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The effects of video game training on the cognitive functioning of older adults: A community-based randomized controlled trial

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ABSTRACT

Context: Using video games may enhance older adults' cognitive skills, including executive function, processing speed, and spatial ability.
Objective: We examined the impact of video game training on the cognitive functioning of community-dwelling adults aged 65 or older and tested the hypotheses that larger training effects would be uncovered for practiced measures and that the employed time-compressed approach would reveal effects comparable to those reported in prior studies on this approach.
Methods: Thirty-five participants from four Senior Centers located in Los Angeles County, California completed the study. Participants were randomly assigned to either an intervention group partaking in 15 h of supervised video game training over five weeks or to a control group completing an assessment battery before and after a five-week period.
Results: After statistically controlling for pretest performance and performance on the Memory Alteration Test, we found significant group differences regarding brief syllable count ($p = .001$, $d = 1.28$) and arithmetic assessments ($p = .003$, $d = 1.10$), as well as marginally significant differences on the Stroop Interference Test ($p = .02$, $d = 0.89$). We also found larger effects among practiced outcome variables ($d = 0.72$) than non-practiced outcome variables ($d = 0.03$); the effects were comparable to those reported in time-extended intervention studies ($d = 0.35$ and 0.36, respectively).
Conclusions and implications: Results suggest that playing an easily accessible video game in older age can enhance cognitive functioning, especially in areas directly tied to the video gaming activities.

1. Introduction

In the present study, we employed an experimental paradigm designed to examine the impact of a video game – a commercially available game purported to enhance cognitive skills – on the cognitive functioning of older adults without major health conditions. Indeed, video games requiring players to make use of the same skills that people utilize while playing a game such as Candy Crush Saga could improve cognitive functioning, particularly in older age (e.g., Ackerman, Kanfer, & Calderwood, 2010; Anguera et al., 2013, Basak, Boot, Voss, & Kramer, 2008; Maillot, Perrot, & Hartley, 2012; McDougall & House, 2012). In early research in this area, non-institutionalized older experimental participants who played a videogame for two months twice a week in 1.5-h sessions achieved significant positive gains on the Wechsler Adult Intelligence Scale – Revised (WAIS-R; $d = 0.89$) and eye-hand coordination ($d = 1.02$; Drew & Waters, 1986). Moreover, among community-dwelling older individuals residing in Holland (aged 69 to 90), those who trained at home for 5 h per week for 5 weeks on Nintendo's Super Tetris improved their scores on Card Sorting Tasks (White & Cunningham, 1987), in contrast to those not receiving training ($d = 1.75$; Goldstein et al., 1997). Furthermore, in a randomized pilot study on cognitively impaired older adults, respondents who played a computer game designed to enhance cognitive rehabilitation for a period of 12 weeks (3 weekly 20 min sessions) exhibited less cognitive impairment on the Mini-Mental State Examination (Folstein, Folstein, & McHugh, 1975) immediately after the 12-week intervention period ($d = .32$) and at a 12-week follow-up ($d = 0.47$; Tarraga et al., 2006). More recently, older adults who received the 20 one-hour game training sessions of a web-based brain-training package, Lumosity, demonstrated a significant decrease in distraction ($d = 0.77$) and a significant increase in alertness, unlike the control group ($d = 0.82$; Mayas, Parmentier, Andrés, &

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Ballestros, 2014). Although the benefits of using video games in older age are well documented, is it possible to transfer these new skills to non-practiced tasks? The next paragraph succinctly covers this issue.

1.1. The rationale and empirical evidence regarding transferability of cognitive skills

A central issue pertinent to the discussion of video games and cognitive functioning is the extent to which the skills that older adults acquire when playing a game are transferable to other tasks not directly practiced in the game (Hertzog, Kramer, Wilson, & Lindenberger, 2009). If this is the case, it has repercussions on the generalizability of the benefits derived from playing the game. For example, to experience broad gains in cognitive functioning, perhaps an individual would have to potentially engage in an assortment of mentally stimulating tasks and/or games. If the generalizability of the skills acquired via games is limited, that would suggest that video games have only a superficial impact on cognitive abilities. In this scenario, rather than affecting core underlying cognitive mechanisms with broad-based gains in various cognitive skills, games may simply serve as tools to sharpen very specific skill sets via extensive practice. On the other hand, if games are found to have a broad-based impact on cognitive functioning, with specific gaming activities associated with gains on an array of cognitive outcomes − whether practiced or not − then such findings would lend credence to the notion that games can serve as robust tools for improving cognitive functioning.

Current empirical evidence in the context of video game interventions does support the transferability hypothesis among older adults. For instance, training for 15 sessions of 1.5 h each over 4 to 5 weeks, in a real-time strategy computer game (i.e., an adapted version of a commercially available game called Rise of Nations), led to posttest improvements across wide-ranging cognitive skills like task switching ($d = 1.02$), working memory ($d = 0.82$), and reasoning ($d = 0.55$; Basak et al., 2008). Similarly, in a study in which the video game training group played several Nintendo Wii games requiring physical exercise on the part of the gamer, after 24 one-hour gaming sessions, trainees demonstrated significant gains across host of cognitive performance measures (11 of the 18 measures yielded *ds* of 0.50 or greater), many of which were not directly practiced in the Wii game (Maillot, Perrot, & Hartley, 2012). Using a specially designed video game and a month-long intervention paradigm, Anguera et al. (2013) also found gains in cognitive abilities such as sustained attention and working memory that were not specifically trained via the game (*ds* range from 0.75 to 0.98). What these three studies have in common is that RPs played video games requiring highly coordinated responses and/or required participants to simultaneously attend to competing stimuli. In the case of Basak et al., participants had to coordinate a host of cognitive processes in order to maintain information in short-term memory and shift attention between various stimuli to make decisions concerning resources and gaming strategies. Maillot et al. asked RPs to play Nintendo Wii games requiring a great deal of cognitive as well as physical coordination. Meanwhile, Anguera et al. specifically compared the effects of single versus multitasking training modes within their video game, finding that the multitasking mode − one that required RPs to simultaneously complete a perceptual discrimination task while correctly maneuvering a vehicle using a joystick − led to the transferability effects not observed among those completing either of the aforementioned tasks in isolation.

Additional support for the transferability of cognitive skills stems from the examination of action, first-person shooter, video games (Achtman, Green, & Bavelier, 2008). In a review of the empirical literature linking cognitive training to visual skills, the authors connected specific skills required by action video games to broader skills like visual function. They argued that what underlies this link is the notion that such games require constant visual attention to unpredictable distractor and target stimuli that call for quick aiming movements. They posited that this cognitive effort, characterized by high levels of attention due to the unpredictable nature of the events and stimuli being presented by the game, is what drives the broad improvement in visual function. This conclusion is further reinforced by additional empirical findings indicating that playing action video games increases the scope of visual attention across one's field of view (e.g., Green & Bavelier, 2006) and that experienced video gamers are more adept than are less experienced gamers at completing eye-hand coordinated activities not specifically practiced in any video game (Granek, Gorbet, & Sergio, 2010).

Given the collective evidence succinctly reported above, it is possible that the extensive use of various coordinated cognitive skills resulted in the aforementioned observed gains across a host of outcome measures. This body of preliminary research supports the notion that cognitive training that goes beyond the practicing of specific skills and focuses on the coordination among complex cognitive strategies could be associated with broad-based gains in cognitive functioning. However, given the limited literature on the topic, especially as it pertains to video game interventions, this is an area that clearly warrants further study.

1.2. Limitations of the available experimental literature

Although past studies' findings point to transferability of skills, upon conducting an in-depth examination of the published results, we could not document any instances of an intentional emphasis on grouping outcome measures on the basis of transferability and on comparing corresponding average effect sizes to discern the relative impact of video game training. Moreover, we found that in most of the previous studies on older adults without cognitive impairments or major health conditions, video game interventions (eight out of 12 based on our review of the literature, see Table 1) offered video game training over a span of up to 12 weeks (10 weeks on average). Given the total hours of offered training (on average 24 h), this corresponds to an average of 2.5 h of training per week. Thus, what is uncertain, given the limited available literature, is whether video game training in a more time-compressed format − for instance, across five weeks rather than 10 weeks − will produce larger (or smaller) effects on cognitive functioning. Although in the eight studies cited in Table 1 researchers reported, on average, a combined effect size (*d*) of 0.36, in two studies with five or more hours of training per week over five weeks (Basak et al., 2008 & Goldstein et al., 1997) they reported effects of 0.21 and 0.77, respectively, suggesting that a time-compressed format may still yield comparable positive effects. Moreover, the findings of experiments on learning in multimedia contexts indicate that time-compressed formats (defined as 25% faster than normal-paced instruction) result in cognitive gains that are comparable to those found in slower

Table 1
Number of Weeks, Average Number of Hours Per Week, and Effect Size ds of Articles on More Than Five-Week Video Game Interventions.

Article	Number of Weeks	Avg. Hours Per Week	Effect Size (*d*)
Ballesteros et al. (2014)	11	1.8	0.16
Bozoki et al. (2013)	6	3.6	0.12
Dustman (1992)	11	3.0	0.29
Eggenberger et al. (2016)	8	1.5	0.14
Maillot, Perrot, and Hartley (2012)	12	2.0	1.09
Mayas et al. (2014)	11	1.8	0.61
Muijden, Band, and Hommel (2012)	7	3.5	0.19
Stern et al. (2011)	12	3.0	0.26
Averages	9.75	2.5	0.36

Note. The effect size *d* values reflect the average *d* across all the outcomes reported in each corresponding article.

paced modes of instruction (Pastore, 2010, 2012). This suggests that studies offering more hours of training per week over a limited number of weeks may uncover effects comparable to those reported by authors of studies implementing interventions over more extended periods of time.

1.3. The present study's hypotheses

We hypothesized that as a result of our training, in addition to observing an increase in the cognitive skills among game-players (relative to non-game players), we would uncover larger effects among outcomes low in transferability (i.e., practiced in the video game training) than among those high in transferability (i.e., not practiced video game training). Outcomes low in transferability are ones that are highly similar or identical to the assessments done via the video game training activities. On the other hand, outcomes high in transferability are ones that are dissimilar to the assessments done via the video game training activities. Gains observed among such outcomes would reflect an acquisition of broader cognitive skills that could potentially enhance the performance of individuals across multiple settings or contexts. Thus, we expected to find that our video game intervention would result in significant gains with respect to high transferability tasks, but also expected that such effects would be smaller in magnitude than the gains associated with low transferability tasks. Moreover, we hypothesized obtaining comparable effects between video game training and cognitive function in a rarely employed time-compressed format in which participants partake in a greater number of hours of gaming activities per week over a more abbreviated period of time.

Our hypotheses concerning the predicted cognitive gains along with the varying levels of transferability of skills are based upon the cognitive enrichment hypothesis proposed by Hertzog et al. (2009). This hypothesis acknowledges the upper and lower limits of cognitive functioning that are shaped by (a) person-specific developmental histories and inherited traits, and (b) biological aging. According to this perspective, the malleability of late-life cognition stems from the interaction between individual differences (resulting from genetic and experiential factors) and biological changes caused by the "normal" process of aging. Hence, the extent to which a given cognitive intervention can impact cognition depends upon the older trainees' upper boundaries of cognitive functioning, which are in turn shaped by individual differences and age-related cognitive declines. Our hypotheses are therefore based on the premise that individuals without cognitive impairments or any significant physical condition, which can otherwise hamper them from completing activities of daily living (e.g., bathing, dressing, eating), are more likely to garner a greater benefit from playing interactive video games than are people with cognitive impairments and/or major health conditions. This is due to the fact that, as posited by the cognitive enrichment hypothesis, higher functioning individuals are more likely to have a genetic predisposition or experiential background that is conducive to cognitive malleability resulting from an intensive cognitive training regimen. We contend that individuals such as our study's participants already possess well-developed skills although they likely underwent declines that are expected as part of normal aging. Moreover, we maintain that video game interventions, much like other intensive cognitive training activities (see Hertzog et al. for a review of such literature), have the potential to enhance cognitive function by virtue of the demands that video games place on an array of cognitive skills, such as working memory and selective attention. In this regard, research on the neural activation associated with playing video games suggests that the areas of the brain associated with selective attention − the frontal-parietal attentional network − are particularly affected by acquiring video game experience (Bavelier, Achtman, Mani, & Focker, 2012). Specifically, such work suggests that individuals with extensive video gaming experience develop proficient attentional abilities that allow them to attend to target stimuli and ignore non-target stimuli (a hallmark feature of selective attention) to a greater degree than individuals with little to no video gaming experience (Bavelier et al., 2012). The limitation of the aforementioned work is that it is based on younger populations (average age was 21); we could not uncover similar investigations among older adult populations. Nevertheless, the aforementioned evidence offers some support to the conceptualization that video game activities could have a discernable impact on substantive cognitive skills and that there is a neural basis for the observed behavioral gains in reaction time and other cognitive measures.

2. Methods

2.1. Research participants (RPs)

Our research team recruited 40 older adults across four Senior Centers in Los Angeles County between January 2010 and February 2011. Five participants (three from the control group and two from the experimental group) withdrew from the study due to lack of interest and/or to scheduling conflicts preventing them from meeting with Research Assistants (RAs). Among the remaining 35 RPs (Mean Age = 74.71, *SD* = 6.07), 32 identified themselves as Caucasian. We found no differences concerning demographics or other measures between the five RPs withdrawing from the study and the remaining 35 RPs. Recruitment occurred through contacts at the Senior Centers; that is, via convenience and snowball sampling procedures. RAs met with participants at either a local Senior Center or at locations chosen by the RP. Data collection began in January 2010 and continued through May 2011. The data collection period ended in accordance with the agreed upon timelines set with the Senior Centers.

2.1.1. Inclusion criteria and rationale

Our goal was to recruit community-dwelling older adults that were likely free of any cognitive impairment. A such, participants had to meet all the following criteria: (a) be age 65 years or older; (b) be fluent in English; (c) obtain an adequate score on a cognitive impairment screening test; (d) be free of epilepsy or seizures, severe arthritis, carpal tunnel syndrome, or Parkinson's disease; (e) live independently (i.e., not residing in an assisted care facility); (f) possess adequate vision (defined below); and (g) be able to provide informed consent (i.e., in addition to being fluent in English, they were required to understand the terms stipulated in the consent form). We excluded those individuals reporting a history of epilepsy or seizures due to a possible link between playing video games and experiencing seizures (Ferrie, De Marco, Grünewald, Giannakodimos, & Panayiotopoulos, 1994; Ricci, Vigevano, Manfredi, & Kasteleijn-NolstTrenité, 1998). Additionally, we did not include older adults reporting carpal tunnel syndrome or Parkinson's disease due to the difficulty that such individuals would likely experience while interacting with the hand-held video game device and stylus utilized in the current study. Lastly, we excluded individuals possessing less than adequate vision function, as visual acuity is necessary to respond effectively to the demands of the video game. Utilization of these inclusion criteria resulted in the exclusion of six participants. This study was approved by our local institutional review board and informed consent was obtained from each participant.

2.2. Measures

2.2.1. Two physical health screeners

The *Health Background List* is a 4-item adaptation of an existing health measure created by Krause (1997). In accordance with the stated inclusion criteria, we asked RPs to indicate whether they currently experienced or had a history of: epilepsy or seizures, Parkinson's disease, carpal tunnel syndrome, or severe arthritis in the hands. If respondents indicated that they were experiencing or had experienced any of these conditions, they were excluded from further participation.

2.2.2. Eye institute visual function questionnaire

Participants also completed the 25-item National Eye institute visual function questionnaire (VFQ-25; Mangione et al., 2001), which is a self-report measure of visual function. It contains 12 sub-scales corresponding to a total summative score ranging from 0 to 100. Individuals experiencing visual impairment typically obtain lower scores than people not experiencing visual impairment (Jampel, Friedman, Quigley, & Miller, 2002). The VFQ possesses strong internal consistency among older adults (α = .96; Revicki, Rentz, Harnam, Thomas, & Lanzetta, 2010). We quantified having a visual impairment on the basis of an existing, empirically-derived cutoff of 83 (Owen et al., 2006).

2.2.3. Demographics and video game background

We used a *brief original list* to inquire about age, gender, ethnic background, education, and income. Additionally, we utilized three items to gather information on prior videogame use and videogame ownership, and asked RPs to indicate whether they had attempted and/or completed any cross-word puzzles or other puzzles (e.g., Sudoku) over the previous five weeks. Moreover, in follow-up items, we inquired about whether such puzzles were completed via an electronic source, such as a computer/internet or a video game.

2.2.4. Exercise participation

The *Exercise Participation Scale* (EPS; Roth, Wiebe, Fillingim, & Shay, 1989) is a questionnaire used to gather information regarding RPs' participation in physical or exercise-related activities over the course of the last three months. Using the EPS, we inquired about how many times respondents, over the preceding three months, had participated in any of the 12 exercises or activities listed, including jogging, hiking/walking, and swimming.

2.2.5. Memory Alteration Test (MAT)

The *MAT* is comprised of 43 items that yield a total possible score of 50 points. It is a quick and easy-to-administer tool for the assessment of older adults' cognitive impairment and early stage Alzheimer's disease. The tool possesses strong internal consistency when used with individuals aged 65 or older (α = 0.92; Rami, Sanchez-Valle, Bosch, & Villar, 2007) and is highly sensitive in correctly identifying cases of cognitive impairment. In fact, it has been shown to correctly distinguish not only between people with minor cognitive impairment and early onset Alzheimer's disease, but also between individuals with minor cognitive impairment and those without objective memory impairment (Rami, Bosch, Valls-Pedret, Caprile, Sanchez-Valle, & Molinuevo, 2009). The employed cutoff score of 37 has a high degree of sensitivity (0.96) and specificity (0.79) in identifying normal functioning older adults and older individuals with minor cognitive impairment, comparing favorably to the Mini Mental State Examination (Folstein, Folstein, & McHugh, 1975).

2.2.6. The Trail Making Test (TMT)

The *TMT* (Reitan, 1958) is a highly reliable tool commonly used to quantify motor function, visual scanning, and cognitive flexibility (Spreen & Strauss, 1991). This classic measure comprises two tasks, each consisting of 25 circles on a sheet of paper, *Trail Making Test A and B*. In Test A, the circles are numbered from 1 to 25 and participants are asked to draw a line connecting these circles in sequential order. In Test B, the circles are labeled by numbers (1–13) and by letters (A–L) and respondents are asked to connect the circles in order, but in an alternating pattern between numbers and letters. We asked RPs to work as quickly as possible while maintaining their pen or pencil on the sheet. The outcome variables were the time taken to complete the tasks and the number of errors that were documented after completion of the task. The distinction between tests A and B was of particular interest to us, as Brain Age offers users practice with Test B but not with Test A.

2.2.7. Raven's Advanced Progressive Matrices

Raven's Matrices Test (Raven, 1962; Raven, Raven, & Court, 1998) is a measure of basic cognitive functioning and abstract reasoning. It was designed to assess (a) the extent to which individuals can think clearly and make sense of complex information (known as "eductive" ability) and (b) the ability to remember and reproduce previously presented information (i.e., reproductive ability; Babcock, 1994; Raven, 2000). This instrument possesses a high degree of reliability and validity in gerontology studies (e.g., Babcock, 1994; Bors & Stokes, 1998). In particular, it has been successfully utilized in research on the effects of video game interventions on older individuals (Basak, Boot, Voss, & Kramer, 2008). Each item comprises a series of diagrams that follow a logical pattern; the diagram that would be in the final cell, however, is missing. We asked our respondents to choose the diagram that completed the pattern from among eight possible solutions. The entire assessment is comprised of one set of 12 items (Set I) and a second set of 36 items (Set II); however, to avoid burdening older adults with a very time-consuming assessment battery (40 min are required to complete Set II while only 5 min are required to administer Set I), we employed only *Set I*.

2.2.8. The Mini Arithmetic Assessment (MAA)

The *MAA* is an original tool specifically developed for the present study by the first author. It is comprised of 20 basic arithmetic problems, namely seven addition, six subtraction, and seven multiplication exercises. We administered the MAA after respondents completed two practice exercises of one addition and one multiplication exercise. RAs recorded the time to completion, in seconds, along with the number of errors. We also calculated preliminary test-retest reliability by testing 10 volunteers recruited from a local Senior Center (aged 65 or older) who completed the assessment before and after a five-week period. The correlation between pre and posttest scores was 0.81, indicating more than adequate test-retest reliability. The 10 people who completed this pilot measure did not participate in the current study, but they possessed demographic characteristics matching those of the present sample.

2.2.9. The Mini Syllable Assessment (MSA)

The *MSA*, developed specifically for this study by the first author, is an original scale comprised of seven phrases for which participants had to identify the number of syllables. RPs were first offered two examples (along with the correct answers) to illustrate the task; consequently, they were asked to complete two practice exercises. After doing so and indicating that they understood the task, RPs completed the seven target phrases. RAs recorded the time from start to completion (in s) along with the number of errors. As with the *MAA*, the same 10 volunteers completed the *MSA*; the pre-post correlation was 0.73, indicating adequate reliability.

2.2.10. The Card-Sorting Tasks (CST)

The *CST* (White & Cunningham, 1987), comprised of three reaction time measures, quantifies RPs' selective attention. Each task requires the use of a 52-card deck. *Card-sorting task 1* (CS 1) asked respondents to sort the cards into two piles based upon color; *card-sorting task 2* (CS 2) required sorting the cards into four piles by suit; and lastly, *card-sorting task 3* (CS 3) asked RPs to sort the cards into 13 piles according to rank. The card-sorting tasks have adequate to good internal consistency among adults aged 65 and older (α = 0.87 for CS1; α = 0.87 for CS2; and α = 0.60 for CS3; Tomer & Cunningham, 1993; White & Cunningham, 1987). Typically, in older age, scores on all three card-sorting tasks are highly related to those on well-established measures of reaction time, such as the *Sternberg Reaction Time* task (Sternberg, 1975) and the *Perceptual Speed Test* (Guilford & Zimmerman, 1948).

2.2.11. Stroop Test

The *Stroop Test* (Stroop, 1935) contains three distinct reaction time

tests to quantify selective attention. The first of these tests, the *color patch naming* task, required respondents to name the colors (i.e., green, blue, and red) of the color patches sequentially printed on a page. RPs were asked to perform this task as quickly as possible without skipping any patch or making mistakes. Participants were allowed a maximum of 90 s to complete the task. RAs recorded the total time in seconds, along with the number of total errors; however, we did not quantify the items to which the examinee did not respond due to reaching the time limit as errors, in line with standard implementation instructions. The second test, the *word reading* task, required RPs to read aloud the words of colors (i.e., green, blue, and red) sequentially printed on a page, again as quickly as possible and without making any errors. Participants were given a maximum of 90 s to complete this task. As with the *color patch naming* task, RAs recorded the total time (in seconds) as well as the number of total errors, again excluding non-responses due to time limitations.

The final test, the *interference* task, required respondents to state whether the words that they were presented with were in different colors. Specifically, RPs were asked to name the color in which color names were printed in, rather than the words themselves. For instance, the word "red" may actually be presented in blue ink, meaning that the correct answer is "blue." As with the previous tasks, RAs asked respondents to read each item sequentially and as quickly as possible. RPs were allotted a maximum of 180 s to complete the task. RAs recorded the total time in seconds, as well as the number of total errors without counting non-responses due to taking excessive time. Several researchers have used this instrument to quantify cognitive function in older age (e.g., Barella, Etnier, & Chang, 2010; Graf, Uttl, & Tuokko, 1995; Liu-Ambrose, Ashe, Graf, Beattie, & Khan, 2008), making this tool an appropriate choice for the present study.

2.3. Design and procedure

We randomly assigned participants to either the treatment (video game training) or the control condition. Before being assigned to groups, potential RPs completed the pretest assessment. After we determined whether they earned a sufficiently high score on the Memory Alteration Test (MAT; cutoff score = 37) and the Visual Function Questionnaire (VFQ; cutoff score = 83), the first author used the software Statistical Package for the Social Science (SPSS) to randomly determine each participant's assignment to either the treatment or control condition. Specifically, the first author created a two-record data file in SPSS − one record was coded as the treatment group and the second record was coded as the control group. Then, for each participant, the first author used the *Random Sample* function in SPSS to randomly select one of the two records. After which point, the participant was assigned to the experimental condition that corresponded with the selected record. In this way, random assignment to conditions was akin to flipping a coin for each participant so that they each had a 50% chance of being in either group. Consequently, the first author communicated that assignment to an assigned RA who, in turn, contacted the corresponding participant within 24 h regarding this group assignment. Participants who failed to earn a sufficiently high score on either of the aforementioned assessments were notified of their ineligibly to continue by RAs within a 24-h period. Blinding was not utilized, as the nature of the study precluded us from being able to withhold such information. That is, via the informed consent process, all participants were made aware of the two experimental conditions, and control participants were allowed to receive video game training at the conclusion of their participation (none of the control participants opted to receive such training).

Each participant in the control condition completed the pre and posttest battery during two one-on-one meetings with the first author or a designated RA, separated by five weeks to match the time interval between these tests for the experimental group. A total of 20 intervention group participants and 15 control group individuals completed the experiment. Each RP assigned to the experimental group trained (i.e., played Brain Age) for three one-hour sessions per week for five weeks, resulting in 15 h total. To exercise tight control over videogame use, the trainer (i.e., the first author or an RA) was present during each training session. Importantly, to control the amount of time spent using the game, the trainer kept possession of the video game device after completion of each one-hour training session.

We made a concerted effort to structure the video game experience to ensure that all experimental RPs had similar exposure levels to the game, not just in terms of the total amount of time spent playing the game, but also concerning specific mini-games and game modes, as well as the order in which such game content was presented. To that end, RAs began the training by familiarizing respondents with the three game modes offered by Brain Age: *Quick Play, Daily Training, and Sudoku*. After participants became familiar with the Quick Play Mode, they attempted Daily Training, which offers players access to nine mini-games, six of which are unlocked over time with continued play, along with Brain Age Check, which provides access to additional games and offers players feedback in the form of a "Brain Age" that varies from 20, reflecting the highest level of achievement, to 80 and older, reflecting a relatively low level of achievement. To track progress in Daily Training, the RPs maintained a Personal Data File within the program under the careful guidance of the first author or an RA. Once familiar with Quick Play Mode and upon creating a Personal Data File in Daily Training, RPs played through all the available games offered to them via Training Mode.

As trainees progressed through Daily Training, more mini-games became available to them. Experimental participants played all the available mini-games, up to nine mini-games by the time they reached the 15th and final session. The mini-games did not increase in difficulty over time; however, as participants garnered additional experience with such games, they generally performed better over time. In each session, RPs completed the Daily Training portion of the game and then proceeded to complete the Brain Age Check, which is comprised of three mini-games not otherwise found in Daily Training. Lastly, once trainees completed Brain Age Check, they proceeded to play Sudoku for at least 15 min. Sudoku is the only mini-game in Brain Age with two levels of difficulty, easy and difficult. Brain Age requires players to complete relatively easy Sudoku puzzles at first. However, after several sessions, we gave trainees the option to complete more difficult puzzles; all participants chose to complete the more difficult puzzles. As noted, RPs followed this protocol for the duration of one hour per game playing session, with an occasional break whenever necessary; breaks were very rarely requested by our RPs, who were usually enthusiastic about playing.

The first author or an RA kept track of the specific mini-games played by each participant and of the scores earned in each of the 15 training sessions. After finishing 15 training sessions during the five-week period, RPs completed the posttest battery, which included all the assessment tools used in the pretest except for the MAT, the demographics, and the background experience measures. The pre and posttest each took approximately 55 to 60 min to complete. All participants completed these two assessment sessions in two 30-min sessions separated by a break, to minimize fatigue and keep the administration of all of the measures as consistent as possible within the sample.

2.4. Analytic strategy

Two analytic approaches were applicable to this study, i.e., analysis of covariance (ANCOVA) and multivariate analysis of covariance (MANCOVA). Like ANCOVA, MANCOVA is used to examine group differences after adjusting for one or more covariates. It also allows one to examine group differences among multiple simultaneously included outcome variables after controlling for (potentially) a combination of covariates (Giles 2002; Tabachnick & Fidell, 2013). Moreover, MANCOVA reduces the Type I error rate resulting from conducting multiple

significance tests. In contrast, ANCOVA enables one to isolate the relationship between each DV, its corresponding covariate(s), and the grouping variable, allowing a more direct examination of the variables in question. This approach offers greater clarity with which to interpret the comparison across condition, thus we selected ANCOVA as our primary analytical approach. To address the inflation of alpha resulting from conducting multiple ANCOVAs, we applied the Šidàk-Bonferonni Correction (Abdi, 2007):

$$\alpha' = 1 - (1 - \alpha)^{1/n}$$

where α represents the family-wise alpha level (i.e., α = 0.05), n the number of total independent comparisons, and α′ the adjusted alpha level for each test. Given the 13 planned comparisons, the resulting alpha per test was set to 0.0039.

ANCOVA analyses included the MAT as an additional covariate. The MAT is highly related to the Mini Mental Status Examination (MMSE), which measures an array of cognitive abilities, including attention and concentration, language, and spatial functioning (Rami et al., 2007, 2009). Based upon this relationship, it stands to reason that performance on the MAT would be associated with performance on several, if not all, cognitive outcome measures utilized in the present study. To further enhance the statistical power associated with group comparisons, we included MAT performance in ANCOVA analyses involving cognitive outcomes. Further bolstering this approach are previous findings stemming from an initial pilot study in which we found that the MAT was significantly associated with performance on both the Trail Making Test A ($r = -0.47$, $p = .007$) and Test B ($r = -0.37$, $p = .04$).

2.4.1. Sample size considerations and statistical power analysis

The sample size was determined by two factors: (a) relevant historical evidence concerning the possible population effect size, including the aforementioned pilot study, and (b) logical and practical constraints associated with implementing a community-based randomized controlled study. During the period of recruitment and study implementation (January 2010 through February 2011), a limited amount of empirical work existed in this area of study. As a result, we used evidence stemming from the available work to estimate a population effect size and conduct a statistical power analyses. Specifically, we computed average standardized mean effect sizes (d) across all the outcome variables included in three separate studies: Goldstein et al. (1997), Basak et al. (2008), and the aforementioned pilot study that we conducted between January 2008 and December 2009 ($d_{avg} = 0.29$). Akin to what one would do in a meta-analysis, we then computed an average of those effect sizes that was weighted by the number of recruited participants per study. This process resulted in an average weighted effect size of 0.46, which served as our estimate of the population effect size reflecting the impact that playing video games has on cognitive function among an older adult population.

Assuming an alpha level of 0.05 and an effect size value of 0.46, we found that we would need 40 participants to achieve an 80% power level within the context of a two-group ANCOVA design. As described earlier, while we recruited 40 participants, five participants subsequently withdrew from participation. We were unable to recruit additional participants due to prior agreements with the Senior Centers regarding the period for recruitment and data collection and due to limited grant funding available for ongoing project implementation. Given the final total of 35 participants, we achieved a power level of 75%, with an alpha level of 0.05 and an effect size of 0.46. We utilized G*Power (Faul, Erdfelder, Lang, & Buchner, 2007) to conduct all our power calculations.

Table 2
Demographic Characteristics Among Participants.

Characteristic	Intervention Group		Control Group	
	N	%	N	%
Gender				
Male	4	20	5	33
Female	16	80	10	67
Education				
Less than high school	0	0	2	13
High School Graduate	3	15	1	7
Completed Trade School	2	10	1	7
Some College	9	45	6	40
Bachelor's Degree	1	5	1	7
Some Grad School	2	10	1	7
Master's Degree	3	15	3	20
PhD/MD/JD	0	0	0	0
Annual Income				
Less than $20k	6	30	3	20
$20–$39k	7	35	2	13
$40–$59k	2	10	2	13
$60–$79k	2	10	4	27
$80–$99k	0	0	1	7
$100 or greater	2	10	1	7
No Response	1	5	2	13
Game Ownership				
No	16	80	13	87
Yes	4	20	2	13
Frequency of Game Play[1]				
Less than once per month	6	50	4	40
More than once per month	6	50	6	60
Puzzle Completion[1]				
No	6	50	4	57
Yes	6	50	3	43

Note. $N = 35$. [1] These items were incorporated into the assessment protocols after starting the study, thus some RPs did not have the opportunity to complete them.

3. Results

3.1. Demographic variables, exercise participation, and visual function

Table 2 illustrates the demographic information for each group. Given that RPs were randomly assigned to conditions, there was no basis upon which to expect the groups to differ significantly on any of the demographic characteristics. However, before implementing the primary analyses, we conducted preliminary inferential tests to assess the equivalence of the groups in question. We did not uncover any significant differences between groups with regards to the following characteristics:

- Gender ($\phi = 0.15$, $p = .37$).
- Educational background ($t = 0.052$, $p = .96$) or income ($t = 1.26$, $p = .21$).
- Likelihood of owning a video game ($\phi = 0.09$, $p = .61$).
- Likelihood of playing a video or computer game ($\phi = 0.10$, $p = .64$).
- Likelihood of attempting or completing any type of puzzles, such as crossword puzzles ($\phi = 0.07$, $p = .76$).

Moreover, we did not uncover any statistically significant age differences between the treatment ($M = 74.95$, $SD = 6.53$) and the control group ($M = 74.40$, $SD = 5.62$; $t(33) = 0.26$, $p = .80$). Also, upon exploring group differences with respect to the total number of physical or exercise-related activities completed over the last three months (as measured by the EPS), we did not detect any significant differences between the treatment ($M = 2.40$, $SD = 1.87$) and the control group ($M = 3.33$, $SD = 1.40$; $t(33) = 1.62$, $p = .12$). Lastly, we did not find any significant differences between the treatment ($M = 92.97$, $SD = 6.28$) and the control group ($M = 93.96$, $SD = 4.36$) on the basis of their VFQ scores, $t(17) = 0.40$, $p = .70$. Ethnic background was not

Table 3
ANCOVA Findings Reflecting the Effects of the Intervention on the Cognitive Outcomes After Adjusting for Corresponding Pretest and MAT Scores.

Outcome Measure	*F*	*df*	*P*
Syllable (Time)	13.96	1/31	.001*
Arithmetic (Time)	10.09	1/31	.003*
Stroop (Interference)	6.60	1/30	0.02
Arithmetic (Errors)	2.98	1/31	0.09
Syllable (Errors)	0.70	1/31	0.41
Card Sorting 3	0.33	1/31	0.57
Stroop (Color Patch)	0.28	1/31	0.60
Raven's Matrices	0.28	1/30	0.60
Stroop (Word Reading)	0.24	1/31	0.63
Card Sorting 1	0.22	1/31	0.64
Trail Making B	0.18	1/30	0.67
Trail Making A	0.11	1/31	0.75
Card Sorting 2	0.08	1/31	0.73

Note. *$p <$ .0039; Sidak-Bonferroni test α = .0039.

compared between groups, as 32 of 35 participants identified themselves as Caucasian.

3.2. Overall cognitive performance

3.2.1. ANCOVA findings

We conducted ANCOVAs on each of the 13 outcome variables after adjusting for corresponding pretest and MAT scores. Table 3 contains our significance test findings and the adjusted cognitive outcomes means. The corresponding effect size *d*s and 95% confidence intervals are displayed in Table 4.

We found a significant group difference with respect to the Mini Syllable Assessment ($F(1, 31) = 13.96$, $p = .001$), as the treatment group (*Msec* = 76.96, *SD* = 21.17) completed the assessment faster than the control group (*Msec* = 104.24, *SD* = 21.22). The corresponding effect size *d* (1.28) indicates that the average experimental RP completed the task more quickly than 90% of the individuals in the control group. The treatment group (*Msec* = 35.49, *SD* = 6.35) also completed the Mini Arithmetic Assessment significantly more quickly than the control group (*Msec* = 42.50, *SD* = 6.38), $F(1,31) = 10.09$, $p = .003$. An effect size *d* of 1.10 indicates that the average participant in the treatment group was quicker at completing the task than 86% of the RPs in the control group. Lastly, we obtained a marginally significant difference on the Stroop Interference Test ($F(1,30) = 6.60$, $p = .02$), as the treatment group (*M*sec = 57.09, *SD* = 19.55) again completed the task more quickly than the control group (*Msec* = 74.58, *SD* = 19.59). The corresponding effect size *d* (0.89) indicates that the average experimental participant was faster at completing the assessment than 82% of the control group participants.

Positive effect size *d*s are indicative of a treatment group advantage over the control group. Thus, with the exception of the Raven's Matrices, lower means among the treatment group reflected quicker task completion (e.g., Mini Syllable Assessment, card sorting tasks) or lower error rates (e.g., Mini Syllable Assessment errors, Mini Arithmetic Assessment errors). Regarding the Raven's Matrices, the outcome measure was the total number of correct responses; the slightly lower mean score achieved by the treatment group ($M_{adjust} = 6.05$, *SD* = 1.60–Treatment; $M_{adjust\ =} 6.34$, *SD* = 1.60–Control) is indicative of a control group advantage over the treatment group, resulting in a non-significant negative effect size value ($d_{adjust} = -0.18$).

Our findings specific to the Mini Syllable Assessment and the Mini Arithmetic Assessment indicate robust group differences on the basis of the adjusted means ($ds_{adjust} = 1.28$ & 1.10, respectively). In fact, the lower bound of the 95% confidence interval for the Mini Syllable and Mini Arithmetic assessments (0.55 & 0.38, respectively) further suggests a large effect among each outcome. Another potentially meaningful effect is the one obtained on the Stroop Interference Test ($d_{adjust} = 0.89$), indicating that the average Stroop Interference Test time generated by the treatment group was lower than 82% of times generated by the control group. Lastly, there was a potentially meaningful group difference concerning the number of errors committed on the Mini Arithmetic Assessment ($d_{adjust} = 0.59$), indicating that the average amount of errors made by the experimental group was lower than the number of errors committed by 73% of control group RPs.

Table 5 displays the correlations between all posttest cognitive variables. As one might expect, results on the card-sorting tasks (i.e., CS1–CS3) were highly associated with each other, as were those on the Stroop tests (i.e., STC, STW, and STI). Noteworthy, however, is the high correlation identified between the Mini Arithmetic Assessment (MAA) and several other cognitive measures. Scores on the MAA were highly related to the Trail Making tasks ($r = 0.54$ for TA; $r = 0.62$ for TB), the Mini Syllable Assessment (MSA; $r = 0.46$), and two of the three Stroop tests ($r = 0.53$ for Color Patch; $r = 0.54$ for Word Reading).

3.3. Impact of the training on transferability

In addition to examining the impact of our training on cognitive function, we also inquired about whether larger effects existed for practiced (i.e., low transferability) over non-practiced (i.e., high transferability) cognitive measures. In support of our hypothesis, we found a robust difference between groups. The average effect size *d* for the practiced effects ($d_{adjust} = 0.72$, *SD* = 0.47) was greater in magnitude than the average effect size *d* for the non-practiced effects ($d_{adjust} = 0.03$, *SD* = 0.17). In fact, the average effect size *d* for practiced effects was greater than the largest effect obtained for the non-practiced effects ($d_{adjust} = 0.20$, *SD* = 0.18). As Table 6 illustrates, the five largest effects that we obtained were all for practiced outcomes. As hypothesized, the effects were positive for both types of outcomes and the average effect for practiced outcomes (i.e., those low in transferability) was higher than the average effect for non-practiced outcomes (i.e., those high in transferability). However, the average effect for non-practiced outcomes was far lower than the 0.20 value that typically denotes meaningful effects (Cohen, 1988).

3.4. Impact of the time-compressed approach

Our secondary hypothesis was that the time-compressed approach that we adopted as part of our video game intervention would yield effects linking video game activities to cognitive function that were comparable to more extended-time based approaches adopted by most researchers in this field. We found that the combined effect size (*d*) in our study across all our measures was 0.35; this, as illustrated in

Table 4
Adjusted Means, Standard Deviations and Effect Size ds for each of the Cognitive Outcomes Examined Via ANCOVA Analyses.

Measure	Intervention		Control		*d*	95% CI (*d*)
	M	*SD*	*M*	*SD*		
Syllable (Time)*	76.96	21.17	104.24	21.22	1.28	0.55 to 2.02
Arithmetic (Time)*	35.49	6.35	42.50	6.38	1.10	0.38 to 1.82
Stroop (Interference)	57.09	19.55	74.58	19.59	0.89	0.18 to 1.60
Arithmetic (Errors)	0.71	1.03	1.32	1.03	0.59	−0.09 to 1.28
Syllable (Errors)	1.44	1.05	1.75	1.05	0.29	−0.38 to 0.97
Card Sorting 3	89.37	21.83	93.71	21.87	0.20	−0.47 to 0.87
Stroop (Color Patch)	30.54	4.15	31.33	4.17	0.19	−0.49 to 0.87
Raven's Matrices	6.05	1.60	6.34	1.60	−0.18	−0.85 to 0.49
Stroop (Word Read)	23.36	2.85	23.84	2.87	0.17	−0.50 to 0.84
Card Sorting 1	38.63	5.55	37.73	5.57	−0.16	−0.83 to 0.51
Trail Making B	79.69	16.72	82.20	16.20	0.15	−0.54 to 0.84
Trail Making A	36.18	9.03	37.21	9.09	0.11	−0.56 to 0.78
Card Sorting 2	59.83	10.04	58.86	10.06	−0.10	−0.77 to 0.58

Note. *N*s ranged from 34 to 35 for each comparison; *$p <$ .0039; Sidak-Bonferroni test α = .0039.

Table 5
Correlation Matrix Among Cognitive Posttest Variables.

Measure	1	2	3	4	5	6	7	8	9	10	11	12
1. Trail Making A.												
2. Trail Making B	0.52*											
3. Raven's	−0.42	−0.43	1									
4. Arith (Time)	0.54*	0.62**	−0.31	1								
5. Arith (Error)	0.06	0.27	−0.30	0.35	1							
6. Syllable (Time)	0.36	0.28	0.03	0.46*	0.15	1						
7. Syllable (Error)	0.22	0.18	−0.42	0.27	0.46*	0.14	1					
8. Card Sorting 1	0.47*	0.41	−0.40	0.40	0.01	0.14	0.07	1				
9. Card Sorting 2	0.32	0.39	−0.40	0.20	0.10	0.15	−0.02	0.76**	1			
10. Card Sorting 3	0.44*	0.47*	−0.24	0.28	0.11	0.35	0.21	0.50*	0.58**	1		
11. Stroop (Color)	0.23	0.40	−0.31	0.53*	0.36	0.26	0.38	0.29	0.32	0.27	1	
12. Stroop (Word)	0.33	0.36	−0.14	0.54*	0.25	0.40	0.29	0.07	0.21	0.28	0.81**	1
13. Stroop (Inter)	0.53*	0.51	−0.42	0.43	0.28	0.36	0.26	0.34	0.33	0.15	0.49*	0.53*

Note. *N*s range from 34 to 35; $^{*}p < .05$; $^{**}p < .01$.

Table 6
Adjusted Effect Size ds by Practiced and Non-Practiced Outcomes.

Outcome Measure	Practiced Outcome *d*	Non-Practiced Outcome *d*
Syllable (Time)*	1.28	
Arithmetic (Time)*	1.10	
Stroop (Interference)	0.89	
Arithmetic (Errors)	0.59	
Syllable (Errors)	0.29	
Card Sorting 3		0.20
Stroop (Color Patch)		0.19
Raven's Matrices		−0.18
Stroop (Word Read)		0.17
Card Sorting 1		−0.16
Trail Making B	0.15	
Trail Making A		0.11
Card Sorting 2		−0.10
Averages (SD)	0.72 (0.47)	0.03 (0.17)

Table 1, is almost identical to the combined effect size (*d*) of 0.36 in the eight identified studies with longer intervention periods. Such findings support our hypothesis that a more time-compressed five-week approach with five hours of gaming activities per week is sufficient to bring about comparable gains in cognitive function.

4. Discussion

4.1. Summary of findings and implications

Consistent with the outcome of recent research on the positive effects of widely accessible video games on cognitive function, our intervention had a significant impact on older adults' multiple cognitive outcomes. This study's findings indicate that participants who played Brain Age performed significantly better than did control participants with regards to brief arithmetic and syllable-counting tasks. We also detected potentially meaningful differences on the Stroop Interference Test. Although not all outcome variables achieved statistical significance, it should be noted that over 80% of the obtained effects favored the treatment group; that is, based upon the adjusted mean differences, 13 of the 16 effects indicated an advantage in favor of those partaking in our intervention. Collectively, such findings suggest that video games may serve as a viable option for older adults looking for an accessible, engaging, and relatively quick way of improving their cognitive skills. What the marginally significant results have in common is that they were all directly practiced within the context of our study's intervention. Indeed, in line with our hypothesis, training had a far larger impact on outcomes that were directly practiced in Brain Age than it did on outcomes not directly practiced. We did, however, expect to find a larger effect sizes for non-practiced outcome measures; the near zero overall effect size we found for these outcomes suggests that our video game intervention did not result in the transfer of acquired skills, at least among the outcomes that we studied.

It is possible that transferability depends upon the nature of tasks offered by the video game in question. Brain Age is, after all, quite different from the fast-paced action games described by scholars like Achtman et al. (2008). Unlike those games, Brain Age does not require onerous attention to wide-ranging and unpredictable stimuli nor coordinated aiming and firing behaviors that are characteristic of those other action games. Given the findings reported by Achtman and colleagues that action games requiring this level of engagement are linked to improvements in visual skills not directly tied to the gaming activities, perhaps games requiring a great deal of coordinated behaviors may be associated with broad-based gains across many cognitive measures. These conclusions are consistent with findings reported by Maillot et al. (2012) regarding research in which broad-based significant effects in cognition surfaced via the employment of exergames requiring extensive coordinated physical exercise. There is research suggesting the existence of a close link between physical exercise and cognitive function (e.g., Colcombe & Kramer, 2003); thus, our findings may reflect an extension of this link inasmuch as playing games requiring extensive motor function and/or coordination may be the key to affecting a broad set of cognitive abilities.

On the other hand, the studies conducted by Basak et al. (2008) and Anguera et al. (2013) did not require RPs to produce the same degree of coordinated motor responses that RPs in Achtman et al. (2008) and Maillot et al. (2012) were required to generate. Responding to first-person shooting games with simulated hand-held weapons (as required by Achtman and colleagues) or Wii video games (as done in Maillot and collaborators' research) arguably requires more coordinated motor effort than does a strategy game (employed by Basak et al.) or custom-designed racing game (studied by Anguera and colleagues). Nevertheless, all of these authors reported findings consistent with a transferability effect. It is possible that video games fostering extensive coordinated cognitive processing − regardless of whether they also require coordinated motor function − drive the transferability effects. After all, these authors utilized video games with a clear cognitive-based multitasking component. More work is warranted on this matter, especially a direct examination of the transferability effects of games requiring coordinated cognitive responses, as opposed to games requiring extensive coordinated cognitive and motor responses.

Even if the benefits of playing a video game are limited to the skills that are directly practiced, there is still a practical benefit to possessing those skills to the extent that they lend themselves to real-world application. For instance, older adults may find that possessing sharpened arithmetic skills is meaningful to their everyday lives; to the extent that this is the case, games like Brain Age offer them an opportunity to enhance practically relevant skills to potentially enhance their quality of life. To this end, researchers have linked the performance on the

Stroop tasks of older adults without cognitive impairments or major health conditions to activities of daily living such as money management, medical administration, as well as meal planning and preparation (e.g., Cahn-Weiner, Malloy, Boyle, Marran, & Salloway, 2000). This means that Brain Age, and possibly games similar to it, have the potential to enhance everyday functioning via two pathways. One of them is the improvement of cognitive abilities that appear to be related to everyday functioning (e.g., Stroop tests). Given the present findings, a second viable pathway could be the regular practice of specific everyday tasks identified as relevant to enhancing the real-world functioning of older adults. Thus, even though video games may enhance only directly practiced skills, the technology exists to design video games to teach specific real-world skills such as money management and meal planning. With the advent of smart phones and tablets, video game applications can be designed to enhance these specific, yet functionally meaningful cognitive skills. Although these are methodological issues that merit further consideration and study, our findings point to the value of developing engaging video games designed to offer older users regular practice with tasks that have clear real-world applications.

We also obtained support for our secondary hypothesis that a time-compressed approach to video game interventions would yield effects comparable to those obtained in past studies implementing similar interventions over a longer period of time. What is also noteworthy is that the aforementioned studies employing a more extended-time approach offered research participants a similar number of total hours of training ($M = 24$ h) as we did in our study. Collectively, these findings suggest that time-compressed video game interventions can still help participants enhance their cognitive function and that individuals seeking to derive the benefits of playing video games may do so sooner by increasing their game playing time per week. Another benefit of a time-compressed approach is that it reduces the likelihood of participants withdrawing from the study prior to its completion − this is particularly problematic when that dropout rate differs across the conditions, potentially undermining the internal validity of the study. Nevertheless, these conclusions are based upon this study and two others that we have uncovered; more research on this topic is needed to better elucidate the impact of time-compressed video game playing activities.

4.2. Future directions

4.2.1. The need for theory building

Whereas this area of work is still in its relative infancy, we are increasing our understanding of the magnitude of the effects associated with video game interventions. Scholars in this area are also making progress towards identifying the conditions under which video game training affects cognition. As we make further advancements in this field of study, we need to be mindful of theoretical frameworks that may potentially help explain and predict the growing knowledge base on this topic. When examining the impact that video game interventions have on cognitive function among older adults, researchers regularly point to the "use it or lose it" hypothesis to offer a perspective and potential interpretations on their findings (e.g., Ackerman et al., 2010; Hertzog et al., 2009). However helpful this perspective is in providing a context under which to interpret findings, this burgeoning area of research is ripe for the development of potential theories that will help explain the various phenomena observed in the present investigation. The cognitive enrichment hypothesis (described earlier) may help influence the development of theories concerning the interaction between age, types of games, duration, and nature of training interventions, as well as outcome measures, all of which are factors that need further investigation for us to progress in our understanding of the link between video games and well-being. In the present study, we examined the aforementioned link among community-dwelling older adults who were free of cognitive impairment and did not have major health conditions. It stands to reason that such individuals possessed upper boundaries of cognitive functioning that, at a minimum, were typical of individuals over the age of 65. In line with the cognitive enrichment hypothesis, one potential reason for our positive training effects is that our trainees possessed upper limits of cognitive functioning that made the malleability possible. According to this reasoning, perhaps video game training effects − and even transferability effects − may depend not just on the nature of the video game intervention, but also on the extent to which targeted populations of older adults possess the cognitive potential that would be necessary for them to be able to benefit from such an intervention.

Further theory building is essential to coalescing research from various strands of empirical work and developing a cohesive body of research in this area. Among other benefits, predictions stemming from such frameworks could help focus researchers' efforts on developing gaming interventions that have particularly robust effects on the cognitive functioning of the growing older adult populations. For instance, in the present study, notwithstanding the various caveats discussed earlier, we have acquired evidence suggesting that gaming interventions generally achieve focused benefits on cognition. Indeed, the cognitive improvements that older individuals gained were generally tied directly to the activities that they completed within the context of the gaming intervention. Nevertheless, we also have examples in the literature (e.g., Anguera et al., 2013) suggesting that these games can foster transferability when engaging RPs in video games that challenge them to simultaneously respond to multiple on-screen stimuli. This knowledge about the potential conditions that could help foster transferability can serve as a starting point from which we could begin to narrow our focus even further around (a) the types of gaming interventions most likely to result in the greatest transfer, (b) the outcome measures best suited for examining this matter in older adults, and (c) the populations of older individuals most likely to benefit from such interventions. This iterative process of designing wide-ranging gaming interventions while using diverse outcome measures may lead us down the path of developing a more comprehensive model through which to understand and explain the still unanswered questions surrounding the transferability of cognitive skills to broader abilities that are more directly tied to older adults' everyday tasks.

4.2.2. Limitations of the study and the need for future research aimed at offsetting them

Our study has various limitations that could be addressed in future research. One limitation is the relatively small sample size; a larger sample would enhance the generalizability of our findings. Nevertheless, some prior studies in this area have used even smaller sample sizes (e.g., Goldstein et al., 1997). Moreover, less than 10% of our RPs were non-Caucasian, consistent with the sample composition of studies in this area (e.g., Maillot et al., 2012); some published investigations do not even contain any specific reference to ethnicity (e.g., Ackerman et al., 2010; Basak et al., 2008). Furthermore over two-thirds of the RPs in the current study were women. Interested researchers should target more diverse and larger samples to test the generalizability of our findings to other geriatric research populations. Moreover, as in the case of other published investigations in this area, our results leave open the possibility that similar cognitive benefits could be also obtained by using non-technologically orientated tools. For instance, perhaps gains in arithmetic and syllable-identifying skills, as well as in Stroop-related attention skills, could be achieved by using corresponding paper-based methods and a stopwatch. To this end, future studies should include such a third comparison group to more closely examine the added benefit offered to users by the technology.

Another limitation of the present investigation is that it did not include an evaluation of the degree to which the improvements obtained after five weeks persisted over a more extended period of time. While gains in cognitive function were observed over this compressed time period, given the findings of past research in this area (for a review, see Hertzog et al., 2009), it is likely that older trainees would

experience degradation in skill after no longer using Brain Age. Interested researchers should investigate such declines in ability, in order to determine whether skills, unless further practiced, revert back to baseline levels within a certain time frame. Encouraging findings on this topic stem from aforementioned research aimed at multitasking enhancement in older age via a custom-designed three-dimensional video game played in multitasking training mode. In this regard, the cognitive gains of 60–85 years-old trainees, measured via electroencephalography, persisted for 6 months (Anguera et al., 2013), suggesting that cognitive training in older age has the potential to last for at least half a year. Nonetheless, a decline in abilities after discontinuing the training for several months should not be surprising, nor should it be a reason to avoid undertaking video game training. Indeed, discontinuing a physical exercise routine for an extended amount of time would likely lead to older adults' muscle loss and decline in stamina, as one has to engage in physical exercise regularly to sustain the gains achieved over time. In the same way, gains in cognitive abilities cannot be expected to last indefinitely if an older individual ceases to engage in the very exercises that helped them achieve those milestones. Thus, video games should not be viewed as a "magic pill" to dramatically and permanently improve cognitive abilities. Instead, as with physical exercise, consistency is the key to achieving and maintaining cognitive performance in older age (as well as at any age).

A third study limitation is the lack of an active control group. We did not include a third active control in our study due to the logistical challenges associated with implementing such a design, especially a time-compressed approach with supervised training. However, having an active control group (especially one asked to complete cognitively-demanding tasks) would further enhance a study's methodological rigor by allowing researchers to identify the extent to which activities that are directly tied to the video game experience uniquely account for any observed gains in cognitive function. Another benefit of an active control group is that it would help control for the potential benefits of social interaction, thus allowing investigators to further differentiate the impact of the video game activities. A third benefit accrued when including an active control group in a computer or technologically-based activity is that researchers can then disentangle the effects unique to playing video games rather than to just using computers or general multimedia devices. This is critical when investigating the impact of technological tools on the cognitive function of older adults, many of whom are less likely to have extensive experience with computers relative to younger individuals (Smith, 2014). It is worth noting that active control groups are rarely employed in this area of research, possibly because of the logistical challenges associated with implementing these kinds of interventions in applied settings. However, we did locate a study (Nouchi et al., 2016) that employed an active control group that engaged in computer-based activities; the authors asked control participants to use a tablet PC to play a knowledge quiz training game over the course of a four-week period − on average, 1.25 h per week. The intervention group also played a custom-designed video game for the same amount of time. Findings revealed a statistically significant advantage in favor of the intervention group related to processing speed and inhibition ability; the effect size (*d*) across all their outcome measures was 0.26. Considering the methodological rigor of this approach, these findings corroborate the conclusions stemming from our video game implementation. Still, more research needs to be conducted with active control groups to further isolate the role that video game activities play in enhancing cognitive function.

It is also possible that diverging expectations for improvement on the part of participants in the two conditions affected our study's findings (Boot, Simons, Stothart, & Stutts, 2013). Participants in the non-active control group may not have expected an increase in cognitive function, while those in the intervention group may have done so; to the extent that such diverging expectations differentially affected their subsequent performance at posttest, such expectations may have affected the internal validity of the study. Thus, future studies should ideally contain both an active and non-active control group, and investigators should strive to match (and measure) the expectations of participants in the groups, particularly between the intervention and active control groups (Boot et al., 2013).

There is great potential for conducting insightful research in this area. In general, video game training offers participants an interactive, engaging learning experience characterized by a high degree of user control, immediate feedback, and interactivity: research has shown that these aspects of technology are beneficial within the context of technology-based learning (Sosa et al., 2011). Indeed, this may be one reason why video games requiring extensive motor function may yield more robust findings. Nevertheless, these facets of training have not been directly measured within the context of video game interventions (including in the present study). One possibility is to have independent judges rate the different technologies (either in the context of a single study or in a meta-analysis) along these characteristics and then examine the extent to which each of these facets is associated with improvements in cognitive performance. Furthermore, researchers have shown the benefits of video games for improving older adults' cognitive functioning almost exclusively for older individuals who are free of cognitive impairment. However, it is possible that older adults experiencing minor cognitive impairment (or even dementia) still possess some level of skill plasticity, which could potentially allow them to enhance their cognitive functioning. Thus, interested scholars could focus on further elucidating whether highly accessible video games, such as Brain Age, can also be useful at enhancing the cognitive abilities of older adults experiencing cognitive impairment. Lastly, as noted earlier, the work exploring the neural activity associated with video gaming activities is based on younger adults; thus, conducting such work on older adults would help us establish whether similar neural regions are affected by video gaming activities by older adults and point to late-life neural malleability, at least among older individuals without cognitive impairments or major health conditions.

Conflict of interest statement

None – no conflict of interest exists among either author.

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