Educational Investment in Spatial Equilibrium: Evidence from Indonesia

Allan Hsiao [*]

Princeton University

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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 students invest in education, then migrate for employment after graduation. I find that the program increased aggregate output by 8%, with large gains for rural students but small gains for rural regions. Labor market integration magnifies each effect, as education and migration are complements: access to high urban wages raises the returns to education, but also encourages students to leave rural regions behind.

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 labor 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

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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. I illustrate these trade-offs by computing (ex-post) optimal allocations under a range of objective functions and tracing out the possibilities frontier.

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

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[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.

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, 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), Adukia et al. (2020), Moneke (2020), Balboni (2021), Zárate (2021), and Milsom (2022).[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

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[2] Other examples include work on roads (Fajgelbaum and Schaal 2020, Gertler et al. 2019, Graff 2022), 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 2022), and buses (Balboni et al. 2020).

Figure 1: INPRES school construction vs. unenrollment rates by district
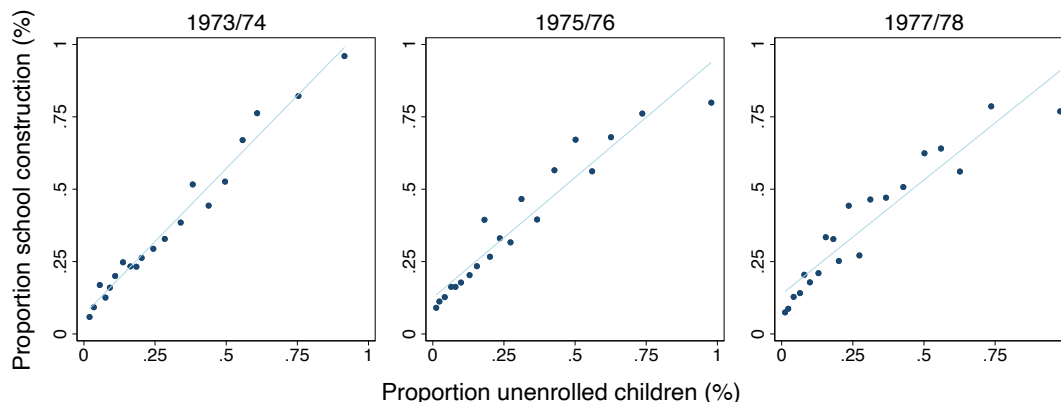
Each figure is a binned scatter plot, and each observation is one district. The y-axis is the proportion of total school construction allocated to each district. The x-axis in 1973/1974 is the pre-program unenrollment rate among children of primary school age, and in other years is how much the rate exceeds 15%. I omit outliers by dropping the 5% of districts with extreme unenrollment rates.

provide portable benefits that out-migration magnifies and distributes. I quantify these benefits for one of the largest school construction programs in history.

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 appendix table A3 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. I study male heads of household to avoid issues of intrahousehold bargaining that might otherwise constrain migration, which is the focus of this paper. "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

**Table 1:** INPRES effects on education and labor

Outcomes	Treatment			Placebo		
	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. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

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 1 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

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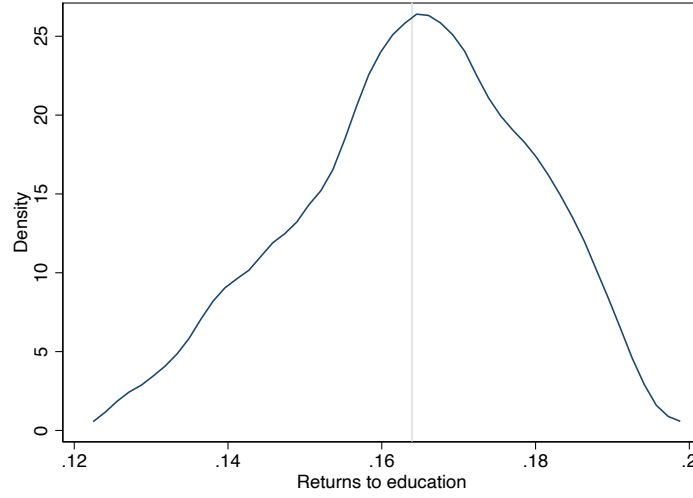
Data come from SUSENAS 2011, 2012, 2013, and 2014 and focus on male heads of household ages 2 to 24 in 1974. I compute the distribution of returns to education by computing the distribution of schooling and wage treatment effects with change-in-changes and taking the ratio. The gray vertical lines shows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

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 labor 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

Figure 3: Migration and market access
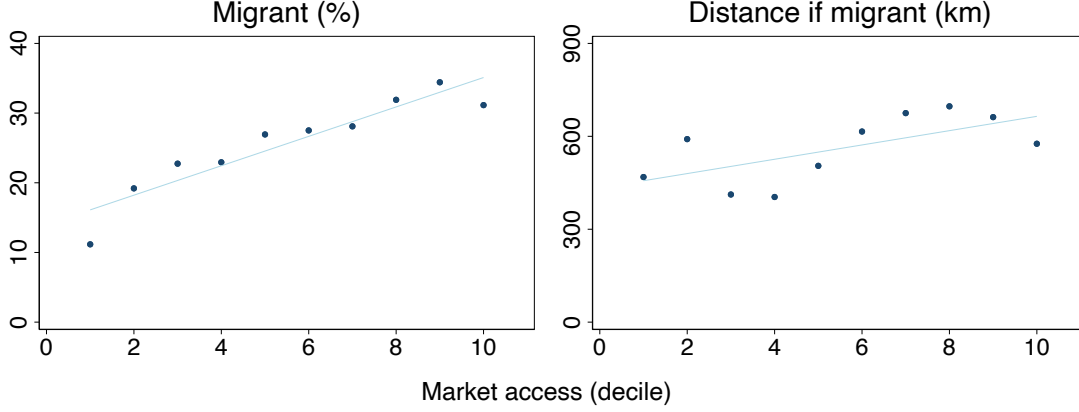
Data come from SUSENAS 2011, 2012, 2013, and 2014 and focus on male heads of household ages 2 to 24 in 1974. Migrants reside outside of their birth districts, and migration distances are Euclidean and between district centroids. Market access is an inverse-distance-weighted average of 1971 population densities across districts.

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

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

Effects increase in market access. Appendix figure A2 shows null effects in the placebo experiment, and appendix table A4 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. I take market access at exogenous, as I construct the measure with 1971 populations

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[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.

Figure 4: INPRES effects by market access
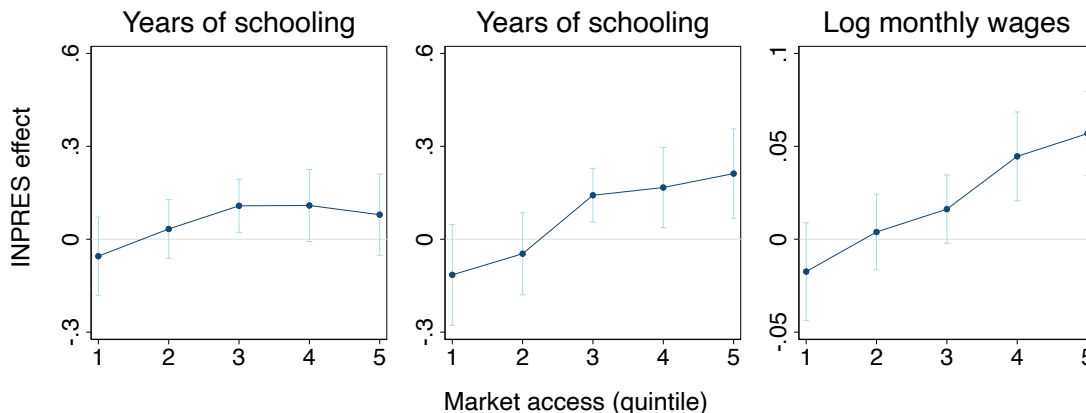
Each figure is one regression. Data come from SUSENAS 2011, 2012, 2013, and 2014 and focus on male heads of household. I compare individuals ages 2 to 6 and those ages 12 to 17 in 1974. I report treatment effects by quartile of market access. Market access is an inverse-distance-weighted average of 1971 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.

that predate INPRES school construction and Euclidean distances that sidestep endogenous road networks. Neither quantity directly enters the allocation rule.

At the same time, table 2 shows that migration patterns do not themselves 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 labor 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 3 shows that people benefit more from school construction than places do. The first three columns show baseline estimates, as in table 1, that take birth-district school construction as treatment. They capture effects on individuals, inclusive of those who migrate

Table 2: INPRES effects on migration

Outcomes	Treatment			Placebo		
	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 3: 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 × 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. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

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]

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[4] While the point estimates are consistently smaller and similarly precise, a significant difference

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 an objective function that includes aggregate output Y and distributional concerns (D_1, D_2), given non-negative weights λ, costs C, and budget constraint $\bar{C}$.

$$\max_a \; \lambda_0 Y(a) - \lambda_1 D_1(a) - \lambda_2 D_2(a) \quad \text{s.t.} \quad \lambda_0 + \lambda_1 + \lambda_2 = 1 \,, \quad C(a) \le \bar{C} \quad (3)$$

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

$$Y_\ell = A_\ell H_\ell \,, \quad Y(a) = \left(\sum_\ell \left[Y_\ell(a) \right]^{\frac{\sigma-1}{\sigma}} \right)^{\frac{\sigma}{1-\sigma}} \quad (4)$$

School construction raises aggregate output by increasing human capital. Distributional concerns apply to both places and people. For people, the output gap between individuals of rural and urban origin captures differences in opportunity.

$$D_1(a) = Y^{U1}(a) - Y^{R1}(a) \quad \text{for} \quad Y_\ell^{U1}(a) = \sum_j U_j Y_{j\ell}(a) \,,$$

with U_ℓ representing urban status and $Y_\ell^{R1}(a)$ defined analogously. For places, the rural-urban output gap captures regional disparities net of migration flows.

$$D_2(a) = Y^{U2}(a) - Y^{R2}(a) \quad \text{for} \quad Y_\ell^{U2}(a) = U_\ell Y_\ell(a)$$

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

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from baseline would require even higher migration rates.

Hsieh et al. (2019), individuals consider amenities and consumption, where consumption is net labor income less the total cost of education. Utility is

$$U(e, \epsilon) = \alpha_\ell \varepsilon_{jk\ell}^\alpha [\underbrace{(1 - \tau_{j\ell}^m) w_\ell e^\eta \epsilon}_{\text{net labor income}} - \underbrace{(1 + \tau_{jk}^e) c \varepsilon_{jk\ell}^c e}_{\text{cost of education}}] \tag{5}$$

for a given destination, with amenities α_ℓ and suppressing subscripts $ijk\ell$. Total labor income is the product of base wage w_ℓ and human capital, which in turn combines 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 $\tau_{j\ell}^m$, 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_{jk}^e. I allow for misspecification $(\varepsilon_{jk\ell}^\alpha, \varepsilon_{jk\ell}^c)$ of amenities and the cost of education. Education and migration costs $(\tau_{jk}^e, \tau_{j\ell}^m)$ are the key frictions, and school construction lowers education costs by increasing access to schooling for treated age cohorts.

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

$$e^* = \arg\max_e \{U(e, \epsilon)\} = \left[\frac{(1 - \tau_{j\ell}^m) w_\ell \eta \epsilon}{(1 + \tau_{jk}^e) c \varepsilon_{jk\ell}^c} \right]^{\frac{1}{1-\eta}}. \tag{6}$$

$$U(\epsilon) = U(e^*, \epsilon) = (1 - \eta) \eta^{\frac{\eta}{1-\eta}} \alpha_\ell \varepsilon_{jk\ell}^\alpha \left[\frac{(1 - \tau_{j\ell}^m) w_\ell \epsilon}{[(1 + \tau_{jk}^e) c \varepsilon_{jk\ell}^c]^\eta} \right]^{\frac{1}{1-\eta}}. \tag{7}$$

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

$$\pi_{jk\ell} = \frac{\tilde{w}_{jk\ell}^{\theta}}{\sum_{\hat{\ell}} \tilde{w}_{jk\hat{\ell}}^{\theta}} \quad \text{for} \quad \tilde{w}_{jk\ell} \equiv \alpha_{\ell}^{1-\eta}(1 - \tau_{j\ell}^{m})w_{\ell} \underbrace{\frac{(\varepsilon_{jk\ell}^{\alpha})^{1-\eta}}{(\varepsilon_{jk\ell}^{c})^{\eta}}}_{\tilde{\varepsilon}_{jk\ell}}. \tag{8}$$

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

In the model, individuals realize their Fréchet productivity shocks across destinations, then make education and migration decisions jointly and upfront. Reality is more complex, as individuals invest in education when young in anticipation of future shocks and migration. The baseline formulation offers tractability, with equation 8 in closed form. And equation 8 is itself a probabilistic expression at the aggregate level, akin to the expectational expression for individuals implied by more complex timing assumptions.[5] At the same time, the static model misses some dynamics around sequential migration, through which migrants might update their realized productivity shocks with information gathered post-migration. However, I capture much of these effects because I observe long-run wages. Relatedly, the model also imposes that individuals realize productivity shocks in all locations. Information frictions may apply for faraway destinations, but such frictions are captured by migration costs.

3.3 Education and wages

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

$$\overline{educ}_{jk\ell} \equiv \mathbb{E}\left[e^* \mid \text{individuals choose } \ell\right]$$

$$= \gamma \left(\frac{1}{\alpha_{\ell}}\right) \left[\frac{\eta}{(1 + \tau_{jk}^{e})c}\right]^{\frac{1}{1-\eta}} \left(\sum_{\hat{\ell}} \tilde{w}_{jk\hat{\ell}}^{\theta}\right)^{\frac{1}{\theta(1-\eta)}} \left(\frac{1}{\varepsilon_{jk\ell}^{\alpha}\varepsilon_{jk\ell}^{c}}\right), \tag{9}$$

$$\overline{wage}_{jk\ell} \equiv \mathbb{E}[w_{\ell}e^{\eta}\epsilon \mid \text{individuals choose } \ell, \ e = e^*]$$

$$= \gamma \left(\frac{1}{\alpha_{\ell}}\right)\left(\frac{1}{1 - \tau_{j\ell}^{m}}\right)\left[\frac{\eta}{(1 + \tau_{jk}^{e})c}\right]^{\frac{\eta}{1-\eta}} \left(\sum_{\hat{\ell}} \tilde{w}_{jk\hat{\ell}}^{\theta}\right)^{\frac{1}{\theta(1-\eta)}} \left(\frac{1}{\varepsilon_{jk\ell}^{\alpha}}\right), \tag{10}$$

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[5] Timing concerns are perhaps more relevant for policy targeting migration costs. For example, road construction only affects older cohorts if they made their education decisions in anticipation of future reductions in migration costs. I focus on policy targeting education costs, and school construction is indeed salient to relevant cohorts at the time of their education decisions.

for $\gamma = \Gamma\left(1 - \frac{1}{\theta(1-\eta)}\right)$, noting that $\mathbb{E}[\epsilon^{\frac{1}{1-\eta}} \mid \text{individuals choose } \ell] = \pi_{jk\ell}^{-\frac{1}{\theta(1-\eta)}}\gamma$. Both are increasing in summation term $\sum_{\hat{\ell}} \tilde{w}_{jk\hat{\ell}}^{\theta}$, which captures labor market access. 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 market access. Base wages do not enter directly – only indirectly through market access – 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. Note that base wages are not the wages observed in the data, and instead only a component of these 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.

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

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

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

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.

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

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

$$w_\ell = p_\ell A_\ell \tag{11}$$

Productivity allows for agglomeration κ, and amenities incorporate congestion μ.

$$A_\ell = \bar{A}_\ell H_\ell^\kappa, \quad \alpha_\ell = \bar{\alpha}_\ell \left(\sum_{j,k} N_{jk} \pi_{jk\ell} \right)^{-\mu} \tag{12}$$

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.

$$Y_\ell = \frac{1}{p_\ell} \sum_{j,k} N_{jk} \pi_{jk\ell} \overline{wage}_{jk\ell} \tag{13}$$

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,

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

For Y_ℓ^{supply} given by equation 4, market clearing condition $Y_\ell^{\text{supply}} = Y_\ell^{\text{demand}}$ implies

$$p_\ell = \left(\frac{Y}{Y_\ell} \right)^{\frac{1}{\sigma}}. \tag{14}$$

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

$$Y_\ell(a) = \left[p_\ell(a) \right]^{\frac{\eta}{1-\eta}} \left[A_\ell(a) \right]^{\frac{1}{1-\eta}} \sum_{j,k} \tilde{N}_{jk\ell} \left[\pi_{jk\ell}(a) \right]^{1 - \frac{1}{\theta(1-\eta)}} \left[1 + \tau_{jk}^e(a) \right]^{-\frac{\eta}{1-\eta}}, \tag{15}$$

where I expand equation 13 and define $\tilde{N}_{jk\ell} = \gamma \left(\frac{\eta}{c} \right)^{\frac{\eta}{1-\eta}} N_{jk} \left(\frac{1-\tau_{j\ell}^m}{\varepsilon_{jk\ell}^c} \right)^{\frac{\eta}{1-\eta}}$.

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.

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

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

$$\frac{\partial \overline{educ}^1_{jk}}{\partial \tau^e_{jk}} = \frac{\partial}{\partial \tau^e_{jk}} \left\{ \gamma \left[\frac{\eta}{(1 + \tau^e_{jk})c} \right]^{\frac{1}{1-\eta}} \left(\sum_\ell \frac{\pi_{jk\ell}}{\alpha_\ell \varepsilon^\alpha_{jk\ell} \varepsilon^c_{jk\ell}} \right) MA_{jk} \right\},$$

$$\frac{\partial \overline{wage}^1_{jk}}{\partial \tau^e_{jk}} = \frac{\partial}{\partial \tau^e_{jk}} \left\{ \gamma \left[\frac{\eta}{(1 + \tau^e_{jk})c} \right]^{\frac{\eta}{1-\eta}} \left(\sum_\ell \frac{\pi_{jk\ell}}{\alpha_\ell (1 - \tau^m_{j\ell}) \varepsilon^\alpha_{jk\ell}} \right) MA_{jk} \right\}.$$

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,

$$\frac{\partial \overline{educ}^2_{jk}}{\partial \tau^e_{jk}} = \frac{\partial}{\partial \tau^e_{jk}} \left\{ \gamma \left[\frac{\eta}{(1 + \tau^e_{jk})c} \right]^{\frac{1}{1-\eta}} \left(\frac{\pi_{jkj}}{\alpha_j \varepsilon^\alpha_{jkj} \varepsilon^c_{jkj}} \right) MA_{jk} \right\},$$

$$\frac{\partial \overline{wage}^2_{jk}}{\partial \tau^e_{jk}} = \frac{\partial}{\partial \tau^e_{jk}} \left\{ \gamma \left[\frac{\eta}{(1 + \tau^e_{jk})c} \right]^{\frac{\eta}{1-\eta}} \left(\frac{\pi_{jkj}}{\alpha_j \varepsilon^\alpha_{jkj}} \right) MA_{jk} \right\}.$$

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

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

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

4   Estimation

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

4.1   Human capital function

First, I estimate the elasticity of human capital η with respect to schooling. In the model, wages at the individual level are proportional to human capital e^η given schooling e. From equation 5,

$$wage_i = w_{\ell(i)} educ_i^\eta \epsilon_i$$

for individuals i in destinations ℓ. Observed $wage_i$ is gross labor income (not net of migration costs), and observed $educ_i$ is schooling e. Taking logs,

$$\log wage_i = \log w_\ell + \eta \log educ_i + \log \epsilon_i . \tag{16}$$

The challenge is that education is endogenous. Productivity shocks ϵ_i affect both wages and education, echoing the typical concern over ability as an omitted variable. INPRES school construction offers an instrument for education, isolating the causal effect of schooling on wages as in Duflo (2001).

$$\log \widehat{educ}_i = \delta_j + \delta_k + \delta_\ell + \beta S_j T_k + \boldsymbol{C}_j T_k \boldsymbol{\phi} + \varepsilon_i$$

The interaction of school construction and treatment cohort $(S_j T_k)$ instruments for education, controlling for origin and cohort fixed effects (δ_j, δ_k), controls $\boldsymbol{C}_j$, and destination fixed effects δ_ℓ that capture the unobserved base wages w_ℓ of equation 16.

Note that estimation at this stage takes migration as given. That is, equation 16 quantifies the extent to which – within destinations – higher education translates into higher wages. The next section relaxes this constraint and thus captures total returns to education, inclusive of destination-varying base wages w_ℓ. Despite a common parameter η, the total returns to education are heterogeneous across space because individuals with low labor market access cannot migrate to destinations with high w_ℓ. Their schooling instead meets low w_ℓ, resulting in low total returns.

4.2 Education and migration costs

Second, I estimate the effects of school construction on education costs, and of distance on migration costs. I do so imposing the following additional structure.

$$1 + \tau_{jk}^e = (1 + S_j T_k)^{-\beta} \delta_j \delta_k (1 + \boldsymbol{C}_j T_k)^{\boldsymbol{\phi}}, \tag{17a}$$

$$1 - \tau_{j\ell}^m = (1 + d_{j\ell}^P)^{-\varphi_1}(1 + d_{j\ell}^D)^{-\varphi_2} \tag{17b}$$

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 ($d_{j\ell}^P, d_{j\ell}^D$) 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.[6]

I difference equations 9 and 10, which describe observed education and wages by origin-cohort-destination ($\overline{educ}_{jk\ell}, \overline{wage}_{jk\ell}$), to obtain

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

Substituting expressions 17,

$$\begin{aligned}
\log \overline{educ}_{jk\ell} - \log \overline{wage}_{jk\ell} = {}& \beta \log(1 + S_j T_k) - \log \delta_j - \log \delta_k - \boldsymbol{\phi} \log(1 + \boldsymbol{C}_j T_k) \\
& - \varphi_1 \log(1 + d_{j\ell}^P) - \varphi_2 \log(1 + d_{j\ell}^D) + \log \frac{\eta}{c} - \log \varepsilon_{jk\ell}^c.
\end{aligned} \tag{18}$$

This specification identifies education and migration costs. The challenge is that school construction S_j is endogenous: it explicitly targeted regions with low enrollment, and thus is correlated with error $\varepsilon_{jk\ell}^c$ in the cost of education. INPRES school construction offers difference-in-differences variation to address this endogeneity, as in section 2. Rather than directly comparing districts with more and less school construction, I instead compare how treated and untreated cohorts differ across such districts. Human capital elasticity η is absorbed by the intercept and thus does not affect these estimates.

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

4.3   Other parameters

Third, I estimate amenities, the Fréchet dispersion parameter, and base wages. I do so by combining equation 8, which describes observed migration by origin-cohort-destination ($\pi_{jk\ell}$), with equations 9 and 10. Each contains summation terms $\sum_{\hat{\ell}} \tilde{w}_{jk\hat{\ell}}^{\theta}$ that are mechanically correlated with the errors. I eliminate these terms by differencing $\Delta_\ell \log X_{jk\ell} \equiv \log X_{jk\ell} - \log X_{jk0}$ with respect to a reference destination.

$$\Delta_\ell \log \overline{educ}_{jk\ell} = -\Delta_\ell \log \alpha_\ell - \Delta_\ell \log \varepsilon_{jk\ell}^\alpha \varepsilon_{jk\ell}^c \,, \tag{19a}$$

$$\Delta_\ell \log \overline{wage}_{jk\ell} = -\Delta_\ell \log \alpha_\ell - \Delta_\ell \log(1 - \tau_{j\ell}^m) - \Delta_\ell \log \varepsilon_{jk\ell}^\alpha \,, \tag{19b}$$

$$\Delta_\ell \log \pi_{jk\ell} = \theta \Delta_\ell \log(1 - \tau_{j\ell}^m) + \theta \Delta_\ell \log(\alpha_\ell^{1-\eta} w_\ell) + \theta \Delta_\ell \log \tilde{\varepsilon}_{jk\ell} \tag{19c}$$

Given migration costs, equations 19a and 19b identify relative amenities $\frac{\alpha_\ell}{\alpha_0}$, and equation 19c identifies Fréchet parameter θ. Given relative amenities and the elasticity of human capital, destination ℓ fixed effects in equation 19c further identify relative base wages $\frac{w_\ell}{w_0}$. Estimates in this section thus depend on those above. Base cost c of education does not enter counterfactuals and need not be estimated.

I estimate these parameters 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 equations 19a, 19b, and 19c and apply the generalized method of moments. I further discipline estimation by adding reduced-form moments to match the education and wage effects estimated in section 2. In doing so, I once again leverage INPRES school construction for estimating the parameters of the empirical model.

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 not estimation or identification of other parameters.

4.4 Estimates

For human capital elasticity η, table 4 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 5 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 program's long-run aggregate and distributional effects, highlighting the equity-efficiency trade-off facing the policymaker.

5.1 Solving the model

I compute the aggregate output $Y(a)$ resulting from a given allocation of school construction with equations 4 and 15. The problem simplifies under zero agglomeration ($\kappa = 0$) and perfect substitution ($\sigma \to \infty$), in which case the only effect of

Table 4: 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 5: 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 17a and 17b. 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: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

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.

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

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 13 and 14 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 14 and

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

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 14 and

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

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 6 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 low-enrollment regions, and both equity and political economy considerations can rationalize such a policy goal.[7] 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 distributional effects. Shutting down mobility entirely by setting migration costs to one, I find that the direct effect of school

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

Table 6: 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

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.

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 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.

5.3 Redesigning the program

I study the design of the program by considering alternative allocations of school construction, subject to a budget constraint. I do so by maximizing the set of objective functions described in equation 3. In particular, I search over allocations to maximize aggregate output ($\lambda_0 = 1$), person-based inequality ($\lambda_1 = 1$), place-based inequality ($\lambda_2 = 1$), and combinations of the three (for $\lambda_0 + \lambda_1 + \lambda_2 = 1$). I then compute each allocation's effect on output and inequality, thereby characterizing the policymaker's possibilities frontier and quantifying the implied equity-efficiency trade-off.

The challenge is that, for each objective function, the optimization problem is difficult to solve. Computing each optimal allocation requires solving a combinatorial problem, as spatial interdependence demands considering locations jointly. Indeed, school construction in one district depends on and affects labor markets in all other districts. The result is a severe curse of dimensionality. I therefore simplify the problem by focusing on allocation rules similar to the one used in reality. The actual rule allocated schools in proportion to 1971 child unenrollment in excess of a cutoff level. This cutoff was 0% for 1973-1974 construction and 15% for 1975-1978.

The analysis proceeds as follows. First, I vary the allocation rule cutoff over a grid of unenrollment cutoffs, and I obtain the resulting allocations. High cutoffs concentrate school construction in low-enrollment regions, which by appendix table A3 tend to be rural and isolated. Second, for each allocation I used the estimated model to compute effects on aggregate output, person-based inequality, and place-based inequality. Third, I consider an objective function focused on aggregate output ($\lambda_0 = 1$), and I select the allocation that maximizes this objective. This step amounts to maximization by grid search over the one-dimensional cutoff grid, greatly simplifying optimization because I search only over cutoffs and not over the full set of possible allocations. Fourth, I repeat the previous step for alternative objective functions, which I obtain by varying the λ weights. The model thus captures the possibilities frontier facing the policymaker. It also generates policy prescriptions: for any given objective function, the model delivers the optimal cutoff rule and the resulting aggregate and distributional effects.

In the first step, note that each allocation rule is subject to the observed budget constraint. I use total expenditures to define the budget, 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. Given a cutoff level, for each district I compute 1971 child unen-rollment in excess of this level. I then distribute the total expenditures budget across districts in proportion to excess unenrollment. For example, if excess unenrollment is 10% in district one and 20% in district two, then district two receives twice as many new schools as district one does. Future work can consider how aggregate and distributional effects vary with the budget, which I currently treat as fixed.

Figure 5 illustrates the results, plotting policymaker preferences alongside the resulting impacts on aggregate output and place-based inequality. For policymaker preferences, I increase weight λ_0 on aggregate output at the expense of weight $\lambda_2 = 1 - \lambda_0$ on place-based inequality, holding fixed weight $\lambda_1 = 0$ on person-based inequality. I focus on aggregate output and place-based inequality to capture the equity-efficiency trade-off, as raising aggregate output comes at the cost of elevated place-based inequality. Person-based inequality is positively correlated with aggregate output and thus avoids a trade-off. I also consider a policymaker balancing person- and place-based inequality, which are negatively correlated, in appendix figure B3.

In figure 5a, more weight on aggregate output implies a higher cutoff, which increases construction in low-enrollment regions and thus both aggregate output and place-based inequality. I highlight the trade-off by reversing the axis for place-based inequality, which enters the objective function negatively. Conversely, more weight on place-based inequality implies a lower cutoff, which generates the opposite effects. Targeting low-enrollment regions thus raises aggregate output gains by enhancing opportunities for underserved students, but at the cost of increased regional disparities. Furthermore, figure 5a suggests a government objective function with approximately equal weights on aggregate output and place-based inequality, as $\lambda_0 = 0.5$ roughly corresponds to the 8% and 12% effects produced by the actual allocation.[8]

In figure 5b, I repeat the analysis under reduced migration costs, which increase mobility and magnify 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. At the same time, place-based inequality also rises. But unlike the baseline scenario, in which rural

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[8] The actual allocation falls below each curve because it departs slightly from the cutoff rule.

Figure 5: Effects on aggregate output vs. place-based inequality

(a) Baseline

(b) Halved migration costs
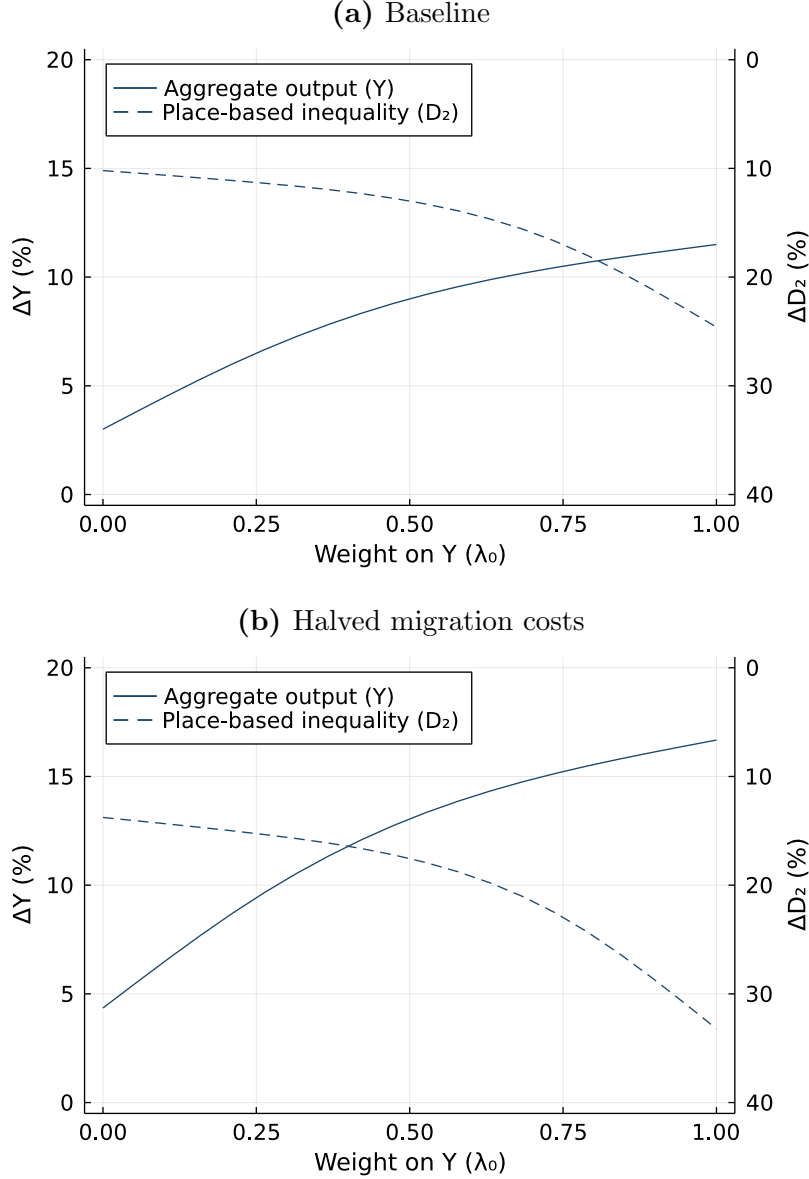
I vary the objective function holding fixed weight $\lambda_1 = 0$ on person-based inequality D_1. I thus vary weight $\lambda_0 \in [0, 1]$ on aggregate output Y, which in turn affects weight $\lambda_2 = 1 - \lambda_0$ on place-based inequality D_2. For each y-axis, higher is better. The left axes are percentage increases in Y relative to zero construction, with Y entering the objective function positively. The right axes are percentage increases in D_2 relative to zero construction, with D_2 entering the objective function negatively and thus flipped axes in the figures. The bottom figure repeats the exercise of the top figure under 50% lower migration costs.

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.

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APPENDIX

A   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)

Outcomes	SUPAS 1976			SUPAS 1995		
	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)

| Outcomes	SUPAS 1976			SUPAS 1995		
	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: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A3: INPRES school construction vs. ruralness and isolation

|	School construction	School construction	School construction
Population density (ruralness)	-0.0748***		-0.0309***
	(0.0155)		(0.0118)
Labor market access (isolation)		-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. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Figure A1: Migration and age cohort
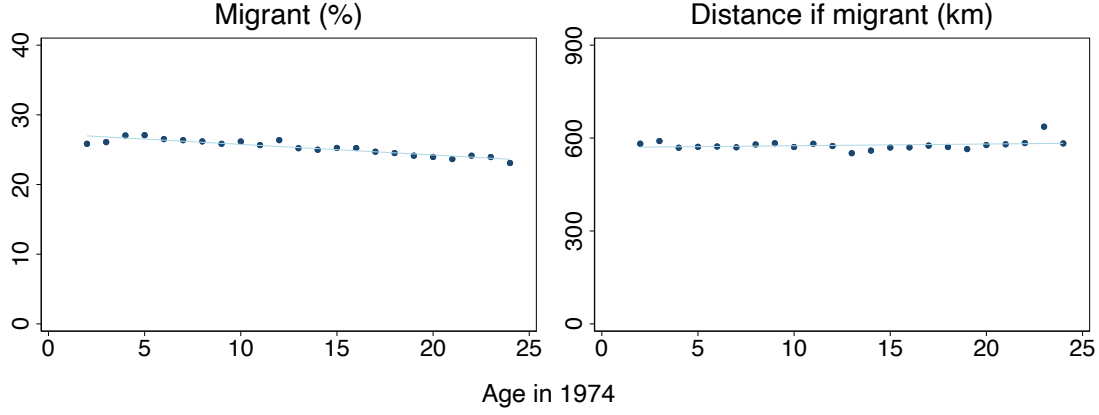
Data come from SUSENAS 2011, 2012, 2013, and 2014 and focus on male heads of household ages 2 to 24 in 1974. Migrants reside outside of their birth districts, and migration distances are Euclidean and between district centroids.

Figure A2: Placebo INPRES effects by market access
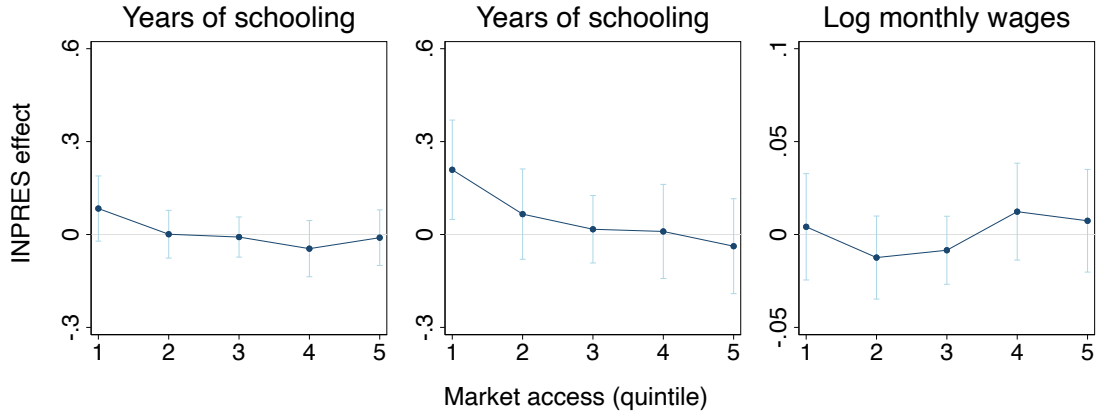
Each figure is one regression. Data come from SUSENAS 2011, 2012, 2013, and 2014 and focus on male heads of household. I compare individuals ages 12 to 17 and those ages 18 to 24 in 1974. I report treatment effects by quartile of market access. Market access is an inverse-distance-weighted average of 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 A4: 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: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

B Counterfactuals

I consider three extensions. First, spatial spillovers imply that investment should be centralized. Table B1 illustrates that construction is greatly reduced if districts must fund school construction themselves. In this case, 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 increases investment by internalizing cross-district spillover effects, raising aggregate output by another three percentage points. It also 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.

Second, more sophisticated allocation rules lead to larger aggregate output effects. Allocation rules can vary in the weights and observables considered. For a

Table B1: 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.

given weighting scheme $\mathcal{P}$, set of observables X, and budget A, I choose weights

$$\rho^* = \arg\max_{\rho \in \mathcal{P}} \left\{ W\big(a(\rho; X, A)\big) \right\}$$

in order to maximize a given objective function $W(a)$. That is, optimization is over weights ρ that correspond to allocations $a(\rho)$ as follows.

$$a_\ell(\rho; X, A) = A\left(\frac{X_\ell \rho}{\sum_\ell X_\ell \rho} \right)$$

Proportional weighting schemes use uniform weights, avoiding optimization altogether ($\mathcal{P}_{\text{prop}} = \{\rho \mid \rho = \mathbf{1}\}$). The actual allocation rule was proportional to unenrollment and thus falls within this set. Linear and quadratic schemes offer more flexibility by admitting weights parameter to optimize over.

Table B2 presents allocation rules of varying complexity. 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 B1). 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 – even a simple proportional one – substantially outperform those that con-

sider unenrollment alone. Unenrollment alone is insufficient because it is uncorrelated with market access (figure B1), and thus misses an important force in the full model. Table B3 shows that agglomeration κ and substitutability σ both magnify differences across rules by increasing the gains from targeting high-value districts.

Third, uncertainty can rationalize the use of simple allocation rules. 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

$$\mathbb{E}_v[Y(a;v)] = \int Y(a;v)f(v)dv\,,$$

subject to multiplicative distortions v to migration costs. Figure B2 shows that a redesigned rule – one proportional to unenrollment and urban distance – 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 – one proportional to unenrollment alone – 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 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).

Table B2: Effects of sophisticated 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 B1:** Pre-INPRES unenrollment vs. market access
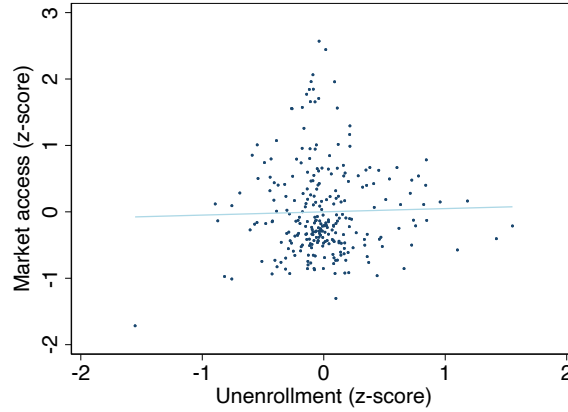
Each observation is one district. Market access is an inverse-distance-weighted average of 1971 population densities across districts. Unenrollment is total unenrolled school-age child population. The figure controls for 1971 population.

**Table B3:** Aggregate output by allocation, agglomeration, and substitutability

Allocation	Agglomeration (κ)				Substitutability (σ)			
	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 B2: Aggregate output by allocation rule under uncertainty
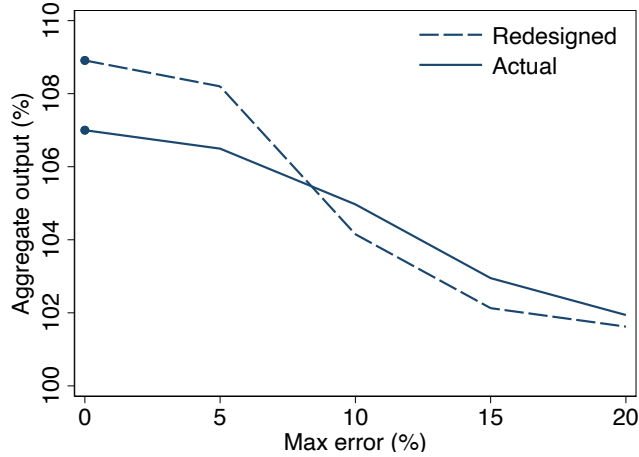
The redesigned rule is proportional to unenrollment and urban distance, and the actual rule is proportional to unenrollment. Aggregate output values are relative to zero construction. Max error $\bar{v}$ implies multiplicative distortions $v \sim U[1 - \bar{v}, 1 + \bar{v}]$ to migration costs.

Figure B3: Effects on person- vs. place-based inequality

(a) Baseline

(b) Halved migration costs
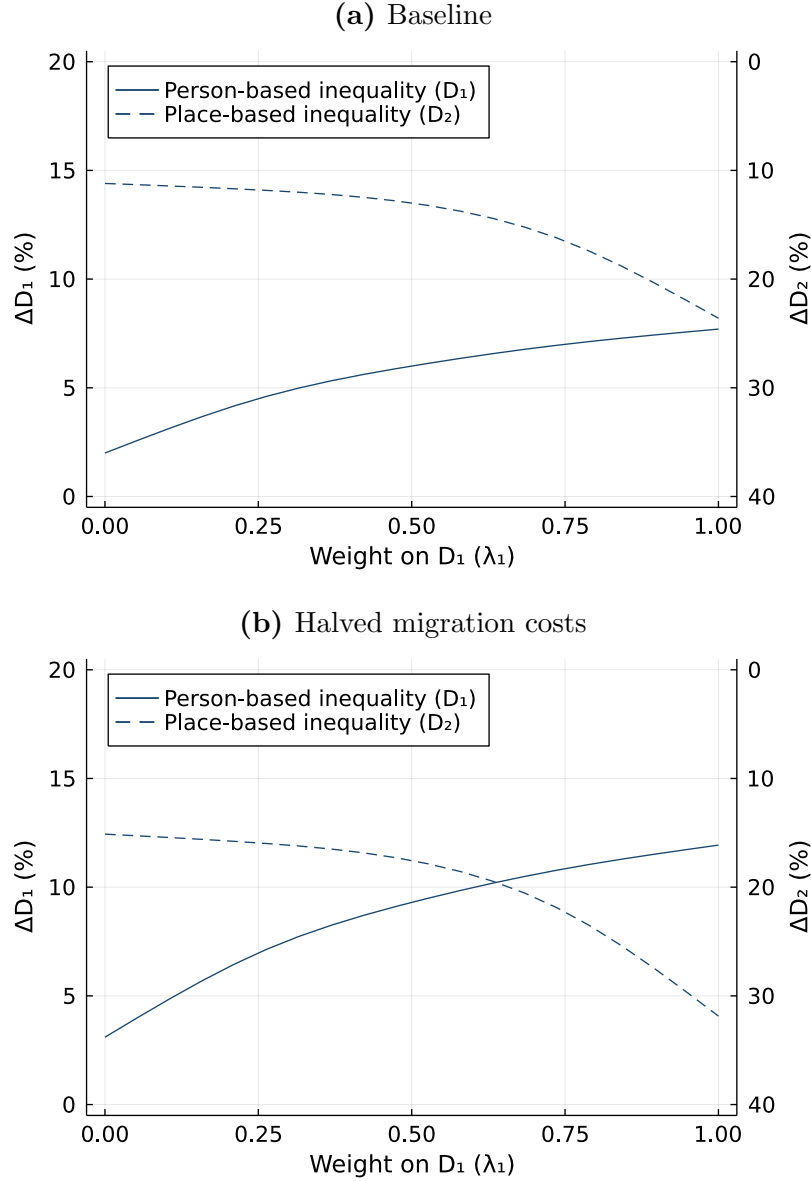
I vary the objective function holding fixed weight $\lambda_0 = 0$ on aggregate output Y. I thus vary weight $\lambda_1 \in [0,1]$ on person-based inequality D_1, which in turn affects weight $\lambda_2 = 1 - \lambda_1$ on place-based inequality D_2. For each y-axis, higher is better. The left axes are percentage decreases in D_1 relative to zero construction, with D_1 entering the objective function negatively. The right axes are percentage increases in D_2 relative to zero construction, with D_2 entering the objective function negatively and thus flipped axes in the figures. The bottom figure repeats the exercise of the top figure under 50% lower migration costs.

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