The School-Based Feeding Program and Learner Attendance: A Difference-in-Differences Study

A Basic Research

[Copy for blind review: the researchers' names, positions, and schools division are removed. The final copy lists them here.]

Conducted April to November 2026

Abstract

This basic research examined whether the School-Based Feeding Program (SBFP) changed the attendance of the learners it fed in School Year (SY) 2025–2026 in one schools division. Beneficiaries were the Grade 1 to 6 learners found severely wasted or wasted at the baseline weighing; their classmates, not fed, were the comparison group. The division's 30 public elementary schools started feeding when their funds arrived, on July 14, September 15, or October 6, 2025, and fed for 120 days. School Form 2's monthly counts for all 8,781 learners enrolled from June 2025 to March 2026, 679 of them fed, gave each learner's monthly attendance rate. Before feeding, beneficiaries attended 92.70% of school days and their classmates 94.56%. A two-way fixed effects model with learner and month effects, standard errors clustered by school, estimated that beneficiaries attended 1.22 percentage points more of their school days in feeding months, 95% CI [0.65, 1.80], about a fifth of a day a month; the Sun–Abraham estimator, which compares each start date only with never-fed learners, gave 1.52 [0.85, 2.20]. In the months after feeding ended, no difference could be shown, −0.03 [−0.69, 0.62]. The test of parallel trends before feeding found no evidence against them, $x^2(3) = 5.26$, $p = .154$, but could have missed a trend steep enough to produce the whole gain. The gain was small, was clear only while meals were served, and closed most of the gap between beneficiaries and their classmates.

Keywords: School-Based Feeding Program, attendance, difference-in-differences, event study, staggered adoption, two-way fixed effects, program evaluation

Acknowledgment

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Introduction of the Research

For SY 2025–2026, the Department of Education (DepEd) funded hot meals or nutritious food products for 3,398,541 learners, "All Kinder + (SW+ W from Grades 1 to 6)", for 120 days at PhP 25.00 a day, PhP 10,195,623,000.00 in all (Office of the Undersecretary for Operations [OUOPS], 2025, item 1). Among children 5 to 10 years old, wasting rose to 8.4% in 2023 from 6.9% in 2021 (Eva, 2025).

Whether the program brings fed learners to school more often is a question DepEd has asked before. Its feeding order of 2015 aimed "to increase classroom attendance from 85% to 100%" (DepEd, 2015, para. 2), and the order of 2017 kept that aim (DepEd, 2017b, para. 2). The order that governed SY 2025–2026 rescinded them and names enrollment, not attendance: the program is to "address hunger and encourage learners to enroll" and improve nutritional status (DepEd, 2021, para. 1). The order issued after the year ended brings attendance back: divisions shall "Improve learner participation, attendance, and academic outcomes" (DepEd, 2026b, Sec. IV), and school implementers shall "monitor daily attendance" (DepEd, 2026b, Sec. IX.D). An impact evaluation of the SBFP in SY 2013–2014 found that fed learners' attendance was already high, a median of 97.5%, and that non-beneficiaries' was comparable; it reported no estimate of the program's effect on attendance (Tabunda et al., 2016, p. 28).

The year offered a way to ask the question with records schools already keep. Feeding did not start everywhere at once: DepEd encouraged divisions to start feeding "by July 14, 2025" (OUOPS, 2025, item 2), but schools whose funds came later started later, and divisions that would start "this October 2025" had to submit catch-up plans (Office of the Undersecretary for Governance and Operations [OUGOPS], 2025, item 5). Within each school, the fed learners sat beside classmates who were not fed. School Form 2 counts every learner's days present each month. A difference-in-differences design compares the change in fed learners' attendance with the change in their classmates' over the same months, and staggered starts give each group of schools months before feeding to compare.

This study therefore followed every Grade 1 to 6 learner of the division's public elementary schools through SY 2025–2026 and asked how the attendance of the learners fed by the SBFP changed, against their classmates', during and after feeding. It addresses the Governance theme of the Basic Education Research Agenda, under Evaluation: "How effective have DepEd policies, programs, and projects been in meeting their stated objectives? What are the unintended consequences?" (DepEd, 2016, pp. 15–16).

Literature Review

The literature reviewed here follows the study's questions: what the program was in SY 2025–2026, what is known about school feeding and attendance, and how difference-in-differences estimates an effect when programs start at different times.

The School-Based Feeding Program in SY 2025–2026

The SBFP's primary beneficiaries are "all severely wasted, wasted, severely stunted, and stunted learners" (DepEd, 2021, para. 3); a wasted child is one whose body mass index (BMI)-for-age falls between the −2 and −3 z-score lines of the World Health Organization Child Growth Standards, and a severely wasted child one below −3 (DepEd, 2021, Sec. III). For FY 2025, DepEd funded all Kindergarten learners and the severely wasted and wasted learners of Grades 1 to 6 (OUOPS, 2025, item 1), so in Kindergarten every learner was fed and no comparison is possible. Schools identified beneficiaries by a baseline weighing from June 16 to July 4, 2025, to be "the basis for identifying the feeding beneficiaries" (DepEd, 2025b, Encl. No. 3, item 1). Feeding ran 120 days for hot meals or nutritious food products; milk ran 15 days for Kindergarten and the severely wasted and wasted learners of Grades 1 to 3 only (OUOPS, 2024, items 1–2).

Two instructions of October 2025 shaped the feeding days. Divisions that would start "this October 2025" had to submit catch-up plans (OUGOPS, 2025, item 5), while suppliers' payments took "about 3 months or more" (item 4). For days lost to class suspensions, divisions and schools were to deliver food packs to beneficiaries, and divisions were advised to include "class suspensions, holidays, semestral breaks and weekends in the counting of feeding days" (OUGOPS, 2025, item 7). A school's 120 feeding days could therefore include days on which the meal was eaten at home.

School Feeding and Attendance

Reviews disagree on how much feeding raises attendance. An early Cochrane review found that attendance in lower-income countries rose by "4 to 6 days a year" (Kristjansson et al., 2007, abstract), and a meta-analysis of three studies found "2.6%; 95% CI = 1.2%, 3.9%" more school days attended (Wang et al., 2021, abstract). A review of education interventions in low- and middle-income countries found a weighted average effect of school feeding on attendance of 0.09 standard deviations, 95% CI [0.03, 0.16] (Snilstveit et al., 2015, p. 89). In Bangladesh, school feeding "increased school attendance by about 1.3 days a month" (Ahmed, 2004, p. ix). The newest Cochrane review found that, in three cluster-randomized trials in low- and middle-income countries, school feeding "may have little to no effect on attendance (MD 0.17%, 95% CI -2.64% to 2.97%)", with low-certainty evidence (Kristjansson et al., 2025, abstract).

Gains were smaller in higher-income countries, possibly because children there "had more regular attendance at baseline" (Kristjansson et al., 2007, p. 21), and Philippine public school attendance is high. In the Philippine Institute for Development Studies (PIDS) evaluation of SY 2013–2014, the median attendance of verified severely wasted beneficiaries was 97.5%, and "School attendance of NB [non-beneficiary] pupils, however, is comparable" (Tabunda et al., 2016, p. 28). Late starts can also weaken the incentive: when funds came late, some schools fed twice a day to finish, which "limits the period during which the feeding serves as incentive for the beneficiary children to attend school" (Albert et al., 2015, p. 6).

Difference-in-Differences With Staggered Starts

A difference-in-differences design lets the change in a comparison group stand in for what would have happened to the treated group without the program: seasonal patterns shared by both groups are "differenced out" (Card & Krueger, 1994, p. 773). Two cautions follow. A learner's attendance in one month is correlated with the next, so standard errors must be clustered at the level at which feeding was assigned, the school; conventional errors can find an effect in "up to 45 percent of the placebo interventions" (Bertrand et al., 2004, abstract), and clustered errors themselves can be too small with few clusters (Cameron & Miller, 2015).

When units start treatment at different times, the usual two-way fixed effects estimate is a weighted average of all two-group comparisons, including comparisons of later starters with earlier ones; it is a clean average only if effects do not change over time (Goodman-Bacon, 2021). Its weights can even be negative (de Chaisemartin & D'Haultfœuille, 2020). In an event study, "the coefficient on a given lead or lag can be contaminated by effects from other periods, and apparent pretrends can arise solely from treatment effects heterogeneity" (Sun & Abraham, 2021, abstract); their interaction-weighted estimator compares each start-date group only with a clean comparison group. Callaway and Sant'Anna (2021) estimate an effect for each start group and period and then average. Finally, a test that finds no difference in trends before treatment is weak evidence: "conventional pre-trends tests may have low power" (Roth, 2022, p. 305), so a study should report how large a trend its test could miss.

Synthesis

The evidence leads to a modest expectation: a gain of one to three points of school days where attendance is low, less where it is already high, and possibly none. The SBFP's beneficiaries are selected for wasting, which may itself go with lower attendance (Omwami et al., 2011), so a comparison with other learners must rest on changes, not levels. The staggered starts of SY 2025–2026 allow such a comparison, provided the estimates use methods that respect the different start dates and say how much the test of parallel trends could miss.

Research Questions

This study sought to answer the following questions:

1. How did the monthly attendance of SBFP beneficiaries and their classmates compare before, during, and after feeding in SY 2025–2026?
2. By how much did beneficiaries' attendance change in feeding months, relative to their classmates'?
3. Did the attendance of beneficiaries and their classmates follow parallel trends before feeding began?
4. Did any change last after feeding ended, and did it differ by start date, by grade (Grades 1 to 3, who also received milk, against Grades 4 to 6), and between severely wasted and wasted learners?

Hypotheses

The following null hypotheses were tested at the .05 level of significance:

1. H01: In feeding months, the change in beneficiaries' attendance does not differ from the change in their classmates'.

2. H02: Before feeding, the attendance of beneficiaries and their classmates followed parallel trends.
3. H03: In the months after feeding ended, the change in beneficiaries' attendance does not differ from their classmates'.
4. H04: The change in feeding months does not differ between Grades 1 to 3 and Grades 4 to 6, or between severely wasted and wasted learners.

Scope and Limitation

The study covered the Grade 1 to 6 learners of the division's 30 public elementary schools enrolled in the same school from June 16, 2025 to March 31, 2026, through the school's own records: School Form 2, the SBFP forms, and the school's record of class suspensions. It did not cover Kindergarten, where every learner was fed, private schools, or secondary schools.

The study has limits. First, beneficiaries were chosen for wasting, not at random; the design removes fixed differences between them and their classmates, but not a change in attendance that would have happened to wasted learners anyway, such as recovery from an illness at the time of weighing. Second, the start dates followed the arrival of funds, which the study assumes was unrelated to attendance trends. Third, learners who moved or left during the year were not in the study, so if feeding kept some learners in school, the study does not see it. Fourth, classmates may have shared in leftover meals, which would make the estimated gain too small. Fifth, take-home packs given for suspended days could not be separated from meals at school. Sixth, with 30 schools, the clustered standard errors rest on few clusters.

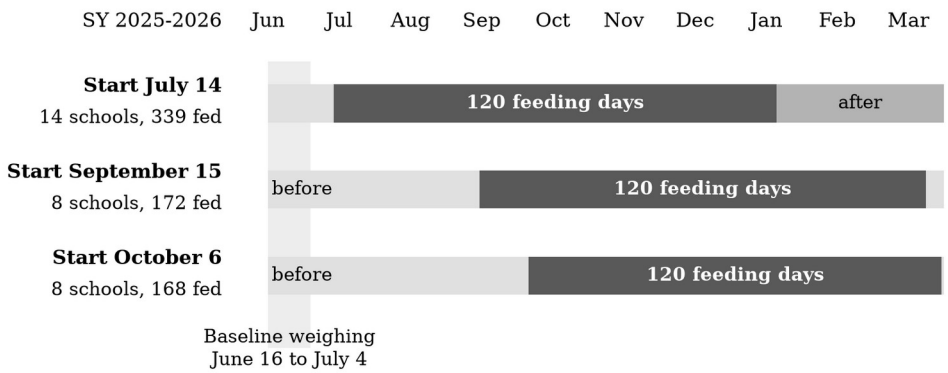
Research Methodology

Research Design

The study used a quasi-experimental difference-in-differences design with staggered adoption, on a panel of learner-months. The treated group was the Grade 1 to 6 learners found severely wasted or wasted at the baseline weighing; the comparison group was their classmates in the same schools, who were not fed in SY 2025–2026. Each school's feeding began when its funds arrived: 14 schools started on July 14, 8 on September 15, and 8 on October 6, 2025 (Figure 1). Each fed for 120 days, counting take-home packs for suspended days, so the July starters' last feeding day was January 21, 2026, the September starters' March 24, and the October starters' March 31, the last day of the year.

Figure 1

The Design of the Study on the School Year



Note. Each bar is a group of schools by start date, from June 16, 2025 to March 31, 2026. Before: months wholly before the school's first feeding month. After: months wholly after its last feeding day.

The outcome was the monthly attendance rate: the days a learner was present in a month over the class's school days that month, in percent.

Suspended days were not counted as school days, so they were not absences: DepEd counts "actual school days, considering class suspensions and holidays" (DepEd, 2024, Encl., Sec. III.8), and provides that learners who miss classes to suspensions "shall be given due consideration regarding their attendance" (DepEd, 2024, para. 3). For each school, a month was before feeding when it came wholly before the school's first feeding month, during feeding from the first to the last feeding month, and after feeding when it came wholly after the last feeding day. Only the July starters had months after feeding: February and March 2026.

A. Sampling

The study did not sample. Its population was every Grade 1 to 6 learner enrolled in the division's 30 public elementary schools from June 16, 2025 to March 31, 2026: 8,781 learners, 4,318 in the 14 July schools, 2,180 in the 8 September schools, and 2,283 in the 8 October schools. Another 268 learners enrolled in June moved or left school before March 31; their monthly records are incomplete, and they were not included. Of the 8,781 learners, 679 (7.73%) were beneficiaries: 545 wasted and 134 severely wasted; 339 in July schools, 172 in September schools, and 168 in October schools.

The number of learners was fixed by the records; what decides the precision of a difference-in-differences estimate clustered by school is the number of schools. Before the data were read, the proposal simulated the planned model 500 times for each number of schools and true effect, from planning values: monthly attendance of about 95% for non-beneficiaries and 93% for beneficiaries, about 8% of learners fed, about 18 days suspended in

a school, as the division's suspension reports showed for the first half of the year, and the division's three start dates. With 30 schools, the simulated studies detected a gain of 1 point in each feeding month 99.6% of the time and a gain of 0.5 point 59.4% of the time; the smallest gain the model could detect with 80% power, α = .05, two-sided, was 0.63 points. Twenty schools would have needed a gain of 0.79 points, and ten a gain of 1.22.

B. Data Collection

Recruitment of Participants

The study used records, not participants. The Schools Division Superintendent approved the study and the use of the division's records in April 2026. Each school replaced its learners' names and Learner Reference Numbers with codes before any record left the school. No learner, parent, or teacher was asked to do anything for the study.

Data Collection Instruments

School Form 2 abstraction sheet. For each learner and month, the researchers copied the days absent from the class's School Form 2 and the class's school days that month; days present are the school days less the days absent. School Form 2 records each school day's attendance and the month's total days absent for each learner (DepEd, 2014).

SBFP records. From each school's SBFP Form 1, the master list of beneficiaries, the researchers took each beneficiary's code and nutritional status at the baseline weighing; from SBFP Form 2, the start and end of feeding; and from SBFP Form 3, the record of daily feeding, each month's days with meals served in school (DepEd, 2021, Sec. XIV.B.1). The school's record

of class suspensions and of take-home packs completed the count of feeding days.

Derived variables. For each learner-month: the attendance rate; whether the month was before, during, or after feeding for the learner's school; for beneficiaries, the months from the school's first feeding month (0 for the first feeding month); and the share of the month's school days with meals served in school.

Validity and Reliability

The records were checked rather than validated by experts, since no scale was built. A second researcher re-entered the days present of 400 learner-months drawn at random, without seeing the first entry. The two entries agreed exactly for 96.8% of the learner-months and within one day for all of them, ICC(A,1) = .998. Disagreements were settled against the school's own School Form 2. The feeding dates on SBFP Form 2 were checked against the daily records of SBFP Form 3.

Data Collection Setting

The records were read at each school, after the end of the school year, when School Form 2 and the SBFP forms for the year were complete.

Data Collection Timeline

Table 1

Data Collection Timeline

Activity	Dates
Approval of the study and the data request	April 6 to 24, 2026
Review by the research ethics committee	April 13 to May 8, 2026
Coding of learners' names in each school	May 11 to 15, 2026
School Form 2 and SBFP abstraction in the 30 schools	May 18 to June 19, 2026

Activity	Dates
Record check (400 learner-months re-entered)	June 22 to 26, 2026
Data analysis	June 29 to August 28, 2026
Final run of the analysis	November 2026

Note. SY 2025–2026 ended on March 31, 2026 (DepEd, 2025a), so every record for the year was complete before it was read.

Data Recording and Storage

The abstraction sheets carried codes only. The key linking codes to learners stayed with each school. The data were kept on an encrypted drive; they will be destroyed three years after the study ends.

C. Ethical Issues

The study followed DepEd's Research Management Guidelines, which ask researchers to ensure "confidentiality in handling data of respondents" (DepEd, 2017a, Sec. VI.C), and the Data Privacy Act of 2012. A learner's nutritional status and attendance are both sensitive personal information under the Act, being "About an individual's health, education" (Data Privacy Act of 2012, 2012, Sec. 3[l][2]). Personal information processed for research purposes is outside the Act only to the minimum extent necessary, "intended for a public benefit, subject to the requirements of applicable laws, regulations, or ethical standards" (National Privacy Commission [NPC], 2016, Sec. 5[c]), and the National Privacy Commission's guidelines on consent provide that research "where the end results will be anonymized and will only disclose the general demographic of the research subjects does not require the consent of the data subject" (NPC, 2023, Sec. 16[C][2]). The study used coded records, kept to the variables it needed; its protocol was reviewed by a research ethics committee; no result names a learner or a school; and no

record was used to decide anything about a learner. The researchers declared their use of an artificial intelligence (AI) tool in Appendix D (DepEd, 2026a).

D. Plan for Data Analysis

The first question was answered by the mean monthly attendance of beneficiaries and their classmates in each group of schools, and by the 2 × 2 difference-in-differences of each group by hand: the change in beneficiaries' mean attendance from the months before feeding to the feeding months, less the same change for their classmates. The second was answered by a two-way fixed effects regression of the monthly attendance rate on indicators for feeding months and for months after feeding, with learner effects, which remove each learner's usual attendance, and month effects, which remove what every learner shared that month, such as the rainy season (Card & Krueger, 1994). Standard errors were clustered by school; with 30 schools, p-values and intervals used a t distribution with 29 degrees of freedom, and a restricted wild cluster bootstrap with 9,999 draws checked the main p-value (Cameron & Miller, 2015). Three checks followed: school-by-month effects in place of month effects, which remove anything that struck a whole school in a month, such as a suspension; the share of the month's school days fed in place of the indicator; and the model without June, the month of the baseline weighing. The third question was answered by an event study: one indicator for each month from the school's first feeding month, the month before left out, and a Wald test that the three months before it were jointly zero. Because a test that finds nothing may lack power (Roth, 2022), the study computed the steady trend before feeding that the test would miss half the time and the

bias it would add after feeding began. Because starts were staggered, the event study was repeated with the Sun–Abraham estimator, which compares each start-date group only with learners never fed (Sun & Abraham, 2021), and averaged over the feeding months and the months after. The fourth question was answered by these averages for each start date and for the months after feeding, and by adding to the main model an interaction of feeding months with Grades 1 to 3 and with severe wasting. The final run of the analysis, in November 2026, used Python 3.13 with linearmodels 7.1 (Sheppard, 2026), checked against pyfixest 0.70.0; the workbook recomputes every statistic Excel can.

Discussion of Results and Recommendations

Who Was Fed, and When

DO 012, s. 2025 counts 197 school days in SY 2025–2026; its weekdays less the breaks and holidays it lists number 188 (Appendix C). The 30 schools lost 13 to 23 of these days each to class suspensions, 17.6 on average, most of them from July to November, close to the "about 20 teaching days" a typical public elementary school lost to suspensions in SY 2025–2026 (Piosang et al., 2026, p. 1); their learners' classes met on 165 to 175 school days. Table 2 gives each group of schools. The July schools served meals in school on 105.4 days on average and gave take-home packs for 14.6 suspended days; the October schools, starting late, served 102.3 days in school and gave packs for 17.8 days, part of them for break days, to reach 120.

Table 2

Schools, Learners, and Feeding Days by Start Date

Start date	Schools	Learners	Fed learners	Last feeding day	Feeding days in school, mean	Take-home pack days, mean
July 14, 2025	14	4,318	339	January 21, 2026	105.4	14.6
September 15, 2025	8	2,180	172	March 24, 2026	111.3	8.8
October 6, 2025	8	2,283	168	March 31, 2026	102.3	17.8
All	30	8,781	679			

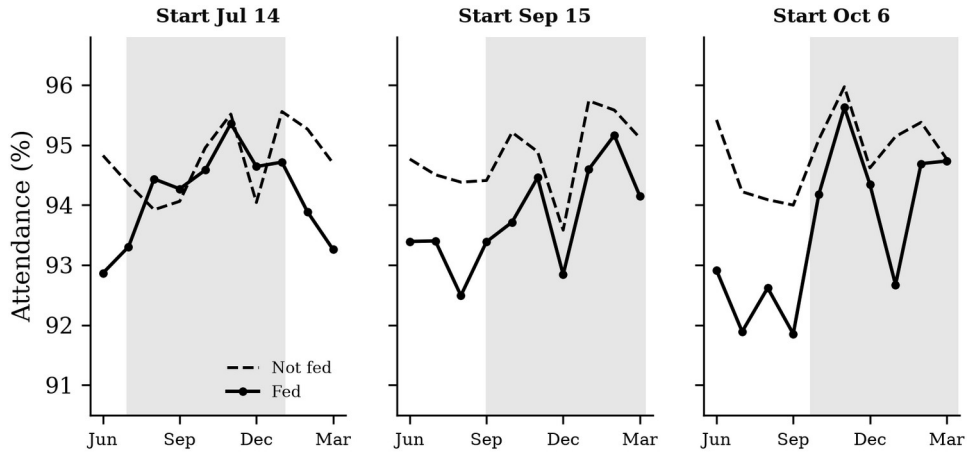
Note. A school's 120 feeding days count meals served in school and take-home packs for suspended days and, for late starters, break days (OUGOPS, 2025, item 7).

Attendance Before, During, and After Feeding

Attendance was high. Learners attended 94.71% of their school days on average over the year; one in thirteen (7.9%) attended less than 90%. Before feeding, beneficiaries attended 92.70% of school days and their classmates 94.56%, a gap of 1.85 points. In every group of schools the gap narrowed once meals began (Figure 2). Table 3 gives the 2 × 2 comparison for each start date: beneficiaries' attendance rose by 0.95 to 2.05 points from the months before feeding to the feeding months, against changes of −0.20 to 0.73 points among their classmates. Weighted by the number of fed learners, the difference-in-differences was 1.37 points.

Figure 2

Mean Monthly Attendance of Fed Learners and Their Classmates, by Start Date



Note. Solid line: fed learners, severely wasted or wasted at the baseline weighing; dashed line: their classmates. The shaded months are each group's feeding months.

Table 3

Mean Attendance Before and During Feeding, and the Difference-in-Differences, by Start Date

Months before	Fed: before	Fed: during	Classmates: before	Classmates: during	Difference-in-differences (points)
June	92.87%	94.47%	94.83%	94.63%	1.80
June to August	93.10%	94.04%	94.55%	94.93%	0.57
June to September	92.32%	94.37%	94.43%	95.16%	1.32
					1.37

Note. Means of the learners' monthly attendance rates. During: from the school's first feeding month to its last.

The Difference-in-Differences Estimate

The two-way fixed effects model estimated that in feeding months, beneficiaries attended 1.22 percentage points more of their school days than their usual attendance and the month would predict from their classmates' change, 95% CI [0.65, 1.80], $t(29) = 4.38$, $p < .001$ (Table 4). The wild cluster

bootstrap agreed, p < .001. The first null hypothesis is rejected. With 17.4 school days in an average feeding month, 1.22 points is about a fifth of a day a month, and about one and a half days over the 117.7 school days of a beneficiary's feeding months. The three checks gave 0.95 to 1.35 points, all with intervals above zero (Figure 3). Without June, the month of the baseline weighing, the estimate fell to 0.95 [0.25, 1.65]: the July schools' only month before feeding was June, so their gain then rested on fewer comparisons.

Table 4

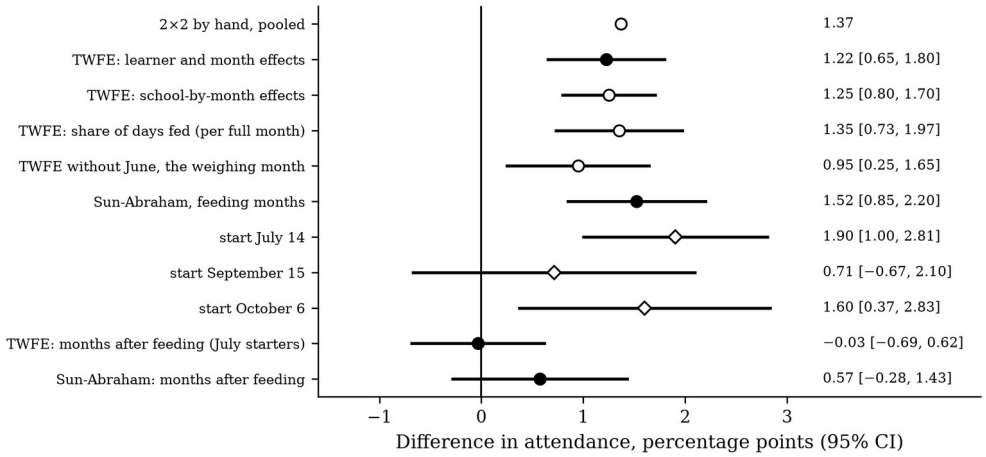
Two-Way Fixed Effects Estimates of the Change in Attendance

Model	Feeding months, points [95% CI]	*p*	Months after feeding, points [95% CI]	*p*
Learner and month effects (main)	1.22 [0.65, 1.80]	< .001	−0.03 [−0.69, 0.62]	.917
Learner and school-by-month effects	1.25 [0.80, 1.70]	< .001	0.18 [−0.37, 0.73]	.503
Share of the month's school days fed (a full month)	1.35 [0.73, 1.97]	< .001	−0.05 [−0.65, 0.56]	.874
Without June, the weighing month	0.95 [0.25, 1.65]	.009	−0.40 [−1.16, 0.37]	.296

Note. 87,810 learner-months of 8,781 learners. Standard errors clustered by 30 schools; *p* and intervals from a *t* distribution with 29 degrees of freedom.

Figure 3

The Estimates Side by Side



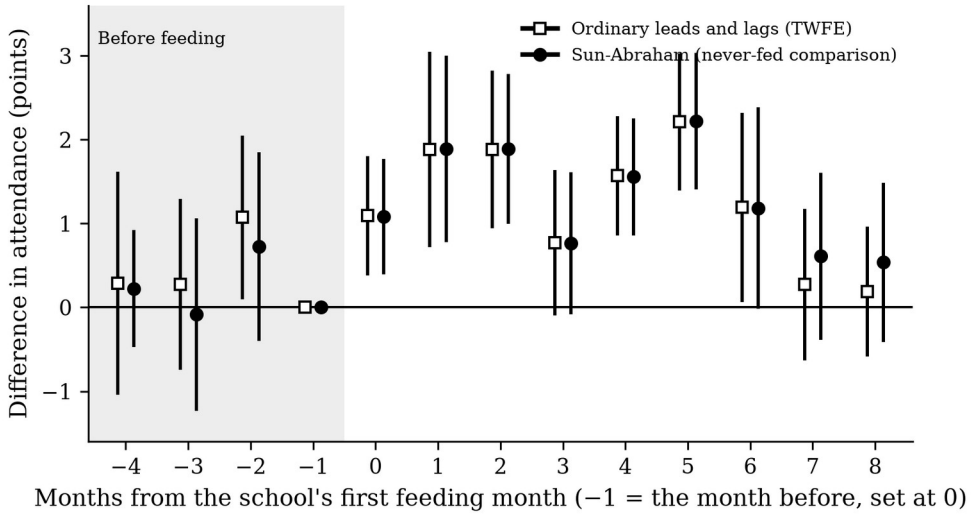
Note. Points and 95% intervals in percentage points of school days. Filled circles: the main estimates and the months after feeding; open circles: the checks; open diamonds: each start date's Sun–Abraham estimate.

Did Attendance Run Parallel Before Feeding?

The event study shows the months before and after each school's first feeding month (Figure 4). In the ordinary event study, the three months before were 0.29, 0.28, and 1.07 points; the last, two months before feeding, had an interval above zero, [0.12, 2.03], but together the three did not differ from zero, $x^2(3) = 5.26$, $p = .154$. In the Sun–Abraham estimates they were 0.22, −0.08, and 0.73 [−0.38, 1.83], $x^2(5) = 2.56$, $p = .768$. The second null hypothesis is retained.

Figure 4

The Event Study: Months From the First Feeding Month



Note. Squares: the ordinary leads and lags with learner and month effects. Circles: the Sun–Abraham estimates, each start-date group against never-fed learners. The month before feeding is the reference, set at 0. Months 7 and 8 are February and March 2026, after feeding, for the July schools only.

Retaining it does not prove the trends parallel. The test had little power: a steady trend of 0.42 points a month in beneficiaries' attendance before feeding would have gone undetected half the time, and two months after feeding began, such a trend alone would have added 1.25 points, about the whole estimated gain. A trend of 0.57 points a month would have been missed one time in five. Nothing in the records points to such a trend, but the test cannot rule it out.

The Sun–Abraham estimate over the feeding months was 1.52 points, 95% CI [0.85, 2.20], $p < .001$, a little larger than the two-way fixed effects estimate. The estimates rose from 1.08 points in the first feeding month, a partial month, to 1.89 in the second and third, and stayed between 0.77 and 2.22 points through the seventh.

After Feeding, and by Group

In the July schools' two months after feeding ended, February and March 2026, the difference fell back: −0.03 points [−0.69, 0.62] in the main model and 0.57 [−0.28, 1.43] in the Sun–Abraham estimate. The third null hypothesis is retained: no lasting gain could be shown, though two months of one group of schools could miss a small one.

The gain differed by start date more in its estimates than its intervals: 1.90 points [1.00, 2.81] in the July schools, 0.71 [−0.67, 2.10] in the September schools, and 1.60 [0.37, 2.83] in the October schools (Table 5). Grades 1 to 3, who also received milk, did not gain significantly more than Grades 4 to 6, 0.15 points [−0.87, 1.16], p = .768, and severely wasted learners did not differ from wasted learners, −0.13 [−1.22, 0.96], p = .812. The fourth null hypothesis is retained.

Table 5

The Change in Attendance by Start Date, After Feeding, and by Group

Estimate	Points [95% CI]	*p*
Sun–Abraham, feeding months, all schools	1.52 [0.85, 2.20]	< .001
Sun–Abraham, feeding months, July 14 schools	1.90 [1.00, 2.81]	< .001
Sun–Abraham, feeding months, September 15 schools	0.71 [−0.67, 2.10]	.300
Sun–Abraham, feeding months, October 6 schools	1.60 [0.37, 2.83]	.012
Sun–Abraham, months after feeding (July schools)	0.57 [−0.28, 1.43]	.180
Difference, Grades 1 to 3 against 4 to 6	0.15 [−0.87, 1.16]	.768
Difference, severely wasted against wasted	−0.13 [−1.22, 0.96]	.812

Note. The Sun–Abraham averages weight each start-date group by its fed learners. The two differences are interactions added to the main model.

Discussion

The fed learners of this division attended about 1.2 to 1.5 points more of their school days while meals were served, relative to their classmates, and

the gain closed most of the 1.85-point gap with which they began the year. The size sits where the reviews would lead one to expect for learners who already attend most days: below the 2.6 points of the meta-analysis of Wang et al. (2021) and within the wide interval of the newest Cochrane review, which found little to no effect on average (Kristjansson et al., 2025). Attendance near 95% leaves little room to rise, the reason Kristjansson et al. (2007, p. 21) suggest for smaller gains where attendance was already regular.

The gain was clear only while feeding lasted. In the July schools, no gain could be shown in the two months after their last feeding day (−0.03 points in the main model; 0.57, with an interval across zero, in the Sun–Abraham estimate), as a meal that draws a learner to school would be expected to stop drawing once it stops. That is consistent with feeding as a reason to come on the day, the incentive the PIDS process evaluation feared late starts would shorten (Albert et al., 2015, p. 6), more than with a change in health that would outlast the meals; but the records cannot tell these apart, and two months of one group of schools are too few to rule out a small lasting gain.

The estimate is not proof that feeding caused the gain. Beneficiaries were chosen for wasting, and the pre-feeding test, though it found no difference in trends, could have missed one large enough to produce the gain. The month of the baseline weighing mattered too: without June the estimate fell to 0.95 points, still above zero. A learner who was ill at the weighing could be both wasted and absent then, and recover after; the event study's months before feeding show no such dip, but the July schools had only June before

feeding to show it. The Sun–Abraham estimates, free of comparisons between early and late starters, agreed with the two-way fixed effects estimates, as they should when most learners are never fed and so make up most of the comparison. The study's clustered standard error, 0.28, was larger than the 0.22 the proposal's simulation expected for 30 schools, a reminder that one study's error can differ from the planned one; the estimate was still well clear of zero.

Recommendations

1. **Report the attendance of beneficiaries, month by month.** DepEd's current guidelines ask school implementers to "monitor daily attendance" and to "Work with class advisers to provide meals to learners with habitual absences" (DepEd, 2026b, Sec. IX.D). School Form 2 already holds what is needed; a school can report each month's attendance of its beneficiaries beside that of its other learners.
2. **Start feeding on time.** The gain was seen in feeding months only, and late starts shorten the months in which meals are served in school. The division should track when each school's funds arrive and its first feeding day, and help schools that start late.
3. **Plan for the months after feeding.** In the July schools no gain could be shown in February and March. Schools whose feeding ends before the school year does should watch their former beneficiaries' attendance in the last months.

4. **Keep feeding and take-home days apart in the records.** SBFP Form 3 should show which feeding days were meals in school and which were packs taken home, so that later studies can tell their effects apart.
5. **Study it further.** A study across several divisions, with more schools and more start dates, would give the robust estimators more comparisons; recording each beneficiary's daily presence at meals would show how much of the gain comes on the days meals are served; and a sensitivity analysis would say how large a departure from parallel trends the conclusion can bear (Rambachan & Roth, 2023).

Dissemination and Advocacy Plans

The findings will be shared with those who can act on them, in the forms each can use (Table 6).

Table 6

Dissemination and Advocacy Plan

Audience	Purpose	Channel	Timeline
School heads and SBFP coordinators	Report beneficiaries' monthly attendance; plan for the months after feeding	Division school heads' conference; a two-page policy brief	November 2026
Division school health and nutrition staff	Track start dates and separate take-home days in the records	Division management committee meeting	December 2026
Class advisers	Know what the attendance records can show	Learning action cell sessions in each school	January to February 2027
Researchers in the region	Share the method and the findings	Regional research congress; the division's research journal	March to June 2027

Note. The policy brief was paid from the grant's dissemination line (Financial Report).

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Financial Report

The study received a division-level Basic Education Research Fund grant of PhP 15,000.00, released in tranches: 40% (PhP 6,000.00) on the submission of the work plan and the instruments and 40% (PhP 6,000.00) on the submission of the data collection and initial findings; the last 20% (PhP 3,000.00) is released on the acceptance of this report (DepEd, 2017a). Expenses totaled PhP 14,600.00, PhP 400.00 less than the grant. The analysis used free software, and the record check was done by the researchers.

Table 7

Statement of Expenses

Date	Particulars	Receipt or document	Amount (PhP)	Category
2026-05-12	Printing of the abstraction sheets (700 pages)	OR 2217	1,400.00	Reproduction
2026-05-18 to 2026-06-19	Transportation to the 30 schools for the abstraction (40 trips)	Itinerary and receipts	4,000.00	Transportation
2026-05-18 to 2026-06-19	Snacks for the abstraction days (4 persons, 20 days)	OR 3391	2,400.00	Food
2026-05-01 to 2026-09-30	Mobile data for the three researchers (five months)	Receipts	4,500.00	Communication
2026-05-12	Folders, envelopes, and an encrypted flash drive	OR 1046	1,100.00	Supplies
2026-11-09	Printing of the policy brief (60 copies)	OR 7158	1,200.00	Dissemination
	Total		14,600.00	

Note. Summary by category: communication, PhP 4,500.00; transportation, PhP 4,000.00; food, PhP 2,400.00; reproduction, PhP 1,400.00; dissemination, PhP 1,200.00; supplies, PhP 1,100.00.

Appendices

Appendix A. The Variables and Their Sources

Learner code (assigned by the school); school, grade, and sex (School Form 1); nutritional status at the baseline weighing, June 16 to July 4, 2025, and beneficiary status (SBFP Form 1); days absent and the class's school days in each month, June 2025 to March 2026 (School Form 2); each school's first and last feeding days (SBFP Form 2), its days with meals served in school by month (SBFP Form 3), and its suspended days and take-home packs (the school's records). Derived: the monthly attendance rate (days present over school days); each month's phase for the learner's school (before, during, or after feeding); the months from the school's first feeding month; and the share of the month's school days with meals served in school.

Appendix B. The Abstraction Sheet

For each learner of the class, by code: the days absent in each month from June 2025 to March 2026, counted from the class's School Form 2, and the class's school days that month; for the class, the days of the month with class suspensions. For the school: the start and end of feeding from SBFP Form 2, and for each month, the days with meals served in school from SBFP Form 3 and the take-home pack days. The adviser of the class was asked only to explain an entry that could not be read.

Appendix C. The Months of SY 2025–2026 and Their Class Days

DO 012, s. 2025 counts 197 school days (Encl. No. 2: June 11, July 23, August 20, September 22, October 23, November 21, December 14, January 21, February 19, March 23), the mid-year break week included. The weekdays

less the breaks and holidays it lists, the days classes could meet, are June (from June 16) 11, July 23, August 19, September 22, October 18 (the mid-year break, October 27 to 31, left out), November 20, December 14 (the year-end break from December 20 left out), January 20 (classes resumed January 5), February 19, and March 22, 188 in all; each class's school days are these less its suspended days (DepEd, 2025a). The workbook's Schools sheet lists every school's school days and feeding days by month.

Appendix D. Declaration of AI Use

We, the researchers, declare that we used [the name of the AI tool, as the researchers would give it] only to check the grammar of the English text of this report, for the purpose of finalizing the research study. The prompt used was: "Check the grammar of this paragraph. Do not change its meaning or add content." We have reviewed, edited, and understood all the content, and we take full accountability for it. No codes, records, or other data from the study were entered into the AI tool.