



Review

The myth of contextual interference learning benefit in sports practice: A systematic review and meta-analysis

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ABSTRACT

The contextual interference (CI) model hypothesizes random practice (high CI) leads to inferior acquisition, but superior retention and transfer capabilities compared to blocked practice (low CI). These phenomena are well established in laboratory settings. However, the transfer to applied settings, particularly in sports practice is still under discussion. The aim of this systematic review and meta-analysis is to evaluate the possible generalisability of the CI phenomenon in sports-based contexts with regard to performance outcomes. Up to April 16th, 2022, five electronic databases (PubMed, Web of Science, Scopus, Taylor & Francis, and SciELO) were used to search for relevant studies investigating differences between blocked and random schedules at post-acquisition, retention, and/or transfer tests. Using the PICOS criteria, a total of 933 records were screened. Outcomes evaluating the acquisition, retention, and/or transfer performances in sports skills in healthy participants were included. The quality of the selected studies was scored using the PEDro scale. Thirty-seven studies were selected, of which twenty-seven were of good quality and the remaining ten were rated as fair quality. Out of 205 overall pooled outcomes, only 43 performance outcomes (21%) agreed with the CI phenomenon after the acquisition (18 out of 103), retention (19 out of 84), and/or transfer (6 out of 36) phases. No statistically significant overall difference between blocked and random practice was detected at post-acquisition (effect size (ES) = 0.1, p = 0.154), retention (ES = -0.159, p = 0.141) or transfer testing (ES = -0.243, p = 0.071). The subgroup analysis showed inferior acquisition and superior retention following random practice only in individuals aged 20–24 years (ES = 0.282, p = 0.030 during the

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acquisition, and $ES = -0.405$, $p = 0.011$ during retention), with no difference at transfer testing for this specific age group. No similar significant effects have been concurrently identified during both acquisition and retention phases in any of the remaining subgroups (e.g., examined based on experience level, sports, skills, and testing protocol categories). The present results suggest CI effects can only be confirmed under very limited conditions, which seriously challenges the extension of CI effects to the sport context in general. Problematic conclusions for children's learning are discussed, as well as future research strategies to better understand counterintuitive learning approaches.

1. Introduction

Optimizing motor learning in a time-efficient manner is of central importance for sport practitioners, coaches, and physical education teachers (Hebert, Landin, & Solmon, 1996; Porter, Greenwood, Panchuk, & Pepping, 2020; Smith et al., 2022). Considerable research has investigated numerous influencing variables on skill performance, with practice order being one of the most examined (Brady, 1998; Magill, 2007; Magill & Hall, 1990; Rink, 2006; Schmidt, Lee, Winstein, Wulf, & Zelaznik, 2011; Wright & Kim, 2019). In particular, the contextual interference (CI) model (Battig, 1972) and, in its setting, the variability of practice (VP) model (Moxley, 1979), derived from Schmidt's schema theory (Schmidt, 1975), have been extensively evaluated in terms of maximizing motor skill learning. Given that the term "model" expresses the appropriateness of these learning approaches and their character as principally simplified projections of reality, each developed under specific circumstances and distinct boundary conditions (Schöllhorn, Rizzi, Slapsinskaitė-Dackevičienė, & Leite, 2022; Stachowiak, 1973), we chose this epistemologically more neutral and general term throughout the manuscript instead of the traditionally used terms "hypothesis," "theory," or "effect." Although both models have been extensively investigated, they are still highly topical. These old models were originally developed for very specific fine motor movements with a dominant optical component (Schöllhorn et al., 2022). However, a more recent approach provides renewed hope for the reformulation and generalization of these models using concepts from the field of system dynamics (Renshaw et al., 2019). It is therefore critical, especially in light of the actual replication crisis (Schöllhorn et al., 2022), not only to examine the spectrum of specific assumptions of these models, but also to gauge the practical consequences derived from them.

The VP model recommends stabilizing automatized movements by repeating, in blocked order, a combination of invariant movement elements in the form of generalized motor programs (GMPs) with variable parameters (Moxley, 1979). The original CI model recommends learning a single movement by means of repetitions that are interleaved randomly with other concrete movements. Etymologically, this was originally expressed by the term "contextual", where the single to-be-learned movement corresponds to the "text," and the other movements take over the role of "con" in context (Schöllhorn et al., 2022). Similarly, the term "interference" mapped the short-term disadvantage of CI during the acquisition phase, which is suggested to be overcompensated with relatively permanent gain in the subsequent retention and/or transfer test (Battig, 1972; Schmidt et al., 2011). Most VP research has focused on automatized movements, with a small number of degrees of freedom (DOF); whereas CI research began with investigating movements with small number of DOF before shifting its focus to the stabilization of movements with a large number of DOF (e.g., sports' skill) (Barreiros, Figueiredo, & Godinho, 2007; Brady, 2008). Whether the number of DOF was small or large, all movements were dominated by a strong visual component (Schöllhorn, 2022).

The earliest experiments in CI distinguished between two CI levels, whereby low CI was assigned to a blocked (AAABBBCCC) and high CI to a random sequence (ACBCBABCA) (Battig, 1979). With the introduction of the VP model, an additional differentiation of the CI model was suggested in terms of content by distinguishing between varying within the same (A_A^A) and varying between different GMPs (ABC) (Wulf & Lee, 1993). A serial practice group was added to the CI paradigms by Lee and Magill (1983), wherein participants perform, during the same acquisition session, several tasks in serial order (e.g., $A_A^A A_A^A A_A^A$ or ABCABCABC). Random and serial scheduling involves the learner continuously adapting the movement coordination pattern to temporal and spatial variables throughout practice and thereby minimizing consecutive repetitions of the same movement.

According to the CI hypothesis, practice under blocked conditions facilitates the acquisition of skilled performance and impairs retention and transfer; however, practicing under random or serial schedules leads to inferior acquisition, but superior retention and transfer capabilities (Battig, 1966; Lee & Magill, 1985; Schmidt et al., 2011; Shea & Morgan, 1979). The impairment during the acquisition phase using a random/serial order has been explained by an overloaded working memory (cognitive load theory: Gentile, 1998). The long-term advantages in the subsequent learning phase (retention and transfer) were most often assigned to three competing theoretical positions, termed, the elaboration (Shea & Zimny, 1983), the reconstruction or forgetting (Lee & Magill, 1983, 1985), and the retroactive inhibition hypothesis (Shea & Graf, 1994; Shewokis, Del Rey, & Simpson, 1998). The elaborative hypothesis suggests random practice engages the learner in a more elaborative and deeper process by using various encoding strategies, resulting in a more distinct and sophisticated representation of the task. Additionally, comparisons among interleaved and to-be-learned movements are stored in working memory (Shea & Zimny, 1983). In contrast, the reconstruction or forgetting hypothesis relies on the assumption that changing task characteristics from trial to trial in random practice remove the motor plan from the learner's working memory (forgetting motor processing). This demands extra retrieval practice to execute the movement and forces the learner to reconstruct the "plan of action" for each task (Lee & Magill, 1983, 1985). Thereby, more detailed and permanent representations of the task are being memorized which may benefit both retention and transfer.

An alternative explanation of the CI phenomenon consists of the retroactive inhibition hypothesis. This hypothesis has been

originally identified as the retention deficit due to intervening activities between practicing a task and taking its retention test (Buxton, 1940). This perspective has mainly emerged to explain the worse retention in blocked performers (Del Rey, Liu, & Simpson, 1994; Shea & Graf, 1994). Nonetheless, previous findings indicate that retroactive inhibition is temporary and can be eliminated with reminder trials (Shea & Titzer, 1993; Shewokis et al., 1998).

A variety of studies have been conducted to examine the CI hypothesis in a wide range of motor skill learning situations with a standardised experimental design, including an acquisition phase followed by an immediate or delayed retention and/or transfer test. Many investigations reported superior performance for the blocked group during the acquisition phase and for the random group during the retention and/or transfer phases (Hajihosseini, 2016; Kalkhoran & Shariati, 2015; Memmert, 2006; Pasand, Fooladiyazadeh, & Nazemzadegan, 2016). However, in other studies, these differences were not observed (Naimo et al., 2013; Porter et al., 2020; Sharp et al., 2020; Yanci, Reina, Los Arcos, & Cámara, 2013; Zetou, Michalopoulou, Giazitzi, & Kioumourtoglou, 2007) or even inverted (Farrow & Maschette, 1997; Hall, Domingues, & Cavazos, 1994; Zipp & Gentile, 2010).

The inconsistent findings have been attributed to various factors that have been suggested to modulate the CI effects, such as the experience levels and the age of the participants, the complexity and nature of the sports-related skill, and the duration of the acquisition phase. Indeed, Schmidt and Lee (1999) and Hebert et al. (1996) indicated the magnitude of the CI effects depends on the participants' experience, with less skilled participants seeming to benefit from higher acquisition under blocked condition, while no major difference existed between blocked and random practices in skilled participants (Hebert et al., 1996). Additional evidence was provided for the significant interaction of the age of the participants with the CI effect. It has been shown that CI effects seem to exist in adults, but not in children, particularly during the acquisition phase where no statistical significance was reported between blocked and random groups (Pollock & Lee, 1997). Interestingly, CI effects seem also to differ across children's age groups. Indeed, some findings support the CI hypothesis for 8, 9 and 14 years old (Bortoli, Robazza, Durigon, & Carra, 1992; Bortoli, Spagolla, & Robazza, 2001), while others did not, showing better retention for blocked practice in 7 (Jarus & Goverover, 1999) and 8–12 years old children (Parab, Bose, & Ganesan, 2018). Additionally, it is generally agreed that the complexity and the nature of the skill influence the CI effects with less apparent CI effects when practicing complex motor skills (e.g., sports-related skills) (Barreiros et al., 2007; Brady, 2004; Newell, Kugler, van Emmerik, & McDonald, 1989; Wulf & Shea, 2002) or set of tasks that shared the same generalized motor program (Magill & Hall, 1990). In this context, it has been suggested that complex tasks (e.g., sport skills) may benefit from blocked practice, both in acquisition and retention (Wulf & Shea, 2002), and that CI effects become more prominent when learning tasks require different GMPs. Moreover, the duration of the acquisition phase has been suggested as a mitigating factor of the CI effect, with a weaker effect for short practice durations (Jarus & Goverover, 1999; Vera, Alvarez, & Medina, 2008).

Taken together, CI has been commonly used in sport practice contexts, but with a lack of scientific consensus regarding the evidence of the CI hypothesis. Therefore, a thorough systematic review and meta-analysis is required to resolve such inconsistencies. A number of narrative (Barreiros et al., 2007; Brady, 1998, 2008; Magill & Hall, 1990; Merbah & Meulemans, 2011) and systematic reviews (Graser, Bastiaenen, & van Hedel, 2019; Wang, Abdullah, & Samsudin, 2022) have been already conducted in this field, but only one of them has included a quantitative synthesis (Brady, 2004). Besides being published over 18 years prior, Brady has pooled studies focusing on fine as well as gross/sports motor skills together and did not exclude studies that failed to guarantee that the random and blocked groups were equivalent at baseline (Brady, 2004). Thus, the results of this meta-analysis should be interpreted with caution and cannot be generalized to sports practice. To generate reliable and transparent findings, performing a meta-analysis following the updated Preferred Reporting Items for Systematic Reviews and Meta-Analyses guidelines (PRISMA) is warranted (Page, Moher, et al., 2021; 2021b).

Similarly, extensive literature has suggested various moderators of the CI hypothesis to explain the inconsistencies between findings (Wulf & Shea, 2002), particularly the lack of differences between random and block practices in many studies. However, no previous subgroup analysis and/or meta-regression has been conducted to confirm these suggestions.

To overcome the gaps in previously published reviews, the present systematic review and meta-analysis was designed to evaluate a possible generalisability of the CI phenomenon in sports practice based on more accurately pooled effect sizes (ES). These ES were extracted from studies investigating the difference between blocked and random schedules in terms of performance outcomes at post-acquisition, retention, and/or transfer tests. The secondary aim of this review was to investigate how moderator variables, including participant characteristics (i.e., age, sex, and experience), acquisition protocol (i.e., practice duration, number of training sessions and acquisition trials, duration between testing sessions, and testing protocol), and investigated sports/skills may influence observed mean effects, while also discussing practical applications that may help guide future motor learning protocols.

Findings may help sports coaches, physical education teachers, and practitioners identify the optimal practice conditions in terms of facilitating sports skill learning and maximizing long-term capability.

2. Method

The Preferred Reporting Items for Systematic Reviews and Meta-Analyses (PRISMA) 2020 statement (Page, Moher, et al., 2021; 2021b) and the recent PRISMA guidelines adapted to sports science (Rico-González, Pino-Ortega, Clemente, & Los Arcos, 2022) were followed in the present systematic review. Since no participants were required for this study, obtaining ethical approval was not necessary.

2.1. Eligibility criteria

Inclusion criteria were applied following the PICO (participants, intervention, comparators, and outcomes) model. Articles were

included if they were conducted on healthy participants and examined the effect of CI on motor learning with an intervention group of random/serial practice order (high CI) and at least a control group with blocked practice order (low CI). No specific criteria were specified for the age of the studied population. Any outcome evaluating the acquisition, retention, and/or transfer of a learned sports/gross motor skill was included. These eligibility criteria resulted in the exclusion of studies conducted in populations with physical or mental disabilities (e.g., mentally retarded, Down syndrome, mental handicap, etc.) and/or conducted with lack of practice for the control (blocked) group. Studies focusing solely on cognitive performance and/or fine motor skills and not describing the effects of CI on at least one performance measure/outcome in a movement task (sport/gross motor skill) were also excluded. Furthermore, narrative and systematic reviews, books, citations, conference proceedings, commentaries, and studies of which only the abstracts were available were excluded to be able to make a reliable risk-of-bias assessment. Importantly, studies that failed to guarantee that the intervention and control groups were equivalent at baseline were excluded from the quantitative synthesis (meta-analysis).

2.2. Search strategy

Up to April 16th, 2022, a comprehensive systematic search of studies was performed in five electronic databases, PubMed, Web of Science, Scopus, Taylor and Francis, and ScieLO; no restrictions were applied based on language or year of publication. Search terms (including mapping to appropriate MeSH terms where appropriate) described low and high contextual interference classes (random practice OR alternating practice OR non-repeated practice OR variable practice OR high interference OR blocked practice OR repeated practice OR low interference), AND with keywords relating to sports and motor skills (sports performance OR sports skill OR motor skill OR motor task OR motor learning OR motor skill learning OR motor skill acquisition OR retention performance OR transfer performance OR skill transfer OR skill retention). The search was applied to titles and abstracts and was conducted with additional filters excluding not human studies (NOT mice OR rats OR animals) and studies in injury/disease (NOT mice OR rats OR animals OR injury OR diseases OR impairment OR rehabilitation OR surgery OR stroke OR Parkinson OR patient OR treatment OR disorder OR review OR meta-analysis OR systematic review OR commentary). Full search strategies for each database are presented in [Appendix S1](#) in the supporting information. To minimize the risk of missing relevant publications, an additional search was performed on April 16th, 2022, by screening the reference lists of the included studies and related review papers using Google Scholar. Personal files were also searched and subject experts in the field were contacted for information on potential upcoming studies. Authors of included studies were contacted for missing information.

2.3. Selection process

The search strategies were combined, and duplicates were removed in “Endnote 20” (Camelot UK Bidco Limited-Clarivate, UK, London). Following the removal of duplicates, titles and abstracts of all unique hits were screened by two authors (AA and MAB) for eligibility, and disagreements were resolved by consensus. Once all relevant articles were located, the full texts were screened for their appropriateness for inclusion in accordance with the predetermined PICO criteria. The reason for excluding an article during each step (abstract and full-text screenings) was recorded.

2.4. Data collection process

To avoid any selection bias and data extraction flaws, two authors independently collected data using a pilot-tested extraction form and resolved disagreements by consensus. Extracted data included (i) author(s) and year of publication, (ii) study design, (iii) sample size (intervention and control groups), (iv) participant characteristics (i.e., age, sex and experience level), (v) sport and specific skill names, (vi) and intervention protocol including the intervention duration, number of training sessions, total number of acquisition trials, duration between test sessions (pre-post; post-retention, and/or post-transfer), and testing protocol (e.g., block and/or random), and (v) the difference between groups at pre-, post-, retention-, and/or transfer-test.

The mean and standard deviation (SD) of the main performance outcomes (sports/gross motor skills) at the different testing sessions (pre, post, retention and/or transfer) were extracted and used to perform the meta-analysis and meta-regression. When data were displayed inside a graphic and no further data was provided upon request, mean and SD were retrieved using WebPlot Digitizer (Pacifica, California, USA, Version: 4.5) (<https://automeris.io/WebPlotDigitizer>, accessed on June 08, 2022). Studies that did not include or mention the results of pre-test measurements, as well as studies showing significant differences between the blocked (control) and random (intervention) groups at pre-test (i.e., baseline), were excluded from the meta-analysis and meta-regression. The same applies to studies that did not include SD with the mean values.

In studies that included only serial and blocked groups, without mentioning random practice, data related to serial practice was compared to blocked practice.

2.5. Quality assessment

To assess the methodological quality of the selected studies, the Physiotherapy Evidence Database (PEDro) scale was used ([Ammar et al., 2020a, 2020b, 2020c; Maher, Sherrington, Herbert, Moseley, & Elkins, 2003](#)). The PEDro scale is a reliable and objective tool based on the Delphi list developed by Verhagen et al. of the Department of Epidemiology, University of Maastricht ([Verhagen et al., 1998](#)). To identify which of the trials are likely to be externally (criteria 1) and internally (criteria 2–9) valid with interpretable results, each paper was independently assessed twice by two independent authors. The 11-item checklist was used to yield a maximum score of

10 (the sum of awarded points for criteria 2–11). Discrepant results were resolved through a consensus meeting. Points are only awarded when a criterion is clearly satisfied. According to a recent article evaluating the measurement properties and summarising the clinimetric properties of the PEDro scale, total PEDro scores of 0–3 were considered ‘poor’, 4–5 ‘fair’, 6–8 ‘good’, and 9–10 ‘excellent’ (Cashin & McAuley, 2019). Additionally, for trials evaluating complex interventions (e.g., exercise) a total PEDro score of 8/10 is considered optimal.

2.6. Meta-analysis and risk of bias assessment

The meta-analysis was conducted following the recent recommendations of Hagger (2022) and Gucciardi, Lines, and Ntoumanis (2022) using commercial Comprehensive Meta-Analysis software (CMA version 3.0, Biostat, Englewood, NJ, USA). Using the random-effects model, standardized mean differences (Hedges’ *g*) and 95% confidence intervals (95% CI) were calculated between the blocked and random trials at post-, retention-, and/or transfer-test using the variable mean and SD values and sample size.

Effect size (ES) Hedges’ *g* of 0.8 reflects a large effect, 0.5 reflects a medium effect, and ≤ 0.2 reflects a small effect (Hedges, 1981). Forest plots were utilized to illustrate point estimates of the effect size and 95% confidence intervals. A positive ES value indicates the performance outcome was in favour of blocked practice (i.e., blocked group performed better at the corresponding test session: post-, retention-, and/or transfer-test), whereas a negative ES indicates the performance outcome was in favour of random practice.

Two statistics, *Q* (Morris, 2008) and *I*² (Higgins, Thompson, Deeks, & Altman, 2003), were utilized to assess statistical heterogeneity. Substantial heterogeneity was considered for *I*² values $> 50\%$ indicating that a random-effects model was to be preferred over a fixed-effects model (Higgins et al., 2003). To identify potential sources of variance and heterogeneity, moderator analysis was performed using subgroup analysis for categorical variables including the age groups (5–9, 10–14, 15–19, 20–24, 25–32, and > 65 years old), the experience level of the participants (i.e., unexperienced, novice/medium, and experienced), the investigated sports and skills, and the testing protocol (i.e., only blocked, only random, or blocked for the control group/random for the intervention group). Additionally, meta-regression for integer or decimal variables including the sex of the participants (i.e., expressed in % of females), intervention duration (days), number of training sessions, total number of acquisition trials, and duration between test sessions (pre-post-acquisition for the post-test analysis, post-retention for the retention test analysis and post-transfer for the transfer test analysis) was also performed. The definition of the present age groups was in accordance with the World Health Organization Standards Database “WHO 2000–2025,” but with a slight adjustment to the age groups 25–29 and 30–34 years old. As only one study focused on the age group 30–34 (32 years old), we elected to combine this age with the six studies focusing on 25–29 years old; thus, we employed the age group of 25–32. Furthermore, the included studies in the present meta-analysis focused either on males, females, or both sexes. However, the studies focusing on both sexes did not employ a 1:1 ratio, and the proportion of females in these studies varied between 27% and 90%. Therefore, we chose to analyze the possible moderation effect of “the sex of the participants” as a decimal variable using meta-regression.

As publication bias tests usually have low statistical power (Macaskill, Walter, & Irwig, 2001), it is suggested to assess publication bias using a variety of methods, instead of relying on the conclusion from a single test (Lin et al., 2018). To examine potential publication bias, we utilized funnel plots’ potential asymmetries, Begg and Mazumdar’s rank correlation test (Kendall’s *S* statistic *P*-*Q*; Begg & Mazumdar, 1994), the Egger’s linear regression test (Egger, Davey Smith, Schneider, & Minder, 1997), and the Duval and Tweedie’s trim-and-fill test (Duval & Tweedie, 2000). The stability of the pooled ES of each study was assessed using sensitivity analyses and involved removing individual studies from the analysis and computing the impact of the excluded study (Higgins & Green, 2011). In addition, a cumulative meta-analysis, which aims to aggregate accumulating evidence with additional studies based on their chronological order, was conducted to further ensure the stability and reliability of the results (Kulinskaya & Koricheva, 2010). A significance level of $p < 0.05$ was adopted for all analyses.

3. Result

3.1. Studies selection

Fig. 1 shows the flow diagram of the search process. The predefined search strategies yielded a preliminary pool of 933 possible papers. From these, 387 were excluded as duplicates (359 using Endnote and 28 manually) and 546 were screened by titles and abstracts for eligibility. From these, 435 were removed and 111 reports remained for full texts assessment. By applying the PICO eligibility criteria, 54 full texts were excluded based on the population (i.e., 6 studies focused on mentally disabled participants) and outcome (i.e., 48 only focus on cognitive performance and/or fine motor skills, with no focus on at least one outcome of performance in a movement task (sport/gross motor skill)). Fifty-seven published studies met the PICO inclusion criteria. After conducting an additional search, 15 articles were identified as potentially relevant studies, resulting in a total of 72 relevant studies. After a careful review of the 72 full texts, a total of 35 studies were excluded from the quantitative synthesis (meta-analysis) for the following reasons: 25 studies failed to guarantee the intervention and control groups were equivalent at baseline (15 studies without pre-test measurement, 9 studies did not mention the pre-test results, and one study failed to recruit groups with equivalent pre-test performances), and 10 studies failed to report SD values in some or all measurements. Detailed references of the excluded full texts are presented in Appendix S2. In total, 37 articles with a wide variety of motor tasks and enough comparable data were included in the quantitative analysis.

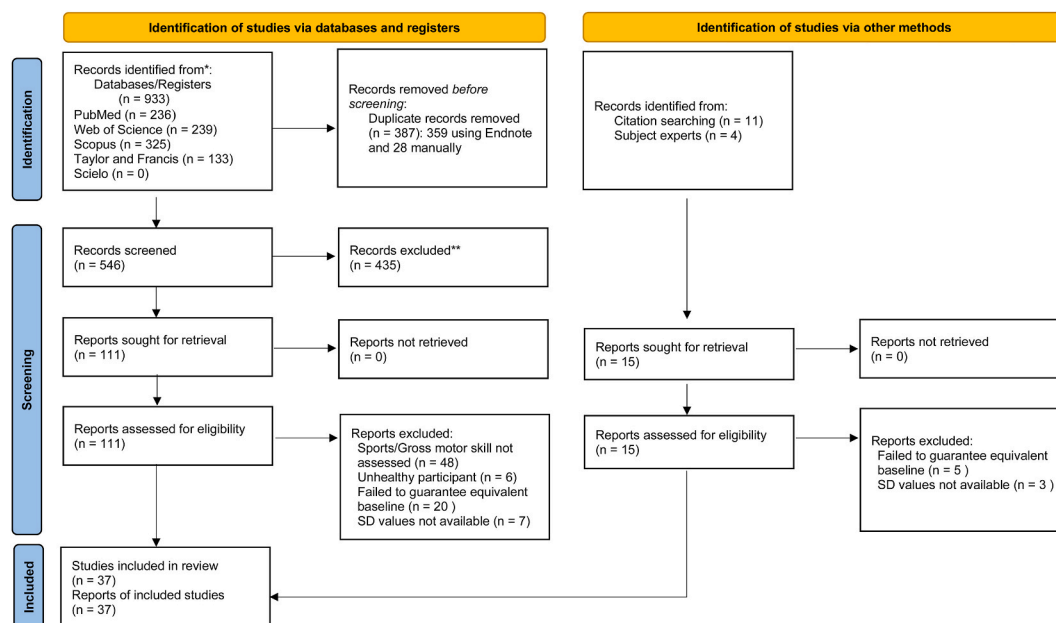


Fig. 1. PRISMA 2020 flow diagram.

3.2. Study characteristics

Characteristics of the 37 studies as well as the difference between the blocked and random group at post-, retention- and/or transfer-test in each performance outcome are presented in Table 1. Most of the studies (27 out of 37) applied a randomized between-subject design. Six studies followed a non-randomised between-subject design with group assignment based on pre-test results, and the four remaining studies applied either a quasi-random or matched between-subject design, a randomised or a counterbalancing within-subject design. Regarding the examined sports-related skills, 7 studies focused on volleyball, 4 studies on golf, 3 studies on tennis, 2 studies on either badminton, baseball, basketball, field hockey, table tennis, or shooting (e.g., pistol or kneeling), 1 study on either football, Australian rules football, gymnastic, dancing or bench press, and the remaining 6 studies focused on some sport related gross motoric skills (e.g., balance, agility, throwing, jumping etc.) without sports specification.

Of the 37 studies included, 23 investigated more than one performance outcome (i.e., different sports/gross motor skills and/or different performance variables). Similarly, most of the studies (31 out of 37) examined different testing phases with (i) 12 studies including post-acquisition, retention and transfer tests, (ii) 15 studies focused on post-acquisition and retention tests, (iii) 3 studies performing a post-acquisition with transfer tests, and (iv) one study including retention and transfer tests without post-acquisition measurement. Only six studies focused on one testing phase (i.e., acquisition phase). Additionally, some studies focused on more than one population (e.g., male vs. female or experienced vs. unexperienced), or testing protocol (e.g., blocked vs. random testing). Therefore, a specific ES was calculated for each outcome, population, and/or test session, and the 37 selected studies yielded a total of 225 ES. These ES are distributed between post- (103 ES), retention- (84 ES) and transfer-test (38 ES).

It should be noted that the data of the dribbling skill from Cheong, Lay, and Razman (2016) and the forearm pass from Jones and French (2007) were not considered in the meta-analysis, as these studies failed to guarantee equivalence at baselines for these specific skills. Additionally, the results of transfer tests from the studies of Jeon et al. (2021) and Hall et al. (1994) were not included in the meta-analysis as no SD values were reported. Similarly, for the meta-regression assessing the effects of between tests duration, we chose to exclude the retention and transfer data of Memmert (2006), which used extreme values for the duration between tests (365 days) compared to the other studies (56 days maximum), as inclusion could have biased the linear trend.

3.3. Participant characteristics

The studies in this systematic review included a total of 1161 participants between blocked ($n = 585$) and random ($n = 576$) groups. The number of participants in each study ranged from 11 to 83 for both groups. These studies targeted healthy populations with a mean age of 19.4 ± 10.2 years. Four studies targeted 5–9 years old participants, seven studies targeted the “10 to 14” years old range, 8 studies targeted the “15 to 19” years old range, nine studies targeted the “20 to 24” years old range, and 7 studies targeted the “25 to 29” years old range. One study focused on adults aged 32 years old, and one study focused on senior adults (more than 65 years old). In most of the studies (24 out of 37), participants from both sexes were recruited. Five studies recruited only male participants and five studies recruited only female participants. The sex distributions of the participants were not included in three studies. Most of the studies (25 out of 37) examined unexperienced populations, while five studies examined experienced athletes, four studies examined

Table 1
Study characteristics.

N	Study	Discipline	Skills/ techniques	Measurements	mean age Y	N participants		Female %	Level of experience	Duration between test session	Study design	Intervention duration	Number of acquisition sessions	Total acquisition trials number	Testing protocol	Block vs. Random/serial			
						Block Grp	Rndm/ serial Grp									Diff at post-test	Diff at retention	Diff at transfer	
1	Boyce and Del Rey (1990)	Kneeling shooting	Kneeling shooting task	Acquisition 1 Acquisition 2 Acquisition 3 Acquisition 4 Retention 1 Retention 2 Transfer	≈ 16	30	30	41,67	Unexperienced	4 days 7 days 10 days 14 days same day 7 days 7days	Randomised between subject design	14 days	4 sessions	20 trials	random block/ random	BIK > Rdm BIK > Rdm BIK > Rdm BIK > Rdm	BIK ≈ Rdm BIK ≈ Rdm	Rdm > BIK	
2	French et al. (1990)	Volleyball skills	Pass Set Overhead serve Pass Set Overhead serve	Post-test Retention	≈ 16	12	12	54,67	Unexperienced	9 days 2 days	Randomised between subject design	9 days	9 sessions	90 trials	block	BIK ≈ Rdm BIK ≈ Rdm BIK ≈ Rdm	BIK ≈ Rdm BIK ≈ Rdm BIK ≈ Rdm		
3	Wrisberg and Liu (1991)	Badminton	Long serves Short serves Long serves Short serves Long serves Short serves	Female Male Female Male Female Male Female Male	Acquisition Retention Transfer	≈ 20	26	26	38,5	Novice/ Medium	14 days same day same day	Counter- balancing within- subjects design	14 days	5 sessions	45 trials	random	BIK ≈ Rdm BIK ≈ Rdm BIK ≈ Rdm BIK ≈ Rdm	BIK ≈ Rdm BIK ≈ Rdm BIK ≈ Rdm Rdm > BIK	BIK ≈ Rdm Rdm > BIK BIK ≈ Rdm BIK ≈ Rdm
4	Hall et al. (1994)	Baseball skills	Combined (Fastballs, Curveballs , Change- up) Random testing Block testing	Acquisition	≈ 19	10	10	0	Experienced	6 weeks	Randomised between subject design	6 weeks	12 sessions	NM	random block	BIK ≈ Rdm Rdm > BIK			
5	Bortoli et al. (1992)	Volleyball	Volley Bump Serve Volley Bump Serve	Retention Short transfer Long transfer Short transfer Long transfer Short transfer Long transfer	≈ 15	13	13	NM	Unexperienced	1 week 1 week	Randomised between subject design	8 weeks	6 sessions	72 trials	block	BIK ≈ Rdm BIK ≈ Rdm BIK ≈ Rdm	BIK ≈ Rdm BIK ≈ Rdm BIK ≈ Rdm BIK ≈ Rdm BIK ≈ Rdm		
6	Goodwin and Meeuw- sen (1996)	Golf	Putting 2.43m Putting 5.47m Putting 3.95m Putting 2.43m Putting 5.47m Putting 3.95m Putting 1.67m Putting 3.19m Putting 6.23m	Acquisition Retention Transfer	≈ 26	10	10	100	Unexperienced	2 days 1 day 1day	Randomised between subject design	2 days	2 sessions	66 trials	block	BIK ≈ Rdm BIK ≈ Rdm BIK ≈ Rdm	BIK ≈ Rdm BIK ≈ Rdm BIK ≈ Rdm	BIK ≈ Rdm BIK ≈ Rdm BIK ≈ Rdm	
7	Hebert et al. (1996)	Tennis table	Forehand and backhand strokes low skills high skills low skills high skills	Acquisition (block testing) Acquisition (random testing)	≈ 22	46 (22 low skills + 24 high skills)	37 (19 low skills + 18 high skills)	45,78	low skills (novice) and high skills (experienced)	9 days	Randomised between subject design	9 days	9sessions	135 trials	block random	BIK ≈ Rdm BIK ≈ Rdm BIK > Rdm BIK ≈ Rdm			
8	Pollato u et al. (1997)	Motor skills	Throwing task Kicking task Throwing task Kicking task	Acquisition Retention	≈ 20	20	21	52,38	Unexperienced	2 weeks 1 week	Randomised between subject design	2weeks	8sessions	80 trials	block/ra- ndom	BIK ≈ Rdm BIK ≈ Rdm	BIK ≈ Rdm BIK > Rdm		
9	Bortoli et al. (2001)	Motor Skill	Underhan- d throwing Quintuple jumping Hurdle running Underhan- d throwing Quintuple jumping Hurdle running	Nonvari- able schedule s Variable schedule s	≈ 9	12	12	50	Experienced in throwing and hurdle running and not experienced in complex jumping	8 weeks	Randomised between subject design	8 weeks	14 sessions	126 trials	block	BIK ≈ Rdm BIK ≈ Rdm BIK ≈ Rdm BIK ≈ Rdm BIK ≈ Rdm			
10	Meira. And Tani (2003)	Volleyball	Underhand serves Overhand serves Asian Floater (precision) Asian Floater (movement pattern) Asian Floater (precision) Asian Floater (movement pattern)	Acquisition Transfer 1 Transfer 2	≈ 13	18	18	100	Unexperienced	8 days same day 7 days	Not randomised (groups assignment based on pretest) between subject design	8 days	8 sessions	288 trials	block/ra- ndom	BIK ≈ Rdm BIK ≈ Rdm	BIK ≈ Rdm BIK ≈ Rdm BIK ≈ Rdm BIK ≈ Rdm		
11	Smith et al. (2003)	Gymnastic- s	Cartwheel Round off	Acquisition Retention Transfer	≈ 20	16	16	50	Unexperienced	same day same day/7days	Randomised between subject design	1 day	1 session	192 trials	block/ra- ndom block/ra- ndom	BIK ≈ Rdm BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ Rdm	

12	Vera and Montilla (2003)	Throwing	Vertical Target	Tennis ball 3m Featherfly ball 3m Tennis ball 5m Featherfly ball 5m Tennis ball 3m Featherfly ball 3m Tennis ball 5m Featherfly ball 5m	Acquisition	≈ 6	34	37	50	Unexperienced	6 weeks	Not randomised (groups assignment based on pretest) between-subject design	6 weeks	18 sessions	1044 trials	block	BIK > Rdm	BIK > Rdm	BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ Rdm	Rdm > BIK	BIK ≈ Rdm																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																		
			Horizontal Target	BIK ≈ Rdm													BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ Rdm	BIK ≈ 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32	Fazeli et al. (2017)	Golf	Golf putting	Acquisition	≈ 27	10	10	0	Unexperienced	6 days	Randomised between subject design	6 days	6 sessions	1080 trials	block/random	BIK ≈ Rdm	
				Retention						7 days						Rdm > BIK	
33	Aiken and Genter (2018)	Golf	Chipping tasks	Acquisition											block/random	Rdm > BIK	
				Retention (block)	≈ 20	12	12	58	Unexperienced	same day	Randomised between subject design	1 day	1 session	54 trials	block	Rdm > BIK	
				Retention (random)											random	Rdm > BIK	
34	North et al. (2019)	Table tennis	Backhand shot	Acquisition	≈ 28	10	10	40	Unexperienced	same day	Not randomised (assignment based on pretest)	1 day	1 session	150 trails	block/random	Rdm > BIK	
				Retention						1 day						Rdm > BIK	
35	Porter et al. (2020)	Basketball	Shot	Acquisition						11 days	Randomised between subject design				block/random	BIK ≈ Rdm	
				Retention 1	≈ 25	12	12	50	Unexperienced	same day		11 days	6 sessions	360 trials	random	BIK ≈ Rdm	
				Retention 2											random	BIK ≈ Rdm	
				Transfer						7 days					block	BIK ≈ Rdm	
36	Sharp et al. (2020)	Baseball	Solid hits/swing (%)	Domina nt Non-dominant	Acquisition	≈ 16	5	6	0	Experienced	4 weeks	Randomised between subject design	4 weeks	8 sessions	90 trials	block	Rdm > BIK
			Solid hits/swing s (ball accuracy)	Domina nt Non-dominant													BIK ≈ Rdm
																	BIK ≈ Rdm
																	Rdm > BIK
			Timed Up and go test (TUG)														BIK > Rdm
			Mobility-test (POMA)	Acquisition						9 days							BIK > Rdm
			Functional-reach test (FRT)														BIK > Rdm
37	Jeon et al. (2021)	Wii Fit Dynamic Balance	TUG	Ret 1	≈ 65	22	19	NM	Unexperienced	3 days	Randomized, pretest-posttest design	10 days	10 sessions	60 trials	block	BIK > Rdm	
				Ret 2						9 days							BIK > Rdm
			POMA	Ret 1						3 days							BIK ≈ Rdm
				Ret 2						9 days							BIK > Rdm
			FRT	Ret 1						3 days							BIK > Rdm
				Ret 2						9 days							Rdm > BIK

Note. Rdm > BIK : superior performance following random compared to blocked practice

BIK > Rdm : superior performance following blocked compared to random practice

BIK ≈ Rdm : no significant difference between performances measured following random vs. blocked practice

block/random : block testing for block-practice group and random testing for random-practice group

NM: not mentioned; 1 RM: One-repetition maximum

novice and/or medium-level athletes, and three studies examined populations with different experience levels.

Taken together, the included studies in the present meta-analysis are characterised by a high level of heterogeneity due to the disparate motor skills, participants' characteristics, and intervention protocols.

3.4. Differences in performance outcomes between blocked and random practices

As Table 1 indicates in the post-acquisition test measurement, 18 performance outcomes were in favour of blocked practice, while 8 performance outcomes were in favour of the random one. There were no significant differences between blocked and random practices in 77 performance outcomes. For retention tests, 19 performance outcomes were in favour of random practice, while 12 performance outcomes were in favour of the blocked one. There were no significant differences between blocked and random practices in 53 performance outcomes. At transfer, 6 performance outcomes were in favour of random practice, while 5 performance outcomes were in favour of the blocked one. There were no significant differences between blocked and random practices in 27 performance outcomes.

These findings revealed that in most of the assessed outcomes, random and blocked schedules did not differ significantly in terms of post-acquisition, retention, and/or transfer performances.

3.5. Meta-analysis results

3.5.1. Difference at the acquisition phase (post-acquisition test)

Data from 36 studies investigating the difference between blocked and random practice at post-acquisition were pooled in our MA. These 36 selected studies yielded a total of 103 ES. A small effect size was computed (Fig. 2) indicating a non-significant difference between random and blocked practice at post-acquisition test ($ES = 0.1$, $CI = -0.038$ to 0.238 , $p = 0.154$). A significant heterogeneity was computed ($Q = 395.125$, $df = 102$, $I^2 = 71.598$, $p = 0.000$). To identify potential sources of heterogeneity, subgroup analysis and meta-regression analysis were performed.

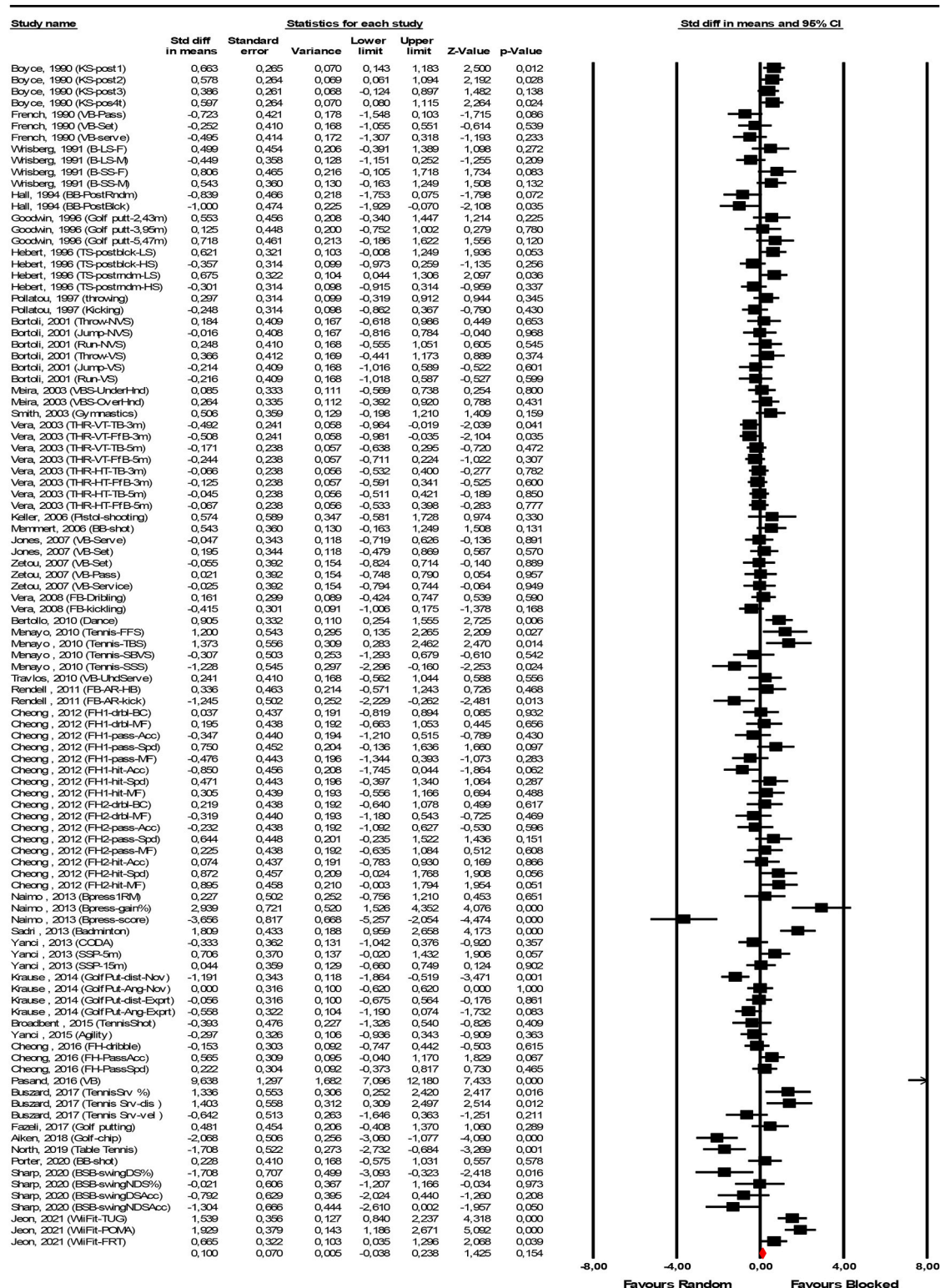


Fig. 2. Forest plot for the difference between blocked and random practices at post-acquisition test.

Abbreviation: KS: Kneeling Shooting; B: Badminton; LS: Long Serves; SS: Short Serves; M: Male; F: Female; TS: Tennis Strokes; LS: low Skills; HS: High Skills; VBS: Volleyball Serve; THR: Throwing; VT: vertical target; HT: horizontal target; Tscore: Ttal score; TB: tennis ball; FfB: Feather fly ball; VB: Volleyball; FB: Football; FFS: flat forehand shot; TBS: topspin backhand shot; SBVS slice backhand volley shot; SSS: slice serve shot; HB: handball; AR: Australian Rules; TUG: timed up and go test, POMA: Tinetti performance-oriented mobility assessment, FRT: Functional reach test;

FH: Field hockey; PassAcc: Push Pass Accuracy; PassSpd: Push Pass Speed; NVS: Nonvariable schedules; VS: Variable schedules; drbl: dribble; BC: ball control; MF: movement form; FH1: Field hockey acquisition 1; FH2: Field hockey acquisition 2; Srv: serve; dis: displacement; Bpress: Bench press; Uhd: underhand; CODA: Change of direction ability SSP: Straight Sprinting performance; BSB: Baseball; DS: dominant side; NDS: non dominant side; Put: Putting; dist: distance; ang: Angle; NOV: novice; Exprt: Experts.

3.5.1.1. Subgroup analysis. Subgroup analysis for the categorical variables, “age groups”, “experience level”, “sports”, “skills”, and “testing protocol” showed these variables did not moderate the difference between blocked and random practice at post-acquisition test (overall p values = 0.232, 0.731, 0.455, 0.426, and 0.155, respectively). No significant differences in performance outcomes between blocked and random practices were detected at post-acquisition test for the majority of analysed subgroups in each category ($p > 0.05$). Better performance outcomes in favour of blocked practice were only apparent for the age groups “20–24 years old” ($ES = 0.282$, $p = 0.030$) and “> 65 years old” ($ES = 1.356$, $p = 0.000$) (Fig. S1), as well as for the “unexperienced” group ($ES = 0.186$, $p = 0.029$) (Fig. S2). Similarly, better performance following a blocked practice was only computed for the category of “other skills (balance, cartwheel, rhythmic step, agility)” with an ES of 0.691 ($SE = 0.253$, $p = 0.006$) (Fig. S3). Additionally, only baseball performances were significantly different between blocked and random practice at post-acquisition test (in favour of the random one with $ES = -0.917$, $p = 0.006$) (Fig. S4).

3.5.1.2. Meta-regression. Meta-regression analyses revealed no moderating effect of female percentage (coefficient 0.005, $p = 0.068$), intervention duration (coefficient -0.003 , $p = 0.510$), number of training sessions (coefficient -0.009 , $p = 0.506$), total number of acquisition trials (coefficient 0.000, $p = 0.899$), and duration between pre- and post-acquisition test sessions (coefficient -0.0029 , $p = 0.440$).

3.5.1.3. Publication bias. Visual inspection of the funnel plot (Fig. 3) revealed no evidence of publication bias, a conclusion confirmed by Begg and Mazumdar’s rank correlation test (Kendall’s S statistic $P-Q = 83.00$; tau without continuity correction = 0.016, $Z = 0.237$, $p = 0.813$; tau with continuity correction = 0.016, $Z = 0.234$, $p = 0.815$) and Egger’s linear regression test (intercept = 0.620, $SE = 0.690$, 95% CI -0.847 to 1.988 , $t = 0.899$, $df = 101$, $p = 0.371$). However, Duval and Tweedie’s trim-and-fill test identified 14 missing studies and an adjusted ES of 0.278 (95% CI 0.129 to 0.428, $Q = 513.680$).

3.5.2. Difference at the retention phase (retention test)

Data from 28 studies investigating differences between blocked and random practice at the post-acquisition test were pooled in our MA. These 28 selected studies yielded a total of 84 ES . A small effect size was computed (Fig. 4), indicating a non-significant difference between random and blocked practice at the retention test ($ES = -0.159$, 95% CI -0.371 to 0.053 , $p = 0.141$). A significant heterogeneity was computed ($Q = 395.125$, $df = 102$, $I^2 = 71.598$, $p = 0.000$); therefore, subgroup analysis and meta-regression analysis were performed.

3.5.2.1. Subgroup analysis. Subgroup analysis revealed that “age groups”, “experience level”, “sports”, “skills”, and “testing protocol” did not moderate the difference between blocked and random practice at the retention test (overall p values = 0.820, 0.180, 0.288, 0.482, and 0.250, respectively). No significant differences in performance outcomes between blocked and random practices were detected at the retention test for the majority of analysed subgroups in each category ($p > 0.05$). Better performance outcomes in favour of random practice were only apparent for the age groups “20–24 years old” ($ES = -0.405$, $p = 0.011$) and “25–32 years old” ($ES = -0.605$, $p = 0.013$), while this significant difference was in favour of blocked practice for the “> 65 years old” age group ($ES = 0.943$, $p = 0.008$) (Fig. S5). Similarly, better performance following random practice, was only computed for the skills category of “golf putting/chipping” ($ES = -0.766$, $p = 0.011$), “kicking” ($ES = -1.248$, $p = 0.025$), and “other skills (balance, cartwheel, rhythmic step, agility)” ($ES = 0.830$, $p = 0.011$), (Fig. S6). Additionally, only golf performances showed to be better following random practice with $ES = -0.771$, and $p = 0.014$) (Fig. S7). The significant difference in Australian football performance was not considered as only 2 ES

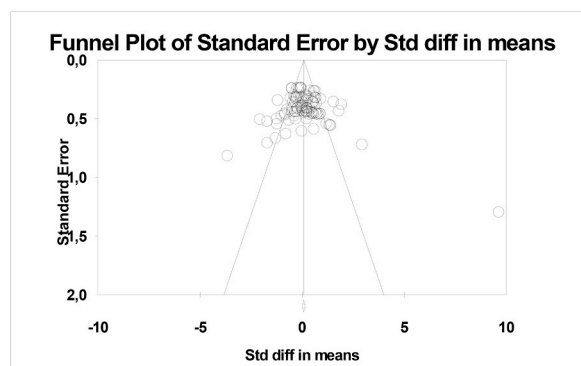
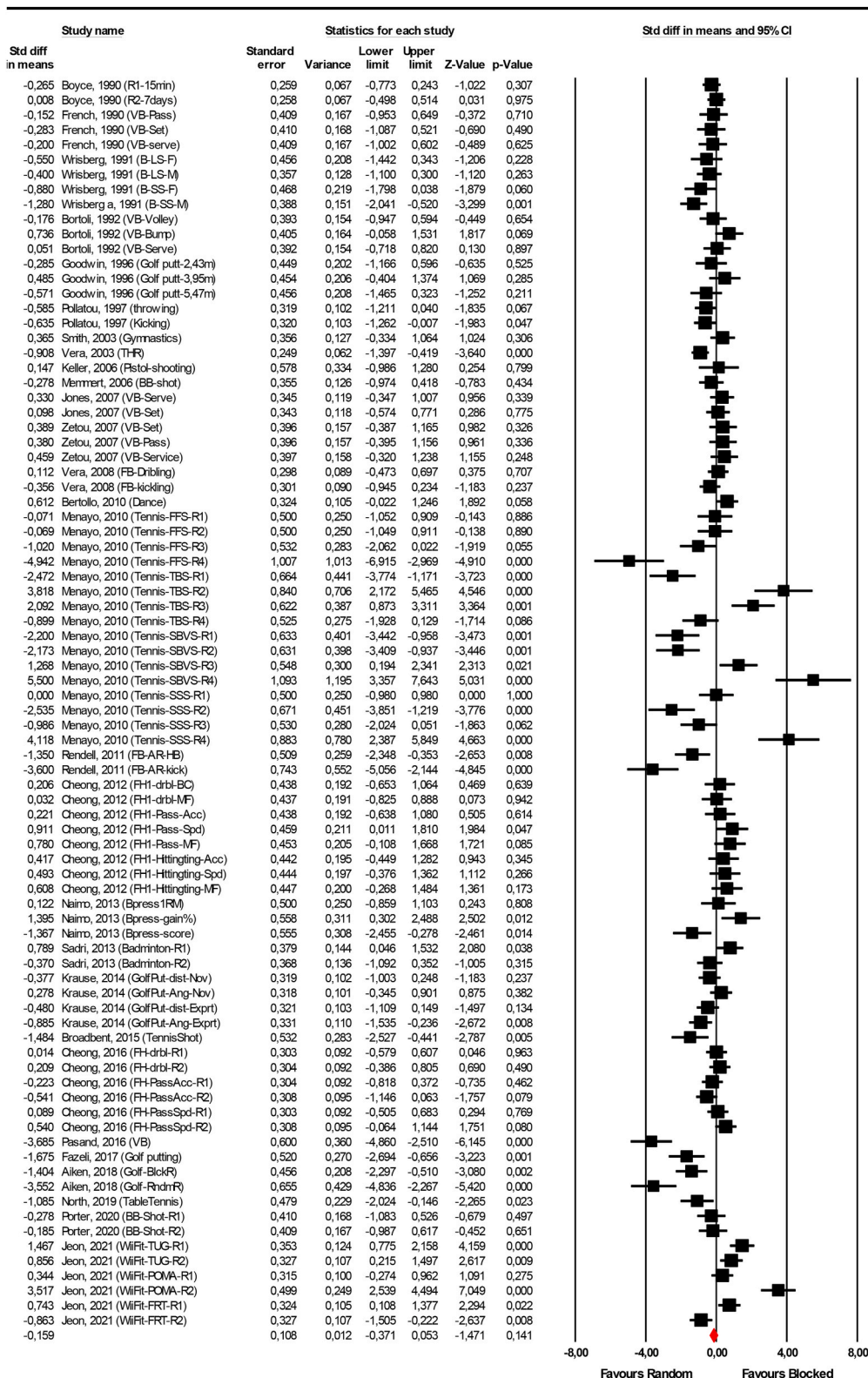


Fig. 3. Funnel plot for performance outcomes at post-acquisition test.



(caption on next page)

Fig. 4. Forest plot for the difference between blocked and random practices at retention-test.

Abbreviation: R: retention; LS: Long Serves; SS: Short Serves; M: Male; F: Female; THR: Throwing; VB: Volleyball; FB: Football; FFS: flat forehand shot; TBS: topspin backhand shot; SBVS slice backhand volley shot; SSS: slice serve shot; AR: Australian Rules; TUG: timed up and go test, POMA: Tinetti performance-oriented mobility assessment, FRT: Functional reach test; drbl: dribble; Acc: Accuracy; Spd: Speed; FH: Field hockey; PassAcc: Push Pass Accuracy; PassSpd: Push Pass Speed; NVS: Nonvariable schedules; VS: Variable schedules; drbl: dribble; BC: ball control; MF: movement form; FH1: Field hockey acquisition 1; FH2: Field hockey acquisition 2; putt: putting; BB: Basket ball; Bpress: Bench press; dist: distance; ang: Angle; NOV: novice; Exprt: Experts.

from the same study were analysed. Moreover, better retention performances were registered following the random practice only when a “random” ($ES = -0.451$, $p = 0.039$) or “block/random” ($ES = -0.479$, $p = 0.042$) testing protocols were employed (Fig. S8).

3.5.2.2. Meta-regression. Meta-regression analyses revealed no moderating effect of female percentage (coefficient 0.005, $p = 0.116$), intervention duration (coefficient 0.011, $p = 0.147$), number of training sessions (coefficient 0.0385, $p = 0.107$), total number of acquisition trials (coefficient 0.000, $p = 0.420$), and duration between pre- and post-acquisition test sessions (coefficient 0.012, $p = 0.264$).

3.5.2.3. Publication bias. Visual inspection of the funnel plot (Fig. 5) revealed no evidence of publication bias, a conclusion confirmed by Begg and Mazumdar’s rank correlation test (Kendall’s S statistic $P-Q = -432.00$; tau without continuity correction $= -0.124$, $Z = 1.669$, $p = 0.095$; tau with continuity correction $= -0.124$, $Z = 1.664$, $p = 0.096$) and Egger’s linear regression test (intercept $= -0.859$, $SE = 0.990$, 95% CI -2.828 to 1.110 , $t = 0.868$, $df = 82$, $p = 0.388$). Duval and Tweedie’s trim-and-fill test identified 12 missing studies and an adjusted ES of 0.278 (95% CI -0.711 to -0.233 , $Q = 783.176$).

3.5.3. Difference at the transfer phase (transfer test)

Data from 16 studies investigating the difference between blocked and random practices at the post-acquisition test were pooled in our MA. These 16 selected studies yielded a total of 38 ES. A small effect size was computed (Fig. 6), indicating a non-significant difference between random and blocked practice at the retention test ($ES = -0.243$, 95% CI -0.506 to 0.021 , $p = 0.071$). A significant heterogeneity was computed ($Q = 168.381$, $df = 37$, $I^2 = 78.026$, $p = 0.000$); therefore, subgroup analysis and meta-regression analysis were performed.

3.5.3.1. Subgroup analysis. Subgroup analysis revealed no moderating effect of “age groups”, “experience level”, “sports”, “skills”, and “testing protocol” categories (overall p values $= 0.109$, 0.139 , 0.344 , 0.322 , and 0.074 , respectively). Except for the skill group “serve,” which showed better outcome performance in favour of random practice ($ES = -0.556$, $p = 0.010$) (Fig. S9), no significant differences were detected between blocked and random practices for all other categories’ subgroups ($p > 0.05$). The significant difference in the skill group “combined volley-ball skills” was not considered, as only one ES from one study was analysed.

3.5.3.2. Meta-regression. Meta-regression analyses revealed no moderating effect of female percentage (coefficient 0.006, $p = 0.243$), intervention duration (coefficient -0.003 , $p = 0.759$), number of training sessions (coefficient -0.003 , $p = 0.944$), total number of acquisition trials (coefficient -0.001 , $p = 0.107$), and duration between pre- and post-acquisition test sessions (coefficient 0.041, $p = 0.287$).

3.5.3.3. Publication bias. Visual inspection of the funnel plot (Fig. 7) showed no evidence of publication bias, a conclusion confirmed by Begg and Mazumdar’s rank correlation test (Kendall’s S statistic $P-Q = -117.00$; tau without continuity correction $= -0.166$, $Z = 1.471$, $p = 0.141$; tau with continuity correction $= -0.165$, $Z = 1.458$, $p = 0.145$) and Egger’s linear regression test (intercept $= -2.134$, $SE = 1.604$, 95% CI -5.387 to 1.120 , $t = 1.330$, $df = 36$, $p = 0.192$). The Duval and Tweedie’s trim-and-fill test identified 12 missing studies and an adjusted ES of -0.243 (95% CI -0.506 to 0.021 , $Q = 168.381$).

In summary, the findings from this section revealed no statistical evidence that random practice leads to systematic lower post-acquisition performance and/or greater retention and/or transfer performances. This paradoxical effect of CI is partially

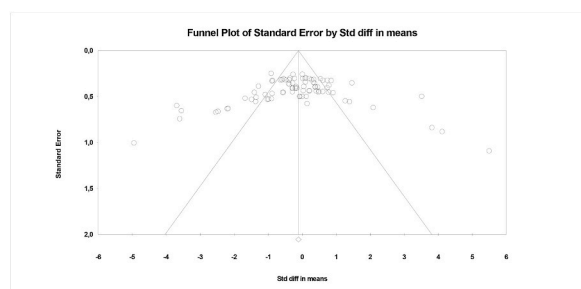


Fig. 5. Funnel plot for performance outcomes at retention test.

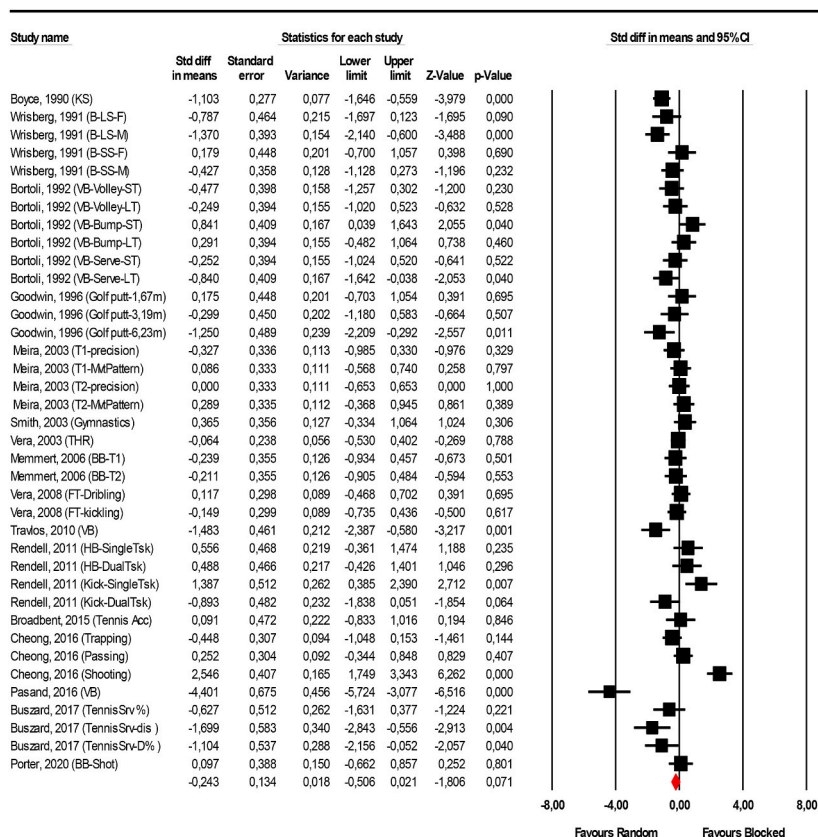


Fig. 6. Forest plot for the difference between blocked and random practices at transfer test.

Abbreviation: KS: Kneeling Shooting; T: Transfer test; THR: Throwing; HB: Handball; LT: long transfer; ST: short transfer; VB: Volley ball; Putt: putting; DT: decision time; BB: Basketball; VB: Volleyball; Acc: Accuracy.

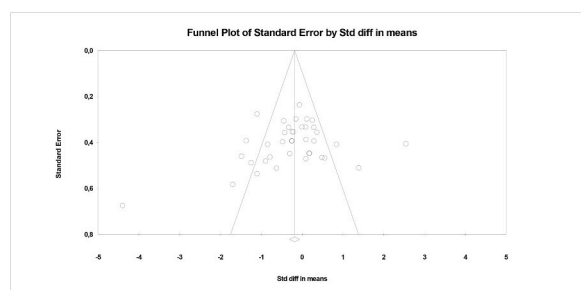


Fig. 7. Funnel plot for performance outcomes at retention test.

corroborated in very specific conditions, with no overall statistical difference between random and blocked schedules at any testing session (post-acquisition, retention, or transfer test session).

3.5.3.4. Sensitivity and cumulative meta-analysis. Results of the sensitivity and cumulative meta-analysis are presented in [Figures S10, S11 and S12](#). Results of the leave-one-out sensitivity analysis indicated the difference between blocked and random practices in all of the above-mentioned pooled effects was robust and not significantly driven by any single study. For the cumulative meta-analysis, no significant fluctuation was found in the results of all the above-mentioned pooled effects, indicating the stability of the results over time.

3.5.3.5. Methodological quality of studies. Overall, the study quality was deemed to be fair to good ([Table 2](#)). The PEDro scale revealed a score of five to seven for all included studies with a mean PEDro score of 5.9 ± 0.66 (mean $\pm$ SD). Of the 37 included studies, 27 investigations received a good score of six (21 trials) or seven (6 trials) out of 10 (i.e., a double-blind but not triple-blind trial). The

Table 2

Methodological quality of the included studies assessed with the PEDro scale.

Included study	Eligibility Criteria	Random Allocation	Concealed allocation	Baseline similarity	Blinding of Subjects	Blinding of Therapists	Blinding of Assessors	Outcome obtained from >85% of allocated subject	Intention to Treat Analysis	Between Group Comparisons	Point Measure and Variability	Score
Boyce and Del Rey (1990)	1	1	0	1	1	0	0	1	1	1	1	7
French et al. (1990)	0	1	0	1	0	0	0	1	1	1	1	6
Wrisberg and Zhan Liu (1991)	0	0	0	1	0	0	0	1	1	1	1	5
Bortoli et al. (1992)	0	1	0	1	0	0	0	1	1	1	1	6
Hall et al. (1994)	1	1	0	1	1	0	0	1	1	1	1	7
Goodwin and Meeuwssen (1996)	1	1	0	1	0	0	0	1	1	1	1	6
Hebert et al. (1996)	1	1	0	1	0	0	0	1	1	1	1	6
Pollatou et al. (1997)	1	1	0	1	0	0	0	1	1	1	1	6
Bortoli et al. (2001)	1	1	0	1	0	0	0	1	1	1	1	6
Meira and Tani (2003)	1	0	0	1	0	0	0	1	1	1	1	5
Smith et al. (2003)	1	1	0	1	1	0	0	1	1	1	1	7
Vera and Montilla (2003)	1	0	0	1	0	0	0	1	1	1	1	5
Keller et al. (2006)	1	1	0	1	1	0	0	1	1	1	1	7
Memmert (2006)	1	0	0	1	0	0	0	1	1	1	1	5
Jones and French (2007)	1	1	0	1	0	0	0	0	1	1	1	5
Zetou et al. (2007)	1	1	0	1	1	0	0	1	1	1	1	7
Vera et al. (2008)	0	1	0	1	0	0	0	1	1	1	1	6
Bertollo et al. (2010)	1	1	0	1	0	0	0	1	1	1	1	6
Menayo et al. (2010)	1	1	0	1	0	0	0	1	1	1	1	6
Travlos (2010)	1	1	0	1	0	0	0	1	1	1	1	6
Rendell et al. (2011)	1	0	0	1	0	0	0	1	1	1	1	5
Cheong et al. (2012)	1	1	0	1	0	0	0	1	1	1	1	6

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Table 2 (continued)

Included study	Eligibility Criteria	Random Allocation	Concealed allocation	Baseline similarity	Blinding of Subjects	Blinding of Therapists	Blinding of Assessors	Outcome obtained from >85% of allocated subject	Intention to Treat Analysis	Between Group Comparisons	Point Measure and Variability	Score
Naimo et al. (2013)	1	0	0	1	0	0	0	1	1	1	1	5
Sadri et al. (2013)	0	1	0	1	0	0	0	1	1	1	1	6
Yanci et al. (2013)	1	1	0	1	0	0	0	1	1	1	1	6
Krause et al. (2014)	1	1	0	1	0	0	0	1	1	1	1	6
Broadbent et al. (2015)	1	0	0	1	0	0	0	1	1	1	1	5
Yanci et al. (2015)	0	1	0	1	0	0	0	1	1	1	1	6
Cheong et al. (2016)	1	0	0	1	0	0	0	1	1	1	1	5
Pasand et al. (2016)	1	1	0	1	0	0	0	1	1	1	1	6
Buszard et al. (2017)	1	1	0	1	0	0	0	1	1	1	1	6
Fazeli et al. (2017)	1	1	0	1	0	0	0	1	1	1	1	6
Aiken and Genter (2018)	0	1	1	1	0	0	0	1	1	1	1	7
North et al. (2019)	1	0	0	1	0	0	0	1	1	1	1	5
Porter et al. (2020)	1	1	0	1	0	0	0	1	1	1	1	6
Sharp et al. (2020)	1	1	0	1	0	0	0	1	1	1	1	6
Jeon et al. (2021)	1	1	0	1	0	0	0	1	1	1	1	6

remaining studies (10 out of 37) received a fair score of 5 as they failed to randomize the trial, conceal the allocation, and blind subjects, therapists and assessors.

4. Discussion

The CI model posits practice under random or serial schedules leads first to inferior acquisition, and second to superior retention and transfer compared to blocked practice (Battig, 1972; Lee & Magill, 1985; Schmidt et al., 2011; Shea & Morgan, 1979). This phenomenon is well established in laboratory settings (Wang et al., 2022). However, according to the findings of the present systematic review and meta-analysis, this hypothesis and the related assumption cannot be generalized to sports-related settings. Indeed, only 43 performance outcomes (21%), out of 205 overall pooled outcomes, agreed with the CI model with no statistically significant overall difference between blocked and random practices during the testing sessions.

The results of sub-group analyses and meta-regressions further disprove the CI effects in sports-related settings and revealed that only in very specific conditions the CI effect can be partially corroborated. The subgroup analyses only revealed inferior acquisition and superior retention following random practice in the student-age group (20–24 years old), with no differences at transfer testing for this specific age group. Furthermore, no CI effects were concurrently identified during both acquisition and retention phases in any of the remaining subgroups (e.g., examined based on experience level, sports, skills, and testing protocol categories). Additionally, out of the 27 studies investigating both acquisition and retention phases (i.e., 12 with and 15 without transfer tests), only 5 studies corroborated the CI-model for both acquisition and retention phases in some performance outcomes (2 out of 8 outcomes, Vera et al., 2003; 2 out of 4 outcomes, Menayo, Sabido, Fuentes, Moreno, & Garcia, 2010; and 1 out of 2 outcomes, Rendell, Masters, Farrow, & Morris, 2011), with only one of them (Pasand et al., 2016) fully conforming the CI-model in the three tests.

According to previous suggestions, the CI effect is less common in sports-related research settings due to the complexity of the performed gross motor skills versus laboratory fine motor skills (Barreiros et al., 2007; Brady, 2004; Newell et al., 1989; Wulf & Shea, 2002). Indeed, due to the increased cognitive and physical demands as a result of a rise in stimulus-response choices and/or required effectors, intra-task interference created by the sequential complexity of repetitive tasks has been reported to supersede the intra-task interference created by high CI schedules, thereby making the benefits of random practice schedules less likely (Albaret & Thon, 1998). The majority of sports skills may be categorized as complex skills since they tend to involve whole-body motions requiring parallel coordination of several limbs (Gebkenjans, Beckmann, & Schöllhorn, 2007). However, levels and different forms of movement complexity, such as parallel and/or serial, and their sensory system specificity (e.g., visual and/or proprioceptive) are still unexplored (Schöllhorn et al., 2022).

4.1. Difference between blocked and random practices at the acquisition phase

Attempts to extend the CI model to applied settings with large number of DGF have yielded equivocal findings. The reviewed studies support the problem of inductive reasoning in the form of generalizing the originally found CI effect to sport performances after the acquisition phase. Only 18 performance outcomes (out of 103 pooled outcomes) agreed with this CI effect hypothesising superior acquisition following blocked compared to random practice, whereas almost five times more (85) outcomes disprove this hypothesis and showed either non-significant differences (77 outcomes) or even a reversed effect (8 outcomes). Overall, the present meta-analysis revealed no statistically significant difference between blocked and random practices after the acquisition phase.

Several moderating factors have been suggested to explain the inconsistencies with the CI model. The most important moderators of the CI effects that have been proposed were participants' characteristics, skill nature, as well as the intervention and testing protocols (Barreiros et al., 2007; Brady, 2004; Wang et al., 2022). Overall, the current subgroup meta-analysis and meta-regressions did not support significant moderating effects of these variables during the acquisition phase. Only trivial ES were revealed for participants' age (0.18) and experience levels (0.04), sports and skills categories (0.09), and testing protocol (0.1). Similarly, regression coefficients for participants' sex, and testing and intervention protocols (i.e., duration, and number of total acquisition sessions and trials), indicated no moderating equivalence.

A detailed examination of each subgroup revealed superior acquisition performances after the blocked practice can only be found under very specific conditions. A closer look at the "experience level" subgroup meta-analysis indicated post-acquisition performances were in favour of blocked practice in "unexperienced" learners, whereas no significant differences between blocked and random practice were computed in the "experienced" and "novice/medium" groups. Del Rey et al. (1982 and 1989) and Shea, Kohl, and Indermill (1990), hypothesized that the CI effect during the acquisition phase would depend on the learners' experience, with low-skilled participants benefiting more from low CI practice and high-skilled participants benefiting more from high CI. The present results partially support this assumption in sports-related settings and indicate inexperienced learners may benefit from higher acquisition under blocked practice conditions. Nevertheless, if the detrimental interference during the acquisition phase is a pre-requisite for a subsequently greater learning effect, according to the CI model, then beginners in particular should be recommended to prefer high CI practice. However, the second part of the assumption was not supported. The "experienced" subgroup tended to perform better on the post-acquisition test under random practice, trivial pooled ES was computed (−0.17) and the advantage over blocked practice was not significant ($p = 0.3$). The present findings are in line with those of Hebert et al. (1996) showing enhanced acquisition for low-skilled students practicing forehand and backhand tennis strokes in isolation compared to those alternating between forehand and backhand, as well as no major differences between both schedules in skilled students.

According to Wulf and Schmidt (1994), high CI practice for unskilled learners may produce excessive response variability, impeding the development of a stable motor pattern. Considering the cognitive load theory, Gentile (1998) assumed complex sports

tasks combined with a random schedule may lead to system overload during the early stage of learning (e.g., unskilled learner) given the high attention, memory, and motor demands. Based on Gentile's (1972) model of skill acquisition stages, and contrary to the logic of the CI-model that considers the acquisition interference as the major presupposition for the subsequent learning advantages, Magill and Hall (1990) and Hebert et al. (1996) reasoned that blocked practice would facilitate an unskilled learner "getting the idea of the movement" by allowing them to perform repeated attempts of a task, while high CI practices would be counterproductive during the early stage of skill learning. Logically consistent, they succumb to the temptation of short-term acquisition success by following the traditional model rather than the counter-intuitive idea of the CI model and recommend that unskilled learners first make repeated attempts at a low CI rather than high CI to explore and establish the basic pattern of coordinated movement (Gentile, 1972), experience the transformation of power (Siedentop, 1983), and/or break the proficiency barrier (Haubenstricker & Seefeldt, 1986).

Similarly, Brady (2004) proposed this interactive effect may interact with the performer's age and stated that while this interactive effect does apply to adults, it is more pronounced in more inexperienced and younger participants due to their less developed information-processing capacity. Investigating the interaction of the participants' age with the CI effect in laboratory settings, Pollock and Lee (1997) approved the interaction between practice schedule and participants' age, showing university-age adults had superior finger-propelling performance under blocked practice during the acquisition phase, while contrary to the expectation of stronger effects in children no difference between blocked and random practices was measured for the 7-year-old children.

The studies reviewed herein support, in sports-related settings, Pollock and Lee's findings and showed (i) trivial ES between blocked and random groups in childhood ($ES = -0.1$ and 0.2 for 5 to 9 and 10–14 years old, respectively), indicating the absence of a significant difference between both schedules at this age, and (ii) a roughly moderate ES of 0.3 for university-age adults (20–24 years old), indicating higher acquisition under blocked practice for this age group. Thereby, most of the participants in age group studies were inexperienced, and in sum, the result is strongly influenced by the extreme positive effect of a single study result with inexperienced participants (Pasand et al., 2016). When considering an older adult population, Jeon et al. (2021) showed three various outcomes performances in favour of blocked practice with an overall large ES of 1.36 . Although, in line with the CI model, a conclusion about this CI effect remains unclear in the aged population, as this was only one study in healthy aged population, focusing only on balance abilities. Altogether, a significant difference in favour of blocked practice was found in two of the six "age" sub-groups, resulting in no overall significant difference between both practices, thus calling the CI effect during the acquisition phase into question.

A closer look at the subgroup meta-analysis based on the "skills" category revealed significant differences between blocked and random practice at post-acquisition test, in favour of blocked, only in the "other skills" (balance, cartwheel, rhythmic step, agility) group (roughly large ES of 0.7), but not in the remaining 11 skills groups. Surprisingly, the CI-effect during acquisition was reversed for "baseball" performances (superior performance using random practice) and absent for all the remaining 12 sports groups, putting again into doubt the generality of CI effects during this phase. Interestingly, the conditions "inexperienced" and only the age group between 20 and 24 provided evidence for the first CI effect. Normally, one would rather associate younger age with "inexperienced". Since the participants were university students, the suspicion of an additional moderator variable in the form of taught learning content or learning strategies is obvious (Bluemke, Brand, Schweizer, & Kahlert, 2010).

The absence of the first CI effect in younger ages is often attributed to lower cognitive processing capacity (Brady, 2004; Gentile, 1998). If these differences between young and old were due to impaired information processing in children, which is commonly associated with lower working memory capacity, this should be reflected in an enhancement of the first CI effect (i.e., random practice should lead to significantly worse acquisition performance than blocked practice). However, current subgroup analysis revealed no significant differences between blocked and random practice in the majority of children's post-acquisition performance. Sana, Yan, Kim, Bjork, and Bjork (2018) indicated the effects of interleaved practice did not depend on working memory capacity. As such, another explanation of the absence of the CI effect in children is needed.

Rather than collecting and listing numerous specific exceptions to the limitations of the CI model, the stochastic resonance metaphor within the differential learning (DL) model offers an alternative and more differentiated explanation subsuming the phenomena related to age, experience level, and complexity under the common principle of noise (Schöllhorn, Mayer-Kress, Newell, & Michelbrink, 2009). With respect to age, the stochastic resonance model suggests that in children, blocked practice already contains movement variance comparable to the variance induced by random practice (Janssen, Gebkenjans, Beckmann, & Schöllhorn, 2010; Schöllhorn, 2016, 2022; Schöllhorn et al., 2009). Evidence for an increase in the variance of movement execution in randomized sequence compared to the variance in blocked sequence was provided by Schöllhorn and coworkers' studies (Gebkenjans et al., 2007; Janssen et al., 2010). The same may apply to the experience level and complexity (in terms of the amount of information to be processed in parallel) of the movements to be learned. Beginners show a larger variance in movement execution at the beginning, even with repetitions; similarly, for complex movements, the variance in repeated learning corresponds to the variance induced by high CI (Janssen et al., 2010; Schöllhorn, 2016, 2022; Schöllhorn et al., 2009). The extent to which these processes are moderated by conscious and unconscious processes, e.g., learning habits, cultural customs or executive functions, and thereby blocking access to the motoric areas in the brain, would be another area of future exploration. (Bluemke et al., 2010).

Taken together, the recommendation to start with blocked training in children is difficult to distil from the present MA data. Further research is needed to determine whether the differences in children's behaviour are also related to different judgments, a higher tolerance for deviations from expectations, and/or the ability to adapt more quickly to changing conditions.

4.2. Difference between blocked and random practices after the retention phase

Retention tests are often defined as specific measures for learning (Magill & Anderson, 2010). The long-term advantages of high CI

practice on the learning process are quite well established in laboratory settings (Brady, 2004; Magill & Hall, 1990; Shea & Morgan, 1979; Wang et al., 2022). Most often, high CI practice has been assumed to involve learners either in (a) deeper and more elaborated information processing, known as the “elaborative hypothesis” suggested by Shea and Zimny (1983), and (b) increased forgetting, which requires extra retrieval practice and reconstruction of tasks’ plans of action, known as the “forgetting/reconstruction hypothesis” suggested by Lee and Magill (1983, 1985). According to both hypotheses, more detailed and permanent representations of the task have been suggested to be memorized during the random practice, which may enhance retention.

However, in sports-related settings, numerous inconsistencies between findings have been reported (Barreiros et al., 2007; Brady, 2004; Shewokis, 1997; Wang et al., 2022), with some studies demonstrating superior performance for the random group during the retention phase (Hajihosseini, 2016; Kalkhoran & Shariati, 2015; Memmert, 2006; Pasand et al., 2016), whereas others report no significant differences between both strategies (Naimo et al., 2013; Porter et al., 2020; Sharp et al., 2020; Yanci et al., 2013; Zetou et al., 2007), and sometimes even an inversed second CI effect (Farrow & Maschette, 1997; Hall et al., 1994; Zipp & Gentile, 2010).

The findings of the present meta-analysis further reflect the difficulty to generalize the CI retention phenomenon to sports-related context. Indeed, out of 84 pooled outcomes, only 19 performance outcomes agreed with this second part of the CI model during retention with superior performances following high CI compared to low CI practice, whereas 65 outcomes did not support this hypothesis and showed either non-significant differences (53 outcomes) or reversed effects (12 outcomes). In sum, the present meta-analysis revealed no statistically significant difference between blocked and random practices after the retention phase. These findings call into question the extension of the CI effects together with the “reconstruction” and “elaboration” theoretical explanations to the sports context. If these two hypotheses only serve to explain the matched cases, it is expected from a scientific theoretical standpoint to state the exact boundary conditions for successful replications and to offer explanatory models for unsuccessful replications from a sport’s practical standpoint. Given the large number of unsuccessful replications of both CI phenomena, this calls for further research before inadmissible generalizations are made public (Schöllhorn et al., 2022).

Aiming to provide a deeper understanding of the interaction between CI and motor learning, as well as objectively evaluating both theoretical positions, more recent behavioural studies have linked the assessment of CI effects on motor learning to neurophysiological measurements. The majority of these MRI-based studies on movements with smaller number of DGF support the extensiveness of motor sequence preparation during high CI practice as demonstrated by (i) the involvement of a broad set of neural regions critical to motor skill acquisition (Hardwick, Rottschy, Miall, & Eickhoff, 2013; Penhune & Steele, 2012; Wymbs & Grafton, 2009), and (ii) the activation of the default or scaffolding network thought to facilitate the handling of either novel or complex tasks (Cross, Schmitt, & Grafton, 2007; Kelly & Garavan, 2005; Wymbs & Grafton, 2009).

However, no evidence has been provided to the emergence of specific behavioural or neural signature or profile during particular CI-practice regimes (blocked and/or random) (Wright et al., 2016). Indeed, it has been speculated that during high CI-practice, individuals can use similar motor programming strategies during extensive training, but perform very differently during subsequent testing phases (Wright, Brueckner, Black, Magnuson, & Immink, 2004); this contradicts the hypothesis derived from these early theoretical positions that the high CI context is distinguished by a more elaborative and deeper processing (Shea & Zimny, 1983) and/or extra retrieval practice and reconstruction operations (Lee & Magill, 1983, 1985). Unexpectedly, another study discovered the involvement of important motor regions recruited for training (i.e., frontal-parietal regions) was reduced during the retention test of a finger sequence production (Lin et al., 2011). The authors suggest learners used data stored in other neural circuits, specifically M1, thought to be crucial for temporary coupled neural circuits and motor memory storage (Lin et al., 2011). Moreover, the brain regions of the main activities seem to differ after random practice at different ages (Lin et al., 2012).

Against the background of recent brain studies (Gu & Kanai, 2014; Huskey, Turner, & Weber, 2020), this distinction seems to be only a first step of rough differentiation to analyze individual and situational responses to different learning scenarios (Horst, Janssen, Beckmann, & Schöllhorn, 2020; Schöllhorn et al., 2022).

Nevertheless, it should be noted that all neurophysiological studies are based on MRI data, which only allow experiments with movements with smaller number of DGF. This in turn raises the epistemological issue of an inadmissible generalization or transfer to sports movements with large number of DGF (Newell et al., 1989; Schöllhorn, 2022; Wulf & Shea, 2002). Few studies on sports movements (large number of DGF) used EEG to analyze brain activity before, during, and after learning sequences in golf (Gallicchio & Ring, 2019), target throwing (Frömer, Stürmer, & Sommer, 2016), and badminton serve (Henz, John, Merz, & Schöllhorn, 2018), mainly showing altered activity depending on the timing and brain area of the measurement. An increase in activity of the prefrontal area mainly in the gamma frequency band that is associated with stress, immediately after high CI was identified (Henz et al., 2018). Whether this increase in post-activation is a necessary trigger for the advantageous retention phenomenon needs further research. Nonetheless, due to the small number of EEG studies with many different designs, premature conclusions should be avoided.

Likewise, the equivocal findings on the CI-effects in sports settings during the retention phase were also frequently attributed to various mediating variables. According to the overall findings of the current subgroups and meta-analysis, neither participant characteristics nor the intervention and testing protocols can serve as significant moderator of this second CI effect after the retention phase of sports’ skill learning. Indeed, trivial ESs were computed for all subgroup analyses ($ES = -0.06, -0.15, -0.21, -0.22$ and -0.25 for “age”, “skills”, “sports”, “experience level” and “testing protocol”, respectively). Similarly, regression coefficients ranging between 0.000 and 0.04 for participants’ sex, testing, and intervention protocols (i.e., duration, and number of total acquisition sessions and trials) indicated no moderating equivalence.

A closer look at the “age” subgroup meta-analysis indicated retention performances were in favour of random practice only in young adult age groups ($ES = 0.41$ for “20–24 years” and 0.61 for “25–32 years”), while no significant advantage of high CI-practice was found for children and adolescent age groups. This result is not surprising and is generally consistent with children’s developmental stages. As a result, deeper processing and strategy formation are not only required in random practice (Parab et al., 2018), but

also for increased intra-task variance, which is associated with increased cognitive processing (Cross et al., 2007; Schöllhorn et al., 2022) and a concomitant increase in attention demand (Henz et al., 2018; Li & Wright, 2000). Indeed, an increase of 13% in secondary task reaction time was previously reported during more pronounced reconstructive activities, indicating high CI requires the learner to employ more active attention-demanding motor planning (Immink & Wright, 1998; Li & Wright, 2000).

Given the absence of systematic statistical differences between blocked and random practices in children at both the acquisition and retention phases, it is logically difficult to comprehend previous recommendations in favour of blocked training in children. Further differentiations towards individual boundary conditions such as intra/extraversion (Meira, Fairbrother, & Perez, 2015) or coping with frustration (Broadbent, Causer, Ford, & Williams, 2015) appear to be more promising here.

In laboratory settings, more apparent advantages of high CI-practice have been reported when learning tasks were controlled using different generalized motor programs (GMP). However, at first glance, sports-related settings produced inconclusive results, with some studies reporting enhanced learning when performing variations of a single task (e.g., same GMP: variations of badminton serve, Wrisberg & Liu, 1991) or tasks that require similar movements (e.g., different GMPs: hitting different pitches in baseball, Hall et al., 1994; or rifle shooting to a different target, Boyce & Del Rey, 1990), while others found no high CI advantage in the retention test when using sport tasks with few similar characteristics (Goodwin & Meeuwse, 1996; Porter et al., 2020). However, initial contradictions can be resolved by taking a closer look at the origins of GMPs and its inapplicability to sports movements.

In the context of schema model, GMPs were introduced as classes of movements with the same relative muscle forces or relative timing (Schmidt, 1975). These were, mainly due to methodical constraints, based on time discrete values. However, with the limitation of the validity of schema model to movements with negligible gravitational and inertial forces (Schmidt, 2003; Schöllhorn, 1997; Schöllhorn et al., 2022), sports movements were excluded from the variability of practice theory (Moxley, 1979), as was the generalization of GMPs to movements with large number of DGF. In contrast, in CI-related studies involving sports movements, gross phenomenological similarity of exercises is commonly used. However, describing both movement forms, large and small numbers of DGF, with process-oriented biomechanical feature patterns (Schöllhorn, 1993; Schöllhorn & Bauer, 1998) and comparing their temporal fluctuations during high CI practice (Janssen et al., 2010), the four random practice scenarios (2 GMPs: similar & unsimilar $\times$ 2 DGF: small & large) can be displayed against their learning effects by means of an inverted U-shape curve. According to the stochastic resonance metaphor within the DL-model (Schöllhorn, 2005), it is hypothesized randomized practice within the same GMP with small number of DGF has too little noise for optimal learning, while at large number of DGF, switching between unsimilar movements has too much noise. Only the noise in movements with small number of DGF between different GMPs and the noise in movements with large number of DGF within similar movements seem to cause an optimal amount of noise for effective learning. However, more research on movements with small number of DGF would be required to rule out the possibility that DL, which has so far been confirmed almost exclusively on sports movements, does not suffer the same fate as CI learning (originally modelled for small number of DGF) in movements with large number of DGF.

Independent therefrom, the optimum of noise corresponds to the optimal subjective information in terms of cybernetic pedagogy (Schöllhorn et al., 2022; Von Cube, 1965), desirable quantification difficulties (Bjork, 1994a; 1994b), or challenge point-hypothesis (Guadagnoli & Lee, 2004). In contrast to the last two proposals, which are difficult to quantify because of their dependence on the learner's experience, the first one offers quantification in terms of individual noise of movement patterns in response to externally imposed scenarios. Consequently, the task of a coach, trainer, therapist, or teacher would be to take care of the right amount of noise in the system to ensure optimal (individual and situative) learning information. Furthermore, an adequate quantification of the similarity of movements is a prerequisite for future satisfactorily investigation (Horst et al., 2020; Schöllhorn, 2016).

A more detailed examination of the "testing protocol" results in the current subgroup analysis revealed better retention performances following random practice when using "random" or "random/blocked" (random for random practice and blocked for blocked practice) testing protocols ($SMD = -0.45$ and -0.48 , respectively), but not during blocked testing. This is in concert with the very first experiments of Shea and Morgan (1979).

A closer look at the subgroup meta-analysis based on the "skills" and "sports" categories revealed significant differences between blocked and random practice in favour of the random one only in the "golf putting/chipping" and "kicks" skill groups, as well as in the "golf" sport, but not in the remaining 10 skill groups and 12 sports groups. The extent to which the results of the golf and kick studies are due to a high visual component (estimating distances and assigning forces in parallel) is still unknown (Schöllhorn et al., 2022). Aside from the possible reliance on the primary involved sensory system, the different results in relation to blocked and randomized retentions test indicate the learning of an additional aspect by high CI practice. It seems in these cases, not only the movements but also the switching ability between them was learned. To what extent this is specific to the exercises practiced or transferable to other exercise groups would be an interesting open question for future research.

CI effects during retention were absent in the "unexperienced," "novice/medium," and "experienced" groups and unexpectedly reversed in the "other skills" (balance, cartwheel, rhythmic step, agility) group and in participants over the age of 65 (better performance using blocked practice), putting once again the CI phenomenon (specifically the effects on retention performance) into doubt. To what extent these effects depend on the specific neurophysiological conditions at post-acquisition test will have to be determined through future research.

4.3. Difference between blocked and random practices at the transfer phase

Magill and Anderson (2010) reported that while retention tests were specific to learning assessment, transfer tests were more reflective of adaptability. As a result, even if retention and transfer overlap, which is to be expected, they assess related but different constructs. Given that CI effects, particularly in laboratory settings, are consistently more robust during transfer than retention tests,

the transfer phase has been proposed as a more reliable indicator of the CI effect (Shewokis, 1997). The current meta-analysis' findings highlight, once again, the difficulty of generalizing the second part of the CI hypothesis to a sports-related context, specifically the CI transfer phenomenon to sports movements. Indeed, out of 38 pooled outcomes, only 6 performance outcomes agreed with this part of the CI hypothesis during transfer with superior performances following random compared to blocked practice, whereas 32 outcomes were in conflict with this hypothesis and showed either non-significant differences (27 outcomes) or reversed effects (5 outcomes). Overall, the present meta-analysis revealed no statistically significant difference between blocked and random practices at the transfer phase. During the transfer test, outcome performance was superior following random practice compared to blocked practice only in "serve" skills. This part of the CI effect was absent in all remaining skills as well as in all "sports," "age," and "experience-level" subgroups.

From a scientific and theoretical standpoint, more differentiated assessments of CI effects in both retention and transfer testing depend on the sport-related exercise scenarios and are highly speculative, especially in the absence of concrete movements' variance quantifications. It ultimately becomes a question of mass rather than content. Additionally, although considered by many to be the "platinum standard of evidence," generalized conclusions based on meta-analysis are similarly problematic, as it has been argued by others that meta-analysis falls far short of that standard (Stegenga, 2011). Indeed, various meta-analyses of the same evidence can reach conflicting conclusions. Additionally, because so many decisions must be made when completing a meta-analysis, which leaves wide latitude for subjective preferences to affect the outcome, meta-analysis falls short of offering objective grounds for intersubjective assessments of hypotheses (Stegenga, 2011). Rather, in the case of contradictory statements, alternative models allowing for more differentiated explanations should be sought. The division into binary (low and high CI) or ternary (blocked-serial-random) scaling does not seem to be very conducive to the now-current problems of the CI model. Approaches to nominal scaling represent a very subjective and crude division (Buszard, Reid, Krause, Kovalchik, & Farrow, 2017). Biomechanical pattern recognition not only seems the most elaborated, but is also the most differentiated (Schöllhorn & Bauer, 1998). Something analogous is the case for the analysis of retention and transfer performances and their dependence on the preceding intervention structure. Differentiated nonbinary interdependence remains inaccessible.

Retention- and transfer-test evidence proves learning performance with different intentions. The retention test is concerned with the learner's temporal change, whereas the transfer test is more concerned with changes in spatial/environmental conditions. While the latter is relatively easy to define and quantify, the former is more difficult. Recent analyses of a ground reaction force database, which show fewer changes in gait patterns on the same day (Horst et al., 2016) and over several days (Horst, Eekhoff, Newell, & Schöllhorn, 2017), suggest temporal changes on the learner's part may be due to a change in the executive system rather than a memory drop. In this case, retention testing would be more concerned with adequately applying what was remembered under the newly altered conditions of the movement system rather than remembering what was learned. If the changes are located within the variances experienced during the exercise process, the interpolation process is activated. If they are located outside of these variances, extrapolation is used (Schöllhorn, 1999a, 1999b, 2000), with interpolation-based performance outperforming extrapolation-based performance (Catalano & Kleiner, 1984; Dean, Kovacs, & Shea, 2008). As a practical consequence, variances during the practice process should be designed to be large enough to account for possible changes over time in order to benefit from interpolation during the later test. The amount of non-intervention change appears to be individually and situationally variable over time (Burdack et al., 2020; Horst et al., 2017). Analogously, better transfer performance can be expected if, during the practice process, the variances of the joint angle trajectories already cover the required areas for solving the spatially changed situation.

4.4. The methodological quality of the studies

The included studies in the present MA have relatively fair to good quality with a maximum score of 7 out of 10. These results are understandable given the nature of motor learning experiments. Indeed, in such context, it is challenging and even impossible to blind therapists, assessors, and participants to the condition to which they are assigned. Future studies should therefore put more effort into resolving the other risk of bias items (i.e., PEDro items 1–4 and 7–10) in their study design to improve the overall methodological quality. Additionally, the Standardised Reporting of Trials (SORT) Statement (The Standards of Reporting Trials Group, 1994) was published in 1994, while the oldest papers included in this systematic review were published in the 1990ies. This implies that checklists for interventional trials were not available for at least 5 studies. This might partly explain the fair score in some of these older studies.

4.5. Strengths, limitations, and implications for future research

This is the first comprehensive systematic review and meta-analysis with subgroup analysis and meta-regression assessing the possibility of generalizing CI effects in sports movements. The comprehensive coverage of the existing literature and the careful appraisal of its quality, as well as the adherence to the most recent PRISMA guidelines (2020), are distinct strengths of the present review. However, some limitations of this SR and MA need to be addressed, and some methodological issues should be addressed in future studies in this field.

Following a careful screening of the results section of each study, we excluded 35 studies that failed to guarantee an equivalent baseline between control and intervention groups and/or to report accurate results (e.g., mean and SD), even though they may have valuable insights into specific aspects of CI. Although such exclusion is argued, the overall conclusion may have been impacted by the missing data from this important piece of research. This necessitates a cautious interpretation of the current results while also encouraging proper reporting of result values in future motor learning research and the assurance of an equal baseline for all groups

tested. Similarly, the high level of heterogeneity in the included studies due to the disparate motor skills, participant characteristics, and intervention protocols highlights once again the need for careful interpretation of the current findings and high-quality follow-up research. Also, the current subgroup analysis based on skill type might not be ideal for a holistic interpretation, suggesting future meta- and subgroup-analyses should consider more important covariates such as the level of “random” variation (e.g., parameterization or similar or different skills) and/or the level of “noise amount” of the training intervention (Schöllhorn et al., 2006, 2009).

To this end, future research should identify and use a proper common metric to quantify this amount (e.g., Schöllhorn, 2016). Furthermore, a key recommendation for future research is to compare various variability-based models (e.g., VP, CI, and DL) simultaneously at the theoretical and practical levels, while using a sufficient sample size that ensures a statistical power of at least 0.8 (Cohen, 1988; Thomas, Lochbaum, Landers, & He, 1997) and considering various confounding variables like motivation, feedback, instruction, etc. The reconstruction and elaboration hypothesis should also be objectively examined; hence, it is advised that future studies combine reliable neurophysiological measurements, appropriate to movements with large number of DGF (e.g., EEG, HRV), with skill performance assessment.

5. Conclusion

To conclude, the CI model hypothesizes that practice under high CI leads to inferior acquisition and superior retention and transfer capabilities compared to low CI. Although this phenomenon is well established in laboratory settings, the present meta-analysis found very limited support for the validity of the CI model in sports settings as no overall significant difference between blocked and random practice was found in either the acquisition, retention, or transfer situation. With this lack of generalizable support, other models based on the CI model, such as that of Renshaw et al. (2019), are compromised. As such, we recommend caution when applying these findings to all populations, skills, and sports, as the subgroup analysis showed that the CI hypothesis can be partially valid in very specific conditions. Precisely, our findings revealed that (i) both acquisition and retention CI effects are significant in students aged 20–24, (ii) the acquisition effect alone in favour of blocked practice was significant in inexperienced populations, (iii) the retention effect alone in favour of random practice was significant in the population aged between 20 and 32 years old and in golf and kicking skills, and (iv) the transfer effect alone in favour of random practice was significant only in serve skills. Nonetheless, the CI phenomena was not fully confirmed in any of the investigated subgroups and was even reversed in others, calling the generalization of the CI model to sports practice into question. Given that the present MA attempted to validate CI effects in a sports context by comparing the performance of blocked vs. random groups at acquisition, retention, and transfer tests, with little attention paid to the possible gain in each phase, the CI phenomenon should be further evaluated in future MAs by focusing on the acute and delayed skilled performance’s gains generalized by each amount of CI. More importantly, future research combining reliable neurophysiological measurements and single case studies with sports skill assessment are required to better understand the less common effect of CI in applied research settings and to evaluate potential benefits from alternative motor-learning models (e.g., differential learning) in sports practice (Schöllhorn et al., 2009).

Author statement

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Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Data availability

Data will be made available on request.

Appendix A. Supplementary data

Supplementary data to this article can be found online at <https://doi.org/10.1016/j.edurev.2023.100537>.

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(denotes studies included in the meta-analysis)

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