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Systematic Review

Effects of Physical Exercise on Adults with Attention-Deficit/Hyperactivity Disorder: A Systematic Review and Meta-Analysis

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Abstract

Background: Physical exercise has been proposed as a potential non-pharmacological intervention for adults with attention-deficit/hyperactivity disorder (ADHD). However, evidence remains uncertain regarding its effects across cognitive, psychological, and functional domains, particularly when distinguishing immediate responses to acute exercise from sustained benefits of chronic training. Methods: Six electronic databases were searched from inception to 14 August 2026, supplemented by additional sources. Eligible studies included adults with clinically diagnosed ADHD or elevated symptoms identified using validated screening instruments. Acute and chronic exercise interventions were evaluated separately. Acute-exercise effects were synthesized using three-level random-effects meta-analysis with Hedges' g, accounting for correlated sampling errors and applying participant-sample-clustered robust variance estimation (CR2). Outcomes with insufficient independent samples and chronic exercise findings were synthesized narratively. Risk of bias and certainty of evidence were assessed using design-appropriate tools and GRADE, respectively. Results: Twenty-three reports were included (15 acute and eight chronic). The primary acute meta-analysis comprised 75 effect sizes from ten reports representing seven conservatively linked participant-sample clusters. Pooled effects were not statistically significant for executive function ($g = 0.26$, 95% CI: −0.01 to 0.53), attention ($g = 0.13$, 95% CI: −0.06 to 0.31), or psychological outcomes (mood, affect, anxiety, stress, calmness, and motivation) ($g = 0.40$, 95% CI: −0.13 to 0.93). Inhibitory control also showed an uncertain effect ($g = 0.19$, 95% CI: −0.21 to 0.60). Processing speed demonstrated a nominally significant improvement ($g = 0.39$, 95% CI: 0.03 to 0.75), but this finding was based on limited independent evidence. Sensitivity analyses indicated that selected findings were influenced by analytical assumptions. Chronic exercise findings were inconsistent across interventions and outcomes, precluding reliable quantitative pooling. The certainty of evidence was very low across all assessed domains. Conclusions: Current evidence is insufficient to establish consistent benefits of acute exercise or sustained clinical effectiveness of chronic exercise in adults with ADHD. Although improvements in selected outcomes remain possible, methodological limitations and very low certainty of evidence preclude firm conclusions. Larger, rigorously controlled trials assessing clinically meaningful outcomes and long-term effects are needed to clarify the therapeutic potential of exercise in this population.

Keywords: attention-deficit/hyperactivity disorder; adults; acute exercise; chronic exercise

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1. Introduction

Attention-deficit/hyperactivity disorder (ADHD) is a neurodevelopmental disorder characterized by persistent symptoms of inattention and/or hyperactivity–impulsivity that interfere with functioning across multiple domains of daily life [1]. Although ADHD is commonly identified during childhood, symptoms and associated impairments frequently persist into adulthood, contributing to substantial clinical and functional burden [2]. A global systematic review and meta-analysis estimated the prevalence of persistent adult ADHD at approximately 2.6% and symptomatic adult ADHD at 6.8% [3]. Beyond its core symptoms, adult ADHD is associated with difficulties in executive functioning, academic and occupational performance, emotional regulation, interpersonal relationships, and quality of life. These multidimensional impairments underscore the importance of evaluating interventions not only in terms of symptom reduction but also in relation to cognitive, psychological, and everyday functioning.

Pharmacological treatment is a cornerstone of ADHD management; however, complementary non-pharmacological approaches may help address aspects of functioning that are not fully captured by symptom-based treatment outcomes. Physical exercise has attracted increasing research interest because of its potential to influence cognitive performance, psychological well-being, and physical health [4,5]. Evidence from broader populations suggests that exercise may benefit several aspects of cognition, including executive functioning, although the magnitude and consistency of these effects may vary across populations and outcome domains [6]. In ADHD research, an important distinction concerns acute exercise, which involves a single bout and is generally evaluated through immediate or short-term responses, and chronic exercise, which involves repeated training over an extended period. These intervention paradigms differ in exposure duration, implementation requirements, and the types of outcomes they are intended to influence. Accordingly, improvements observed during or shortly after a single exercise session should not be assumed to indicate sustained symptom reduction or clinically meaningful improvements in everyday functioning following repeated training.

Much of the evidence concerning exercise interventions for ADHD has been generated in children and adolescents. Previous systematic reviews and meta-analyses have reported potential improvements in ADHD symptoms, executive functions, motor performance, and psychosocial outcomes among younger populations [7–9]. However, the applicability of these findings to adults remains uncertain. ADHD symptom presentation, functional responsibilities, medication use, and the practical demands of maintaining exercise participation may differ across developmental stages. Adults must often manage exercise participation alongside occupational, educational, and family responsibilities, potentially influencing intervention feasibility and adherence. Consequently, adult-specific evidence is needed to determine whether exercise-related changes observed in younger populations extend to clinically relevant outcomes in adulthood.

Existing systematic reviews of exercise in adults with ADHD have primarily examined core symptoms and selected aspects of executive functioning [10–12]. Yang et al. focused on inhibitory control [13], while Xu et al. synthesized evidence concerning executive functions and core ADHD symptoms following acute and chronic exercise [14]. These reviews have contributed to understanding the potential effects of exercise, but several issues warrant further consideration. First, outcomes beyond core symptoms and executive functioning, including psychological well-being, physical function, and quality of life, have received comparatively limited attention in quantitative evidence synthesis. Physical function captures potential general-health gains, such as better fitness or movement capacity. Quality of life captures the perceived impact on daily life and complements ADHD symptom and impairment measures. Improvement in physical function or quality of life alone cannot

establish a reduction in ADHD-related impairment. Second, studies frequently report multiple outcomes, experimental conditions, or assessments derived from the same participants, and related publications may draw on overlapping samples. Failure to adequately account for these dependencies may overstate the amount of independent evidence and affect the precision of pooled estimates. Third, differences in study design, comparator conditions, outcome selection, and effect-size standardization can complicate interpretation and comparison across syntheses. These considerations highlight the need for a comprehensive evaluation that distinguishes the breadth of reported outcomes from the strength and independence of the underlying evidence.

Therefore, this systematic review aimed to critically evaluate the effects of acute and chronic exercise on clinical, cognitive, psychological, and functional outcomes in adults with clinically diagnosed ADHD or elevated ADHD symptoms identified using validated screening instruments. Building on previous reviews, we examined a broader range of outcome domains while explicitly considering study design, comparator type, overlapping participant samples, and dependencies among multiple effect sizes. For acute exercise, controlled comparisons were synthesized using three-level random-effects models incorporating correlated sampling errors and cluster-robust statistical inference, where sufficient independent evidence was available. Outcomes supported by sparse independent samples, as well as the heterogeneous chronic-exercise evidence, were evaluated narratively. By integrating these methodological considerations with assessments of risk of bias and certainty of evidence, this review sought to clarify the extent to which existing findings support reliable short-term exercise effects and sustained clinical benefits in adults with ADHD.

2. Methods

2.1. Protocol

This systematic review and meta-analysis was conducted and reported in accordance with the Preferred Reporting Items for Systematic Reviews and Meta-Analyses (PRISMA) 2020 statement [15]. The completed PRISMA checklist is provided in Supplementary Material File S1 Table S1. The review protocol was prospectively registered in the International Prospective Register of Systematic Reviews (PROSPERO; CRD420261478379).

2.2. Eligibility Criteria

Eligibility criteria were established according to the population, intervention, comparator, outcomes, and study design (PICOS) framework. Eligibility for inclusion in the systematic review was distinguished from eligibility for the primary quantitative synthesis, as not all included intervention studies provided comparisons or data suitable for meta-analysis.

2.2.1. Population

Studies were eligible if they included adults aged ≥ 18 years with either clinically diagnosed ADHD or elevated ADHD symptoms identified using validated screening or symptom-rating instruments. Clinical diagnosis was established through specialist assessment, a structured or semi-structured diagnostic interview, or documentation of a previous clinical diagnosis. Participants identified solely through screening instruments were distinguished from those with clinically confirmed ADHD, as screening positivity does not necessarily establish a clinical diagnosis.

Studies involving mixed-age populations were eligible only when data for adult participants could be extracted separately. For studies that also recruited healthy controls, only data from participants with ADHD or elevated ADHD symptoms were considered for the

ADHD-specific quantitative synthesis. Sensitivity analyses restricted to clinically diagnosed ADHD excluded screening-positive and mixed-ascertainment samples.

Medication use was not an exclusion criterion. Participant age at assessment, sex, medication status, and ADHD ascertainment method were extracted when available. Because age at initial ADHD diagnosis was inconsistently reported, it was not examined as an effect modifier, and the available evidence could not establish whether age at diagnosis influenced exercise-related outcomes.

2.2.2. Interventions

Eligible interventions involved structured or purposeful physical exercise delivered either as a single bout or through repeated training. Exercise modalities included aerobic exercise, cycling, walking or running, high-intensity interval training, resistance or combined exercise, Pilates, yoga, Tai Chi, coordination-based exercise, and multicomponent training programs.

Acute exercise was defined as a discrete exercise bout designed to assess immediate or short-term responses during or following exercise. Studies involving repeated laboratory visits were classified as acute-exercise investigations when each visit represented a distinct exercise bout with an eligible comparison. For the primary quantitative synthesis, the earliest eligible assessment for each task-specific endpoint and exercise condition was retained to minimize dependence arising from repeated measurements of the same outcome. Subsequent assessments were not treated as additional primary effect sizes. Symptom and psychological outcomes assessed on the following morning were identified separately and excluded in a sensitivity analysis examining the influence of assessment timing.

Chronic exercise was operationally defined as repeated training lasting at least four weeks. Consistent with earlier reviews that distinguished single-bout from repeated exercise [13,14], this lower bound required several weeks of training exposure while retaining short structured programmes. The four-week cutoff is an eligibility rule, and the minimum duration needed for sustained clinical benefit remains unknown. Shorter programmes were eligible only if they provided a separately extractable single-bout comparison.

2.2.3. Comparator

The primary quantitative synthesis was restricted to comparisons between exercise and non-exercise conditions. Eligible comparators included seated rest, sedentary attention-control conditions, wait-list controls, and treatment as usual. These conditions were distinguished because they may differ in participant expectations, contact time, and background care.

Studies comparing different exercise modalities or intensities, walking in natural versus urban environments, or exercise with pharmacological treatment were retained for narrative synthesis. Such comparisons were not interpreted as estimates of exercise efficacy relative to inactivity because they addressed different intervention contrasts. Similarly, uncontrolled pre–post findings were synthesized narratively, as observed changes could not be reliably distinguished from practice effects, expectancy effects, or other time-related influences.

2.2.4. Outcomes

Eligible outcomes encompassed ADHD core symptoms, cognitive performance, psychological functioning, and broader physical or functional health.

For acute exercise, outcomes were classified into five domains: (1) ADHD core symptoms, (2) executive function, (3) attention, (4) psychological outcomes, and (5) motor activity. Executive-function outcomes were further categorized into inhibitory control, working memory, processing speed, and response-time variability. Outcome measures were assigned to domains according to the cognitive or behavioral construct primarily assessed. Specifically, the concentration score (edited targets minus errors) from the editing task was

classified as an attention outcome; general trial reaction-time measures were categorized as processing speed; and Flanker interference and Stroop incongruent-condition outcomes were classified as inhibitory control. To avoid retaining closely related measures of the same performance construct as separate primary effects, complementary accuracy and omission outcomes from continuous performance tests (CPTs) were not simultaneously included. Adaptive stop-task success rates and non-monotonic response-style measures were excluded from primary efficacy estimation when their direction could not be consistently interpreted as indicating better or worse performance. Psychological outcomes included measures of mood, affect, anxiety, stress, calmness, and motivation. Subjective stimulant-like responses assessed using the Addiction Research Center Inventory (ARCI) were excluded from the primary psychological domain because they were not considered directly comparable with the other psychological outcomes.

For chronic exercise, outcomes were evaluated separately and included ADHD symptom severity, cognitive and attention-related measures, psychological outcomes, physical function, quality of life, sleep, and global clinical severity. Given the limited number of independent and sufficiently comparable trials, chronic-exercise outcomes were synthesized narratively rather than combined into a common pooled treatment-effect estimate.

2.2.5. Study Design

Randomized parallel-group trials, randomized crossover studies, counterbalanced experimental studies, non-randomized controlled interventions, and prospective pre–post studies were eligible for the systematic review. However, inclusion in the systematic review did not automatically establish eligibility for quantitative synthesis. The primary acute-exercise meta-analysis required an exercise-versus-non-exercise contrast within the eligible adult ADHD population, together with sufficient numerical information to calculate a standardized effect size and its sampling variance. Accordingly, uncontrolled studies and investigations involving only active exercise comparators were retained for narrative synthesis rather than combined with controlled efficacy estimates.

Reports were excluded from the systematic review if they (1) did not provide separable data for eligible adult participants; (2) did not investigate structured physical exercise; (3) met neither the acute- nor the chronic-exercise exposure definition; (4) reported no eligible outcomes; (5) did not present original intervention data; or (6) lacked an accessible complete report. Reports meeting the review-level eligibility criteria but lacking an appropriate exercise-versus-non-exercise contrast or sufficient quantitative information were retained in the narrative synthesis and excluded only from the primary meta-analysis.

2.3. Search Strategy

A systematic literature search was conducted in six electronic databases—PubMed, Embase, Web of Science, the Cochrane Central Register of Controlled Trials (CENTRAL), SPORTDiscus, and the China National Knowledge Infrastructure (CNKI)—from inception to August 14, 2026. Search strategies combined controlled vocabulary and free-text terms, adapted to the indexing systems and search functions of individual databases. Medical Subject Headings (MeSH) and Emtree terms were incorporated where applicable, alongside relevant title, abstract, keyword, and topic-field terms.

The search strategy encompassed three principal concepts: attention-deficit/hyperactivity disorder (ADHD), physical exercise or activity, and adult populations. ADHD-related terms included "attention deficit hyperactivity disorder," "attention-deficit/hyperactivity disorder," and "ADHD." Exercise-related terms included "physical activity," "exercise," "aerobic exercise," "cycling," "walking," "running," "high-intensity interval training," "resistance training," "yoga," "Pilates," "Tai Chi," and "coordination training." Population-

related terms included "adult," "young adult," "university student," and related expressions. Synonymous terms were combined using the Boolean operator OR, and the three search concepts were combined using AND. No restrictions were imposed on outcome domains to facilitate the identification of studies assessing diverse clinical, cognitive, psychological, physical, and functional outcomes.

Electronic database searches were supplemented by searches of Google Scholar and manual screening of reference lists from relevant publications. These supplementary searches identified three additional records: two through Google Scholar and one through reference-list screening. The available database-specific search strategies, search yields, and limitations of the retained search documentation are presented in Supplementary Material File S1 Table S2.

2.4. Study Selection

Database records were managed and deduplicated in EndNote X9 (Clarivate Analytics). Two reviewers independently screened the retained database titles and abstracts against the eligibility criteria. Records considered potentially eligible by either reviewer were retrieved for full-text assessment. The same reviewers independently assessed the full texts to determine final eligibility, with disagreements resolved through discussion and consensus.

Inter-rater agreement between the two reviewers was assessed separately for the title/abstract screening and full-text review stages using Cohen's kappa coefficient. Cohen's kappa values >0.80 were interpreted as indicating almost perfect inter-rater agreement [16].

2.5. Data Extraction and Effect-Size Calculation

The available data-extraction records were verified against the original study reports to ensure consistency in study design, participant characteristics, ADHD ascertainment, intervention and comparator conditions, outcome measures, assessment timing, and numerical data. Where available, means and standard deviations (SDs) were extracted directly. When SDs were not reported, they were estimated from standard errors (SEs) or 95% confidence intervals (CIs) using established statistical conversions. Alternative conversion assumptions were examined in sensitivity analyses where appropriate. Numerical data extracted from published figures were considered approximate, and their influence was assessed by excluding graph-derived estimates in a sensitivity analysis.

Potential participant overlap across reports was examined using trial registration identifiers, recruitment settings, study protocols, participant characteristics, and reported outcome data. Huan et al. [17] and the two reports by Shi et al. [18,19] shared the trial registration ChiCTR2300067833 and closely matching participant characteristics and were therefore treated as reports from the same trial. Duplicated Stroop outcomes were included only once, while distinct n-back and Positive and Negative Affect Schedule (PANAS) outcomes were retained. The two reports by Mehren et al. [20,21] were assigned to the same participant-sample cluster because the later publication identified their association with a larger research project. Wang et al. [10] and Kuo et al. [11] shared recruitment settings and protocol characteristics, although participant overlap could not be confirmed. These reports were conservatively assigned to the same cluster in the primary analysis, with separate clustering examined in a sensitivity analysis. Thus, participant-sample linkage represented a conservative approach to accounting for potential statistical dependence rather than confirmation of identical participants across all linked reports.

For the quantitative synthesis, effect sizes were expressed as Hedges' g, with standardization based on marginal outcome-score variability rather than the variability of change scores or paired differences. For crossover and other paired comparisons, the

exercise–control mean difference was standardized using the square root of the average marginal variances of the two conditions. Effect sizes, small-sample corrections, and sampling variances were calculated using the SMCRPH option in the metafor package. When the within-person correlation between exercise and control measurements was not reported, a value of 0.50 was assumed, with alternative values of 0.30 and 0.70 examined in sensitivity analyses. For independent parallel-group comparisons, post-intervention mean differences were standardized using the pooled between-group SD and Hedges' small-sample correction, implemented using the standardized mean difference (SMD) option in metafor. Data labeled as pre- and post-intervention measurements in the original extraction file were interpreted as control and exercise conditions only when this interpretation was supported by the corresponding source report.

Effect directions were harmonized so that positive values consistently indicated outcomes favoring exercise, including lower symptom severity, fewer errors, faster responses where appropriate, or improved cognitive or psychological performance. Because post-intervention comparisons may be influenced by baseline differences, an additional sensitivity analysis excluded reports for which the original numerical extraction involved controlled pre–post comparisons rather than paired exercise–control condition measurements.

Two distinct sources of correlation were considered. First, the within-person correlation between exercise and control measurements was incorporated into the calculation of individual paired-effect sampling variances and small-sample corrections. Second, potential correlations among the sampling errors of multiple effect sizes from the same participant-sample cluster were represented using a working sampling covariance matrix. The primary analysis assumed a within-cluster sampling-error correlation of 0.50, with alternative values of 0.20 and 0.80 examined in sensitivity analyses. Covariances were derived from the assumed correlations and the corresponding sampling variances, while sampling covariances between distinct clusters were set to zero. Importantly, correlations between repeated pre–post measurements were not treated as interchangeable with correlations between paired exercise and control conditions.

2.6. Risk of Bias Assessment

Risk of bias was assessed using design-specific assessment tools. Randomized studies were evaluated using the revised Cochrane Risk of Bias tool (RoB 2) [22], which considers bias arising from the randomization process, deviations from intended interventions, missing outcome data, outcome measurement, and selection of reported results. For crossover trials, additional considerations regarding intervention sequence, period effects, and potential carryover effects were incorporated where applicable. Overall risk-of-bias judgments were classified as low risk, some concerns, or high risk.

Non-randomized intervention studies were assessed using the Risk of Bias in Non-randomized Studies of Interventions (ROBINS-I) tool [23]. The assessment covered seven domains: confounding, participant selection, intervention classification, deviations from intended interventions, missing data, outcome measurement, and selection of reported results. Overall judgments were categorized as low, moderate, serious, or critical risk of bias, or no information, as appropriate.

2.7. Certainty of Evidence

The certainty of evidence was evaluated for each outcome domain using the Grading of Recommendations Assessment, Development and Evaluation (GRADE) approach [24]. Assessments considered five domains: risk of bias, inconsistency, indirectness, imprecision, and publication bias. Risk-of-bias judgments were informed by the RoB 2 and ROBINS-

I assessments, while inconsistency was evaluated in relation to variability across study findings and, where available, estimates of statistical heterogeneity.

Imprecision was assessed by considering the width of confidence intervals, the number of independent participant-sample clusters, and the stability of statistical inference. Prediction intervals, when available, were used to inform the interpretation of potential variation in effects across settings, rather than as an automatic or additional reason for downgrading certainty. Indirectness was assessed with particular attention to differences between clinically diagnosed and screening-positive ADHD populations and the applicability of laboratory-based outcomes to everyday clinical functioning. Potential publication bias was considered in light of the available evidence, recognizing the limited informativeness of formal asymmetry tests when few independent samples were available. Certainty ratings were classified as high, moderate, low, or very low.

2.8. Statistical Analysis

For acute exercise outcomes, three-level random-effects meta-analyses were performed using restricted maximum likelihood (REML) estimation to account for multiple effect sizes nested within linked participant-sample clusters. A working sampling covariance matrix was incorporated to account for potential correlations among effect sizes derived from the same participant sample. Cluster-robust variance estimation with bias-reduced linearization (CR2) was applied at the participant-sample level, with Satterthwaite-adjusted degrees of freedom used to calculate two-sided *p*-values and 95% CIs. Separate models were fitted for each outcome domain and executive-function subdomain when sufficient independent samples were available. No overall effect estimate combining different outcome domains was calculated.

For outcomes represented by fewer than three independent participant-sample clusters, findings were primarily synthesized narratively. Exploratory quantitative analyses of working memory and response-time variability, each supported by only two participant-sample clusters, were conducted separately. For these analyses, multiple effects within each sample were first combined into a covariance-adjusted composite estimate, followed by two-sample random-effects meta-analysis using REML estimation and modified Knapp–Hartung inference. Where at least three independent sample clusters were available, approximate 95% prediction intervals were calculated to illustrate potential variation in effects across future settings. Given the limited number of independent samples, these intervals were interpreted cautiously.

Sensitivity analyses were conducted to examine the robustness of the findings to methodological and statistical assumptions. These analyses included varying the assumed within-person correlations for paired comparisons and the working sampling-error correlations, restricting analyses to randomized studies or clinically diagnosed ADHD samples, excluding graph-digitized estimates and next-morning assessments, and examining alternative clustering assumptions for the Wang and Kuo reports [10,11]. Additional analyses evaluated alternative sample sizes reported for the Rassovsky study [25] and different methods of converting confidence intervals to standard deviations. Leave-one-cluster-out analyses were performed to assess the influence of individual participant-sample clusters, rather than individual publications, on pooled estimates.

Exploratory meta-regression analyses were conducted for executive-function outcomes to examine potential differences according to ADHD ascertainment method (clinically diagnosed versus screening-positive samples, excluding mixed-ascertainment samples) and study design (randomized versus non-randomized). Other potential moderators were not formally examined because the limited number of independent samples and incomplete or heterogeneous reporting would have produced unreliable estimates. Formal tests for funnel

plot asymmetry were not performed when fewer than ten independent participant-sample clusters contributed to an analysis.

Chronic-exercise findings were synthesized narratively according to intervention, comparator, and outcome domain. Quantitative pooling was not undertaken because the available studies were limited in number and differed substantially in intervention characteristics, comparison conditions, assessment methods, and outcome measures, precluding a sufficiently interpretable common treatment-effect estimate.

All quantitative analyses were performed using the metafor and clubSandwich packages in R version 4.5.3.

3. Results

3.1. Study Selection

The database searches identified 2161 records across PubMed (n = 874), Embase (n = 642), Web of Science (n = 277), CENTRAL (n = 143), CNKI (n = 32), and SPORTDiscus (n = 193). After duplicate removal, 358 records underwent title and abstract screening, of which 316 were excluded because of ineligible populations (n = 246), interventions (n = 54), or outcomes (n = 16). The remaining 42 database records, together with three additional records identified through supplementary searches (two through Google Scholar and one through reference-list screening), yielded 45 reports sought for retrieval.

Of 45 reports sought for retrieval, one was unavailable. Of 44 full texts assessed, 21 were excluded because of an ineligible population (n = 9), an ineligible exercise intervention (n = 10), or the absence of an eligible control or comparator (n = 2). The review included 23 reports (15 acute and eight chronic). Ten acute reports representing seven conservatively linked participant-sample clusters contributed to the primary meta-analysis. The remaining eligible findings, including those from all 8 chronic-exercise reports, were synthesized narratively. The study-selection process is illustrated in Figure 1.

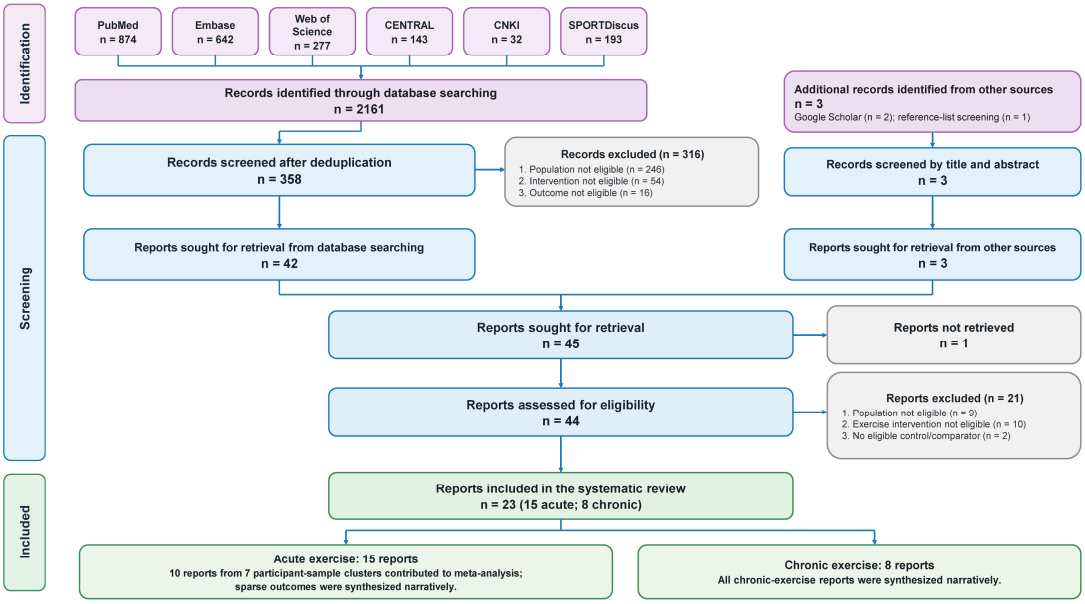

Figure 1. PRISMA flow diagram of study selection.

Inter-rater agreement between the two reviewers was high throughout the study-selection process (Supplementary Material File S1 Table S3). The Cohen's kappa coefficient was 0.83 (95% CI: 0.75–0.92) for title and abstract screening and 0.91 (95% CI: 0.79–1.00) for full-text review.

3.2. Characteristics of Included Studies

A total of 23 reports met the eligibility criteria, comprising 15 acute-exercise and 8 chronic-exercise reports (Tables 1 and 2). Across all 15 acute reports, ADHD or screening-positive sample sizes ranged from 10 to 82, summing to 413 at the report level. This sum uses 13 participants for Rassovsky et al. [25] after the reported exclusion and omits healthy comparator groups. Overlapping reports and incomplete linkage information precluded an exact unique-participant count. Of the acute-exercise reports, 10 contributed 75 effect sizes to the primary meta-analysis, representing 7 conservatively linked participant-sample clusters. The reported analyzed sample sizes ranged from 18 to 36, with a cumulative report-level sample count of 248. However, because several publications originated from the same or potentially overlapping participant samples, this cumulative count does not represent the number of unique participants. Under the specified sample-linkage assumptions, the estimated number of distinct participants ranged from 184 to 228, although the exact total could not be determined. Studies also varied in ADHD ascertainment methods, medication status, and recruitment settings.

Acute-exercise interventions included cycling, treadmill-based exercise, other aerobic activities, yoga, and walking in different environments (Table 1). Outcome assessments were conducted during exercise, immediately after exercise, following a recovery period, or on the subsequent morning. For the primary quantitative synthesis, the earliest eligible assessment for each outcome and exercise condition was selected. Huan et al. [17] and Shi et al. [18,19] reported complementary outcomes from the same registered trial, with duplicated Stroop results included only once. Other linked or potentially overlapping reports were accounted for through participant-sample clustering.

Chronic-exercise interventions varied considerably in modality, intensity, duration, and frequency (Table 2). Programs included multicomponent exercise combining aerobic, resistance, and flexibility training; Pilates; yoga; Tai Chi; aerobic training; progressive aerobic interval training; and coordination-based exercise. Reported exercise intensities generally ranged from moderate to vigorous, including approximately 60–90% of maximum heart rate (HRmax) in the START programs and progression from 50–60% to 70–80% HRmax in the aerobic interval intervention. Sessions typically lasted 45–90 min and were delivered two to three times per week, although some interventions also incorporated unsupervised exercise or home-based practice. Comparator conditions included treatment as usual, wait-list or inactive controls, continuation of usual treatment, pharmacotherapy combined with ADHD education, and no coordination training.

The eight chronic-exercise reports included three publications associated with the START program [26–28], all referencing the same trial registration (NCT05049239). The 2025 and 2026 publications reported complementary outcomes from the same main trial involving 63 participants, whereas the 2023 publication described a 14-participant pilot study. Whether participants from the pilot study were subsequently included in the main trial could not be established. Accordingly, these publications were not considered three independent trials. Across the eight reports, the sample sizes used for the report-level summary ranged from 14 to 63 and summed to 316. The total counts both main START reports at their shared parent-trial size and combines enrolled and analyzed samples where these were the counts used in Table 2. A reliable unique-participant total was unavailable. Most chronic-exercise reports involved clinically diagnosed ADHD populations, whereas Fritz and O'Connor [29] recruited women identified through symptom screening. Additional study characteristics, participant counts, and outcome availability are provided in Supplementary Material File S1 Table S4.

Table 1. Characteristics of included acute exercise reports.

Study	Country/ Region	Design	Participants	ADHD Ascertainment/Medication	Modality	Intensity	Session Duration	Frequency	Comparator	Assessment Timing	Eligible Outcomes
Wang 2024 [10]	Taiwan, China	Randomized within-subject crossover	n = 24; 15 M/9 F; age 21.5 ± 2.3 y	Psychiatrist-confirmed ADHD using Conners' Adult ADHD Diagnostic Interview; not receiving ADHD medication	Stationary cycling	Low: 30% HRR; moderate: 50–60% HRR; high: 70–80% HRR	30 min (5-min warm-up + 20-min exercise + 5-min cool-down)	Single acute session per condition; ≥1-week washout	Seated on bicycle while watching a nature documentary	Pre and immediately post	SST (SSRT, go RT); cortical excitability
Huan 2024 [17]	China	Randomized crossover controlled trial	n = 20; young adults; age 20.9 ± 2.71 y	ASRS inattentive symptom score ≥ 17	HIIT cycling and MICT cycling	HIIT: 80%/30% VO_2max alternating 1 min each; MICT: 55% VO_2max	20 min	Single acute session per condition; 7-day washout	30-min sedentary control	Pre, 15-min post, and 45-min post	Working memory (1-back/2-back accuracy and RT); mood; prefrontal HbO
Shi 2025a [18]	China	Randomized crossover controlled trial	n = 20; all male; age 20.90 ± 2.71 y	ASRS inattentive subscale ≥ 17; mean ASRS 20.05 ± 3.38	HIIT cycling and energy-expenditure-matched MICT cycling	MICT ≈75.92% HRmax; HIIT peak ≈ 88.81% HRmax	Not explicitly reported	Single acute session per condition; ≥7-day washout	Sedentary rest	Pre, 20-min post, and 50-min post	Color-word Stroop accuracy/RT; prefrontal HbO
Shi 2025b [19]	China	Randomized crossover controlled trial	n = 20 male college students	ASRS screening for attention-deficit symptoms	MICT and HIIT cycling	Not explicitly reported	Not explicitly reported	Single acute session per condition; ≥7-day washout	Sedentary control	Pre, 20-min post, and 50-min post	Color-word Stroop accuracy/RT; heart-rate variability
Kuo 2024 [11]	Taiwan, China	Randomized, counterbalanced within-subject crossover	ADHD n = 26; 16 M/10 F; age 23.5 ± 3.2 y (plus 26 matched healthy controls)	Psychiatrist-confirmed ADHD using Conners' Adult ADHD Diagnostic Interview; not receiving ADHD medication	Stationary cycling	50–70% HRreserve	30 min (5-min warm-up + 20-min exercise + 5-min cool-down)	Single acute session; sessions ≥ 1 week apart	Seated on bicycle watching nature documentaries for 30 min	Cognitive testing within 10 min post	SST; SRTT motor learning; TMS
Dinu 2023 [30]	UK	Randomized parallel-group trial (cycling vs. Hatha yoga)	ADHD n = 82; 68 female (83%); age 26.62 ± 5.26 y (plus 77 healthy controls)	Existing clinician diagnosis plus ASRS-A ≥ 14; 50 medicated, 32 unmedicated	Moderate cycling or beginner Hatha yoga	Cycling ≈ 59% HRmax in ADHD; yoga ≈ 38% HRmax	10 min	Single acute session	Other active exercise arm; no non-exercise control	Immediately pre and post exercise	TOVA attention/motor impulsivity; delay discounting; Iowa Gambling Task; actigraphy

Table 1. *Cont.*

Study	Country/ Region	Design	Participants	ADHD Ascertainment/Medication	Modality	Intensity	Session Duration	Frequency	Comparator	Assessment Timing	Eligible Outcomes
LaCount 2022 [12]	USA	Counterbalanced within-subject condition comparison	ADHD n = 18; 9 male; age 20.8 ± 1.7 y (plus 18 matched non-ADHD controls)	DSM-5 ADHD via Adult ADHD Diagnostic Interview; stimulant use reported	HIIT on cycle ergometer	Target ≥ 85% HRmax or RPE 17	19 min (3-min warm-up + 16-min HIIT)	Single acute session	Non-HIIT laboratory session	CPT after 15-min recovery; BAARS-M/DASS-M next morning	AX-CPT errors, RT and RTV; ADHD symptoms; depression, anxiety, stress
Kallweit 2021 [31]	Germany	Counterbalanced within-subject standard vs. experimental condition	ADHD n = 36; 20 M/16 F; age 31.3 ± 8.88 y (plus 36 healthy controls)	Adult ADHD confirmed using DSM-IV-based diagnostic procedures and structured instruments; stimulant/atomoxetine use reported	Bicycle ergometer exercise	After 3 min, ≥70% max pulse; then 10 min at 70–85% max pulse	13 min	Single acute session; assessments separated by 1 week	Standard condition without pre-task cycling	Exercise immediately before editing task	Selective-attention KL score; task-specific inattention/hyperactivity; concentration/calmness ratings
Mehren 2019a, Go/No-go [20]	Germany	Counterbalanced within-subject exercise vs. active-control condition	ADHD n = 20 final; 3 female; age 31.4 ± 9.6 y (plus 20 matched healthy controls)	Specialist psychiatrist diagnosis based on DSM-IV; stimulant-treated patients paused medication ≥ 48 h	Stationary cycling	Target 50–70% individual HRmax; mean ≈ 66.4% HRmax	30 min	Single acute session	30-min active movie/social-cognition control	Go/No-go began ≈ 20.9 ± 1.8 min after exercise/control	Go/No-go d-prime, hit rate, inhibition rate, RT, RT variability; fMRI
Mehren 2019b, Flanker [21]	Germany	Counterbalanced within-subject exercise vs. active-control crossover	ADHD n = 20 final; 4 F/16 M; age 29.9 ± 9.5 y (plus 20 matched healthy controls)	Specialist-confirmed DSM-IV ADHD; stimulant-treated patients discontinued medication ≥ 48 h	Continuous stationary cycling	Target 50–70% individual HRmax	30 min	Single acute session	≈30-min movie/social-cognition active control	Flanker task began ≈ 9.8 ± 1.8 min after exercise/control	Flanker RT/interference, RT variability, error/omission rates; fMRI
Rassovsky 2019 [25]	Israel	Counterbalanced within-subject sitting vs. treadmill-walking condition	31 students initially; 14 formally diagnosed ADHD and 17 controls; one 42-y ADHD participant excluded	Formal neurologist/psychiatrist ADHD diagnosis; 71% had stimulant prescriptions and abstained on experiment day	Treadmill walking	5 km/h	14 min	Single acute session	Seated CATA condition	Attention assessed during exercise; order counterbalanced	CATA detectability, hit RT, omission/commission errors, response style

Table 1. *Cont.*

Study	Country/ Region	Design	Participants	ADHD Ascertainment/Medication	Modality	Intensity	Session Duration	Frequency	Comparator	Assessment Timing	Eligible Outcomes
Fritz 2016 [32]	USA	Block-randomized within-participant crossover	n = 32 men; age 18–33 y; mean 21 ± 3 y	Screen-positive for adult ADHD symptoms using ASRS-v1.1; 2 had formal ADHD diagnosis; no current CNS-acting medication	Cycle ergometry	Designed to elicit 65% VO_2peak	20 min	Single acute session per condition	20-min seated-rest control on cycle ergometer	Repeated assessments at baseline and Post 1/Post 2; selected outcomes through Post 3	CPT and Bakan vigilance; simple RT; leg accelerometry; POMS mood; ARCI amphetamine scale; motivation
Gapin 2015 [33]	USA	Single-group pre–post within ADHD; non-ADHD comparison cohort also exercised	ADHD n = 10; 5 F/5 M; age 21.1 ± 2.02 y (plus 10 non-ADHD controls)	Written documentation of ADHD diagnosis required; 80% reported ADHD medication use	Supervised treadmill aerobic exercise	50–65% HRR	40 min (5-min warm-up + 30-min exercise + 5-min cool-down)	Single acute session	No non-exercise control; comparison cohort also completed same exercise bout	Cognitive tests immediately pre and post exercise	Stroop Color/Color-Word; TMT-A/B; Digit Span Forward/Backward; serum BDNF
Thal 2014 [34]	USA	Parallel field trial; alternating assignment to nature vs. urban walk	n = 40 university students with ADHD; 20 nature-walk and 20 urban-walk	Professionally diagnosed with ADHD at some point; medication use recorded	Self-paced walking	Self-paced; no objective intensity criterion reported	20 min	Single acute session	20-min individual walk in an urbanized street setting	Cognitive tests and symptom questionnaire immediately before and after walk	Digit Span Backward; Stroop incongruent latency/% correct; self-reported ADHD symptoms
Jones 2015 [35]	USA	Randomized parallel-group controlled trial	n = 32 young adults aged 18–25 y; exercise n = 18, control n = 14	Prior ADHD diagnosis or clinical ADHD characteristics on ASRS-v1.1; CNS-active medication-free for ≥1 day	Stationary cycling	Mean 57.16% HRR; RPE 12.44	30 min	Single acute session	30-min sedentary reading condition	Executive-function tests immediately pre and post assigned condition	Stroop Color-Word; WAIS Digit Span Backward; STOP-IT SSRT

Note. Linked reports are not independent samples; analytic placement is detailed in Supplementary Material File S1 Table S4. Abbreviations: ADHD, attention-deficit/hyperactivity disorder; ASRS, Adult ADHD Self-Report Scale; BAARS-IV, Barkley Adult ADHD Rating Scale-IV; BFIS, Barkley Functional Impairment Scale; CATA, Continuous Attention Test Amsterdam; CPT, Continuous Performance Test; HbO, oxygenated hemoglobin; HIIT, high-intensity interval training; HRmax, maximum heart rate; HRR, heart-rate reserve; MICT, moderate-intensity continuous training; RPE, rating of perceived exertion; RT, reaction time; RTV, reaction-time variability; SRTT, Serial Reaction Time Task; SSRT, stop-signal reaction time; SST, stop-signal task; TMT, Trail Making Test; TOVA, Test of Variables of Attention; VO_2max/VO_2peak, maximal/peak oxygen uptake.

Table 2. Characteristics of included chronic exercise reports.

Study	Country	Design	Participants	ADHD Ascertainment	Modality	Intensity	Session Duration	Frequency/Intervention Duration	Comparator	Outcomes/Time Points
Svedell 2023 [28]	Sweden	Randomized controlled pilot trial, 2:1 allocation; START programme NCT05049239	Exercise n = 9, TAU n = 5; age 33.7 ± 8.1 vs. 43.0 ± 12.6 y; 6F/3M vs. 3F/2M	Structured neuropsychiatric evaluation by multidisciplinary team; stable medication permitted	Multicomponent: cardio, resistance, and flexibility	60–90% HRmax	50 min	3 sessions/week for 12 weeks	Treatment as usual (TAU)	ASRS, CGI-S/CGI-I, PGI-I, EQ-VAS, MADRS-S, ISI, cognitive tasks, body awareness, physical fitness; baseline, week 6, week 12
Kouhbanani 2023 [36]	Iran	Randomized controlled trial with 6-month follow-up	Analyzed: Pilates n = 25, control n = 27; all female; age 35.24 ± 11.49 vs. 35.40 ± 11.01 y	DSM-5 ADHD; CAARS ≥ 65; IQ ≥80; age 20–50 y	Contemporary Pilates	Progressive intensity; exact criterion not reported	45 min	3 sessions/week for 24 weeks	Pre-existing/usual treatment maintained	WCST attention switching and CPT sustained attention; pretest, posttest, 6-month follow-up
Svedell 2025 [27]	Sweden	Parallel randomized controlled trial, 2:1 allocation; START NCT05049239	START n = 43, TAU n = 20; overall age 36.1 ± 9.7 y; 42F/21M	DSM-5/Swedish guideline diagnosis confirmed through specialist neuropsychological evaluation	Multicomponent: endurance, strength, flexibility, and mindful body focus	60–90% HRmax	50 min supervised sessions	≥2 supervised sessions/week plus additional activity; goal ≥150 min/week for 12 weeks	Treatment as usual (TAU)	ASRS total/inattention/hyperactivity, CGI-S/CGI-I, PGI-I, EQ-VAS, ISI; baseline, week 6, week 12
Svedell 2026 [26]	Sweden	Parallel randomized controlled trial, 2:1 allocation	Parent trial: START n = 43, TAU n = 20; body-awareness complete cases n = 26 vs. n = 13; movement-quality total n = 38	Same clinically confirmed adult ADHD population as START parent trial	Same START multicomponent mixed-exercise intervention	60–90% HRmax	50 min supervised sessions	Same as START parent trial; 12 weeks	Treatment as usual (TAU)	BAS MQ-E movement quality and body-awareness total/subscales; baseline and post-intervention
Fritz & O'Connor 2022 [29]	USA	Pilot randomized controlled trial	Yoga n = 16, wait-list n = 16; all female; overall age 20.16 ± 1.46 y (18–24); 27/32 completed	ASRS-5 screen positive (score ≥ 14); clinician-confirmed ADHD not required; regular prescription medication excluded	Bikram yoga	Perceived as hard; no objective intensity criterion reported	90 min	2 sessions/week for 6 weeks	Wait-list control; maintain usual activity and avoid yoga	NIH Toolbox Flanker, DCCS, working memory; anxiety, POMS mood, BFIS functional impairment, mindfulness, hyperactivity, leg strength; baseline, week 3, week 6

Table 2. *Cont.*

Study	Country	Design	Participants	ADHD Ascertainment	Modality	Intensity	Session Duration	Frequency/Intervention Duration	Comparator	Outcomes/Time Points
Converse et al. 2020 [37]	USA	Three-arm parallel feasibility trial; allocation partly based on class availability	Baseline: Tai Chi n = 9, active cardio n = 5, inactive control n = 7; follow-up: 9, 4, 6; overall age 20.7 ± 1.5 y; 67% female	Documented ADHD diagnosis; medication/comorbidity permitted; 16/21 reported ADHD medication use	Tai Chi or non-contact kickboxing/cardio-aerobic class	Not reported	60 min	2 sessions/week for 7 weeks; home practice encouraged	Inactive no-contact control; active exercise comparator also present	CAARS inattention/hyperactivity, Flanker/Card Sort, postural stability, HR/HRV, PSQI, FFMQ, GRE, SF-36; baseline and post-intervention
Oliel 2018 [38]	Canada	Randomized pilot trial	Randomized AE n = 21, medication n = 10; mixed-model sample AE n = 15, medication n = 8; age 32.53 ± 8.83 vs. 35.64 ± 13.15 y	DSM-5 ADHD; standardized diagnostic assessment including CAADID/SCID-5; FSIQ >85; current ADHD treatment excluded at entry	Progressive aerobic interval exercise	50–60% progressing to 70–80% HRmax	60 min (10-min warm-up + 40-min aerobic + 10-min cool-down)	2 sessions/week for 8 weeks	Stimulant pharmacotherapy plus weekly 60-min ADHD education	CSS-S/CSS-O ADHD symptoms, BAI, BDI-II, SDS functional impairment, ISE self-esteem; baseline, post, 3- and 6-month follow-up
Shalit & Hantiu 2014 [39]	Israel	Three-group randomized study	EWMN n = 13 (mean age 26.8 y), general coordination n = 14 (25.2 y), control n = 13 (25.8 y); all female	Medically diagnosed ADHD before graduate studies	Coordination training (EWMN or general coordination)	Not reported	Not reported	3 practices/week (1 supervised + 2 individual) for 13 weeks	No coordination practice	Matorin Test of general coordination, clockwise and counterclockwise; pre-test and post-test

Note: The three Svedell reports belong to the START programme and do not represent three established independent samples; analytic placement is detailed in Supplementary Material File S1 Table S4. Abbreviations: ADHD, attention-deficit/hyperactivity disorder; ASRS, Adult ADHD Self-Report Scale; BAI, Beck Anxiety Inventory; BDI-II, Beck Depression Inventory-II; BFIS, Barkley Functional Impairment Scale; CAARS, Conners' Adult ADHD Rating Scales; CAADID, Conners' Adult ADHD Diagnostic Interview; CGI, Clinical Global Impression; CPT, Continuous Performance Test; DCCS, Dimensional Change Card Sort; EWMN, Exercise With Movement Novelty; FFMQ, Five Facet Mindfulness Questionnaire; HRmax, maximum heart rate; HRV, heart-rate variability; ISI, Insomnia Severity Index; POMS, Profile of Mood States; PSQI, Pittsburgh Sleep Quality Index; SCID-5, Structured Clinical Interview for DSM-5; SF-36, 36-Item Short-Form Health Survey; START, structured physical exercise program for adults with ADHD; TAU, treatment as usual; WCST, Wisconsin Card Sorting Test.

3.3. Risk of Bias

Risk-of-bias assessments for the 23 included reports are presented in Figure 2, with detailed domain-level judgments and supporting evidence provided in Supplementary Material File S1 Table S5. Assessments were conducted separately for randomized and non-randomized studies using RoB 2 and ROBINS-I, respectively.

A. RoB 2 (randomized reports)

Study	Randomization process	Deviations from intended interventions	Missing outcome data	Measurement of the outcome	Selection of the reported result	Overall
Acute exercise reports						
Wang 2024	Some concerns	Low	Low	Low	Some concerns	Some concerns
Huan 2024	Low	Low	Low	Low	Some concerns	Some concerns
Shi 2025a	Low	Low	Low	Low	Some concerns	Some concerns
Shi 2025b	Low	Low	Low	Low	Some concerns	Some concerns
Kuo 2024	Low	Low	Low	Low	Some concerns	Some concerns
Dinu 2023	Low	Some concerns	Some concerns	Low	Some concerns	Some concerns
Fritz 2016	Low	Low	Low	Low	Some concerns	Some concerns
Jones 2015	Low	Low	Low	Low	Some concerns	Some concerns
Chronic exercise reports						
Svedell 2023	Some concerns	Some concerns	High / Serious	Some concerns	Some concerns	High / Serious
Kouhbanani 2023	Some concerns	Some concerns	High / Serious	Low	Some concerns	High / Serious
Svedell 2025	Low	Some concerns	High / Serious	Some concerns	Some concerns	High / Serious
Svedell 2026	Low	Some concerns	High / Serious	Some concerns	Some concerns	High / Serious
Fritz & O'Connor 2022	Low	Some concerns	High / Serious	Some concerns	Some concerns	High / Serious
Oliel 2018	Some concerns	Some concerns	High / Serious	Some concerns	Some concerns	High / Serious
Shalit & Hantiu 2014	Some concerns	Some concerns	Low	Some concerns	Some concerns	Some concerns

B. ROBINS-I (non-randomized reports)

Study	Confounding	Selection of participants	Classification of interventions	Deviations from intended interventions	Missing data	Measurement of outcomes	Selection of the reported result	Overall
Acute exercise reports								
LaCount 2022	Moderate	Moderate	Low	Low	Moderate	Low	Moderate	Moderate
Kallweit 2021	Moderate	Moderate	Low	Low	Low	Low	Moderate	Moderate
Mehren 2019a (Go/No-go)	Moderate	Moderate	Low	Low	Moderate	Low	Moderate	Moderate
Mehren 2019b (Flanker)	Moderate	Moderate	Low	Low	Moderate	Low	Moderate	Moderate
Rassovsky 2019	Moderate	Moderate	Low	Low	Moderate	Low	Moderate	Moderate
Gapin 2015	High / Serious	Moderate	Low	Low	Low	Low	Moderate	High / Serious
Thal 2014	Moderate	Moderate	Low	Moderate	Low	Low	Moderate	Moderate
Chronic exercise reports								
Converse 2020	High / Serious	High / Serious	Low	High / Serious	Moderate	High / Serious	Moderate	High / Serious

Low · Some concerns · Moderate · High / Serious

Figure 2. Risk-of-bias assessment of included reports. Five common RoB 2 domains shown; crossover-specific domain is reported in Supplementary Material File S1 Table S5. Rows represent reports, not independent trials. Outcome scope and reasons are given in Supplementary Material File S1 Table S5 [10–12,17–21,25–39].

Among acute-exercise reports, randomized studies were generally judged to have some concerns, primarily related to the randomization process, selection of reported results, and, in some cases, missing outcome data. Although objective cognitive tasks were less susceptible to subjective measurement bias, unblinded assessments of ADHD symptoms and psychological outcomes remained vulnerable to expectancy and reporting effects. Multiple publications involving the same or potentially overlapping participant samples were evaluated at the report level but were not considered independent evidence of methodological quality.

Among chronic-exercise reports, six of the seven randomized-report assessments were classified as high risk of bias, mainly because of missing outcome data and post-randomization exclusions. The remaining randomized report, Shalit and Hanţiu [39], was judged to have some concerns. Importantly, the two main START publications represented

linked reports from the same trial and were not interpreted as independent randomized studies.

Non-randomized acute-exercise reports were predominantly judged to have moderate overall risk of bias according to ROBINS-I. Gapin et al. [33] was classified as having serious risk of bias, primarily because the absence of a non-exercise comparison condition limited the ability to distinguish exercise-related effects from practice or time-related changes. Rassovsky et al. [25] also presented inconsistencies in reported participant numbers, which complicated the interpretation of its findings and contributed to its exclusion from the primary quantitative synthesis. Among chronic-exercise studies, Converse et al. [37] was judged to have serious overall risk of bias, particularly because of concerns regarding confounding, participant selection, deviations from intended interventions, and outcome measurement.

Overall, the risk-of-bias assessments identified important methodological limitations across both acute- and chronic-exercise research. The implications of these limitations were considered separately for each outcome domain in the GRADE certainty-of-evidence assessment rather than assuming that study-level judgments applied uniformly to all outcomes.

3.4. Acute Exercise Effects

The primary meta-analysis did not demonstrate statistically significant average effects of acute exercise on executive function ($g = 0.26$, 95% CI: −0.01 to 0.53, $p = 0.056$; six participant-sample clusters, 40 effect sizes), attention ($g = 0.13$, 95% CI: −0.06 to 0.31, $p = 0.099$; four clusters, 12 effect sizes), or psychological outcomes ($g = 0.40$, 95% CI: −0.13 to 0.93, $p = 0.088$; four clusters, 15 effect sizes) (Figure 3; Supplementary Material File S1 Table S6). The corresponding approximate 95% prediction intervals were −0.84 to 1.36, −0.07 to 0.33, and −1.33 to 2.13, respectively, indicating considerable uncertainty regarding effects across settings. Statistical inference was particularly limited for attention and psychological outcomes, for which the Satterthwaite-adjusted degrees of freedom were 2.19 and 2.44, respectively.

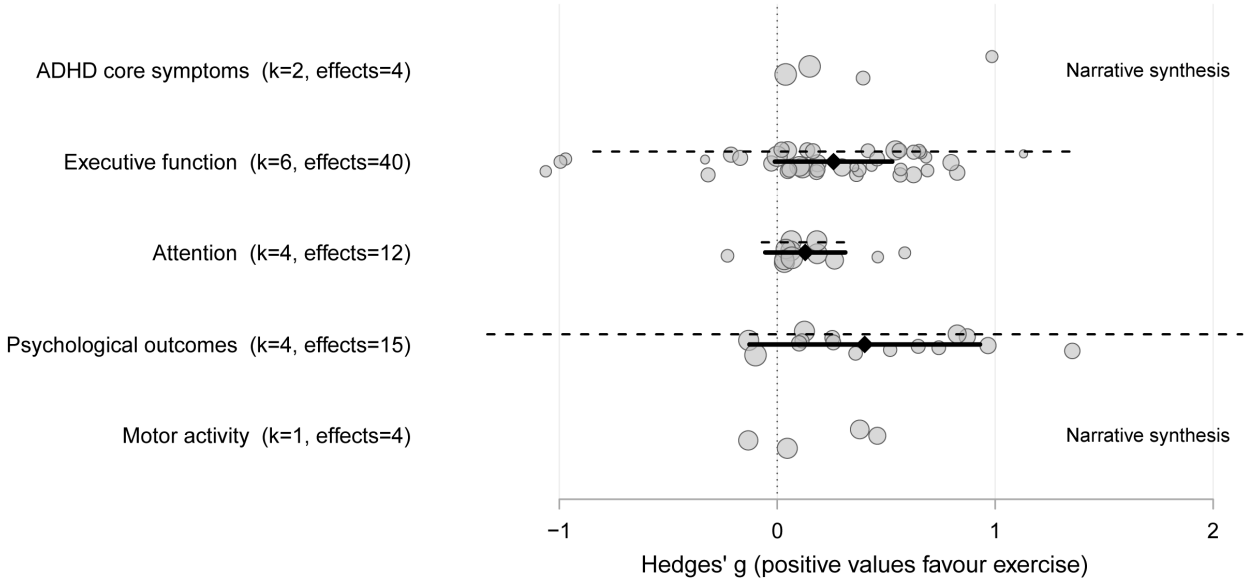

Figure 3. Orchard plot of the effects of acute exercise. Gray circles indicate individual effect sizes and black diamonds indicate pooled estimates. Thick solid lines show CR2 95% confidence intervals. Offset dashed lines show approximate 95% prediction intervals. The dotted vertical line marks zero. k is the number of participant-sample clusters. See Supplementary Material File S1 Figure S7 for sample-level estimates.

ADHD core symptoms and motor activity were not quantitatively pooled because of insufficient independent participant samples. Core symptom outcomes were reported by Kallweit et al. [31] and LaCount et al. [12], with differences in assessment timing and outcome measures. All four motor-activity effect sizes originated from a single study by Fritz et al. [32] and therefore did not represent independent replications. These findings were synthesized narratively, with available sample-level estimates presented in Supplementary Material File S1 Figure S7.

3.4.1. Executive-Function Subdomains

Subdomain analyses showed no statistically significant average effect on inhibitory control (g = 0.19, 95% CI: −0.21 to 0.60, $p = 0.272$; six participant-sample clusters, 19 effect sizes) (Figure 4; Supplementary Material File S1 Table S8). The corresponding approximate 95% prediction interval ranged from −0.90 to 1.29, suggesting substantial uncertainty in effects across settings.

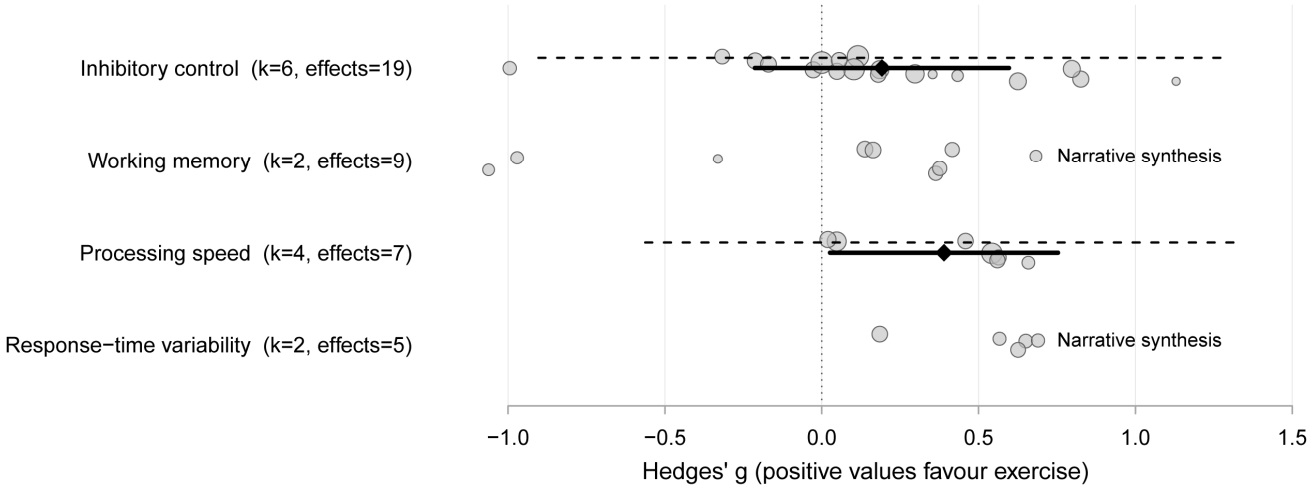

Figure 4. Acute executive-function subdomain orchard plot for the primary analysis. Gray circles indicate individual effect sizes and black diamonds indicate pooled estimates. Thick solid lines show CR2 95% confidence intervals. Offset dashed lines show approximate 95% prediction intervals. The dotted vertical line marks zero. k is the number of participant-sample clusters. Working memory and response-time variability have no primary pooled estimate. See Supplementary Material File S1 Table S7 for sample-level composites and Supplementary Material File S1: S9 for exploratory pooling.

A positive pooled effect was observed for processing speed (g = 0.39, 95% CI: 0.03 to 0.75, $p = 0.043$; four clusters, seven effect sizes), with an approximate 95% prediction interval of −0.56 to 1.34. However, this finding should be interpreted cautiously because the Satterthwaite-adjusted degrees of freedom were only 2.65, and no adjustment was made for multiple subdomain comparisons. Accordingly, the observed statistical significance should be regarded as exploratory rather than conclusive.

Working memory (two clusters, nine effect sizes) and response-time variability (two clusters, five effect sizes) were not included in the primary pooled analyses because of insufficient independent samples. Separate exploratory analyses are presented in Supplementary Material File S1: S9; however, their estimates were highly uncertain and did not establish reliable benefits. Overall, the available evidence did not support definitive conclusions regarding differential effects across executive-function subdomains.

3.4.2. Sensitivity Analyses

Sensitivity analyses indicated that the executive-function findings were influenced by several analytical assumptions and study-selection decisions (Supplementary Material File S1 Table S10). Excluding graph-digitized effect sizes yielded a pooled estimate of g = 0.23 (95% CI: −0.03 to 0.50, $p = 0.070$). Restricting the analysis to randomized studies produced a smaller and more uncertain effect (g = 0.20, 95% CI: −0.31 to 0.71, $p = 0.288$; four clusters).

When Wang et al. [10] and Kuo et al. [11] were treated as separate participant-sample clusters, the executive-function estimate reached nominal statistical significance (g = 0.27, 95% CI: 0.03 to 0.51, $p = 0.034$), whereas the primary analysis conservatively linking these reports did not. Restricting the analysis to clinically diagnosed ADHD samples also yielded a nominally significant effect (g = 0.40, 95% CI: 0.01 to 0.79, $p = 0.048$). This exploratory estimate was based on three independent clusters and 1.81 Satterthwaite-adjusted degrees of freedom. It provides limited evidence about the effect in clinically diagnosed adults, and differences by ADHD ascertainment remain uncertain.

Exploratory meta-regression analyses did not identify statistically significant associations between executive-function effects and ADHD ascertainment method ($p = 0.132$) or randomized versus non-randomized study design ($p = 0.460$). Given the small number of independent samples and low effective degrees of freedom, the analyses provided insufficient evidence to determine whether ADHD ascertainment or study design modified the effects. Restricting psychological outcomes to same-day assessments yielded an uncertain effect (g = 0.41, 95% CI: −0.62 to 1.44).

Leave-one-cluster-out analyses further demonstrated the sensitivity of the executive-function estimates to individual participant-sample clusters. Although pooled point estimates remained positive, statistical significance was not consistent across analyses. Excluding the linked Huan–Shi trial [17–19] produced an estimate of g = 0.34 (95% CI: 0.14 to 0.54), whereas exclusion of other influential clusters yielded confidence intervals that included the null. Together, these findings indicate that the apparent magnitude and statistical significance of acute-exercise effects depend partly on the limited independent evidence and assumptions regarding sample dependence.

3.4.3. Publication Bias and Small-Study Effects

Formal assessments of funnel plot asymmetry or small-study effects were not undertaken because fewer than ten independent participant-sample clusters contributed to each acute-exercise outcome domain. Consequently, publication bias and selective reporting could not be reliably assessed or ruled out. These uncertainties were considered when interpreting the overall findings and evaluating the certainty of evidence.

3.5. Chronic Exercise Effects

3.5.1. Clinical and Cognitive Outcomes

Chronic-exercise findings were synthesized narratively because the available trials differed substantially in intervention characteristics, comparator conditions, outcome measures, and methodological quality. Although several studies reported improvements in selected clinical or cognitive outcomes, the findings were inconsistent and subject to important limitations.

The main START trial [27] reported improvements in ADHD symptoms compared with treatment as usual. However, primary outcome data at 12 weeks were unavailable for 22 of the 63 participants, raising concerns about attrition bias. A subsequent START publication [26] examined body awareness and movement quality in the same trial population and therefore did not provide independent evidence of symptom improvement. The earlier START pilot study [28] also reported favorable changes, but its small sample size,

predominantly per-protocol analyses, and inconsistencies in reported percentage-change statistics limited the reliability of these findings. These percentage-change data were not converted into standardized effects because the underlying raw-score information could not be reliably reconstructed.

Kouhbanani et al. [36] reported improvements in cognitive task performance following 24 weeks of Pilates training. However, only 52 of the 60 randomized participants were included in the final analysis, following the exclusion of participants with poor attendance and control participants who initiated stimulant medication. In contrast, Fritz and O'Connor [29] found no statistically significant group-by-time interactions for the primary cognitive outcomes or secondary measures after six weeks of yoga. This study recruited women identified through ADHD symptom screening and reported 31% attrition in the yoga group. Similarly, Converse et al. [37] found no statistically significant group-by-time effect on the primary inattention outcome following Tai Chi compared with an inactive control (reported coefficient = −1.7, SE = 2.4, $p = 0.50$). Interpretation was further limited by non-randomized allocation partly determined by participant availability.

Collectively, these studies provided mixed findings across substantially different interventions, populations, and outcome measures. Their results did not support a reliable pooled estimate of chronic-exercise effects on ADHD symptoms or cognitive functioning.

3.5.2. Comparator and Outcome Specificity

The interpretation of chronic-exercise findings was further influenced by differences in comparator conditions and the clinical relevance of the measured outcomes. Oliel [38] compared an eight-week exercise intervention with pharmacological treatment rather than an inactive or non-exercise control. Observer-rated ADHD symptoms favored medication at post-intervention (reported $p = 0.026$). Consequently, this study informed the comparative effects of exercise and medication but could not establish exercise efficacy relative to inactivity.

Other studies assessed outcomes closely related to the specific activities performed. Shalit and Hanţiu [39] examined coordination performance following coordination-based exercise, whereas the START secondary analysis [26] evaluated body awareness and movement quality. Although these outcomes may reflect intervention-specific improvements, they are conceptually distinct from reductions in core ADHD symptoms or improvements in everyday functioning.

Differences in outcome definitions, statistical reporting, and comparator conditions further limited cross-study interpretation. In particular, model-based estimates and transformed outcomes were distinguished from unadjusted post-intervention measurements. Given the limited number of comparable trials, no pooled effect estimates or between-study prediction intervals were calculated for chronic-exercise outcomes.

3.5.3. Publication Bias and Reporting Limitations

The number of independent and sufficiently comparable chronic-exercise trials was too small to support informative assessments of funnel plot asymmetry or small-study effects. Therefore, publication bias and selective reporting could not be reliably evaluated or excluded. Narrative synthesis was used to preserve available clinical, cognitive, psychological, and functional findings from studies that could not contribute to a common quantitative estimate.

3.6. Certainty of Evidence

The certainty of evidence was rated as very low across all evaluated acute-exercise outcome domains, including ADHD core symptoms, executive function, attention, psy-

chological outcomes, and motor activity (Supplementary Material File S1 Table S11). The reasons for these judgments varied by domain. For executive function, important concerns included risk of bias, imprecision, and indirectness arising from the inclusion of screening-positive populations and the predominance of laboratory-based cognitive assessments. Although the pooled estimate favored exercise, its confidence interval remained compatible with little or no benefit as well as potentially meaningful improvement.

For attention and psychological outcomes, certainty was particularly limited by the small number of independent participant-sample clusters, imprecise effect estimates, and unstable statistical inference. Evidence for ADHD core symptoms and motor activity was also insufficient for reliable quantitative synthesis because of the limited independent samples. Accordingly, the available acute-exercise findings could not establish consistent or clinically meaningful benefits across outcome domains.

The certainty of chronic-exercise evidence was likewise rated as very low across ADHD symptom, cognitive, psychological, physical-function, quality-of-life, and global-severity outcomes. Major limitations included high or serious risk of bias in several studies, small or selectively analyzed samples, heterogeneous interventions and comparators, and limited applicability of some outcomes to everyday functioning. Furthermore, potentially overlapping trial reports reduced the amount of independent evidence available.

Overall, the very low certainty ratings indicate substantial uncertainty regarding the magnitude, consistency, and reproducibility of exercise-related benefits in adults with ADHD. These judgments should not be interpreted as evidence that exercise is ineffective, but rather that the current evidence is insufficient to support confident conclusions about its clinical effectiveness.

4. Discussion

This systematic review and meta-analysis evaluated the effects of acute and chronic exercise on clinical, cognitive, psychological, and functional outcomes in adults with ADHD or elevated ADHD symptoms. The findings indicate that current evidence does not establish consistent benefits of acute exercise or sustained clinical effectiveness of chronic exercise. Although pooled estimates generally favored acute exercise, the effects on executive function ($g = 0.26$), attention ($g = 0.13$), and psychological outcomes ($g = 0.40$) were not statistically significant. Among executive-function subdomains, processing speed showed a nominally significant improvement, whereas inhibitory control did not. However, the processing-speed finding was based on only four participant-sample clusters and remained subject to substantial statistical uncertainty. Evidence for acute effects on ADHD core symptoms and motor activity was insufficient for reliable quantitative synthesis. Chronic-exercise findings were heterogeneous and could not be meaningfully pooled. Importantly, the certainty of evidence was very low across all evaluated domains, indicating limited confidence in the magnitude and reproducibility of exercise-related benefits.

These findings should be considered alongside previous systematic reviews examining exercise interventions in adults with ADHD. Yang et al. [13] reported potential benefits for inhibitory control, while Xu et al. [14] synthesized evidence on executive functions and ADHD symptoms following acute and chronic exercise. In contrast, the present analysis did not identify a statistically significant average effect on inhibitory control. Such differences may partly reflect variations in study eligibility, outcome classification, comparator conditions, and statistical methods. In particular, several included publications originated from identical or potentially overlapping participant samples, and individual studies frequently contributed multiple correlated outcomes. Treating these observations as independent may overestimate the amount of available evidence and produce overly precise estimates. By accounting for participant-sample linkage and statistical dependence through three-level

random-effects models and cluster-robust inference, the present review provides a more cautious interpretation of the existing evidence. Nevertheless, these methodological differences do not necessarily invalidate previous findings but highlight the sensitivity of pooled conclusions to assumptions regarding sample independence and analytical approaches.

The variability observed across acute-exercise outcomes may reflect differences in cognitive demands, exercise characteristics, and assessment timing. Inhibitory control, working memory, and processing speed involve related but distinct cognitive processes, potentially contributing to their different responses to exercise. Acute physical activity may transiently influence arousal, attentional engagement, and cognitive-control processes, although the extent to which these mechanisms explain improvements in adults with ADHD remains uncertain. Furthermore, cognitive outcomes were assessed during exercise, immediately afterward, or following recovery, which may capture different physiological and behavioral states. The nominal improvement in processing speed should therefore be interpreted as exploratory rather than evidence of greater responsiveness than other executive-function domains. Similarly, positive estimates for psychological outcomes may reflect short-term changes in mood or motivation without necessarily indicating improvements in core ADHD symptoms or everyday functioning. Accordingly, the available findings do not establish a consistent pattern of responsiveness across cognitive or psychological domains.

Chronic-exercise studies reported mixed findings across different intervention modalities and clinical outcomes. The START program demonstrated improvements in ADHD symptoms relative to treatment as usual, but missing outcome data and overlapping publications limited confidence in these findings [26–28]. Pilates training was associated with improved cognitive task performance, although post-randomization exclusions raised concerns about potential bias [36]. In contrast, yoga and Tai Chi interventions did not demonstrate consistent improvements in their principal cognitive or clinical outcomes [29,37]. The interventions also differed considerably in modality, intensity, frequency, duration, and comparator conditions, limiting their comparability. These intervention characteristics were described narratively rather than formally examined as potential moderators of treatment effects. Therefore, they should be considered possible contributors to the inconsistent findings, rather than empirically established explanations for differences in treatment response. Moreover, improvements in proximal outcomes, such as coordination, body awareness, or movement quality, should not be interpreted as equivalent to reductions in ADHD symptom severity or improvements in everyday functioning. These limitations prevented a reliable pooled estimate of chronic-exercise efficacy. Nevertheless, the absence of quantitative pooling does not indicate that chronic exercise is ineffective; rather, it highlights the need for more rigorous and comparable trials to determine whether repeated exercise training produces sustained clinical benefits.

Several strengths and limitations should be considered when interpreting this review. A key strength was the separate evaluation of acute and chronic exercise, together with explicit consideration of overlapping participant samples, correlated effect sizes, and differences in comparator conditions. The use of three-level random-effects models, cluster-robust inference, sensitivity analyses, and outcome-specific GRADE assessments strengthened the transparency of the evidence synthesis. However, the number of independent participant-sample clusters was small despite the relatively large number of reported effect sizes, resulting in imprecise estimates and unstable statistical inference. Some analyses were sensitive to assumptions regarding participant overlap and within-study correlations, while data extraction required graphical estimates or statistical conversions for certain outcomes. Additional limitations included inconsistent ADHD ascertainment, variable medication status, heterogeneous exercise protocols, missing outcome data, and substantial risk of bias in several studies. Furthermore, most acute-exercise studies assessed short-term laboratory

performance rather than clinically meaningful everyday outcomes, and the limited number of independent studies prevented reliable assessment of publication bias. These limitations should be considered when interpreting the findings and their potential applicability to clinical practice.

Future research should prioritize adequately powered, preregistered randomized controlled trials involving clinically diagnosed adults with ADHD. Exercise interventions should be described using standardized parameters, including modality, intensity, frequency, duration, and progression, while medication use, adherence, and assessment timing should be carefully documented. Appropriate non-exercise control conditions and consistent outcome measures are needed to distinguish exercise-specific effects from practice, expectancy, and time-related influences. Longer-term follow-up should evaluate whether observed changes extend beyond laboratory cognitive tasks to clinically meaningful outcomes, including ADHD symptom severity, functional impairment, emotional regulation, quality of life, and occupational or academic functioning. Researchers should also report complete numerical data and sufficient information to identify overlapping participant samples and calculate dependent effect sizes. Although physical exercise may offer complementary benefits for general health and well-being, current evidence remains insufficient to recommend it as an established standalone treatment for adult ADHD. Further high-quality evidence is needed to clarify its clinical effectiveness and potential role alongside established ADHD interventions.

5. Conclusions

This systematic review and meta-analysis found insufficient evidence to establish consistent benefits of acute exercise or sustained clinical effectiveness of chronic exercise in adults with ADHD. Although acute exercise showed positive trends in selected cognitive and psychological outcomes, the pooled effects were generally statistically uncertain, and the nominal improvement in processing speed requires cautious interpretation. Evidence from chronic-exercise interventions was heterogeneous and methodologically limited, preventing reliable conclusions regarding long-term efficacy. The certainty of evidence was very low across all assessed outcomes, primarily because of limited independent samples, risk of bias, and imprecision. Therefore, physical exercise may be considered a complementary approach to supporting general health and well-being, but its effectiveness as a specific treatment for adult ADHD remains uncertain. Future adequately powered, rigorously controlled trials with standardized interventions and clinically meaningful long-term outcomes are needed to clarify its therapeutic potential.

Supplementary Materials: The following supporting information can be downloaded at https://www.mdpi.com/article/10.3390/healthcare14234051/s1. Table S1. PRISMA checklist; Table S2. Full search strategies; Table S3. The inter-rater agreement for selecting studies; Table S4. Report eligibility and sample linkage; Table S5. Risk-of-bias profiles and supporting reasons; Table S6. Primary acute domain results; Figure S7 Sample-level estimates; Table S8. Primary executive-function subdomain results; S9. Exploratory two-sample executive-function pooling; Table S10. Sensitivity analyses and exploratory moderators; Table S11. Certainty of evidence.

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