BIRTH ORDER EFFECT CHANGES UNDER A CASH TRANSFER PROGRAM

KENSUKE MAEBA*

Graduate School of Economics, University of Tokyo

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Abstract

This paper discusses the hypothesis that the underlying mechanism of the birth order effect on educational outcomes in developing countries is liquidity constraint, by exploiting a cash transfer program in rural Nicaragua. This program is unique in that it offers two types of cash transfers; One is unconditional on school attendance and another is conditional on it. By estimating the effect of the former treatment, I can test the hypothesis. I also estimate the latter treatment on the birth order effect for comparison. I find that the cash transfer unconditional on school attendance does not change the initial birth order effect, which implies that the hypothesis is unlikely in this context. On the other hand, the cash transfer conditional on school attendance reinforces the initial birth order effect so that the gap between the first born children and the second or later born children is widened after the intervention.

Keywords: Cash transfer, Education, Intra-household allocation, Human capital investment

JEL codes: C13, D13, O15, 022, O54

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1 Introduction

Since Becker (1962) first proposes that education, or human capital investment, is the driving factor for economic growth, there have been many researches in economics trying to examine the impact of prevalence of education, and now the importance of education is widely accepted all over the world. As a result, the Millennium Development Goals (MDGs) officially states the world-wide provision of primary education by 2015 as one of its goals.

Despite such acknowledgement of the power of education, the degree of its achievement has not been satisfying, especially it appears as underinvestment in education by people in developing countries (World Bank (2009)). In development economics, underinvestment in education by poor households in developing countries has been discussed in a lot of studies. This investment pattern may be represented in the form of the birth order effect. The definition of the birth order effect is that education attainments of children in the same households differ by the birth order. In particular, the birth order effect appears to be positive on education in developing countries (Akresh et al. (2012a), Haan et al. (2014), Lafortune and Lee (2014), Tanikue and Verheyden (2010)). That is, the levels of education attainment of early born children are lower than those of the later born children in the same households. However while there is accumulation of studies that find the positive birth order effect on education in developing countries, there are few that address the question of what causes it.

This paper sheds light on liquidity constraint as the primary underlying mechanism that causes the positive birth order effect in poor countries. The logic is that households with liquidity constraint binding are likely to send the early born children to work and invest in the later born children using the income gains from the child labor.[1] If this is true, then by giving them enough cash exogenously, the positive birth order effect would change through relaxing their liquidity constraint. If not, then it is inferred that there are other stronger factors that generate it.

I exploit the conditional cash transfer program ran in rural Nicaragua from 2000 to 2002, *Red de Protecciòn Social*, as a source of exogenous income shock. In Nicaragua, almost half of the population was classified as the poor and around 60% of adults older than 25 did not complete primary school education in 1990s (World Bank (2001)). In addition, child labor is problematic, especially in rural areas. Specifically, more than 25% of boys in rural areas engaged in child labor and about one third of children went to school and worked in 1990s (World Bank (2001)). Based on those statistics, the government of Nicaragua cooperated with International Food Policy Research Institute (IFPRI) and the Inter-American Development Bank, and conducted the program in order to encourage poor households to invest in children until they complete at least grade 4 of primary school. The program provides the two types of cash transfers, which are distributed to poor households randomly in order to evaluate their effectiveness. Both cash transfers are classified as conditional cash transfer but are different from each other in terms of the conditions. One is *Bono alimentario* (Food security transfer in English) that requires mothers to attend the workshops about health care for children, and bring their children younger than 5 years old for health check-up if they have. Another is *Bono escolar* (School attendance transfer in English) that requires children to attend school on a regular basis.

[1] This mechanism has been anecdotal in the literature. See Strauss and Thomas (1995) for the survey.

Households who are randomly assigned to the treatment group are able to receive both of the treatment if they satisfy the requirements. Under this experimental design, I estimate the effect of each type of the cash transfers as well as the overall effect of them, on the birth order effect on education. Since children in the treatment group who are not eligible for the school attendance transfer are only eligible for the food security transfer, there is variation in the treatment status; (1) children are eligible for the food security transfer but not for the school attendance transfer, (2) children who are eligible for both cash transfers, and (3) children who are ineligible for both cash transfers. By comparing (1) and (3), I can estimate how much the birth order effect changes when households receive the food security transfer. This can be interpreted as the wealth effect on the birth order effect because the changes in children's educational outcomes are due to the increased income independent of children's behaviors. By comparing (2) and (3), I can estimate the total effects on the birth order effect. Finally by comparing (1) and (2), the difference should be due to the marginal effect of adding the condition on school attendance (Condition effect).

This paper has two main contributions to the literature. One is that I can examine whether the birth order effect is due to liquidity constraint or not, by relaxing it through the exogenous income gains. To the best of my knowledge, there has been no paper other than this addressing the underlying mechanisms of the birth order effect directly. Another is that I can show the marginal effect of the condition in the conditional cash transfer, school attendance requirement, by comparing the two different kinds of cash transfers. In the literature, there has been much discussion of whether conditioning on children's behaviors matters to their schooling performance in addition to the effect of cash transfer on it.[2] My empirical results would accumulate another evidence in this context.

My empirical evidence consist of the four findings. First of all, as in the previous studies, there exists the positive birth order effect in the targeting areas. Specifically, compared with the second or later born children, the first born children are worse both in schooling and in child labor. Secondly, the treatment effect of the food security transfer does not affect the initial birth order effect. This leads to the conclusion that the hypothesis that the underlying mechanism of the birth order effect in developing countries is liquidity constraint is unlikely. As noted earlier, since the food security transfer is given independent of children's behaviors, it would affect schooling performance only through relaxing liquidity constraint that the households face. My results show that this wealth effect does not have the particular impact on the first born children, and this implication still holds for households assumed to have low income. Thirdly, the marginal effect of the requirement for the school attendance transfer improve the outcomes of the first born children with small degree than those of the other children. Since the first born children are initially left behind, this effect indeed leaves them behind even further. Although the first born children can benefit from the conditional transfer, it is not desirable as the sibling inequality becomes greater. In other words, unconditional cash transfer is more preferred than conditional cash transfer to reduce the gap between siblings in developing countries.

In the next section, I review the literature that examines the birth order effect in developing countries as well as the one that discusses the effectiveness of conditional cash transfer and unconditional cash transfer. Section 3 describes the experimental design and its data I use in this paper. Section 4 explains my identification strategy and its estimation

[2]Baird et al. (2014) for the survey of the literature about the effectiveness of conditional cash transfers and unconditional cash transfers in developing countries.

method. Section 5 shows the empirical results and their interpretations. Section 6 does the further analysis of those in Section 5. Section 7 concludes this paper. All the tables are displayed in the last part of this paper.

2 Related Literature

2.1 Birth Order Effect

A number of papers have examined the birth order effect in developing countries theoretically and empirically.[3] The birth order effect on educational outcomes in developing countries is often found to be positive in the literature. Strauss and Thomas (1995), which survey the researches of intra-household allocation problems in developing countries, propose the liquidity constraint, difference in initial ability of children, and the parents' preferences over the birth order, as the factors causing the positive birth order effect.[4]

Dammert (2010) is the best comparison with my paper. She uses the household survey data of Nicaragua as well as of Guatemala and shows that the elder siblings are more likely to engage in market work or domestic work than their younger siblings. This pattern remains even after controlling the family size by household fixed effects. Then she concludes that this finding supports the mechanism that elder siblings in liquidity constrained households give up schooling in order to increase the scarce family resources that are partially used for the younger siblings' schooling. However, her paper leaves the gap between the empirical results and the interpretation, as she does not examine the effects of changes in liquidity constraint on such pattern directly.[5]

Tanikue and Verheyden (2010) argue that in developing countries, liquidity constraint makes the elder children work and their earnings are used for the younger children. They show that their empirical results can be interpreted in this way. Haan et al. (2014) follow the same logic as Tanikue and Verheyden (2010), and also find the positive birth order effect on human capital accumulation. They also mention that this is possibly because children with low birth order receive the smaller amount of parental time and the shorter time of breastfeeding, in addition to the fact that they are from the poor households.

[3]There is also accumulation of researches discussing the birth order effect in developed countries. The early work of Behrman and Taubman (1986) theoretically formulate the mechanisms that causes the birth order effect and show the negative relationship between birth order and schooling outcomes for young adults in the US. Hanushek (1992) finds the U-shaped birth order effect on test scores in the US, which means that the first born and the latest born perform better than the middle born. Black et al. (2005) find that among Norwegian children, those with higher birth order attain the statistically and economically significant lower education levels. Booth and Kee (2009) use the British household survey data and conclude that there is the negative birth order effect in educational attainment.

[4]The underlying logic of why those factors bring the positive birth order effect is as follows; (1) Households whose liquidity constraint is binding are likely to send their children of low birth order to labor market. As a result, the resources these children earn can be used for investment in education for children of the higher birth order. (2) Since the relatively old mothers are likely to give birth to children with lower weight, and birth weight is positively correlated with intelligence such as IQ, children of higher birth order have lower initial ability. Parents who learn this difference tend to invest in education for children with higher ability. (3) Parents simply prefer the first born children over the others because of cultural backgrounds. (4) The marginal cost of schooling for young siblings is lower than the older because there exist both physical capital and human capital externalities among children in the same households.

[5]She also obtains the similar empirical results when using the data of Guatemala.

Lafortune and Lee (2014) also specify the liquidity constraint as the cause of the positive birth order effect in developing countries. They use the national survey data of the US, South Korea, and Mexico and confirm that the birth order effect on children's education level is positive in South Korea and Mexico, whose households are more likely to face the binding liquidity constraint than the US, while that is negative in the US.

Ejrnaes and Portner (2004) are different in that they explain this observation by the difference in initial ability of children and find, by using a household survey in Philippine, that the empirical results are consistent with the theoretical implication. This mechanism is backed up by Akresh et al. (2012a), which exploit the panel survey in Burkina Faso. They consider the differences in ability among siblings as one of the determinants of parental investment decisions and obtain statistically significant positive birth order effect on children's current school enrollment rate along with the finding that the cognitive ability of children in the same households positively affect their enrollment rate.

Although those papers confirm the existence of the birth order effect and provide the possible explanations,[6] they do not specify the particular mechanism. This paper makes contribution in this literature because it empirically addresses the question of whether the birth order effect is caused by liquidity constraint, by examining changes in birth order effect when a household experiences the positive income shock by cash transfer. In this sense, this paper takes more direct approach to investigate the causes of the birth order effect.

2.2 Cash Transfer Program

A cash transfer program aims to encourage people in developing countries to improve their underinvestment in children's education. It begun with "PROGRESA" in Mexico, which is the conditional cash transfer program that requires children whose grades range between 3 to 9 to attend school more than 85 % of school days,[7] and has been expanded mainly in Latin American countries.

Among the papers that evaluate the effectiveness of the conditional cash transfer program in Nicaragua, which is used in this paper as well, Lincove and Parker (2015) is the most related to this paper. They investigate in detail the spillover effect of the program within the households as well as the direct effect. In particular, their empirical results show that the treatment has the positive effect on school attendance rate and the negative effect on hours worked not only for children who are eligible to the program but also for children who are ineligible but whose siblings are eligible.[8]

Although conditional cash transfer programs have been spread rapidly after the success of PROGRESA, there are three potential drawbacks. One is that it confounds the wealth effect and the condition effect. Second is that it would be costly for monitoring whether people comply with the conditions properly. Third is that the program is beneficial only for a certain range of people. To see whether those shortcomings are the significant problems, many randomized experiments have been conducted in developing countries.

[6]There are a few papers that observe the negative birth order effect in developing countries. For example, Qian (2009) finds it in rural China in 1990s and suggests that the possible mechanisms are economies of scale in schooling, and the increased mother's labor supply.

[7]For more information about PROGRESA, see Skoufias et al. (2001), Schultz (2004), and Todd and Wolpin (2006).

[8]Barham et al. (2013) investigate the long-run effect of this program.

Benhassine et al. (2015) resolve those problems by conducting quasi-conditional cash transfer ("labeled cash transfer" in the paper). That is, parents in poor regions of Morocco are given unconditional cash transfer that the researchers, however, ask them to use for children's education. By comparing the effect of the labeled cash transfer and that of the cash transfer conditioning on school attendance, they find no significant difference between them.

In contrast, Baird et al. (2011) randomly assign the eligibility of cash transfer that requires children to attend school on a regular basis and that of cash transfer without such requirement. They find the statistically and economically significant difference between the effects of two cash transfers on educational outcomes. Akresh et al. (2013) do the similar randomized experiment in Burkina Faso and show that cash transfer with the condition on children's study behaviors outperforms the counterpart without the condition in terms of educational outcomes of the marginal children such as children with low ability.[9] Moreover, Burstzyn and Coffman (2012) find that such condition on schooling attendance is even preferred by parents because it reduces the costs of their monitoring on children's behaviors.

Regarding the fact that those papers above show the mixed results of whether conditional cash transfer is more effective than unconditional cash transfer on education in developing countries, this paper makes contribution to the literature by adding another empirical results of the difference in the effects of cash transfer conditional on and unconditional on children's behaviors on educational outcomes.

3 Data

3.1 General Information

In order to estimate the changes in the birth order effect when households can relax their liquidity constraint by participating a cash transfer program, I exploit the data from the *Red de Protecciòn Social* (hearafter RPS), which is the conditional cash transfer program in Nicaragua from 2000 to 2002. The aims of the program are to encourage poor households to care about health status for children under age of five, keep children in primary school until they complete fourth grade, and increase the expenditures on food. The government of Nicaragua prepared two types of conditional cash transfers targeting poor households in rural Nicaragua[10] in the form of randomized control trial. International Food Policy Research Institute (IFPRI) with the Inter-American Development Bank collected the data for the evaluation of the effectiveness of RPS. Nicaraguan government first conducted the pilot in the departments of Madriz and Matagalpa, where around 80% of people there lived in poverty in 1998, and expanded the program afterwards.

[9]With respect to the effects of conditions on parents' behaviors, Attanasio et al. (2015) show that in Columbia, such conditions on parents bring the economically and statistically significant improvement on the rates of parental visit on health-checks for their infants.

[10]In 1998, 48% of the population were classified as the poor and 75% lived in rural areas of Nicaragua (Maluccio and Flores (2004)).

3.2 Experimental Design

With respect to selection of the targeting population, the government first chooses the six municipalities[11] that joined in *Microplanificaciòn Participativa*, a participatory development program. Those municipalities were considered to be poor in comparison with the national average at that time. Then for each of all 59 rural administrative areas (*comarcas*) in the six municipalities, the marginality index is calculated, based on the national census in 1995. This index ranges 1 to 4, in which 1 indicates the poorest areas. The government eventually chooses the 42 comarcas whose values of the index are either 1 or 2, as the targeting areas of RPS.

After the targeting areas are selected, the random assignment of the treatment status is conducted at comarcas level, in order to evaluate the effectiveness of RPS. There are 21 comarcas classified as the treatment group and the remaining 21 comarcas in the control group. The treatment group is offered two types of conditional cash transfers: *Bono alimentario* (Food security transfer) and *Bono escolar* (School attendance transfer). Households lose the eligibility once they fail to meet the requirements. From each comarcas, 42 households are randomly chosen as the samples for the evaluation of the program. Therefore the initial number of the sample households is 1764.

The timeline of the evaluation design is as follows. The baseline survey is collected in August and September of 2000, the first follow-up survey in October of 2001, and the endline survey in October of 2002. After 2002, the control group also becomes eligible for the program. Households in the treatment group are able to receive the cash transfer for 5 months by the first follow-up survey and for 11 months by the endline survey. Note that this experimental design was intended to last for a year at the beginning and was unexpectedly extended one more year.

3.3 The Interventions

Bono alimentario (Food security transfer)

Bono alimentario (hereafter the food security transfer, or FT) is the conditional cash transfer offered to all the households in the treatment group. However the requirement for the transfer depends on family structure.[12] The cash transfer is as frequent as every other month and the total amount of money the households receive is C$2880 per year, which is approximately 13% of their annual expenditure.

Bono escolar (School attendance transfer)

Bono escolar (hereafter the school attendance transfer, or ST) is the conditional cash transfer offered to households with children of age 6 to 12 who have not completed fourth

[11]The six municipalities are Totogalpa,Yalaüina, Terrabona, Esquipulas, El Tuma-La Dalia, and Ciudad Darío.

[12]For households with no children or with children who are older than 6, the condition is for mothers to attend health education workshops every other month. For households with children who are younger than 3, the condition is for mothers to bring the children for monthly health check as well as to attend the bimonthly workshops. For households with children whose ages are between 2 and 5, the condition is for mothers to bring the children for health check and attend the workshop once in two months.

grade in primary school.[13] In order to benefit from the program, the children should attend school more than 85% of schooling days. The cash transfer is bimonthly and the yearly amount is the half of that provided by the food security transfer.[14]

3.4 Sampling

I use the data in 2001 and restrict the samples to children of age 6 to 16 in 2000 who are not married, reside in the households, have more than or equal to one sibling, and are observed in both years. Out of them, I drop the observations that have the missing values in the dependent variables or in the birth order variable. As a consequence, the remaining sample size is 2557 for each year.

4 Identification Strategy

4.1 Wealth Effect and Total Effect on Birth Order Effect

The objectives of the estimations are to see whether the birth order effect changes when households experience the positive income shock so that their liquidity constraints are relaxed to some extent. In RPS, there are two sources of income gains: the food security transfer and the school attendance transfer. The difference between these two types of transfers is that the former does not condition on children's behavior while the latter does. In this sense, it is possible to see the food security transfer as unconditional cash transfer and the school attendance transfer as conditional cash transfer, when estimating the effects on children's education. Specifically, while the former changes the birth order effect through the wealth effect, the latter does through the condition effect as well as the wealth effect. I estimate the effects of the cash transfers on the birth order effect by dividing the samples with respect to the treatment eligibility.

I focus on the birth order effect on educational outcomes as well as labor outcomes because the aim of the program is to improve education and to reduce child labor in Nicaragua. As explained in Section 1, children in Nicaragua enrolled in primary school but often failed to complete it partly because of child labor. Since this is associated with birth order, it is important to evaluate how the interventions change it. For this purpose, I choose school attendance days, grade progression[15], enrollment, weekly hours worked, and whether children work or not in a week for dependent variables.[16]

Since the assignment of the treatment status is randomized in RPS at district level, I consider the following specifications. For the effect of the two cash transfers combined, restricting the samples to those who are 6 to 12 years old and have not completed grade 4 in primary school at the baseline,

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[13]These households are also eligible for *Mochila escolar* (School supplies transfer), which provides C$275 annually for them to purchase the school stuff such as uniforms if they enroll in new grade. However, this requirement was never enforced during the entire program period.

[14]Note that the school attendance both the food security transfer and the school attendance transfer are given per household, not per child.

[15]Grade progression is a dummy variable that takes 1 if children progress their grades by one from the baseline to the first follow-up. Note that this also takes 1 if children enroll who are not in school at the baseline enroll at the first follow-up.

[16]For more details, see the footnote of Table 1.

$$
\begin{aligned}
Y_{ihk} &= \beta_0 + \beta_1 \times 1st\ born_{ihk} + \beta_2 \times (FT\ +\ ST)_k \\
&+ \beta_3 \times (FT\ +\ ST)_k \times 1st\ born_{ihk} \\
&+ W'_{ihk}\gamma + X'_{hk}\delta + \epsilon_{ihk},
\end{aligned}
$$

and for the effect of the food security transfer, restricting the samples to those who have completed grade 4 in primary school at the baseline,

$$
\begin{aligned}
Y_{ihk} &= \beta_0 + \beta_1 \times 1st\ born_{ihk} + \beta_2 \times FT_k \\
&+ \beta_3 \times FT_k \times 1st\ born_{ihk} \\
&+ W'_{ihk}\gamma + X'_{hk}\delta + \epsilon_{ihk},
\end{aligned}
$$

where i indexes individuals, h indexes households, and k indexes the district at which the randomization was conducted .

$1st\ born_{ihk}$ is a dummy variable that takes 1 if a child is first born in the households. $(FT\ +\ ST)_{hk}$ is the indicator variable for whether a household is eligible for both the food security transfer and the school attendance transfer, and FT_{hk} is the indicator variable for whether a household is eligible for the food security transfer.[17] W_{ihk} is the vector of individual characteristics such as age, sex, years of schooling, and spacing to each sibling in the same households.[18] X_{hk} is the vector of household characteristics such as the number of children, family size, household head's age and his/her spouse's age, household head's education attainment and his/her spouse's education attainment, and natural logarithm of total annual household expenditure at the baseline.[19]

Regarding the parameters, in both specifications, β_1 captures the effect that the first born children in the control group receive, β_2 is the corresponding treatment effect,[20] and β_3 is the change in the effect on the first born children due to the cash transfers.

The interpretations are as follows. If $\beta_2 + \beta_3$ is statistically different from 0, then there exists the treatment effect on the first born children. On the one hand, in the first specification, by comparing the sing of β_1 with $\beta_2 + \beta_3$ I can see whether the birth order effect is reinforced or undermined by the combination of the wealth effect and the condition effect. On the other hand, in the second specification, if the treatment effect brought by the food security transfer has the different sign from that of β_1, which means the treatment effect undermines the initial birth order effect only through the wealth effect, I can conclude that the birth order effect is caused by liquidity constraint. If it has the same sign, then it

[17]In my specifications, $(FT\ +\ ST)_{hk}$ and FT_{hk} are the same as the indicator variable for the treatment group.

[18]The variables for spacing are absolute age difference between an individual and his/her sibling of each birth order.

[19]The data do not have information about household income. Thus I proxy it for annual household expenditure.

[20]Note that this is the intention to treat (ITT) effect because I use the information about who were able to benefit from the program. Because the take-up rate can be calculated in my data, which is 0.969 (= $\frac{0.498}{0.514}$), I can recover the average treatment effect on treated (ATT) by dividing the ITT estimates by it.

may be the case that the birth order effect is generated through the other mechanisms. It is worth noting that since more than 90% of children who have completed grade 4 in primary school at baseline have the younger siblings who are eligible for the school attendance transfer, most children are likely to be exposed to the wealth effect with the same degree. Therefore, if there appears some difference in response in those specifications in terms of the birth order effect, it comes from the marginal effect of the condition effect. In Section 4.2, I extract it in more rigorous way.

4.2 Condition Effect on Birth Order Effect

The previous specifications do not allow me to compare the wealth effect with the condition effect. In order to isolate it, I exploit the difference in differences method (DID). Figure 1 is the idea of how to apply it in this context.[21] Then $(A-B)-(C-D)$ will provide the condition effect, given the wealth effect, under a couple of the strong assumptions.[22]

	Treatment	Control
Eligible for ST	A	C
Ineligible for ST	B	D

Figure 1 Difference in differences method

In more detail, using the children of age 6 to 16 as the samples, the specification is

$$\begin{aligned}
Y_{ihk} &= \beta_0 + \beta_1 \times 1st\ born_{ihk} + \beta_2 \times Treatment\ group_k + \beta_3 \times ST\ eligible_{ihk} \\
&+ \beta_4 \times 1st\ born_{ihk} \times Treatment\ group_k \\
&+ \beta_5 \times Treatment\ group_k \times ST\ eligible_{ihk} \\
&+ \beta_6 \times ST\ eligible_{ihk} \times 1st\ born_{ihk} \\
&+ \beta_7 \times Treatment_k \times ST\ eligible_{ihk} \times 1st\ born_{ihk} \\
&+ W'_{ih}\gamma + X'_h\delta + \epsilon_{ih},
\end{aligned}$$

where $Treatment\ group_k$ is an indicator variable for the treatment group.[23] $ST\ eligible_{ihk}$ is a dummy variable that takes 1 if children are eligible for the school attendance transfer

[21] I estimate the combination effects by $A-C$ and the wealth effect by $B-D$ in the previous specification.

[22] First one is the common trend for the treatment group and the control group. It means that the difference between children who are eligible only for the food security transfer and children who are eligible for the both transfer in treatment group is considered to be the same difference between the counterparts in the control group, in the absence of the school attendance transfer. Second one is that the effect of the food security transfer is common to children in any grade. This is the strong assumption because it might not be the case that the progression from grade 1 to grade 2 brings the same difficulty to children as that from grade 5 to grade 6. Third one is that the wealth effect and the condition effect are additive separable. Without the last two assumptions, the relationship that the subtraction of FT from FT + ST implies ST does not hold. Note that there are no children who are eligible only for the school attendance transfer.

[23] $Treatment_k$ is also the indicator variable for the food security transfer because all households in the treatment group are able to receive the food security transfer if they satisfy the requirements.

and 0 otherwise. The definitions of the other variables are the same as in the previous specifications.

The parameter of interest is β_7. This coefficient alone expresses the condition effect on the birth order effect in addition to the wealth effect. Thus comparing with β_1, I can examine whether and how the birth order effect is changed by the condition effect after changed by the wealth effect.

5 Empirical Results

5.1 Sample Balance

I first conduct the balancing tests by using the individual and household characteristics at the baseline. The summary statistics for the pooled samples, the treatment group, and the control group are in Table 1. The balancing tests are done by

$$Characteristics = \beta_0 + \beta_1 \times Treatment\ group + \epsilon,$$

for the pooled sample (children of age between 6 to 16 at the baseline9, the two cash transfer eligible sample (children of age 6 to 12 who have not completed grade 4 in primary school at the baseline, "FT + ST"), and the food security transfer eligible sample (children who have completed grade 4 in primary school at the baseline, "FT").

$Characteristics$ is each of both individual and household characteristics. I estimate β_1 by OLS, clustering at *comarcas* level (*comarcas* is the randomization unit) and weighting by population of each *comarcas*. If β_1 shows no significance, then it implies that the samples I use are well balanced across treatment status.

The first table in Table 2 shows the results of balancing tests for the different samples. For the pooled sample and FT sample, whether having a ST eligible child or not is not balanced between the treatment group and the control group. For FT + ST sample, education attainment of household head and that of household head's spouse displays the significant difference. Therefore, I control for those variables in estimation. Overall, the treatment status is considered to be randomized in terms of the observables.

5.2 Attrition

I begin with a sample of 2921 children at baseline, and among them, 2557 children are tracked in the first follow-up survey after the sample restrictions in Section 3.4. Thus, the attrition rate is approximately 13%. In this section, I examine whether this attrition sample of 364 is systematically different from the non-attrition sample of 2557, and also examine whether the treatment assignment is random in the attrition sample. For comparison between the attrition sample and the non-attrition sample, I estimate

$$Characteristics = \beta_0 + \beta_1 \times Attrition + \epsilon,$$

where *Attrition* is the indicator variable for attrition. For comparison between the treatment group and the control group in the attrition sample, I estimate

$$Characteristics = \beta_0 + \beta_1 \times Treatment\ group + \epsilon.$$

The results are shown in the second table of Table 2. First of all, children in attrition perform worse and engage in child labor more than those in my sample. Moreover, among them, those in control group achieve even lower educational outcomes. This implies that there is a selection problem in my data. Therefore, I need to interpret my empirical results with caution that the estimated effects are on the relatively advantaged children in the targeting areas.

5.3 Birth Order Effect

Before the Interventions

I first show empirically that the positive birth order effect exists in the samples at baseline. The specification here is as follows.

$$Y_{ihk} = \beta_0 + \beta_1 \times 1st\ born_{ihk} + W'_{ihk}\gamma + X'_{hk}\delta + \epsilon_{ihk}.$$

The definitions of the variables are explained in Section 4.1, and the estimation results are shown in Table 3.[24] To be consistent with the previous studies that confirm the existence of the positive birth order effect in developing countries, β_1 is expected to be negative when the first born dummy variable is used

Figure 2 is the mean comparison in the educational outcomes and labor outcomes between the first born children and the second or later born children for the pooled sample. It clearly describes that the first born children are more disadvantaged than the later born children. On average, they attend school a few days less and their enrollment rate is around 10% lower. Additionally, they are more likely to work by 15% and work longer by around 5 hours weekly. Those results are robust in terms of statistical significance when controlling for individual and household characteristics. According to Table 3, for the pooled sample, the first born children attend approximately 2.2 days fewer and work almost 3 hours longer. Furthermore, the probability of their enrollment is 11% lower and that of working is 5% higher. For FT + ST sample and FT sample, the similar patterns are observed, though the magnitudes are somewhat different. Therefore, in my sample, it is highly likely that the positive birth order effect is in presence among schooling age children.

After the Interventions

Now I turn to the empirical results of how and to what extent the initial birth order effect changes after households experience the positive income shock by RPS. Table 4

[24]Because I do not have data one year prior to the baseline, I cannot construct grade progression at the baseline.

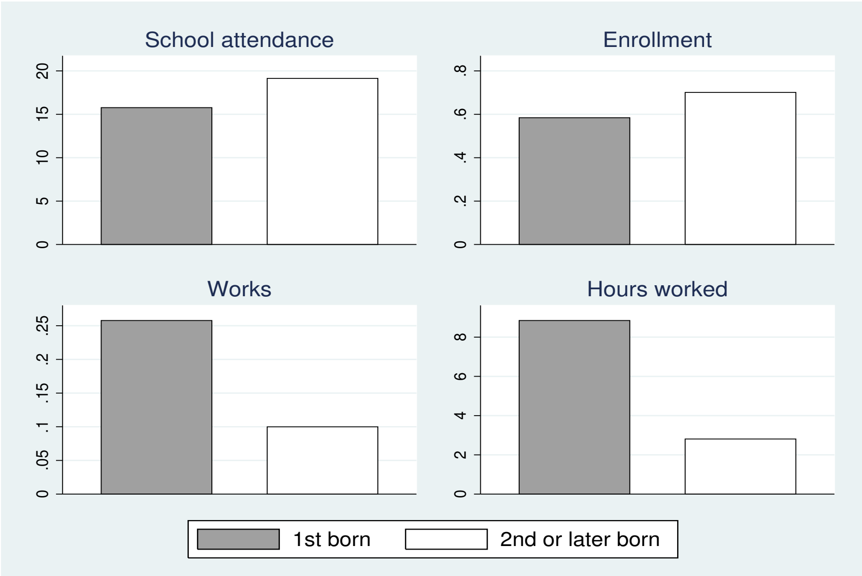

Figure 2 Baseline mean difference 1st born and 2nd or later born

and Table 5 are the estimation results for children eligible for the two transfers and for those eligible for the food security transfer only, respectively. The outcomes of interest are schooling performance (school attendance, grade progression, and enrollment) and child labor (weekly hours worked and the probability of child labor). Also, those can be classified in terms of intensive or extensive margin. That is, school attendance, grade progression, and hours worked are the intensive margin, and the rest is the extensive margin.

The findings from Table 4 are as follows. First of all, the treatment effect of two interventions combined is all positive on school outcomes and negative on labor outcomes with statistical significance. Thus, the program does improve the situation children in rural Nicaragua face. Second, in this sample, the firs born children are not as disadvantaged in education as seen in Table 3. However, they are more likely to do child labor than other children. Therefore, the first born children in this cohort go to school as well as go for work. Third, the treatment effects particularly for the first born children have no significant impact on any outcomes, except for grade progression where the first born children tend to move up to higher grade more than other children and the interventions reduce the gap so that the advantage the first born children receive is almost offset. In sum, while the positive birth order effect is ambiguous in schooling, that does exist in child labor and the treatment effects from the two cash transfers do not affect it.

Similarly, in Table 5, I find that the treatment effects of the food security transfer, which is the wealth effect, is precisely estimated and improve not only children's schooling performance but also their child labor. Then, unlike FT + ST sample in Table 4, there is the positive birth order effect in FT sample. Specifically, the first born children attend about 3 schooling days fewer, are less likely to move up to higher grade by 6.4% and to enroll in school by 10%. Moreover, they work 6 hours per week longer and have the probability of working by 11%. However, the interventions are not concentrated on them. Rather, children receive the same benefits regardless of their birth order. Therefore, the initial gap between the first born children and the later born children remains unchanged even after the program started.

With respect to the hypothesis about the underlying mechanism of the positive birth order effect in Section 2.1, I conclude that liquidity constraint is unlikely to cause it. This draws from the results in Table 5 that the wealth effect is not associated with the birth order, rather it affects children equally. In Section 6.1, I examine the robustness of this finding using the subpopulation that is considered to be severely liquidity constrained.

Wealth Effect VS. Condition Effect

In order to abstract the condition effect on the birth order effect, I subtract the wealth effect from the summation of the wealth effect and the condition effect. To validate this specification, there are three assumptions. One is the common trend assumption that the difference between children of age 6 to 12 who have not completed grade 4 in primary school at baseline and those who have completed grade 4 in primary school at baseline is common across treatment status, in the absence of the school attendance transfer. Second is that the effect of the food security transfer is common across years of schooling. Third is that the wealth effect and the condition effect are additive separable. Under those assumptions, I can obtain the difference between the total effect and the wealth effect as the condition effect.

Table 6 summarizes the estimation results. The basic patterns are the same as before. That is, the positive birth order effect exists in the sample, and the wealth effect from being in the treatment group improve not only school outcomes but child labor outcomes statistically significantly. Regarding the condition effect, I observe that while the condition effects on all the children have the positive and significant effect on the educational outcomes, they do not reduce child labor. Those positive effects are economically large as school attendance increases by 4.6 days, the probability of grade progression by 21%, and the enrollment rate by 15.6%. However, when the condition effect on schooling is estimated to be smaller for the first born children than the later born children. In particular, the condition effect increases school attendance of the first born children only by about 2 days, the probability of grade progression and the enrollment rate by approximately 3%. Considering the fact that the first born children are initially disadvantaged, the gap between them and the second or later born children are indeed expanded by the condition effect.

When I compare the condition effect with the wealth effect, the difference is striking. While the wealth effects improve children's outcomes towards more school and less child labor with the same degree for all the children, the condition effects do so with the smaller degree for the first born children. This can be partly due that the first born children who are eligible for ST increase their school attendance until they attend school just more than the requirement for the transfer. Therefore the condition effect on their school attendance is reduced as minimum as possible. As a result, the initial positive birth order effect is reinforced by the condition effect. This is inconsistent with Akresh et al. (2013), in which they find the conditional cash transfer program is helpful to the marginal children, such as the first born children in my context. Further research will be required to reveal the cause of that difference.

6 Further Analysis

6.1 Robustness Check

Liquidity Constrained Households

In this section, I do the robustness checks on the results that liquidity constraint is unlikely to be the underlying mechanism of the birth order effect in developing countries. As Table 5 shows, the positive birth order effect does not change after households receive the food security transfer, which means that the wealth effect does not affect the birth order effect. This might be backed up if I obtain the same results for children from households with low income, as their liquidity constraint should be more severely binding because the wealth effect on them is expected to be larger than that on the whole sample. Therefore, I estimate the treatment effect on children from the households with their Engel's coefficients larger than the median and from those whose household head has no education, respectively.[25]

The estimation results are displayed in Table 7. The specifications and the control variables are the same as the previous estimations, and I estimate them on the subsamples described above. Clearly, the wealth effects associated with the first born children have no significant impact on any outcomes in both samples. This demonstrates that even among children from the seemingly severely liquidity constrained households, the birth order effect is not related with the wealth effect brought by the food security transfer. Thus, my finding that liquidity constraint is unlikely to be the underlying mechanism of the birth order effect is robust in my data.

Absolute Birth Order

The robustness of my findings has to be tested against the different specifications. For this purpose, I use the absolute birth order as the birth order variable, which is often used in the literature, instead of the dummy variable for the first born children, and estimate the total effect, the wealth effect, and the condition effect.

The three tables in Table 8 correspond to each of them. Firstly, in the estimation of the total effect, the results are consistent with those in Table 4. The total effect on children regardless of the birth order is significantly positive on school outcomes and negative on child labor outcomes, and the positive birth order effect is found only for child labor outcomes. Then the total effects associated with the birth order have no clear impact on any outcomes. Secondly, the estimated wealth effects are also similar in those in Table 5, except that the wealth effect through the absolute birth order is significantly negative on school attendance and on enrollment rate. Although this casts doubt on the robustness of my results, it might be simply because using the absolute birth order weighs the later born children more so that the effect is magnified. Finally, with respect to the condition effect, the difference from Table 6 appears in the condition effect through the absolute birth order. Specifically, that on school attendance and on grade progression is imprecisely estimated. Apart from that, the pattern shows the similarity with that in Table 6.

[25]Because my data have no information about household income, I assume that those households would have the income lower than the sample median.

Overall, my findings are robust in most cases when I use the absolute birth order instead of the dummy variable for the first born children. However, some results are different from those in the previous sections due to how I weight children of each birth order. For example, the absolute birth order weights the second or later born children differently while the dummy variable treats them equally. Because this is beyond the scope of this paper, the further discussion is needed to determine which specification is appropriate when estimating the birth order effect.

6.2 Heterogeneous Treatment Effect

Cost of Sending Children to School

Since the school attendance transfer is given per household, the costs of satisfying its requirement differ by the number of children who are eligible for it. Specifically, if more children in the households are eligible for the school attendance transfer, higher costs of satisfying its condition they need to incur because they lose more labor force. Then their reactions to the transfer might be expected to change accordingly. In order to examine the conjecture, I estimate the coefficients of interaction terms between the treatment effect for the first born children and the dummy variable for the high share of ST children[26] for each of the total effect, the wealth effect, and the condition effect, respectively.

The first three tables in Table 9 are the estimation results. To see whether there is heterogeneity in the treatment effects focusing on the first born children or not, I focus on the interaction terms between the treament effect for the first born children and the dummy variable indicating the high share of ST children. For the total effect, I find that the effects do not differ between children from the households with the share of ST eligible children higher than the sample median and those from the households with that lower or equal to the sample median. Similarly, the wealth effects and the condition effects are also not heterogeneous in terms of that. This implies that the costs of sending children to school is so negligible that they do not affect the birth order effect.

Gender Difference

In developing countries, girls are likely to receive less education than boys.[27] If this is true, then the birth order effect could be different by gender. For instance, it is expected that girls are disadvantaged in schooling regardless of birth order.

The estimation results in the last three tables in Table 9 show that there is no heterogenouse treatment effect on any outcomes of the first born children. However, it is worse noting that while boys and girls are treated equally in terms of schooling, boys are engaged in child labor more than girls. Therefore, combining those facts, it is implied that there is inequality in child labor between boys and girls even after the interventions.

[26]It takes 1 if the share of ST eligible children is higher than the sample median and 0 otherwise.
[27]Duflo (2012) surveys the researches about inequality in men and women in developing countries.

7 Conclusion

This paper focuses on the investment behaviors in education for children in developing countries and investigates empirically whether it is caused by liquidity constraint. Furthermore, this paper compares the effectiveness of two types of cash transfers in terms of how the birth order effect on education changes; one is conditional on children's school attendance and another is unconditional on it.

I use the data collected along with the RPS, which is the conditional cash transfer program that aims to raise the education level of the rural Nicaragua. This program is conducted in a form of randomized control trial. During the program period, households in the treatment group are able to receive the two different cash transfers: Food security transfer and School attendance transfer. The key differences are that the former does not require children to attend school regularly while the latter does, and all the households in the treatment group are eligible for the former while only those who have children of 6 to 12 who have not completed grade 4 in primary school are eligible for the latter. Thus, children who have completed grade 4 in primary school at baseline are exposed to the wealth effect while those who have not are exposed to both the wealth effect and the condition effect. This unique framework allows me to estimate the effect of each cash transfer on the birth order effect separately and the total effect of the two cash transfers.

The empirical results show that there is the positive birth order effect on schooling and on child labor before RPS, which means that the first born children in the households are less likely to attend school and more likely to do child labor than the other children. While this pattern remains unchanged even after the households experience the wealth effect, it is reinforced by the condition effect so that the initial gap between the first born children and the second or later born children is expanded after the interventions. However the summation of the two effects does not affect the initial birth order effect. Those findings are tested both under the different sample and the different specifications and show their robustness at least on the subsample consist of the households with severe financial situation. When investigating the heterogeneity in the effects concentrated on the first born children, I find no supportive evidence for it.

This paper makes two contributions to the different literature. First, it would be the first paper to address the question of what causes the birth order effect in developing countries. As the previous literature suggests, one of the possible underlying mechanisms of the birth order effect is liquidity constraint. That is, in the households with their liquidity constraint binding, the elder children are sent to work to increase the scarce family resources and the younger go to school by using them. In my analysis, I estimate the effect of relaxing liquidity constraint on the birth order effect and show no significant effect. Thus the mechanism discussed in the literature is unlikely to hold, at least, in rural Nicaragua. Second, this paper accumulates the new empirical results to discussion of the effectiveness of conditional transfer compared to unconditional cash transfer. According to my results, while the wealth effect alone does not improve children's schooling performance, adding the condition effect, surprisingly, makes it worse off in a sense that the first born children are more disadvantaged after the interventions though they indeed gain the benefits to some extent. This suggests that in terms of inequality in siblings in the same households, unconditional cash transfer is preferred over conditional cash transfer. However, this result is with caution because unlike the previous studies, I do not have conditional cash transfer and unconditional cash transfer independently so that the targeting population is different by the specifications. For example, while the total effect and the condition effect are the effects on children of 6 to 12 years old who have not completed grade 4 in primary school at the baseline, the wealth effect is on children who have completed grade 4 in primary school at the baseline. To improve on this point, it is necessary to conduct a field experiment that distributes conditional cash transfer and unconditional cash transfer separately and randomly.

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Table 1 Summary statistics for the overall samples

	Obs.	Mean	Sd.	Min.	Max.
Treatment Status					
Treatment group (Household)	927	0.52	0.5	0	1
Treatment group (Individual)	2,557	0.571	0.495	0	1
Eligible for ST (Individual)	2,557	0.685	0.465	0	1
Individual Characteristics					
School attendance	2,557	17.38	13.54	0	30
Enrollment	2,557	0.649	0.477	0	1
Works	2,540	0.154	0.361	0	1
Hours worked per week	2,540	4.777	12.89	0	70
1st born	2,557	0.333	0.472	0	1
Years of schooling	2,557	1.697	1.942	0	11
Age	2,557	10.39	3.067	6	16
Male	2,557	0.510	0.500	0	1
Primary school	2,557	0.596	0.491	0	1
Secondary school	2,557	0.0293	0.169	0	1
Household Characteristics					
Number of children	927	4.257	1.77	2	11
Family size	927	7.481	2.615	3	24
Have a ST child	927	0.924	0.264	0	1
Have a small child	927	0.736	0.441	0	1
Household head education	927	1.567	2.145	0	16
Spouse education	784	1.666	2.19	0	14
Household head age	925	44.391	13.035	21	87
Spouse age	784	38.043	10.959	15	92
Household head hours worked per week	921	36.238	19.101	0	126
Spouse hours worked per week	779	5.13	13.642	0	84
ln(Total annual expenditure)	927	9.838	0.588	7.57	11.368
ln(Food expenditure)	927	9.469	0.646	6.48	11.094
ln(Total annual expenditure per capita)	927	7.884	0.612	5.49	9.892
ln(Food expenditure per capita)	927	7.516	0.662	4.559	9.397
Engel's coefficient	927	0.712	0.146	0.099	0.966

Note: As individual characteristics, "School attendance" is school attendance days in the last month prior to the baseline, "Enrollment" is a dummy variable that takes 1 if children enroll in school at the baseline and 0 otherwise, "Works" is a dummy variable that takes 1 if children engage in any work in the last month prior to the baseline and 0 otherwise, "Hours worked per week" is the number of hours children worked in the last month prior to the baseline, "1st born" is the dummy variable that takes 1 if an individual is the first born child and 0 otherwise, "Age" is individual age, 'Male" is a dummy variable that takes 1 if an individual is male and 0 otherwise, "Years of Schooling" is individual current years of schooling, and "Primary" is a dummy variable that takes 1 if an individual is in primary school at the baseline and 0 otherwise ("Secondary" is defined in the similar way).

Table 1 (*Continued*) Summary statistics for the treatment group

	Obs.	Mean	Sd.	Min.	Max.
Treatment Status					
Eligible for ST (Individual)	1,314	0.695	0.461	0	1
Individual Characteristics					
School attendance	1,314	17.31	13.32	0	30
Enrollment	1,314	0.660	0.474	0	1
Works	1,310	0.144	0.351	0	1
Hours worked per week	1,310	4.273	12.19	0	70
1st born	1,314	0.332	0.471	0	1
Years of schooling	1,314	1.706	1.898	0	11
Age	1,314	10.32	3.046	6	16
Male	1,314	0.511	0.500	0	1
Primary school	1,314	0.611	0.488	0	1
Secondary school	1,314	0.0254	0.157	0	1
Household Characteristics					
Number of children	482	4.212	1.791	2	11
Family size	482	7.39	2.708	3	24
Have a ST child	482	0.938	0.242	0	1
Have a small child	482	0.705	0.456	0	1
Household head education	482	1.66	2.249	0	11
Spouse education	414	1.734	2.163	0	14
Household head age	482	44.378	13.244	21	87
Spouse age	414	37.795	10.957	15	81
Household head hours worked per week	480	37.16	19.149	0	126
Spouse hours worked per week	412	5.204	13.354	0	84
ln(Total annual expenditure)	482	9.86	0.581	7.57	11.241
ln(Food expenditure)	482	9.489	0.64	6.712	11.094
ln(Total annual expenditure per capita)	482	7.923	0.61	5.49	9.855
ln(Food expenditure per capita)	482	7.552	0.658	4.632	9.352
Engel's coefficient	482	0.71	0.148	0.115	0.966

Note (Continued): For household characteristics, "Number of children" is the number of children in a household, "Family size" is the number of family members, "Have a ST child" is a dummy variable that takes 1 if a household has at least one child eligible for ST and 0 otherwise, "Have a small child" is a dummy variable that takes 1 if a household has at least one child who are younger than 6 so that he/she does not attend school and 0 otherwise", "Household head education" is years of schooling household head completed, "Spouse education" is years of schooling household head's spouse completed, "Household head age" is the age of household head, 'Spouse age" is the age of household head's spouse, "Household head hours worked per week" is the number of hours household head worked in the last month prior to the baseline, "Spouse hours worked per week" is the number of hours household head's spouse worked in the last month prior to the baseline, "ln(Total annual expenditure)" is logarithm of the amount of total household expenditure in the last year prior to the baseline, 'ln(Food expenditure)" is logarithm of the amount of food expenditure in the last year prior to the baseline, "ln(Total annual expenditure per capita)" is logarithm of the amount of total household expenditure per capita in the last year prior to the baseline, "ln(Food expenditure per capita)" is logarithm of the amount of food expenditure per capita in the last year prior to the baseline, and "Engel's coefficient" is Engel's coefficient.

Table 1 (*Continued*) Summary statistics for the control group

	Obs.	Mean	Sd.	Min.	Max.
Treatment Status					
Eligible for ST (Individual)	1,243	0.672	0.470	0	1
Individual Characteristics					
School attendance	1,243	17.46	13.84	0	30
Enrollment	1,243	0.635	0.482	0	1
Works	1,230	0.168	0.374	0	1
Hours worked per week	1,230	5.455	13.76	0	60
1st born	1,243	0.335	0.472	0	1
Years of schooling	1,243	1.684	1.999	0	10
Age	1,243	10.48	3.093	6	16
Male	1,243	0.509	0.500	0	1
Primary school	1,243	0.574	0.495	0	1
Secondary school	1,243	0.0345	0.183	0	1
Household Characteristics					
Number of children	445	4.306	1.748	2	11
Family size	445	7.58	2.51	3	18
Have a ST child	445	0.91	0.286	0	1
Have a small child	445	0.769	0.422	0	1
Household head education	445	1.467	2.024	0	16
Spouse education	370	1.589	2.219	0	14
Household head age	443	44.406	12.817	22	85
Spouse age	370	38.322	10.97	18	92
Household head hours worked per week	441	35.234	19.021	0	84
Spouse hours worked per week	367	5.046	13.977	0	77
ln(Total annual expenditure)	445	9.814	0.595	7.624	11.368
ln(Food expenditure)	445	9.448	0.653	6.48	11.013
ln(Total annual expenditure per capita)	445	7.842	0.612	5.711	9.892
ln(Food expenditure per capita)	445	7.477	0.664	4.559	9.397
Engel's coefficient	445	0.713	0.144	0.099	0.949

Table 2 Balancing test

Sample	Pooled		FT + ST		FT	
	Coefficient	p-value	Coefficient	p-value	Coefficient	p-value
	(1)	(2)	(3)	(4)	(5)	(6)
Individual Characteristics						
School attendance	0.000	0.980	-0.000493	0.850	0.00123	0.533
Enrollment	0.0299	0.563	0.0177	0.772	0.0557	0.232
Works	-0.0427	0.341	-0.0478	0.409	-0.0322	0.604
Hours worked per week	-0.00166	0.120	-0.00220	0.200	-0.00126	0.396
1st born	-0.00284	0.806	0.0112	0.702	-0.00445	0.842
Years of schooling	0.00286	0.836	0.0126	0.645	0.00503	0.633
Age	-0.00344	0.406	-0.00204	0.716	-0.00379	0.721
Male	0.00430	0.846	-0.00508	0.860	0.000884	0.982
Primary school	0.0388	0.355	0.0280	0.606	0.0563	0.148
Secondary school	-0.0666	0.473	-	-	-0.0558	0.491
Household Characteristics						
Number of children	-0.00388	0.729	-0.00459	0.654	-0.00227	0.890
Family size	-0.00206	0.791	-0.00166	0.820	-0.00220	0.846
Have a ST child	0.171*	0.0525	-	-	0.174**	0.0375
Have a small child	-0.0693	0.184	-0.0569	0.294	-0.100	0.106
Household head education	0.0157*	0.0862	0.0196**	0.0482	0.00878	0.441
Spouse education	0.0149	0.110	0.0184*	0.0713	0.00939	0.444
Household head age	0.000633	0.673	0.000943	0.552	0.000264	0.891
Spouse age	-0.000891	0.624	0.000	0.968	-0.00290	0.257
Household head hours worked per week	0.000927	0.513	0.00120	0.461	0.000362	0.785
Spouse hours worked per week	0.00133	0.369	0.00139	0.379	0.00121	0.530
ln(Total annual expenditure)	0.0327	0.502	0.0559	0.269	-0.0130	0.822
ln(Food expenditure)	0.0243	0.537	0.0366	0.367	0.000876	0.986
ln(Total annual expenditure per capita)	0.0414	0.396	0.0646	0.178	-0.00369	0.952
ln(Food expenditure per capita)	0.0319	0.412	0.0442	0.247	0.00684	0.894
Engel's coefficient	-0.0312	0.857	-0.124	0.453	0.130	0.572

Note: "Pooled" for children of age between 6 to 16 at the baseline, "FT + ST" for children of age 6 to 12 who have not completed grade 4 in primary school at the baseline, and "FT" for children who have completed grade 4 in primary school at the baseline. I do OLS regression clustering at *comarcas* level (*comarcas* is the randomization unit) and weighting by population of each *comarcas*, with specifications in Section 5.1. The coefficients and p-values for each variable are reported. The variables are taken from the baseline. See the footnote of Table 1 for the definitions of the variables.
*** p<0.01, ** p<0.05, * p<0.1.

Table 2 (*Continued*) Analysis of attrition

Analysis	Attrition VS Non-attrition		Treatment VS Control	
Sample	All		Attrition	
Dependent variable	$\mathbf{1}\{Attrition\}$		$\mathbf{1}\{Treatment\}$	
	Coefficient	p-value	Coefficient	p-value
	(1)	(2)	(3)	(4)
Individual Characteristics				
Treatment group	-0.0314	0.366	-	-
School attendance	-0.00247**	0.165	-0.00788**	0.0181
Enrollment	-0.0666**	0.0120	-0.172***	0.00272
Works	0.0608**	0.0261	-0.00409	0.958
Hours worked per week	0.00247***	0.00402	0.000302	0.876
1st born	0.0143	0.286	0.0236	0.699
Years of schooling	-0.00688	0.194	-0.0212	0.328
Age	0.00926***	0.000577	0.0178*	0.0714
Male	-0.00597	0.603	0.0356	0.459
Primary school	-0.0651***	0.00472	-0.152**	0.00288
Secondary school	0.0207	0.634	-0.160	0.429
Household Characteristics				
Number of children	0.0125*	0.0943	0.00436	0.873
Family size	0.00816	0.131	-0.00265	0.882
Have a ST child	-0.0437	0.246	-0.0678	0.572
Have a small child	0.0296	0.170	-0.0270	0.805
Household head education	0.00213	0.743	0.0145	0.523
Spouse education	-0.00817	0.138	-0.0312	0.160
Household head age	-0.00138	0.105	-0.00556*	0.0895
Spouse age	0.000383	0.602	-0.00369	0.347
Household head hours worked per week	0.000438	0.549	-0.00219	0.519
Spouse hours worked per week	0.000337	0.533	0.00200	0.447
ln(Total annual expenditure)	-0.0143	0.472	-0.0443	0.664
ln(Food expenditure)	-0.0114	0.491	-0.0968	0.243
ln(Total annual expenditure per capita)	-0.0313*	0.0992	-0.0439	0.619
ln(Food expenditure per capita)	-0.0251*	0.0799	-0.103	0.139
Engel's coefficient	-0.0132	0.874	-0.692**	0.0162

Note: "All" for children of age between 6 to 16 at the baseline without the sample restrictions in Section 4.1, and "Attrition" for children of age 6 to 16 who are observed at the baseline but not at the first follow-up after the restrictions. I do OLS regression clustering at *comarcas* level (*comarcas* is the randomization unit) and weighting by population of each *comarcas*. See the footnote of Table 1 for the definitions of the variables.
*** p<0.01, ** p<0.05, * p<0.1.

Table 3 Birth order effect before the interventions: School outcomes

Sample	Pooled		FT + ST		FT	
Dependent variable	School attendance	Enrollment	School attendance	Enrollment	School attendance	Enrollment
	(1)	(2)	(3)	(4)	(5)	(6)
1st born	-2.179***	-0.110***	-0.271	-0.0547*	-1.548	-0.0623*
	(0.691)	(0.0273)	(0.795)	(0.0295)	(0.956)	(0.0355)
Constant	16.40***	0.520***	13.09***	0.421***	40.51***	1.376***
	(4.764)	(0.173)	(4.577)	(0.142)	(6.391)	(0.235)
Observations	2,215	2,215	1,511	1,511	704	704
R-squared	0.514	0.530	0.593	0.646	0.769	0.790
2nd or later born mean	19.137	0.701	18.330	0.678	22.283	0.789
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Note: Cluster (*comarcas*) robust standard errors in parentheses. The control variables are individual age, the dummy for being male, years of schooling, the number of children living in the same households, family size, the dummy for having a ST eligible child (only for (1), (2), (5), and (6)), logarithm of the amount of total household expenditure, household head's education, household head's spouse's education, household head's age, household head's spouse's age, and absolute age spacing to each sibling in the same household. See the footnote of Table 1 for the definitions of the variables and that of Table 2 for analysis sample.
*** p<0.01, ** p<0.05, * p<0.1.

Table 3 (*Continued*) Birth order effect before the interventions: Labor outcomes

Sample	Pooled		FT + ST		FT	
Dependent variable	Hours worked	Works	Hours worked	Works	Hours worked	Works
	(1)	(2)	(3)	(4)	(5)	(6)
1st born	2.854***	0.0575*	0.409	-0.00112	4.237**	0.116**
	(0.864)	(0.0313)	(0.931)	(0.0335)	(1.827)	(0.0525)
Constant	-17.84**	-0.558**	-6.385	-0.332**	-41.11*	-1.223**
	(7.511)	(0.215)	(4.178)	(0.140)	(22.29)	(0.604)
Observations	2,204	2,204	1,504	1,504	700	700
R-squared	0.289	0.296	0.201	0.243	0.339	0.345
2nd or later born mean	2.809	0.0999	2.125	0.0819	5.491	0.171
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Note: Cluster (*comarcas*) robust standard errors in parentheses. The control variables are individual age, the dummy for being male, years of schooling, the number of children living in the same households, family size, the dummy for having a ST eligible child (only for (1), (2), (5), and (6)), logarithm of the amount of total household expenditure, household head's education, household head's spouse's education, household head's age, household head's spouse's age, and absolute age spacing to each sibling in the same household. See the footnote of Table 1 for the definitions of the variables and that of Table 2 for analysis sample.
*** p<0.01, ** p<0.05, * p<0.1.

Table 4 Total effect on birth order effect: School outcomes

Dependent variable	School attendance		Grade progression		Enrollment	
	(1)	(2)	(3)	(4)	(5)	(6)
FT + ST	8.789***	5.365***	0.330***	0.238***	0.270***	0.157***
	(1.503)	(0.881)	(0.0528)	(0.0575)	(0.0473)	(0.0258)
1st born	0.705	0.729	0.0960**	0.150*	0.0404	0.0469
	(1.107)	(1.352)	(0.0474)	(0.0818)	(0.0360)	(0.0394)
(FT + ST) × 1st born	-2.083	-1.482	-0.208***	-0.167*	-0.0733	-0.0562
	(1.335)	(1.093)	(0.0691)	(0.0882)	(0.0437)	(0.0364)
Constant	19.65***	12.00*	0.383***	0.469	0.697***	0.525**
	(1.465)	(7.084)	(0.0389)	(0.316)	(0.0465)	(0.219)
Observations	1,433	1,244	1,433	1,244	1,433	1,244
R-squared	0.149	0.516	0.090	0.329	0.127	0.505
Control mean		21.26		0.429		0.751
Controls	No	Yes	No	Yes	No	Yes
Prob. (No treatment effect on 1st born)	0.00157	0.00438	0.0916	0.265	0.00238	0.0152

Note: Cluster (*comarcas*) robust standard errors in parentheses. For "FT + ST", the indicator variable for treatment group is used. The control variables are individual age, the dummy for being male, years of schooling, the number of children living in the same households, family size, logarithm of the amount of total household expenditure, household head's education, household head's spouse's education, household head's age, household head's spouse's age, and absolute age spacing to each sibling in the same household. See the footnote of Table 1 for the definitions of the variables. I do the F-tests in each regression with the null hypothesis that (FT + ST) + {(FT + ST) × 1st born} = 0. The corresponding p-values are reported in the bottom row.

*** p<0.01, ** p<0.05, * p<0.1.

Table 4 (*Continued*) Total effect on birth order effect: Labor outcomes

Dependent variable	Hours worked		Works	
	(1)	(2)	(3)	(4)
FT + ST	-2.147***	-0.818	-0.0521***	-0.0206
	(0.525)	(0.544)	(0.0178)	(0.0179)
1st born	0.783	2.463*	0.0252	0.0676*
	(0.955)	(1.248)	(0.0301)	(0.0394)
(FT + ST) × 1st born	-0.557	-0.329	-0.0252	-0.0107
	(1.097)	(0.963)	(0.0364)	(0.0371)
Constant	2.941***	-4.993	0.0941***	-0.142
	(0.483)	(5.356)	(0.0144)	(0.183)
Observations	1,433	1,244	1,433	1,244
R-squared	0.022	0.214	0.014	0.174
Control mean		2.511		0.0843
Controls	No	Yes	No	Yes
Prob. (No treatment effect on 1st born)	0.0276	0.321	0.0440	0.434

Note: Cluster (*comarcas*) robust standard errors in parentheses. For "FT + ST", the indicator variable for treatment group is used. The control variables are individual age, the dummy for being male, years of schooling, the number of children living in the same households, family size, logarithm of the amount of total household expenditure, household head's education, household head's spouse's education, household head's age, household head's spouse's age, and absolute age spacing to each sibling in the same household. See the footnote of Table 1 for the definitions of the variables. I do the F-tests in each regression with the null hypothesis that (FT + ST)+ {(FT + ST)×1st born} = 0. The corresponding p-values are reported in the bottom row.

*** p<0.01, ** p<0.05, * p<0.1.

Table 5 Wealth effect on birth order effect: School outcomes

Dependent variable	School attendance		Grade progression		Enrollment	
	(1)	(2)	(3)	(4)	(5)	(6)
FT	4.661***	2.472***	0.119**	0.0669*	0.130***	0.0580**
	(1.345)	(0.808)	(0.0489)	(0.0336)	(0.0431)	(0.0277)
1st born	-7.488***	-2.801***	-0.244***	-0.0638**	-0.259***	-0.103***
	(0.925)	(0.823)	(0.0336)	(0.0257)	(0.0335)	(0.0280)
FT × 1st born	-0.146	0.905	-0.0161	-0.00532	0.0180	0.0542
	(1.562)	(0.876)	(0.0505)	(0.0430)	(0.0531)	(0.0333)
Constant	20.71***	40.39***	0.677***	1.408***	0.734***	1.337***
	(1.149)	(7.723)	(0.0402)	(0.348)	(0.0367)	(0.268)
Observations	1,124	971	1,124	971	1,124	971
R-squared	0.105	0.744	0.081	0.676	0.094	0.755
Control mean		17.335		0.552		0.615
Controls	No	Yes	No	Yes	No	Yes
Prob. (No treatment effect on 1st born)	0.00347	0.000103	0.0463	0.0664	0.00462	0.000232

Note: Cluster (*comarcas*) robust standard errors in parentheses. For "FT", the indicator variable for treatment group is used. The control variables are individual age, the dummy for being male, years of schooling, the number of children living in the same households, family size, the dummy for having a ST child, logarithm of the amount of total household expenditure, household head's education, household head's spouse's education, household head's age, household head's spouse's age, and absolute age spacing to each sibling in the same household. See the footnote of Table 1 for the definitions of the variables. I do the F-tests in each regression with the null hypothesis that FT + {FT × 1st born} = 0. The corresponding p-values are reported in the bottom row.

*** p<0.01, ** p<0.05, * p<0.1.

Table 5 (*Continued*) Wealth effect on birth order effect: Labor outcomes

Dependent variable	Hours worked		Works	
	(1)	(2)	(3)	(4)
FT	-4.854***	-3.718***	-0.125***	-0.105**
	(1.379)	(1.375)	(0.0352)	(0.0406)
1st born	8.428***	6.294***	0.165***	0.108***
	(0.908)	(1.211)	(0.0267)	(0.0298)
FT × 1st born	-0.951	-0.915	0.00148	0.00314
	(1.590)	(1.215)	(0.0424)	(0.0349)
Constant	8.893***	-38.64**	0.260***	-0.923**
	(1.249)	(17.35)	(0.0299)	(0.369)
Observations	1,124	971	1,124	971
R-squared	0.067	0.387	0.055	0.381
Control mean		12.120		0.319
Controls	No	Yes	No	Yes
Prob. (No treatment effect on 1st born)	0.00389	0.00507	0.00845	0.00701

Note: Cluster (*comarcas*) robust standard errors in parentheses. For "FT", the indicator variable for treatment group is used. The control variables are individual age, the dummy for being male, years of schooling, the number of children living in the same households, family size, the dummy for having a ST child, logarithm of the amount of total household expenditure, household head's education, household head's spouse's education, household head's age, household head's spouse's age, and absolute age spacing to each sibling in the same household. See the footnote of Table 1 for the definitions of the variables. I do the F-tests in each regression with the null hypothesis that FT + {FT × 1st born} = 0. The corresponding p-values are reported in the bottom row.
*** p<0.01, ** p<0.05, * p<0.1.

Table 6 Condition effect on birth order effect, given wealth effect: School outcomes

Dependent variable	School attendance		Grade progression		Enrollment	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment group	4.661***	2.131**	0.119**	0.0717**	0.130***	0.0479
	(1.345)	(0.829)	(0.0489)	(0.0338)	(0.0431)	(0.0287)
Eligible for ST	-1.061	3.550***	-0.294***	-0.0787	-0.0378	0.128***
	(1.320)	(0.904)	(0.0568)	(0.0529)	(0.0401)	(0.0288)
1st born	-7.488***	-2.511***	-0.244***	-0.0488	-0.259***	-0.0926***
	(0.925)	(0.657)	(0.0336)	(0.0359)	(0.0335)	(0.0235)
Treatment group × Eligible for ST	4.128***	4.664***	0.211***	0.211***	0.140***	0.156***
	(1.490)	(1.377)	(0.0710)	(0.0675)	(0.0462)	(0.0435)
Treatment group × Eligible for ST	-1.937	-2.685*	-0.192*	-0.194*	-0.0913	-0.122**
× 1st born	(2.224)	(1.473)	(0.103)	(0.110)	(0.0758)	(0.0493)
Constant	20.71***	17.72**	0.677***	0.708***	0.734***	0.662***
	(1.149)	(6.882)	(0.0402)	(0.250)	(0.0367)	(0.211)
Observations	2,557	2,215	2,557	2,215	2,557	2,215
R-squared	0.160	0.611	0.089	0.425	0.144	0.615
Control mean		19.545		0.483		0.692
Controls	No	Yes	No	Yes	No	Yes

Note: Cluster (*comarcas*) robust standard errors in parentheses. The control variables are individual age, the dummy for being male, years of schooling, the number of children living in the same households, family size, the dummy for having a ST child, logarithm of the amount of total household expenditure, household head's education, household head's spouse's education, household head's age, household head's spouse's age, and absolute age spacing to each sibling in the same household. See the footnote of Table 1 for the definitions of the variables.
*** p<0.01, ** p<0.05, * p<0.1.

Table 6 (*Continued*) Condition effect on birth order effect, given wealth effect: Labor outcomes

Dependent variable	Hours worked		Works	
	(1)	(2)	(3)	(4)
Treatment group	-4.854***	-3.384**	-0.125***	-0.0900**
	(1.379)	(1.371)	(0.0352)	(0.0413)
Eligible for ST	-5.952***	-3.861**	-0.165***	-0.0790*
	(1.293)	(1.651)	(0.0285)	(0.0448)
1st born	8.428***	5.701***	0.165***	0.0998***
	(0.908)	(1.085)	(0.0267)	(0.0357)
Treatment × Eligible for ST	2.707*	2.560	0.0733**	0.0625
	(1.444)	(1.527)	(0.0358)	(0.0470)
Treatment × Eligible for ST	0.393	1.026	-0.0266	0.00264
× 1st born	(1.626)	(1.377)	(0.0481)	(0.0474)
Constant	8.893***	-14.14*	0.260***	-0.376*
	(1.250)	(8.259)	(0.0299)	(0.210)
Observations	2,557	2,215	2,557	2,215
R-squared	0.139	0.356	0.118	0.322
Control mean		6.709		0.187
Controls	No	Yes	No	Yes

Note: Cluster (*comarcas*) robust standard errors in parentheses. The control variables are individual age, the dummy for being male, years of schooling, the number of children living in the same households, family size, the dummy for having a ST child, logarithm of the amount of total household expenditure, household head's education, household head's spouse's education, household head's age, household head's spouse's age, and absolute age spacing to each sibling in the same household. See the footnote of Table 1 for the definitions of the variables.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 7 Robustness check: Wealth effect on those with *Engel's coefficient* > *Median*

Dependent variable	School attendance	Grade progression	Enrollment	Hours worked	Works
	(1)	(2)	(3)	(4)	(5)
FT	2.769**	0.114**	0.0621	-6.843**	-0.188**
	(1.170)	(0.0541)	(0.0465)	(2.761)	(0.0792)
1st born	-3.883***	-0.0821	-0.140***	7.013***	0.0712
	(1.427)	(0.0572)	(0.0512)	(2.446)	(0.0539)
FT × 1st born	0.951	-0.0363	0.0480	0.255	0.0730
	(1.688)	(0.0725)	(0.0694)	(3.386)	(0.0811)
Constant	35.53***	1.334***	1.137***	-40.94	-0.515
	(9.341)	(0.393)	(0.332)	(28.45)	(0.533)
Observations	427	427	427	427	427
R-squared	0.756	0.668	0.751	0.396	0.389
Controls	Yes	Yes	Yes	Yes	Yes
Prob. (No treatment effect on 1st born)	0.00630	0.148	0.0446	0.0357	0.0756

Note: Cluster (*comarcas*) robust standard errors in parentheses. For "FT", the indicator variable for treatment group is used. The sample are children from households with their Engel's coefficient higher than the sample median among those who have competed grade 4 in primary school at baseline. The control variables are individual age, the dummy for being male, years of schooling, the number of children living in the same households, family size, the dummy for having a ST child, logarithm of the amount of total household expenditure, household head's education, household head's spouse's education, household head's age, household head's spouse's age, and absolute age spacing to each sibling in the same household. See the footnote of Table 1 for the definitions of the variables. I do the F-tests in each regression with the null hypothesis that FT + (FT × 1st born) = 0. The corresponding p-values are reported in the bottom row.
*** p<0.01, ** p<0.05, * p<0.1.

Table 7 (*Continued*) Wealth effect on those with *Household head's education = 0*

Dependent variable	School attendance	Grade progression	Enrollment	Hours worked	Works
	(1)	(2)	(3)	(4)	(5)
FT	1.997*	0.0883**	0.0489	-4.783**	-0.123*
	(1.119)	(0.0434)	(0.0395)	(2.315)	(0.0615)
1st born	-3.632***	-0.0674	-0.142***	6.531**	0.0716
	(1.208)	(0.0458)	(0.0403)	(2.680)	(0.0591)
FT × 1st born	1.785	-0.0297	0.0704	-0.290	0.0343
	(1.257)	(0.0615)	(0.0486)	(2.463)	(0.0565)
Constant	50.39***	1.500***	1.625***	-32.80	-0.665
	(12.89)	(0.503)	(0.459)	(26.06)	(0.550)
Observations	469	469	469	469	469
R-squared	0.746	0.678	0.743	0.426	0.423
Controls	Yes	Yes	Yes	Yes	Yes
Prob. (No treatment effect on 1st born)	0.000437	0.191	0.00281	0.0577	0.0582

Note: Cluster (*comarcas*) robust standard errors in parentheses. For "FT", the indicator variable for treatment group is used. The sample are children from households with their Engel's coefficient higher than the sample median among those who have competed grade 4 in primary school at baseline. The control variables are individual age, the dummy for being male, years of schooling, the number of children living in the same households, family size, the dummy for having a ST child, logarithm of the amount of total household expenditure, household head's education, household head's spouse's education, household head's age, household head's spouse's age, and absolute age spacing to each sibling in the same household. See the footnote of Table 1 for the definitions of the variables. I do the F-tests in each regression with the null hypothesis that FT + (FT × 1st born) = 0. The corresponding p-values are reported in the bottom row.
*** p<0.01, ** p<0.05, * p<0.1.

Table 8 Total effect on absolute birth order effect

Dependent variable	School attendance	Grade progression	Enrollment	Hours worked	Works
	(1)	(2)	(3)	(4)	(5)
FT + ST	4.671***	0.0941	0.120***	-2.607*	-0.0694*
	(1.170)	(0.0593)	(0.0408)	(1.368)	(0.0402)
Absolute birth order	0.153	-0.0278	-0.00402	-1.531**	-0.0477**
	(0.372)	(0.0217)	(0.0124)	(0.635)	(0.0195)
(FT + ST) × Absolute birth order	0.156	0.0408	0.00967	0.597*	0.0160
	(0.372)	(0.0250)	(0.0138)	(0.319)	(0.00982)
Constant	12.26*	0.617*	0.563**	-0.910	-0.0210
	(7.072)	(0.335)	(0.222)	(4.637)	(0.163)
Observations	1,244	1,244	1,244	1,244	1,244
R-squared	0.516	0.327	0.504	0.223	0.184
Controls	Yes	Yes	Yes	Yes	Yes
Prob. (Birth order effect unchanged)	0.000	0.00409	0.000144	0.0677	0.0975

Note: Cluster (*comarcas*) robust standard errors in parentheses. For "FT + ST", the indicator variable for treatment group is used. "Absolute birth order" is the absolute birth order that assigns 1 to the earliest child in the household. The control variables are individual age, the dummy for being male, years of schooling, the number of children living in the same households, family size, logarithm of the amount of total household expenditure, household head's education, household head's spouse's education, household head's age, household head's spouse's age, and absolute age spacing to each sibling in the same household. See the footnote of Table 1 for the definitions of the variables. I do the F-tests in each regression with the null hypothesis that (FT + ST) + {(FT + ST) × Absolute birth order} = 0. The corresponding p-values are reported in the bottom row.
*** p<0.01, ** p<0.05, * p<0.1.

Table 8 (*Continued*) Wealth effect on absolute birth order effect

Dependent variable	School attendance	Grade progression	Enrollment	Hours worked	Works
	(1)	(2)	(3)	(4)	(5)
FT	4.796***	0.0912**	0.157***	-5.871***	-0.134***
	(1.045)	(0.0442)	(0.0388)	(1.792)	(0.0441)
Absolute birth order	1.383***	0.0131	0.0432**	-1.421*	-0.0377*
	(0.499)	(0.0158)	(0.0162)	(0.763)	(0.0196)
FT × Absolute birth order	-1.056*	-0.0149	-0.0404**	0.925	0.0170
	(0.541)	(0.0225)	(0.0196)	(0.712)	(0.0199)
Constant	37.69***	1.384***	1.248***	-35.05**	-0.841**
	(7.763)	(0.346)	(0.273)	(17.12)	(0.364)
Observations	971	971	971	971	971
R-squared	0.744	0.675	0.754	0.380	0.377
Controls	Yes	Yes	Yes	Yes	Yes
Prob. (Birth order effect unchanged)	0.000	0.0105	0.000	0.00129	0.00162

Note: Cluster (*comarcas*) robust standard errors in parentheses. For "FT", the indicator variable for treatment group is used. "Absolute birth order" is the absolute birth order that assigns 1 to the earliest child in the household. The control variables are individual age, the dummy for being male, years of schooling, the number of children living in the same households, family size, logarithm of the amount of total household expenditure, household head's education, household head's spouse's education, household head's age, household head's spouse's age, and absolute age spacing to each sibling in the same household. See the footnote of Table 1 for the definitions of the variables. I do the F-tests in each regression with the null hypothesis that FT + (FT × Absolute birth order) = 0. The corresponding p-values are reported in the bottom row.

*** p<0.01, ** p<0.05, * p<0.1.

Table 8 (*Continued*) Condition effect on absolute birth order effect

Dependent variable	School attendance	Grade progression	Enrollment	Hours worked	Works
	(1)	(2)	(3)	(4)	(5)
Treatment group	4.240***	0.0816*	0.138***	-5.409***	-0.122***
	(0.967)	(0.0413)	(0.0367)	(1.758)	(0.0425)
Eligible for ST	6.664***	0.0651	0.256***	-6.218***	-0.119**
	(1.123)	(0.0506)	(0.0458)	(2.207)	(0.0490)
Absolute birth order	1.268***	0.0125	0.0399***	-1.943***	-0.0501**
	(0.381)	(0.0161)	(0.0132)	(0.702)	(0.0217)
Treatment × Eligible for ST	1.963	0.0465	0.0306	2.588	0.0525
	(1.652)	(0.0682)	(0.0617)	(2.269)	(0.0516)
Treatment group × Eligible for ST	1.036	0.0490	0.0438*	-0.0581	-0.00135
× Absolute birth order	(0.633)	(0.0320)	(0.0223)	(0.826)	(0.0218)
Constant	14.88**	0.701**	0.573***	-8.932	-0.251
	(6.731)	(0.260)	(0.207)	(8.152)	(0.207)
Observations	2,215	2,215	2,215	2,215	2,215
R-squared	0.611	0.422	0.614	0.353	0.322
Controls	Yes	Yes	Yes	Yes	Yes

Note: Cluster (*comarcas*) robust standard errors in parentheses. “Absolute birth order” is the absolute birth order that assigns 1 to the earliest child in the household. The control variables are individual age, the dummy for being male, years of schooling, the number of children living in the same households, family size, logarithm of the amount of total household expenditure, household head’s education, household head’s spouse’s education, household head’s age, household head’s spouse’s age, and absolute age spacing to each sibling in the same household. See the footnote of Table 1 for the definitions of the variables.

*** p<0.01, ** p<0.05, * p<0.1.

Table 9 Heterogeneity by share of ST eligible children: Total effect

Dependent variable	School attendance	Grade progression	Enrollment	Hours worked	Works
	(1)	(2)	(3)	(4)	(5)
FT + ST	3.155***	0.186**	0.0969***	0.965	0.00960
	(1.134)	(0.0716)	(0.0322)	(0.689)	(0.0255)
1st born	4.009	0.315*	0.192*	2.587*	0.0670
	(3.071)	(0.167)	(0.0997)	(1.354)	(0.0429)
(FT + ST) × 1st born	-4.287	-0.0911	-0.155	-0.861	-0.0121
	(3.021)	(0.168)	(0.0985)	(0.881)	(0.0336)
Share of ST children > Median	-1.604	-0.0401	-0.0401	2.417***	0.0698**
	(1.109)	(0.0631)	(0.0315)	(0.793)	(0.0304)
(FT + ST) × 1st born	2.726	-0.125	0.0965	1.029	0.00743
× (Share of ST children > Median)	(3.385)	(0.191)	(0.106)	(1.550)	(0.0612)
Constant	14.68*	0.481	0.586**	-8.655	-0.244
	(7.647)	(0.333)	(0.245)	(5.634)	(0.183)
Observations	1,244	1,244	1,244	1,244	1,244
R-squared	0.522	0.337	0.510	0.220	0.181
Controls	Yes	Yes	Yes	Yes	Yes

Note: Cluster (*comarcas*) robust standard errors in parentheses. For "FT + ST", the indicator variable for treatment group is used. "Share of ST children > Median" is a dummy variable that takes 1 if the share of ST eligible children in the households is higher than the sample median and 0 otherwise. The control variables are individual age, the dummy for being male, years of schooling, the number of children living in the same households, family size, logarithm of the amount of total household expenditure, household head's education, household head's spouse's education, household head's age, household head's spouse's age, and absolute age spacing to each sibling in the same household. See the footnote of Table 1 for the definitions of the variables.
*** p<0.01, ** p<0.05, * p<0.1.

Table 9 (*Continued*) Heterogeneity by share of ST eligible children: Wealth effect

Dependent variable	School attendance	Grade progression	Enrollment	Hours worked	Works
	(1)	(2)	(3)	(4)	(5)
FT	2.065**	0.0518	0.0472	-3.772**	-0.121**
	(0.905)	(0.0351)	(0.0320)	(1.457)	(0.0453)
1st born	-2.896***	-0.0772***	-0.102***	6.720***	0.0929*
	(0.726)	(0.0284)	(0.0299)	(1.869)	(0.0470)
FT × 1st born	0.520	-0.0114	0.0480	0.192	0.0504
	(0.894)	(0.0383)	(0.0380)	(1.713)	(0.0459)
Share of ST children > Median	-1.000	-0.0651	-0.0108	1.237	-0.0204
	(0.983)	(0.0497)	(0.0390)	(2.578)	(0.0684)
FT × 1st born	-0.340	-0.0207	-0.0261	-3.547	-0.176
× (Share of ST children > Median)	(2.417)	(0.112)	(0.0848)	(4.080)	(0.122)
Constant	41.39***	1.444***	1.362***	-38.50**	-0.905**
	(7.800)	(0.343)	(0.269)	(16.54)	(0.352)
Observations	971	971	971	971	971
R-squared	0.747	0.678	0.756	0.390	0.383
Controls	Yes	Yes	Yes	Yes	Yes

Note: Cluster (*comarcas*) robust standard errors in parentheses. For "FT", the indicator variable for treatment group is used. "Share of ST children > Median" is a dummy variable that takes 1 if the share of ST eligible children in the households is higher than the sample median and 0 otherwise. The control variables are individual age, the dummy for being male, years of schooling, the number of children living in the same households, family size, logarithm of the amount of total household expenditure, household head's education, household head's spouse's education, household head's age, household head's spouse's age, and absolute age spacing to each sibling in the same household. See the footnote of Table 1 for the definitions of the variables.
*** p<0.01, ** p<0.05, * p<0.1.

Table 9 (*Continued*) Heterogeneity by share of ST eligible children: Condition effect

Dependent variable	School attendance	Grade progression	Enrollment	Hours worked	Works
	(1)	(2)	(3)	(4)	(5)
Treatment group	1.807*	0.0583	0.0390	-3.425**	-0.107**
	(0.953)	(0.0347)	(0.0332)	(1.538)	(0.0482)
Eligible for ST	5.838***	-0.00845	0.197***	-6.013***	-0.148**
	(1.327)	(0.0652)	(0.0393)	(1.847)	(0.0591)
1st born	-3.216***	-0.0858**	-0.111***	6.156***	0.0870*
	(0.724)	(0.0423)	(0.0277)	(1.722)	(0.0506)
Treatment × Eligible for ST	2.116	0.157*	0.0835**	4.793***	0.121**
	(1.473)	(0.0790)	(0.0410)	(1.761)	(0.0576)
Treatment × Eligible for ST	-4.738	-0.0798	-0.200	-1.537	-0.0709
× 1st born	(3.637)	(0.176)	(0.121)	(2.303)	(0.0778)
Share of ST children > Median	-0.808	-0.0642	-0.00842	1.731	-0.00758
	(0.891)	(0.0521)	(0.0398)	(2.997)	(0.0774)
Treatment group × Eligible for ST	2.702	-0.135	0.111	6.400	0.232
× 1st born × (Share of ST children > Median)	(4.402)	(0.229)	(0.153)	(4.924)	(0.156)
Constant	19.93***	0.746***	0.719***	-16.21*	-0.414**
	(6.958)	(0.251)	(0.219)	(8.033)	(0.203)
Observations	2,215	2,215	2,215	2,215	2,215
R-squared	0.617	0.430	0.619	0.361	0.326
Controls	Yes	Yes	Yes	Yes	Yes

Note: Cluster (*comarcas*) robust standard errors in parentheses. “Share of ST children > Median” is a dummy variable that takes 1 if the share of ST eligible children in the households is higher than the sample median and 0 otherwise. The control variables are individual age, the dummy for being male, years of schooling, the number of children living in the same households, family size, logarithm of the amount of total household expenditure, household head’s education, household head’s spouse’s education, household head’s age, household head’s spouse’s age, and absolute age spacing to each sibling in the same household. See the footnote of Table 1 for the definitions of the variables.

*** p<0.01, ** p<0.05, * p<0.1.

Table 9 (*Continued*) Heterogeneity by sex: Total effect

Dependent variable	School attendance	Grade progression	Enrollment	Hours worked	Works
	(1)	(2)	(3)	(4)	(5)
FT + ST	5.424***	0.255***	0.158***	0.895*	0.0272*
	(0.941)	(0.0599)	(0.0283)	(0.467)	(0.0161)
1st born	0.177	0.115	0.0144	1.428	0.0357
	(1.677)	(0.0989)	(0.0581)	(1.230)	(0.0373)
(FT + ST) × 1st born	-1.322	-0.0902	-0.0328	0.771	0.0115
	(1.632)	(0.108)	(0.0578)	(0.954)	(0.0245)
Male	-0.493	0.0372	-0.00603	4.501***	0.148***
	(0.635)	(0.0289)	(0.0200)	(0.866)	(0.0291)
(FT + ST) × 1st born × Male	-0.206	-0.160	-0.0423	-2.238	-0.0431
	(2.358)	(0.118)	(0.0949)	(2.148)	(0.0674)
Constant	12.29*	0.478	0.541**	-4.692	-0.131
	(7.191)	(0.306)	(0.228)	(5.569)	(0.190)
Observations	1,244	1,244	1,244	1,244	1,244
R-squared	0.516	0.331	0.505	0.229	0.185
Controls	Yes	Yes	Yes	Yes	Yes

Note: Cluster (*comarcas*) robust standard errors in parentheses. For "FT + ST", the indicator variable for treatment group is used. "Share of ST children > Median" is a dummy variable that takes 1 if the share of ST eligible children in the households is higher than the sample median and 0 otherwise. The control variables are individual age, the dummy for being male, years of schooling, the number of children living in the same households, family size, logarithm of the amount of total household expenditure, household head's education, household head's spouse's education, household head's age, household head's spouse's age, and absolute age spacing to each sibling in the same household. See the footnote of Table 1 for the definitions of the variables.
*** p<0.01, ** p<0.05, * p<0.1.

Table 9 (*Continued*) Heterogeneity by sex: Wealth effect

Dependent variable	School attendance	Grade progression	Enrollment	Hours worked	Works
	(1)	(2)	(3)	(4)	(5)
FT	1.691	0.0555	0.0425	-0.934	-0.0441
	(1.296)	(0.0576)	(0.0464)	(1.417)	(0.0380)
1st born	-3.686***	-0.121***	-0.125***	3.266	0.0160
	(1.234)	(0.0392)	(0.0391)	(2.545)	(0.0515)
FT × 1st born	2.066	0.0291	0.0820	-1.756	0.0256
	(1.524)	(0.0563)	(0.0527)	(3.032)	(0.0672)
Male	-0.532	-0.00862	0.0170	12.58***	0.357***
	(0.941)	(0.0389)	(0.0271)	(1.885)	(0.0545)
FT × 1st born × Male	-2.266	-0.0687	-0.0544	1.464	-0.0473
	(1.801)	(0.0729)	(0.0601)	(4.872)	(0.123)
Constant	41.14***	1.433***	1.352***	-39.86**	-0.962**
	(7.711)	(0.364)	(0.272)	(17.66)	(0.370)
Observations	971	971	971	971	971
R-squared	0.745	0.678	0.755	0.398	0.392
Controls	Yes	Yes	Yes	Yes	Yes

Note: Cluster (*comarcas*) robust standard errors in parentheses. For "FT", the indicator variable for treatment group is used. "Share of ST children > Median" is a dummy variable that takes 1 if the share of ST eligible children in the households is higher than the sample median and 0 otherwise. The control variables are individual age, the dummy for being male, years of schooling, the number of children living in the same households, family size, logarithm of the amount of total household expenditure, household head's education, household head's spouse's education, household head's age, household head's spouse's age, and absolute age spacing to each sibling in the same household. See the footnote of Table 1 for the definitions of the variables.
*** p<0.01, ** p<0.05, * p<0.1.

Table 9 (*Continued*) Heterogeneity by sex: Condition effect

Dependent variable	School attendance	Grade progression	Enrollment	Hours worked	Works
	(1)	(2)	(3)	(4)	(5)
Treatment group	1.435	0.0622	0.0343	-0.903	-0.0368
	(1.344)	(0.0583)	(0.0481)	(1.222)	(0.0337)
Eligible for ST	3.960***	-0.0826	0.154***	0.489	0.0348
	(0.956)	(0.0655)	(0.0353)	(1.284)	(0.0336)
1st born	-3.481***	-0.103**	-0.117***	2.614	0.0104
	(0.991)	(0.0426)	(0.0346)	(2.261)	(0.0467)
Treatment group × Eligible for ST	5.361***	0.236**	0.168***	1.538	0.0519
	(1.774)	(0.0876)	(0.0622)	(1.334)	(0.0382)
Treatment group × Eligible for ST	-3.761	-0.161	-0.127	2.311	-0.0142
× 1st born	(2.408)	(0.132)	(0.0817)	(3.037)	(0.0731)
Male	-0.0497	0.0174	0.0331	12.61***	0.362***
	(0.982)	(0.0373)	(0.0307)	(1.903)	(0.0548)
Treatment group × Eligible for ST	2.041	-0.0782	0.00679	-4.163	-0.00845
× 1st born × Male	(3.307)	(0.154)	(0.127)	(5.422)	(0.144)
Constant	17.76**	0.718***	0.655***	-18.78*	-0.498**
	(6.949)	(0.246)	(0.215)	(9.394)	(0.235)
Observations	2,215	2,215	2,215	2,215	2,215
R-squared	0.613	0.427	0.617	0.398	0.363
Controls	Yes	Yes	Yes	Yes	Yes

Note: Cluster (*comarcas*) robust standard errors in parentheses. “Share of ST children > Median” is a dummy variable that takes 1 if the share of ST eligible children in the households is higher than the sample median and 0 otherwise. The control variables are individual age, the dummy for being male, years of schooling, the number of children living in the same households, family size, logarithm of the amount of total household expenditure, household head’s education, household head’s spouse’s education, household head’s age, household head’s spouse’s age, and absolute age spacing to each sibling in the same household. See the footnote of Table 1 for the definitions of the variables.

*** p<0.01, ** p<0.05, * p<0.1.