Discussion of

**“Long-term and Intergenerational Effects of Education: Evidence from School Construction in Indonesia”**

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## Summary


*Objective*: investigate effect of INPRES (a large primary school construction program in Indonesia in the 1970s) on a range of outcomes among individuals exposed to the program as primary school children and on education and wellbeing of their children.

*Data*: INPRES data on school construction combined with nationally representative data from Susenas 2016. Potential sample size: 290k+ households and 1m+ individuals.

*Identification strategy*: difference-in-differences: focus on differences in outcomes between older individuals who were not directly exposed to the program and younger individuals who were– across districts that were differentially affected by INPRES.

*Main results*: Effects on individuals who were directly exposed to the program:

- large positive effect on educational attainment
- positive effect on men’s work related outcomes (e.g., higher likelihood of being a formal sector worker)
- positive effect on migration (e.g., more likely to move away from one’s birth district)
- positive effect on several measures of expenditures, taxes, proxies for housing quality and household assets
- positive effect on women’s calorie intake
- positive effect on measures of health spending, but few effects on health outcomes
- positive effect on the educational attainment of one’s spouse (improvement in matching outcomes)
- no effect on utilization of welfare programs

Effects on children of individuals who were exposed to the program:

- positive effect on educational attainment of children, especially when the mother (as opposed to the father) was exposed to INPRES
- limited effect on wellbeing proxies


## Major comments

1. Was INPRES the <u>only</u> program that Indonesian government was pursuing in the 1970s that may have affected primary school-age children or their families? Perhaps not only did poorer districts get more of new schools, but also more of pediatric facilities (or other relevant public investments). In other words, are you estimating the effect of new primary schools brought into existence per se,

or a combined effect of new schools and all other relevant policy changes? The answer to this question will inform the cost-benefit analysis, because you may be understating the true cost.

2. Why is $School_j$ fixed for all individuals in the 1968-72 birth cohort from district $j$? Wouldn't individuals born later (e.g., in 1972 vs. 1968) have a larger number of schools available when time came for them to start schooling? After all, school building construction takes time. An implication is that Equation (1) may embeds measurement error in the interaction term $School_j \cdot Young_{it}$.

3. Was the quality of schools uniform? Was the design the same, access to transportation the same, etc.? If the government rushed to construct schools in poorer districts, wouldn't you expect the quality of schools to systematically differ between poorer and richer districts? In such a case, is $School_j$ a good variable to use in the first place (as opposed to some measure of quantity adjusted for quality)?

4. Your results may be explained in terms of a likely marginal effect of schools. The district fixed effect helps to account for the impact of the pre-existing stock of schools, among other things. Since poorer districts had fewer primary schools per capita than richer districts before INPRES, it could be the case that the marginal effect of a school constructed under INPRES is larger in a poor district compared to a rich one. Then, if the government constructed more schools in poorer districts, the positive coefficient on $School_j \cdot Young_{it}$ is not due to a linear effect of $School_j$, but rather an aggregation of comparatively larger (but diminishing) marginal effects in poor districts. I feel that the specification of Equation (1) is missing out on the nonlinearity of the school effect.

5. I'm a bit unsure what to make of your discussion of migration. In one place, you say that by 2016, about a quarter of the sample had migrated away from their birth district. But in another place, you indicate that migration was very small. These statements need to be reconciled. In any event, unless migration between birth and primary school-starting age is negligible, controlling for the number of schools in an individual's place of birth, as opposed to the district in which the individual went to school, may induce measurement error. I acknowledge a potential endogeneity issue here (if families migrate for schooling reasons), but replacing the endogeneity problem with a measurement error problem doesn't seem like an attractive solution.

6. The range of cost-benefit estimates in Table 14 is wide. Which one is closest to reality (or is your preference)? Scenario 5? Scenario 10? It would help to provide some guidance for the reader as to what you think is most realistic and why. Also, if general equilibrium effects could be quantified in your setting, would you expect the cost-benefit analysis to substantially change?

**Minor comments**

1. Since very little can be said definitively about general equilibrium effects, why not move that discussion to an appendix?

2. The $X_j B_t$ term doesn't seem 100% OK to me. If it's supposed to capture district-specific time-varying factors, it should have a double index $jt$. For example, the availability of sanitation programs can change within a district over time.

3. What does the clustering of standard errors at the district level really achieve, especially since you already control for $\mu_j$? Explain.

4. The five-level index on the outcome variable in Equation (2) will be a challenge for many readers.

5. Given the notation in Equation (1), it won't be immediately obvious to a non-econometrically-minded reader that you perform a difference-in-differences analysis. It may help to clarify that $School_j$ is not included in the equation because of the presence of the district fixed effect $\mu_j$. Similarly, $Young_{it}$ is not included because of the presence of the cohort fixed effect $\delta_t$.

6. Could expenditures be systematically underreported? What would be the implications for your analysis?

7. Much of the discussion in the paper is relegated to footnotes. Consider incorporating some of that information into the text body.

8. A brief comment may be needed that you ultimately care about health outcomes, whereas larger health expenditures are not necessarily indicative of better health: health expenditures are often higher among sicker individuals. Perhaps health expenditure regressions need to control for health status in the first place.

9. p. 23, footnote 35: indictor → indicator

10. Section 5.3 can be moved to an appendix.

11. Conclusion should probably contain a brief discussion of costs vs. benefits and some suggestions for policy makers regarding future programs.