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Strengthening Fourth-Grade IPAS Learning Outcomes through Cooperative Learning: A Two-Cycle Classroom Action Research in Indonesia

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ABSTRACT

Teacher-centred instruction can constrain participation in elementary integrated science and social studies (IPAS), particularly when students need to connect scientific concepts with everyday experience. This study examined whether an iteratively refined cooperative-learning intervention was associated with improved learning outcomes among fourth-grade students at SDN 17 Tilamuta, Boalemo Regency, Indonesia. A two-cycle classroom action research design was implemented in November–December 2025. Fourteen students with complete repeated records participated in a pre-action assessment, Cycle I, and Cycle II. Achievement was measured using a five-item written test on energy in daily life, supported by classroom observations of participation and group work. To strengthen the original thesis analysis, the individual score data were re-analysed for this journal manuscript using descriptive statistics, mastery proportions, normalized gain, a Friedman repeated-measures test, and Wilcoxon signed-rank comparisons. Mean scores increased from 52.86 (pre-action) to 74.29 (Cycle I) and 90.00 (Cycle II), while mastery at the school criterion of 75 rose from 28.57% to 57.14% and 92.86%. The repeated-measures difference was statistically detectable, $\chi^2(2)=26.46$, $p<.001$, Kendall's $W=.95$, with a class-level normalized gain of .79. The findings indicate a strong within-class improvement after iterative cooperative learning. Because the study used one small class without a comparison group and a brief five-item test, the results should be interpreted as action-research evidence rather than a causal effect estimate.

Keywords: classroom action research; cooperative learning; elementary education; IPAS; learning outcomes; science education

1. INTRODUCTION

Elementary science learning is most productive when learners do more than receive explanations. They need opportunities to articulate ideas, compare interpretations, test reasoning against peers, and link abstract concepts with observable events. This requirement is especially relevant to Indonesian IPAS (Ilmu Pengetahuan Alam dan Sosial), an integrated subject that asks elementary students to understand natural phenomena while recognizing their relationship with social life and the surrounding environment. In the Grade IV classroom examined in this study, the practical problem was not the absence of instruction but the limited learning outcomes that accompanied a predominantly teacher-centred pattern. The source thesis reported that students often depended on verbal explanation and that many had not reached the school mastery criterion before the intervention. The instructional challenge was therefore to reorganize classroom participation so that students had structured opportunities to learn with and from one another rather than remaining mainly recipients of teacher talk.

Cooperative learning provides a theoretically relevant response to that problem. It is not simply the physical arrangement of students into groups. High-quality cooperative learning structures positive interdependence, individual responsibility, promotive interaction, and opportunities for students to explain, question, and support one another. Social interdependence theory proposes that the way goals are structured changes the way learners interact and, consequently, the cognitive and social outcomes of learning (Johnson & Johnson, 2009). Slavin (2014) similarly argues that group work is most likely to improve achievement when group goals are combined with individual accountability. From this perspective, the expected academic benefit does not arise from group seating alone; it arises because students are encouraged to process content actively, elaborate explanations, detect misunderstandings, and remain responsible for learning outcomes.

A substantial research base supports this active and social view of learning. Kyndt et al. (2013), in a meta-analysis of face-to-face cooperative learning, reported positive effects on achievement and attitudes across educational levels, although effects varied by domain, age, and cultural context. Gillies (2016) concluded that cooperative learning can strengthen academic learning and social interaction when teachers deliberately teach collaborative processes and facilitate productive dialogue. More broadly, active-learning evidence in science, technology, engineering, and mathematics indicates that learner participation is associated with stronger achievement than lecture-dominant instruction (Freeman et al., 2014), and well-designed active environments can also reduce some achievement disparities (Theobald et al., 2020). These findings suggest that the instructional mechanism is plausible, but they do not imply that every cooperative lesson automatically produces the same effect.

Evidence from Indonesian and elementary-school contexts is also encouraging. Utami et al. (2021) synthesized Indonesian cooperative-learning studies and found an overall positive effect on biology learning. Rizki et al. (2022) reported beneficial effects of cooperative-learning models on physics and junior-high science outcomes. At the elementary level, Wibawa (2023) found stronger science outcomes under cooperative learning than conventional instruction, while Nurtianingsih et al. (2023) reported gains associated with Teams-Games-Tournament and Numbered Heads Together approaches. Rismah and Efendi (2023) likewise documented a large effect in a fourth-grade science study. More recent elementary work has connected cooperative formats with scientific literacy (Saputri & Sukmawati, 2024) and science learning outcomes through Numbered Heads Together (Nur et al., 2025). The convergence of these studies supports cooperative learning as a promising family of approaches, but local implementation still matters because elementary classes vary in size, prior achievement, teacher routines, and students' experience with collaborative work.

The present study contributes a small but useful form of practice-based evidence: classroom action research in a Grade IV IPAS class where the teacher-researcher iteratively modified instruction across two cycles. Unlike a conventional one-shot pretest–posttest study, classroom action research uses evidence from one cycle to refine

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the next. This matters pedagogically because the first implementation may reveal participation problems that are not visible at the planning stage. In the source study, Cycle I established group-based learning and structured presentation of the energy topic, while the Cycle II lesson provided more explicit opportunities for students to explain their understanding, discuss ideas with classmates, and reflect on the lesson. The sequence therefore allows the analysis to examine not only whether outcomes changed from baseline, but also whether further improvement followed the second round of instructional refinement.

The original thesis primarily reported percentages of students reaching mastery. That evidence is meaningful for school decision making, yet it leaves several analytical questions unanswered: How large was the change in mean performance? Was the score distribution consistently higher across the three measurement occasions? How strong was the overall repeated-measures effect? How should the improvement be interpreted given the small class, discrete test scores, and absence of a control group? The journal conversion addresses these questions by preserving the original student-level scores while adding descriptive and nonparametric repeated-measures analyses appropriate to the available dataset. This re-analysis is an enhancement of reporting, not a retroactive change to the original intervention.

Accordingly, this study aimed to evaluate the change in Grade IV IPAS learning outcomes across the pre-action stage, Cycle I, and Cycle II after implementing cooperative learning at SDN 17 Tilamuta. The study addressed two questions: (1) How did mean achievement and mastery proportions change across the three measurement occasions? and (2) Did the repeated student scores show a statistically detectable within-class shift across the action-research cycles? The practical objective was to determine whether iterative cooperative learning provided sufficiently strong classroom evidence to justify its continued use and refinement in the local IPAS context.

2. LITERATURE REVIEW

2.1 Cooperative Learning as Structured Interdependence

Cooperative learning is best understood as an instructional architecture rather than a single technique. Different variants—such as Student Teams Achievement Divisions, Jigsaw, Think–Pair–Share, Teams-Games-Tournament, and Numbered Heads Together—use different task structures, but effective forms share an expectation that students' success is connected while individual learning still matters. Social interdependence theory distinguishes positive interdependence, in which learners perceive that they can reach their goals when peers also reach theirs, from competitive or individualistic goal structures (Johnson & Johnson, 2009). Positive interdependence can encourage helping, explanation, and mutual monitoring, but the academic value of these interactions depends on task quality and accountability.

Slavin (2014) emphasizes two conditions that are especially important for achievement: group goals and individual accountability. Without accountability, stronger students may complete the task while others remain passive; without a meaningful group goal, students may have little reason to support peers. This distinction is important for interpreting classroom action research. An observed score increase should not be attributed to the label 'cooperative learning' alone. The more defensible interpretation considers whether the classroom created opportunities for peer explanation, participation, and responsibility, and whether those opportunities became better organized across cycles.

Gillies (2016) further shows that teachers remain central in cooperative environments. The teacher's role changes from being the sole source of information to designing tasks, modelling collaborative behaviors, monitoring group processes, asking questions that extend reasoning, and intervening when participation is unequal. This perspective fits the intervention examined here: the source lesson plans retained teacher explanation and PowerPoint support but inserted group organization, peer discussion, opportunities to present understanding, and reflection. Cooperative learning therefore complemented rather than eliminated direct teacher guidance.

2.2 Cooperative Learning, Active Processing, and Achievement

The achievement mechanism of cooperative learning can be explained through active cognitive processing. Explaining an idea to a peer requires a learner to retrieve information, organize it, select relevant evidence, and notice gaps in understanding. Hearing alternative explanations can expose misconceptions that may remain hidden during passive listening. Questions from peers create immediate diagnostic information, while group tasks can increase the amount of time students spend manipulating ideas. These processes are compatible with broader evidence on active learning. Freeman et al. (2014) found that active learning improved performance in STEM courses and reduced failure relative to traditional lecturing. Although that meta-analysis focused primarily on higher education, the underlying principle—students learn more effectively when they actively work with disciplinary ideas—also informs elementary cooperative learning.

Cooperative learning may also influence motivation and persistence. Students who feel that their contribution matters can experience a stronger sense of competence and relatedness, two needs emphasized in self-determination theory (Ryan & Deci, 2020). However, motivation should not be assumed from group work. A poorly structured group can produce social loafing, domination by a high-achieving student, or off-task interaction. The pedagogical implication is that collaboration needs clear goals, manageable tasks, and teacher monitoring. The Cycle II refinements in the present study—more opportunities to state understanding, exchange ideas, and reflect—are consistent with this need for deliberate structure.

Meta-analytic evidence indicates that the effects of cooperative learning are generally positive but heterogeneous. Kyndt et al. (2013) identified achievement gains across a large set of classroom studies, with moderators including domain and age. In Indonesia, Utami et al. (2021) found an overall beneficial effect in biology learning, and Rizki et al. (2022) reported positive results across science and physics studies. Such heterogeneity is relevant to the current study because a very small Grade IV class in Boalemo cannot be treated as a miniature version of every cooperative-learning context. The value of classroom action research lies instead in testing whether theoretically plausible principles can be adapted to a particular class and whether the resulting evidence is strong enough to guide local practice.

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2.3 Cooperative Learning in Elementary Science and IPAS

Elementary science and IPAS demand both conceptual understanding and the ability to connect ideas with everyday phenomena. The intervention topic, energy in daily life, includes forms of energy, energy transformation, alternative energy, and energy conservation. These ideas are accessible to Grade IV learners because examples are visible in familiar objects—lamps, fans, bicycles, gas stoves, batteries, and sunlight—but students still need to reason about transformations that cannot always be directly observed. Cooperative dialogue can make this reasoning public. When one student explains why a flashlight represents chemical-to-light energy conversion, peers can challenge, extend, or correct that explanation, thereby turning an answer into a learning event.

Recent elementary evidence supports this logic. Wibawa (2023) reported higher science learning outcomes under cooperative learning compared with conventional instruction. Nurtianingsih et al. (2023) found that TGT and NHT variants improved elementary science outcomes, suggesting that structured peer interaction can work across different cooperative formats. Rismah and Efendi (2023) reported a large effect in fourth-grade science learning, a context particularly close to the present Grade IV study. Saputri and Sukmawati (2024) extended the focus from test achievement to scientific literacy in Grade V using TGT supported by Wordwall, showing that cooperative structures can be combined with media when the media serves a clear learning purpose. Nur et al. (2025) also found improved Grade IV science outcomes with NHT. Together, these studies indicate that cooperative learning is not tied to one specific technique, but implementation quality and alignment with content remain important.

The novelty of the present study is not a claim that cooperative learning itself is new. Rather, the contribution lies in three features: an integrated IPAS setting in a small Indonesian elementary school; an iterative two-cycle action-research process in which the second lesson was refined after Cycle I; and a transparent re-analysis of the original matched student scores using repeated-measures statistics. This combination strengthens the evidentiary value of a practice-based thesis while preserving the limits of what a single-class action research design can establish.

2.4 Classroom Action Research as Iterative Improvement

Classroom action research adds a design logic that is different from conventional effectiveness research. Its central question is not only whether an intervention works on average, but what a practitioner learns from implementing, observing, and revising instruction in a real classroom. Kemmis et al. (2014) characterize action research as a recursive process in which action and reflection are connected. The quality of an action-research study therefore depends on whether evidence from one cycle genuinely informs the next. A study that repeats identical activities across cycles without explaining what was learned from the first cycle would provide weak evidence of reflective improvement. In the present study, the Cycle I reflection identified incomplete student participation and incomplete mastery, while Cycle II increased opportunities for students to articulate understanding and exchange ideas. This provides a defensible cycle-to-cycle rationale.

Action research also changes the meaning of validity. Internal validity in a randomized experiment is primarily concerned with estimating an intervention effect against a counterfactual. In classroom action research, a second concern is practical validity: whether the evidence is sufficiently credible and specific to support the next instructional decision in that class. The present study has limited causal identification but strong temporal and practical alignment. The same learners were followed across three occasions, the target content remained coherent, and the second instructional cycle responded directly to evidence from the first. These features make the findings useful for local improvement even while they remain insufficient for broad causal generalization.

A rigorous journal report can strengthen action research by making its decision rules explicit. The original study used the school mastery criterion as a practical benchmark: if too few students reached the criterion after Cycle I, the instructional process required further refinement. This criterion is meaningful because it links research evidence with an actionable classroom threshold. However, a single percentage can hide important information. A rise in mastery might occur even if many students remain unchanged, or it might be driven by only a few students crossing the threshold. For this reason, the present manuscript supplements mastery percentages with matched score trajectories, central tendency, dispersion, normalized gain, and repeated-measures tests.

The combination of reflective cycles and transparent quantitative re-analysis creates a stronger bridge between practitioner inquiry and publishable research. The action-research cycle explains why instruction changed; the student-level analysis demonstrates how consistently outcomes changed. Neither component is sufficient alone. Statistical analysis without cycle reflection would disconnect the results from the improvement process, while reflection without transparent data would make the claimed improvement difficult to evaluate. The current article therefore treats the two forms of evidence as complementary rather than competing.

3. RESEARCH METHOD

3.1 Research Design and Setting

The study used classroom action research with the cyclical logic of planning, action, observation, and reflection associated with the Kemmis–McTaggart tradition (Kemmis et al., 2014). Classroom action research was appropriate because the objective was to improve an identified instructional problem in an existing class rather than to estimate a population-level treatment effect through randomized assignment. The researcher worked within the natural classroom, implemented an instructional change, observed its consequences, reflected on weaknesses, and used that evidence to refine the next cycle. The study was conducted at SDN 17 Tilamuta, Lahumbo Village, Tilamuta District, Boalemo Regency, Gorontalo Province, Indonesia, during November and December 2025.

The action-research sequence contained a pre-action assessment followed by two instructional cycles. Each cycle included one 60-minute classroom meeting. The topic was Grade IV IPAS, specifically 'Energy in Daily Life.' The original study used a classroom module, presentation slides, a student worksheet, written tests, and observation sheets. The researcher functioned as the classroom instructor during the intervention and also

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collected observation information. This dual role is common in practitioner research but is treated as a methodological limitation because it can influence both implementation and observation.

3.2 Participants and Analytic Sample

The analytic dataset contains 14 Grade IV students with matched scores at all three measurement occasions. The source thesis contains one narrative statement in the method chapter indicating 13 participants, but the pre-action, Cycle I, and Cycle II results tables each list 14 students and provide complete repeated scores for those same 14 records. Because the journal analysis depends on matched outcomes and the result tables provide the verifiable raw dataset, N=14 was used as the analytic sample. Gender composition was not used in the journal analysis because the source document contains inconsistent class-count information across sections and gender was not necessary to answer the research questions.

The class was selected because it was the site of the identified practice problem: many students had not reached the expected IPAS learning criterion under the existing instruction. This was therefore an intact-class action-research sample rather than a probability sample. No claims of statistical representativeness are made. The value of the sample is local and analytic: it allows repeated observation of the same learners while the instructional routine is refined.

3.3 Cooperative-Learning Intervention

During Cycle I, the researcher prepared the teaching module, PowerPoint material, post-test, and student worksheet. The lesson opened with learning objectives and an explanation of the cooperative-learning procedure. In the core activity, the researcher introduced the topic of energy in daily life, organized students into learning teams, presented instructional slides, explained key concepts, and then administered the cognitive assessment. Students could ask questions and report their learning experience during closure. The lesson therefore introduced cooperative grouping but still retained a substantial teacher-explanation component.

Reflection after Cycle I indicated that some students remained less active and that six students had not yet reached the mastery criterion. Cycle II maintained the same 60-minute structure but strengthened the participatory elements. Students were explicitly invited to express deeper understanding of the material, discuss and exchange ideas with classmates, complete a related task, and participate in a joint reflection and evaluation. These changes are important because they suggest that the second cycle increased opportunities for verbal elaboration and peer interaction rather than merely repeating the first lesson. The intervention is described as a general cooperative-learning model because the source thesis did not consistently identify a specific formal subtype such as STAD, Jigsaw, TGT, or NHT.

3.4 Measures

Cognitive learning outcomes were measured with a five-item written multiple-choice test. The source instrument covered the meaning of energy, examples of energy transformation, use of alternative energy, and energy conservation. Items used familiar contexts such as lamps, electric fans, bicycles, flashlights, gas stoves, solar panels, and household electricity use. Scores in the result tables appear in 20-point increments from 0 to 100, consistent with five equally weighted items. The school mastery criterion used throughout the result tables was 75; students with scores of 80 or 100 therefore met mastery, whereas scores of 60 or below did not. Although one sentence in the source analysis section contains an inconsistent value of 65, the test tables, cycle interpretations, and KKM statements consistently use 75, so 75 was retained for the journal re-analysis.

Classroom observation sheets were used to document affective and psychomotor aspects of participation. The affective indicators included enthusiasm in answering questions, curiosity when responding to teacher questions, and active participation in discussion. Psychomotor/participation indicators included completing assigned tasks, cooperating within the group, and presenting discussion results confidently. The source thesis summarized these observations descriptively and reported broader participation by Cycle II. However, the observation tables contain formatting and category-coding inconsistencies that prevent a defensible reconstruction of a continuous observation score for inferential analysis. Consequently, the journal manuscript treats observation evidence as contextual support rather than as a second quantified outcome.

The source thesis does not report a formal item-analysis study, reliability coefficient, or independent content-validity index for the five-item achievement test. This limitation is retained rather than repaired with invented psychometric evidence. The test is therefore interpreted as a classroom-aligned achievement measure suitable for formative action research, not as a standardized measure with established measurement precision.

3.5 Data Analysis and Journal-Manuscript Re-analysis

The original thesis summarized student scores and the percentage of students who reached mastery. For the present journal manuscript, those original student-level scores were re-analysed without changing any observed value. Descriptive statistics included the mean, standard deviation, median, range, number and percentage meeting the mastery criterion, and change in the class mean between measurement occasions. A class-level normalized gain from pre-action to Cycle II was calculated as $g=(\text{post}-\text{pre})/(100-\text{pre})$, using the class means. This index describes improvement relative to the remaining possible score range but should be read cautiously because the test has only five items and several students reached the maximum score.

Because the same 14 students were measured three times, the observations are dependent. The scores were also discrete and showed substantial ceiling values by Cycle II, making a normal-theory repeated-measures ANOVA less attractive for this small sample. The overall change was therefore examined using the Friedman test. Kendall's W was reported as an effect-size index for the three-occasion rank difference, calculated from the Friedman statistic as $W=\chi^2/[N(k-1)]$, where $k=3$ occasions. Follow-up paired comparisons used two-sided Wilcoxon signed-rank tests for pre-action versus Cycle I, Cycle I versus Cycle II, and pre-action versus Cycle II. A Bonferroni-adjusted alpha of .0167

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was used for the three follow-up comparisons. Analyses were conducted during manuscript preparation using Python/SciPy on the exact scores reported in the source thesis.

These inferential analyses are used descriptively to evaluate the consistency of within-class change; they do not transform the classroom action research into a causal experiment. Without a concurrent comparison group, improvements may reflect multiple influences, including instruction, repeated testing, maturation, teacher attention, and familiarity with the content. The statistical tests therefore answer whether the observed repeated scores shifted systematically, not whether cooperative learning alone caused the change.

3.6 Analytic Robustness and Reporting Transparency

Several sensitivity considerations were applied to avoid overstating the re-analysis. First, the achievement scale contains only six possible raw outcomes if scored in equal 20-point increments, so conventional assumptions of a continuous normally distributed outcome are weak. Rank-based repeated-measures procedures were therefore preferred. Second, mastery was analysed using the criterion documented in the result tables rather than treating a formatting inconsistency in the method chapter as a second threshold. Third, all analyses use complete matched cases, so changes across occasions cannot be attributed to different students entering or leaving the analytic dataset.

As an additional descriptive robustness check, paired mean gains were examined with a nonparametric bootstrap during manuscript preparation. Resampling the 14 matched student records 200,000 times produced a 95% percentile interval of approximately 30.00 to 44.29 points for the pre-action-to-Cycle II mean gain of 37.14 points. The corresponding intervals were approximately 20.00 to 24.29 points for pre-action-to-Cycle I and 10.00 to 21.43 points for Cycle I-to-Cycle II. These intervals are exploratory because bootstrap performance is limited by the small sample and coarse score scale, but they show that the observed average improvement is not dependent on one or two extreme records.

No missing-value imputation, synthetic augmentation, or reconstruction of unreported observation scores was performed. Where the source thesis was internally inconsistent, the manuscript states the inconsistency and uses the most directly verifiable evidence. This reporting strategy follows a basic publication principle: analytical sophistication should increase transparency, not conceal limitations in the source data.

3.7 Ethical and Administrative Considerations

The source thesis documents implementation in collaboration with the school and Grade IV classroom context, but it does not provide a formal institutional ethics-committee approval number or a detailed record of parental consent/assent procedures. No ethics approval number is therefore claimed in this manuscript. Before submission, the author should verify that the journal's requirements for research involving minors are satisfied and should insert only the authentic permission, consent, and ethics information that applied to the study. Student names are not reproduced in the journal tables below; records are presented as anonymous participant codes.

4. RESULTS

4.1 Descriptive Change across the Action-Research Cycles

The score trajectory shows a clear upward pattern across all three occasions. The pre-action mean was 52.86 (SD=21.64), increasing to 74.29 (SD=21.38) after Cycle I and to 90.00 (SD=13.01) after Cycle II. Median performance increased from 50 to 80 and then 100. At baseline, only 4 of 14 students reached the mastery criterion of 75 (28.57%). Eight students reached mastery in Cycle I (57.14%), and 13 students did so in Cycle II (92.86%). Thus, the absolute mastery increase from baseline to Cycle II was 64.29 percentage points.

Table 1. Descriptive Learning Outcomes across Measurement Occasions

Occasion	N	Mean	SD	Median	Range	Mastery n (%)
Pre-action	14	52.86	21.64	50	20–80	4 (28.57%)
Cycle I	14	74.29	21.38	80	40–100	8 (57.14%)
Cycle II	14	90.00	13.01	100	60–100	13 (92.86%)

Note. Mastery criterion = 75. Values are re-analysed from the individual scores reported in the source thesis; no outcome value was imputed or simulated.

The class mean increased by 21.43 points from pre-action to Cycle I, by a further 15.71 points from Cycle I to Cycle II, and by 37.14 points across the full action-research sequence. The class-level normalized gain from pre-action to Cycle II was .79. Although normalized gain is sensitive to ceiling effects, the value indicates that the class recovered a large proportion of the score increase that remained possible after the baseline assessment. The declining standard deviation in Cycle II also suggests that scores became more concentrated near the upper end, although the five-item scale limits fine-grained interpretation of dispersion.

Inspection of the matched student records strengthens the descriptive conclusion. Every student increased from pre-action to Cycle II; no student showed a negative full-sequence change. Individual gains ranged from 20 to 60 points. Four students who had already scored 80 at baseline reached 100 by Cycle II, while several students beginning at 20–40 improved to 80 or 100. One participant increased from 20 to 60 but remained below the mastery criterion in Cycle II. This pattern is educationally important because the class-level improvement was not produced only by a small subset of high achievers.

Table 2. Anonymous Matched Student Scores

Participant	Pre-action	Cycle I	Cycle II	Pre-to-II gain
P01	40	60	80	+40
P02	60	80	100	+40
P03	60	80	100	+40
P04	80	100	100	+20
P05	80	100	100	+20

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Participant	Pre-action	Cycle I	Cycle II	Pre-to-II gain
P06	80	100	100	+20
P07	40	60	80	+40
P08	60	80	100	+40
P09	40	60	80	+40
P10	40	60	80	+40
P11	40	80	100	+60
P12	20	40	80	+60
P13	80	100	100	+20
P14	20	40	60	+40

Note. Participant codes replace student names. The ordering follows the repeated score tables in the thesis.

4.2 Repeated-Measures Statistical Re-analysis

The Friedman test confirmed that the three repeated score distributions were not equivalent, $\chi^2(2)=26.46$, $p<.001$. Kendall's W was .95, indicating a very strong separation of ranks across the three measurement occasions in this class. Follow-up Wilcoxon signed-rank tests were all below the Bonferroni-adjusted significance threshold of .0167: pre-action versus Cycle I, W=0, $p<.001$; Cycle I versus Cycle II, W=0, $p=.002$; and pre-action versus Cycle II, W=0, $p<.001$. The zero Wilcoxon statistics reflect the directionally consistent non-zero changes among the paired observations; ties occur where students were already at or near the test ceiling.

These results provide stronger evidence than mastery percentages alone that score improvement was systematic within the class. Nevertheless, statistical significance should not be conflated with causal identification. The test evaluates the probability of observing such ordered within-student differences under a no-shift rank model. It does not control for a counterfactual group that experienced the same passage of time without the cooperative-learning intervention.

Table 3. Nonparametric Repeated-Measures Analysis

Analysis	Comparison	Statistic	df	p	Interpretation
Friedman	Pre, Cycle I, Cycle II	$\chi^2=26.46$	2	<.001	Overall repeated-score shift
Wilcoxon	Pre vs Cycle I	W=0	—	<.001	Higher after Cycle I
Wilcoxon	Cycle I vs Cycle II	W=0	—	.002	Further increase in Cycle II

Analysis	Comparison	Statistic	df	p	Interpretation
Wilcoxon	Pre vs Cycle II	W=0	—	<.001	Higher after two cycles

Note. Two-sided Wilcoxon signed-rank tests; Bonferroni-adjusted alpha = .0167 for the three pairwise comparisons. Kendall's W for the Friedman test = .95.

4.3 Classroom Observation and Cycle Reflection

The qualitative cycle evidence is consistent with the score trajectory. After Cycle I, the source reflection noted that activity had improved but some students were still not optimally involved. Six students remained below mastery, so the cycle was not considered sufficient. The Cycle II plan therefore increased the opportunities for students to state their understanding, exchange ideas, work with classmates, and participate in end-of-lesson reflection. This refinement corresponds to a core action-research principle: evidence from the first action is used to redesign the next action rather than merely repeating the same lesson.

By the end of Cycle II, the source thesis described wider affective participation and reported that 13 students reached cognitive mastery. Because the observation coding in the source document is not sufficiently consistent for a reliable numerical reconstruction, this manuscript does not test an association between observation ratings and achievement. The observational evidence is used more modestly: it helps explain what changed instructionally between cycles and supports the interpretation that Cycle II involved more active peer exchange.

4.4 Mastery Transitions and Gain Sensitivity

Mastery transitions provide a second way to interpret the practical meaning of the score changes. All four students who were already at mastery in the pre-action assessment remained at mastery in Cycle I and Cycle II. Four additional students crossed the mastery threshold in Cycle I, and five more crossed it in Cycle II. Thus, 9 of the 10 students who were initially below mastery eventually reached the criterion. The only student who remained below mastery nevertheless improved by 40 points from a baseline score of 20 to a Cycle II score of 60. This transition pattern indicates that the improvement was not accompanied by regression among initially successful students and that most initially struggling students made sufficient progress to cross the school criterion.

The gain pattern also suggests that Cycle II was not simply redundant repetition. Ten students showed a positive score change from Cycle I to Cycle II, while four students were tied because they had already reached the 100-point ceiling. Among students with room to improve, the direction of change was uniformly positive. This explains why the Wilcoxon Cycle I-to-Cycle II comparison remains statistically detectable despite several ties. The result should still be interpreted with caution: once a student reaches 100 on a five-item test, additional learning cannot be represented numerically, so the true amount of Cycle II improvement among high performers may be underestimated or may be indistinguishable from test ceiling.

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The bootstrap intervals for paired mean gains reinforce the same descriptive conclusion. The pre-action-to-Cycle II gain was 37.14 points with an exploratory 95% bootstrap interval of 30.00–44.29. The Cycle I-to-Cycle II gain was 15.71 points with an interval of 10.00–21.43. These results indicate that the average gains remain positive under repeated resampling of the observed class. Because the sample consists of one intact classroom, the intervals describe uncertainty in this dataset rather than population-level generalizability.

Table 4. Mastery Transition Summary

Transition indicator	Pre-action	Cycle I	Cycle II
Students at mastery (n)	4	8	13
Students below mastery (n)	10	6	1
New mastery cases from prior stage	—	4	5
Mastery percentage	28.57%	57.14%	92.86%

Note. A new mastery case is a student who was below 75 at the preceding occasion and reached at least 75 at the next occasion.

5. DISCUSSION

5.1 Interpreting the Magnitude and Pattern of Improvement

The central finding is a large and internally consistent improvement in Grade IV IPAS scores across the two-cycle intervention. The mean rose by more than 37 points from baseline to Cycle II, mastery increased by 64.29 percentage points, and every student improved across the full sequence. The Friedman and Wilcoxon analyses show that the change was not limited to a shift in the class average caused by a few extreme observations. Rather, students' paired scores moved upward in a highly ordered fashion. For a classroom action research project, this pattern is practically meaningful because the intervention goal was not to produce an abstract standardized effect but to move a specific class toward successful mastery of the targeted IPAS content.

The second cycle appears particularly important. Cycle I produced a substantial mean increase and doubled the mastery proportion from 28.57% to 57.14%, yet the class still fell short of the desired mastery condition. The source reflection identified incomplete participation, and Cycle II explicitly expanded opportunities for students to express understanding and exchange ideas. The additional 15.71-point mean gain and rise to 92.86% mastery after this refinement are consistent with the logic that cooperative learning becomes more effective when interaction is not merely permitted but deliberately structured. The pattern aligns with Slavin's (2014) argument that group work requires structures that make learning consequential for individual students and with Gillies' (2016) emphasis on teacher facilitation of productive peer dialogue.

The findings also fit the broader active-learning literature. Cooperative tasks require learners to retrieve and articulate knowledge instead of only recognizing it during teacher explanation. In a topic such as energy transformation, verbalizing why an electric fan converts electrical energy into motion or why a battery stores chemical energy forces students to organize causal relations. This kind of active processing is consistent with the mechanisms underlying the achievement benefits synthesized by Freeman et al. (2014). The present study does not replicate that meta-analysis, which was conducted in different educational settings, but it demonstrates how the general active-learning principle can be operationalized in a small elementary IPAS classroom.

5.2 Relation to Cooperative-Learning Evidence

The direction of the results is consistent with meta-analytic evidence that cooperative learning can improve achievement. Kyndt et al. (2013) found positive effects across many face-to-face cooperative-learning studies while also identifying context as a moderator. The present result should be read within that contextual frame: it is evidence from one small Indonesian school, not a universal effect estimate. The magnitude of the within-class change is large, but direct comparison with standardized effect sizes from controlled studies would be misleading because this study lacks a control group and uses a brief classroom test.

Indonesian evidence provides a closer comparison. Utami et al. (2021) found positive effects of cooperative learning in a meta-analysis of biology learning, and Rizki et al. (2022) reported positive impacts across physics and junior-high science studies. Although the educational levels and content differ, those syntheses support the proposition that science-related learning can benefit from interactional structures that move students from passive reception toward explanation and joint problem solving. The present study extends that proposition to an integrated Grade IV IPAS topic in Boalemo and shows how action research can be used to refine the approach when the first cycle is not sufficient.

The elementary findings of Wibawa (2023), Nurtianingsih et al. (2023), Rismah and Efendi (2023), Saputri and Sukmawati (2024), and Nur et al. (2025) are particularly relevant. Across different designs and cooperative variants, these studies report improvements in science achievement or scientific literacy. The current study converges with that literature while differing in design: it is not a quasi-experiment comparing cooperative and conventional classes, but a practitioner-oriented repeated intervention in a single intact class. This distinction reduces causal certainty but increases ecological relevance for the teacher who must decide whether and how to modify instruction in the next lesson.

A useful implication is that the label of the cooperative subtype may be less important than the presence of core cooperative processes. The source thesis used a general cooperative model rather than a consistently specified STAD, TGT, Jigsaw, or NHT protocol. This prevents claims about the superiority of any particular subtype. At the same time, the Cycle II improvement suggests that participation, explanation, discussion, and reflection are plausible active ingredients. Future studies should document these components more precisely so that implementation fidelity can be analyzed rather than inferred from lesson descriptions.

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5.3 Why Cooperative Interaction May Have Supported IPAS Learning

Three mechanisms may explain the observed improvement. First, peer explanation can reduce the gap between hearing information and understanding it. Students who explain a concept must generate a coherent account, and peers can signal when the account is incomplete. Second, cooperative interaction can distribute attention. In a whole-class lecture, a hesitant student can remain silent for an entire lesson; in a small group, the social structure can create more frequent opportunities to answer, ask, and check. Third, cooperative tasks can connect cognitive and motivational processes. Students may persist longer when they feel responsible to a group and when the social environment supports competence and relatedness, consistent with self-determination theory (Ryan & Deci, 2020).

These mechanisms are particularly plausible for elementary IPAS because the subject is inherently connected to observable phenomena. A group can compare examples from home and school, classify forms of energy, discuss why energy conservation matters, and correct everyday misconceptions. Such dialogue can move students from memorizing labels toward relational understanding. However, the current five-item test mainly captures classroom achievement rather than deep conceptual transfer. The strong score increase therefore suggests improved mastery of the taught content but does not establish that students could transfer the concepts to novel situations or retain them over a long interval.

The Cycle II ceiling also deserves attention. Ten of the 14 students obtained 100, and the median was 100. This may indicate very successful mastery, but it may also indicate that the test was too easy to differentiate higher levels of understanding after instruction. A more rigorous future assessment should include more items, a wider range of cognitive demand, and perhaps constructed-response tasks that require students to explain energy transformations rather than select an option. Better measurement would help distinguish genuine conceptual growth from familiarity with a narrow item pool.

5.4 Methodological Contribution of the Re-analysis

Converting a thesis into a journal article should not simply shorten chapters. It should also improve the alignment between the research question, data structure, and analysis. The original thesis appropriately used mastery percentages for classroom decision making, but the availability of matched student scores allowed a stronger analysis. The Friedman test respects the repeated-measures design, and the Wilcoxon follow-ups show that improvement occurred both after Cycle I and again after Cycle II. Reporting the mean, standard deviation, median, range, mastery proportion, normalized gain, and matched scores also makes the evidence more transparent and reproducible.

Equally important, the re-analysis preserves the boundary between statistical improvement and causal inference. A common weakness in classroom action research is to use the word ‘effect’ as though a pre–post increase proves that the intervention alone caused the change. This study cannot support that conclusion. The same teacher implemented and observed the intervention, students encountered the content repeatedly, and there was no control class. The correct claim is therefore that cooperative-learning cycles were followed by strong, statistically detectable within-class improvement. This wording is more conservative, but it increases scientific credibility because it matches the design actually used.

The participant-count discrepancy in the source thesis illustrates another publication-quality issue. Rather than silently choosing one number, the journal manuscript bases the analytic sample on the 14 complete repeated records in the results tables and explicitly reports why. Similar transparency is applied to the test reliability and ethics information: neither is fabricated. These choices may seem less polished than filling every field with a number, but international publication standards depend on traceability and verifiability.

5.5 Practical Implications for Elementary Teachers and Schools

For classroom teachers, the results support a staged approach to cooperative learning. Teachers can begin with a clearly defined concept, form manageable groups, state both the learning goal and the expected collaboration behavior, and then monitor whether every learner contributes. A first implementation should be treated as diagnostic. If discussion is dominated by a few students, the next lesson can assign roles, use turn-taking prompts, require individual responses before group consensus, or ask randomly selected members to explain the group's reasoning. The present Cycle II gains demonstrate the practical value of revising the interaction structure after observing Cycle I rather than abandoning the model after an imperfect first attempt.

Teachers should also align assessment with the collaborative task. If groups discuss complex examples but the test only checks recall, the assessment will underrepresent the learning objective. Conversely, if students can earn a group product without individual understanding, group performance can mask weak individual learning. Combining individual exit questions, short written explanations, and group discussion can preserve accountability while retaining cooperative support. This principle is consistent with Slavin's (2014) emphasis on individual accountability within group goals.

For schools, professional development should focus on the quality of cooperative interaction, not only on model names. Teachers need examples of how to form groups, pose questions, monitor equitable participation, and debrief group work. Simple instructional media such as presentation slides can still be useful, but they should support discussion rather than restore lecture dominance. The literature also shows that technology can enhance collaborative learning when it is integrated with an instructional purpose (Talan, 2021); in resource-constrained settings, however, strong cooperative pedagogy does not depend on sophisticated technology.

5.6 Limitations and Directions for Future Research

Several limitations constrain interpretation. First, the study involved one intact class of 14 students and therefore has low external generalizability. Second, there was no comparison group, so the design cannot isolate cooperative learning from repeated exposure, practice effects, maturation, or other classroom influences. Third, each action cycle consisted of only one 60-minute meeting. The study demonstrates rapid classroom improvement but provides no evidence about long-term retention or whether cooperative routines would remain effective over a full unit or semester.

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Fourth, the achievement instrument contained only five multiple-choice items. The 20-point score increments and Cycle II ceiling indicate limited measurement resolution. The source thesis also does not provide a psychometric reliability coefficient or a documented independent content-validity procedure. Future research should use a larger item pool, blueprint the items across cognitive levels, conduct expert review, report item difficulty/discrimination when feasible, and include a delayed post-test. Fifth, the researcher's dual role as instructor and observer creates a risk of expectancy bias. Independent observers or video-based coding with inter-rater agreement would strengthen future action research.

Sixth, the cooperative-learning procedure was described generically. This is adequate for reporting what the source thesis actually implemented, but it limits reproducibility. Future studies should specify group size, grouping criteria, roles, accountability rules, task structure, duration of peer interaction, teacher prompts, and the exact criteria used to move from one cycle to the next. A fidelity checklist would make it possible to connect changes in cooperative processes to changes in outcomes. Finally, because the study involved elementary students, future publication should document school permission, parental consent, student assent where applicable, confidentiality procedures, and institutional ethics requirements in a form that can be verified by the journal.

5.7 Implications for Future Study Design and Journal Reporting

The present conversion also highlights how undergraduate research can be strengthened for international journal communication without changing the underlying data. First, the research question should be narrowed to claims the design can support. A classroom action study can credibly report improvement across iterative cycles, but words such as 'caused' or 'effectiveness' require stronger counterfactual evidence. Second, the statistical method should reflect the data structure. Repeated scores from the same students should not be analysed as though each measurement were independent. Third, publication-quality reporting should expose rather than hide data limitations, including small samples, coarse instruments, ceiling effects, participant-count inconsistencies, and absent reliability information.

A stronger follow-up study could preserve the action-research cycle while adding a comparison element. For example, several Grade IV classes could use the same IPAS unit, with cooperative learning implemented in one or more classes and usual instruction observed in others. Repeated measures could then be analysed with multilevel or mixed-effects methods that distinguish student change from class-level differences. A delayed assessment two to four weeks after instruction would add evidence about retention. More importantly, a richer test blueprint could measure recall, application, explanation, and transfer so that improvement is not compressed at the upper limit of a five-item test.

Future research should also measure the cooperative process itself. Observation can document whether students actually explain ideas, ask elaborative questions, share responsibility, and participate equitably. When these process indicators are measured reliably, researchers can investigate why one cycle works better than another rather than treating the instructional model as a black box. This mechanism-focused approach would connect the present local result with the wider cooperative-learning literature and would make the study more informative for teachers who need to know which interactional features deserve attention.

Finally, international publication requires administrative completeness in addition to analytical quality. Author email and ORCID, authentic ethics or school-permission information, funding status, conflict-of-interest declarations, and data-availability conditions should be confirmed by the author rather than inferred by an editor or language tool. These details do not change the score results, but they are part of research integrity. The present manuscript intentionally leaves unsupported declarations unclaimed so that the final submitted version can be completed with verifiable author information.

5.8 Design Principles for Cooperative IPAS Lessons

The findings can be translated into several design principles for similar IPAS lessons. The first principle is to make interdependence academically meaningful. Students should need information, explanations, or checking from peers in order to complete the learning task, rather than merely sitting together while answering identical questions individually. For an energy unit, a teacher might distribute different everyday examples to group members and require the group to construct a shared explanation of the energy source, transformation, and conservation implication. This structure gives each member a reason to contribute and creates a natural basis for comparison and correction. It also reflects Johnson and Johnson's (2009) emphasis on positive interdependence while avoiding the assumption that physical grouping is sufficient.

The second principle is to preserve individual accountability. A group product can support discussion, but every learner should still demonstrate personal understanding. Short individual exit items, randomly selected explanations, or individual reflection after group work can reduce passive dependence on stronger classmates. The present score trajectory suggests that broad class improvement is possible when cooperative activity is paired with individual assessment. In future implementations, accountability can be strengthened further by requiring each student to explain one part of the group reasoning before the class or to submit a brief individual explanation after discussion. This would make the learning process more consistent with Slavin's (2014) explanation of why group goals and individual accountability work together.

The third principle is to use teacher monitoring as formative evidence. During group interaction, the teacher can record which students speak, which misconceptions recur, which examples produce confusion, and whether group roles are balanced. These observations should inform immediate prompts and the next lesson plan. The present action-research sequence demonstrates this logic at a basic level: Cycle I revealed incomplete participation and mastery, and Cycle II responded by increasing opportunities for explanation and exchange. A more systematic version could use a brief observation checklist and a reflection memo after each lesson, creating a traceable link between observed classroom problems and the specific modification introduced in the next cycle.

The fourth principle is to align task complexity with assessment complexity. Cooperative dialogue is most valuable when students must reason, justify, compare, or apply ideas; if the task asks only for simple recall, peer interaction may add little beyond answer sharing. Likewise, an assessment should sample more than the easiest form of

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recognition. For the energy topic, future assessments could include a diagram of an unfamiliar device, ask students to identify multiple energy transformations, require a written justification, and include a conservation decision in a household context. Such tasks would reduce ceiling effects, test transfer, and provide stronger evidence that cooperative interaction improved conceptual understanding rather than only short-term familiarity with practiced examples.

Together, these principles show why the study should not be interpreted as a recommendation to replace teacher explanation with unrestricted group work. Effective cooperative learning is a guided design in which teacher explanation, peer interaction, individual responsibility, assessment, and reflection are coordinated. The strongest lesson from the two-cycle pattern is therefore procedural: instructional quality improved when the classroom treated participation as something that could be designed, observed, and revised. That principle is transferable even when future teachers choose a different cooperative subtype or a different IPAS topic.

6. CONCLUSION

The two-cycle classroom action research showed a strong improvement in Grade IV IPAS learning outcomes after cooperative-learning instruction was introduced and refined at SDN 17 Tilamuta. Mean achievement rose from 52.86 at pre-action to 74.29 in Cycle I and 90.00 in Cycle II. Mastery increased from 28.57% to 57.14% and then 92.86%, while the repeated-measures re-analysis showed a statistically detectable shift across occasions, $\chi^2(2)=26.46$, $p<.001$, Kendall's $W=.95$. The class-level normalized gain from baseline to Cycle II was .79.

The educational significance lies in the iterative process. Cycle I improved outcomes but left substantial non-mastery; Cycle II increased opportunities for students to explain, exchange ideas, and reflect, followed by further improvement. The findings support continued use of carefully structured cooperative learning for elementary IPAS, particularly when teachers treat the first cycle as evidence for refinement. At the same time, the small single-class design, absence of a control group, short intervention, brief five-item test, and incomplete psychometric and ethics documentation mean that the results should be presented as strong local action-research evidence rather than a definitive causal effect. Future research should use longer interventions, stronger measurement, explicit cooperative-learning fidelity indicators, independent observation, and comparison designs while retaining the reflective cycle that made the present classroom improvement possible.

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AUTHOR CONTRIBUTIONS

Ismail Harjan: conceptualization, classroom action-research design, instructional implementation, data collection, original analysis, and thesis writing. For this journal conversion, the original score data were additionally subjected to descriptive and nonparametric repeated-measures re-analysis; the submitting author should verify the final manuscript and all declarations before submission.

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DATA AVAILABILITY

The individual achievement scores analysed in this manuscript are reproduced anonymously from the pre-action, Cycle I, and Cycle II result tables in the source thesis. Additional classroom records, if retained, are subject to school and participant-confidentiality requirements.

CONFLICTS OF INTEREST

A conflict-of-interest declaration was not included in the source thesis. The submitting author should provide and confirm the declaration required by the journal; no conflict status is invented in this manuscript.

GENERATIVE ARTIFICIAL INTELLIGENCE DISCLOSURE

OpenAI ChatGPT was used during manuscript preparation to assist English-language translation, structural editing, expansion of the literature-based discussion, and statistical re-analysis of the author's original student-score data. No synthetic participants or outcome scores were created. The submitting author remains responsible for verifying the accuracy of the manuscript, references, statistical reporting, and all publication declarations before submission.

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