Intergenerational Education Mobility and Conditional Cash Transfers in the Philippines*

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Abstract

Conditional cash transfer (CCT) programs are widely viewed as effective tools for increasing intergenerational mobility. However, direct empirical tests of this hypothesis are hard to find, and little attention has been paid to the magnitude of potential effects. In this paper, we use full count census data from the Philippines to calculate average educational attainment, along with measures of relative and upward mobility, for each municipality and birth cohort. We take advantage of the staggered rollout of a national CCT program across municipalities to estimate its effect on these outcomes. We find that the program led to modest increases in educational attainment and upward mobility, though effects on relative mobility are more mixed.

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1 Introduction

Intergenerational mobility – the degree to which individuals' socioeoconomic outcomes are determined by those of their parents – is a key metric reflecting equality of opportunity in a society. In addition to being an important societal goal in its own right, a high level of mobility can contribute to economic growth and social stability (Narayan et al., 2018). But what can governments to do achieve this? Scholars have proposed early childhood health interventions, public education investments, schooling cost reductions, and labor market reforms as potential policy levers (Arenas and Hindriks, 2021; Black and Devereux, 2011; Lee and Seshadri, 2019; Narayan et al., 2018; Restuccia and Urrutia, 2004). However, due in large part to data limitations, empirical evidence on the ability of specific policies to improve intergenerational mobility remains limited, especially in developing countries.

Social protection policies are widely viewed as effective tools for increasing intergenerational mobility (Alvaredo et al., 2018; Black and Devereux, 2011; Narayan et al., 2018; World Bank, 2022; World Bank Group, 2022). In the developing world, the last few decades have seen the wide expansion of one specific form of social protection – conditional cash transfer (CCT) programs, which provide transfers to poor households who satisfy certain education and health requirements. A large literature documents that CCT programs, which primarily target the poor, can improve various socioeconomic outcomes for their beneficiaries (Molina-Millán et al., 2019; Parker and Todd, 2017). In addition, several studies that examine heterogeneous effects have documented larger effects in poorer villages or households (Dammert, 2009; Galiani and McEwan, 2013; Maluccio and Flores, 2005). Based on these two points, it might seem natural to conclude that CCTs generate improvements in socioeconomic mobility, but whether this is true depends on the existence and nature of spillovers from beneficiaries to non-beneficiaries, which have been documented in both Mexico and the Philippines (Angelucci and De Giorgi, 2009; Filmer et al., 2023). Moreover, even without spillovers, it is not clear by how much CCTs can improve mobility, and magnitudes are an important consideration if equality of opportunity is a policy goal.

In this paper, we estimate the effects of the Filipino CCT program, *Pantawid Pamilyang Pilipino Program*, also known as the 4Ps program, on intergenerational education mobility. Using full count census data from the Philippines (2000, 2007, 2010, and 2015), we begin by estimating municipality-cohort-level measures of education mobility. The first measure we use is the correlation between parent and child education levels, a commonly used measure that captures decreasing relative mobility (or increasing relative persistence). The second measure, which also captures decreasing relative mobility, is the slope in a regression of child education on parental education. The third measure is the average percentile rank (in the national education distribution for children) of children born to parents in the 25th percentile of the national education distribution of adults.

Higher values of this variable reflect greater upward mobility, or the ability of children with parents of low socioeconomic status to move up the ranks. Because we have data on the entire population of the Philippines, sample size is not an issue when estimating these measures at the municipality-cohort level, which we can then use as outcome variables in our main analysis. Like previous work that has focused on generating sub-national measures of mobility (Alesina et al., 2021; Asher et al., 2024; Chetty et al., 2014), we document substantial variation in our mobility measures across municipalities.

To estimate the effects of the 4Ps program on intergenerational mobility, we use the fact that the program was rolled out to different municipalities in different years, from 2008 to 2013. This generates variation across municipalities and across cohorts in the number of years an individual has been exposed to the 4Ps program when we observe them in the 2010 and 2015 censuses. Using a regression that controls for municipality, census year, and age-by-year fixed effects, we estimate the effect of 4Ps exposure on four outcomes of interest: average educational attainment, the intergenerational correlation, the intergenerational slope, and upward mobility, all measured at the municipality-cohort level.

One additional year of 4Ps exposure led to a 0.03 year increase in average municipality-level educational attainment and a 0.21 unit reduction in the average percentile rank of children born to parents at the 25th percentile (i.e., an increase in upward mobility). Interestingly, despite the fact that our two measures of relatively mobility are highly correlated, the evidence on relative mobility is mixed: although we detect no significant effects on the intergenerational correlation, we find a 0.32 unit reduction in the intergenerational slope coefficient. Scaling by the average length of 4Ps exposure in our 2015 sample (4.5 years), the effects on educational attainment, upward mobility, and the intergenerational slope translate into an effect between 12 to 20% the size of the inter-quartile range of each of these outcomes. Effects are similar for boys and girls, and they are largely driven by municipalities with higher 4Ps participation, which provides support for the validity of our empirical strategy.

This paper contributes to the literature on the effects of government policies on intergenerational mobility. Early work on this topic relied on cross-sectional comparisons of intergenerational mobility across countries or states with different government spending patterns (Ichino et al., 2011; Mayer and Lopoo, 2008). More recently, fueled in part by the growing availability of high-quality administrative data in developed countries, researchers have been able to take advantage of quasi-exogenous policy variation in these countries and have documented that school tracking reforms, school spending reforms, and universal childcare have all generated improvements in intergenerational mobility, measured using both income and education (Biasi, 2023; Havnes and Mogstad, 2015; Lange and von Werder, 2017; Pekkarinen et al., 2009). In developing countries, where high quality administrative income data are rare, studies have focused primarily on education mobility. Both compulsory schooling laws (Cornelissen and Dang, 2022) and sanitation programs (Wang and Zou, 2023)

have been found to generate improvements. We expand on this nascent literature by providing evidence on CCTs, one of the most common forms of social protection in the developing world. While most of the aforementioned papers, on both developed and developing countries, regress a child outcome on a parent outcome, policy variable, and their interaction, we take a different approach that involves using measures of mobility as our outcome variables of interest. We also separately examine relative mobility, the focus of the aforementioned papers, and upward mobility, which has received less attention.

2 Data

2.1 Mobility Measures

We calculate municipality-level measures of intergenerational mobility using the full population censuses of the Philippines, conducted by the Philippines Statistics Authority in 2000, 2007, 2010, and 2015. For each household, basic demographic information – including gender, age, relationship to the household head, and educational attainment – is recorded for each household member. Because the census does not collect information about income, we focus on intergenerational education mobility, which is positively correlated with intergenerational income mobility across countries, especially in developing countries (Narayan et al., 2018). Although educational attainment may be a more narrow measure of socioeconomic status than lifetime income or consumption, it is an important causal driver of these more comprehensive outcomes of interest (Card, 1999). Focusing on education mobility also has several advantages, including less measurement error and the ability to use data from younger individuals who have completed (most of) their schooling.

Measuring intergenerational education mobility requires knowledge about the educational attainment of the parents of the individuals in the generation of interest. Unfortunately, the census does not explicitly ask about parental education levels, but we are able to obtain parental education for children of household heads by linking within households using the "relationship to household head" variable. This allows us to link a large share of children to their parents. As we show in Appendix Figure A1, for children aged 16 and younger, linkage rates are over 80%. This begins to decline at age 17, as children begin moving out of their parents' homes likely due to school, marriage, or job-related migration. By age 24, linkage rates are less than 50%.

Though linkage rates are higher for younger children, educational attainment measures are still incomplete for them. Keeping this tradeoff in mind, we focus our analysis on children aged 15-18: we have parental education information for over 80% of this cohort, and they are in theory old enough to have completed at least nine years of schooling. In Appendix section C, we examine an alternative approach, linking children (a younger generation) to the previous generation (as opposed to parents specifically), as Alesina et al. (2021)

do. This allows us to link a larger share of children.

We calculate three different measures of education mobility at the municipality-cohort level:

1. **Intergenerational correlation** (ρ): Correlation between child educational attainment and the maximum of both parents' educational attainment.

2. **Intergenerational slope** (β): Slope coefficient in a regression of child educational attainment on the maximum of both parents' educational attainment.

3. **Upward Mobility**: Average percentile rank in the national age-specific education distribution of children born to parents at the 25th percentile of the national adult education distribution, predicted using a municipality-cohort-specific regression of child rank on parental rank.

The intergenerational correlation and intergenerational slope are commonly used measures of relative mobility (Narayan et al., 2018; Van der Weide et al., 2024). These measure the degree to which individuals' education levels are determined by those of their parents and therefore captures decreasing relative mobility (i.e., increasing relative persistence). Improvements in equality of opportunity should translate into a reduction of the intergenerational correlation and slope, but reductions in relative persistence could be driven by the worsening of education outcomes of children with highly educated parents, not just the improvement of education outcomes of children with less educated parents. To calculate these measures, we use the maximum of the child's parents' educational attainment. This measure is highly correlated with measures that use the mean of the parents', only the father's, or only the mother's educational attainment instead (all correlations greater than 0.95).

The formulae for the intergenerational correlation, the intergenerational slope, and the relationship between the two can be summarized as follows:

$$\rho = \frac{Cov(E_c, E_p)}{\sigma_p \sigma_c}$$
$$\beta = \frac{Cov(E_c, E_p)}{\sigma_p^2}$$
$$\rho = \beta \frac{\sigma_p}{\sigma_c},$$

where E represents educational attainment, σ represents standard deviations, and subscripts p and c indicate parents and children, respectively. These equations indicate the two measures are closely linked, although the intergenerational correlation also depends on the variance of child educational attainment, which could in theory also be affected by the policy in question.

Our third and final measure aims to capture upward mobility, or the extent to which children born to

parents on the lower end of the socioeconomic distribution can move up the ranks. Specifically, for each municipality, we calculate the average percentile rank (in the national education distribution of children of the relevant age) of children born to parents who are at the 25th percentile of the national education distribution among adults. This measure is the education analog of the "absolute upward mobility" measure used by Chetty et al. (2014) in their examination of income mobility and similar in spirit to "upward interval mobility" in education used by Asher et al. (2024). Unlike for the intergenerational correlation, improvements in our measure of upward mobility (holding fixed the average education rank for children of parents with higher education levels) are more unambiguously welfare-enhancing.[1]

2.2 Descriptive Statistics

In Table 1, we report the mean and various percentiles of the distribution of municipality-level outcomes, after averaging across cohorts aged 15-18 in the 2015 census. For educational attainment, the inter-quartile range is 0.86 years of schooling, but there is a much wider gap between the 10th and 90th percentiles (which are 7.65 and 9.14, respectively). For upward mobility, the inter-quartile range is 7 rank positions and the gap between the 10th and 90th percentile is 15 rank positions. There is also considerable variation in the intergenerational correlation and slope. Based on the intergenerational correlation, the 25th percentile (a municipality with relatively high mobility) would rank in the top (most mobile) quintile of country rankings (according to estimates from Narayan et al. (2018)), while the 75th percentile municipality would land in the 3rd quintile and the 90th percentile in the 4th quintile globally.

Table 1 also reports all pairwise correlations between these measures. Mean educational attainment is highly correlated with upward mobility ($r = 0.87$), and the intergenerational correlation and slope are also highly correlated ($r = 0.89$), which makes sense given the mechanical relationship between the two, as described above. Correlations are weaker though still fairly strong (with magnitudes ranging from 0.35 to 0.63) across these two pairs of outcomes.

3 4Ps Program

3.1 Program Details

The *Pantawid Pamilyang Pilipino Program*, commonly known as the 4Ps, is a conditional cash transfer program that provides cash to eligible households who meet a set of education and health requirements. It

[1] As Chetty et al. (2014) argue with respect to their absolute upward mobility measure, even though education rank is technically a relative outcome, the municipality-level average education rank in the national distribution is essentially an absolute outcome given that changes in the education distribution of one municipality are unlikely to affect the national distribution. To avoid confusion, however, we refrain from using the word "absolute" to describe our upward mobility measure.

began in 2008 and has since become the primary social protection program of the Philippines. Households are eligible for the 4Ps if they have at least one child (or a pregnant woman) and if their poverty score falls below a pre-determined cutoff.

As part of the education component of the program, households receive a cash transfer for each child (up to a maximum of three children) enrolled in school and attending at least 85% of school days. Initially, this incentive applied to children aged 6 to 14, but this was expanded to include children up to age 18 in 2014. The health component of the program provides a cash transfer to households who fulfill a set of health-related conditions, including regular health visits for young children and pregnant women in the household. Based on the transfer amounts in the first year of the program, a household who met all program conditions would receive a maximum transfer of PhP 1400, estimated to be approximately one quarter of beneficiary monthly income at the time.

When it was first launched in 2008, the 4Ps program was targeted to the poorest households in the 20 poorest provinces (known as "Set 1"). In each subsequent year, the program was expanded to new "sets" of households according to a list of targeting criteria based on various region-specific poverty score cutoffs, indigenous population shares, and other vulnerability indicators. Set 2 households were added in 2009, set 3 in 2010, and so on. We take advantage of the program's staggered rollout to estimate its impact on our outcomes of interest.

3.2 Program Rollout Data

Information about the 4Ps program rollout comes from the Department of Social Welfare and Development (DSWD). We use their reports of the number of 4Ps households in each municipality, as of December 2010, March 2013, and March 2016. Importantly, the counts are broken down by set number, which helps us predict when a municipality first received the 4Ps program.

To identify municipalities that received the program in 2008 and 2009, we use the earliest available report (December 2010). The existence of any set 1 household in a municipality does not necessarily imply that the program was rolled out to that municipality in 2008, as set 1 households from a different municipality could have moved into that municipality after their initial enrollment into the 4Ps. We therefore use, for our main analysis, a cutoff of 10 households (but explore robustness to 1 and 100 as alternative cutoffs). If a municipality is recorded as having at least 10 set 1 households in 2010, we record the municipality's 4Ps introduction year as 2008. If any of the remaining municipalities are listed has having at least 10 set 2 households in 2010, we record its 4Ps introduction year as 2009.

For 4Ps introduction years of 2010-2012 (associated with sets 3-5), we use the 2013 report and follow

the same approach. For the municipalities still unclassified after this, we use the 2016 report.[2] Using this approach, 2013 (associated with set 6) is the latest year in which any of the municipalities in the report first received the 4Ps program.[3] Less than 1% of municipalities are not included in the DSWD reports, presumably because they did not have any 4Ps beneficiaries in 2010, 2013, or 2016, and we exclude them from our analysis because they are likely to be systematically different from the rest of the municipalities we study.

Figure 1 summarizes the cumulative number of municipalities with 4Ps exposure by year, using our baseline cutoff rule of 10 households and then using the two alternative cutoffs, 1 and 100. The patterns are very similar for cutoffs 10 and 100, though with the 100-household cutoff, there are a small number of municipalities recorded as having received the program first in 2014. The pattern for the 1-household cutoff is somewhat similar as well, but it shows a noticeably more rapid increase in the number of municipalities exposed to the 4Ps from 2009 to 2011.

3.3 Existing Evidence

There exist several evaluations of the 4Ps program, which we categorize into three groups: short-run experimental evaluations (Kandpal et al., 2016; World Bank Group, 2012), regression discontinuity (RD) evaluations (Onishi et al., 2019; Orbeta et al., 2014, 2021b), and longer-run evaluations relying on experimental variation (Dervisevic et al., 2021; Orbeta et al., 2021a). The studies in the first category rely on a randomized controlled trial (RCT) conducted for evaluation purposes in 2008. 65 pilot villages were assigned to a treatment group that received the program 2.5 years earlier than 65 control villages. The papers that use this RCT estimate the effect of the 4Ps program by comparing eligible households in villages exposed to the program for approximately 2.5 years to eligible households in villages yet to be introduced to the program.

The second group uses RD methods, comparing households just below and just above the poverty score cutoff that determined eligibility for the program. Three separate RD evaluations used surveys conducted in 2011-2012 (Onishi et al., 2019), 2013 (Orbeta et al., 2014), and 2017-2018 (Orbeta et al., 2021b), capturing the effect of being eligible for the 4Ps for anywhere from 2-9 years (relative to not being eligible at all).

The last group of studies aims to estimate long-run effects of the program by taking advantage of the experimental variation from the original RCT. Because the control group eventually received the program, both studies focus on specific cohorts who were of critical ages during the RCT. Orbeta et al. (2021a) focus on children born between April 2009 and April 2013, therefore capturing the effect of being exposed to the 4Ps program during the first 1000 days of life, relative to being exposed later on. Dervisevic et al. (2021)

[2] By using the report closest to the actual set's implementation year, we aim to minimize categorization errors due to migration.

[3] In the data, there are set 7 and 8 households, but these households are in municipalities that had already received the program.

focus on those aged 12.5-14 years old, who would have aged out of the program by the time the control group received it, and therefore capture the effect of being exposed to the 4Ps during these adolescent years (relative to not being exposed at all).[4]

Overall, these studies document positive effects of the 4Ps program on various health and education outcomes: for example, higher school enrollment and attendance, along with lower rates of severe stunting. Despite these generally positive effects, some evidence of negative unintended consequences have also been documented. For example, De Hoop et al. (2019) finds that the 4Ps program increased child labor while increasing school participation, likely because the cash transfer only served as a partial education subsidy and children needed to work to make up the shortfall. Filmer et al. (2023) find that, by resulting in higher prices, the 4Ps program increased stunting among ineligible children, particularly in remote villages with larger shares of eligible households.

Despite the existence of the many evaluations of the 4Ps program, it is still not clear what the program has done for intergenerational education mobility in the Philippines. One reason is because not many of these evaluations have examined educational attainment (as opposed to enrollment or attendance) as an outcome.[5] Another reason is because these evaluations rely on household-level comparisons – between those currently eligible for the program to those not, for example – which could over or under-estimate the program effects on the broader area as a whole if there are spillovers from beneficiaries to non-beneficiaries. In the next section, we describe the municipality-level approach we take to estimating effects on educational attainment and intergenerational education mobility.

4 Empirical Strategy

Unlike the evaluations of the 4Ps program described above, which rely on household-level comparisons, our empirical strategy involves comparing municipalities that received the 4Ps program in different years. The main motivation for this municipality-level approach is that our main outcome of interest – intergenerational education mobility – is a municipality-level variable. However, even when we examine educational attainment (an individual-level variable) as an outcome, we do this by first calculating municipality-level averages, as the lack of 4Ps status information in the census precludes an individual-level comparison of beneficiaries to non-beneficiaries. It is important to note, however, that this municipality-level approach allows us to estimate the effect of 4Ps exposure on the municipality as a whole. This parameter is different from what has been estimated in the past and is only possible because we have information on the entire population, unlike

[4]One feature that complicates the interpretation of both of these papers is the fact that the control group received back payments for grants they did not receive while the treatment group was receiving benefits (Orbeta et al., 2021a).

[5]The exceptions are World Bank Group (2012) and Dervisevic et al. (2021), neither of which find significant effects when using the initial experimental variation.

existing work that has focused on samples collected with an RCT or RD design in mind. Given the Filmer et al. (2023) evidence on negative spillovers to ineligible households (for health outcomes), our estimate may be different from – and if so, is potentially more policy relevant than – the result of an individual-level approach.

The staggered rollout of the program across municipalities generates variation in the number of years a municipality-cohort has been exposed to the program when we observe them in the census. In the 2015 census, for those aged 15 to 18 (our age cohort of interest), exposure ranges from two to eight years (see Appendix Figure A2). Relying only on cross-sectional variation in exposure would be problematic, however, because exposure is likely to be correlated with both observed and unobserved municipality-level characteristics. We therefore also use the 2010 census (at which point the maximum exposure was two years), which will allow us to control for municipality fixed effects. More specifically, for outcome Y_{jya} of cohort aged a in municipality j observed in census year y (either 2010 or 2015), we estimate the following regression:

$$Y_{jya} = \beta \text{Exposure}_{jya} + \mu_j + \gamma_{ay} + \epsilon_{jya}, \tag{1}$$

Y_{jya} represents one of four outcome variables of interest: average educational attainment, the intergenerational correlation between parental education and child education, the intergenerational slope in a regression of child education on parental education, and upward mobility (as defined above) measured among ages a ranging from 15 to 18 in municipality j and census year y. We first examine these measures calculated using all children, then separately examine boys and girls. Exposure_{jya} captures the number of years an individual of age a in municipality j could have potentially been exposed to the 4Ps program by census year y, based on what year the program first reached that municipality and the ages eligible for the education component of the 4Ps (6-14 until 2014, when this was expanded to 6-18). This varies across municipalities, census years, and age cohorts, which allows us to control for both municipality fixed effects (μ_j) and year-by-age fixed effects (γ_{ay}). The latter allows for an age trend in our outcomes that varies across years. We cluster standard errors at the municipality level and weight by population size.

Potential exposure, which is what is captured by Exposure_{jya}, is not the same as actual exposure because not everyone in a municipality where the 4Ps program is available is eligible for or enrolled in the program. Our estimates should therefore be interpreted as intent-to-treat estimates. We are not asking what would happen to overall mobility or average educational attainment if everyone in a municipality were exposed to the 4Ps for an additional year. Rather, we are asking what would happen if all eligible and enrolled 4Ps households (given the existing targeting rules) were exposed to an additional year of it. In some ways, this is

the more policy relevant question because the 4Ps program, like most conditional cash transfer programs, was never intended to be a universal program and will continue to only be targeted to a subset of households. That said, it is worth noting that β in equation (1) will represent the average intent-to-treat effect across municipalities with varying shares of the population participating in the 4Ps. We therefore also estimate this regression separately for those with above and below median 4Ps enrollment rates to verify that our estimates are indeed larger in municipalities where a large share of households are actually participating in the program.

In order to interpret β as the causal effect of an additional year of potential 4Ps exposure, we need to assume that there are no time-varying municipality-specific variables that might be correlated with exposure as well as our outcomes of interest. If outcome Y_{jya} in municipalities where the 4Ps was introduced earlier (likely of lower socioeconomic status) was growing faster than in municipalities where the 4Ps was introduced later, for reasons other than the 4Ps program, this would generate a spurious positive coefficient in the above regression. To address this concern, we also estimate specifications that allow for differential trends for municipalities with different poverty levels by controlling for a 2015 year dummy interacted with dummies for each decile of baseline poverty (measured in 2003). In addition to this, we run a falsification test using the 2000 and 2007 censuses, treating 2007 as if it were 2015 and assigning exposure accordingly. That is, we create a "false" 4Ps exposure variable that shifts back the date of each municipality's initial 4Ps implementation by 8 years. This results in municipalities in 2007 being assigned the same exposure value as in 2015, even though all municipalities had zero exposure in 2007. We run the following regression, restricting to years 2000 and 2007:

$$Y_{jya} = \alpha \text{PlaceboExposure}_{jya} + \mu_j + \gamma_{ay} + \epsilon_{jya}, \tag{2}$$

A statistically significant α in these regressions would indicate that outcome Y_{jya} was trending differently in areas that received the 4Ps program earlier and areas that received the 4Ps program later – even prior to the implementation of the program. This would cast doubt on the validity of our approach in equation (1).

5 Results

5.1 Preliminary Evidence

Before reporting the results of our main regression specification, we begin by comparing trends in outcomes across municipalities that received the 4Ps in different years. Using our main sample (cohorts aged 15 to 18

in the 2010 and 2015 censuses), we estimate the difference in outcomes across census years separately for each 4Ps introduction year and plot the estimates in Figure 2. Specifically, we plot the β_t coefficients from the following regression:

$$Y_{jya} = \sum_{t=2008}^{2013} \beta_t 1(\text{4Ps Introduction Year} = t) \times 1(y = 2015) + \mu_j + \gamma_{ay} + \epsilon_{jya}. \quad (3)$$

Each β_t coefficient reflects the difference in outcome Y_{jy} measured among 15-18-year-olds in 2010 and 15-18-year-olds in 2015, for each 4Ps introduction year group. Because the early recipient municipalities saw larger increases in their 4Ps exposure between 2010 and 2015 (as shown in Appendix Figure A2), if the 4Ps had a causal impact on outcomes, we would expect to see larger changes in these outcomes for early recipient municipalities compared to the late recipient municipalities.

In panel A of Figure 2, this appears to be the case. The municipalities showing the smallest change in educational attainment were those that received the program last, in 2013 (the omitted category). The individuals in this group were exposed to the program for 2-3 years in 2015, while those in the same municipalities in 2010 had not been exposed at all, implying a 2-3-year increase in exposure between the two censuses. Increases in educational attainment are larger for the 2012 group, which experienced a maximum of a 4-year increase in exposure, and even larger for the 2011 and 2010 groups, which saw increases of up to 5 and 6 years, respectively. Both the 2008 and 2009 groups experienced the largest increase in exposure – up to 7 years – and consistent with this, the 2009 group also demonstrated the largest change in educational attainment. The increase in educational attainment for the 2008 group, however, was more muted, similar to those of the 2010 and 2011 groups. With the exception of this 2008 coefficient, the pattern of coefficients is consistent with the existence of a positive, causal effect of the 4Ps program on educational attainment.

Unlike panel A, the pattern of coefficients in panel B is relatively flat, suggesting that the 4Ps program did not have a large effect on the intergenerational correlation: municipalities experienced similar reductions in the intergenerational correlation regardless of how long they had been exposed to the 4Ps program. For the intergenerational slope and upward mobility, however, we see a more distinct pattern. The largest decreases in the intergenerational slope (representing an improvement in mobility) took place in the 2009 group, with magnitudes decreasing with later introduction years. For upward mobility, there is a pattern similar to that of educational attainment, suggesting improvements in upward mobility due to the 4Ps program.

5.2 Main Results

We now move on to our main regression specification. Table 2 reports the effects of 4Ps exposure on educational attainment and our three measures of education mobility, beginning with our baseline specification in panel

A. In panel B, we control for a 2015 dummy interacted with separate indicators for baseline poverty deciles, in order to allow for areas with different levels of poverty (a determinant of the timing of 4Ps implementation) to trend differently over time. Across all outcomes, estimates are similar though slightly smaller in panel B. We focus on this specification for the remainder of the paper, as it ensures that our estimates are not being driven by different trends in high income and low income areas.

Based on the results in panel B, one additional year of potential 4Ps exposure at the municipality level led to a statistically significant 0.03 year increase in average municipality educational attainment. At the average level of 4Ps exposure, which was 4.5 years among 15-18 year olds in 2015, this corresponds to an effect of 0.135 years, which is approximately 16% of the inter-quartile range in educational attainment as reported in Table 1. To put this magnitude into perspective, an additional 18 months of exposure to the widely studied Mexican CCT program, PROGRESA, led to an increase of about 0.2 years of schooling among those eligible for the program (Behrman et al., 2011). Our estimates would imply an increase of 0.045 years of schooling in response to a 1.5 year increase in 4Ps exposure, averaged over the entire population, not just those eligible for the 4Ps.[6]

Consistent with the flat pattern of coefficients in panel B of Figure 2, the intergenerational correlation, a measure of relative mobility, appears unaffected by the 4Ps program. Column 2 of panel B of Table 2 yields a positive, small, and statistically insignificant coefficient of 0.04 (compared to a mean of 36.93). In contrast, the intergenerational slope – also a measure of relative mobility – was significantly affected. An additional year of potential 4Ps exposure led to a reduction of 0.32 percentage points (an improvement in mobility), which means that the average level of exposure (4.5 years) corresponds to an effect size approximately 20% of the inter-quartile range. Given the relationship between these two measures, these conflicting results could be due to the 4Ps program narrowing the variation in child educational attainment while also affecting the covariance between child and parental education.

Absolute mobility increased in response to 4Ps exposure. An additional year of 4Ps exposure increased the average percentile rank of a child born to parents at the 25th percentile by 0.21 positions. For the average level of exposure (4.5), this translates into an effect of 0.9 rank positions, which is about 12% of the inter-quartile range.

In Table 3, we repeat our analysis for different groups, using the specification controlling for trends by baseline poverty status (though Appendix Table A1 reveals very similar results when we drop these controls). We first repeat our regressions using the same municipality-level outcome variables calculated separately for each gender. Coefficient estimates are almost identical for girls (panel A) and boys (panel B).

We also explore heterogeneity by municipality characteristics. Municipalities vary in the share of the

[6] In 2016, the 4Ps program had reached 4.2 million households. There were about 25 million households in the 2015 census.

population actually eligible for and enrolled in the 4Ps, which means the coefficients estimated in the previous tables are likely masking substantial heterogeneity. Because our outcome variables use the entire municipality, holding all else constant, effect sizes should be larger in municipalities with a larger share of 4Ps households. To verify this is the case, we split the sample into municipalities with above-median and below-median shares of the population enrolled in the 4Ps program. In panels C and D of Table 3, we indeed find that effects are larger in and almost completely driven by high-enrollment municipalities.

5.3 Robustness Checks

To what extent are these results driven by differential trends in outcomes unrelated to 4Ps exposure? While our preferred specification controls flexibly for differential trends by baseline poverty status, there may be other sources of differential trends that we are unable to control for. To explore the possibility of this, we run the placebo test described by equation (2), where we assign municipalities in 2000 and 2007 to the exposure they would have had if the program started in 2001 instead of 2008. This allows us to evaluate whether the outcomes of early and late recipients of the 4Ps program were already trending differently prior to the program's inception. Reassuringly, the estimates in Table 4 are all small in magnitude and statistically insignificant, whether or not we control for trends by baseline poverty rates. This means we only see differential trends between early and late recipients once the 4Ps had been begun, and not before, which is in line with our interpretation of the significant coefficients in Table 2 as driven by the 4Ps program itself.

All previous results used an exposure variable generated using 10 households as the cutoff for categorizing a municipality as having received the 4Ps program in a particular year. In Appendix Table A2, we explore the robustness to using 1 and 100 as alternative cutoffs. Results are almost identical across all cutoffs.

Our measures of intergenerational mobility can only be calculated using linked parent-child pairs. In Appendix section B, we show that our estimates of the intergenerational correlation are similar to those obtained from a dataset that collects information about parental education from all respondents. In Appendix section C, we explore an alternative approach to linking generations, similar to the one used by Alesina et al. (2021). Instead of linking children to parents, we link younger generations to older generations, which reduces the number (and perhaps also the extent of selection) of children we have to drop from our analysis. Our results are almost identical when we use this approach. Nevertheless, even given the reassuring similarities when it comes to the intergenerational correlations in Appendix section B and the results in Appendix section C, if the 4Ps program changes the number and types of pairs we are able to link, this could complicate the interpretation of our education mobility effects discussed above.

To investigate this possibility further, we repeat our main regression using the percent of children

successfully linked to their parents as the outcome variable. The result, reported in column 1 of Appendix Table A3, confirms that 4Ps exposure does indeed appear to affect municipality-level linkage rates. Specifically, 4Ps exposure appears to increase linkage rates, perhaps because the program reduces the need for parents to migrate away for better job opportunities or because the conditions of the program are easier to satisfy when at least one parent is living at home. Reassuringly, however, this effect is very small: an additional year of exposure increases linkage rates by 0.42 percentage points (compared to a mean of 80.82 percent). Perhaps unsurprisingly given the size of this effect, when we control for the percent linked and repeat our regressions on the intergenerational correlation, intergenerational slope, and upward mobility, our results are very similar to those reported in columns 2 to 4 of Table 2. Although this is not our preferred specification because it involves the potentially problematic inclusion of an endogenous outcome as a control, the fact that the results are so similar to our main results helps alleviate concerns about compositional effects.

6 Conclusion

In this paper, we examine municipality-level measures of intergenerational education mobility across the Philippines. We document substantial variation in terms of the intergenerational correlation and slope (which capture relative mobility), as well as upward mobility. Taking advantage of multiple waves of census data and the staggered rollout of the 4Ps program across municipalities, we estimate the effect of the 4Ps program on average educational attainment and these measures of mobility. We find the program had modest effects on educational attainment and upward mobility. Effects on relative mobility were more mixed. Results suggest that the program also reduced the variance of child educational attainment while reducing the covariance between child and parental education, leading to a null effect on the intergenerational correlation despite a reduction in the slope coefficient.

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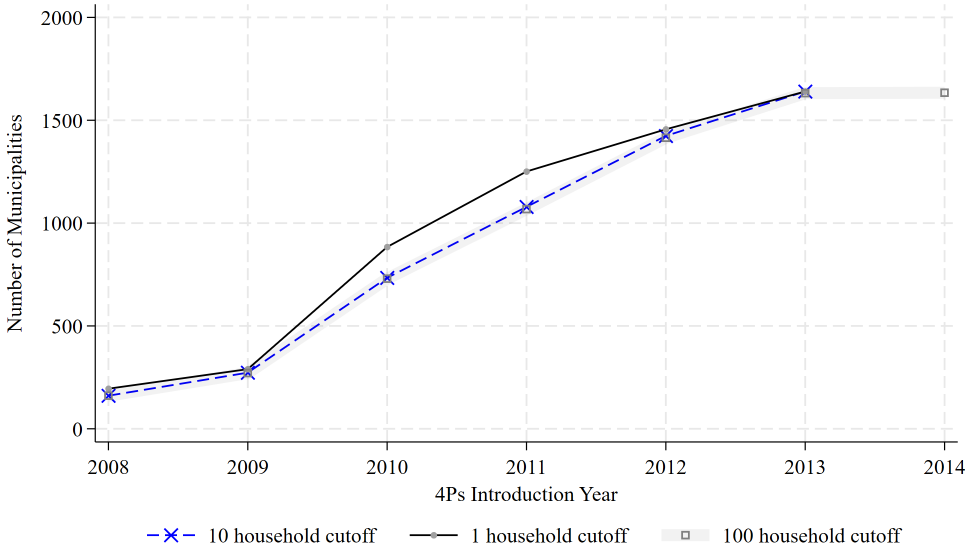
Tables and Figures

Table 1: Descriptive Statistics

	(1) Educational Attainment	(2) Intergenerational Correlation	(2) Intergenerational Slope	(3) Upward Mobility
Mean	8.61	0.35	0.21	40.14
10th percentile	7.65	0.26	0.14	32.73
25th percentile	8.28	0.30	0.18	36.28
Median	8.77	0.35	0.21	40.04
75th percentile	9.14	0.39	0.25	44.19
90th percentile	9.37	0.45	0.29	47.73
Correlation with Educational Attainment	1	-0.35	-0.59	0.87
Correlation with Intergenerational Correlation		1	0.89	-0.47
Correlation with Intergenerational Elasticity			1	-0.63

Notes: Statistics calculated after averaging cohort-municipality-level measures across all cohorts aged 15 to 18 in 2015.

Figure 1: Program Rollout Across Municipalities



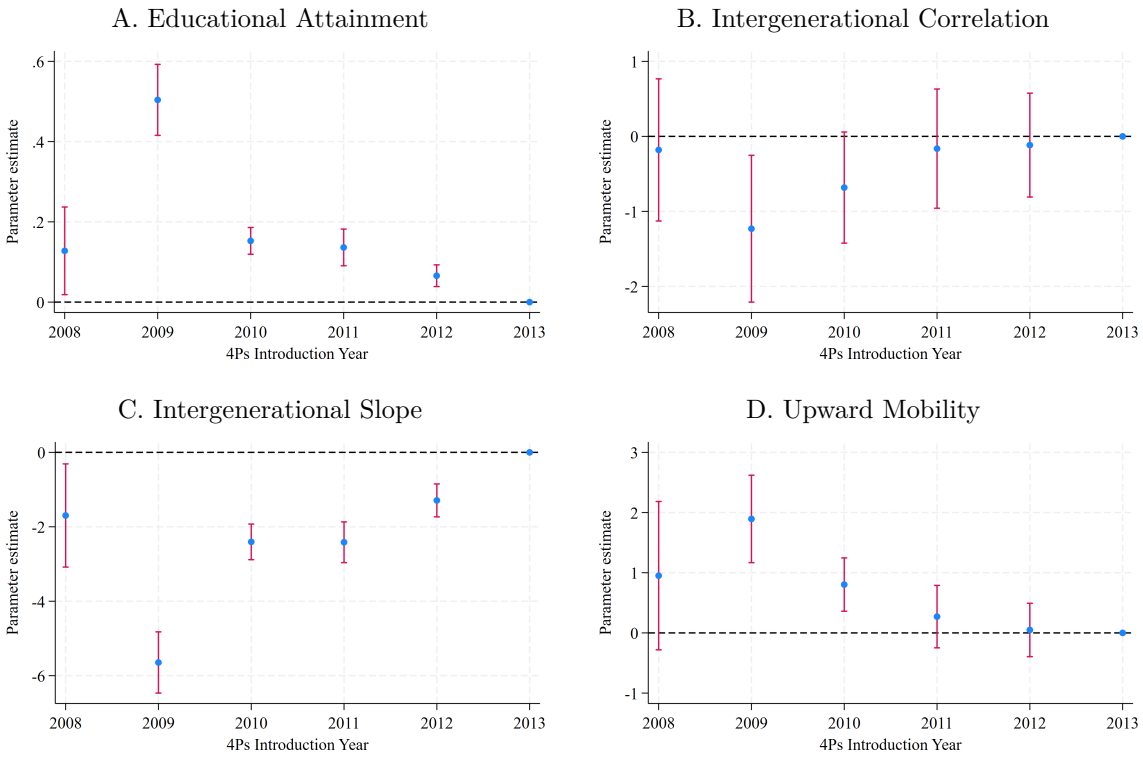
Notes: Cumulative number of municipalities classified into each 4Ps introduction year. "Household cutoff" refers to the number of households in a set required to classify a municipality as having received the program in the year associated with that set.

Table 2: Effects of 4Ps Exposure on Educational Attainment and Education Mobility

	(1) Educational Attainment	(2) Intergen. Correlation	(3) Intergen. Slope	(4) Upward Mobility
A. Baseline specification				
4Ps Exposure	0.04**	-0.10	-0.47***	0.22**
	(0.02)	(0.09)	(0.16)	(0.10)
B. Controlling for trends by baseline poverty				
4Ps Exposure	0.03***	0.04	-0.32***	0.21*
	(0.01)	(0.10)	(0.10)	(0.11)
Observations (Unweighted)	13,100	13,100	13,100	13,100
Observations (Weighted)	15,958,065	12,897,724	12,897,724	12,897,724
Outcome mean	8.67	36.93	22.95	40.41

Notes: Standard errors (clustered at the municipality level) are in parentheses * p< 0.1, ** p< 0.05, *** p< 0.01. All regressions use the 2010 and 2015 censuses and control for municipality and birth cohort fixed effects. Panel B also controls for a year dummy interacted with dummies for baseline poverty share deciles. The intergenerational correlation and slope are multiplied by 100 for readability.

Figure 2: Outcome Trends by 4Ps Introduction Year



Notes: Coefficients and 95% confidence intervals for the β_t coefficients in equation (3) are reported. Standard errors are clustered at the municipality level. The intergenerational correlation and slope are multiplied by 100.

Table 3: Effects of 4Ps Exposure on Educational Attainment and Mobility, by Group

	(1) Educational Attainment	(2) Intergenerational Correlation	(3) Intergen. Slope	(4) Upward Mobility
A. Females				
4Ps Exposure	0.03***	0.08	-0.31***	0.18*
	(0.01)	(0.12)	(0.10)	(0.10)
Observations (Unweighted)	13,100	13,100	13,100	13,100
Observations (Weighted)	7,820,376	6,050,861	6,050,861	6,050,861
Outcome mean	9.03	33.52	18.25	46.99
B. Males				
4Ps Exposure	0.03**	0.02	-0.32***	0.23*
	(0.01)	(0.11)	(0.11)	(0.12)
Observations (Unweighted)	13,100	13,100	13,100	13,100
Observations (Weighted)	8,137,689	6,846,863	6,846,863	6,846,863
Outcome mean	8.32	40.44	26.70	34.79
C. Above Median Eligibility Shares				
4Ps Exposure	0.12***	0.20	-0.83***	0.58***
	(0.01)	(0.23)	(0.13)	(0.12)
Observations (Unweighted)	6,540	6,540	6,540	6,540
Observations (Weighted)	4,776,287	3,986,614	3,986,614	3,986,614
Outcome mean	7.94	40.28	27.08	36.17
D. Below Median Eligibility Shares				
4Ps Exposure	0.01	-0.04	-0.18	0.14
	(0.01)	(0.11)	(0.11)	(0.13)
Observations (Unweighted)	6,560	6,560	6,560	6,560
Observations (Weighted)	11,181,778	8,911,110	8,911,110	8,911,110
Outcome mean	8.98	35.43	21.10	42.31

Notes: Standard errors (clustered at the municipality level) are in parentheses * p< 0.1, ** p< 0.05, *** p< 0.01. All regressions use the 2010 and 2015 censuses and control for municipality and birth cohort fixed effects, along with a year dummy interacted with dummies for baseline poverty share deciles. The intergenerational correlation and slope are multiplied by 100 for readability.

Table 4: Placebo Tests

	(1) Educational Attainment	(2) Intergen. Correlation	(3) Intergen. Slope	(4) Upward Mobility
A. Baseline specification				
4Ps Placebo Exposure	0.00	0.00	-0.14	-0.09
	(0.01)	(0.16)	(0.14)	(0.10)
B. Controlling for trends by baseline poverty				
4Ps Exposure	-0.00	0.15	-0.01	-0.11
	(0.01)	(0.11)	(0.09)	(0.09)
Observations (Unweighted)	12,860	12,860	12,860	12,860
Observations (Weighted)	13,869,829	11,264,275	11,264,275	11,264,275
Outcome mean	8.19	37.06	23.94	40.17

Notes: Standard errors (clustered at the municipality level) are in parentheses * p< 0.1, ** p< 0.05, *** p< 0.01. All regressions use the 2000 and 2007 censuses and control for municipality and birth cohort fixed effects. Panel B also controls for a year dummy interacted with dummies for baseline poverty share deciles. Placebo exposure is the number of years of 4Ps exposure a municipality-cohort would have had if the first year of implementation was in 2000 instead of 2008. The intergenerational correlation and slope are multiplied by 100 for readability.

A Appendix Tables and Figures

Figure A1: Share of children with parental education information, by age

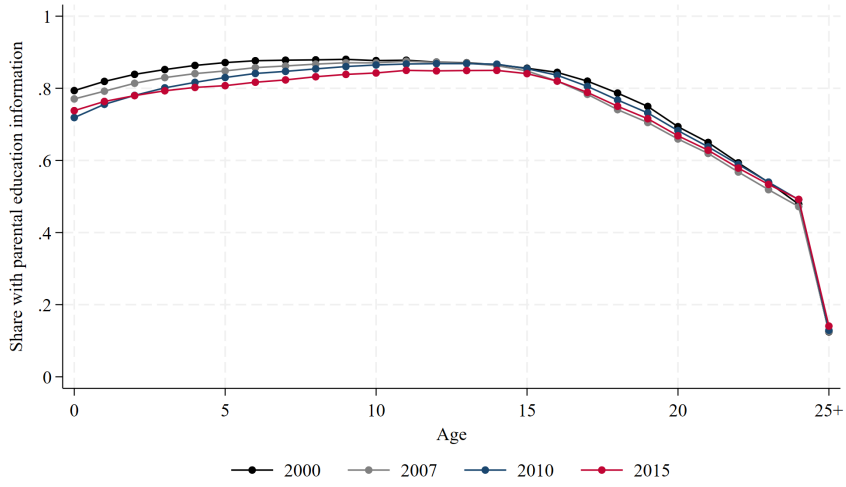


Table A1: Effects of 4Ps Exposure on Educational Attainment and Education Mobility, by Group (No Trends by Baseline Poverty)

	(1) Educational Attainment	(2) Intergenerational Correlation	(3) Intergen. Slope	(4) Upward Mobility
A. Females				
4Ps Exposure	0.04***	-0.02	-0.44***	0.21**
	(0.01)	(0.11)	(0.17)	(0.09)
Observations (Unweighted)	13,100	13,100	13,100	13,100
Observations (Weighted)	7,820,376	6,050,861	6,050,861	6,050,861
Outcome mean	9.03	33.52	18.25	46.99
B. Males				
4Ps Exposure	0.04**	-0.14	-0.49***	0.23**
	(0.02)	(0.10)	(0.16)	(0.11)
Observations (Unweighted)	13,100	13,100	13,100	13,100
Observations (Weighted)	8,137,689	6,846,863	6,846,863	6,846,863
Outcome mean	8.32	40.44	26.70	34.79
C. Above Median Eligibility Shares				
4Ps Exposure	0.12***	-0.02	-0.91***	0.57***
	(0.01)	(0.18)	(0.12)	(0.10)
Observations (Unweighted)	6,540	6,540	6,540	6,540
Observations (Weighted)	4,776,287	3,986,614	3,986,614	3,986,614
Outcome mean	7.94	40.28	27.08	36.17
D. Below Median Eligibility Shares				
4Ps Exposure	0.00	-0.09	-0.09	0.15
	(0.01)	(0.12)	(0.14)	(0.14)
Observations (Unweighted)	6,560	6,560	6,560	6,560
Observations (Weighted)	11,181,778	8,911,110	8,911,110	8,911,110
Outcome mean	8.98	35.43	21.10	42.31

Notes: Standard errors (clustered at the municipality level) are in parentheses * p< 0.1, ** p< 0.05, *** p< 0.01. All regressions use the 2010 and 2015 censuses and control for municipality and birth cohort fixed effects. The intergenerational correlation and slope are multiplied by 100 for readability.

Figure A2: Years of 4Ps Exposure by Age, Introduction Year, and Census Year

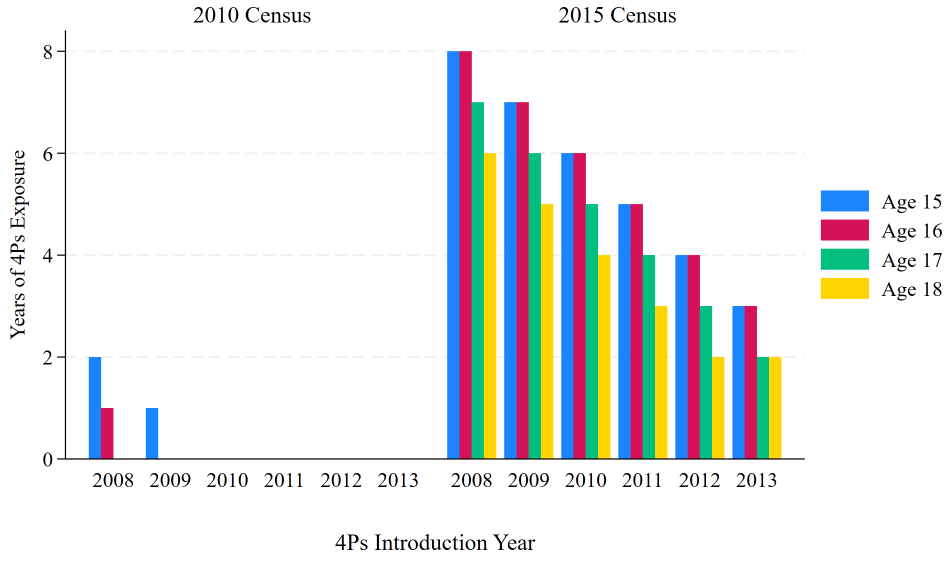


Table A2: Robustness to Other Cutoff Definitions

	(1) Educational Attainment	(2) Intergen. Correlation	(3) Intergen. Slope	(4) Upward Mobility
A. Cutoff of 1				
4Ps Exposure	0.03**	0.05	-0.28***	0.19*
	(0.01)	(0.11)	(0.10)	(0.11)
Observations (Unweighted)	13,100	13,100	13,100	13,100
Observations (Weighted)	15,958,065	12,897,724	12,897,724	12,897,724
Outcome mean	8.67	36.93	22.95	40.41
B. Cutoff of 100				
4Ps Exposure	0.03***	0.03	-0.33***	0.21**
	(0.01)	(0.10)	(0.10)	(0.11)
Observations (Unweighted)	13,052	13,052	13,052	13,052
Observations (Weighted)	15,953,368	12,894,486	12,894,486	12,894,486
Outcome mean	8.67	36.93	22.95	40.41

Notes: Standard errors (clustered at the municipality level) are in parentheses * p< 0.1, ** p< 0.05, *** p< 0.01. All regressions use the 2010 and 2015 censuses and control for municipality and birth cohort fixed effects, along with a year dummy interacted with dummies for baseline poverty share deciles. The intergenerational correlation and slope are multiplied by 100 for readability. "Cutoff" refers to the number of households used to define whether a municipality had received the 4Ps by a particular year.

Table A3: Investigating the Role of Parent-Child Linkage Rates

	(1) Percent Linked	(2) Intergen. Correlation	(3) Intergen. Slope	(4) Upward Mobility
4Ps Exposure	0.42***	0.09	-0.34***	0.21*
	(0.12)	(0.10)	(0.09)	(0.11)
Percent linked		-0.12***	0.06***	-0.04**
		(0.03)	(0.02)	(0.02)
Observations (Unweighted)	13,100	13,100	13,100	13,100
Observations (Weighted)	15,958,065	12,897,724	15,958,065	15,958,065
Outcome mean	80.82	36.93	23.04	40.43

Notes: Standard errors (clustered at the municipality level) are in parentheses * p< 0.1, ** p< 0.05, *** p< 0.01. All regressions use the 2010 and 2015 censuses and control for municipality and birth cohort fixed effects, along with a year dummy interacted with dummies for baseline poverty share deciles. The intergenerational correlation and slope are multiplied by 100 for readability. Percent linked is the percentage of the municipality-cohort with parental education information.

B Alternative Source for the Intergenerational Correlation

Because our measures of intergenerational mobility can only be calculated for a subset of the population (children of the household head and/or spouse), we attempt to compare our measures with those obtained from a dataset that collects information about parental education from all respondents: the 1999 International Social Survey Program (ISSP).

Using the 1999 ISSP and restricting to their youngest age category (18-24), which consists of 219 respondents in the Philippine survey, we calculate an intergenerational correlation of 0.42. Using the closest census (from 2000), restricting to respondents aged 18-24, we obtain an intergenerational correlation of 0.46. This discrepancy is not large, and we argue that it would be even smaller for our population of interest (the age 16 cohort), which has much higher parent-child linkage rates than the age 18-24 group. We are able to link 84% of 16-year-olds and only 65% of those aged 18-24.

C Alternative Linking Strategy

In this section, we describe the methods and results from an alternative strategy for calculating intergenerational correlations and upward mobility. Following Alesina et al. (2021), instead of linking children with their parents, we link children with anyone from the older generation in their household. There are four generations that can potentially be identified in a household using the "relationship to household head" variable, which means there are three possible ways to define an older-younger pair. The first "older" generation consists of the household head's father, mother, uncles, and aunts. This generation's educational attainment was linked to the household head. Because of our age restrictions, the children in our sample are unlikely to be linked to their older generation using this method. More relevant to our analysis, the second "older" generation consists of the household head, household head's spouse(s), and the household head's siblings, who are linked to a "younger" generation of the household head's biological children, stepchildren, nephews, and nieces. The third "older" generation consists of the household head's sons, daughters, sons-in-law, and daughters-in-laws, who are linked to the "younger" generation consisting of the household head's grandchildren. In this analysis, for each child, we calculate the maximum years of education across all of their older generation household members.

The correlations between this new measure and our original measure of the intergenerational correlation, intergenerational slope, and upward mobility are very high – 0.93, 0.99, and 0.99, respectively. Given this, it is unsurprising that our main results look almost identical when we repeat our regressions using the new measures as our outcomes (Table C1). One attractive feature of these new measures is that we are able to link a larger share of children to someone from the older generation, as shown in Figure C1: 88% of 15 to 18-year-olds in 2015 (compared to the 80% of 15 to 18-year-olds we were able to link to their parents in 2015). It is therefore reassuring that we come to similar conclusions with this wider coverage.

Table C1: Effects of 4Ps Exposure on Education Mobility (Using Alternative Linking Strategy)

	(1) Intergen. Correlation (Older Generation)	(2) Intergen. Slope (Older Generation)	(3) Upward Mobility (Older Generation)	(4) Intergen. Correlation (Older Generation)	(5) Intergen. Slope (Older Generation)	(6) Upward Mobility (Older Generation)
4Ps Exposure	-0.15	-0.41**	0.21**	-0.02	-0.28***	0.19*
	(0.09)	(0.16)	(0.10)	(0.10)	(0.10)	(0.11)
Observations (Unweighted)	13,100	13,100	13,100	13,100	13,100	13,100
Observations (Weighted)	13,948,569	15,958,065	15,958,065	13,948,569	15,958,065	15,958,065
Outcome mean	34.24	22.25	40.38	34.24	22.25	40.38
Additional Controls	None	None	None	Year x Baseline Poverty	Year x Baseline Poverty	Year x Baseline Poverty

Notes: Standard errors (clustered at the municipality level) are in parentheses * p< 0.1, ** p< 0.05, *** p< 0.01. All regressions use the 2010 and 2015 censuses and control for municipality and birth cohort fixed effects. "Year x Baseline Poverty" indicates a 2015 year dummy interacted with dummies for baseline poverty share deciles. The intergenerational correlation and slope are multiplied by 100 for readability.

Figure C1: Share of children with education information from older generation, by age

