

Game Boards as a Strategy for Strengthening Attention Span and Working Memory in Group A Children Aged 4–5 Years: A Classroom Action Research Study Using the Kemmis and McTaggart Model

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Abstract: Focus and working memory are routinely invoked in early childhood classrooms as though they were self-evident, directly observable traits rather than constructs that require operational definition before any claim of change can be defended. This classroom action research, following the Kemmis and McTaggart model across two cycles, examined whether structured board game sessions shifted attentional persistence and short-term recall in ten children aged four to five in a Group A kindergarten class in Bandung, Indonesia. Data was collected through participant observation, teacher interviews, and documentation across two cycles of six sessions each. Results indicated a rise in on-task attentional behavior, with ninety percent of children meeting the study's attentional criterion by the second cycle, alongside gains in sequential recall of game rules and turn order. These findings converge with recent international evidence that structured play activates executive processes relevant to attention and working memory. The design, lacking a control group and relying on a small, non-randomized sample, cannot isolate the intervention's causal contribution from maturation or teacher scaffolding, a limitation the discussion treats as an occasion for methodological honesty rather than rhetorical minimization. The article argues that board games should be treated as a family of mechanically distinct interventions whose cognitive demands must be specified before causal claims are warranted, and that collapsing this family into a single undifferentiated pedagogical tool is a category error common to the wider early childhood literature.

Keywords: Attentional control; working memory; board game; early childhood; classroom action research.

Abstrak: Fokus dan memori pada anak usia dini kerap disebut dalam laporan pembelajaran seakan keduanya sifat yang tampak begitu saja, padahal keduanya konstruk yang menuntut definisi operasional sebelum layak diklaim meningkat atau menurun. Penelitian tindakan kelas ini, mengikuti model Kemmis dan McTaggart dalam dua siklus, menguji apakah sesi permainan papan (board game) yang terstruktur menggeser ketahanan atensi dan daya ingat jangka pendek pada sepuluh anak kelompok A berusia empat hingga lima tahun di sebuah TK di Bandung. Data dikumpulkan melalui observasi partisipan, wawancara guru, dan dokumentasi sepanjang dua siklus, masing-masing enam pertemuan. Hasil menunjukkan kenaikan perilaku on-task dalam mengikuti instruksi, dengan sembilan puluh persen anak memenuhi kriteria atensi penelitian pada siklus kedua, disertai peningkatan kemampuan mengingat urutan aturan dan giliran bermain. Temuan ini sejalan dengan bukti internasional mutakhir bahwa permainan terstruktur mengaktifkan proses eksekutif yang relevan bagi atensi dan memori kerja. Desain penelitian ini, tanpa kelompok kontrol dan bertumpu pada sampel kecil non-acak, tidak mampu memisahkan kontribusi kausal intervensi dari efek pematangan atau pendampingan guru, sebuah keterbatasan yang dalam pembahasan diperlakukan sebagai kejujuran metodologis, bukan basa-basi penutup. Artikel ini menegaskan bahwa permainan papan adalah keluarga intervensi

yang berbeda-beda secara mekanis, masing-masing dengan tuntutan kognitif tersendiri yang harus dirinci lebih dahulu sebelum klaim kausal dapat dipertanggungjawabkan. Memperlakukannya sebagai satu alat pedagogis tunggal dengan efek seragam adalah kekeliruan kategori yang lazim ditemukan dalam literatur PAUD yang lebih luas.

Kata kunci: kendali atensi; memori kerja; permainan papan; anak usia dini; penelitian tindakan kelas

Introduction

Every claim of pedagogical success rests on two foundations that are rarely examined together: the accuracy of the definition of the construct claimed to have changed and the strength of the design used to detect that change. Action research in early childhood education settings in Indonesia tends to devote significant energy to the second foundation as evidenced by the density of tables and percentages that fill the results section while overlooking the first foundation, as if the definitions of “focus” and “memory” were self-evident to anyone who has ever taught in a preschool. This article reverses that order. Before asking whether board games improve children’s focus and memory, a more fundamental question must first be answered: what exactly do these two terms mean, and do the instruments used measure what they claim to measure?

A child is described as “focused” in a kindergarten teacher’s daily report when he or she is quiet and staring at something. This linguistic convention is fundamentally flawed. Silence is not an indicator of attention, and a blank stare at the blackboard often signals the opposite that the child’s mind is wandering. *Attentional control* in cognitive psychology is the capacity to maintain a mental representation of a goal while filtering out irrelevant stimuli a process that is most evident when a child is actively sorting cards, comparing patterns, or taking turns, not when they are frozen in their seat. This kind of folk-psychology misconception is no trivial matter in classroom action research (PTK) in early childhood education settings, because the observation instruments used to measure “improved focus” often unconsciously perpetuate that erroneous definition, and the results produced by poorly designed instruments remain flawed even if the percentage figures appear convincing.

The same issue applies to “memory.” Early childhood education teachers and researchers generally use this term to refer to a child’s ability to recall the sequence of activities, whereas the neurocognitive literature clearly distinguishes between long-term memory, episodic memory, and *working memory*, which is the capacity to actively store and update a small amount of information while a task is in progress. In the context of a board game, a child is not memorizing facts to recall years later. They are actively updating the positions of the game pieces, keeping track of whose turn is next, and determining which rules apply to the newly revealed card. This is a function of working memory, not declarative memory, and this distinction is not merely a semantic exercise because the two constructs are measured by different instruments, develop along different age trajectories, and respond differently to pedagogical interventions as well.

This conceptual confusion takes on practical significance when it intersects with the dominant genre of classroom action research in Indonesian early childhood education

journals. This format typically includes two cycles, ten to thirty subjects, self-constructed observation instruments without validity testing, and conclusions stating that the intervention is “proven effective” based on a percentage increase from cycle one to cycle two. This format is not something that should be discarded, as classroom action research is indeed designed for a reflective-practitioner logic rather than a logic of statistical generalization; however, confusion begins to arise when classroom action research reports claim a causal status that can actually only be supported by an experimental design with a control group. This article stems from that concern and strives to avoid falling into the same trap.

International literature over the past decade has shown that the relationship between play and executive function is far more complex than the “play automatically trains the brain” model assumes. Gibb and colleagues designed the *Building Brains and Futures* program, consisting of ten simple games for children ages three to five, and found that deliberate adult involvement in structuring the games was the key factor in the program’s success in building executive function skills not merely the presence of the games themselves (Gibb et al., 2021). This finding is important for this type of action research because it shifts the unit of analysis from “board games as objects” to “board games as structured, teacher-guided events.”

Bai, Huang, and Ouyang (2022) added another layer of evidence through a cluster-randomized controlled trial involving preschoolers aged four to five years and found that group play interventions involving moderate to high physical intensity improved both executive function and motor skills. This study is particularly relevant because it demonstrates that the effects on executive function are not exclusive to quiet, cognitive games such as board games; therefore, early childhood education researchers should exercise caution before attributing any improvements in attention to the “pure” cognitive mechanisms of the board games themselves, rather than to accompanying factors such as physical activity, social interaction, or simply a more predictable, scheduled routine for the children.

Gasteiger and Moeller (2021) provide further insight by showing that in conventional board games for children aged four to six, the type of dice used determines numerical learning outcomes. Conventional dotted dice were found to promote counting skills and the recognition of numerical structures more effectively than dice with color symbols or pictures, and this effect was still detectable one year after the intervention ended.

This complexity became most evident in the study by Moya-Higueras and colleagues (2023), a quadruple-blind randomized clinical trial that compared children who played modern board games with mechanisms designed to activate basic executive functions with those who played games using different mechanisms. Contrary to the researchers’ initial hypothesis, the group whose games were not designed to directly train executive functions showed greater improvement, a pattern the researchers explained through far transfer and multicomponent intervention effects. Vita-Barrull and colleagues (2022a, 2022b) support this interpretation through an expert-panel approach to assessing the cognitive profiles of games, arguing that each board game activates several cognitive processes at once rather than a single process.

Therefore, labeling a game simply as a “focus game” or “memory game” is an oversimplification that the data do not support.

At a more structural level, Lucchini and colleagues (2024) conducted a group-based randomized controlled trial in elementary schools and found that integrating board games into classroom routines can support both cognitive development and academic achievement, provided that the integration is done consistently and not as occasional, ad-hoc activity. These findings link the literature on executive function to questions more closely related to the reality of preschool teachers, namely, what dosage, frequency, and type of curricular integration consistently yield the benefits of board games, rather than simply addressing whether board games are beneficial in the abstract.

From this body of evidence, two schools of thought have emerged that engage in productive tension. The first school, represented by the research of Vita-Barrull, Moya-Higueras, and their colleagues, views executive functions as a cognitive system that can be specifically trained through the precise manipulation of game mechanics; thus, the researchers’ task is to map the cognitive profile of each game before claiming its benefits. The second camp, which aligns more closely with the sociocultural tradition in early childhood education, emphasizes the role of social mediation specifically, teacher guidance and peer interactions as the true engines of change, with board games serving merely as a medium that provides opportunities for such mediation to occur. Gibb and colleagues (2021) stand at the intersection of these two camps when they emphasize the role of intentional adult involvement, and classroom action research such as this manuscript which by design involves teachers as active facilitators in each session actually sits precisely at that intersection without ever explicitly acknowledging it.

There is a third camp that is rarely explicitly mentioned in the literature on board games and executive functions, yet its presence is felt in every aspect of the methodological discussion above: the camp of measurement skeptics, who focus on what Moya -Higueras and his colleagues (2023) refer to as the issue of “task impurity” namely, the fact that there is virtually no single instrument that truly measures one cognitive process in isolation from others. Tests designed to measure cognitive flexibility still require attentional control so that children can understand the instructions, and tasks designed to measure working memory still require children’s motivation and cooperation throughout the test. This camp does not deny the existence of the constructs of focus and memory as real phenomena, but it demands methodological humility from anyone who claims to have measured them in isolation a humility that becomes increasingly urgent when the instruments used are self-constructed observation sheets without formal psychometric testing, such as those used in this classroom action research and the majority of similar studies in the Indonesian early childhood education (PAUD) setting.

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Methods

This study employs the Kemmis and McTaggart classroom action research design, which organizes the research process into a spiral cycle comprising the stages of planning, action, observation, and reflection. This model was chosen because its spiral structure explicitly incorporates a self-correcting mechanism, rather than simply because it is popular in the early childhood education field, namely, that reflection at the end of the first cycle directly shapes the action plan for the second cycle. It is this characteristic that distinguishes classroom action

research from one-shot experiments, and it is this characteristic that must be emphasized so that readers do not mistakenly equate the validity of classroom action research with the validity of randomized controlled trials such as those conducted by Moya-Higueras and colleagues (2023). PTK is not designed to isolate independent variables from confounders, and methodological integrity demands that this be acknowledged upfront, rather than in a footnote listing limitations at the end of the article.

The research subjects were ten children in Group A, aged four to five years, at a kindergarten in Bandung, selected using total sampling because they constituted the entire population of Group A for the current school year. A sample size of this magnitude is common in action research, as classroom action research is essentially a reflective case study of a single classroom, not a study designed to achieve statistical power. By comparison, Moya-Higueras and colleagues (2023) calculated their sample size requirements through a formal power analysis procedure prior to data collection, targeting eleven participants per group to detect an effect at a 95 percent confidence level. This comparison puts the claims of this study into proper perspective and is not intended to undermine the legitimacy of classroom action research; that is, this study generates context-rich practitioner knowledge but is intentionally not designed to be generalized to the broader population of preschool children.

The primary instrument was a structured observation sheet developed by the researcher in collaboration with the classroom teacher, designed to operationalize two main constructs. Attention indicators included the duration a child remained engaged in the game task without shifting attention to other stimuli, the frequency with which a child returned to the task on their own after being distracted without needing to be redirected by the teacher, and the child's initiative in following game instructions without the need for more than one repetition of the instructions. Working memory indicators include the child's accuracy in recalling and restating the sequence of turns in the game, the child's accuracy in applying game rules that have just been explained without errors, and the child's ability to remember the position or content of cards relevant to the current stage of the game. Each indicator is rated on a four-point ordinal scale, ranging from "not yet evident" to "consistently evident" an improvement intentionally implemented over the binary "pass" and "fail" categories commonly used in early childhood education action research reports, but which lack clear cutoff criteria.

A framework linking constructs, indicators, and data collection techniques is presented in Table 3, so that other researchers who wish to replicate or modify this study will not have to guess how the terms "focus" and "memory" are operationalized in the field.

Construction	Indicators	Data Collection Techniques
Attention (attention control)	The length of time a child remains engaged in a game task without shifting their attention to other stimuli	Participant observations per session
	The frequency with which students return to their tasks on their own after being distracted, without further guidance from the teacher	Participant observations per session

	The initiative to follow game instructions without having the instructions repeated more than once	Participant observation; field notes
Working memory	Accuracy in remembering and recalling the order of turns	Participant observation; documentation
	Accuracy in applying the newly explained game rules without any errors in application	Participant observations per session
	The ability to remember the position or content of cards relevant to the next move in the game	Participant observation; teacher interviews

The three attention indicators and the three working memory indicators in Table 3 are designed to complement one another without excessive overlap, with the attention indicators focusing on the maintenance and recovery of attention, and the working memory indicators focusing on the updating and application of newly received information. It is important to emphasize this distinction because PTK PAUD reports that do not specify such criteria are prone to conflating the two constructs into a single indicator.

The validity of the instrument was established through source triangulation that is, by comparing the researcher's observations with the notes from interviews with the mentor teachers and visual documentation of each session as well as through a joint review by the research team and the mentor teachers of the researcher's interpretation of the observed behaviors. This approach mirrors the concurrent validity logic used by Wolf and colleagues (2025) in developing the *Playful Learning Across the Years* classroom observation instrument, albeit on a much smaller scale and without formal factor analysis. Wolf and colleagues demonstrated that the validity of early childhood classroom observation instruments must be tested through comparison with independent instruments and teacher characteristics, rather than being assumed valid merely because they appear superficially plausible. The instrument used in this study has not yet been tested to that extent, and this limitation is explicitly acknowledged as an area for improvement in the next research cycle, rather than being hidden behind reassuring percentage tables.

Scoring for each session was conducted by the researcher as the primary observer, with the co-teacher serving as a second rater who independently completed a brief observation sheet during two randomly selected sessions in each cycle, as a simple measure to check the consistency of ratings across observers. The agreement between the researcher and the teacher during these double-checked sessions was qualitatively high, although this study did not calculate a formal agreement coefficient such as Cohen's kappa due to the limited amount of data, which was too small to produce a stable estimate. It is important to acknowledge these limitations upfront, because a claim of "validated observations" without reported agreement figures is a claim that readers cannot verify; it is better to honestly acknowledge the simple verification procedure than to cloak it in technical terms that imply a level of precision that was not actually achieved.

Data collection was conducted through observation of participants during each play session, semi-structured interviews with the supervising teacher at the end of each cycle, and

documentation in the form of photographs and field notes. Each cycle consisted of six sessions, with small groups of three to four children playing together with a supervising teacher for twenty-five to thirty minutes per session, following the duration range commonly used in play-based intervention programs for preschoolers, as reflected in the ten-game program developed by Gibb and colleagues (2021). In the first cycle, the games were introduced with a full explanation of the rules by the teacher during each session. In the second cycle, based on the observation that repeating the full explanation actually reduced the children's opportunities to practice their own working memory, the teacher reduced the amount of explanation and gave the children more opportunities to recall the rules independently, providing corrections only when necessary.

Ethical considerations were addressed through verbal and written consent from parents or guardians before children were involved in play sessions, with an explanation that the observation was part of the class's regular learning practices and not an experimental procedure involving risks beyond those associated with everyday play activities. The children's identities were anonymized in all field notes and documentation used for the purposes of writing this article, in accordance with the minimum ethical standards for educational research involving children, although this study did not undergo formal review by an ethics committee, as is typically required for clinical trials such as those conducted by Moya-Higueras and colleagues (2023). The absence of a formal ethics committee review is common in classroom-scale action research; however, this commonality itself is worth noting as another structural difference between the action research genre and larger-scale experimental research designs it is not something that can simply be assumed to be equivalent.

That is, children who showed no improvement despite completing both full cycles were intentionally tracked separately in the data analysis, in accordance with the principle that data deviating from the general pattern often contain the most valuable information regarding the limits of an intervention's effectiveness. In this study, one to two children for each indicator remained in the "not yet consistent" category through the end of the second cycle, and field notes on these children were maintained separately so they would not be lost in group averages, which tend to obscure individual variation. The supporting teachers reported that children in this category exhibited less consistent attendance patterns throughout the cycle, a confounding factor that is important to note rather than ignore for the sake of neat statistics.

Data analysis was conducted using a descriptive qualitative approach through three intertwined stages: data reduction from field notes and interview results; presentation of data in the form of tables and thematic narratives; and drawing of conclusions that were continuously re-examined against the raw data throughout the analysis process. Quantitative data in the form of indicator achievement percentages were calculated as the proportion of children who fell into the "consistently meeting" category for each indicator at the end of each cycle; these figures are presented as a descriptive summary of observed patterns of change, not as evidence of causation, with causal interpretations deferred until the discussion section, where alternative explanations are explicitly considered.

Results and Discussion

Dynamics of Cycle I and Pedagogical Adjustments: Guidance on Inhibition During Moments of Waiting

This section places each figure within an interpretive framework that explicitly considers counter-explanations before arriving at claims of benefit, rather than merely reporting the figures. The sequence follows the chronology of the two cycles, followed by a synthesis that places these local findings alongside the body of international evidence mapped out in the introduction.

In the first cycle, observations showed that most children still displayed an initial interest in board games but lacked stable attention spans. The children tended to be enthusiastic during the first five to seven minutes of each session, but their attention began to wander when waiting for their turn felt too long or when the game instructions seemed complicated. Field notes indicate that distractions most frequently occur precisely during moments of waiting for a turn, rather than during active play a pattern consistent with the understanding that the attention of four- to five-year-olds remains highly dependent on active engagement and is not yet able to sustain itself for long periods during passive waiting.

Interviews with the supporting teachers at the end of the first cycle confirmed these observations. The teachers reported that the children began to show curiosity and actively ask questions about the rules and their turns to play an early sign of engagement that the teachers interpreted positively, even though it was not yet accompanied by the ability to recall the sequence of the game independently. At this stage, the children's reliance on repeated instructions from the teacher remained high, and most children required repeated verbal reminders before they were able to apply the rules correctly.

The finding that distraction peaks while waiting for one's turn rather than during active play should be interpreted as a mechanistic clue rather than merely a descriptive observation. The waiting period is when the demand for inhibition is at its highest, as children must resist the urge to act while simultaneously monitoring the course of the game to be ready when their turn arrives a dual burden on both attentional control and working memory monitoring. The difficulties experienced by children in Group A during this moment rather than during active play are consistent with the understanding that response inhibition and active monitoring develop more slowly than the ability to simply engage in the activity unfolding before their eyes. The implication is that game design for this age group should ideally intentionally shorten waiting intervals or provide parallel tasks during the wait, rather than assuming that children will automatically wait patiently simply because the rules of the game require it.

Reflections at the end of the first cycle led to two adjustments for the second cycle. The first adjustment was a gradual reduction in the teacher's verbal explanations, with the goal of giving children space to practice their own working memory rather than continuing to rely on external repetition. The second adjustment involved modifying the visual elements of the game, including the use of more contrasting colors on the cards and the addition of gradually

increasing challenges, with the goal of sustaining the children's engagement during the waiting period between turns a time that had previously proven to be a point of distraction.

Table 1 summarizes the findings from observations and interviews conducted during both cycles, organized according to the three stages the researcher used to track changes: the observation stage, the questioning stage, and the experimentation stage.

No	Stages	Stages	Findings from Cycle II
1	Observing	The child is beginning to try to follow instructions but gets easily distracted while waiting for his or her turn.	Children stay engaged in play-based tasks for longer and require fewer repeated verbal reminders from their teachers.
2	Asking	The child shows curiosity and begins to actively ask questions about the rules and whose turn it is to play.	The child asks more specific questions about the consequences of an action, indicating more active monitoring of the rules.
3	Trying	Children still rely entirely on their teachers' guidance to remember the order of play and the contents of the cards.	Children can remember the sequence of play and recite the contents of the cards on their own, with the teacher needing to correct them less frequently.

Deconstructing Quantitative Outcomes: The Fragility of Arithmetic and the Limitations of Action Research Design

In the second cycle, the most notable change was observed during the trial phase specifically, the children's ability to recall the sequence of play and restate the contents of the cards without full guidance from the teacher. This change is consistent with the theory of working memory as a capacity that strengthens through structured repetition, as found by Gasteiger and Moeller (2021) in a numerical dice intervention whose effects were still detectable one year after the intervention ended. An important distinction must be emphasized here, as Gasteiger and Moeller isolated the dice element as a controlled manipulated variable, whereas this study modified several elements simultaneously in the second cycle namely, the proportion of teacher instruction and the visual design of the cards making it impossible to determine which element was most responsible for the observed improvements. This is a structural limitation of the action research design itself, which alters multiple variables simultaneously across cycles for the purpose of improving practice rather than for the purpose of causal isolation; consequently, it cannot be remedied simply by a more careful analysis of the data that has already been collected.

Table 2 presents the final results for the two main indicators in the second cycle: children's attention to the game and their memory of the sequence and rules of play.

No	Indicator	Consistency (%)	Consistency (%)
1	Children's attention to games (task persistence and resistance to distraction)	90	10
2	Children's attention to games (task persistence and resistance to distraction)	85	15

The figures of 90 percent for the attention indicator and 85 percent for the working memory indicator seem convincing at first glance, but they need to be viewed with greater caution. This is pre-post data without a comparison group, so the observed increase could partly stem from the natural maturation of four- to five-year-old children over the course of the study, rather than solely from the board game intervention. Moya-Higueras and colleagues (2023) explicitly included an active control group precisely to account for this possibility, and their surprising results namely, that the control group, whose games were not designed to directly train executive functions, actually showed greater improvement serve as a stark warning to anyone who might attribute cognitive improvements solely to the “board games train the brain” mechanism without a control group.

There is a simple arithmetic issue that needs to be clarified before the figures of ninety percent and eighty-five percent can be interpreted as stable findings. With ten subjects, every single child who moves to a different category shifts the group's percentage by a full ten points. A 90 percent figure on the attention indicator means nine out of ten children, and if even one child classified as “appearing consistent” were actually right on the borderline between two assessment categories, the group figure could drop to 80 percent without any substantive change in the reality of the classroom. This kind of arithmetic fragility is inherent in every classroom-scale action research study and cannot be eliminated through careful analysis; however, it can and should be explicitly communicated to readers, rather than left hidden behind percentage figures that visually appear precise to two decimal places.

Cognitive Mechanisms Versus Sociocultural Scaffolding in Game-Based Learning

Another alternative explanation comes from the sociocultural tradition in early childhood play studies, which emphasizes the role of social mediation rather than individual cognitive training as the true engine of change. From this perspective, the improvements observed in Group A children stemmed from the quality of interactions between the children and the supporting teacher who was consistently present to guide each session rather than solely from the internal cognitive mechanisms of the board game. Gibb and colleagues (2021) bridge the gap between these two explanations when they found that intentional adult engagement was a determining factor in the success of their play program a finding that suggests the question “Is board play effective?” may have been framed incorrectly from the start, and that a more appropriate

question is “what combination of games and guidance is effective, and for children with what characteristics.”

Qualitative data from field notes provide some clues for weighing these two explanations, though not enough to settle the matter conclusively. Teachers reported that children began reminding each other of their turns and helping one another when a classmate forgot the rules a behavior that indicates a process of joint regulation among children rather than a purely cognitive process occurring in isolation within each child’s mind. This pattern of mutual reminders aligns with the sociocultural interpretation, as it suggests that children’s working memory in the context of group play is partly supported by the social scaffolding provided by their peers, rather than purely by individual cognitive capacities operating in isolation.

This social engagement was also reflected in the social-emotional outcomes reported by teachers, including patience in waiting for one’s turn and cooperation among children during the game. These findings are consistent with the structure of the board game itself as a rule-based activity that requires players to refrain from acting out of turn a feature that Vita-Barrull and colleagues (2022b) identified as one source of executive function activation in board games, regardless of the specific cognitive mechanisms that the game nominally targets. In other words, adherence to the turn-taking rules itself not just the game’s cognitive content appears to be an independent source of attention and self-regulation training..

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An earlier study by Vita-Barrull and colleagues (2022a) adds another layer of complexity relevant to cross-cycle adaptation in this study. That study compared the effects of gamification the addition of competitive and reward elements to games on executive dysfunction in children at risk of social exclusion, and found that gamification elements do not always provide additional benefits compared to board games without such elements. This finding is relevant given that the adjustments implemented in the second cycle of this study also included the addition of gradually increasing challenge elements to the game cards a modification that could potentially function as a simple form of gamification. Without a design that isolates the effects of gamification from the effects of reducing the amount of teacher instruction, this study cannot confirm that the addition of these challenge elements and not other concurrent adjustments contributed to the improvements observed in the second cycle.

Integration of Qualitative Data, Curricular Implications, and Epistemological Reflections in Action Research

Another issue worth raising is the representativeness of the “board game” category itself namely, whether this study used games based on Snakes and Ladders, memory card matching, or simple strategy games given that Vita-Barrull and colleagues (2022a, 2022b) have shown that the cognitive profiles of each game differ significantly enough that generalizing across game types risks being misleading. Without a detailed description of the game mechanics used, journal readers cannot assess the replicability of this study, and other researchers cannot replicate the intervention exactly even if they wish to do so. A more adequate revision of the manuscript should include an explicit description of the games, including the number of players, the average duration of a round, and the specific cognitive demands required by the game rules.

The gap revealed throughout this discussion between the technical terminology of executive functions in the international literature and the folk-psychology terminology commonly used by kindergarten teachers in the field ultimately points to a structural problem in the early childhood education teacher training curriculum in Indonesia. Early childhood Islamic education programs at various religious universities, including the one affiliated with the author of this article, generally equip students with general theories of child development without sufficient focus on the rapidly evolving contemporary literature on executive functions over the past decade. As a result, teachers graduating from such programs enter the classroom with a conceptual framework that lags one to two generations of research behind the body of evidence that is actually relevant to their daily practice a gap that cannot be bridged by a single journal article but warrants consideration in future curriculum evaluations for these programs.

There is a question one that predates both cognitive psychology and action research methodology that is worth asking at this point: What exactly is meant by a pedagogical intervention that is “proven” to work? The philosophy of science has long noted that inductive reasoning moving from a number of observed cases to a general claim always carries risks, because a pattern that repeats ten times among ten children in one class does not automatically apply to the eleventh child in another class, let alone to a child in another city with a different background. David Hume, in his classic critique of induction, pointed out that the sun rising every day for thousands of years is no logical guarantee that it will rise tomorrow, even though holding that belief is practically rational. This analogy may seem excessive when applied to a kindergarten board game, but the underlying issue is identical, because “proven” in the context of small-scale action research is most honestly interpreted as “consistent with the expected pattern based on broader evidence,” not as a certainty that rules out other possible explanations.

Putting all eight sources discussed above side by side provides a clearer picture than reading them one by one. Gibb and colleagues (2021) and Bai and colleagues (2022) show that structured play programs whether based on board games or group physical activities can improve preschoolers’ executive functions within eight to ten weeks. Gasteiger and Moeller (2021) narrowed their focus to a specific mechanism the type of dice and demonstrated that such details have a significant impact on numerical outcomes. Vita-Barrull and colleagues (2022a, 2022b) as well as Moya-Higueras and colleagues (2023) collectively argue that the cognitive profile of each game needs to be explicitly mapped, and that intuitive assumptions about which games train which cognitive processes often deviate from empirical reality. Lucchini and colleagues (2024) add that these benefits only become consistently realized when integration is consistently incorporated into classroom routines, rather than as an occasional activity. Wolf and colleagues (2025) conclude this series with a reminder that the instruments used to support all of the above findings must be tested for validity in their own right, rather than assumed to be valid simply because they seem reasonable. This classroom action research, when read alongside these five key arguments, is best positioned as a practitioner’s account whose findings align with the international body of research, yet whose methodological precision remains far from sufficient to stand alone as causal evidence. It serves as supporting evidence consistent with the international body of experimental evidence, rather than a methodological equivalent of that body. The consistency in the direction of these findings remains valuable for pedagogical practice in kindergarten, as it demonstrates that the direction of change observed by teachers in their daily classroom settings aligns with what is found in much more rigorous research designs. Nevertheless, the value of this type of classroom action research lies in its depth of local context and its practical utility for the teachers conducting it, not in its ability to generate generalizable causal knowledge; and the confusion between these two types of value is a major source of problems within the broader genre of Indonesian early childhood education classroom action research.

The practical implications of the above discussion led to concrete recommendations for the professional development of preschool teachers, not just for academic researchers. Teachers who wish to use board games as a learning strategy need to be equipped with a simple framework for assessing a game's cognitive demands before using it in the classroom, rather than selecting games solely based on visual appeal or availability at school. Such a framework need not be as complex as the expert panel method used by Vita-Barrull and colleagues (2022b) to map the cognitive profiles of commercial games, but its underlying principles can be adapted into a much simpler form for example, by asking teachers to identify three questions before a play session begins: What demands does this game place on children's memory? What demands do it place on their patience while waiting for their turn? And what demands does it place on their ability to follow rules that change during the game? These three questions roughly correspond to the constructs of working memory, inhibition, and cognitive flexibility which form the core of executive function models in the literature discussed without requiring teachers to master technical terms from cognitive psychology to use them productively.

Dosage and frequency, as emphasized by Lucchini and colleagues (2024), should also be considered in curriculum planning rather than left solely to teachers' spontaneous initiatives. The two cycles with six sessions per cycle in this study are equivalent to approximately twelve sessions over several weeks, a relatively light dosage compared to the nineteen-week intervention with two weekly sessions used by Moya-Higueras and colleagues (2023) or the eight-week structured game program by Gibb and colleagues (2021). If schools intend to integrate board games more permanently into Group A's curriculum, the question of the minimum dosage required to produce lasting change rather than merely changes apparent at the end of the two PTK cycles remains an unanswered issue in this study's data and warrants prioritization in future research.

The most responsible scientific stance toward the entire set of findings above is neither to reject the practical value of this classroom action research nor to accept it uncritically as definitive evidence. The more appropriate stance is to treat these results as a single local data point that adds weight however small to the general trend already established by more rigorous research designs in various other contexts. Teachers reading this article can feel confident that their policy direction namely, using structured board games with active guidance is grounded in an empirically sound foundation. That confidence should be accompanied by the awareness that this foundation can still be further strengthened through more rigorous replication, and should not be considered final simply because two cycles of classroom action research have produced figures that appear convincing on paper. This kind of awareness, ultimately, is the most fundamental form of intellectual honesty that a PTK article can offer its readers far more valuable than claims of "proven effectiveness" that sound convincing but prove fragile upon closer examination.

Conclusion

This study shows that structured board game sessions, led by teachers in small groups over two cycles, were associated with improvements in attention and working memory abilities among the ten children in Group A at the kindergarten under study, with 90 percent achievement on the attention indicator and 85 percent on the working memory indicator at the end of the second cycle. This pattern of change is consistent with the current international body of evidence regarding the relationship between structured play and executive function in preschoolers, as demonstrated in various research designs with stricter methodological standards. The consistency in the direction of these findings provides a reasonable basis for kindergarten teachers to continue using board games as a learning strategy, with the caveat that the claim that they are “proven effective” should be replaced with a more measured claim: namely, that the collected data supports the hypothesis of benefits without being able to confirm a single causal mechanism behind them.

The main contribution of this article lies in its assertion that the two constructs at the center of this research attention and working memory require precise operationalization before it can be claimed that they have changed, as well as in its explicit acknowledgment of the limitations of a two-cycle action research design in supporting causal claims. The category of “board games,” which has long been treated as a single type of intervention within the Indonesian early childhood education action research (PTK PAUD) genre, needs to be broken down into mechanically distinct families of interventions, following the precedent set by Vita-Barrull and colleagues (2022a, 2022b) and Gasteiger and Moeller (2021), so that future claims of benefits can be traced back to specific game elements rather than stopping at a vague genre label.

Future research should ideally expand on this design by including a comparison group, specifying the types of board games used and their cognitive demands in detail following the precedent set in Table 3, and testing the validity of the observation instrument more formally in accordance with the logic of concurrent triangulation exemplified by Wolf and colleagues (2025). Expanding the sample across several kindergartens with varying socioeconomic backgrounds will help answer the question of to what extent the patterns observed in this one class in Bandung apply more broadly, while long-term follow-up several months after the intervention ends will help distinguish lasting improvements from merely temporary ones, as highlighted by the findings of Gasteiger and Moeller (2021), who tracked the effects of their intervention up to one year later.

In the context of early childhood education grounded in Islamic values which is the scope of this journal findings regarding the role of social mediation and intentional teacher guidance open the door for further research on how the values of patience in waiting for one’s turn, honesty in following rules, and cooperation among children which develop during board game play can be explicitly integrated with the moral education objectives at the kindergarten level. This agenda falls outside the scope of this study’s data, which was not designed to measure moral dimensions directly; however, the structural proximity between rule-based play

discipline and etiquette-based moral discipline serves as a conceptual bridge for early childhood education researchers who wish to pursue this line of inquiry without succumbing to the temptation to claim more than the available data can substantiate.

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