DECEMBER 2023

DISCUSSION PAPER SERIES NO. 2023-43

Who Gets Monitored among Philippines' 4Ps Children and Why It Matters for Their Nonmonitored Siblings

Michael R.M. Abrigo, Kean Norbie F. Alicante, and Kris Ann M. Melad

CONTACT US:

RESEARCH INFORMATION DEPARTMENT
Philippine Institute for Development Studies

18th Floor, Three Cyberpod Centris - North Tower
EDSA corner Quezon Avenue, Quezon City, Philippines

publications@pids.gov.ph
(+632) 8877-4000

https://www.pids.gov.ph

Who Gets Monitored among Philippines' 4Ps Children and Why It Matters for Their Nonmonitored Siblings

Michael R.M. Abrigo
Kean Norbie F. Alicante
Kris Ann M. Melad

PHILIPPINE INSTITUTE FOR DEVELOPMENT STUDIES

December 2023

Abstract

Which children benefit from conditional cash transfers (CCT)? Using a sample of poor and near-poor households in the Philippines, we show that children in households that receive cash transfers from 4Ps, the country's flagship anti-poverty program, have parents with relatively low educational attainment. Within CCT-recipient households, children of heads are more likely to be enrolled for education monitoring. We find no evidence that households select children for education monitoring to maximize 4Ps cash payout. While children's ranking based on birth timing and on an earlier 4Ps prioritization rule predict child monitoring status, these instruments are at best weak, which may effectively limit their use in impact assessments. We confirm earlier findings that 4Ps raise school enrollment on average, which is likely driven by its impact on boys and on older children. We also corroborate earlier results of perverse impacts on non-monitored children that worsen with age, are more severe for boys, and appear to be universal across household compliance types. Contrary to expectations, we show that children in households who select out of 4Ps even when eligible, i.e., never treated, are likely to benefit greatly from the program, while those from households that selects into the program even when ineligible based on proxy means tests, i.e., always treated, are not necessarily better off as a result of the program.

Keywords: poverty, 4Ps, education, marginal treatment effect, Philippines

Table of Contents

1. Introduction 1
2. Study design 3
   2.1. Evaluation framework 3
   2.2. Complier analysis 3
   2.3. Marginal treatment effect 5
3. Data 5
4. Results 6
   4.1. First stage regression 6
   4.2. Complier characteristics 7
   4.3. Marginal treatment effects 12
   4.4. Some qualifications 14
5. Conclusion 21
References 22

List of Tables

Table 1. First stage estimates: *Listahanan* 1 poverty status and 4Ps receipt 8
Table 2. Characteristics by treatment compliance: All children 9
Table 3. Characteristics by treatment compliance: Index and non-4Ps children 10
Table 4. Characteristics by treatment compliance: Non-index and non-4Ps children 11
Table 5. First stage regression: Impact of policy on treatment 20

List of Figures

Figure 1. 4Ps coverage and centered predicted per capita income 7
Figure 2. Marginal treatment effect of 4Ps-receipt on school attendance: All children 13
Figure 3. Marginal treatment effect of 4Ps-receipt on school attendance: Female children 15
Figure 4. Marginal treatment effect of 4Ps-receipt on school attendance: Male children 16
Figure 5. Sensitivity to inclusion of additional control variables: All children 17
Figure 6. Proportion of eligible children monitored by rank order 19

Who Gets Monitored among Philippines' 4Ps Children and Why It Matters for Their Nonmonitored Siblings

Michael R.M. Abrigo☼, Kean Norbie F. Alicante, and Kris Ann M. Melad[1]

1. Introduction

In its earlier years, the Pantawid Pamilyang Pilipino Program (4Ps), the Philippines' flagship anti-poverty conditional cash transfer program, enrolls eligible children for education outcomes monitoring based on some age-based prioritization rule. This was later replaced by an open selection regime to allow parents free hand in choosing which child(ren) to enroll in the 4Ps. To the extent that such selection is not correlated with innate children skills or additional preferential interventions of parents on their children, then simple comparison of child outcomes by 4Ps monitoring and beneficiary status provide unbiased estimates of 4Ps impacts. This may be difficult to defend in practice however as households are theoretically more likely to select children who are more likely to benefit from the program due to their skills, or to select children who may provide the highest 4Ps payout to the household by virtue of their age or education level. This ultimately complicates the evaluation of the impact of 4Ps on children by monitoring status.

Early evidence from past 4Ps evaluations suggest material differences in outcomes between monitored and non-monitored children (e.g. Melad, 2019; Orbeta, 2021; Raitzer, et al., 2021). Except for Raitzer, et al. (2021), previous 4Ps evaluations have not corrected for possible non-random selection of 4Ps children by monitoring status. Raitzer, et al. (2021), on the other hand, used instrumental variable regression using several selection rules as instruments to correct for possible selection, although the strength of the correlation between the excluded instrument and monitoring status, i.e., first stage regression statistics, had not been presented.

In this study, we revisit the potential heterogeneous impacts of the 4Ps on children's school attendance propensity by distinguishing between monitored and non-monitored children in 4Ps-recipient households. Unlike previous studies on 4Ps that only looked at children in complier households, i.e., those who were induced to receiving 4Ps by being tagged as poor in the government's proxy means test, we also estimate the marginal treatment effects for children in never treated and in always treated households. Further, we characterize children from these households by comparing the "average" child across household compliance and treatment types using the methodology proposed by Kowalski (2016) following earlier expositions by Heckman and Vytlacil (1999) and Vytlacil (2002). This allows us to identify who selects into and out of the program, and how the program (could) have affected their children.

Who gets monitored has important practical and theoretical implications. On the one hand, past studies have shown that the impact of conditional cash transfers, particularly in the Philippines, depend on the child's monitoring status (e.g. Melad, 2019; Orbeta, 2021; Raitzer, et al., 2021). Monitored children are likely to benefit from the program while their unmonitored siblings are often negatively impacted, potentially as a result of intra-household optimization or bargaining processes (c.f. Raitzer, et al., 2021). This has clear implications in designing similar programs, especially with regard to optimizing program benefits without harming non-participants.

[1] Fellow II, Research Analyst II and Supervising Research Specialist, respectively, at the Philippine Institute for Development Studies.
☼ Corresponding author

On the other hand, evaluation studies that aim to assess the potential heterogeneous impact of the program by child monitoring status impose different assumptions on the data generating process during estimation. Understanding how households actually select children for monitoring, especially when it is not randomized as in the case of the 4Ps, is therefore crucial for researchers in selecting which empirical strategy may best provide impact estimates with the least bias and with acceptable precision.

The results of our complier analysis show that children with parents that have low educational attainment are more likely to select into the 4Ps. Conversely, those with better educated parents are less likely to do so, even when they are eligible through a proxy means test. However, we document a non-trivial proportion of children in never treated households who have parents with low educational attainment that could have potentially benefited from the program.

We also show that children of household heads are more likely to be selected for education monitoring among 4Ps-recipient households. While earlier-born, and therefore older, children are also more likely to be selected for monitoring, this advantage may not be very dramatic, at least for the sample of children included in our study. Indeed, further probing shows that birth order and rule order rankings are at best weak instruments for household selection of children for education monitoring. We also find no evidence that the average 4Ps household select children to maximize their 4Ps cash payouts.

The results we present here largely confirm the estimates in Orbeta, et al. (2021) that used optimized sample sizes in a continuity-based regression discontinuity (RD) framework. We depart from their strategy in this study by employing a local randomization RD approach to leverage the whole sample that they collected. Similar to their results, we show that older children have benefited more greatly from the 4Ps through a larger increase in their school attendance rates, which we document to be driven mainly by the program's impact on boys. We also confirm the negative effect on school attendance among non-monitored children, which attenuates the average impact of the program. The negative impact on non-monitored children's school attendance worsens with age, is more perverse for boys, and appears to be universal across household compliance types.

It may be reasonable to expect that those who select into the 4Ps, or any intervention for that matter, do so for the benefits that the program may provide. In the same way, we can anticipate that those who select out of the program even when eligible may have limited expected returns from participation. However, we show that the average impact for children in never treated households (i.e., those who will select out of the program regardless of eligibility) may actually be substantial and even larger compared with those for complier households, although the negative impact on non-monitored children persists. The impact on children from always treated households (i.e., those who will select into the program regardless of eligibility), on the other hand, are not better than those from complier households. This may be an interesting result that needs further probing.

The rest of the study is organized as follows. In the next section, we discuss the empirical strategy that we employ in our estimation. We underscore the assumptions required, and highlight insights from the literature. In Section 3, we then briefly describe the data that we used. This is followed in Section 4 with a discussion of the results. Finally, in Section 5, we conclude with a brief summary and some implications for policy.

2. Study design

2.1. Evaluation framework

We consider the impact of conditional cash transfers on monitored and unmonitored children in recipient households by applying the latent index model developed in Heckman and Vytlacyl (1999) and Vytlacyl (2002) and later expanded to discrete instruments (Kowalski, 2016; Brinch, et al., 2017). Suppose households are each assigned to treatment $D \in \{0,1\}$, where $D = 1$ represents a household receiving an intervention. Unlike the setting considered by Heckman and Vytlacil (1999) and Vytlacil (2002), however, households in the treatment group assign monitoring status $M \in \{0,1\}$ to a subset of its members. That is, treatment M is nested under D with the assumption that those in the control group all receive $M = 0$. We refer to individuals assigned $M = 1$ as index members, while those with $M = 0$ as non-index members. In our case, the benefits received by a treated household depends on the performance of index children, i.e., school attendance.

Let $Y(D, M)$ denote the potential outcome of an individual based on the treatment bundle that it may receive, $(D, M) = \{(0,0), (1,0), (1,1)\}$. In an ideal setting, all three potential outcomes may be observed, and the impact of the intervention may be readily assessed by forming contrasts of the potential outcomes. However, in the real world the econometrician only observes one outcome:

$$Y = (1 - D)Y_c + D(1 - M)Y_u + DMY_m. \tag{1}$$

Following the previous literature, a household selects into treatment D if the net benefit of receiving the treatment, $H = p_z - U$, is at least equal to some threshold, which we arbitrarily set to zero. Without loss of generality, we assume that benefits p_z and costs U are normalized to range between zero and one, with U coming from a continuous uniform probability distribution. As may be standard in the literature, p_z depends on a binary instrumental variable (IV) $Z \in \{0,1\}$, where we assume $p_1 > p_0$ and $(Y_c, Y_u, Y_m, U) \perp Z$.

Among treated households, members are assigned an index status based on a latent variable, $I = q_w - R$, analogous to H above. We assume that q_w and R are also defined on the unit interval, with R also drawn from a continuous uniform distribution. When an individual-level IV, $Q \in \{0,1\}$, where $q_1 > q_0$ and $(Y_u, Y_m, R) \perp Q$, is available, then it can be used to estimate the impact of child monitoring status among treated households. However, there may be instances when such IV is not available.

Suppose instead that $(Y_u, Y_m, R) \perp M$ are independent conditional on household and child characteristics X. Then the program impact by child monitoring status may be estimated by using ZM to instrument for DM. Alternatively, the implied IV model using Z as instrument may be implemented using the sample of non-4Ps children with either monitored or non-monitored children to estimate the impact by index status.

2.2. Complier analysis

In any public intervention, it is important to understand who selects into receiving treatment. In the case of 4Ps, for example, it may be policy-relevant to know whether households with low human capital opt into the program before households with higher educational attainment. Understanding how 4Ps households prioritize children for monitoring may be important in designing countervailing measures against any potential adverse impacts. In this section, we apply insights in the literature on estimating average observable characteristics of study subpopulations (e.g. Katz, et. al., 2001; Abadie, 2003; Kowalski, 2016) to the nested design we presented previously.

By construction, households with $0 \leq U < p_0$ will always select into treatment. In our context, these are always takers who will enroll into 4Ps regardless of their household proxy means score. This includes households who are originally ineligible for 4Ps based on the *Listahanan*, but have had sought ways, such as through household reassessment, to qualify and receive 4Ps benefits. That is, these are households with $D = 1$, i.e., 4Ps recipient, and $Z = 0$, i.e., with proxy means score above the poverty threshold, in the data. It is important to note that there are also always takers with $D = 1$ and $Z = 1$, but they cannot readily be observed in the data.

At the other end of the distribution are households with $p_1 \leq U \leq 1$ that will never opt in for treatment regardless of their eligibility, i.e., never takers. These are households whose costs of being in the program are at least as high as the benefit that they expect to receive. While there may be no upfront cost to be part of the 4Ps, households are expected to meet program conditionalities to qualify for the 4Ps benefits. Households may incur monetary and psychic costs from complying with program conditionalities, such as sending children to school and attending monthly seminars. Households who had foregone enrollment in 4Ps $(D = 0)$ despite being eligible $(Z = 1)$ are included in this group. There are also those who are not eligible $(Z = 0)$ but would not opt in anyway.

In the middle of the distribution are compliers who have $p_0 \leq U < p_1$. These are households who opt into 4Ps $(D = 1)$ if they are eligible based on the *Listahanan* $(Z = 1)$, but do not $(D = 0)$ if they are not originally eligible $(Z = 0)$. Unlike other subgroups, however, the compliers cannot be readily observed in the data since there are always taker households with $(Z = 1)$ and never taker households with $(Z = 0)$.

Following Kowalski (2016), it can be shown that the average characteristics of always takers (AT), untreated compliers (UC), treated compliers (TC) and never takers (NT) may be calculated based on a linear regression of characteristics X on Z and D, and of D on Z. As shown by Kowalski (2016), the average characteristics of untreated and treated compliers may be respectively calculated from

$$\mu_x^{UC} \equiv E[X|D = 0, p_0 \leq U < p_1] = \frac{(1 - p_0)E[X|D = 0, Z = 0] - (1 - p_1)E[X|D = 0, Z = 1]}{(p_1 - p_0)}; \quad (2)$$

$$\mu_x^{TC} \equiv E[X|D = 1, p_0 \leq U < p_1] = \frac{p_1 E[X|D = 1, Z = 1] - p_0 E[X|D = 1, Z = 0]}{(p_1 - p_0)}. \quad (3)$$

The above moments, in turn, have direct counterparts from the following linear regressions:

$$X_{ij} = \lambda_0 + \lambda_d D_j + \lambda_z Z_j + \lambda_{dz} D_j Z_j + \epsilon_{ij}; \quad (4)$$

$$D_{ij} = p_0 + (p_1 - p_0) Z_j + \zeta_{ij}, \quad (5)$$

where i and j identify individuals and households, respectively, while ϵ_{ij} and ζ_{ij} are the model residuals with the usual desired properties. The average characteristic of always takers and never takers correspond to $\mu_x^{AT} = \lambda_0 + \lambda_d$ and $\mu_x^{NT} = \lambda_0 + \lambda_z$, respectively.

2.3. Marginal treatment effect

As shown by Kowalski (2016), the marginal treatment effect by compliance group may be calculated from the average characteristics of always treated, untreated compliers, treated compliers and never treated based on the regression models in (4) and (5), but using program outcomes instead of pre-treatment characteristics as follows:

$$E[Y_1 - Y_0 | 0 \leq U < p_0] = \mu_Y^{AT} - \mu_Y^{UC} \qquad \text{(Always takers)} \qquad (6)$$

$$E[Y_1 - Y_0 | p_0 \leq U < p_1] = \mu_Y^{TC} - \mu_Y^{UC} \qquad \text{(Compliers)} \qquad (7)$$

$$E[Y_1 - Y_0 | p_1 \leq U < 1] = \mu_Y^{TC} - \mu_Y^{NT} \qquad \text{(Never takers)} \qquad (8)$$

The regression in (4) may also be used to test for selection into treatment or for heterogeneous treatment effect by compliance group, depending on the variable being studied. As shown in Kowalski (2016), the marginal treatment effect is flat in p whenever $\lambda_{dz} = 0$, suggesting no selection or treatment effect heterogeneity.

3. Data

We apply the complier analysis and marginal treatment effect estimation using data from the 4Ps third-wave impact evaluation (4Ps-3IE) study (Orbeta, et al., 2021). The 4Ps-3IE survey covers 6,775 4Ps and non-4Ps households who were selected based on the distance of the household's predicted per capita income in *Listahanan* 1, the country's household targeting system, relative to the official provincial poverty line. Following the regression discontinuity evaluation design employed in the 4Ps-3IE study, households with predicted per capita incomes closer to the poverty line were more likely to be invited to answer the survey. Among 4Ps households, only those that had been enrolled in the program between 2008 and 2014 were included. The household survey was implemented between November 2017 and January 2018, which collected information on households' socio-economic characteristics and participation in government programs, female reproductive history and contraceptives use, and child education and health outcomes, among others.

In line with the evaluation design of the 4Ps-3IE study, we use the household poverty status in *Listahanan* 1 as excluded instrument for 4Ps-receipt among households. Unlike in Orbeta, et al. (2021), however, we employ a local randomization approach instead of the more popular continuity approach in the regression discontinuity design. As shown in Abrigo, Astilla-Magoncia, Tam and Yee (2022), the marginal and joint distributions of household characteristics[2] of *Listahanan* 1-poor and near-poor household included in the 4Ps-3IE are statistically indistinguishable from each other, which supports the local randomization-based inference. This also allows us to use a more expansive sample compared with those employed in Orbeta, et al. (2021).

[2] In their balance tests, Abrigo, Astilla-Magoncia, Tam and Yee (2022) excluded variables used to predict household per capita income in the *Listahanan* 1 proxy means model.

In our complier analysis, we focus on household- and child-specific characteristics that are likely to be predetermined relative to the program, such as parents' education, age, employment and number of children, and children's birth order, age, sex and parenthood. On the other hand, we estimate marginal treatment effects of 4Ps- receipt on child school attendance by age and sex. We control for household and the remaining child characteristics (i.e., birth order and parenthood) by including them as additional covariates in estimating equations (4) and (5). We center these additional covariates relative to their mean in order to recover p_0 and λ_0 relative to the average child.

We limit our analysis to children aged 6 to 19 years at the time of the survey to allow us to estimate the potential moderating effects of child monitoring status. In the 4Ps, education grants are given to households for up to three children who are monitored for the program. Until early 2015, children are selected for monitoring based on a program rule that prioritized children aged 6 to 14 years relative to those 5 years old or younger, with children closer to age 6 given higher importance in the selection. This was eventually replaced by an "open" enrollment, wherein households elect the children they want to be included for education monitoring.[3] We use the child monitoring status reported by parents/caregivers during the survey. This may not necessarily be the child's official status reported and actually monitored in the program.

4. Results

4.1. First stage regression

As may be obvious from (2) and (3), a critical assumption in our estimation is that $(p_1 - p_0)$ is not equal to zero. That is, the instrument is relevant in predicting treatment assignment. We show this visually in Figure 1 where we plot the proportion of children living in households that receive 4Ps benefits against predicted per capita household income centered on the province's poverty threshold. Children living in poor households, i.e. with negative centered per capita income, were about 70-percentage points more likely to be living in a 4Ps recipient household compared with children living in near-poor households.

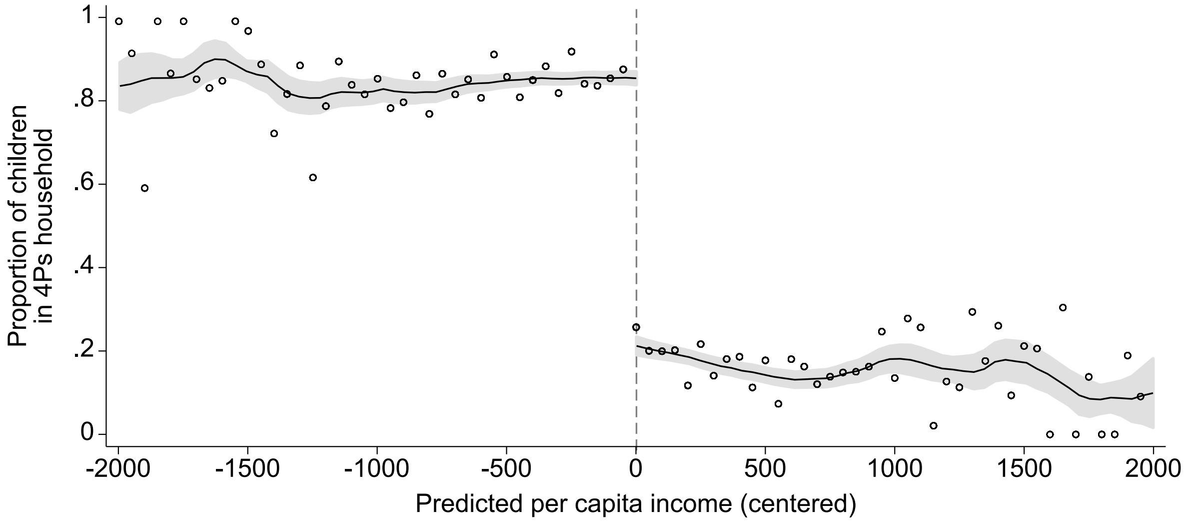
This is confirmed in Table 1 that shows linear regression estimates for equation (4), which models 4Ps-receipt among children aged 6 to 19 years in the 4Ps-IE3 survey as a function of household poverty status and additional control variables. We present the intercept, which corresponds to the proportion of always takers, p_0, and the coefficient for poverty status, which corresponds to the proportion of compliers, $p_1 - p_0$, in the data. The estimates appear to be fairly robust to inclusion of additional explanatory variables.

The estimates show that about a fifth of children in the 4Ps-IE3 survey live in households that were originally not eligible for 4Ps through their proxy means score, but were enrolled in the program through other mechanisms, e.g. through reassessment. About seven in ten children, on the other hand, were in complier households, i.e., enrolled in 4Ps if eligible and not if otherwise. This leaves about one in ten children living in households who would not enroll in 4Ps even if they are eligible by having scores below the provincial poverty threshold.[4]

[3] See Orbeta, et al. (2021) for a more detailed description of the program and the 4Ps-IE3 survey and data.

[4] This may theoretically also include households who were initially in the program but have exited or have waived their eligibility at the time of the survey. Households may exit the 4Ps for several reasons, such as failing to comply with program conditionalities, having children aged out of the program, and voluntarily waiving eligibility, among others.

Figure 1. 4Ps coverage and centered predicted per capita income



Source: Authors' calculations based on 4Ps-IE3 survey data from Orbeta, et al. (2021). Note: For visual presentation, we only included the sample of children aged 6 to 19 years whose household predicted per capita income are within PHP2,000 of the provincial poverty line used in *Listahanan 1*.

In Table 1, we also present statistical tests to measure our instrument's strength in predicting 4Ps-receipt. It has been well-documented that employing weak instruments result in various estimation issues, such as biased impact estimates and poor statistical test coverage (Bound, et al., 1995; Stock and Yogo, 2005). These concerns are generally not alleviated even as the sample size grows indefinitely. The F statistics for tests for weak instrument and for under-identification commonly used in instrumental variable regressions suggest that our estimation does not suffer from such issues.

4.2. Complier characteristics

Table 2 presents the selected characteristics of children across different household compliance and treatment groups: never treated, untreated compliers, treated compliers, and always treated. The estimates show that sampled children from these groups generally have similar predicted per capita income, and age and employment status of parents. The number of children and the children's sex ratios in these households are also roughly quite similar. These children are also of about the same age and birth order.

However, they differ on their parents' highest educational attainment. As may be expected, children with low educational attainment parents are more likely to select into 4Ps. Among always treated households, only five percent of children have fathers who have had reached college. Only ten percent of children in this group have mothers who have had reached college. This is significantly lower compared with children from other groups, especially those from never treated households, wherein almost 20- and 30-percent of children have fathers and mothers who have had respectively reached college.

Table 1. First stage estimates: *Listahanan* 1 poverty status and 4Ps receipt

	(1)	(2)	(3)	(4)
Poor = 1, $p_1 - p_0$	0.679***	0.677***	0.709***	0.714***
	(0.014)	(0.017)	(0.019)	(0.020)
Constant, p_0	0.177***	0.184***	0.184***	0.185***
	(0.013)	(0.015)	(0.019)	(0.014)
PMT score		Yes	Yes	Yes
Household characteristics			Yes	Yes
Parent's characteristics			Yes	Yes
Child characteristics			Yes	Yes
Barangay fixed effects				Yes
Observations	12,052	12,052	7,870	7,870
Kleibergen-Paap weak identification F	2,439	1,588	1,336	1,323
Kleibergen-Paap under-identification F	159	125	118	119
Adjusted R-sq.	0.462	0.463	0.551	0.579
Bayesian Information Criterion	9,950	9,965	5,165	4,102

Source: Authors' calculations based on 4Ps-IE3 survey data from Orbeta, et al. (2021). Note: The sample include children aged 6 to 19 years at the time of the survey. Household characteristics include number of children aged below 20 years living in the household. Parents' characteristics include both parents' age, educational attainment and employment status. Child characteristics include child's sex, age, and parenthood. All variables are centered relative to the mean. *, **, and *** denote statistical significance at the 10-, 5-, and 1-percent alpha levels, respectively. PMT – Proxy means test, i.e., *Listahanan* 1 predicted household per capita income.

While children from never treated households appear to have a higher propensity of having parents who have had reached college, a significant proportion have parents with relatively low educational attainment. Similar with those from always treated households, about ten percent of children from never treated households have fathers who have had not reached at least basic education compared with only three to four percent for children from complier households. About five percent of children in never treated households have mothers who have had not reached at least basic education, while virtually all mothers in complier households have had at least attended some primary education.

In addition to parents' education, children across compliance and treatment groups also appear to differ in their household structure. More specifically, children from complier households are more likely to be children of the household head. About 80- to 85-percent of children in complier households are direct progenies of the household head, compared with only 61 percent in never treated households and 66 percent in always treated households.

In Tables 3 and 4, we subset the children in 4Ps households by index status to compare the characteristics of monitored and non-monitored children. It shows that monitored children have a slight advantage in terms of birth order and age when compared with their non-monitored siblings, but the differences are rather small. Female children appear to have no advantage over their male siblings in terms of index status.

Table 2. Characteristics by treatment compliance: All children

	Untreated		Treated		All sample
	Never treated	Untreated compliers	Treated compliers	Always treated	
Father, age (years)	43.61	43.20	42.98	42.67	43.10
	(0.55)	(0.33)	(0.36)	(0.49)	(0.09)
Father, reached primary school (=1)	0.91	0.96	0.97	0.89	0.94
	(0.02)	(0.01)	(0.01)	(0.02)	(0.00)
Father, reached high school (=1)	0.66	0.62	0.63	0.50	0.63
	(0.03)	(0.02)	(0.02)	(0.03)	(0.01)
Father, reached college (=1)	0.19	0.11	0.13	0.05	0.12
	(0.03)	(0.02)	(0.01)	(0.02)	(0.00)
Father, employed (=1)	0.86	0.90	0.90	0.88	0.89
	(0.02)	(0.01)	(0.01)	(0.02)	(0.00)
Mother, age (years)	40.43	40.06	39.93	39.81	40.05
	(0.49)	(0.30)	(0.33)	(0.45)	(0.08)
Mother, reached primary school (=1)	0.95	0.99	1.00	0.93	0.98
	(0.01)	(0.01)	(0.01)	(0.02)	(0.00)
Mother, reached high school (=1)	0.73	0.76	0.75	0.63	0.75
	(0.03)	(0.02)	(0.02)	(0.03)	(0.00)
Mother, reached college (=1)	0.29	0.16	0.18	0.10	0.18
	(0.03)	(0.02)	(0.02)	(0.02)	(0.00)
Mother, employed (=1)	0.48	0.46	0.45	0.46	0.48
	(0.03)	(0.02)	(0.02)	(0.03)	(0.00)
Child, Birth order (rank)	1.90	2.02	1.88	2.00	1.90
	(0.02)	(0.07)	(0.03)	(0.05)	(0.01)
Child, Age (years)	12.40	12.26	12.34	11.92	12.21
	(0.16)	(0.10)	(0.10)	(0.14)	(0.03)
Child, Female (=1)	0.48	0.48	0.47	0.48	0.48
	(0.02)	(0.01)	(0.01)	(0.02)	(0.00)
Child, Offspring of head (=1)	0.61	0.80	0.85	0.66	0.75
	(0.02)	(0.01)	(0.02)	(0.03)	(0.00)
Number of children (count)	2.20	2.48	2.58	3.14	2.57
	(0.07)	(0.05)	(0.06)	(0.09)	(0.01)
PMT score (PhP)	14,909	14,900	14,759	15,063	14,951
	(91.1)	(66.8)	(83.0)	(135.0)	(18.6)

Source: Authors' calculations based on 4Ps-IE3 survey data from Orbeta, et al. (2021). Note: The figures show the average of each variable for sampled children aged 6 to 19 at the time of the survey following the exposition of Kowalski (2016) and using regression estimates for equations (4) and (5). Figures in parentheses are heteroskedasticity-robust standard errors clustered at the household level.

Table 3. Characteristics by treatment compliance: Index and non-4Ps children

	Untreated		Treated		All sample
	Never treated	Untreated compliers	Treated compliers	Always treated	
Father, age (years)	43.56	43.20	43.03	42.54	43.10
	(0.55)	(0.35)	(0.34)	(0.53)	(0.09)
Father, reached primary school (=1)	0.91	0.96	0.96	0.90	0.95
	(0.02)	(0.01)	(0.01)	(0.02)	(0.00)
Father, reached high school (=1)	0.66	0.62	0.63	0.50	0.63
	(0.03)	(0.02)	(0.02)	(0.03)	(0.01)
Father, reached college (=1)	0.19	0.11	0.13	0.06	0.12
	(0.03)	(0.02)	(0.01)	(0.02)	(0.00)
Father, employed (=1)	0.86	0.90	0.90	0.89	0.89
	(0.02)	(0.01)	(0.01)	(0.02)	(0.00)
Mother, age (years)	40.41	40.07	40.07	39.99	40.10
	(0.49)	(0.32)	(0.31)	(0.51)	(0.09)
Mother, reached primary school (=1)	0.95	0.99	1.00	0.95	0.98
	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)
Mother, reached high school (=1)	0.73	0.76	0.75	0.64	0.76
	(0.03)	(0.02)	(0.02)	(0.03)	(0.00)
Mother, reached college (=1)	0.29	0.15	0.18	0.10	0.19
	(0.03)	(0.02)	(0.02)	(0.02)	(0.00)
Mother, employed (=1)	0.48	0.46	0.44	0.47	0.48
	(0.03)	(0.02)	(0.02)	(0.03)	(0.01)
Child, Birth order (rank)	2.03	2.06	1.82	1.95	1.92
	(0.05)	(0.04)	(0.03)	(0.05)	(0.01)
Child, Age (years)	12.44	12.28	12.56	12.02	12.30
	(0.16)	(0.10)	(0.10)	(0.15)	(0.04)
Child, Female (=1)	0.48	0.48	0.47	0.50	0.48
	(0.02)	(0.01)	(0.01)	(0.02)	(0.00)
Child, Offspring of head (=1)	0.61	0.80	0.89	0.71	0.77
	(0.02)	(0.02)	(0.01)	(0.03)	(0.00)
Number of children (count)	2.20	2.49	2.45	2.93	2.46
	(0.07)	(0.06)	(0.05)	(0.09)	(0.01)
PMT score (PhP)	14,911	14,900	14,772	15,069	15,005
	(92.1)	(71.1)	(75.4)	(139.7)	(20.7)

Source: Authors' calculations based on 4Ps-IE3 survey data from Orbeta, et al. (2021). Note: The figures show the average of each variable for sampled children aged 6 to 19 at the time of the survey following the exposition of Kowalski (2016) and using regression estimates for equations (4) and (5). Figures in parentheses are heteroskedasticity-robust standard errors clustered at the household level.

Table 4. Characteristics by treatment compliance: Non-index and non-4Ps children

	Untreated		Treated		All sample
	Never treated	Untreated compliers	Treated compliers	Always treated	
Father, age (years)	43.74	42.95	42.70	43.07	43.11
	(0.60)	(0.62)	(0.53)	(0.72)	(0.12)
Father, reached primary school (=1)	0.92	0.97	0.95	0.84	0.94
	(0.02)	(0.02)	(0.02)	(0.04)	(0.00)
Father, reached high school (=1)	0.67	0.58	0.61	0.50	0.64
	(0.03)	(0.04)	(0.03)	(0.05)	(0.01)
Father, reached college (=1)	0.18	0.08	0.09	0.03	0.13
	(0.03)	(0.03)	(0.02)	(0.02)	(0.00)
Father, employed (=1)	0.87	0.92	0.90	0.86	0.89
	(0.03)	(0.03)	(0.02)	(0.03)	(0.00)
Mother, age (years)	40.30	40.05	39.18	39.38	39.96
	(0.54)	(0.56)	(0.51)	(0.66)	(0.11)
Mother, reached primary school (=1)	0.95	1.00	1.00	0.87	0.98
	(0.01)	(0.01)	(0.01)	(0.04)	(0.00)
Mother, reached high school (=1)	0.75	0.77	0.73	0.60	0.76
	(0.03)	(0.03)	(0.03)	(0.05)	(0.01)
Mother, reached college (=1)	0.30	0.09	0.15	0.10	0.20
	(0.03)	(0.03)	(0.02)	(0.03)	(0.01)
Mother, employed (=1)	0.48	0.44	0.46	0.42	0.49
	(0.03)	(0.04)	(0.03)	(0.05)	(0.01)
Child, Birth order (rank)	2.01	2.11	2.12	2.14	2.03
	(0.06)	(0.07)	(0.05)	(0.09)	(0.01)
Child, Age (years)	12.31	12.27	11.58	11.73	12.12
	(0.19)	(0.19)	(0.19)	(0.28)	(0.05)
Child, Female (=1)	0.48	0.47	0.48	0.45	0.48
	(0.02)	(0.02)	(0.02)	(0.03)	(0.01)
Child, Offspring of head (=1)	0.59	0.89	0.68	0.55	0.71
	(0.03)	(0.03)	(0.02)	(0.04)	(0.01)
Number of children (count)	2.19	2.61	3.15	3.62	2.57
	(0.08)	(0.09)	(0.08)	(0.13)	(0.01)
PMT score (PhP)	14,971	14,856	14,881	15,051	15,208
	(104.1)	(119.9)	(103.5)	(182.2)	(24.4)

Source: Authors' calculations based on 4Ps-IE3 survey data from Orbeta, et al. (2021). Note: The figures show the average of each variable for sampled children aged 6 to 19 at the time of the survey following the exposition of Kowalski (2016) and using regression estimates for equations (4) and (5). Figures in parentheses are heteroskedasticity-robust standard errors clustered at the household level.

Monitored and non-monitored children differ in their direct progenitor however. About five in every ten non-index children in always treated households and three in every ten in treated complier households are direct progenies of household members except of the household head. This is in contrast with index children, wherein only 29 percent in always treated household and 11 percent in treated complier households are children of non-household head members. Further inspection of the data reveals that these non-head progenies are predominantly grandchildren (76%) or nephews/nieces (11%) of the reported household head, although there are also a significant proportion of non-relatives (7%) and siblings/siblings-in-law (5%).

4.3. Marginal treatment effects

Figure 2 presents the marginal treatment effect of household receipt of 4Ps on children's propensity of attending school. The estimates for compliers should be equivalent to the IV-based estimates in Orbeta, et al. (2021) using the whole sample of children in the 4Ps-IE3 survey. In addition, however, we also include the effect of 4Ps receipt on children from never treated and always treated households.

By and large, our results are in line with those in Orbeta, et al. (2021). That is, the average impact on children from complier households vary by age group (see Figure 2, Panel A). Among primary school-aged children, the effect of 4Ps on average are not economically and statistically significant, which may likely be due to the already near-universal school attendance in this age group. However, among older children, the effects of 4Ps are quite substantial – raising enrollment among aged 12 to 14 years by 2.6 percentage points, and among aged 15 to 17 years by 4.6 percentage points.

Similar to Orbeta, et al. (2021), we also find substantial disparity in impacts among siblings with different monitoring status, especially among older children. Among those aged 15 to 17 years, for example, 4Ps increased school enrollment of index children by 8.2 percentage points on average, while it depresses those for non-index children by 28.8 percentage points.

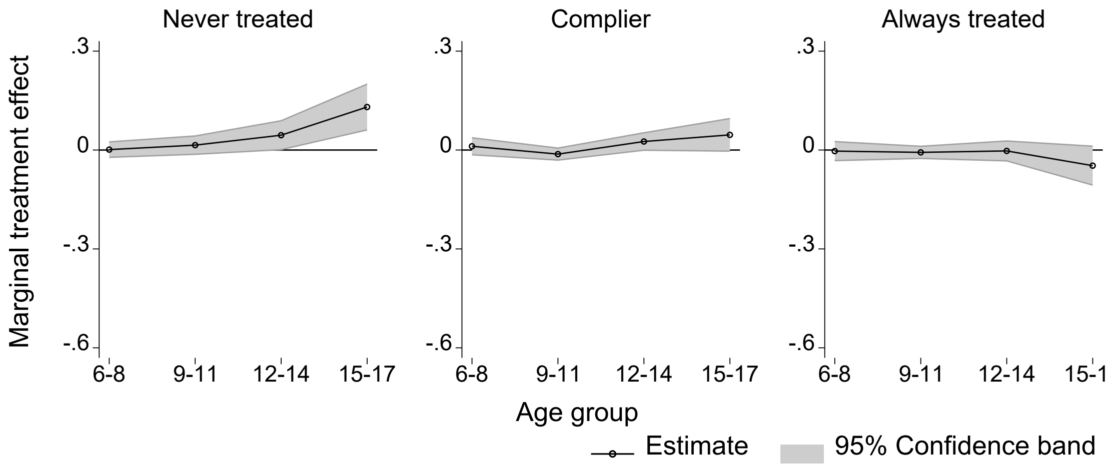
The estimates in Figure 2 also shows interesting results for children from always treated and never treated households in addition to those that have already been documented for children from complier households. First, the impact on children from always treated households are not necessarily the highest. This may be contrary to expectations given their parents' low educational endowment. Visual inspection of the average impact on all 4Ps children (Panel A) and on monitored 4Ps children (Panel B) from always treated households (Column 3) show that the estimates are not statistically significant from those from complier households.

Second, children from never treated households may benefit greatly from actually enrolling in the 4Ps as it could raise school enrollment among aged 12 to 14 years by 4.5 percentage points and among those aged 15 to 17 years by 13.1 percentage points on average. The impact on monitored children are much more pronounced at 5.9- and 18.4-percentage points for the same age groups, respectively.

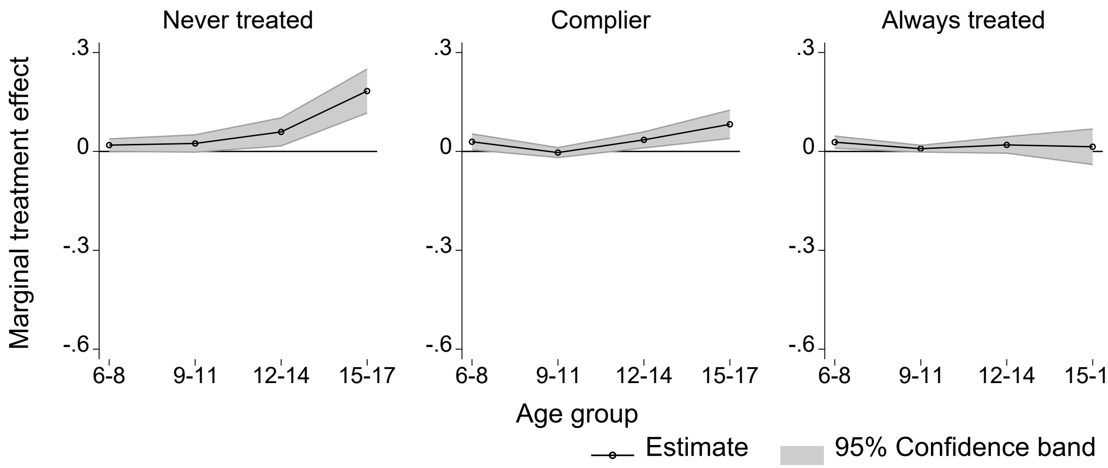
Finally, the impact of 4Ps on non-monitored children appear to be universally negative across the different households by compliance type, and qualitatively stronger among older children.

Figure 2. Marginal treatment effect of 4Ps-receipt on school attendance: All children

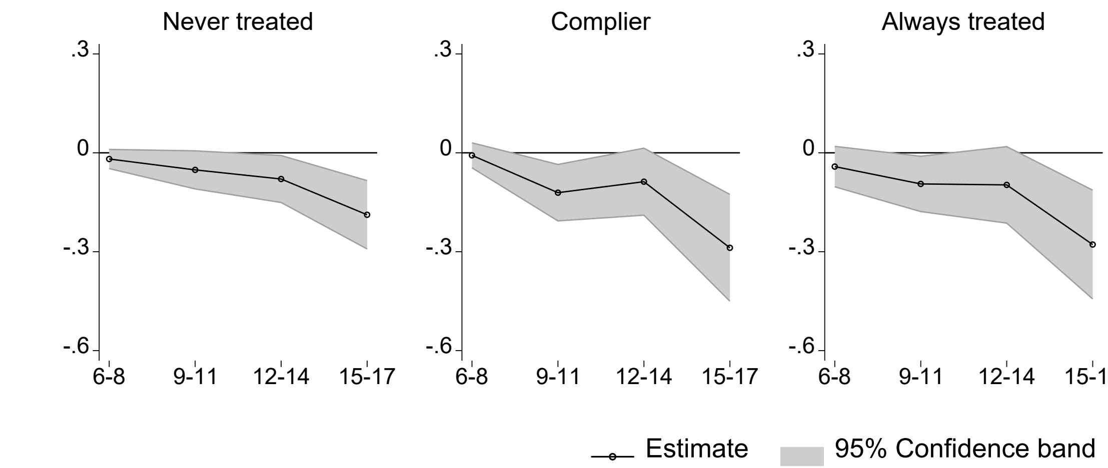
A. All 4Ps children



B. Monitored 4Ps children



C. Non-monitored 4Ps children



Source: Authors' calculations.

Further disaggregation by child's sex suggest that the impact of 4Ps on school enrollment are likely driven by its impact on boys (see Figures 3 and 4). While the impacts are not significantly different between boys and girls when considering all 4Ps children together, the impact on school enrollment among monitored boys are statistically higher compared with those for girls, but much worse among non-monitored boys relative to girls, especially at older ages. This is particularly true for children from never treated and from complier households.

Among monitored children aged 15 to 17 from complier households, for instance, the impacts between boys and girls span about 7.7 percentage points, with boys gaining 11.8 percentage points in school enrollment propensity compared with girls' 4.1 percentage points increase. Among non-monitored children of the same age from complier households, however, enrollment among boys decline by 45.5 percentage points compared with a drop of only 16.9 percentage points among girls from the same age group.

4.4. Some qualifications

A critical assumption in our marginal treatment effect estimates is that the selection of child index status among 4Ps households are independent of potential outcomes once conditioned on observable characteristics. Without adjusting for such non-random selection could result in biased in impact estimates, especially between monitored and non-monitored children. A utility maximizing household, for example, will likely enlist children from whom household gains may be highest, such as selecting children who are more likely to succeed in schooling due to their skills. Alternatively, a household may choose children whose inclusion are more likely to result in higher (discounted) lifetime 4Ps payout to the household. This, in turn, may confound estimated impacts when not appropriately accounted.

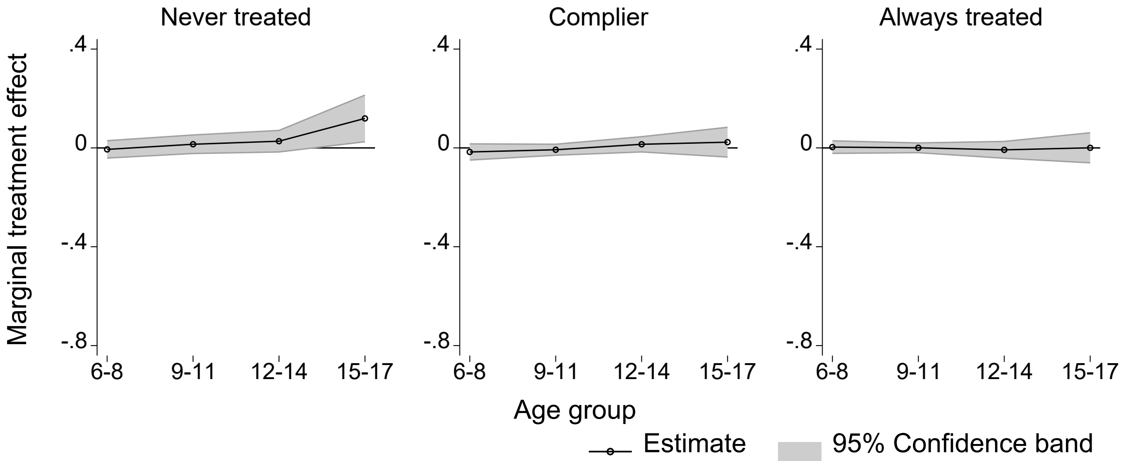
In the early years of the 4Ps, this issue may be less of a concern as selection of children for monitoring status is based on a rule designed to prioritize children aged 6 to 14 years, with children closer to being six years old being of higher priority, over those aged 0 to 5 years for at most three children. To the extent that skills are unrelated with the rule-based rank order of children at the time of 4Ps enrollment, then direct comparison of outcomes by 4Ps monitoring status are free from such selection bias. However, this rule has been superseded by an open enrollment regime starting in 2015 that has allowed 4Ps households to elect the children that they wish to include in the 4Ps as long as these children meet some set eligibility criteria. This introduces the selection issue we identified above.

In order to demonstrate how such unobserved confounding may affect our estimates, we control for household, parental and child characteristics that could potentially affect household decisions regarding child monitoring status in our regression models used to estimate marginal treatment effects. We also included barangay (village) fixed effects to account for potential location-invariant unobserved confounding. The results shown in Figure 5 suggest that our estimates are robust to the inclusion of these additional variables, although the marginal treatment effect estimates are measured with greater uncertainty.[5]

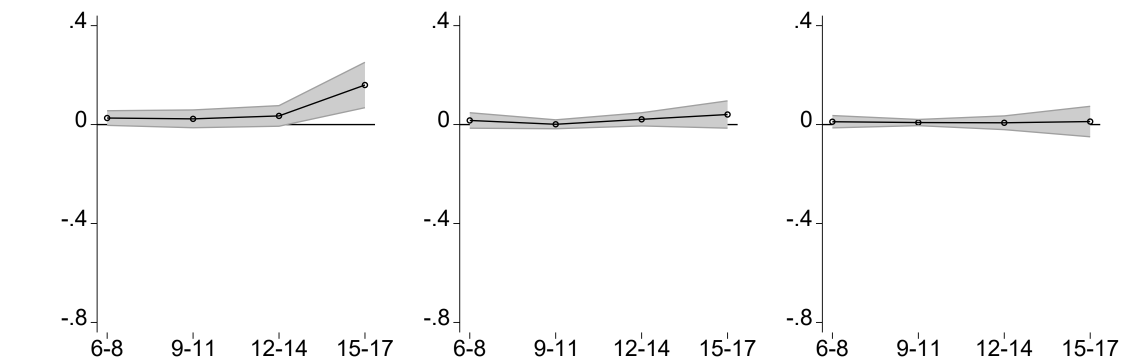
[5] The confidence bands for these additional models are not shown on Figure 5 for clarity in visual exposition but are available from the authors upon request.

Figure 3. Marginal treatment effect of 4Ps-receipt on school attendance: Female children

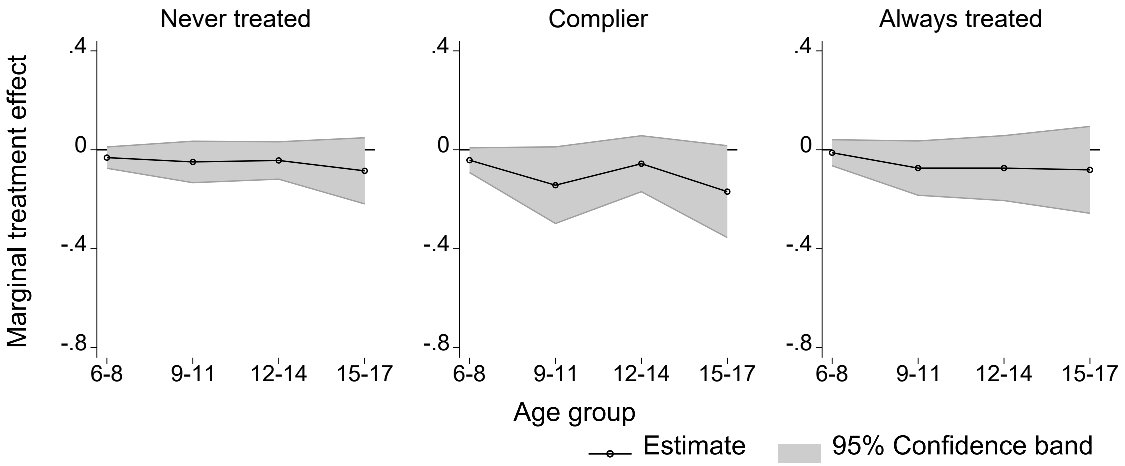
A. All 4Ps children



B. Monitored 4Ps children



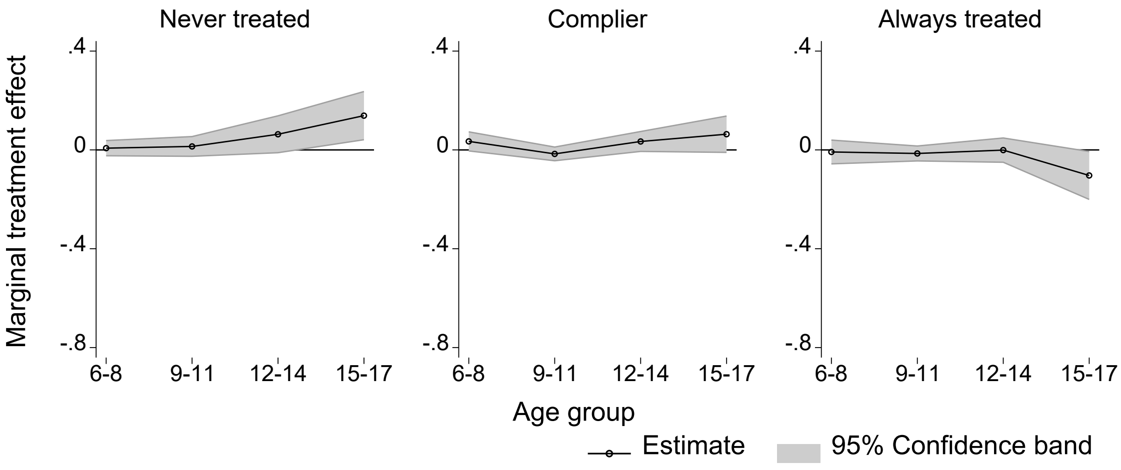
C. Non-monitored 4Ps children



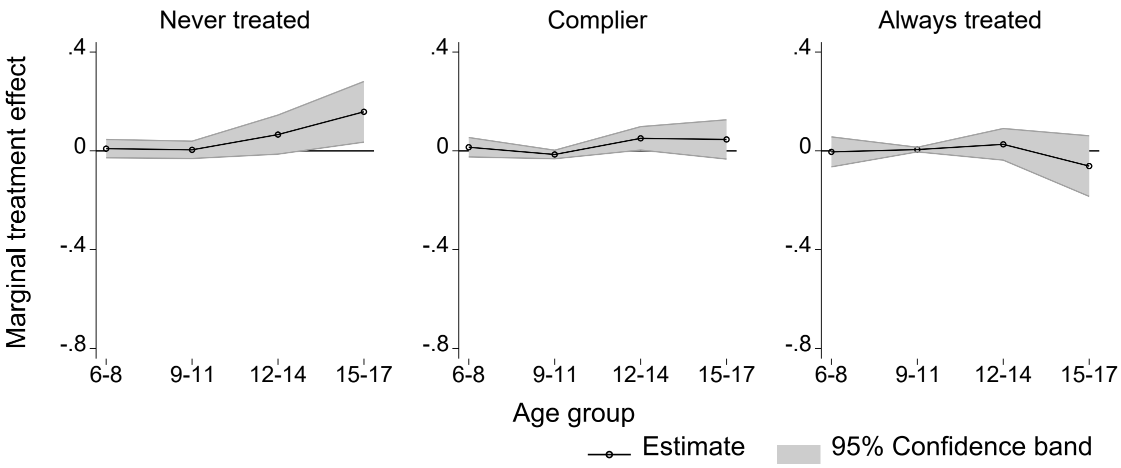
Source: Authors' calculations.

Figure 4. Marginal treatment effect of 4Ps-receipt on school attendance: Male children

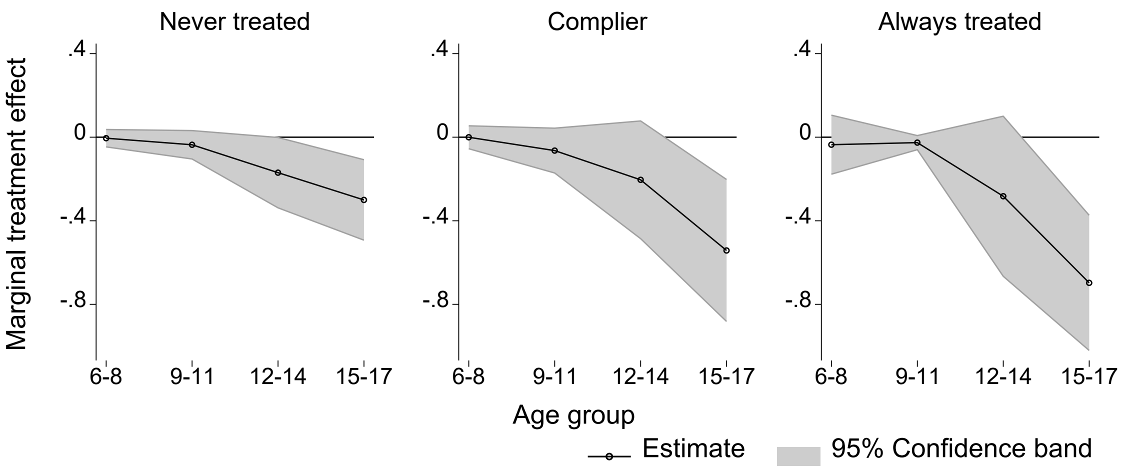
A. All 4Ps children



B. Monitored 4Ps children



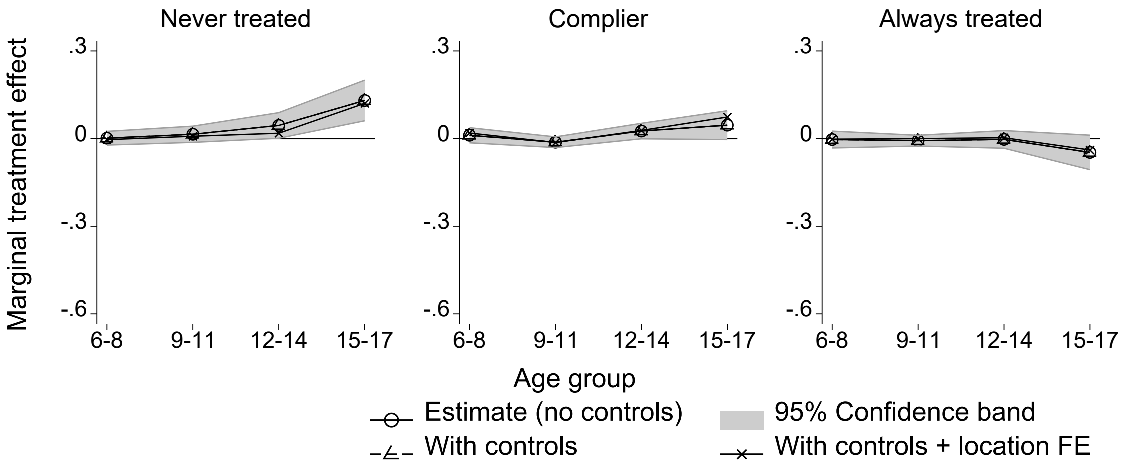
C. Non-monitored 4Ps children



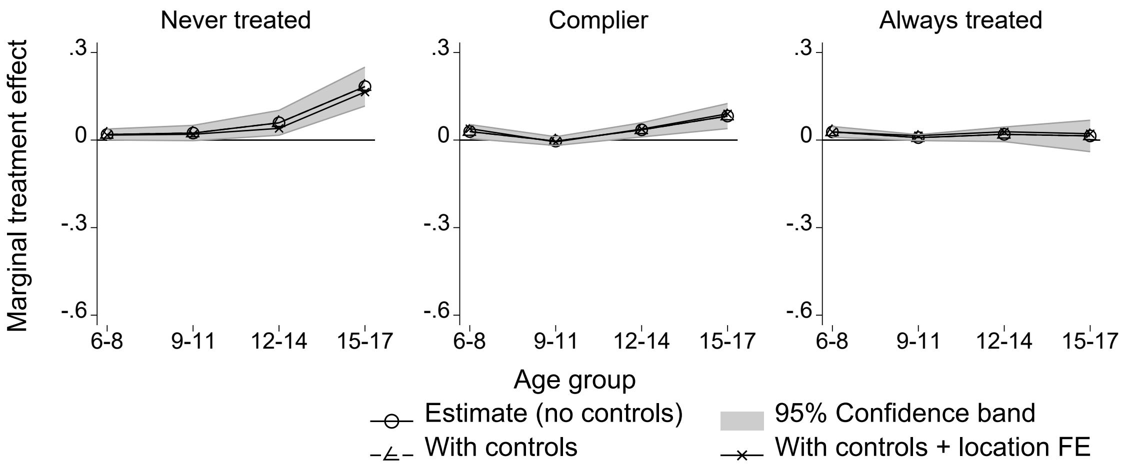
Source: Authors' calculations.

Figure 5. Sensitivity to inclusion of additional control variables: All children

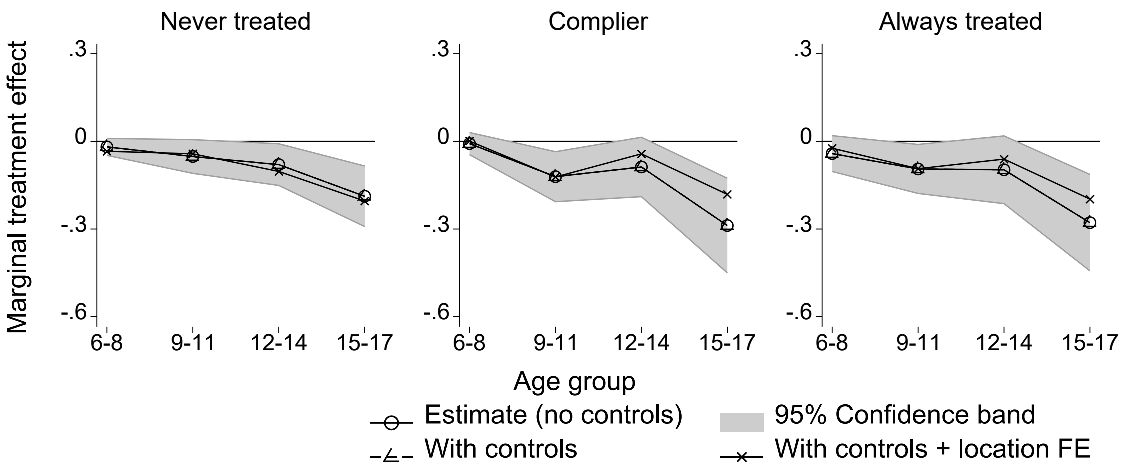
A. All 4Ps children



B. Monitored 4Ps children



C. Non-monitored 4Ps children



Source: Authors' calculations.

While we have controlled for some observable characteristics in our estimates, it may still be possible that there are other variables not observed by the researcher that households consider in choosing children for 4Ps monitoring that could bias our estimates. We look at three such possible selection mechanisms using the sample of children from 4Ps households.

If a household that had been enrolled in the earlier years of the 4Ps did not opt to change their children's monitoring status with the open enrollment introduced in 2015, then we expect that children aged 6 to 14, with preference for those closer to age 6, at the time of the 4Ps enrollment would be more likely to have been selected for monitoring. For households enrolled more recently in the 4Ps and those who opted for open enrollment starting in 2015, then we expect a strategic household to have had selected children who would provide the largest net benefit to the household. If a household aims to maximize lifetime 4Ps payout, for example, then it would likely choose for monitoring their children who are around Grade 5 at the time of 4Ps enrollment to benefit from the seven-year maximum duration of the cash grant and the higher benefits for those enrolled in secondary education based on current program protocols.[6]

Typically, we would also want to incorporate household selection due to perceived or actual cognitive and non-cognitive skills differential among children, however this may be infeasible given the available data. As an alternative, we instead consider birth order effects on 4Ps selection. While who gets to be born first among siblings may be set purely by chance, several studies have documented that first-born children generally have both higher cognitive and non-cognitive abilities (Heiland, 2009; Rohrer, et al., 2015; Black, et al., 2018; Alabassi, et al., 2021), although this may be culturally specific (Botzet, 2020).

Based on the above possible assignment mechanisms, we rank children in 4Ps households in the 4Ps-IE3 survey. These ranks are calculated relative to the year their household was enrolled in 4Ps. We considered all children identified in the household roster, even those yet to be born at the time of 4Ps enrollment. We limit the ranking exercise to children aged 18 years and below at the time of 4Ps enrollment since older children would have been automatically ineligible for the education grant. The birth order and rule order ranks are moderately positively correlated (Pearson's $\rho = 0.68$), while their respective correlations with payout order ranks are both weak ($\rho = -0.09$ and $\rho = -0.23$).

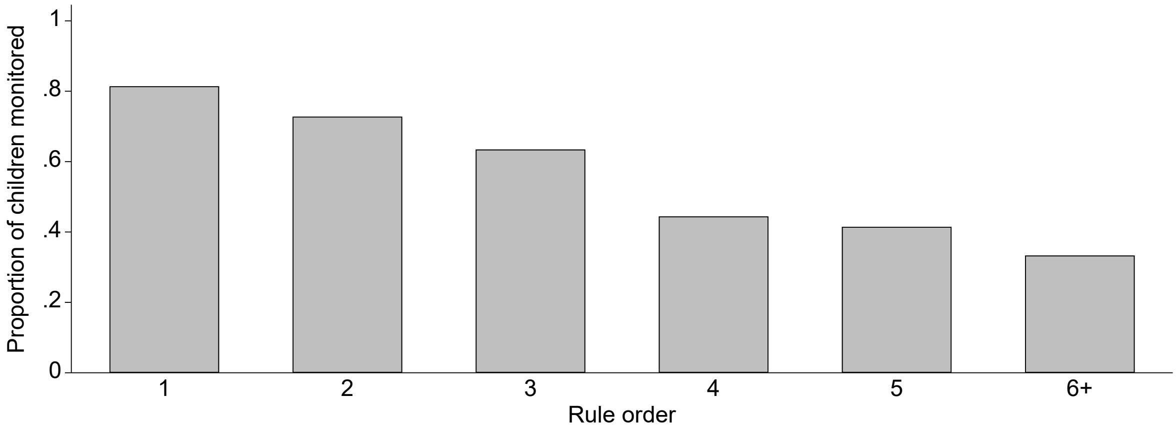
Figure 6 plots the proportion of monitored children by rank based on rule order, birth order and payout order as we described above. As may be expected, children in the top three ranks based on rule order or birth order are more likely to be assigned as index children in 4Ps households. However, the change in the assignment probability between the third and fourth ranked children are not as dramatic if households solely rely on such simple assignment rules. Further, if we believe that first-born children indeed have higher cognitive and non-cognitive skills relative to their siblings and that their parents are able to recognize this, then we expect that first-born children would be selected for education monitoring with greater probability compared with their later-born siblings. However, this is not supported by the data.

There appears to be also no empirical support that 4Ps households select children for education monitoring to maximize lifetime payout from the program. As shown in Figure 6, there is no apparent discontinuity in assignment probability between third and fourth ranked children by payout order, which would have been the expectation of households using such strategy.

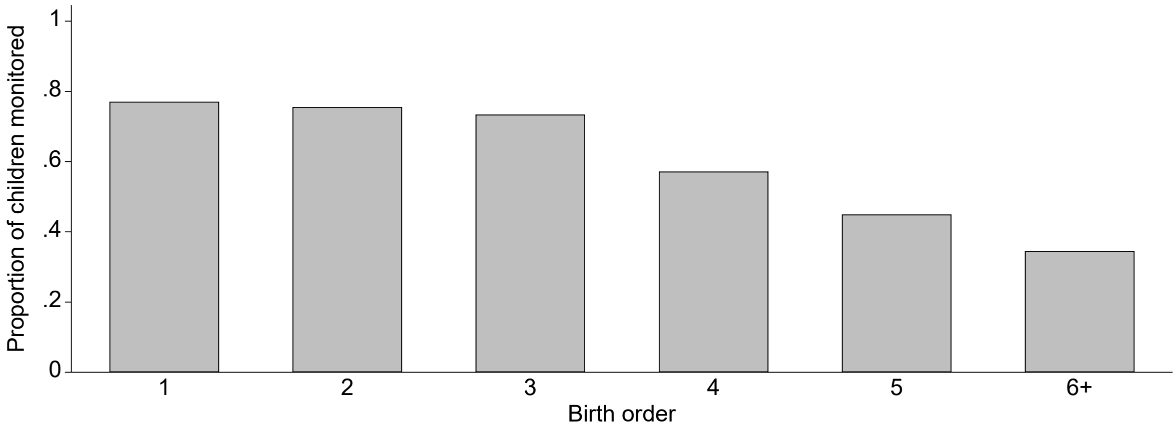
[6] The seven-year cap was introduced was introduced only in 2019 with the enactment of Republic Act 11310 or the 4Ps Law. In earlier years, households exit the program when all monitored children have aged out of the program at age 18. In this case, households should prioritize their youngest children to maximize 4Ps payout.

Figure 6. Proportion of eligible children monitored by rank order

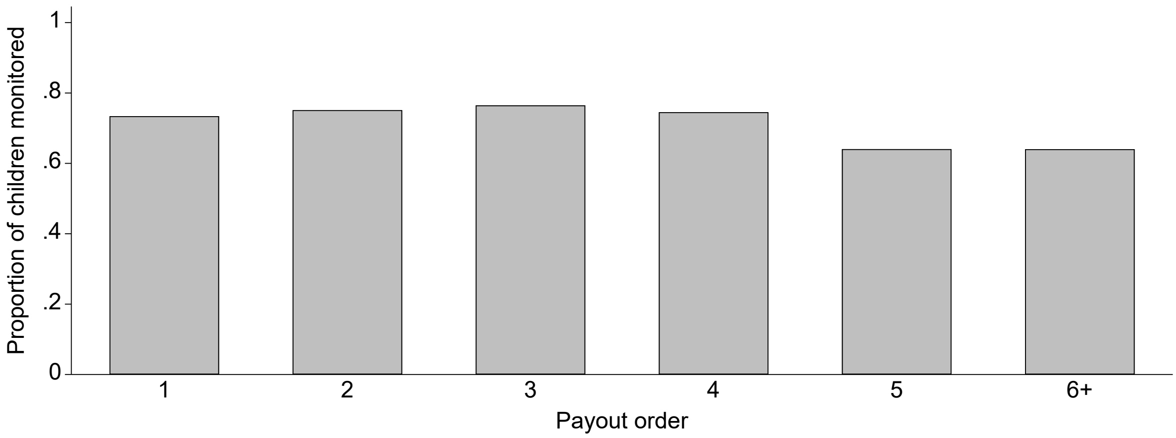
A. Rule order



B. Birth order



C. Payout order



Source: Authors' calculations.

We formally assess the strength of these potential instruments in a regression discontinuity framework by regressing monitoring status with indicators for whether a child is within the top three ranks for the above rules similar to those presented in Table 1. As may be standard, we flexibly control for trends in assignment probability by including linear trends of ranks with slopes that may vary on either side of the assignment threshold. We are interested in the value of the parameter of the indicator variable, corresponding to $p_1 - p_0$ in equation (5), which measures the strength of the assignment rules in determining child index status.

Table 5 presents the model estimates with and without linear trends of the ranks. Based on the Akaike Information Criterion, the models with linear trends are preferred over those without trend in all of these potential instruments. The results shown in Table 5 suggests that the instruments are generally weak, with partial coefficients of determination all below 0.5 percent when linear trends are included. The F statistics for the instruments in our preferred models are also all below the conventional F = 10 rule-of-thumb proposed in the literature (Stock and Yogo, 2005), which further points to weak instruments.

Together, these results suggest that 4Ps households, on average, may not be strategic when selecting children for education monitoring. While there may be some indication that rule-order and birth-order ranking raises the probability of a child being selected into 4Ps monitoring by around ten percentage points, the correlations are at best weak. In any case, that our estimates are robust to the inclusion of birth order as controls in our marginal treatment effect estimation provide some confidence that selection into monitoring may be of limited concern in our case.

Table 5. First stage regression: Impact of policy on treatment

	Monitored child (4Ps households only)					
	1(Rule order ≤ 3)		1(Birth order ≤ 3)		1(Payout order ≤ 3)	
	(3)	(4)	(5)	(6)	(7)	(8)
$p_1 - p_0$	0.332***	0.121***	0.228***	0.098***	0.038***	0.004
	(0.026)	(0.047)	(0.024)	(0.041)	(0.019)	(0.029)
p_0	0.430***	0.475***	0.531***	0.630***	0.708***	0.779***
	(0.026)	(0.044)	(0.020)	(0.039)	(0.018)	(0.026)
Polynomial order	0	1	0	1	0	1
Observations	6,401	6,401	6,401	6,401	6,401	6,401
Adjusted R-sq.	0.034	0.053	0.019	0.021	0.001	0.003
Partial R-sq.	0.206	0.002	0.193	0.001	0.179	0.000
F	163	7	90	6	4	0
AIC	7,370	7,248	7,469	7,455	7,588	7,578

Source: Authors' calculations based on 4Ps-IE3 survey data from Orbeta, et al. (2021). Note: The sample include children aged 6 to 19 years at the time of the survey. Children from 4Ps households include those aged 18 years and below at the time of 4Ps enrollment of the household. Figures in parentheses are heteroskedasticity-robust standard errors clustered at the household level. The F statistic refers to a Wald test that the parameter estimates for the excluded instrument is equal to zero. *, **, and *** denote statistical significance at the 10-, 5-, and 1-percent alpha levels, respectively. AIC – Akaike Information Criterion.

5. Conclusion

In this paper, we document that poor and near-poor households with low education parents select early into the Philippines' 4Ps, the country's flagship conditional cash transfer program for the poor. We show that children of household heads are more likely to be selected by 4Ps-recipient household for education monitoring and ultimately for 4Ps' education grants. We also show that children's birth order and rule order ranks are weak predictors of child monitoring status in 4Ps households, thereby potentially limiting its use in impact assessments. We also find no evidence that 4Ps households select children to maximize 4Ps cash payout.

The impact on school attendance of children from households induced to receive 4Ps by virtue of being tagged as poor in the program's proxy means test are nothing new, and have been documented elsewhere. We confirm that 4Ps induce higher school enrollment on average, especially among older children and among boys. We also confirm the perverse impact on non-monitored children, which we show to worsen with age, is more severe for boys, and appears universal across household compliance types.

The latter results suggest that the 4Ps may have inadvertently worsened schooling outcomes of children not monitored for the program, thereby violating the non-maleficence principle: *primum non nocere* (first, do no harm). But providing education grants to all 4Ps children may pose at least a couple of other issues. First, it may be both fiscally unsustainable and politically unpopular to provide monthly allowance to all children in poor households. Second, providing education grants to all children in 4Ps households may result in other perverse outcomes, particularly raising female fertility, which defeats the purpose of the intervention.

Restructuring the program's child monitoring protocol without raising the number of education grants per household may be the best compromise at least in the immediate term. All children in 4Ps households may be monitored for education outcomes, while at most three is provided cash grants like in the current practice. The three children who would be the basis for the cash grants may be selected at random every payout to encourage households to send all their eligible children to school. Admittedly, this may entail additional costs to administer, but could potentially further raise 4Ps' impact. An innovation that can be pursued to offset added cost in operations is to pursue the interoperability of the 4Ps compliance monitoring system and the Department of Education's Learner Information System (LIS). This will allow ease of gathering school attendance records of children even in the case of dynamic assignment of monitored children. Other mechanisms of operationalization or program restructuring may be explored through experimentation. It must be emphasized that discontinuing 4Ps, at least on the basis of the perverse effect on non-monitored children, should be the least preferred option given the program's positive impacts at least on monitored children.

In addition to estimating the schooling impacts for children in complier households, we also estimate marginal treatment effects for children in never treated and in always treated households, which are seldom considered if at all in previous impact studies on the 4Ps. Contrary to expectations, we find that children from households that always selects into 4Ps are not necessarily better off because of the program, while children from households that always selects out of the program are likely to benefit greatly from participation otherwise. This opens new exciting questions that may be relevant to policy. Why do never treated households opt into or exit from 4Ps when they are eligible? Why do children from always treated households not benefit more greatly from the program? What constraints do never (always) treated households face that limits their participation in (benefits from) 4Ps? These are just some of the questions, which we reserve for future research.

References

Abrigo, M.R.M., D. Astilla-Magoncia, Z.C. Tam, and S. Yee (2022). Conditional cash transfers in resource-poor environments: Evidence from the Philippine 4Ps. PIDS Discussion Paper No. 2022-45. Quezon City, Philippines: Philippine Institute for Development Studies.

Abrigo, M.R.M., T. Halliday and T. Molina (2022). Expanding health insurance for the elderly in the Philippines. *Journal of Applied Econometrics*, 37(3), 500-520.

Alabbasi, A.M.A., H. Tadik, S. Acar, and M.A. Runco (2021). Birth order and divergent thinking: A meta-analysis. *Creativity Research Journal*, 33(4), 331-346.

Black, S.E., E. Grönqvist, and B. Öckert (2018). Born to lead? The effect of birth order on noncognitive abilities. *The Review of Economics and Statistics*, 100(2): 274-286.

Botzet, L.J., J.M. Rohrer, and R.C. Arslan (2020). Analysing effects of birth order on intelligence, educational attainment, big five and risk aversion in an Indonesian sample. *European Journal of Personality*, 35(2), 234-248.

Bound, J., D.A. Jaeger, and R.M. Baker (1995). Problems with instrumental variables estimation when the correlation between the instrument and the endogenous explanatory variable is weak. *Journal of the American Statistical Association*, 90(430), 443-450

Brinch, C.N., M. Mogstad, and M. Wiswall (2017). Beyond LATE with a discrete instrument. *Journal of Political Economy*, 125(4), 985–1039.

Heckman, J. J., and E.J. Vytlacil (1999). Local instrumental variables and latent variable models for identifying and bounding treatment effects. *Proceedings of the National Academy of Sciences*, 96(8), 4730–4734.

Heiland, F. (2009). Does the birth order affect the cognitive development of a child? *Applied Economics*, 41(14), 1799-1818.

Kowalski, A. (2016). Doing more when you are running LATE: Applying marginal treatment effect methods to examine treatment effect heterogeneity in experiments. NBER Working Paper 22363. Cambridge, MA: National Bureau of Economic Research.

Melad, K.A.M. (2019). Treatment effect of Pantawid Pamilya on monitored and non-monitored children of household beneficiaries. Unpublished thesis (Master in Development Economics). Quezon City, Philippines: University of the Philippines – Diliman.,

Orbeta, A.C. Jr., K.A.M. Melad, and N.V.V. Araos (2021). Reassessing the impact of the Pantawid Pamilyang Pilipino Program. PIDS Discussion Paper No. 2021-05. Quezon City, Philippines: Philippine Institute for Development Studies.

Raitzer, D.A., O. Batmunkh and D. Yarcia (2021). Intrahousehold responses to imbalanced human capital subsidies: Evidence from the Philippine conditional cash transfer program. ADB Economics Working Paper Series No. 645. Mandaluyong City, Philippines: Asian Development Bank.

Rohrer, J.M., B. Elgoff, and S.C. Schmukle (2015). Examining the effects of birth order on personality. *Proceedings of the National Academy of Sciences*, 112(46), 14244-14229.

Stock, J.H., and M. Yogo (2005). Testing for weak instruments in linear IV regression. In D.W.L. Andrews and J.H. Stock, editors, *Identification and inference for econometric models: Essays in honor of Thomas Rothenberg*. Cambridge: Cambridge University Press.

Vytlacil, E. (2002). Independence, monotonicity, and latent index models: An equivalence result. *Econometrica*, 70(1), 331-341.