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School Construction and Intergenerational Mobility in Indonesia

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1) Introduction, Summary, and Review of Related Literature

The past fifteen years have witnessed a surge of interest in the phenomenon of the intergenerational persistence of economic status, or the high degree of economic similarity between parents and children, which many see as a measure of a society's failure to provide equality of opportunity to all.[1] Most empirical papers on this topic have used North American or Northern European data, but interest in this issue has increased lately among development economists as well (cf. World Bank 2005), and estimates of generational income persistence have now begun to emerge for developing and transition economies (Lillard and Kilburn 1995; Grawe 2001; Hertz 2001). In virtually all such countries, however, the lack of long-running longitudinal surveys makes it difficult to estimate the intergenerational persistence of long-run average, or permanent, income, and this problem becomes particularly acute when attempting to estimate time-trends in this descriptive statistic.[2] Indeed, Hertz *et al.* (2007) argue that it is basically impossible to estimate trends in the intergenerational persistence of permanent income using existing data from the developing and transition economies, and they recommend using education (which is a more or less permanent characteristic in adulthood, and which follows an identifiable trajectory in childhood) as an observable, but coarse, proxy for long-run socioeconomic status, a strategy we adopt here.

While trends and cross-country differences in the intergenerational persistence of educational attainment are relatively easy to calculate, it remains a challenge to explain their

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[1] Others argue that intergenerational persistence is at best a weak indicator of unfairness in the distribution of economic opportunity (Jencks and Tach 2005).

[2] Estimates of this trend will be biased if more recent birth cohorts are observed at younger average ages (Haider and Solon 2006; Hertz 2007), which will be the case in even a long-running panel, if all available data are utilized.

origins in a rigorous way. In this paper, we document a long-run decrease in intergenerational educational persistence in Indonesia, or an increase in educational mobility[3], which we argue can be explained in part by the increasing availability of primary schools. We follow Esther Duflo (2001), who notes that the Indonesian government's construction of nearly 64,000 primary schools between 1973 and 1978, as part of the INPRES program, serves as a unique natural experiment, from which the effects of changes in the availability of public education can be estimated. Duflo shows, among other things, that exposure to the program raised educational attainment for Indonesian men. Using similar methods, in a different dataset, we demonstrate that school construction also raised intergenerational mobility, by increasing educational attainment the most for men whose parents had little or no education. Interestingly, although women's schooling increased more rapidly than did men's, we find no solid evidence that these gains were due to school construction, or that school construction weakened the effect of parental education. We thus conclude that increases in women's schooling, and in their educational mobility, were primarily the result of other forces.

It is important to be clear about which effects we hold to be causal, and which we view as statistical associations of interest, but not estimates of a structural parameter. We argue, like Duflo, that our strategy produces an estimate of the causal impact of school construction on educational attainment. We further argue that school construction caused a change in the estimated slope of the linear relation between parents' and children's schooling. That slope,

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[3] For the purposes of this discussion, the regression coefficient associated with the parental attribute, in a regression predicting their children's outcomes, will be treated as the measure of intergenerational persistence, or what Fields (2000) terms "time dependence," and decreases in persistence will be identified with increases in mobility. In the final section of this paper we consider other definitions of persistence and mobility.

however, is not a structural parameter, but rather a descriptive statistic, one which conflates the effects of parental education with those of parental income, genetics, and other factors that are omitted from the equation. This formulation is a hallmark, not a weakness, of the descriptive literature on intergenerational mobility, whose first goal is just to characterize the statistical association between parents' and children's outcomes. Yet one may still seek to identify the forces and government policies that cause this statistic to vary over time and place, and this is our aim here.[4]

Authors whose work is closely related to ours include Hannum (1999), who asks whether changes in school resource allocations in China altered the urban/rural attainment gap, which is one source of the intergenerational association of parents' and children's education, since urban parents generally have urban children, and likewise for the rural. She finds that the evolution of this gap mirrors the trend in the number of teachers assigned to work in urban versus rural areas. While she notes that this correlation is not conclusive, she also points out that China explicitly prioritized rural education during the Cultural Revolution, and it was at this time that the urban/rural schooling gap narrowed. Later, in the 1980s, China adopted a more liberal approach, which had the effect of favoring urban areas, whereupon the urban/rural gap re-emerged. That these stated policy goals should have had their intended effect should not be surprising, and the result should have been a rise and then fall in overall educational mobility.[5]

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[4] For ease of expression, we still use "parental education effect" as a synonym for "parental education parameter."

[5] We do not know of a nationally representative survey for China that would allow one to test this prediction. Hertz *et al.* (2007) report results for a portion of rural China only, which show a steady increase in educational mobility through about the 1968 birth cohort.

Similarly, Hertz, Meurs and Selcuk (2007) argue that a steep decline in educational mobility in Bulgaria can be explained by changes in economic conditions and educational access following the transition from socialism. Their argument is based on differences across two household surveys, taken six years apart, in children's' exposure to a period of economic and fiscal crisis, which led to school closures, increased out-of-pocket-costs, and reductions in the quality of instruction. This coincided with an absolute deterioration in educational outcomes for the children of the least-well educated parents, which is to say, a decline in educational mobility. As with Hannum, these conclusions are based on plausible, but circumstantial, as opposed to an econometric, evidence.

Behrman, Birdsall, and Székely (2000) take a cross-country panel-data approach, using twenty years' worth of household surveys from 16 Latin American nations. They find that government expenditures on primary schooling (per student of primary age), as well as the average level of education of their teachers, are positively associated with an educational mobility index of their own design, as is financial depth (M2 over GDP), which they interpret as an indication of the degree to which the economy is "marketized."

Our contribution is to bring micro-econometric evidence to bear on the question of the relation between educational policy and educational mobility, based on the natural experiment represented by Indonesia's school-construction program. The data we use are described in the next section; following that we present our econometric specification, and its results. A final section looks at alternative explanations for the convergence of male and female education levels, which we argue was not a clear by-product of the INPRES school construction program. We also discuss the divergent trends followed by two different measures of educational persistence.

2) Data and Program Overview

The key covariate for our analysis, parental education, is not available in the dataset used by Duflo, the 1995 intercensal survey of Indonesia. Instead, we turn to a smaller but more detailed source, the RAND Indonesia Family Life Survey (IFLS). Our sample includes those who were between the ages of 24 and 50 at the time of the third wave of the IFLS; this was conducted in 2000, so our sample was born between 1950 and 1976.[6] This survey covered only 13 of Indonesia's 27 provinces, and so is not nationally representative, but these provinces do contain 83 percent of the population. Exposure to new schools depended on district and year of birth, both of which were collected in the survey. We dropped those who left their birthplace prior to age 12, so as not to miss-measure their exposure to new schools at the time they were of primary-school-going age. Parents' education was calculated as the average of the reported number of years of schooling of the mother and father, if both were available. After dropping a small number of cases with missing education data, our sample size was 10,884, of which 5,742 (53 percent) were women, and 5,142 were men. This compares to Duflo's sample of 150,000 men, with obvious implications for the relative statistical power of the two studies.

Administrative data on the number of INPRES schools built, by district and year, were generously supplied by Esther Duflo herself. These were then merged with the IFLS data, a process that was greatly complicated by the changes in district names and coding schemes, and the sub-division of districts, that have occurred between the 1970s and the present. Details of the INPRES program's evolution may be found in Dulfo's 2001 paper, as well as in an earlier version (Duflo 2000). In broad outline, INPRES involved the construction of roughly 64,000 schools between 1973 and 1978, with the first schools coming on line in 1974. (Hence the first

[6] The lower age limit is high enough to reduce the number of children who are still enrolled to a negligible fraction.

cohort with positive potential exposure are those who were in their final year of primary school, presumed to take place at age 12, in 1974, i.e. those born in 1962.) The intention was to build schools in proportion to the number of unenrolled school-aged children in a given district, and Duflo demonstrates that the program's implementation did not deviate greatly from this plan. It is also important to note that primary school fees were abolished in 1978 (World Bank 1990), and that the INPRES program delivered improved water and sanitation services to many areas as well as new schools, factors that need to be accounted for in the econometrics that follow.

3) Methods

Like Duflo, we exploit that fact that there are significant differences in program intensity across geographic districts, and that these have different effects on different birth cohorts. Duflo identifies this effect by interacting program intensity (total number of schools built in the district of birth per 1000 children between the ages of 5 and 14 in 1971) with year of birth, while controlling for both district and cohort fixed effects, and a number of related interaction terms, detailed below. We depart slightly from this approach in that we construct an explicit measure of exposure for each person, which is simply the average number of new schools that had been built prior to each of their six years of primary schooling, which we assume took place at ages seven through twelve. We thus take advantage of the fact that districts built schools at different times in the INPRES period, generating additional variation in the level of exposure that is not captured by Duflo's equation.[7]

The identifying assumption is that district-year specific variations in exposure are conditionally exogenous, an assumption that may be made more plausible by controlling for several other variables that are likely to be correlated with both educational outcomes and exposure. In particular, Duflo notes that the fact that schools were built in proportion to the number of children who were not enrolled in school in a given district creates a potentially problematic correlation between exposure and prior enrollment rates. She also notes that the number of schools built per child was negatively correlated with the number of children in the district in 1971. To prevent these correlations from biasing the results, Duflo introduces

[7] In Duflo's specification, a district that built all of its new schools in 1974 is assigned the same program intensity as one of comparable size that built the same number of new schools, but in 1978. Our formulation assigns a lower level of exposure for the latter district, for all those who attended primary school in any year before 1978.

interactions between year of birth and both the district's population of school-aged children in 1971 and their enrollment rate, and we follow suit. We also follow her advice to include interactions between year of birth and the district's allocation of water and sanitation programs, which were the second-largest component of INPRES. The argument here is that the exposure effect would be biased if these other programs improved educational outcomes, and if their allocation over time and space were correlated with that of the new schools.

Our primary interest, however, is in determining whether program exposure weakened the observed relation between parents' and children's education. To test this hypothesis, we interact our exposure variable with a quadratic in parental education. As will be seen below, this yields strong and significant estimates of the exposure effect for that third of the male sample whose parents have zero schooling (for whom the interaction variable is zero). It also yields a significant negative interaction, meaning that the effects of exposure are lower for the sons of better-educated parents. These estimates, however, would be biased if the effect of parents' education were changing over time, or differed by district, for reasons unrelated to differences in exposure, but in ways that were correlated with variations in exposure. To address this potential problem, we also include interactions between parental education and both the district dummies and the year of birth dummies, yielding the following specification:

[1] $$y_i = \alpha + \beta(Pared_i) + \gamma(Expo_{dc}) + \delta_1(Pared_i * Expo_{dc}) + \delta_2(Pared_i^2 * Expo_{dc}) + \sum_d D_d \eta_d + \sum_c D_c \theta_c + \sum_d D_d Pared_i \pi_d + \sum_c D_c X_i \Psi_c + u_i$$

The notation is as follows: education for the i^{th} son or daughter (y_i) is estimated as a function of an overall intercept (α), followed by a linear term in parents' education ($Pared_i$)

whose coefficient is β; then a linear term in exposure, which varies by district and cohort ($Expo_{dc}$) and whose coefficient is γ. Then come interactions of parental education and its square with program exposure, which have coefficients δ_1 and δ_2. Next are the district fixed effects (η_d), which are implemented by including more than 200 district dummies (D_d); and cohort fixed effects (θ_c), which attach to the 25 cohort dummies (D_c). Note that one year of birth (1950) and one large representative district are omitted from these vectors of dummies to accommodate the overall intercept. The next summation term is the interaction of the district indicators with parental education, yielding the parameters π_d. Finally, year of birth is interacted with a matrix of individual and district-level characteristics (X_i) that will include some or all of the following: parental education, the 1971 enrollment rate, the 1971 school-aged population, and the allocation of water and sewer programs; these yield the parameter matrices Ψ_c. Finally, there is the regression error term, u_i, which is assumed to have zero conditional mean, but is permitted to be heteroskedastic, and to exhibit an intracluster correlation at the level of the enumerator area, of which there are 321.

4) Results

Table 1 describes the dataset and illustrates the decline over time in the intergenerational persistence of education. The columns divide the sample into those who were born too soon to benefit from the new primary schools ("No exposure," years of birth 1950 to 1961), those who would have seen some new schools constructed during their primary school years ("Partial exposure," born 1962-71), and those who entered school in 1979 or later, after the vast majority of the INPRES schools were on line ("Full exposure," born 1972-76). In the first row, for both men (upper panel) and women (lower), we see that average exposure rises from zero, to roughly one new school per 1000 students, to roughly two new schools, confirming that exposure for this subset of the Indonesian population was similar to that of the nation as a whole (Duflo 2001, page 795.) The next three rows of each panel document the steady increase in educational attainment both for the parents and their adult offspring over this period.

Two important trends are evident in these data, namely, that schooling rose most rapidly for those whose parents had no education, and for women. Men whose parents had no education gained 1.7 years from the first column to the third, and women gained 2.3 years, while for the sons and daughters of better educated parents (those with more than a primary education), the gains were 0.4 and 0.9 years. Note that roughly a third of the children in the sample were born to parents with no formal schooling (across all years of birth) while about 15 percent had parents with more than six years of schooling.

That schooling increased more rapidly for the sons and daughters of the least-well-educated parents implies that the intergenerational regression coefficient fell, and thus that intergenerational mobility rose, if this is measured by one minus the regression coefficient.[8]

[8] One minus the regression coefficient is the usual measure of the rate of regression to the mean.

These coefficients appear in the next-to-last row of each panel. For men, they fall from 0.72 to 0.67 to 0.57; the t-tests on the right indicate that the initial decline (of 0.05) is not statistically significant, but that the subsequent change (of 0.10) is. For women, the coefficient at first rises from 0.76 to 0.79 (but this rise is not significant) and then falls by 0.18 (which decline is significant at the one percent level).

Table 2 presents the results of the regression equations described above, for men. In the first column, the covariates include only our exposure measure, parental education, the birth-district and year-of-birth fixed effects, and the interaction between year of birth and the number of children in the district. The coefficient for parental education is significant, and in line with estimates reported by Hertz *et al.* (2007), and there also appears to be a significant effect of exposure. This effect roughly doubles after interacting exposure with a quadratic in parents' schooling (column [2]). The interpretation is now that for men whose parents had no schooling, an additional unit of exposure (an additional school per 1000 children) raised educational attainment by more than one-half (0.571) of one year. The negative coefficients on the interaction terms imply that the effect of exposure falls with rising parental education.

Columns [3] and [4] introduce controls for the district's prior enrollment rate, and the density of water and sanitation programs, as interactions with year of birth. The table reports the p-values from tests of the joint statistical significance of these interaction terms, which reveal that the interactions with the enrollment rate are marginally significant, but the other interaction terms are not. Their introduction reduces the estimated main effect of exposure by about 20 per cent, to 0.458, and increases its standard error by about one-third. Note that the data on enrollment and, especially, on the water and sanitation programs, are incomplete, forcing a loss of about seven per cent of the sample. But the change in the exposure coefficient, and its loss of precision, are due to the change in specification, not the reduction in the sample, as may be ascertained by re-running the equation of column [2] in the sample used for column [4] (results not reported in table.)

In columns [5] and [6], we allow for the possibility that the effect of parents' education could be changing over time, or could differ by district, for reasons unrelated to differences in exposure, by introducing interactions of parental education with the district dummies, in column [5], and with the year-of-birth dummies, in column [6]. Joint tests of the district/parental education interaction terms produced enormous F-statistics, making a strong case for their inclusion in all equations. (Note that a large district, with a representative value for the parental education effect, was chosen as the omitted category; this choice determines the reported coefficient for parental education, which becomes district-specific, but has no impact on the exposure coefficient or the interaction terms between exposure and parents' schooling.)

In column [5], the estimated effect of exposure for the sons of uneducated parents rebounds somewhat (to 0.529), and the interaction terms now indicate that this effect falls off at a declining rate as parental education increases. In column [6], the inclusion of the year-of-birth/parental education dummies reduces the main effect of exposure to 0.420, and further increases its standard error, pushing it just below the 10 per cent threshold of statistical significance (p= 0.113). Yet the F-tests reveal that these newly added terms are not remotely significant (are jointly indistinguishable from zero).

The inclusion of the additional controls, which we hope will reduce bias in the estimated effect of exposure, thus comes at a non-trivial cost in terms of efficiency, and with very little gain in overall explanatory power – note that the adjusted R^2 in column [6] is no higher than that of column [1]. The results in column [6] should, in principle, represent the low-bias end of a bias/efficiency trade-off. At the other, more precise, end of the spectrum are the results in column [7], which are obtained by including only those additional interaction terms that are statistically significant at the ten percent level or better, namely, the interactions between place of birth and parents' education. Taken together, the results suggest that exposure to an additional new school per 1000 children increased attainment for men by 0.42 to 0.56 years, and that this effect fell off rapidly for the sons of better-educated parents. Using the results from column [7], the exposure effect remains positive and statistically distinguishable from zero (at the ten percent level) up to 2.5 years of parental education, which is to say, for 45 percent of the sons in the sample. Between 3 and 5 years of parental education the estimated effect of exposure is positive but insignificant; thereafter the estimated effect is negative, although never statistically significant at the ten percent level.[9]

For women, in Table 3, none of the specifications yields a large and/or statistically significant estimate for the exposure effect. As a result, we cannot be confident that exposure to school construction had an effect on women's educational attainment, even for the daughters of uneducated parents. In only one of the specifications, that of column [5], do we find evidence that exposure weakened the effect of parents' schooling. Our preferred specification, in column [7], is again the one the retains only those control variable sets that are jointly significant. Here there is no evidence that exposure to new schools had any effect, even for the poorly educated, and no evidence that it reduce the observed association between parental and child schooling, or raised mobility. Instead, the reduction in the effect of parents' education, documented in Table 1, appears to have been captured by the year of birth / parental schooling interaction terms, whose coefficients fall sharply for those born in 1972 and later, as plotted in Figure 1. The

[9] Similar results were had when a quartic in parental education was interacted with exposure.

average effect of an extra year of parental education was 0.696 for the period 1950 to 1971, with the annual estimates displaying wide fluctuations, but no trend. For 1972 to 1976 the average estimated effect was 0.511, a reduction of -0.185, which was statistically significant at the one percent level.

Note that all of the women's equations have higher adjusted R-squareds than their male counterparts, reflecting a greater share of variance explained by the control variables, in particular, those relating to year of birth. These measure educational time-trends that are not attributable to exposure to school construction. In the next section we discuss other possible explanations for the secular reduction in the association between parental and child schooling for daughters.

5) Conclusions

Our results imply that the national average increase in the number of schools, of two per 1000 children, should have raised mean educational attainment for men born to uneducated parents by roughly a year, a figure which is, plausibly, about three times as large as Duflo's result for men in general. For women, we find no effect, which raises the question of why their educational attainment increased more rapidly than men's. One possibility is that this was a response to the elimination of primary school tuition, in 1978. This roughly coincides with the sharp drop in the effect of parental education seen in Figure 1, which began with the 1972 cohort, who should have entered school in 1979. Note, however, that public education is still not free: tuition fees, although outlawed, are still collected by some schools (Human Rights Documentation Centre 2003), along with other miscellaneous fees, and the costs of textbooks and workbooks are also non-trivial. Affordability is still cited as a major barrier to schooling, particularly for girls (Human Rights Watch 2005). The fact that fees have a gender-biased impact supports that claim that reductions in fees since 1978 have narrowed the gender gap.[10]

A second explanation comes from the work of Angeles, Guilkey, and Mroz (2005), who use data from the 1993 wave of the IFLS to estimate a pair of massive structural models of women's education, knowledge of family planning practices, marriage, husband's education, and fertility as simultaneously determined endogenous variables, which are shaped by exogenous changes in state policies governing secondary school student-teacher ratios, health expenditures, and family planning program availability. They then simulate the difference in outcomes that

[10] By contrast, religious factors, *per se*, are not widely believed to stand in the way of girl's education. According to a report to the United Nation's Commission Human Rights, "The prevalent interpretation of Islam in Indonesia is apparently egalitarian regarding education of girls and boys..." (Tomaševski 2002).

would have been generated by the changes in policies that occurred between 1970 and 1993, concluding that "the effect of long-term family planning programs on education levels is three times larger than the impact of the improved schools (p. 197)." While the improvements in question relate to the quality of secondary, as opposed to the quantity of primary, schooling, their results suggest that family planning has had an important effect on women's education.[11]

Last, we note an interesting disparity between trends in two different measures of the intergenerational persistence of educational attainment for Indonesia, reported by Hertz *et al.* (2007). This is the fact that while the intergenerational regression coefficient has declined over time, the equivalent correlation coefficient has, in fact, risen, as illustrated in Figure 2, below. These two linear time trends, of opposite sign, are both statistically significant at the ten percent level. This serves to remind us that both persistence and mobility may be measured in different ways, and these need not be mirror images of each other, forcing us to choose our words carefully. In particular, most would likely agree that if school construction tended disproportionately to raise the absolute educational attainment of the sons of poorly educated parents, this represents an absolute increase in "upward mobility" for those families. The correlation coefficient, by contrast, measures the similarity in rank, or relative position, of parents and children.[12] The fact that it has risen in Indonesia over this same period means that

[11] Strangely, the effect of family planning on the education of the husband in their simulation is actually larger than on that of the wife, but this they attribute to a change in marriage patterns – "women marrying men with higher education levels (p. 197)" – rather than (in addition to?) an increase in male educational attainment.

[12] Although not equivalent, simple correlation coefficients are generally close to Spearman's rank-order correlations (the inverse of which sociologists often term a measure of "exchange mobility") in part because education data are not plagued by large outliers.

even though the prospects of the children of poorly educated parents have improved in absolute terms, parental education is now a better predictor of one's relative position in the educational distribution than it used to be.

The first of these changes is likely in part the result of school construction, at least for sons. The second remains unexplained: it is not an inevitable consequence of increasing average attainment, but it does seem to be a typical pattern for Asian economies. Of the ten Asian nations studied by Hertz *et al.*, seven saw declines in their regression coefficients, but only two saw the correlation coefficient fall, while for four countries, including Indonesia, it displayed a significant positive trend over the past 50 years. By contrast, among seven Latin American nations, most of which started at quite high initial levels of intergenerational persistence, five experienced a statistically significant reduction in their intergenerational regression coefficient, a slightly different group of five saw reductions in the parent-child schooling correlation, but in no country did this correlation display a positive trend.

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Table 1
Summary Statistics By Gender and Level of Exposure to School Construction Program

		No Exposure (Born 1950-61)	Partial Exposure (Born 1962-71)	Full Exposure (Born 1972-76)	t-test Partial vs. None	t-test Full vs. Partial	t-test Full vs. None
Men	Average exposure	0.00	0.88	1.94	51.57	34.65	77.07
		(0.00)	(0.78)	(0.86)			
	Parents Years of School	2.8	3.8	4.6	9.14	6.34	14.11
		(3.1)	(3.5)	(3.6)			
	Years of School	6.9	8.2	9.1	9.30	6.11	14.87
		(4.5)	(4.4)	(3.7)			
	Years of School | Parents = 0	4.7	5.3	6.4	4.92	7.99	12.71
		(3.8)	(3.9)	(3.2)			
	Years of School | Parents > 6	11.9	12.4	12.3	4.34	-0.47	3.73
		(3.3)	(3.1)	(2.6)			
	Intergen. Regression Coefficient	0.72	0.67	0.57	-1.02	-2.64	-3.35
	(Robust standard error)	(0.04)	(0.03)	(0.03)			
	Number (Total = 5,142)	1,910	2,057	1,175			
Women	Average exposure	0.00	0.91	1.93	53.4	35.8	84.9
		(0.00)	(0.83)	(0.82)			
	Parents Years of School	2.6	3.8	4.4	11.2	5.2	14.6
		(3.2)	(3.5)	(3.5)			
	Years of School	5.26	7.01	8.32	13.5	9.6	22.4
		(4.18)	(4.45)	(3.64)			
	Years of School | Parents = 0	3.2	3.7	5.5	5.6	16.1	21.0
		(3.09)	(3.32)	(3.23)			
	Years of School | Parents > 6	10.8	11.9	11.7	10.9	-1.5	8.7
		(3.61)	(3.15)	(2.78)			
	Intergen. Regression Coefficient	0.76	0.79	0.61	0.53	-4.46	-3.60
	(Robust standard error)	(0.03)	(0.03)	(0.03)			
	Number (Total = 5,742)	2,096	2,356	1,290			

Notes: Standard deviations in parentheses. Standard errors for the intergenerational regression coefficients are robust to heteroskedasticity and clustering by enumerator area.

Table 2
Regression Models of the Effect of Exposure to New School Construction, Men
Dependent Variable: Years of Schooling, for 24 to 50-Year-Olds

	[1]	[2]	[3]	[4]	[5]	[6]	[7]
Exposure (New Schools / 1000 Children)	0.300**	0.571***	0.510***	0.458**	0.529**	0.420†	0.560***
	(0.137)	(0.163)	(0.164)	(0.221)	(0.224)	(0.264)	(0.149)
Parents' Education	0.555***	0.619***	0.611***	0.620***	0.509***	0.534***	0.515***
	(0.021)	(0.027)	(0.028)	(0.030)	(0.095)	(0.183)	(0.094)
Exposure * Parents' Education		-0.067*	-0.059	-0.057	-0.128***	-0.102**	-0.139***
		(0.036)	(0.036)	(0.037)	(0.041)	(0.050)*	(0.038)
Exposure * Parents' Education^2		-0.0014	-0.0015	-0.0016	0.0055*	0.0060	0.0056*
		(0.0028)	(0.0028)	(0.0028)	(0.0032)	(0.0034)	(0.0031)
Year-of-birth and District indicators	Yes	Yes	Yes	Yes	Yes	Yes	Yes
F-tests of joint significance of additional control variables (p-values reported)							
Years of birth X Child population in 1971	0.142	0.176	0.539	0.471	0.632	0.645	--
Years of birth X Enrollment in 1971	--	--	0.071	0.110	0.422	0.536	--
Years of birth X Water & sewer programs	--	--	--	0.369	0.414	0.579	--
Districts X Parental education	--	--	--	--	0.000	0.000	0.000
Years of birth X Parental education	--	--	--	--	--	0.862	--
Adjusted R-squared	0.391	0.394	0.394	0.382	0.392	0.390	0.406
Sample Size	5142	5142	5116	4768	4768	4768	5142

Notes: Standard errors (in parentheses) are robust to heteroskedasticity and clustering by enumerator area.

* $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$
† $p=0.113$

Table 3
Regression Models of the Effect of Exposure to New School Construction, Women
Dependent Variable: Years of Schooling, for 24 to 50-Year-Olds

	[1]	[2]	[3]	[4]	[5]	[6]	[7]
Exposure (New Schools / 1000 Children)	0.064	0.228	0.222	0.025	0.161	-0.051	0.110
	(0.127)	(0.144)	(0.156)	(0.183)	(0.186)	(0.230)	(0.187)
Parents' Education	0.580***	0.618***	0.614***	0.626***	0.672***	0.611***	0.665***
	(0.018)	(0.023)	(0.023)	(0.024)	(0.069)	(0.139)	(0.130)
Exposure * Parents' Education		-0.026	-0.020	-0.022	-0.083***	-0.029	-0.042
		(0.028)	(0.029)	(0.029)	(0.031)	(0.043)	(0.040)
Exposure * Parents' Education^2		-0.0019	-0.0021	-0.0018	0.0039*	0.0038	0.0042*
		(0.0021)	(0.0022)	(0.0022)	(0.0023)	(0.0023)	(0.0023)
Year-of-birth and District Indicators	Yes	Yes	Yes	Yes	Yes	Yes	Yes
F-tests of joint significance of additional control variables (p-values reported)							
Years of birth X Child population in 1971	0.008	0.010	0.007	0.015	0.043	0.054	0.015
Years of birth X Enrollment in 1971	--	--	0.001	0.001	0.003	0.125	0.079
Years of birth X Water & sewer programs	--	--	--	0.003	0.208	0.352	--
Districts X Parents’ education	--	--	--	--	0.000	0.000	0.000
Years of birth X Parents’ education	--	--	--	--	--	0.116	0.084
Adjusted R-squared	0.509	0.510	0.511	0.507	0.517	0.518	0.524
Sample Size	5742	5742	5705	5382	5382	5382	5705

Notes: Standard errors (in parentheses) are robust to heteroskedasticity and clustering by enumerator area.
* p< 0.10; ** p< 0.05; *** p < 0.01

Figure 1
Estimated Coefficient for Parental Education By Year of Birth, for Women in Equation [7]

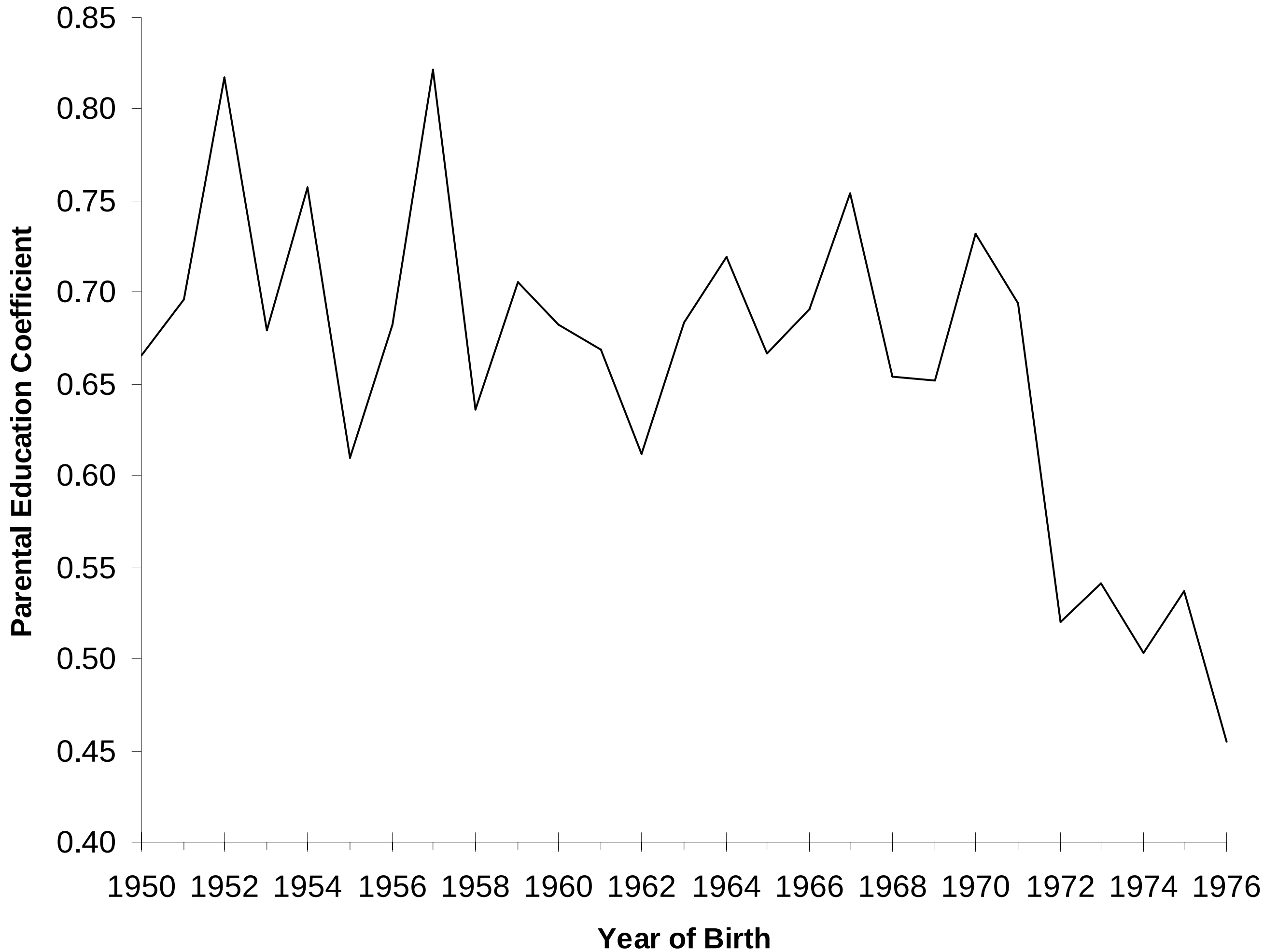
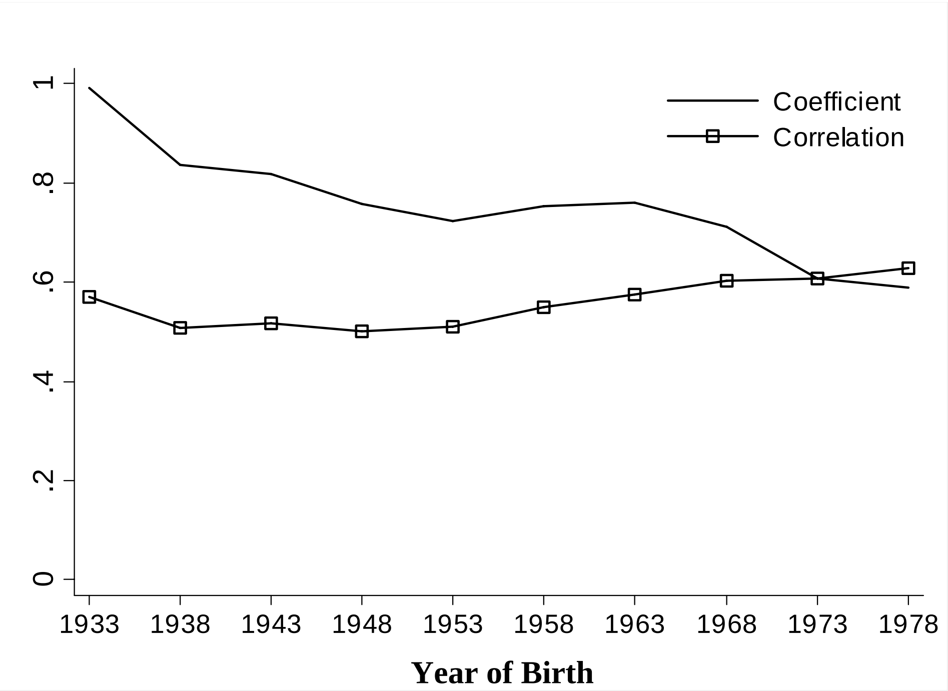


Figure 2
Intergenerational Educational Regression Coefficients and Correlations for Indonesia
By Five-Year Birth Cohorts, 1933 to 1978
Men and Women



Source: Hertz *et. al.* (2007)