

PRACTICING WITH GRADUAL INCREASES IN CONTEXTUAL INTERFERENCE: METHODS FOR TESTING THE PREDICTIONS OF THE PARALLEL DEVELOPMENT HYPOTHESIS

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Response

I would first like to thank the Editorship of Kinesiology for allowing me to submit a response to the Letter to Editor (Kearney, 2017) they received regarding my recent publication (Porter & Beckerman, 2016). I would also like to thank Kearney (2017) for submitting his perspectives and for acknowledging the contributions I and my colleagues have made to the existing body of motor learning research, particularly on the topic of contextual interference (CI). Kearney (2017) has raised two conceptual limitations of my work. The first being that I classify a serial practice schedule as producing a moderate amount of CI rather than producing a high amount of CI, similar to a random practice schedule. The second limitation raised by Kearney (2017) is that I have not used the learners' performance as an indicator as to when the amount of CI increases during practice. Rather, my research has required learners to follow a predetermined practice schedule starting with blocked practice (i.e., low CI), followed by serial practice (i.e., moderate CI) and concluding with random practice (i.e., high CI). Kearney (2017) concludes that both limitations need to be addressed in future research, and I completely agree.

I will first address the concern that I have classified a serial practice schedule as producing a moderate rather than high amount of CI. It is well established that CI exists on a continuum ranging from low (i.e., blocked practice) to high (i.e., random practice) (Porter & Beckerman, 2016; Porter & Magill, 2010). The amount of CI existing within a practice context is determined based on the order in which skills are practiced. Kearney (2017) contends that a serial practice schedule and a random practice schedule produce equally high amounts of CI. This conclusion appears to be based on the results of previous research that has compared the performance and learning results of participants practicing with serial and random practice schedules (e.g., Bortoli, Robazza, Durigon, & Carra, 1992; Lee & Magill, 1983) and the theoretical explanations offered in early research on the topic (Lee & Magill, 1983; Shea & Morgan, 1979; Shea & Zimney, 1983). I would like to point out a few important details. First, Shea and Morgan (1979) and Shea

and Zimney (1983) did not compare the learning effects of practicing with serial and random practice trial arrangements. Second, Lee and Magill (1983) did compare the learning effects of practicing with serial and random practice schedules and observed similar learning effects. However, Lee and Magill (1983) never suggested in their paper that serial and random practice produced equal amounts of CI; they only commented on the similarity of task switches as a mechanism for the similar learning effects between serial and random practice schedules. Lee and Magill (1983) do argue that a serial practice schedule is more cognitively effortful compared to a blocked practice schedule, which facilitates motor learning similarly to random practice, which also requires enhanced cognitive effort relative to blocked practice. I do not feel that it is accurate to conclude that serial practice and random practice schedules produce equally high amounts of contextual interference simply because they, at times, produce similar learning effects. With this in mind, it is worth pointing out that several studies have reported null differences between blocked and random practice during post-testing (e.g., Aloupis, Guadagnoli, & Kohl, 1995; Brady, 1997; French, Rink, & Werner, 1990; Jarus & Gutman, 2001; Jelsma & Pieters, 1989; Shewokis, 2003). One would not then use the results of those studies to conclude that blocked and random practice produce equal amounts of CI because both forms of practice resulted in the similar learning effects.

Just to be clear, I am not disputing that serial practice, at times, results in similar learning effects compared to random practice (Lee & Magill, 1983). In fact, there is some evidence that a serial practice schedule may result in learning advantages exceeding those of random scheduling (see Landin & Hebert, 1997, for an example). My position is that serial practice produces less contextual interference (i.e., moderate) relative to a random practice schedule. Consistent with my viewpoint, in Richard Magill's textbook (Magill & Anderson, 2017) he used a serial practice schedule as an example of a practice schedule that produces a moderate amount of contextual interference falling in the middle of the CI continuum between the extremes of blocked

and random trial arrangements (p. 389). It is also important to note that Goode and Magill (1986) referred to a serial schedule as a “mixed” form of practice combining blocked and random scheduling. Goode and Magill (1986) never suggested that a serial practice schedule produced the same amount of CI as a random schedule.

Kearney (2017) makes a valuable suggestion that future research should investigate practicing with gradual increases in CI with alternative forms of practice trial orderings in the middle portion of the practice session. As this is certainly a topic that deserves additional attention, I encourage others to test the predictions of the parallel development hypothesis (Porter & Beckerman, 2016; Porter & Magill, 2010) using alternative forms of CI progressions. I think many researchers and practitioners would be interested in the results.

Finally, Kearney (2017) proposes adopting a Win-Shift/Lose-Stay approach (Simon, Lee, & Cullen, 2008) to investigate alternative forms of

practicing with changing amounts of CI. This form of practice would allow repeated trials (i.e., blocked practice) until the learner achieved a specified level of mastery, then the learner would begin practicing a different task. Such a form of practice would likely result in an alternative form of an increasing practice schedule progressing from low to high CI with increased amounts of task switches as the learner becomes more skilled. I agree with the author of the letter, this is an avenue of research that needs to be pursued.

In closing, I would again like to thank the editorship of *Kinesiology* for allowing me the opportunity to write this response. I would also like to thank Kearney (2017) for sharing his perspectives on my earlier work and the works of others related to the topic of CI. I appreciate the author’s criticism and value the constructive feedback. Exchanges such as these are how new ideas are proposed, allowing the field to evolve in new ways in practice and theory.

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