Unloading the dice

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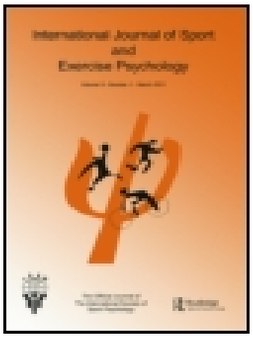
Citation for published version:
Bobrownicki, R, Carson, HJ, MacPherson, AC & Collins, D 2022, 'Unloading the dice: Selection and design of comparison and control groups in controlled trials to enhance translational impact within motor learning and control research', *International Journal of Sport and Exercise Psychology*, vol. 20, no. 5, pp. 1330-1344. https://doi.org/10.1080/1612197X.2021.1956567

Digital Object Identifier (DOI):
10.1080/1612197X.2021.1956567

Published In:
International Journal of Sport and Exercise Psychology



International Journal of Sport and Exercise Psychology

ISSN: (Print) (Online) Journal homepage: https://www.tandfonline.com/loi/rijs20

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To cite this article: Ray Bobrownicki, Howie J. Carson, Alan C. MacPherson & Dave Collins (2021): Unloading the dice: selection and design of comparison and control groups in controlled trials to enhance translational impact within motor learning and control research, International Journal of Sport and Exercise Psychology, DOI: 10.1080/1612197X.2021.1956567

To link to this article: https://doi.org/10.1080/1612197X.2021.1956567

INTERNATIONAL JOURNAL OF SPORT AND EXERCISE PSYCHOLOGY
https://doi.org/10.1080/1612197X.2021.1956567

Unloading the dice: selection and design of comparison and control groups in controlled trials to enhance translational impact within motor learning and control research

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ABSTRACT

In order to evaluate an experimental intervention, it should be contrasted against at least one relevant comparison group. Without meaningful and relevant comparisons, results can be difficult to interpret, effect sizes may be unduly minimised or exaggerated, and any resulting recommendations for practice could be called into question. Despite recognition of the importance of control groups in study design, however, there is currently limited guidance for sport-related research with regard to the selection and design of comparison groups. Furthermore, we have become increasingly concerned with the recurrent use of comparison groups, particularly in motor learning and control studies, that may initially appear well designed in experimental terms but ultimately possess limited relevance to—and in turn limited utility for informing—applied practice. To address these issues, we first set forth and discuss the primary types of control groups available for sport research, which include no-treatment, placebo or alternative-task, variable-delivery and active-treatment groups. We then present seven key principles to consider—upon identifying the appropriate type of control—in order to maximise internal validity, enhance interpretability and best inform real-world practice for sport psychology and motor learning and development. It is intended that the principles and recommendations detailed herein could support sport-related study design to the benefit of researchers and applied practitioners alike.

ARTICLE HISTORY
Received 13 November 2020
Accepted 18 June 2021

KEYWORDS
Applied practice; constraints-led approach; implicit motor learning; intervention; methodology

Introduction

Selecting control groups in sport science research constitutes a critical consideration for study design (Bobrownicki et al., 2018; Goginsky & Collins, 1996). In this regard, well-designed control groups assure the interpretability of findings (Lindquist et al., 2007),

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maximise study validity and credibility (Kinser & Robins, 2013) and promote translational impact (Collins et al., 2016). If control groups are inappropriately selected, this can influence effect sizes (Bobrownicki et al., 2015), result in the misinterpretation of results (Lin et al., 2012) and, in turn, lead to suboptimal recommendations for applied practice (Bobrownicki et al., 2019). Indeed, sport science-related research in imagery (Goginsky, 1992), priming (Winter & Collins, 2013) and motor learning (Bobrownicki et al., 2015) has demonstrated the impact of control group design on such outcomes. The selection of control groups is just as significant as the development of any intervention conditions (Kinser & Robins, 2013), so proper consideration must be invested in the development of these groups. Although researchers in other domains—such as depression treatment (e.g., Kinser & Robins, 2013), reconstructive surgery (e.g., Malay & Chung, 2012) and acupuncture (e.g., Lin et al., 2012)—have advised on the development and selection of reference groups due to this recognised importance, similar advice remains unavailable for researchers in sport-related research areas due, perhaps, to limited cross-disciplinarity. We contend that specific advice and examples for motor learning and control are important in helping to promote research that is not only well designed in experimental terms, but is also meaningful and informative for applied practitioners. As Ely et al. (2021) commented, "having the *intent* to impact applied practice in sport psychology is one thing, actually impacting applied practice is another" (p. 12) and there is a need to both improve translation from scholarship to practice, as well from practice to scholarship. Despite Christina (1987) stressing this cyclical link between fundamental and applied research in motor learning and control, it appears that the flow from fundamental *to* applied and applied *to* fundamental remains limited within the literature, in part, due to the design of control and comparison groups.

With the above in mind, the present paper aims to provide guidance when selecting and designing comparison and control groups for researchers in sport psychology and sport coaching, particularly with an interest in motor learning and control, in order to enhance translational impact and help to bridge the gap between practice and research. To do this, we first establish four types of comparison and control groups available to sport researchers, which are adapted from research and guidance from other fields (e.g., European Medicines Agency, 2001; Kinser & Robins, 2013; United States Food and Drug Administration, 1998). As part of this discussion, we define these groups, set forth advantages and disadvantages of these approaches and provide examples pertinent to motor learning and control where appropriate. Following this, we then present important principles to consider when designing these types of groups that address common and persistent issues observed in the motor learning and control literature and relate specifically to comparison and control group design, rather than wider methodological concerns that are relevant to experimental groups (e.g., random allocation of participants to control for gender, skill level and experience) and have been discussed elsewhere in the sport science literature (e.g., Collins & Carson, 2021; Goginsky & Collins, 1996; Swann et al., 2015).

Types of comparison and control groups

In the following sections we outline the available comparison and control groups within research, however, Table 1 (below) provides a summary of these approaches and notes implications for internal validity for ease of contrast.

Table 1. Descriptions and Characteristics of Various Comparison and Control Group Designs for Motor Learning and Control Research.

	No Treatment	Placebo/Alternative Task	Variable Delivery	Active Treatment
Summary	Participants are not exposed to treatments or traditional practices.	An inert, innocuous, inactive, or ineffective treatment possessing some face validity that serves as comparison to intervention. In placebo conditions, participants are blinded to the experimental manipulations. In sport, however, this might not always be possible, so tasks that evoke similar processes should be used.	Appropriate after initial efficacy is established, variable delivery manipulates elements of the intervention to investigate factors that impact its effectiveness.	Comparing experimental intervention to a known or usual treatment, strategy, or intervention that should typically represent best or current practice.
Strengths	Some potential to attribute observed outcomes to intervention.	Can control for expectancy, which affords better evaluation of experimental manipulation, and promote engagement across participants. Includes a treatment that could be more relevant. Does not remove the participant from their sport. This provides appropriate and robust comparison when there is not already an existing treatment or intervention.	Allows practitioners to understand what optimises or limits intervention or outcome effectiveness. Comparison tasks are more relevant to the participation activity or task.	Presents fewer ethical issues. Potentially promotes collaboration with coaches and other end users. Direct relevance to practice due to meaningful comparison, which makes impact easier to interpret and communicate for real-world sport.
Limitations	Potentially unrepresentative of real-world practice and real-world participation (e.g., a group of athletes reducing/removing typical training practices or activities). Withholding a treatment you expect to benefit athletes could raise ethical issues in applied settings. OR Withholding any treatment could be detrimental to real-world athletes.	Not appropriate where there are already existing treatments because those existing treatments might be ethically necessary and represent better bases for comparison.	Relies on rigorous and sufficient volume of previous data.	Relies on comparison groups that appropriately reflect real-world treatments or interventions, so unrepresentative comparisons that do not carry practical or theoretical relevance may misrepresent effects.
Impact on Internal Validity	Does not account for expectancy effects, so may obscure extent of intervention effectiveness. Can create artificial comparisons that inflate apparent intervention efficacy.	With placebo, it controls for expectancy and enhances internal validity. Comparisons might still be artificial, which can impact effect sizes and conclusions if there is an existing treatment that has not been compared or if physical, cognitive, and skill loadings are imbalanced.	Because participants in comparison groups are receiving the same treatments, expectancy is controlled and comparisons are meaningful.	With use of known and effective treatment, expectancy is controlled and comparisons should be meaningful.

No-treatment groups

In no-treatment control groups, participants receive no intervention (Malay & Chung, 2012) and are aware of their group assignment (European Medicines Agency, 2001). Because the control groups in these instances do not receive any treatment, it is possible to attribute observed outcomes to the intervention (Malay & Chung, 2012). No-treatment controls can be inappropriate, however, because they may be unrepresentative of real-world practice or it may be unethical to refuse interventions, treatment or care to participants (Houle, 2015). Moreover, no-treatment groupings do not account for expectancy effects—which are associated with improvements in sporting performance (e.g., Maganaris et al., 2000)—and, as such, their use may impact internal validity and obscure the extent of intervention effectiveness.

Placebo or alternative-task groups

To counter expectancy effects, researchers could employ placebo or alternative-task control. With placebo control groups, participants believe they have been randomly allocated to the treatment or intervention group, but they will, in fact, receive an inert, innocuous, inactive or ineffective treatment, therapy or intervention. Ideally, such control conditions will possess some face validity, such as the use of learning Japanese philosophy against an active condition of imagery use in martial arts (cf. Seabourne et al., 1985). Because the participants have been blinded, this approach should control for potential influences on the course of an intervention (e.g., expectancy, motivation, conditioning) except for those arising from the experimental manipulation (European Medicines Agency, 2001). In sporting contexts though, traditional placebo administration may not always be possible. Consequently, placebo groups may take the form of alternative tasks. For instance, in an investigation of verbal overshadowing in a golf-putting task, Flegal and Anderson (2008) asked participants in their control condition to perform a verbal-distractor task between the learning and testing phases, while those in the experimental condition were instructed to describe in writing how they executed the task. Such alternative-task groups can help to control for expectancy or other elements of the intervention that are not specific to the experimental condition, especially if they are designed to offer apparent face validity as an intervention in their own right. While useful in certain circumstances, however, placebos and alternative tasks are not typically advised where effective and established treatments already exist (Kinser & Robins, 2013), because existing interventions may not only be ethically necessary, but may also represent better bases for comparison.

Variable-delivery groups

Variable delivery, the third control group, is based on the dose–response concurrent group typically used in drug research (e.g., European Medicines Agency, 2001) where the experimental condition is compared to different courses of the same study treatment or intervention (e.g., effects of different regimens of an interval-training programme on cycling performance; Stepto et al., 1999). Variable-delivery controls are ordinarily most valuable after initial testing or investigation (Kinser & Robins, 2013) and enable critical

exploration of intervention effectiveness, while controlling for expectancy effects (see implications for internal validity in Table 1). Examples might include comparisons of proximal versus distal external instructions where researchers have manipulated the distance of participants' external (i.e., outside the body) attentional focus in a sport task (Bell & Hardy, 2009; Castaneda & Gray, 2007; Neumann et al., 2020), having initially compared internal and external instructions in previous studies. Further examples of variable delivery could include manipulating the amount of feedback provided during motor learning (see Schmidt, 1991). For myriad reasons, however, variable-delivery controls have seen limited deployment in some areas of sport science. For instance, Bobrownicki et al. (2018) noted that variable-delivery controls are currently needed to critically investigate the concept of analogy instruction, a manipulation which has often been treated as universally effective to date, despite indications that its impact may differ with respect to specific characteristics of those analogy instructions (e.g., number, length, complexity, or valence of instructions). Variable-delivery control groups can offer meaningful value to applied practitioners who must not only know what works, but also what is less effective (Collins et al., 2015).

Active-treatment groups

In active-treatment comparison and control groups, the experimental intervention is typically compared to a known or usual treatment, strategy or intervention (Houle, 2015) which should typically represent best or current practice. For instance, in a recent motor learning study, Meier et al. (2020) compared an individualised analogy intervention (i.e., visual representation of the movement based on previously acquired concepts) to an individualised explicit-instruction condition (i.e., mechanically specific instructions) that was designed to reflect traditional coaching practice and represent an active-treatment control. In using such an active-treatment group, the aim is to determine the efficacy of the intervention under investigation by demonstrating that it is either as good as or better than the known or usual treatment. Because there will often be existing treatments used by practitioners in sport, active-treatment groups typically represent the most appropriate controls or reference groups, as they present fewer ethical issues (Kinser & Robins, 2013) and offer more informative comparisons, enhancing insights for applied practice in the process. It is critical, however, that the comparison or control treatment carry both practical and theoretical equivalence to the intervention applied (see principles below; cf. Winter & Collins, 2013).

Important principles for the design of comparison and control groups

The capability for these types of comparison groups to deliver on their potential advantages will depend on the researchers' appropriate selection and design. For placebo/alternative-task, variable-delivery and active-treatment groups, researchers must limit differences between the experimental and control conditions to relevant theoretical and practical considerations of the respective interventions, while maintaining equivalence, where possible, for all other elements and characteristics between these groups (Lindquist et al., 2007). When designing these comparison and control groups, researchers should consider the following principles, which we argue have received limited

consideration, otherwise results may be difficult to interpret and translational impact may be reduced or compromised. Notably, these principles will have relevance across all of the comparison-group types to different extents, therefore, judgement will be required to consider how control and comparison groups can be designed in relation to these principles.

Representative of real-world practice

First, it is often important that these comparison groups represent real-world practice, particularly for active-treatment controls, and that these control groups also facilitate face validity. In this regard, the control groups should be designed to reflect common, accepted or historical interventions for the topic of interest and that these comparison interventions enable the study to test what it intends to measure. Although carefully designed and context-valid parameters should represent a requisite element of investigative methodologies (Collins et al., 2016), research in sport has not always applied such concepts across experimental and comparison group designs. Indeed, some studies are ostensibly well designed in experimental terms, but the resulting comparison groups possess limited relevance and meaning in practical terms. For instance, expert coaching often takes place in a focused, clear and progressive fashion (e.g., one or two instructions during a session; Schempp et al., 2004), but studies investigating implicit learning methods (e.g., Capio et al., 2020; Hardy et al., 1996; Masters, 1992) have typically compared these learning interventions to unrepresentative and overly explicit instruction conditions that—while representative of the types of instruction sometimes provided to athletes (Hodges & Franks, 2002)—feature long lists of rules that are unrepresentative of their real-world delivery, potentially distorting results and recommendations for application (Bobrownicki et al., 2018). Such is the prevalence of this misrepresentation that some have even referred to these practices as "traditional" pedagogies or coaching (Correia et al., 2019, p. 125) with limited evidence. Therefore, to ensure face validity and translational impact, active-treatment control groups must ensure that the control group design accounts for real-world usage according to the research aims and what, most ideally, would be commonly and currently considered as "good coaching" practice.

Timing

In addition to these concerns, research should also consider the correspondence of temporal factors (e.g., with relation to timing or frequency of intervention delivery) between experimental and control conditions. For instance, Winter and Collins (2013) noted that Ashford and Jackson (2010), while otherwise conducting a carefully designed study, inequitably compared a *pre*-performance priming intervention to an *in*-performance explicit focus condition when investigating the effectiveness of priming paradigms. Although the priming intervention appeared to benefit participants in Ashford and Jackson's study, based on comparison to the explicit-focus reference group, Winter and Collins (2013) argued that critical contextual differences with, for instance, timing (i.e., *before* versus *during* competition) prevented evaluation of priming as a pre-performance strategy. The importance of aligning contextual factors such as time was later empirically demonstrated when Winter and Collins (2013) found that the pre-performance priming

intervention used by Ashford and Jackson (2010) was actually less effective for motor performance than the PETTLEP motor imagery approach, an already established pre-performance technique. There will inevitably be instances where timing may differ for control groups, but these instances should have a clear rationale for any deviation (e.g., the timing is closely intertwined with the nature of the typical intervention strategy and relevant for comparison to the experimental condition). If timing represents a key consideration of the study (e.g., investigating how timing of intervention impacts its efficacy), then the use of variable-delivery control groups may be appropriate.

Cognitive loading and attentional focus

The aforementioned study by Winter and Collins (2013) not only highlighted the importance of carefully considering temporal factors with this design though, but their change in comparison group also addressed imbalances concerning cognitive load and attentional focus. In this regard, Ashford and Jackson's (2010) pre-performance priming strategy would have been more likely to promote *unconscious*, automatised motor performance than their in-performance explicit focus condition, which is typically associated with *conscious* control and impaired performance (Baumeister, 1984). Although differences like these can obscure findings and resulting recommendations for practice, differences in cognitive load between experimental and comparison groups represent a common and ongoing issue in sport psychology and motor learning literature (e.g., Capio et al., 2020; Hu & Xu, 2009; Koedijker et al., 2007, 2011; Lam et al., 2009a, 2009b; Law et al., 2003; Liao & Masters, 2001; Poolton et al., 2006, 2007; Schücker et al., 2013; Tse, Wong, et al., 2017). In parallel with cognate branches of psychology (e.g., educational psychology), where instructional designs have commonly eschewed established limitations of working memory (Sweller et al., 2019), scholars in motor learning and control research must better account for documented working memory constraints and carefully consider how cognitive load is operationalised. For instance, taking into account the number of instructions (i.e., the number of discrete rules), the volume of any instructions (i.e., the number of words or components), and the consumption of memory resources between experimental and comparison group designs might represent possible examples. For experimental design, Peck et al. (2012) suggested that researchers may go even further by considering participants' cognitive traits, cognitive states and experience/bias (e.g., Mahfoudh & Zoudji, 2021). Nevertheless, except in instances where the differences in cognitive load or attention specifically relate to the research aims (which might suit intra-intervention comparisons via variable delivery), researchers should carefully consider how control groups compare to the experimental condition in terms of cognitive demands and the formalised way in which these can be assessed within future research (e.g., using psychometric scales or psychophysiological markers; Antonenko et al., 2010).

Skill and physical loading

In addition to considering cognitive loading and effort, researchers in sport science should also consider differences in skill, physical loading and effort. Encouragingly, there are examples of researchers already accounting for skill level (e.g., Couvillion &

Fairbrother, 2018; Nasu et al., 2014; Savelsbergh et al., 2002), although researchers should continue to ensure appropriate steps are taken to maximise real-world relevance and limit possible emergence of confounding variables. For instance, when recruiting novices, it would be advisable to screen participants not only for skill and experience in the primary task, but also in similar sports, activities or skills that may transfer to that primary task (Bobrownicki et al., 2019). Notably, these transferred skills might not only pertain to outward technical skills (e.g., screening for throwing-related experiences in javelin or American soccer in a dart-throwing study), but also to relevant psychological skills, strategies or approaches that did not necessarily develop in a closely related task (e.g., novice participants with elite experience in seemingly unrelated sports may—even unknowingly—possess knowledge of, for example, pre-performance routines or quiet-eye techniques that might benefit study performance).

Researchers should also consider the physical loading of—and the physical effort required for—the task, which could vary, for example, with respect to expertise or specific qualities of the participants (e.g., body composition or anthropometrics), to ensure that experiences and expenditures are similar for comparison groups. Indeed, we have seen research in which the task arguably interacts with participant characteristics—such as expertise (e.g., the task interacts with a range of participant skill levels; Christina & Alpenfels, 2014), fitness (e.g., many learning trials over several days; Lam et al., 2009b) or other physical attributes (e.g., variability in skill components could be more or less demanding depending on physical and biomechanical differences between participants; Giblin et al., 2015)—to create the potential for differentially impacting study outcomes. In studies where the interventions involve physical activity or training, it would be prudent for researchers to ensure that comparison groups are also physically engaged, perhaps through an alternative-task or active-treatment control, rather than no-treatment groups that lack corresponding activity. In addressing concerns regarding corresponding skill and physical demands, researchers are afforded opportunities for more informative data and, in turn, may offer practitioners clearer recommendations for real-world practice.

Meaning and understanding of provided instruction

Another key principle relates to the correspondence of control-group instructional content to the experimental condition. In this regard, instructions should align, where appropriate, in meaning and understanding with the experimental condition. In motor learning or coaching contexts, this principle has been overlooked in a number of studies (e.g., Capio et al., 2020; Lam et al., 2009b; Liao & Masters, 2001; Poolton et al., 2006; Tse, Wong, et al., 2017) with instructions for experimental conditions focusing on specific movement mechanics *during* the skill (e.g., topspin forehand), while comparison groups have been provided with not only corresponding explicit instructions to perform during performance but also *before* and *after* (e.g., stance and footwork). In these instances, timing of the instruction delivery has been similar for all groups (i.e., prior to the start of data collection), but the content or meaning of this information in how it was to be used was clearly inherently different. Demonstrating the interdependent nature of these control-group principles, these differences in instructional content could also have impacted on cognitive loading relative to the experimental condition.

A key additional element of this principle is that researchers should check that participants have understood any instructions as intended, as such information can be interpreted in ways unanticipated by researchers. For example, a recent study involving dart throwing (Bobrownicki et al., 2019) found that the analogy-based instruction to "move your arm like a catapult" led some participants to move their arms consistent with the motion of a catapult device, while others mimicked the motion of the trebuchet device, a different mediaeval warfare weapon, resulting in unexpected differences in throwing accuracy and kinematics. In other words, the single analogy generated both pitching-like (as in baseball) and straight arm bowling-like (as in cricket) responses. Differences in interpretation and understanding suggest that it may be necessary to sample participant understanding to ensure consistency between experimental and control conditions otherwise results may be difficult to compare and interpret. This may be accomplished by probing understanding via the adoption of mixed methods approaches or verbal protocols, which have traditionally been used *after* data collection (e.g., Bobrownicki et al., 2015; Lam et al., 2009b; Masters, 1992), *during* the process of data collection (e.g., in between blocks of trials). This approach may also provide insight into the development of task and/or movement knowledge as well.

Epistemology

According to Loland (1992), in sport biomechanics and motor learning, acquisition of sporting technique is traditionally rooted in analytical approaches that depend on Newtonian mechanics, mathematical terminology and straightforward causal explanations. This mechanistic epistemology is today often demonstrated through dynamical systems or constraints-led approaches (CLA) to skill acquisition and motor control which emphasise the direct perception of environmental affordances (cf. the ecological psychology approach of Gibson, 1979). As Loland (1992) also noted, however, such mechanistic approaches that understate the role of cognition in the organisation of movement do not seem to explain, for instance, creative technical innovations in long-established sports, such as the Fosbury flop in high jump or Boklöv's V-style in ski jumping. Indeed, there are alternative epistemologies such as phenomenological or constructivist approaches that have greater appreciation or acknowledgement for the role of cognition and consciousness that may better explain the development of revolutionary techniques in decades-old sports (cf. Carson & Collins, 2011). At present, however, there is limited comparison of these different epistemologies, with studies in dynamical systems theory typically lacking comparison groups that permit any critical exploration. Positively, Gray (2018) did recently undertake a comparison between a CLA-based baseball-batting intervention that may offer a starting point in this regard (and also represents an example of an active-treatment comparison). Even in this study, however, the comparison groups featured fixed instructions that (1) were simplified compared to corresponding training methods in the literature that more readily incorporate internal focus and cognition through a more longitudinally complex series of stages (e.g., the Five-A Model; Carson & Collins, 2011) and (2) did not vary based on participant performance, unlike the CLA intervention condition. As Loland (1992) has argued, studies aimed at exploring the acquisition of sporting technique should consider their basic methodological and epistemological premises, aim to compare with relevant, alternative approaches

and be open to critique by both empirical and non-empirical arguments. The exploration of alternative epistemologies through well designed and representative control groups can afford new perspectives that enrich understanding and also further real-world application (Loland, 1992), which often relies on interdisciplinary, multifaceted approaches (Carson & Collins, 2019).

History or precedent

A final consideration when designing control groups relates to past, outdated or historical interventions or precedents. In many instances, there are control groups that would not make sense to include based on best practice but are necessary in order to facilitate comparison to previous work or highlight potential issues. For example, there is limited rationale for providing more than three movement instructions at a time due to limitations of working memory (see Cowan, 2001), but there are many coaching and motor learning studies that exceed this (e.g., Capio et al., 2020; Kim et al., 2020; Lam et al., 2009b; Schücker et al., 2010; Tse, Fong, et al., 2017). Because of this issue, future studies may need to temporarily follow these questionable precedents to enable comparison with past work. To illustrate this, in a study investigating analogy instruction, Bobrownicki et al. (2015) included not only an evidence-based explicit-instruction condition matched in word length to the analogy instructions, but also a "traditional" explicit condition that included the customary, yet ill-advised, eight explicit rules that reflected common convention in the literature (cf. Goginsky & Collins, 1996 for a quantitative evaluation of the impact of different control groups). Upon theoretical, methodological, or practical developments that indicate any historical comparison groups to be outdated, flawed, or ineffective, however, these traditional comparison groups should be discontinued rather than further reinforced (cf. Kim et al., 2020)

Conclusion

Data cannot be interpreted in isolation and typically require at least one comparable contrast group to assure meaning and interpretability (Campbell & Stanley, 1963). Indeed, control groups represent critical, but periodically underdeveloped, elements of study design. The aim of this paper was to define four main types of control groups for sport-related research and then offer key principles in designing these groups. In identifying these issues with examples from the literature, we have demonstrated that there is need for consideration of these principles when designing motor learning and control research. We intend that this paper will serve as a resource or basis for empirically investigating or even revisiting (where appropriate) concepts, theories or practices to ensure that they have been robustly tested and properly considered. Indeed, if research deviates from the principles outlined in this paper, then findings and resulting recommendations for practice may lack applied relevance and impact, thereby requiring additional scrutiny and cautious interpretation. It is our opinion that a number of areas in particular would benefit from greater adherence or adoption of the concepts within this paper, including but not limited to coach decision making (e.g., naturalistic versus classical decision-making approaches via active-treatment control groups), motor learning (e.g., CLA versus cognitive approaches using active treatment control groups) and coaching

instruction (e.g., critical comparisons of types of analogies via variable-delivery controls). With careful and systematic selection and composition of control groups as part of a robust study design process that also incorporates further recent and relevant advice (e.g., Collins & Carson, 2021; Ranganathan et al., 2021; Swann et al., 2015), sport science and sport psychology could be enhanced to better serve and inform practitioners, researchers, coaches and athletes alike, driving both more complete theory and effective practice.

Data availability statement

There is no data set associated with this manuscript.

Disclosure statement

No potential conflict of interest was reported by the author(s).

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