



Video game experience and optimized executive control skills—On false positives and false negatives: Reply to Boot and Simons (2012)

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ABSTRACT

In this reply, we respond to methodological points raised by Boot and Simons (2012) to a paper on video game experience and optimization of executive control processes (Strobach, Frensch, & Schubert, 2012). In sum, we assume that differences in strategies to recruit expert game players and novices cannot explain performance differences in cognitive tasks between both groups of participants as a sole account. Further, in contrast to Tetris training, exclusive effects after training with an action game on complex task situations including two different tasks certainly do not result from differences in the levels of motivation between both training types during the transfer tests. Finally, a lack of retest effects from pre- to post-test during transfer may result from relatively short durations of these tests. We discuss these points in conjunction with a perspective for balancing false positive and false negative errors in training research.

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The present reply is a response to Boot and Simons (2012), who wrote a commentary on Strobach, Frensch, and Schubert (2012). Boot and Simons aimed at a clarification of their methodological guidelines for performing valid video game training and expert studies (Boot, Blakely, & Simons, 2011). In the present article, we first respond to four critical points that were raised by these authors about how their guidelines were realized and interpreted in the study by Strobach et al. Subsequently, we propose a further guideline aimed at ensuring the best possible efficiency of training studies without losing scientific validity by balancing the false positive and false negative errors in training research.

In their first point, Boot and Simons (2012) argue that differences in the overt strategy to recruit video game experts and novices could contribute to measured differences between both groups of participants performing cognitive tasks (Strobach et al., 2012, Experiment 1). That is, if “participants know that they are being recruited because they are expert action gamers, they might be motivated to perform well on cognitive tasks” (p. 2). Although we generally do not argue against this statement, we assume that differences in recruitment strategies cannot

explain expert/novice differences in task performance as a sole account. So far to our knowledge, there has been no empirical evidence for such a motivational effect in video game literature. We assume that for such an effect to occur, prior knowledge of a relation between gaming expertise and improved performance in cognitive tasks would be required (Shipstead, Redick, & Engle, 2012). In fact, there is no guarantee that experts in our study were aware of such knowledge about our research question and research aim, but is rather speculation.

Moreover, there is evidence in our data that even speaks against the assumption of a motivational effect of the applied recruitment strategy on cognitive performance. As we report (Strobach et al., 2012), there are differences between experts and novices in very specific conditions such as dual-task (in the dual-task test) or switching conditions (in the task-switching test). There are, however, no differences in the single-task conditions of both tests. This result pattern shows that motivational effects, if there were any, would not boost cognitive performance in general but would be beneficial only for specific processes that are particularly related to executive control in coordinating two different tasks. We consider such specific effect as implausible since a motivation effect should also speed up simple stimulus–response coupling processes (e.g., Mir et al., 2011) resulting in single-task differences between experts and novices; however, there is no evidence for such an effect in the present data. Further, in Experiment 2 of Strobach et al. (2012), we performed a training study in which we tested effects of action gaming (in the present case *Medal of Honor*), puzzle (i.e., Tetris) gaming or no practice from pre- to post-

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test. All participants in this experiment were recruited through an identical strategy and were randomly assigned to different training groups after this recruitment. In this way, we can exclude an effect of the recruitment strategy on performance after action game training in contrast to Tetris gaming or no practice. Compared to that optimal training approach, any expert-novice comparison can only be suggestive of gaming benefits even if optimal recruiting strategies had been applied with respect to the participants (e.g., recruiting without mentioning video games) and its potential findings must be interpreted with caution (Boot et al., 2011).

In a second point, Boot and Simons (2012) argue that our training with an action game may result in increased motivation when performing the transfer tests (i.e., the dual-task and task-switching test) in contrast to the level of motivation after playing Tetris (defined as “placebo effects”). In our view, the specific result pattern in the transfer tests renders the assumption implausible that different motivation or placebo effects after action video gaming and Tetris gaming are responsible for the observed group differences. Our data demonstrated exclusive effects of action game training on dual-task/switching situations, while there was no effect on single tasks. If there were placebo effects, then these potential effects should have had a very specific impact on performance during transfer; that is, on situations requiring the control of two tasks, but not on single-task situations. The specific effect pattern would indicate that motivation does not boost performance per se, but specifically cognitive control processes to coordinate two different tasks (Liepelt, Strobach, Frensch, & Schubert, 2011; Strobach, Frensch, Soutschek, & Schubert, *in press*). In fact, if there were differential placebo effects between action and Tetris gaming on transfer tasks, then one should also expect such an effect on single-task processing. This is because single tasks require rapid perception of stimuli, processing of stimulus–response information and the execution of responses. This is exactly what many action games require and should follow in single-task differences after action vs. Tetris gaming.

While these arguments object to a placebo effect explanation for the findings of Experiment 2 in Strobach et al. (2012), we have doubts about the validity of the placebo argument of Boot and Simons (2012) in general. In our view, it seems highly implausible that a placebo expectation effect can be the sole factor causing performance differences between two training groups in the current setting of cognitive tasks. If a researcher would accept such a placebo assumption, then, at the same time, he/ she should show that it is possible to induce strong performance differences in task switching and dual-task performance simply by making people believe that they have better task coordination skills than others. For illustration, let us imagine the following experiment: In this experiment, a researcher puts the idea into the minds of a group of participants that they are better in task switching and dual-task performance than another group and then measures their performance in task switching and dual-task situations after induction. To the best of our knowledge there are no studies providing evidence for the idea that one can induce task coordination performance differences between two groups of participants simply by persuading one of the two groups of their superior task coordination skills. Unless this has been shown, the claim by Boot and Simons (2012) must be regarded as a speculative explanation for the performance differences between two training groups such as those in Strobach et al. (2012). We would like to note that we do not exclude the assumption that awareness of expertise in a certain type of tasks may have an influence on how people apply their knowledge and skills in training and transfer tasks. This awareness may represent an additional factor contributing to the skilled practice effects in general, but belief alone (instead of hard training) is unlikely to work.

In a third point, Boot and Simons (2012) argued that effects of improved performance in transfer tests from pre- to post-test in the action game group ought to be contrasted to such effects in a Tetris and/ or no-practice groups. The authors argue that mostly those research

groups found effects of action video games on cognition that did not find any re-test effects in the control groups. In our study there were re-test effects in the transfer tasks. The fact that these re-test effects were rather small (and mostly non-significant) is probably due to the short duration of the applied transfer tests. This short duration was specially selected in order to minimize possible learning effects between pre- and post-test sessions that were not caused by training but by task repetition. Consistent with this expectation, a number of other studies also showed no significant re-test effects in relatively short transfer tests (e.g., Basak, Boot, Voss, & Kramer, 2008; Mahncke et al., 2006).

As a fourth point, Boot and Simons (2012) claim that studies on practice effects, including those on the effects of video game practice, ought to report the findings of all transfer tasks they had administered during pre- and post-test situations. We believe that this point does not relate to the study by Strobach et al. (2012) in reporting the findings of administered task-switching and dual-task situations. In fact, we selected only two transfer tests to generate optimal conditions for the occurrence of transfer effects on task coordination skills; note that this number of tests is close to the recommended number of only one test per training and transfer domain, which, according to Boot et al. (2011), allows minimizing the possibility of aftereffects that may occur when participants perform a whole battery of transfer tests. It is important to control for such potential aftereffects because they may counteract the occurrence of positive transfer effects in large scale training studies (see for discussion Salminen, Strobach, & Schubert, 2012; Schmeichel, 2007). Video game training studies that included 12 or even more transfer tests and that found only minimal or even no transfer effects (e.g., Boot, Kramer, Simons, Fabiani, & Gratton, 2008) are consistent with this assumption. In sum, we think that we carefully selected plausible methodological procedures to investigate the relationship between video game experience and performance when controlling two different tasks systematically.

Research on video gaming effects on cognition is a highly promising area of research and a number of studies and findings suggest the high potential of video gaming for the plasticity of cognition (e.g., Green & Bavelier, 2003). In this sense, the guidelines proposed by Boot et al. (2011) are important because they are directed at constraining the probability of false positives. Therefore, these guidelines are aimed at keeping our knowledge clear of erroneous assumptions about positive training effects, when there is in fact no reliable effect but simply an artifact. While this is a commendable endeavor, we ought to protect the significance of our findings and efforts from the other side as well; that is, as researchers, we ought to take care to create those experimental conditions that are most appropriate to reveal improvements after training, even if these are rather small and difficult to detect. That is, we should try to keep the possibility of false negatives at an acceptable level, while equally controlling false positives and find an acceptable balance between minimizing false positive and false negative errors. Otherwise, researchers may invest a lot of research efforts and end up with negative conclusions regarding training effects on the grounds that actual differences between conditions were not recognized due to the strength of methodological constraints.

In the current early “developmental” stage of the research on video gaming effects, we are facing a period of heterogeneous research reports in which positive findings about the effects of video gaming are questioned by negative effects and researchers are searching for appropriate methodological standards. At the present, it seems difficult to make a conclusive claim about the true potential of action video gaming on certain cognitive functions. In such a situation it is necessary to consider an important rule for experimentation: We ought to find a balance between the minimization of possible confounding effects and the maximization of potential training effects. The latter means to provide experimental conditions that are sufficient and most suitable to provide an experimental effect of training. Consequently, when minimizing possible confounding effects we should not create experimental conditions that may constrain rather than maximize the

possible manifestations of experimental effects (Huber, 2009; Sarris, 1992).

Important factors, which may constrain (in addition to those already discussed) the manifestation of positive training effects on cognition are the amount of training which might not be sufficient in some studies not reporting reliable training effects and a sufficient amount of commitment to training (Klingberg, 2010). Especially with respect to the latter factor, research in educational psychology has shown that an important pre-requisite for successful knowledge acquisition is the appropriate motivational state of the scholars (Fredricks, Blumenfeld, & Paris, 2004). In the case of complex and highly demanding training situations, motivational factors may be decisive for the efficiency of the training and its appropriate level may differ between different situations and personal prerequisites such as genetic predispositions (Colzato, van Muizen, Band, & Hommel, 2011). In turn, motivational effects can also be a desired outcome of training to increase its efficiency in boosting cognitive improvements (in this case, theoretical conclusions about pure cognitive training effects are however limited). Given the occasionally negative reports of training effects in large scale training studies with loosely controlled training commitment and the apparent positive effects in other highly controlled training studies, we believe that an adjustment of the appropriate motivation level should be considered as a factor for proper balancing between the avoidance of false positive and false negative errors in training research.

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