On the Rank-Rank Model of Intergenerational Mobility: Pitfalls for Policy Evaluation

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ABSTRACT

We analyze the challenges in adopting the rank-rank model of intergenerational mobility for policy evaluation. For rank-based analysis of intergenerational mobility, it is standard to calculate cohort-specific ranks from the national distribution, but separately for children's and parents' generations. This ensures that children's inherited socioeconomic status and their life outcomes are measured on common scales irrespective of location and social groups. However, national ranks put the treatment and comparison groups together, and thus, a policy intervention leads to mechanical changes in ranks in the comparison group when the ranks of the treated individuals change because of the policy. We discuss how to deal with this contaminated comparison problem in the context of widely-used research designs: RCTs, Instrumental Variables (IV), and Difference-in-Difference (DiD). In a RCT design with a binary treatment assignment, a simple solution is to calculate the ranks separately for the treatment and control groups. In an IV design, the ranks should be calculated separately for different values of the instrument. For a DiD design, an additional concern is how to avoid mechanical changes in the ranks of the pre cohorts following the policy intervention: calculate the ranks separately for pre and post periods. If the policy affects only the children, then, for all research designs, it is desirable to keep the parental ranks at the national level so that children's inherited socioeconomic status is measured on a common scale. As an empirical application, we provide evidence on the effects of Inpres schools on intergenerational educational mobility in Indonesia using the DiD design developed by Duflo (2001). The evidence suggests that the conclusions regarding the impact of Inpres schools depend critically on the way ranks are calculated. If we follow the current practices when calculating the ranks, the DiD estimates suggest that the 61,000 primary schools failed to affect relative mobility even though it improved absolute mobility for the children from low-educated families. In contrast, when the ranks are calculated to tackle the mechanical contamination problem, the evidence, especially from the correct functional form (quadratic), suggests that Inpres schools improved both relative and absolute mobility of the disadvantaged children. The Inpres schools led to higher intercept and quadratic coefficient of the mobility equation while reducing the linear coefficient. The analysis presented here has important implications for economists and sociologists working on intergenerational mobility.

JEL Codes: I24, J62, O12, D3

Key Words: Rank-Rank Model, Intergenerational Mobility, Causal Effects, Policy Evaluation, Mechanical Changes in Ranks, Contaminated Comparison, Inpres Schools, Indonesia

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(1) Introduction

Interest in intergenerational mobility among policymakers, researchers, and general population has experienced a surge in recent decades, and this is true in both developed and developing countries. This resurgence in interests in understanding the pattern and mechanisms of intergenerational mobility owes its roots to two factors. First, political interest in, and in some cases commitment to, making mobility the central policy goal, rather than inequality.[2] There is broad support among people for the idea that inequality due to factors inherited by birth is undesirable, but inequality that arises from heterogeneity in effort is fair. Second, highly influential empirical work using big data initiated by the seminal papers by Chetty and his co-authors in the context of USA.

The publication of Chetty et al. (2014) (on income mobility in the United States of America) can be considered a methodological watershed in the literature with a clear preference for rank-based measures of mobility in the recent literature while the earlier literature focused on mobility models based on log income (for income mobility) and years of schooling (for educational mobility).[3] Most of the existing papers relying on rank-based measures of intergenerational mobility are descriptive, with a special emphasis on mapping out spatial heterogeneity (e.g., the opportunity atlas for USA by Opportunity Insights). As a natural evolution of this research agenda, many researchers are taking the next step to explore the effects of government policies on intergenerational mobility using the rank-rank model: examples include Chetty and Hendren (2018a), Chetty and Hendren (2018b), Asher et al. (2023), Agarwal et al. (2023), Yu et al. (2020), and Manian et al. (2023).

While the rank-rank model provides important evidence as descriptive measures of mobility across space and time, there are some conceptual difficulties in adopting the rank-rank model for causal analysis and thus for policy evaluation. The focus of this paper is on a particular issue that has largely been ignored in the recent literature. The standard rank-rank model of intergenerational mobility relies on national ranks calculated separately for children's and parent's generations.[4] The national distributions ensure that all the children

[2]President Obama delivered remarks on economic mobility on December 04, 2013 saying "the premise that we're all created equal is the opening line in the American story. And while we don't promise equal outcomes, we have strived to deliver equal opportunity −− the idea that success doesn't depend on being born into wealth or privilege, it depends on effort and merit."

[3]The surveys of the literature on intergenerational mobility by Solon (1999), Black and Devereux (2011), Bjorklund and Salvanes (2011) do not discuss the rank-based model of intergenerational mobility.

[4]In some applications, ranks for children are calculated separately for different age cohorts. See, for example,

irrespective of their location are measured on a fixed scale, for both measuring children's inherited socioeconomic status (national rank in parents generation) and children's own life outcomes (national rank in children's generation). However, government policy interventions are often not of national scope, and more likely to affect a subgroup, for example, based on geographic location or other socioeconomic characteristics such as ethnicity, caste, or income level (means-tested programs). When a government policy intervention affects a subset of the children and changes their ranks, then, by construction, the ranks of some other children who did not receive the treatment are mechanically altered in the post intervention period when we calculate ranks from children's national distribution. We do not have a comparison/control group which remains unaffected by a policy intervention and the stable unit treatment value assumption (SUTVA) is violated.[5] This is expected to bias the estimated effects, and it is, in general, not possible to pin down the direction of the bias a priori. Also, it is difficult to define the concept of causal effects without the SUTVA.[6]

We provide an in-depth analysis of this contaminated comparison/control group problem in the context of widely-used research designs. Our primary goal is to analyze the issues relevant for observational data, as most of the existing studies on intergenerational mobility rely on observational data, and exploit policy or natural experiments to estimate causal effects. Our focus is on the Instrumental Variables (IV) and Difference-in-Difference (DiD) designs, but we also analyze the issues related to data generated by Randomized Control Trials (RCTs). The RCT design offers a convenient benchmark to understand some of the fundamental issues in a transparent manner. For data from a RCT where imperfect compliance is not a major issue, a simple strategy to deal with the contaminated comparison problem is to calculate the ranks separately for treatment and comparison groups. In this case, when a randomized

Chetty and Hendren (2018a), and Manian et al. (2023).

[5]It is important to note that there are some contexts where the concern about contaminated comparison may not a serious one. For example, when studying a small scale policy intervention if the comparison group in the data set is large enough, the mechanical changes in the ranks of comparison group might be relatively small, and the estimates using national ranks are likely to be credible. An example of this is the effects of "moving to opportunity" in the USA studied by Chetty and Hendren (2018b) and Chetty et al. (2020). There are some polar cases where the contaminated comparison is not an issue (e.g, if a program increases income of the highest percentile) because they leave the ranks undisturbed. But these polar cases are of little relevance for actual data sets.

[6]The SUTVA is necessary to define causal effects not only in potential outcomes framework, but also in the approaches developed by Judea Pearl and his co-authors (do-calculus), and Heckman (econometric causality). Some Bayesian statisticians, however, define causality in terms of decision theory that does not rely on the idea of counterfactual. See, for example, Dawid (2000), and for a critique, see Pearl (2000).

policy intervention changes the ranks of children in the treatment group, the comparison group remains unaffected. However, note that, if everyone accepts the treatment offer, a policy can change ranks among the treatment group only if there is heterogeneity in the treatment effect.[7] When compliance is imperfect, the ranks can change among the children in the treatment group even if the treatment effect is constant. With imperfect compliance, it is standard to estimate the intent to treat effect by using the randomization as an instrument for the actual treatment status in an IV design. This suggests that, in a just-identified IV model with a binary instrument, we can tackle the contaminated comparison problem by calculating the ranks separately for the two groups defined by the instrument dummy. A straightforward generalization to the multi-valued instrument is: we should calculate the ranks separately for each value of the instrument.[8] In a DiD design, there are two sources of mechanical changes in ranks: across pre and post groups, and across treatment and comparison groups. To deal with the mechanical changes in ranks for the pre-intervention period, it is desirable to calculate the ranks separately for pre and post periods within the treatment and the comparison groups. A general insight for all research designs is that if the policy affects only the children's generation, then it is desirable to calculate the ranks for parental generation from the national distribution. This ensures that the socioeconomic status of children are measured in a common scale across treatment and control groups.

As an empirical application of the methodological issues and guidelines discussed above, we report estimates of the effects of the Inpres school construction in Indonesia on intergenerational educational mobility, a policy experiment originally studied by Duflo (2001, 2004). The research design of Duflo (2001) is a DiD, and we follow her approach closely. The evidence suggests that the conclusions regarding the effects of Inpres school construction on intergenerational educational mobility depend critically on the way the ranks are calculated. The evidence on functional form rejects the widely-used linear model in favor of a quadratic model and we focus on the correct quadratic model.[9] When the ranks are calculated for each

[7]If there is 100% compliance and the treatment effect is constant, then the outcome is shifted by the same amount for everyone in the treatment group, and the ranks within the group remain unchanged. This means that we cannot capture such economy-wide average effects of a program. However, this is not an issue because the main argument for using a rank-rank model is that we want to get rid of such economy-wide growth effects.

[8]For a continuous instrument, we need to define intervals, for example, quartiles, to utilize this approach.

[9]We report the estimates for the linear model as a benchmark, as it is widely-used in the current literature. Evidence suggests that one can get very different conclusions if the linear model is used when the correct functional form of the mobility CEF is quadratic.

generation at the national level including both the pre and post intervention periods, the estimates from the DiD design (quadratic mobility model) suggests that the 61,000 primary schools constructed under the Inpres program failed to affect absolute and relative educational mobility in Indonesia, and this conclusion holds for both sons and daughters. But these estimates suffer from two sources of bias: mechanical changes in ranks across both pre-post and treatment-comparison groups. When the ranks are calculated separately for pre and post cohorts (eliminating pre-post source of bias), but at the national level for combined treatment and comparison groups, the estimates suggest that there is no impact on relative mobility even though absolute mobility improved, especially for children from disadvantaged families. These estimates, however, suffer from mechanical changes in ranks across treatment and comparison groups. To deal with this remaining source of bias, we calculate children's ranks at the district level given that treatment intensity varies across districts. To measure children's inherited socioeconomic status on a common scale, we calculate the parental ranks at the national level, but separately in the pre and post periods. The estimates from this preferred approach suggest very different conclusions: the Inpres schools substantially improved both relative and absolute mobility of the children born to low-educated fathers. We also report estimates from binary indicators of treatment intensity, using median and quartiles. For quartiles, we estimate the effects for the 2nd, 3rd, and 4th quartile separately, with the 1st quartile as the common comparison group. An advantage of the binary treatment set-up is that there is a one-to-one mapping from rank of a child to years of schooling within the treatment group which enables meaningful comparisons across districts and social groups.[10] The evidence again shows that the conclusions are substantially different when we adopt the rank calculations guidelines discussed above.

The remainder of this paper is organized as follows. In section (2), we provide a brief discussion on the rank-rank model of intergenerational mobility in economic literature. Section (3) discusses the challenges in adopting the rank-rank model of intergenerational mobility for policy evaluation with a focus on the mechanical contamination of the comparison group problem. Section (4) lays out the ways to deal with the contaminated comparison problem in

[10]When we use district level variation in treatment intensity and calculate the ranks at the district level, the same rank is assigned to different levels of schooling depending on the district. While the district-level evidence is useful to understand the role of local interactions, and may be of interest to local policymakers, for national policymakers the estimates from the binary set-up might be more useful.

three widely-used research designs: Randomized Control Trials (RCTs), Instrumental Variables (IV), and Difference-in-Difference (DiD). Section (5) provides a brief discussion on the role of policymakers objective. We report evidence from an empirical application in section (6) which estimates the effects of Inpres primary schools in Indonesia on intergenerational educational mobility. The paper ends with a summary of the main results and guidelines for researchers working with the rank-rank model for policy evaluation.

(2) The Rank-Rank Model of Intergenerational Mobility in Economics: A Brief Account

To the best of our knowledge, the rank-based empirical model of intergenerational mobility was first used in the economics literature by Dahl and DeLeire (2008) in the context of income mobility in the USA, and it was adopted by Chetty et al. (2014).[11] The motivation for Chetty et al. (2014) was empirical. They were looking for a scalar mobility measure that can summarize the income mobility pattern across different sub-national levels (commuting zones). The standard measure of relative mobility in a vast literature on intergenerational income mobility is intergenerational elasticity (IGE), estimated as the slope of a regression of log of children's income on log of parent's income. They find that the log-log income conditional expectation function (CEF) is concave, implying that IGE varies by the income level of parents, and it is not possible to summarize the mobility in a commuting zone with a single summary statistic. In contrast, they show that the CEF of income ranks is linear in the USA, and thus, one can use the rank-rank slope as a convenient measure of mobility.[12] They also define a measure that represents the expected income rank of children born to the lower half of the parental income distribution, denoted as P_{25}. With a linear CEF, P_{25} can be estimated by the expected rank of the children whose parents are located at the 25th percentile of parental distribution. Most of the rank-based studies of intergenerational mobility (for both income and education) follow the approach developed by Chetty et al. (2014) which relies on ranks calculated from the national distribution of an outcome, separately for each generation.

Conceptually, the rank-rank model is substantially different from the measures of mobility based on log of income or years of schooling. In the context of income mobility, Chetty et al.

[11] Chetty et al. (2014) acknowledge that they adopted the rank-based model from Dahl and DeLeire (2008) which remains unpublished to date.

[12] The literature that followed shows that the linearity of the rank-rank CEF found for income mobility in USA does not hold in a number of countries. See the discussion by Deutscher and Mazumder (2023).

(2014) emphasized two aspects: growth and inequality. When we calculate percentile ranks of income, we create a social ladder with 100 rungs in each generation, and any increase in the income at the top of the distribution in children's generation because of economic growth is neutralized.[13] The distribution of calculated income ranks is uniform by construction, and the variance (inequality) in income remains the same across generations. Thus, the rank-based measures of mobility are not affected by economic growth and changes in inequality across generations. For a continuous variable such as income, the rank-rank slope is a copula, and is independent of changes in the marginal distributions. However, for discrete variables such as education (years of schooling), ranks usually do not yield a uniform distribution, and thus the variances across generations differ.[14]

(3) The Pitfalls: Comparable Scales vs. Credible Comparison Group

A central empirical choice for a rank-based mobility analysis is that we have to decide if the ranks are calculated at the national level or separately for different subgroups. The analysis of Chetty et al. (2014) uses ranks calculated from the national distribution, but the rank distributions are generation specific. The logic behind ranks in the national distribution is that it puts different geographic areas in a common scale and thus enables meaningful comparison of mobility across different commuting zones of USA which is the focus of their analysis. When the goal is to provide descriptive evidence on the spatial heterogeneity and evolution of mobility in a country, this clearly is the right approach.

However, if, instead, the focus of a study is on estimating the effects of a government policy, we have to consider the implications for the treatment and comparison groups. When ranks are calculated at the national level, it includes both the treatment and comparison groups. As noted earlier, when a policy intervention improves the ranks of treated individuals, in a national rank distribution, the rank of other individuals are mechanically affected. By construction, it is not possible to keep the ranks of the comparison group unaffected by a policy intervention. We call this mechanical contamination of the comparison group problem (henceforth "contaminated comparison" problem). The main contribution of this paper is to analyze the contaminated comparison problem in the context of widely-used research designs

[13] As discussed in detail by Ahsan et al. (2022), for education, we do not need to worry about such growth at the top because the maximum education remains the same (Ph.D.) across generations.

[14] For a more complete discussion on the conceptual issues related to the rank-rank model in the context of intergenerational educational mobility, please see Ahsan et al. (2022).

such as Randomized Control Trials (RCTs), Instrumental Variables (IV), and Difference-in-Difference (DiD).

An obvious solution to the contaminated comparison problem is to calculate the ranks separately for treatment and comparison groups.[15] However, the price we pay is that different groups are then measured in different scales, for example, a 10th percentile rank in the income distribution of a minority group eligible for a government program (say, the public sector employment quotas in India for lower castes such as Scheduled Castes and Scheduled Tribes) will, in general, mean a substantially different level of real income (and consumption) relative to the real income of the 10th percentile in the majority group. The problem of different scales is, fortunately, more easily dealt with, for example, by converting the rank-based estimates of a policy impact to income level or years of schooling.

Another important implication of group-specific ranks is emphasized by Deutscher and Mazumder (2023): when ranks are calculated separately for different groups in a generation, the concept of mobility captured in such an analysis is different from that when we rely on ranks in a national distribution. If we increase the income/education of everyone by a given amount, the ranks in the national distribution do not change, so the estimates at the national level give us a measure of relative mobility. In contrast, when the ranks are calculated from the national distribution, but the rank-rank regression is estimated at the subnational level (say district or county), the estimated measures of mobility, including the rank-rank slope, capture absolute mobility. If we increase the income/education of the people in a district or a county by the same amount, the ranks of the people in this district increases in the national distribution because the income/education in the other districts/counties have not changed. Thus, measures of mobility calculated from a rank-rank CEF estimated at the subnational level do not refer to relative mobility when ranks are calculated using the national distribution.

Now consider the case where the ranks are calculated separately at the subnational level. If we increase the income/education of everyone by the same amount in a district/county, the calculated ranks do not change. The same conclusion holds if we increase income/education of everyone by the same amount in all the districts (nationally). This implies that the estimated

[15] One might argue that the national distribution provides a better picture of the broad cross section of a society because it includes the comparison/control groups. However, note that if the identification is valid in a research design, then the rank-rank CEFs must be similar across treatment and control groups in the absence of policy intervention (please see the next footnote on RCTs). Thus, we do not learn anything new about the socioeconomic structure by including the control group when calculating the ranks.

measures of mobility based on subnational ranks give us relative mobility irrespective of whether we run the rank-rank regression at the national or subnational levels.

(4) Dealing with the Contaminated Comparison Problem in Alternative Research Designs

We consider how to deal with the problem of contaminated comparison group problem in commonly-used research designs: RCTs, Instrumental Variables (IV), and Difference-in-Difference (DiD). For RCT and IV designs, we consider the following empirical model:

$$R_i^c = \theta_0 + \theta_1 R_i^p + \theta_2 D_i^T + \theta_3 \left(R_i^p \times D_i^T \right) + \epsilon_i \tag{1}$$

where R_i^c is the rank of child i in a suitably defined distribution of children's outcome (income, education) and R_i^p is the rank of the parents of child i in a suitably defined distribution of the same outcome in parental generation. D_i^T is a treatment dummy which takes on the value of 1 for the children (and/or parents) who are affected by a policy intervention, and zero otherwise.

(4.1) Randomized Control Trial (RCT)

If a policy intervention is randomized, and the incidence of noncompliance is ignorable, then we can estimate equation (1) by OLS. The critical question is how should we calculate the ranks for children and parents? As noted above, a simple way to deal with the mechanical contamination of the control group is to calculate the ranks separately for treatment group and control group.[16] But should we calculate separate ranks in both children's and parent's generations?

The answer depends on the nature of the policy intervention. Some policy interventions affect only the children's outcomes (income/education) without affecting the parental outcomes. For example, randomized provision of school inputs such as books and computers affects children's educational outcomes, and this effect may vary by the socioeconomic background of a child as measured by parental education. But such school interventions do not affect parental education or income. In this case, it is desirable to calculate the ranks in parental generation at the national level so that children's socioeconomic status is measured in a common scale across the treatment and comparison groups. Thus the appropriate strategy is to calculate

[16]Since the treatment is randomized, we would expect the counterfactual rank-rank CEF in the treatment group to be similar to the rank-rank CEF in the control group.

the ranks separately for treatment and control groups in children's generation, but calculate the parental ranks from the national distribution.

Note that the appropriate strategy also depends on the outcome of interest. For example, randomized microcredit interventions in developing countries are expected to affect the income of a treated household, but it does not affect the education level of the borrowers because they have already completed their education.[17] When estimating the effects of access to microcredit by parents on intergenerational *educational* mobility of children, we can thus treat the parental ranks as unaffected by the microcredit interventions, and implement the rank calculation guidelines discussed above. In contrast, a long-term analysis of the effects of microcredit on intergenerational *income* mobility will need to deal with mechanical changes in ranks of the control households in national distributions for both parents and children. In this case, the appropriate strategy is to calculate ranks separately for treatment and comparison groups in both children's and parental generations.

The above discussion assumes that imperfect compliance is not an issue in the randomized interventions which is rarely the case. In fact, in many cases, the uptake of randomized interventions is low: for example, it is not uncommon to have a 20%-30% uptake rate in the microcredit interventions in developing countries. In this case, the dummy for randomization is used as an instrument for the actual treatment status to estimate the intent-to-treat parameter. We discuss the issues in the IV research design next.

(4.2) Instrumental Variables (IV)

We consider the case of a just-identified IV model where there is a single binary instrument Z_i which takes on the value of 1 if a child (or parent) is offered the treatment, and zero otherwise. This instrument is used to estimate the effects of binary treatment status D_i^T which consists of the subset who accept the treatment conditional on receiving the invitation to participate. The instrument can be generated by a RCT as discussed above, or it can be based on a natural or policy experiment.

Following the logic of the discussion above in section (4.1), to deal with the contamination of comparison group, we need to calculate the ranks separately for the groups defined by the

[17]Most of the microcredit loans are given for productive activities, and only a few programs offer education loans for the children of the borrowers. Among the exceptions, BRAC and Grameen Bank in Bangladesh offer such educational loans, especially for higher education.

instrument. This mimics the set-up for RCT in section (4.1). The argument carries over readily to the case where the instrument is a discrete variable, for example, defined at the district level. The ranks should be calculated at the district level in this case. The basic insight here is that we need to calculate ranks at the different values of the instrument, with the canonical binary instrument as a special case where it takes only two values: 0 and 1. A limitation of this approach is that it may not be feasible to implement this approach when the instrument takes on a large number of values (too few observations in a group to calculate ranks meaningfully), with a continuous instrument as the limiting case. We consider some alternatives for such cases in sub-section (6.2) below.

The issues related to whether to calculate ranks at the national distribution for parental generation discussed in subsection (4.1) remain valid for an IV design. The guiding principle is that if an intervention does not affect the parental outcomes, then the ranks should be calculated at the national level for parental generation, even though the ranks for children are calculated at the different values of the IV.

Note that the discussion above implicitly assumes that we have a just identified model, i.e., one instrument for one endogenous variable. With multiple instruments, one can first predict the endogenous variable using the set of instruments (along with other exogenous variables in the model) from the first stage regression of the standard 2SLS set-up, and then use the predicted endogenous variable as a single instrument for IV estimation.[18] This procedure converts an over-identified IV model into a just-identified IV model, and has some attractive features (see Angrist and Kolesár (2023)). For example, the weak instrument bias is the least in a just-identified model. The exclusion restrictions imposed are also weaker because we do not impose exclusion on each of the instruments separately (see Kolesár et al. (2015)).

(4.3) Difference-in-Difference (DiD)

We focus on the canonical 2×2 DiD set-up (two time periods: pre and post intervention, and two groups: treatment vs. comparison), as it is suitable for the exposition of the main issues involved. The empirical model is:

$$\begin{aligned} R_{it}^{c} =& \delta_0 + \delta_1 D_i^T + \delta_2 D^{post} + \delta_3 \left(D_i^T \times D^{post}\right) + \delta_4 R_{it}^{p} + \delta_5 \left(D_i^T \times R_{it}^{p}\right) \\ &+ \delta_6 \left(D^{post} \times R_{it}^{p}\right) + \delta_7 \left(D_i^T \times D^{post} \times R_{it}^{p}\right) + \varepsilon_{it} \end{aligned} \tag{2}$$

[18]Some authors call the prediction regression a "zero-stage" regression to differentiate it from the first stage of a 2SLS (see, for example, Rajan and Subramanian (2008), and Emran et al. (2020)).

where R_i^c is the rank of child i in a suitably defined distribution of children's outcome (income, education) and R_i^p is the rank of the parents of child i in a suitably defined distribution of the same outcome in parental generation. D^{post} is a dummy variable that takes on the value of 1 when the period t is after the policy intervention, and zero for the pre-intervention period. The focus of the policy evaluation is on the parameters δ_3 and δ_7 which capture the intercept effect and the slope effect of the policy.

The two dummy variables, D_i^T and D^{post}, define four groups, and one can think of calculating the ranks for each generation in a number of different ways. First, one can calculate the ranks at the national level (including all districts or counties) and including pre and post periods. Second, the ranks can be calculated at the national level, but separately for pre and post periods. Third, we can calculate ranks separately for treatment and comparison but including both pre and post observations. Fourth, ranks can be calculated separately for each of the four sub-groups defined by the two dummy variables. If we follow the first approach, the calculated ranks include two sources of contamination: in addition to the mechanical effects of rank changes caused by the intervention on the comparison group, there is also similar mechanical changes in ranks across pre and post groups. This implies that it is necessary to calculate the ranks separately for the four different subsamples defined by the treatment and post dummies in equation (2). However, whether we can rely on the national distribution for parents depends on the policy under consideration. As noted before, when the policy does not affect the outcome in parental generation, we can use national rank distribution to ensure that children's socioeconomic status is measured in a common scale.

Many DiD studies rely on treatment variations across districts or counties, see, for example, Duflo (2001, 2004), Topalova (2010), and Edmonds et al. (2010). The discussion above suggests that the ranks should be calculated at the district level separately for pre and post periods in such a research design. In section (6) below, we provide evidence on these issues by estimating the effects of a large scale public primary school construction in Indonesia on intergenerational educational mobility in a DiD design developed by Duflo (2001).

(5) The Role of Policymaker's Objective

A basic premise of the analysis so far is that it is desirable to have comparability across the full sample of data. This implicitly assumes that the relevant policymaker is at the national (federal) level, and is focused on the comparisons across different regions and groups.

However, when the policymaker is at the local level, for example, a state minister in India, or the governor of a state in the USA, the primary focus may not be at the national level. In such case, it might be more appropriate to calculate the ranks at the state level even though the policy under evaluation is of national (federal) scope.

A related case is when the focus of the policymaker is to understand the role of social interactions (neighborhood role model effects, for example). The rank in the national distribution will be too coarse for understanding the local level interactions. It might be more appropriate to use local ranks to estimate the policy impacts in such a case.

(6) An Empirical Application

To understand whether the issues and guidelines discussed above for policy evaluation using the rank-rank model of mobility are empirically important, we provide evidence using alternative rank calculations. Note that when a policy intervention affects a small subgroup, and the comparison group in the data set is large, the mechanical changes in the ranks of individuals in the comparison group in a national rank distribution is likely to be small. As a result, the bias in the estimated policy effect may not be large enough to change major conclusions. A prominent example is the moving to opportunity in the USA, studied by Chetty and Hendren (2018b) and Chetty et al. (2020).[19] But many policy interventions are large enough to result in substantial mechanical contamination of the comparison group when ranks are calculated at the national level. In this section, we analyze a large educational policy in Indonesia to understand the empirical importance of the issues raised above. The research design for this policy experiment is a DiD, and thus the discussion in subsection (4.3) is of special relevance.

We study the effects of Inpres school construction program in Indonesia in the early 1970s, originally studied by Pitt et al. (1993) and Duflo (2001, 2004). The Inpres school program built more than 61,000 primary schools in the early 1970s, and doubled the number of primary schools in Indonesia in 5 years.

(6.1) Data, Empirical Strategy, and Estimating Equations

We use full count census data from the 2000 census in Indonesia. Following Duflo (2001),

[19] This point was made by John Friedman in his talk at the Research Network on Intergenerational Mobility (RNIM) virtual seminar series, January 17, 2024.

we define the children born between 1968 and 1972 as exposed (post cohorts in equation (2) above), and the children born between 1957-1962 as unexposed (pre-cohorts). The exposed and unexposed cohorts together give us an estimation sample of 2,048,164 father-child pairs. This large sample size is especially useful for our empirical exercise because the treatment intensity varies by district (see below), and we have large enough sample at the district level to calculate the ranks with confidence.[20] The household heads are the fathers, and the children and their father were living in the household at the time of the census. The years of schooling for both children and fathers is based on the education level a respondent has completed. The treatment variable is the number of Inpres schools in a district per 1000 children (provided by Esther Duflo). The maximum value of treatment intensity is 8.6 and the minimum is 0.59, with a mean intensity of 1.86.

We follow closely the empirical strategy of Duflo (2001), and include all the controls used in her study. The plausibility and validity of this research design has been tested in a series of papers that analyzed the effects of Inpres schools on a myriad of outcomes (see, for example, Ashraf et al. (2020), Bazzi et al. (2020), and Mazumder et al. (2019)). The accumulated evidence provides strong support in favor of this identification strategy. We check the validity of the parallel trend assumption in our context using the event-study approach. The evidence from event study graphs is consistent with the parallel trend assumption (omitted for the sake of brevity). As discussed by Ahsan et al. (2023), it is highly unlikely that the "no anticipation" condition of a DiD design is violated in this context. While the parents can invest in educational materials in anticipation of new school, these expenditures are for the children who will go to these schools, and they do not affect the unexposed older cohorts as they either already completed primary schooling, or discontinued education.

In section (4.3), we couched the discussion on the DiD design in terms of a two-way fixed effects model. The basic model was extended to capture the effects of policy on the intergenerational persistence between parent and children (the slope effect). For the empirical analysis, we adapt the model to reflect the fact that our treatment intensity (new public primary schools) varies across districts. A second issue is the functional form assumption. The empirical models in section (4) are built on the assumption that the conditional expectation function of children's outcome given parental income is linear. Although, the linear model is

[20]Other data sets such as Indonesia Family Life Survey (IFLS) is not suitable for this exercise, as the district level sample size becomes too s mall for some of the districts.

widely-used in the existing evaluation literature, the recent empirical evidence suggests that the rank-rank CEF may be concave or convex (see Deutscher and Mazumder (2023), Ahsan et al. (2024), and Emran et al. (2021)). We thus use both a linear and a quadratic rank-rank CEF for our estimation. Following Duflo (2001), we include children's birth district and birth year fixed effects, and interaction of birth year with 1971 enrollment (before Inpres program), birth year interacted with the number of school-age children in 1971, and birth year interacted with water sanitation program, with all the variables measured at the birth district level.

The estimating equation for the linear mobility CEF is:

$$\begin{aligned} R^c_{idt} = &\delta_0 + \delta_3 \left(I_d \times D^{post}\right) + \delta_4 R^p_{idt} + \delta_5 \left(I_d \times R^p_{idt}\right) + \delta_6 \left(D^{post} \times R^p_{idt}\right) \\ &+ \delta_7 \left(I_d \times D^{post} \times R^p_{idt}\right) + \beta_d + \lambda_t + \sum_t X_d \times \lambda_t + \varepsilon_{it} \end{aligned} \tag{3}$$

where R denotes ranks defined similarly to equation (2) above, subscript d stands for district, and I_d is intensity of Inpres school construction in district d, β_d is birth district fixed effect, λ_t is birth year fixed effect, and X_d is the vector of district-level variables which are interacted with birth year fixed effect λ_t, as discussed above. $D^{post} = 1$ for the birth cohorts who started primary schooling after the Inpres schools and $D^{post} = 0$ for the birth cohorts who finished primary schooling before Inpres. We omit the level effect of treatment intensity I_d and the pre-post dummy D^{post} because the district fixed effects absorb the level effect of I_d and the year fixed effects absorb the pre-post dummy.

The corresponding estimation equation for the case when the mobility CEF is quadratic is given by:

$$\begin{aligned} R^c_{idt} = &\delta_0 + \delta_3 \left(I_d \times D^{post}\right) + \delta_4 R^p_{idt} + \delta_5 \left(I_d \times R^p_{idt}\right) + \delta_6 \left(D^{post} \times R^p_{idt}\right) + \delta_7 \left(I_d \times D^{post} \times R^p_{idt}\right) \\ &+ \delta_8 \left(R^p_{idt}\right)^2 + \delta_9 \left(\left(R^p_{idt}\right)^2 \times I_d\right) + \delta_{10} \left(\left(R^p_{idt}\right)^2 \times D^{post}\right) \\ &+ \delta_{11} \left(I_d \times D^{post} \times \left(R^p_{idt}\right)^2\right) + \beta_d + \lambda_t + \sum_t X_d \times \lambda_t + \varepsilon_{it} \end{aligned} \tag{4}$$

(6.2) Estimation Results

Estimates from the Full Sample (Sons+Daughters)

Table 1 reports the estimated effects of Inpres schools on intergenerational educational mobility: panel A for the linear mobility CEF and panel B for the quadratic mobility CEF. Tests of functional forms for the pre and post intervention periods suggest that the mobility CEF is quadratic in both cases (see online appendix). So the estimates for the quadratic model are the most relevant, but we also report the estimates for the linear model as a benchmark as many existing papers rely on the linear model for policy evaluation. In each panel, column (1) reports the estimates when the ranks for each generation are calculated at the national level, including both the pre and post cohorts.[21] Second column is based on ranks calculated at the national level, but separately for the pre and post periods in each generation.[22] The third column refers to the case where the ranks are calculated separately for pre and post periods in each generation, but children's ranks are at the district level while parental ranks are at the national level.[23] The district-level ranks for children are motivated by the fact that the treatment intensity varies by districts in the DiD set-up.

The evidence in panel A of Table 1 shows that the estimated causal effects vary dramatically depending on how the ranks are calculated. If we follow the current practice in the literature, then the estimates in the first and second columns are the relevant ones. The estimates in column (1) suggest that the 61,000 primary schools constructed under the Inpres program failed to affect intergenerational educational (rank) mobility in Indonesia. This conclusion is robust: none of the estimated effects for both linear and quadratic specifications are significant at the 10 percent level. In contrast, the estimates in second column, especially for the linear model, suggest that the Inpres schools had substantial impacts. The estimates from the linear model suggest a positive impact on the intercept, and a negative impact on the slope. However, the estimates from the quadratic model indicates no impact on relative mobility (the effects on the linear and quadratic coefficients are not significant at the 10 percent level). The estimates in second column are not subject to the mechanical contamination across pre and post groups (compared to column (1)), but they might still suffer from substantial contamination across treatment and comparison groups in the children's generation. The last column takes care of this issue by calculating ranks for children at the district level (at which treatment intensity varies) separately for pre and post periods. As noted above, parental ranks in column (3) are at the national level, calculated separately for pre and post periods. The conclusions regarding the effects of Inpres schools, especially for the quadratic model, are substantially different. In contrast to columns (1) and (2), the evidence in column (3) in panel B of Table 1 suggests that Inpres schools had a positive effect on the intercept

[21]Some existing studies follow this rank calculation, see, for example, Agarwal et al. (2023).

[22]Many existing studies, both descriptive and causal, fall in this category because they calculate age cohort-wise national ranks, but separately for children and parents. See, for example, Chetty and Hendren (2018b), Manian et al. (2023), Asher et al. (2023).

[23]We are aware of only one study that falls in this category. See Yu et al. (2020).

and quadratic coefficients, while a negative impact on the linear coefficient of the mobility equation.

Although the linear mobility model is widely-used for policy evaluation, evidence reported in the online appendix suggests that the intergenerational educational mobility equation is quadratic in both the pre and post periods. Thus, the estimates in Table 1 suggest that the conclusions regarding the effects of Inpres schools depend critically on both the correct functional form of the mobility equation and the appropriate rank calculations. The estimates in column (3) of panel B are preferable on these grounds.

Impacts on Absolute and Relative Mobility

The estimated effects on the parameters of intergenerational mobility equation (conditional expectation function, or CEF) can be used to understand the impact of Inpres schools on absolute and relative educational mobility. While one can compute a variety of measures of mobility based on the estimates of the conditional expectation function, we use the following measures of absolute and relative mobility. Absolute mobility is measured by the expected years of schooling of children conditional on father's schooling, while relative mobility is defined by the slope of the rank-rank CEF (called intergenerational rank-rank slope, IRRS for short).[24] In a linear model, relative mobility is constant across father's distribution, but it varies with father's schooling in a quadratic model. Absolute mobility varies with father's schooling in both linear and quadratic mobility CEF.

Since linearity is rejected by the data, we focus on the quadratic CEF estimates in the lower panel of Table 1 for understanding the effects of Inpres schools on absolute and relative mobility. The estimates in the first column (national ranks calculated from combined pre and post periods) suggests that Inpres schools failed to have significant effects on any of the parameters of the mobility CEF, thus absolute or relative mobility was not affected. The second column (national ranks, separate for pre and post periods) suggests that there is no impact on relative mobility (the impact on both the slope parameters is not significant at the

[24]This definition of absolute mobility is motivated by the definition used by Chetty et al. (2014) where the expected income rank of children conditional on parent's income rank is a measure of absolute mobility. Their P_{25} is the expected rank of children whose parents are located at the 25th percentile of parental income distribution. With a linear CEF, this gives the expected rank for the lower half of the parental distribution. It is standard to measure relative mobility by the slope of the mobility CEF. For example, the rank-rank slope is a measure of relative mobility in Chetty et al. (2014), and a vast literature on intergenerational income elasticity (IGE) measures relative mobility by the slope of a log-log income mobility CEF.

10 percent level). In contrast, the estimates in the last column where ranks are not subject to mechanical changes suggest significant impacts on all the parameters of the mobility equation. The impacts on the linear and quadratic coefficients are different in signs, and this implies that Inpres schools affect relative mobility differently across the distribution.

The estimates of absolute and relative mobility based on the estimated parameters in column 3 of Table 1 are reported in Table 2. We report estimates for a number of locations in parental distribution: 10th, 25th, 50th, 75th, and 90th percentiles. The estimates of relative mobility are in the top panel of Table 2, while the corresponding estimates for absolute mobility are in the lower panel. We report the estimates for two levels of treatment intensity: high intensity=1 and mean intensity=0.215. The evidence suggests that Inpres schools improved relative mobility (reduced intergenerational rank-rank slope (IRRS)) for children born to father's in the lower part of the schooling distribution. In contrast, the IRRS increased for children of fathers at the right tail (75th percentile and higher). The evidence on absolute mobility suggests that the school expansion helped the disadvantaged children climb up the ladder, and lowered the expected schooling rank for the children born to more educated parents.

Heterogeneity Analysis: Gender

A primary focus of many recent papers on intergenerational mobility is heterogeneity across different spatial and social groups. In this section, we report estimates for sons and daughters separately. The estimates from the linear model are reported in Table 3: the top panel for sons and the bottom panel for daughters. Again, the tests of functional form reject the linear model in favor of the quadratic model (see online appendix), and the estimates from the quadratic model are in Table 4. Note that, for all the estimates in Tables (2) and (3), the calculated ranks are not gender specific. This reflects the fact that our treatment is not gender-specific.[25] This ensures that the ranks of sons and daughters are measured on a common scale, and it is meaningful to draw conclusions about gender convergence (or divergence) from these estimates. However, in cases where a policy targets a specific group (e.g., the employment quota for Scheduled Castes and Scheduled Tribes in India), then treatment and comparison are different social groups. To avoid the contamination of comparison, we need to calculate ranks separately for these social groups in such a case.

[25] An example of gender specific treatment is stipend for girls attending high schools in Bangladesh.

We begin with the estimates for the linear model in Table 3: the evidence suggests that how we calculate the ranks matter for the substantive conclusions, especially for the sons. For sons, the estimates from the national ranks (combined pre and post cohorts) suggest no impact on relative mobility (the estimated effect on the slope parameter is not significant at the 10 percent level). In contrast, columns (2) and (3) suggest significant impact on relative mobility. The evidence in Table 4 suggests that how we calculate the ranks matter much more for the quadratic model, which is the correct functional form for the mobility CEF for both sons and daughters. The estimates in the first two columns of Table 4 suggest no significant impacts on both linear and quadratic coefficients, while column 3 suggests significant impacts on all three coefficients of the quadratic mobility equation, for both sons and daughters.

Improving Comparability: Evidence from Dichotomized Samples

The estimates in column (3) of Tables (1), (3), and (4) deal with the contamination of comparison problem by calculating ranks at the district level because treatment intensity varies by district. However, a drawback of this approach is that we assign the same rank to individuals with different years of schooling living in different districts. While this may not be a problem in some contexts (for example, when the focus is on capturing the local level social interactions), in other applications, this can be an issue. A simple way to deal with this issue is to use a binary treatment variable, for example, by defining a treatment dummy that takes on the value of 1 for children in districts with higher than median treatment intensity and zero other wise. To analyze nonlinearity with respect to treatment intensity, we can divide the sample into four groups in terms of quartiles of treatment intensity, and estimate the effects for the three top quartiles, with the 1st quartile as the common comparison group. In this approach, children are measured on a common scale within a given treatment group irrespective of the district they are born in.

The estimates from this approach are reported in Table 5 (for median) and Tables 6-8 (for quartiles). The estimates again show that the conclusions depend substantially on the calculation of ranks adopted. As before, the functional form of the mobility equation also matters. For the standard linear model, the estimates in columns (1) and (2) suggest that Inpres schools affected intergenerational educational mobility, but the estimates in column (3) using the rank calculation that deal with mechanical rank changes do not show any significant effects. If we focus on the correct functional form (quadratic), then the estimates in column

(3) based on our preferred ranks again differ substantially from the other two columns. Even in those cases where the signs and statistical significance are similar across the three columns, the estimates based on appropriate ranks differ substantially in terms of the magnitude of the effect. For example, the quadratic coefficient for the third quartile (Q3) is positive and significant at the 5 percent level in all three columns, but the estimate is almost 50% larger in column (3) when compared to that in column (2).

Conclusions

Following the publication of Chetty et al. (2014) and Chetty and Hendren (2018b), the rank-rank model of intergenerational mobility has been widely adopted by researchers working on both developed and developing countries. However, adoption of the rank-rank model for policy evaluation raises some conceptual issues that have been largely ignored in the literature. This reflects the fact that these concerns are not relevant for descriptive evidence on the spatial heterogeneity and across cohorts evolution of intergenerational mobility which has been the focus of most of the existing literature. The standard approach in the current literature is to calculate cohort-specific ranks from the national distribution separately for children's and parent's generations. The national ranks for parent's generation ensure that all children's inherited socioeconomic status is measured on a common scale. Similarly, national ranks for children imply that we can compare the life outcomes of children irrespective of their location in a country. However, the pitfall in this approach for policy evaluation and causal analysis is that we put the treatment and comparison groups together. This leads to mechanical changes in ranks of the comparison group when a policy intervention changes the ranks of the treated children (or parents, or both). We provide the first analysis of this "contaminated comparison" problem in adopting the rank-rank model of intergenerational mobility for policy evaluation.

We discuss the contaminated comparison problem in the context of three widely-used research designs: RCTs, IV, and DiD. In a RCT design (with full compliance), a simple way to deal with the contaminated comparison problem is to calculate the ranks separately for the treatment and control groups. In an IV design with a binary instrument, the ranks should be calculated separately for the two groups defined by the binary instrument. For multi-valued instrumental variable, a natural generalization is to calculate the ranks separately for each value of the instrument. When the instrument is continuous (or take on a large number of values), we can use a few intervals, for example, quartiles of the values of the instrumental variable. This allows us to take advantage of the binary IV set-up discussed above, by comparing the 2nd, 3rd, and 4th quartiles one at a time with the 1st quartile as the comparison. In a DiD design, there is an additional source of mechanical changes in ranks: across the pre and post periods. To deal with this, it is necessary to calculate the ranks separately for pre and post periods in a DiD design. If a policy intervention affects only the children's generation, it is desirable, for all research designs, to calculate the parental ranks from the national distribution so that children's inherited socioeconomic status is consistently measured on a common scale.

We explore the empirical importance of alternative rank calculations for causal analysis by estimating the effects of Inpres primary schools in Indonesia on intergenerational educational mobility. We follow the DiD design originally developed by Duflo (2001), and consider both linear and quadratic mobility equations for the estimation. The linear estimates provide a benchmark, comparable to many existing studies where linearity is a maintained assumption. The evidence on the functional form rejects linearity in favor of a quadratic mobility equation for both the pre and post cohorts in our data. The evidence suggests that the conclusions regarding the effects of Inpres schools on intergenerational educational mobility vary dramatically depending on the way ranks are calculated and the functional form of the mobility equation. The estimates based on the standard rank calculations, especially for the correct functional form (quadratic CEF), suggest that the Inpres school construction had no impacts on relative mobility. In contrast, when the ranks are calculated carefully to avoid the mechanical contamination of the comparison group and the pre-Inpres period, the estimates from the DiD design suggest that the Inpres schools affected both relative and absolute mobility. Inpres schools increased the intercept and quadratic coefficient of the rank-rank mobility CEF while reducing the linear coefficient. The analysis presented here has far-reaching implications for policy evaluation with the rank-rank model of intergenerational mobility, and would be of interest to both economists and sociologists working on intergenerational mobility.

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Table 1: **Impact of Inpres on Intergenerational Mobility with Different Rank Calculations**

	Rank Calculations		
	Child: National (Pre+Post) Father: National (Pre+Post)	Child: National (Pre-Post Separate) Father: National (Pre-Post Separate)	Child: District (Pre-Post Separate) Father: National (Pre-Post Separate)
	Panel A: Linear CEF		
	(1)	(2)	(3)
$D^{post} \times$ I	4.165	14.205***	6.882***
	(3.008)	(2.997)	(1.686)
Fa Edu Rank $\times$ D^{post} $\times$ $\times$ I	0.008	-0.135***	-0.096***
	(0.050)	(0.042)	(0.026)
R2	0.369	0.352	0.236
Observations	2048164	2048164	2048164

	Panel B: Quadratic CEF		
	(1)	(2)	(3)
$D^{post} \times \times$ I	3.772	14.823***	14.125***
	(3.436)	(3.913)	(4.050)
Fa Edu Rank $\times$ D^{post} $\times$ $\times$ I	-0.024	-0.198	-0.473**
	(0.145)	(0.157)	(0.191)
Fa Edu Rank Sq $\times$ D^{post} $\times$ $\times$ I	0.001	0.001	0.004**
	(0.002)	(0.001)	(0.002)
R2	0.375	0.358	0.244
Observations	2048164	2048164	2048164

Notes: Dependent variable is child's rank in schooling. Fa Edu Rank is father's rank in schooling. Sq is square. **CEF** is conditional expectation function. Robust standard errors are in parentheses, clustered at the district of birth (*** p<0.01, ** p<0.05, * p<0.1). Sample corresponds to children born between 1957 and 1962, or 1968 to 1972. Variable D^{post} takes the value of 1 if the respondent is born between 1968 to 1972, and 0 otherwise. Inpres (I) measures the number of Inpres schools per 1000 children at the district level divided by the highest number of schools received by one district. **Children's Rank calculations:** column (1) national rank from full sample (pre+post) cohorts, column (2) national rank but separate for pre and post cohorts (3) district rank but separate for pre and post cohorts. **Father's Rank calculations:** column (1) national rank from full sample (pre+post) cohorts, columns (2) and (3) national rank but separate for pre and post cohorts. Covariates include birth district FE, year of birth×1971 enrollment, year of birth×1971 number of children, year of birth×water sanitation program, year of birth dummies, following Duflo (2001). Data sources: Indonesia's full count census 2000 and Duflo (2001).

Table 2: Relative and Absolute Mobility

	Panel A: Relative Mobility			
	High Intensity=1		Mean Intensity=0.215	
		Normalized		Normalized
$\Delta IRRS_{10}$	-0.400	-0.80%	-0.086	-0.17%
	(0.156)		(0.156)	
$\Delta IRRS_{25}$	-0.292	-0.58%	-0.063	-0.13%
	(0.104)		(0.104)	
$\Delta IRRS_{50}$	-0.110	-0.22%	-0.024	-0.05%
	(0.030)		(0.030)	
$\Delta IRRS_{75}$	0.072	0.14%	0.015	0.03%
	(0.081)		(0.081)	
$\Delta IRRS_{90}$	0.181	0.36%	0.039	0.08%
	(0.133)		(0.133)	
	Panel B: Absolute Mobility			
	High Intensity=1		Mean Intensity=0.215	
		Normalized		Normalized
ΔESR_{10}	9.757	19.51%	2.049	4.10%
	(2.455)		(0.012)	
ΔESR_{25}	4.567	9.13%	0.959	1.92%
	(1.175)		(0.012)	
ΔESR_{50}	-0.451	-0.90%	-0.095	-0.19%
	(1.645)		(0.016)	
ΔESR_{75}	-0.929	-1.86%	-0.195	-0.39%
	(1.353)		(0.014)	
ΔESR_{90}	0.963	1.93%	0.202	0.40%
	(1.873)		(0.014)	

Notes: Sample corresponds to children born between 1957 and 1962 (pre cohorts), or 1968 to 1972 (post cohorts). Integenerational Rank-Rank Slope (IRRS) measures relative mobility, which is the slope of Conditional Expectation Function (CEF). $\Delta IRRS_y = \delta_7 + 2\delta_{11} R^p_{iy} \times I$, where R^p_{iy} represents father's schooling rank for $y = 10, 25, 50, 75, 90$. Absolute Mobility is measured by expected schooling rank (ESR) of a child's schooling rank conditional on father's schooling rank. $\Delta ESR_y = \delta_3 \times I + \delta_7 \times I \times R^p_{iy} + \delta_{11} \times Inpres \times (R^p_{iy})^2$, where R^p_{iy} represents father's schooling rank for $y = 10, 25, 50, 75, 90$. The variable Inpres(I) intensity measures the number of Inpres schools per 1000 children at the district level divided by the highest number of schools received by one district. Both IRRS and ESR are obtained using equation (4) in the text. The Normalized IRRS/ESR is the IRRS/ESR value relative to IRRS/ESR of children for father's schooling median rank and zero Inpres intensity. The IRRS and ESR values are based on coefficients reported in column(3) of Table 1. Data source: Indonesia's full count census 2000.

Table 3: **Impact of Inpres on Intergenerational Mobility with Different Rank Calculations Gender Heterogeneity with Linear CEF**

	Rank Calculations		
	Child: National (Pre+Post) Father: National (Pre+Post)	Child: National (Pre-Post Separate) Father: National (Pre-Post Separate)	Child: District (Pre-Post Separate) Father: National (Pre-Post Separate)
	Panel A: Sons		
	(1)	(2)	(3)
$D^{post} \times$ I	8.682***	17.458***	11.406***
	(3.060)	(3.568)	(2.895)
Fa Edu Rank $\times$ $D^{post} \times$ I	-0.084	-0.207***	-0.167***
	(0.051)	(0.055)	(0.040)
R2	0.333	0.325	0.222
Observations	1199814	1199814	1199814
	Panel B: Daughters		
	(1)	(2)	(3)
$D^{post} \times$ I	-3.207	7.642**	-0.890
	(4.205)	(3.496)	(3.191)
Fa Edu Rank $\times$ $D^{post} \times$ I	10.351	-0.053	-0.017
	(7.967)	(0.051)	(0.050)
R2	0.424	0.399	0.270
Observations	848350	848350	848350

Notes: Dependent variable is child's rank in schooling. Fa Edu Rank is father's rank in schooling. **CEF** is conditional expectation function. Robust standard errors are in parentheses, clustered at the district of birth (*** p<0.01, ** p<0.05, * p<0.1). Sample corresponds to children born between 1957 and 1962, or 1968 to 1972. Variable D^{post} takes the value of 1 if the respondent is born between 1968 to 1972, and 0 otherwise. Inpres (I) measures the number of Inpres schools per 1000 children at the district level divided by the highest number of schools received by one district. **Children's Rank calculations:** column (1) national rank from full sample (pre+post) cohorts, column (2) national rank but separate for pre and post cohorts (3) district rank but separate for pre and post cohorts. **Father's Rank calculations:** column (1) national rank from full sample (pre+post) cohorts, columns (2) and (3) national rank but separate for pre and post cohorts. Covariates include birth district FE, year of birth×1971 enrollment, year of birth×1971 number of children, year of birth×water sanitation program, year of birth dummies, following Duflo (2001). Data sources: Indonesia's full count census 2000 and Duflo (2001).

Table 4: **Impact of Inpres on Intergenerational Mobility with Different Rank Calculations Gender Heterogeneity with Quadratic CEF**

	Rank Calculations		
	Child: National (Pre+Post) Father: National (Pre+Post)	Child: National (Pre-Post Separate) Father: National (Pre-Post Separate)	Child: District (Pre-Post Separate) Father: National (Pre-Post Separate)
	Panel A: Sons		
	(1)	(2)	(3)
$D^{post} \times$ I	8.453**	17.895***	17.606***
	(3.302)	(4.429)	(4.919)
Fa Edu Rank $\times$ D^{post} $\times$ I	-0.132	-0.261	-0.487**
	(0.152)	(0.170)	(0.206)
Fa Edu Rank Sq $\times$ D^{post} $\times$ I	0.001	0.001	0.003*
	(0.002)	(0.002)	(0.002)
R2	0.341	0.332	0.231
Observations	1199814	1199814	1199814

	Panel B: Daughters		
	(1)	(2)	(3)
$D^{post} \times$ I	-2.435	9.853**	8.314*
	(4.672)	(4.416)	(4.584)
Fa Edu Rank $\times$ D^{post} $\times$ I	0.015	-0.199	-0.498**
	(0.168)	(0.166)	(0.208)
Fa Edu Rank Sq $\times$ D^{post} $\times$ I	0.001	0.002	0.005**
	(0.002)	(0.002)	(0.002)
R2	0.430	0.404	0.277
Observations	848350	848350	848350

Notes: Dependent variable is child's rank in schooling. Fa Edu Rank is father's rank in schooling. Sq is square term. **CEF** is conditional expectation function. Robust standard errors are in parentheses, clustered at the district of birth (*** p<0.01, ** p<0.05, * p<0.1). Sample corresponds to children born between 1957 and 1962, or 1968 to 1972. Variable D^{post} takes the value of 1 if the respondent is born between 1968 to 1972, and 0 otherwise. Inpres (I) measures the number of Inpres schools per 1000 children at the district level divided by the highest number of schools received by one district. **Children's Rank calculations:** column (1) national rank from full sample (pre+post) cohorts, column (2) national rank but separate for pre and post cohorts (3) district rank but separate for pre and post cohorts. **Father's Rank calculations:** column (1) national rank from full sample (pre+post) cohorts, columns (2) and (3) national rank but separate for pre and post cohorts. Covariates include birth district FE, year of birth×1971 enrollment, year of birth×1971 number of children, year of birth×water sanitation program, year of birth dummies, following Duflo (2001). Data sources: Indonesia's full count census 2000 and Duflo (2001).

Table 5: **Impact of Inpres on Intergenerational Mobility by Median Exposure with Different Rank Calculations**

	Rank Calculations		
	Child: National (Pre+Post) Father: National (Pre+Post)	Child: National (Pre-Post Separate) Father: National (Pre-Post Separate)	Child: Median (Pre-Post Separate) Father: National (Pre-Post Separate)
	Panel A: Linear CEF		
	(1)	(2)	(3)
$D^{post} \times$ I> $M(=1)$	-0.418	1.494*	0.301
	(0.754)	(0.821)	(0.807)
Fa Edu Rank $\times$ $D^{post} \times$ I> $M(=1)$	0.011	-0.022*	-0.019*
	(0.013)	(0.011)	(0.011)
R2	0.369	0.352	0.343
Observations	2048164	2048164	2048164
	Panel B: Quadratic CEF		
	(1)	(2)	(3)
$D^{post} \times$ I> $M(=1)$	0.391	2.544**	2.446**
	(0.793)	(1.037)	(1.034)
Fa Edu Rank $\times$ $D^{post} \times$ I> $M(=1)$	-0.048	-0.088***	-0.142***
	(0.030)	(0.034)	(0.035)
Fa Edu Rank Sq $\times$ $D^{post} \times$ I> $M(=1)$	0.001*	0.001**	0.001***
	(0.000)	(0.000)	(0.000)
R2	0.376	0.358	0.350
Observations	2048164	2048164	2048164

Notes: Dependent variable is child's rank in schooling. Fa Edu Rank is father's rank in schooling. Sq is square term. **CEF** is conditional expectation function. Robust standard errors are in parentheses, clustered at the district of birth (*** p<0.01, ** p<0.05, * p<0.1). Sample corresponds to children born between 1957 and 1962, or 1968 to 1972. Variable D^{post} takes the value of 1 if the respondent is born between 1968 to 1972, and 0 otherwise. Inpres (I) measures the number of Inpres schools per 1000 children at the district level divided by the highest number of schools received by one district. M represents median Inpres intensity. **Children's Rank calculations:** column (1) national rank from full sample (pre+post) cohorts, column (2) national rank but separate for pre and post cohorts (3) rank by median Inpres intensity areas but separate for pre and post cohorts. **Father's Rank calculations:** column (1) national rank from full sample (pre+post) cohorts, columns (2) and (3) national rank but separate for pre and post cohorts. Covariates include birth district FE, year of birth×1971 enrollment, year of birth×1971 number of children, year of birth×water sanitation program, year of birth dummies, following Duflo (2001). Data sources: Indonesia's full count census 2000 and Duflo (2001).

Table 6: **Impact of Inpres on Intergenerational Mobility for Second Quartile with Different Rank Calculations**

	Rank Calculations		
	Child: National (Pre+Post) Father: National (Pre+Post)	Child: National (Pre-Post Separate) Father: National (Pre-Post Separate)	Child: Quartile (Pre-Post Separate) Father: National (Pre-Post Separate)
	Panel A: Linear CEF		
	(1)	(2)	(3)
D^{post} × Q2(=1)	-1.551	-0.407	0.118
	(1.053)	(1.236)	(1.203)
Fa Edu Rank × D^{post} × Q2(=1)	0.036**	0.011	0.004
	(0.017)	(0.016)	(0.017)
R2	0.369	0.352	0.327
Observations	2048164	2048164	2048164
	Panel B: Quadratic CEF		
	(1)	(2)	(3)
D^{post} × Q2(=1)	-0.227	-0.397	-0.724
	(1.190)	(1.605)	(1.587)
Fa Edu Rank × D^{post} × Q2(=1)	-0.031	-0.008	0.009
	(0.037)	(0.045)	(0.044)
Fa Edu Rank Sq × D^{post} × Q2(=1)	0.001	0.000	0.000
	(0.000)	(0.000)	(0.000)
R2	0.376	0.359	0.334
Observations	2048164	2048164	2048164

Notes: Dependent variable is child's rank in schooling. Fa Edu Rank is father's rank in schooling. Sq is square. **CEF** is conditional expectation function. Robust standard errors are in parentheses, clustered at the district of birth (*** p<0.01, ** p<0.05, * p<0.1). Sample corresponds to children born between 1957 and 1962, or 1968 to 1972. $Q2$ represents second quartile. The omitted group is the bottom quartile. Variable D^{post} takes the value of 1 if the respondent is born between 1968 to 1972, and 0 otherwise. Inpres (I) measures the number of Inpres schools per 1000 children at the district level divided by the highest number of schools received by one district. **Children's Rank calculations:** column (1) national rank from full sample (pre+post) cohorts, column (2) national rank but separate for pre and post cohorts (3) rank by quartile Inpres intensity areas but separate for pre and post cohorts. **Father's Rank calculations:** column (1) national rank from full sample (pre+post) cohorts, columns (2) and (3) national rank but separate for pre and post cohorts. Covariates include birth district FE, year of birth×1971 enrollment, year of birth×1971 number of children, year of birth×water sanitation program, year of birth dummies, following Duflo (2001). Data sources: Indonesia's full count census 2000 and Duflo (2001).

Table 7: **Impact of Inpres on Intergenerational Mobility for Third Quartile with Different Rank Calculations**

	Rank Calculations		
	Child: National (Pre+Post) Father: National (Pre+Post)	Child: National (Pre-Post Separate) Father: National (Pre-Post Separate)	Child: Quartile (Pre-Post Separate) Father: National (Pre-Post Separate)
	Panel A: Linear CEF		
	(1)	(2)	(3)
D^{post} × Q3(=1)	-2.012*	-0.274	0.220
	(1.078)	(1.102)	(1.050)
Fa Edu Rank × D^{post} × Q3(=1)	0.038**	0.004	-0.006
	(0.018)	(0.015)	(0.015)
R2	0.369	0.352	0.327
Observations	2048164	2048164	2048164

	Panel B: Quadratic CEF		
	(1)	(2)	(3)
D^{post} × Q3(=1)	-0.030	1.045	2.589**
	(1.181)	(1.374)	(1.286)
Fa Edu Rank × D^{post} × Q3(=1)	-0.079**	-0.091*	-0.156***
	(0.040)	(0.047)	(0.048)
Fa Edu Rank Sq × D^{post} × Q3(=1)	0.001**	0.001**	0.002***
	(0.000)	(0.000)	(0.000)
R2	0.376	0.359	0.334
Observations	2048164	2048164	2048164

Notes: **CEF** is conditional expectation function. Robust standard errors are in parentheses, clustered at the district of birth (*** p<0.01, ** p<0.05, * p<0.1). Sample corresponds to children born between 1957 and 1962, or 1968 to 1972. $Q3$ represents third quartile. The omitted group is the bottom quartile. Variable D^{post} takes the value of 1 if the respondent is born between 1968 to 1972, and 0 otherwise. Inpres (I) measures the number of Inpres schools per 1000 children at the district level divided by the highest number of schools received by one district. **Children's Rank calculations:** column (1) national rank from full sample (pre+post) cohorts, column (2) national rank but separate for pre and post cohorts (3) rank by quartile Inpres intensity areas but separate for pre and post cohorts. **Father's Rank calculations:** column (1) national rank from full sample (pre+post) cohorts, columns (2) and (3) national rank but separate for pre and post cohorts. Covariates include birth district FE, year of birth×1971 enrollment, year of birth×1971 number of children, year of birth×water sanitation program, year of birth dummies, following Duflo (2001). Data sources: Indonesia's full count census 2000 and Duflo (2001).

Table 8: **Impact of Inpres on Intergenerational Mobility for Fourth Quartile with Different Rank Calculations**

	Rank Calculations		
	Child: National (Pre+Post) Father: National (Pre+Post)	Child: National (Pre-Post Separate) Father: National (Pre-Post Separate)	Child: Quartile (Pre-Post Separate) Father: National (Pre-Post Separate)
	Panel A: Linear CEF		
	(1)	(2)	(3)
D^{post} × Q4(=1)	-0.172	3.100***	0.500
	(0.945)	(1.003)	(0.900)
Fa Edu Rank × D^{post} × Q4(=1)	0.017	-0.035**	-0.020
	(0.017)	(0.014)	(0.014)
R2	0.369	0.352	0.327
Observations	2048164	2048164	2048164

	Panel B: Quadratic CEF		
	(1)	(2)	(3)
D^{post} × Q4(=1)	0.378	3.977***	2.232*
	(1.091)	(1.293)	(1.164)
Fa Edu Rank × D^{post} × Q4(=1)	-0.029	-0.092**	-0.128***
	(0.040)	(0.044)	(0.043)
Fa Edu Rank Sq × D^{post} × Q4(=1)	0.000	0.001	0.001***
	(0.000)	(0.000)	(0.000)
R2	0.376	0.359	0.334
Observations	2048164	2048164	2048164

Notes: **CEF** is conditional expectation function. Robust standard errors are in parentheses, clustered at the district of birth (*** p<0.01, ** p<0.05, * p<0.1). Sample corresponds to children born between 1957 and 1962, or 1968 to 1972. $Q4$ represents fourth (top) quartile. The omitted group is the bottom quartile. Variable D^{post} takes the value of 1 if the respondent is born between 1968 to 1972, and 0 otherwise. Inpres (I) measures the number of Inpres schools per 1000 children at the district level divided by the highest number of schools received by one district. **Children's Rank calculations:** column (1) national rank from full sample (pre+post) cohorts, column (2) national rank but separate for pre and post cohorts (3) rank by quartile Inpres intensity areas but separate for pre and post cohorts. **Father's Rank calculations:** column (1) national rank from full sample (pre+post) cohorts, columns (2) and (3) national rank but separate for pre and post cohorts. Covariates include birth district FE, year of birth×1971 enrollment, year of birth×1971 number of children, year of birth×water sanitation program, year of birth dummies, following Duflo (2001). Data sources: Indonesia's full count census 2000 and Duflo (2001).

Appendix Tables

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Table A.1: **Checking Functional Form of CEFs with Different Rank Calculations**

	Post Cohorts	**Pre Cohorts**
	Panel A	
	(1)	(2)
Fa Edu Rank	0.156***	0.186***
	(0.029)	(0.032)
Fa Edu Rank Sq	0.004***	0.004***
	(0.000)	(0.000)
R2	0.314	0.312
Observations	1796198	251966
	Panel B	
	(1)	(2)
Fa Edu Rank	0.173***	0.155***
	(0.029)	(0.036)
Fa Edu Rank Sq	0.004***	0.004***
	(0.000)	(0.000)
R2	0.317	0.299
Observations	1796198	251966
	Panel C	
	(1)	(2)
Fa Edu Rank	0.103***	0.066
	(0.025)	(0.044)
Fa Edu Rank Sq	0.003***	0.003***
	(0.000)	(0.000)
R2	0.202	0.178
Observations	1796198	251966

Notes: **CEF** is conditional expectation function. Robust standard errors are in parentheses, clustered at the district of birth (*** p<0.01, ** p<0.05, * p<0.1). Sample corresponds to children born between 1957 and 1962 (pre cohorts), or 1968 to 1972 (post cohorts) and excludes Inpres areas which are second and third quartile intesnity. **Children's Rank calculations:** Panel A: national rank from full sample (pre+post), Panel B: national rank but separate for pre and post cohorts, Panel C: rank by district but separate for pre and post cohorts and . **Father's Rank calculations:** Panel A: national rank from full sample (pre+post cohorts), Panels B and C: national rank but separate for pre and post cohorts. Data source: Indonesia's full count census 2000.

Table A.2: **Checking Functional Form of CEFs–Sons' Sample with Different Rank Calculations**

	Post Cohorts	**Pre Cohorts**
	Panel A	
	(1)	(2)
Fa Edu Rank	0.096***	0.155***
	(0.027)	(0.033)
Fa Edu Rank Sq	0.004***	0.004***
	(0.000)	(0.000)
R2	0.289	0.277
Observations	1079989	119825
	Panel B	
	(1)	(2)
Fa Edu Rank	0.110***	0.092**
	(0.027)	(0.036)
Fa Edu Rank Sq	0.004***	0.004***
	(0.000)	(0.000)
R2	0.291	0.266
Observations	1079989	119825
	Panel C	
	(1)	(2)
Fa Edu Rank	0.063**	0.084*
	(0.025)	(0.047)
Fa Edu Rank Sq	0.003***	0.003***
	(0.000)	(0.000)
R2	0.176	0.147
Observations	1079989	119825

Notes: **CEF** is conditional expectation function. Robust standard errors are in parentheses, clustered at the district of birth (*** p<0.01, ** p<0.05, * p<0.1). Sample corresponds to children born between 1957 and 1962 (pre cohorts), or 1968 to 1972 (post cohorts) and excludes Inpres areas which are second and third quartile intesnity. **Children's Rank calculations:** Panel A: national rank from full sample (pre+post), Panel B: national rank but separate for pre and post cohorts, Panel C: rank by district but separate for pre and post cohorts and . **Father's Rank calculations:** Panel A: national rank from full sample (pre+post cohorts), Panels B and C: national rank but separate for pre and post cohorts. Data source: Indonesia's full count census 2000.

Table A.3: **Checking Functional Form of CEFs–Daughters' Sample with Different Rank Calculations**

	Post Cohorts	**Pre Cohorts**
	Panel A	
	(1)	(2)
Fa Edu Rank	0.243***	0.213***
	(0.033)	(0.036)
Fa Edu Rank Sq	0.003***	0.004***
	(0.000)	(0.000)
R2	0.352	0.351
Observations	716209	132141
	Panel B	
	(1)	(2)
Fa Edu Rank	0.266***	0.198***
	(0.033)	(0.041)
Fa Edu Rank Sq	0.003***	0.004***
	(0.000)	(0.000)
R2	0.356	0.332
Observations	716209	132141
	Panel C	
	(1)	(2)
Fa Edu Rank	0.162***	0.009
	(0.025)	(0.046)
Fa Edu Rank Sq	0.003***	0.004***
	(0.000)	(0.000)
R2	0.242	0.210
Observations	716209	132141

Notes: **CEF** is conditional expectation function. Robust standard errors are in parentheses, clustered at the district of birth (*** p<0.01, ** p<0.05, * p<0.1). Sample corresponds to children born between 1957 and 1962 (pre cohorts), or 1968 to 1972 (post cohorts) and excludes Inpres areas which are second and third quartile intesnity. **Children's Rank calculations:** Panel A: national rank from full sample (pre+post), Panel B: national rank but separate for pre and post cohorts, Panel C: rank by district but separate for pre and post cohorts and . **Father's Rank calculations:** Panel A: national rank from full sample (pre+post cohorts), Panels B and C: national rank but separate for pre and post cohorts. Data source: Indonesia's full count census 2000.