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Long-term Effects of India's Childhood Immunization Program on Earnings and Consumption Expenditure: Comment

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Long-term Effects of India's Childhood Immunization Program on Earnings and Consumption Expenditure Comment

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Abstract

Summan, Nandi, and Bloom (2023; SNB) finds that exposure of babies to India's Universal Immunization Programme (UIP) in the late 1980s increased their weekly wages in early adulthood by 0.138 log points and per-capita household consumption 0.028 points. But the results are attained by regressing on age, in years, while controlling for year of birth—two variables that, as constructed, are nearly collinear. The results are therefore attributable to trends during the one-year survey period, such as inflation. A randomization exercise shows that when the true impacts are zero, the SNB estimator averages 0.088 points for wages and 0.039 points for consumption.

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Summan, Nandi, and Bloom (2023; SNB) contributes to the literature on the long-term economic impacts of immunization programs (see also Bleakley 2010, Daramola et al. 2022, Atwood 2022, Atwood and Pearlman 2023). For identification, it exploits the staggered rollout of India’s Universal Immunization Programme (UIP). The UIP aimed to immunize all pregnant women and 85% of infants against six diseases (Lahariya 2014). The campaign was launched in 1985 in 31 of India’s districts, and by 1990 expanded to all the districts, which by then numbered about 466.

SNB estimates the long-term impacts of the campaign by linking two data sets, one for treatment, one for follow-up. The first identifies the fiscal year that the UIP reached each district. Pursuant to the open access policy of the Bill and Melinda Gates Foundation, which financed the research, the corresponding author of SNB shared that data set. The second data source is the labor section of India’s 68th National Sampling Survey (NSS), which was fielded in 2011–12. The data, which are publicly available, provide a snapshot in early adulthood of the first beneficiaries of the UIP.¹

Given the schedule for a staggered treatment and a single cross-sectional follow-up, a typical research approach would be two-way fixed-effect (TWFE) estimation (e.g., Duflo 2001). One would regress outcomes of interest on a treatment dummy while including dummies for the cross-sectional units, districts, and the temporal units, age in years. Instead, the SNB regressions control for all *interactions* of the two. As the text says, “The source of variation at the individual level is from the year of birth, controlling for district and age.” It is not obvious what it means to identify off of variation in year of birth while controlling for age.

What resolves this paradox is that the 12-month follow-up survey period overlapped with two calendar years, as it ran from July 1, 2011, to June 30, 2012. Thus, two people interviewed at opposite ends of the survey period could report being the same age in completed years, yet have different estimated years of birth. India’s fiscal year begins on April 1; in the SNB data, fiscal year 1985–86 is coded as 1986, 1986–87 as 1987, and so on. A 26-year-old interviewed at the start of the survey, on July 1, 2011, would be estimated to have been born in 1985. If, for example, that

¹ Linking these two data sets is complicated by the tendency of Indian districts to subdivide and even merge over time. I independently constructed the mapping from late-1980s districts to 2011–12 districts, relying primarily on Kumar and Somanathan (2016). The original and new mappings agree for 595 out of 627 2011–12 districts. The main source of discrepancy is the handling of “multi-parent districts” that combine territory from more than one older district. I code the treatment variable as missing if district has “mixed parentage,” meaning that its parents did not all receive UIP treatment on the same schedule. More precisely, I code treatment as missing if the population-weighted standard deviation of the UIP treatment years of a district’s parents exceeds a *de minimus* 0.1.

person was born in the Anantapur district of Andhra Pradesh, which received the UIP in the 1985–86 round, coded as 1986, then the person would be deemed treated in the SNB analysis. The opposite would hold for a 26-year-old compatriot interviewed just shy of a year later, at the end of the survey.

The identifying variation in SNB therefore arises from the *progression of time during the follow-up survey*. As the months passed, people of a given reported age became more likely to be estimated as treated. Since SNB finds positive impacts on wage earnings and consumption, these too must have risen as the survey progressed. Figure 1 confirms as much. It shows systematic climbs in both variables among 21–26-year-olds.

However, the figure shows that similar rises occurred among other people. Since these age cohorts—the 27-year-olds, 28-year-olds, and so on—are each homogenous in intention to treat, being all born before or all born after the UIP rollout, the SNB causal theory cannot explain why their wages and spending rose during the survey round. This strongly suggests that some factor other than vaccination is contributing to the positive associations between survey date, income, and expenditure in all cohorts. One possible factor is inflation: during the survey, consumer prices in India rose 10.6%.² This 0.1 log point rise is about half the rises seen in Figure 1. There may also be survey effects. While the NSS is a stratified random sample, the *sequence* of the sampling is not completely randomized. Indeed, the correlated fluctuations in the log expenditure curves in the bottom pane of Figure 1 appear to be caused by lumpiness in the time distribution of urban interviews.

These observations lead naturally to a couple of robustness tests for the main SNB estimation procedure: adjust the dependent variables for monthly inflation, and, more aggressively, control for survey month fixed effects to address non-random survey sequencing.

The results of introducing these changes to the main SNB specification for 21–26-year-olds appear in Table 1. First, the original results are replicated almost exactly (column 1).³ Second, the regressions are modified in a few ways. Survey weights are incorporated to correct for potentially

² Rate based on monthly consumer price index figures from International Monetary Fund, International Finance Statistics, data.imf.org, accessed May 1, 2023.

³ The match is exact with data and code that Amit Summan sent me in correspondence.

endogenous sampling.⁴ Fixed-effect singletons are dropped (Correia 2015).⁵ The reweighting modestly reduces the estimated impact on wages and increases that on consumption, bringing the two closer together. Next, the dependent variables are deflated (third column); this reduces both somewhat, making them harder to statistically distinguish from zero. Instead introducing survey month fixed effects erases any sign of impact, which is to be expected since it absorbs nearly all the identifying variation otherwise available in the SNB specification.

If the SNB results flow primarily from survey period effects such as rising inflation or non-random survey sequencing, then they should persist even when the data set is modified to eliminate the effects of vaccination. One way to do that is to perform randomization inference, in particular to jumble the treatment variable, year of program arrival, across districts.

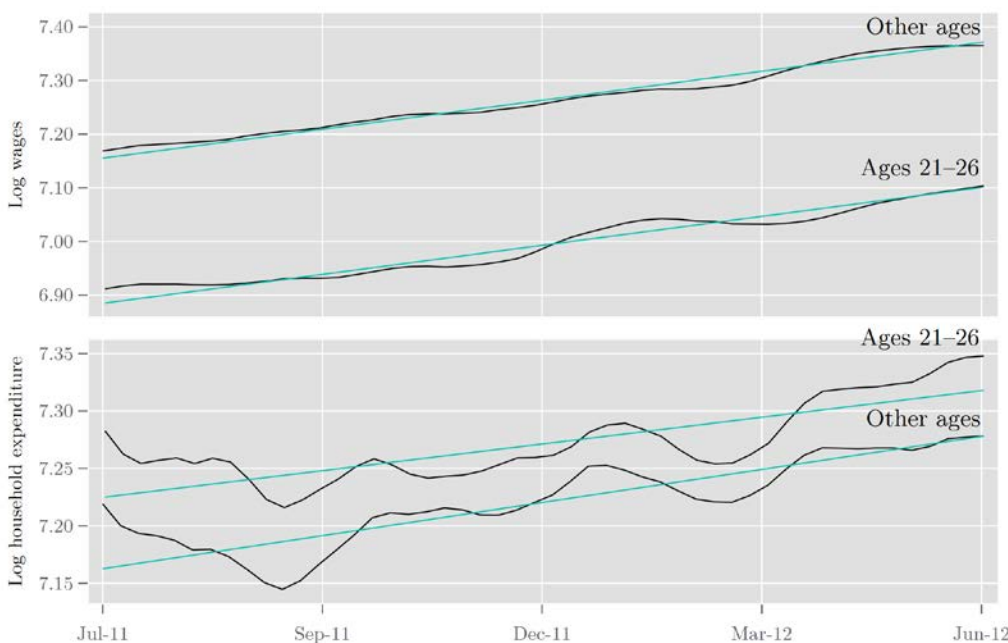
Focusing on the original and revised regressions reported in the first two columns of Table 1, Figure 2 shows the distribution of the point estimates under 1,000 random jumbings of the treatment. For the original log wage regression, the average estimated impact—when the impact is engineered by randomization to be zero—is 0.088 log points. I call that quantity the design effect. For log per-capita expenditure, the design effect is 0.039 points. Both are marked in red in the left half of the figure. Under these empirical distributions, the actual point estimate of 0.129 for log wages is somewhat above average; the two-tailed p value for the null hypothesis that the point estimate equals the design effect under this empirical distribution is 0.15. For log expenditures, the SNB estimate of 0.031 is *below* the randomization mean ($p = 0.22$). Switching to the revised specifications originally reported in the second column of Table 1 produces more internally consistent results. The log wage estimate of 0.089 is close to the design effect of 0.076 ($p = 0.79$) while the expenditure estimate of 0.056 almost exactly equals the design effect ($p = 0.99$).

In sum, a causal interpretation of the SNB estimates as long-term effects of vaccination on income and spending deserves almost no weight. The results are best seen as artifacts of inflation during the survey, as well perhaps as other survey sequencing effects.

⁴ In the second sampling stage, NSS splits households into three based on their per-capita expenditure and samples them disproportionately. See icssrdataservice.in/datarepository/index.php/catalog/92/export. Unweighted estimates are therefore presumptively biased by the endogenous sampling (Hausman and Wise 1981; Solon, Haider, and Wooldridge 2015).

⁵ In addition, a few observations with missing marital status are dropped rather than being coded as unmarried. And in constructing the SNB proxy for the probability that a person is a migrant, which is based on an initial fitting of a probit model to separate data, the constant term from the fit is correctly incorporated.

Figure 1. Log wages and log per-capita expenditures as a function of survey date in 68th NSS: linear and locally smooth fits

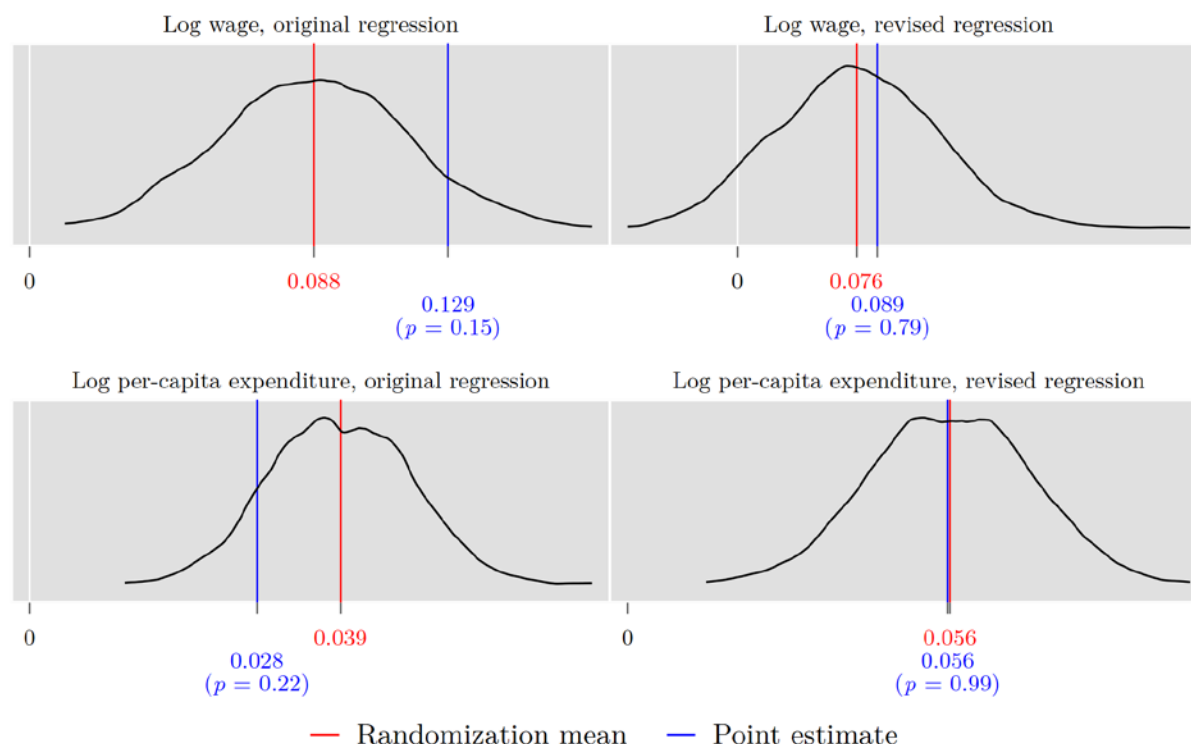


Black lines show smoothed moving averages of the indicated variables with respect to survey date. Green lines show linear fits.

Table 1. Estimated impact of UIP of log wages and log monthly per-capita expenditures, 21–26-year-olds

Outcome	Original	Revised	Revised, inflation-adjusted	Revised, with survey month fixed effects
Log wage	0.13 (0.029)	0.089 (0.052)	0.055 (0.052)	0.0066 (0.058)
Observations	10,781	9,800	9,800	9,800
Log monthly per-capita expenditure	0.028 (0.011)	0.056 (0.017)	0.025 (0.017)	−0.0024 (0.017)
Observations	46,557	46,352	46,352	46,352

Results in the first column nearly match those in the upper left corners of SNB, Tables 2 and 3. Regressions reported in second column incorporate survey weights, and incorporate several minor changes. Regressions in third column also adjust the dependent variables for inflation using monthly consumer price index from the International Monetary Fund's *International Financial Statistics*. In the final column, monthly fixed effects are instead introduced as controls. Standard errors in parentheses, clustered by district.

Figure 2. Distribution of SNB estimate under randomization of treatment

Each plot corresponds to a regression result reported in Table 1. The black curves depict the distribution of the estimate when randomly scrambling the treatment variable by district, with 1,000 draws. The vertical red lines show the “design effects,” i.e., the means of the distributions. The blue lines show the original point estimates. The displayed p values are for the two-tailed test under these distributions that the point estimates differ from the design effects.

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