Dynamic Complementarities in Human Capital Formation: Long-Term Evidence from Preschool and School Feeding

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Abstract

We study whether two large-scale educational investments act as complements in the production of human capital. We combine the staggered expansion of public preschools in Colombia with the subsequent decentralized roll-out of the national school feeding program. Using nearly two decades of administrative records on educational trajectories, we find that these investments are complementary for academic progression: students exposed to preschool are more likely to complete grades 9 and 11, less likely to drop out, and more likely to enroll in higher education *when also exposed to school feeding*. These complementarities are larger when school feeding is introduced shortly after preschool and fade when introduced in secondary school. Complementarities in academic performance on the high school exit exam are absent on average but also emerge if school feeding is introduced by the end of primary school. While preschool alone has limited effects, school feeding alone yields sizable medium- and long-term gains, suggesting that later investments can partially remediate the absence of early ones.

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1 Introduction

Access to early- and later-life interventions has expanded worldwide, with increased preschool enrollment, the scale-up of school feeding programs, and broader access to financial aid, all aimed at promoting human capital accumulation and improving long-term outcomes. However, the rapid expansion of multiple programs means that children are often exposed to overlapping interventions, potentially at different stages of development (Johnson and Jackson, 2019; Rossin-Slater and Wüst, 2020). Early investments can yield important benefits (Heckman and Mosso, 2014; Almond, Currie, and Duque, 2018; Hoynes, Schanzenbach, and Almond, 2016); theory suggests that later investments can sustain and possibly amplify these benefits – a concept known as *dynamic complementarity* (Cunha and Heckman, 2007). If such complementarities exist, identifying the effects of any single investment, especially in the long run, requires accounting for interactions with concurrent or subsequent interventions. In practice, whether complementarities arise is ex ante ambiguous and likely depends on the timing and nature of investments.

In this paper, we ask whether two of the most widespread programs targeting vulnerable children, public preschool and school meals, act as complementary investments in the production of human capital. Identifying such complementarities requires exogenous variation in both programs (Almond and Mazumder, 2013), a key feature of the context we study. We focus on a staggered expansion of free public preschools in Colombia between 2005 and the mid-2010s, using 12 to 18 years of administrative data that follow the universe of students who entered first-grade between 2006 and 2011. Efforts to expand access to public preschool began in the late 1990s, leading to a 30% increase in supply over the following decade. We combine this variation in preschool exposure with the subsequent roll-out of a second program: starting in 2012, the national school feeding program underwent a major expansion, largely determined by targeting rules. To improve retention and student health, the program provided free food rations during the school day, either as snacks or lunch.

The two expansions generate quasi-experimental variation in timing and exposure, resulting in cohorts of students who experienced neither, one, or both interventions at different points in their schooling trajectories. The latter allows us to test whether earlier exposure to school feeding leads to stronger complementarities with preschool. Moreover, we ask two additional critical questions. First, can preschool lead to lasting educational gains without subsequent investments? Second, can school meals remediate the absence of early investments? To answer these questions, we estimate program impacts among students only exposed to preschool and only exposed to school feeding, respectively.[1]

Our central finding is that students exposed to both preschool and school feeding experience lower dropout, higher secondary completion and post-secondary enrollment, than those only exposed to preschool, consistent with dynamic complementarity. These complementarities are larger when school feeding is introduced no later than seven years after preschool exposure and fade thereafter. While we do not detect average complementarities in performance on the high school exit exam, they emerge and are stronger the earlier school feeding arrives. Complementarities are concentrated among relatively advantaged students, while school feeding alone generates sizable gains for lower-income children. Overall, preschool alone has limited effects without follow-up investments, while the school feeding program alone produces substantial gains (albeit smaller than in the presence of early investments).

We begin by estimating the long-run effects of each program separately, and then test for complementarities using variation in exposure overlap. First, we use a staggered difference-in-differences design that leverages the expansion of public preschools across municipalities. Although all municipalities had at least one preschool by 2005, when our analysis begins, most students still lived more than 2 kilometers from the nearest one. Thus, we do not study the introduction of preschool, but rather the expansion of access in areas where it was already present. We classify municipalities as treated if a new public preschool opened within 2 kilometers of the geographic centroid after 2006, while those with a preschool already within that radius in 2005 are classified as always-treated, and the remainder as never-treated. We estimate intent-to-treat (ITT) effects using two-way fixed effects (TWFE), and contrast our findings with alternative estimators that are robust to treatment effect heterogeneity.

After the opening of a new preschool nearby, on average, the distance to the nearest preschool falls by 2.5 km and enrollment increases by 7.1 percentage points (p.p.) (11% relative to the pre-expansion mean of those not exposed). Effects are larger for students initially farther away (above 4 km in 2005): the distance decreases by 3.2 km and enrollment rises by 10 p.p. (17%). These results indicate that our measure of preschool exposure captures meaningful improvements in access, consistent with preschool enrollment being sensitive to costs and distance. Preschool exposure has sizable and lasting effects on academic outcomes: it increases the probability of reaching 5th, 9th, and 11th grade by 3, 2.1, and 2 p.p., respectively; reduces dropout

[1]Testing whether preschool and school meals are complementary involves comparing the effect of preschool alone to that of preschool plus meals. In the presence of complementarities, preschool may still yield significant but smaller effects on its own, or it may have no measurable impact. This distinction matters when long-term gains are attributed solely to early investments without accounting for later ones.

by 2 p.p.; and raises higher education enrollment by 1.3 p.p. We find similar effects across subgroups (e.g., gender, socioeconomic level, urban/rural). Scaling the intent-to-treat estimates by the first-stage enrollment effects implies treatment-on-the-treated (TOT) effects of 25–40 percentage points, substantially larger than long-term preschool estimates from other developed settings (Bailey, Sun, and Timpe, 2021; Gray-Lobe, Pathak, and Walters, 2023). Our *implied* TOT estimates, however, average over students with and without subsequent exposure to school feeding.

Starting in 2012, nearly half the students in our sample received school meals, with the share of beneficiaries increasing from 20% in our oldest cohort to 80% in our youngest cohort. To identify the separate impact of the feeding program, we leverage variation in the year it was first implemented in each student's first-grade school. We treat the first-grade school as the unit of exposure, since we observe all students in this grade regardless of preschool or school feeding, avoiding contamination from endogenous schooling transitions.

We observe no differential trends in the probability of receiving meals across schools, prior to treatment. Post-treatment, we estimate an increase in the probability of receiving school meals by 14 p.p. (45%). School feeding reduces dropout by 8.7 p.p. (14%), increases the probability of reaching grades 5, 9, and 11 by 7.9, 8.3, and 8.1 p.p. (13%, 24%, and 24%), respectively, and increases higher education enrollment by 4.5 p.p. (33%). Moreover, it significantly improves academic performance, increasing the probability of scoring in the top of the distribution in both math and reading. Effects are strongest among low-income rural children, in line with the program's targeting rules, while we find no significant differences by gender.

We next explore whether school feeding can boost the effect of preschool. That is, we test whether the returns to school feeding are higher for children exposed to preschool versus those who were not exposed. Identification relies on both investments being independent. In addition to evidence indicating no differential trends prior to preschool or school feeding exposure, we further assess the independence assumption by estimating event studies exploiting the variation in the timing of preschool (school feeding) exposure on binary indicators of year of arrival of school feeding (preschool).

We find no evidence that one investment predicts the arrival of the other. Additionally, event studies of the school feeding program, estimated separately for students with and without preschool exposure, show no differential pre-trends between the two groups – either in the probability of receiving meals or in any educational outcome. These pre-treatment patterns lend further credibility to our identification strategy. Moreover, post-treatment, exposure to the school feeding program increases the probability of receiving meals by about 14 p.p., regardless of preschool exposure.

Hence, we implement a triple difference-in-differences (DiDiD) strategy to identify the medium- and long-term effects of preschool exposure (capturing whether early investments produce lasting gains without reinforcement), school feeding exposure (capturing whether later investments can remediate the absence of earlier ones), and their interaction (capturing complementarities) on educational outcomes.

Preschool and school feeding act as complementary investments, on average, for academic progression through compulsory schooling and into post-secondary education. Students exposed to preschool are 2–2.5 percentage points more likely to complete grades 9 and 11 and enroll in higher education when later exposed to school feeding, compared to those without preschool exposure. However, we do not find evidence of such complementarities for academic performance. We interpret these average interaction effects on grade completion and dropout as consistent with the design of the school feeding program: while effective at supporting retention in school, its additional benefits may not extend to improvements in learning or cognitive outcomes.

Nevertheless, if the second program arrives during sensitive developmental periods or at schooling transitions, it may still reinforce earlier investments. Hence, we further explore whether the timing of school feeding– relative to preschool exposure– amplifies complementarities or reveals interaction effects in outcomes where none were detected on average. Indeed, we find that the effect of preschool on grade completion, dropout reduction, and higher education enrollment is larger when school feeding is introduced 2-7 years after preschool, and vanishes when the gap between investments exceeds eight years. For academic performance, where average interaction effects are not detected, we estimate sizable complementarities when school feeding follows shortly after preschool particularly within two to three years, when students are still in early primary grades. Overall, these findings underscore the importance of aligning later investments with key academic transitions.

Complementarities across all educational outcomes are significantly stronger for students from higher socioeconomic levels. We estimate interaction effects on the probability of reaching grades 9 and 11 of about 10 p.p. among students in strata 2–4, which are statistically different from the null effects observed among students in the lowest stratum.[2] In contrast, we do not observe significant gender differences and, although the interaction effects are somewhat larger for students in urban areas, these estimates are imprecise across the board. Using detailed records on household composition, parental education attainment and labor market participation at baseline for a subsample of students, we find that students in higher strata also have more

[2]Colombia classifies households into six socioeconomic strata for the purpose of targeting public subsidies and services. Stratum 1 is the lowest, and 6 is the highest.

educated mothers who are less likely to report domestic work as their main economic activity, and live in smaller households. We interpret these results as suggestive of reinforcing behaviors at home, if more educated parents with fewer children can invest more in their children. However, we lack more granular measures to confidently attribute our results to parental behaviors.

While we estimate positive effects of preschool alone on completing grade 5, it does not significantly improve academic progression or test performance in the absence of a subsequent investment. If anything, the implied TOTs for academic performance are negative, albeit imprecisely estimated. In contrast, school feeding can partially remediate the absence of preschool. We find large and significant effects on academic progression and test performance from exposure to school feeding alone, including in long-term outcomes such as higher education enrollment.

Related Literature. We contribute to the recent literature on dynamic complementarities in skill formation between public or private investments. While the theoretical foundations are well established (Cunha and Heckman, 2007; Heckman and Mosso, 2014), the empirical evidence using experimental (Bjorvatn, Ferris, Gulesci, Nasgowitz, Somville, and Vandewalle, 2025, 2024) or quasi-experimental variation (Rossin-Slater and Wüst, 2020; Bau, Rotemberg, Shah, and Steinberg, 2020; Goff, Malamud, Pop-Eleches, and Urquiola, 2023; Bharadwaj, Eberhard, and Neilson, 2018; Kinsler, 2016) largely finds no complementarities or even substitution between investments.[3] Most studies focus on high-income countries, while the evidence for middle- and lower-income countries analyzes interactions between parental investments or family environments (Bharadwaj et al., 2018; Goff et al., 2023), classroom inputs (Carneiro, Cruz-Aguayo, Hernandez-Pachon, and Schady, 2022), and natural disasters or rainfall shocks (Bau et al., 2020) and the remediation potential of cash transfers or health interventions (Adhvaryu, Molina, Nyshadham, and Tamayo, 2024; Duque, Rosales-Rueda, and Sanchez, 2023; Gunnsteinsson, Molina, Adhvaryu, Christian, Labrique, Sugimoto, Shamim, and West, 2022). We provide novel evidence from a middle-income country on complementarities between two large-scale public educational investments, each with distinct oversight and goals. Exploiting variation in their relative timing, we show that shorter intervals between investments amplify complementarities – an approach rarely feasible when investments are delivered contemporaneously, as in most experimental settings.

Within this literature, we add to scant work examining the interaction between

[3]For exceptions, see Gilraine (2017) and Johnson and Jackson (2019), who document complementarities between educational investments in the U.S. using quasi-experimental variation. Cunha, Heckman, and Schennach (2010) and Attanasio, Meghir, and Nix (2020) find complementarities between parental investments using dynamic factor models. In contrast to the latter, we focus on complementarities between public investments.

early childhood education or environments and subsequent investments. Existing evidence points mainly to limited or negative interactions: a nurse home visiting program and preschool are substitutes (Rossin-Slater and Wüst, 2020); better family environments reduce the payoff to high school quality (Goff et al., 2023); and experimental evidence shows no interaction between free childcare and cash transfers (Bjorvatn et al., 2025, 2024).[4] Our findings most closely resemble Johnson and Jackson (2019), who find complementarities between Head Start and per-pupil spending in public schools in the U.S. Similar to their case, complementarities in our setting are likely to arise between two education programs that target different parameters of the production of skills: preschool, which rarely provided nutritional or health services, and school feeding, which directly enhances these inputs. Our results nonetheless suggest that complementarities are concentrated among relatively better-off children, indicating that better home environments may be a necessary condition to fully realize the benefits of multiple public investments.[5]

We also contribute to the literature evaluating the long-term impacts of preschool on educational outcomes. Recent work for the U.S. shows sizable impacts of Head Start on educational attainment (Bailey et al., 2021) and of preschools in Boston on college enrollment and behavioral outcomes (Gray-Lobe et al., 2023). In Latin America, Berlinski, Galiani, and Manacorda (2008) and Berlinski, Galiani, and Gertler (2009) document positive effects on attainment and test scores in Uruguay and Argentina, and Behrman, Gomez-Carrera, Parker, Todd, and Zhang (2024) show that Mexico's preschool mandate improved test scores in 5th and 6th grade, educational attainment, and noncognitive skills in the long-term. Similar to the latter, we study an expansion relevant for students from diverse socioeconomic backgrounds (rather than targeting the most vulnerable, as other programs such as Head Start). Although we find sizable preschool impacts, most of these are explained by interactions with the school feeding program. Our contribution lies in showing that, in a context of heterogeneous infrastructure and service quality, preschool contributes little to achievement or test performance during compulsory schooling unless reinforced by later investments.[6]

[4]Novel work on genetic endowments has also analyzed complementarities with investments. For the U.K., Muslimova, Van Kippersluis, Rietveld, Von Hinke, and Meddens (2024) find that genetic endowments are complementary with investments, whereas Biroli, Galama, von Hinke, van Kippersluis, Rietveld, and Thom (2025) report substitution with school inputs.

[5]Other papers have explored how early programs interact with later inputs. Bailey et al. (2021) find indicative evidence that the impacts of Head Start in educational attainment are driven by complementarities with health services, and are substitutes with access to Food Stamps. For Head Start, Currie and Thomas (1995) also argue that, unless early investments are sustained, their effects would decline in later years. Similarly, Chakraborty and Jayaraman (2019) find suggestive evidence of complementarities with the national school feeding program in India and teacher inputs, but not with school infrastructure. Importantly, these studies do not use exogenous variation in additional/subsequent investments and hence cannot credibly test for dynamic complementarities.

[6]The absence of test score effects from preschool exposure alone is consistent with the fade-out

High-quality early childhood programs in Colombia have been shown to improve child development outcomes (Bernal and Ramírez, 2019; Bernal, Attanasio, Peña, and Vera-Hernández, 2019). Existing studies focus on integrated services for vulnerable children, particularly under the national early childhood strategy launched in 2011 for children ages 0–5.[7] Andrew, Attanasio, Bernal, Sosa, Krutikova, and Rubio-Codina (2024) provide experimental evidence showing that preschools under this strategy, with higher teaching quality, improved children cognitive development 18 months after the intervention. We extend this literature by studying a national expansion of public preschools, typically attached to primary schools, characterized by heterogeneous quality, and serving students from broader socioeconomic backgrounds. Our focus is on the final preschool grade—mandatory by regulation but weakly enforced—which precedes entry into primary education.[8] We show that, when followed by a subsequent investment, even preschools of uneven quality and without standardized curricula can have long-term educational impacts.

Last, although our paper is not the first to evaluate the impacts of school meals in Colombia, we contribute with new evidence showing that their effects, while lessened in the absence of preschool, are sizable and significant. Collante-Zárate, Rodríguez, and Sánchez (2024) evaluate the national feeding program, using a different identification strategy for a larger sample, and find similar results to ours: it increases grade completion, improves academic performance in math and reading, and higher education enrollment. In other middle- and low-income countries, the evidence is mixed (Maluccio, Hoddinott, Behrman, Martorell, Quisumbing, and Stein, 2009; McEwan, 2013).[9] We also find that earlier arrival of school meals, relative to preschool exposure, has the largest complementarities on compulsory schooling and academic per-

literature, which finds that early cognitive gains often dissipate while noncognitive improvements persist and shape longer-term outcomes (Currie and Thomas, 1995; Deming, 2009; Heckman, Moon, Pinto, Savelyev, and Yavitz, 2010; Heckman, Pinto, and Savelyev, 2013; Bailey et al., 2021; Gray-Lobe et al., 2023). We lack measures of socioemotional skills or disciplinary behavior to test this hypothesis in our context.

[7]See Bernal and Ramírez (2019) and Bernal et al. (2019) for evaluations of the impacts of "From Zero to Forever" (*De Cero a Siempre*, in Spanish), a national policy that improved the provision of home-based care services, expanded towards large integral development centers, and provided caregiver training, among others strategies, for children from the lowest socioeconomic levels. Unlike the preschools we study, those under this policy offered a more standardized service with pedagogical guidelines for cognitive and socioemotional skills, fine and motor development, and nutritional meals. Importantly, children in our sample were above the policy's target age at the time of its implementation in 2011.

[8]Because our analysis centers on the final preschool grade (known as "Transición", the third year in Colombia's preschool level), we cannot assess variation in effects by exposure length. Prior studies find mixed results: longer preschool duration can improve cognitive outcomes (Behrman, Cheng, and Todd, 2004) and grade progression (Behrman et al., 2024), but also reduce later attainment (Berlinski et al., 2008).

[9]For instance, Maluccio et al. (2009) finds impacts 25 years later of nutritious meals on educational attainment and reading comprehension, while McEwan (2013) studies the context of high calorie meals provided to students in Chile and finds no effects on primary education outcomes.

formance. These results are consistent with the evidence from high-income countries showing that earlier or longer exposure to school feeding generates stronger gains (Hoynes et al., 2016; Bütikofer, Mølland, and Salvanes, 2018; Lundborg, Rooth, and Alex-Petersen, 2022).[10] Our paper contributes to this large literature by showing that school meals are more productive if preceded by an earlier investment (preschool, in our case), but nonetheless can on their own improve outcomes for children with initial disadvantages.

2 Context and Background

2.1 Preschool

We study the expansion of free public preschools in Colombia between 2005 and 2015. Most of the regulatory framework supporting preschool enrollment predates this period. According to Article 67 of the 1991 Constitution, the final year of preschool education (known as "Transición"; preschool, hereafter) is mandatory for children under the age of six in public institutions. The earlier grades of "Pre-jardín" (age 3) and "Jardín" (age 4) are not required for preschool entry, and attendance in preschool is not a prerequisite for entering primary school (Decree 2247 of 1997). The main goals of preschool education included promoting children's integral development through play-based learning, and although health and nutrition were recommended, they were not mandatory or part of the pedagogical framework. In 2003, the Ministry of Education established guidelines to allocate preschool slots, including prioritizing children enrolled in services managed by the Colombian Institute of Family Welfare (*ICBF*, in Spanish).[11] Despite these regulations, enforcement was weak. As a result, and notwithstanding its mandatory status, the net preschool enrollment rate was around 30% in the early 2000s (Ministerio de Educación Nacional, 2002) and fewer than half of first-grade students in 2006 had attended preschool.

In parallel to policies on enrollment, the legal framework also encouraged the expansion of public preschool providers. Law 115 of 1994 and Decree 2247 of 1997 mandated a gradual rollout of preschool grades across all public institutions, specifically those offering primary education. By 2005, every municipality had at least one preschool, yet many children still lived far from the nearest one. Appendix Figure A1 shows that between 2005 and 2014, the number of preschools grew by 30%, the average distance to the nearest one fell by 2 kilometers, and enrollment increased by more

[10] See Alderman, Bundy, and Gelli (2024) and Ayllón and Lado (2025) for reviews of the impacts of school meals on education, health, and other outcomes.

[11] In 2005, most of ICBF services consisted of small, home-based care centers (*Hogares Comunitarios*, in Spanish) serving the most vulnerable children aged 0-5 (Bernal and Ramírez, 2019).

than 20 percentage points. By 2015, the net preschool enrollment rate had reached 62%, remaining at similar levels in subsequent years (Ministerio de Educación Nacional, 2025).

Where did public preschools open, and how did they differ from existing ones? Appendix Table A1 shows that most were located in rural areas. Compared to preschools operating in 2005, newly opened preschools were more likely to offer a full school day, have smaller class sizes, and be located within schools offering primary grades. While we lack direct measures of quality, these patterns suggest that the expansion may have occurred through relatively better services. Newly opened preschools were also less likely to provide services for children 3-4, indicating that the expansion primarily targeted the mandatory grade. Despite these improvements, the public preschool system in the 2000s remained characterized by uneven service quality and limited oversight, in contrast to ICBF services, which operated under clearer national standards, albeit often with low-quality indicators (Bernal, 2014).

2.2 School Feeding Program

Between 2011 and 2012, the provision of school meals in public schools in Colombia underwent a major institutional transformation, shifting from centralized delivery by ICBF to a decentralized model overseen by the Ministry of Education (MEN, hereafter), with the objective of achieving universal coverage (Law 1450 of 2011). Although it is one of the country's oldest social programs (operating in various forms since the 1920s), by 2010 it mainly served students from the lowest socioeconomic level in urban areas (Departamento Nacional de Planeación, 2013).[12] At the onset of MEN's oversight in 2012, only 10% of students were registered as receiving meals; by 2019, this number had increased to 73% (Collante-Zárate et al., 2024). Our analysis focuses on the impacts of the program in its current form, implemented under MEN since 2012.

The school feeding program has traditionally provided free nutritional supplements to students in public schools in the form of snacks, breakfast, or lunch. Its primary goal is to support enrollment and attendance, while also promoting healthy habits. To implement the program, MEN provides guidelines to Local Education Authorities ("Secretarías de Educación", in Spanish) which are responsible for allocating resources, identifying beneficiaries, and selecting schools to participate in the program. Priority is given to full-day schools in rural areas, and to urban schools serving ethnic minorities, students with disabilities, or low-income students. Within partici-

[12] Coverage estimates for 2008-2010 are based on household surveys with self-reported receipt of school meals, in contrast to the administrative data we use from 2012, which contains official records of beneficiaries.

pating schools, implementation must begin with the lower grades and expand to students in upper grades depending on available resources, prioritizing low-income students from minorities, with disabilities, or victims of conflict (Collante-Zárate et al., 2024).

In theory, these guidelines (depending on budgetary constraints) determine which schools receive school meals, when they receive them, and which students benefit. In practice, the share of full-time students, enrolled in primary grades, or victims of conflict are strong predictors of the probability that schools are assigned to receive meals (Collante-Zárate et al., 2024). Within schools, allocation began from the lower grades, as suggested, but did not adhere to other prioritization criteria. Instead, regardless of student characteristics, either all or none of the students in a given grade received meals (Collante-Zárate et al., 2024). This suggests that whether a student received meals mainly depended on the timing of arrival of the program at their school (determined by Local Education Authorities) and the student's grade at that time, with younger students more likely to benefit earlier.

3 Data

We match nearly two decades of several administrative records to identify students' preschool attendance, school meals, academic progression, performance on the high school exit exam, and higher education enrollment. Our primary data source is MEN's Integrated Enrollment System (SIMAT), which contains individual-level longitudinal records of student enrollment from "Pre-jardín" to 11th grade for the period 2005-2023, annual indicators of whether each student received school meals (2012–2023), and school-level characteristics. Using SIMAT, we identify whether students completed primary school (reaching at least 5th grade), progressed to 9th and 11th grade, or ever dropped out.[13] To measure academic performance, we match SIMAT to data from "Saber 11", Colombia's national high school exit exam administered by the Colombian Institute for the Evaluation of Education (ICFES). These records, available from 2010–2023, include test scores in math and reading. Finally, we identify post secondary enrollment by linking SIMAT to the National Higher Education Information System (SNIES), also maintained by MEN, which covers the years 2013–2023 and records student enrollment in bachelor's, technical, or technological programs.

For a subset of students, we can also match household records from the System for Identifying Potential Beneficiaries of Social Programs (Sisbén, in Spanish), adminis-

[13] We define ever dropping out as a binary variable equal to one if the student disappears from SIMAT records in t, $t+1$, and $t+2$, and does not reach grade 11th or takes the high school exit exam.

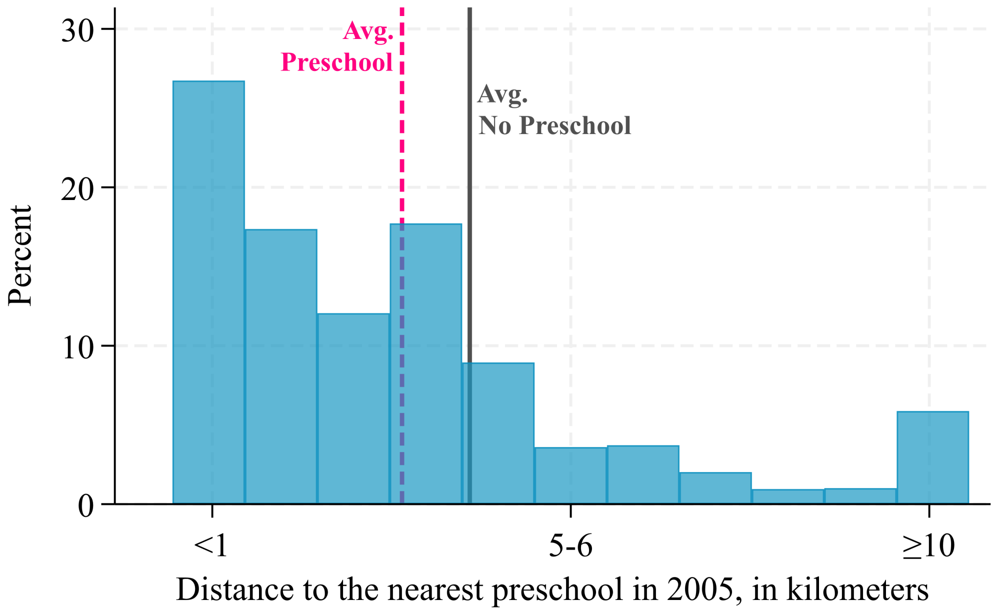
tered by the National Planning Department (DNP, in Spanish). These records include information on household composition, assets, educational attainment, and employment status of household members from interviews conducted between 2003-2010. We complement these individual-level records with municipal-level information from the National Department of Statistics (DANE) and the Center for Economic Development Studies (CEDE). Using DANE's 2005 National Geostatistical Framework (MGN 2005), we compute geographic centroids for all municipalities. The Municipal Panel from CEDE provides data for 2004 on education levels, poverty rates, population, homicides, municipal area, and distance to the capital and main markets.

3.1 Sample Definition and Descriptive Statistics

To identify preschool attendance and its long-term impacts, we focus on the universe of students enrolled in 1st grade of primary school in the public system between 2006-2011 (cohorts, hereafter) using SIMAT. By focusing on these cohorts, all students, including those in 1st grade in 2011, have at least two years after on-time high school graduation to enroll in higher education. We restrict the sample to new students in each cohort (*i.e.*, students appearing in 1st grade for the first time). For each cohort, we link students to preschool enrollment records from the preceding year ($t-1$), from 2005-2010. We define a binary indicator for preschool attendance equal to one if the student appears in the preschool records in $t-1$, and zero otherwise. Of roughly 3.3 million students who entered first grade between 2006 and 2011, only 66% had attended preschool.

What explains the preschool enrollment gap? We argue that a key driver is the availability of preschools at the time families make enrollment decisions. To measure preschool availability or exposure, we begin by computing the minimum distance from the geographic centroid of a student's municipality of birth to the nearest preschool in 2005, our baseline year. For this, we use data from MEN's School Identification System (SISE, in Spanish), which provides the precise geographic coordinates of all schools nationwide. Figure 1 shows the distribution of this distance: the median distance is 2 km, and on average, the nearest preschool was 1 km closer for students who enrolled (dashed pink line) compared to those who did not enroll (solid gray line). To leverage the expansion of preschools between 2005 to 2015, described in Section 2.1, we restrict the sample to students whose baseline distance was greater than 2 km. We classify students within 2 km at baseline as always-treated (namely, those who were already relatively close to preschool and thus likely unaffected by subsequent openings). This restriction results in a sample of 1,704,991 students across 483 municipalities, representing 44% of all municipalities in the country.

Figure 1. Distribution of Average Distance to the Nearest Preschool, at Baseline



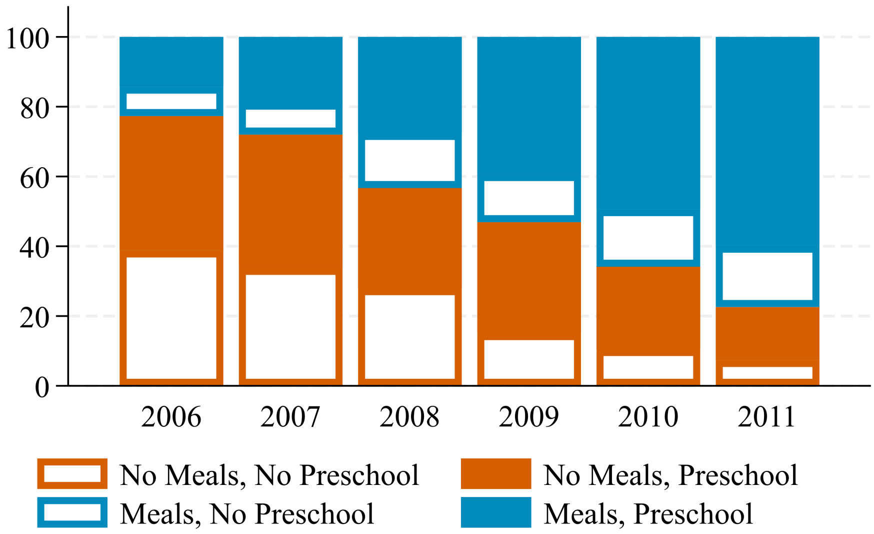
Note: The figure displays the distribution of the average distance from the geographic centroid of a student's municipality of birth to the nearest preschool in 2005 (the baseline year). The dotted pink line denotes the average distance at baseline for students who went to preschool, while the solid gray line denotes the average distance for students who did not go to preschool. Source: SIMAT and DANE.

Importantly, Appendix Table A2, Panel A, shows that students in the restricted sample (column ”Sample”) are, on average, similar to those in the full sample (column ”All Students”) in terms of observable characteristics, with only small differences in income distribution and baseline math and reading scores. Meanwhile, the share of low-income rural students is higher among those who did not enroll in preschool (column ”No Preschool”) compared to those who did (column ”Preschool”). Descriptively, we also observe higher dropout rates, slower grade progression, and worse performance on the high school exit exam among students who did not attend preschool.

We next examine how preschool openings evolved across municipalities over time for students in our estimating sample. Because we do not observe students' residential addresses, we define exposure at the level of the student's municipality of birth, which predates preschool enrollment. Specifically, we compute the distance from the geographic centroid of each municipality to the nearest preschool using annual data on preschool supply from 2006 to 2015 from SIMAT. We then define the first year in which a preschool was available within a 2 km (the median distance at baseline) radius of the centroid as the timing of exposure. Among the 483 municipalities in our sample, 21% experienced such preschool openings between 2006 and 2015. Appendix Figure A2 illustrates the spatial and temporal variation in preschool exposure according to this definition.

To explore what predicts preschool exposure, we regress an indicator for whether a municipality was ever exposed, as well as the year of exposure, on municipal characteristics measured in 2004. The number of ICBF beneficiaries is marginally significant in predicting exposure in 2007. In addition, only municipal area and distance to the departmental capital, invariant factors across municipalities, are statistically significant predictors (Appendix Table A3).

Figure 2. Evolution of Preschool Enrollment and Share of School Meals Beneficiaries, by Cohort



Note: The figure shows the share of students who went to preschool, got school meals, both, or neither by cohort (*i.e.*, the year of entry into first grade). Source: SIMAT.

With the scale-up of the national feeding program beginning in 2012, how did school meal coverage evolve across our cohorts? We identify school meal beneficiaries using SIMAT records and define a binary indicator equal to one if a student received school meals in any year. Appendix Table A2 shows that 48% of students in our sample received school meals. This share is higher among students who attended preschool compared to those who did not (54% versus 38%, respectively). Reflecting the staggered rollout of the program, the share of students receiving meals increased from roughly 20% in the 2006 cohort to nearly 80% in the 2011 cohort (Figure 2). Notably, the figure also shows that preschool enrollment rose over the same period, from under 50% in the 2006 cohort to nearly 80% in the 2011 cohort. In terms of student characteristics, we do not observe differences by school meals participation in age, sex, or income level; yet, students who received meals are less likely to drop-out, and more likely to complete primary and secondary, and enroll in higher education (see Appendix Table B1).

The national school feeding program expanded gradually across schools after 2012. By that time, some students in our cohorts had already repeated grades or dropped out, making grade-based measures of program uptake potentially endogenous to prior student characteristics, including preschool attendance. To address these concerns, we use the first year in which each student's first-grade school received the program as a proxy, since first-grade enrollment predates both the national scale-up and later schooling transitions. Of 32,270 first-grade schools, the vast majority received school meals at some point between 2012-2019. Appendix Figure B1 shows the distribution of program rollout across first-grade schools, with most entering between 2012-2014. As expected from the prioritization rules, Appendix Table B2 shows that rollout timing correlates with observable school characteristics, including full-time school days, rural status, the share of preschool and primary students, and the share of low income students in 2011.

4 Identification and Estimation

In this section, we begin by describing how we identify and estimate the medium- and long-run effects of each intervention separately, before testing for complementarities using variation in exposure overlap. For both interventions, we use a staggered difference-in-difference design comparing students affected by each expansion to those unaffected. Given the identification and estimation challenges associated with dynamic complementarities, we adopt a parsimonious strategy and estimate our main results using two-way fixed effects (TWFE). Nonetheless, we assess the robustness of our findings using alternative estimators (Cengiz, Dube, Lindner, and Zipperer, 2019; Sun and Abraham, 2021).

Throughout this section, Y denotes one of the following student-level outcomes of interest: the probability of ever dropping out; the probability of reaching grade 5, 9, or 11; standardized math and reading test scores on the high school exit exam; and the probability of enrolling in higher education.

4.1 Preschool

To identify the effects of preschool on medium-to long-term academic outcomes, we use a staggered difference-in-differences design that leverages the expansion of public preschools across municipalities. We classify students as treated if they lived more than 2 kilometers from the nearest preschool in 2005 and a new preschool opened within 2 kilometers of the centroid of their municipality of birth between

2006 and 2015.[14] We assume that exposure is an absorbing state, such that once a preschool opens the municipality remains treated thereafter. Overall, our approach captures variation along the intensive margin of access–specifically, improvements in geographic proximity–rather than the introduction of preschool itself.

Our design compares outcomes across students who gained access to preschool earlier and those who had not yet gained access—or never did—by the time they entered preschool. That is, our control group includes both not-yet-treated and never-treated students. Identification relies on the following assumptions: (i) parallel trends–in the absence of preschool openings, potential outcomes would have evolved in parallel for all exposed and unexposed groups, (ii) no spillovers– outcomes in unexposed areas are not affected by nearby preschool openings, and (iii) no anticipation–outcomes in municipalities where preschools opened were not affected by the upcoming expansion. Under these assumptions, our design recovers the intent-to-treat (ITT) effect of preschool exposure. Moreover, we can also recover *implied* treatment-on-the-treated (TOT) effects as the ratio of each outcome ITT to the (first-stage) ITT on preschool enrollment.

First, to determine whether preschool exposure affects preschool enrollment (*i.e.*, the first-stage), we use the following event-study model:

$$D_{i,s} = \alpha_{m(s)} + \alpha_{t(i)} + \sum_{j=-5, j\neq -1}^{4} \beta_j \mathbb{1}[\text{Exp}_{m(s),t(i)} = j] + \alpha_{d(s)} \times \alpha_{t(i)} + \epsilon_{imt}, \quad (1)$$

where $D_{i,s}$ is a binary variable equal to one if student i in school s from municipality m enrolled in preschool, and zero otherwise. $\text{Exp}_{m(s),t(i)} = t - \text{Open}_{m(s)}$, where t denotes the student's preschool year (cohort year-1), and $\text{Open}_{m(s)}$ is the year a preschool first became available within 2 km of the centroid of the student's municipality of birth. We restrict the exposure window to 5 years for each cohort ($j \in \{-5, 4\}$, omitting $j = -1$). $\alpha_{m(s)}$ are municipal fixed-effects, which capture m characteristics invariant overtime, and $\alpha_{t(i)}$ are cohort fixed-effects invariant across space. We also include a department d trend, $\alpha_{d(s)} \times \alpha_{t(i)}$, which captures common time-varying factors between municipalities in the same department. ϵ_{imt} is the error term.

To assess the effects of preschool exposure on academic outcomes, we follow a similar event-study specification where the dependent variable is replaced by an outcome of interest for student i in school s from municipality m,

[14]We set the 2 km threshold based on both data and prior evidence. First, 2 km corresponds to the median distance to the nearest preschool at baseline in 2005. Second, low travel time or distance is a well-known determinant of childcare use (Attanasio, Maro, and Vera-Hernández, 2013; Bernal and Fernández, 2013). Nonetheless, in Section 5.1 we assess the robustness of our results to employing different radius.

$$Y_{i,s} = \alpha_{m(s)} + \alpha_{t(i)} + \sum_{j=-5, j\neq -1}^{4} \beta_j \mathbb{1}[\text{Exp}_{m(s),t(i)} = j] + \alpha_{d(s)} \times \alpha_{t(i)} + \epsilon_{imt}. \tag{2}$$

We also estimate pooled versions of equations (1) and (2) with TWFE as follows,

$$Y_{i,s} = \alpha_{m(s)} + \alpha_{t(i)} + \beta \mathbb{1}[\text{Exp}_{m(s),t(i)} \geq 0] + \alpha_{d(s)} \times \alpha_{t(i)} + +\epsilon_{imt}, \tag{3}$$

where β identifies the overall dynamic effect of preschool exposure. For inference, in equations (1) to (3), we cluster standard errors at the municipality level.

Because our ITTs rely on the parallel trends assumption, we use the leads in equations (1) and (2), β_j for $j < -1$, to examine whether exposed and unexposed groups were trending similarly prior to preschool openings. That is, we test whether the pre-exposure coefficients are statistically indistinguishable from zero. A second threat to identification is spillovers: families in unexposed municipalities may change their enrollment choices in response to preschool openings in neighboring municipalities. However, given the importance of proximity for preschool enrollment decisions, it is unlikely that families would regularly cross municipal boundaries to access newly opened schools.

Last, TWFE suffers from "forbidden comparisons" (Borusyak, Jaravel, and Spiess, 2024). Hence, we contrast our results with alternative estimators robust to treatment effect heterogeneity and dynamic effects (Sun and Abraham, 2021; Cengiz et al., 2019).[15]

4.2 School Feeding Program

We next focus on the medium-and long-term effects of the scale-up of the feeding program from 2012 onward. There are two important considerations for identification. First, neither receiving school meals nor the grade at which students receive them is orthogonal to student characteristics. Second, the treatment year is inherently unobserved for students who never received school meals. Restricting the analysis to meal recipients would introduce bias, particularly when estimating dynamic complementarities.

[15]We use the estimators from Sun and Abraham (2021) and the stacked event-study design of Cengiz et al. (2019) to assess robustness for two reasons. First, our focus on dynamic complementarities makes TWFE the most straightforward estimator to implement and interpret, as most recent multiple-treatment estimators do not accommodate staggered adoption. Second, both alternatives are computationally more efficient, given our sample size, and produce dynamic estimates that treat pre-treatment and post-treatment coefficients symmetrically (Roth, 2024; Wooldridge, 2025) and are more directly comparable to TWFE.

To address the first concern, we define treatment as the first year the student's first-grade school received school meals. To address the second, we assign a *placebo* treatment year to non-recipients, defined as the average year of meals receipt among students of the same cohort, sex, and municipality. Using a staggered difference-in-differences design, we compare outcomes between students whose first-grade school (s) had entered the program by the year they received school meals (or the assigned *placebo* year for non-recipients) and those whose first-grade school had not yet received the program in that year.[16] Identification relies on the parallel trends assumption, such that in the absence of school meals, potential outcomes would have evolved in parallel for all groups of first-grade schools that received the program in different years. We assume no spillovers: the rollout of school meals in one school does not affect outcomes in schools that have not yet received the program. Under these assumptions, we can identify the ITT of school feeding exposure and recover the *implied* TOTs, dividing outcome ITTs by the first-stage ITT.

For estimation, we use an event-study model,

$$F_{i,s} = \alpha_{m(s)} + \alpha_{t\_\text{sfp}(i)} + \alpha_{t(i)} + \sum_{j=-4, j\neq -1}^{5} \gamma_j \mathbb{1}[\text{Exp}^{\text{sfp}}_{s,t\_\text{sfp}(i)} = j] + \alpha_{d(s)} \times \alpha_{t(i)} + \mu_{imt}, \quad (4)$$

where $F_{i,s}$ is a binary variable equal to one if student i of school s ever received school meals, and zero otherwise. $\text{Exp}^{\text{sfp}}_{s,t\_\text{sfp}(i)} = t\_\text{sfp} - \text{SFP}_s$, where SFP_s is the first year in which school s (the student's first-grade school) received the school feeding program (SFP), and t_sfp is the year the student received meals for the first time or the *placebo* year (for non-beneficiaries). The model includes municipality fixed-effects, preschool cohort fixed-effects, and department trends. Plus, timing of school feeding fixed-effects, $\alpha_{t\_\text{sfp}(i)}$, to capture common trends across students first exposed to the program in the same year. We control for school-day type (full-day vs. half-day). Last, to account for the "imputation" of treatment year for non-beneficiaries, we additionally control for sex. μ is the idiosyncratic error term. Equation (4) denotes the first-stage of school feeding exposure; that is, whether our treatment definition predicts receipt of school meals.

For outcomes, we estimate:

$$\begin{aligned} Y_{i,s} = {} & \alpha_{m(s)} + \alpha_{t\_\text{sfp}(i)} + \alpha_{t(i)} \\ & + \sum_{j=-4, j\neq -1}^{5} \gamma_j \mathbb{1}[\text{Exp}^{\text{sfp}}_{s,t\_\text{sfp}(i)} = j] + \alpha_{d(s)} \times \alpha_{t(i)} + \mu_{imt}, \end{aligned} \quad (5)$$

[16] In the context of school feeding and our treatment definition, 5% of students fall into the never-treated group.

and pooled versions of equations (4) and (5) with TWFE,

$$Y_{i,s} = \alpha_{m(s)} + \alpha_{t\_\mathrm{sfp}(i)} + \alpha_{t(i)} + \gamma \mathbb{1}[\mathrm{Exp}^{\mathrm{sfp}}_{s,t\_\mathrm{sfp}(i)} \geq 0] + \alpha_{d(s)} \times \alpha_{t(i)} + \mu_{imt}, \tag{6}$$

where γ identifies the overall dynamic effect of school feeding exposure. We cluster standard errors that the first-grade school (s) level in equations (4) to (6).[17]

To assess the validity of the parallel trends assumption, we test whether the lead terms in equations (4) and (5), γ_j for $j < -1$, are statistically different from zero. For robustness, because TWFE makes clean and forbidden comparisons, we use alternative estimators robust to heterogeneous effects and variation in treatment timing (Sun and Abraham, 2021).[18]

4.3 Interaction between Investments

We combine variation in exposure to preschool and to the school feeding program to estimate dynamic complementarities. Our design compares schools exposed to the feeding program with those not yet exposed, in municipalities with and without preschool expansions.

Identification relies on the assumption that the two shocks–preschool and school feeding–are exogenous and independent. That is, preschool exposure should not predict the timing of school feeding, and vice versa. In our setting, preschool expansion largely precedes the scale-up of the national school feeding program. However, given its focus on early grades, the introduction of school meals may have incentivized local authorities to expand preschool provision. This seems unlikely in practice, as opening a preschool typically requires substantial infrastructure investments and staffing beyond what would be justified by the arrival of meals alone. Conversely, it is possible that municipalities with preschool access were prioritized for earlier rollout of school meals, for instance due to existing infrastructure. Yet, since preschool exposure is defined based on whether a new preschool opened within 2 km of the municipality centroid, it is not clear that such localized expansions would affect the timing of school meals rollout.

To address these concerns, we assess the plausibility of the independence assumption in two ways. First, we test whether the timing of one shock is predicted by

[17] We exclude school fixed-effects from equations (4)-(6) to preserve the cross-municipality variation in preschool exposure, which is defined at the municipality level and is central to identifying dynamic complementarities between preschool and school feeding. Since each school belongs to a single municipality, including school fixed effects would absorb all variation needed to estimate the preschool effect.

[18] Unlike the case of preschool, we refrain from using stacked designs in this case since the share of never-treated school feeding students is only 5% leading to noisy estimates when estimating cohort(stacks)-specific effects.

exposure to the other. Specifically, we re-estimate equation (1) replacing the dependent variable with a set of year-specific indicators for school feeding rollout, $\mathbb{1}[\text{SFP}_s = y]$, for $y \in [2012, 2022]$. Similarly, we re-estimate equation (4) using indicators for preschool opening year, $\mathbb{1}[\text{Open}_{m(s)} = y]$, for $y \in [2006, 2015]$, as the dependent variable. Under the independence assumption, the event-time coefficients (leads and lags) should be statistically indistinguishable from zero and display no systematic pattern.

Second, we examine whether trends in outcomes prior to the rollout of school feeding differ by preschool exposure status. To do so, we re-estimate equations (4) and (5) saturated by preschool exposure (*i.e.*, we include each $\mathbb{1}[\text{Exp}^{\text{sfp}}_{s,t\_\text{sfp}(i)} = j]$ and its interaction with $\mathbb{1}[\text{Exp}_{m(s),t(i)} \geq 0]$). Pre-treatment coefficients should be statistically indistinguishable from zero in both groups, suggesting that trends were parallel before the arrival of school feeding, regardless of preschool exposure. Under independence, we can recover ITTs of preschool exposure, school feeding exposure, and their interaction.

For estimation, we use the following pooled TWFE model,

$$\begin{aligned} Y_{i,s} = \alpha_{m(s)} + \alpha_{t\_\text{sfp}(i)} + \alpha_{t(i)} & \\ & + \beta \mathbb{1}[\text{Exp}_{m(s),t(i)} \geq 0] \\ & + \gamma \mathbb{1}[\text{Exp}^{\text{sfp}}_{s,t\_\text{sfp}(i)} \geq 0] \\ & + \theta \mathbb{1}[\text{Exp}_{m(s),t(i)} \geq 0] \times \mathbb{1}[\text{Exp}^{\text{sfp}}_{s,t\_\text{sfp}(i)} \geq 0] + \alpha_{d(s)} \times \alpha_{t(i)} + u_{imt}, \end{aligned} \tag{7}$$

where β captures the effect of exposure to preschool in the absence of shool feeding, while γ captures the effect of exposure to school feeding without preschool. θ is our parameter of interest: it identifies the additional effect of exposure to school feeding when preceded by preschool. For inference, we cluster standard errors at the first-grade school, s, level.

Last, we recover *implied* TOTs of preschool with and without school feeding, by estimating their corresponding first-stages. In specific, we re-estimate equation (7) with $D_{i,s} \times F_{i,s}$ and $D_{i,s} \times (1 - F_{i,s})$ as dependent variables.

5 Results

5.1 Preschool

Our measure of preschool exposure determines both enrollment and proximity. Figure 3 presents event-study estimates of equation (1) for the probability of preschool enrollment in panel (a), and the distance to the nearest preschool in panel (b). We show results for the full estimation sample (diamonds) and separately by baseline proximity in 2005, splitting children above (squares) and below (triangles) the median distance of 4 km. Across both sets of outcomes, the pre-treatment coefficients are close to zero, indicating that enrollment and availability were evolving in parallel prior to the opening of a new preschool within 2km. This pre-treatment pattern provides support for the validity of the assumption of parallel trends. After preschool openings, attendance increases by 7.1 percentage points (11%) on average, alongside a reduction of 2.5 km in the distance to the nearest preschool. Children who were initially farther away (above the 2005 median distance) experience larger gains: enrollment rises by 10 p.p. (17%) and distance falls by 3.2 km on average (Appendix Table A4).

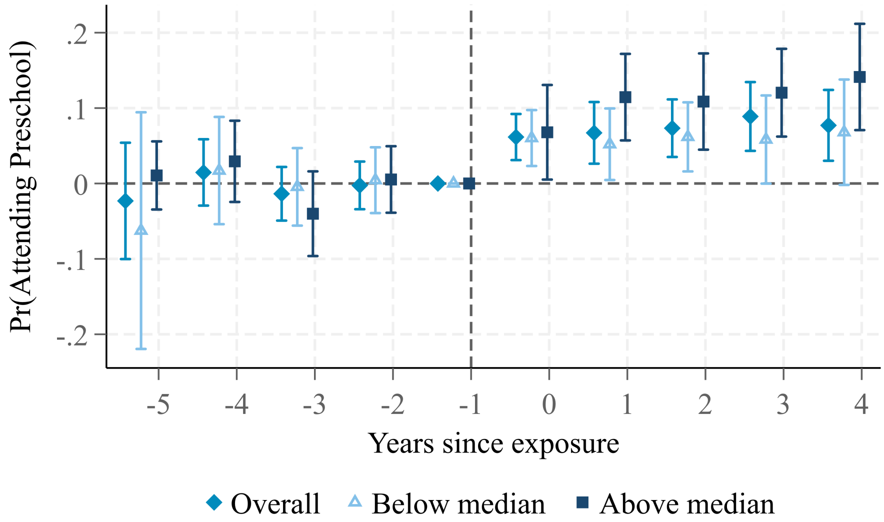
The empirical evidence supports our choice of a 2 km radius to measure exposure to preschool openings. First, the average dynamic effect of openings on attendance declines with distance: at 0.5km the effect is 10.5 percentage points, while at 3km it falls to 3.3 percentage points (Appendix Table A5 and Figure A3). Second, although the 2km estimates are somewhat smaller than those at 1.5km, they are more persistent and precise. We therefore interpret the 2km results as conservative estimates of preschool exposure. Moreover, our findings remain largely unchanged when we implement a stacked design (Cengiz et al., 2019) or apply the Sun and Abraham (2021) estimator (Appendix Table A6 and Figure A4).

We find persistent effects of preschool exposure on academic trajectories. Table 1 presents pooled estimates from equation 3 for the probability of reaching different grades, dropping out, and enrolling in higher education. Outcome-specific event-studies estimates do not show violations of parallel trends (Appendix Figure A5, first row). Preschool exposure increases the probability of reaching 5th, 9th, and 11th grade by 3, 2.1, and 2 percentage points, respectively. The probability of ever dropping out decreases by 2p.p. (4%), while the probability of enrolling in higher education increases by 6%. These results are robust to alternative estimators (Appendix Table A7). Combining these estimates with the first-stage (Appendix Table A4, Panel A, column (1)), results in *implied* treatment-on-the-treated effects (TOTs) of 25-40p.p., well above estimates found in other contexts. However, we argue that these sizable *implied* TOTs reflect not only the effect of preschool but also the fact that exposed children might have benefited from additional interventions and programs. Thus, we interpret these results as capturing the overall long-term effects of preschool exposure, combining children who were and were not subject to subsequent investments.

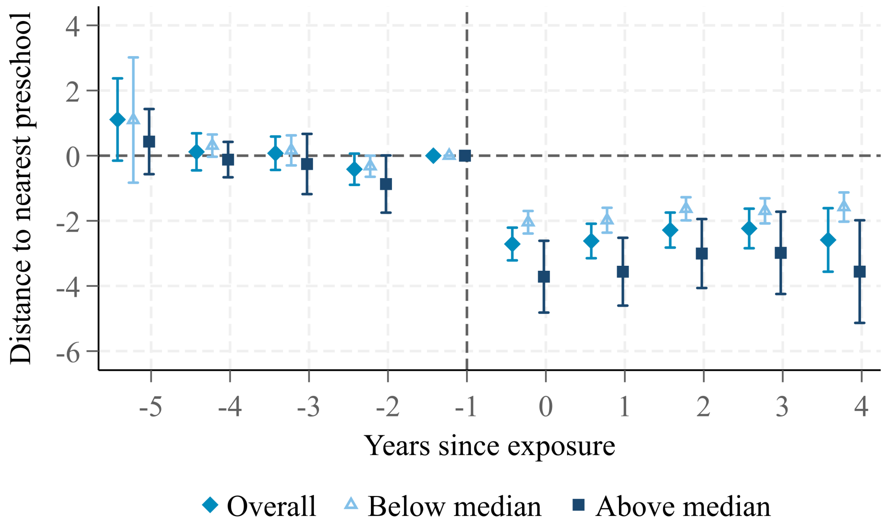
Preschool exposure improves student performance in the high school exit exam. Because preschool increases the probability of taking the test by 2 percentage points (Table 2, Panel A, column (1)), restricting our analysis to test takers, for whom we

Figure 3. Effect of Preschool Exposure on Enrollment and Distance to Nearest School

(a) Enrollment in preschool



(b) Distance to the nearest preschool



Note: Panel (a) shows event-study estimates for the probability of enrolling in preschool, using equation (1). Panel (b) shows estimates using the distance to the nearest preschool from the centroid of the student's municipality of birth as dependent variable. Results shown for the full estimating sample ("Overall") and split by below ("Below median") and above ("Above median") the median distance of 4 kilometers (in the estimating sample). Confidence intervals at the 95% level, calculated with clusteres standard errors at the municipality level.

observe scores, would bias our estimates. Hence, we perform two exercises following the literature. First, we adopt the parametric approach in Angrist, Bettinger, and Kremer (2006) and impute the lowest percentile score for non-test takers and for students falling below that threshold. Under this approach, preschool exposure increases math and reading scores by about 0.03 standard deviations (Table 2, Panel A, columns (2)-(3)), equivalent to 2-3 months of business-as-usual schooling (Evans and Yuan, 2019). The *implied* TOTs are between 0.3-0.4 standard deviations, large effects

Table 1. Effect of Preschool Exposure on Academic Trajectories

	(1)	(2)	(3)	(4)	(5)
	Probability of reaching grade			**Pr(Ever**	**Pr(Higher**
	5th	**9th**	**11th**	**Drop-out)**	**Education)**
Exposed×Post	0.029**	0.021**	0.020**	-0.019*	0.013**
	(0.013)	(0.009)	(0.008)	(0.010)	(0.007)
Number of obs.	1,704,991	1,704,991	1,704,991	1,704,991	1,704,991
Number of clusters	483	483	483	483	483
Mean	0.745	0.546	0.479	0.483	0.228

Note: Each column reports the effect of preschool exposure on the relevant outcome. Clustered standard errors at the municipality level in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

likely reflecting a combination of impacts of preschool and later investments.

Table 2. Effect of Preschool Exposure on Performance in the High School Exit Exam

	(1)	(2)	(3)
Panel A: Test Taking and Parametric Scores			
	Took Test	**Math**	**Reading**
Exposed×Post	0.020***	0.030**	0.026**
	(0.008)	(0.012)	(0.012)
Number of observations	1,704,991	1,704,991	1,704,991
Number of clusters	483	483	483
Mean	0.457	0.006	0.006
Mean (test takers)		0.789	0.806
Panel B: Probability of Math Score above quartile			
	Bottom	**Median**	**Top**
Exposed×Post	0.015***	0.007**	0.001
	(0.005)	(0.004)	(0.003)
Number of observations	1,704,991	1,704,991	1,704,991
Number of clusters	483	483	483
Mean	0.336	0.219	0.107
Panel C: Probability of Reading Score above quartile			
	Bottom	**Median**	**Top**
Exposed×Post	0.012**	0.004	0.000
	(0.005)	(0.004)	(0.002)
Number of observations	1,704,991	1,704,991	1,704,991
Number of clusters	483	483	483
Mean	0.336	0.223	0.108

Notes: Each column reports the effect of preschool exposure on the relevant outcome. Clustered standard errors at the municipality level in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

Second, test score gains could arise from improvements at different points in the distribution. Following Gray-Lobe et al. (2023), we construct binary variables taking the value of one if a student takes the test and scores above a quartile threshold, and zero otherwise (including those who did not take the test). Panel B of Table 2 shows that preschool exposure increases the probability of scoring above the 25th percentile in math by 1.5 percentage points (4.4%) and above the median by 0.7 p.p. (3.2%). In reading, preschool exposure can boost students above the bottom quartile, but has no effect at the median or higher thresholds (Table 2, Panel C). Taken together, these results suggest that test score gains are concentrated among lower-performing students and are partly driven by the extensive-margin effect of increased test taking. Event-study estimates across all performance outcomes show no evidence of pre-trend violations (Appendix Figure A5, second row) and our pooled results remain largely unchanged under alternative estimators (Appendix Table A8).

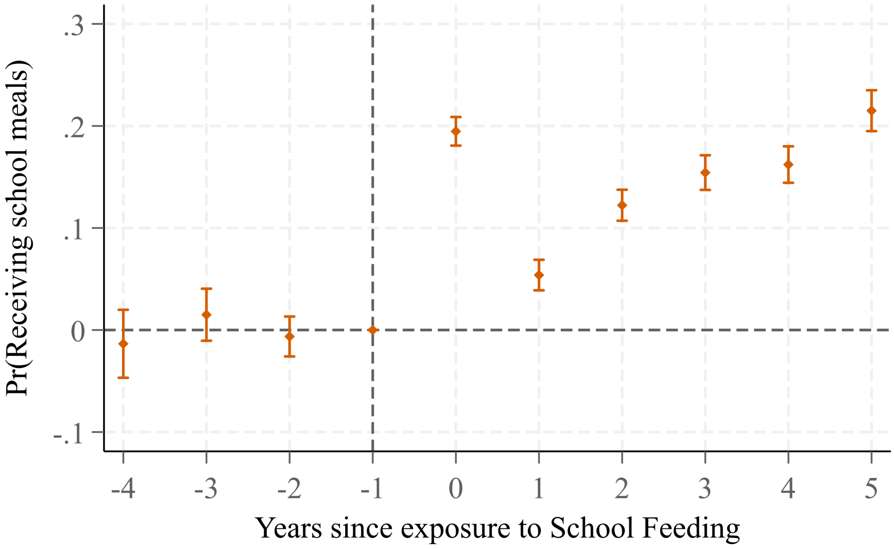
Last, we find similar effects of preschool expansions across subgroups: there are no significant differences in academic outcomes by gender or urban–rural status (Appendix Figures A7 and B7). We observe somewhat larger effects for children from higher socioeconomic backgrounds, consistent with a stronger first-stage in this group (Appendix Figure A6), rendering differences in *implied* TOTs negligible. Among urban children, the *implied* TOTs are larger compared to rural children as a result of a weaker first-stage among the former (Appendix Figure A6).

5.2 School Feeding Program

In this section, we examine the aggregate effects of school feeding on academic outcomes irrespective of whether students attended preschool. Although our sample, identifying variation, and estimation method differ, the results we present below are largely similar to the evaluation of the school feeding program by Collante-Zárate et al. (2024).

First, our measure of school feeding exposure determines the receipt of school meals. Figure 4 shows an immediate increase in the probability of receiving school meals after treatment, followed by a drop in the next period and a sustained increased afterwards. The pre-treatment coefficients are centered around zero, providing support of no differential trends in receiving meals across schools prior to treatment. The post-treatment pattern results from our use of an "indirect" measure of exposure to school meals. By 2012 children from the 2005-2006 cohorts were less likely to be in primary or lower grades, making them less likely to receive meals rightaway. In contrast, students in the 2009-2010 cohorts, who were in earlier grades in 2012 when the school feeding program began its expansion, were more likely to receive meals right after the program's arrival given the priorization rules (see Section 2.2). Nonetheless, our measure of exposure to school feeding increases the probability of receiving meals by 14 percentage points, corresponding to a large increase of 45% with respect to the mean (Table 3, Panel A, column (1)).[19,20]

Figure 4. Effect of School Feeding Exposure on the Probability of Receiving School Meals



Note: Event-study estimates for the probability of receiving school meals, using equation (4). Confidence intervals at the 95% level, calculated with clustered standard errors at the school level.

Consistent with its overarching objective of keeping children in school, exposure to the feeding program has sizable effects on academic progression, in reducing dropout, and in increasing higher education enrollment. The probability of reaching grade 5th increases by 8 p.p. (13%), of reaching grade 9th by 9.3 p.p. (24%), and grade 11th by 8.1 p.p. (24%) (see Table 3, Panel A, columns (2)-(4)). We estimate that the probability of ever dropping-out reduces by 9 p.p. (13%) (see Table 3, Panel B, columns (1)). Last, the probability of enrolling in higher education increases in 4.5p.p. (33%) (see Table 3, Panel B, column (2)).[21]

[19] Appendix Figure B2 splits the dynamics of school meal receipt by the grade in which students were first exposed to the program. We observe that the pattern of Figure 4 is driven by an immediate increase in the probability of receiving meals in grades 4-5, which decreases in the following periods, while the probability of receiving meals in secondary rises gradually overtime.

[20] Appendix Figure B5 shows that the first-stage of school feeding is larger for low-income children from rural areas, consistent with the program's prioritization rules.

[21] Our estimates of the effects of school feeding on academic progression and dropout are broadly consistent with Collante-Zárate et al. (2024), who report effects of 20–29% on grade 11 completion and 11–30% on dropout. We estimate a larger effect on higher education enrollment (35% vs. 10–15%), likely due to a lower baseline enrollment rate in our control group (8.4% vs. 30–35% in Collante-Zárate et al. (2024)), resulting from the authors restricting their analysis to students in 6th grade in 2012-2013.

Table 3. Effect of School Feeding Exposure on Receiving School Meals and Academic Outcomes

	(1)	(2)	(3)	(4)
Panel A: First-stage and Academic Progression				
	Pr(Received School Meals)	**Probability of reaching grade**		
		5th	**9th**	**11th**
Exposed to Feeding×Post	0.136***	0.079***	0.093***	0.081***
	(0.005)	(0.005)	(0.005)	(0.005)
Number of observations	1,704,854	1,704,854	1,704,854	1,704,854
Number of clusters	32,270	32,270	32,270	32,270
Mean	0.296	0.614	0.381	0.328
Panel B: Drop-out, Enrollment in Higher Education, and Parametric Scores				
	Pr(Ever Drop-out)	**Pr(Higher Education)**	**Math**	**Reading**
Exposed to Feeding×Post	-0.087***	0.045***	0.138***	0.131***
	(0.005)	(0.003)	(0.011)	(0.011)
Number of observations	1,704,854	1,704,854	1,704,854	1,704,854
Number of clusters	32,270	32,270	32,270	32,270
Mean	0.649	0.136	-0.295	-0.289

Notes: Each column reports the effect of school feeding exposure on the relevant outcome. Clustered standard errors at the school level in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

In addition, and well beyond its permanence goal, school feeding improves academic performance. Nutrition and food security have been associated with improved learning outcomes (Maluccio et al., 2009; Anderson, Gallagher, and Ramirez Ritchie, 2018; Belot and James, 2011). Panel B, columns (3) and (4), of Table 3 show that exposure to school feeding increases the average parametric scores in math and reading in 0.13 standard deviations, similar to previous findings. Unlike preschool, which solely improves scores for low-achieving students, the feeding program also boosts the likelihood of achieving test scores in the top of the distribution (Appendix Table B3). The probability of scoring above the 75th percentile in math and reading increases by about 2 percentage points. As in the case of preschool, the *implied* TOTs of academic outcomes are considerably large but might reflect complementarities between early and late investments. We will explore this hypothesis in the next section.[22,23]

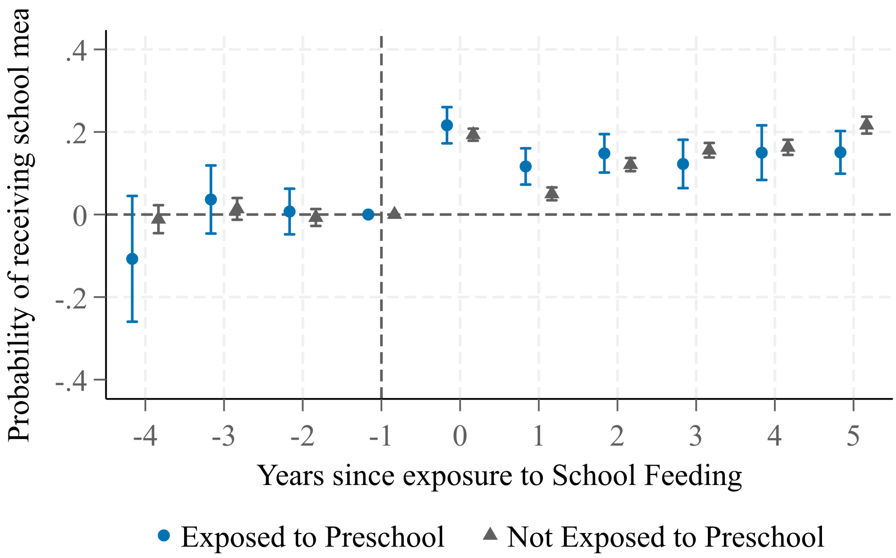
[22]Outcome-specific event-studies for the school feeding program show no evidence of pre-trends (Appendix Figure B3). Our results for the school feeding program are largely unchanged when implementing the Sun and Abraham (2021) estimator using never-treated and the last-treated cohort as control. See Figure B4 Tables B4-B6 in the Appendix.

[23]We do not observe heterogeneous effects of school feeding by gender. Consistent with the first-stage results, ITT estimates are higher among low-income, rural children for outcomes such as reaching grades 5, 9, and 11, and reducing the probability of dropping out. Lastly, while the effects on the math and reading test scores are also stronger for these groups, we do not find significant differences in the probability of scoring above the median or top of the distribution.

5.3 Interaction between Preschool and School Feeding

We have shown that preschool and school feeding each have meaningful effects on academic outcomes. In this section, we examine whether the returns to school feeding are higher for children exposed to preschool versus those who were not exposed. That is, we test whether preschool and school feeding are complementary investments (Cunha and Heckman, 2007).

Figure 5. Effect of School Feeding Exposure on the Probability of Receiving School Meals, by Exposure to Preschool



Note: Event-study estimates for the probability of receiving school meals, using a saturated equation (4) where each lead and lag is interacted by a binary indicator of exposure to preschool. Confidence intervals at the 95% level, calculated with clustered standard errors at the school level.

First, we test whether the rollout of one intervention predicts the timing of the other, as outlined in Section 4.3. Appendix Figure C1 shows no systematic patterns in leads or lags, and most coefficients are statistically indistinguishable from zero. Second, Figure 5 displays event-study estimates of exposure to *school feeding*, by exposure to *preschool*. That is, we estimate a saturated version of equation (4), as detailed in Section 4.3. The pre-treatment coefficients are not significant and are centered around zero for both groups, implying that trends in receiving meals were parallel prior to the program's arrival regardless of whether students were exposed to preschool.[24] Taken together, these results support the validity of our research design to identify dynamic complementarities between investments. Post-treatment, school feeding exposure increases the probability of receiving meals in a similar pattern

[24] Appendix Figures C2 and C3 show event-study estimates of exposure to school feeding by exposure to preschool for academic outcomes. The pre-treatment coefficients are centered around zero and largely not significant across all academic outcomes.

and magnitude regardless of preschool exposure, although the effects are more noisy among those exposed due to a smaller sample size.

On average, we find evidence of dynamic complementarities between preschool and school feeding during compulsory schooling, with effects that extend into pursuing post-secondary degrees. Table 4 reports estimates from equation (7), showing interaction effects suggesting that preschool exposure amplifies the effects of school feeding. The effect of school feeding on completing grades 9 and 11 is 2–2.5 percentage points larger for students previously exposed to preschool, relative to those who were not. We observe interaction effects of similar magnitudes on the probability of enrolling in higher education. The latter is consistent with dynamic complementarity, such that earlier investments increase the returns to later ones. We also observe that, without the boost from school feeding which targets academic permanence, preschool exposure has no effect on dropout or academic progression.[25]

In contrast, we estimate a positive standalone effect of preschool on grade 5 completion, but no interaction effect at this margin. This likely reflects the timing of the feeding program's rollout: most cohorts in our sample had already exited primary school before school feeding expanded.

We recover *implied* TOTs by dividing the estimates in rows 1 and 4 of Table 4 by the corresponding first-stage effects in Appendix Table C1, row 1, column (3), and row 4, column (5), respectively. Among compliers (*i.e.*, students who enrolled in preschool due to improved access and later received school meals) the probability of reaching 9th and 11th grade increased by 28 percentage points, with similarly large effects on higher education enrollment.

[25]Results for the probability of taking the high school exit exam are qualitatively the same to those of the probability of reaching grade 11th. In Colombia, the high school exit exam is a requirement to graduate high school resulting in marginal differences between these two outcomes. For instance, the baseline share of students taking the test is only 3p.p. lower than the share of students reaching 11th grade.

Table 4. Effect of Preschool Exposure on Academic Trajectories

	(1)	(2)	(3)	(4)	(5)
	Probability of reaching grade			**Pr(Ever**	**Pr(Higher**
	5th	**9th**	**11th**	**Drop-out)**	**Education)**
Preschool	0.027*	-0.005	-0.001	0.009	-0.008
	(0.014)	(0.013)	(0.012)	(0.008)	(0.006)
Feeding	0.079***	0.091***	0.080***	-0.086***	0.044***
	(0.005)	(0.006)	(0.006)	(0.006)	(0.005)
Preschool×School Feeding	0.002	0.025**	0.020*	-0.027**	0.020**
	(0.014)	(0.013)	(0.011)	(0.012)	(0.008)
Total Preschool	0.029***	0.020***	0.019***	-0.018***	0.012***
	(0.005)	(0.005)	(0.004)	(0.004)	(0.003)
Implied TOT	0.574	-0.106	-0.002	0.191	-0.166
(Preschool without Feeding)	[0.044]	[0.686]	[0.908]	[0.458]	[0.118]
Implied TOT	0.403	0.278	0.264	-0.25	0.166
(Preschool and Feeding)	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Number of observations	1,704,854	1,704,854	1,704,854	1,704,854	1,704,854
Number of clusters	32,270	32,270	32,270	32,270	32,270
Mean	0.6108	0.3805	0.3270	0.6494	0.0861

Note: Each column reports estimates from equation (7) on the relevant outcome. Clustered standard errors at the school level in parentheses. * $p<0.10$, ** $p<0.05$, *** $p<0.01$. p-values for *implied* TOTs in brackets, computed using bootstrap.

We do not find robust evidence of dynamic complementarity between preschool and school feeding on test performance. Interaction effects are largely absent for standardized scores in math and reading, with only modest gains observed in the upper tail of the reading distribution (Appendix Table C2). Although test scores improve significantly among children exposed to both programs, these effects are not statistically different from those among children exposed to preschool alone. In addition, school feeding on its own generates substantial gains, while preschool alone shows little effect (*implied* TOTs of preschool alone are not statistically significant).[26] When combined with school feeding, we estimate *implied* TOT effects of preschool of approximately 14–18 percentage points in increasing the probability that students score above the bottom quartile in math and reading; standardized test scores increase by roughly 0.3 standard deviations. These patterns suggest that school meals are the primary driver of gains in academic performance, while preschool contributes little additional value on its own. However, given the size of the point estimates, we may be under-powered to detect moderate interaction effects.

5.4 Mechanisms and Discussion

5.4.1 Timing between investments

While the results of the previous section suggest complementarities in academic permanence and progression rather than in cognition, the timing of the second investment may also shape the extent of these interactions. Two considerations motivate our focus on the timing of exposure to school feeding. First, the theory of dynamic complementarity posits the logic of sensitive periods, when investments are more productive (Cunha et al., 2010), and implies that the effectiveness of later investments depends on the stock of early acquired skills and may lose effectiveness when subsequent inputs arrive too late in the life cycle (Heckman and Mosso, 2014). Second, empirical studies of school feeding programs find that earlier (Hoynes et al., 2016; Bütikofer et al., 2018) or longer (Chakraborty and Jayaraman, 2019; Lundborg et al., 2022) exposure results in larger gains. Hence, in what follows we explore whether school feeding is more complementary to preschool when implemented earlier.

A natural candidate for measuring the timing of school feeding exposure is the grade in which students first received meals. In fact, half of the students in our sample received school meals for the first time during primary school, with the average grade of first exposure equal to sixth grade. However, using grade of exposure as

[26] These results are consistent with the finding that early childhood interventions may not lead to cognitive gains in the long-term, commonly known as test score fade-out (see Cascio (2021) for a review). In our case, we find that preschool exposure can have long-term impacts in academic performance only in the presence of additional investments.

the timing variable would introduce bias, since grade attainment is itself affected by preschool. Instead, we use the difference between the year of exposure to school feeding and the year of exposure to preschool. This measure captures the timing of the feeding program arrival relative to preschool arrival, and is not determined by endogenous school progression. Among students exposed to preschool and school feeding, only 22% were exposed to the latter two to five years after preschool, while 42% were exposed six to seven years later (see Figure C4).[27]

We estimate the following equation:

$$
\begin{aligned}
Y_{i,s} = \alpha_{m(s)} + \alpha_{t(i)} + \beta \mathbb{1}[\text{Exp}_{m(s),t(i)} \geq 0] + \gamma \mathbb{1}[\text{Exp}^{\text{sfp}}_{s,t\_\text{sfp}} \geq 0] \\
+ \sum_{g=1}^{4} \theta_g \mathbb{1}[\text{Window}_s = g] + \alpha_{d(s)} \times \alpha_{t(i)} + +\epsilon_{imt},
\end{aligned}
\tag{8}
$$

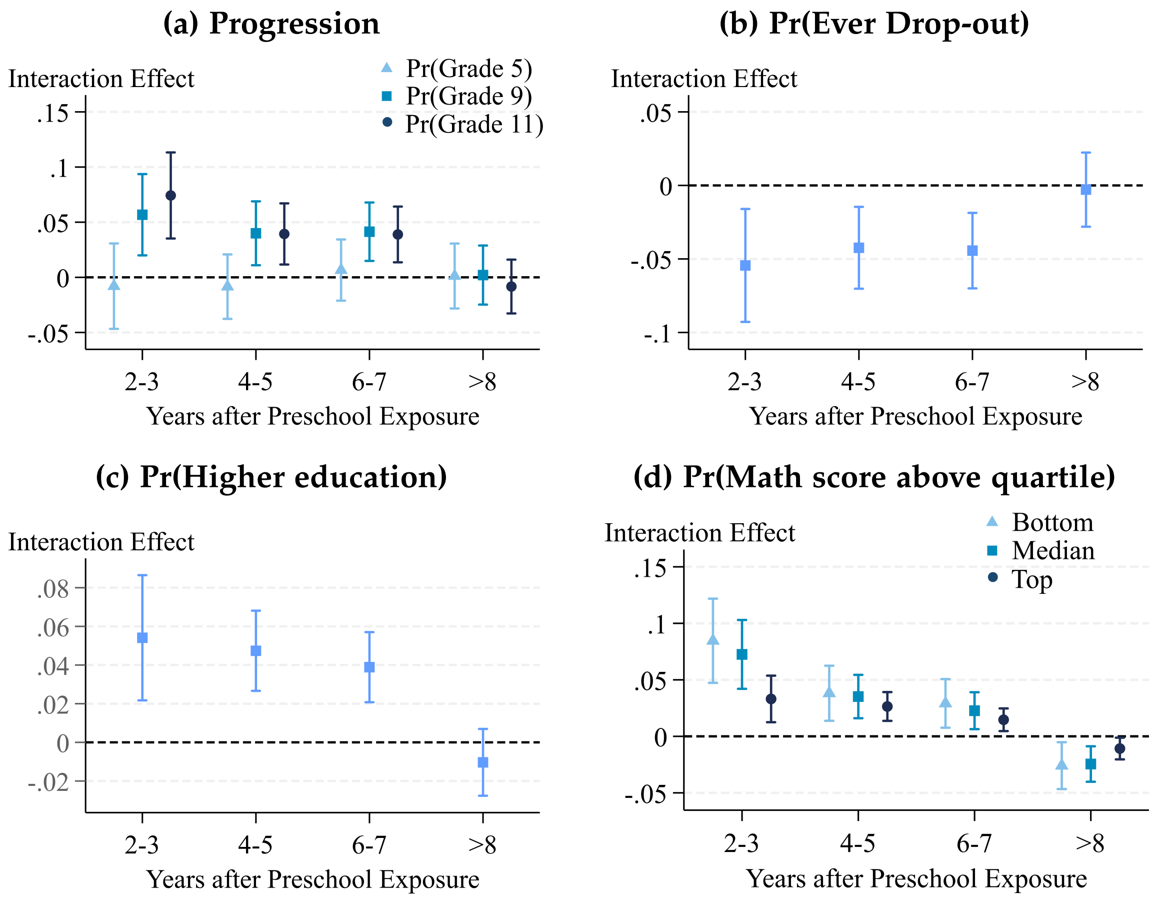
where $\text{Window}_s = \text{SFP}_s - \text{Open}_{m(s)}$ if $\mathbb{1}[\text{Exp}^{\text{sfp}}_{s,t\_\text{sfp}} \geq 0] = 1$, and 0 otherwise. It denotes the difference in years between school feeding and preschool exposure, with 1 denoting 2-3 years, 2 for 4-5 years, 3 for 6-7 years, and 4 more than 8 years. If $\theta_1 \geq \theta_2 \geq \theta_3 \geq \theta_4 > 0$, then the return to preschool is higher in the presence of other early investments. To assess the validity of our identification strategy, we estimate the same specification using the probability of reaching grade 5 as the outcome. This serves as a placebo test: since most students in our cohorts would have completed primary school before the introduction of school feeding (post-2012), and few had exposure windows as short as two to three years, we should not expect to find any differential effects by timing for this outcome.

We find that the effect of preschool on grade completion and dropout reduction is greater when school feeding is introduced earlier (see Figure 6). Panel (a) shows that the interaction effect between preschool and school feeding exposure on the probability of reaching grades 9 and 11 declines as the time between the two investments increases. For students with two to three years between preschool and school feeding exposure, the interaction effect on reaching grades 9 and 11 exceeds 5 percentage points, roughly twice the average interaction effect reported in Table 4, and largely disappears when the gap between investments exceeds eight years. Moreover, consistent with the placebo test outlined above, we find no differential effect by timing on reaching grade 5. Panel (b) shows a similar gradient for the probability of ever dropping out: it falls by up to 5 percentage points when the gap between investments is

[27] Similar to Lundborg et al. (2022), earlier arrival also means longer exposure in our context. Appendix Figure C5, Panel (a), shows a negative slope between total years with meals and timing of arrival. That is, earlier arrival (on the x-axis) is associated with longer receipt of school meals (on the y-axis). Hence, we cannot disentangle the effects of timing from the effects of length exposure in our context.

two to three years, but there is no interaction effect when the gap exceeds eight years.

Figure 6. Interaction Effect of Preschool and School Feeding, by time elapsed between preschool and school feeding exposure



Note: The figures plot estimates from equation (8) of the interaction effects between preschool and school feeding exposure, by relative arrival between the two investments. Confidence intervals at the 95% level, calculated with clustered standard errors at the school level.

We also find that preschool and school feeding can be complementary for higher education enrollment when the latter investment reaches students at the right time. Panel (c) of Figure 6 shows a positive interaction effect of 2-6 percentage points between preschool and school feeding when the gap between investments is two to seven years, with no detectable interaction effect for longer gaps. To unpack these differential effects, Panel (b) Appendix Figure C5 shows the average grade at which student's received school meals for the first time versus the exposure window between investments. Shorter (longer) gaps between preschool and feeding are generally associated with earlier (later) grades of feeding receipt. Notably, students below the 7 year window typically receive meals during primary school. This pattern suggests that complementarities with preschool for higher education enrollment are strongest when subsequent investments align with schooling transitions, rather than arriving too late (more than eight years after preschool).[28]

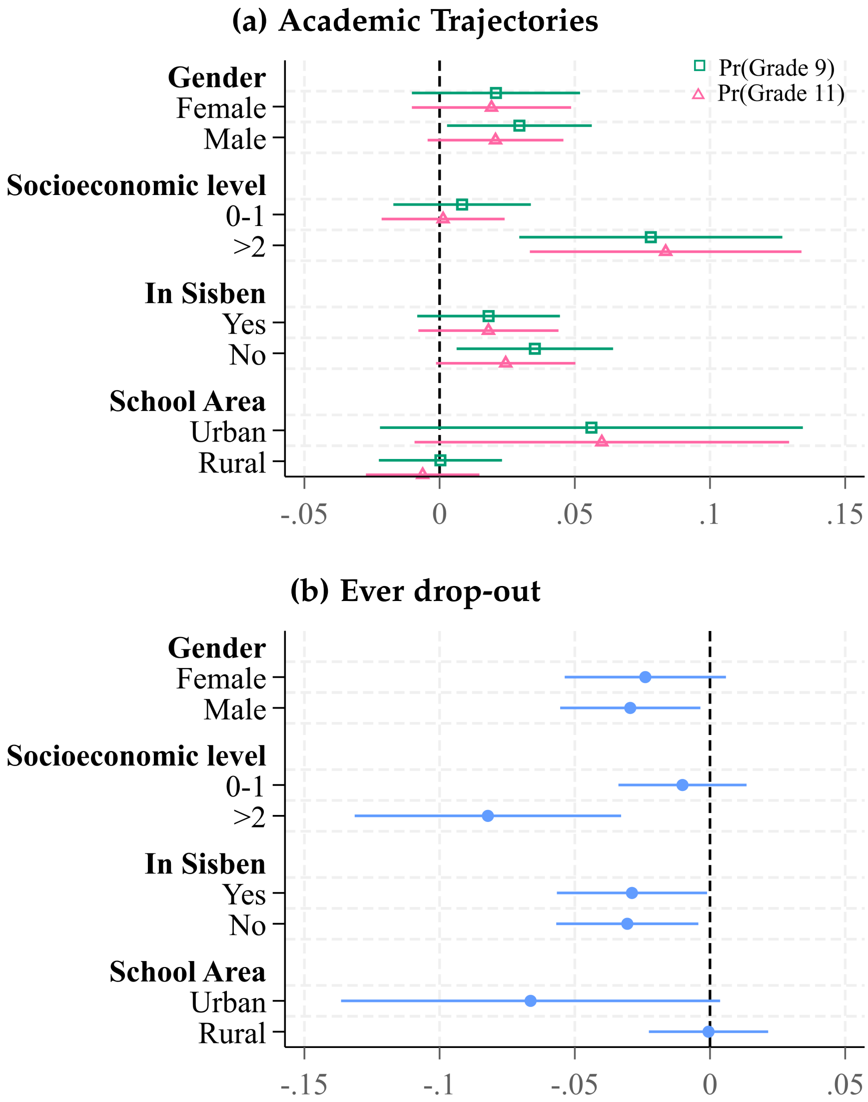
Finally, we estimate sizable interaction effects between preschool and school feeding on academic performance when the latter is introduced two to three years after preschool exposure (see Panel (d) Figure 6, and Appendix Figure C6). Within that window, the interaction effect on the probability of scoring above the bottom quartile in Math is approximately 8 percentage points, falling by half when the gap between investments is four to seven years, and turning negative when the gap exceeds eight years. We observe a similar pattern for the interaction effect on the probability of scoring above the median. Moreover, although smaller in magnitude (about 2.5 percentage points), the interaction effect on scoring above the top quartile is also positive when school feeding follows preschool by two to five years, suggesting that early exposure can also improve performance at the upper end of the distribution. We find qualitatively similar patterns for the probability of scoring above each quartile in Reading, as well as for standardized Math and Reading test scores (see Appendix Figure C6).

5.4.2 Complementarities by student characteristics

Although we do not find significant heterogeneity in the intent-to-treat effects of each investment, it is plausible that their complementarity varies across subgroups. Most existing studies on dynamic complementarities find limited evidence of interaction effects, particularly among low-income populations (Rossin-Slater and Wüst, 2020), or show that families may compensate by either reinforcing or offsetting investments (Bharadwaj et al., 2018; Goff et al., 2023). In contrast, some evidence suggests that cash transfers to low-income families can remediate the negative effects of early-life shocks (Adhvaryu et al., 2024; Duque et al., 2023). Motivated by these findings, we estimate equation (7) separately by gender, socioeconomic stratum, Sisbén status, and whether the student attended a rural or urban school in first grade.

[28]Some students in our cohorts may have been exposed to financial aid programs. *Ser Pilo Paga*, the country's largest need-based and merit-based financial aid initiative (Londoño-Vélez, Rodríguez, and Sánchez, 2020) operated from 2014 to 2018, overlapping with the timing of higher education decisions for part of our sample. In 2018, it was replaced by *Generación E*, which maintained similar eligibility criteria and expanded coverage for low-income students (Londoño-Vélez, Rodríguez, Sánchez, and Álvarez Arango, 2025). To assess whether our results for higher education enrollment are confounded by the expansion of financial aid, we estimate equation (8) using the probability of being eligible for aid as the outcome. Exposure to school feeding between two and seven years after preschool increases eligibility by at most 0.2 percentage points, suggesting that financial aid programs are unlikely to drive the higher education patterns we observe (see Appendix Figure C6, Panel (d)).

Figure 7. Interaction Effect of Preschool and School Feeding on Academic Trajectories, by student attributes



Note: The figures plot estimates of interaction effects between preschool and school feeding exposure, by selected student characteristics. Confidence intervals at the 95% level, calculated with clustered standard errors at the school level.

Consistent with previous studies, we find no evidence of complementarities among low-income students on any outcome (see Figure 7 and Appendix Figure C7). In addition, we do not observe significant gender differences in the interaction effect between preschool and school feeding. The interaction effects are somewhat larger for students in urban schools, but these estimates are consistently imprecise. In contrast, complementarities are significantly stronger among children from relatively higher socioeconomic strata. For instance, Panel (a) of Figure 7 shows that the interaction effect on the probability of reaching grades 9 and 11 is nearly 10 percentage points among students in strata 2–4, and statistically different from the null effect observed among students in the lowest stratum. Moreover, the absence of differences by Sisbén status indicates that complementarities vary along more granular socioeconomic levels than broad poverty classifications reveal. We observe a similar pattern across all outcomes, suggesting that complementarities are concentrated among relatively better-off students.

To unpack the sizable interaction effects among students from higher socioeconomic strata, we use a subsample matched to detailed household records from Sisbén. These records include information on household composition, members' labor market participation and education, housing conditions, and other socioeconomic indicators. About 47% (55%) of students in strata 0-1 (2-4) can be linked to Sisbén (see Appendix Table C3). Students in Sisbén are more likely to enroll in preschool and receive school meals than those not in Sisbén, but are otherwise similar in age and gender composition. We focus the analysis on this matched subsample. As expected, students from higher socioeconomic levels tend to live in smaller households, have more educated mothers who are more likely to work, and are less likely to report domestic work as their main economic activity (Appendix Table C4).[29] These patterns are suggestive of dynamic complementarities that may depend on reinforcing environments at home, although we cannot directly observe parental investments to credibly test this argument.

Complementarities among relatively better-off students may also stem from the relative quality of public preschool compared to other available options. For these students, public provision may represent a lower-quality substitute for higher-quality private preschools. In the absence of the expansion of public preschools, higher-income children may have attended private preschools of arguably better quality, while their lower-income peers may not have enrolled in any early education at all. These differences in fallback options are likely important drivers of differential program effects (Kline and Walters, 2016). We observe that among students in strata 2–4, preschool exposure alone has negative albeit imprecise effects on all academic outcomes, while among lower-income students, effects are negligible but not negative (Appendix Figure C9). These patterns are unlikely to be driven by differential take-up of preschool or school meals (Appendix Figure C10). Notably, school feeding has sizable positive effects across the board, with the largest gains among lower-income students. Taken together, these findings suggest that complementarities among better-off students may arise because public preschool is a lower-quality alternative to their outside options, offering limited or even negative returns unless followed by subsequent investments. In contrast, for lower-income students, school

[29]For a smaller subsample, we also observe paternal education and employment status. Appendix Table C5 shows negligible differences in the share of fathers who work, although fathers of students in strata 2–4 are more educated.

feeding alone yields substantial gains, regardless of prior preschool exposure.

6 Conclusion

We study dynamic complementarities in human capital formation between two large-scale public investments: preschool and school meals. Our empirical strategy exploits the staggered expansion of public preschools in Colombia (2005–2015) and the subsequent scale-up of the national school feeding program starting in 2012. The two expansions resulted in some students exposed to both, one, or neither intervention, with the timing of school feeding determined by program targeting rules. Under the assumption that exposure to each intervention and their overlap are plausibly exogenous, we test: (i) whether investments are more productive when preceded by early ones, and (ii) whether the timing of follow-up investments matters. We further examine whether preschool alone (*i.e.*, an early investment without follow-ups) and school meals alone (*i.e.*, later remediation for early disadvantage) can yield sustained impacts.

Using 12-18 years of administrative records, we follow cohorts of primary school entrants from 2006-2011 through their academic trajectories. Consistent with dynamic complementarity, we find that students exposed to both interventions (in particular when school feeding was introduced shortly after preschool), experience lower dropout, higher secondary completion and post-secondary enrollment, and improved test scores. In contrast, preschool alone has limited effects without follow-up investments, while the school feeding program alone produces substantial gains (though smaller than in the presence of preschool).

Complementarities across all educational outcomes are substantially stronger for students from relatively higher socioeconomic levels. If anything, complementarities seem to be negligible among low-income students. This pattern aligns with prior evidence from Rossin-Slater and Wüst (2020) and Bjorvatn et al. (2025), who document limited or even negative interaction between preschool and other investments for vulnerable children. Additional analyses using a subsample of students with detailed household records suggest that family environments, with more and better resources, are a potential channel behind these heterogeneous results. Although we lack direct measures of parental investments, our findings are consistent with the hypothesis that compensatory (or offsetting) behaviors at home among low-income households may undo school-based complementarities (Bau et al., 2020; Goff et al., 2023). In contrast, we speculate that among relatively better-off families, reinforcement at home (or even the absence of interference) may allow these complementarities to emerge.

Our findings also show that preschool alone has limited long-term effects. A *naive* analysis of the preschool expansion suggests sizable impacts across all outcomes, with large *implied* TOT estimates. However, once we disentangle direct effects from interaction effects, most of the observed gains are concentrated among students who were also exposed to subsequent investments.

From a policy perspective, this highlights the importance of follow-up interventions to sustain and amplify early gains. From an empirical perspective, our results serve as a cautionary tale: evaluating early interventions may overstate their effectiveness if later *complementary* investments are not taken into account. This concern is not unique to our setting, as children around the world are often exposed to multiple and overlapping investments. But it is also a limitation to our own analysis: we acknowledge that many children in our sample likely benefited from additional programs, as our study period coincides with a broader expansion of early childhood and educational policies in the country. Whether other educational investments, such as the expansion of financial aid programs in the 2010s, also act as complements in the production of skills remains an important and open question for future research.

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APPENDIX

A Preschool

A.1 Additional Tables

Table A1. Characteristics of Preschools, by Opening Period (Existing versus New, 2006–2015)

		Preschools	
	All	Existing	New
Rural schools (%)	0.75	0.70	0.82
Full-day schools (%)	0.26	0.24	0.29
Size and grades offered (2005 or at opening)			
Average preschool size	18.09	23.94	9.49
Schools with grades -1 and -2 (%)	0.04	0.05	0.02
Schools with primary grades (%)	0.81	0.77	0.88
Schools with primary and secondary grades (%)	0.17	0.21	0.11
Size and grades offered (2005-2015)			
Average preschool size	16.60	21.70	9.10
Schools with grades -1 and -2 (%)	0.02	0.03	0.01
Schools with primary grades (%)	0.80	0.75	0.88
Schools with primary and secondary grades (%)	0.18	0.23	0.11
Number of preschools	43,407	25,832	17,575

Notes: Authors' calculations using data from SIMAT. "All" includes preschools operating at any point between 2005 and 2015. "Existing" refers to preschools that opened before or during 2005 (the baseline year). "New" includes preschools that opened between 2006 and 2015 (the period of observed preschool expansion). Grades -1 and -2 correspond to "Jardín" and "Pre-jardín", the two years preceding transition to first grade.

Table A2. Descriptive Statistics by Sample and Preschool Enrollment

	All Students	Sample	Preschool	No Preschool
Panel A: Socioeconomic Characteristics				
Age at 1st grade	6.77	6.77	6.74	6.83
Female	0.49	0.48	0.49	0.48
0, 1	0.65	0.71	0.67	0.78
2, 3 y 4	0.35	0.29	0.33	0.22
Urban	0.64	0.61	0.67	0.51
Panel B: Academic Trajectories and Performance				
School dropout	0.47	0.48	0.41	0.61
Primary Completion (5th grade)	0.76	0.75	0.82	0.62
9th grade	0.56	0.55	0.62	0.42
Secondary Completion (11th grade)	0.49	0.48	0.55	0.36
Enrolled in Higher Education	0.14	0.13	0.15	0.10
Standardized values of Math	0	-0.04	-0.02	-0.11
Standardized values of Reading	0	-0.05	-0.03	-0.10
Received School Meals	0.49	0.48	0.54	0.38
Number of observations	3,298,805	1,704,991	1,092,755	612,236
Number of municipalities	952	483	483	483

Notes: "All Students" refers to the unrestricted sample of first-grade entrants from 2006-2011. Column "Sample" restricts to students who were above 2 kilometers from the nearest school in 2005. The last two columns restrict to students who enrolled and did not enroll in preschool.

Table A3. Predictors of Exposure to Preschool

Variable	(1) Ever	(2) 2006	(3) 2007	(4) 2008-2010	(5) 2011-2015
Total Population (in millions)	0.250	0.018	-0.018	0.088	0.162
	(0.212)	(0.117)	(0.067)	(0.090)	(0.103)
Area (per 10,000 squared km)	-0.075**	-0.014	-0.020	-0.028	-0.013
	(0.033)	(0.011)	(0.014)	(0.018)	(0.009)
Distance to Capital (per 100 km)	0.022	0.000	-0.011	0.007	0.025**
	(0.020)	(0.010)	(0.007)	(0.013)	(0.012)
Total GDP (millions COP)	-0.005	0.006	0.005	-0.017	0.001
	(0.023)	(0.016)	(0.009)	(0.010)	(0.007)
Ratio of Urban/Rural Population	0.005	0.002	0.000	0.002	0.001
	(0.003)	(0.002)	(0.001)	(0.001)	(0.001)
Total Public Schools	0.000	0.000	0.000	-0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Preschool Students (per 10,000)	-0.242	-0.009	-0.037	-0.076	-0.120
	(0.204)	(0.093)	(0.096)	(0.106)	(0.082)
Primary Students (per 10,000)	-0.044	-0.029	-0.020	0.004	0.001
	(0.041)	(0.023)	(0.015)	(0.018)	(0.018)
Homicide Rate (per 1,000)	0.000	0.000	-0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
ICBF Beneficiaries (per 1,000)	0.019	0.008	0.011*	0.013	-0.014
	(0.017)	(0.011)	(0.006)	(0.008)	(0.008)
Number of municipalities	947	947	947	947	947
Mean	0.107	0.037	0.024	0.030	0.016

Notes: Regression estimates for the probability of preschool exposure. "Ever", in column (1), is a binary variable equal to one if a new preschool open in a radius of 2 km from the municipality centroid between 2006-2015, zero otherwise. Columns (2)-(5) present estimates for the probability of openings in the corresponding years. All regressions include department fixed-effects. Robust standard errors in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

Table A4. Effect of Preschool Exposure on Preschool Attendance and Distance to Nearest School - Pooled Estimates

	(1)	(2)	(3)
Panel A: Probability of Attending Preschool			
	Overall	**Below Median**	**Above Median**
Exposed× Post	0.071***	0.058***	0.098***
	(0.015)	(0.019)	(0.027)
Number of observations	1,704,991	1,039,092	665,899
Number of clusters	483	236	247
Mean	0.639	0.681	0.573
Panel B: Distance to nearest preschool in km			
	Overall	**Below Median**	**Above Median**
Exposed× Post	-2.450***	-1.839***	-3.122***
	(0.243)	(0.145)	(0.489)
Number of observations	1,704,991	1,039,092	665,899
Number of clusters	483	236	247
Mean	5.234	3.136	8.522

Notes: Each column reports the effect of preschool exposure on the relevant outcome. Clustered standard errors at the municipality level in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

Table A5. Effect of Preschool Exposure at different radii on Preschool Attendance - Aggregate Estimates

Variable	0.5km	1km	1.5km	2km	2.5km	3km
Exposed×Post	0.105**	0.054*	0.089***	0.074***	0.060***	0.033*
	(0.043)	(0.030)	(0.022)	(0.018)	(0.020)	(0.020)
Number of observations	1,704,991	1,704,991	1,704,991	1,704,991	1,704,991	1,704,991

Notes: Each column reports the effect of preschool exposure on the relevant outcome. Clustered standard errors at the municipality level in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

Table A6. Effect of Preschool Exposure on Preschool Attendance - Alternative Estimators

	TWFE	Sun and Abraham	Stacked
Exposed×Post	0.071***	0.065***	0.073***
	(0.015)	(0.017)	(0.015)
Number of observations	1,704,991	1,704,991	15,693,124

Notes: Each column reports the effect of preschool exposure on the relevant outcome. Clustered standard errors at the municipality level in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

Table A7. Effect of Preschool Exposure on Academic Progression - Alternative Estimators

	Probability of reaching grade			**Pr(Ever Drop-out)**	**Pr(Higher Education)**	**Pr(Took Test)**
	5th	**9th**	**11th**			
			TWFE			
Exposed×Post	0.029**	0.021**	0.020***	-0.019*	0.013**	0.020***
	(0.013)	(0.009)	(0.008)	(0.010)	(0.005)	(0.008)
Number of observations	1,704,991	1,704,991	1,704,991	1,704,991	1,704,991	1,704,991
			Sun and Abraham (2021)			
Exposed×Post	0.025**	0.017**	0.017**	-0.016**	0.011**	0.017**
	(0.012)	(0.008)	(0.007)	(0.008)	(0.005)	(0.007)
Number of observations	1,704,991	1,704,991	1,704,991	1,704,991	1,704,991	1,704,991
			Stacked			
Exposed×Post	0.032**	0.024**	0.022***	-0.022**	0.014***	0.021***
	(0.015)	(0.010)	(0.008)	(0.005)	(0.005)	(0.008)
Number of observations	15,693,124	15,693,124	15,693,124	15,693,124	15,693,124	15,693,124

Notes: Each column reports the effect of preschool exposure on the relevant outcome. Clustered standard errors at the municipality level in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

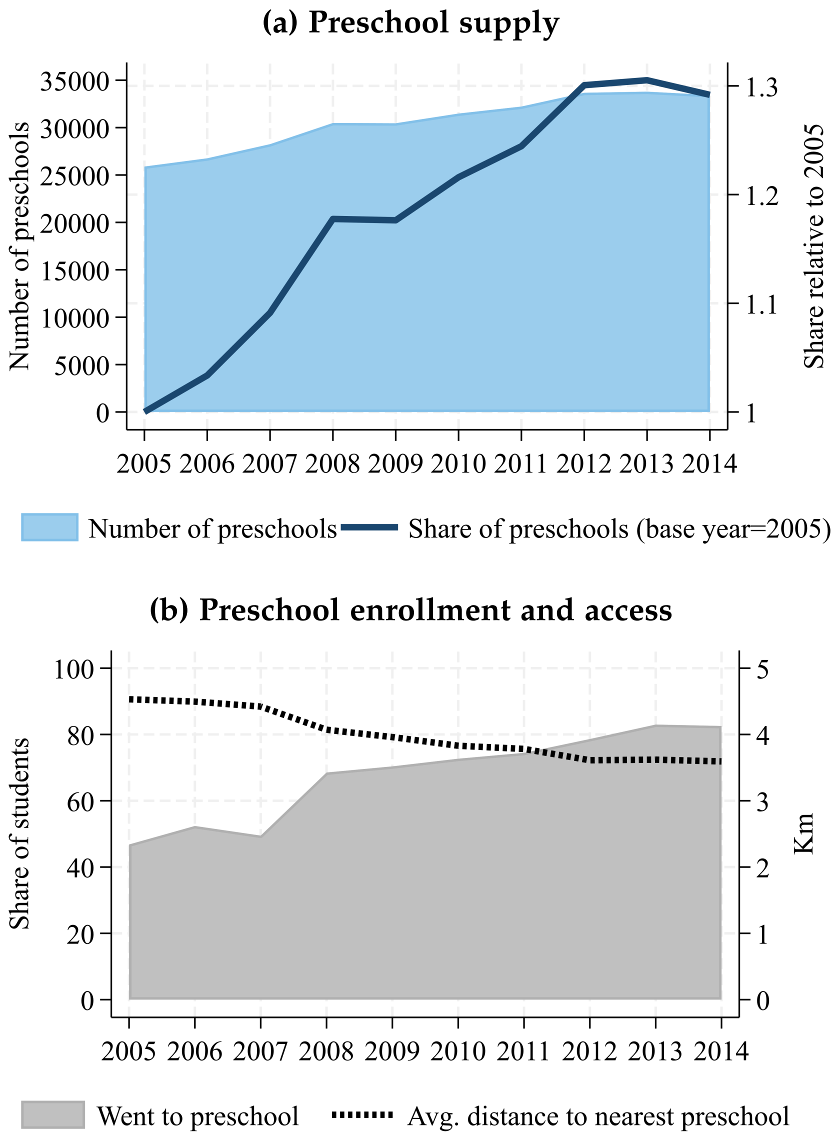
Table A8. Effect of Preschool Exposure on Academic Performance - Alternative Estimators

	Parametric Scores		**Pr(Reading Score above quartile)**			**Pr(Math Score above quartile)**		
	Math	**Reading**	**Bottom**	**Median**	**Top**	**Bottom**	**Median**	**Top**
				TWFE				
Exposed×Post	0.030**	0.026**	0.015***	0.007**	0.001	0.012**	0.004	0.000
	(0.012)	(0.012)	(0.005)	(0.004)	(0.003)	(0.005)	(0.004)	(0.002)
Number of observations	1,704,991	1,704,991	1,704,991	1,704,991	1,704,991	1,704,991	1,704,991	1,704,991
				Sun and Abraham (2021)				
Exposed×Post	0.024**	0.020*	0.012**	0.005	0.001	0.008	0.003	0.001
	(0.012)	(0.012)	(0.005)	(0.004)	(0.002)	(0.005)	(0.004)	(0.002)
Number of observations	1,704,991	1,704,991	1,704,991	1,704,991	1,704,991	1,704,991	1,704,991	1,704,991
				Stacked				
Exposed×Post	0.033***	0.028**	0.016***	0.008**	0.002	0.013***	0.005	0.000
	(0.012)	(0.012)	(0.005)	(0.003)	(0.002)	(0.005)	(0.003)	(0.002)
N	15,693,124	15,693,124	15,693,124	15,693,124	15,693,124	15,693,124	15,693,124	15,693,124

Notes: Each column reports the effect of preschool exposure on the relevant outcome. Clustered standard errors at the municipality level in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

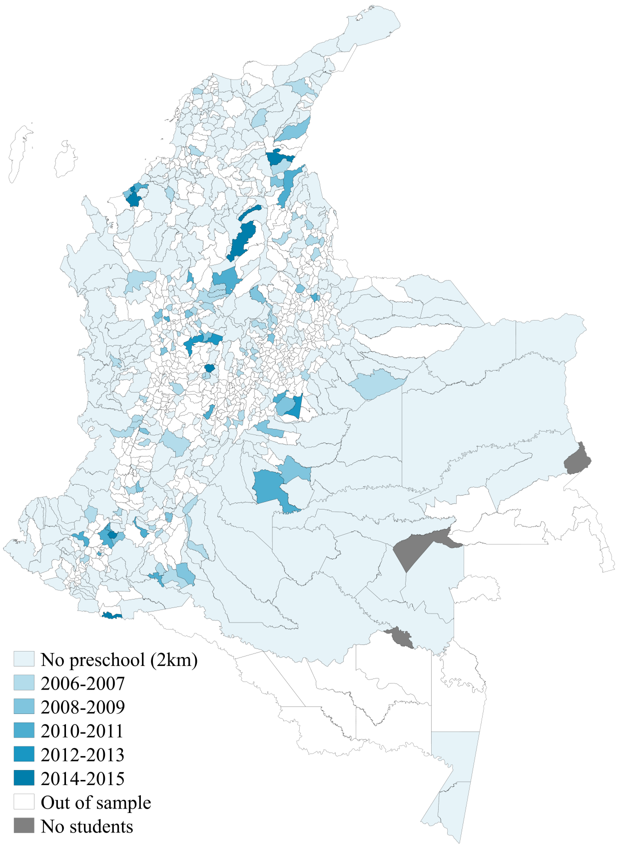
A.2 Additional Figures

Figure A1. Evolution of Preschool Supply and Enrollment (2005–2014)



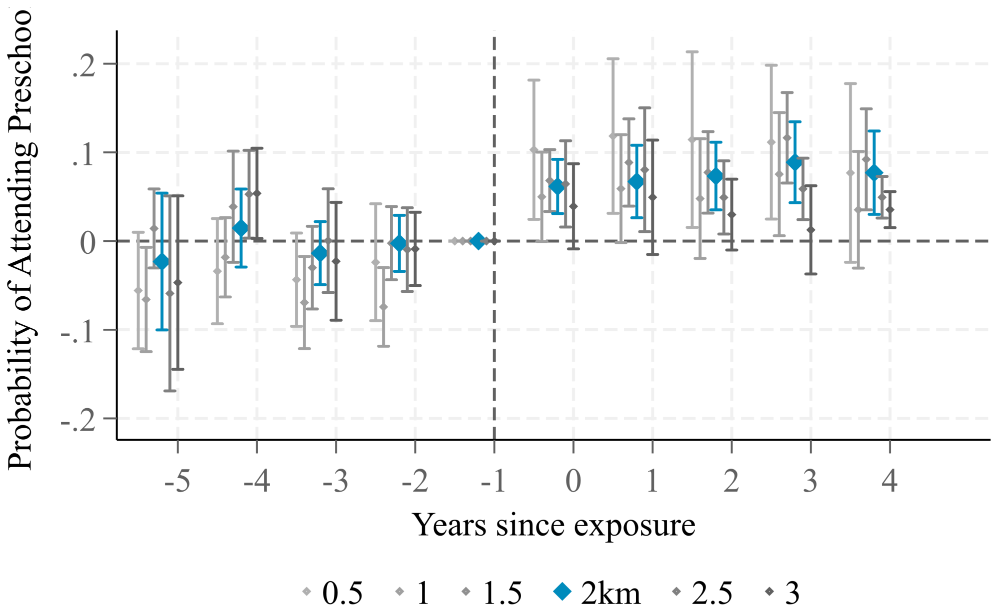
Note: Authors' calculations using data from SIMAT. The share of students who attended preschool is calculated over new entrants to first grade in the following year.

Figure A2. Availability of preschool over space and time



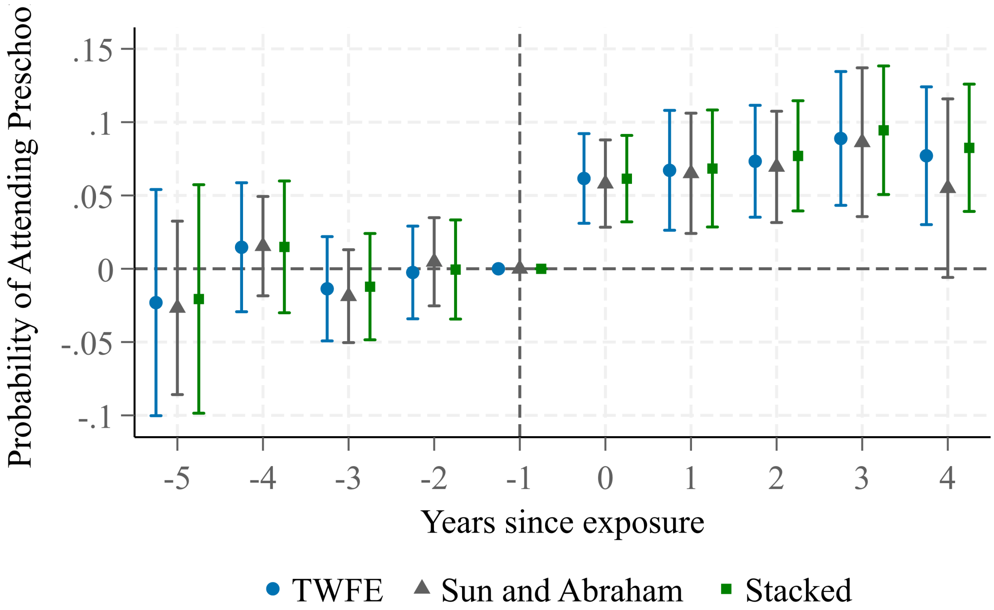
Note: The map displays the geographical distribution of preschool openings, categorized by timing. "Out of sample" refers to students who were below 2 kilometers from the nearest preschool at baseline. "No students" refers to municipalities with no students entering first grade between 2006–2011 in SIMAT.

Figure A3. Effect of Preschool Exposure at Different Radii on the Probability of Attending Preschool



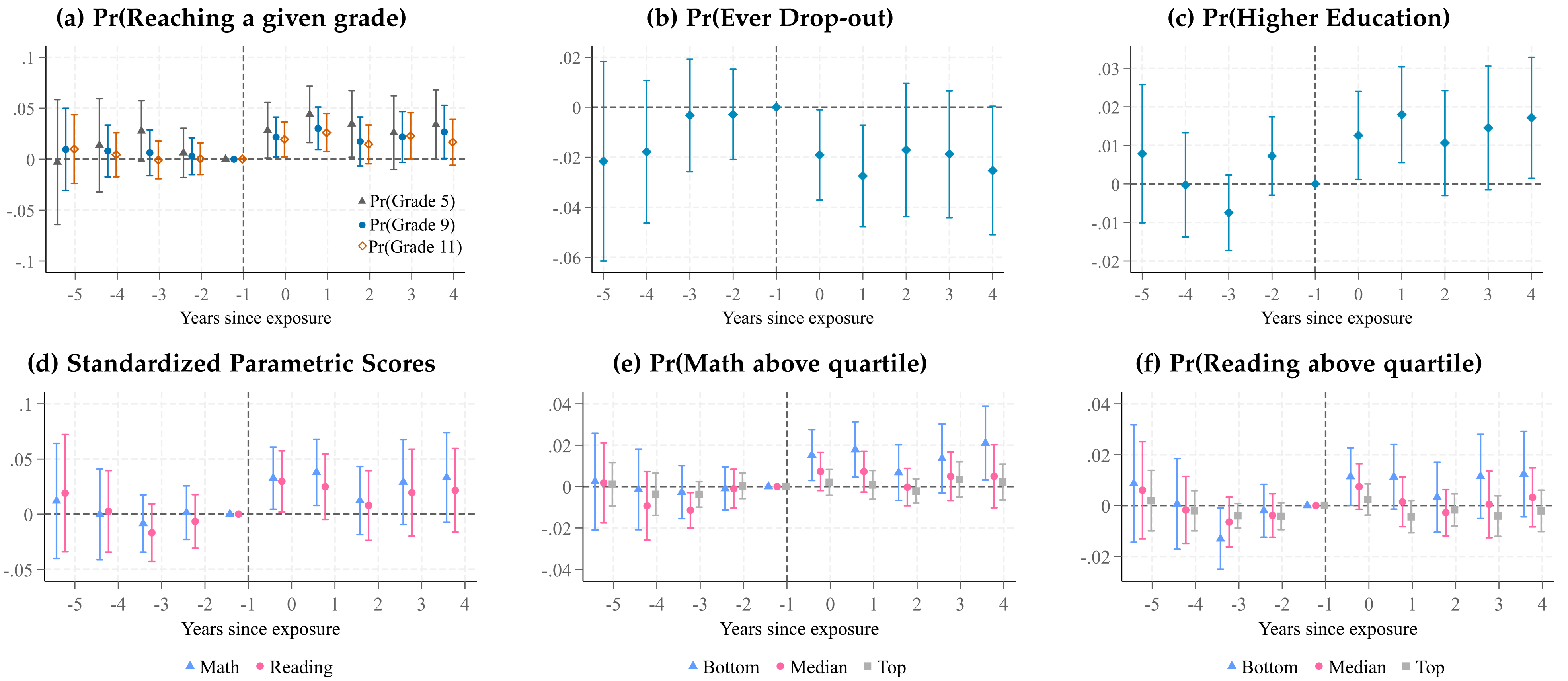
Note: The figure presents event-study estimates by different radii. Confidence intervals at the 95% level, computed with clustered standard errors at the municipality level.

Figure A4. Effect of Preschool Exposure on the Probability of Attending Preschool, by Alternative Estimators



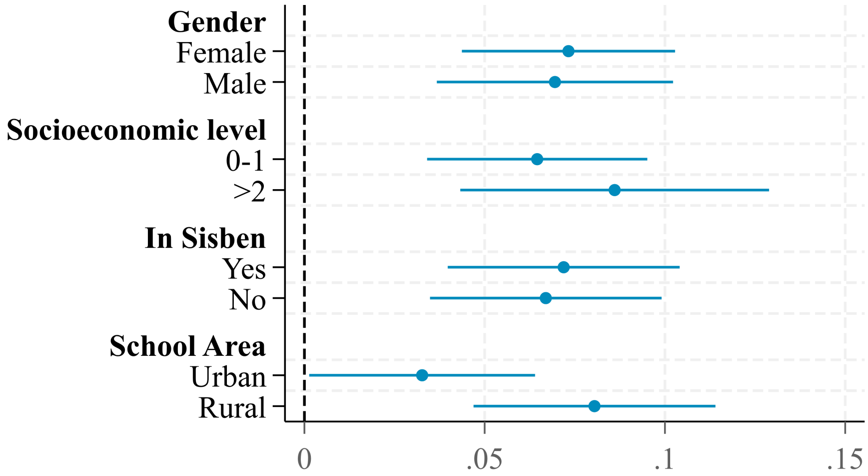
Note: The figure presents dynamic difference-in-difference estimates using TWFE and alternative estimators. Confidence intervals at the 95% level, computed with clustered standard errors at the municipality level.

Figure A5. Event-study estimates of Preschool Exposure on Academic Outcomes



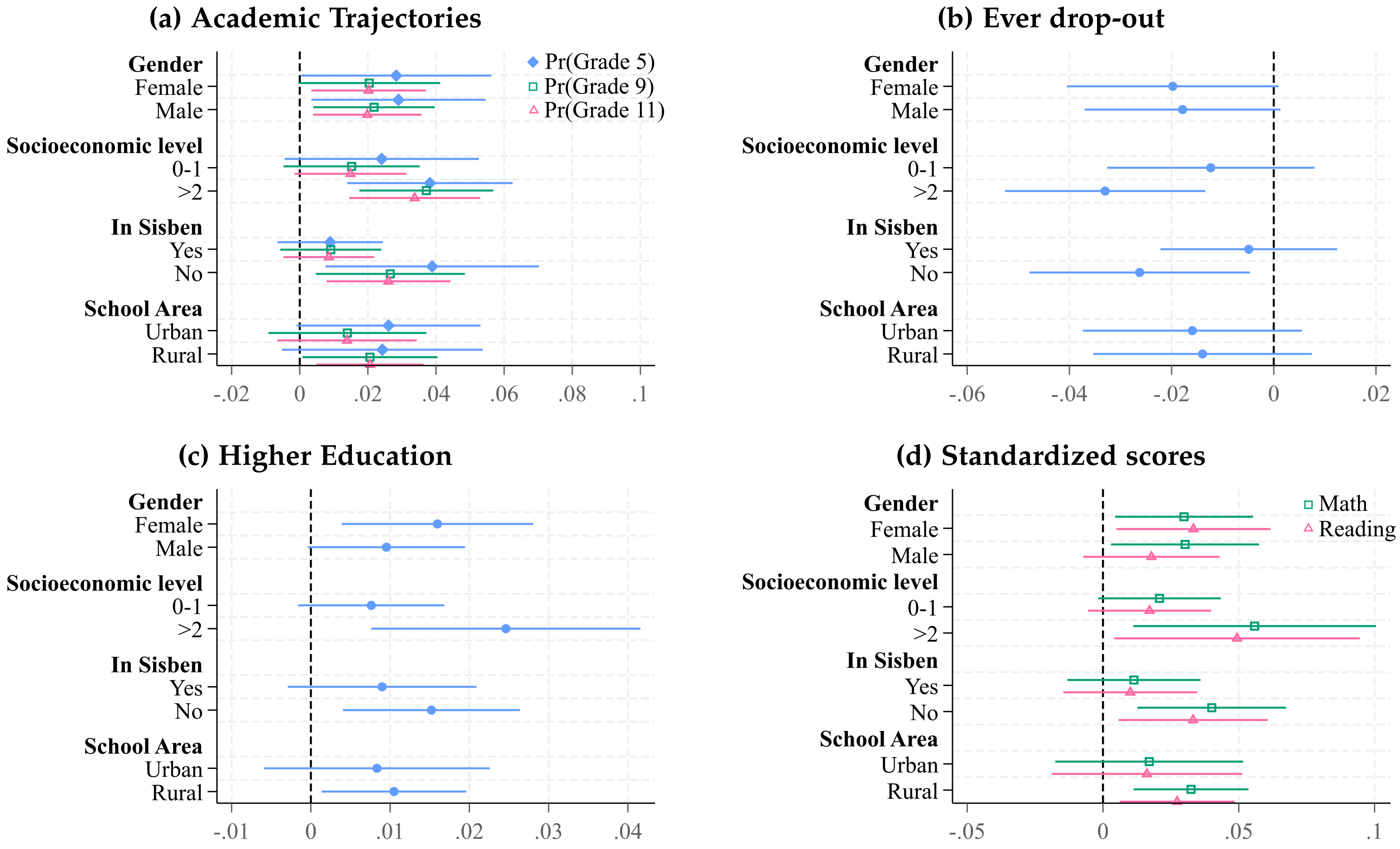
Note: The figures present event-study estimates for the relevant outcomes. Confidence intervals at the 95% level, computed with clustered standard errors at the municipality level.

Figure A6. Heterogeneous Effects of Preschool Exposure on Preschool Enrollment, by Student Characteristics



Note: Note: The figures present event-study estimates for the relevant outcomes. Confidence intervals at the 95% level, computed with clustered standard errors at the municipality level in panel (a) and at the school level in panel (b).

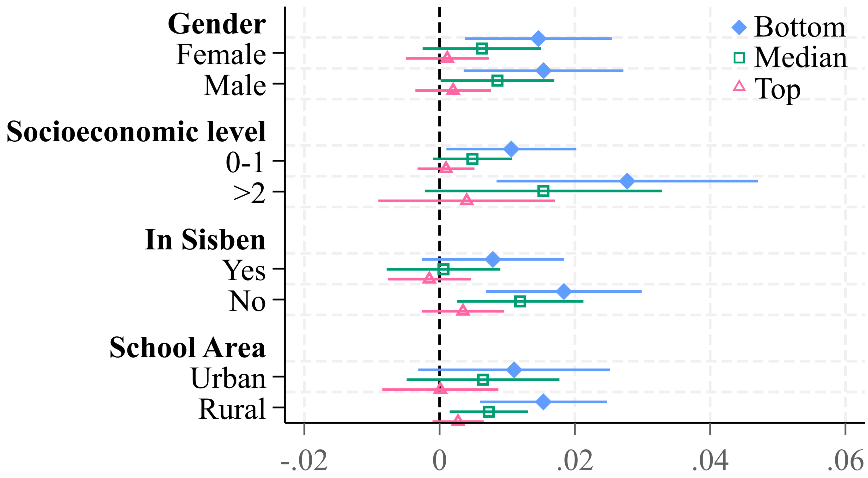
Figure A7. Heterogeneous Effects of Preschool Exposure on Academic Trajectories and Parametric Test Scores, by Student Characteristics



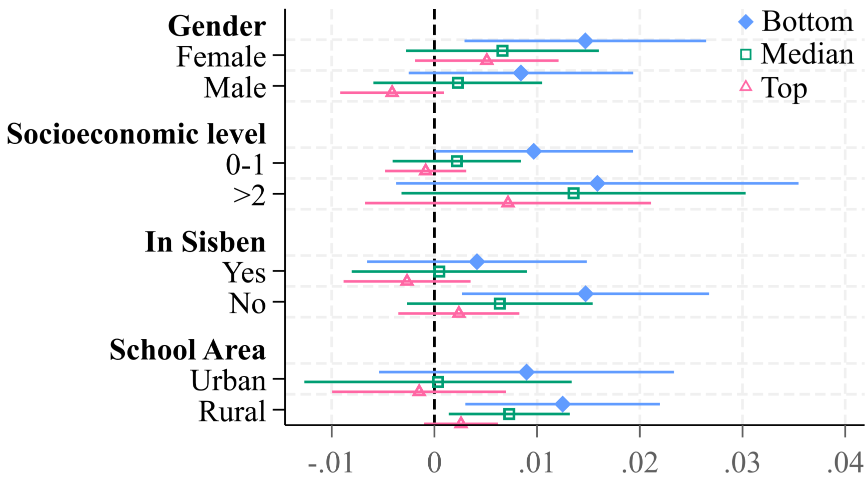
Notes: The figures present event-study estimates by subgroups. Confidence intervals at the 95% level, computed with clustered standard errors at the municipality level.

Figure A8. Heterogeneous Effects of Preschool Exposure on Academic Performance, by Student Characteristics

(a) Pr(Math score above quartile)



(b) Pr(Reading score above quartile)



Notes: The figures present event-study estimates by subgroups. Confidence intervals at the 95% level, computed with clustered standard errors at the municipality level.

B School Feeding Program

B.1 Additional Tables

Table B1. Descriptive Statistics by School Meals Participation

	School Meals	No School Meals
Panel A: Socioeconomic Characteristics		
Age at 1st grade	6.74	6.80
Female	0.48	0.49
0, 1	0.72	0.70
2, 3 y 4	0.28	0.30
Urban	0.61	0.62
Panel B: Academic Trajectories and Performance		
School dropout	0.30	0.65
Primary completion (5th grade)	0.96	0.55
9th grade	0.74	0.37
Secondary completion (11th grade)	0.63	0.34
Enrolled in Higher education	0.14	0.13
Standardized values of Math	0.19	-0.18
Standardized values of Reading	0.19	-0.17
Number of observations	820,535	884,456
Number of schools	29,435	29,397

Notes: Summary statistics, by school meal participation.

Table B2. Predictors of Exposure to School Feeding

Variable (2011)	(1) Year	(2) 2012-2013	(3) 2014	(4) 2015	(5) 2016	(6) 2017-2019
Full-time school	0.147***	-0.011*	-0.082***	0.033***	0.070***	-0.011**
	(0.020)	(0.006)	(0.006)	(0.007)	(0.006)	(0.005)
Rural school	0.893***	-0.135***	-0.161***	0.063***	0.098***	0.132***
	(0.018)	(0.006)	(0.007)	(0.006)	(0.005)	(0.004)
Share of preschool students	1.695*	-0.138	-0.169	0.197	-0.211	0.253
	(0.874)	(0.208)	(0.272)	(0.285)	(0.266)	(0.271)
Share of primary students	0.408***	-0.074***	0.029	-0.082***	0.069***	0.051**
	(0.077)	(0.020)	(0.021)	(0.023)	(0.022)	(0.020)
Share of secondary students	-0.435***	0.050**	0.135***	-0.083***	-0.030	-0.074***
	(0.074)	(0.020)	(0.021)	(0.023)	(0.021)	(0.018)
Share of low income students	0.287**	-0.003	-0.109**	-0.073*	0.210***	-0.017
	(0.121)	(0.040)	(0.045)	(0.040)	(0.035)	(0.022)
Number of schools	32,270	32,270	32,270	32,270	32,270	32,270
Mean	2013.9	0.242	0.200	0.245	0.184	0.126

Notes: Regression estimates for the probability of school feeding exposure. The outcome variable in column (1) is the year when the school feeding program first arrived. Columns (2)-(6) present estimates for the probability of receiving school meals in the corresponding years. All regressions include department fixed-effects. Robust standard errors in parentheses. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table B3. Effect of School Feeding Exposure on Performance in the High School Exit Exam

	(1)	(2)	(3)
Panel A: Test Taking and Parametric Scores			
	Took Test	**Math**	**Reading**
Exposed to Feeding×Post	0.083***	0.138***	0.131***
	(0.005)	(0.011)	(0.011)
Number of observations	1,704,854	1,704,854	1,704,854
Number of clusters	32,270	32,270	32,270
Mean	0.302	-0.295	-0.289
Panel B: Probability of Math Score above quartile			
	Bottom	**Median**	**Top**
Exposed to Feeding×Post	0.058***	0.037***	0.019***
	(0.005)	(0.004)	(0.002)
Number of observations	1,704,854	1,704,854	1,704,854
Number of clusters	32,270	32,270	32,270
Mean	0.202	0.120	0.052
Panel C: Probability of Reading Score above quartile			
	Bottom	**Median**	**Top**
Exposed to Feeding×Post	0.055***	0.034***	0.016***
	(0.005)	(0.004)	(0.002)
Number of observations	1,704,854	1,704,854	1,704,854
Number of clusters	32,270	32,270	32,270
Mean	0.204	0.125	0.056

Notes: Each column reports the effect of school feeding exposure on the relevant outcome. Clustered standard errors at the school level in parentheses. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table B4. Effect of School Feeding Exposure on Receiving School Meals - Alternative Estimators

		Sun and Abraham	
	TWFE	Never-treated	Last-treated
Exposed to Feeding×Post	0.136***	0.137***	0.136***
	(0.005)	(0.005)	(0.005)
Number of observations	1,704,854	1,704,854	1,682,012

Notes: Each column reports the effect of school feeding exposure on the relevant outcome. Clustered standard errors at the school level in parentheses. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table B5. Effect of School Feeding Exposure on Academic Progression - Alternative Estimators

	Probability of reaching grade 5th	**9th**	**11th**	**Pr(Ever Drop-out)**	**Pr(Higher Education)**	**Pr(Took Test)**
			TWFE			
Exposed to Feeding×Post	0.079***	0.093***	0.081***	-0.087***	0.045***	0.083***
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Number of observations	1,704,854	1,704,854	1,704,854	1,704,854	1,704,854	1,704,854
			Sun and Abraham: Never-treated			
Exposed to Feeding×Post	0.081***	0.095***	0.083***	-0.089***	0.047***	0.085***
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Number of observations	1,704,854	1,704,854	1,704,854	1,704,854	1,704,854	1,704,854
			Sun and Abraham: Last-treated			
Exposed to Feeding×Post	0.080***	0.094***	0.082***	-0.088***	0.047***	0.085***
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Number of observations	1,682,012	1,682,012	1,682,012	1,682,012	1,682,012	1,682,012

Notes: Each column reports the effect of school feeding exposure on the relevant outcome. Clustered standard errors at the school level in parentheses. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

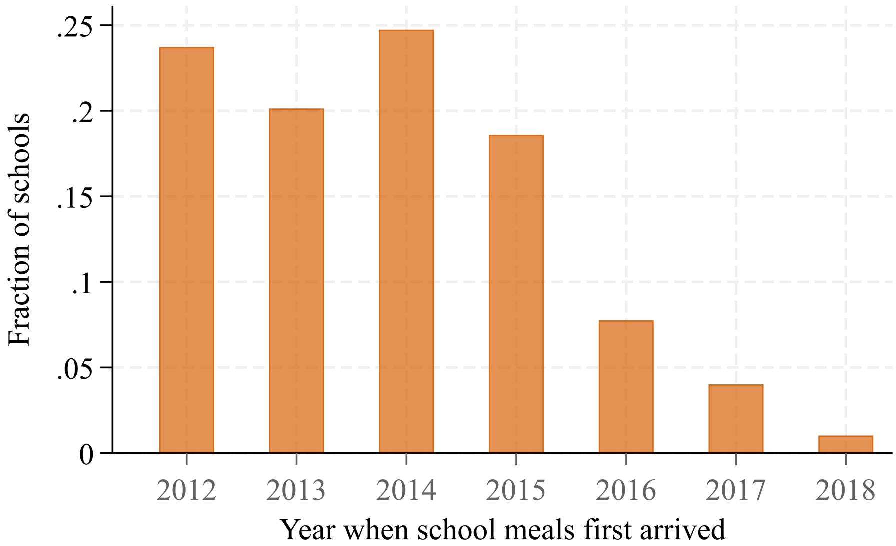
Table B6. Effect of School Feeding Exposure on Academic Performance - Alternative Estimators

	Parametric Scores		**Pr(Reading Score above quartile)**			**Pr(Math Score above quartile)**		
	Math	**Reading**	**Bottom**	**Median**	**Top**	**Bottom**	**Median**	**Top**
TWFE								
Exposed to Feeding×Post	0.138***	0.131***	0.058***	0.037***	0.019***	0.055***	0.034***	0.016***
	(0.011)	(0.011)	(0.005)	(0.004)	(0.002)	(0.005)	(0.004)	(0.002)
N	1704854	1704854	1704854	1704854	1704854	1704854	1704854	1704854
Sun and Abraham: Never-treated								
Exposed to Feeding×Post	0.142***	0.135***	0.060***	0.038***	0.020***	0.057***	0.036***	0.016***
	(0.011)	(0.011)	(0.005)	(0.004)	(0.002)	(0.005)	(0.004)	(0.002)
Number of observations	1,704,854	1,704,854	1,704,854	1,704,854	1,704,854	1,704,854	1,704,854	1,704,854
Sun and Abraham: Last-treated								
Exposed to Feeding×Post	0.142***	0.135***	0.060***	0.039***	0.020***	0.057***	0.036***	0.017***
	(0.011)	(0.011)	(0.005)	(0.004)	(0.002)	(0.005)	(0.004)	(0.002)
Number of observations	1,682,012	1,682,012	1,682,012	1,682,012	1,682,012	1,682,012	1,682,012	1,682,012

Notes: Each column reports the effect of school feeding exposure on the relevant outcome. Clustered standard errors at the school level in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

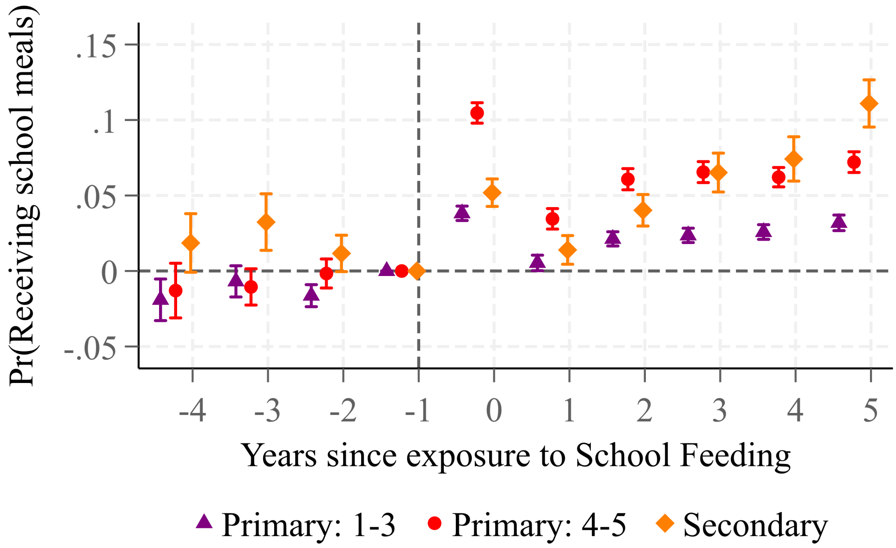
B.2 Additional Figures

Figure B1. Distribution of school meal program arrival across first-grade schools



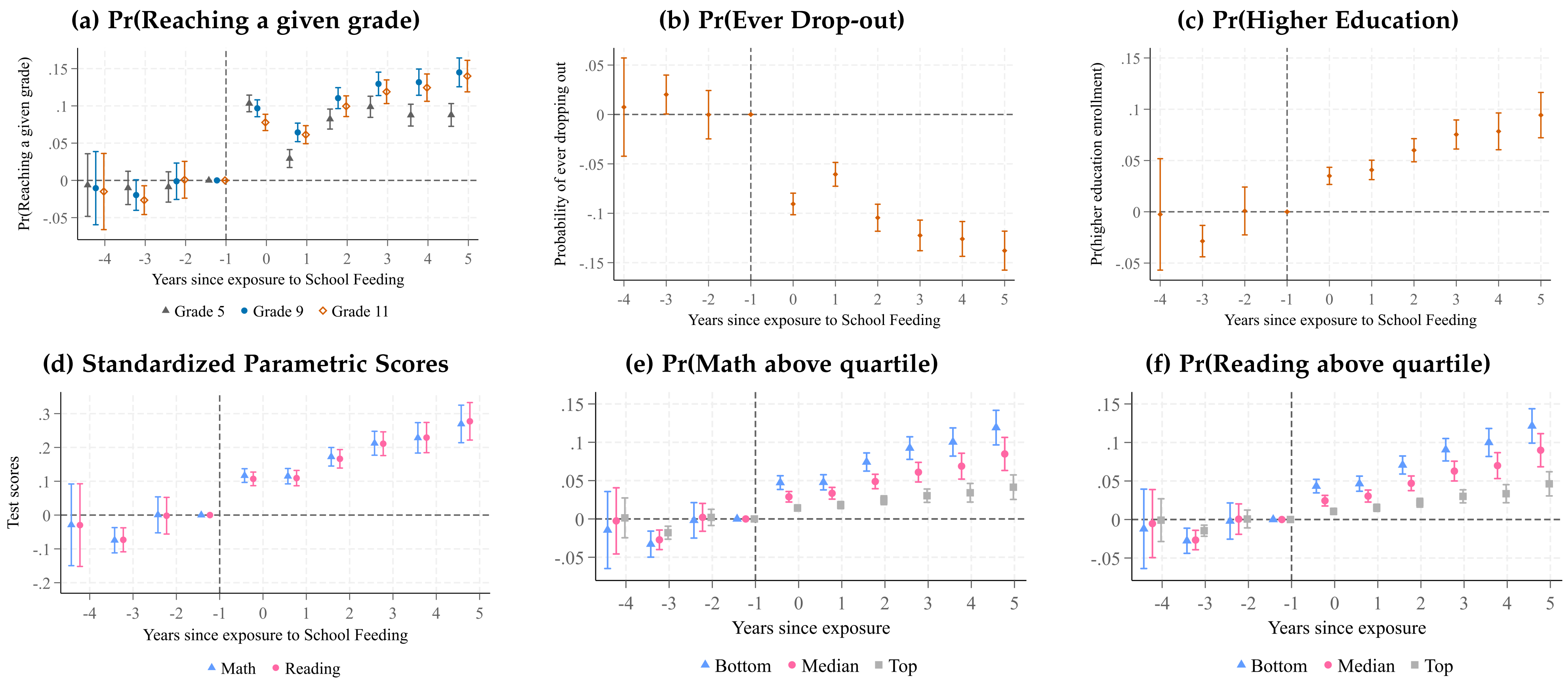
Notes: The figure presents the distribution of arrival of school meals across first-grade schools for students in our sample.

Figure B2. Effect of School Feeding Exposure on the Probability of Receiving School Meals in Primary and Secondary



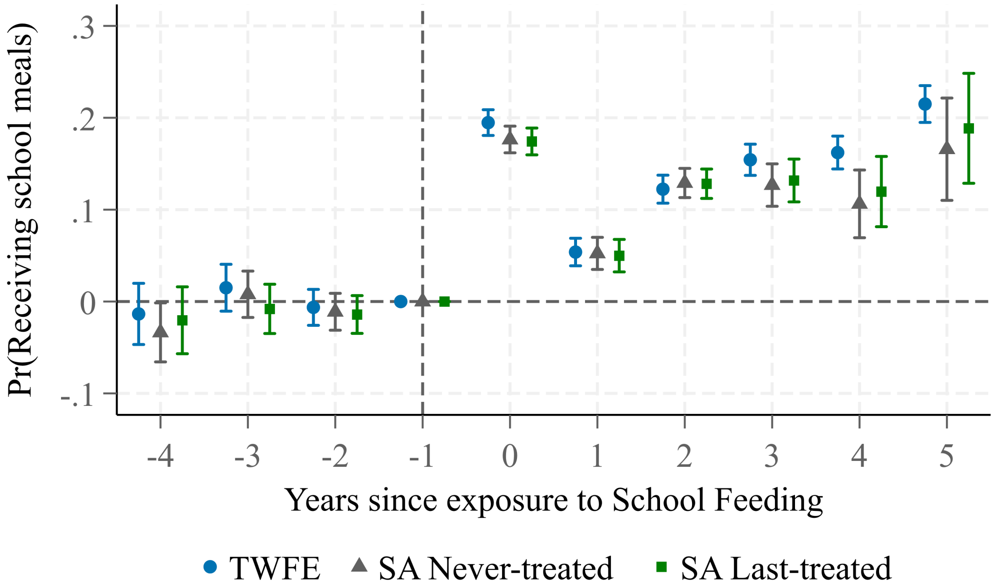
Notes: The figures present event-study estimates for the probability of receiving school meals in different grades. Confidence intervals at the 95% level, computed with clustered standard errors at the municipality level.

Figure B3. Event-study estimates of Exposure to School Feeding on Academic Outcomes



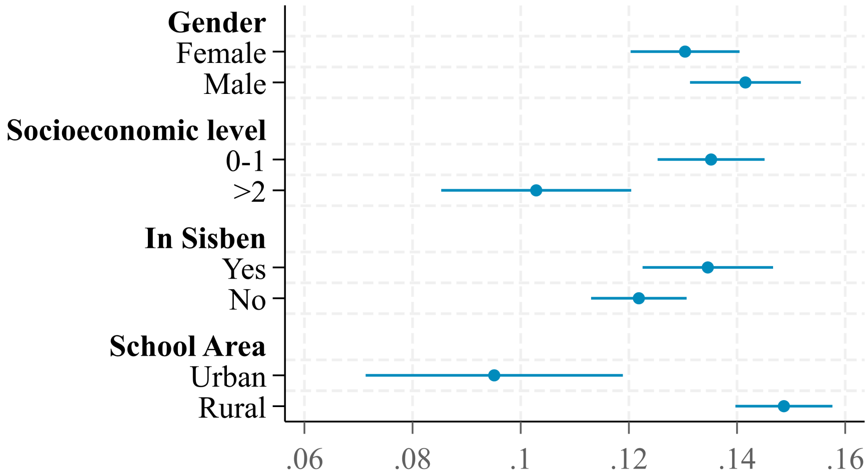
Notes: The figures present event-study estimates for relevant outcomes. Confidence intervals at the 95% level, computed with clustered standard errors at the school level.

Figure B4. Effect of School Feeding Exposure on the Probability of Receiving School Meals, by Different Estimators



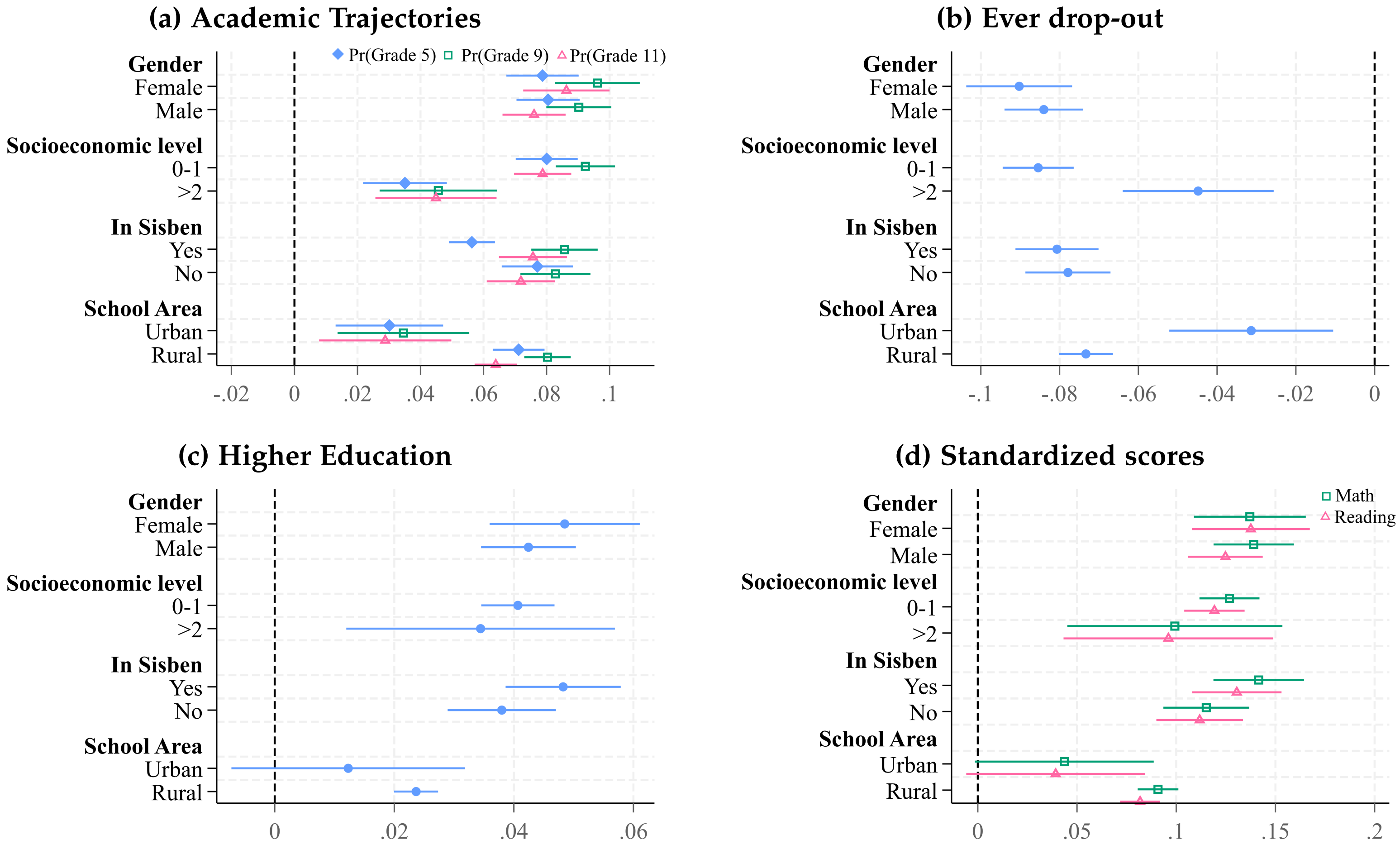
Notes: The figures present event-study estimates for relevant outcomes. Confidence intervals at the 95% level, computed with clustered standard errors at the school level.

Figure B5. Heterogeneous Effects of School Feeding Exposure on Receiving School Meals, by Student Characteristics



Notes: The figures present event-study estimates by subgroups. Confidence intervals at the 95% level, computed with clustered standard errors at the school level.

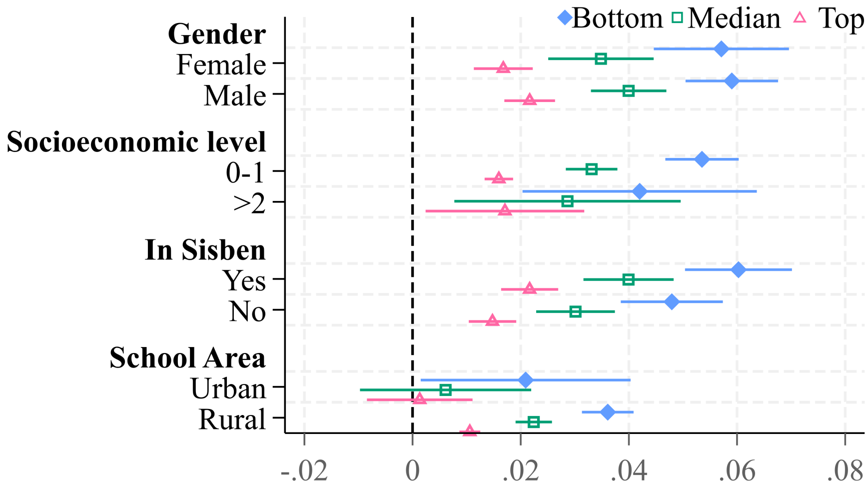
Figure B6. Heterogeneous Effects of School Feeding Exposure on Academic Trajectories and Parametric Test Scores, by Student Characteristics



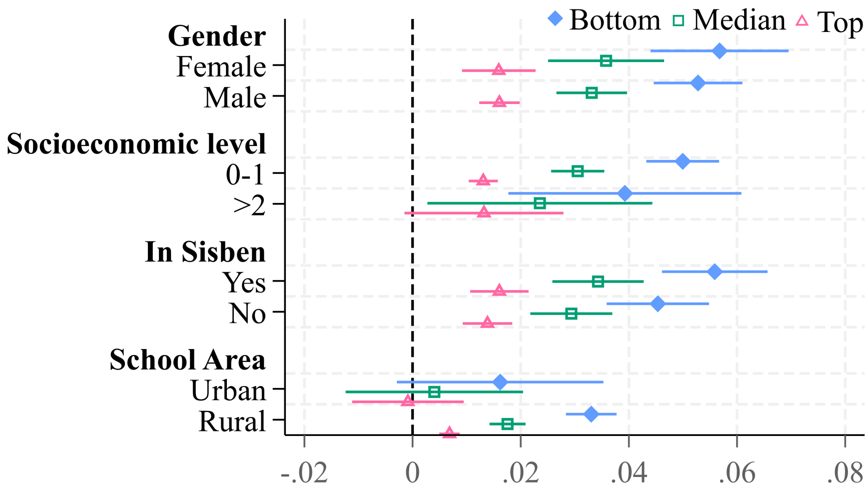
Notes: The figures present event-study estimates by subgroups. Confidence intervals at the 95% level, computed with clustered standard errors at the school level.

Figure B7. Heterogeneous Effects of School Feeding Exposure on Academic Performance, by Student Characteristics

(a) Pr(Math score above quartile)



(b) Pr(Reading score above quartile)



Notes: The figures present event-study estimates for relevant outcomes. Confidence intervals at the 95% level, computed with clustered standard errors at the school level.

C Complementarities

C.1 Additional Tables

Table C1. Dynamic Complementarity between Preschool and School Feeding Exposure on Treatments

	(1) **Both**	(2) **Only Meals**	(3) **Only Preschool**	(4) **Neither**	(5) **Total Preschool**
Preschool	0.000	0.003	0.047***	-0.051***	0.047***
	(0.012)	(0.010)	(0.014)	(0.014)	(0.015)
Feeding	0.121***	0.014***	-0.040***	-0.094***	0.081***
	(0.004)	(0.002)	(0.006)	(0.005)	(0.005)
Preschool×School Feeding	0.047***	-0.022**	-0.023*	-0.003	0.024*
	(0.012)	(0.010)	(0.014)	(0.014)	(0.014)
Total Preschool	0.048***	-0.018***	0.024***	-0.054***	0.072***
	(0.005)	(0.004)	(0.006)	(0.006)	(0.007)
Number of observations	1,704,854	1,704,854	1,704,854	1,704,854	1,704,854
Number of clusters	32,270	32,270	32,270	32,270	32,270
Mean	0.1706	0.1180	0.3417	0.3697	0.5123

Notes: Each column reports estimates from equation (7) on the relevant outcome. Clustered standard errors at the school level in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

Table C2. Dynamic Complementarity between Preschool and School Feeding Exposure on Academic Performance

	(1)	(2)	(3)	(4)
	Probability of scoring > quartile			
Panel A: Math	**Bottom**	**Median**	**Top**	**Score**
Preschool	0.004	0.000	-0.005	-0.003
	(0.010)	(0.007)	(0.005)	(0.021)
Feeding	0.058***	0.037***	0.019***	0.136***
	(0.005)	(0.004)	(0.002)	(0.011)
Preschool×School Feeding	0.009	0.006	0.006	0.031
	(0.010)	(0.007)	(0.005)	(0.021)
Total Preschool	0.013***	0.006**	0.001	0.027***
	(0.004)	(0.003)	(0.002)	(0.008)
Implied TOT	0.085	0.000	-0.106	-0.064
(Preschool without Feeding)	[0.669]	[0.984]	[0.367]	[0.864]
Implied TOT	0.181	0.083	0.014	0.375
(Preschool and Feeding)	[0.001]	[0.055]	[0.594]	[0.001]
Number of observations	1,704,854	1,704,854	1,704,854	1,704,854
Number of clusters	32,270	32,270	32,270	32,270
Mean	0.201	0.120	0.053	-0.294
	Probability of scoring > quartile			
Panel B: Reading	**Bottom**	**Median**	**Top**	**Score**
Preschool	0.001	-0.003	-0.010**	-0.012
	(0.010)	(0.008)	(0.004)	(0.021)
Feeding	0.054***	0.034***	0.015***	0.129***
	(0.005)	(0.004)	(0.002)	(0.011)
Preschool×School Feeding	0.009	0.006	0.011**	0.035*
	(0.010)	(0.007)	(0.004)	(0.021)
Total Preschool	0.010***	0.003	0.000	0.023***
	(0.004)	(0.003)	(0.002)	(0.008)
Implied TOT	0.021	-0.064	-0.213	-0.255
(Preschool without Feeding)	[0.941]	[0.723]	[0.070]	[0.548]
Implied TOT	0.139	0.042	0.000	0.319
(Preschool and Feeding)	[0.012]	[0.342]	[0.891]	[0.006]
Number of observations	1,704,854	1,704,854	1,704,854	1,704,854
Number of clusters	32,270	32,270	32,270	32,270
Mean	0.203	0.125	0.056	-0.288

Notes: Each column reports estimates from equation (7) on the relevant outcome. Clustered standard errors at the school level in parentheses. * p<0.10, ** p<0.05, *** p<0.01. p-values for *implied* TOTs in brackets, computed using bootstrap.

Table C3. Average characteristics by socioeconomic level and Sisben subsamples

	Level=0-1		Level=2-4	
	Sisben=No	Sisben=Yes	Sisben=No	Sisben=Yes
Age at 1st grade	6.85	6.74	6.74	6.69
Female	0.48	0.48	0.49	0.49
Urban	0.46	0.55	0.90	0.89
Preschool enrollment	0.52	0.70	0.65	0.79
School meals	0.39	0.59	0.40	0.52
Number of observations	633,574	571,492	224,417	275,508
Share	0.53	0.47	0.45	0.55

Notes: "Level" refers to the student socioeconomic level reported in SIMAT.

Table C4. Differences in household size and maternal characteristics, by socioeconomic level (Sisben subsample)

	Socioeconomic Level		
	0-1	2-4	Difference
Household size:			
< 4	0.333	0.449	0.116***
	(0.001)	(0.001)	(0.001)
5	0.207	0.219	0.012***
	(0.001)	(0.001)	(0.001)
$6-7$	0.257	0.207	-0.049***
	(0.001)	(0.001)	(0.001)
> 8	0.203	0.124	-0.079***
	(0.001)	(0.001)	(0.001)
Mother's employment status:			
Working	0.192	0.336	0.144***
	(0.001)	(0.001)	(0.001)
Unemployed	0.019	0.036	0.017***
	(0.000)	(0.000)	(0.000)
Housework	0.722	0.575	-0.147***
	(0.001)	(0.001)	(0.001)
Mother's education level:			
Primary	0.622	0.375	-0.247***
	(0.001)	(0.001)	(0.001)
Secondary	0.358	0.575	0.218***
	(0.001)	(0.001)	(0.001)
Higher Education	0.021	0.050	0.029***
	(0.000)	(0.000)	(0.000)
Number of observations	571,490	275,508	846,998

Notes: Subsample of students matched to Sisbén records. "Housework" refers to domestic work.

Table C5. Differences in paternal characteristics, by socioeconomic level (Sisben subsample)

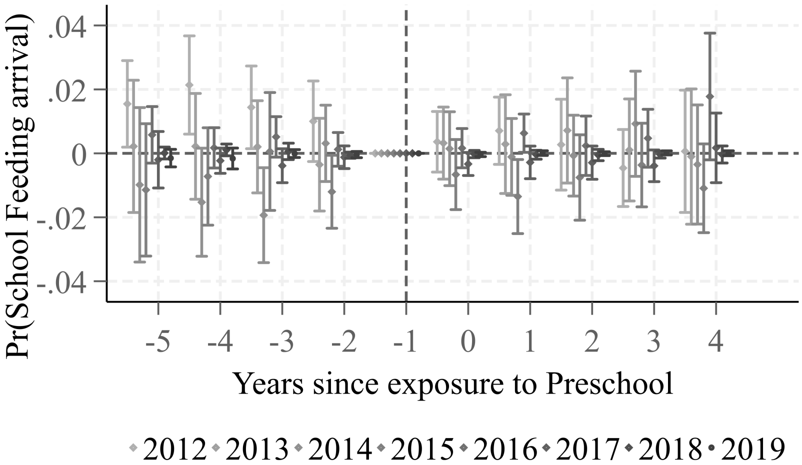
	Socioeconomic Level		
	0-1	2-4	Difference
Father's employment status:			
Working	0.897	0.899	0.002**
	(0.000)	(0.001)	(0.001)
Unemployed	0.047	0.060	0.013***
	(0.000)	(0.001)	(0.001)
Housework	0.007	0.006	-0.001***
	(0.000)	(0.000)	(0.000)
Father's education level:			
Primary	0.689	0.451	-0.238***
	(0.001)	(0.001)	(0.001)
Secondary	0.293	0.502	0.209***
	(0.001)	(0.001)	(0.001)
Higher Education	0.018	0.047	0.030***
	(0.000)	(0.000)	(0.000)
Number of observations	424,114	192,120	616,234

Notes: Subsample of students matched to Sisbén records with information on father's employment and educational level. "Housework" refers to domestic work.

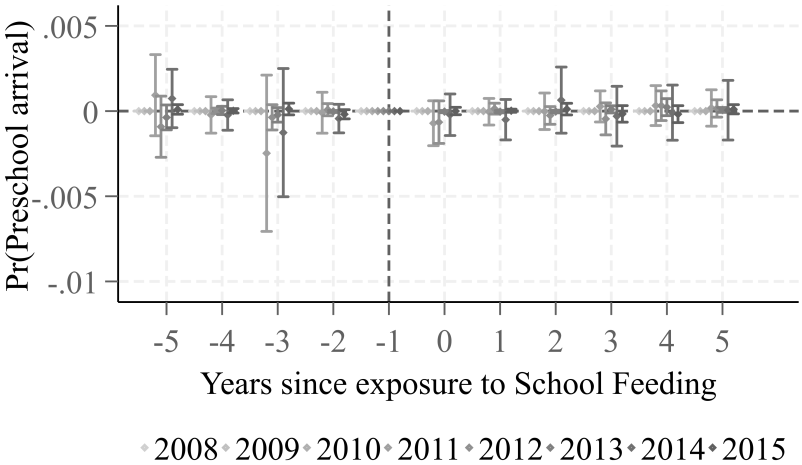
C.2 Additional Figures

Figure C1. Independence between Investments

(a) Preschool $\nrightarrow$ School Feeding

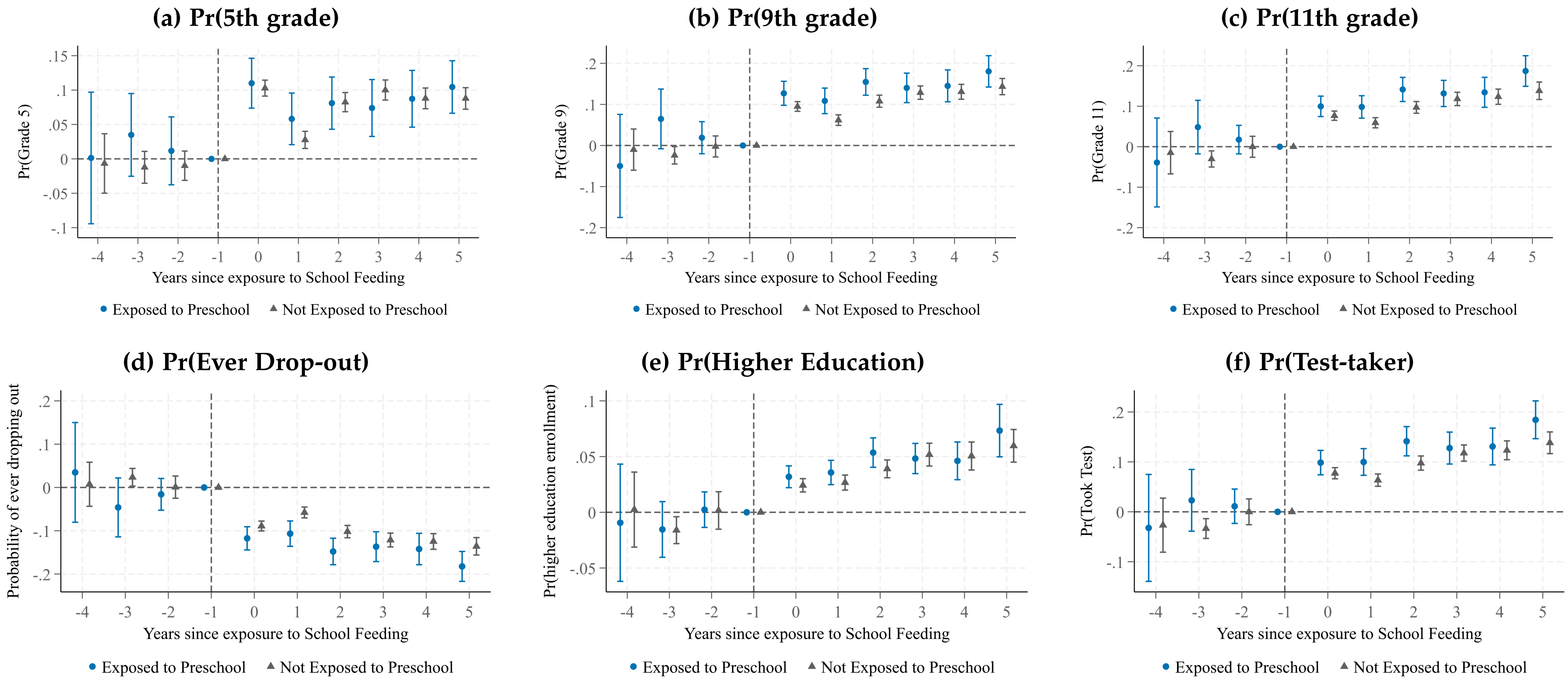


(b) School Feeding $\nrightarrow$ Preschool



Note: The figure plots event-study coefficients estimating the probability of investment arrival as a function of exposure to the other intervention. Panel (a) shows coefficients from regressions of school feeding arrival (2012–2019) on a set of indicator variables for each year relative to the timing of preschool exposure. Panel (b) shows coefficients from regressions of preschool arrival (2008–2015) on a set of indicator variables for each year relative to the timing of school feeding exposure. Confidence intervals at the 95% level.

Figure C2. Effect of School Feeding Exposure on Academic Outcomes, by Exposure to Preschool



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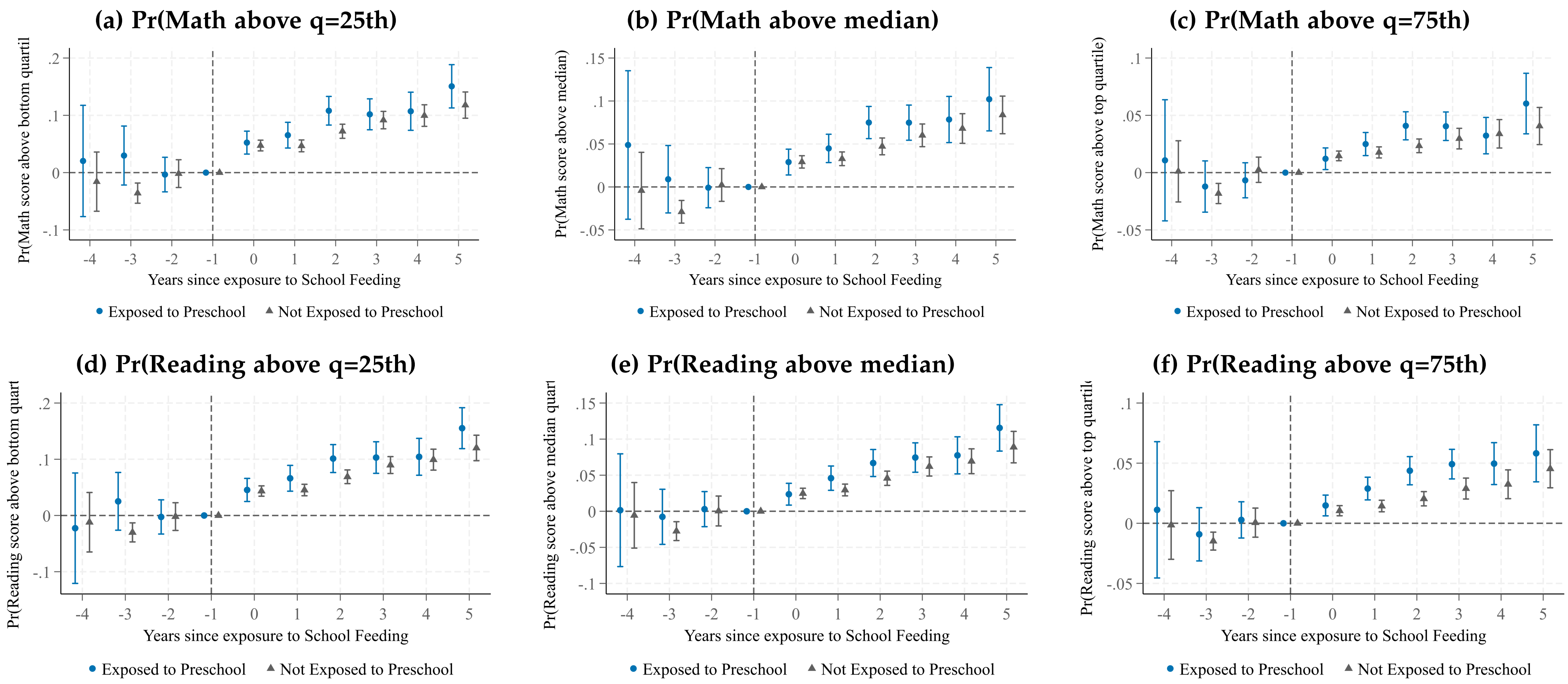
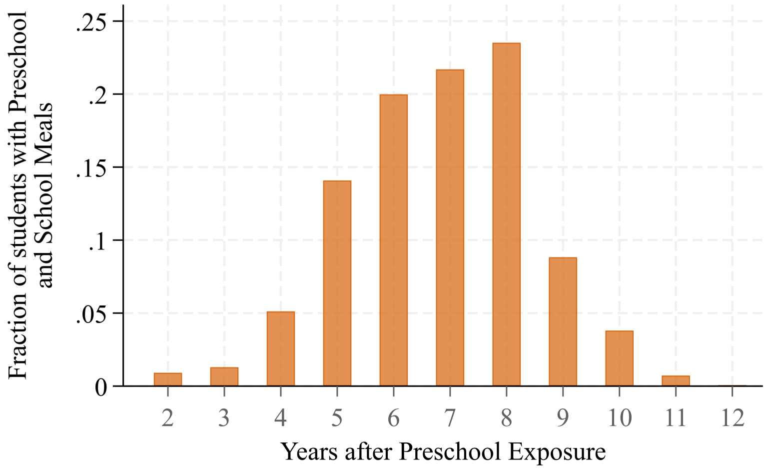
Notes: The figures present event-study estimates for relevant outcomes. Confidence intervals at the 95% level, computed with clustered standard errors at the school level.

Figure C3. Effect of School Feeding Exposure on Academic Performance, by Exposure to Preschool



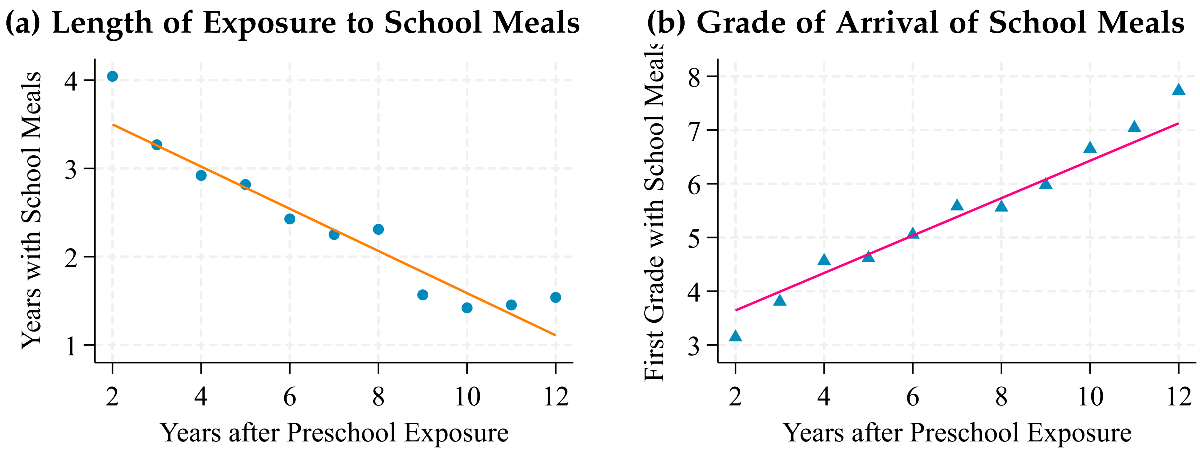
Notes: The figures present event-study estimates for relevant outcomes. Confidence intervals at the 95% level, computed with clustered standard errors at the school level.

Figure C4. Distribution of years between preschool and school meals exposure



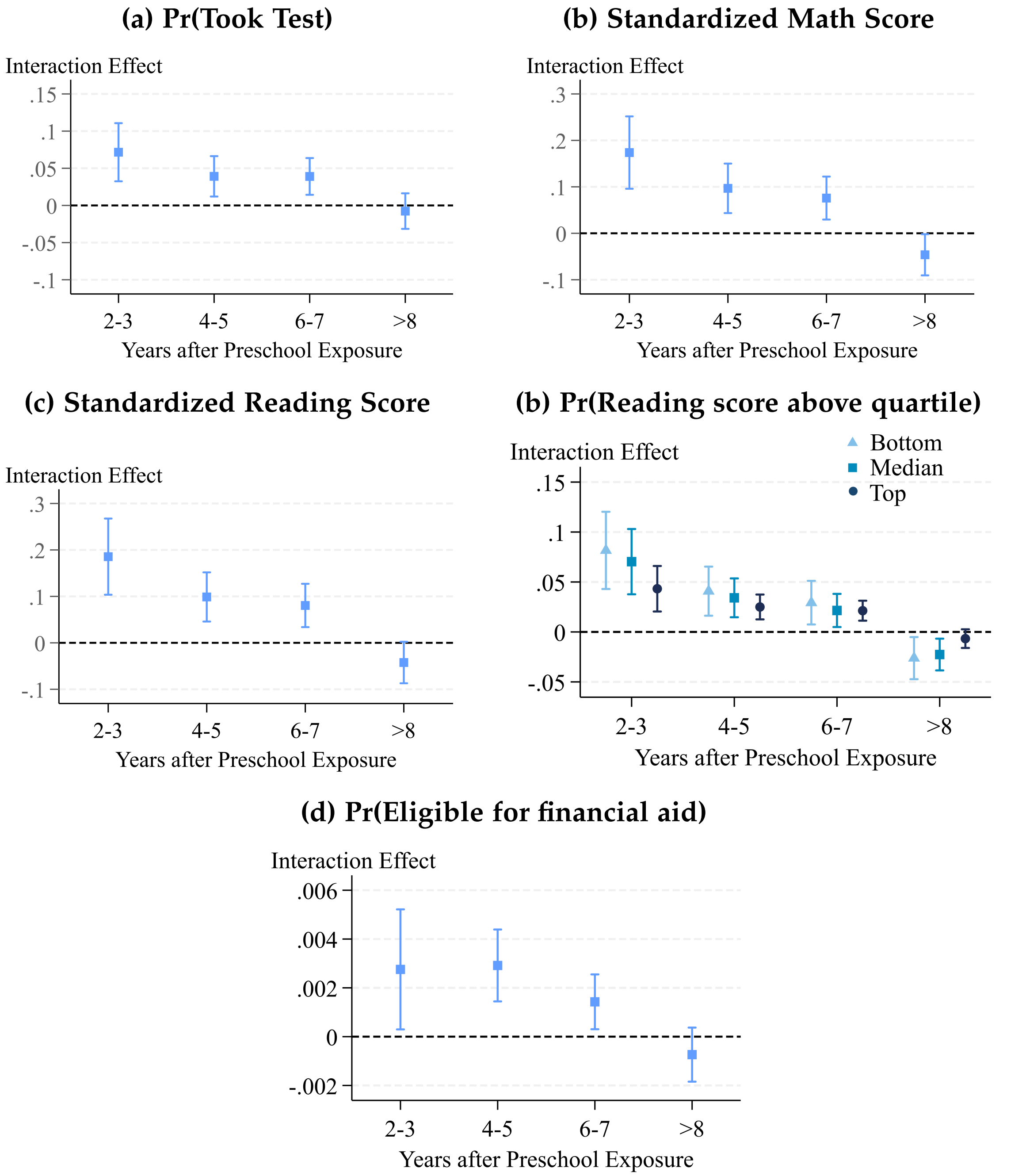
Notes: The figure shows the distribution of years between exposure to preschool and exposure to school meals.

Figure C5. Timing of School Feeding Arrival since Preschool Exposure



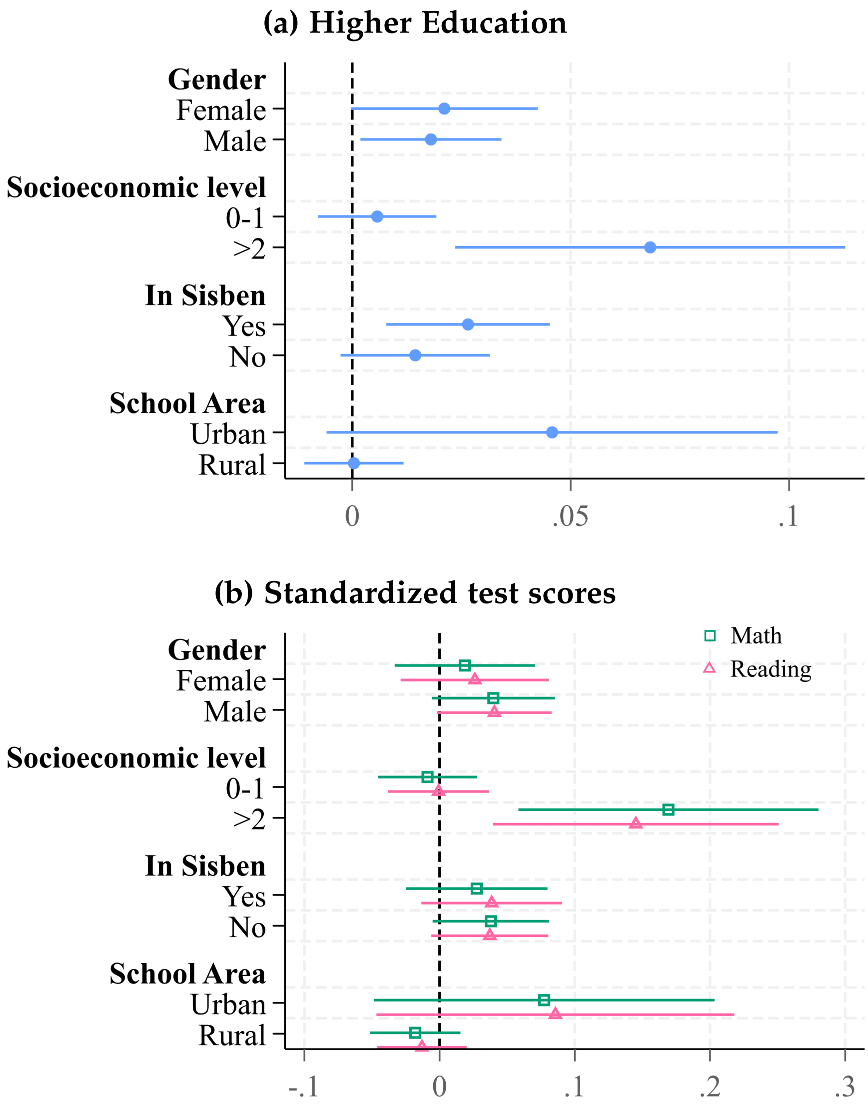
Notes: Panel (a) of the figure presents a scatter plot of years with school meals and years after preschool exposure, Panel (b) presents a scatter plot of grade when the student first received school meals and years after preschool exposure.

Figure C6. Arrival



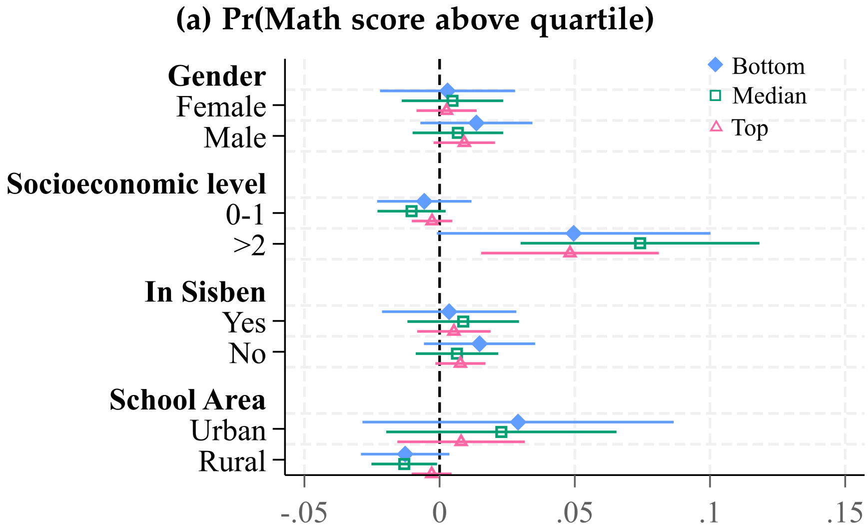
Notes: The figures present event-study estimates for relevant outcomes. Confidence intervals at the 95% level, computed with clustered standard errors at the school level.

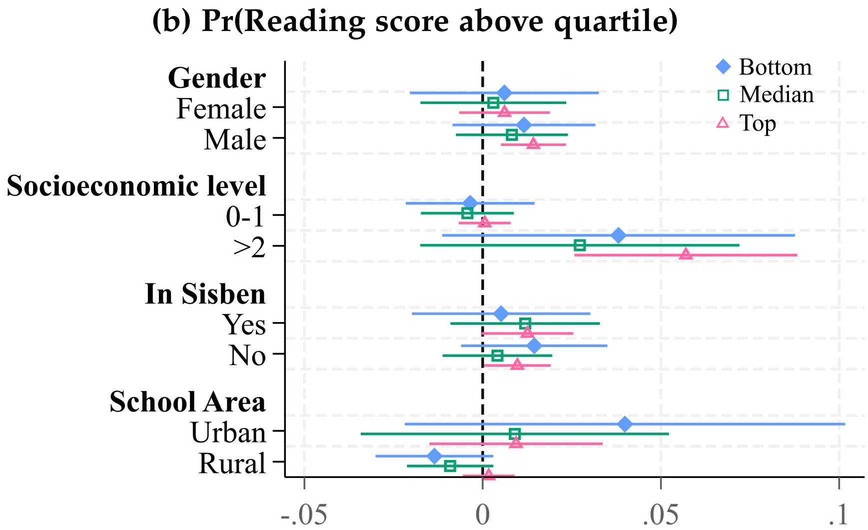
Figure C7. Interaction Effect of Preschool and School Feeding on Academic Trajectories, by student attributes



Note: The figures present event-study estimates by subgroups. Confidence intervals at the 95% level, computed with clustered standard errors at the school level.

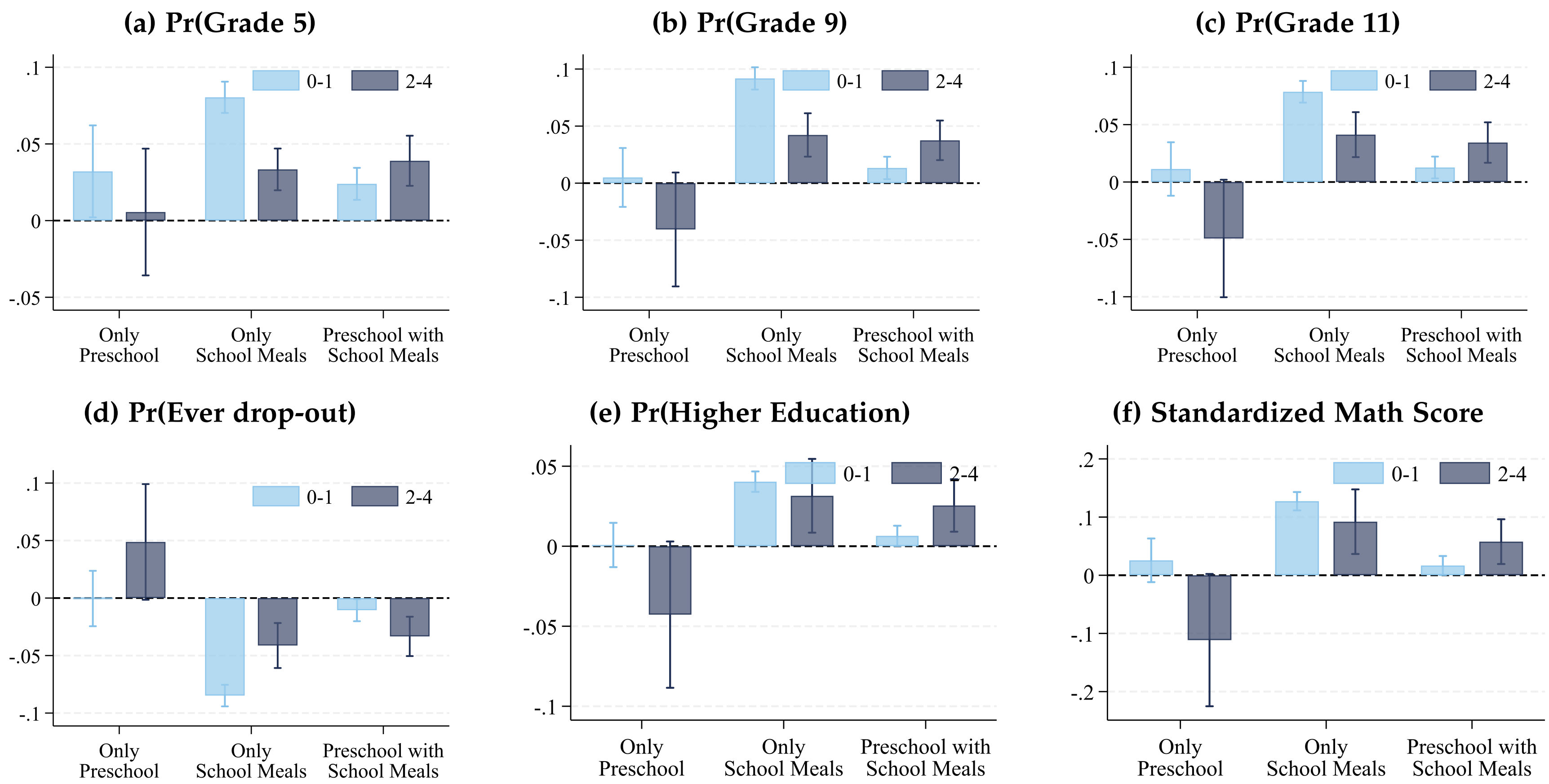
Figure C8. Interaction Effect of Preschool and School Feeding on Academic Trajectories, by student attributes





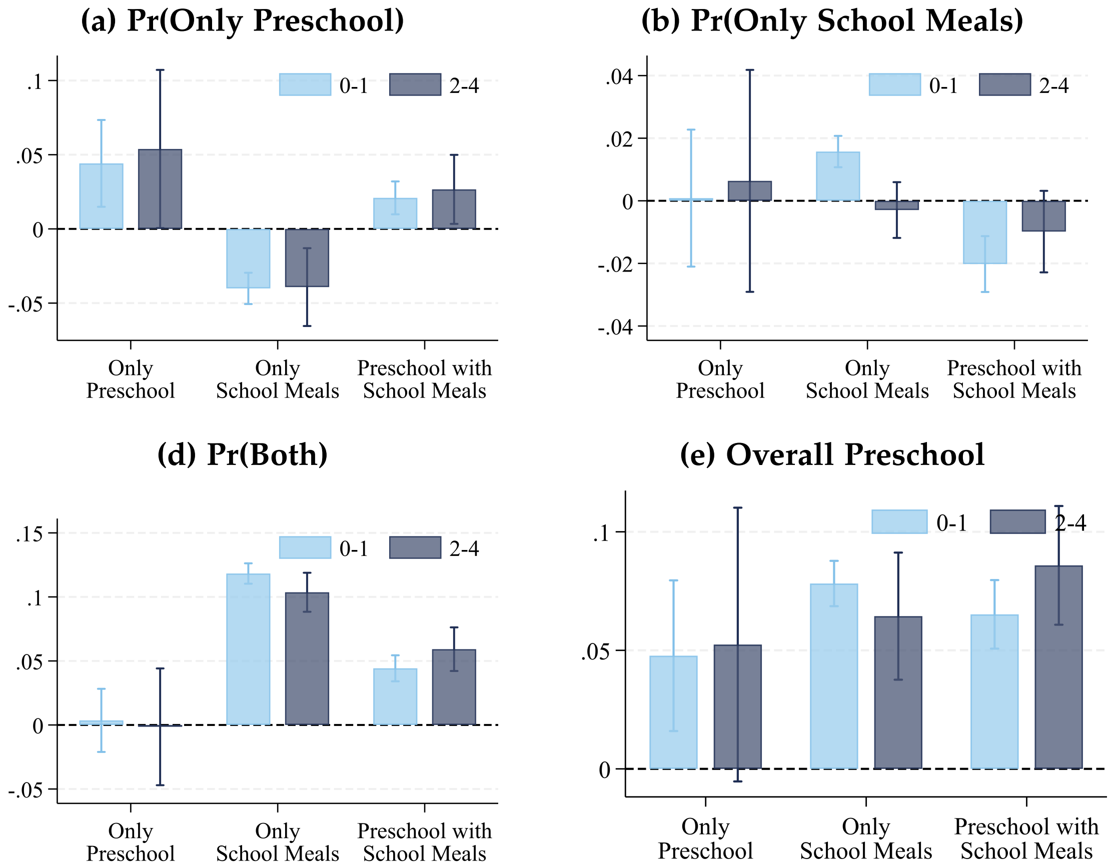
Notes: The figures present event-study estimates by subgroups. Confidence intervals at the 95% level, computed with clustered standard errors at the school level.

Figure C9. Pooled Effect of Preschool Exposure and School Feeding Exposure on Academic Outcomes, by socioeconomic level



Notes: The figures present pooled event-study estimates for relevant outcomes. Confidence intervals at the 95% level, computed with clustered standard errors at the school level. Light blue bars correspond to students in socioeconomic strata 0–1 (low-income), and dark blue bars to students in strata 2–4.

Figure C10. Pooled Effect of Preschool Exposure and School Feeding Exposure on Preschool Enrollment and School Meals Receipt, by socioeconomic level



Notes: The figures present pooled event-study estimates for relevant outcomes. Confidence intervals at the 95% level, computed with clustered standard errors at the school level. Light blue bars correspond to students in socioeconomic strata 0–1 (low-income), and dark blue bars to students in strata 2–4.