

## Commentary

### Title: Comment on Czyż et al. (2024) on Contextual Interference in Motor Learning

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#### Abstract

On June 10th, 2024, a paper by Czyż et al. titled “High contextual interference improves retention in motor learning: systematic review and meta-analysis” was published in the esteemed journal, *Scientific Reports*. Given its relevance to our research area and its close similarity to two recent meta-analyses our research group published on the effect of contextual interference (CI) on the acquisition and retention of motor skills in sport contexts (Ammar et al., 2023; Ammar et al., 2024), we read it with great interest. While we find the topic to be timely and the paper to be well-written, several concerns have arisen from our review of Czyż et al. This commentary summarizes the main concerns.

Among other issues, we were surprised by the unsubstantiated offensive statements against our previous work, such as “the review of Ammar et al. cannot be considered reliable and valid,” and by the definitive conclusion that “the CI effect is a robust phenomenon in motor learning,” which is not supported by the statistical findings of the paper. We have detailed our concerns regarding this paper in the manuscript, which we believe would be of interest to the motor learning community.

**Key words:** interference effect; contextual interference; random practice; blocked practice; applied setting; sport; motor skills; meta-analysis; systematic review; commentary

## 1. Introduction

Meta-analysis (MA) is a central statistical method used to accumulate knowledge and pool research findings from multiple individual studies within a common research field (Hansen et al., 2022). The primary goal of a MA is to increase statistical power, improve estimates of the size of an effect, and resolve uncertainties when individual reports disagree (Gurevitch et al., 2018). Beyond serving as a synopsis of a research question, as offered by narrative and systematic reviews, a MA provides a quantitative assessment of relationships between variables or the effectiveness of interventions (Gurevitch et al., 2018). It can also be used to test competing theoretical assumptions or identify important moderators (Aguinis et al., 2011; Bergh et al., 2016).

While MA have the potential to improve precision, answer questions not posed by individual studies, and settle controversies arising from conflicting claims, they also have the potential to mislead if specific study designs, within-study biases, variation across studies, and reporting biases are not carefully considered (Deeks et al., 2023). Rooted in the synthesis of the effectiveness of medical and psychological interventions in the 1970s (Glass, 2015; Gurevitch et al., 2018), MA has become a widely established method to provide a more comprehensive understanding of various research topics, including motor learning and specifically contextual interference (CI) effects.

Several MA (e.g., Brady, 2004; Sattelmayer et al., 2016; Ammar et al., 2023, 2024) have already been conducted to evaluate the CI hypothesis, which assumes that random practice (high CI) leads to inferior acquisition (1<sup>st</sup> effect), but superior retention and transfer capabilities (2<sup>nd</sup> effect) compared to blocked practice (low CI) (Battig, 1966). Brady (2004) was a pioneer in conducting a basic MA on CI, comparing the cumulative effect size between laboratory and applied studies as well as between young and adult learners. However, Brady's analysis focused only on the second CI effect. The MA by Sattelmayer et al. (2016) approached both CI effects but was limited to studies related to physiotherapy and medical education. Given the increasing number of studies in sports contexts and advances in MA methods since Brady's (2004) work, Ammar et al. (2023, 2024) addressed the contradictory findings related to both CI effects by conducting two MA with subgroup and meta-regression analyses. All these MA reported insufficient evidence to make definitive recommendations in favour of high CI, particularly in applied setting.

Surprisingly, on June 10th, 2024, a MA by Czyż et al. titled "High contextual interference improves retention in motor learning: systematic review and meta-analysis" was published in the esteemed journal *Scientific Reports* and made definitive conclusions on the CI effect, such as "High contextual interference improves retention in motor learning" (title). More surprisingly, although this paper mainly updated the MA of Brady (2004) while following the same limited approach of only focusing on the second CI effect, and despite the low quality of the majority of included studies in this MA and the potential presence of publication bias, the authors concluded with a generalized and misleading statement that "The CI effect is a robust phenomenon in motor learning." Therefore, caution is urged when interpreting the findings of this MA, particularly avoiding consideration of its generalized and affirmative conclusions.

Furthermore, considering recent emphasis on the potential issues with the massive production of systematic reviews and MA amidst a low number of high-quality randomized controlled studies, which can be unnecessary, redundant, misleading, and/or serve conflicted interests (Ioannidis, 2016), and given the low quality of the majority of original research already conducted in the CI field (Ammar et al., 2023, 2024; Czyż et al., 2024), we emphasize the need

for future high-quality and robust randomized studies. Additionally, caution should be exercised when interpreting and concluding findings in existent and future MA in this field. This caution is further supported by recent critiques of the current statistical “ritual” and the associated potential “replication delusions” (as termed by Gigerenzer, 2018), which are suspected to largely eliminate statistical thinking, especially in the social sciences (Gigerenzer, 2004).

## **2. Raised Matters**

Given its relevance to our research area and its close similarity to two recent MA our research group published on the effect of CI (Ammar et al., 2023; Ammar et al., 2024), we read it with great interest. While we find the topic to be timely and the paper to be well written, several concerns have arisen from our review of Czyż et al.:

### **A. Title misleading**

In the abstract, the authors report: “We found that the random practice schedule in laboratory settings effectively improved motor skills retention. On the contrary, in the applied setting, the beneficial effect of random practice on retention was almost negligible.” However, the title of the paper, “High contextual interference improves retention in motor learning: systematic review and meta-analysis,” uses an affirmative and generalized statement. This is misleading. In its current form, the title suggests that high CI improves retention in motor learning in general, encompassing both laboratory and applied settings. Nevertheless, the results of the MA do not support this statement. The authors could specify “in laboratory settings”. However, given the high number of low-quality studies included in the present MA and the potential for publication bias, this should be approached with caution. An uncertainty word like “seems” should be incorporated, revising the title to “High contextual interference seems to improve motor learning retention in laboratory but not applied settings:...” Alternatively, the authors could change the title to a non-affirmative form, such as phrasing it as a question.

### **B. Inaccurate information related to Graser et al. study and inaccurate criticism of our previous work**

In the introduction, the authors stated, “the meta-analyses on CI conducted by Graser<sup>22</sup> and Sattelmayer<sup>23</sup> provided the inclusion criteria.”. We would like to bring to the authors' attention that the study by Graser et al. (2019) was a systematic review and did not include a MA.

Importantly, we were surprised to read the following sentences related to our previous publication: “In 2023, Ammar and colleagues published their meta-analysis. Unfortunately, it was poorly performed. For example, they searched the Taylor and Francis database, though it is a publisher base, not a scientific one. At the same time, they did not screen the EBSCO database, which consists, among others, of APA PsycArticles, APA PsychInfo, SPORTDiscus with Full Text, Medline, and Academic Search Complete. Their review was not preregistered, which is a standard procedure these days. Given these methodological flaws, the review of Ammar et al. cannot be considered reliable and valid.”. We would like, therefore, to point out that the two criticisms mentioned here are not accurate.

- (i) First, the authors criticized our search of the Taylor and Francis database and the non-inclusion of the EBSCO database. This argument is misleading and inaccurate,

as they omitted to mention that Taylor and Francis was one of five databases we searched (PubMed, Web of Science, Scopus, Taylor and Francis, and SciELO). This multi-database approach is more than sufficient, as typical practice suggests searching at least two databases, with multiple databases recommended. In this context, a recent meta-research study by Ewad et al. (2022) concluded that searching two or more databases improves coverage and recall and decreases the risk of missing eligible studies.

- (ii) Regarding the second point, we adhered to the latest PRISMA guidelines (2020) for conducting systematic reviews and MA. While PRISMA highly encourages registration, it does not state that it is mandatory.

Taken together, it seems that the authors of the current paper offensively criticized our recent MA to argue the need for replication. However, given their poor arguments, the rationale for their study remains unclear. Importantly, these authors claim that our review cannot be considered reliable and valid based on these poor arguments. Here, the authors are encouraged to provide a valid reference and scientific basis showing that not registering a systematic review and including the Taylor and Francis database, in addition to four others, made our systematic review invalid and unreliable. Otherwise, this statement and the poor arguments need to be retracted from their manuscript. We encourage the authors to review their understanding of the concepts of validity and reliability, particularly in relation to systematic reviews and MA.

### **C. Inclusion and exclusion criteria: Lack of equivalent baseline control**

In the introduction, the authors mentioned, "...random practice condition hinders performance during acquisition, although it facilitates retention and transfer." This sentence suggests that a comprehensive assessment of CI effects requires focusing on both the first effect (suggested to be negative on acquisition) and the second effect (suggested to be positive on retention and transfer) of CI. The authors of the present MA elected to focus only on the second effect, particularly on pooling the results of the retention test in their analyses. However, they did not control for the confounding variable of higher baseline performance in one group (e.g., random) compared to the other (e.g., blocked), which can partly explain the observed benefits of high CI in retention. Indeed, they did not exclude studies that failed to guarantee that the random and blocked groups were equivalent at baseline.

In their methodological considerations, Ammar et al. (2023) emphasized that future original and review studies in motor learning need to ensure that intervention and control groups are equivalent at baseline. This equivalence is essential to attribute higher acquisition, retention, or transfer more confidently to the tested learning models rather than baseline inequivalence, particularly when the analysis is only based on comparing retention performance between groups. Accordingly, they excluded 25 studies that did not guarantee this equivalence. While the excluded studies may provide valuable insights into specific aspects of motor learning, including studies with non-equivalent baselines between groups in a MA necessitates focusing not solely on retention test results. Instead, the analysis should consider the delta change between baseline and retention and/or between acquisition (post-intervention) and retention. This approach was adopted in the most recent multi-level critical MA by Ammar et al. (2024) on the effects of CI learning on the acquisition and relatively permanent gains in skilled performance.

### **D. Discussion Section: Comparing incomparable findings and poor explainability**

In the discussion, the authors summarize the main findings of their MA, reporting a medium pooled effect size ( $SMD = 0.63$  and  $0.71$  when applying the three-level mixed model and the random-effects model, respectively). However, they found an effect size slightly above negligible ( $SMD = 0.23$  in the three-level mixed model and  $SMD = 0.28$  in the random-effects model) when analyzing data from applied studies, which turned out to be statistically insignificant, as they reported.

When comparing these findings with previous literature, particularly our MA (Ammar et al., 2023), they state: “These results are different from those reported by Ammar and colleagues. We found that the pooled effect size was medium  $0.63$  while Ammar et al. reported small.” This is not accurate. Our study mainly focused on sports settings, and thus should be logically compared with the negligible effect size found in the current MA when only data from applied studies were included. Comparing effect sizes calculated based on pooled data vs. those based on sport contexts (Ammar et al., 2023) doesn’t make sense, especially since the authors of the current study have already computed separate effect sizes for applied setting studies.

Interestingly, the authors tried to explain these incomparable results by criticizing our MA with the same poor argument (including Taylor and Francis and omitting EBSCO): “Probably the differences we found may be attributed to the search strategies, number of studies, and effect sizes included in both MA. Ammar et al. omitted the EBSCO database (including APA PsycArticles, APA PsychInfo, SPORTDiscus with Full Text, Medline, and Academic Search Complete), searching a publisher database instead (Taylor and Francis).” Here, we would like to point out that our search strategy, which combined the aforementioned five databases and applied more robust PICOS criteria, resulted in the inclusion of 37 studies, with 29 included in the retention phase analysis. This largely exceeds the number of studies included (only 19) in the applied setting analysis of the current MA (Figure 5). This could indicate that our methodology (Ammar et al., 2023) was more robust in improving coverage and recall while decreasing the risk of missing eligible studies. This could be due to the omission of PubMed by the authors of the current study. However, this could not be a critical issue as they also applied a multi-database approach. Nevertheless, as around 10 studies in the sports context were included in the study of Ammar et al. (2023) and not in the present analysis of the applied context, the authors of the present study are encouraged to argue why this important number of studies were not included, especially since they can easily identify and include them by screening the studies included in Ammar et al. (2023) and Ammar et al. (2024).

To conclude, the present findings further support the conclusion by Ammar et al. regarding the insufficient evidence supporting the beneficial effect of high CI in applied motor learning. Therefore, the authors of the current study are encouraged to focus more on the possible underlying mechanisms of the superiority of high CI benefits in laboratory compared to applied settings, rather than focusing on criticizing our MA.

### **E. Conclusion Overgeneralization**

The authors conclude with: “The CI effect is a robust phenomenon in motor learning.” Again, we do not agree with such an affirmative and generalized statement, which is not supported by the results of the current study, particularly those related to the applied setting. Additionally, the analysis conducted by the present authors only focused on the second CI effect, dealing with retention. The authors are therefore encouraged to avoid generalization and revise their conclusion to reflect only the current results. This recommendation is further supported by the potential publication bias and the fact that mainly low-quality studies were included in the present MA. Indeed, the authors stated in the “Low quality and bias problem” section of the

discussion, "...we need to consider that the CI effect as described here may be biased. According to the Quality Assessment Tool for Quantitative Studies, only three articles out of 54 presented moderate or high quality." The authors are therefore encouraged to be more careful with the interpretation of their findings and to revise their generalized and affirmative statements on the beneficial effect of high CI throughout their manuscript.

### **3. Concluding remarks**

Taken together, we believe multiple corrections are warranted for this manuscript. Specifically, the authors should avoid continuous attacks on similar previous work (eg., Ammar et al. 2023) throughout the manuscript based on poor arguments and focus more on clarifying the rationale for replicating the MA one year later. The authors could highlight that Ammar et al. (2023) mainly focused on sports settings, while they include both sports and laboratory settings. In this context, the authors should deepen their discussion of the poorer beneficial effect in sports compared with laboratory settings. Here, we recommend that the authors refer to our paper, which they criticized (Ammar et al., 2023), as well as our recent MA on motor learning gains (Ammar et al., 2024), to further develop their discussion and obtain helpful insights to strengthen the paper's rationale. It is also recommended to avoid generalized affirmative statements in favour of high CI that are not supported by the current findings, especially those related to the applied setting.

### **Additional Information**

#### **Competing interests**

The authors declare no competing interests.

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### **Author contributions statements**

AA wrote the main manuscript text, and both authors reviewed the manuscript.