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Video Game Training Does Not Enhance Cognitive Ability: A Comprehensive Meta-Analytic Investigation

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As a result of considerable potential scientific and societal implications, the possibility of enhancing cognitive ability by training has been one of the most influential topics of cognitive psychology in the last two decades. However, substantial research into the psychology of expertise and a recent series of meta-analytic reviews have suggested that various types of cognitive training (e.g., working memory training) benefit performance only in the trained tasks. The lack of skill generalization from one domain to different ones—that is, far transfer—has been documented in various fields of research such as working memory training, music, brain training, and chess. Video game training is another activity that has been claimed by many researchers to foster a broad range of cognitive abilities such as visual processing, attention, spatial ability, and cognitive control. We tested these claims with three random-effects meta-analytic models. The first meta-analysis ($k = 310$) examined the correlation between video game skill and cognitive ability. The second meta-analysis ($k = 315$) dealt with the differences between video game players and nonplayers in cognitive ability. The third meta-analysis ($k = 359$) investigated the effects of video game training on participants' cognitive ability. Small or null overall effect sizes were found in all three models. These outcomes show that overall cognitive ability and video game skill are only weakly related. Importantly, we found no evidence of a causal relationship between playing video games and enhanced cognitive ability. Video game training thus represents no exception to the general difficulty of obtaining far transfer.

Public Significance Statement
This meta-analytic investigation indicates that playing video games has negligible effects on cognitive ability, and adds further evidence against the alleged broad benefits of cognitive training.

Keywords: cognitive ability, meta-analysis, training, transfer, video game

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Is it possible to train cognitive ability? If so, do the benefits generalize to a broad range of different skills? Alternatively, does cognitive training have an impact limited to the trained tasks? The answers to these questions are crucial to understanding how humans acquire and use knowledge. In addition, whether and to what extent cognitive ability is malleable has huge societal implications. Consider the academic advantages of fostering cognitive ability in youth or the benefits—for the global economy and public health—or slowing down cognitive decline in adulthood.

Given these considerable potential implications, many studies have investigated the effects of several types of cognitive training in the last two decades. The research has provided mixed results, and no agreement among researchers in the field has been reached. A striking example of this divergence of opinions is offered by two open letters about the putative benefits of commercial brain-training programs. The first letter, issued by the Stanford Center on Longevity and the Max Planck Institute for Human Development and signed by 75 neuroscientists and psychologists, expressed serious doubts about the ability of brain games to improve cognitive ability ("A Consensus on the Brain Training Industry from the Scientific Community," 2014). The second one, posted on the Cognitive Training Data website (www.cognitivetrainingdata.org) and signed by a group of 133 researchers, claimed that certain cognitive-training programs can benefit cognitive function.

The Curse of Specificity: The Difficulty of Far Transfer

The question of the alleged benefits of cognitive training is strictly linked to the issue of transfer of learning. Transfer of learning occurs when a set of skills acquired in one domain generalizes to other domains (e.g., Barnett & Ceci, 2002). It is

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customary to distinguish *near transfer*—that is, the transfer taking place across two domains tightly related to each other—and *far transfer*, where the source domain and the target domain are only loosely related. In a seminal article, Thorndike and Woodworth (1901) proposed that transfer of learning is a function of the extent to which two domains share common features. Thorndike and Woodworth's (1901) "common elements" hypothesis thus predicts that, although near transfer is fairly common, far transfer is infrequent at best. As a direct consequence, the effects of cognitive training are expected to be limited to the trained task and other similar tasks.

Thorndike and Woodworth's (1901) common elements theory has received robust corroboration from research on the psychology of expertise. For example, the research on expert chess players has shown that expert performance relies, to a large extent, on domain-specific perceptual information—such as chunks, that is, perceptual and meaningful configurations of elements—acquired in years of training (Chase & Simon, 1973; Gobet & Simon, 1996; Sala & Gobet, 2017a). Beyond chess, perceptual information has been found to play an essential role in the acquisition of expertise in a wide range of fields, such as music (Knecht, 2003; Sloboda, 1976), programming (Adelson, 1981; Guerin & Matthews, 1990), and sports (Allard, Graham, & Paarsalu, 1980; Allard & Starkes, 1980; Abernethy, Neal, & Konig, 1994; Williams, Davids, Burwitz, & Williams, 1993). As predicted by chunking theory (Chase & Simon, 1973) and template theory (Gobet & Simon, 1996), perceptual information is scarcely transferable to other fields, or even across subspecialties in the same fields (e.g., Bilalić, McLeod, & Gobet, 2009; Rikers, Schmidt, & Boshuizen, 2002), because of its high specificity (Ericsson & Charness, 1994; Gobet, 2016).

Training Domain-General Cognitive Abilities: The Cases of Working Memory Training, Chess, and Music

Although domain-specific training rarely transfers across domains, some researchers have argued that training domain-general cognitive abilities—rather than domain-specific skills—can positively affect performance in a wide variety of fields that rely on those cognitive abilities. One theoretical foundation of this hypothesis is neural plasticity, that is, the ability of the neural system to adapt and modify under the pressure of the environment (Strobach & Karbach, 2016). Cognitive training is thought to lead to changes in the neural system, which, in turn, are supposed to account for the improvements on cognitive tests (Johnson, Munakata, & Gilmore, 2002; Karbach & Schubert, 2013). Another element in favor of the putative broad effects of cognitive training is that domain-general cognitive abilities correlate with performance in a wide variety of domain-specific skills. For example, fluid intelligence predicts academic achievement (Deary, Strand, Smith, & Fernandes, 2007; Rohde & Thompson, 2007) and general intelligence is positively associated with job proficiency (Hunter & Hunter, 1984; Hunter, Schmidt, & Le, 2006). Thus, it is plausible to suggest that fostering overall cognitive ability by training affects people's academic and professional lives positively.

According to Taatgen (2016), there are two ways to train domain-general cognitive abilities: (a) deliberately training the particular skill(s) by practicing cognitive tasks (e.g., *n*-back in working memory training), or (b) engaging in cognitively demanding activities (e.g., playing chess to train spatial working memory and planning). Whereas in the former case the improvement of general cognitive abilities is a direct consequence of training these abilities, in the latter case it is the by-product of learning domain-specific skills. Either way, the enhancement of domain-general cognitive abilities is supposed to improve one's performance in activities requiring these cognitive abilities.

Both methods have been extensively investigated. For example, in a seminal study by Chase and Ericsson (1982), a student expanded his digit span from 7 digits to 82 digits over 44 weeks of training. However, his ability to remember a large number of items was limited to digits and, for example, did not transfer to memory for consonants. This outcome highlights that human cognition is highly malleable to training but also that the benefits of learning are domain-specific.

A more recent example of the difficulty of generalizing a cognitive ability is offered by research into working memory (WM). A classical result in cognitive psychology is that WM capacity strongly correlates with fluid intelligence (Kane, Hambrick, & Conway, 2005). Searching for a possible causal relationship, Jaeggi, Buschkuehl, Jonides, and Perrig (2008) tested the effects of WM training on a test of fluid intelligence (Raven's Progressive Matrices) in a sample of healthy adults. The treated participants showed a significant improvement compared with the control group. Following this experiment, the research has been extended to the effects of WM training on other cognitive abilities (e.g., cognitive control and spatial cognition) and academic achievement (e.g., mathematics, literacy). Despite the initial promising results, a series of meta-analyses have provided strong evidence against the hypothesis that WM training enhances fluid intelligence, overall cognitive ability, or academic achievement (Dougherty, Hamovitz, & Tidwell, 2016; Melby-Lervåg, Redick, & Hulme, 2016; Sala & Gobet, 2017b). Interestingly, these meta-analyses found that when the treated groups were compared with active control groups, the overall effect sizes were essentially null. The only exception to this pattern of results was the robust effect of the training on other measures of WM capacity such as span tasks (i.e., near transfer). These outcomes suggest that although WM training is effective at improving performance in similar tasks, the far-transfer effects of this type of training are limited to placebo effects. Thus, although WM capacity and fluid intelligence are strongly correlated, training a domain-general cognitive ability such as WM capacity seems to provide no genuine benefits to one's fluid intelligence or any of the skills correlated to fluid intelligence (e.g., academic achievement).

When the focus shifts to the potential far-transfer effects of engaging in cognitively demanding activities, the story remains essentially unaltered. For example, the research on chess players has shown that chess skill correlates with fluid intelligence and other cognitive abilities such as WM, short-term memory (STM), and processing speed (Burgoyne et al., 2016). Moreover, chess players appear to possess a superior overall cognitive ability when compared with the general population of nonchess players, even when the level of education is controlled for (Sala et al., 2017). Although a recent meta-analysis (Sala & Gobet, 2016) has shown that chess training exerts a small effect on academic achievement and cognitive ability, almost all the studies in the field tested the alleged benefits of chess training using a passive control group only (Sala, Foley, & Gobet, 2017). The absence of an active control group suggests that the moderate effects of chess training are mostly attributable to nonspecific elements such as placebo effects.

This pattern is even more evident in the field of music. In a study by Ruthsatz, Detterman, Griscom, and Cirullo (2008), a group of expert musicians outperformed the novices in the Raven's Progressive Matrices. Similarly, Lee, Lu, and Ko (2007) found positive correlations between music skill and WM, while Schellenberg (2006) reported positive correlations between engagement in music and IQ in children and undergraduates. However, there does not seem to be any causal relationship between engagement in music activities and superior cognitive ability. Using a cotwin control design, Mosing, Madison, Pedersen, and Ullén (2016) reported that the members of the twin pairs that were music trained did not have a higher IQ than the relative cotwins not trained in music. In the same vein, a recent meta-analysis (Sala & Gobet, 2017c) found no evidence of positive effects of music training on a broad range of cognitive abilities (e.g., intelligence, spatial ability, and phonological processing) or academic attainment (e.g., mathematics and literacy). Crucially, the size of the effects was moderated by the type of control group. Just like WM training, when the music-treated samples were compared with active control groups, the differences were minimal or null.

Finally, WM training, chess, and music are not the only instances of failed far transfer. For example, teaching the computer language LOGO to improve pupils' thinking skills has produced unsatisfactory results (De Corte & Verschaffel, 1986; Gurtner, Gex, Gobet, Núñez, & Restchitzki, 1990). Research on spatial training points to the same conclusion. It is known that spatial ability is highly malleable to spatial training, as shown by Uttal et al.'s (2013) meta-analysis. Thus, considering that spatial ability is strongly associated with mathematical ability (Wai, Lubinski, & Benbow, 2009), it is reasonable to expect that spatial training fosters mathematical ability. Regrettably, the efforts to generate such a far-transfer effect have been unsuccessful so far (e.g., Xu & LeFevre, 2016). However, given the small number of experimental studies in the field, caution is recommendable. Finally, a systematic review by Simons et al. (2016) has claimed that there is no convincing evidence that brain-training programs provide benefits that go beyond the trained skill or task. The key point risen by Simons and colleagues is that there is an inverse relationship between the size of the positive effects of the treatments and the design quality of the experiments. This finding thus appears to generalize across several domains of cognitive training (e.g., WM, chess, music, and brain-training).

The Case of Video Game Training

As just seen, recent experimental evidence and meta-analytic reviews have highlighted the limitations, rather than the benefits, of many different types of cognitive training. Cognitive-training regimens seem to affect only the trained skills, whereas no effect is exerted on nontrained tasks. This applies to both those activities that specifically train cognitive abilities (e.g., *n*-back tasks in WM training, spatial training, and brain-training programs) and cognitively demanding activities such as chess and music. The converging evidence provided by the research into expertise acquisition and cognitive training strongly suggests that the occurrence of far transfer is rare at best.

Video game training offers another potential avenue for cognitive enhancement. Unlike chess and music training, where the number of studies is limited, video game training has been extensively studied for the last 20 years. The deep interest of scientists and policymakers for this activity has made the research on video games one of the most important domains in which to test the occurrence of far transfer. Action video game players have been found to outperform nonplayers in a variety of attentional and perceptual tasks (Green, Li, & Bavelier, 2010). Crucially, several experimental studies (e.g., Bejjanki et al., 2014; Green & Bavelier, 2003) have provided some evidence of a causal relationship between action video game training and improvement in cognitive ability. Notably, even the US Navy has been attracted by these promising results (Hsu, 2010).

The most influential explanation proposed to account for those positive results is the "learning to learn" theory (Bavelier, Green, Pouget, & Schrater, 2012). According to this theory, experience with action video games leads to an improvement in probabilistic inference. It is argued that the tasks that are used to compare the performance of video game players and nonvideo game players all require computing the probability of a choice being true given the available information. In other words, video game players are better at using such information, and this improved computational ability leads to better performance across tasks.

Finally, nonaction video game training seems to offer some benefits as well. For example, Okagaki and Frensch (1994) reported that a 6-hr training of the game Tetris improved the spatial abilities in a group of older adolescents. Similar results were obtained in Goldstein et al. (1997). Finally, Basak, Boot, Voss, and Kramer (2008) investigated the effects of playing a real-time strategy game (Rise of Nations) on a group of older adults. The participants showed some improvement in mental rotation, task switching, and working memory.

Playing video games also seems associated with neural changes (functional and anatomical). For example, enhanced attentional control attributable to video gaming is consistent with several fMRI studies revealing that video game players have superior functional integration between working memory and attention networks involving frontoparietal areas (Gong et al., 2016), as well as enhanced white matter connectivity from the visual area to frontal cortex (Kim et al., 2015). Wu et al. (2012) trained nonvideo game players with an action video game (Medal of Honor) for 10 hours and measured event-related potentials. After video game training, high-performing players showed larger amplitudes of P3 waves, which have been implicated in top-down control of attention.

The research in the field has, however, failed to consistently replicate the above mentioned positive results that show significant improvements in cognitive tasks following video game training (e.g., Basak et al., 2008; Bejjanki et al., 2014; Goldstein et al., 1997; Green et al., 2010; Green & Bavelier, 2003; Okagaki & Frensch, 1994). Terlecki, Newcombe, and Little (2008) found no difference in mental rotation ability between the training group (playing Tetris) and the control group. Similarly, Minear et al.'s (2016) study of real-time strategy video game provided no evidence of training effects on several measures of WM, STM, spatial ability, and fluid intelligence. Boot, Kramer, Simons, Fabiani, and Gratton (2008) questioned the effectiveness of action video game training at enhancing a broad set of cognitive abilities (e.g., enumeration, span, and *n*-back tasks). Finally, Oei and Patterson (2013, 2014, 2015) have challenged the "learning to learn" theory claiming that action video game training fosters, at best, those cognitive abilities necessary to play a particular video game. To test this hypothesis, Oei and Patterson (2015) used, as training tasks, four different action video games, differing from each other with regard to their cognitive demands (e.g., different speed and levels of selective attention). In line with Thorndike and Woodworth's (1901) common elements theory, participants' improvements were restricted to the cognitive abilities targeted by the video game they played.

Other researchers have also raised doubts about the alleged superior cognitive ability of video game players over nonplayers. For example, in Gobet et al. (2014), the group of action video game players failed to outperform the nonvideo game players in a flanker task and a change detection task. Similarly, Murphy and Spencer (2009) found no difference between a group of action video game players and a group of nonplayers in a set of visual-attention tasks. Comparable outcomes were obtained by Castel, Pratt, and Drummond (2005) and Irons, Remington, and McLean (2011).

A further source of skepticism about the relationship between video game playing and superior cognitive ability comes from several correlational studies. If video game training is effective, more skilled and experienced video game players should show superior cognitive ability compared with novice video game players. However, Hambrick, Oswald, Darowski, Rench, and Brou (2010) reported near-zero correlations between the participants' video game experience and several measures of processing speed, WM capacity, and fluid reasoning. Hambrick et al.'s (2010) results were replicated by Unsworth et al. (2015). Notably, the implications of Unsworth et al.'s findings for the field of video game training have recently been the subject of a lively debate among researchers (Green et al., 2017; Redick, Unsworth, Kane, & Hambrick, 2017).

The Meta-Analytic Evidence

The research about video game and cognitive ability has provided mixed results in both experimental, quasi-experimental (i.e., comparison between players and nonplayers), and correlational studies. To disentangle these discrepancies, Powers, Brooks, Aldrich, Palladino, and Alfieri (2013) ran two meta-analyses collecting the available evidence about the effects of playing video games on cognitive ability. The first meta-analysis, comparing players with nonplayers, reported medium to large effect sizes showing that video-game players were superior to nonplayers in measures of visual processing, executive functioning, and spatial imagery, among others. The second meta-analysis, focusing on true experiments, found positive, yet slightly smaller, effects of video game training on the same measures. Overall, the results suggested optimism about the ability of video game training to enhance a broad range of cognitive abilities.

However, several serious methodological flaws make Powers et al.'s (2013) findings unreliable. To begin with, the inclusion criteria appear too loose, especially because of the inclusion of training studies without a control group controlling for testing effects, studies mixing video game experience with general computer use (e.g., Li & Atkins, 2004), and studies dealing with the effects of exergaming (i.e., games for physical training; e.g., Maillot, Perrot, & Hartley, 2012; Staiano, Abraham, & Calvert, 2012). Another problem is that Powers et al.'s (2013) meta-analysis included only one publication bias analysis able to provide corrected estimates (i.e., trim-and-fill; Duval & Tweedie, 2000). The use of multiple methods for the detection of publication bias is fundamental for triangulating the true effect size estimate (Kepes, Bushman, & Anderson, 2017; Kepes & McDaniel, 2015).

Most importantly, in the two meta-analytic models (and hence in the submodels), too many of the effect sizes (up to 28) were extracted from the same samples and were often referring to the same cognitive construct, without any correction for statistical dependence. Even if the violation of the assumption of statistical independence does not necessarily cause a systematic bias in the estimation of overall meta-analytic means (i.e., $\bar{r}$ and $\bar{d}$; Schmidt & Hunter, 2015), the features of a particular meta-analytic model may lead to an accidental inflation (or reduction) of the overall means. Moreover, the violation of the assumption of statistical independence is associated with an underestimation of sampling error inflating the variability between studies (Schmidt & Hunter, 2015), with possible consequent biases in moderator analysis (e.g., Type I error).

More recently, three other smaller meta-analytic investigations were carried out. Powers and Brooks (2014) reanalyzed their previous findings by using a more fine-grained categorization to examine the impact of particular video-game genres on specific cognitive skills. Toril, Reales, and Ballesteros (2014) reviewed 20 studies regarding the effects of video game training on older adults' overall cognitive ability and reported a positive overall effect size ($\bar{g} = 0.37$). Finally, Wang et al. (2016) meta-analyzed 19 studies and found a positive effect of action video game training on healthy adults' cognitive ability ($\bar{g} = 0.57$).

Like Powers et al. (2013), these three meta-analyses suffer from severe methodological flaws. Toril et al.'s (2014) meta-analysis included several exergaming and brain-training studies (e.g., Ackerman, Kanfer, & Calderwood, 2010; Anguera et al., 2013; Maillot et al., 2012). When these exergaming and brain-training studies are excluded, the number of the studies is reduced to 12. Moreover, no quantitative estimation of publication-bias was calculated. Finally, no appropriate correction for statistically dependent effect sizes was applied. The dependent effect sizes were simply merged into a single effect size irrespective to whether they measure the same cognitive ability or not. The very same considerations apply to Wang et al.'s (2016) meta-analysis. Finally, although Powers and Brooks's (2014) investigation was an interesting attempt to understand the impact of different types of video game training in more detail, there were not enough studies to produce reliable models. In fact—action video games excluded—the other six categories of video-game genre included only three to nine studies.

The Present Meta-Analytic Investigation

The field of video game training might be a significant exception to Thorndike and Woodworth's (1901) common elements hypothesis. The potential theoretical and practical implications of such an anomaly would be huge. It is thus imperative to test—comprehensively and with rigorous statistical methods—the claim that video game training produces far-transfer effects.

We thus ran three meta-analytic models. The first meta-analysis assessed the correlation between video-game skill and cognitive ability in populations of video game players. To the best of our knowledge, no such meta-analysis had been carried out before. The second meta-analysis tested whether the population of video game players significantly differed from the population of non-video game players in terms of cognitive ability. Thus, the difference between the first and the second meta-analysis is that the first deals with correlations within the population of video game players while the second compares two different populations: video game players and nonplayers. The third meta-analysis examined the effects of video game training on cognitive ability.

In line with Unsworth et al.'s study (2015) and the debate surrounding it, the first two meta-analyses represent an important test for the hypothesis according to which video game training exerts a positive influence on cognitive ability, although the correlational nature of the data limits the conclusions that may be drawn. If video game experience/skill is not correlated with cognitive ability, or video game players are not better than nonvideo game players, then it is hard to claim that video game training enhances cognitive ability. However, positive correlations and between-groups differences would not necessarily imply that video game training causes cognitive enhancement. For example, a possible alternative explanation is that individuals with superior cognitive ability are more likely to engage and excel in video games.

In addition, it must be noted that, just like correlation and between-groups differences do not imply a causal relationship between two variables, the absence of correlation or between-groups differences does not necessarily imply that there is no causation. For example, if there is a nonmonotonic curvilinear relationship (e.g., U-patterns) between two variables, then there is no linear correlation. Also, while providing overall null results, a correlational analysis may fail to detect actual between-groups differences moderated by specific covariates (i.e., Simpson's paradox); for example, in our case, video-game training might have a positive effect on cognition with beginners, but a negative effect with advanced players. However, it seems improbable that either or both of these conditions occur in the case of video game training. In fact, assuming that video game training enhances cognitive function, the prolonged exposure to video games reported in natural groups should result in clear-cut differences between players and nonplayers and positive correlations between video-game skill and cognitive skills.

Thus, even though we cannot logically infer the direction or even the presence of causation, the information provided by correlational (Meta-analysis 1) and cross-sectional (Meta-analysis 2) studies still represents suggestive evidence in support or against claims about the benefits of video game training. Furthermore, these two meta-analyses shed light on the potential cognitive correlates of video game expertise.

General Method

Literature Search

We used a systematic search strategy to find the relevant studies (PRISMA statement; Moher, Liberati, Tetzlaff, & Altman, 2009). ERIC, PsycINFO, MEDLINE, JSTOR, Science Direct, and ProQuest Dissertation & Theses databases were searched to identify all the potentially relevant studies, using the following combination of keywords: ("video gam*" OR videogame) AND (intelligen* OR IQ OR "executive function*" OR percept* OR cognit* OR attention* OR visual* OR vision OR inhibition OR memory OR motor OR "dual task" OR "switching task" OR flanker OR "object tracking" OR spatial). Also, we examined previous reviews, and we e-mailed researchers in the field ($n = 135$) asking them for inaccessible data.

Inclusion Criteria

The studies were included in accordance with the following four general criteria:

1. The variable of interest (e.g., video game experience, skill, and training) was successfully isolated. For example, we excluded studies reporting correlations and comparisons between treated and nontreated groups regarding the general use of digital media. Similarly, we excluded studies combining video game playing with physical training (i.e., exergames).

2. During the study, at least one measure of domain-general cognitive ability nonrelated to video gaming was collected.

3. The participants of the study suffered from no specific learning disability (e.g., developmental dyslexia), behavioral disorder (e.g., aggressive behavior), or clinical condition (e.g., video game addiction, amblyopia).

4. The data presented in the study, or provided by the authors, were sufficient to calculate an effect size.

The additional criteria for each of the three meta-analyses are reported in the three relevant Method sections.

To identify studies meeting these criteria, we searched for relevant published and unpublished articles until December 31st, 2016, and scanned reference lists. Forty-two authors replied to our e-mails. Twenty-five provided unpublished data.

We found 66 studies reporting correlations between cognitive ability and video game skill, including 8,141 participants and 310 effect sizes. We found 98 studies reporting comparisons (i.e., quasi-experimental design) between players and nonplayers, including 6,166 participants and 315 effect sizes. Finally, we found 63 studies regarding the effects of video game training on cognitive ability, including 3,286 participants and 359 effect sizes. The procedure is summarized in Figure 1.

The details regarding the effect sizes, sample sizes, and moderators of the three meta-analyses are summarized in Tables SE1, SE2, and SE3 in the supplemental material available online.

Outcome Measures

We categorized the effect sizes into five broad measures: (a) *Visual attention/processing*, including all those tests measuring visual-perception skills (e.g., visual search tasks, flanker task, useful field of view [UFOV] tasks, and change detection tasks); (b) *Spatial ability*, including tests such as mental rotation and folding tasks; (c) *Cognitive control*, including tests such as task switching,

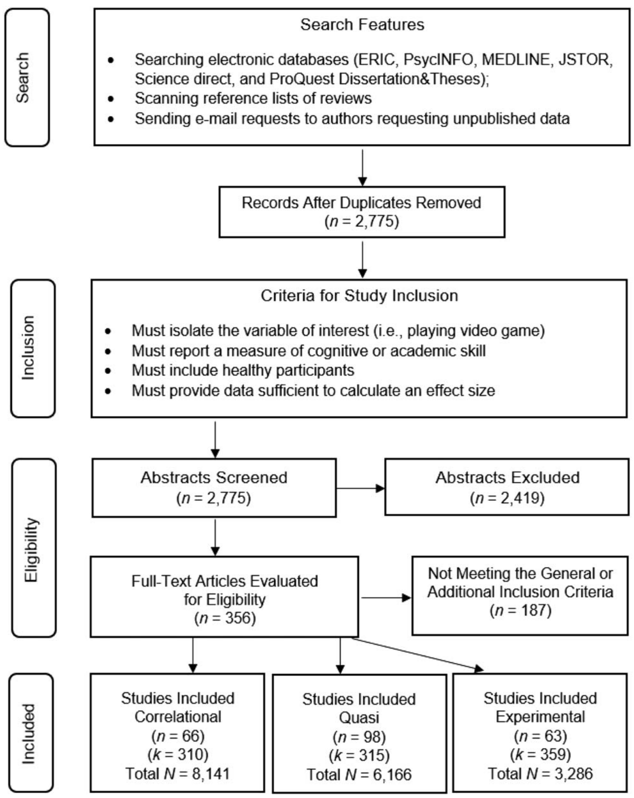

Figure 1. Flow diagram of the studies included in the meta-analyses.

go/no-go, Simon, and Stroop tasks; (d) *Memory*, including tests such as span, *n*-back, and recall tasks; and (e) *Intelligence/reasoning*, including tests of fluid intelligence/reasoning (e.g., Raven's matrices) and comprehension knowledge (e.g., verbal fluency). Table SE4 (supplemental material) includes a summary of all the tasks used in the reviewed studies sorted by outcome measure.

This categorization was used as the main moderator and named outcome measure in all the three meta-analyses. When analyzing the other categorical moderators, we sorted the effect sizes by outcome measure, and calculated the relative overall meta-analytic means.

The first and second authors coded each effect size independently. The Cohen's kappa was $\kappa = .85$, 95% CI [.82, .88]. The authors resolved every discrepancy.

Statistical Dependence of the Samples

We calculated the effect sizes for each dependent variable reported in the studies. For each independent sample, those effect sizes referring to the same type of measure (e.g., RTs) and extracted from the same test (e.g., different stimulus onset asynchronies in the UFOV task) were merged into one effect size. We used this procedure to calculate more reliable estimates and reduce the number of statistically dependent effect sizes in the model (Schmidt & Hunter, 2015). For those effect sizes that were statistically dependent and referred to different constructs or were extracted from different tests, we applied Cheung and Chan's (2004) correction for statistically dependent samples. This method decreased the weight of dependent samples in the analysis by calculating an adjusted (i.e., smaller) *N* in each meta-analytic model.

When the study presented multiple-group comparisons—for example, between one group (e.g., action video game players) and several comparison groups (e.g., nonvideo game players, nonaction video game players)—we calculated as many effect sizes as the number of comparisons. Since Cheung and Chan's (2004) method cannot be used for partially dependent samples, we ran our analyses as if these effect sizes were statistically independent. This relatively minor limitation was nearly absent when the effect sizes were sorted by type of video game.

Furthermore, we carried out a parallel set of analyses using the Robust Variance Estimation method (RVE; Hedges, Tipton, & Johnson, 2010; Tanner-Smith & Tipton, 2014). This technique allows one to build hierarchical meta-analytic models in the presence of nested effect sizes and is thus another method to deal with statistically dependent data in meta-analysis. We ran the analyses with the Robumeta software package (Fisher, Tipton, & Zhipeng, 2017) and report the results, corrected for small sample size (Tanner-Smith, Tipton, & Polanin, 2016), in the supplemental material available online. The whole procedure was implemented to test whether the outcomes of the meta-analytic models were sensitive to the type of meta-analytic technique used to model dependent effect sizes.

Sensitivity Analysis

Several studies reported the participants' performance on both accuracy and reaction times (RTs) in the same tasks. We reported both these measures to check for possible trade-off effects. In fact, concurrent decrease in RTs and decrease in accuracy were sometimes observed after a video-game training program (e.g., Nelson & Strachan, 2009).

In some studies, RTs were the only measures expected to improve from pre- to posttest assessments while accuracy was considered just a variable to be controlled for (i.e., no effect expected). In these cases, calculating both the sets of effect sizes might have lowered the overall effect sizes because null effects (accuracy) were possibly averaged with positive effects (RTs). To check for this potentially confounding variable, we ran three sensitivity analyses (one for each meta-analysis) including all the experiments that reported effect sizes for both accuracy and RTs in the same cognitive tasks, and analyzed accuracy and RTs separately.

Calculations of the Overall Meta-Analytic Means

We used random-effect models to estimate the overall meta-analytic means. First, we ran a model including all the effect sizes (main model) for each of the three meta-analyses. The overall meta-analytic means of the three main models represented a measure of the relationship between video game playing and overall cognitive ability. We built a series of meta-analytical submodels to assess the effects of categorical moderators in all the three meta-analyses. To run the models, we used the Comprehensive Meta-Analysis (CMA; Version 3.3; Biostat, Englewood, NJ) software package.

Publication Bias Analysis

Publication bias occurs when nonsignificant results are systematically suppressed from the literature. This problem has been documented in the field of video gaming (e.g., Boot, Blakely, & Simons, 2011). Moreover, because the response rate to our e-mails requesting for unpublished data was modest (25 positive responses out of 135 requests), a rigorous analysis of the effects of publication bias was imperative.

To investigate whether the results were affected by publication bias, we used Duval and Tweedie's (2000) trim-and-fill analysis and Vevea and Woods's (2005) selection model analysis. Trim-and-fill analysis estimates the symmetry of a funnel plot representing the relation between effect size and standard error. In the presence of publication bias, the trim-and-fill analysis estimates the number of missing studies from the funnel plot—either left or right of the meta-analytic mean—and imputes missing effect sizes based on the data's asymmetry to generate a more symmetrical funnel plot. We used CMA to perform trim-and-fill analyses.

Vevea and Woods's (2005) selection model analysis estimates four adjusted values by preweighted functions of p values distributions. If all (or most of) the four adjusted values are shown not to differ significantly from the meta-analytic mean, then it can be reliably concluded that the results are not affected by publication bias (Schmidt & Hunter, 2015). Also, this analysis stays reliable even when the number of effect sizes is modest. For this reason, we ran only this publication bias analysis in those models that had fewer than 30 effect sizes. Finally, the trim-and-fill and selection model analyses can estimate adjusted values both smaller and greater than the meta-analytic mean. We used the Metafor software package (Viechtbauer, 2010) for conducting selection model analyses.

Influential Cases Analysis

Finally, to evaluate whether some effect sizes had an unusually large influence on the meta-analytic means, we performed Viechtbauer and Cheung's (2010) influential cases analysis in every meta-analytic model. Together with publication bias analysis, influential cases analysis was adopted to test the robustness of the overall results. We used the Metafor software package for conducting these analyses.

Meta-Analysis 1: Meta-Analysis of Correlational Data Among Video Game Players

Here, we report the first ever meta-analysis examining the relationship between video game skill and cognitive ability in video game players. As stated in the introduction, a positive correlation between video game skill and cognitive ability may suggest that video game training exerts positive effects on cognitive ability. Also, the results of the present meta-analysis are a significant contribution to the study of the cognitive correlates of video game expertise.

Method

Additional inclusion criteria. The studies were included in the present meta-analysis when meeting the following two additional criteria:

1. The study provided information about how video game skill was assessed.

2. The participants had some experience of video games. For example, participants reporting zero hours of video game play per week were excluded.

Additional moderators. Along with outcome measure, we analyzed the effects of two additional moderators:

1. Skill measure (categorical moderator). This variable has two levels: (a) video game skill measured by the frequency of video game play (hours per week), and (b) video game skill measured by video game score obtained. Also, this moderator controls for the potential differences between natural groups of video game players and individuals undergoing video game training in true experiments. In fact, whereas in natural groups video game skills are measured by frequency of play, individuals involved in true experiments are assessed by video game score.

2. Type of video game (categorical moderator). This variable has three levels: (a) Action video games, (b) Nonaction video games, and (c) Mixed video games. The category of Action video games refers to those video games classified as shooter (e.g., Unreal Tournament) and racing (e.g., Mario Kart) video games. The category of Nonaction video games includes those video games that are not action video games, but are clearly classifiable as other types of video games (e.g., strategy, puzzle, and role playing). Finally, the category of Mixed video games refers to general video game experience rather than the practice of a specific genre of video game. The category of Mixed was thus used when the data collected in the primary studies did not specify the genre of video game played. The first and second authors coded each effect size independently. The Cohen's kappa was $\kappa = .96$, 95% CI [.94, .99]. The authors resolved every discrepancy.

Effect sizes. The correlations between video game skill and cognitive outcomes were taken from the data reported in the primary studies or calculated with the data provided by the authors. When the samples of video game players were artificially dichotomized and group-level comparisons (e.g., intermediate players vs. experts) were reported ($k = 16$), we calculated point-biserial correlations (Schmidt & Hunter, 2015). When the data were extrapolated from experimental studies with both pre- and posttest assessments, we used the correlations between performance on the cognitive test at the posttest assessment and either the difference between posttest and pretest video-game performance or, when provided, the video-game posttest scores. In these experimental studies, participants had no previous experience with the target video game at the beginning of the experiment. Thus, posttest video-game scores and score gains between post- and pretest video-game scores were expected to be highly correlated and, therefore, equally valid measures of video game skills. Finally, when possible, we sorted the samples by type of video game and gender.

Results

As described in the General Method section, we adopted a systematic approach to examine the correlation between video game skill and cognitive ability. First, we calculated the overall correlation with all the effect sizes. Then, we investigated the potential effects of the moderators and ran the relative submodels. We tested the robustness of the results of each model with the abovementioned publication bias and influential case analyses.

Main model. We ran a model comprising all the correlations. The random-effects meta-analytic overall correlation was $\bar{r} = .07$, 95% CI [.05, .09], $k = 310$, $p < .001$. The degree of heterogeneity between effect sizes was $I^2 = 52.19$. The I^2 statistic refers to the percentage of between-study variance attributable to true heterogeneity and not to random error (Higgins, Thompson, Deeks, & Altman, 2003). The higher the value of the I^2 statistic, the higher the percentage of between-study variance attributable to true heterogeneity. A degree of heterogeneity (I^2) around 25.00 is considered low, around 50.00 moderate, and around 75.00 high (Higgins et al., 2003). A degree of heterogeneity of 52.19 thus suggests that some moderators had a potential effect.

The contour-enhanced funnel plot (Peters, Sutton, Jones, Abrams, & Rushton, 2008) depicting the relation between effect size and standard error is shown in Figure 2.

The trim-and-fill analysis filled 16 studies left of the mean. The estimated correlation was $\bar{r} = .05$, 95% CI [.03, .08]. The estimates of the selection model analysis were $\bar{r} = .03$, $\bar{r} = .01$, $\bar{r} = .04$, and $\bar{r} = .04$ for moderate one-tailed selection, severe one-tailed selection, moderate two-tailed selection, and severe two-tailed selection, respectively. The two publication bias analyses thus suggested that the overall correlation ($\bar{r} = .07$) was a slight overestimation.

Finally, Viechtbauer and Cheung's (2010) analysis detected four influential effect sizes. The overall correlation without these effect sizes was $\bar{r} = .06$, 95% CI [.04, .08], $k = 306$, $p < .001$, $I^2 = 41.08$. Therefore, the exclusion of the influential cases did not substantially alter the results.

Moderator analysis. Given the presence of some true heterogeneity in the main model, we ran a metaregression model including all the three moderators, $Q(7) = 58.68$, $k = 310$, $p < .001$. (Running separate analyses for each moderator does not control for potential interactions between moderators. Thus, when the power of the model is sufficient, including all the moderators in a single analysis should be preferred.) Outcome measure and Skill measure were significant moderators ($p = .005$ and $p < .001$, respectively). Type of video game was marginally significant ($p = .059$).

We calculated the overall correlations of the five outcome measures. The results provided near-zero correlations in four measures and a small correlation ($\bar{r} = .18$) in spatial ability. The publication bias and influential case analyses did not evidence any substantial difference with the unadjusted correlations. The results are summarized in Table 1.

Skill measure. To examine the effect of Skill measure further, we ran two submodels. The first submodel comprised all the correlations between the outcome measures and video game skill measured by frequency of video game playing. The random-effects meta-analytic overall effect size was $\bar{r} = .03$, 95% CI [.00, .05], $k = 156$, $p = .024$. The degree of heterogeneity between effect sizes was $I^2 = 40.05$.

Trim-and-fill analysis filled four studies left of the mean. The estimated correlation was $\bar{r} = .02$, 95% CI [.00, .04]. The estimates of the selection model analysis were $\bar{r} = .01$, $\bar{r} = -.02$, $\bar{r} = .02$, and $\bar{r} = .01$ for moderate one-tailed selection, severe one-tailed selection, moderate two-tailed selection, and severe two-tailed selection, respectively. The estimates of the publication bias analyses thus did not significantly differ from the overall correlation in this model.

Three influential effect sizes were detected. The overall correlation without these effect sizes was $\bar{r} = .02$, 95% CI [.00, .04], $k = 153$, $p = .048$, $I^2 = 21.56$. Therefore, the exclusion of the influential cases did not substantially alter the results.

We finally calculated the overall correlations sorted by outcome measure. All the five overall correlations were close to zero. The publication bias and influential case analyses did not evidence any substantial difference with the unadjusted correlations. The results are summarized in Table 2.

We carried out the same analysis for the correlation between cognitive ability and video game scores as a measure of skill. We ran a model comprising all the correlations between the outcome measures and video game skill measured with video game scores. The random-effects meta-analytic overall effect size was $\bar{r} = .16$,

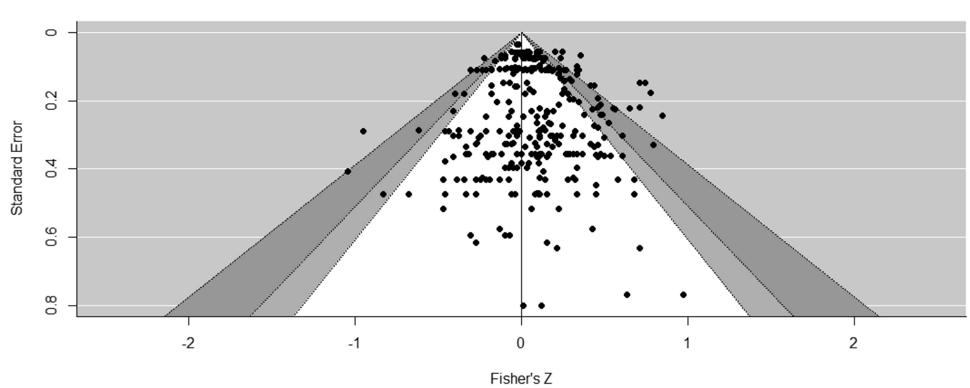

Figure 2. Contour-enhanced funnel plot of standard errors and effect sizes (Fisher's *z*s) in the meta-analysis of the correlational data. Contour lines are at 1%, 5%, and 10% levels of statistical significance.

Table 1
Meta-Analytic and Publication Bias Results of the Main Model Sorted by Outcome Measure (Meta-Analysis 1)

	Model						Model without influential cases[a]			
Outcome measure	k	$\bar{r}$	p value	I^2	T&F	SM	k	$\bar{r}$	p value	I^2
Visual attention/processing	122	.06 [.03, .10]	.001	24.23	.07 [.03, .11]	.04; .00; .06; .05;	106	.07 [.02, .11]	.003	11.52
Spatial ability	50	.18 [.13, .23]	<.001	60.03	.14 [.09, .19]	.14; .12; .14; .13;	50	.18 [.13, .23]	<.001	60.03
Cognitive control	38	−.02 [−.09, .06]	.693	32.33	−.11 [−.20, −.02]	−.06; −.10; −.03; −.02;	33	−.04 [−.14, .05]	.335	.00
Memory	43	.01 [−.03, .04]	.623	.00	.01 [−.03, .05]	−.01; −.04; .01; .01;	39	.02 [−.02, .07]	.317	.00
Intelligence/reasoning	57	.05 [.00, .10]	.033	75.43	.00 [−.05, .06]	.00; −.02; .01; .01;	54	.01 [−.02, .04]	.536	29.12

Note. k = number of effect sizes; $\bar{r}$ = random-effects meta-analytic overall correlation with 95% confidence intervals (in brackets); p value of the meta-analytic overall correlation; I^2 = ratio of true heterogeneity; T&F = trim-and-fill estimates with 95% confidence intervals (in brackets); SM = moderate one-tailed selection, severe one-tailed selection, moderate two-tailed selection, and severe two-tailed selection models estimates.
[a] When no influential cases are found, the statistics are the same as in the uncorrected model.

95% CI [.12, .20], $k = 154$, $p < .001$. The degree of heterogeneity between effect sizes was $I^2 = 48.06$.

Trim-and-fill analysis filled 19 studies right of the mean. The estimated correlation was $\bar{r} = .21$, 95% CI [.16, .26]. The estimates of the selection model analysis were $\bar{r} = .12$, $\bar{r} = .09$, $\bar{r} = .13$, and $\bar{r} = .12$ for moderate one-tailed selection, severe one-tailed selection, moderate two-tailed selection, and severe two-tailed selection, respectively. The two publication bias analyses thus provided a different pattern of results. All the estimated overall correlations were small but greater than zero.

Three influential effect sizes were detected. The overall correlation without these effect sizes was $\bar{r} = .17$, 95% CI [.13, .21], $k = 151$, $p < .001$, $I^2 = 34.88$. Therefore, the exclusion of the influential cases did not substantially alter the results.

We finally calculated the overall correlations sorted by outcome measure. Three overall correlations (i.e., spatial ability, cognitive control, and intelligence/reasoning) appeared to be greater than the others. The influential case analysis showed that removing the influential case detected in the cognitive control model significantly lowered the estimated overall correlation (from $\bar{r} = .16$ to $\bar{r} = .07$; $p = .044$ and $p = .445$, respectively). Regarding the overall correlation for spatial ability ($\bar{r} = .24$), the publication bias analyses calculated slightly smaller estimates (ranging between .15 and .18). Finally, the overall correlation for intelligence/reasoning ($\bar{r} = .14$) was found to be moderately underestimated (between .17 and .22, according to publication bias analysis). The results are summarized in Table 3.

Type of video game. We carried out a set of analyses to examine the potential moderating role of type of video game. First, we ran a model comprising all the correlations referring to action video games. The random-effects meta-analytic overall effect size was $\bar{r} = .11$, 95% CI [.06, .16], $k = 69$, $p < .001$. The degree of heterogeneity between effect sizes was $I^2 = 38.32$.

Trim-and-fill analysis filled seven studies right of the mean. The estimated correlation was $\bar{r} = .13$, 95% CI [.08, .18]. The estimates of the selection model analysis were $\bar{r} = .06$, $\bar{r} = .03$, $\bar{r} = .07$, and $\bar{r} = .06$ for moderate one-tailed selection, severe one-tailed selection, moderate two-tailed selection, and severe two-tailed selection, respectively. All the estimates of the publication bias analyses were small (between $\bar{r} = .03$ and $\bar{r} = .13$) and thus did not substantially differ from the overall correlation in this model ($\bar{r} = .11$).

Four influential effect sizes were detected. The overall correlation without these effect sizes was $\bar{r} = .15$, 95% CI [.11, .19], $k = 65$, $p < .001$, $I^2 = 31.99$. Therefore, the exclusion of the influential cases showed that the overall correlation calculated for this model ($\bar{r} = .11$) might have been a moderate underestimation.

We finally calculated the overall correlations sorted by outcome measure. The only overall correlation significantly different from zero was the one concerned with spatial ability ($\bar{r} = .30$). Per the publication bias analyses, this value was probably an overestimation (between $\bar{r} = .18$ and $\bar{r} = .26$). The results are summarized in Table 4.

We carried out the same set of analyses for the nonaction video games. We first ran a model comprising all the correlations referring to nonaction video games. The random-effects meta-analytic overall effect size was $\bar{r} = .07$, 95% CI [.04, .10], $k = 144$, $p < .001$. The degree of heterogeneity between effect sizes was $I^2 = 30.69$.

Table 2
Meta-Analytic and Publication Bias Results Sorted by Outcome Measure, With Video Game Skill Measured by Frequency of Video Game Playing (Meta-Analysis 1)

	Model						Model without influential cases			
Outcome measure	k	$\bar{r}$	p value	I^2	T&F	SM	k	$\bar{r}$	p value	I^2
Visual attention/processing	65	.05 [.01, .09]	.006	.00	.05 [.01, .09]	.03; −.01; .04; .03;	60	.07 [.03, .11]	.001	.00
Spatial ability	15	.09 [.01, .17]	.028	72.55	—	.10; .09; .11; .10;	15	.09 [.01, .17]	.028	72.55
Cognitive control	19	−.06 [−.15, .03]	.212	38.94	—	−.08; −.12; −.05; −.04;	18	−.09 [−.17, −.02]	.016	10.79
Memory	23	.00 [−.04, .04]	.936	.00	—	−.02; −.04; .00; .00;	23	.00 [−.04, .04]	.936	.00
Intelligence/reasoning	34	.01 [−.02, .05]	.498	36.25	.01 [−.03, .04]	−.01; −.03; .00; .00;	32	−.01 [−.04, .02]	.417	.00

Note. See note to Table 1 for abbreviations.

Table 3
Meta-Analytic and Publication Bias Results Sorted by Outcome Measure, With Video Game Skill Measured by Video Game Scores (Meta-Analysis 1)

Outcome measure	Model						Model without influential cases			
	k	$\bar{r}$	p value	I^2	T&F	SM	k	$\bar{r}$	p value	I^2
Visual attention/processing	57	.07 [−.01, .16]	.098	33.33	.15 [.05, .24]	.08; .04; .10; .09;	53	.06 [−.03, .16]	.204	12.39
Spatial ability	35	.24 [.18, .30]	<.001	43.40	.18 [.12, .25]	.17; .15; .18; .16;	35	.24 [.18, .30]	<.001	43.40
Cognitive control	19	.16 [.00, .30]	.044	4.51	—	.10; .01; .14; .11;	18	.07 [−.11, .24]	.445	.00
Memory	20	.05 [−.04, .14]	.302	.00	—	.02; −.02; .04; .03;	18	.09 [−.10, .27]	.358	.00
Intelligence/reasoning	23	.14 [−.06, .33]	.162	63.75	—	.21; .17; .22; .19;	23	.14 [−.06, .33]	.162	63.75

Note. See note to Table 1 for abbreviations.

Trim-and-fill analysis filled nine studies left of the mean. The estimated correlation was $\bar{r}$ = .06, 95% CI [.03, .10]. The estimates of the selection model analysis were $\bar{r}$ = .04, $\bar{r}$ = .01, $\bar{r}$ = .05, and $\bar{r}$ = .04 for moderate one-tailed selection, severe one-tailed selection, moderate two-tailed selection, and severe two-tailed selection, respectively. All the estimates of the publication bias analyses were close to zero and did not substantially differ from the unadjusted overall correlation ($\bar{r}$ = .07).

Twenty-five influential effect sizes were detected. The overall correlation without these effect sizes was $\bar{r}$ = .11, 95% CI [.05, .16], k = 119, p < .001, I^2 = 25.48. Therefore, the exclusion of the influential cases moderately increased the overall correlation (from $\bar{r}$ = .07 to $\bar{r}$ = .11). In summary, all the estimated overall correlations ranged from $\bar{r}$ = .01 to $\bar{r}$ = .11.

We finally calculated the overall correlations sorted by outcome measure. The overall correlation referring to spatial ability ($\bar{r}$ = .19) was greater than the other ones (all smaller than $\bar{r}$ = .10). The publication bias analyses provided significantly smaller estimates (between $\bar{r}$ = .06 and $\bar{r}$ = .10). The results are summarized in Table 5.

Finally, a model comprising all the correlations referring to mixed video games was run. The random-effects meta-analytic overall effect size was $\bar{r}$ = .04, 95% CI [.01, .08], k = 97, p = .024. The degree of heterogeneity between effect sizes was I^2 = 69.22.

Trim-and-fill analysis filled four studies right of the mean. The estimated correlation was $\bar{r}$ = .05, 95% CI [.01, .09]. The estimates of the selection model analysis were $\bar{r}$ = .01, $\bar{r}$ = −.01, $\bar{r}$ = .02, and $\bar{r}$ = .02 for moderate one-tailed selection, severe one-tailed selection, moderate two-tailed selection, and severe two-tailed selection, respectively. The estimates of the publication bias analyses thus did not significantly differ from the overall correlation in this model.

Four influential effect sizes were detected. The overall correlation without these effect sizes was $\bar{r}$ = .01, 95% CI [−.01, .04], k = 93, p = .332, I^2 = 37.38. Therefore, the exclusion of the influential cases did not substantially alter the results.

We finally calculated the overall correlations sorted by outcome measure. Four overall correlations were not significantly different from zero. The only exception was the small overall correlation referring to spatial ability ($\bar{r}$ = .11). The publication bias and influential case analyses did not evidence any substantial difference with the unadjusted correlations. The results are summarized in Table 6.

Sensitivity analysis. As discussed in General Method section, we tested whether including measures of both RTs and accuracy affected the results. We ran two separate meta-analytic models for effect sizes referring to RTs and accuracy.

Regarding RTs, the random-effects meta-analytic overall effect size was $\bar{r}$ = .09, 95% CI [.03, .15], k = 42, p = .004. The degree of heterogeneity was I^2 = 16.69. Thus, considering only reaction time (RT)-related measures did not provide substantially different results compared with the main model ($\bar{r}$ = .09 and $\bar{r}$ = .07, respectively). Trim-and-fill analysis filled four studies right of the mean. The estimated correlation was $\bar{r}$ = .10, 95% CI [.04, .16]. The estimates of the selection model analysis were $\bar{r}$ = .06, $\bar{r}$ = .03, $\bar{r}$ = .08, and $\bar{r}$ = .06 for moderate one-tailed selection, severe one-tailed selection, moderate two-tailed selection, and severe two-tailed selection, respectively. Four influential effect sizes were detected. The overall correlation without these effect sizes was $\bar{r}$ = .13, 95% CI [.05, .20], k = 38, p = .001, I^2 = 7.28. Therefore, the

Table 4
Meta-Analytic and Publication Bias Results Sorted by Outcome Measure, for the Correlations Referring to Action Video Games (Meta-Analysis 1)

Outcome measure	Model						Model without influential cases			
	k	$\bar{r}$	p value	I^2	T&F	SM	k	$\bar{r}$	p value	I^2
Visual attention/processing	39	.05 [−.01, .11]	.087	.00	.04 [−.02, .10]	.02; −.02; .04; .04;	36	.09 [.00, .18]	.058	.00
Spatial ability	11	.30 [.20, .39]	<.001	19.87	—	.26; .24; .26; .24;	10	.32 [.23, .41]	<.001	.00
Cognitive control	6	−.17 [−.28, −.05]	.005	.00	—	−.18; −.21; −.15; −.14;	5	−.07 [−.26, .13]	.505	.00
Memory	7	−.01 [−.08, .06]	.810	.00	—	−.02; −.04; −.01; −.01;	6	.05 [−.08, .17]	.444	.00
Intelligence/reasoning	6	.12 [−.04, .26]	.142	50.85	—	.03; .02; .04; .04;	5	.21 [.08, .33]	.002	.00

Note. See note to Table 1 for abbreviations.

Table 5
Meta-Analytic and Publication Bias Results Sorted by Outcome Measure, for the Correlations Referring to Non-Action Video Games (Meta-Analysis 1)

Outcome measure	Model						Model without influential cases			
	k	$\bar{r}$	p value	I^2	T&F	SM	k	$\bar{r}$	p value	I^2
Visual attention/processing	53	.08 [.01, .14]	.019	33.14	.09 [.02, .16]	.06; .03; .07; .06;	43	.09 [−.02, .20]	.096	11.70
Spatial ability	25	.19 [.10, .27]	<.001	55.30	—	.09; .06; .10; .08;	25	.19 [.10, .27]	<.001	55.30
Cognitive control	17	.09 [−.09, .27]	.310	.00	—	.02; −.08; .08; .06;	17	.09 [−.09, .27]	.310	.00
Memory	24	.02 [−.03, .07]	.427	.00	—	.00; −.03; .02; .01;	22	.03 [−.03, .09]	.342	.00
Intelligence/reasoning	25	.02 [−.04, .08]	.500	17.30	—	.00; −.03; .02; .01;	22	.07 [.00, .14]	.064	.00

Note. See note to Table 1 for abbreviations.

exclusion of the influential cases did not substantially alter the results.

Regarding accuracy-related measures, the random-effects meta-analytic overall effect size was $\bar{r} = .05$, 95% CI [−.03, .13], $k = 42$, $p = .246$. The degree of heterogeneity was $I^2 = 41.05$. No missing effect size was found by the trim-and-fill analysis. The estimates of the selection model analysis were $\bar{r} = .03$, $\bar{r} = -.01$, $\bar{r} = .04$, and $\bar{r} = .04$ for moderate one-tailed selection, severe one-tailed selection, moderate two-tailed selection, and severe two-tailed selection, respectively. Four influential effect sizes were detected. The overall correlation without these effect sizes was $\bar{r} = .00$, 95% CI [−.08, .08], $k = 38$, $p = .995$, $I^2 = 14.97$.

Discussion

The main model and most of the submodels showed weak correlations between video game skill and cognitive ability. For example, the overall correlations between video game skill and visual attention/processing measures are all smaller than .10. Similarly, none of the correlations regarding the measures cognitive control, memory, or intelligence/reasoning were greater than .16.

The only exception to this pattern of results was spatial ability. The overall correlations between video game skill and spatial ability were all significant with a range of values between .09 and .30. Given that $\bar{r} = .30$ is probably an overestimation (see publication bias estimates, Table 4), video game skill explains between approximatively 1% and 6% of the variance in the participants' spatial ability. The correlation between video game skill and spatial ability, although limited in size, may represent a characteristic trait of video game expertise. In support of this hypothesis, the correlation between spatial ability and video game skill was stronger when one specific genre of video games was considered (i.e., action video games; Table 4). That said, no direction of causality can be inferred from correlations. Thus, it is yet to be clarified whether video-game practice slightly fosters spatial cognition or whether players with superior spatial skills are more likely to be skilled in video-game playing.

As expected, the overall correlation was higher when video game skill was measured with scores rather than hours ($\bar{r} = .16$ and $\bar{r} = .03$, respectively). Score tends to be a more reliable measure of video game skill than the weekly frequency of play reported in a questionnaire. Thus, it is possible that the correlation between cognitive ability and hours of play per week was more affected by measurement error than the correlation between cognitive ability and video game scores.

Importantly, the influential case analyses showed no substantial differences in the overall correlations between the models with and without influential effect sizes. Regarding the publication bias analysis, most of the corrected estimates were only slightly smaller (or, in a few cases, greater) than the random-effects overall correlations. Moreover, the RVE analyses provided similar overall correlations in all the meta-analytic models (for details, see the supplemental material available online), and so did the sensitivity analysis. Thus, the results are robust. Overall, the results suggest that video game skill is not related or only weakly related to cognitive ability in general.

Meta-Analysis 2: Meta-Analysis of Quasi-Experimental Data

The meta-analysis of the correlational data showed little evidence of the cognitive benefits of playing video games. However,

Table 6
Meta-Analytic and Publication Bias Results Sorted by Outcome Measure, for the Correlations Referring to Mixed Video Games (Meta-Analysis 1)

Outcome measure	Model						Model without influential cases			
	k	$\bar{r}$	p value	I^2	T&F	SM	k	$\bar{r}$	p value	I^2
Visual attention/processing	30	.04 [−.02, .11]	.208	40.83	.05 [−.02, .12]	.02; −.01; .04; .03;	29	.06 [.00, .13]	.067	.00
Spatial ability	14	.11 [.04, .18]	.004	66.21	—	.12; .11; .13; .12;	14	.11 [.04, .18]	.004	66.21
Cognitive control	15	.00 [−.12, .12]	.987	56.16	—	−.02; −.06; .00; .00;	15	.00 [−.12, .12]	.987	56.16
Memory	12	−.02 [−.10, .06]	.610	.00	—	−.10; −.21; −.03; −.02;	11	−.02 [−.13, .09]	.742	.00
Intelligence/reasoning	26	.06 [−.01, .13]	.082	90.01	—	−.01; −.03; .00; .00;	23	−.02 [−.05, .01]	.147	.00

Note. See note to Table 1 for abbreviations.

it is possible that such benefits occur regardless of skill, as long as individuals engage in video game playing. It is thus necessary to examine whether video game players outperform nonvideo game players in the cognitive measures examined above. Like in the previous meta-analysis, this outcome may suggest, yet not necessarily imply, that video game training positively impacts cognitive ability. Finally, like in the meta-analysis of the correlational data, the results of this meta-analysis will contribute to the research into the cognitive correlates of video game expertise.

Method

Additional inclusion criteria. The studies were included in the present meta-analysis when meeting the following two additional criteria:

1. The study provided clear information about how video game status was assessed (e.g., hours of play per week). Note that some studies did not explicitly report the precise cut-off point that was used for players to be included in the video-game group but rather referred to the criterion used in a previous study, such as that used by Green and Bavelier (2007). The studies reported several different criteria defining video-game players and nonvideo game players (for details, see Table SE2). For this reason, we ran a sensitivity analysis including only those studies meeting Green and Bavelier (2007)'s criteria: at least five hours of action video game play per week in the past six months for action video-game players and no action-video game play for nonvideo game players. The results are reported in the supplemental material available online.

2. The study compared participants with experience of video game playing (in general or in a specific genre of video game) with participants with negligible or null experience in video game playing (in general, or in that specific genre of video game).

Additional moderators. Along with the outcome measure, we analyzed the effects of one additional moderator: Type of video game (categorical moderator). This variable has three levels: (a) Action video game, (b) Nonaction video game, and (c) Mixed video game. Action video game refers to the comparisons between action video game (shooter and racing) players versus nonaction video game players and nonvideo game players. Most of the studies involving action video game players adopted Green and Bavelier's (2003, 2007) criteria (see Table SE2). Thus, action video-game players were compared with nonaction video-game players without distinguishing between nonplayers and players of nonaction video games. The category of Nonaction video game includes the comparisons between nonaction video game players and nonplayers. Finally, consistent with Meta-Analysis 1, the category of Mixed video game refers to the comparisons between video game players (with no specific genre specialization) and nonvideo game players. The first and second authors coded each effect size independently. The Cohen's kappa was $\kappa = .95$, 95% CI [.92, .99]. The authors resolved every discrepancy.

Effect sizes. We calculated the standardized mean difference (i.e., Cohen's *d*) between the two groups with the following formula:

$$d = (M_e - M_c)/SD_{pooled} \tag{1}$$

where SD_{pooled} is the pooled standard deviation, and M_e and M_c are the means of the experimental group (i.e., video game players) and the control group (i.e., nonvideo game players), respectively. When the comparisons between video game players and nonvideo game players were expressed with *t* or *F* values, we used CMA to convert them into Cohen's *d*s. We excluded the *F* statistics referring to interactions between group and others conditions. Finally, to correct the effect sizes for upward bias, we used CMA to convert Cohen's *d*s into Hedges's *g*s (Hedges & Olkin, 1985).

Results

We adopted the systematic approach described in the General Method section to examine the difference between video game players and nonvideo game players in terms of cognitive ability. First, we calculated the overall effect sizes including all the effects. Then, we investigated the potential effects of the moderators and ran the relative submodels. We tested robustness of the results of each model with the two publication bias analyses and Viechtbauer and Cheung's (2010) influential case analysis.

Main model. In the model comprising all the effect sizes, the random-effects meta-analytic overall effect size was $\bar{g} = 0.33$, 95% CI [0.28, 0.39], $k = 315$, $p < .001$. The degree of heterogeneity between effect sizes was low, $I^2 = 33.79$. The contour-enhanced funnel plot is shown in Figure 3.

The trim-and-fill analysis filled 73 studies left of the mean. The estimated effect size was $\bar{g} = 0.18$, 95% CI [0.12, 0.24]. The estimates of the selection model analysis were $\bar{g} = 0.24$, $\bar{g} = 0.17$, $\bar{g} = 0.27$, and $\bar{g} = 0.23$ for moderate one-tailed selection, severe one-tailed selection, moderate two-tailed selection, and severe two-tailed selection, respectively. The two publication bias analyses thus suggested that the unadjusted overall effect size ($\bar{g} = 0.33$) was significantly inflated by the suppression from the literature of several smaller-than-average effect sizes.

Finally, Viechtbauer and Cheung's (2010) analysis detected two influential effect sizes. The overall effect size without these effect sizes was $\bar{g} = 0.32$, 95% CI [0.27, 0.38], $k = 313$, $p < .001$, $I^2 = 30.59$. Therefore, the exclusion of the influential cases did not substantially alter the results.

Moderator analysis. We ran a metaregression model including the two moderators, $Q(6) = 13.72$, $k = 315$, $p = .033$. Neither outcome measure nor type of game was significant ($p = .174$ and $p = .101$, respectively).

We calculated the overall effect sizes for the five outcome measures. The results showed positive effect sizes (range 0.19 to 0.41) in all the measures. The influential case analyses did not highlight any substantial difference with the unadjusted overall effect sizes. By contrast, the estimates provided by the publication bias analyses were systematically smaller than the unadjusted values. This pattern of results was particularly evident in the visual attention/processing- and memory-related measures. The results are summarized in Table 7.

Type of video game. Like in the meta-analysis regarding the correlational data, we ran a series of analyses to examine the

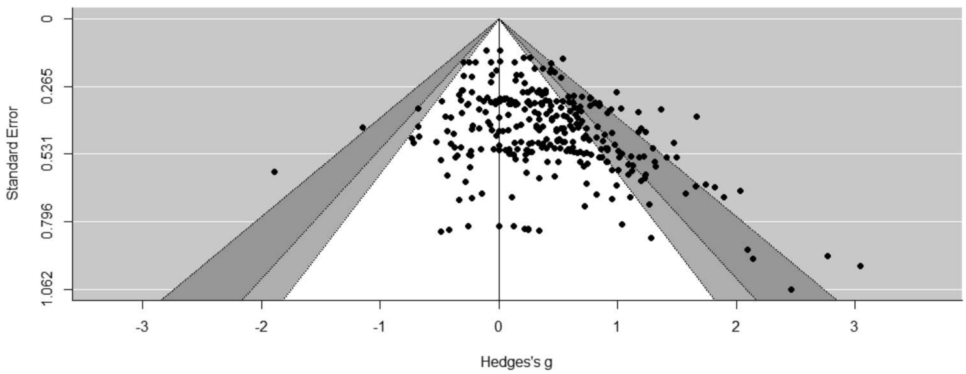

Figure 3. Contour-enhanced funnel plot of standard errors and effect sizes (*gs*) in the meta-analysis of the quasi-experimental data. Contour lines are at 1%, 5%, and 10% levels of statistical significance.

potential moderating role of type of video game. First, we ran a submodel comprising all the effect sizes referring to action video games. The random-effects meta-analytic overall effect size was $\bar{g}$ = 0.40, 95% CI [0.33, 0.47], k = 199, p < .001. The degree of heterogeneity between effect sizes was I^2 = 33.10.

Trim-and-fill analysis filled 38 studies left of the mean. The estimated overall effect size was $\bar{g}$ = 0.26, 95% CI [0.18, 0.34]. The estimates of the selection model analysis were $\bar{g}$ = 0.34, $\bar{g}$ = 0.26, $\bar{g}$ = 0.37, and $\bar{g}$ = 0.31 for moderate one-tailed selection, severe one-tailed selection, moderate two-tailed selection, and severe two-tailed selection, respectively. Thus, the publication bias analyses suggested that the unadjusted overall effect size ($\bar{g}$ = 0.40) was an overestimation.

Two influential effect sizes were detected. The overall effect size without these effect sizes was $\bar{g}$ = 0.38, 95% CI [0.31, 0.45], k = 197, p < .001, I^2 = 28.37. Therefore, the exclusion of the influential cases did not substantially alter the results.

We finally calculated the overall effect sizes sorted by outcome measure. Four measures provided statistically significant and overall effect sizes (the only exception was intelligence/reasoning). Viechtbauer and Cheung's (2010) influential case analysis evidenced no significant differences between adjusted and unadjusted values. The publication bias analyses estimated smaller overall effect sizes in all the measures. The results are summarized in Table 8.

Second, we ran a model comprising all the effect sizes concerned with nonaction video games. The random-effects meta-analytic overall effect size was $\bar{g}$ = 0.33, 95% CI [0.11, 0.55], k = 14, p = .003. The degree of heterogeneity between effect sizes was I^2 = .00.

The estimates of the selection model analysis were $\bar{g}$ = 0.27, $\bar{g}$ = 0.19, $\bar{g}$ = 0.30, and $\bar{g}$ = 0.25 for moderate one-tailed selection, severe one-tailed selection, moderate two-tailed selection, and severe two-tailed selection, respectively. No outlier was detected. Because of the scarcity of the effect sizes, we did not run any submodels of outcome measures.

Finally, we carried out a systematic set of analyses for mixed video games. For the model comprising all the effect sizes referring to mixed video games, the random-effects meta-analytic overall effect size was $\bar{g}$ = 0.23, 95% CI [0.15, 0.31], k = 102, p < .001. The degree of heterogeneity between effect sizes was I^2 = 33.15.

Trim-and-fill analysis filled 25 studies left of the mean. The estimated overall effect size was $\bar{g}$ = 0.12, 95% CI [0.04, 0.20]. The estimates of the selection model analysis were $\bar{g}$ = 0.15, $\bar{g}$ = 0.07, $\bar{g}$ = 0.17, and $\bar{g}$ = 0.14 for moderate one-tailed selection, severe one-tailed selection, moderate two-tailed selection, and severe two-tailed selection, respectively. Thus, the publication bias analyses once again suggested that the unadjusted overall effect size ($\bar{g}$ = 0.23) was an overestimation. Only one influential effect size was detected. The overall effect size without this effect size was $\bar{g}$ = 0.22, 95% CI [0.14, 0.30], k = 101, p < .001, I^2 = 29.87.

We finally calculated the overall effect sizes sorted by outcome measure. All the overall effect sizes were small (see Table 9). The

Table 7
Meta-Analytic and Publication Bias Results of the Main Model Sorted by Outcome Measure (Meta-Analysis 2)

	Model						Model without influential cases			
Outcome measure	k	$\bar{g}$	p value	I^2	T&F	SM	k	$\bar{g}$	p value	I^2
Visual attention/processing	186	.41 [.33, .49]	<.001	32.89	.27 [.19, .35]	.34; .26; .36; .31;	184	.39 [.32, .47]	<.001	28.15
Spatial ability	28	.24 [.13, .34]	<.001	25.33	—	.18; .13; .20; .18;	28	.24 [.13, .34]	<.001	25.33
Cognitive control	53	.24 [.12, .36]	<.001	25.68	.17 [.04, .30]	.18; .12; .21; .18;	53	.24 [.12, .36]	<.001	25.68
Memory	32	.20 [.03, .37]	.019	45.14	−.04 [−.23, .15]	.09; .01; .13; .10;	32	.20 [.03, .37]	.019	45.14
Intelligence/reasoning	16	.19 [.00, .38]	.055	7.37	—	.13; .05; .16; .13;	16	.19 [.00, .38]	.055	7.37

Note. k = number of effect sizes; $\bar{g}$ = random-effects meta-analytic mean with 95% confidence intervals (in brackets); p value of the meta-analytic overall effect size; I^2 = ratio of true heterogeneity; T&F = trim-and-fill estimates with 95% confidence intervals (in brackets); SM = moderate one-tailed selection, severe one-tailed selection, moderate two-tailed selection, and severe two-tailed selection models estimates.

Table 8
Meta-Analytic and Publication Bias Results Sorted by Outcome Measure, for the Effect Sizes Referring to Action Video Games (Meta-Analysis 2)

	Model						Model without influential cases			
Outcome measure	k	$\bar{g}$	p value	I^2	T&F	SM	k	$\bar{g}$	p value	I^2
Visual attention/processing	132	.45 [.36, .54]	<.001	33.10	.27 [.17, .38]	.38; .30; .41; .34;	131	.43 [.34, .52]	<.001	29.52
Spatial ability	8	.47 [.21, .74]	.001	.00	—	.43; .36; .44; .38;	8	.47 [.21, .74]	.001	.00
Cognitive control	33	.27 [.09, .46]	.004	28.07	.18 [−.01, .38]	.19; .10; .23; .19;	33	.27 [.09, .46]	.004	28.07
Memory	17	.31 [.06, .57]	.017	50.93	−	.22; .14; .25; .21;	17	.31 [.06, .57]	.017	50.93
Intelligence/reasoning	9	.17 [−.21, .54]	.377	51.92	—	.09; −.01; .14; .11;	9	.17 [−.21, .54]	.377	51.92

Note. See note to Table 7 for abbreviations.

influential case analysis evidenced no significant differences between adjusted and unadjusted values. The publication bias analyses estimated smaller overall effect sizes in all the measures.

Sensitivity analysis. Like in Meta-Analysis 1, we tested whether including measures of both RTs and accuracy affected the results. We ran two separate meta-analytic models for effect sizes referring to RTs and accuracy.

Regarding RTs, the random-effects meta-analytic overall effect size was $\bar{g} = 0.46$, 95% CI [0.36, 0.56], $k = 71$, $p < .001$. The degree of heterogeneity was $I^2 = 23.10$. The overall effect size referring to RT-related measures thus appeared to be slightly larger than the one of the main model ($\bar{g} = 0.46$ and $\bar{g} = 0.33$, respectively). The trim-and-fill analysis filled 16 studies left of the mean. The estimated effect size was $\bar{g} = 0.34$, 95% CI [0.22, 0.45]. The estimates of the selection model analysis were $\bar{g} = 0.40$, $\bar{g} = 0.34$, $\bar{g} = 0.42$, and $\bar{g} = 0.36$ for moderate one-tailed selection, severe one-tailed selection, moderate two-tailed selection, and severe two-tailed selection, respectively. No influential effect sizes were detected.

Regarding accuracy-related measures, the random-effects meta-analytic overall effect size was $\bar{g} = 0.05$, 95% CI [−0.06, 0.17], $k = 71$, $p = .368$. The degree of heterogeneity was $I^2 = 46.35$. Trim-and-fill analysis filled 16 studies left of the mean. The estimated effect size was $\bar{g} = -0.13$, 95% CI [−0.26, 0.00]. The estimates of the selection model analysis were $\bar{g} = -0.04$, $\bar{g} = -0.14$, $\bar{g} = 0.02$, and $\bar{g} = 0.02$ for moderate one-tailed selection, severe one-tailed selection, moderate two-tailed selection, and severe two-tailed selection, respectively. One influential effect size was detected. The overall effect size without this effect size was $\bar{g} = 0.07$, 95% CI [−0.03, 0.18], $k = 70$, $p = .182$, $I^2 = 38.36$.

Discussion

The overall effect sizes of the main model and submodels showed significantly positive effect sizes, indicating that video game players outperformed nonplayers in all the five broad measures of cognitive ability. This superiority occurred regardless of the type of game considered. However, the publication bias analysis calculated a reduced estimate for many of the largest overall effect sizes. Most of these corrected overall effect sizes remained significant or marginally significant. The influential cases analysis did not substantially modify the overall effect sizes. Like in Meta-analysis 1, the RVE analyses provided similar overall effect sizes in all the meta-analytic models (for details, see the supplemental material available online). This convergence of results confirms the reliability of the meta-analytic models.

Interestingly, the sensitivity analyses showed that the advantage of video game players over nonplayers was slightly more pronounced in RT-related measures ($\bar{g} = 0.46$). With regard to Green and Bavelier's (2007) criteria, no meaningful difference was found. In fact, when considering only those studies meeting those criteria, the overall effect size was very similar to the one of the action video game model ($\bar{g} = 0.42$ and $\bar{g} = 0.40$, respectively).

Overall, the results suggest that video game players do differ from nonvideo game players in terms of cognitive ability. Nonetheless, the size of the effects is substantially smaller than the ones reported in Powers et al. (2013). Although quasi-experiments do not allow any strong inference with respect to causality, the outcomes of this meta-analysis may suggest that engagement in video games exerts some modest effects on overall cognitive ability. However, the results do not exclude the possibility that individuals with higher cognitive abilities are more likely to play video games.

Table 9
Meta-Analytic and Publication Bias Results Sorted by Outcome Measure, for the Effect Sizes Referring to Mixed Video Games (Meta-Analysis 2)

	Model						Model without influential cases			
Outcome measure	k	$\bar{g}$	p value	I^2	T&F	SM	k	$\bar{g}$	p value	I^2
Visual attention/processing	44	.33 [.18, .47]	<.001	35.35	.21 [.05, .36]	.25; .18; .28; .23;	44	.33 [.18, .47]	<.001	35.35
Spatial ability	16	.21 [.06, .36]	.005	37.63	—	.13; .08; .15; .12;	16	.21 [.06, .36]	.005	37.63
Cognitive control	20	.21 [.06, .36]	.006	30.14	—	.18; .13; .20; .17;	18	.20 [.07, .34]	.002	.00
Memory	15	.06 [−.13, .26]	.523	36.25	—	−.02; −.10; .03; .02;	14	−.05 [−.21, .12]	.578	1.50
Intelligence/reasoning	7	.20 [.00, .40]	.052	.00	—	.15; .09; .18; .15;	7	.20 [.00, .40]	.052	.00

Note. See note to Table 7 for abbreviations.

If so, no causal relationship in which playing video games leads to superior cognitive abilities needs to be postulated to account for these results.

Meta-Analysis 3: Meta-Analysis of Experimental Training Data

Overall, the two previous meta-analyses provided weak evidence in favor of the hypothesis according to which playing video games enhances cognitive ability. This hypothesis, however, cannot be properly evaluated without testing it directly. This meta-analysis thus examines the effects of video game interventions on participants' cognitive ability.

Method

Additional inclusion criteria. Studies were included in the present meta-analysis when meeting the following additional criteria:

1. The study included at least one control group.

2. The study included participants with no (or negligible) experience, at the beginning of the experiment, in the video game(s) used during training.

3. The training video game was not purposely designed to improve cognitive ability (e.g., Lumosity brain-training video games).

Additional moderators. Along with outcome measure, we analyzed the effects of four additional moderators:

1. Random allocation (dichotomous moderator): whether participants were allocated or not to the groups by randomization. This moderator was included to control for potential confounding effects attributable to differences at baseline level.

2. Hours of training (continuous moderator): the duration of training in hours. This moderator was also dichotomized (cut-off point = 3 hours) to control for potential differences between studies looking for priming effects (very short training programs with immediate posttest assessment) and short-to-medium-term effects. The analyses and results are reported in the supplemental material available online.

3. Type of video game (categorical moderator). This variable has three levels: (a) Action versus nonaction video game players, where the action video game training (e.g., Unreal Tournament) was compared with an active control group training in a nonaction video game (e.g., The Sims); (b) Action video game training, where the action video game training was compared with a control group not engaged in video game playing; and (c) Nonaction video game training, where the nonaction video game training (e.g., Tetris, The Sims, and Rise of Nations) was compared with a control group not engaged in video game playing. Furthermore, we ran another model comprising all the effect sizes referring to samples trained with Tetris and Tetris-like games (i.e., Tetrus and Blockout) separately. Finally, a small group of effect sizes (k = 15) from three studies did not fit any of the above categories and were excluded from the analyses regarding Type of video game. The first and the second authors coded each effect size independently. No discrepancies were found. Table SE6 reports a list of the included video games sorted by category of video game (i.e., action, nonaction, and Tetris-like). Note that no moderator distinguishing between active and passive control groups was included. In most of the cases, active control groups consisted of people playing another type of video game. Thus, running models sorted by the type of control group (i.e., active or passive) would substantially duplicate the results of the moderator Type of video game.

4. Age (categorical moderator). This variable has three levels: (a) Adult, where the participants were aged 18 to 55; (b) Older, where the participants were older than 55; and (c) Younger, where the participants were younger than 18.

Effect sizes. We calculated the standardized mean difference (i.e., Cohen's d) between the two groups with the following formula:

$$d = (M_{g-e} - M_{g-c})/SD_{pooled-pre} \quad (2)$$

where $SD_{pooled\text{-}pre}$ is the pooled standard deviation of the two pretest standard deviations, and $Mg\text{-}e$ and $Mg\text{-}c$ are the gain of the experimental group and the control group, respectively (Schmidt & Hunter, 2015, p. 353). When means and standard deviations were not available, t or F values were converted into Cohen's ds with CMA. More specifically, the t and F statistics referring to pre–post improvements within groups were converted to ds and then subtracted to calculate the standardized mean difference between the experimental and control groups. Alternatively, the statistics referring to between-groups differences at pre- and posttests were converted to ds and then subtracted. Like in Meta-Analysis 2, we excluded the statistics referring to interactions between group and other conditions. Finally, to correct the effect sizes for upward bias, we used CMA to convert Cohen's ds into Hedges's gs (Hedges & Olkin, 1985).

Effect sizes in three-group designs. Studies about action video games (e.g., Strobach, Frensch, & Schubert, 2012) often implement three-group designs (i.e., one experimental group, one active control group and one passive control group). In such cases, we calculated two sets of effect sizes: one referring to the comparison between the experimental group (action video games) and the active control group; and one referring to the comparison between the experimental group and the passive control group.

It is worth noting that the active control groups often played another type of video game (i.e., nonaction). We decided not to calculate the effect sizes referring to the comparisons between active (nonaction video games) and passive controls, mainly for two reasons. First, in the original design of the experiments, the active control groups were not expected to achieve any meaningful pre-post-test improvement. For example, people playing The Sims are very unlikely to improve their visuo-attentional skills. Thus, including tens of near-zero effect sizes into the meta-analytic models would have artificially driven the overall effect sizes toward the null. Second, including these effect sizes would have significantly increased the degree of statistical dependence between effect sizes.

That said, all the effect sizes representing the difference between the active and passive control groups' performance can be retrieved from the supplemental material available online. In fact, these effect sizes can be calculated by subtracting the other two sets of effect sizes (experimental vs. active and experimental vs. passive).

Results

We ran a set of analyses to investigate whether video game training provided any benefit for the participants' cognitive ability. Like in the two previous meta-analyses, we first calculated the overall effect sizes including all the effects. Then, we examined the potential effects of the moderators and ran the relative submodels. We tested the robustness of the results of each model with the two publication bias analyses and Viechtbauer and Cheung's (2010) influential case analysis.

Main model. The random-effects meta-analytic overall effect size was $\bar{g}$ = 0.07, 95% CI [0.02, 0.12], k = 359, p = .004. The degree of heterogeneity between effect sizes was I^2 = 18.02. The contour-enhanced funnel plot is shown in Figure 4.

The two publication bias analyses lowered the already small effect size further. The trim-and-fill analysis filled 37 studies left of the mean. The estimated effect size was $\bar{g}$ = −0.01, 95% CI [−0.06, 0.05]. The estimates of the selection model analysis were $\bar{g}$ = −0.01, $\bar{g}$ = −0.11, $\bar{g}$ = 0.06, and $\bar{g}$ = 0.05 for moderate one-tailed selection, severe one-tailed selection, moderate two-tailed selection, and severe two-tailed selection, respectively.

Viechtbauer and Cheung's (2010) analysis detected three influential effect sizes. The overall effect size without these effect sizes was unaltered, $\bar{g}$ = 0.07, 95% CI [0.03, 0.12], k = 356, p = .002, I^2 = 10.80.

Moderator analysis. We ran a metaregression model including all the five moderators, $Q(10)$ = 33.34, k = 341, p < .001. (Most of the missing values [k = 15] in the model were attributable to the moderator Type of video game. The remaining three missing values came from the moderator age. Given the small percentage of missing values [about 5%], the results of this moderator analysis can be considered highly reliable.) In line with the low degree of heterogeneity, the effect of the moderators was modest. Random allocation and hours of training were not significant moderators, p = .321 and p = .826, respectively. Outcome measure and age were marginally significant, p = .058 and p = .056, respectively. Type of video game was the only significant moderator, p = .008.

Similar to the other two meta-analyses, we calculated overall effect sizes for the five outcome measures. The results showed null or small effect sizes in all the measures. No substantial difference emerged from the influential case and publication bias analyses. The results are summarized in Table 10.

Type of video game. We analyzed this moderator to test the potential differences between types of video game training. First, we ran a submodel comprising all the effect sizes referring to action versus nonaction video game players. The random-effects meta-analytic overall effect size was $\bar{g}$ = 0.10, 95% CI [−0.01, 0.20], k = 96, p = .068. The degree of heterogeneity between effect sizes was I^2 = 14.37.

The publication bias analyses once again showed that the effect size was inflated. Trim-and-fill analysis filled 18 studies left of the mean. The estimated overall effect size was $\bar{g}$ = −0.03, 95% CI [−0.14, 0.09]. The estimates of the selection model analysis were $\bar{g}$ = −0.01, $\bar{g}$ = −0.13, $\bar{g}$ = 0.06, and $\bar{g}$ = 0.05 for moderate one-tailed selection, severe one-tailed selection, moderate two-tailed selection, and severe two-tailed selection, respectively. No influential effect sizes were detected.

We finally calculated the overall effect sizes sorted by outcome measure. All the overall effect sizes were small or null. While the influential case analysis detected no outliers, the publication bias analyses estimated moderately smaller overall effect sizes in all the measures. The results are summarized in Table 11.

The previous submodel examined the effects of action video game training compared with nonaction video game training. We now consider the comparison action video game players versus nonvideo game players. In the submodel comprising all effect sizes, the random-effects meta-analytic overall effect size was $\bar{g}$ = −0.12, 95% CI [−0.25, 0.01], k = 88, p = .072. The degree of heterogeneity between effect sizes was I^2 = 49.08.

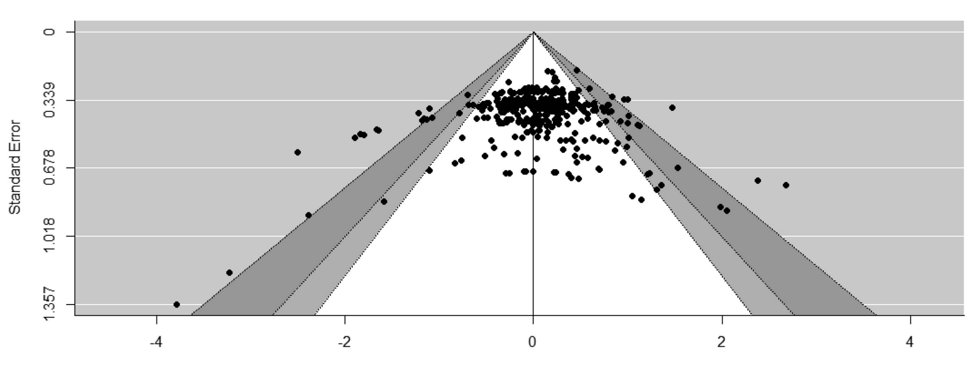

Figure 4. Contour-enhanced funnel plot of standard errors and effect sizes (gs) in the meta-analysis of the experimental data. Contour lines are at 1%, 5%, and 10% levels of statistical significance.

Table 10
Meta-Analytic and Publication Bias Results of the Main Model Sorted by Outcome Measure (Meta-Analysis 3)

Outcome measure	Model						Model without influential cases			
	k	$\bar{g}$	p value	I^2	T&F	SM	k	$\bar{g}$	p value	I^2
Visual attention/processing	131	.09 [.01, .18]	.033	3.23	−.03 [−.13, .06]	.01; −.11; .08; .06;	131	.09 [.01, .18]	.033	3.23
Spatial ability	75	.14 [.05, .22]	.002	.00	.14 [.05, .22]	.07; −.02; .13; .10;	73	.13 [.04, .23]	.004	.00
Cognitive control	55	.02 [−.12, .17]	.738	27.04	.14 [−.02, .30]	−.04; −.16; .03; .02;	55	.02 [−.12, .17]	.738	27.04
Memory	67	.13 [.03, .22]	.010	.00	.22 [.11, .33]	.05; −.06; .11; .09;	67	.13 [.03, .22]	.010	.00
Intelligence/reasoning	31	−.14 [−.36, .08]	.206	55.62	−.18 [−.40, .04]	−.15; −.27; −.07; −.06;	29	−.02 [−.21, .16]	.799	31.87

Note. k = number of effect sizes; $\bar{g}$ = random-effects meta-analytic mean with 95% confidence intervals (in brackets); p value of the meta-analytic overall effect size; I^2 = ratio of true heterogeneity; T&F = trim-and-fill estimates with 95% confidence intervals (in brackets); SM = moderate one-tailed selection, severe one-tailed selection, moderate two-tailed selection, and severe two-tailed selection models estimates.

Trim-and-fill analysis filled 15 studies left of the mean. The estimated overall effect size was $\bar{g} = -0.27$, 95% CI [−0.40, −0.13]. The estimates of the selection model analysis were $\bar{g} = -0.23$, $\bar{g} = -0.39$, $\bar{g} = -0.14$, and $\bar{g} = -0.11$ for moderate one-tailed selection, severe one-tailed selection, moderate two-tailed selection, and severe two-tailed selection, respectively. These small negative estimated values were probably statistical artifacts.

Only one influential effect size was detected. The overall effect size without these effect sizes was $\bar{g} = -0.10$, 95% CI [−0.22, 0.03], $k = 87$, $p = .123$, $I^2 = 43.81$.

Finally, we calculated the overall effect sizes sorted by outcome measure. Four overall effect sizes were small or null. The only exception was the large negative overall effect size referring to intelligence/reasoning measures. However, because of the small number of effect sizes ($k = 8$), this overall effect size is not a reliable estimate. The influential case analysis detected one outlier in the cognitive control and memory models. The adjusted values were significantly closer to zero compared with the negative unadjusted values. The results are summarized in Table 12.

The third submodel of this moderator analysis comprised all the effect sizes referring to nonaction video game players versus nonvideo game players. The random-effects meta-analytic overall effect size was $\bar{g} = 0.13$, 95% CI [0.07, 0.18], $k = 160$, $p < .001$. The degree of heterogeneity between effect sizes was $I^2 = .00$.

Trim-and-fill analysis filled 10 studies right of the mean. The estimated overall effect size was $\bar{g} = 0.17$, 95% CI [0.11, 0.22]. The estimates of the selection model analysis were $\bar{g} = 0.08$, $\bar{g} = -0.01$, $\bar{g} = 0.13$, and $\bar{g} = 0.10$ for moderate one-tailed selection, severe one-tailed selection, moderate two-tailed selection, and severe two-tailed selection, respectively. The estimates of the publication bias analyses thus did not substantially differ from the unadjusted overall effect size.

Two influential effect sizes were detected. The overall effect size without these effect sizes was close to the unadjusted one, $\bar{g} = 0.12$, 95% CI [0.07, 0.18], $k = 158$, $p < .001$, $I^2 = .00$.

Finally, we calculated the overall effect sizes sorted by outcome measure. All the overall effect sizes were small. The influential case and publication bias analyses had no substantial impact on the estimated values. The results are summarized in Table 13.

In addition, we ran a submodel comprising all the effects referring to the effects of Tetris or similar games (for details, see Table SE4 in the supplemental material available online). The random-effects meta-analytic overall effect size was $\bar{g} = 0.07$, 95% CI [−0.03, 0.17], $k = 72$, $p = .160$. The degree of heterogeneity between effect sizes was $I^2 = .00$.

Trim-and-fill analysis filled 10 studies right of the mean. The estimated overall effect size was $\bar{g} = 0.15$, 95% CI [0.05, 0.26]. The estimates of the selection model analysis were $\bar{g} = 0.02$, $\bar{g} = -0.10$, $\bar{g} = 0.08$, and $\bar{g} = 0.06$ for moderate one-tailed selection, severe one-tailed selection, moderate two-tailed selection, and severe two-tailed selection, respectively. The two publication bias analyses thus provided a slightly different pattern of results. All the estimated overall effect sizes were small or null.

One influential effect size was detected. The overall effect size without this effect size was close to the unadjusted one, $\bar{g} = 0.07$, 95% CI [−0.04, 0.17], $k = 71$, $p = .202$, $I^2 = .00$.

Finally, we calculated the overall effect sizes sorted by outcome measure. The influential case and publication bias analyses had no

Table 11
Meta-Analytic and Publication Bias Results Sorted by Outcome Measure, for the Effect Sizes Referring to Action Video Game Players vs. Non-Action Video Game Players (Meta-Analysis 3)

Outcome measure	Model						Model without influential cases			
	k	$\bar{g}$	p value	I^2	T&F	SM	k	$\bar{g}$	p value	I^2
Visual attention/processing	54	.22 [.06, .38]	.007	30.28	.15 [−.02, .32]	.09; −.03; .15; .12;	54	.22 [.06, .38]	.007	30.28
Spatial ability	14	−.04 [−.26, .18]	.732	10.91	—	−.11; −.24; −.03; −.03;	14	−.04 [−.26, .18]	.732	10.91
Cognitive control	17	−.03 [−.29, .23]	.820	14.32	—	−.13; −.29; −.03; −.03;	17	−.03 [.05, .40]	.820	14.32
Memory	11	.11 [−.12, .34]	.346	.00	—	.04; −.07; .10; .08;	11	.11 [−.12, .34]	.346	.00
Intelligence/reasoning	0	—	—	—	—	—	0	—	—	—

Note. See note to Table 10 for abbreviations.

Table 12
Meta-Analytic and Publication Bias Results Sorted by Outcome Measure, for the Effect Sizes Referring to Action Video Game Players vs. Non-Video Game Players (Meta-Analysis 3)

Outcome measure	Model						Model without influential cases			
	k	$\bar{g}$	p value	I^2	T&F	SM	k	$\bar{g}$	p value	I^2
Visual attention/ processing	35	−.01 [−.16, .13]	.844	9.80	−.02 [−.18, .12]	−.10; −.23; −.02; −.02;	35	−.01 [−.16, .13]	.844	9.80
Spatial ability	22	.12 [−.08, .32]	.248	.00	—	.04; −.10; .12; .09;	22	.12 [−.08, .32]	.248	.00
Cognitive control	11	−.27 [−.72, .17]	.230	62.77	—	−.33; −.50; −.23; −.19;	10	−.10 [−.46, .25]	.568	35.87
Memory	12	−.10 [−.43, .22]	.541	38.56	—	.07; .07; .07; .07;	11	.02 [−.24, .27]	.902	.00
Intelligence/ reasoning	8	−1.17 [−1.83, −.51]	.001	69.33	—	−1.05; −1.17; −1.01; −.96;	8	−1.17 [−1.83, −.51]	.001	69.33

Note. See note to Table 10 for abbreviations.

substantial impact on the estimated values. The results are summarized in Table 14.

Age. This section investigates the potential moderating role of age. First, we examined adult video game players. In the submodel comprising all the effect sizes, the random-effects meta-analytic overall effect size was $\bar{g} = 0.10$, 95% CI [0.05, 0.15], $k = 239$, $p < .001$. The degree of heterogeneity between effect sizes was $I^2 = .00$.

The two publication bias analyses provided slightly smaller estimates. Trim-and-fill analysis filled 21 studies left of the mean. The estimated overall effect size was $\bar{g} = 0.04$, 95% CI [−0.01, 0.10]. The estimates of the selection model analysis were $\bar{g} = 0.03$, $\bar{g} = -0.07$, $\bar{g} = 0.09$, and $\bar{g} = 0.07$ for moderate one-tailed selection, severe one-tailed selection, moderate two-tailed selection, and severe two-tailed selection, respectively. Five influential effect sizes were detected. The overall effect size without these effect sizes was not different from the unadjusted effect size, $\bar{g} = 0.10$, 95% CI [0.05, 0.15], $k = 234$, $p < .001$, $I^2 = .00$.

Finally, we calculated the overall effect sizes sorted by outcome measure. All the overall effect sizes were small. The influential case and publication bias analyses had no substantial impact on the estimated values. The results are summarized in Table 15.

Second, we analyzed the results for the older video game players. In the submodel comprising all effect sizes, the random-effects meta-analytic overall effect size was $\bar{g} = -0.08$, 95% CI [−0.21, 0.04], $k = 92$, $p = .184$. The degree of heterogeneity between effect sizes was $I^2 = 48.90$.

Trim-and-fill analysis filled 21 studies left of the mean. The estimated overall effect size was $\bar{g} = -0.28$, 95% CI [−0.41, −0.16]. The estimates of the selection model analysis were $\bar{g} = -0.18$, $\bar{g} = -0.32$, $\bar{g} = -0.09$, and $\bar{g} = -0.07$ for moderate one-tailed selection, severe one-tailed selection, moderate two-tailed selection, and severe two-tailed selection, respectively. One influential effect size was detected. The overall effect size without these effect sizes was $\bar{g} = -0.06$, 95% CI [−0.18, 0.05], $k = 91$, $p < .001$, $I^2 = 43.88$.

Finally, we calculated the overall effect sizes sorted by outcome measure. Four overall effect sizes were small or null. The overall effect size referring to intelligence/reasoning-related measures was significantly negative ($\bar{g} = -0.63$). No influential case was detected. The publication bias analyses estimated values similar to the unadjusted effect sizes. The results are summarized in Table 16.

Lastly, we ran a submodel comprising all the effect sizes referring to the young video game players. The random-effects meta-analytic overall effect size was $\bar{g} = 0.21$, 95% CI [0.06, 0.36], $k = 28$, $p = .007$. The degree of heterogeneity between effect sizes was $I^2 = 26.99$.

The estimates of the selection model analysis were moderately smaller than the unadjusted effect size, $\bar{g} = 0.15$, $\bar{g} = 0.07$, $\bar{g} = 0.19$, and $\bar{g} = 0.15$ for moderate one-tailed selection, severe one-tailed selection, moderate two-tailed selection, and severe two-tailed selection, respectively. No influential effect size was detected. Because of the scarcity of the effect sizes, we did not run any submodels of the outcome measures.

Sensitivity analysis. Like in Meta-Analyses 1 and 2, we checked whether including measures of both RTs and accuracy

Table 13
Meta-Analytic and Publication Bias Results Sorted by Outcome Measure, for the Effect Sizes Referring to Non-Action Video Game Players vs. Non-Video Game Payers (Meta-Analysis 3)

Outcome measure	Model						Model without influential cases			
	k	$\bar{g}$	p value	I^2	T&F	SM	k	$\bar{g}$	p value	I^2
Visual attention/processing	39	.06 [−.06, .18]	.338	.00	.12 [.00, .24]	.02; −.09; .08; .07;	39	.06 [−.06, .18]	.338	.00
Spatial ability	39	.20 [.09, .31]	<.001	.00	.20 [.09, .31]	.14; .06; .18; .15;	37	.21 [.09, .33]	.001	.00
Cognitive control	22	.14 [−.04, .32]	.130	27.34	—	.09; .00; .14; .11;	22	.14 [−.04, .32]	.130	27.34
Memory	40	.17 [.05, .29]	.006	.00	.22 [.10, .34]	.16; .16; .16; .16;	40	.17 [.05, .29]	.006	.00
Intelligence/reasoning	20	.08 [−.09, .25]	.362	.89	—	.01; −.10; .07; .05;	20	.08 [−.09, .25]	.362	.89

Note. See note to Table 10 for abbreviations.

Table 14
Meta-Analytic and Publication Bias Results Sorted by Outcome Measure for the Effect Sizes Referring to Tetris-Like Games (Meta-Analysis 3)

	Model						Model without influential cases			
Outcome measure	k	$\bar{g}$	p value	I^2	T&F	SM	k	$\bar{g}$	p value	I^2
Visual attention/processing	22	−.05 [−.28, .17]	.645	23.13	—	−.19; −.50; −.02; −.01;	22	−.05 [−.28, .17]	.645	23.13
Spatial ability	30	.22 [.08, .36]	.003	.00	.22 [.08, .36]	.05; −.22; .19; .15;	29	.23 [.08, .38]	.003	.00
Cognitive control	5	.19 [−.21, .60]	.352	.00	—	.03; −.26; .19; .15;	4	.36 [−.12, .84]	.140	.00
Memory	11	−.09 [−.39, .20]	.541	.00	—	−.27; −.61; −.07; −.05;	10	−.20 [−.51, .11]	.216	.00
Intelligence/reasoning	4	−.06 [−.68, .56]	.849	.00	—	−.28; −.66; −.05; −.04;	4	−.06 [−.68, .56]	.849	.00

Note. See note to Table 10 for abbreviations.

affected the results. We ran two separate meta-analytic models for effect sizes referring to RTs and accuracy.

Regarding RTs, the random-effects meta-analytic overall effect size was $\bar{g} = 0.09$, 95% CI [−0.05, 0.24], $k = 44$, $p = .200$. The degree of heterogeneity was $I^2 = 7.89$. Thus, considering only RT-related measures did not provide substantially different results compared with the main model ($\bar{g} = 0.09$ and $\bar{g} = 0.07$, respectively). The trim-and-fill analysis filled 10 studies right of the mean. The estimated effect size was $\bar{g} = 0.21$, 95% CI [0.07, 0.35]. The estimates of the selection model analysis were $\bar{g} = 0.06$, $\bar{g} = -0.05$, $\bar{g} = 0.12$, and $\bar{g} = 0.10$ for moderate one-tailed selection, severe one-tailed selection, moderate two-tailed selection, and severe two-tailed selection, respectively. Thus, the two publication bias analyses provided slightly different results, probably because of a false positive in the trim-and-fill analysis. Finally, one influential effect size was detected. The overall effect size without this effect size was, $\bar{g} = 0.06$, 95% CI [−0.07, 0.18], $k = 43$, $p = .379$, $I^2 = .00$.

Regarding accuracy-related measures, the random-effects meta-analytic overall effect size was $\bar{g} = -0.03$, 95% CI [−0.18, 0.11], $k = 44$, $p = .656$. The degree of heterogeneity was $I^2 = .00$. The trim-and-fill analysis filled eight studies left of the mean. The estimated effect size was $\bar{g} = -0.17$, 95% CI [−0.32, −0.02]. The estimates of the selection model analysis were $\bar{g} = -0.16$, $\bar{g} = -0.30$, $\bar{g} = -0.07$, and $\bar{g} = -0.06$ for moderate one-tailed selection, severe one-tailed selection, moderate two-tailed selection, and severe two-tailed selection, respectively. One influential effect size was detected. The overall effect size without this effect size was $\bar{g} = -0.06$, 95% CI [−0.20, 0.08], $k = 43$, $p = .417$, $I^2 = .00$.

Discussion

The main model showed a near-zero effect of video-game training on overall cognitive ability ($\bar{g} = 0.07$). Moreover, this effect was found to be a slight overestimation by the publication bias analyses. The same pattern of results occurred in nearly every submodel, regardless of the type of game—neither action nor nonaction video game training exerted any substantial effect on the participants' cognitive ability—and the age of the participants. The RVE analyses provided similar results (for details, see the supplemental material available online). Finally, the sensitivity analyses did not show any pattern of interest.

A significant exception to the lack of effect of video-game training was the negative effect of video game training on intelligence/reasoning-related measures in the sample of older adults ($\bar{g} = -0.63$). Given the small number of the effect sizes in that model ($k = 15$) and the high degree of heterogeneity ($I^2 = 63.45$), the overall effect size was probably biased. In line with this hypothesis, the homologous RVE analysis provided a nonsignificant result for this model ($\bar{g} = -0.35$, $p = .302$; Table S16). Finally, duration of training was not a significant moderator. This latter outcome is further evidence against the hypothesis according to which video game training affects cognitive ability: if training were effective, one should expect a positive relationship between duration of training and size of the effects.

General Discussion

This paper has addressed the question of the impact of video games on cognitive ability. The three meta-analyses offer a con-

Table 15
Meta-Analytic and Publication Bias Results Sorted by Outcome Measure, for the Effect Sizes Referring to Adult Video Game Players (Meta-Analysis 3)

	Model						Model without influential cases			
Outcome measure	k	$\bar{g}$	p value	I^2	T&F	SM	k	$\bar{g}$	p value	I^2
Visual attention/processing	96	.12 [.03, .22]	.013	.00	.01 [−.10, .12]	.04; −.08; .10; .08;	94	.14 [.05, .23]	.002	.00
Spatial ability	60	.11 [.02, .21]	.021	.00	.06 [−.03, .16]	.05; −.05; .10; .08;	59	.11 [.01, .21]	.029	.00
Cognitive control	36	.02 [−.16, .20]	.842	38.19	.12 [−.07, .32]	−.03; −.15; .03; .03;	36	.02 [−.16, .20]	.842	38.19
Memory	32	.10 [−.03, .23]	.134	.00	.15 [.03, .28]	.03; −.07; .09; .07;	32	.10 [−.03, .23]	.134	.00
Intelligence/reasoning	15	.16 [−.01, .33]	.073	14.49	—	.10; .02; .14; .11;	15	.16 [−.01, .33]	.073	14.49

Note. See note to Table 10 for abbreviations.

Table 16
Meta-Analytic and Publication Bias Results of the Effect Sizes Referring to the Older Video Game Players Sorted by Outcome Measure (Meta-Analysis 3)

	Model						Model without influential cases			
Outcome measure	k	$\bar{g}$	p value	I^2	T&F	SM	k	$\bar{g}$	p value	I^2
Visual attention/processing	26	.00 [−.21, .22]	.965	31.74	—	−.06; −.20; .02; .01;	26	.00 [−.21, .22]	.965	31.74
Spatial ability	12	.06 [−.20, .33]	.646	.00	—	−.03; −.17; .05; .04;	12	.06 [−.20, .33]	.646	.00
Cognitive control	17	.07 [−.15, .29]	.539	9.32	—	−.03; −.16; .05; .04;	17	.07 [−.15, .29]	.539	9.32
Memory	22	−.01 [−.24, .22]	.922	18.87	—	−.12; −.28; −.03; −.02;	22	−.01 [−.24, .22]	.922	18.87
Intelligence/reasoning	15	−.63 [−1.06, −.20]	.004	63.45	—	−.66; −.85; −.57; −.49;	15	−.63 [−1.06, −.20]	.004	63.45

Note. See note to Table 10 for abbreviations.

sistent picture: weak correlations between skill and cognitive ability, small differences between video game players and nonplayers, and no or negligible differences between the participants who underwent video game training and the participants in the control groups. In those few cases reporting slightly greater effect sizes, the estimates of publication bias analysis were significantly smaller (e.g., Visual attention/processing overall effect sizes in Table 7). Crucially, most of the models showed a small (or zero) degree of heterogeneity, indicating that most of the variability between studies was attributable to sampling error (or a few influential cases) rather than some moderating variable. This was particularly evident in the meta-analysis of experimental data ($I^2 = 18.02$ and $I^2 = 10.80$ including and excluding influential cases, respectively). The small degree of heterogeneity often observed in the submodels also suggests that the categorization of the effect sizes into the five outcome measures is highly reliable. Crucially, the outcomes were not sensitive to the type of meta-analytic technique used to model dependent effect sizes. In fact, both Cheung and Chan's (2004) correction and Hedges et al.'s (2010) robust variance estimation method produced the same pattern of results.

The findings of the present meta-analytic investigation differed significantly from the more positive results of previous meta-analyses (e.g., Powers et al., 2013; Toril et al., 2014; Wang et al., 2016). This difference is probably attributable to our more restrictive inclusion criteria and our more accurate procedure for calculating the effect sizes and correcting for statistical dependence. That said, it is worth mentioning that the present meta-analytic investigation also shows that playing certain types of video games may be related to specific cognitive abilities. For example, there seem to be a reliable, yet small, correlation between video game skill and spatial ability. Analogously, video game players show a better performance in RT-related measures than nonplayers. Also, action video game players appear to outperform nonplayers in tasks related to visual attention/processing. Thus, the field of video game playing may present characteristics analogous to other domains of expertise, such as chess and music. Playing video games in general, or some genre in particular, may be associated with specific cognitive abilities predicting, to some extent, a player's skill. However, just like the fields of music and chess, we found no evidence of a causal relationship linking playing video games and enhanced cognition. Rather, it is more plausible that individuals with superior cognitive skills tend to engage and excel in video games.

Theoretical and Practical Implications

Along with substantial research into expertise acquisition and other types of cognitive training, the results of the present meta-analytic investigation point toward a clear direction: while it is evident that training a skill improves that skill, far transfer is extremely unlikely to occur. Video game training is no exception.

The most significant implication of these results is that the lack of generalization across different domains of skills acquired by training appears to be a constant in human cognition. Domain-general cognitive abilities are malleable to training, but the benefits, when any, are domain-specific (Chase & Ericsson, 1982; Gobet, 2016). Moreover, as highlighted by Shipstead, Redick, and Engle (2012), such limited benefits, observed after training, probably represent only trainees' improved ability to perform a task. In other words, after undergoing video game training, people may get better at solving cognitive tasks similar to the training task, and yet not show any genuine improvement in cognitive ability. This account also explains why video game training has sometimes been associated with improvements in particular tasks (e.g., UFOV; Feng, Spence, & Pratt, 2007), whereas no effect has been found in broader cognitive constructs (e.g., visual attention/processing; Table 10).

Second, far transfer must be considered a fundamental litmus test for theories of human cognition. The failure of generalization of skills in the field of video gaming represents a further corroboration of those theories of cognition that predict no (or limited) far transfer, such as chunking theory (Chase & Simon, 1973) and template theory (Gobet & Simon, 1996). More generally, our results support the hypothesis according to which expertise acquisition relies to a large extent on domain-specific, and hence non-transferable, information. By contrast, those theories predicting the occurrence of far transfer after video game training (e.g., "learning to learn;" Bavelier, Achtman, Mani, & Föcker, 2012) and cognitive training in general (for a review, see Strobach & Karbach, 2016) are not supported.

Third, given the small or null effects exerted by video gaming on cognitive tests, the neural changes and patterns observed in video game players in several studies (e.g., Colom et al., 2012) probably reflect modifications in domain-specific abilities (e.g., video game skills) rather than domain-general improvements of cognitive ability. Interestingly, the concurrent presence of specific neural patterns (functional and anatomical) and absence of significant effects on cognitive tests have also been observed in other domains such as music (e.g., Tierney, Krizman, & Kraus, 2015), chess (e.g., Hänggi, Brütsch, Siegel, & Jäncke, 2014), and working memory training (Clark, Lawlor-Savage, & Goghari, 2017). Whether this pattern of results occurs regardless of the domain considered will be a requirement for future research.

Beyond theoretical aspects, the absence of far transfer has important practical implications. If trained skills rarely generalize across different domains, then deliberately training a skill remains the most effective and maybe the only way to acquire that skill. This consideration may appear trivial. However, this conclusion is in contrast with common belief and practice in education and the professions. For instance, considerable emphasis has been given to teaching students transferable skills in recent years (e.g., Pellegrino & Hilton, 2012). However, in light of the findings provided by the research on expertise acquisition and cognitive training, the common view that training and possessing transferable skills is one effective way to progress in a particular field appears incorrect. Our conviction is that educational and professional training should focus on subject-related contents rather than general skills or principles without any explicit reference to any specific discipline.

Recommendations for Future Research

Given the current evidence, insisting on searching for improbable generalized effects of video game training on cognitive function appears pointless. Rather, the field should focus on investigating the exact cognitive correlates of video game expertise. Specifically, further research is needed to understand whether video game players exhibit general superior cognitive ability or excel only at tasks related to video game expertise. For example, chess masters can recall entire chess positions, even when the material is presented only for a few seconds (e.g., Gobet & Simon, 2000). However, the correlation between chess skill and performance on tests of STM is modest ($\bar{r}$ = .22; Burgoyne et al., 2016). Like chess players, video game players may possess exceptional cognitive abilities only with domain-specific material. A series of experiments testing video game players' performance with both domain-general and domain-specific tasks (e.g., recall of video game scenarios) would clarify whether and in what contexts video game players show superior cognitive ability.

Investigating the relationship between video game players' domain-general cognitive skills and preferences for different video game genres deserves some attention too. Video game players are usually categorized according to the video game genre they play the most (e.g., Green & Bavelier, 2003). Conversely, nonvideo game players are often defined as individuals not playing a specific video game genre. However, video game players are often engaged in more than one genre, and nonvideo game players rarely do not play any video game. Therefore, to appropriately account for the complexity of the phenomenon, zero-order correlations are probably insufficient. The research on the correlates of video game expertise should adopt more sophisticated experimental designs. A good example is represented by Redick et al. (2017). In that study, the authors carried out two structural equation models (SEMs) to analyze the relationship between the experience of the participants in different video game genres and working memory, fluid intelligence, and attentional control. Along with controlling for video game players' experience in different genres, using SEMs allows the experimenter to investigate the relationship between video game experience and latent cognitive constructs rather than single cognitive tasks.

With regard to the effects of video game training, research in the field should investigate the relationship between the degree of transfer and trainees' baseline cognitive ability. It may be possible that people with below-average and compromised cognitive ability benefit from video game training more than do people with normal (or superior) cognitive function. The idea is that video game training could slow down cognitive decline in older adults and possibly restore impaired cognitive ability. Although the meta-analysis of experimental studies reported null effects for video game training on the older adults' cognitive ability and thus does not support this hypothesis, the topic probably deserves further investigation, given that no clinical population was included.

Conclusion

Our comprehensive meta-analytic investigation showed that the relationship between cognitive ability and playing video games is weak. Small or null correlations were obtained in the first meta-analysis. The second meta-analysis reported that video game players' overall advantage over nonplayers was modest. Finally, the third meta-analysis found no meaningful effect of video game training on any of the reviewed outcome measures. These findings are in line with substantial research into expertise and cognitive training in domains such as music, chess, WM, and brain training. To date, far transfer remains a chimera.

The generalized absence of far transfer has profound implications. Theories of human cognition predicting (or assuming) the occurrence of far-transfer effects find no support. Conversely, theories predicting no far-transfer effects are corroborated. As for academic and professional education, the lack of far transfer should encourage educators, trainers, and policymakers to implement curricula extensively focused on subject-related material.

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