Advances in video game methods and reporting practices (but still room for improvement): A commentary on Strobach, Frensch, and Schubert (2012)

Walter R. Boot a,⁎, Daniel J. Simons b,c

a Department of Psychology, Florida State University, United States
b Psychology Department, University of Illinois at Urbana–Champaign, United States
c Beckman Institute for Advanced Science and Technology, University of Illinois at Urbana–Champaign, United States

A B S T R A C T

Strobach, Frensch, and Schubert (2012) presented evidence that action video game experience improves task-switching and reduces dual-task costs. Their design commendably adhered to many of the guidelines proposed by Boot, Blakely and Simons (2011) to overcome common method and interpretation problems in this literature. Adherence to these method guidelines is necessary in order to reduce the influence of demand characteristics, placebo effects, and underreporting that might otherwise produce false positive findings. In their paper, Strobach et al. (2012) appear to have misinterpreted some of these proposed guidelines, meaning that their methods did not eliminate possible sources of demand characteristics and differential placebo effects. At this important, early stage of video game research, reducing the likelihood of false positive findings is essential. In this commentary we clarify our methodological critiques and guidelines, identify ways in which this new study did and did not meet these guidelines, and discuss how these methodological issues should constrain the interpretation of the reported evidence.

⁎ Corresponding author at: Florida State University, Department of Psychology, 1107 W. Call Street, Tallahassee, FL 32306-4301, United States. Tel.: +1 850 645 8734; fax: +1 850 644 7739.
E-mail address: boot@psy.fsu.edu (W.R. Boot).

In their study linking action video game experience to improved dual-task performance and reduced task-switching costs, Strobach, Frensch, and Schubert (2012) adhere to many of the methodological guidelines we proposed in our recent critique of this literature (Boot, Blakely, & Simons, 2011). We commend the authors for explicitly describing how they recruited participants, identifying all of their outcome measures, and most importantly, confirming cross-sectional game effects with a training study. However, this promising new study does not adhere to all of the guidelines we suggested, likely due to some misinterpretation of our claims. In this brief commentary, we clarify our claims and explain why they matter when interpreting the results of Strobach et al.’s new study.

In our critique, we argued that differences in motivation could contribute to measured cognitive differences between expert and novice gamers: if participants know that they are being recruited because they are expert action gamers, they might be motivated to perform well on cognitive tasks. For their expert/novice study, Strobach et al. used an overt recruitment strategy in which one flyer sought participants with a high level of action video game experience and a different flyer sought participants with little game experience. The authors claim that their recruiting strategy addresses their concern because the use of separate flyers “potentially equalizes the general level of motivation to conduct the experimental series.” However, motivation to participate in a study differs from motivation to perform well on the cognitive tasks assessed in a study. Our discussion of motivation differences was a critique of exactly the sort of overt recruitment strategy Strobach et al. used. If participants know they were recruited because they are gamers, they may be more motivated to perform well on cognitive tasks that they perceive as related to their gaming skills. Only covert recruitment, in which participants have no idea they are part of a video game study, can eliminate the potential contamination from motivation differences and demand characteristics.

Our critique also raised the issue of differential placebo effects in training studies. Many transfer tasks require rapid judgments and decisions, just as action games demand rapid visual processing, quick action, and task-switching abilities. Fast-paced action games may be seen as more closely related to these cognitive transfer tasks than are puzzle games like Tetris® or The Sims™. If so, then participants trained with action games may expect to perform well on these transfer tasks. Strobach et al. argued that this sort of differential placebo effect could not explain their results because motivation measured during game training was equivalent. Equating the level of engagement during training is important and laudable, but it does not address our concern that subjects might be more motivated when performing a transfer task because of a perceived link between that task and their training game.

Strobach et al. note that studies finding no link between gaming and cognition provide a way to understand boundary conditions of gaming effects. We agree. In trying to account for the lack of video game effects in a study we and our colleagues conducted (Boot, Kramer, Simons, Fabiani, & Gratton, 2008), Strobach et al. speculate that the absence of game effects might have resulted either from...
fatigue during the cognitive assessment or interference between our many assessment tasks. Strobach et al. used just two transfer tasks and, commendably, they stated explicitly that those were the only tasks they used. As described in our critique, such explicit reporting of all outcome measures unfortunately is lacking in the literature (Boot et al., 2011). Some studies have tested the same participants with multiple outcome measures without explicitly stating the number and nature of other transfer tasks completed as part of that training experiment. In some cases, outcome measures have been spread across several papers without mention of the overlapping training groups or the use of other outcome measures. Consequently, many of the positive effects of video games on cognition reported in the literature might have used a substantially larger battery of outcome measures, just as we did. More broadly, if action game effects are fragile enough to be eliminated through simple task interference effects, their potential to improve performance meaningfully in everyday situations becomes suspect.

Finally, in our critique, we noted an anomaly in most published studies finding benefits of game training on cognition (see Ackerman, Kanfer, and Calderwood (2010) for a similar discussion): many studies finding a benefit of action video games relative to a control game condition (e.g., Tetris) show no test–retest improvement in that control condition. Strobach et al. similarly found no test–retest improvement in their non-action game groups on measures of dual-tasking and task-switching. Performance should improve when retaking a task, and the reason for this anomaly is unclear (see Shipstead, Redick, and Engle (2012) for additional recent discussion of the importance of test–retest effects in interpreting transfer data). The benefits of action games must be calculated by comparing the size of the improvement after action game training to test–retest effects of the control conditions. The lack of such test–retest improvements in a control condition raises the possibility that the advantage of action game training is due to an inadequate baseline rather than to a benefit of action games.

We would like to conclude by again commending Stobach et al. for proactively addressing many of these common methodological pitfalls in their study. We hope that their study, along with these clarifications, will lead to more definitive tests of the potential benefits of action games on cognition.

References


