Educational Investment in Spatial Equilibrium: Evidence from Indonesia

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This paper quantifies the long-run aggregate and distributional effects of Indonesia's Sekolah Dasar INPRES program, one of the largest school construction programs in history. I do so with a spatial equilibrium model in which graduates migrate and seek employment nationally. I find that the program increased aggregate output by 8%, with especially large gains for rural students. Rural students realize these gains by leaving rural regions behind, increasing regional inequality by 12% despite the program's goal of encouraging regional convergence.

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1 Introduction

Governments invest more than $3 trillion in education annually (World Bank 2022). This investment targets students locally, but graduates migrate and seek employment nationally. This paper studies how migration shapes educational investment in the context of Indonesia's Sekolah Dasar INPRES program, an unprecedented school construction effort that established 61,807 new primary schools from 1973 to 1978. Differences in mobility generate substantial spatial heterogeneity in the returns to education, and I show how these differences inform the design of the program.

I begin by analyzing the program with the difference-in-differences approach of Duflo (2001). In particular, I compare exposed (young) and unexposed (old) age cohorts in districts with high and low levels of school construction. National socioeconomic survey data from 2011 to 2014 capture a range of long-run education and employment outcomes, including years of schooling and monthly wages, and data on district of birth provide the link to school construction. I document two stylized facts.

First, the returns to education vary greatly over space. I estimate the program's impact on years of schooling and wages, and I find positive long-run effects. The ratio of the schooling and wage effects, which correspond to a first stage and reduced form, gives average returns to education. I complement this analysis with the change-in-changes approach of Athey and Imbens (2006) to estimate the full distribution of treatment effects. Average effects mask considerable heterogeneity for schooling and wages, which in turn reveal large variation in the returns to education across districts.

Second, variation in mobility explains much of the variation in returns to education. I measure mobility with labor market access, which I compute for each district as an inverse-distance-weighted average of pre-program population densities across nearby districts. This measure captures workers' proximity to high-wage urban labor markets, and I validate it by showing that migration rates are highest where market access is high. I find that districts with high market access drive the program's schooling and wage effects, and that they enjoy the highest returns to education.

I capture these stylized facts with a spatial equilibrium model in which a government constructs schools, then individuals pursue education and migrate for employment. Frictions include education costs and migration costs, and I interpret school construction in a given district as decreasing education costs in that district. Unlike typical place-based policies that provide only local benefits, schools build portable human capital. The model thus captures two margins of spatial interactions. First, the returns to education depend on market access. Mobility gives rural students access to high urban wages, which reward high human capital and thus raise the incentives to invest in education. Second, school construction has both local and non-local effects. Mobility implies that rural construction may not lead to regional convergence, as rural students leave after graduation and contribute to urban output.

I estimate the model using the same difference-in-differences variation described previously. Applying the variation directly, I estimate two key parameters: the elasticity of human capital with respect to education and the elasticity of education costs with respect to school construction. In some special cases, these parameters alone are sufficient for counterfactuals. In other cases, I estimate the rest of the model by Poisson pseudo-maximum likelihood. Applying the variation indirectly, I discipline estimation by adding moments to match the reduced-form estimates. In the spirit of Dekle et al. (2008), I then compute counterfactual outcomes as a function of estimated parameters and observed quantities.

I use the model to quantify the aggregate and distributional effects of the program. In particular, I compare observed outcomes with outcomes under a counterfactual with zero school construction. The model then allows me to decompose the effects of mobility by mechanism, and to separate each from the general equilibrium effects generated by this large-scale program. The difference-in-differences analysis does not rely on the model, but it only captures net effects. Finally, I study the design of the program by simulating alternative allocations of school construction.

Quantifying aggregate effects, I find that the program increased output by eight percent. A decomposition exercise allows me to assess the impact of mobility. Without migration, the program has a direct effect of only two percent. Migration has three effects. First, holding schooling decisions and wages fixed, allowing individuals to sort into high-productivity regions increases output by another one percentage point. Second, holding wages fixed, larger returns to education raise investment in schooling, increasing output by a further four percentage points. Third, selection and human capital complementarities affect wages in equilibrium, increasing output by another percentage point on net. Bryan et al. (2014) find large gains from sorting, but endogenizing education would raise them further, including in general equilibrium.

Quantifying distributional effects, I find that rural students benefit most. The program expanded opportunities for less-advantaged rural students with high marginal returns, and in doing so decreased inequality between rural and urban students by five percent. At the same time, the program explicitly aimed to encourage regional convergence, but mobility places convergence in tension with output gains. Without mobility, rural residents stay in rural regions but face low wages. Regional inequality falls, but so do output gains. With mobility, rural-to-urban migration fuels output gains, but rural regions gain little net of out-migration. Even so, they are better off than under zero construction, such that the program remains Pareto-improving. Regional inequality rises only because urban regions gain much more.

I conclude with guidance for Indonesian policy, which faces an equity-efficiency tradeoff under mobility. Rural school construction generates large returns, but also slows convergence between rural and urban regions. Investments in connected districts are especially effective, but these districts benefit least because most graduates leave. An alternative is to complement school construction with transportation infrastructure that improves mobility itself. Doing so boosts the effects of school construction, but not in a Pareto-improving way: rural regions suffer as out-migration rises.

My main contribution is to show how large-scale educational investment interacts with migration in general equilibrium. To this end, I build on a literature that studies educational infrastructure and student outcomes in developing countries (Burde and Linden 2013; Kazianga et al. 2013; Khanna 2021; Dinerstein et al. 2022), including work on the INPRES program itself (Duflo 2001, 2004; Martinez-Bravo 2017; Mazumder et al. 2019; Ashraf et al. 2020; Akresh et al. 2021; Bazzi et al. 2021).[1] I highlight meaningful spatial heterogeneity in the returns to education, and I quantify aggregate and distributional effects over the long run. Relative to Khanna (2021) and Dinerstein et al. (2022), who also study large-scale school construction programs, I focus on how mobility contributes to the returns to education in spatial equilibrium,

[1] US-focused studies include Cellini et al. (2010), Neilson and Zimmerman (2014), Goncalves (2015), Hong and Zimmer (2016), and Conlin and Thompson (2017). A related literature on moving to opportunity, as reviewed by Chyn and Katz (2021), finds that children experience positive education effects after moving to better neighborhoods, subject to disruption effects that sometimes dominate (Chetty et al. 2016; Chetty and Hendren 2018; Chyn 2018; Laliberté 2021; Nakamura et al. 2021; Rojas-Ampuero and Carrera 2021). By contrast, I focus on post-schooling migration.

as well as the implications of migration for program design.

I also build on a literature that applies quantitative spatial equilibrium models to studying the allocation of human capital over space, as reviewed by Redding and Turner (2015) and Redding and Rossi-Hansberg (2017). This work largely focuses on transportation, with recent examples in developing countries that include Tsivanidis (2019), Moneke (2020), Balboni (2021), and Zárate (2021).[2] I show how spatial concerns apply to educational infrastructure via migration, and I provide new evidence on endogenous human capital formation in a spatial setting. Relative to Eckert and Kleineberg (2021) and Agostinelli et al. (2022), who also apply spatial frameworks to studying education, I quantify the effects of school construction at the national scale. The INPRES program provides quasi-experimental variation and allows me to study long-run labor market outcomes.

I evaluate the program with a spatial equilibrium model that captures individuals' education and migration decisions. The model builds on Bryan and Morten (2019) and Hsieh et al. (2019) within a broader literature on selection into occupations (Roy 1951; Heckman 1974; Heckman and Sedlacek 1985; Keane and Wolpin 1997) and migration (Dahl 2002; Kennan and Walker 2011; Moretti 2011; Young 2013). I emphasize the interaction between mobility and the returns to education, leverage quasi-experimental variation for estimation, and connect to infrastructure investment with an emphasis on distributional effects.

Finally, I engage with the literature on place-based policy, as reviewed by Glaeser and Gottlieb (2008), Kline and Moretti (2014a), Neumark and Simpson (2015), and Austin et al. (2018). Existing empirical work studies spatially targeted infrastructure investment (Kline and Moretti 2014b; Balboni et al. 2020) and enterprise subsidies (Neumark and Kolko 2010; Ham et al. 2011; Busso et al. 2013; Wang 2013; Criscuolo et al. 2019). These policies provide only local benefits, which in-migration can offset by increasing local prices or draining non-local productivity. By contrast, schools provide portable benefits that out-migration magnifies and distributes. I quantify these benefits for one of the largest school construction programs in history.

[2] Other examples include work on roads (Fajgelbaum and Schaal 2020; Gertler et al. 2019; Graff 2019), highways (Allen and Arkolakis 2014, 2022; Faber 2014; Alder 2016; Yang 2017; Morten and Oliveira 2018), railroads (Donaldson and Hornbeck 2016; Donaldson 2018; Hornbeck and Rotemberg 2021; Fajgelbaum and Redding 2022), railways (Heblich et al. 2020; Severen 2021), and buses (Balboni et al. 2020).

2 Data and Stylized Facts

This section describes the INPRES program and the data, then evaluates the program with a difference-in-differences approach.

2.1 The INPRES program

The program had the stated goal of constructing 62,000 primary schools nationwide: 6,000 in the fiscal year beginning in 1973, 6,000 in 1974, 10,000 in 1975, 10,000 in 1976, 15,000 in 1977, and 15,000 in 1978 (*Inpres* No. 10/1973, 6/1974, 6/1975, 3/1976, 3/1977, 6/1978). In 1973 and 1974, schools were distributed across districts in proportion to pre-program unenrollment rates for children of primary school age. From 1975 to 1978, unenrollment was instead defined relative to a 15% threshold, with no new schools for districts with unenrollment rates below 15%. Figure 1 shows that school construction is indeed proportional to unenrollment rates in the data, and table 1 documents the resulting emphasis on rural, isolated districts. INPRES refers to the "presidential instructions" that established the program.

2.2 Data

District-level data on INPRES school construction come from Duflo (2001), which draws on data from the Ministry of National Development Planning (*Bappenas*) and the 1971 population census. The data record the number of primary schools constructed, the number of pre-program primary schools, 1971 child populations and enrollment rates, and INPRES water and sanitation spending per capita. I compute population densities by dividing 1971 populations by land area, and I use these population densities as a measure of ruralness. For each district, I compute labor market access as a weighted average of 1971 population densities across districts, where weights $(1 + \text{dist}_{dd'})^{-2}$ are inversely proportional to distance. Thus, districts that either contain or are close to urban centers have high market access, such that this measure captures proximity to high-wage urban labor markets.

The main individual-level data come from the 2011, 2012, 2013, and 2014 National Socioeconomic Surveys (SUSENAS). I observe districts of residence and birth, with the latter providing the link to INPRES program exposure. The data record educational and employment outcomes, including educational attainment and monthly wages. Self-employment activity is observed, but self-employment income is not. I restrict attention to male heads of household ages 2 to 24 in 1974 – when the first INPRES schools were completed – and I adjust districts to 1971 boundaries for consistency over time. "Districts" refer sub-provincial urban *kota* and rural *kabupaten*.

2.3 Education and wage effects

I estimate program effects by difference-in-differences as in Duflo (2001). Individuals ages 2 to 6 in 1974 – those young enough to benefit from new primary schools – form the treatment group, and those ages 12 to 17 in 1974 form the control group. I compare these groups in regions with high versus low levels of school construction.

$$Y_{ijk} = \delta_j + \delta_k + \beta S_j T_k + \boldsymbol{C}_j T_k \boldsymbol{\phi} + \varepsilon_{ijk}\,, \tag{1}$$

for individuals i born in district j and age cohort k. It includes outcome variable Y_{ijk}, district-of-birth fixed effect δ_j, year-of-birth fixed effect δ_k, school construction intensity S_j, treatment dummy T_k, district-of-birth controls $\boldsymbol{C}_j$, and error term ε_{ijk}. School construction intensity is the number of schools constructed per 1,000 children, and controls include 1971 child populations, 1971 enrollment rates, and INPRES spending on water and sanitation projects. I also include survey-year fixed effects because I pool SUSENAS data from multiple waves. The coefficient of interest is β, which captures the causal effect of school construction assuming common trends in high- and low-construction regions absent the program. As a placebo experiment, I compare two unexposed groups: those ages 12 to 17 and those ages 18 to 24 in 1974.

Table 2 shows the long-run effects of the program on education and labor market outcomes. Consistent with the medium-run findings of Duflo (2001), school construction increases years of schooling, both in the full sample and for wage earners alone, and it increases log monthly wages. The education effects are driven by increased primary and middle school completion. The wage effects are not driven by increased employment, which suggests increased wage rates. These results also imply that the program does not meaningfully affect selection into the sample of wage earners. Placebo estimates are insignificant throughout.

I then compute the implied returns to education by dividing the wage effect by the schooling effect. These effects correspond to the reduced form and first stage of a standard Wald estimator. In particular, I compute the proportional change in wages, as measured in log points, resulting from an additional year of education. I further consider the distribution of treatment effects in a change-in-changes framework, as formalized by Athey and Imbens (2006). Given a rank-invariance assumption, the empirical distributions of control and treatment outcomes reveal the full distributions of potential outcomes. I define districts with below-median school construction as control and those with above-median school construction as treatment. I then take the ratio of these estimates and obtain a distribution of returns to education. Figure 2 shows the result and reveals the considerable heterogeneity masked by the average.

2.4 Migration and market access

Figure 3 shows that baseline migration levels are high, particularly for districts with high labor market access, as individuals seek opportunities nationally. The average migration rate is 26%, and the average migration distance conditional on migration is 576 kilometers. The cross-province migration rate is 16%, compared to a cross-state migration rate of 31% in the United States, where mobility is relatively high.[3] Appendix figure A1 shows similar patterns across cohorts, with modestly higher levels of migration among younger, treated cohorts. Thus, spatial forces matter in equilibrium because many of those exposed to new schools migrate elsewhere.

Figure 4 shows that market access amplifies the INPRES treatment effect. I report interaction coefficients for quartiles $\boldsymbol{X_j}$ of birth-district market access.

$$Y_{ijk} = \delta_j + \delta_k + \boldsymbol{X_j} S_j T_k \boldsymbol{\beta} + \boldsymbol{C_j} T_k \boldsymbol{\phi} + \varepsilon_{ijk} \tag{2}$$

Effects increase in market access. Appendix figure A2 shows null effects in the placebo experiment, and appendix table A3 presents the regression table. Effects are indistinguishable from zero for districts with low market access, as barriers to migration limit the effective pool of job opportunities and thus the incentive to invest in schooling.

At the same time, table 3 shows that migration patterns do not themselves

[3] I use 2013 and 2014 American Community Survey data to compute American migration rates. In doing so, I define migration as I do in the Indonesian context. Restricting attention to those born in the United States, which I take to include the 48 contiguous states plus the District of Columbia, I calculate the proportion of individuals residing outside of their state of birth.

respond strongly to the program. Migration rates do not increase on the extensive margin, nor do migration distances on the intensive margin, and migration to both urban and rural destinations remains stable for urban and rural origins alike. This invariance is indeed consistent with the empirical model to come: in the model, school construction lowers education costs but has no direct effect on either migration costs or migration itself. Moreover, even if the program changes neither migration nor market access over time, there remains large variation in the cross section that shapes the effects of school construction (as figures 3 and 4 show). Finally, this result invites the study of how school construction is affected when market access and migration do change, as I will emphasize in counterfactuals.

Indeed, consistent with mobility as a driver of wage gains, table 4 shows that people benefit more from school construction than places do. The first three columns show baseline estimates, as in table 2, that take birth-district school construction as treatment. They capture effects on individuals, inclusive of those who migrate away. The last three columns instead take current-district construction as treatment, capturing effects on districts themselves. The latter estimates are indistinguishable from zero, suggesting that local gains dissipate as those who benefit most from the program eventually leave.[4]

3 Model

This section presents a spatial equilibrium model in which a government constructs schools, then individuals invest in education and migrate for work.

3.1 School construction

A government allocates school construction $a = \{a_\ell\}$ across districts $\ell \in \mathcal{L}$ to maximize aggregate output Y, subject to costs C and distributional concerns D.

$$\max_a \; Y(a) \quad \text{s.t.} \quad C(a) \leq \bar{C}, \quad D(a) \leq \bar{D}\,,$$

[4] While the point estimates are consistently smaller and similarly precise, a significant difference from baseline would require even higher migration rates.

Districts produce differentiated goods as a function of productivity and human capital. Aggregate output sums over districts with constant elasticity of substitution $\sigma > 1$.

$$Y_\ell = A_\ell H_\ell, \quad Y(a) = \left(\sum_\ell \left[Y_\ell(a) \right]^{\frac{\sigma-1}{\sigma}} \right)^{\frac{\sigma}{1-\sigma}} \tag{3}$$

Schools raise human capital. Distributional concerns can apply to both places and people. For people, the output gap between individuals of rural and urban origin is

$$D^1(a) = Y^{U1}(a) - Y^{R1}(a) \quad \text{for} \quad Y_\ell^{U1}(a) = \sum_j U_j Y_{j\ell}(a)\,,$$

which captures differences in opportunity. U_ℓ is urban status, and $Y_\ell^{R1}(a)$ is defined similarly. For places, the rural-urban output gap captures regional disparities.

$$D^2(a) = Y^{U2}(a) - Y^{R2}(a) \quad \text{for} \quad Y_\ell^{U2}(a) = U_\ell Y_\ell(a)$$

3.2 Utility and migration

For each destination ℓ, individuals i born in origin district j and age cohort k realize skill draws and choose schooling. Building on Bryan and Morten (2019) and Hsieh et al. (2019), utility for a given destination is

$$U(e,\epsilon) = \alpha_\ell \varepsilon^\alpha_{jk\ell} [\underbrace{(1-\tau^m_{j\ell}) w_\ell h_{jk} \varepsilon^h_{jk\ell} e^\eta \epsilon}_{\text{net labor income}} - \underbrace{(1+\tau^e_{jk}) c \varepsilon^c_{jk\ell} e}_{\text{cost of education}}]\,, \tag{4}$$

where I suppress subscripts $ijk\ell$. Individuals consider amenities α_ℓ and consumption, where consumption is net labor income less the total cost of education. Total labor income is the product of base wage w_ℓ and human capital, which in turn combines base human capital h_{jk}, schooling e, human capital elasticity η, and skill draw ϵ. Human capital is concave in schooling for $\eta < 1$, reflecting diminishing marginal returns. Cohorts are perfect substitutes conditional on human capital and thus face common base wages. Labor income is net of migration costs $\tau^m_{j\ell}$, which capture the consumption-denominated costs – financial, psychological, and otherwise – of being away from home. The total cost of education is the product of base cost c and schooling e, amplified by education costs τ^e_{jk}. I allow for finite misspecification $\varepsilon^\alpha_{jk\ell}$,

$\varepsilon^h_{jk\ell}$, and $\varepsilon^c_{jk\ell}$ in amenities, base human capital, and the cost of education, respectively. The key frictions are education and migration costs, and school construction lowers education costs for treated age cohorts by increasing access to schooling.

For each destination, individuals choose schooling conditional on their skill draw. Schooling increases human capital and thus labor income, but also increases the total cost of education. The optimal schooling choice and resulting utility are

$$e^* = \arg\max_e \{U(e,\epsilon)\} = \left[\frac{(1-\tau^m_{j\ell})w_\ell h_{jk}\varepsilon^h_{jk\ell}\eta\epsilon}{(1+\tau^e_{jk})c\varepsilon^c_{jk\ell}}\right]^{\frac{1}{1-\eta}}. \tag{5}$$

$$U(\epsilon) = U(e^*,\epsilon) = (1-\eta)\eta^{\frac{\eta}{1-\eta}}\alpha_\ell\varepsilon^\alpha_{jk\ell}\left[\frac{(1-\tau^m_{j\ell})w_\ell h_{jk}\varepsilon^h_{jk\ell}\epsilon}{[(1+\tau^e_{jk})c\varepsilon^c_{jk\ell}]^\eta}\right]^{\frac{1}{1-\eta}}. \tag{6}$$

Both are decreasing in education costs, which make schooling costly, and in migration costs, which lower net labor income and thus the returns to schooling.

Conditional on skill draws and schooling choices, individuals compare utilities across destinations and migrate to maximize utility. Skill draws are Fréchet distributed, following McFadden (1974) and Eaton and Kortum (2002).

$$F(\epsilon_1,\ldots,\epsilon_L) = \exp\left\{-\sum_\ell \epsilon_\ell^{-\theta}\right\}$$

High θ implies low skill dispersion. I obtain migration choice probabilities

$$\pi_{jk\ell} = \frac{\tilde{w}^\theta_{jk\ell}}{\sum_{\hat\ell}\tilde{w}^\theta_{jk\hat\ell}} \quad \text{for} \quad \tilde{w}_{jk\ell} \equiv \alpha_\ell^{1-\eta}(1-\tau^m_{j\ell})w_\ell \underbrace{\frac{(\varepsilon^\alpha_{jk\ell})^{1-\eta}\varepsilon^h_{jk\ell}}{(\varepsilon^c_{jk\ell})^\eta}}_{\tilde{\varepsilon}_{jk\ell}}. \tag{7}$$

Migrants prefer destinations with low migration costs, high amenities, and high base wages. Education costs do not enter directly because they affect destinations equally.

3.3 Education and wages

Average education and wages by origin j, cohort k, and destination ℓ are

$$
\begin{aligned}
\overline{educ}_{jk\ell} &\equiv \mathbb{E}\left[e^* \,|\, \text{individuals choose } \ell\right] \\
&= \gamma\left(\frac{1}{\alpha_\ell}\right)\left[\frac{h_{jk}\eta}{(1+\tau^e_{jk})c}\right]^{\frac{1}{1-\eta}}\left(\sum_{\hat{\ell}} \tilde{w}^\theta_{jk\hat{\ell}}\right)^{\frac{1}{\theta(1-\eta)}}\left(\frac{1}{\varepsilon^\alpha_{jk\ell}\varepsilon^c_{jk\ell}}\right), \qquad (8)\\
\overline{wage}_{jk\ell} &\equiv \mathbb{E}[w_\ell h_{jk}\varepsilon^h_{jk\ell}e^\eta\epsilon \mid \text{individuals choose } \ell,\, e=e^*] \\
&= \gamma\left(\frac{1}{\alpha_\ell}\right)\left(\frac{1}{1-\tau^m_{j\ell}}\right)\left[\frac{h^{1/\eta}_{jk}\eta}{(1+\tau^e_{jk})c}\right]^{\frac{\eta}{1-\eta}}\left(\sum_{\hat{\ell}} \tilde{w}^\theta_{jk\hat{\ell}}\right)^{\frac{1}{\theta(1-\eta)}}\left(\frac{1}{\varepsilon^\alpha_{jk\ell}}\right), \qquad (9)
\end{aligned}
$$

for $\gamma = \Gamma\big(1 - \frac{1}{\theta(1-\eta)}\big)$, noting that $\mathbb{E}[\epsilon^{\frac{1}{1-\eta}} \mid \text{individuals choose } \ell] = \pi_{jk\ell}^{-\frac{1}{\theta(1-\eta)}}\gamma$. Conditional on destination, education costs decrease education and wages by reducing schooling and thus human capital. Migration costs directly increase wages because those that overcome higher barriers are positively selected. Migration costs indirectly decrease education and wages through the summation terms, which capture market access. Fewer effective Fréchet draws result in lower returns to schooling. Base wages do not directly enter because higher base wages attract individuals with increasingly poor skill draws, decreasing average education and wages. With Fréchet draws, this countervailing force exactly offsets higher base wages. Amenities decrease education and wages because they attract individuals independent of schooling choices.

3.4 Equilibrium and output

In equilibrium, base wages w_ℓ clear human capital markets in each destination.

$$
\sum_{j,k} H^{\text{supply}}_{jk\ell} = H^{\text{demand}}_\ell
$$

Schooling and migration choices by individuals determine the supply of human capital.

$$
H^{\text{supply}}_{jk\ell} = N_{jk}\pi_{jk\ell}\underbrace{\mathbb{E}[h_{jk}\varepsilon^h_{jk\ell}e^\eta\epsilon \mid \text{individuals choose } \ell,\, e=e^*]}_{\bar{h}_{jk\ell}},
$$

where N_{jk} is labor force size, $\pi_{jk\ell}$ captures migration, and $\bar{h}_{jk\ell}$ is average worker quality. Representative firms determine the demand for human capital, which they use to produce output subject to their productivity. These firms maximize profits taking prices p_ℓ and productivity A_ℓ as given.

$$H_\ell^{\text{demand}} = \arg\max_{H_\ell} \underbrace{\big(p_\ell A_\ell H_\ell - w_\ell H_\ell\big)}_{\Pi_\ell}$$

Since perfect competition implies zero profits, base wages reflect marginal revenues.

$$w_\ell = p_\ell A_\ell \tag{10}$$

Productivity allows for agglomeration κ, and amenities incorporat e congestion μ.

$$A_\ell = \bar{A}_\ell H_\ell^\kappa\,, \quad \alpha_\ell = \bar{\alpha}_\ell \Bigg(\sum_{j,\,k} N_{jk} \pi_{jk\ell} \Bigg)^{-\mu} \tag{11}$$

Strong agglomeration raises wages most in high-wage places, amplifying the extent to which access to these markets increases returns to schooling.

Output Y_ℓ in each location expands to a sum of price-adjusted wages.

$$Y_\ell = \frac{1}{p_\ell} \sum_{j,\,k} N_{jk} \pi_{jk\ell} \overline{wage}_{jk\ell} \tag{12}$$

Prices p_ℓ clear the market for destination-specific goods Y_ℓ, which competitive downstream firms use to produce final good Y. Assuming costless trade across destinations,

$$Y_\ell^{\text{demand}} = \arg\max_{Y_\ell} \big(Y - p_\ell Y_\ell\big).$$

For Y_ℓ^{supply} given by equation 3, market clearing condition $Y_\ell^{\text{supply}} = Y_\ell^{\text{demand}}$ implies

$$p_\ell = \left(\frac{Y}{Y_\ell}\right)^{\frac{1}{\sigma}}. \tag{13}$$

School construction a directly lowers education costs τ_{jk}^e and in doing so also affects prices p_ℓ, productivities A_ℓ, and migration $\pi_{jk\ell}$. Output becomes

$$Y_\ell(a) = \big[p_\ell(a)\big]^{\frac{\eta}{1-\eta}} \big[A_\ell(a)\big]^{\frac{1}{1-\eta}} \sum_{j,\,k} \tilde{N}_{jk\ell} \big[\pi_{jk\ell}(a)\big]^{1-\frac{1}{\theta(1-\eta)}} \big[1 + \tau_{jk}^e(a)\big]^{-\frac{\eta}{1-\eta}}, \tag{14}$$

where I expand equation 12 and define $\tilde{N}_{jk\ell} = \gamma\big(\frac{\eta}{c}\big)^{\frac{\eta}{1-\eta}} N_{jk} \big(\frac{1-\tau_{j\ell}^m}{\varepsilon_{jk\ell}^c}\big)^{\frac{\eta}{1-\eta}} \big(h_{jk} \varepsilon_{jk\ell}^h\big)^{\frac{1}{1-\eta}}$.

3.5 Market access

For people, market access amplifies the returns to education and thus the effects of school construction. I focus on people by summing over destinations for a given district j. Doing so gives the origin-cohort terms of the baseline analysis in section 2.

$$\overline{educ}^{1}_{jk} = \sum_{\ell} \overline{educ}_{jk\ell}\pi_{jk\ell}\,, \quad \overline{wage}^{1}_{jk} = \sum_{\ell} \overline{wage}_{jk\ell}\pi_{jk\ell}$$

Taking partial derivatives with respect to district-j education costs,

$$\begin{aligned}
\frac{\partial \overline{educ}^{1}_{jk}}{\partial \tau^{e}_{jk}} &= \frac{\partial}{\partial \tau^{e}_{jk}} \left\{ \gamma \left[\frac{h_{jk}\eta}{(1+\tau^{e}_{jk})c} \right]^{\frac{1}{1-\eta}} \left(\sum_{\ell} \frac{\pi_{jk\ell}}{\alpha_{\ell}\varepsilon^{\alpha}_{jk\ell}\varepsilon^{c}_{jk\ell}} \right) MA_{jk} \right\}, \\
\frac{\partial \overline{wage}^{1}_{jk}}{\partial \tau^{e}_{jk}} &= \frac{\partial}{\partial \tau^{e}_{jk}} \left\{ \gamma \left[\frac{h^{1/\eta}_{jk}\eta}{(1+\tau^{e}_{jk})c} \right]^{\frac{\eta}{1-\eta}} \left(\sum_{\ell} \frac{\pi_{jk\ell}}{\alpha_{\ell}(1-\tau^{m}_{j\ell})\varepsilon^{\alpha}_{jk\ell}} \right) MA_{jk} \right\}.
\end{aligned}$$

Market access $MA_{jk} = \left(\sum_{\hat{\ell}} \tilde{w}^{\theta}_{jk\hat{\ell}}\right)^{\frac{1}{\theta(1-\eta)}}$ thus magnifies the effects of education costs, as access to high wages in j' increases the returns to schooling. As a result, individuals gain more from school construction.

For places, market access reduces the local gains from school construction because it encourages graduates to migrate elsewhere. I focus on places by summing over origins for a given district j.

$$\overline{educ}^{2}_{jk} = \sum_{j'} \overline{educ}_{j'kj}\pi_{j'kj}\,, \quad \overline{wage}^{2}_{jk} = \sum_{j'} \overline{wage}_{j'kj}\pi_{j'kj}$$

Taking partial derivatives with respect to district-j education costs,

$$\begin{aligned}
\frac{\partial \overline{educ}^{2}_{jk}}{\partial \tau^{e}_{jk}} &= \frac{\partial}{\partial \tau^{e}_{jk}} \left\{ \gamma \left[\frac{h_{jk}\eta}{(1+\tau^{e}_{jk})c} \right]^{\frac{1}{1-\eta}} \left(\frac{\pi_{jkj}}{\alpha_{j}\varepsilon^{\alpha}_{jkj}\varepsilon^{c}_{jkj}} \right) MA_{jk} \right\}, \\
\frac{\partial \overline{wage}^{2}_{jk}}{\partial \tau^{e}_{jk}} &= \frac{\partial}{\partial \tau^{e}_{jk}} \left\{ \gamma \left[\frac{h^{1/\eta}_{jk}\eta}{(1+\tau^{e}_{jk})c} \right]^{\frac{\eta}{1-\eta}} \left(\frac{\pi_{jkj}}{\alpha_{j}\varepsilon^{\alpha}_{jkj}} \right) MA_{jk} \right\}.
\end{aligned}$$

School construction has smaller effects on places than it does on people, particularly for places with high market access. Out-migration limits local gains.

$$\frac{\partial \overline{educ}^2_{jk}}{\partial \tau^e_{jk}} \leq \frac{\partial \overline{educ}^1_{jk}}{\partial \tau^e_{jk}}, \quad \frac{\partial \overline{wage}^2_{jk}}{\partial \tau^e_{jk}} \leq \frac{\partial \overline{wage}^1_{jk}}{\partial \tau^e_{jk}},$$

with strict inequalities when $\pi_{jkj} < 1$.

4 Estimation

This section describes identification of the spatial equilibrium model, the estimation procedure, and the parameter estimates themselves.

4.1 Moments and identification

Equations 7, 8, and 9 describe $\pi_{jk\ell}$, $\overline{educ}_{jk\ell}$, and $\overline{wage}_{jk\ell}$ and provide the basis of estimation. I measure each with data on migration, education, and wages, and I add the following additional structure on education and migration costs.

$$1 + \tau^e_{jk} = (1 + S_j T_k)^{-\beta} \delta_j \delta_k (1 + \boldsymbol{C}_j T_k)^{\boldsymbol{\phi}}, \tag{15a}$$

$$1 - \tau^m_{j\ell} = (1 + d^P_{j\ell})^{-\varphi_1} (1 + d^D_{j\ell})^{-\varphi_2} \tag{15b}$$

School construction S_j decreases education costs for treated cohorts ($T_k = 1$), subject to origin- and cohort-specific factors δ_j and δ_k and controls $\boldsymbol{C}_j$. This relationship maps counterfactual school construction onto education costs and thus outcomes. The underlying assumption is that school construction changes education costs but not other parameters, including amenities. Physical and demographic distances ($d^P_{j\ell}, d^D_{j\ell}$) increase migration costs, which are zero for non-migrants and bilaterally symmetric for migrants by construction. Physical distance is Euclidean, and demographic distance captures (pre-INPRES) dissimilarity in religion and language.[5]

Equations 7, 8, and 9 contain summation terms $\sum_{\hat{\ell}} \tilde{w}^{\theta}_{jk\hat{\ell}}$ that are mechanically

[5] Physical distance captures differences in latitude and longitude. Demographic distance captures differences in Muslim share and Indonesian share in 1971.

correlated with the error terms. Differencing eliminates these terms.

$$\log \overline{educ}_{jk\ell} - \log \overline{wage}_{jk\ell} = \log \frac{\eta}{c} - \log(1+\tau_{jk}^{e}) + \log(1-\tau_{j\ell}^{m}) - \log \varepsilon_{jk\ell}^{c}\,, \tag{16a}$$

$$\Delta_\ell \log \overline{educ}_{jk\ell} = -\Delta_\ell \log \alpha_\ell - \Delta_\ell \log \varepsilon_{jk\ell}^{\alpha} \varepsilon_{jk\ell}^{c}, \tag{16b}$$

$$\Delta_\ell \log \overline{wage}_{jk\ell} = -\Delta_\ell \log \alpha_\ell - \Delta_\ell \log(1-\tau_{j\ell}^{m}) - \Delta_\ell \log \varepsilon_{jk\ell}^{\alpha}, \tag{16c}$$

$$\Delta_\ell \log \pi_{jk\ell} = \theta \Delta_\ell \log(1-\tau_{j\ell}^{m}) + \theta \Delta_\ell \log(\alpha_\ell^{1-\eta} w_\ell) + \theta \Delta_\ell \log \tilde{\varepsilon}_{jk\ell} \tag{16d}$$

The first equation differences equations 8 and 9, and the last three equations difference with respect to a reference destination for $\Delta_\ell \log X_{jk\ell} \equiv \log X_{jk\ell} - \log X_{jk0}$.

Given human capital elasticity η, these moments identify education and migration costs, amenities, the Fréchet dispersion parameter, and base wages. Equation 16a identifies education and migration costs. By equations 15a and 15b, it becomes

$$\begin{aligned}\log \overline{educ}_{jk\ell} - \log \overline{wage}_{jk\ell} = \beta \log(1+S_j T_k) - \log \delta_j - \log \delta_k - \boldsymbol{\phi} \log(1+\boldsymbol{C}_j T_k) \\ - \varphi_1 \log(1+d_{j\ell}^{P}) - \varphi_2 \log(1+d_{j\ell}^{D}) + \log \frac{\eta}{c} - \log \varepsilon_{jk\ell}^{c}\,.\end{aligned}$$

School construction S_j is endogenous: it explicitly targeted regions with low enrollment, and thus is correlated with error $\varepsilon_{jk\ell}^{c}$ in in the cost of education. I address this endogeneity with the same difference-in-differences variation described in section 2. Rather than directly comparing districts with high and low levels of school construction, I instead compare how treated and untreated cohorts differ across such districts. Given migration costs, equations 16b and 16c identify relative amenities $\frac{\alpha_\ell}{\alpha_0}$, and equation 16d identifies Fréchet parameter θ. Given relative amenities and the elasticity of human capital, destination ℓ fixed effects in equation 16d further identify relative base wages $\frac{w_\ell}{w_0}$. Base human capital h_{jk} and base cost of education c do not enter counterfactuals and thus need not be estimated. Although human capital elasticity η is estimated separately, as I discuss below, it only affects the estimates of base wages.

4.2 Estimation procedure

First, I directly apply the INPRES variation to estimate the elasticity of human capital η with respect to schooling. In the model, wages are proportional to human capital e^{η}, which depends on schooling e. Taking logs, I obtain the following specification for individuals i of origin j and cohort k.

$$\log wage_{ijk} = \delta_j + \delta_k + \eta \log educ_{ijk} + \boldsymbol{C}_j T_k \boldsymbol{\phi} + \varepsilon_{ijk}\,, \tag{17a}$$

$$\log educ_{ijk} = \delta_j + \delta_k + \beta S_j T_k + \boldsymbol{C}_j T_k \boldsymbol{\phi} + \varepsilon_{ijk}\,, \tag{17b}$$

where I instrument for individual education $\log educ_{ijk\ell}$ with the interaction of school construction and treatment cohort ($S_j T_k$). I thus capture the causal effect of schooling on wages as in Duflo (2001). Note that the model produces heterogeneous returns to schooling despite a common parameter η governing the human capital curve. Different positions along the curve yield different effects of an additional year of education on human capital, and human capital gains translate into large wage gains only when individuals have access to high base wages. This market access multiplier is absorbed by origin fixed effects in the log-log specification above.

Second, I estimate the rest of the model by Poisson pseudo-maximum likelihood (PPML), as is common in spatial models (Santos Silva and Tenreyro 2006). Estimation in logs cannot accommodate zeros in observed migration probabilities, and taking logs is a non-linear transformation that introduces bias from heteroskedasticity. The PPML approach addresses both concerns. For a model $y_i = \exp(x_i\beta) + \varepsilon_i$, PPML uses the set of first-order conditions

$$\sum_{i=1}^{n} [y_i - \exp(x_i\hat{\beta})]x_i = 0$$

as the basis of estimation. I form these conditions for each of equations 16a, 16b, 16c, and 16d and apply the generalized method of moments. In doing so, I add moments to match the reduced-form effects on education and wages estimated in section 2.

Finally, I set general equilibrium parameters (κ, μ, σ) for agglomeration, congestion, and substitution. I follow Bryan and Morten (2019) in setting $(\kappa, \mu, \sigma) = (0.05, 0.075, 8)$ in the baseline. These parameters affect counterfactuals, but they affect neither estimation nor identification of other parameters.

4.3 Estimates

For human capital elasticity η, table 5 presents an IV estimate of 0.7 compared to an OLS estimate of 0.4. The IV estimate is larger than the OLS estimate, as is the case in Duflo (2001). There is a relatively strong first stage that indeed disappears in the placebo experiment. For the US, Hsieh et al. (2019) choose a value of 0.1 that corresponds to the fraction of output spent on human capital accumulation. They obtain this value by dividing education spending by the labor share of GDP. I take my IV estimate of $\eta = 0.7$ as a baseline value, but also I consider robustness to the OLS value of 0.4 and the Hsieh et al. (2019) value of 0.1.

Table 6 shows estimates for education and migration costs. The β parameter captures the relationship between school construction and education costs. Higher values imply higher initial gains from school construction. The estimate is positive and significant in the treatment sample, which is exposed to school construction, and insignificant in the placebo sample, which is not exposed. The φ parameters capture the relationship between distance and migration costs. Higher values imply higher gains from market access. The estimates suggest that migration costs are driven by physical distance, with demographic distance having little effect. The treatment and placebo groups face equal distances and thus produce similar estimates, as school construction affects neither physical nor demographic distance directly.

5 Counterfactuals

This section quantifies the long-run aggregate and distributional effects of the program in spatial equilibrium.

5.1 Solving the model

I compute the aggregate output $Y(a)$ resulting from a given allocation of school construction with equations 3 and 14. The problem simplifies under zero agglomeration ($\kappa = 0$) and perfect substitution ($\sigma \to \infty$), in which case the only effect of school construction is to lower education costs. Prices and productivity remain fixed, implying fixed wages and thus fixed migration.

$$p_\ell = 1\,, \quad A_\ell = \bar{A}_\ell\,, \quad w_\ell = p_\ell A_\ell = \bar{A}_\ell$$

It follows that counterfactual output Y'_ℓ in each destination is a simple function of counterfactual school construction S'_j, parameter β, and observed quantities.

$$Y'_\ell = \sum_{j,k} N_{jk}\pi_{jk\ell}\overline{wage}_{jk\ell}\left(\frac{1+S'_jT_k}{1+S_jT_k}\right)^{\frac{\beta\eta}{1-\eta}}$$

Parameters β and η alone are sufficient for counterfactuals, with observed quantities proxying for other fundamentals as in the exact-hat algebra of Dekle et al. (2008).

More generally, prices, productivity, and migration respond to changes in education costs. School construction affects productivity under agglomeration, and it affects prices under imperfect substitution. In both cases, wages and thus migration also respond. I use the following algorithm to solve for each quantity in equilibrium.

1. Compute $(p_\ell, Y_\ell, Y_{jk\ell}, \bar{A}_\ell)$ given data $(N_{jk}, \pi_{jk\ell}, \overline{wage}_{jk\ell})$ and estimates (w_ℓ, α_ℓ).
   (a) Solve for (p_ℓ, Y_ℓ) jointly with equations 12 and 13 across destinations.
   (b) Compute $Y_{jk\ell} = \frac{1}{p_\ell}N_{jk}\pi_{jk\ell}\overline{wage}_{jk\ell}$.
   (c) Compute $\bar{A}_\ell = Y_\ell / H_\ell^{\kappa+1}$ for $H_\ell = \sum_{jk} H_{jk\ell}$ and $H_{jk\ell} = \frac{1}{w_\ell}N_{jk}\pi_{jk\ell}\overline{wage}_{jk\ell}$.
2. Compute (p'_ℓ, Y'_ℓ) given $(p_\ell, Y_{jk\ell})$ ignoring changes in $(A_\ell, \pi_{jk\ell})$.
   (a) Solve for (p'_ℓ, Y'_ℓ) jointly with equation 13 and

$$Y'_\ell = \left(\frac{p'_\ell}{p_\ell}\right)^{\frac{\eta}{1-\eta}} \sum_{j,k} Y_{jk\ell}\left(\frac{1+S'_jT_k}{1+S_jT_k}\right)^{\frac{\beta\eta}{1-\eta}}.$$

3. Compute $(A'_\ell, \pi'_{jk\ell})$ given $(p'_\ell, Y'_\ell, \bar{A}_\ell)$.
   (a) Compute $A'_\ell = (\bar{A}_\ell)^{\frac{1}{\kappa+1}}(Y'_\ell)^{\frac{\kappa}{\kappa+1}}$ and $w'_\ell = p'_\ell A'_\ell$.
   (b) Solve for $\pi'_{jk\ell}$ with

$$\pi'_{jk\ell} = (\tilde{w}'_{jk\ell})^\theta / \sum_{\hat{\ell}} (\tilde{w}'_{jk\hat{\ell}})^\theta, \quad \tilde{w}'_{jk\ell} = \pi_{jk\ell}^{1/\theta}\left(\frac{w'_\ell}{w_\ell}\right)\left(\frac{\sum_{j,k} N_{jk}\pi'_{jk\ell}}{\sum_{j,k} N_{jk}\pi_{jk\ell}}\right)^{-\mu(1-\eta)}.$$

4. Recompute (p'_ℓ, Y'_ℓ) given $(A'_\ell, \pi'_{jk\ell})$.
   (a) Solve for (p'_ℓ, Y'_ℓ) jointly with equation 13 and

$$Y'_\ell = \left(\frac{p'_\ell}{p_\ell}\right)^{\frac{\eta}{1-\eta}} \left(\frac{A'_\ell}{A_\ell}\right)^{\frac{1}{1-\eta}} \sum_{j,k} Y_{jk\ell}\left(\frac{\pi'_{jk\ell}}{\pi_{jk\ell}}\right)^{1-\frac{1}{\theta(1-\eta)}} \left(\frac{1+S'_jT_k}{1+S_jT_k}\right)^{\frac{\beta\eta}{1-\eta}}.$$

5. Iterate steps (3) to (4) until convergence and compute aggregate output Y.

In relying on adjustments to the observed equilibrium, I minimize parameter estimates needed for counterfactuals. Also note that relative base wage and amenity estimates are sufficient, as the normalizations cancel.

5.2 Evaluating the program

Table 7 presents the aggregate and distributional effects of the program. The program increases aggregate output by eight percent relative to zero construction. Students from rural regions experience the largest gains, as new schools bring greater benefits to people from less-educated rural regions relative to more-educated urban ones. In increasing the opportunities available to rural students, the program decreases inequality between people from rural and urban regions by five percent. That is, inequality across people falls as urban and rural students converge following nationwide school construction.

The government may also value convergence between rural and urban regions themselves (net of out-migration). Reducing inequality across places was an explicit motivation for targeting INPRES school construction to high-unenrollment regions, and both equity and political economy considerations can rationalize such a policy goal.[6] I find that the program increases inequality between rural and urban places by twelve percent. Rural-to-urban migration fuels output gains by connecting rural human capital to high urban wages, but it does so at the expense of rural regions. The program remains a Pareto improvement relative to zero school construction because rural regions still benefit from modest output gains and higher human capital. But regional inequality rises because urban regions benefit even more.

Mobility drives both aggregate and distribution effects. Shutting down mobility entirely by setting migration costs to one, I find that the direct effect of school construction is to increase output by only two percent. Inequality among people and inequality among places coincide and are similar to inequality under zero construction. Although rural students have higher marginal returns to education, they are confined to low-wage labor markets and thus invest little in education. Urban students have

[6] Indonesia's transmigration program of the 1980s is another example of a policy aimed at developing "lagging" regions.

access to high urban wages and thus larger incentives to invest in education, but they also face lower marginal returns given higher baseline levels of education.

Allowing migration boosts output in three ways. First, conditional on schooling and wages, individuals sort into high-productivity districts with larger returns to schooling, raising output by another one percentage point. Second, conditional on wages, these larger returns in turn increase investment in schooling, raising output by another four percentage points. Third, wages adjust in general equilibrium, raising output by another percentage point on net. Equilibrium forces include selection, which depresses urban wages with rising in-migration, and agglomeration-based human capital externalities, which increases urban wages as total urban human capital rises. Previous work has focused on sorting, but the education effect dominates here even net of general equilibrium effects (Bryan et al. 2014). At the same time, each set of output gains is driven by rural students migrating to high urban wages, and thus is in tension with the desire to reduce inequality across places.

I study the design of the program by considering alternative allocations of school construction. The program explicitly targeted low-enrollment regions with an allocation rule based on unenrollment. Doubling low-enrollment construction, subject to the budget constraint, leads to higher aggregate output and lower inequality among people, but higher inequality among places. Halving low-enrollment construction has the opposite effects. Thus, targeting rural regions leads to large aggregate output gains and boosts opportunities for rural students, but a government concerned with long-run inequality across places must trade these benefits off against increased regional disparities.

I repeat both exercises under a smaller set of migration costs, which increases mobility and magnifies the equity-efficiency tradeoff. Given the interaction between migration and education costs, lowering migration costs – such as by investing in roads – greatly amplifies the output gains from school construction. Like the baseline scenario, inequality between rural and urban places rises. But unlike baseline, where rural regions experience small but nonetheless positive gains, these counterfactuals involve meaningful losses to rural regions. The reason is that lower migration costs increase rural-to-urban migration, which drains rural populations. When agglomeration forces are strong, the resulting losses to rural regions are especially severe. Thus, although coordinated investment in schools and roads is substantially more effective than school construction alone, it is also no longer Pareto-improving relative to zero construction.

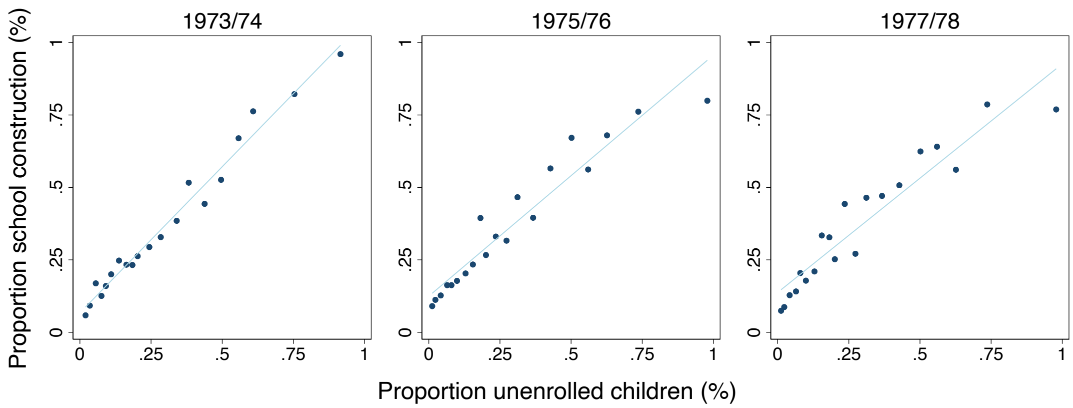
6 Conclusion

Spatial effects are crucial for evaluating large-scale educational investment because graduates migrate for employment. Mobility amplifies the returns to education, increasing output but draining rural regions. This paper capture these forces with a spatial equilibrium model and uses it to quantify the aggregate and distributional effects of Indonesia's Sekolah Dasar INPRES program, which constructed 62,000 primary schools in the mid-1970s. I find that the program increased long-run aggregate output by seven percent, but also increased regional inequality by twelve percent. Migration accounts for nearly all of each effect.

Several lines of inquiry are left for future work. First, future work might quantify the complementary effects of joint investment in schools and roads. Such an approach may be valuable given the interaction between education and migration costs. Second, I assume that school construction lowers education costs by increasing physical access, but the effects of new schools might also depend on factors like school quality and interactions with existing schools. Third, public school construction may prompt equilibrium responses by the private sector that affect aggregate outcomes. Such work informs policymakers' ongoing efforts to invest in education, which remains fundamental to economic development.

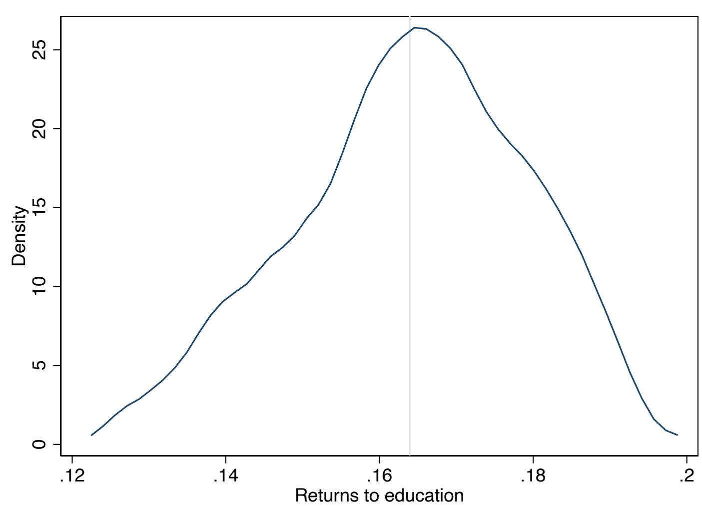
Figures

Figure 1: INPRES school construction vs. unenrollment rates by district



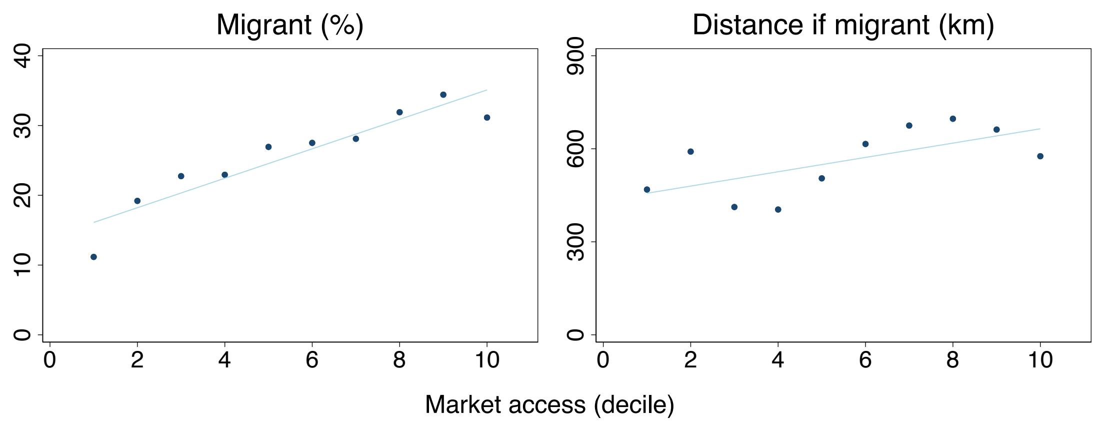
Each figure is a binned scatter plot, and each observation is one district. The y-axis is the proportion of total school construction allocated to each district. The x-axis in 1973/1974 is the pre-program unenrollment rate among children of primary school age, and in other years is how much the rate exceeds 15%. I omit outliers by dropping the 5% of districts with extreme unenrollment rates.

Figure 2: Returns to education



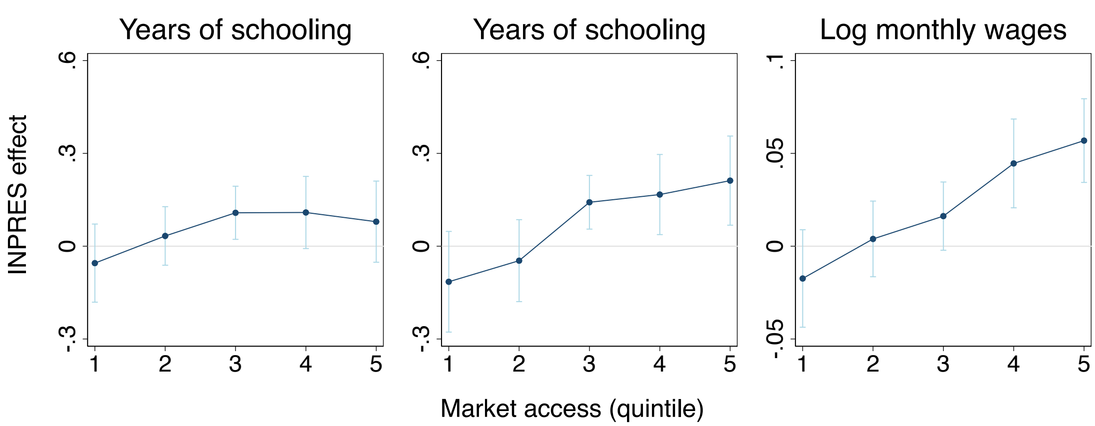
Data come from SUSENAS 2011, 2012, 2013, and 2014 and focus on male heads of household ages 2 to 24 in 1974. I compute the distribution of returns to education by computing the distribution of schooling and wage treatment effects with change-in-changes and taking the ratio. The gray vertical lines shows the mean.

Figure 3: Migration and market access



Data come from SUSENAS 2011, 2012, 2013, and 2014 and focus on male heads of household ages 2 to 24 in 1974. Migrants reside outside of their birth districts, and migration distances are Euclidean and between district centroids. Market access is an inverse-distance-weighted average of 1971 population densities across districts.

Figure 4: INPRES effects by market access



Each figure is one regression. Data come from SUSENAS 2011, 2012, 2013, and 2014 and focus on male heads of household. I compare individuals ages 2 to 6 and those ages 12 to 17 in 1974. I report treatment effects by quartile of market access. Market access is an inverse-distance-weighted average of 1971 population densities across districts. Regressions control for birth district, birth year, and survey year fixed effects, as well as 1971 child population, 1971 enrollment rates, and INPRES spending on water and sanitation projects. Standard errors are clustered by birth district. Error bars shows 95% confidence bands.

Tables

Table 1: INPRES school construction vs. population density and market access

	School construction	School construction	School construction
Population density	-0.0748***		-0.0309***
	(0.0155)		(0.0118)
Market access		-0.417***	-0.359***
		(0.0759)	(0.0851)
Observations	282	282	282

Each row is one regression, and each observation is a district. Population density is 1971 population divided by land area. Market access is an inverse-distance-weighted average of 1971 population densities across districts. School construction is INPRES schools built per million children in 1971. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 2: INPRES effects on education and labor

	Treatment			Placebo		
Outcomes	Estimate	SE	Obs	Estimate	SE	Obs
Years of schooling	0.103**	(0.0424)	233,517	-0.0176	(0.0318)	196,308
— For wage earners	0.121**	(0.0495)	89,404	0.0120	(0.0566)	55,091
Log monthly wages	0.0195**	(0.00916)	89,404	-0.00765	(0.00890)	55,091
Primary school completion	0.0585**	(0.0291)	233,517	-0.0134	(0.0167)	196,308
Middle school completion	0.0480**	(0.0207)	233,517	0.00573	(0.0156)	196,308
High school completion	0.0292	(0.0180)	233,517	-0.00167	(0.0140)	196,308
University completion	-0.0236	(0.0196)	233,517	-0.00792	(0.0214)	196,308
Employment	0.0304	(0.0278)	241,173	0.0309	(0.0216)	203,995
Wage employment	0.000376	(0.0131)	241,173	-0.0204	(0.0189)	203,995
Self-employment	-0.00219	(0.0119)	241,173	0.0140	(0.0142)	203,995
Weekly hours	-0.136	(0.102)	229,662	-0.00968	(0.109)	183,840

Each row is one treatment and one placebo regression. Data come from SUSENAS 2011, 2012, 2013, and 2014 and focus on male heads of household. Treatment compares individuals ages 2 to 6 and those ages 12 to 17 in 1974; placebo compares individuals ages 12 to 17 and those ages 18 to 24 in 1974. I run logit regressions for dummy outcomes. Regressions control for birth district, birth year, and survey year fixed effects, as well as 1971 child population, 1971 enrollment rates, and INPRES spending on water and sanitation projects. Standard errors are clustered by birth district. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 3: INPRES effects on migration

	Treatment			Placebo		
Outcomes	Estimate	SE	Obs	Estimate	SE	Obs
Migrant	0.0244	(0.0194)	244,793	-0.0249*	(0.0129)	210,543
Distance if migrant (km)	-5.097	(7.706)	62,717	-0.659	(6.656)	51,445
Migrant to urban	0.0284	(0.0307)	242,646	-0.0170	(0.0212)	207,096
Migrant to rural	0.0259	(0.0236)	244,793	-0.0169	(0.0170)	210,543
Migrant from urban to urban	0.0468	(0.0445)	116,594	0.0141	(0.0291)	105,664
Migrant from urban to rural	0.0449	(0.0276)	116,594	-0.0213	(0.0242)	105,664
Migrant from rural to urban	-0.00490	(0.0375)	126,052	-0.0484	(0.0335)	101,432
Migrant from rural to rural	-0.0113	(0.0260)	128,199	-0.0203	(0.0244)	104,879

Each row is one treatment and one placebo regression. Data come from SUSENAS 2011, 2012, 2013, and 2014 and focus on male heads of household. Treatment compares individuals ages 2 to 6 and those ages 12 to 17 in 1974; placebo compares individuals ages 12 to 17 and those ages 18 to 24 in 1974. I run logit regressions for dummy outcomes. Regressions control for birth district, birth year, and survey year fixed effects, as well as 1971 child population, 1971 enrollment rates, and INPRES spending on water and sanitation projects. Standard errors are clustered by birth district. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 4: INPRES effects for people vs. places

	People			Places		
	Years of schooling	Years of schooling	Log wages (month)	Years of schooling	Years of schooling	Log wages (month)
INPRES $\times$ young	0.103**	0.121**	0.0195**	0.0517	0.0260	0.0112
	(0.0424)	(0.0495)	(0.00916)	(0.0452)	(0.0506)	(0.00760)
Observations	233,517	89,404	89,404	232,915	89,252	89,252

Each row is one regression. Data come from SUSENAS 2011, 2012, 2013, and 2014 and focus on male heads of household. I compare individuals ages 2 to 6 and those ages 12 to 17 in 1974. Treatment is school construction in the district of birth (people) or residence (places). I run logit regressions for dummy outcomes. Regressions control for birth district, birth year, and survey year fixed effects, as well as 1971 child population, 1971 enrollment rates, and INPRES spending on water and sanitation projects. Standard errors are clustered by birth district. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 5: Human capital elasticity η

	Treatment			Placebo		
	OLS	IV	First stage	OLS	IV	First stage
Log years of schooling	0.393***	0.688**		0.394***	-1.357	
	(0.00721)	(0.311)		(0.00678)	(3.523)	
INPRES × young			0.0284***			0.00564
			(0.00899)			(0.0110)
Observations	89,404	89,404	89,404	55,091	55,091	55,091
F-statistic			9.97			0.26

Each column is one regression. Data come from SUSENAS 2011, 2012, 2013, and 2014 and focus on male heads of household. Treatment estimates compare individuals ages 2 to 6 and those ages 12 to 17 in 1974; placebo estimates compare individuals ages 12 to 17 and those ages 18 to 24 in 1974. The outcome variable is log monthly wages, and the instrument for log years of schooling is the interaction of INPRES program intensity and treatment cohort. Regressions control for birth district, birth year, and survey year fixed effects, as well as 1971 child population, 1971 enrollment rates, and INPRES spending on water and sanitation projects. Standard errors are clustered by birth district. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 6: Education cost β and migration costs (φ_1, φ_2)

	Treatment		Placebo	
	Estimate	SE	Estimate	SE
β	0.110**	(0.0467)	0.0514	(0.0457)
φ_1	0.0415***	(0.00353)	0.0388***	(0.00423)
φ_2	0.0184	(0.0500)	-0.0299	(0.0658)

Estimates correspond to parameters of equations 15a and 15b. Data come from SUSENAS 2011, 2012, 2013, and 2014 and focus on male heads of household. Treatment estimates compare individuals ages 2 to 6 and those ages 12 to 17 in 1974; placebo estimates compare individuals ages 12 to 17 and those ages 18 to 24 in 1974. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 7: INPRES aggregate and distributional effects

	Aggregate output	Inequality (people)	Inequality (places)
Evaluating the program			
Zero construction	1.00	1.00	1.00
Actual INPRES allocation	1.08	0.95	1.12
Decomposing migration effects			
Direct effect of construction	1.02	0.99	0.99
+ Migration	1.03	0.98	1.02
+ Migration-induced schooling	1.07	0.96	1.11
+ New equilibrium wages	1.08	0.95	1.12
Alternative allocation 1			
Doubling low-enrollment construction	1.09	0.93	1.14
+ Halving migration costs	1.13	0.90	1.18
Alternative allocation 2			
Doubling high-enrollment construction	1.04	0.97	1.06
+ Halving migration costs	1.08	0.93	1.12

Each row is one counterfactual. Data come from SUSENAS 2011, 2012, 2013, and 2014 and focus on male heads of household. Values are ratios relative to zero construction. In the second panel, I start with INPRES school construction under infinite migration costs. Next, I lower migration costs to those estimated but hold schooling decisions fixed. Finally, I allow school decisions to adjust. In the third and fourth panels, I analyze alternative allocations under baseline migration costs, then I lower all bilateral migration costs to half of their baseline levels.

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A Appendix

A.1 Data and Stylized Facts

The 1976 and 1995 Intercensal Population Surveys (SUPAS) allow for additional placebo experiments with individual-level data for earlier periods. As in the baseline placebo experiment, I compare age cohorts that are both unexposed. In the 1995 SUPAS data, I compare individuals ages 12 to 17 and those ages 18 to 24 in 1974 – the same cohorts in the primary placebo experiment. This comparison replicates the placebo experiments in Duflo (2001). In the 1976 SUPAS data, I compare individuals ages 12 to 17 and those ages 18 to 24 in 1955. I focus on these earlier cohorts because those in the primary placebo experiment are not yet of working age. Tables A1 and A2 show that these experiments largely produce insignificant estimates for the outcomes considered in the main analysis.

Table A1: INPRES effects on education and labor (placebo)

	SUPAS 1976			SUPAS 1995		
Outcomes	Estimate	SE	Obs	Estimate	SE	Obs
Years of schooling	-0.0175	(0.0703)	18,173	0.0411	(0.0470)	64,392
— For wage earners	0.169	(0.164)	6,461	-0.0185	(0.0765)	25,159
Log monthly wages	0.00219	(0.0280)	6,461	0.000511	(0.00808)	25,159
Primary school completion	-0.0489	(0.0479)	18,061	0.00535	(0.0252)	64,392
Middle school completion	-0.01000	(0.0535)	18,135	0.0213	(0.0219)	64,392
High school completion	0.103	(0.0738)	17,838	0.0408	(0.0287)	64,392
University completion	0.239	(0.151)	12,598	-0.0357	(0.0520)	63,828
Employment	0.0900	(0.119)	16,135	-0.0125	(0.0487)	68,595
Wage employment	-0.000908	(0.0526)	18,098	0.00264	(0.0171)	69,114
Self-employment	0.00755	(0.0568)	18,067	-0.00622	(0.0180)	69,114
Weekly hours	-0.250	(0.270)	16,903	0.0277	(0.128)	66,423

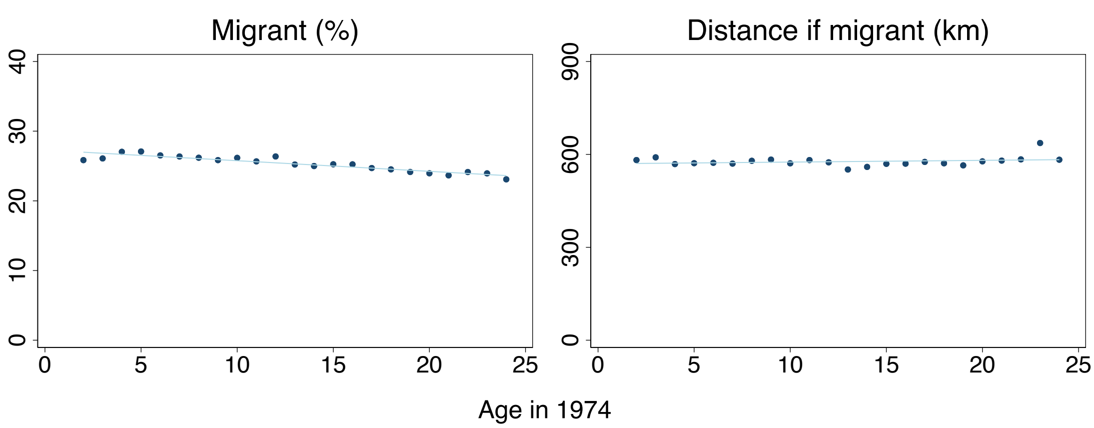
Each row is two placebo regressions. Data focus on male heads of household. SUPAS 1976 compares individuals ages 12 to 17 and those ages 18 to 24 in 1955; SUPAS 1995 compares individuals ages 12 to 17 and those ages 18 to 24 in 1974. I run logit regressions for dummy outcomes. Regressions control for birth district, birth year, and survey year fixed effects, as well as 1971 child population, 1971 enrollment rates, and INPRES spending on water and sanitation projects. Standard errors are clustered by birth district. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A2: INPRES effects on migration (placebo)

	SUPAS 1976			SUPAS 1995		
Outcomes	Estimate	SE	Obs	Estimate	SE	Obs
Migrant	0.0480	(0.0590)	16,860	-0.0224	(0.0232)	69,114
Distance if migrant (km)	11.57	(22.65)	5,584	25.47**	(12.32)	20,639
Migrant to urban	0.0544	(0.0625)	17,058	-0.0128	(0.0321)	66,207
Migrant to rural	-0.0395	(0.0950)	15,967	-0.0167	(0.0280)	69,114
Migrant from urban to urban	-0.0532	(0.0780)	11,751	0.0352	(0.0419)	38,415
Migrant from urban to rural	-0.0571	(0.123)	10,735	-0.00784	(0.0460)	38,415
Migrant from rural to urban	0.238**	(0.110)	5,307	-0.0908*	(0.0507)	27,792
Migrant from rural to rural	-0.0758	(0.154)	5,232	0.00545	(0.0377)	30,699

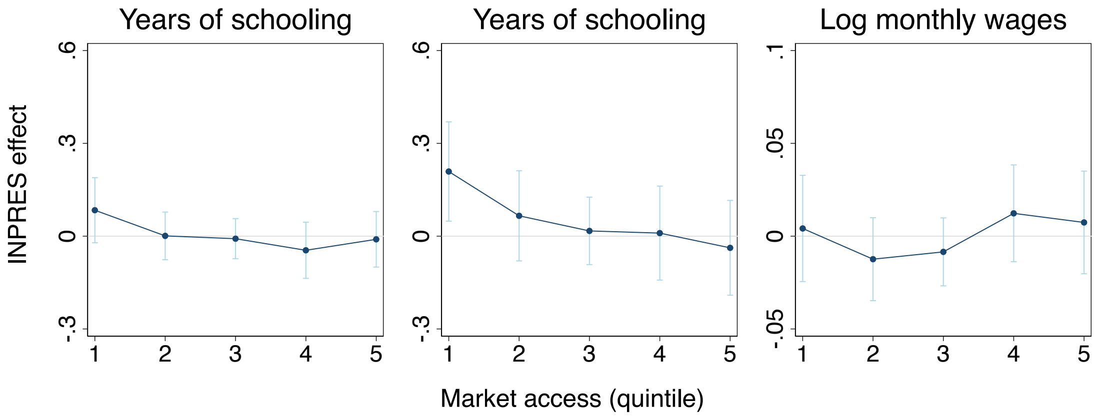
Each row is two placebo regressions. Data focus on male heads of household. SUPAS 1976 compares individuals ages 12 to 17 and those ages 18 to 24 in 1955; SUPAS 1995 compares individuals ages 12 to 17 and those ages 18 to 24 in 1974. I run logit regressions for dummy outcomes. Regressions control for birth district, birth year, and survey year fixed effects, as well as 1971 child population, 1971 enrollment rates, and INPRES spending on water and sanitation projects. Standard errors are clustered by birth district. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Figure A1: Migration and age cohort



Data come from SUSENAS 2011, 2012, 2013, and 2014 and focus on male heads of household ages 2 to 24 in 1974. Migrants reside outside of their birth districts, and migration distances are Euclidean and between district centroids.

Figure A2: Placebo INPRES effects by market access



Each figure is one regression. Data come from SUSENAS 2011, 2012, 2013, and 2014 and focus on male heads of household. I compare individuals ages 12 to 17 and those ages 18 to 24 in 1974. I report treatment effects by quartile of market access. Market access is an inverse-distance-weighted average of 1971 population densities across districts. Regressions control for birth district, birth year, and survey year fixed effects, as well as 1971 child population, 1971 enrollment rates, and INPRES spending on water and sanitation projects. Standard errors are clustered by birth district. Error bars shows 95% confidence bands.

Table A3: INPRES effects by market access

	Treatment			Placebo		
	Years of schooling	Years of schooling	Log wages (month)	Years of schooling	Years of schooling	Log wages (month)
INPRES × young	0.0309	-0.0457	-0.0167	0.0157	0.0817	-0.0152
	(0.0489)	(0.0612)	(0.0103)	(0.0367)	(0.0679)	(0.0106)
—— × MA	0.0899**	0.207***	0.0449***	-0.0412	-0.0855*	0.00923
	(0.0350)	(0.0427)	(0.00624)	(0.0256)	(0.0451)	(0.00733)
Observations	233,517	89,404	89,404	196,308	55,091	55,091

Each column is one regression. Data come from SUSENAS 2011, 2012, 2013, and 2014 and focus on male heads of household. Treatment compares individuals ages 2 to 6 and those ages 12 to 17 in 1974; placebo compares individuals ages 12 to 17 and those ages 18 to 24 in 1974. Market access is an inverse-distance-weighted average of 1971 population densities across districts. Regressions control for birth district, birth year, and survey year fixed effects, as well as 1971 child population, 1971 enrollment rates, and INPRES spending on water and sanitation projects. Standard errors are clustered by birth district. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A4: INPRES effects on aggregate output

	Aggregate output
Zero construction	1.00
Uniform construction	1.02
Actual INPRES allocation	1.08
District-based investment	1.01
+ Internalizing spillovers	1.04
+ Internalizing complementarities	1.08

Each row is one counterfactual. Data come from SUSENAS 2011, 2012, 2013, and 2014 and focus on male heads of household. Values are ratios relative to zero construction.

A.2 Counterfactuals

Spatial spillovers imply that investment should be centralized, as table A4 illustrates. If districts must fund school construction themselves, then construction is greatly reduced, and the program increases aggregate output by only one percent. The reason is that districts realize only part of the benefits of local construction, and they do not internalize benefits for other districts. I determine construction levels by computing the marginal social benefit of construction in each district under the observed allocation, then reducing construction in each district until the marginal district benefit of construction matches the computed social benefit. I do so taking other districts' investment to be zero. Centralized investment has two effects. First, it increases investment by internalizing cross-district spillover effects, raising aggregate output by another three percentage points. Second, it takes advantage of complementarities in investment. Taking other districts' investment to instead be at observed levels, aggregate output increases by a further three percentage points.

I also consider alternative allocations of school construction, subject to the observed budget constraint. Spatial interdependence demands joint optimization over locations, as school construction in one district depends on and affects labor markets in all other districts. The result is a combinatorial allocation problem that suffers from a curse of dimensionality. As a result, the optimal allocation is difficult to compute. I simplify the problem by focusing on allocation rules similar to the one used in reality. The actual rule allocated schools with weights proportional to child unenrollment. Alternative rules vary in the weights and observables considered. For a given weighting scheme $\mathcal{P}$, set of observables X, and budget A, I choose weights

$$\rho^* = \arg\max_{\rho \in \mathcal{P}} \left\{ Y\big(a(\rho; X, A)\big) \right\} \quad \text{for} \quad a_\ell(\rho; X, A) = A\left(\frac{X_\ell \rho}{\sum_\ell X_\ell \rho}\right).$$

Optimization is simplified because I choose among weights, rather than among the full set of possible allocations. Proportional weighting schemes use uniform weights, avoiding optimization altogether ($\mathcal{P}_{\text{prop}} = \{\rho \mid \rho = \mathbf{1}\}$). Linear schemes admit one weight parameter, and quadratic schemes admit two. I take total expenditures as the budget constraint, and indeed the INPRES program specified costs by district. For 1973, these costs range from 2.5M IDR for non-urban districts in Sumatra, Java, Bali, and Kalimantan to 7M IDR for districts in Greater Jakarta.

Table A5 shows the effects of alternative allocations on aggregate output. The actual rule captures diminishing marginal returns by conditioning on unenrollment. Indeed, this rule is more effective than rules conditioned on other single observables. It is also more effective than a uniform rule that neglects observables entirely (table A4). More flexible weighting schemes increase effectiveness, but larger gains come from combining unenrollment and other observables. Ruralness is a rough proxy for market access that is already commonly considered in regional planning, and distance to the nearest urban district is an even better proxy. Rules that combine unenrollment and urban distance substantially outperform those that consider unenrollment alone. Unenrollment alone is insufficient because it is uncorrelated with market access (figure A3), and thus misses an important force in the full model. Table A6 shows that agglomeration κ and substitutability σ both magnify differences across rules by increasing the gains from targeting high-value districts.

Uncertainty can rationalize the use of the actual allocation rule over the redesigned one. Long-run migration costs are uncertain at the time of allocating school construction, and these migration costs have important effects on schooling and wages as previously discussed. I therefore consider expected aggregate output

$$\mathbb{E}_\upsilon[Y(a;\upsilon)] = \int Y(a;\upsilon) f(\upsilon) d\upsilon \,,$$

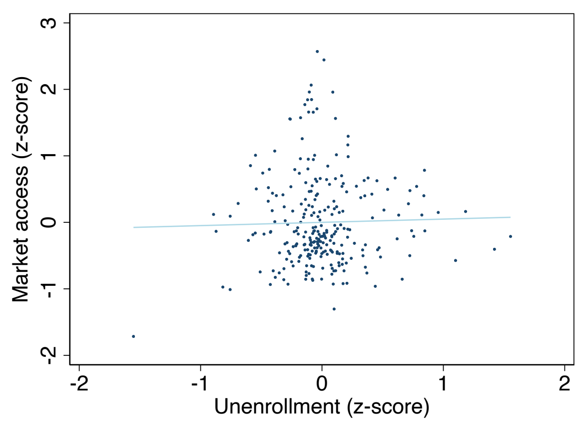
subject to multiplicative distortions υ to migration costs. Figure A4 shows that the

Table A5: Effects of alternative allocation rules on aggregate output

	Weighting scheme			
Observables	Proportional	Cutoff	Linear	Quadratic
Child population	1.05	1.05	1.05	1.06
Unenrollment	1.07	1.07	1.08	1.08
Ruralness	1.04	1.04	1.04	1.05
Urban distance	1.04	1.04	1.05	1.05
Child population + ruralness	1.07	1.07	1.08	1.08
Child population + urban distance	1.07	1.08	1.08	1.09
Unenrollment + ruralness	1.08	1.09	1.09	1.09
Unenrollment + urban distance	1.09	1.09	1.09	1.10

Each row is one counterfactual. Data come from SUSENAS 2011, 2012, 2013, and 2014 and focus on male heads of household. Values are ratios relative to aggregate output under zero construction. Unenrollment is unenrolled school-age child population, and urban distance is Euclidean distance to the nearest urban district.

Figure A3: Pre-INPRES unenrollment vs. market access



Each observation is one district. Market access is an inverse-distance-weighted average of 1971 population densities across districts. Unenrollment is total unenrolled school-age child population. The figure controls for 1971 population.

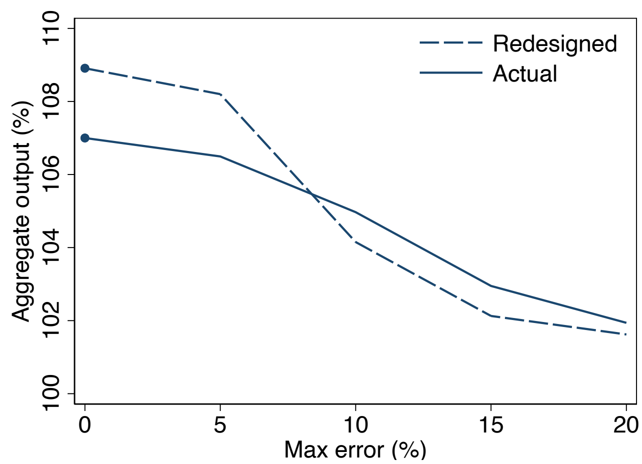
redesigned rule dominates when uncertainty is low, as it allows more precise targeting. But it also involves weight parameters fit in expectation, effectively overfitting to mean error scenarios. As such, the actual rule dominates when uncertainty is high. That is, complex rules can be more effective, but simpler rules are more robust. Indeed, policymakers often employ simple rules in complex environments, including population cutoffs and ranked lists for public investment. Future work can consider

Table A6: Aggregate output by allocation, agglomeration, and substitutability

	Agglomeration (κ)				Substitutability (σ)			
Allocation	0	0.025	0.05	0.075	4	8	16	∞
None	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00
Actual	1.04	1.05	1.07	1.10	1.07	1.07	1.08	1.09
Redesigned	1.05	1.07	1.09	1.14	1.08	1.09	1.11	1.12

Each row is one counterfactual. Data come from SUSENAS 2011, 2012, 2013, and 2014 and focus on male heads of household. Values are ratios relative to aggregate output under zero construction. The redesigned allocation is proportional to unenrolled school-age child population and Euclidean distance to the nearest city.

Figure A4: Aggregate output by allocation rule under uncertainty



The redesigned rule is proportional to unenrollment and urban distance, and the actual rule is proportional to unenrollment. Aggregate output values are relative to zero construction. Max error $\bar{\upsilon}$ implies multiplicative distortions $\upsilon \sim U[1-\bar{\upsilon}, 1+\bar{\upsilon}]$ to migration costs.

a similar exercise with uncertainty in the effects of school construction on education costs. More broadly, rationalizing the actual allocation rule is possible with other objective functions as well. Political concerns are one example, and I pursue this line of inquiry in related work on healthcare infrastructure in Indonesia (Hsiao 2021).