received…Polio vaccine, that is, drops in the mouth?" Mothers who respond "yes" are then asked the month and year in which the child received the first, second, third and fourth doses.[6] We track the year in which mothers report their child received the first dose (usually at the time of birth), and then using information on mother's district of residence, we calculate the earliest year in which a particular district gained access to OPVs. We use information on the second through fourth doses to calibrate that the year in which the first dose was received in a district is correct. That is, we know that the second dose cannot have been received in an earlier year to the first dose, the third cannot be before the second, and so on.[7] This procedure was implemented for the earliest NFHS round from 1992-93 and then repeated for the remaining rounds to construct information on the time-varying diffusion of OPVs.

[5] District Level Household and Facility Survey (DLHS) data track the presence of cold-storage facilities which might be used to proxy for vaccine access. However, the NFHS data are widely considered to be more reliable in its immunization measures as compared to the DLHS.
[6] We cannot use the month information as NSSO does not report an individual's month of birth.
[7] This is the procedure used to minimize measurement error. Moreover, given Panel B of Figure 1 that shows almost universal adoption from mid 1990s on, we are less concerned with selection bias in take-up (or lack of it). See Huang and Danovaro-Holliday (2021) for a review of the literature that collects immunization information from the DHS surveys.

The availability of OPVs in a district may coincide with the availability of other vaccines. Information on vaccines for tuberculosis, diphtheria, pertussis and tetanus, and measles, is collected consistently in the NFHS surveys from the earliest round in 1992-1993. In order to gauge the impact of OPVs alone on disability outcomes, information on access to these alternate vaccines is tracked in a manner similar to the collection of information on OPVs, and all models condition on whether a district had access to these alternate immunizations in an individual's year of birth.

Finally, district boundaries have changed substantially over the time-period of our analysis because new states and districts were created.[8] Using a crosswalk that maps newly created districts to their parent district over the 1988 to 2011 time period, we matched districts to the extent possible over the time-span of our study.

The third source of data we use is the Family Planning Statistics of India manuals that provide information on an annual basis for Government sponsored polio campaigns from 1988 to 2011 on performance measures related to OPV disbursement at the state-level, including the percent of the target (number of children in thousands who were slated to receive the vaccine) that was completed. This variable controls for overall public investment directed towards polio eradication across districts over the time period of interest. We create the sample for analysis by appending the NSSO rounds with disability data from 2002 and 2018 (a repeated cross-section at the district-level), and then by merging the NFHS information on earliest year of access to OPVs and alternate vaccines on the basis of districts. Data from the Family Planning Statistics are consequently merged on the basis of state identifiers.

[8] In our data, there were 382 districts in 25 states and union territories in the earliest time period and 640 districts in 36 states and union territories in the latest time period.

Appendix Table 1 presents weighted summary statistics for the variables we use on those born from 1988 to 2011. Panel A denotes estimates for the outcome variables in the full sample, while Panel B shows estimates for the outcomes at baseline (1988). As noted above, we focus our analysis on individuals with year of birth from 1988 onwards as this is the year from which immunization campaigns began in earnest and extended to all regions of India. We end in 2011 as from then on, essentially all children had access to OPVs. Statistics in Panel A reveal that 2.6% of the sample had any disabilities, 1.5% had a locomotor disability, and 0.4% had a polio-related disability. Considering conditional proportions increases these estimates. The proportion with any disability in the sample has a weighted mean of 12.6%, while the proportion of those with locomotor disabilities among those with any disability has a weighted mean of 50.6%. This is consistent with evidence that the largest proportion of any disability is due to locomotor impairments. The proportion with polio disability among those with locomotor disabilities has a weighted mean of 21.5%, again consistent with evidence that the largest cause of locomotor-related disabilities is polio.[9] Panel B statistics indicate that the incidence of these disabilities was higher in the baseline year of 1988 – the somewhat lower proportions in the full sample that includes more recent years thus suggests improvements in disability status over time.

The individual characteristics in Panel C reveal that the mean age is 17.0 years and 46.4% of the sample is women.[10] About 4.2% of the respondents had parents who are related (a significant predictor of any disability), and close to 9.0% percent of the sample is illiterate or has

[9] The 12.6%, 50.6%, and 21.5% measures for any, locomotor and polio disability, respectively, are weighted averages of proportional estimates. That is, if we take the ratio of those with any disability to the number of people in the sample, the weighted mean of that ratio is 12.6%. Similarly, for the other two estimates. These proportions are not used in any of the regressions in the paper; they are reported for descriptive purposes mainly and their unweighted counterparts are depicted in Panel A of Figure1.

[10] An advantage of this relatively younger age-group is that we are more confident that their current district of residence is also likely to be their district of birth. While Munshi and Rosenzweig (2009) notes that permanent migration rates in India are relatively low, the average age-group of our sample gives us additional confidence that we are tracking district-level exposure at the time of birth relatively accurately.

had only informal levels of schooling. Those completing primary or middle schools constitute 53.9% and are the largest fraction of the sample. About 28.1% have completed secondary or higher secondary while 9.2% has completed graduate school or above. These proportions of educational achievement in the sample echoes broader trends in India. The proportion that is married is 76.6%, again roughly consistent with broader patterns for the whole country. In terms of exposure (born in the same year or in the year after the district gains access), 87.0% of the sample is exposed to OPVs in their year of birth consistent with the rapid expansion in access evident in Panel B of Figure 1. In terms of access to alternate vaccines, 88.6% of the sample is born in the same year or after their district gains access to tuberculosis, DPT or measles vaccines.

Turning next to household characteristics, Panel D shows estimates for the household-level variables including caste status, household size, land area owned, monthly per capita expenditure, and whether the household is classified as rural. In these data, 73.5% of the households are low-caste and approximate household size is 5.4 members (the 2002 NSSO disability module does not contain information on religion; we thus consider caste-status instead as this variable is available in both the 2002 and 2018 NSSO rounds). Average land area owned is about 0.01 hectares which is about 0.2 acres, signaling that households on average are relatively poor (also consistent with the average monthly per capita consumer expenditure value noted in Appendix Table 1). The sample is predominantly rural as 70.1% identify with this regional/sectoral affiliation. Finally, Panel E indicates that the mean year of OPV access at the district-level is 1991, and Panel F reports that the mean percent of the state-level targets completed is 98.5%.

4. Empirical Methodology

4.1. Naïve Specification: Two-Way Fixed-Effects

Our study of the impacts of OPVs is undertaken by leveraging both the timing of when access is gained and the identity of districts that gain access. The distribution of OPVs across districts may not be random in that wealthier areas could have gained earlier access or alternatively, districts with a higher number of cases could have been targeted earlier. As is known, this manner of non-random program placement can lead to biased results (Pitt et al. 1993, Allcott 2015). Allcott (2015) in particular demonstrates the existence of location selection bias by showing that information from a limited number of sites in earlier periods positively predicts effects in a substantially larger subsequent set of sites. Further, Panel B of Figure 1 shows that there was variation in OPV exposure before 1995. In light of these reasons, we undertake an event-study analysis to test for parallel trends. Figure 2 provides a graphical depiction of this test by plotting coefficients that estimate the effect of being exposed to OPVs in the individual's year of birth with corresponding 95% confidence intervals. The vertical line at 1995 denotes the year of the Pulse Polio campaign, and thus helps to demarcate the pre-treatment and post-treatment periods of our analysis more clearly (those born before 1995 are in the pre-treat periods whereas those after are in the post-treat periods). Across the outcomes we consider it is clear that prior to 1995, there is little to no impact of OPVs across districts as the coefficients are not significantly different from zero. The effect of concerted polio campaigns is mostly seen from 1996 onwards. Since then, significant declines in disabilities are evident in all three panels of Figure 2. We focus on the five years that precede 1995's concerted polio campaign and the years that follow thereafter (1990-2003) in Figure 2. This allows for a sufficient number of pre-periods to ascertain that outcomes were evolving in a parallel way in the event study. The parallel trend test in Figure 2 gives us confidence that the difference-in-differences estimates that follow are not potentially confounded with unobserved characteristics (we report formal statistics

as well in the main results of Table 2 below). Further, we undertake falsification and "no anticipation" tests, which we discuss in detail below.

The conventional method to estimate impacts as districts gain access in different years is a staggered difference-in-differences (DD) design of the following form:

$$y_{ijt} = \alpha_j + \theta_t + \beta \times Exp_{ijt} + \gamma \times X_{ijt} + \varepsilon_{it} \qquad (1)$$

Where y_{ijt} is an indicator for the presence of various types of disabilities (any, locomotor or polio-related) for individual i in district j in year of birth t, α_j are district fixed-effects, θ_t are year of birth fixed-effects, Exp_{ijt} is an indicator that takes the value of one for an individual if they are born either in the same year or in a year after which their district gains access to OPVs (and thus was "Exposed" to OPVs), and X_{ijt} are individual and household-specific controls.[11] The district fixed-effects, α_j, control for time-invariant district-level differences including unobserved characteristics that may lead to endogenous OPV access across areas resulting from possible location selection (Pitt et al. 1993). The parameter of interest, β, is thus identified from within-district variation, which is relatively less likely to be affected by potentially endogenous district-specific OPV access. Since we model exposure and access to OPVs given lack of data on individuals' vaccination status in early life, β estimates intent-to-treat effects.[12]

New development in the DD literature note that if treatment effects vary over time and across units of analysis, then two-way fixed-effects models can yield biased results where the coefficient of interest β differs from the true ITT effect. This is especially true when earlier

[11] We include the *i* subscript because even though the analysis conditions on districts and year, exposure to OPVs can differ by individual and household level characteristics as noted in Hajizadeh 2018, Pande and Yazbek 2003 and Shrivastwa et al. 2015 (please see discussion in Section 4.2. of the paper). Consequently, our main results reported in Table 2 include the controls in Panels C, D, E and F of Appendix Table 1 which are individual and household specific.

[12] This might lead us to underestimate average treatment effects on the treated, that is, we find a lower bound for this measure since we potentially include some people who might not have received the vaccine.

treated units are used as controls for later treated units, the so-called "forbidden" comparisons. We undertake diagnostics using methods developed in Goldring (2019) and Goodman-Bacon (2021) to gauge the extent to which this is an issue in our case. As detailed below, results reveal that such inappropriate comparisons potentially affect a relatively large part of our sample.

4.2. Difference-in-Differences with Multiple Time Period Models

Given the results of diagnostic tests, we adopt newly developed methods in Sant'Anna and Zhao (2020), Callway and Sant'Anna (2021), and Rios-Avila et al. (2021) as our preferred specification. This framework allows the estimation of group-time specific effects that are insulated against the "forbidden" comparisons by using "never treated" and/or "not yet treated" units as the control group in a staggered implementation design where once the "treatment" turns on, it remains on.[13] Given the widespread diffusion of OPVs across the districts of India during our time-period of analysis, the size of the "never treated" group in our case is relatively small (there is only one district in our sample that constitutes this group). We thus follow guidelines in Sant' Anna and Zhao (2020) and Callway and Sant'Anna (2021) and use both never treated and not yet treated units as the control group. We report the "simple" aggregation of these group-time specific effects that uses the size of the group-year cell as weights, however, averages across time for a group, or averages across groups for specific time periods are also estimated.

We present results that condition on year of earliest access and individual, household, and state-level characteristics. Including these variables as controls is important in our context as relevant studies suggest that socio-economic inequalities are major barriers to childhood vaccination in low and middle income countries (Hajizadeh 2018, Pande and Yazbek 2003,

[13] Note that tests show that pre-treatment (pre-OPV access) health and socio-economic characteristics in districts that received OPVs relatively early do not differ from those in districts that received them relatively late, as reported in Appendix Table 2 and discussed below.

Shrivastwa et al. 2015). Models are weighted using weights provided in the NSSO, and standard errors are clustered at the district-level. Results from these models are reported in Table 2.

The identifying assumption for the Sant' Anna and Zhao (2020) and Callway and Sant'Anna (2021) estimators' "not yet treated" version is that the trajectory in potential outcomes evolves in the same way for treated groups and never treated and/or not yet treated groups. We provide several checks to show that this is the case here. First, we plot the dynamic treatment effects estimated by the new estimator in the pre-treatment and post-treatment time periods. These are presented in Panels A (any disability), B (locomotor disability) and C (polio disability) of Appendix Figure 2, and show that pre-treatment average effects are null, while significant average declines in the incidence of disabilities are evident in the post-treatment periods. Second, we report tests from the Sant' Anna and Zhao (2020) , Callway and Sant'Anna (2021) and Rios-Avila et al. (2021) estimator that checks for parallel trends in the periods before treatment in the main results table (Table 2). In all cases, we cannot reject the null that pre-trends are absent. We also implement falsification exercises and checks to ensure that effects were not anticipated.

5. Results

5.1. Naïve Specification: Two-Way Fixed-Effects Results

Results from the specification in equation (1) that conditions on controls in Appendix Table 1 and district fixed-effects and year of birth fixed-effects are reported in Table 1. The treatment indicator is negative and significant across all columns in the table. Following Bellemare and Wichman (2020) and Prem et al. (2021), we report the percentage change in the outcomes as per a hyperbolic sine transformation of $e^{\widehat{\beta}} - 1$. The coefficient in column (1) for any disability translates into an 7.13% decrease on average after the district gains access to

OPVs, which is about a two-fold decline relative to the baseline mean. The estimates in columns (2) and (3) imply a 3.34% and 0.89% decline, respectively. These are measurable gains in relation to the baseline means for these disability categories.

Results from the Goldring (2019) and Goodman-Bacon (2021) decomposition are presented at the bottom of Table 1. These reveal that 66.6% of the comparisons are of the "forbidden" type where earlier treated units are used as controls for later treated ones. Since such a large proportion of earlier treated units are being used as controls for later treated ones, it is likely that the results in Table 1 underestimate the true impact of OPVs on disabilities as control groups include those who benefitted earlier. To ascertain this, we next discuss results from our preferred specification.

5.2. Preferred Specification: Difference-in-Differences with Multiple Time Period Models

We begin by noting tests that show that pre-treatment (pre-OPV access) health and socio-economic characteristics in districts that received OPVs relatively early did not differ from those in districts that received them relatively late, as seen in Appendix Table 2. Among all the observables we consider, in no case is the difference statistically significant between districts that obtained access relatively early (before 1991, the mean value for the earliest year in which OPVs become available as noted in Appendix Table 1) versus those that obtained access relatively late (in or after 1991, or never obtained access). Thus these early and late-access districts were comparable on observable dimensions.

Group-time aggregate ITT estimates from DD with multiple time period models using Sant'Anna and Zhao (2020), Callway and Sant'Anna (2021), and Rios-Avila et al. (2021)'s procedure on weighted sample data with standard errors clustered at the district-level are presented in Table 2, along with reports on chi-squared statistics and the corresponding *p*-values

from tests for the absence of pre-trends in all periods before treatment. The models include all controls listed in Appendix Table 1 including access to alternate vaccines and in each case, the control group is those who were never treated and not yet treated.

Reporting the percentage change in the outcomes as per a hyperbolic sine transformation following Bellemare and Wichman (2020) and Prem et al. (2021), the ITT coefficient of -0.23 in column (1) for any disability implies a 20.5% decline on average after the district gains access to OPVs. This is about a six-fold decline in the incidence of any disabilities relative to the baseline mean. Considering locomotor disabilities next, the coefficient in column (2) indicates that with district access to OPVs, there was an average 11.6% decline in the incidence of this type of disability. The corresponding decline for polio disability is 7.2% on average; a ten-fold decrease relative to the mean at baseline. As expected, these are significantly larger impacts as compared to the two-way fixed-effects results in Table 1 which do not condition on "clean" control groups.[14]

The difference in the size of the ITT effects in Table 2 is noteworthy. While the effect of OPVs on polio-related disability in column (3) is evident, the impact of OPVs on any disability in column (1) is about three times larger and measured more precisely. This indicates that while OPVs had the expected effect in reducing polio-related challenges, they also had beneficial consequences on other disability types including locomotor, hearing, visual, speech and mental-

[14] We collected additional socio-economic variables at the state-level in the initial period including per capita net state domestic product in 1990, the literacy rate in 1991 and per capita expenditures on health at the state-level in 1991 from the *National Health Profile of India* published by the Ministry of Health and Family Welfare and the *Handbook of Statistics on Indian States* published by the Reserve Bank of India. We then included these variables interacted with time trends in the main results reported in Table 2 to find that if anything, our results become even larger. For example, the coefficient on any disability implies an 84.9% decline on average after the district gains access to OPVs. These results are available on request as we are reluctant to include directly in the paper because the sample size is about 7,000 observations lower in this case. That is because we have a lot of missing values in these variables for relatively big states that did not exist in the late 1980s and 1990s (like Uttarakhand, Jharkhand, and Chhattisgarh), which do, however, exist in our disability data from 2002 and 2018.

related disability. Hence unintentional positive spillover benefits on other disability types resulted from the introduction of vaccines that targeted only polio. We evaluated the impact of OPVs on the separate components of any disability: mental, hearing, visual, speech and locomotor. The negative impact is measured precisely in the case of hearing and visual-related disability, while the *p*-value is close to 0.1 in the case of both mental and speech-related disability.[15] As we discuss above, polio can cause physical paralysis and deformities of limbs that may result in hearing and visual-related challenges in addition to mental-related illnesses such as depression. Hence declines in these alternate categories with the spread of OPVs is broadly in keeping with intuition.[16]

Continuing with the discussion of results in Table 2, test statistics for absence of pre-trends in all earlier periods cannot reject the null that pre-trends are absent. To be clear, using the start of the Pulse Polio campaigns in 1995 as the benchmark, we test for absence of pre-trends in all seven years of our data (1988-1994) that precede this year. There is no evidence for pre-trends. These results in Table 2 thus underline that the arrival of OPVs brought substantial benefits to populations in districts that attained access.

6. Robustness Checks and Extensions

Results detailed above indicate that access to OPVs brought significant positive impacts in terms of the disability measures we consider. In this discussion, we note that since we infer access for all children in the district based on the child whose mother reported earliest year of

[15] These results are available on request (locomotor-related disability results are reported in the paper).
[16] Changes in parental behavior may also play a complementary role. Unfortunately, our disability data does not contain any health-related information specific to parents and children beyond that which we already include. We are thus unable to examine this channel closely. There is some evidence that years of exposure is positively correlated to completion of primary/middle school and to an indicator variable that the individual is married. These factors may have a protective influence on diverse disability types, thus possibly accounting for some of the positive externalities of OPVs.

access, and since we do not directly observe whether each subsequent child is administered the OPV, it is possible that access does not imply full compliance, and there may be some fraction of eligible children with access who did not receive the OPVs. Or received only a subset of doses instead of the full four-dose course. However, this would again be a conservative bias for our results. That is, with full compliance among those who had access at the district-level, our results would increase in magnitude.

We undertake two additional checks of the DD with multiple time period models results. The first is a falsification check where we artificially move the year in which the district gained access to OPVs to 5 years later. In this alternate scenario, many individuals would now have become too old to have received OPVs in the crucial first five years of life, and thus should not benefit. These results are presented in Panel A of Table 3 and show that ITT equals zero in all cases. Correspondingly, when we artificially move the year of access to 5 years earlier, the magnitude of the coefficients in Table 2 becomes larger as many more people benefitted (results available on request).

Next, in order to confirm the results of the pre-trend tests in Table 2, we restrict the analysis to 1988 through 1994, which are the years before there was a concentrated effort to increase OPV access at the district-level. If there is no anticipation, ITT effects should not be significantly different from zero for all outcomes in this pre-treatment time period (Rambachan and Roth 2022; Roth 2022). Results in Panel B of Table 3 confirm that this is the case.

7. Heterogeneity in the Preferred Specification Results

We examine heterogeneity in the main results of Table 2 by splitting the sample by gender, rural/urban status, and between richer and poorer households. Such an evaluation is important in order to understand whether OPV benefits accrued uniformly or whether they were

limited to certain subgroups of the population. Classification by gender and rural/urban status is straightforward as these variables are directly measured in the data. In order to demarcate rich and poor households, we use the median value of household average monthly per capita consumer expenditure.[17] Households earning the median value (Rs. 18) or higher are classified as "richer households," while those earning lower than the median value are "poorer households." We present results from the heterogeneity analyses in Table 4.

Estimates in Table 4 are measured with significance mostly in the case of any disability and indicate that for this outcome, women benefitted relatively more than men. The ITT coefficient in column (1) for any disability in the women's sample implies a 12.3% decline on average after the district gains access to OPVs. The estimate in the men's sample is measured with error. A reason why impacts may be relatively more significant for women is because their access to vaccines in early life is likely quite low given son preference in India (Corsi et al. 2009). This could drive larger measurable gains for women (girls') given their initial relatively low baseline OPV status.

Considering rural/urban areas next, the effect is relatively more pronounced in urban settings where the coefficient implies a 11.3% decline on average after the district gains access. This may be reflective of relatively better health infrastructure in urban areas, and because of the likely disparities between rural and urban regions in educational attainment and income levels. Finally, the impact of OPVs is significant for the sample of poorer households where the ITT coefficient translates to a 15.2% decline on average with OPV access. Again, this may derive from the relatively larger gains for poorer households compared to richer households, given their overall lower exposure to OPVs at baseline.

[17] We could have used the amount of land owned instead, but not every household in our sample owns land.

There are no significant effects in the case of locomotor and polio disability, perhaps because the variation in the incidence of these disability types is lower as compared to any disability as shown in Appendix Table 1, and/or because refining samples by dimensions of interest curtails this variation further.

8. Other Outcomes

We evaluate whether OPVs had an impact on other outcomes such as educational attainment, marital status, amount of land owned by the household, and average household income (household average monthly per capita consumer expenditure). The data allows us to consider educational attainment and marital status on an individual basis, but we have land ownership and average income at the household level only. Results are presented in Table 5 and show that there are no measurable effects of OPVs on these outcomes. This is consistent with findings in Serratos-Sotelo et al. (2019) which evaluated the long-term economic effects of the 1957 introduction of the polio vaccine in Sweden. As in that study, it is possible that the null effects of OPVs in India are also explained by the pathological facets of the infection and the overall lack of scarring effects.[18] Another aspect that is likely here is selection. As discussed in Section 2 above, a proportion of polio cases can result in death, which implies that some fraction of the sample that we use to evaluate disability is positively selected. The fraction of survivors in our sample are likely stronger than the average individual; an aspect that contributes to overall lack of scarring on the alternate outcomes considered here. The positively selected sample also likely implies that the main results in Table 2 are an underestimate of the true benefits of OPVs (impacts may have been larger in the absence of positively selected individuals in the sample).

9. Discussion and Implications for Policy

[18] We lack data to examine in this aspect in detail.

Using a sample of individuals born from 1988 to 2011, we demonstrate causally that access to OPVs for children in the 0-5 years age-group across the districts of India brought significant beneficial impacts on measures of disability. Our preferred specification, DD with multiple time period models that control for individual and household demographic and SES characteristics, estimates ITT effects that indicate that the incidence of any disability (locomotor, hearing, visual, speech and mental) declined by 20.5%, of locomotor disability declined by 11.6%, and of polio-related disability decreased by 7.2%.

These are large gains (in keeping with control over polio being achieved in a relatively short time-span) and consistent with estimates for early-life vaccine/vitamin effectiveness from other parts of the world. For instance, there was a 99% decline in the incidence of nine diseases for which vaccines were recommended regularly in the US (Andre et al. 2008), and clinical studies with European, Native American, Chilean and African children confirm that vaccine efficacy was over 90% against Haemophilus influenza type b (Andre et al. 2008). Recent work on the protective impacts of vitamin A indicates that supplementation in early-life can almost completely eliminate large negative effects of natural disasters (Gunnsteinsson et al. 2022). In particular, detrimental impacts on 0-3-month-old infant's mid-upper arm circumference of up to 0.4 standard deviations attributed to a tornado in Bangladesh were fully mitigated in the sample of comparable infants who had received the vitamin A supplementation (Gunnsteinsson et al. 2022). Since the ITT effect for any disability is relatively larger in magnitude than that for polio-related disability, we conclude that OPVs brought beneficial spillover effects on other categories of disabilities as well. A closer examination reveals that this was particularly true for hearing and visual-related disability. We test for absence of pre-trends and present estimates from alternate tests and specifications as robustness checks for our main results.

In terms of policy lessons learned, India may have been too slow in mobilizing efforts to control polio especially in light of the significant disease burden. But the relative efficacy with which the disease was brought into check once attention had been focused is laudable. In particular, using the start of the Pulse Polio campaign in 1995 as the benchmark, the country was wild polio virus – free in about 19 years. In contrast, the United States, starting from a lower incidence rate (yet still high by international standards) in the early 1950s, took about 29 years to be polio virus – free (Nathanson 1982). India's success is essentially an under-appreciated health economics success story from the developing world in the twenty-first century. This success is thrown into stronger relief when we consider that polio remains endemic today only in two countries - Afghanistan and Pakistan - although it is to be acknowledged that these countries face major security concerns and community resistance that pose significant challenges to eradication (Ataullahjan et al. 2021).

The socio-economic benefits of wild polio virus eradication in India have been substantial, and as we note above, this accomplishment is one of the unheralded health economics achievements of this century. Given that, to the best of our knowledge, this is the first study to use the lens of economics to evaluate the effect of OPVs on disability rates in a developing country (or the effects of vaccines in general on disability), and that there is little to no research that gauges success in eliminating disabilities by experimenting with different rates of OPV administration while remaining cognizant of district-level heterogeneities, "counterfactual" estimates on how many more disabilities could have been averted by even better targeting and implementation in the years of our analysis are hard to judge. However, there is little doubt that the dividends have been substantial. Estimates in studies such as Nandi et al. (2016) suggest that the economic productivity gain from polio eradication from the early

1980s to 2012 was about 2011 USD 1.7 trillion (2022 USD 2.1 trillion). Our rough back-of-the-envelope calculation for our time period of interest based on statistics from the 2011 Census of India, Prinja et al. (2014), and Nandi et al. (2016), suggests that our estimated ITT effect for the reduction in polio-related disability translates into an overall benefit of 2011 USD 5.6 billion (2022 USD 6.9 billion). This is higher than the overall cost of the Pulse Polio campaign which was about 2011 USD 4.7 billion (2022 USD 5.8 billion).[19] Hence this vaccination campaign was cost-effective. If we were to conduct the cost-benefit analysis on the basis of the estimated ITT effect for any disability, which is relatively larger in magnitude as compared to the effect for polio-related disability since it takes into account the positive spillover impacts on other disability categories as well, it is clear that the benefits would further outweigh the costs, strongly underlining how cost-effective the polio eradication efforts have been.

Lessons from this success fuels optimism that other diseases such as dengue, tuberculosis, typhoid, and malaria that are holoendemic in India, and which also impose significant health and economic burdens, may be brought under control through similar concerted efforts. Recommendations include using the framework of the successful polio eradication programs that extended outreach of OPVs coupled with attention on improving

[19] Focusing on the estimated ITT coefficient of -0.075 for polio-related disability in Table 2 which implies a 7.2% decline on average when a district gains access, and using Nandi et al. (2016) which notes a pre-vaccine polio incidence rate of 15 per 100,000 people, and based on the district average population of about 1.8 million people from the 2011 Census of India, this ITT coefficient translates to about 20.4 fewer people on average per district contracting polio after the vaccination campaign. The associated economic productivity value based on estimates in Nandi et al. (2016) (which calculates the number of paralytic cases averted and the associated productivity gain based on a value of statistical life method) is approximately 2011 USD $8.9 million per district. Since India had 640 districts in the 2011 Census year, this is a total benefit of about 2011 USD 5.6 billion (2022 USD 6.9 billion). On the cost side, Prinja et al. (2014) notes that the cost of the Pulse Polio campaign over the relevant years was $28.80 per child. Given that the 2011 Census counts about 164.5 million children in the vaccine eligible group of 0-6 years, this is an approximate total cost of about 2011 USD 4.7 billion or 2022 USD 5.8 billion (the 2011 Census does not report statistics for only the 0-5 years age-group; the total cost is thus likely to be somewhat of an over-estimate).

sanitation, nutrition, and educational interventions, to raise literacy and awareness to replicate achievements in these other disease domains.

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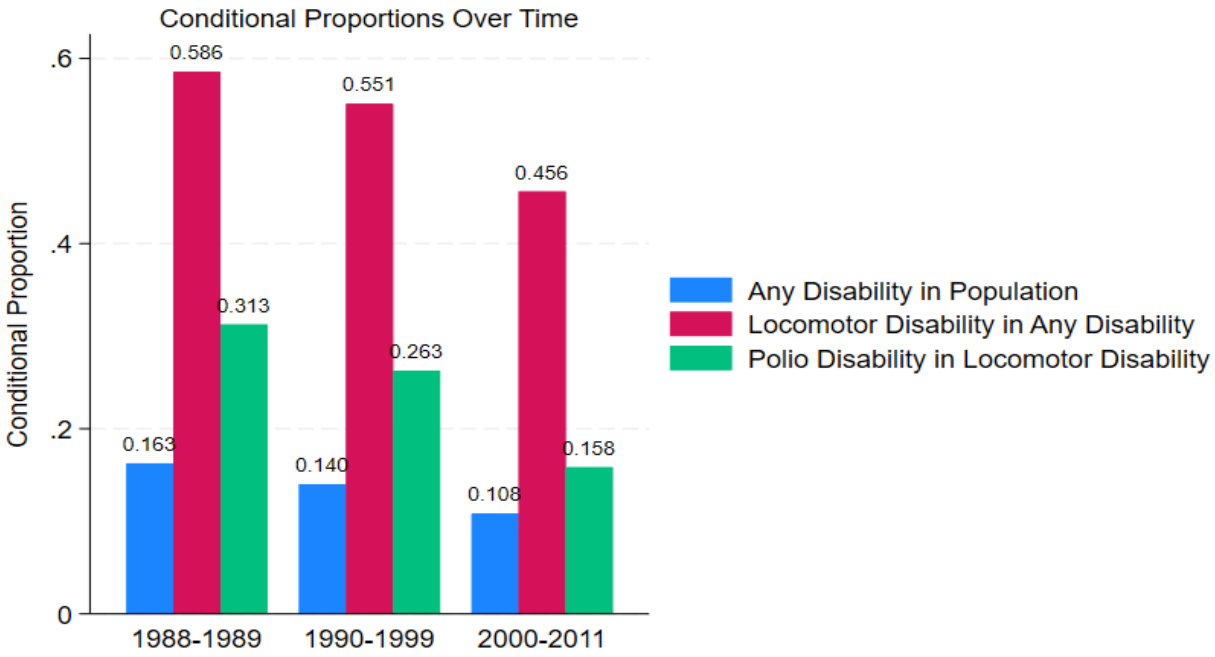
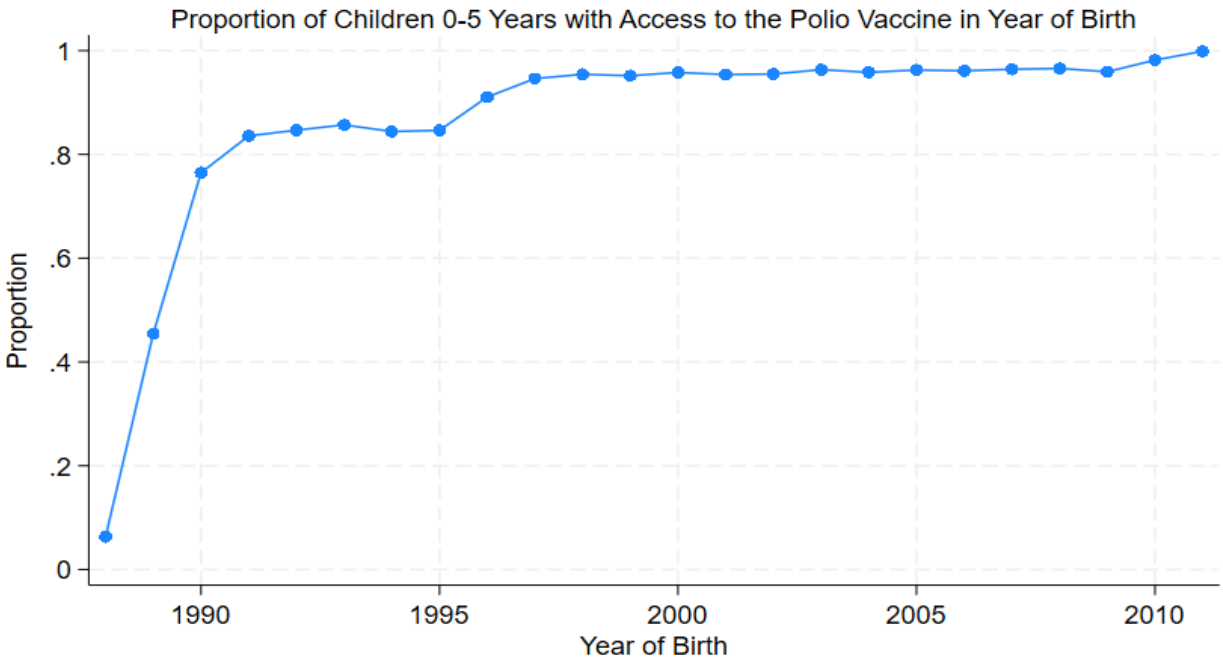
Figure 1: Trends in disability proportions and children 0-5 with OPV access

Panel A: Proportion with any disability in population, locomotor disability in any disability, and polio disability in locomotor disability, over time

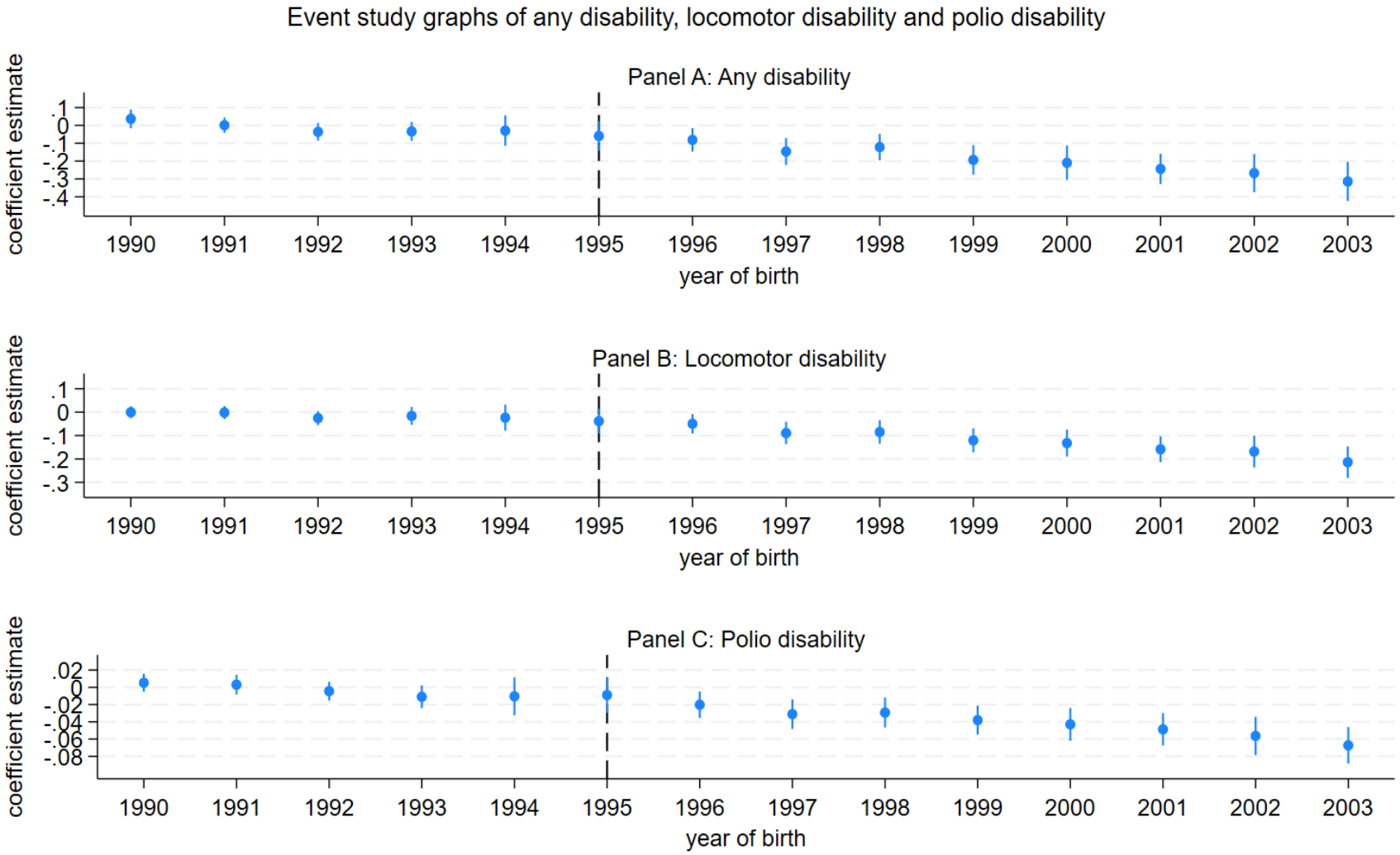


Panel B: Proportion of children 0-5 with access to the polio vaccine in year of birth



Notes: Authors' calculations using NSSO and NFHS data. Sample includes repeated cross-sections of 93,548 individuals in 70,429 households across 1129 districts and 64 states (in 2002 and 2018) whose year of birth is 1988 or after. Panel A reports unweighted means given the bar graph format.

Figure 2: Variation underlying the Difference-in-Differences design



Notes: Regressions include all characteristics in Appendix Table 1 and estimates are weighted. Standard errors are clustered at the district-level. Each point is the interaction coefficient from a regression where birth cohort dummies from 1990-2003 are interacted with an indicator that the individual was exposed to OPVs in their year of birth. Figure plots 95% confidence intervals.

Table 1: Two-way fixed-effects model results of OPV access on types of disabilities

Variable	Any disability (1)	Locomotor disability (2)	Polio disability (3)
Exposed	-0.074***	-0.034**	-0.009*
	(0.018)	(0.013)	(0.005)
Observations	64,920	64,920	64,920
R-squared	0.493	0.325	0.107
Goodman-Bacon decomposition			
Earlier treated (T) vs. later treated (C)	0.166		
Later treated (T) vs. earlier treated (C)	0.666		
Treated (T) vs. never treated (C)	0.018		
Treated (T) vs. already treated (C)	0.151		
T=treatment; C=Control/comparison			

Notes: Sample includes repeated cross-sections of 93,548 individuals in 70,429 households across 1129 districts and 64 states (in 2002 and 2018) whose year of birth is 1988 or after. Table presents weighted statistics with standard errors clustered at the district-level. Controls include variables listed in Appendix Table 1. Models include district fixed-effects and year of birth fixed-effects. *** Denotes significance at the 1% level, ** at the 5% level and * at the 10% level.

Table 2: ITT effects from difference-in-difference with multiple time periods models of OPV access on types of disabilities

	Any disability	Locomotor disability	Polio disability
	(1)	(2)	(3)
Intent-to-treat effect	-0.230***	-0.123***	-0.075*
	(0.087)	(0.045)	(0.044)
Absence of pre-trends:			
χ^2 (7)	7.860	5.090	8.190
Prob > χ^2 (7)	[0.345]	[0.649]	[0.316]
Observations	59,999	59,999	59,999

Notes: Sample includes repeated cross-sections of 93,548 individuals in 70,429 households across 1129 districts and 64 states (in 2002 and 2018) whose year of birth is 1988 or after. Table presents weighted statistics with standard errors clustered at the district-level. Controls include variables listed in Appendix Table 1. Absence of pre-trends is tested for seven periods before treatment (test results at longer thresholds are similar). In these difference-in-difference with multiple time periods models the outcome model is least squares, the treatment model is inverse probability, and the control group is those who were never treated and not yet treated (see Sant'Anna and Zhao (2020), Callway and Sant'Anna (2021) and Rios-Avila et al. (2021) for details). *p*-values in square brackets. *** Denotes significance at the 1% level, ** at the 5% level and * at the 10% level.

Table 3: ITT effects from difference-in-difference with multiple time periods models of OPV access on types of disabilities

	Any disability	Locomotor disability	Polio disability
	(1)	(2)	(3)
Panel A: Falsification			
Intent-to-treat effect	0.116	-0.046	0.005
	(0.120)	(0.050)	(0.034)
Observations	55,839	55,839	55,839
Panel B: No anticipation			
Intent-to-treat effect	-0.009	0.045	0.034
	(0.015)	(0.056)	(0.046)
Observations	20,710	20,710	20,710

Notes: Sample includes repeated cross-sections of 93,548 individuals in 70,429 households across 1129 districts and 64 states (in 2002 and 2018) whose year of birth is 1988 or after. Table presents weighted statistics with standard errors clustered at the district-level. Controls include variables listed in Appendix Table 1. In these difference-in-difference with multiple time periods models the outcome model is least squares, the treatment model is inverse probability, and the control group is those who were never treated and not yet treated (see Sant'Anna and Zhao (2020), Callway and Sant'Anna (2021) and Rios-Avila et al. (2021) for details). In the falsification analyses, the year in which a district gains access to OPVs is artificially moved back by 5 years so that many individuals were now too old to receive this vaccine in their 0-5 years age group. In the no anticipation analyses, year of birth is restricted to 1988 through 1995 which are the pre-treatment years. *** Denotes significance at the 1% level, ** at the 5% level and * at the 10% level.

Table 4: ITT effects from difference-in-difference with multiple time periods models of OPV access on types of disabilities: Heterogeneity

	Any disability (1)	Locomotor disability (2)	Polio disability (3)
Panel A: Women			
Intent-to-treat effect	-0.131**	-0.087	0.045
	(0.054)	(0.086)	(0.085)
Panel B: Men			
Intent-to-treat effect	-0.022	0.041	0.076
	(0.043)	(0.045)	(0.047)
Panel C: Rural			
Intent-to-treat effect	-0.036	-0.064	-0.012
	(0.086)	(0.085)	(0.057)
Panel D: Urban			
Intent-to-treat effect	-0.120*	0.038	0.104
	(0.066)	(0.081)	(0.075)
Panel E: Richer households			
Intent-to-treat effect	-0.064	0.052	0.140
	(0.065)	(0.074)	(0.076)
Panel F: Poorer households			
Intent-to-treat effect	-0.165**	-0.194	-0.126
	(0.075)	(0.160)	(0.079)

Notes: Sample includes repeated cross-sections of 93,548 individuals in 70,429 households across 1129 districts and 64 states (in 2002 and 2018) whose year of birth is 1988 or after. Table presents weighted statistics with standard errors clustered at the district-level. Controls include variables listed in Appendix Table 1. In these difference-in-difference with multiple time periods models the outcome model is least squares, the treatment model is inverse probability, and the control group is those who were never treated and not yet treated (see Sant'Anna and Zhao (2020), Callway and Sant'Anna (2021) and Rios-Avila et al. (2021) for details). *** Denotes significance at the 1% level, ** at the 5% level and * at the 10% level.

Table 5: ITT effects from difference-in-difference with multiple time periods models of OPV access on other outcomes

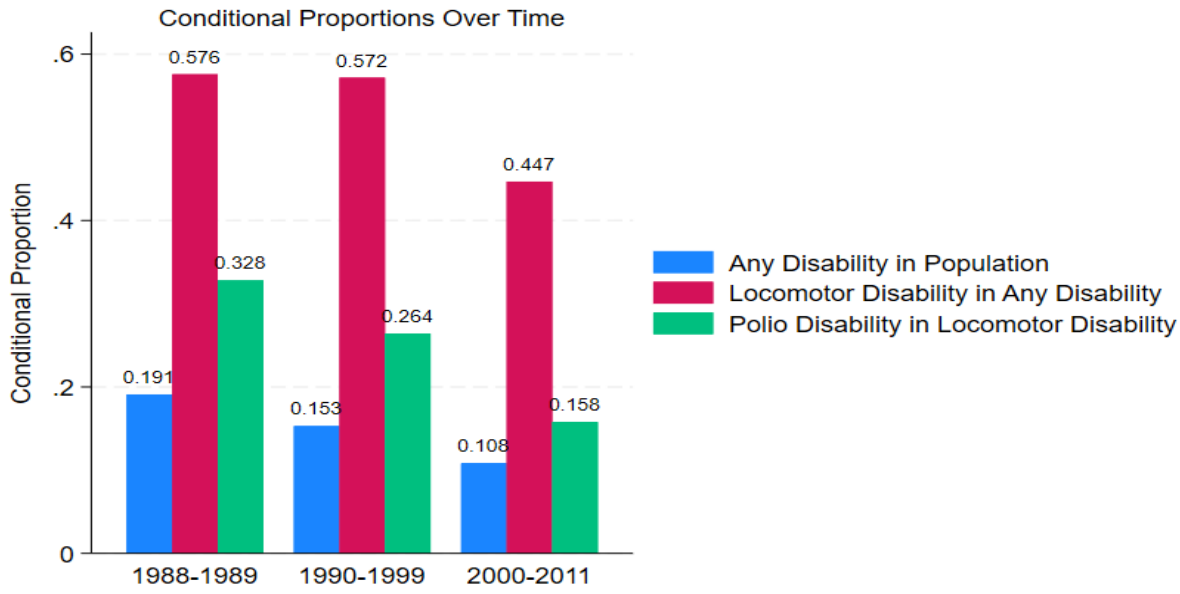
	Illiterate/ informal education	Completed primary/middle school	Completed secondary or higher secondary	Completed graduate or above	Married	Household land area	Household average monthly per capita consumer exp.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Intent-to-treat effect	0.047	0.123	-0.148	-0.021	-0.002	-0.095	2.900
	(0.106)	(0.140)	(0.105)	(0.070)	(0.065)	(0.097)	(7.756)

Notes: Sample includes repeated cross-sections of 93,548 individuals in 70,429 households across 1129 districts and 64 states (in 2002 and 2018) whose year of birth is 1988 or after. Table presents weighted statistics with standard errors clustered at the district-level. Controls include variables listed in Appendix Table 1. Absence of pre-trends is tested for seven periods before treatment (test results at longer thresholds are similar). In these difference-in-difference with multiple time periods models the outcome model is least squares, the treatment model is inverse probability, and the control group is those who were never treated and not yet treated (see Sant'Anna and Zhao (2020), Callway and Sant'Anna (2021) and Rios-Avila et al. (2021) for details). *p*-values in square brackets. *** Denotes significance at the 1% level, ** at the 5% level and * at the 10% level.

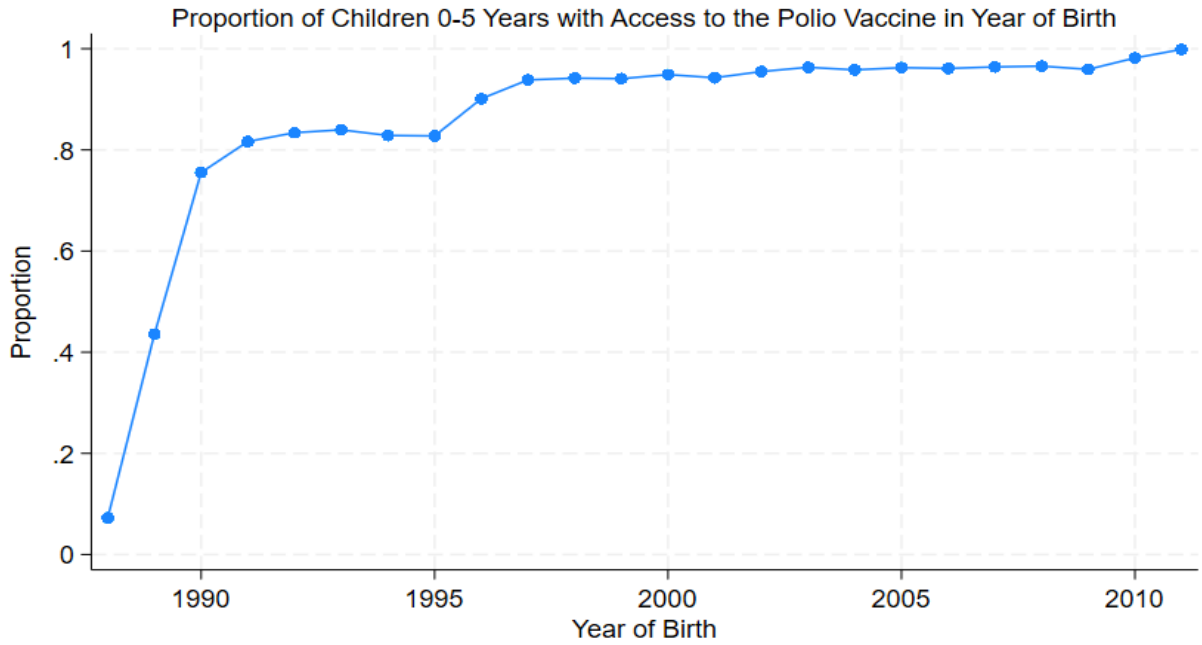
APPENDIX

Appendix Figure 1: Trends in disability proportions and children 0-5 with OPV access: Sample restricted to individual below 17 years of age

Panel A: Proportion with any disability in population, locomotor disability in any disability, and polio disability in locomotor disability, over time



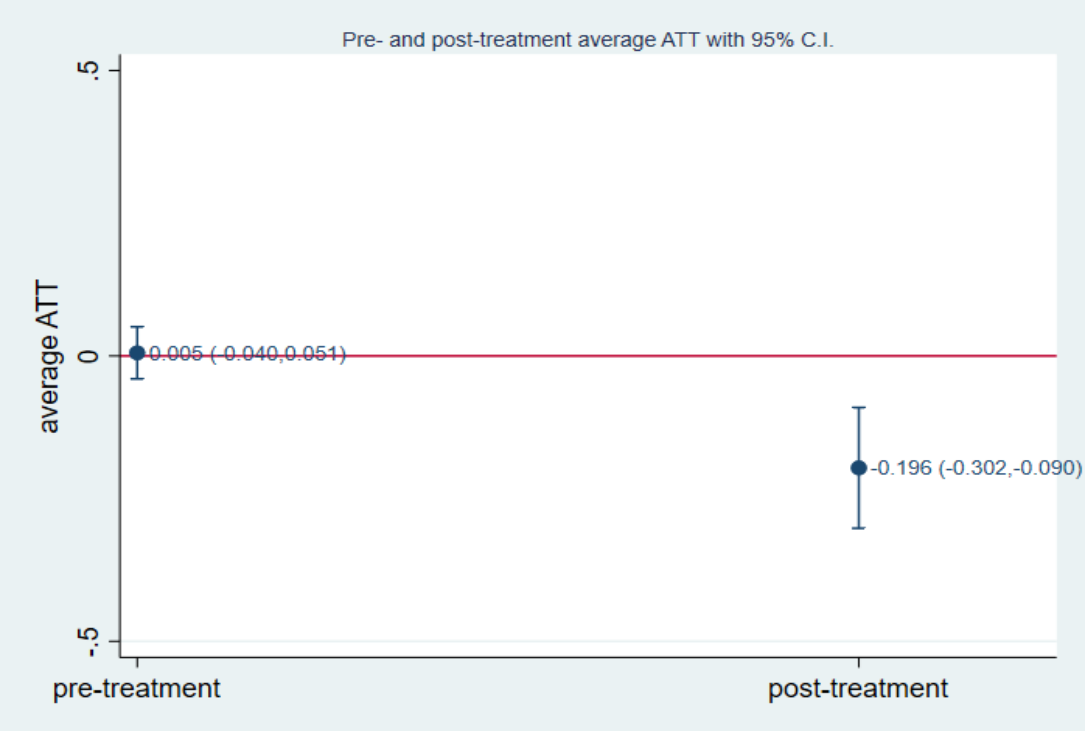
Panel B: Proportion of children 0-5 with access to the polio vaccine in year of birth



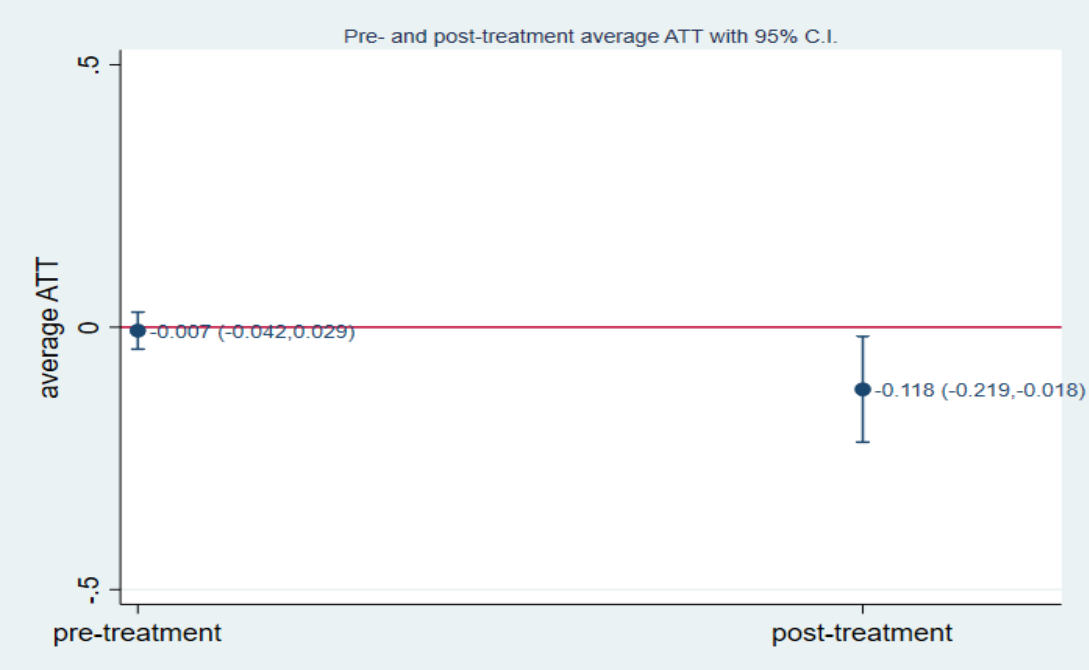
Notes: Authors' calculations using NSSO and NFHS data. Sample includes repeated cross-sections of 58,758 individuals between the ages of 0 and 16 years. Panel A reports unweighted means given the bar graph format.

Appendix Figure 2: Intent-to-treat estimates for any, locomotor and polio disability in pre- and post-treatment time periods with 95% confidence intervals

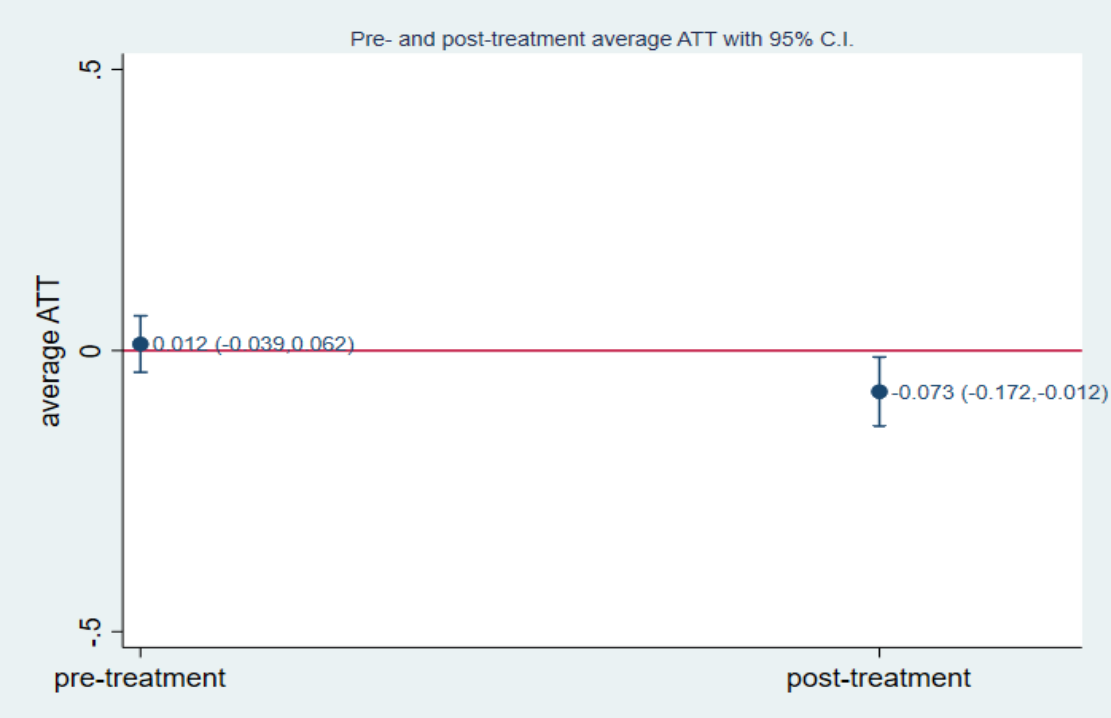
Panel A: Any disability



Panel B: Locomotor disability



Panel C: Polio disability



Notes: Authors' calculations using NSSO and NFHS data. Sample includes repeated cross-sections of 93,548 individuals in 70,429 households across 1129 districts and 64 states (in 2002 and 2018) whose year of birth is 1988 or after. Models include all controls in Appendix Table 1. Control group includes those who were never treated and not yet treated (see Sant'Anna and Zhao (2020), Callway and Sant'Anna (2021) and Rios-Avila et al. (2021) for details).

Appendix Table 1: Summary statistics for the repeated cross-section sample

Variable	Mean	Standard Deviation	Observations
Panel A: Outcomes (full sample):			
Indicator for any disability	0.026	0.159	93,548
Indicator for locomotor disability	0.015	0.121	93,548
Indicator for polio disability	0.004	0.064	93,548
Proportion with any disability in population	0.126	0.022	93,548
Proportion with locomotor dis. among those with any dis.	0.506	0.061	93,548
Proportion with polio dis. among those with loco. dis.	0.215	0.074	93,548
Panel B: Outcomes at baseline (1988):			
Indicator for any disability	0.035	0.183	5,994
Indicator for locomotor disability	0.022	0.147	5,994
Indicator for polio disability	0.007	0.086	5,994
Panel C: Controls: Individual specific			
Age in year	17.037	7.471	93,548
Woman	0.464	0.499	93,548
Parents are blood related	0.042	0.200	65,626
Illiterate/informal education	0.089	0.284	65,630
Completed primary or middle school	0.539	0.499	65,630
Completed secondary or higher secondary	0.281	0.449	65,630
Completed graduate or above	0.092	0.289	65,630
Married	0.766	0.423	93,544
Exposed to OPVs in year of birth	0.870	0.337	93,269
Exposed to tuberculosis, DPT or measles vac. in yr. of birth	0.886	0.318	93,548
Panel D: Controls: Household specific			
Low-caste household	0.735	0.441	70,429
Household size	5.449	2.466	70,429
Household land area in hectares	0.088	0.699	70,419
Household average monthly per capita consumer expen. (Rs.)	20.633	17.647	70,428
Rural household	0.701	0.458	70,429
Panel E: Control: District specific			
Year first dose of OPV became available in district	1991	5.017	1,129
Panel F: Control: State specific			
Median percent completed of state-level targets under polio campaign 1988-2016	98.487	8.436	64

Notes: Table presents weighted statistics. Sample includes repeated cross-sections of 93,548 individuals in 70,429 households across 1129 districts and 64 states (in 2002 and 2018) whose year of birth is 1988 or after. Estimates in Panels A, B and C are at the individual level. Estimates in Panel D are at the household level, in Panel E are at the district-level, and in Panel F are at the state-level.

Appendix Table 2: Differences in the pre-treatment time period in health and socio-economic characteristics by when districts gain access to OPVs

	Early access district (1)	Late access District (2)	Difference (3)
Panel A: Health outcomes			
Indicator for any disability	0.070 (0.005)	0.083 (0.009)	-0.013 (0.009)
Indicator for locomotor disability	0.043 (0.004)	0.050 (0.007)	-0.008 (0.007)
Indicator for polio disability	0.013 (0.002)	0.016 (0.004)	-0.004 (0.003)
Panel B: Socio-economic characteristics			
Age in years	24.857 (0.217)	24.371 (0.418)	0.486 (0.436)
Woman	0.474 (0.013)	0.470 (0.023)	0.004 (0.025)
Parents are blood related	0.055 (0.007)	0.053 (0.012)	0.003 (0.014)
Illiterate/informal education	0.179 (0.011)	0.210 (0.021)	-0.031 (0.023)
Completed primary or middle school	0.388 (0.015)	0.384 (0.025)	0.003 (0.029)
Completed secondary or higher secondary	0.281 (0.014)	0.256 (0.023)	0.024 (0.027)
Completed graduate or above	0.153 (0.011)	0.150 (0.019)	0.004 (0.022)
Married	0.393 (0.015)	0.431 (0.026)	-0.038 (0.029)
Low-caste household	0.747 (0.013)	0.751 (0.023)	-0.004 (0.025)
Household size	6.018 (0.080)	6.129 (0.129)	-0.111 (0.153)
Household land area in hectares	0.251 (0.022)	0.260 (0.040)	-0.009 (0.043)
Household average monthly per capita consumer expen. (Rs.)	22.542 (0.488)	23.722 (1.273)	-1.180 (1.115)
Rural household	0.718 (0.014)	0.703 (0.025)	0.014 (0.027)

Notes: Author's calculations. Columns report percentages unless otherwise specified. "Early access" includes districts that attained access before 1991, "Late access" includes districts that attained access in or after 1991, or never gained access (1991 is the mean value for the year first dose of OPV became available in district – see Appendix Table 1). Statistics reported at the district-level for the pre-treatment time period which includes year of birth before 1991. Table presents weighted statistics with standard errors in parentheses. *** Denotes significance at the 1% level, ** at the 5% level and * at the 10% level.