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Video gaming and working memory: a large-scale cross-sectional correlative study

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Abstract

Studies have indicated that video gaming is positively associated with cognitive performance in select cognitive domains, but the magnitudes of these associations have been called into question, as they have frequently been based on extreme groups analyses that have compared video gamers with non-gamers. When including the whole range of participants, and not just extreme cases, these effects were observed to reduce markedly (Unsworth et al., 2015). To further study this issue, we compared the associations between video gaming and aspects of working memory (WM) performance in an extreme groups design to those of a design that includes the full range of participants in a large adult sample (n = 503). WM was measured with three composite scores (verbal WM, visuospatial WM, n-back). The extreme groups analyses showed that video gamers performed better than non-gamers on all three WM measures, while the whole sample analyses indicated weak positive associations between the time spent playing video games and visuospatial WM and n-back performance. Thus, study design modulated the effects, but two of the three associations between WM and video gaming were consistent across both analysis techniques. A separate study confirmed that our questionnaire-based estimate of gaming hours was reliable when compared with one-week diaries of videogame playing. While the present cross-sectional results preclude causal inferences, possible mechanisms of WM - videogame playing associations and future research directions are discussed. Overall, our results indicate that cognition - videogame playing relationships, albeit weak, are not solely due to recently discussed methodological artefacts concerning the particular analytical approach and survey reliability.

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Keywords

video game; working memory; cognition; playing time; self-report

Introduction

With the growing popularity of video games, there has been an increasing interest in investigating whether and how video gaming (the habit of playing video or computer games) is related to cognitive abilities. One particularly interesting cognitive domain in this respect is working memory (WM), a short-term memory system that is involved in the maintenance and processing of currently active mental information (e.g., Cowan, 2014). Its importance lies in its key position in human cognition: WM is considered to be engaged in every conscious thought process, and it is associated with many important skills and outcomes such as mathematical achievement (Bull, Andrews Espy, & Wiebe, 2008) and fluid intelligence (Conway, MacNamara, & Engel de Abreu, 2013). Moreover, the possibility to use video gaming as a form of cognitive training has also garnished significant interest (Bediou et al., 2018; Sala, Tatlidil, & Gobet, 2018). A potential mechanism for cognitive enhancement through video gaming is provided by the so-called core training hypothesis, according to which repeated strain on a cognitive system induces plastic changes in its neural substrates and thereby leads to performance improvements (Anguera et al., 2013). Another proposed underlying mechanism is learning to learn, according to which video gaming (especially of the action game genre, see below) improves skills such as rule learning, cognitive resource allocation, and probabilistic inference that are used in many different situations (Bavelier, Green, Pouget, & Schrater, 2012).

Several previous cross-sectional studies that have investigated associations between cognition and video gaming suggest that video gamers perform better in various cognitive domains as compared to non-video gamers. Examples of such performance advantages include WM updating as measured by the n-back task (Colzato, van den Wildenberg, Zmigrod, & Hommel, 2013; Moisala et al., 2017), action cascading (i.e., goal-directed multi-component behavior) (Steenbergen, Sellaro, Stock, Beste, & Colzato, 2015), encoding speed of visual information into short-term memory (Wilms, Petersen, & Vangkilde, 2013), visual change detection (Clark, Fleck, & Mitroff, 2011), and multisensory perception and integration (Donohue, Woldorff, & Mitroff, 2010). Narrative reviews indicate that gamers show advantages especially in visuospatial aspects of cognition (Hubert-Wallander, Green, & Bavelier, 2010; Oei & Patterson, 2014), and quantitative meta-analyses have supported these conclusions to some extent (Powers, Brooks, Aldrich, Palladino, & Alfieri, 2013; Bediou, Adams, Mayer, Tipton, Green, & Bavelier, 2018; Sala et al., 2018). It is important to note that the different video games and game genres are not seen as equals in their cognitive demands and in their expected skill-transfer outcomes. Many previous studies have compared action video gamers with non-gamers due to the perceived demanding nature of action video games. Action video games have been described to be fast-paced, to set high perceptual, motor, and cognitive demands, to emphasize peripheral vision and divided attention, and to require constant predictions of future game events (Green & Bavelier, 2012). Action games are typically exemplified by the first-person shooter genre that includes games such as Halo, Doom, and Call of Duty.

Recently, the study designs used to quantify the associations between video gaming and cognition have raised discussion. Unsworth, Redick, McMillan, Hambrick, Kane, and Engle (2015) reported two experiments where they examined the cognitive advantages associated with video gaming with an extreme groups design (group comparison) vs. a whole-group design (regression analysis). When the latter method was used, many of the advantages seen in the extreme groups design were only weak or disappeared completely. This led Unsworth et al. (2015) to argue that the effects in previous meta-analyses have been overestimated because they have included a significant amount of studies comparing extreme groups with small samples and increased likelihood of Type 1 errors (see also Boot, Blakely, & Simons, 2011). Comparing two extreme groups can involve problems such as magnifying minor results and losing important information from the middle of the distribution (Preacher, Rucker, MacCallum, & Nicewander, 2005). However, Green et al., (2017) criticized the videogaming questionnaires used by Unsworth et al. (2015), in which the participants were to estimate the number of playing hours per week per game type. According to Green et al. (2017) this leads to unreliable estimates (especially if participants play multiple game types) that are suboptimal at measuring finer gradations of behavior that are relevant for whole-group analyses (for a response, see Redick, Unsworth, Kane, & Hambrick, 2017).

Given the controversy on the adequacy of the study designs and videogaming questionnaires employed in this field, further research on the relationships between videogaming and cognition is warranted. Here we attempted a systematic replication of the study by Unsworth et al. (2015; see also Redick et al., 2017) by using a somewhat different videogaming questionnaire that we also validated in a separate videogaming diary study. To avoid potential sample- or task-specific confounds, we compared the outcomes of extreme groups vs. whole-group designs with a single large sample by using WM composites derived from a factor analysis of multiple WM tasks performed by the same basic sample (Waris et al., 2017).

In Study 1, we examined cross-sectional associations between WM performance and common video gaming habits in a large adult sample (n = 503), and compared the outcomes of extreme groups analyses (video gamers vs. non-gamers[1]) with whole-sample analyses, both conducted within the same sample. Our self-report video gaming questionnaire had participants estimate their total playing time in hours per week, as well as the percentage of playing time devoted to a set of game types. This differed from Unsworth et al. (2015) and Green et al. (2017), who asked participants to estimate how many hours they spent playing games within certain genres (i.e., estimated genre by genre). A secondary aim was to investigate whether we would observe differential associations between visuospatial vs. numerical-verbal WM and video gaming, as spatial cognition has been shown to be more strongly related to video gaming than verbal cognition (Bediou et al., 2018; Sala et al., 2018). When considering our three WM composites (numerical-verbal WM, visuospatial

[1]In this context, the word “extreme” is used to define the study design, rather than describing a person who plays video games for an extended time each week, and as such displays extreme behavior.

WM, n-back), the core training hypothesis would predict a stronger association between visuospatial WM and video gaming than for verbal WM because video games are a visually dominant media and the cognitive demands of video games are more pronounced in the visual domain (e.g., short-term maintenance of object/enemy/etc. locations that are rapidly updated, tracking multiple moving objects, predicting trajectories, navigating in a virtual world etc.). In the same vein, n-back tasks that require flexible updating of the WM contents should also show a stronger association with video gaming than verbal WM. The learning-to-learn hypothesis would further predict a more general advantage that is not related to any specific domain, but rather depends on how well the skills hypothetically boosted by video gaming can be implemented in a specific WM task. However, it is also important to point out that self-selection or task-specific learning could explain any observed cross-sectional associations.

Furthermore, in Study 2 we evaluated the reliability of our questionnaire on video gamer's self-estimated video gaming time. As noted above, the reliability of video gamers' selfevaluations and thereby the whole-group analysis approach on video gaming - cognition relationships has been called into question (Green et al., 2017). There is surprisingly little empirical evidence on the reliability of self-estimated video gaming time, but thus far the results have been rather disheartening. Greenberg et al. (2005) reported correlations of only .207 (offline gaming hours) and .289 (online gaming hours) between self-reported estimated playing time and diary-reports (diary-reports refers to participants keeping track of their gaming time for a specific time period). However, their estimates and diaries only encompassed single separate days, which makes the measures susceptible to significant temporary variation. Furthermore, Greenberg et al. (2005) did not evaluate the accuracy of the diaries in any way. Similarly, Kahn, Ratan, and Williams (2014) found a correlation of only .365 between estimates of weekly playing time on a single game (EverQuest II) with game log data on playing time. However, as pointed out by the authors, the game log data encompassed the entire existence of the player's game account while the survey question was vague on the time frame (i.e., how many hours the person usually plays), which could result in higher discrepancy if the player had changed playing habits. Furthermore, the game log data, which counted every second a player was logged on to their account, did not apparently distinguish between actual playing time and time when the player was away from the keyboard. This could account for a portion of the discrepancy as players in these types of games (so-called massively multiplayer online games) can gain substantial in-game benefits without actively playing (e.g., keeping the game running over a night) by using automated commands (Ducheneaut, Moore, & Nickell, 2007). Considering the scarcity of research on the topic, we therefore correlated video gamers' estimates of weekly video gaming time in a self-report questionnaire with their diaries of gaming time during one week. The second study also allowed us to test if the number of genres a person plays affects their estimates of time spent playing video games. This point of criticism was made by Green et al. (2017), who reported that subdividing the estimated playing time on multiple genres lead to overestimations of playing time.

Study 1

This study tested whether video gaming was associated with higher WM performance, and whether the possible WM advantages would weaken or dissipate when moving from extreme groups analysis (video gamers vs. non-gamers) to whole-sample analysis (Unsworth et al., 2015). To increase the reliability of our study, we conducted the two analyses within a single sample and employed WM composite measures (rather than single WM task scores) that were derived from a latent variable analysis (Waris et al., 2017).

Methods

Ethical statement

The study was approved by the Joint Ethics Committee at the Departments of Psychology and Logopedics, Abo Akademi University, and by the Human Research Review Board at the University of California, Riverside. Informed consent was obtained from all participants, participation was anonymous, and all participants were informed of their right to withdraw from the study at any time.

Procedure

Every aspect of the study was completed online. Participants were recruited via the crowdsourcing site Amazon Mechanical Turk, and an extensive background questionnaire (concerning, e.g., video gaming habit, medical history, and education) and all WM tasks were administered with an in-house developed web-based test platform. Participants were paid $10 US for completing the entire study, which took 1.5h on average to complete. The background questionnaire was completed first, followed by ten WM tasks (see below). The order of the WM tasks was randomized for every participant with one exception: the forward simple span was always followed by the respective simple span backward.

Participants

711 participants completed the entire study. We excluded 38 participants as they reported using external tools (such as note-taking) to help them solve one or more of the WM tasks. Four participants were excluded for having missing values on one or more of the WM measures and one participant was excluded for taking more than one day to complete the entire study. Next, in order to minimize the effect that depression possibly plays in WM performance (Christopher & MacDonald, 2005; Harvey et al., 2004; Rose & Ebmeier, 2006), we excluded 136 participants who reported a depression score that corresponded to moderate, severe, or very severe depressive symptoms according to the QIDS (Quick Inventory of Depressive symptoms, Rush et al., 2003). Sixteen participants were additionally excluded for having missing depression scale data. Finally, 13 participants were excluded for being multivariate outliers on the WM task variables according to Mahalanobis distance. This gave us a final sample of 503 participants. The mean age of this sample was 34.2 years (SD = 10.6, range: 18-71); the gender distribution was 56.5% female, 43.3% male, and 0.2% other; 53.7% of the sample reported having a Bachelor's or Master's degree; and the mean estimated household wealth during childhood was 3.88 (on a scale from 1, very poor, to 7, very wealthy; see Waris, Soveri, Lukasik, Lehtonen, & Laine, 2018). To test whether

possible prior experience with the WM tasks used here was not an issue in our sample, we ran independent samples t-tests where we compared the WM composite score performances (verbal, visuospatial, n-back, see below) of those who post assessment reported any prior experience with similar tasks (n = 81) with those who reported no prior experience (n = 422). All t-tests were non-significant (verbal WM, $t(501)=1.27$, $p = .204$; visuospatial WM, $t(501)= 0.05$, $p = .962$; n-back: $t(501)=0.03$, $p = .973$), which indicates that this was not an issue in our sample.

Working memory measures

The WM measures are only briefly described here, as further details are provided in Waris et al. (2017). We assessed WM with ten separate tasks that involved four different task paradigms. The task paradigms were simple span (both forward and backward), complex span, running memory span, and n-back. There was one numerical-verbal and one visuospatial variant of each task paradigm (hence 5×2 = 10 tasks). The numerical-verbal tasks used the digits 1-9 as stimuli, while the visuospatial tasks used locations in a 3×3 grid. For the complex span tasks, the distracting items that were placed between every to-be-remembered item consisted of simple arithmetic problems in the verbal task and mental combination of partially filled matrices in the visuospatial task. Accuracy rates were used as outcome measures in all tasks. For the simple, complex, and running memory spans, the number of correctly recalled individual items was used as the outcome variable. For the n-back tasks, the outcome variable was the corrected recognition score, that is, the total number of hits (correct targets) minus the total number of false alarms (no-targets that were incorrectly selected as targets).

We first Box-Cox transformed the WM measures to better approximate normal distributions (Osborne, 2010). Then, following the exploratory factor analysis results of Waris et al. (2017), three WM composite scores were created by summing and then averaging the respective z-transformed WM measures. The composites were (1) numerical-verbal WM that consisted of the verbal simple, complex, and running memory spans, (2) visuospatial WM that consisted of the visuospatial simple, complex, and running memory spans, and (3) n-back that consisted of the verbal and visuospatial n-back tasks. The first two composites allowed us to specifically assess numerical-verbal and visuospatial WM. The Box-Cox and z-transformations were done separately for the whole sample and extreme groups sample.

Video gaming

To assess video gaming habits, the participants were asked whether they regularly played computer, console, or similar games, and how many hours they played on average per week. Additionally, they were asked to evaluate percentage-wise how much they played nine separate game types and whether they typically played it alone (single player) or with others (multiplayer) (see Table 1). The game types were: (1) Card, (2) Mobile, (3) Action, (4) Shooter/First person shooter, (5) Exercise, music, and party, (6) Adventure, puzzle, and role-playing, (7) Simulation, (8) Strategy, and (9) Brain training and educational (see Table 2). We note that these categories are only a rough approximation of the activities performed across individual video game play (e.g. different playing styles, diversity of game-types in a genre, etc., see Dale & Green, 2017) and that a detailed comparison of aspects of game play and WM performance are beyond the scope of the present manuscript.

Our goal was to broadly investigate the associations between the total number of video gaming hours and WM performance; but, considering previous research, we were also interested in exploring associations between specific game types (especially of the first-person shooter genre, see Green & Bavelier, 2012; Green & Seitz, 2015) and WM performance. In order to obtain estimates of the time spent playing each game type, each corresponding percentage was multiplied by the total number of hours played per week. However, prior to this, standardized percentages for each game type were calculated because the total percentage did not always amount to 100 (or sometimes exceeded 100) for all participants. This was done by summing up all percentages and then dividing each individual game type percentage by the individual's total percentage. A discrepancy between gaming hours and percentage play time per game type concerned 51 of our 503 participants, and thus, it was not considered a major issue here.

Participants' missing values on the questions related to how many hours they play video games during a week were marked as zero if they had separately reported that they do not play video games on a regular basis (n=238). If a participant had ticked "No" to the first video gaming question related to regular play, but marked a number on the second question related to the number of hours played, that participant (n = 5) was considered to play regularly, and the reported gaming hours were considered in the analyses.

For *the extreme groups analyses,* we divided the sample into non-gamers (n = 254) who reported playing zero hours per week, and video gamers (n = 143), who reported playing five or more hours per week (in line with Unsworth et al., 2015). As we were mainly interested in general video gaming habits, we did not create subgroups according to different game genres. Thus, our video gamers consisted of a heterogeneous group in terms of the games they played. Note that restricting ourselves to the genre "pure" first-person shooter gamers (i.e., participants who reported playing only shooter games 5 or more hours per week) would have yielded a subsample of only three participants. This indicates that very few people who play video games restrict their gaming habits to the shooter game genre, and that such a category represents a highly select group of individuals.

Statistical analyses

For the extreme groups analyses, we performed separate one-way ANOVAs for each of the three WM composites (comparing video gamers with non-gamers).

For *the complete sample analyses* probing the associations between WM and video gaming, we used hierarchical multiple regression. The three WM composites (verbal, visuospatial, and n-back) served as separate dependent variables. In Step 1, we entered the background variables age, education, and a subjective estimate of wealth during childhood as independent variables that we wanted to control for. In Step 2, we entered the video gaming variable(s). For one set of analyses, we only entered the total number of hours spent playing video games (of any kind), and in a second set of analyses, we entered the nine game type variables. This resulted in six separate regression analyses. The statistical analyses including the hierarchical multiple regression analyses were conducted with SPSS version 25. Bayes factors for the ANOVAs and regression models were calculated with JASP version 0.8.6 (JASP Team, 2018). For the Bayesian regression analyses, the background variables in Step 1 were compared to a null model that only included the intercept. Next, the video gaming variables, which constituted Step 2, were compared to a null model where the background variables were included.

Results

Tables 3 and 4 present descriptive information of the participants' video gaming habits and zero-order correlations between the WM composites and the amount of video gaming for the different game types (see also Supplementary materials).

The extreme groups analyses.

The one-way ANOVAs comparing the WM performances of the video gamers with the non-gamers were significant for all three WM variables: Verbal WM, $F(1, 395) = 9.28, p = .002$, $f^2 = .024$, $BF_{10} = 9.68$; Visuospatial WM, $F(1, 395) = 17.66, p < .001$, $f^2 = .045$, $BF_{10} = 491.06$; N-back, $F(1, 395) = 21.54, p < .001$, $f^2 = .055$, $BF_{10} = 2966.19$. In terms of Bayes factors, there was moderate support for the model involving verbal WM, and extreme support for the models involving visuospatial WM and n-back (Jeffreys, 1961). Overall, the video gamer group outperformed the non-gamers on all three WM variables (see Fig 1).

The complete sample analyses.

The hierarchical multiple regression analyses for the weekly total hours played, showed that Step 1 (age, education, subjective wealth during childhood) was significant for all three WM measures (Table 5). In terms of Bayes factors, the model involving visuospatial WM was strongly supported ($BF_{10} = 34.17$), and the model involving n-back was moderately supported ($BF_{10} = 5.21$), but the model involving verbal WM was not supported ($BF_{10} = 0.14$). Thus, individual background predictors from this last model will not be reported here. Of the background factors, age was negatively associated with both visuospatial WM and n-back performance. Step 2 (hours of video gaming per week) was significant for visuospatial WM and n-back performance, but not for verbal WM performance. With regard to Bayes factors, there was very strong evidence for the model involving visuospatial WM ($BF_{10} = 41.44$) and extreme evidence for the model involving n-back performance ($BF_{10} = 1042.92$), but no support for the model involving Verbal WM ($BF_{10} = 0.48$).

Step 2 for the more specific analyses involving the different game types, was significant for n-back performance only (Table 6). However, Bayes factors supported the null model for Verbal WM ($BF_{01} = 1005.75$) and Visuospatial WM ($BF_{01} = 86.01$), and provided only anecdotal support to the model involving N-back ($BF_{10} = 1.20$).

Discussion

In Study 1, we investigated the associations between videogaming habits and WM performance by using two different study designs, a videogaming questionnaire, and three WM domain measures. The comparison of the extreme groups (group comparison) vs.

whole-group (multiple regression) study designs was prompted by a current controversy on the adequacy of these setups (Unsworth et al., 2015; Green et al., 2017, Redick et al., 2017).

Our results indicated that in the extreme groups design, video gamers consistently outperformed non-gamers on all three WM measures (verbal, visuospatial, n-back). Notice, however, that the effect sizes (Cohen's f^2) indicate that the group differences were only small (the mean differences in standard scores between the groups varied from 0.24 to 0.42). In comparison, when all participants were included in a set of regression analyses, the number of hours spent playing video games during a week was positively associated with visuospatial WM and n-back performance, but not with verbal WM performance. The weekly amount of gaming explained an additional 2.1% of the variance in visuospatial WM performance and 3.4% of the variance in n-back performance when controlling for age, education, and estimated household wealth during childhood. Thus, the relationship between number of hours spent playing and WM performance (visuospatial & n-back) was statistically significant, but weak. With regard to different game genres, bivariate correlations suggested that the Mobile, Action, Shooting, and Adventure game types were associated with the WM measures to some degree (Table 4). However, in a separate set of regression analyses where the nine different game types served as predictors, only the model involving n-back was statistically significant, but it was only anecdotally supported by the Bayes factor. Therefore, while there is some support for the notion that games with "action" elements are specifically related to some advantage in cognitive performance (Green & Bavelier, 2012), no strong conclusions can be drawn on the basis of our results, also considering the limitations of our game categories discussed further below. Overall, our results are broadly in line with previous studies that have linked video gaming to predominantly visual aspects of cognitive processing (Bediou et al., 2018; Sala et al., 2018) as well as n-back performance (Colzato et al., 2013). The results are further discussed in the General Discussion section.

Study 1 had some limitations that should be mentioned. The questions related to the participants' video gaming habits used in our study could have been more precise, and the division into game genres was likely not optimal: some of the game examples might have fit better into other categories than the ones provided, and some games contain elements from multiple genres. Also, our Mobile game category was very broad as it could basically encompass any kind of game type from the other genres (Action, Card etc.), which could have influenced the results of our regression analyses. In hindsight, a potentially better option would be to remove it as a game category and instead probe for each remaining genre whether it was typically played on a mobile device. We probed the time spent on each specific game type in percent, which possibly reduces overestimations, but the final estimates could be partly incorrect for some individuals. However, these participants represented only 10% of the whole sample, and removing them from the analyses does not change the main pattern of results[2]. Second, we did not probe lifetime gaming experience and our questions were somewhat vague about the time reference. More specifically, it was

[2]We repeated the extreme groups and complete sample analyses using subsamples where the 51 participants with discrepant reports of gaming hours per week and distribution between game types were removed. For the extreme groups analyses (n = 356), the results of the one-way ANOVAs were roughly the same as with the slightly larger sample. For the whole sample analyses (n = 455), the results

not explicitly stated for how long a person should have had to play video games to be counted as “regularly”. Hence, we cannot rule out the possibility that, for example, some of our current non-gamers had a history of playing video games. With regard to the WM battery, as discussed by Waris et al. (2017), a potential limitation is represented by the verbal complex span that was strongly negatively skewed, which could have impacted the results. Further, as every aspect of our study was completed online, the experimenter’s control was very limited, which could increase error variance in WM task performance due to e.g., differing testing conditions. Finally, a key concern for this study and the research field as a whole, is the reliability of participants’ self-reported gaming hours (Green et al., 2017; Redick et al., 2017). Therefore, we investigated this issue in a separate study.

Study 2

In this study, we evaluated the reliability of video gamers’ self-estimated video gaming time as assessed by the questionnaire that was used in Study 1. Concerns about the reliability of such questionnaires were raised by Green et al. (2017) and discussed by Redick et al. (2017). To address these concerns, we collected new data where a sample of video gamers first filled out our self-report questionnaire asking them about their video gaming habits, and then they were asked to keep a diary on their playing times for the following week. Furthermore, we also tested whether the number of game genres participants reported playing affected their estimates of gaming time (see Green et al. 2017, for this argument).

Methods

Ethical statement

We did not apply for separate ethical permission for Study 2, as, compared to Study 1, it entailed substantially less effort from participants and included no questions on, e.g., medical history. Informed consent was obtained from all participants, participation was anonymous, and all participants were informed of their right to withdraw from the study at any time.

Procedure

Every aspect of the study was completed online. Participants were recruited via the crowdsourcing site Prolific (https://www.prolific.ac/). We used Prolific’s built-in pre-screening tool to target participants who had an interest in video gaming. That is, we restricted the participant pool to only those who had selected “Video games” on the pre-screening question: “*Which of the following categories of hobbies are you strongly interested in? Please choose up to THREE (3) categories - the ones you're most interested in*:”. This gave us a pool of 11,598 potential participants. In the first part of the study, participants completed a short survey, where we probed age, gender, and education (highest attained degree), as well as details regarding their video gaming habits and history of gaming. The time spent playing video games was assessed with the same video gaming

of the regression analyses were also roughly the same as with the larger sample, but here the model involving n-back and the nine game type predictors was significant and also supported (very strong) by the Bayes factor, $\Delta F(9, 442) = 3.87, p < .001$, $\Delta R^2 = .071$, $\Delta f^2 = .076$, $BF_{10} = 47.49$.

questionnaire as in Study 1 (whether the participant played regularly, the number of hours of play time during a typical week, and percentage of playing time devoted to the nine game types). In the second part of the study, participants kept a week-long diary of their daily gaming time (hours and minutes for each game for each day) that they then reported in a separate survey. The second survey also included questions related to the reliability of their diary (see below). Participants were paid £2.5 for completing the study.

Participants

Out of 123 participants who completed the first part[3], 117 also completed the second part of the study. To examine how data quality affected the results, we analyzed the data using three participant samples with differing degrees of data quality. In this context, data quality mainly refers to how systematically participants' reported keeping their diaries and how well they perceived that the diary-week corresponded to a typical week (concerning video gaming). The full sample consisted of all 117 participants who completed both parts of the study. The second sample (n = 87/117) employed inclusion criteria that were liberal. It included participants whose video gaming diaries included no days that contained six separate games (n = 2, our questionnaire only allowed six separate games per day, and therefore we could not know if a person actually had played more games) and who reported that: (i) they played video games regularly, (ii) the week during which they kept the diary was within +/− 4h of a typical week concerning their gaming time, (iii) the diary was at least to some extent accurate (i.e., participants might have skipped their diary-keeping on some days and tried to recall their gaming on these days on a later date), and (iv) they responded that they had honestly estimated their gaming time. The third sample (n = 47/117) employed strict inclusion criteria. It used the same criteria as the more liberally selected subsample described above, but here the week during which they kept the diary was to match a typical week (i.e., no reports of subjectively perceived deviations in total gaming time), and only those who reported that they kept their diary consistently on a day-to-day basis (i.e., no skipped days of diary-keeping) were included. The full sample and the two subsamples were relatively equal in a set of background variables and their estimates of time spent playing video games (see Table 7). Approximately 60% of participants (65.0%, 59.8%, and 57.4% in the full, liberally, and strictly selected samples, respectively) reported that their estimated weekly gaming time covered a timeframe of more than three years.

Statistical analyses

The reliability of the participants' estimated weekly gaming time was tested by correlating the initial questionnaire-based estimates of weekly gaming time with the gaming time obtained from the week-long diary. To test whether the number of game genres played affected the discrepancy in gaming time as measured by the questionnaire vs. the diary, a separate set of bivariate correlations were run for the three samples. If the number of genres affects estimates of gaming time, with less reliable estimates being presented by participants who play games of several genres, there should be a positive correlation between the extent of discrepancy and the number of genres played.

[3]We intended to recruit 120 participants, but due to a technical issue three additional participants took part in the study.

Results

The bivariate correlations between the participants' questionnaire-based video gaming time and their diary-based reports are presented in Figure 2, together with some further details on the associations. The association between the number of game genres a participant played during a typical week and the discrepancy between the questionnaire-based and diary-based weekly video gaming time is presented in Figure 3.

Discussion

The statistically significant positive bivariate correlations between the video gamers' estimates of typical weekly gaming time in the questionnaire and their gaming time according to a week-long diary indicate that video gamers are quite accurate at estimating their typical weekly gaming time. This was especially true for the strictly selected subsample where the data quality of the participants' diaries was highest ($r = .788$). One might obtain even higher correlations with a diary covering a longer time interval, as there is undoubtedly temporary variation in the diaries that covered only a single week. Our results contradict previously reported low correlations (Greenberg et al., 2007; Kahn et al., 2014) and Green et al.'s (2017) argument that the reliability of video gamers' self-estimates is too poor to make finer gradations. However, the correlations were not perfect, and it is a matter of debate of how high the correlation should be (and how low the discrepancy) for the estimates to provide adequate information of actual gaming time.

A limitation of the present study is the fact that we used another self-report measure (i.e., the diaries) to verify our questionnaire-based estimations of gaming time (see Prince et al., 2008, for a relevant discussion concerning physical activity). As the diaries do not provide objective data on gaming time, they could also be subject to biases, for example, if the respondent feels embarrassed or ashamed of the behavior, has an inherent difficulty in tracking time, or if the diary keeping affects the behavior in some way. A more stringent approach would call for objective indicators, such as tracking gaming time with computer logs, by video monitoring, or by stopwatch. However, as discussed in the Introduction, computer logs do not necessarily provide exact data either, video monitoring faces a challenge in tracking playing time on mobile devices, and a stopwatch relies on accurate timekeeping by the participant, which is difficult to verify by the researcher. Any of these methods could also affect the behavior of the person being monitored, if the person is aware of it.

The number of game genres a participant reported playing in the questionnaire was not related to the discrepancy between the questionnaire-based vs. diary-based weekly video gaming time. This goes against the results of Green et al. (2017) who argued that playing games of multiple genres hampers players' estimations of total gaming time. This contradictory finding is likely related to two significant differences between our study and that of Green et al. (2017), namely how discrepancy was measured and how participants estimated their video gaming time. Green et al. (2017) evaluated discrepancy by comparing estimated total gaming time (i.e., estimated weekly gaming time) with a summed estimate of genre-based estimates (i.e., genre 1 estimated gaming time + genre 2 estimated gaming time etc.). We, on the other hand, evaluated discrepancy by comparing estimated total weekly gaming time with diary-based weekly video gaming time. The discrepancy measures were therefore different. Concerning the questions themselves, Green et al. (2017) asked participants to evaluate how many hours they spent playing games belonging to different genres (and summed these for a weekly total), while we asked participants to first estimate their total weekly gaming time in hours and then indicate in percent how much of that time was spent playing games belonging to nine different genres. Considering our diary results, it seems clear that asking participants to separately evaluate gaming time in hours for different genres from which a total is then calculated is not an optimal way of obtaining reliable estimates of total gaming time (unless the person only plays games of one genre). We therefore recommend using our style of questions if gaming time is evaluated with self-reports (i.e., estimate of total gaming time in hours followed by percentage-wise distribution of gaming time[4], or distribution of the total hours, on separate genres).

General discussion

Study 1 confirms the observation made by Unsworth et al. (2015) that the choice of analysis approach can play a significant role in the outcomes of studies that address cognition - video gaming relationships. Nevertheless, we did find statistically significant effects for visuospatial WM and n-back using both extreme groups and whole-group analysis techniques. While Unsworth et al. (2015) raised the issue that previous extreme group analyses with relatively small sample sizes have probably inflated the differences, the ensuing discussion indicates that the question is more complex (Green et al., 2017; Redick et al., 2017; Sobczyk, Dobrowolski, Skorko, Michalak, & Brzezicka, 2015). For example, the two setups operate on partly different data, the statistical models are different (univariate ANOVA vs. multiple regression), the distribution of video gaming habits in a given sample can modify the extreme groups vs. whole-group results, game type classifications are problematic, and the questionnaire data may be less accurate when analyzed at the game type level. Our findings call for future research to implement more sophisticated methods to classify games and game genres, to use objective measures to assess the time spent playing video games (e.g., computer logs), more advanced linear mixed effects models that account for both subject- and item-based variation, as well as to include large-scale samples that provide sufficient variation in the critical variables.

The results from Study 2 indicated that the relative weakness of video gaming - WM associations was not due to inaccurate estimation of average video gaming time by our questionnaire (see the criticism by Green et al., 2017). Thus, it seems that our particular question format runs a lesser risk for producing unreliable estimates as compared to that of, for example, Green et al. (2017). This is an important methodological point, also given the scarce previous findings indicating low reliability of self-estimations of videogaming time (Greenberg et al., 2005; Kahn et al., 2014). At the same time, it is clear that a simple question on the average number of videogame playing hours per week does not necessarily

[4]Note that the percent-wise distribution of gaming time on different game genres did not always amount to 100% for all participants, i.e., the percent-wise distribution did not function flawlessly. For 88.8% of participants (missing data for one participant) the total amounted to 100%, while for the rest, the total subceeded (7.8%) or exceeded (3.5%) 100%. A survey that has a built-in requirement for the total to equal 100% would negate this issue.

reflect an individual's lifetime history of videogame playing that may show considerable variation from time to time.

The weak positive associations between video gaming and aspects of cognitive performance irrespective of study design, raises the question of the origins of these associations. Does video gaming improve certain cognitive abilities or are people with certain cognitive strengths more prone to become video game players? Naturally enough, our cross-sectional study does not enable us to draw any causal inferences. We cannot rule out the possibility that the observed associations in Study 1 could reflect a self-selection process where individuals who possess certain cognitive strengths that are beneficial for gaming performance are more likely to enjoy and play video games. Neither can we discard the possibility that the small advantage could reflect more general aspects such as computer habit and know-how, fast internalization of computer task instructions, and/or, as discussed by Boot et al. (2011), shifts in strategic approaches to such tasks. As regards causality, one needs to turn to intervention studies, but the current literature is not consistent concerning the observed training effects. The two most recent meta-analyses report different overall effect sizes for the intervention studies showing either small to moderate ($g = 0.34$; Bediou et al., 2018), or no effects of playing video games on cognition ($g=0.07$; Sala et al., 2018). Thus, these meta-analyses draw opposite conclusions: Bediou et al. (2018) conclude that video gaming enhances cognition while Sala et al. (2018) state that there is a lack of a causal relationship. The reasons for these discrepancies could lie in differences in study and/or outcome variable selection and in the applied meta-analytic methods. However, both studies reported a probable publication bias that suggests that the effects are most likely overestimated. Moreover, both studies failed to find a dose-response relationship, i.e., that the training effect would be moderated by the amount of video game training, which would seem to weaken possible causal claims. It is, however, important to acknowledge that the intervention studies have typically been conducted on adults only, and that the amount of training is minimal when compared to the lifetime experience of gaming that many active gamers have accumulated. Thus, the crucial issue on the origins of videogaming - cognition associations remains open, and longitudinal, large-scale and methodologically more stringent intervention studies are needed to address it.

In conclusion, our results indicate associations between video gaming and aspects of WM performance as assessed in a cross-sectional design. The strength of these associations was related to the particular study design, with stronger effects observed with the extreme groups design. Thus, regarding the debate between Unsworth et al. (2015) and Green et al. (2017), our data provide evidence consistent with each of their approaches, and suggest that future studies should report both the distributional and extreme cut-off analyses to provide a more balanced view of the data. Relying only on the extremes precludes an understanding of the fuller population, while ignoring the extremes overlooks important individual differences and the possibility that estimations of video game play can suffer from systematic inaccuracies, that games are highly diverse in how they load onto cognition, that effects may accumulate over time, and that clustering players into groups is likely more sensitive than relying upon linear, continuous measures that make potentially inappropriate distributional assumptions. That being said, our second study suggests that video gamers are relatively accurate at estimating their average weekly gaming times precluding this argument from the list of objections against whole-group analyses in this context.

Supplementary Material

Refer to Web version on PubMed Central for supplementary material.

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Highlights:

Video gaming and working memory: a large-scale cross-sectional correlative study

- Video gaming is associated with working memory performance in our cross-sectional design
- Study design modulated the strength of the associations
- An extreme groups design yielded stronger effects as compared with a whole-group design
- People are relatively accurate at estimating their video game playing time

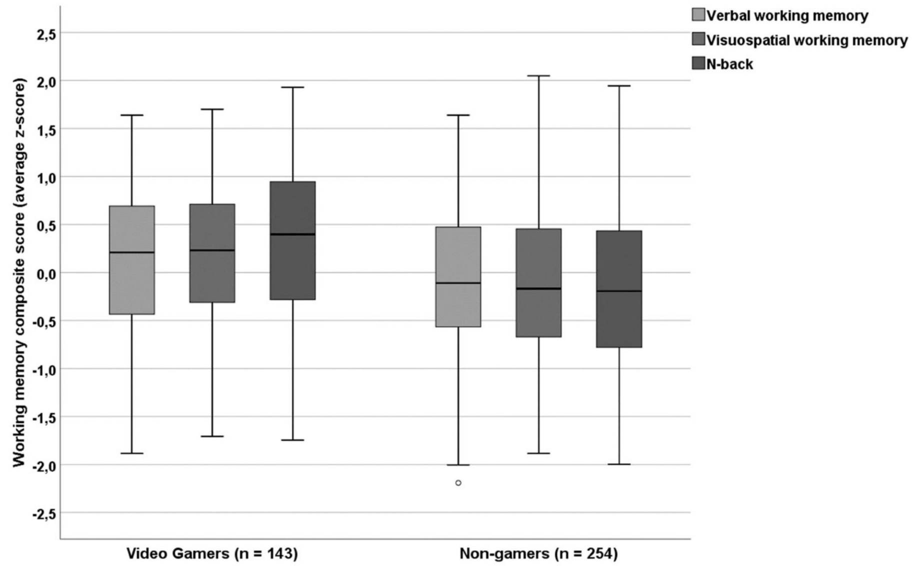

Figure 1.

Video gamers' and Non-gamers' performance on three working memory composites (the circle marks one outlier in the Non-gamer group that was included in the statistical analyses). Whiskers mark minimum and maximum values (except the single outlier).

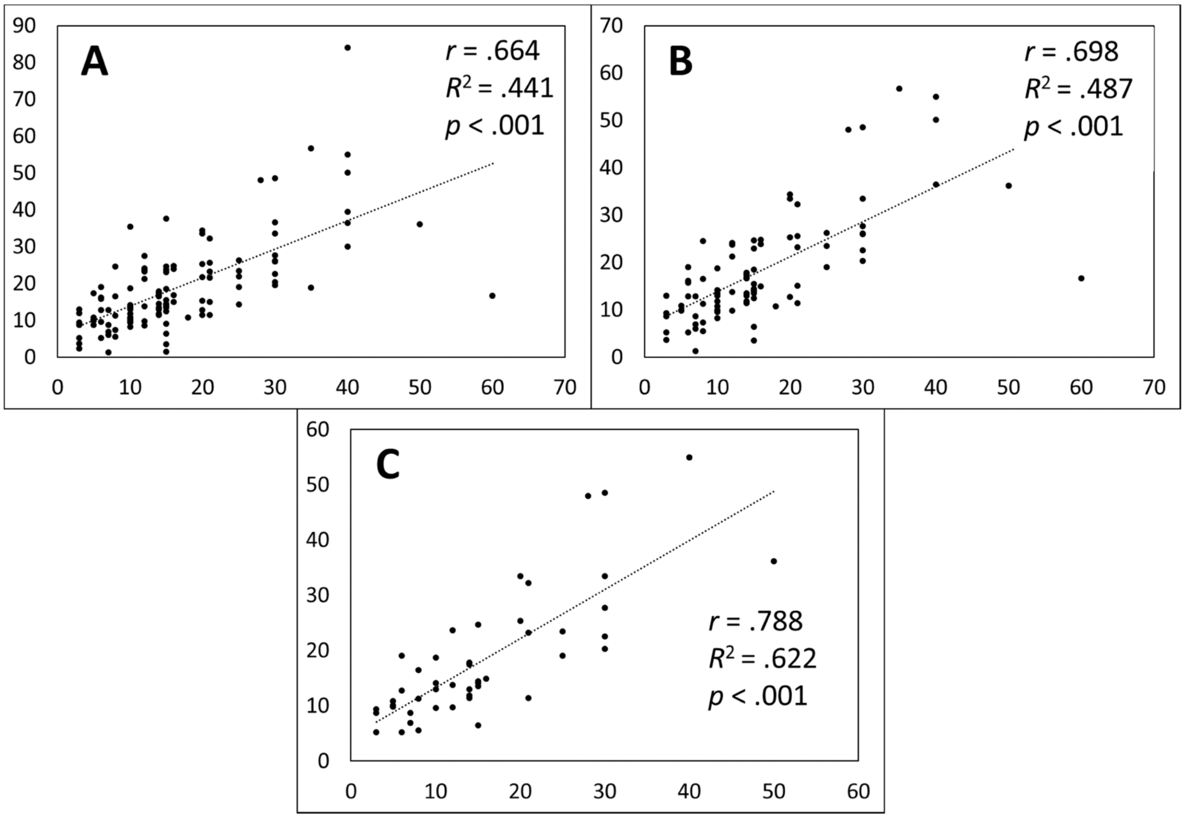

Figure 2.

In all panels, the x-axes represent the estimated total hours of weekly video game play, and the y-axes represent the hours of video game play during a week according to the participants' gaming diaries. The dotted lines represent the fitted linear associations. Panel A depicts the association for the full sample (n = 117) where the average difference between the questionnaire and diary data on total gaming time was 6.9 h (*SD* = 7.3 h). Panel B depicts the association for the liberally selected subsample (n = 87) where the average difference between the questionnaire and diary data on total gaming time was 6.2 h (*SD* = 6.4 h). Panel C depicts the association for the strictly selected subsample (n = 47) where the average difference between the questionnaire and diary data on total gaming time was 5.8 h (*SD* = 5.0 h).

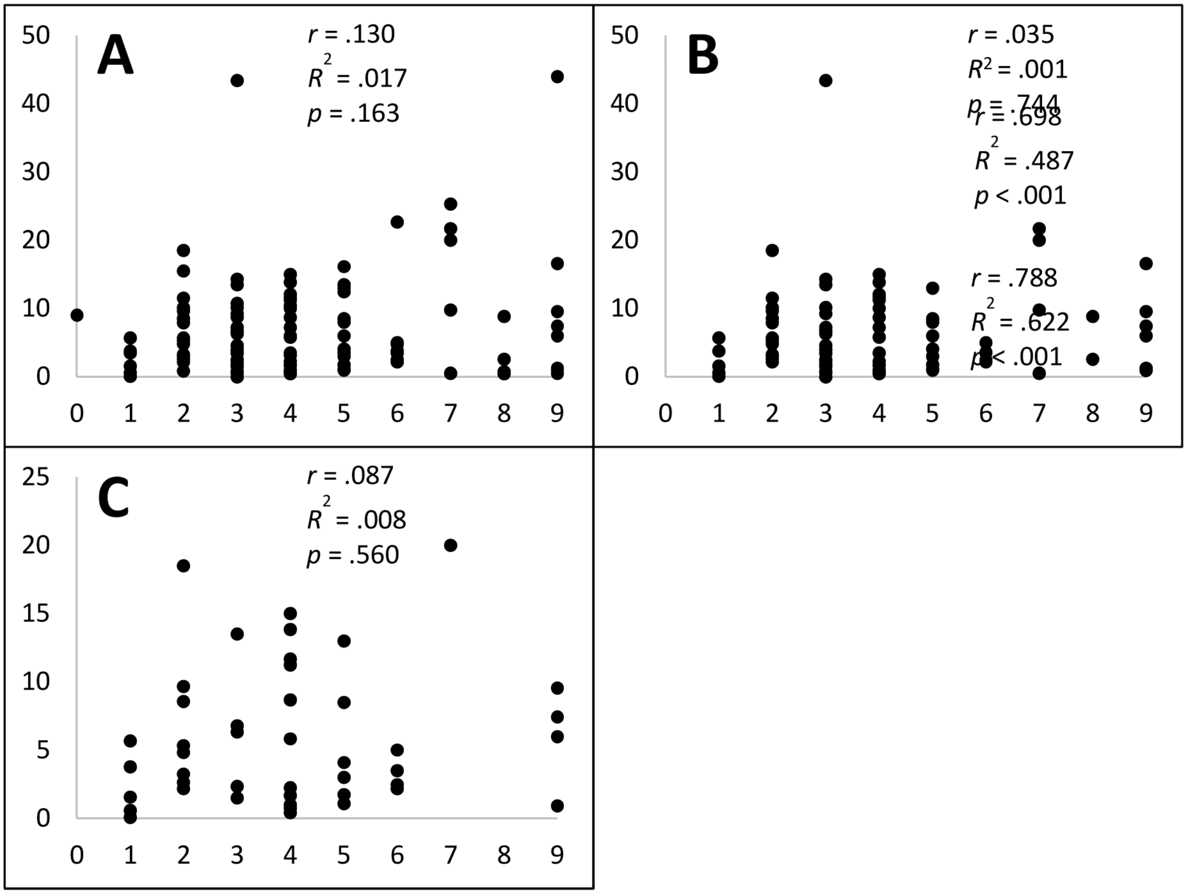

Figure 3.
Scatter plots showing the associations between the number of played game genres reported in the questionnaire (x-axis) and the discrepancy (in hours) between the questionnaire-based and diary-based weekly video gaming time (y-axis). Panel A shows the association for the full sample (n = 117), Panel B for the liberally selected sample (n = 87), and Panel C for the strictly selected sample (n = 47).

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Table 1

Video gaming items and response alternatives in the background questionnaire.

Questionnaire Item	Response alternative
Do you regularly play computer, console or other similar games?	Yes/No
If yes, how many hours a week do you play these types of games (on average)	0-150
What percentage of this time do you spend playing the games mentioned below (adds up to 100%). Please indicate also if you usually play the particular type of game alone ("Single player") or with others ("Multiplayer").	0-100 (per game type)

Table 2

Video game types and example games (provided in the questionnaire) for each game type.

Game type description	Game examples
Card games and poker	Slots, Internet poker, Windows Solitaire
Mobile games	Snake, Angry Birds, Tetris, Candy Crush Saga
Action games (for example, fighting games, car games, platformers)	Mortal Combat, Super Mario Galaxy, Tomb Raider, NFL/NHL/FIFA, Gran Turismo, MOBA; World of Warcraft
Shooter / First Person Shooter games	Call of Duty, Battlefield, Halo
Exercise, music and party games	Wii Sports, SingStar, GuitarHero, DanceCentral
Adventure, puzzle and role play games	Monkey Island, Elder Scrolls, Portal
Simulation games	Flight Simulator, IL-2 Sturmovik, Silent Hunter
Strategy games	Civilization, Total War, Command and Conquer, SimCity
Brain training and educational games	(No examples provided)

Table 3

Number of participants who reported playing different types of video games (> 0h per week) together with descriptive information of their video gaming habits.

Game type	Players	Range, h/week	Average h/week (SD)
Total per week	249	1-71	8.4 (8.8)
Card games and poker	59	0.1 – 10.0	1.8 (2.3)
Mobile games	147	0.1 – 20.0	2.8 (3.4)
Action games	106	0.2 – 42.6	3.5 (5.5)
Shooter / First Person Shooter games	96	0.03 – 35.5	3.6 (4.7)
Exercise, music and party games	27	0.04 – 7.1	1.1 (1.6)
Adventure, puzzle and role play games	120	0.02 - 40	3.4 (5.8)
Simulation games	39	0.1 - 15	2.4 (3.4)
Strategy games	88	0.1 - 21	2.7 (3.5)
Brain training and educational games	60	0.1 – 7.1	1.3 (1.3)

Table 4

Zero-order correlations (Pearson's *r*) between the working memory measures and gaming frequency (hours/week).

	Verbal WM	Visuospatial WM	N-back	Total h / week	Card	Mobile	Action	Shooting	Fitness	Adventure	Simulation	Strategy	Brain training
Verbal WM	-												
Visuospatial WM	.556**	-											
N-back	.414**	.562**	-										
Total h / week	.049	.149*	.191**	-									
Card	-.007	.052	.072	.298**	-								
Mobile	.105*	.088*	.0153**	.364**	.125**	-							
Action	.016	.112*	.127**	.657**	.223**	.089*	-						
Shooting	.052	.104*	.074	.490**	.019	−.005	.207**	-					
Fitness	−.014	.014	.024	.343**	.285**	.110*	.460**	−.014	-				
Adventure	−.015	.090*	.112*	.578**	−.016	.036	.142**	.109*	.000	-			
Simulation	−.024	.001	.066	.292**	.032	−.027	.096*	.137**	−.009	.066	-		
Strategy	.039	.016	.038	.397**	.031	−.014	.079	.080	.021	.177**	.131**	-	
Brain training	−.033	.038	.013	.340**	.209**	.098*	.351**	−.030	.458**	.050	.062	.084	-

Note. $N = 503$. WM = working memory.

* $p < .05$,

** $p < .01$

Table 5

Hierarchical regression analyses for variables predicting three separate working memory measures.

	Verbal WM					Visuospatial WM					N-back				
Predictor	**Δ*F***	**Δ*R*²**	**Δ*f*²**	***β***	***B***	**Δ*F***	**Δ*R*²**	**Δ*f*²**	**β**	***B***	**Δ*F***	**Δ*R*²**	**Δ*f*²**	**β**	***B***
Step 1	2.71*	.016	.016			6.67***	.039	.041			5.32**	.031	.032		
Age				−.073	−.005				−.187***	−.014				−.172***	−.014
Education				.110*	.062				.072	.041				.035	.023
Subjective wealth during childhood				.008	.005				.024	.015				.032	.022
Step 2	1.71	.003	.003			11.32**	.021	.021			18.25***	.034	.035		
Video gaming: h per week				.059	.006				.148**	.015				.187***	.022

Note. $N = 503$. WM = Working memory.

* $p < .05$.

** $p < .01$.

*** $p < .001$.

Table 6

Hierarchical regression analyses for variables predicting three separate working memory measures.

	Verbal WM					Visuospatial WM					N-back				
Predictor	ΔF	ΔR^2	Δf^2	β	*B*	ΔF	ΔR^2	Δf^2	β	*B*	ΔF	ΔR^2	Δf^2	β	*B*
Step 1	2.71*	.016	.016			6.67***	.039	.041			5.32**	.031	.032		
Age				−.073	−.005				−.187**	−.014				−.172***	−.014
Education				.110*	.062				.072	.041				.035	.023
Subjective wealth during childhood				.008	.005				.024	.015				.032	.022
Step 2	1.08	.019	.019			1.82	.031	.032			2.97**	.050	.053		
Card				−.014	−.011				.043	.034				.055	.050
Mobile				.109*	.037				.071	.025				.139**	.055
Action				.026	.007				.083	.022				.100	.030
Shooter				.058	.018				.067	.021				.016	.006
Exercise, music, party				−.015	−.025				−.065	−.112				−.048	−.093
Adventure, puzzle, roleplaying				−.036	−.009				.063	.015				.083	.023
Simulation				−.024	−.016				−.013	−.009				.056	.043
Strategy				.054	.023				.000	.000				.015	.007
Brain train, educational				−.028	−.034				.052	.066				−.008	−.012

Note. $N = 503$. The predictors in Step 2 involve hours played per week of each video game type. WM = Working memory.

* $p < .05$.

** $p < .01$.

*** $p < .001$.

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Table 7

Descriptive information of the participant samples.

	Full sample (n = 117)	Liberal inclusion (n = 87/117)	Strict inclusion (n = 47/117)
Age *M* (*SD*)	28.5 (7.6)	29.0 (7.7)	29.5 (7.6)
Gender (% male)	77.8	74.7	72.3
Education (%)			
Upper secondary	34.2	33.3	36.2
University: Bachelor's	29.5	31.0	21.3
University: Master's	17.9	19.5	19.1
Other	18.9	16.0	23.5
Years of video gaming *M*(*SD*)	18.1 (6.8)	18.8 (6.7)	19.4 (6.8)
Video gaming: h / week *M*(*SD*)	16.6 (10.8)	16.2 (10.8)	15.7 (10.3)