

The effects of video game playing on attention, memory, and executive control

Walter R. Boot*, Arthur F. Kramer, Daniel J. Simons, Monica Fabiani, Gabriele Gratton

Beckman Institute, Department of Psychology, University of Illinois, Urbana-Champaign, Urbana, IL 61801, USA

ARTICLE INFO

Article history:

Received 6 March 2008

Received in revised form 7 September 2008

Accepted 8 September 2008

Available online 16 October 2008

PsycINFO classification:

2300

2323

2343

2346

Keywords:

Practice

Transfer

Video games

Learning

ABSTRACT

Expert video game players often outperform non-players on measures of basic attention and performance. Such differences might result from exposure to video games or they might reflect other group differences between those people who do or do not play video games. Recent research has suggested a causal relationship between playing action video games and improvements in a variety of visual and attentional skills (e.g., [Green, C. S., & Bavelier, D. (2003). Action video game modifies visual selective attention. *Nature*, 423, 534–537]). The current research sought to replicate and extend these results by examining both expert/non-gamer differences and the effects of video game playing on tasks tapping a wider range of cognitive abilities, including attention, memory, and executive control. Non-gamers played 20+ h of an action video game, a puzzle game, or a real-time strategy game. Expert gamers and non-gamers differed on a number of basic cognitive skills: experts could track objects moving at greater speeds, better detected changes to objects stored in visual short-term memory, switched more quickly from one task to another, and mentally rotated objects more efficiently. Strikingly, extensive video game practice did not substantially enhance performance for non-gamers on most cognitive tasks, although they did improve somewhat in mental rotation performance. Our results suggest that at least some differences between video game experts and non-gamers in basic cognitive performance result either from far more extensive video game experience or from pre-existing group differences in abilities that result in a self-selection effect.

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1. Introduction

Recent research suggests that playing video games, even for a relatively short period of time, improves performance on a number of tasks that measure visual and attentional abilities. In fact, a number of studies have found that having participants play action video games for as few as 10 h can improve performance on laboratory tasks that, on the surface, are dissimilar to the games they were asked to play (e.g., Feng, Spence, & Pratt, 2007; Green & Bavelier, 2003, 2006a, 2006b, 2007). Thus, video game experience appears to improve basic skills that can be applied to novel tasks and stimuli.

The recent surge of interest in video games as a means to improve basic perceptual and cognitive abilities builds on earlier video game findings. For example, playing video games such as Donkey Kong and Pac Man was found to significantly improve the reaction times of older adults as compared to controls who did not play (Clark, Lanphear, & Riddick, 1987). In 1989, a special issue of *Acta Psychologica* was devoted to Space Fortress, a video

game specifically designed by cognitive psychologists as a training and research tool (Donchin et al., 1989). The skills acquired while playing Space Fortress appeared to transfer to other tasks as well. For example, young adults who played Space Fortress performed better than controls on a test of physics knowledge (Frederiksen & White, 1989), and Israeli Air Force flight school cadets who played Space Fortress significantly outperformed a no-game control group on actual flight performance, suggesting that skills learned from the game transferred to flight control (Gopher, Weil, & Bareket, 1994). In Space Fortress, players must focus attention to multiple demanding and overlapping component tasks, so improved flight performance might result from improved attentional control. Space Fortress training was considered so successful that it was subsequently added to the training program of the Israeli Air Force. Similarly, helicopter pilots trained on Space Fortress outperformed pilots trained on a helicopter flight simulation game (Hart & Battiste, 1992).

More recently, Green and Bavelier (2003), Green and Bavelier (2006a, Green and Bavelier (2006b, Green and Bavelier (2007) provided evidence that video game playing can improve performance on a number of attentional and perceptual tasks. Both action video game players and non-video game players who were given 10 h of action video game experience (a first-person shooter called Medal of Honor, Electronic Arts) demonstrated superior performance in

* Corresponding author. Current address: Florida State University, Department of Psychology, 1107 W. Call Street, Tallahassee, FL 32306-4301, USA. Tel.: +1 850 645 8734; fax: +1 850 644 7739.

E-mail address: boot@psy.fsu.edu (W.R. Boot).

the attentional blink task, a measure of attentional flexibility over time (Green & Bavelier, 2003). Similarly, performance in the Functional Field of View (FFOV) task, a measure of the breadth of visual attention, was also improved by the video game experience. Habitual action video game players also demonstrated an *increased* flanker effect, indicating greater visual resources, and participants who played an action game for only 10 h demonstrated a greater subitizing capacity as well. Importantly, participants who played Tetris, a puzzle game with similar motor components compared to Medal of Honor but fewer attentional demands, did not improve on these tasks. Action video game experience also resulted in the ability to monitor a greater number of moving objects simultaneously (Green & Bavelier, 2006a). Most surprisingly, video game players and participants who practiced video games demonstrated a change in visual acuity, or the ability to make fine discrimination judgments of visually crowded stimuli (Green & Bavelier, 2007). Additional evidence for superior performance of video game experts comes from the work of Castel, Pratt, and Drummond (2005) that shows that in a number of tasks, video game players demonstrate faster response times than non-gamers.

When considered together, these studies suggest that playing video games promise improved performance in a wide variety of situations; the transfer of video game experience appears to be broad. The transfer tasks described above (helicopter and jet piloting, the FFOV, the attentional blink task, etc.) were different from the games participants played both in the displays and the required responses, but transfer still occurred. Moreover, improvements in skills occurred quickly, in some instances with as little as 10 h of game experience.

The purpose of this study was to determine whether video game benefits are restricted to visual and attentional tasks, or whether improvements might be broader. In addition to measures of visual attention, we assessed the effect of video game playing on a number of memory, reasoning, and executive control tasks. We also examined the effect of game type. Participants either played a fast-paced action game, a slower-paced strategy game, or a puzzle game. It is easy to imagine how a highly complex strategy game might better improve executive control, planning, and memory as compared to a first-person shooter or puzzle game. Successful game play would seem to require such skills (e.g., memory for where enemies and resources are located, switching between several tasks as the complex demands of the game changes, remembering the complex sequence of events required to attain multiple simultaneous goals). Thus, we expected game-specific effects based on the nature of the video game participants were asked to play. Broadly, we expected the action game to improve visual/attentional skills, the strategy game to improve executive control skills, and the puzzle game to improve some spatial skills. Both longitudinal and cross-sectional approaches were taken. The longitudinal approach had participants complete more than 20 h of video game practice, and perceptual and cognitive abilities were assessed before practice, after ten h of practice, and then after 21 h of practice. Additionally, the cross-sectional approach compared the abilities of expert gamers and non-video game players. To preview the results, although a number of non-gamer/expert differences were found, these differences, for the most part, did not emerge even after 20+ h of video game experience.

2. Methods

2.1. Participants

2.1.1. Cross-sectional groups (expert vs. non-gamers)

Eleven expert video game players and ten non-video game players were recruited from the Urbana-Champaign community. Par-

ticipants were considered experts if they played seven or more hours of video games per week for the past two years. Experts were selected such that they had high levels of expertise with action video games such as Halo, Grand Theft Auto and Unreal Tournament. However, expert video game players also had experience with a wide variety of game genres including role-playing, strategy, and sports games. Non-gamers were selected such that they played video games one hour a week or less. During initial participant recruitment, it was found that males were much more likely to report having video game expertise, and thus cross-sectional groups were restricted to male participants.

2.1.2. Longitudinal groups (game practice and passive control)

Eighty-two college students and members of the Urbana-Champaign community participated in the longitudinal portion of the study. To maximize the likelihood of observing improvements, all participants in the longitudinal groups were non-gamers and reported playing less than one hour of video games a week over the past 2 years. We included as many participants as possible who reported playing 0 h/week. Females were much more likely to report being non-gamers, and thus longitudinal groups were primarily female (potential gender implications are discussed later).

All participants were right-handed and demonstrated normal visual acuity and normal color vision. All reported no major medical or psychological conditions. Demographics for each group are listed in Table 1.

2.1.3. Recruitment

Potential participants were contacted either through flyers posted in campus buildings and businesses or through advertisements posted to online bulletin boards. People responding to these flyers and advertisements completed a survey of their video game habits. Sixty-three participants in the longitudinal portion of the study were randomly assigned to one of three video game practice conditions: (a) Medal of Honor, an action game ($n = 20$), (b) Rise of Nations, a strategy game ($n = 23$), or (c) Tetris, a puzzle game ($n = 20$). An additional nineteen participants were assigned to a no-practice control group.

2.2. Apparatus

Four Pentium 4-based PCs were used for the majority of cognitive testing and game playing. These computers were connected to 21-inch monitors. Additionally, one eMAC with a 17-inch monitor was used for two of the cognitive tasks. For all testing and game playing sessions, seating was adjusted so that participants were approximately 57 cm from the monitor. All PC-based tasks were programmed with the E-prime software (Psychology Software Tools, www.pstnet.com). All Mac tasks were programmed with the Vision Shell software (<http://www.visionshell.com/>).

Table 1

Demographic information for participants in each group, standard deviations are in parentheses

	Mean age	Years of Ed	Male/female
Expert	21.10 (2.51)	14.50 (1.90)	11/0
Novice	22.20 (2.70)	15.90 (2.80)	10/0
Control	21.40 (1.96)	15.25 (1.83)	0/19
MOH	21.35 (2.54)	15.10 (1.62)	2/18
TET	21.50 (1.88)	15.38 (1.22)	2/18
RON	21.74 (2.82)	15.41 (2.00)	3/20

Differential shading identifies groups with the same comparison. MOH, Medal of Honor; TET, Tetris; RON, Rise of Nations.

2.3. The games

2.3.1. Medal of honor

Allied Assault (Electronic Arts, 2002) is a World War II-based first-person shooter game. Players are presented with a first-person, egocentric view of a complex virtual environment. Like most first-person shooters, Medal of Honor (MOH) primarily focuses on combat. Players complete a number of missions in which they must kill enemies and avoid being killed. MOH is a fast-paced game that is perceptually demanding. The game requires players to successfully localize enemies (enemies can occur almost anywhere on screen, including in the far distance and in the periphery) and to deal with multiple enemies simultaneously or within close temporal proximity. Previous research (Green & Bavelier, 2003) found MOH to improve a number of visual and attentional abilities, so we expected MOH experience to improve performance in tasks that might involve these skills. MOH experience might also improve visuo-spatial working memory (remembering location and identity of objects in the environment) and performance in tests of executive function (switching between various goals such as killing enemies, navigating, and locating supplies).

Tetris (ValuSoft, 2004) is a popular puzzle game in which players rotate and move blocks descending from the top of the screen so that these blocks form lines at the bottom of the screen. After a complete line with no gaps is formed, the line disappears. If no lines are formed, the blocks pile higher and higher until the block pile reaches the top of the screen, at which point the game ends and the player loses. The goal is to keep the game going as long as possible by forming complete lines. As the game progresses, the blocks descend faster, giving players less time to choose where to place each block. Like MOH, Tetris is a fast-paced game. However, Tetris does not involve the same attentional demands as MOH. In Tetris, players are concerned with only one moving object at a time, whereas in MOH players attend to several enemies simultaneously. We used a version of Tetris that minimized planning by eliminating any preview of upcoming blocks (see Green & Bavelier, 2003). Previous research found Tetris improved some spatial skills (De Lisi & Wolford, 2002; Sims & Mayer, 2002), but Tetris experience did not appear to transfer beyond those specific tasks as much as MOH did.

Rise of Nations (Microsoft, 2003) is a highly complex real-time strategy game in which players develop and defend a civilization. In *Rise of Nations* (RON), players initially manage a single city of a primitive society. They collect resources to advance their civilization and build new structures, eventually building more cities and exploring the map. The resources (e.g., wood, stone, oil, food, and money) can be invested in a number of ways such as building a larger and stronger army or investing in technology. While players are building their civilization one or multiple computer players are building their own civilizations and these other civilizations occasionally attack and try to take over the player's cities. The game is won by either taking control of 70% of the map or completely conquering all other civilizations. This game was chosen over several other similar strategy games since RON offered the most complexity and the most opportunity for participants to engage in strategic behavior. Multi-tasking is an important component of game play since players must manage several cities, collect needed resources, defend their cities, and also attack enemy cities, all at the same time. Players must also switch among these tasks in response to their current situation (i.e., participants might need to switch from building a city to defending a city if a city comes under attack). Unlike MOH, game play tends to be slower and focuses more on planning and resource management rather than quick actions. Consequently, we predicted that RON would improve performance on tasks requiring executive control. Working memory also plays an important role in the game as partici-

pants need to keep complex goals and information in mind as they build their civilization. Spatial working memory may be especially important since participants must remember spatial locations on a large map of cities, buildings, and resources.

2.4. Practice and testing schedule

Participants in the longitudinal practice groups completed fifteen game sessions in the laboratory over a period of four to five weeks. The duration of thirteen of these game sessions were 1.5 h, but the duration of the first and last session was only 1 h (the remaining .5 h of those sessions were devoted to completing a portion of the cognitive battery described later. Participants assigned to the MOH and RON groups started game practice by completing a game tutorial. Given the relative simplicity of Tetris, participants were given a brief explanation of the game but did not complete a tutorial. This schedule resulted in a total practice time of 21.5 h for each participant in each of the longitudinal game groups. Given the number of practice hours required and the number of participants, all participants did not start in the study simultaneously, but instead participants were run in waves of 15 to 20 participants. Each wave did not overlap with the previous wave. Thus, all data were collected in the span of approximately six months.

Participants in the MOH group played straight through the game. Although difficulty was not explicitly adjusted, game play becomes more difficult as the game progresses. At the end of each session, game progress was saved and participants began the next session at this point. A variety of game scenarios were created for the RON group to play. These scenarios increased in difficulty, with participants first competing against one computer enemy, and later switching to two enemies after the first few games. These scenarios varied the nationality of the player and computer players, and also the geography of the map (each nationality within the game has particular strengths and weaknesses). If a participant did not finish a game within the session, the game was saved and participants started from this point at the start of the next session. Participants in the Tetris group played each game until completion, after which a new game was started. Participants started a new game at the beginning of each session.

Game performance was recorded for each participant. At the end of each mission, MOH lists the number of enemies killed, the number of hits taken, firing accuracy, and a number of other statistics. At the end of each RON scenario, scores relating to military and civic accomplishments are listed. Tetris reports two scores (total score and number of lines) at the end of each game. If a Tetris game was not completed due to the session ending, the score until that point was recorded and it was noted that this game was incomplete. On the final practice session, participants in the MOH and RON groups repeated the first scenario/mission completed at the beginning of the study to measure the degree of improvement in game performance.

Participants in the control group played no video games, but were tested on all cognitive tests three times. The time between each testing session matched that of participants who were in the MOH, Tetris, or RON game groups.

2.5. Expert and non-gamer schedule

Expert and non-gamer participants received no game practice and were not asked to play video games in the laboratory. These participants received only the cognitive tasks described below once (whereas the longitudinal groups received the battery three times: once before, once half-way through game practice, and finally once after all game practice was completed). Expert and non-gamer participants were not tested on the Tower of London

or the Ravens tests described below; otherwise they completed the same tasks, in the same order, as the longitudinal groups.

2.6. The cognitive battery

Longitudinal participants (including the control group) completed a battery of cognitive tests three times. The majority of each cognitive battery could be administered during two 1.5 to 2 h assessment sessions. Additionally, the first half hour of the first and last game practice sessions was also used for cognitive testing for participants in the game groups. The battery included a number of tasks that were completed in a fixed order, with each task taking 8–30 min to complete. Tasks fell into three general categories: (1) visual and attentional tasks, (2) spatial processing and spatial memory tasks, and (3) executive control tasks. Three of the visual/attentional tasks included in the battery (functional field of view, attentional blink, and enumeration) were those in which Green and Bavelier (2003) found to improve after ten h of MOH experience. All tasks are described below.

2.6.1. Visual and attentional tasks

2.6.1.1. Functional field of view. Participants searched for a white triangle within a circle (4.3° diameter) among square distractors ($4.3^\circ \times 4.3^\circ$) in a briefly presented (12 ms) display (see Green & Bavelier, 2003). Search items were arrayed in eight radial arms, and targets occurred with equal probability on each arm at eccentricities of 10, 20, or 30° from fixation. The search display was followed by a bright, colorful mask (100 ms). After this mask, a response screen containing lines representing the radial arms of the search display appeared and participants click the arm that had the target. After one block of practice trials (24 trials), participants completed 120 test trials.

2.6.1.2. Attentional blink (Raymond, Shapiro, & Arnell, 1992). Participants viewed a rapid sequence of letters (approximately 1° high) on a gray background at the center of the screen and reported two things about each letter sequence: (1) the identity of the one white letter in the sequence of black letters and (2) whether an X was present sometime after the white letter (50% of trials). Each letter appeared for 12 ms, followed by an 84 ms blank interval before the next letter. The sequence varied in length from 16 to 22 letters, with the white letter appearing unpredictably after either the 7th, 10th, or 13th letter. In this task, participants often fail to report the X when it appears approximately three items after the first target. Green and Bavelier (2003) found improved detection of the X following videogame experience, with the largest advantage occurring when the X occurred 3, 4, 5, or 6 letters after the first target, so we used these lags between the white letter and the X (with an equal number of trials for each). Participants completed 15 practice trials and 144 test trials.

2.6.1.3. Enumeration. Participants viewed briefly presented arrays of 1–8 dots (diameter $.25^\circ$) and indicated how many dots appeared. Dots were randomly positioned in a 7×7 matrix ($7^\circ \times 7^\circ$) with the exception that no dot could appear at the center location. Each trial started with a small fixation point at the center of the screen (900 ms), followed by a blank screen (600 ms), and then by the test array (50 ms). Participants entered the number of dots that appeared using the number keys at the top of the keyboard, after which the next trial began. Participants completed 32 practice trials followed by 160 test trials.

2.6.1.4. Multiple object tracking (Pylshyn & Storm, 1988). Participants initially saw seven green circles (distractors) and three red circles (targets). After a button press, the red items turned green and were identical to the distractors. Participants then pressed the right

arrow key and the circles started moving. Additional presses of the right arrow increased the speed of the objects, and pressing the left arrow slowed the objects. Participants tried to find the speed at which the circles moved as fast as possible while they could still keep track of the three target circles for at least five seconds (Alvarez & Franconeri, 2005). If participants lost track of one or more target circles they could show the targets (i.e., make the targets red again), slow the items down, and then hide them again. When participants found the correct speed they pressed the space bar. Participants repeated this procedure three times, and the speeds on these three trials were averaged. Participants were then tested on their ability to keep track of three out of ten circles moving at this average speed. During test, participants saw three red circles and seven green circles. Then the red circles turned green and all circles moved at the speed participants set. After 8 s, one circle in the display turned red, and participants were asked whether the red circle was a target (one of the initially red circles) or a distractor circle.

2.6.1.5. Visual short-term memory (Luck & Vogel, 1997). Participants viewed displays containing colored lines (red, green, blue, pink, and black) at different orientations (vertical, horizontal, tilted to the left, or tilted to the right). Each line measured $.2^\circ \times 1.6^\circ$ and had a center-to-center distance of at least 3.5° . Participants first viewed a display containing 2, 4, or 6 lines for 100 ms. This memory display was followed by a blank screen for 900 ms, and then a test display. On half of all trials, one item in the test display either changed color or orientation compared to the memory display, and participants indicated whether anything changed. Accuracy was emphasized over speed. Participants completed 24 practice trials and 144 test trials.

2.6.2. Spatial processing and spatial memory

2.6.2.1. Spatial 2-back. Participants viewed displays in which letters appeared one at a time at different spatial locations and they pressed one key if the letter was in the same location as the letter presented two items previously and a different key if it was in a different location (e.g., Braver et al., 1997). Letters measured roughly 1.75° and they appeared at one of ten equally spaced locations around an imaginary circle with a diameter of 15.5° (the center to center distance of adjacent locations was 5.0°). Each letter appeared for 500 ms with an inter-stimulus interval of 2000 ms. On 75% of trials the letter location was different from the location of the item presented 2-items back, and on 25% of trials the location was the same. Both speed and accuracy were stressed, and participants completed 100 trials.

2.6.2.2. Corsi block-tapping task (Corsi, 1972). Participants viewed displays of nine gray squares ($2.3^\circ \times 2.3^\circ$) in an irregular pattern on the computer screen. One at a time these items could change from gray to white, then back to gray. At the end of each trial they tried to click the boxes in the same order that they had changed. Participants completed four trials of sequence length 3, then four of length 4, etc. until they had completed four trials with length 9. We emphasized accuracy over speed.

2.6.2.3. Mental rotation (Cooper & Shepard, 1973). Participants tried to determine whether two simultaneously presented shapes were the same or different. They responded as quickly and as accurately as possible by pressing one of two keys. The shape on the right was either the same shape or a mirror image of the shape on the left, and the two shapes differed in orientation by 0, 45, 90, 135, 180, 225, 270, or 315° . These shapes were based on those appearing in Tetris. Each measured approximately $2.4 \times 2.4^\circ$ and was presented 3° from the center of the screen. Two shapes (the Z and the backwards Z shape) only appeared at orientations of 0, 45, 90, and 135 since any further rotation would cause the shape to ro-

tate into itself. All other shapes could appear equally often at each rotation from 0 to 315°. Participants completed 30 practice trials followed by 128 test trials.

2.6.3. Executive control and reasoning

2.6.3.1. Task switching (Pashler, 2000). Participants completed a task that required them to switch between judging whether a number (1, 2, 3, 4, 6, 7, 8, or 9) was odd or even and judging whether it was low or high (i.e., smaller or larger than 5). Numbers were presented individually for 1500 ms against a pink or blue background at the center of the screen, with the constraint that the same number did not appear twice in succession. If the background was blue, participants used one hand to report as quickly as possible whether the letter was high (“X” key) or low (“Z” key). If the background was pink, participants used their other hand to report as quickly as possible whether the number was odd (“N” key) or even (“M” key). Participants completed four single task blocks (2 blocks of odd/even and 2 blocks of high/low) of 30 trials each. They then completed a practice dual task block in which they switched from one task to the other every five trials for 30 trials. Finally, they completed a dual task block of 160 trials during which the task for each trial was chosen randomly.

2.6.3.2. Tower of London (Tunstall, 1999). Participants viewed an apparatus with three pegs and four discs and tried to rearrange the discs on the pegs to match a target arrangement shown in a picture. Only one disk could be moved at a time – a move consisted of shifting a disk from one peg to another. Participants tried to complete each problem within a certain number of moves, and had three chances to solve each problem. Participants attempted nine problems of increasing difficulty, and they were told that accuracy was important rather than speed. Solving a problem correctly on the first attempt merited three points. With each additional attempt, the number of points awarded was decreased by one. This test was administered once at the beginning of the study and once at the end of the study.

2.6.3.3. Working memory operation span (Turner & Engle, 1989). Participants solved math problems (e.g., $IS(9/1) + 2 = 9?$) while simultaneously trying to remember sets of 3–6 words. After each set of 3–6 words, participants were asked to recall the words in the set. Since this test was administered three times, three versions of the test were used.

2.6.3.4. Ravens matrices (Raven, Court, & Raven, 1990). Participants completed a version of the Ravens Advanced Matrices. This test involved presenting participants with a complex visual pattern with a piece cut out of it. The task of the participant was to find the missing piece that completed the pattern. The full version of the Ravens was divided into three sub-tests of approximately equal difficulty, with each test containing 12 items. During the first testing battery, participants were given 5 min to complete a practice version of the test before the first actual test. Participants were given 20 min to complete each 12 item test, once at the beginning of the study, once in the middle, and once at the end of the study.

3. Results

All group means for each cognitive task, when not reported in graphical form, are available in the form of an [online appendix](#) (see [Appendix A](#)). Given the large number of tasks and analyses, we discuss only critical effects and interactions. All ANOVA terms not reported in the results section are also available online. The central questions are whether expert and non-gamers differed in their perceptual and cognitive abilities, and whether practice on

a particular game had a differential effect on task performance on the tasks in the assessment battery over time – that is, whether video game experience led to greater task improvement, and whether the tasks affected by game experience differed depending on the type of video game. These questions were tested by examining whether expert and non-gamer performance differed on the tasks in the assessment battery, and second by testing within the game practice groups whether performance on the assessment battery tasks interacted with group (CONTROL, MOH, TETRIS, RON) and assessment session (Session1, Session 2, Session 3). To reduce the influence of within-participant outliers, we analyzed median rather than mean response times unless otherwise noted. Furthermore, if it was apparent from the data that a participant confused the response mappings or simply did not understand the task, data from that participant were not included in the analysis of that task. Practice blocks were not included in any of the analyses.

3.1. Game performance

Before analyzing the effects of transfer from the video games to the tasks in the assessment battery, we first examined whether practice led to improvement on the practiced game. Performance on the first level was compared before and after practice.¹ For MOH and RON players, the first level followed the game tutorials. Because Tetris had no tutorial, performance on the first day was treated as a “tutorial,” and the second session was treated as the first practice session.

All game groups improved significantly. For MOH, firing accuracy improved significantly (45% and 52% pre- and post-practice, respectively, $t(18) = 3.36, p < .01$) as did the ratio of hits taken to enemies killed (.72 and 1.01 pre- and post-practice, respectively, $t(18) = 2.43, p < .05$). Following practice, MOH participants hit targets more accurately and were more efficient. RON players also improved significantly. Initially, only 57% of participants achieved victory on the first scenario they were asked to complete. At the end of the practice period, 97% were victorious with the same scenario. Scores generally decreased overall. However, this was simply due to participants achieving victory too quickly to gain many points. Whereas initially it took participants on average 146 min to complete the scenario, post-practice it only took participants 56 min ($t(19) = 4.27, p < .001$). Although scores generally decreased, two scores increased significantly. Participants’ “territory” score, an index of how much land participants controlled at the end of the game, improved (473 and 583 pre- and post-practice, respectively, $t(19) = 2.81, p < .05$). Additionally, participants’ “wonders” scores improved as well (329 and 749 pre- and post-practice, respectively, $t(19) = 3.10, p < .01$). With an advanced enough society, participants may choose to build world wonders and are awarded points for doing so. Thus, with practice, participants were able to build larger, more advanced societies in less time, and also achieve victory more often. Finally, following practice, Tetris participants showed a large and significant improvement in total score (167,328 and 302,635 pre- and post-practice, respectively, $t(19) = 3.31, p < .01$).²

3.2. Visual and attentional tasks

3.2.1. Functional field of view task

Based on earlier results (Green & Bavelier, 2003), we expected the expert group to outperform the non-gamer group, and for MOH practice to differentially improve participants’ ability to detect

¹ Due to a computer error, game scores were lost for 1 MOH participant. Data from three RON participants were recorded incompletely and were not included in the analysis of game scores.

² Total score is reported, but all three measures of Tetris performance (score, level, number of lines) were highly correlated.

and localize the target. Although expert video game players enjoyed an advantage in this task compared to non-gamers, this effect did not reach significance ($F(1, 19) = .95, p = .34$). Furthermore, target eccentricity did not interact with expertise ($F(2, 38) = 1.30, p = .29$).

The performance of all longitudinal groups improved ($F(2, 152) = 120.79, p < .001$). However, participants who received MOH experience did not improve more than participants who received experience on Tetris or no video game experience at all: group did not interact significantly with testing session ($F(6, 152) = 1.57, p = .16$) nor with testing session and eccentricity ($F(12, 304) = .49, p = .92$).

3.2.2. Attentional blink

Based on earlier results (Green & Bavelier, 2003) we expected participants who played MOH to show the largest reduction in the attention blink effect and for experts to outperform non-gamers. That is, we expected them to show improved accuracy in detecting T2 (the second target) in the RSVP stream given that T1 (the first target) was correctly detected. Data from one expert participant were lost due to computer error.

Similar to FFOV data, although data were in the predicted direction, the difference between expert and novice groups was not significant when T2 data were entered into an ANOVA with lag (3, 4, 5, 6) as a within-participant factor and group (expert vs. non-gamer) as a between-participant factor ($F(1, 19) = 1.75, p = .20$). Additionally, there was no reliable lag \times group interaction ($F(3, 57) = 1.07, p = .37$), nor did experts outperform non-gamers in T1 detection (99% vs. 95% respectively, $F(1, 19) = 1.79, p = .20$).

Longitudinal data were entered into an ANOVA with T2 lag and testing session as within participant factors and group as a between participant factor. Participants demonstrated the classic attention blink effect. Accuracy improved across testing sessions ($F(2, 156) = 69.63, p < .001$), especially for early lags as indicated by a significant testing session \times lag interaction ($F(6, 468) = 5.71, p < .001$). However, group did not interact with testing session ($F(6, 156) = .90, p = .50$) nor with the testing session and lag ($F(18, 468) = .76, p = .75$). Thus, our data provide no evidence that participants in the MOH group gain any significant advantage from MOH experience over Tetris experience, RON experience, or no-game experience at all.

To rule out the possibility that differences in T1 performance masked T2 improvements, we compared T1 accuracy across testing sessions and groups. This analysis revealed no reliable effects of group ($F(3, 78) = .62, p = .60$), testing session ($F(2, 156) = 1.96, p = .15$), or group \times testing session interaction ($F(6, 156) = 1.31, p = .26$). Thus, the longitudinal groups did not differ reliably in their ability to detect T1.

3.2.3. Enumeration

Based on earlier results (Green & Bavelier, 2006b), we expected experts to outperform non-gamers and for MOH experience to enhance the ability to report the number of dots in a briefly presented display. Again, although numerically, experts outperformed non-gamers, this effect was not significant ($F(1, 19) = 2.10, p = .17$), nor did groups interact with the number of items displayed ($F(7, 133) = 1.10, p = .37$). Next, we examine the effect of game practice. Benefits of practice, if present, should occur when the number of objects in the display exceeds the subitizing range, as indicated by a session \times group \times number of objects interaction. An ANOVA revealed decreasing accuracy as the number of objects increased ($F(7, 539) = 132.18, p < .001$), no effect of testing session ($F(2, 154) = .50, p = .61$), and no effect of group ($F(3, 77) = .15, p = .93$). Critically, these factors did not interact significantly ($F(42, 1078) = 1.15, p = .24$) and no other interaction with group approached statistical significance (all p 's $> .80$).

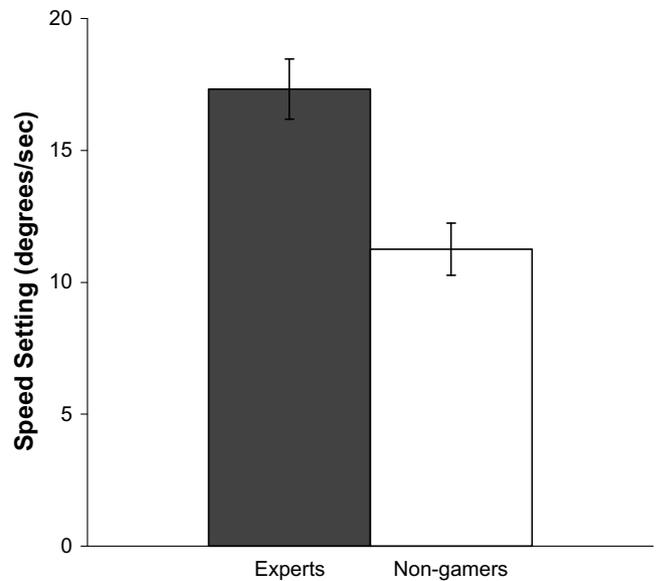


Fig. 1. Expert vs. non-gamer performance in the multiple object tracking task. Speed setting represents the maximum speed participants could track three items perfectly. Error bars represent plus and minus 1 SEM.

3.2.4. Multiple object tracking

The primary measure of interest in this task is the speed at which participants could track three items while maintaining near perfect accuracy. Three participants were not included in the current analysis due to experimenter error (two CONTROL, one RON).

Experts far outperformed non-gamers in their ability to track at higher speeds (Fig. 1, $F(1, 19) = 15.82, p < .001$). Accuracy did not differ significantly for expert and non-gamers ($F(1, 19) = .77, p = .39$, 94% and 98%, respectively). To further ensure that this speed effect was not influenced by a speed-accuracy trade-off (in the sense that experts were setting the speed higher at the cost of less accurate performance), the same analysis was performed for participants who only performed at 100% accuracy during testing. This analysis also resulted in a significant expertise effect ($F(1, 14) = 12.7, p < .01$).

Although experts outperformed non-gamers, a video game advantage was not evident in the longitudinal groups. Participants generally set the speed faster with repeated testing ($F(3, 75) = 4.96, p < .05$), but no significant group \times session interaction was present ($F(6, 150) = .73, p = .63$).³

3.2.5. Visual short-term memory

The primary measure in this task is the accuracy of change detection accuracy. Experts far outperformed non-gamers in this task, especially in the large set size condition as indicated by a significant group effect ($F(1, 18) = 19.72, p < .001$) and a significant group by set size interaction ($F(2, 36) = 3.63, p < .05$, Fig. 2).⁴ However, there was no effect of group ($F(3, 78) = .64, p = .59$), nor did

³ Participants were tested on their ability to track multiple objects at the speed setting they selected. Accuracy data suggest that some of the observed speed increases in the longitudinal groups might be due to an increasingly liberal bias in setting the speed. The proportion correct during this testing phase did not vary by group ($F(3, 75) = 1.16, p = .33$), but participants tended to be less accurate across sessions ($F(2, 150) = 2.99, p = .05$), with accuracy dropping from 97% at session1 to 95% at session3. This pattern was consistent across groups, with no significant testing session \times group interaction ($F(6, 150) = .50, p = .81$); the speed/accuracy trade-off did not differentially affect one group more than another.

⁴ One expert was excluded from this analysis. This participant appeared to have rested his hands too heavily on the response keys, resulting in the response keys being always depressed and chance performance.

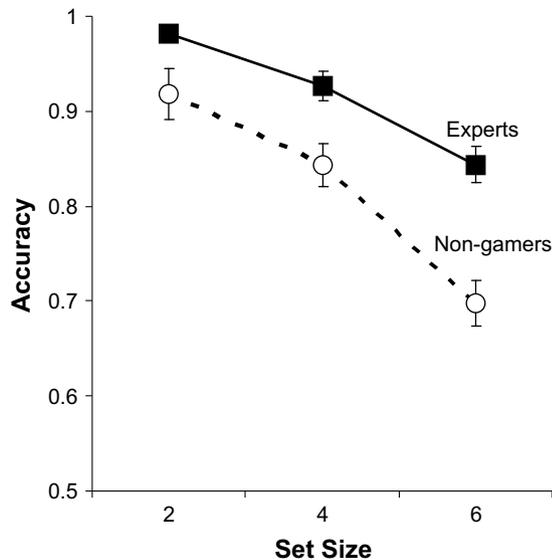


Fig. 2. Expert vs. non-gamer performance in the visual short-term memory task. Error bars represent plus and minus 1 SEM.

group membership interact with assessment session ($F(6, 156) = 1.38, p = .23$) nor with session and set size ($F(12, 312) = .91, p = .53$).

3.3. Spatial processing and spatial memory tasks

3.3.1. Spatial 2-back task

The primary measures of interest are the speed and accuracy in determining whether the spatial location of a letter was the same as the location of the letter presented two items earlier. Given the spatial aspects of Tetris, and to some degree RON, we predicted differential improvement on this spatial task. We also predicted that experts would outperform non-gamers in this task.

Experts showed a trend to be faster than non-gamers in this task (514 vs. 687 ms, respectively, $F(1, 19) = 4.18, p = .06$), but were no more accurate (90% vs. 90%, $F(1, 19) < 1, p = .99$). For the longitudinal groups, response times decreased with testing session ($F(2, 156) = 44.22, p < .001$) but the groups did not differ ($F(3, 78) = .23, p = .89$) and groups did not interact with testing session ($F(6, 156) = .65, p = .69$). In short, participants in all groups were faster with practice on the task, and game experience did not lead to differential improvements in response speed.

In terms of accuracy, there was no significant difference between groups ($F(3, 78) = 1.9, p = .14$). Unlike response times, accuracy remained relatively constant over repeated testing ($F(2, 156) = 1.52, p = .22$). Again, groups did not interact with testing session ($F(6, 156) = .38, p = .89$), meaning that game experience had no differential effect on accuracy. Overall, these results do not support the prediction that Tetris can differentially improve spatial memory ability.

3.3.2. Corsi block-tapping task

The primary measure in this task is proportion correctly recalled for each sequence at each set size.

No significant difference was observed between expert and non-gamers ($F(1, 18) = .71, p = .41$), and in general, performance was better for smaller set sizes ($F(5, 90) = 57.04, p < .001$). Overall, the performance of longitudinal participants improved with repeated testing ($F(2, 154) = 7.38, p < .01$) and was better for smaller set sizes ($F(6, 462) = 766.46, p < .001$). These factors did not interact significantly ($F(12, 924) = 1.21, p = .27$). Additionally, there was no effect of group ($F(3, 77) = .88, p = .48$), and groups did not

interact with set size ($F(18, 462) = .75, p = .76$), with testing session ($F(6, 154) = 1.43, p = .21$), or with set size and testing session ($F(36, 924) = .67, p = .93$). Video game experience did not enhance performance beyond what occurred solely due to repeated testing on this task. More specifically, Tetris, which involves a spatial memory component, did not enhance performance beyond the no-game control group.

3.3.3. Mental rotation

The primary measures in this task are the speed and accuracy of performance as a function of the extent of rotation required by the display. Given that Tetris requires mental rotation and that the shapes used in this task were modeled on those from Tetris, Tetris experience should lead to differential improvements in performance over time.

For expert and non-gamers, response times showed a classic mental rotation pattern, with slower responses up to 180° of rotation in either direction (Fig. 3, $F(7, 133) = 45.61, p < .001$). Although showing a definite trend, expert video game players were not significantly faster than non-gamers ($F(1, 19) = 2.51, p = .13$). Degree of rotation and groups did not interact ($F(1, 19) = .63, p = .73$). Accuracy data were similar, indicating a trend for video game players to be more accurate ($F(7, 133) = 2.95, p = .10$). Rotation and

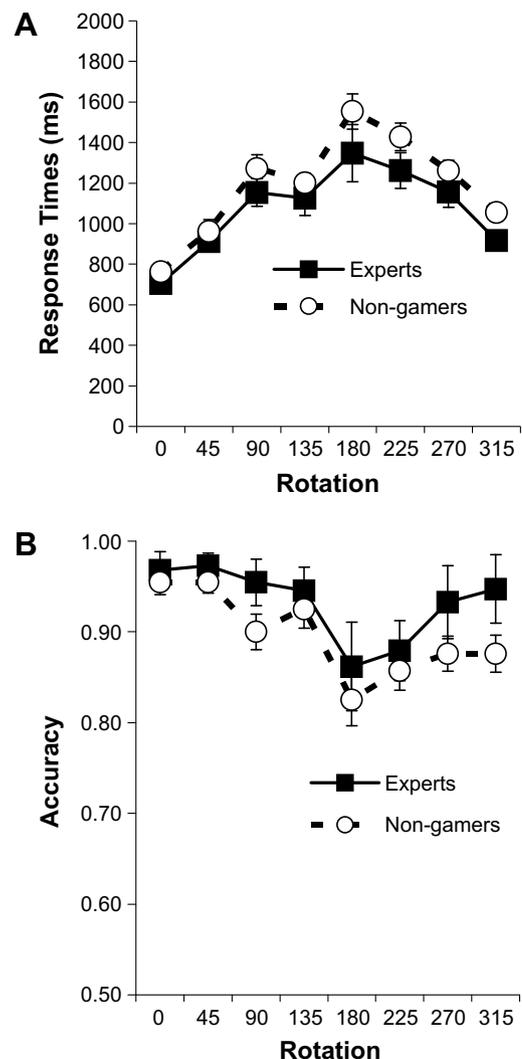


Fig. 3. (A) Response times for expert vs. non-gamers in the mental rotation task. (B) Accuracy for expert vs. non-gamers in the mental rotation task. Error bars represent plus and minus 1 SEM.

groups did not interact ($F(7, 133) = .44, p = .88$). Note that a one-tailed test would be justified giving the directional nature of our hypothesis, and such a test would lead to a near-significant and significant expertise effect for response time and accuracy, respectively. A composite measure of performance (Townsend & Ashby, 1983) that takes both speed and accuracy into account revealed a significant advantage for gamers over non-gamers ($F(1, 19) = 6.05, p = .03$). Given that this benefit was general (i.e., it did not interact with rotation, $F(1, 19) = 1.03, p = .42$), it does not appear to be related to mental rotation per se, but just to an overall advantage in processing and response speed.

As with the experts and non-gamers, the response times of longitudinal groups showed a classic mental rotation effect ($F(7, 546) = 184.01, p < .001$). Overall, participants were faster with repeated testing ($F(2, 156) = 107.57, p < .001$), and the testing session interacted with the extent of rotation such that response latencies improved more for greater extents of rotation ($F(14, 1092) = 11.24, p < .001$). Interestingly, though, this pattern interacted with the group ($F(42, 1092) = 1.64, p = .01$) – Tetris players improved most, especially when the shape required the most rotation (Fig. 4).⁵

Accuracy data mirrored RT data except for the absence of either a session by group interaction ($F(6, 156) = 1.15, p = .34$) or a session by group by rotation interaction ($F(42, 1092) = 1.05, p = .39$). Thus, the group effect was evident in the RT but not in the accuracy data. These results suggest that Tetris playing did differentially enhance performance on mental rotation (see also De Lisi & Wolford, 2002; Sims & Mayer, 2002). Note, however, that transfer in this case is limited, given that the mental rotation task was both visually and conceptually similar to the video game.

3.4. Executive function tasks

3.4.1. O-span

For this analysis three participants were excluded due to failure to comply with instructions (one TETRIS, two RON), two were excluded due to poor math performance (one TETRIS, one MOH), and two were excluded due to experimenter error (two CONTROL). The primary measure in this task was the number of correctly recalled words on each test. Expert gamers did not differ from novice gamers ($M = 33.5$ and 35.8 respectively; $F(1, 19) = .59, p = .45$). Overall, performance improved across testing sessions for the longitudinal groups ($F(2, 140) = 15.42, p < .001$). Critically, training groups did not interact with testing session ($F(6, 140) = 1.02, p = .41$), suggesting no effect of video game experience.

3.4.2. Tower of London

The primary measure in this task was based on the number of times participants solved the problem correctly and how many attempts they needed to do so. The task was completed only at the beginning and end of the game practice period. This task was not included in the expert/novice task battery. Overall, participants performed better the second time they performed the task ($F(1, 78) = 63.89, p < .001$). However, there was no differential improve-

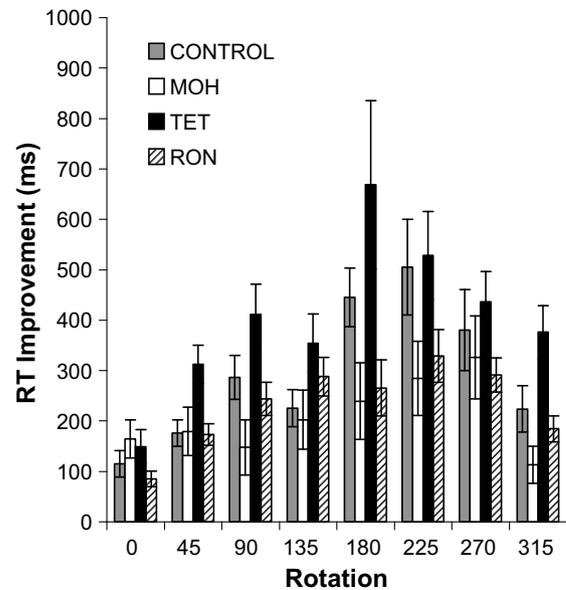


Fig. 4. Response time improvement in the mental rotation task for each rotation and group comparing session 1 and session 3. Error bars represent plus and minus 1 SEM.

ment as a function of group ($F(3, 78) = .722, p = .54$). Thus, game experience did not modulate this improvement.

3.4.3. Task switching

The primary measure in this task is switch cost during the dual task blocks: the difference in performance for trials when the preceding trial involved the same task and those when the preceding trial was of the other task. Switch costs were calculated by subtracting the response time for non-switch trials from the response time for switch trials. It was clear from error rates that three participants in the expert group, and one in the novice group confused the response mappings during at least on block of the task. Additionally, in the longitudinal groups four participants confused response mappings (one TETRIS, one CONTROL, two RON). Data from these participants were excluded. Experimenter error resulted in the loss of data from two participants (one MOH, one RON). It was predicted that switch costs should decrease more for participants given practice on videogames that require task switching compared to practice on games that do not or no game experience, and for expert video game players to outperform non-gamers.

Experts showed a smaller switch cost compared to non-gamers ($F(1, 15) = 5.87, p < .05$, Fig. 5). Group (expert vs. non-gamers) interacted with task (high/low vs. odd/even), suggesting that experts demonstrated a smaller cost than non-gamers, but primarily for the easier high/low task ($F(1, 15) = 4.40, p = .05$). A reduction in switch cost was not the result of lower accuracy, given there was no overall effect of group ($F(1, 15) = .08, p = .79$) or of an interaction between group and task ($F(1, 15) = .82, p = .39$).

Turning to the longitudinal groups, switch costs diminished with repeated testing ($F(2, 144) = 11.84, p < .001$). However, the groups did not differ ($F(3, 72) = 2.07, p = .12$) and groups did not interact with testing session ($F(6, 144) = .73, p = .63$) or with testing session and task ($F(6, 144) = .82, p = .55$). Thus, there is no evidence that video game experience had any effect on the ability to quickly switch between two tasks.

In general, accuracy data mirrored the response time data, with switch costs generally declining across sessions ($F(2, 144) = 91, p < .01$). Again, there was no effect of group ($F(3, 72) = 1.19$,

⁵ The observant reader might notice in the online appendix that although Tetris players improved most, they were initially slower compared to the other three groups. This is almost exclusively due to three participants in the Tetris group with abnormally long response times and steep mental rotation slopes in this task. Excluding these three participants from analysis equates response time baselines almost perfectly, and the time x group x rotation interaction remains significant ($F(42, 1050) = 1.56, p < .05$). From Session 1 to Session 3, this interaction is evident by Tetris players improving significantly more compared to RON players (at the .05 level) at rotations of 45, 90, 180, 270, and 315°, MOH players at rotations of 90, 180, and 315°, and CONTROL participants at the 45° rotation. Thus it is unlikely that differences in initial baseline performance can explain differential improvement in the mental rotation task. No significant group effects or interactions with group were observed in the accuracy data.

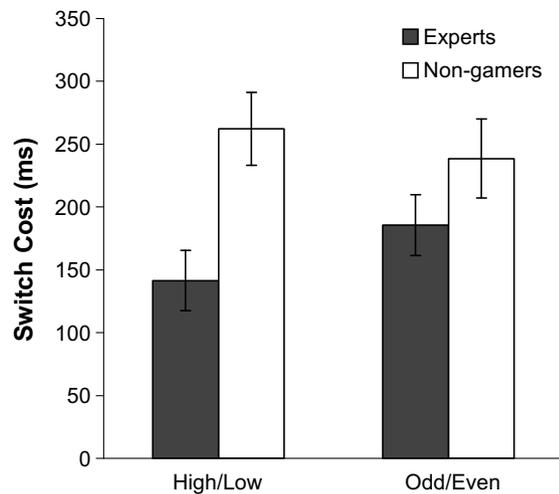


Fig. 5. Expert vs. non-gamer performance in the task switching task (response time cost). Error bars represent plus and minus 1 SEM.

$p = .32$) and group did not interact with assessment session ($F(6, 144) = 1.59, p = .15$).

3.4.5. Ravens matrices

For each 12-problem test, training participants were given one point for each problem solved correctly. Ravens scores were entered into an assessment session (session 1, session 2, session 3) by group ANOVA. This ANOVA revealed no effect of session ($F(2, 156) = .25, p = .78$), no effect of group ($F(3, 78) = .60, p = .62$), and no interaction between session and group ($F(6, 156) = 1.17, p = .33$). Thus, game experience did not appear to modulate performance.

4. Discussion and conclusions

Two basic questions were investigated in our study. First, whether video-game experience results in broad and differential improvements in perceptual and cognitive abilities, as could be identified by an examination of the interactions between longitudinal groups, assessment session and task. In addition, we were also interested in establishing whether habitual gamers (experts) and non-gamers differ in their performance on the same battery of transfer tasks used with the longitudinal groups. Because experts and non-gamers differed greatly in the amount of time they had spent playing video games, a comparison of these two groups can in principle address the question of whether some differences may only emerge with much longer game practice periods that can be afforded by an experimental study.

In a number of tasks, video game experts outperformed non-gamers. Experts were able to track objects moving at greater speeds, perform more accurately in a visual short-term memory test, switch between tasks more quickly, and make decisions about rotated objects more quickly and accurately. However, with the exception of Tetris, practicing video games for twenty-one h was not enough to engender benefits in non-video game players. Action or strategy game practice regimens did not significantly improve performance on any of the transfer tasks over and above improvement related to performing the transfer task multiple times (i.e. the control group).

Interestingly, even tasks in which video game experience has been found to be beneficial in the past did not reveal significant video game effects, including the FFOV task, the attention blink task, and the enumeration blink task (e.g., Feng et al., 2007; Green & Bavelier, 2003; Green & Bavelier, 2006a; Green & Bavelier,

2006b). The fact that experts did not perform significantly better on these tasks as compared to non-gamers (although the expert data do indicate clear trends, see online data appendix) suggests that differences between our tasks and tasks used previously by other researchers may be playing an important role. For example, it is known that factors such as the intensity of the mask relative to the intensity of the target and the time between the offset of the target and offset of the mask can have large effects on the effectiveness of a visual mask (Fehrer & Smith, 1962; Macknik & Livingstone, 1998). This may account for the discrepancy between our FFOV results and the results of others. The masking stimulus in our FFOV task was of a much shorter duration (100 ms) and was bright and colorful (compared to the black and gray mask used by Green & Bavelier, 2003; Green & Bavelier, 2006a). Masking in our task may have been more similar to masking by light as compared to metacontrast masking or pattern masking (see Breitmeyer & Ögmen, 2006 for a review of visual masking). Additionally, the nature of the mask may have led to stronger backwards masking, which is supported by the much lower initial performance compared to previous reports (e.g., Green & Bavelier, 2006a). Thus the masking of the target may have been quantitatively or qualitatively different from the masking which is amenable to video game expertise effects. Future work needs to determine the exact mechanisms that allow video game experts and those trained to play video games to better localize briefly presented targets in the periphery, perhaps by varying stimulus properties of the task. Interestingly, although the mask was very effective, performance improved substantially with repeated testing. Participants were learning something about how to better perform this task, which may have obscured our ability to detect video game experience effects.

Other task differences may also have played an important role. In the attention blink task, we restricted the lag between the first target and the second target to be between lags 3 and 6, where the greatest video game effects have been observed previously (Green & Bavelier, 2003). However, we may have unintentionally reduced our ability to observe video game training effects by reducing the temporal uncertainty of the second target. It is more difficult to explain the lack of significant training effects in the enumeration task given the simplicity of the task and the close replication of experimental paradigm. Our version of the task only included set sizes from 1 to 8, whereas previous video game experiments have used set sizes up to 12 (but only analyzed and reported data up to 8). Additionally, in our task, accuracy was emphasized over speed for two reasons: 1) video game players have faster key press responses overall (Orosy-Fildes & Allan, 1989), and in general may be more familiar with the layout of the numeric keys of the keyboard and 2) Green and Bavelier (2003), Green and Bavelier (2006b) have reported effects of game experience on enumeration for accuracy only. Our emphasis on accuracy is in contrast to previous work emphasizing speed or response, which may give video game players an additional advantage as compared to non-gamers (Green & Bavelier, 2003; Green & Bavelier, 2006b). As for the multiple-object tracking task, we did observe expert/non-gamer differences but did not observe game practice effects. However, it is important to note the large differences between the MOT paradigm we used, which measured tracking speed, and the paradigm used by Green and Bavelier (2006b), which measured tracking capacity.

Could differences in the schedule of game practice sessions have an effect on the degree to which practice transferred to perceptual and cognitive tasks? It is possible, but in our opinion, unlikely. We had participants visit the laboratory four times a week (on separate days), for 1.5 h each session, while Green and Bavelier (e.g., Green & Bavelier, 2006b) had participants complete ten 1-hour sessions within the span of 15 days. Thus, sessions were more spaced-out

in time for our participants (although participants played more during each individual session, and completed a greater number of sessions). However, many results suggest that in a number of different contexts, a more spaced or distributed practice schedule is actually more effective (e.g., *Baddeley & Longman, 1978; Shebilske, Corrington, & Jordan, 1994*). Currently, little is known regarding how the schedule of video game practice affects transfer to other perceptual and cognitive abilities, but this is an interesting and meaningful question for future research.

Could gender be an important factor in our observation of no differential task improvement as a function of video game practice? Whereas previous studies have used a mixed group of males and females (e.g., *Green & Bavelier, 2003; Green & Bavelier, 2006a; Green & Bavelier, 2006b*), our longitudinal game groups were comprised mainly of female participants. Gender differences in spatial cognition are well known (e.g., *Casey, Nuttall, Pezaris, & Benbow, 1995; Geary, Saults, Liu, & Hoard, 2000; Terlecki & Newcombe, 2005*). However, it is very unlikely that the lack of transfer observed is the result of a primarily female sample. *Feng et al. (2007)* observed a larger game practice benefit for female participants in the FFOV task and a mental rotation task as compared to male participants. In fact, video game training was able to completely eliminate gender differences in the FFOV task (while in the mental rotation task, differences were reduced substantially). It is interesting to note that our own FFOV accuracy data appear to replicate this pattern of gender differences. When non-gamer males and females are compared directly, males significantly outperform females ($F(1,88) = 11.07, p < .01, M_s = .35$ vs. $.25$ for males and females, respectively). Yet, action game practice did not differentially improve performance in our study. Ruling out gender differences as a factor suggests again that stimulus and task factors are likely the explanation for why we observed little transfer of practice while previous studies have found substantial and significant video game effects.

Could methodological shortcomings of our study have an impact on our ability to observe transfer effects? This is a possibility, but again, we feel that this is unlikely. Given the multitude of tasks we asked participants to perform, it is possible that similar tasks in the cognitive battery trained each other and this may have been responsible for the general response time and accuracy improvements across sessions we observed. These general improvements may have masked any effect of video game practice. Furthermore, multiple assessment sessions (three rather than the usual two typically used by *Green and Bavelier*) may have had an influence as well. Our purpose in choosing three assessment points was to gain information about the rate of improvement over time and the dose-response function of video game experience. However, greater experience with the transfer tasks themselves might have masked game effects (although if this were the case, we would have observed differential improvements from session 1 to session 2, which were not present). The quite substantial improvements in response time and accuracy that were often observed (e.g., in the FFOV task, the attention blink task, and the mental rotation task) from simply repeated testing may have practical implications. When it comes to improving performance on a particular task, practice or repeated experience with that task may prove the most efficient route compared to a general video game practice intervention.

The inability to observe transfer effects and strong expertise effects in tasks similar or nearly identical to tasks that have exhibited training effects in the past may be important in understanding the nature of video game effects and their ultimate practical implication. Whereas it is difficult to pinpoint the exact reason for our regimens inability to produce significant improvements on these tasks, our results suggest that there exist important boundary conditions on the effectiveness of the use of video games to improve performance on other tasks. What may seem like inconsequential

procedural and stimulus changes can significantly alter the degree to which video game experience transfers to other tasks. This raises concerns regarding whether video game practice may transfer, not only to laboratory tasks, but also to the complex and dynamic tasks we perform every day outside the laboratory. It is also unclear exactly how efficient video game interventions, with the purpose of improving perceptual and cognitive performance, may be in certain situations. It would be ideal to have data on how much video game experience is required to improve performance on a transfer task to a certain level as compared to how much practice on the actual transfer task is required to reach the same level of performance. To our knowledge, the only evidence published in a journal of video game experience transferring to complex, real-world tasks has been the case of Space Fortress training improving the flight performance of Israeli air force pilots (*Gopher et al., 1994*).

For the tasks in which video game experts outperformed non-gamers, twenty-one h of video game training did not produce these effects except for the case of mental rotation. It is entirely possible that, in order to see these benefits, many more hours of experience are necessary (but see *Green & Bavelier, 2003* in which only 10 h of training were needed to achieve significant transfer). Most experts in our expert group reported playing video games starting from very early childhood. This means that it is likely that these participants had tens of thousands of hours of video game experience when we tested them. Of course it is important to note that other explanations may account for the higher transfer task performance by video game experts. For example, video game experts may demonstrate superior perceptual, attentional, and cognitive skills due to self-selection. These skills may encourage video game expertise given that they are required for successful gaming. Other non-causal mechanisms might be speculated as well, including differences in households in which video game systems are present. Video game expertise may in fact be a complicated confluence of causal and non-causal variables.

Although it may be the case that video games can produce broad transfer to a number of tasks, it is important to understand why transfer might be broad (or appears to be broad). The answer to this question has important implications for theories of learning and expertise. One potential explanation is that video game training encourages flexible strategies and results in general improvements in attentional control, which can in turn be applied to a number of different tasks, as is the case with variable priority training (e.g., *Fabiani et al., 1989; Gopher, Weil, & Siegel, 1989; Kramer, Larish, & Strayer, 1995*). The complexity of modern video games, which typically have many different goals and sub-goals, would appear to encourage strategies centered on dynamically shifting attention to different elements of the game. Another possibility is that transfer occurs due to the same skills being critical to both the game and transfer task. However, the key may be that broad transfer is observed because complex video games require the same skills to be executed in a variety of different contexts (*Schmidt & Bjork, 1992*). As an example, action games require participants to track multiple moving enemies, much in the same way that participants must track circles in a multiple object-tracking task. However, in the game different enemies might have many different speeds or ways of moving, encouraging participants to generalize the skills they learn in the gaming context to novel stimuli. Answering such questions would require either the deconstruction of complex video games into simpler sub-games or strategy manipulations to encourage participants to emphasize certain aspects of the game over others, much in the same way Space Fortress training and transfer has been studied (e.g., *Fabiani et al., 1989*).

The exact mechanisms of improved performance on video games and transfer tasks is still somewhat uncertain (i.e., what

is it that players actually learn from playing video games). In an interesting series of papers Maglio and colleagues (Kirsh & Maglio, 1994; Maglio & Kirsh, 1996; Maglio, Wenger, & Copeland, 2008) examined expertise in Tetris. Tetris expertise was related to strategy shifts that utilized epistemic actions, or actions that decreased mental computations (e.g., rotating Tetris blocks on the screen rather than in the mind). Given that our mental rotation task did not allow for the use of such epistemic actions, it is unlikely that this strategy shift is the explanation for improved performance in the transfer mental rotation task. However, it is possible that participants used some combination of epistemic actions and more efficient mental rotation to improve their Tetris performance, and our transfer task tapped only mental rotation ability. Unfortunately, our battery of cognitive tasks did not have a clear task in which measures of epistemic actions are available, but it would be interesting to examine whether Tetris training might make participants to utilize such strategies in other tasks as well. Strategies learned during video game play (in addition to improved visual and attention processing) may be an important and neglected factor in explaining video game expertise effects.

To date, video game training appears to be one of the more interesting and promising means to improve perceptual, attentional, and cognitive abilities. One of its promises is that, compared to traditional training, it can be engaging and entertaining. This has led some companies to begin to market video games for the specific purpose of improving cognition. For example, Nintendo advertises Big Brain Academy™ as a game that “trains your brain with a course load of mind-bending activities across five categories: think, memorize, analyze, compute, and identify” (Nintendo, 2008a). Players are assigned a “brain weight” score based on their performance, with a higher brain weight corresponding to better performance. Nintendo states that these puzzles are “designed to help you increase the weight of your mighty brain”. Brain Age™, another game developed by Nintendo that appears specifically targeted to older adults, makes the claim that players can “train their brain in minutes a day” (Nintendo, 2008b). Like Big Brain Academy™, players initially receive a performance score known as their “brain age”. This score is based on response speed and accuracy on a number of simple reaction time and perceptual tests. As players train on these tasks their “brain age” score decreases, implying that the training transfers broadly to other aspects of brain function.

However, it is clear that much more research needs to be conducted before researchers might recommend a certain game to an individual to improve performance on a task of interest. For example, it should be noted that the reaction time, logic, and perceptual training offered by these “brain training” games bear a striking resemblance to the training tasks of the ACTIVE trial (Ball et al., 2002), which produced little meaningful immediate transfer to real-world tasks (in contrast to the visual search training paradigm developed by Ball and colleagues which transfers to driving performance, Roenker, Cissell, Ball, Wadley, & Edwards, 2003). Thus, buying one of these games for the purpose of improving ones cognitive abilities may be premature. While our laboratory has demonstrated that older adults who practice the game Rise of Nations show significant improvements in tasks measuring memory, task-switching ability, reasoning ability, and spatial skills, (Basak, Boot, Voss, & Kramer, in press), future research should investigate whether these gains transfer to complex, real-world tasks. It should also investigate the dose-response curve for video game experience and potential benefits, the relationship between the nature of the game and the nature of transfer, and the exact mechanism or mechanisms that improve task performance, including potential interactions with individual differences and age.

Appendix A. Supplementary data

Supplementary data associated with this article can be found, in the online version, at doi:10.1016/j.actpsy.2008.09.005.

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