Full-Day School Program and Higher Education: Evidence from the Peruvian Program

Abstract

Full-day school programs aim to promote development by improving students' learning outcomes. We estimate the impact of Peru's full-day secondary school reform, *Jornada Escolar Completa* (JEC), on higher education attainment. Using data from the Young Lives Study, we construct a grouped-sibling sample linking younger siblings who could attend JEC secondary schools with their older siblings who were not exposed. The identification strategy relies on within-household variation and a difference-in-differences framework comparing changes in higher education attainment between households affected and not affected by the JEC program. We find that JEC increases the likelihood of pursuing higher education, with effects concentrated in technical rather than university studies. Although differences are not statistically significant, larger gains among students from disadvantaged backgrounds suggest the potential of JEC to narrow inequalities in access to higher education. Time-use data suggest that JEC students remain engaged in learning, showing no increase in absenteeism or decline in study time at home. Rather, they compensate for longer school days by cutting back on leisure and work activities. Overall, the evidence indicates that extending instructional time increases educational attainment, but its effects on upward mobility may remain limited without complementary measures that ease access to university education.

Keywords:

Full-day school

Higher education attainment

Longer school-day hours

Time allocation

Highlights:

- Peru's full-day school program (JEC) increases the likelihood of higher education attainment.
- The effect of JEC is concentrated in technical rather than university studies.
- Evidence suggests the potential of JEC to narrow inequalities in access to higher education.
- JEC students remain engaged in learning, with no increase in absenteeism or decline in study time at home.

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1. Introduction

To enhance educational outcomes, policymakers have long debated the optimal length of the school-day as a means to improve the quality of education (Bellei, 2009; Dağli, 2019; Patall et al., 2010). Although international evidence remains inconclusive (Dağli, 2019; Patall et al., 2010), 15 out of 26 countries in the Latin America and the Caribbean (LAC) region have implemented full-day school (FDS) programs (Arias et al., 2024). Those FDS programs are heterogeneous in design. Some programs increase instructional time only, either by adding hours to the school day or by adding days to the school year, while others combine longer schedules with improvements in teaching and learning resources. Additional instructional time may be distributed between academic and non-academic activities. Likewise, FDS programs were implemented either at the primary level or at both primary and secondary levels.[1] In this paper, we examine an FDS program implemented in Peru that features longer daily instructional hours allocated to academic subjects alongside additional teaching and learning resources (MINEDU, 2014). Besides, the FDS program implemented in Peru is the only one among LAC countries that is limited to secondary education (Arias et al., 2024).

In Peru, the government implemented an educational reform program known as *Jornada Escolar Completa* (JEC) to close the gap between private and public schools. The results of national and international standardized exams have historically indicated that students in private schools outperform their peers in public schools.[2] In particular, the JEC program aims to mimic some features of private schools, such as longer school hours (Agüero et al., 2021). Schools that adopted JEC shifted from offering nearly one hour less of daily

instruction than private schools to exceeding them by 20 minutes.[3]

While extensive research on FDS programs has highlighted their influence on student performance at the primary and secondary levels, much less is known about their effects on long-term educational trajectories. This study seeks to address this gap in the literature by estimating the effect of the Peruvian's FDS program on higher education attainment. Given that the FDS program in Peru aims to improve the quality of education and thus student performance, it is important to assess its effects on attainment beyond secondary school, since higher education plays a critical role in the promotion of social and economic mobility (Patel-Campillo & Salas, 2022).

For our empirical approach, we rely on longitudinal survey data from the Young Lives Study that tracks households from 2002 to 2020 and contains information about the educational trajectories of their members. As such, we can link younger siblings who were young enough to potentially attend school under the JEC program with their older siblings who were old enough not to be affected by the JEC program. Then, we can implement a within-household difference-in-differences (DiD) strategy, in which we compare the gap in higher education attainment between younger and older siblings across households treated and not treated by the JEC program. Since our sample shows balance in pre-JEC within-household gaps in observable characteristics between JEC and non-JEC households, this DiD strategy supports identification of the average treatment effect on the treated (ATT) of the JEC program on higher education attainment.

Theoretically, greater time invested in learning activities is expected to result in better educational outcomes (Ben-Porath, 1967), such as obtaining higher education. First, students who spend longer hours at school reduce their unsupervised time and exposure to risky behaviors, including crime and teenage pregnancy (Berthelon & Kruger, 2011), thereby

lowering the likelihood of dropping out of school (Kane et al., 2013; Kirk & Sampson, 2012). Second, children who spend longer hours in school may develop a stronger interest in and demand for education (Dominguez & Ruffini, 2023; Vidigal & Vidigal, 2022). Third, the additional time at school allows for broader and deeper coverage of school subjects, which provides more opportunities to complete homework under supervision, and fosters closer teacher–student relationships, all of which can result in better learning outcomes (Bellei, 2009; Cabrera-Hernández, 2020; Patall et al., 2010). Thus, FDS programs can facilitate the transition from secondary to higher education.

The possibility that FDS programs have negative effects should not be ruled out, as staying longer hours at school may produce fatigue and disengagement among students, parents and teachers. For students, extended time in school has been associated with the development of negative attitudes toward schooling (Cooper et al., 2010) and with reduced self-confidence in specific subjects (Agüero et al., 2021). In addition, students attending FDS may devote less time to learning activities at home to compensate for the longer school day (Dominguez & Ruffini, 2023). For parents, they could become less involved in their children's learning processes like supporting them with homework, assuming that the additional hours at school will be used to complete all assignments (Agüero et al., 2021; Gullo, 1990). For teachers, FDS programs impose longer working hours that can reduce effort and increase stress levels, particularly when compensation is not adjusted accordingly (Agüero et al., 2021; Vidigal & Vidigal, 2022). In such cases, instead of improving quality, FDS may compromise students' preparedness for further education beyond secondary school.

The returns to extended instructional time, however, are shaped by the program design as well as student and school characteristics (Baker et al., 2004; Rivkin & Schiman, 2015). Regarding the design, some FDS programs only change schooling time, while others combine

additional time with complementary interventions. The education level targeted by FDS programs may also differ across countries. For example, FDS programs in Brazil and Mexico increased instruction time at both primary and secondary schools and also provided school lunches, yet the results have been associated with null effects on school enrollment in Mexico (Kozhaya & Martínez, 2022), and with higher grade repetition rates (Vidigal & Vidigal, 2022) and lower mathematics test scores (Almeida et al., 2016) in Brazil. In contrast, the FDS program in Uruguay aimed only at primary schools and incorporated additional interventions beyond meal provision and longer school-day, such as classroom construction, smaller class sizes, teacher training, and the supply of teaching materials (Cerdan-Infantes & Vermeersch, 2007). Thus, Cerdan-Infantes and Vermeersch (2007) find that the FDS in Uruguay increases mathematics and language test scores. Peru adopted a similar resource-oriented design but applied it only at the secondary level and without meals. In this context, Agüero et al. (2021) show that extended hours in Peru improved students' performance in mathematics and reading. Besides, the additional time in Peru was mainly allocated to academic subjects (Agüero et al., 2021), whereas in Brazil (Vidigal & Vidigal, 2022), Mexico (Kozhaya & Martínez, 2022), and Uruguay (Cerdan-Infantes & Vermeersch, 2007) the academic instruction was combined with cultural, artistic, sports, and other complementary activities.

Student characteristics also influence the impacts of FDS programs on academic outcomes. Consistent with the 'skills beget skills' hypothesis (Cunha & Heckman, 2007), evidence from Chile (Bellei, 2009) and Switzerland (Cattaneo et al., 2016) indicates that high school students at the top of the classroom rank distribution benefit the most from longer hours of instruction, achieving higher test scores in mathematics and reading. A similar pattern is found in the United States of America (US) where kindergarten (Hayes & Gershenson, 2016) and primary (Sims, 2008) students exposed to more instructional

days experienced greater gains in mathematics and reading at the top of the achievement distribution. Furthermore, the effects of FDS programs may be conditioned by students' socioeconomic background, since access to family and community resources shapes their ability to benefit from school (Bourdieu, 1986). Prior research shows that students living in rural areas in Chile (Bellei, 2009) and Colombia (Hincapie, 2016), as well as those from low socioeconomic families in developed countries (Lavy, 2015) and Mexico (Padilla-Romo, 2022), achieve larger gains in mathematics and reading from extended instructional hours. Conversely, evidence from the US indicates that wealthier students achieve higher mathematics scores (Sims, 2008), while no significant differences are observed between rural and urban students in the US (Sims, 2008) or in Mexico (Padilla-Romo, 2022). Regarding family background, Lee and Lee (2021) show that reducing the number of school days at each education level in South Korea is more detrimental to children with less educated parents, as their engagement in learning activities outside school decreases.

The school characteristics and the learning environment, in turn, condition the impacts of FDS programs. Sims (2008) and Rivkin and Schiman (2015) argue that the benefits of extended instructional time may be more limited in poorer contexts where schools lack complementary inputs. Evidence from Mexico shows that adding instructional days produces smaller improvements in mathematics and reading test scores among students attending poorer primary schools (Agüero & Beleche, 2013). The opposite pattern is found by Cabrera-Hernández (2020) in primary schools in Mexico and by Hincapie (2016) in primary and secondary schools in Colombia, showing that lengthening the school day leads to higher test scores in mathematics and reading among children in poorer schools.

To the best of our knowledge, there are only a few studies that explore the link between school length and higher education. One study for Canada by Krashinsky (2014)

shows that one less year of secondary school, from five to four years, implemented in one province of Canada in 1999 was detrimental to the university performance of students from the four-year cohort, even after matching observable characteristics such as age. The reduction in schooling time was accompanied by fewer courses and weaker academic preparation, which was associated with a 0.3 percentage points (p.p.) decline in performance and a 10 p.p. increase in the likelihood of dropping out during students' first year of university. Regarding higher education enrollment, three studies in Germany (Marcus & Zambre, 2019; Meyer & Thomsen, 2016; Schwerter et al., 2024) examine a staggered school reform introduced in the early 2000s. The reform intensified learning by compressing the same academic preparation into a shorter duration of secondary school, from nine to eight years, while simultaneously increasing weekly instructional hours. Evidence from nationally representative data indicates that the school length reform delays university entry by 2 months (Schwerter et al., 2024) or 1 year (Marcus & Zambre, 2019), and reduces enrollment by 4 p.p. to 8 p.p. (Marcus & Zambre, 2019). In contrast, Meyer and Thomsen (2016), using data from a single German state, find that the same reform delays university entry by as much as 3 years, but only for women. Moreover, Marcus and Zambre (2019) also show, consistent with findings from Canada, that shortening school duration is associated with higher dropout rates.

More related to our work, we were able to find only two studies for Latin America that explore the relationship between FDS programs and higher education attainment. For Argentina, Llach et al. (2009) explore the effect of a reform implemented in 1971 that increased daily instructional time in primary schools in the city of Buenos Aires by approximately 4 hours. The additional hours were allocated to academic activities and complementary activities. Their ATT shows that longer school-days have mixed effects on higher education attainment. They find that the additional time in school does not affect

the decision to pursue postsecondary studies, but it increases by 15% the rate of finishing a second tertiary study after completing the first one. Besides, they find that FDS was associated with a reduction in the probability of pursuing postgraduate studies.

The other related study examines a Chilean reform introduced in 1997 that extended daily instructional time and improved teaching resources in public schools at both the primary and secondary levels. The reform increased 22% the length of the school day but the additional hours, unlike the Peruvian reform, could be allocated to extracurricular activities. Based on an intent-to-treat approach, Dominguez and Ruffini (2023) find that children exposed to one additional year of full-day school were 0.7 p.p. more likely to enroll in higher education. Although they find that longer school-day hours have a similar effect (between 0.5 p.p. and 0.8 p.p.) on college enrollment by gender and socioeconomic background, they do show that men (1.9 p.p.) and wealthier individuals (1.9 p.p.) are more likely to graduate from college. Since the studies for Argentina and Chile use data from individuals in their 30s, they can also explore the relationship between FDS and labor market outcomes.[4] While the study for Argentina finds no effect on earnings, the FDS in Chile does increase earnings in early adulthood.

Our paper contributes to the literature in three important ways. First, it adds to the limited body of causal evidence on the long-term effects of FDS programs by evaluating the JEC reform implemented in 2015. To the best of our knowledge, our paper is the first to explore the effect of JEC on higher education using information from former JEC students. As such, our dataset contains information about the educational trajectory of individuals while they were school-age and after they graduated from the secondary school. The existing literature is focused on four countries (i.e., Canada, Germany, Argentina, and Chile) with higher enrollment rate in tertiary education than Peru (World Bank, 2025b). Besides,

the reforms implemented in those countries differ in timing and design from the reform implemented in Peru, which could lead to different results. In terms of timing, the Peruvian reform was instituted in 2015, whereas the reform in the other countries began in the 1990s or early 2000s; in fact, the Argentinian one dates back to 1971. In terms of design, three differences stand out. First, the FDS in Canada and Germany shortened schooling duration, whereas the FDS in Peru increased daily instructional time. Second, the Peruvian FDS is the only case in the LAC region that targeted secondary schools exclusively, whereas the FDS in Argentina focused on primary schools and the one in Chile encompassed both primary and secondary schools. Third, while schools in Chile have the autonomy to allocate the additional instructional time (Bellei, 2009), the use of the extra time in Peruvian schools was decided by the Ministry of Education. Moreover, the studies for Argentina and Canada were confined to a single province, whereas our analysis covers the entire country.

Second, our paper refines the identification of previous estimates in the causal literature on FDS programs in two ways. We are the first, to the best of our knowledge, to exploit variation within-households to estimate the effects of FDS programs on higher education.[5] This allows us to mitigate concerns that family characteristics influence education outcomes. In addition, we are not aware of other studies that use school-switching information to address non-random selection into FDS. Leveraging the longitudinal nature of our dataset, we detect school changes and restrict our analysis to households where the younger sibling did not switch between schools before or after JEC implementation.[6] Together, these two strategies strengthen our causal claims on the effect of FDS programs.

Third, unlike the works for Argentina (Llach et al., 2009) and Chile (Dominguez & Ruffini, 2023), we separate technical from university studies since they have different entry requirements and different future economic implications. Although average monthly income

in Peru increased across all education levels between 2007 and 2021, university graduates consistently earned the highest wages (INEI, 2022). In 2021, university graduates earned 2,425.6 *nuevos soles* while individuals with technical education earned 1,506.4 *nuevos soles*, and those with secondary education earned 1,118.4 *nuevos soles*.[7] These wage differentials underscore that university education remains a key determinant of economic returns.

Our main findings are as follows. First, JEC helps to attain higher education. Results from our preferred specification show that younger siblings who attended a JEC school are 17.1 p.p. more likely to pursue higher education than their older siblings who did not attend a JEC school. This effect represents 39.8% of the average higher education attainment among siblings from households not treated by the JEC program. Our estimated coefficient remains positive and statistically significant for alternative samples and specifications. Nevertheless, the effect of JEC is limited to technical studies, which can affect the long-term effects of this program as returns from university studies are higher (INEI, 2022). Second, we find that JEC may enhance students' preparation to access higher education as they are more likely to complete secondary school and develop better math skills. Furthermore, JEC students do not increase school absenteeism or reduce their time devoted to study at home; instead they reduce their time allocated to leisure and working activities to compensate for longer hours at school. Third, our findings indicate that JEC mainly benefits students from disadvantaged backgrounds, which could help narrow inequalities in access to higher education.

The rest of the paper is organized as follows: the educational system in Peru is presented in Section 2, which also includes details about the JEC program. Section 3 describes the data, while Section 4 presents the empirical strategy. Results are discussed in Section 5. In Section 6, we present some potential mechanisms. We conclude in Section 7 by highlighting the main results and offering a brief discussion.

2. Background

2.1. Education system in Peru

The basic education system in Peru comprises three levels: preschool (ages 3-5 years old), primary education (starting at age 6 and lasting six years), and secondary education (starting at age 12 and lasting five years). This structure requires children to complete primary education by age 12 and secondary education by age 17. Basic education in Peru is compulsory and provided free of charge by the government through public schools. Additionally, the private sector also offers educational services that vary widely in quality and associated costs. Public schools operate Monday to Friday, mid-March through mid-December, and students are not restricted from enrolling outside their resident district.

Although figures from the National Population Census show an increasing percentage of Peruvian population aged 25 or older who have reached secondary education, from 56.6% in 1993 to 70.4% in 2017, students perform poorly in international and national evaluations, especially those from public schools. Since the initial participation of Peru in the Programme for International Student Assessment (PISA) in 2000, Peruvian students have consistently performed at or below the minimum proficiency thresholds in mathematics and reading (MINEDU, 2017, 2024a). In 2015, the first year of JEC implementation, the minimum threshold score in PISA was 420 in mathematics and 407 in reading, while Peruvian students obtained average scores of 387 and 398, respectively (MINEDU, 2017). These scores, however, conceal performance disparities between students in private and public schools. Students in private schools scored 56 and 67 points higher than their peers in public schools in mathematics and reading, respectively (MINEDU, 2017). A similar pattern is observed in the Peruvian Student Evaluation (PSE) administered by the Ministry of Education. In 2015, the gap was 45 points in mathematics (students in private schools obtained 582 points while those

in public schools obtained 537 points) and 53 points in reading (students in private schools obtained 601 points while those in public schools obtained 548 points). This low performance of students in public schools may deter their educational progress toward higher education.[8]

After completing secondary education, students may opt to pursue higher education in either technical institutes or universities. Despite the high percentage of Peruvians with primary (93.4%) and secondary (70.4%) education recorded in the last Population Census of 2017, this percentage declines to 25.7% of those over 24 years old who hold a higher education degree. By 2018, the share of Peruvian population with higher education (21.9%)[9] was below the average of LAC countries (23.6%) as well as that of European countries (43.1%) and the US (45.2%) (UIS, 2024). We also observe that younger cohorts show higher educational attainment, as 30.9% of individuals aged 25–34 in 2017 hold a tertiary degree, compared with 28.7% among those aged 35–44 and 21.3% among those 45 and older. These figures indicate gradual progress across generations but also reveal that Peru still lags behind advanced economies in expanding access to higher education.

Among Peruvians with higher education, technical degrees are more common (39.4%) than university degrees (15.6%). Particularly, university education entails greater financial and time commitments compared to technical education. Typically, obtaining a university degree requires five years and at least US$ 20,000, whereas a technical degree takes three years and at least US$ 13,000 (Arias et al., 2025). Further, while each tertiary institution sets its own entry exam, admission to universities has been consistently more competitive than that of technical institutes. Although the percentage of applicants accepted into universities has increased from 26.5% in 2000 to 53.0% in 2016, this is still lower than the percentage for technical studies that remains higher than 70% (72.5% in 2000 and 77.3% in 2016) (Díaz, 2008; ESCALE, 2025; TUNI, 2025). These figures suggest that the demand for higher

education exceeds the capacity of universities and technical institutes.

2.2. The full-day school program

In 2015, the Peruvian government implemented a new program known as *Jornada Escolar Completa* (JEC) or Full-day School Program aiming to improve the quality of education in the country. In addition to extended instructional hours, JEC schools benefit from new or improved resources. Students receive mentoring and educational support, whereas teachers receive training to enhance their pedagogical strategies. Both students and teachers may receive support from psychologists. The JEC program also seeks to ensure access to adequate furnishings, physical infrastructure, and pedagogical resources (MINEDU, 2014). Besides, more personnel were recruited and salaries were adjusted to account for augmenting school-day hours (MEF, 2015). Overall, the JEC program in Peru comprises not only an increase in the length of the school-day but also an improvement in facilities and teaching resources. Thus, in 2015 the Peruvian government invested approximately 6,500 *nuevos soles* per student in JEC schools (Alcázar, 2016) compared with 3,600 *nuevos soles* per student in regular schools (INEI, 2025).

The JEC program was implemented only for secondary education in public schools, which by 2015 served 74.9% of students attending the secondary level. The main characteristic of the JEC program is a lengthened weekly instructional time by 28.6%, passing from 35 to 45 instructional hours per week (an instructional hour lasts 45 minutes), while keeping the number of school-days unchanged. Thus, schools that adopted JEC shifted from offering nearly one hour less of daily instruction than private schools in 2014 (5 hours 50 minutes vs. 6 hours 40 minutes) to slightly exceeding them by 2017 (7 hours 10 minutes vs. 6 hours 50 minutes).[10] Specifically, 3 instructional hours were added in English (as foreign language, from 2 to 5); 2 instructional hours were added in mathematics (from 4 to 6) and

sciences, technology, and environment (from 3 to 5); and 1 instructional hour was added in reading (from 4 to 5), civic education (from 2 to 3), and work-oriented education (from 2 to 3).

It is relevant to note that school authorities were not able to influence the selection of schools for the JEC program. The initial selection was based on schools' characteristics observed in 2013, whereas the program was announced by the end of 2014; thus, schools' characteristics could not be modified in advance to meet the selection requirements (Agüero et al., 2021). The rules for school selection, however, were changing over time. By the first year of JEC implementation, schools should have at least 8 sections,[11] operate only in the morning shift,[12] and have room availability. Since 2016, having 8 sections per grade and operating only in the morning shift were no longer required; but new requirements were added: schools should have at least 100 students in rural areas and 125 in urban areas as well as room availability in the morning and afternoon shifts (MINEDU, 2015). By 2023, the same conditions remained, but the requirement to operate only in the morning shift was reinstated (MINEDU, 2023).

At the beginning, 1,000 schools nationwide were selected to participate in the JEC program (MINEDU, 2014), but the number of JEC schools increased to 2,118 by 2023, which accounts for 21.2% of all public schools. Figure 1 shows that a higher number of JEC schools are concentrated on the Peruvian coast. Moreover, the number of JEC schools has changed over time as new schools have been incorporated into the JEC program, while some JEC schools no longer participate as school principals may request to leave the program.

[Figure 1 about here.]

Despite JEC schools operating from morning until mid-afternoon, they did not provide lunch to students. In response, some children brought lunch boxes from home or money to

buy meals or snacks from school kiosks, while others could not afford to buy food (Alcázar, 2016). In certain cases, parents delivered lunch directly to the school or children returned home to eat if they lived nearby (Alcázar, 2016). Three years after the introduction of JEC, the meal program known as Qali Warma was offered in 2018 only in some JEC schools (MIDIS, 2020). However, we do not expect the presence of Qali Warma to drive our results for two reasons. First, only 16.3% of JEC schools received this program in 2018. Second, the evidence shows that Qali Warma is not a relevant factor to explain differences in educational results (Lavado & Barrón, 2019).

3. Data

For our analysis, we use survey data from the Young Lives Study (YLS), which collects information for two cohorts of children in Peru, named older and younger cohorts. We use the younger cohort that tracks 2,052 children (YL children) born between 2001 and 2002.[13] The first round (R1) was conducted in 2002, shortly after the YL child was born, with subsequent rounds in 2006 (round 2), 2009 (round 3), 2013 (round 4), 2016 (round 5), and 2020 (round 6 by telephone).[14] The last round (R6) took place when YL children were around 18 and 20 years old, beyond the statutory age for secondary school graduation, which allows us to observe their higher education attainment.[15]

Although YLS focuses on YL children, information about other household members was also collected,[16] and this allows us to construct our sample based on households where the YL child has at least one older sibling. From here onward, we use the term "younger siblings" to refer to the only YL child per household. We work with younger and older siblings who, at some point, have attended secondary school. Thus, we can observe the type of school and the number of hours that siblings spent at secondary schools.

To select older siblings, we impose two conditions. First, we require older siblings

to be 12 or younger in at least one round between R1 and round 3 (R3). This requirement aligns older siblings' age with their younger siblings' age of 11 or 12 in round 4 (R4), the statutory age for completing primary school.[17] This approach ensures we measure siblings' characteristics at a similar age before they enter the education level at which JEC was implemented, while also having variation in household-level information between younger and older siblings.[18] Second, we exclude any older sibling who reported having attended a JEC school.

In the case of younger siblings, we use round 5 (R5) to identify those who attended a JEC school. This round provides information on the type of school attended in 2015 and 2016, after JEC implementation, when younger siblings were 14 to 16 years old and were enrolled in secondary schools.[19] We impose two restrictions to ensure comparability between younger siblings who attended and those who did not attend a JEC school. First, since all JEC schools are public, we only work with households where the younger sibling attended a public school, regardless of the JEC status of the school. Second, to address non-random selection into JEC schools due to household decisions, we exclude households where the younger sibling switched between schools from 2014 to 2016.[20]

The main sample comes from 365 households, including 365 younger siblings and 507 older siblings. There are 142 households with more than two older siblings.[21] Table 1 summarizes the variables from our main sample (Panel A) divided by siblings and JEC households' status (Panel B for younger siblings and Panel C for older siblings). JEC households, defined as those where the younger sibling attended a JEC school, represent 33% of the households in our sample (119 younger siblings). Attrition rates for YLS in rounds 2 to 5 range from 3.5% to 6.1%, which are relatively low compared to other longitudinal studies (Favara et al., 2020; Outes-Leon & Dercon, 2009). The exception is R6, when attrition rose to

14.6% due to the COVID-19 pandemic. This attrition rate falls to 11.9% for households that met the eligibility criteria for our main sample by R5. As Appendix Table A2 shows, this rate is similar for both JEC and non-JEC households. The same table also shows some differences in baseline characteristics (i.e., R1) between households that participated in R6 and those that dropped out of YLS. Nevertheless, R6 attrition is unlikely to lead to significant bias as most of these differences are similar between JEC and non-JEC households (see last column of Appendix Table A2).[22]

[Table 1 about here.]

Although the final sample comprises 17.8% of the original data,[23] Appendix Table A4 presents evidence (see Columns 5 and 6) that younger siblings retained in our sample are comparable to those excluded in terms of sex, age, household composition (members under 5 and those 65 or over), and household's wealth index. The main differences are related to the educational attainment of mothers and other family members; however, households in our sample have less educated members. Besides, the standardized mean difference for these variables remains below the 0.25 threshold, suggesting that imbalance is not substantial (Imbens & Rubin, 2015). The remaining differences reflect the selection criteria, which require the younger sibling to have at least one older sibling. Consequently, households in our sample tend to be larger, mothers are older, and YL children occupy later positions in the birth order.

The outcome variable in this paper measures higher education attainment with two categories: (1) no higher education and (2) higher education which includes complete and incomplete technical or university studies. In a complementary analysis, we distinguish higher education between technical and university studies. Higher education attainment is measured using the round when siblings were 18 years or older (i.e., outcome round) to

observe their higher education attainment level after the statutory age for secondary school graduation. For younger siblings, we use R6 when they were around 18 and 20 years old. For older siblings, we use the earliest round in which they were 18 or older. Table 1 shows that JEC households have a larger share of siblings, particularly younger siblings, who attain higher education compared to those who live in non-JEC households. This initial finding suggests that JEC may contribute to improvements in educational attainment.

To control for characteristics before JEC implementation in secondary school, we construct a set of variables using sibling and household information. At the sibling level, we use sex assigned at birth, age,[24] and birth order. At the household-level, we use number of members living in the household, number of school-age siblings, number of members aged 65 years or over, number of members under 5 years old, number of members with completed higher education, presence of both parents, wealth index, mothers' higher education, and mothers' age. Given the age differences among siblings, each of them has a specific round (i.e., control round) from which we collect the household information. For younger siblings, we use R4 when they were around 12 years old, the age limit for completing primary school.[25] For older siblings, we use the latest round, up to R3, in which they were 12 years old or younger.[26] As a result, since data for each sibling comes from different rounds, we control for their age at both the outcome and control rounds.

As we observe in Table 1, there are some variables with significant differences between JEC and non-JEC households. For the whole sample (Panel A) and older siblings sub-sample (Panel C), the main differences are age, the number of members living in the household, number of members under 5 years old, the presence of both parents in the household, and the wealth index. Meanwhile, for the younger sibling sub-sample (Panel B) there are only three differences in members living in the household, presence of both parents,

and wealth index. Then, a simple comparison of higher education attainment between siblings of JEC and non-JEC households would result in biased outcomes.

4. Empirical strategy

We use a within-household DiD strategy to analyze whether longer school-days affect higher education attainment. For our analysis, we use households with two types of siblings: younger siblings who could be exposed to JEC and older siblings who were not exposed to JEC as the JEC program started after they finished secondary school. We use different rounds for younger and older siblings to look at outcomes when they were approximately the same age. Thus, we can compare within-household differences in higher education attainment between younger and older siblings, and then compare this sibling gap across JEC and non-JEC households. We define our model as follows:

$$Y_{ih} = \lambda_h + \beta_1 SIB_{ih} + \beta_2 (SIB_{ih} \times JEC_h) + \beta_3 X_{ih} + \mu_{ih} \tag{1}$$

where our main outcome, Y_{ih}, is a dichotomous variable that equals 1 if sibling i in household h attains higher education and 0 otherwise. The indicator SIB_{ih} equals 1 for younger siblings and 0 for older siblings. The indicator JEC_h equals 1 if household h is a JEC household and 0 otherwise. Besides, JEC is not singled out since it is absorbed by household fixed effects. The vector X_{ih} includes the control variables described in the Section 3. The λ_h term represents the household fixed effects, which absorb all time-invariant household characteristics and isolate within-household variation in the outcome. Finally, μ_{ih} is the error term.

Standard errors are clustered at the satellite site level, which corresponds to the primary sampling unit in the YLS.[27] An alternative clustering level would be at school, which is the treatment assignment level, but this information is not available in our data. Nevertheless, by clustering at the satellite site, we are already considering a broader clustering

level than school, as schools are nested within the satellite site. Using narrower clusters instead, such as households, would ignore potential dependence among siblings from different households attending the same school. Given the small number of satellite sites (20), we also calculate and report 95% confidence intervals (CIs) using the wild bootstrap procedure (Roodman et al., 2019).

The parameter of interest, β_2, represents the double-difference effect. The first difference captures the gap in higher education attainment between younger and older siblings, while the second difference compares this sibling gap between JEC and non-JEC households. Thus, β_2 measures the ATT of the JEC program on higher education attainment.

The key identifying assumption in our design requires that, in the absence of JEC, the within-household sibling gap in higher-education attainment would have been similar on average across JEC and non-JEC households. We take four steps to support this identifying assumption. First, we use JEC's timing and our sample restrictions to reduce the risk of schools and siblings self-selected into the program. We rule out school selection since JEC assignment was based on schools' characteristics from 2013 while the implementation of JEC was announced by the end of 2014 (Agüero et al., 2021). This timing also supports the no-anticipation condition from DiD designs, as it is unlikely that higher education attainment for older siblings was affected prior to JEC implementation. In the case of students' nonrandom selection due to parental decisions, our main sample excludes younger siblings who switched secondary schools before or during JEC implementation.[28]

Second, we test whether pre-JEC differences between younger and older siblings differ systematically between JEC and non-JEC households. For each control variable, we compute within-household differences between the younger sibling and the average of the older siblings. Following Imbens and Rubin (2015), we report the standardized

mean difference (SMD) and the log standard-deviation ratio (LR) for each difference in Appendix Table A5. The SMDs show that only two variables (members under age 5 and mother's age) exceed the 0.25 rule of thumb in absolute value (Imbens & Rubin, 2015), while their LRs are close to zero. In addition, the three variables with the largest LRs (age at outcome round, members living in household and lives with both parents) have SMDs with absolute values smaller than 0.25. These two measures, SMD and LR, indicate balance in our sample. Following Baker et al. (2025), we complement this analysis by assessing balance in R1 baseline characteristics between JEC and non-JEC households. Although we also find acceptable balance (see Appendix Table A6), we re-estimate Equation (1) on matched samples based on the within-household differences and the R1 baseline characteristics.

Third, to complement previous tests, we implement several placebo tests that compare the sibling gap in higher-education attainment between JEC and non-JEC households for siblings whose outcomes were realized before JEC implementation. This placebo is analogous to a pre-trend test, as it compares pre-JEC outcomes for which no double-difference effect should be present. For this analysis, we use a sub-sample of households with at least two older siblings and modify Equation (1) by defining the closest older sibling as the placebo JEC-treated sibling in JEC households.

The last step of our identification strategy relies on the following robustness checks. First, we assess robustness to pre-JEC school backgrounds. We re-estimate the model in two separate sub-samples, one with younger siblings who were always enrolled in public schools and another with younger siblings who remained in the same school. Holding school backgrounds fixed further limits self-selection into JEC and strengthens the plausibility of our identifying assumption. Second, to address concerns about correlations between JEC exposure and other policies or regional factors, we re-estimate Equation (1) on a sub-sample

restricted to regions where JEC was implemented. The common policy environment increases comparability between JEC and non-JEC households. Third, we test if grade repetition or late entry into school can determine JEC exposure or higher education attainment. We therefore re-estimate the model using a sub-sample of siblings whose primary-school grade–age status implies the potential completion of secondary school before the outcome round. We do not use secondary completion as it could be affected by JEC.

To complement our findings, we estimate the effect of JEC on each type of higher education. We adapt Equation (1) to a multinomial logit model, where Y_{ih} equals 2 for university, 1 for technical, and 0 for no higher education. Because household fixed effects in multinomial logit do not address unobserved household heterogeneity, we include household averages of the time-varying control variables as additional controls when estimating this model (Wooldridge, 2010). For identification in the multinomial setting, we should expect that in the absence of the JEC program the within-sibling gap in the probability of each outcome category would have evolved in the same way for JEC and non-JEC households, conditional on observed covariates (Wooldridge, 2023). To validate this, we implement placebo tests using a multinomial logit where we compare the outcomes of older siblings whose outcomes were realized before JEC, similar to the placebo tests used for the main results.

5. Results

5.1. Main results

Table 2 shows our main results for higher education attainment, with and without control variables. We report the estimated values for β_1 and β_2 from Equation (1).[29] Standard errors clustered at the satellite site level are in parentheses, whereas 95% CIs calculated with the wild bootstrap procedure are in brackets. Column (1) reports the results using the main

sample that includes households with one or more older siblings. Columns (2) and (3) report the results using restricted samples in which the younger sibling is paired with just one older sibling. For Column (2), we use the closest older sibling. For Column (3), we use the oldest older sibling. Finally, Column (4) reports the results using a restricted sample that includes households with older siblings who were at least 18 years old in R4.

[Table 2 about here.]

Our findings in Table 2 indicate that compared to non-JEC households, JEC increases the gap in higher education attainment between younger and older siblings, regardless of the sample used. This suggests that the JEC program improves educational outcomes as younger siblings who attended a JEC school are more likely to pursue higher education compared to their older siblings who did not attend a JEC school. In Panel A, without control variables, JEC estimates range from 13.9 p.p. to 17.6 p.p. This represents around 32.4%–41.0% of the average higher education attainment among siblings from non-JEC households. Adding control variables in Panel B barely changes the estimates for β_2 presented in Panel A. Since some control variables, such as wealth index and parental education, are correlated with access to higher education, results from Panel B reduce concerns that differences in access to higher education may play a significant role in our findings. In both Panels A and B, the estimates for β_2 in all the columns, except Panel A, Columns (2) and (4), are statistically significant at the 5% level, whether using clustered standard errors or the 95% CIs calculated using the wild bootstrap procedure. In the case of Panel A, Columns (2) and (4), the coefficients are statistically significant at the 10% level.

Results in Table 2 suggest that a specific sibling composition does not change JEC's influence on higher education attainment. We do note, however, that the magnitude of our estimated coefficient decreases as the age difference between younger siblings and older

siblings shortens.[30] In Column (2), we use the closest older siblings, who on average are 6 years older than the younger siblings. In this case, the magnitude of the coefficient β_2 decreases compared to that in Column (1), but it remains positive and statistically significant in both Panels A and B. In contrast, in Column (3), where the age difference increases to 7.2 years as we use the oldest older sibling, the magnitude of the coefficients increases in both panels compared to Column (1). Thus, our results suggest that JEC can help offset the potential negative effects of sibling proximity (Calimeris & Peters, 2023). Further, in Column (4), to reduce concerns about spillovers, we restrict our sample to include only older siblings who were old enough to graduate from secondary school before JEC implementation.[31] Results show that the β_2 estimates in both Panels A and B remain similar in magnitude and statistical significance to those in Column (1). This reinforces our findings and reduces concerns about spillovers and competition for household resources.

5.2. Robustness checks

Although our results in Table 2 are consistent with the addition of control variables and different siblings composition structures, we explore a larger set of robustness checks presented in Table 3 where we replicate our preferred results in Table 2, Panel B, Column (1) but for different sub-samples. The results from Table 3 are similar in magnitude and statistical significance to our preferred results, showing that our estimated effects of JEC on higher education are robust to different samples.

[Table 3 about here.]

In the first two columns of Table 3, we deal with potential imbalance between JEC and non-JEC households. In Column (1), we match JEC households to non-JEC households using the within-household differences of the control variables. We use the closest neighbor approach without replacement with a caliper of 0.1.[32] In Column (2), we follow a similar

procedure, but matching households on the baseline characteristics from R1.[33] Results in Columns (1) and (2) show that using a matching approach does not alter our main findings. This reduces the risk of systematic imbalance between JEC and non-JEC households in our main sample.

To further address self-selection of students into JEC schools, we narrow our sample to households whose younger siblings always attended public schools (Table 3, Column 3) or always attended the same school (Table 3, Column 4). These results remain similar in magnitude and significance to our main results, suggesting that our estimated JEC effects are not determined by different school backgrounds. As an additional robustness check, we show that JEC effects remain the same if we restrict our sample to households from regions with at least one JEC household (Table 3, Column 5).[34] This suggests that our results are not driven by potential correlations between the JEC reform and other regional characteristics.

Finally, we show that grade repetition or late entry into school do not bias our results. Specifically, we use the year of primary completion to restrict the sample to siblings who could potentially have finished secondary school before the outcome round. This restriction does not exclude siblings who completed primary school on time but remained longer in secondary education, as these delays may be endogenous to the JEC intervention and therefore should be treated as post-treatment outcomes. For younger siblings, we use the annual records from YLS to identify the year in which they finished primary school. For older siblings, we infer the moment they completed primary school based on their grade–age status from the control round as YLS does not provide their annual information. When the control round is R1, which reports level but not grade, we instead use R2. As shown in Table 3 Column (6), this restriction excludes less than 10% of the main sample, and the results remain similar to our preferred estimates.

5.3. Placebo tests

We further support our identification strategy by showing that, once we remove younger siblings and the JEC influence from the analysis, the differences in higher education attainment between JEC and non-JEC households disappear. For that purpose, we identify in our sample households with at least two older siblings who were 18 or older in the rounds previous to JEC (R4 or before), which ensures that their outcome was not influenced by JEC. The resulting sub-sample, i.e., the placebo sample, includes 113 households with 303 older siblings. To avoid losing a larger number of households, we also include those older siblings without information from the control round, though this prevents us from controlling for household-level variables. We use this placebo sample to re-estimate Equation (1), still comparing younger to older siblings, as shown in Table 4, Column (1). The results show that the JEC effect persists and is nearly twice as large as the coefficient from our preferred specification in Table 2, Panel B, Column (1). This estimate, however, should be interpreted with caution as we cannot fully account for unobserved heterogeneity due to the absence of household-level controls.

[Table 4 about here.]

To perform the placebo tests, we select the closest older sibling (COS_SIB) as the placebo JEC-treated sibling. We then re-estimate our double-difference effect by comparing the gap in higher education attainment between the placebo sibling and other older siblings across JEC and non-JEC households. All other estimation details follow the structure of Equation (1), except for including household-level controls. Results appear in Columns (2) to (4) of Table 4. Whether we compare the placebo sibling to all other older siblings (Column 2), to the second closest older sibling (Column 3), or to the oldest older sibling (Column 4) in the placebo sample, the estimated coefficients are not statistically different from zero. While

the placebo subsamples have smaller sizes, which naturally lowers precision, the estimated coefficients magnitudes are no larger than one-third of the main estimate from Column (1). Taken together, the placebo results are consistent with the identifying assumption that differences between JEC and non-JEC households arise from younger siblings' treatment exposure rather than pre-existing differences.

5.4. Results by type of higher education

In Table 5, we adapt Equation (1) to a multinomial logit model to explore differences of JEC on higher education by separating university and technical studies attainment.[35] The results of the multinomial logit model are consistent with our main findings showing that JEC improves educational attainment, since all columns show a statistically significant reduction in the likelihood of not attaining higher education. Results in Column (1) show that JEC is associated with a statistically significant 14.6 p.p. decrease in the likelihood of not attaining higher education and a 14.9 p.p. increase in the likelihood of attaining technical studies. Columns (2) through (4) confirm that JEC increases the likelihood of obtaining technical education regardless of sibling composition, yet the coefficient in Column (4) is not significant. The effect of the JEC program, however, is not statistically significant in explaining university studies. Thus, the influence of JEC on long-term living standards can be limited as university education offers higher wage returns than those of technical education.

To verify that our results from the multinomial logit model are not driven by our estimation strategy, we re-estimate our preferred specification in Table 2, Panel B, but following the same strategy for the multinomial setting; i.e., including household averages instead of household fixed effects. The results in Appendix Table A9 show that JEC coefficients remain similar in magnitude and significance to those in Table 2, Panel B. Further, in Appendix Table A10 we present results from our placebo test for the multinomial

setting. Although the smaller sample size reduces precision, the marginal effects from the multinomial logit model reported in Appendix Table A10, Columns (2) to (4), are lower in magnitude and not statistically significant compared to those in Column (1) for the younger–older sibling comparison. Overall, the tests presented support the validity of our findings.

[Table 5 about here.]

5.5. Heterogenous effects

As discussed in the Introduction section, the effect of JEC on educational attainment may differ across population groups. In Table 6, we use a triple-difference framework to estimate how pre-JEC characteristics moderate the program's effects on higher education attainment. All the characteristics are measured before JEC implementation at the earliest round with available information. We use R1 to measure wealth, residence area, and mother's education. We use R2 for cognitive skills and R4 for food assistance.

[Table 6 about here.]

First, we examine the design of the JEC program. Initially JEC schools followed the same design established by the Ministry of Education, which did not include food assistance. Three years after the reform began; however, the Peruvian government expanded Qali Warma, the national school feeding program that initially targeted only students in primary schools, to some JEC schools (MIDIS, 2020). By 2018, only 16.3% of JEC schools participated in Qali Warma. Since the YLS does not collect information on Qali Warma participation while YL children were attending secondary school, we identify those who received any kind of food assistance while attending primary school. Consistent with Lavado and Barrón (2019), who find that Qali Warma has no discernible effect on educational outcomes, our estimates show

no statistically significant differences in the impact of JEC on higher education attainment between students who received the program and those who did not (Table 6, Column 1).

Second, we examine heterogeneity by student and family characteristics. While there are no statistically significant differences in the effect of JEC on higher education among these groups, certain patterns emerge. To assess cognitive skills, we use the Cognitive Developmental Assessment (CDA) provided by YLS, which captures quantitative reasoning abilities (Espinoza & Scott, 2022). Findings in Table 6 suggest that students in the top half of the cognitive skills distribution (Column 2) and those from the top half of the wealth distribution (Column 3) benefited less from JEC. Although students with highly educated mothers (Column 4) also show smaller JEC effects, the difference relative to their peers with less-educated mothers is negligible. In contrast, students who reside in rural areas appear to gain the most from attending JEC schools (Column 5) compared to their peers in urban areas, although their advantage is small.

6. Mechanisms

In this section, we analyze potential mechanisms underlying the effects of the JEC program on higher education attainment. We focus on the mechanisms summarized in Table 7 and Figure 2. For secondary school completion (Table 7, Column 1) and time use (Figure 2),[36] we apply the within-household difference-in-differences strategy. For the other mechanisms in Table 7, due to data limitations, we restrict the analysis to younger siblings. In these cases, we compare households whose younger siblings attended JEC schools with households whose younger siblings did not.

[Table 7 about here.]

An important mechanism operates through secondary school completion, which is required to take any tertiary entry exam and therefore access higher education. Yet, longer

school hours under JEC may also increase the risk of dropout if they generate fatigue or negative attitudes toward schooling (Cooper et al., 2010). Consistent with Dominguez and Ruffini (2023) for Chile and Llach et al. (2009) for Argentina, we find that attending JEC schools increases the probability of secondary school completion by 8.9 p.p. (Table 7, Column 1). Moreover, it seems that children who attend JEC schools do not reduce their effort in academic activities, as they are 4.5 p.p. less likely to report absenteeism (Table 7, Column 4) and more likely to reduce their involvement in leisure (Figure 2) or working (Figure 2, statistically significant at the 10% level.) activities while increasing their time spent at school (Figure 2). These latter results are similar to Agüero et al. (2021), who also show that Peruvian students attending JEC schools report fewer hours spent in leisure and working activities. Overall, these results suggest that JEC raises school graduation and reallocates students' time to academic activities, potentially improving students' preparation for higher education.

[Figure 2 about here.]

In addition, longer school hours may also influence higher education attainment through channels operating at both the student and parent levels. One possible mechanism is to improve academic preparation. In line with the previous literature, our results show that JEC increases the percentage of correct answers in mathematics by 3.3 p.p. (Table 7, Column 2). This gain in mathematics skills may strengthen students' prospects of pursuing tertiary education. Another mechanism operates through parents' involvement in their children's learning, yet our results show that JEC does not affect the likelihood that parents help their children with homework (Table 7, Column 5). This resonates with findings from Agüero et al. (2021) who used a different data and found that JEC did not lead to any change in parental behavior. Finally, teacher quality could also represent a potential channel. We use parents'

perceptions of their children's teachers, and consistent with evidence for Peru by Agüero et al. (2021), we find no significant effect of JEC on teacher quality.

7. Discussion and conclusions

The state of education in Peru and other Latin American countries is marked by low enrollment rates in higher education and persistent inequalities in access to quality schooling (Sánchez & Singh, 2018). In response, governments have implemented reforms in public schools to mimic some characteristics of private schools, aiming to close the educational gap between children from low- and high-income families. FDS programs, such as JEC in Peru, have become a frequent policy intervention, mainly in the LAC region, that extends school-day hours to provide deeper coverage of specific school subjects, accompanied by mentoring and educational support for students in public schools to strengthen students' educational outcomes.

In this study, we take advantage of the longitudinal YLS dataset to implement a double-difference strategy that relies on within-household gaps in higher-education attainment between younger and older siblings across JEC and non-JEC households. By exploiting this intra-family variation in program exposure, our estimates improve on previous causal studies of FDS programs' impact on educational outcomes. While the majority of the prior research shows that FDS programs increase test scores in specific school subjects (see Agüero et al. (2021), Bellei (2009), Hincapie (2016), among others), there is limited evidence regarding the impact of this kind of programs on higher education attainment. Hence, our study contributes to the empirical literature by examining an FDS program and its effects on higher education attainment in Peru. We are aware of only two studies for Latin America that show mixed findings. Llach et al. (2009) find that extended school hours have mixed effects on higher education attainment in Argentina, whereas Dominguez and Ruffini (2023) find a positive

effect in Chile. Unlike these two studies, we extend the empirical literature by separating technical from university studies. Moreover, the Peruvian FDS reform differs from those in Argentina and Chile, as it was implemented later, targeted only secondary schools, and adopted a more centralized design, which may result in different outcomes across contexts.

Our results are consistent with the argument that FDS programs increase educational attainment. Similar to the evidence for Chile (Dominguez & Ruffini, 2023), we find that children who attended a FDS were more likely to continue higher education, regardless of the sample or specification we analyzed. The estimated effect of JEC in Peru ranges from 13.9 p.p. to 17.6 p.p., which is larger than the 0.7 p.p. reported for Chile (Dominguez & Ruffini, 2023). The larger effect found in our work may reflect that we not only measure university studies, as in the Chilean analysis, but also include technical studies in our indicator of higher education, which have higher admission rates than universities. Moreover, the share of the population with higher education remains lower in Peru than in Chile (World Bank, 2025b). In this context of lower educational attainment, FDS may yield larger returns.

When we divide higher education into technical and university studies, our results show that JEC increases the likelihood to pursue technical studies, whereas the effect of JEC on university studies is not statistically significant. This result suggests that JEC has a limited effect on long-term economic outcomes, as a university degree is associated with higher wage returns than those obtained by technical studies (INEI, 2022). Although we find that JEC improves standardized test scores in mathematics and increases secondary school completion, differences in academic ability may still influence students' tertiary education trajectories. In our sample, younger siblings from JEC schools who enrolled in universities scored higher (320) on the CDA than their peers who pursued technical studies (299). As university admission is more selective than admission to technical institutes (Arias

et al., 2025), students with higher abilities are more likely to secure places at universities. In addition, university education poses a higher financial burden than technical studies (Arias et al., 2025). Not surprisingly, in our sample, younger siblings from JEC schools who enrolled in universities come from households with a higher wealth index (0.479) than their peers in technical institutes (0.334). Therefore, for FDS programs like JEC to contribute to better long-term economic outcomes and upward mobility, there is a need to consider income incentives and remedial courses targeting JEC alumni to ease their pathway into university studies.

In addition, we implement several robustness checks and placebo tests. Our results remain similar in magnitude and significance, regardless of the sibling composition within the household and the use of a matched sample to improve balance in our sample. Further, we explore potential mechanisms underlying the observed effects of JEC on higher education attainment. Consistent with previous evidence, we find that JEC improves students' academic preparedness, as reflected in higher mathematical skills and reduced absenteeism. Moreover, similar to Agüero et al. (2021) in Peru, we show that JEC students spend less time playing or working to compensate for longer school hours. These results suggest that the FDS program in Peru does not reduce learning effort, as JEC has no statistically significant effect on the time students spend studying at home. In addition, similar to the reforms in Argentina (Llach et al., 2009) and Chile (Dominguez & Ruffini, 2023), JEC in Peru increases the likelihood of completing secondary education, a prerequisite for accessing higher education.

Finally, we examine heterogeneous effects of JEC on higher education attainment. We find no statistically significant differences across the characteristics analyzed; however, some patterns emerge. The point estimates of JEC on higher education attainment are larger in magnitude for students from rural areas and low-income families. These patterns are

consistent with evidence from other Latin American countries, where FDS programs benefited rural students (Bellei, 2009; Hincapie, 2016) and low-socioeconomic students (Padilla-Romo, 2022), but through higher achievement in mathematics and reading. We also find that JEC has smaller effects on the likelihood of continuing higher education among students at the top of the skill distribution and those with more educated mothers. This suggests that gains are mainly concentrated among individuals from disadvantaged backgrounds, highlighting JEC's potential to reduce inequalities in access to higher education.

Even though the program entails roughly twice the per-student cost of regular schools (about US$ 850 per student-year in 2015), a back-of-the-envelope calculation indicates that, under a conservative scenario where JEC only increases access to technical education, the present value of additional earnings over the first 20 years of working life amounts to 63.1% of the per-student spending. This percentage increases to 113.9% if we assume that one third of the JEC-induced increase in higher education attainment is allocated to university. These figures are consistent for the Chilean case, as Dominguez and Ruffini (2023) reports gains between 60% and 120% of the increase in per-pupil spending over a 20-year horizon.[37]. As Peru moves toward membership in the Organisation for Economic Co-operation and Development (OECD), which will require higher secondary-level spending (World Bank, 2025a), JEC represents a credible investment to expand higher-education access.

The results in this study are not without caveats. First, we are not able to separate JEC components, which prevents us from isolating the effect of longer hours at school on higher education attainment. Second, the sample size limits precision in some specifications, such as the multinomial logit. Third, our ATT applies only to non-school switchers which limits external validity, but our analysis remains policy relevant for scaling up within public schools, as school switching in Peruvian public secondary schools is typically below 10%.[38]

Despite these limitations, our results have shown consistency through the different samples used in this study. From a policy perspective, our results justify the importance of investing in improved school resources along with increasing school-day hours. In particular, the results provide robust and consistent evidence that JEC increases the likelihood of attaining higher education in Peru.

Declaration of generative AI and AI-assisted technologies in the writing process

During the preparation of this paper, the authors used ChatGPT and Grammarly for language edits. After using these tools, the authors reviewed and edited the content as needed and take full responsibility for the content of the publication.

Data availability

The authors do not have permission to share data. However, the data used in this paper are publicly available via the UK Data Archive. Replication code and documentation will be released in an open repository after the review process.

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Notes

[1]See Dağli (2019) and Patall et al. (2010) for further details on the characteristics of FDS programs.

[2]See results from the Peruvian Student Evaluation in 2023 (MINEDU, 2024b) and PISA in 2022 (OECD, 2023).

[3]Own calculations based on Peruvian School Censuses for 2014 and 2017.

[4]This is beyond the scope of our work as we use data from 2020, when children who attended the FDS program analyzed in this paper were still in their 20s at most.

[5]Cepaluni and Driscoll (2025) and Sánchez et al. (2020) use a similar strategy to estimate the effects of conditional cash transfer programs in Latin America.

[6]We apply this restriction only to younger siblings since they could attend or not JEC schools, whereas older siblings were not exposed to JEC schools.

[7]*Nuevos soles* is the Peruvian currency. One U.S. dollar (US$) was equivalent to 3.98 *nuevos soles* by the end of 2021 and 3.41 *nuevos soles* by the end of 2015 (BCRP, 2025).

[8]Although, the performance gap between private and public schools had narrowed in 2022, students in private schools still outperformed their peers in public schools (MINEDU, 2024a, 2024b).

[9]The figure reported by UNESCO Institute for Statistics (UIS) is based on the National Household Survey.

[10]Own calculations based on Peruvian School Censuses for 2014 and 2017.

[11]A section refers to the room where students from the same grade receive their lectures; similar to a "homeroom" in the US.

[12]In Peru, public schools may operate under two-shift schemes with a different group of students in each shift.

[13]We do not use children from the older cohort since they had already completed secondary school by the time JEC was implemented in 2015.

[14]Round 7 was collected around 2022-2023, but it was not publicly available while we wrote this paper.

[15]We show that our results are robust to excluding siblings for whom, given late primary completion, timely completion of secondary school before the outcome round was uncertain.

[16]Due to COVID-19 pandemic, only YL children responded to the survey in R6.

[17]From 365 younger siblings selected, 188 (51.5%) were 11 years old, 175 (47.9%) were 12 years old, and 2 (0.6%) were 13 years old.

[18]We do not use earlier rounds to measure sibling characteristics before JEC implementation to avoid losing

older siblings' observations. As we need to ensure comparability, we work with information from siblings around the same age. Thus, if we use round 2 for younger siblings, when they were around 5-6 years old, we would exclude 79% of older siblings from our sample. If we use R3 for younger siblings, when they were around 8-9 years old, we would exclude 38% of the older siblings from our sample. We do not use R1 because household-level information remains constant for younger and older siblings, preventing comparisons among them.

[19]Between 2015 and 2016, some schools changed status from non-JEC to JEC. This affected only 7% of younger siblings in our sample and does not alter the results (see Appendix Table A1, Column 1). No younger sibling in our sample attended a school that changed from JEC to non-JEC during these years.

[20]Our results remain the same if we restrict the sample to younger siblings who always attended a public school or to younger siblings who always attended the same school.

[21]For complementary analysis, we use sub-samples restricted to only one older sibling per household.

[22]There is only one exception. JEC households that participated in R6 have more members under 5 years old than those that dropped out, whereas non-JEC households show the reverse pattern. This exception increases caregiving responsibilities for siblings in JEC households. Thus, if any, the attrition would generate a lower bound estimate of the effect of JEC on higher education attainment.

[23]See Appendix Table A3 for details on the selection criteria applied during the data construction process.

[24]Some siblings in the YLS report inconsistent ages across rounds. To correct these inconsistencies, we adjust age using the age reported in R1 and the year of each round. Since the adjustment only affects 26 siblings in our main sample, results remain similar whether using the adjusted or the originally reported age (see Appendix Table A1, Column 2).

[25]We use R3 only for two younger siblings who did not participate in R4 but were interviewed in R6.

[26]For 88% of older siblings, the information comes from a round when they were between 10 and 12 years old, for 9% we use a round when they were 9 years old, and for 3% we use a round when they were younger than 9 years old. Our results remain similar if we restrict the sample only to households where at least one older sibling has the same age or is at most one year older than the younger sibling at their respective control rounds (see Appendix Table A1, Column 3).

[27]The satellite site represents a province or district with more than 125,000 inhabitants.

[28]In the YLS, 22% of eligible younger siblings switched schools and are therefore excluded from our main

sample (see last row of Table A3). Nevertheless, results remain statistically significant at the 5% level when we include households with younger siblings who switched schools (see Appendix Table A1, Column 4).

[29]Appendix Table A7 reports the estimated coefficients for all control variables, using the specification from Table 2, Panel B.

[30]Differences in access to higher education over time are unlikely to explain these patterns as the 2013 University Reform restricted rather than expanded university opportunities (Torres et al., 2023).

[31]The cost of this approach is a reduction in the number of households and observations as not all households have older siblings who meet this age requirement.

[32]The propensity scores are based on the results from Appendix Table A8 Column (1). Six JEC households are excluded from the analysis as they fall outside the common support.

[33]Besides the control values of R1, we match on individuals' characteristics of siblings (sex, age in R1, and birth order), an index of access to public services, the self-perceived level of wealth, the house ownership status, and an indicator for having a mother with formal education. The propensity scores are based on the results from Appendix Table A8 Column (2). Nine JEC households are excluded from the analysis as they fall outside the common support.

[34]Peru is divided into 25 administrative regions. JEC was not implemented in all the regions during 2015 and 2016.

[35]Due to computational limitation, for non-linear models we report bootstrap CIs instead of wild boostrap CIs.

[36]We estimate results on time use only for households with available data. Appendix Table A1 Column (5) shows that JEC effect on higher education attainment remains positive, while loses statistical significance due to a smaller sample size.

[37]In our calculations, we use the education–earnings differentials described in Section 2.1, five years of secondary-school spending, and a 3% social discount rate as in Dominguez and Ruffini (2023)

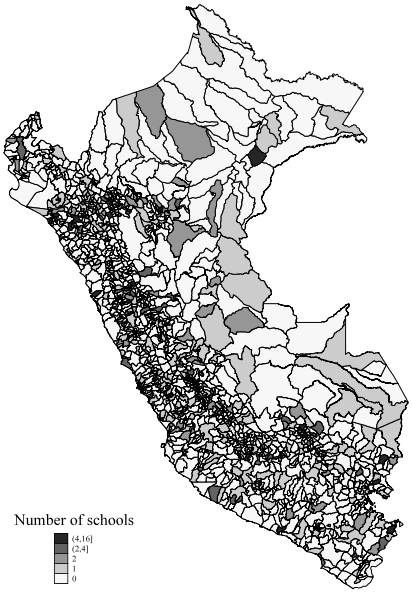
[38]Own calculations based on the Peruvian School Censuses from 2015 to 2019.

1

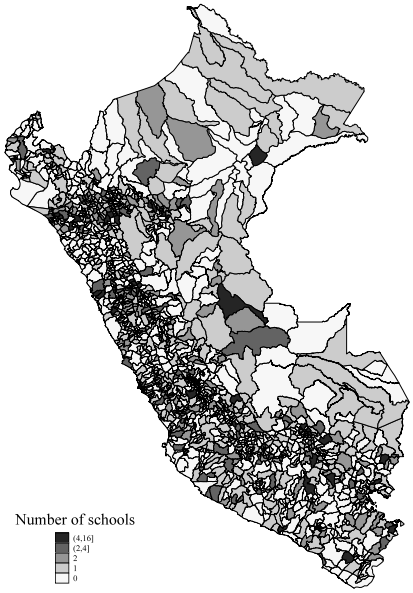
Figures

Figure 1

District-level distribution of JEC schools



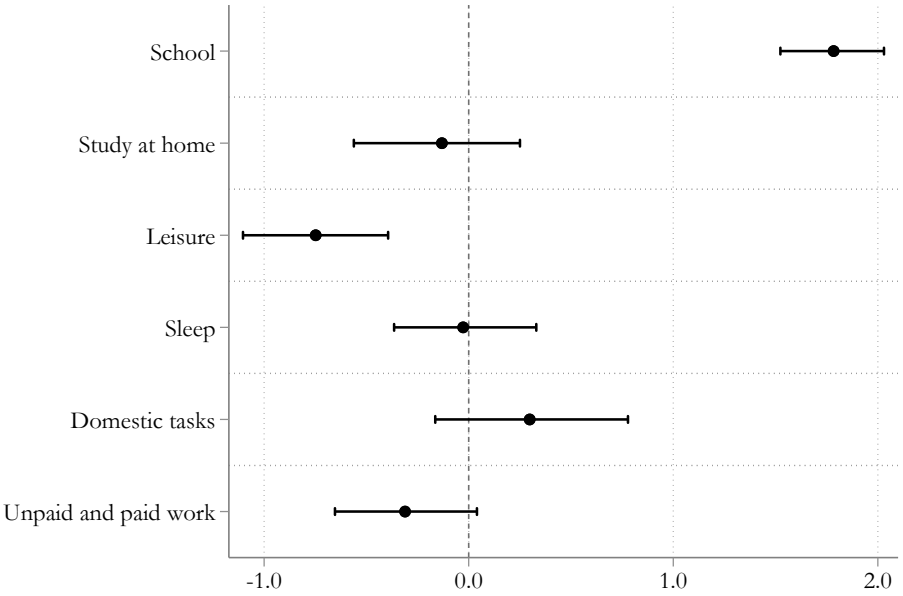
(a) District-level distribution of JEC schools in 2015



(b) District-level distribution of JEC schools in 2023

Figure 2

Estimated effects on time distribution



Note: All results are estimated following the same structure of Equation (1). Only considering households with time distribution data. Each result shows the estimate of β_2 for a different regression, in which the dependent variable appears in the vertical axis. Dots show the estimated coefficient, while lines show the 95% CIs calculated with the wild bootstrap procedure considering satellite site clusters.

2

Tables

Table 1

Descriptive statistics

	All HHs		JEC HHs		non-JEC HHs		JEC vs. non-JEC
	Mean	SD.	Mean	SD	Mean	SD	*p-value*
A. All members in sample							
Attended JEC school	0.136	0.343	0.405	0.492	0.000	0.000	0.000
Higher education attainment	0.453	0.498	0.500	0.501	0.429	0.495	0.047
Male sibling	0.533	0.499	0.548	0.499	0.526	0.500	0.545
Age (outcome round)	18.851	1.250	18.983	1.406	18.784	1.158	0.026
Age (control round)	11.141	0.985	11.048	1.001	11.189	0.975	0.046
Birth order	2.765	1.752	2.881	1.790	2.706	1.731	0.163
Members living in household	6.131	1.867	6.429	1.904	5.979	1.831	0.001
Siblings in school age	1.745	1.247	1.786	1.222	1.725	1.259	0.496
Members aged 65 years or over	0.081	0.309	0.105	0.329	0.069	0.298	0.102
Members under 5 years old	0.669	0.723	0.759	0.752	0.623	0.704	0.009
Members with higher education	0.493	0.865	0.452	0.864	0.514	0.866	0.322
Live with both parents	0.940	0.237	0.980	0.142	0.920	0.271	0.000
Wealth index	0.527	0.211	0.469	0.208	0.556	0.207	0.000
Mother attained higher education	0.149	0.356	0.133	0.340	0.157	0.365	0.332
Mother's age	37.609	6.624	37.330	6.890	37.751	6.486	0.375
Obs	872		294		578		
B. Only younger siblings							
Attended JEC school	0.326	0.469	1.000	0.000	0.000	0.000	.
Higher education attainment	0.496	0.501	0.613	0.489	0.439	0.497	0.002
Male sibling	0.504	0.501	0.538	0.501	0.488	0.501	0.372
Age (outcome round)	18.271	0.514	18.286	0.570	18.264	0.486	0.709
Age (control round)	11.463	0.585	11.395	0.641	11.496	0.555	0.123
Birth order	3.655	1.770	3.857	1.791	3.557	1.755	0.129
Members living in household	5.729	1.715	5.966	1.808	5.614	1.659	0.065
Siblings in school age	1.381	1.151	1.395	1.106	1.374	1.174	0.871
Members aged 65 years or over	0.104	0.348	0.118	0.349	0.098	0.348	0.606
Members under 5 years old	0.351	0.552	0.370	0.519	0.341	0.569	0.647
Members with higher education	0.655	0.924	0.630	0.882	0.667	0.945	0.725
Live with both parents	0.926	0.262	0.966	0.181	0.907	0.292	0.041
Wealth index	0.605	0.182	0.559	0.184	0.628	0.178	0.001
Mother attained higher education	0.175	0.381	0.160	0.368	0.183	0.387	0.585
Mother's age	41.600	5.831	41.807	5.724	41.500	5.891	0.638
Obs	365		119		246		
C. Only older siblings							
Attended JEC school	0.000	0.000	0.000	0.000	0.000	0.000	.
Higher education attainment	0.422	0.494	0.423	0.495	0.422	0.495	0.980
Male sibling	0.554	0.498	0.554	0.498	0.554	0.498	0.999
Age (outcome round)	19.268	1.443	19.457	1.596	19.169	1.347	0.032
Age (control round)	10.909	1.138	10.811	1.126	10.961	1.143	0.160
Birth order	2.124	1.430	2.217	1.458	2.075	1.415	0.289
Members living in household	6.420	1.919	6.743	1.908	6.250	1.906	0.006
Siblings in school age	2.008	1.248	2.051	1.228	1.985	1.259	0.569
Members aged 65 years or over	0.065	0.277	0.097	0.316	0.048	0.253	0.059
Members under 5 years old	0.897	0.745	1.023	0.773	0.831	0.722	0.006
Members with higher education	0.377	0.801	0.331	0.833	0.401	0.785	0.356
Live with both parents	0.951	0.217	0.989	0.107	0.931	0.254	0.004
Wealth index	0.471	0.213	0.408	0.202	0.504	0.212	0.000
Mother attained higher education	0.130	0.337	0.114	0.319	0.139	0.346	0.441
Mother's age	34.736	5.595	34.286	5.891	34.973	5.428	0.189
Obs	507		175		332		

Note: For each variable, the last column shows the *p-value* of the mean test comparing JEC and non-JEC households.

Table 2

Effects on higher education attainment

	Type of older siblings			
	All	Closest one	Oldest one	With $\geq$ 18 yrs. at R4
	(1)	(2)	(3)	(4)
A. Without control variables				
SIB (β_1)	0.009	0.020	0.000	0.047
	(0.039)	(0.040)	(0.038)	(0.043)
	[-0.071, 0.089]	[-0.058, 0.102]	[-0.079, 0.080]	[-0.045, 0.132]
SIB $\times$	0.163**	0.139*	0.176**	0.166*
JEC HH (β_2)	(0.073)	(0.072)	(0.076)	(0.088)
	[0.018, 0.313]	[-0.012, 0.284]	[0.017, 0.336]	[-0.035, 0.368]
Siblings	872	730	730	561
Households	365	365	365	247
N clusters	20	20	20	20
B. With control variables				
SIB (β_1)	0.004	-0.064	-0.070	0.047
	(0.060)	(0.083)	(0.092)	(0.121)
	[-0.114, 0.127]	[-0.233, 0.112]	[-0.258, 0.120]	[-0.250, 0.294]
SIB $\times$	0.171**	0.152**	0.186**	0.172**
JEC HH (β_2)	(0.064)	(0.067)	(0.067)	(0.081)
	[0.035, 0.308]	[-0.006, 0.293]	[0.037, 0.331]	[-0.011, 0.353]
Siblings	872	730	730	561
Households	365	365	365	247
N clusters	20	20	20	20

Note: All columns include household fixed effects. Satellite site clustered standard errors appear in parentheses. 95% CIs calculated with the wild bootstrap procedure appear in brackets. Stars indicate significance levels using the satellite site clustered standard errors. *** *p-value* < 0.01, ** *p-value* < 0.05, * *p-value* < 0.10.

Table 3

Robustness checks. Effects on higher education attainment

	Matched samples		School background of younger siblings		Only regions with JEC HHs	Excluding non-lagged students
	Within households differences	Baseline characteristics R1	Always public schools	Always same school		
	(1)	(2)	(3)	(4)	(5)	(6)
SIB (β_1)	-0.131	0.102	-0.056	0.003	0.022	0.017
	(0.102)	(0.086)	(0.152)	(0.059)	(0.068)	(0.065)
	[-0.352, 0.121]	[-0.095, 0.309]	[-0.412, 0.291]	[-0.121, 0.127]	[-0.116, 0.171]	[-0.107, 0.150]
SIB × JEC HH (β_2)	0.247***	0.159**	0.260**	0.169**	0.163**	0.135**
	(0.078)	(0.071)	(0.104)	(0.065)	(0.068)	(0.060)
	[0.084, 0.408]	[0.002, 0.309]	[0.002, 0.551]	[0.031, 0.304]	[0.010, 0.305]	[0.003, 0.261]
Siblings	558	532	351	830	806	801
Households	226	220	148	346	334	340
N clusters	20	20	18	20	18	20

Note: Each column represents a separate estimation of Equation (1) using different restricted samples. Results can be compared relative to our preferred specification in Table 2, Panel B, Column (1). All columns include control variables from Table 1 and household fixed effects. Satellite site clustered standard errors appear in parentheses. 95% CIs calculated with the wild bootstrap procedure appear in brackets. Stars indicate significance levels using the satellite site clustered standard errors. *** *p-value* < 0.01, ** *p-value* < 0.05, * *p-value* < 0.10.

Table 4

Placebo tests

	Younger vs. older siblings	Closest older sibling vs.		
		Other older siblings	Second closest older sibling	Oldest sibling
	(1)	(2)	(3)	(4)
SIB	-0.139**			
	(0.060)			
	[-0.279, -0.022]			
SIB × JEC HH	0.358**			
	(0.147)			
	[-0.014, 0.693]			
COS_SIB		-0.020	0.032	-0.090
		(0.064)	(0.086)	(0.118)
		[-0.165, 0.101]	[-0.207, 0.227]	[-0.358, 0.151]
COS_SIB × JEC HH		0.078	0.104	0.056
		(0.135)	(0.124)	(0.143)
		[-0.201, 0.374]	[-0.152, 0.359]	[-0.269, 0.361]
Siblings	416	303	227	226
Households	113	113	113	113
N clusters	20	20	20	20

Note: Each column represents a separate estimation of Equation (1) using the placebo sample. We control for older sibling indicator, male, age at outcome round, birth order and household fixed effects. Satellite site clustered standard errors appear in parentheses. 95% CIs calculated with the wild bootstrap procedure appear in brackets. Stars indicate significance levels using the satellite site clustered standard errors. *** *p-value* < 0.01, ** *p-value* < 0.05, * *p-value* < 0.10.

Table 5

Marginal effects of JEC by type of higher education

	Type of older siblings			
	All	Closest one	Oldest one	With $\geq$ 18 yrs. at R4
	(1)	(2)	(3)	(4)
No higher education	-0.146**	-0.127*	-0.169***	-0.145**
	(0.058)	(0.067)	(0.064)	(0.067)
	[-0.263, -0.029]	[-0.269, 0.016]	[-0.296, -0.041]	[-0.281, -0.008]
Tecnical education	0.149***	0.114**	0.167**	0.120
	(0.056)	(0.051)	(0.066)	(0.081)
	[0.041, 0.256]	[0.015, 0.214]	[0.037, 0.298]	[-0.043, 0.283]
University education	-0.003	0.012	0.001	0.025
	(0.045)	(0.041)	(0.054)	(0.056)
	[-0.100, 0.094]	[-0.082, 0.107]	[-0.112, 0.114]	[-0.091, 0.141]
Siblings	872	730	730	561
Households	365	365	365	247
N clusters	20	20	20	20

Note: Results come from a multinomial logit model based on Equation (1) and estimated with maximum likelihood. Each row shows the marginal effect for the corresponding type of higher education. All columns include control variables from Table 1 and the household-level averages for these variables. Satellite site clustered standard errors appear in parentheses. 95% bootstrap CIs appear in brackets. Stars indicate significance levels using the satellite site clustered standard errors. *** *p-value* < 0.01, ** *p-value* < 0.05, * *p-value* < 0.10.

Table 6

Effects on higher education attainment. Heterogenous effects

	(1)	(2)	(3)	(4)	(5)
SIB (β_1)	0.042	-0.051	-0.006	0.010	0.021
	(0.053)	(0.064)	(0.080)	(0.063)	(0.065)
	[-0.067, 0.155]	[-0.171, 0.079]	[-0.175, 0.179]	[-0.117, 0.142]	[-0.111, 0.153]
SIB ×	0.219**	0.195**	0.254**	0.175**	0.142*
JEC HH (β_2)	(0.089)	(0.083)	(0.091)	(0.070)	(0.074)
	[0.031, 0.494]	[0.025, 0.372]	[0.051, 0.457]	[0.021, 0.328]	[-0.033, 0.312]
SIB ×	-0.071				
JEC HH ×	(0.118)				
Food program	[-0.324, 0.172]				
SIB ×		-0.044			
JEC HH ×		(0.102)			
Top cog. skills		[-0.265, 0.154]			
SIB ×			-0.222		
JEC HH ×			(0.148)		
Top wealth			[-0.568, 0.128]		
SIB ×				-0.068	
JEC HH ×				(0.140)	
Mother higher educ.				[-0.479, 0.232]	
SIB ×					0.092
JEC HH ×					(0.123)
Rural					[-0.174, 0.341]
Siblings	862	872	872	872	872
Households	361	365	365	365	365
N clusters	20	20	20	20	20

Note: Satellite site clustered standard errors appear in parentheses. 95% CIs calculated with the wild bootstrap procedure appear in brackets. Stars indicate significance levels using the satellite site clustered standard errors.

*** *p-value* < 0.01, ** *p-value* < 0.05, * *p-value* < 0.10.

Table 7

Mechanisms

	All siblings	Only younger siblings				
		Student level variables				
	Finish secondary school	R5 Math test	R5 Reading test	Misses classes	Parents help with homework	Most teachers with high quality
	(1)	(2)	(3)	(4)	(5)	(6)
SIB ×	0.089*	3.327*	1.133	-0.045**	0.006	-0.034
JEC HH (β_2)	(0.046)	(1.839)	(1.603)	(0.021)	(0.036)	(0.025)
	[-0.011, 0.194]	[-0.830, 7.389]	[-2.358, 4.897]	[-0.091, -0.001]	[-0.062, 0.082]	[-0.086, 0.012]
Control for type of sibling	X					
Control for R3 skills		X	X			
Control for R4 value				X	X	X
Siblings	872	336	332	361	356	352
Households	365	336	332	361	356	352
N clusters	20.00	20.00	20.00	20.00	20.00	20.00
Mean dep var.	0.89	37.76	64.30	0.05	0.12	0.17

Note: Column (1) reports estimates from our main sample using Equation (1). Columns (2)–(6) use only younger sibling data (one observation per household), with all outcomes measured in R5. For each case, the outcome variable is indicated at the top of the column. All columns include control variables from Table 1. Only column (1) includes household fixed effects. In Columns (2) and (3), we also control for math and reading skills measured in R3. In Columns (4)–(6), we include as control variable the pre-JEC value of the outcome variable from R4. Satellite site clustered standard errors appear in parentheses. 95% CIs calculated with the wild bootstrap procedure appear in brackets. Stars indicate significance levels using the satellite site clustered standard errors. *** *p-value* < 0.01, ** *p-value* < 0.05, * *p-value* < 0.10.

3

Appendix

Table A1

Effects on higher education attainment, considering different restricted samples

	Adjustments				
	Same JEC status between 2015-2016 (1)	Using age originally reported (2)	Siblings with similar age at control round (3)	Without restricting by school switching (4)	Siblings with time use information (5)
SIB (β_1)	0.009	0.019	0.011	-0.037	-0.008
	(0.059)	(0.063)	(0.070)	(0.052)	(0.063)
	[-0.108, 0.125]	[-0.115, 0.146]	[-0.127, 0.166]	[-0.141, 0.073]	[-0.132, 0.129]
SIB × JEC HH (β_2)	0.167*	0.169**	0.148*	0.134**	0.129
	(0.082)	(0.061)	(0.074)	(0.060)	(0.076)
	[-0.025, 0.355]	[0.038, 0.294]	[-0.013, 0.306]	[0.003, 0.264]	[-0.031, 0.296]
Siblings	810	876	737	1116	737
Households	339	366	324	467	317
N clusters	20	20	20	20	20

Note: Each column represents a separate estimation of Equation (1) using different restricted samples. Results can be compared relative to our preferred specification in Table 2, Panel B, Column (1). All columns include control variables from Table 1 and household fixed effects. Satellite site clustered standard errors appear in parentheses. 95% CIs calculated with the wild bootstrap procedure appear in brackets. Stars indicate significance levels using the satellite site clustered standard errors. *** *p-value* < 0.01, ** *p-value* < 0.05, * *p-value* < 0.10.

Table A2

Attrition rate between R5 and R6, using R1 characteristics

	JEC HHs			Non-JEC HHs			*p-value* ((1)-(2)) -((3)-(4))
	R6 (1)	Drop-out (2)	*p-value* (1)-(2)	R6 (3)	Drop-out (4)	*p-value* (3)-(4)	
Male younger child	0.538	0.412	0.334	0.488	0.625	0.145	0.101
Share of male older siblings	0.541	0.441	0.398	0.560	0.620	0.485	0.275
Age of younger child	0.964	0.941	0.765	0.988	1.042	0.331	0.416
Average age of older siblings	8.123	7.559	0.323	7.414	7.630	0.607	0.273
Birth order of younger child	3.857	3.706	0.743	3.557	3.812	0.444	0.474
Average birth order of older siblings	2.031	2.059	0.936	1.868	1.891	0.927	0.989
Members living in household	6.496	6.353	0.799	6.012	6.156	0.691	0.656
Siblings in school age	2.218	2.176	0.910	1.882	1.969	0.750	0.781
Members aged 65 years or over	0.118	0.176	0.546	0.085	0.000	0.134	0.170
Members under 5 years old	1.471	1.353	0.438	1.451	1.688	0.032	0.059
Members with higher education	0.370	0.235	0.566	0.390	0.125	0.053	0.605
Live with both parents	0.983	0.941	0.273	0.935	0.938	0.956	0.526
Wealth index	0.361	0.262	0.067	0.459	0.342	0.006	0.795
Mother attained higher education	0.109	0.118	0.918	0.122	0.031	0.126	0.326
Mother's age	31.034	30.706	0.829	30.780	30.438	0.758	0.994
Obs	119	17		246	32		
Attrition rate	0.125			0.115			

Note: Columns (1) to (4) report the mean values of the listed variables. The analysis includes only households that meet all sample requirements by R5. Columns (1) and (3) include households observed in both R5 and R6, while Columns (2) and (4) include households observed in R5 that dropped out in R6. All variables correspond to R1 data.

Table A3: Filters applied to the sample

		Loss		
Conditions	N Households	Obs.	Percent (%)	Cumulative %
A. Initial				
All households	2,052			
B. Inconsistencies in data				
Without inconsistencies in reported kinship ties to the YL child	2,011	41	2.0%	2.0%
C. With eligible older siblings				
With siblings	1,842	169	8.4%	10.2%
With older siblings	1,243	599	32.5%	39.4%
Older siblings with outcome data (18 years old or older in R5)	778	465	37.4%	62.1%
Older siblings with secondary studies but did not attend JEC	735	43	5.5%	64.2%
Older siblings with control variables data (12 years or younger in R1)	657	78	10.6%	68.0%
D. With eligible YL child				
YL child with information about type of secondary attended (R5)	627	30	4.6%	69.4%
YL child with outcome data (R6)	545	82	13.1%	73.4%
YL child attended public school	467	78	14.3%	77.2%
YL child stayed in the same school between 2014–2016	365	102	21.8%	82.2%

All conditions are cumulative.

Table A4

Representative households: included vs. excluded

	Included		Excluded		Included vs excluded	
	Mean	SD	Mean	SD	*p-value*	*SMD*
	(1)	(2)	(3)	(4)	(5)	(6)
Male younger child	0.504	0.501	0.500	0.500	0.879	0.009
Age of younger child	0.980	0.294	0.957	0.295	0.184	0.077
Birth order of younger child	3.655	1.770	2.228	1.622	0.000	0.841
Members living in household	6.170	2.030	5.603	2.383	0.000	0.256
Siblings in school age	1.992	1.436	0.769	1.256	0.000	0.906
Members aged 65 years or over	0.096	0.338	0.104	0.342	0.669	-0.025
Members under 5 years old	1.458	0.576	1.426	0.615	0.364	0.054
Members with higher education	0.384	0.819	0.483	0.938	0.060	-0.113
Live with both parents	0.951	0.217	0.811	0.392	0.000	0.442
Wealth index	0.427	0.226	0.424	0.240	0.804	0.015
Mother attained higher education	0.118	0.323	0.171	0.377	0.012	-0.151
Mother's age	30.863	5.832	25.969	6.659	0.000	0.782
Obs	365		1687			

Note: All variables correspond to R1. For each variable, Column (5) reports the *p*-value from a mean comparison test between included and excluded households. Column (6) reports the standardized mean difference (SMD) between the two groups.

Table A5

Balance analysis between JEC and non-JEC households, using the control variables

	JEC HHS		non-JEC HHs		Balance measures	
	Mean	SD	Mean	SD	*SMD*	*Log ratio SD*
	(1)	(2)	(3)	(4)	(5)	(6)
Male sibling	-0.008	0.654	-0.073	0.696	0.096	-0.062
Age (outcome round)	-1.167	1.655	-0.857	1.261	-0.211	0.272
Age (control round)	0.511	1.209	0.499	1.178	0.010	0.027
Birth order	1.818	0.879	1.685	0.839	0.155	0.047
Members living in household	-0.604	2.110	-0.359	1.504	-0.133	0.338
Siblings in school age	-0.444	1.268	-0.391	1.165	-0.043	0.084
Members aged 65 years or over	0.001	0.349	0.045	0.313	-0.131	0.108
Members under 5 years old	-0.644	0.814	-0.433	0.814	-0.260	-0.000
Members with higher education	0.262	0.752	0.247	0.848	0.019	-0.120
Live with both parents	-0.017	0.129	-0.018	0.177	0.010	-0.315
Wealth index	0.137	0.151	0.115	0.150	0.148	0.000
Mother attained higher education	0.021	0.136	0.033	0.172	-0.074	-0.231
Mother's age	7.882	2.528	7.001	2.590	0.344	-0.024
Obs	119		246			

Note: For each control variable, the value used is the within-household difference: the younger sibling's value minus the average of the older siblings. In column (5), SMD refers to the standardized mean difference between JEC and non-JEC households. In column (6), log ratio SD refers to the difference between the natural log of the standard deviation in JEC households and the natural log of the standard deviation in non-JEC households.

Table A6

Balance analysis between JEC and non-JEC households, using R1 information

	JEC HHS		non-JEC HHs		Balance measures	
	Mean	SD	Mean	SD	*SMD*	*Log ratio SD*
	(1)	(2)	(3)	(4)	(5)	(6)
Male younger child	0.538	0.501	0.488	0.501	0.100	-0.000
Share of male older siblings	0.546	0.447	0.561	0.465	-0.032	-0.038
Age of younger child	0.964	0.301	0.988	0.291	-0.081	0.035
Average age of older siblings	8.006	2.180	7.308	2.260	0.314	-0.036
Birth order	3.857	1.791	3.557	1.755	0.169	0.020
Average birth order of older siblings	2.039	1.378	1.872	1.279	0.126	0.074
Members living in household	6.496	2.220	6.012	1.916	0.233	0.148
Siblings in school age	2.218	1.439	1.882	1.425	0.235	0.010
Members aged 65 years or over	0.118	0.372	0.085	0.321	0.093	0.149
Members under 5 years old	1.471	0.594	1.451	0.568	0.033	0.046
Members with higher education	0.370	0.938	0.390	0.757	-0.024	0.213
Live with both parents	0.983	0.129	0.935	0.247	0.245	-0.649
Wealth index	0.361	0.210	0.459	0.228	-0.447	-0.084
Mother attained higher education	0.109	0.313	0.122	0.328	-0.040	-0.046
Mother's age	31.034	5.791	30.780	5.862	0.043	-0.012
Obs	119		246			

Note: For each control variable, the value comes from R1. In column (5), SMD refers to the standardized mean difference between JEC and non-JEC households. In column (6), log ratio SD refers to the difference between the natural log of the standard deviation in JEC households and the natural log of the standard deviation in non-JEC households.

Table A7

Effects on higher education attainment

	Type of older siblings			
	All	Closest one	Oldest one	With $\geq$ 18 yrs. at R4
	(1)	(2)	(3)	(4)
SIB (β_1)	0.004	-0.064	-0.070	0.047
	(0.060)	(0.083)	(0.092)	(0.121)
SIB $\times$ JEC HH (β_2)	0.171**	0.152**	0.186**	0.172**
	(0.064)	(0.067)	(0.067)	(0.081)
Male sibling	-0.088	-0.063	-0.075	-0.097
	(0.051)	(0.058)	(0.053)	(0.062)
Age (outcome round)	0.052***	0.082***	0.041**	0.037**
	(0.014)	(0.020)	(0.018)	(0.013)
Age (control round)	0.022	0.033	0.056*	0.042
	(0.026)	(0.029)	(0.027)	(0.027)
Birth order	0.032	0.042	0.053	0.018
	(0.040)	(0.048)	(0.042)	(0.049)
Members living in household	0.043	0.043	0.045	0.070***
	(0.025)	(0.033)	(0.029)	(0.021)
Siblings in school age	-0.105***	-0.093**	-0.112**	-0.113***
	(0.036)	(0.040)	(0.043)	(0.035)
Members aged 65 years or over	0.020	-0.036	0.034	0.028
	(0.095)	(0.086)	(0.096)	(0.104)
Members under 5 years old	-0.083*	-0.098*	-0.048	-0.087*
	(0.042)	(0.047)	(0.059)	(0.047)
Members with higher education	-0.200***	-0.168***	-0.212***	-0.285***
	(0.044)	(0.043)	(0.050)	(0.048)
Live with both parents	0.163	0.248	0.126	-0.074
	(0.239)	(0.258)	(0.292)	(0.261)
Wealth index	0.259	0.152	0.246	0.365
	(0.197)	(0.252)	(0.237)	(0.270)
Mother attained higher education	0.149	0.375**	0.222	0.080
	(0.174)	(0.175)	(0.173)	(0.178)
Mother's age	-0.010	0.004	-0.007	-0.009
	(0.013)	(0.014)	(0.012)	(0.019)
Constant	-0.713	-1.996***	-1.001	-0.596
	(0.492)	(0.666)	(0.675)	(0.732)
Siblings	872	730	730	561
Households	365	365	365	247
N clusters	20	20	20	20

Note: This table displays the results of Table 2, Panel B, showing the estimates for all control variables. All columns include household fixed effects. Satellite site clustered standard errors appear in parentheses. Stars indicate significance levels using the satellite site clustered standard errors. *** *p-value* < 0.01, ** *p-value* < 0.05, * *p-value* < 0.10.

Table A8

Propensity score estimation of JEC status

	Within household differences (1)	Baseline characteristics R1 (2)
Male sibling	0.178	
	(0.220)	
Male younger child		0.160
		(0.304)
Share of male older siblings		-0.206
		(0.219)
Age (outcome round)	-0.080	
	(0.080)	
Age (control round)	-0.126	
	(0.116)	
Age of younger child		-0.224
		(0.341)
Average age of older siblings		0.235***
		(0.064)
Birth order	-0.044	
	(0.182)	
Birth order of younger child		-0.667*
		(0.354)
Average birth order of older siblings		0.472
		(0.392)
Members living in household	0.013	0.062
	(0.097)	(0.126)
Siblings in school age	0.011	0.317
	(0.164)	(0.272)
Members aged 65 years or over	-0.569**	0.290
	(0.271)	(0.331)
Members under 5 years old	-0.203	0.380
	(0.222)	(0.420)
Members with higher education	-0.054	0.253
	(0.127)	(0.200)
Live with both parents	0.356	1.770**
	(0.558)	(0.881)
Wealth index	0.399	-3.560*
	(0.922)	(1.872)
Wealth perception (out of 5)		-0.092
		(0.148)
Mother without any formal education		-0.090
		(0.432)
Mother attained higher education	-0.570	0.405
	(0.585)	(0.465)
Mother's age	0.134**	-0.035
	(0.061)	(0.035)
Housing services index (0-1)		0.185
		(0.906)
House ownership		0.482
		(0.384)
Households	365	365

Note: The table presents the results from a logit model estimating the likelihood of a household being classified as a JEC household. Column (1) uses the within-household differences of the corresponding variables. Column (2) uses the R1 information of the corresponding variables. Not all variables are available in both cases. Clustered satellite site standard errors appear in parentheses. Stars indicate significance levels using the satellite site clustered standard errors. *** *p-value* < 0.01, ** *p-value* < 0.05, * *p-value* < 0.10.

Table A9

Effects on higher education attainment, using household's averages as extra controls

	Type of older siblings			
	All (1)	Closest one (2)	Oldest one (3)	With $\geq$ 18 yrs. at R4 (4)
SIB (β_1)	0.004 (0.061) [-0.113, 0.128]	-0.064 (0.084) [-0.232, 0.112]	-0.070 (0.093) [-0.259, 0.119]	0.047 (0.123) [-0.250, 0.294]
SIB $\times$ JEC HH (β_2)	0.171** (0.065) [0.034, 0.308]	0.152** (0.068) [-0.006, 0.293]	0.186** (0.067) [0.038, 0.330]	0.172** (0.082) [-0.011, 0.353]
Siblings	872	730	730	561
Households	365	365	365	247
N clusters	20	20	20	20

Note: All columns include the control variables from Table 1 and the household-level averages for these variables. Satellite site clustered standard errors appear in parentheses. 95% CIs calculated with the wild bootstrap procedure appear in brackets. Stars indicate significance levels using the satellite site clustered standard errors. *** *p-value* < 0.01, ** *p-value* < 0.05, * *p-value* < 0.10.

Table A10

Marginal effects of JEC by type of higher education, using the placebo sample

	Younger vs. older siblings	Closest older sibling vs.		
		Other older siblings	Second closest older sibling	Oldest sibling
	(1)	(2)	(3)	(4)
No higher education	-0.331**	-0.090	-0.138	-0.069
	(0.132)	(0.142)	(0.130)	(0.153)
	[-0.618, -0.043]	[-0.796, 0.615]	[-0.756, 0.479]	[-0.632, 0.495]
Tecnical education	0.199	0.047	0.070	0.034
	(0.122)	(0.125)	(0.122)	(0.135)
	[-0.054, 0.453]	[-0.289, 0.383]	[-0.295, 0.434]	[-0.258, 0.325]
University education	0.131*	0.044	0.069	0.035
	(0.068)	(0.077)	(0.073)	(0.088)
	[-0.117, 0.380]	[-0.900, 0.987]	[-0.688, 0.825]	[-0.673, 0.743]
Siblings	416	303	227	226
Households	113	113	113	113
N clusters	20	20	20	20

Note: Results come from a multinomial logit model based on Equation (1) and estimated with maximum likelihood using the placebo sample. Each row shows the marginal effect for the corresponding type of higher education. We control for older sibling indicator, male, age at outcome round and birth order, and the household-level averages for these variables. Satellite site clustered standard errors appear in parentheses. 95% bootstrap CIs appear in brackets. Stars indicate significance levels using the satellite site clustered standard errors. *** *p-value* < 0.01, ** *p-value* < 0.05, * *p-value* < 0.10.