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Examining the Relationship Between Action Video Game Experience and Performance in a Distracted Driving Task

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Abstract We conducted two experiments to assess the hypothesis that experienced action video game players will exhibit superior performance in a distracted driving task. In the first experiment, experienced gamers and controls drove in a driving simulator, with and without distraction. Experienced AVG players exhibited fewer lane deviations during driving as compared to non-gamers; however, video game experience was not associated with fewer lane deviations while distracted. These results showed evidence for the video game experience effect however, no evidence of improved cognitive ability was found. In the second experiment, we informed participants of the hypothesis to replicate the methods of studies that do not mask the purpose of the research. We found video game experience again was associated with fewer driving errors, but was still not associated with better driving performance while distracted; however, gamers recalled more details of the distracting conversation and reported lower workload while driving than non-gamers. We use these results to argue for caution in interpreting research with experienced gamers and increased replication with attention paid to recruitment methodology within this research domain. Finally, our results indicate that understanding the nature of AVG experience on task performance requires careful attention to motivational factors.

Keywords Videogame playing · Distracted driving · Action games

The study of Action Video Game (AVG) players and the hypothesis that they possess superior cognitive abilities is a lively research topic that has recently generated considerable attention. There is growing support for the idea that individuals with extensive experience playing AVGs possess superior executive functioning and other cognitive abilities, under the presumption that intensive AVG play engages and

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strengthens attention and executive control to engender broad cognitive improvements (e.g. Green et al. 2012); we call this the AVG experience effect.

There has been, however, some criticism of the nature and extent of this claim, in particular with regard to the generalizability of some of the research findings to real-world performance. Two such issues are lack of replicability across studies (Boot et al. 2008), and lack of real world tasks. Many studies supporting the AVG experience effect tend to use computer based laboratory measures of cognitive function exclusively, including executive control (Dye et al. 2009; Green et al. 2012), visual function (Li et al. 2009), reaction time, and accuracy (Mishra et al. 2011). It is to this latter issue that we wish to draw more attention in the current work.

There may be at least three hypotheses to consider in terms of the AVG experience effect. The first, and strongest, is that training on AVGs can be used to improve cognitive functioning and cause broad improvements in cognition. Overall improvements such as these would be expected to transfer to any and all tasks that use cognitive resources. For example, if a game improved executive control, then the improvements in this aspect of cognition would be manifest not just on laboratory measures of executive function, but also in any real world task that involved executive control.

A second, more moderate form of the AVG experience effect is that increased game experience may stimulate the player to generate efficient and effective strategies that can be used to improve performance within the game or other games. Additionally, these strategies can also be applied across a variety of situations such as on cognitive tests and in real world situations (e.g. Greenfield et al. 1994). This explanation of the AVG experience effect emphasizes that the game leads to developments in strategy, rather than developments in overall cognitive skill. For example, while playing players may learn which resources are most the valuable or which locations are the most common for threats and develop specific eye scanning patterns to those locations. Strategies such as these may lead to an advantage during play; however, the same scanning patterns used in game may also be useful in the real world in certain contexts. This was the case with one study that found more experienced video game players had broader search patterns than those with less skill and this strategic improvement assisted them in being more proficient at a change detection task (Clark et al. 2011).

Lastly, it may be that the nature of the AVG experience effect is that increased game experience only improves performance within the game, and on other, very similar tasks. In this view, the game serves as a form of task-specific skill training. Many computer-based tests of cognitive function may be game-like in nature, in the sense that such tests can involve displays depicting artificial stimuli and requiring speeded and/or accurate responses, and often involve the same hardware and controllers used with games (e.g., personal computers, keyboards, mice, etc.). Thus, it may be that improvements on such tasks reflect a much nearer transfer-of-training effect involving task specific skill, rather than strategic or cognitive skills and thus would not be expected to transfer to real world tasks. This is not necessarily the case, but one potential limitation to video game based studies.

We must ultimately ask to what degree does the AVG experience effect as measured in the lab relate to real world performance? Do experienced AVG players perform better in cognitively demanding jobs, such as floor trading on Wall Street, working as head chef in a busy restaurant, and so on? Do experienced AVG players perform better in cognitively demanding everyday activities such as carrying on conversations with multiple people at a party or other noisy environments, bargain shopping in a crowded supermarket while attending to the needs of restless children, and so on? Or perhaps do they only perform better in cognitively demanding tasks that involve control-display interfaces that might be seen as game-like in nature, such as air traffic control, piloting aircraft or other complex vehicles, perhaps also including driving while engaged in secondary tasks such as talking on a cell phone. Ultimately, this concern over real-world tasks is one related to the generality of videogame based cognitive training (e.g., Gaspar et al. 2013; Irons et al. 2011; Sims and Mayer 2002). Does the experience of playing AVGs make one better only at playing other AVGs or tasks that are similar to video games, or can they transfer to more real world tasks? Furthermore, if AVG experience does transfer to real world tasks, do players achieve these performance improvements by applying strategies or knowledge learned from playing AVGs or from improvements to overall functioning?

Testing the predictions of both the cognitive-skill and the strategic development hypotheses is challenging; it requires careful selection and design of both the game-based training and the performance tests. However, the task-specific improvement hypothesis can be distinguished from these other two simply by assessing the degree to which AVG experience is associated with performance on real-world, and hence not game-like, tests. While some have argued in favor of the cognitive transfer effect (e.g. Bavelier 2011) we have identified only four studies that have addressed the issue of real world tasks. Of these, only one found evidence of an AVG experience effect, which was assessed in a distracted driving task (Telner et al. 2009). However, another study of AVG experience using a distracted driving task failed to replicate the effect (Donohue et al. 2012). In another real-world task, researchers found no effect of gaming experience on the task of crossing the street while talking on a phone (Gaspar et al. 2013). Another study found a gamer advantage for navigation in a virtual environment, but that effect failed to transfer to a similar navigation task in the real-world (Richardson et al. 2011).

Given the increasing attention given to the AVG experience effect in psychological research, the importance of replication, and insufficient research aimed at establishing the reliability of AVG player superiority in real life tasks, we aim to expand the research in this domain.

Experiment 1

In Experiment 1, we sought to investigate the claim that the improved cognitive and perceptual skills of AVG players would generalize to a real-world task, namely distracted driving. We expect that if individuals' prior AVG experience does transfer to the driving task it could do so in three ways. Consistent with the cognitive skill explanation of the AVG experience effect, the improved cognitive abilities of the experienced gamers will provide them with the executive control and attentional resources needed to operate the driving simulator effectively, which includes monitoring speed and lane position, while observing traffic. Hence, they should exhibit fewer driving errors. Further, when the distraction manipulation is introduced, their improved attentional resources should enable them to manage the cognitive load of the conversation while driving. This leads to the prediction that the effect of distraction should be less pronounced among the experienced gamers compared to others.

Second, the strategic skill explanation of the AVG experience effect would predict that experience with games might engender strategies for game-like tasks. In some respects, the simulator is a game-like task, as it involves controlling a moving point-of-observation through a three-dimensional virtual world, similar to many first-person view video games. Strategies might involve an acquired understanding of control-display dynamics, giving the experienced gamers an understanding of how to best control their navigation in the simulator. This strategy is an application of acquired knowledge and familiarity with simulated environments, rather than overall cognitive functioning, and thus would not predict any particular gamer advantages when faced with a distracting task. The task-specific skill explanation of the AVG experience effect would make the same prediction, though for a different reason. In this case, the hypothesis would predict that experience with first-person based games, and perhaps driving games in particular, have trained specific navigational and steering skills in virtual environments. From this view, the driving simulator is just a driving game. Again, no specific gaming advantage would be expected in terms of distracted driving. In addition to measuring driving performance, we also employ subjective workload measures, which may help reveal differences in the cognitive performance of the participants during the tasks.

Method

Participants

Forty-two adult males with normal or corrected to normal visual acuity, color vision, and contrast sensitivity participated in the study. We recruited participants at a south-eastern university in the US and received partial course credit for their participation. At the time of recruitment, participants completed an online survey screening which contained demographic, motion sickness history (MHQ), and video game experience questionnaires. As part of the video game experience survey, participants indicated the average hours per week, over the past 6 months that they had played action video games. We used these reports to split participants into three groups (non-player (NAVGP), low player (LAVGP), & high player (HAVGP)). We took effort to mask the purpose of the research and minimize demand characteristics. The video game questionnaire was integrated with the MHQ, and respondents were led to believe that experience with certain games was relevant to motion and simulator sickness. We strived to divorce the video game screening from participation in the study. We required participants to wait a minimum of 5 days between filling out this questionnaire and participating in the study; with the goal of minimizing any potential priming effects that may arise from any connection between video game experience and study performance (e.g. Boot et al. 2011) that may arise from exposure to questions about video game experience. Finally, to further obscure this conflation we led the participants to believe that the purpose of the research was to test the calibration of the driving simulator.

NAVGPs reported no hours of AVG play, LAVGPs reported between 1 and 6 h of play per week (M=3.00, SD=1.52), and HAVGPs reported between 10 and 30 h of game play per week (M=14.82, SD=7.70). Both LAVGPs and HAVGPs stated experience with at least one or more of the following games (most participants played a combination): *Call of Duty*: *Black Ops*, *Modern Warfare*, *Assassin's Creed*, *Counter-*

Strike Source, *Battlefield 3*, *Doom 3*, *World of Warcraft*, *Halo 2* and *3*, *Halo Reach*, and *Skyrim*. We show the demographic, driving, and game experience self-reports in Table 1.

Materials

We used Motion History Questionnaire (MHQ) and Simulator Sickness Questionnaire (SSQ) to both measure participants tendency to become sick during the study and as an in-situ measure of simulator sickness throughout the study to reduce the likeliness of simulator sickness (Kennedy et al. 1992, 1993). Participants completed Snellen far acuity, contrast sensitivity, and Ishihara color blindness measures on an Optec vision screener (Stereo-optical Co.). The NASA Task Load Index (NASA-TLX), a multi-dimensional rating scale based on six subscales (mental workload, physical workload, temporal workload, subjective rating of performance, effort and frustration) was used to capture self-reported workload (Hart and Staveland 1988). We utilized a GE I-SIM PatrolSim Mark-II+ high fidelity fixed-base driving simulator for the driving task. The simulator was equipped with three projection screens. One screen sat directly in front of the participant, with two screens adjacent on each side. These present the driving environment from the perspective of the driver, which includes the left and right side mirrors and partial windows, complete front windshield and rearview mirror displayed at a resolution of 1280 × 1024 pixels. The simulator presented driving scenarios on three screens using three separate projectors (one for each screen). These screens all have the same dimensions (0.98 m vertical × 1.07 m horizontal). The participant was free to adjust the driver's seat for comfort. Due to the variability of seat position, the participants are located approximately 1.32 m from the side screens and 1.54 m away from the center screen. The visual angle of the display ranged from 25.00 to 40.00 deg. vertically and 99.00 to 115.00 deg. horizontally, with variation dependent on seat adjustments made by the participant. The simulator also has the dashboard, instrument panel, seat, steering wheel, and seatbelt of a 1990's era Ford Crown Victoria sedan; in short, the simulator was a replica of the real vehicle; however, the simulator did not replicate the vibration and motion cues of a real vehicle.

Design

The study was a 3 × 3 mixed factorial design. The between-subject factor was level of AVG play (None, Low, High) and the within-subjects factor was the distraction condition for driving (Not Distracted, Distracted, and Not Distracted). Two non-distraction drives were used to create a pre-test/post-test design instead of counterbalancing due to the concern that practice effects may be asymmetrical and create a confound. Prior to beginning the research, we conducted an a priori power analysis in order to decide on an appropriate sample size. We set the desired power (1 - β) at 0.95 and $\alpha = .05$, based on a two-tailed test, which suggested a total sample size of 33 participants, which was exceeded in this study.

For the distraction condition, we used a hands-free wireless phone device, on which participants held a conversation. During this condition, we asked the participants questions of varying difficulty, which were randomized and prerecorded. The questions asked were all open-ended and required participants to recall past events, such as

Table 1 Means and standard deviations for subject demographic variables in Experiment 1 and 2

Variable	Experiment 1			Experiment 2		
	NAVGP (n=11) *M* (*SD*)	LAVGP (n=20) *M* (*SD*)	HAVGP (n=11) *M* (*SD*)	NAVGP (n=12) *M* (*SD*)	LAVGP (n=12) *M* (*SD*)	HAVGP (n=12) *M* (*SD*)
Licensed to drive (years)	4.60 (3.94)	4.60 (3.11)	3.10 (1.81)	3.88 (6.11)	3.04 (1.71)	2.80 (1.05)
Miles driven (miles/year)	15,818 (4251)	11,648 (6561)	14,369 (6516)	11,000 (8068)	10,500 (6665)	9875 (3681)
AVG gameplay (hr/week)	0.00 (0.00)	3.00 (1.52)	14.82 (7.70)	0.00 (0.00)	4.58 (1.44)	12.68 (5.19)
AVG preference	4.45 (1.86)	6.10 (1.29)	6.40 (1.02)	4.58 (1.50)	6.00 (1.48)	6.83 (0.83)
Driving gameplay (hr/week)	3.18 (1.83)	2.33 (1.67)	3.09 (1.22)	2.25 (1.14)	2.75 (1.82)	2.00 (1.41)

AVG preference: 1 = strongly avoid, 7 = strongly prefer

AVG action video games, *NAVGP* non-gamers, *LAVGP* low-play gamers, *HAVGP* high-play gamers

“What did you do last Tuesday?” use working memory to compile a list of objects, such as “What are three items you could not live without?,” or recall information, such as “Please recite the last ten letters of the English alphabet backwards starting from Z.”. We instructed the participants to think about the question before responding and to maximize performance on both the conversation and the drive. We designed the questions so that participants experienced a continuous conversation; as soon as a participant stopped speaking, we asked them another question and this procedure was repeated until the end of the drive. Although the participants answering pace could vary slightly due to individual differences, research assistants presenting the questions to the participants kept the pace of the questions as similar as possible for each participant. The simulator recorded a video of each drive for every participant; two trained independent raters coded these videos. We calculated their level of agreement for each session. These raters coded lane deviations, defined as the number of times the tires of the participant’s vehicle crossed the lane marker lines. We made exceptions for purposeful lane changes signaled by using turn signals and did not count those as lane deviations. We instructed participants to drive as if the simulator was a real car and should treat each situation realistically by driving in a safe and legal manner, for example to adhere to a posted speed limit of 55 MPH, maintain lane position and avoid crashes.

Procedure

After the online prescreening (demographics, MHQ, and video game experience), participants were contacted by email to come into the lab. Upon arrival, participants completed the informed consent, vision screening, and the NASA-TLX baseline in that order. We took the NASA-TLX baseline after participants completed a simple task (adjusting the seat in the simulator). Next, participants completed a practice drive in a 10-min city scenario to become accustomed to the simulator, followed by the remaining drives (non-distraction pre-test, distraction, non-distraction post-test). We utilized the non-distraction post-test as a control based on concerns of test-retest reliability as raised by Boot and Simons (2012). The purpose was to ensure that participants returned to baseline levels of performance, and thus we attribute changes observed during the second drive to the distracting conversation, and not to practice or fatigue effects. All drives were 10-min in length along the same stretch of highway in light to moderate traffic density. Four cars were in view of the driver throughout most of the scenario. The only exception was if a participant’s speed dropped below the preset speed of the other vehicles in the environment. In that case, the other vehicles would pass the participant and drive until the vehicle was no longer in view of the participant, at such time new vehicles would enter the scene behind the participant. Between drives, we gave participants the NASA-TLX and the SSQ.

After the distraction drive, we gave participants a surprise recall task in which they were to recite as many questions as possible from the previous drive in 1 min. We administered the recall test was administered after the NASA-TLX, SSQ, and immediately before the subsequent drive. In total, the participants received four administrations of the NASA-TLX and SSQ. After completing all drives, we debriefed the each participant. During the debriefing, we asked the participant to list their understanding of the study’s objectives. None of the participants believed we were looking for AVG game play differences, this served as a manipulation check. We outline procedure in Fig. 1.

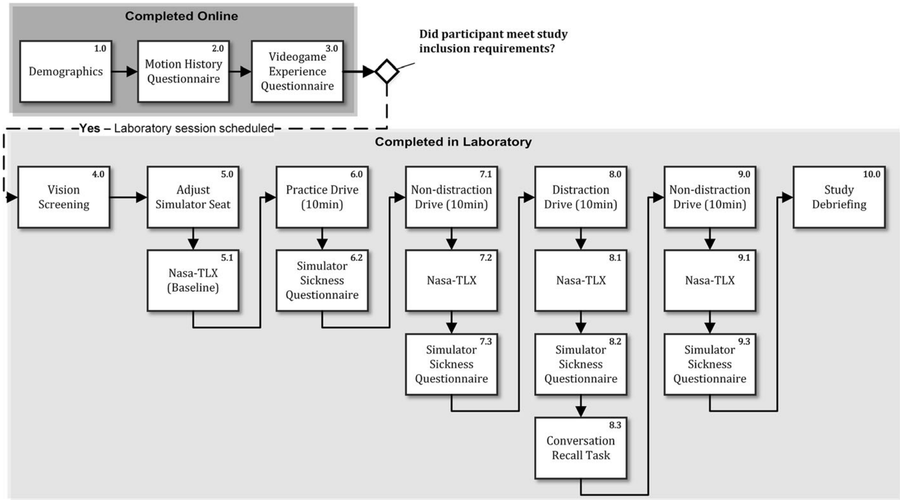

Fig. 1 General experimental procedure followed in both Experiment 1 and 2. The *dotted line* indicates the separation between the online screening and the laboratory portion of the experiments

Results and Discussion

We obtained three dependent variables of interest 1) lane deviations during each drive, NASA-TLX workload ratings, and the percentage of questions recalled following the distracted drive. These variables were not uniformly correlated across conditions and measures. Thus, each was analyzed in a separate univariate ANOVA. Bayesian posterior probabilities of the null hypothesis given the observed data were calculated as described in Masson (2011), in which a value of $p_{\mathrm{BIC}}(\mathrm{H_0}|\mathrm{D})$ less than 0.5 favors the alternative hypothesis and a value of greater than 0.5 favors the null hypothesis.

Lane deviations were obtained by averaging ratings provided from two trained raters. An inter-rater reliability analysis, using Cohen's Kappa, found κ=.93, p<.01 for ratings of lane deviations, which indicated a high level of agreement between the raters. A mixed factorial 3×3 ANOVA was conducted to test the influence of the three driving tasks (non-distraction, distraction, non-distraction) and AVG experience (NAVGP, LAVGP, and HAVGP) on the number of lane deviations. For all statistical tests we calculated eta squared (η^2), partial eta squared (η_p^2), and omega squared (ω^2) and present the results for the reader to evaluate the clinical significance of each effect throughout the paper. ω^2 is an unbiased measure of effect size in the population. Additionally, η^2, as reported in this paper are equivalent to R^2 and are often much smaller than η_p^2 values, we direct the reader to Levine and Hullett (2002) for a detailed discussion of effect sizes. Additionally, in cases of one-way ANOVAs η_p^2 and η^2 are equal therefore we only report η_p^2 when they differ.

We initially considered an ANCOVA using speed as a potential covariate however, the results yielded the same pattern with and without the use of the covariate, and therefore for simplicity we conducted the analysis using only the untransformed lane deviation values. There was a main effect for drive condition on lane deviations, $F(2, 78)=5.62$, $p<.01$, $\eta_p^2=.14$, $\eta^2=.05$, $\omega^2=.11$, $p_{\mathrm{BIC}}(\mathrm{H_0}|\mathrm{D})=0.69$. Tests of within-subjects contrasts indicate that the number of lane deviations was higher in the distraction drive, $p<.05$ (M=3.93, SD=4.48), than the non-distraction drives, drive 1 (M=2.30, *SD*=2.76) and drive 3 (*M*=2.78, *SD*=3.07). Drives 1 and 3 were not significantly different from each other. There was also a main effect for AVG group, $F(2, 39)=3.70$, $p<.05$, $\eta_p^2=.16$, $\eta^2=.11$, $\omega^2=.14$, $p_{BIC}(H_0|D)=0.52$. Comparisons using Tukey HSD indicated a significant difference among groups, $p<.05$. NAVGPs (*M*=4.67, *SE*=0.85) had significantly more lane deviations across all drives than HAVGPs (*M*=1.42, *SE*=0.85), but not LAVGPs (*M*=2.91, *SE*=0.63). However, the HAVGP and LAVGP groups did not significantly differ from each other ($p=.568$). Importantly, no significant interaction between drive and AVG group was found, $F(4,78)=1.67$, $p=.166$, $\eta_p^2=.079$, $\eta^2=.03$, $\omega^2=.039$, $p_{BIC}(H_0|D)=0.997$. We display the lane deviation data in Fig. 2a.

However, visual examination of Fig. 2a indicates a pattern that is consistent with an interaction that would support the AVG experience effect, as lane deviations appeared to increase in the distracted drive more markedly for non-gamers than for the high and low gaming groups. Our sample of 42 individuals was similar to other AVG studies (e.g. Cain et al. 2014; Wilms et al. 2013). However, to ensure that the sample size did not limit the statistical power in the study and hindering the ability to find a potential interaction between the distraction and AVG play conditions a post hoc power analysis was conducted using G*Power 3 (see Faul et al. 2007 for a technical description of the program and procedure used). The analysis determined that the power (1 - β) was 0.966, which was sufficient to find an effect if one was present. These results match the a priori power analysis completed during initial study design. Importantly, our null findings were not only found using traditional ANOVA testing, but were also found using posterior probabilities. The use of the Bayesian approach adds concurrent evidence that the null findings are not Type II errors (Masson 2011). The sum of the evidence supports the null hypothesis that driving performance did not vary across drives as a functioning of AVG experience.

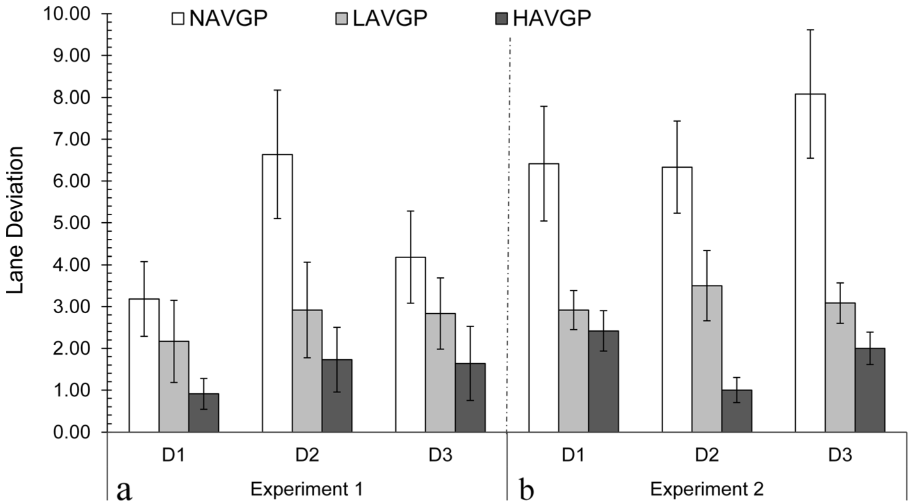

Fig. 2 Lane deviations for both experiment 1 (**a**) and experiment 2 (**b**) for non-action video gamers (NAVGPS), Low action video gamers (LAVGPS), & high action video gamers (HAVGPS) across all three drives. *Note*: drive 2 was the distraction drive. *Error bars* indicate standard error of the mean (SEM)

Next, a mixed factorial 3×3 ANOVA was conducted on total workload ratings. We found a main effect for workload across drives, $F(3, 117)=24.46$, $p<.001$, $\eta_p^2=.39$, $\eta^2=.26$, $\omega^2=.37$, $p_{\mathrm{BIC}}(H_0|D)=0.01$. Tests of within-subjects contrasts indicated that participants rated lower workload in drive 1 ($M=33.03$, $SD=16.39$) compared to drive 3 ($M=41.08$, $SD=14.54$), $p<.005$. Participants rated significantly higher workload ($p<.001$) in drive 2 ($M=54.39$, $SD=14.66$) when compared to drive 1 and drive 3 or baseline ($M=29.59$, $SD=18.64$). There was no main effect of game experience on workload, $F(2,37)=0.024$, $p=.976$, $\eta_p^2=.048$, $\eta^2=.03$, $\omega^2=0.02$, $p_{\mathrm{BIC}}(H_0|D)=0.976$, nor did game experience interact with the administrations of the TLX, $F(4,74)=0.42$, $p=.794$, $\eta_p^2=.022$, $\eta^2=.02$, $\omega^2=.017$, $p_{\mathrm{BIC}}(H_0|D)=0.99$. See the mean workload ratings in Fig. 3a for more information.

Finally, we conducted a one-way ANOVA on the percentage of correctly recalled questions following the distracting drive across the three AVG player groups (See Fig. 4a). There were no significant differences between the percent recalled by NAVG Ps ($M=25.18$, $SD=12.36$), LAVGPs ($M=25.40$, $SD=15.45$), and HAVGPs ($M=18.77$, $SD=5.94$), $F(2, 31)=1.09$ $p=.348$, $\eta_p^2=.066$, $\omega^2=0.005$, $p_{\mathrm{BIC}}(H_0|D)=0.91$. Because we asked participants questions continuously throughout the second drive and we allowed participants to answer at their own pace, it may be that percent recall is confounded with the number of questions completed, which may have varied across the groups. Thus, we used an ANOVA to examine if differences existed in the number of questions each participant received. A small, but significant difference in the number of questions answered was found, $F(2,31)=4.180$, $p=.024$, $\eta_p^2=.20$, $\omega^2=.16$, $p_{\mathrm{BIC}}(H_0|D)=0.43$. However, we feel that this difference was too small to affect the overall percentage of questions recalled given that the groups differed by approximately one question (See Fig. 5a), with NAVGPs answering on average 22.63 ($SD=1.56$), LAVGPs 23.91 ($SD=2.23$) and HAVGPs 21.90 ($SD=0.83$).

The current results indicated an AVG experience effect was present in our data, that participants with higher AVG experience were better drivers in the simulation across all

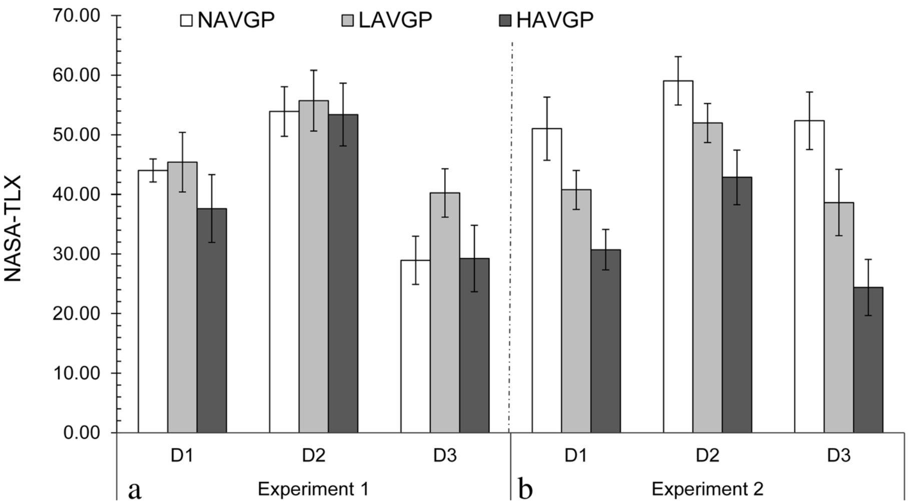

Fig. 3 Subjective workload (NASA-TLX) for both experiment 1 (**a**) and 2 (**b**) for NAVGPS, LAVGPS, & HAVGPS across all three drives. *Note*: drive 2 was the distraction drive. *Error bars* indicate SEM

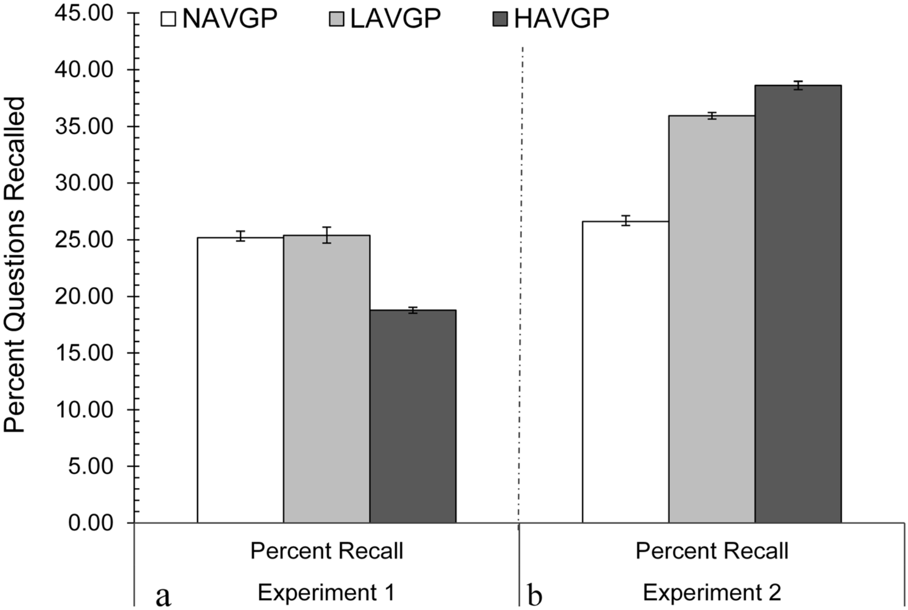

Fig. 4 Percent questions recalled during the surprise recall task administered immediately following drive 2 for experiment 1 (**a**) and 2 (**b**). *Error bars* indicate SEM

drives, which indicated evidence of task related transfer; however, our data did not support cognitive ability transfer from the AVG to the driving task. We did not find that increased AVG experience improves driving performance while distracted as evident

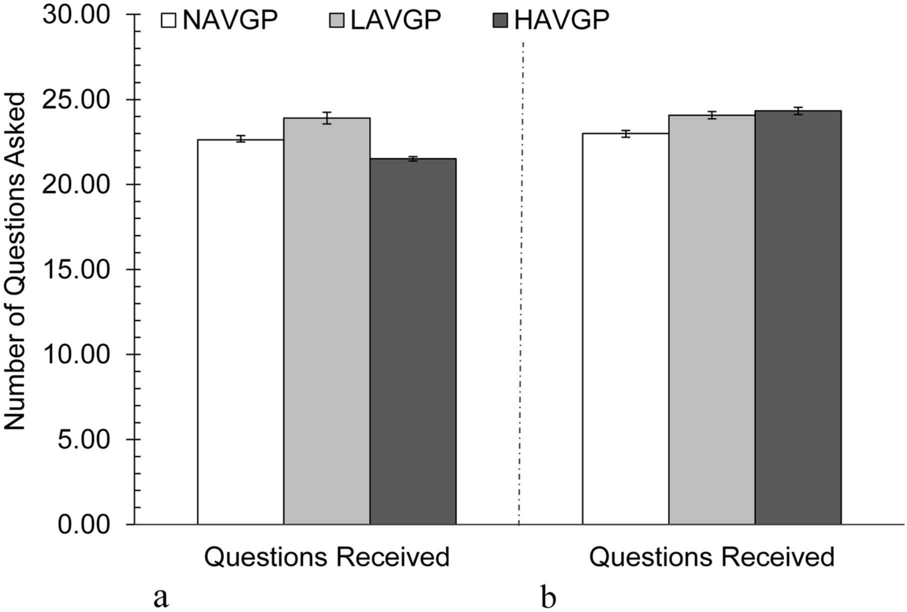

Fig. 5 The number of questions received for each participant group (NAVGP, LAVGP, & HAVGP) across experiment 1 (**a**) and experiment 2 (**b**) during the surprise recall task administered immediately following drive 2

from the lack of interaction between game experience on both lane deviations and workload measures, as well as the lack of a significant difference in post distraction recall. Further, if the experienced gamers had more cognitive resources at their disposal to perform the tasks, they ought to have reported lower overall mental workload, on the supposition that the tasks would have occupied a smaller proportion of their total available resources compared to the other participants. The lack of workload differences between the groups suggests then that the gamers did not exhibit improved cognitive functioning.

All of the action games that our participants reported playing were 3D games that allowed players to manipulate the camera from a first person view. Additionally, previous studies (e.g. Spence and Feng 2010) have argued that the 3D environment of the game (including realistic physics) is similar enough to real-world 3D environments, in terms of spatial relationships, and that these games require players to locate objects and navigate within the environment. Thus, the results seem to support either the strategic development or task-specific skill explanations of the AVG experience effect, but not the cognitive skill-training explanation. Future research that moves the testing from simulated driving to real-world driving is required to unravel these explanations further.

Experiment 2

The current results replicate the lack of differences between game players and non-players in a distracted driving task as reported by Donohue et al. (2012). However, before we may dismiss the cognitive skill explanation of the AVG experience effect, we must address why these findings differ from the positive results reported by Telner et al. (2009). One important difference between the studies is the timing of the video game experience questionnaires. Telner et al. (2009) surveyed differences in game experience in the beginning of the study, whereas Donohue et al. (2012) postponed these surveys until the end of the study. The administration of surveys after participation has been mentioned (e.g., Boot et al. 2011) as a means to reduce demand characteristics that may bias experienced gamers to outperform non-gamers in the study.

Overt or non-specified recruitment methods are a methodological issue in previous AVG research (Boot et al. 2011). Anytime participants are selected based on their expertise in a certain area, a potential exists that the selection process will affect their performance. Recruitment methods may further bias participants since they may believe, or infer, that video game experience improves skills related to the tasks employed by the researchers (Ozdamli et al. 2011). Further, those with low video game experience may exhibit decreased performance due to the belief that they do not have the required expertise to perform well (Boot et al. 2011). One study showed that priming participants with the belief that they would perform better could even improve visual acuity (Langer et al. 2010) and this effect along with issues of overt recruitment may explain previous positive AVG effects (e.g. Li et al. 2009).

Thus, recruitment methods are a significant concern in tests of the AVG experience effect. The simple act of including a video game screening questionnaire or using a flyer seeking video game players may be enough to activate these effects. Once activated, both positive (placebo) and negative (nocebo) effects are possible (Benedetti et al. 2007), in which those who characterize themselves as gamers may be motivated while those who characterize themselves as non-gamers may be demotivated, or experience stereotype threat (e.g., Schmader et al. 2008). Moreover, videogames often have many challenges that players have to overcome in order to be complete the game. Working through these repeated obstacles may foster increased levels of motivation amongst avid gamers (McGonigal 2011). Carstens and Beck (2005) further stated that gamers self-report being more competitive than non-gamers and believe winning is an important aspect of life. This so-called "gamer mentality," activated by a demand characteristic, might explain some of the performance advantages of gamers compared to non-gamers in the literature.

Even though we surveyed game experience prior to participation in Experiment 1, we explicitly masked the purpose of the study by integrating the gaming questions with the MHQ, introduced a 5-day delay between administration of these questions and participation in the driving tasks, and further misled participants about the true purpose of the study. In this way, our study was more similar to that of Donohue et al. (2012) in that we employed a covert recruitment method. Hence, we suggest that it is for these reasons that our results differ from the positive effects reported by Telner et al. (2009). While it is true that studies have reported evidence of AVG superiority with covert recruitment (e.g., Clark et al. 2011; Donohue et al. 2010), these studies employed laboratory measures of attention and cognitive function, rather than the more externally valid tests of interest in the current studies. If this interpretation is correct, we should be able to obtain results in favor of the AVG experience effect by informing participants about the purpose of the study in Experiment 2. Thus, we repeated Experiment 1 with the exception that we informed participants that the purpose of the study was to demonstrate the superiority of AVG players.

Method

Participants

Thirty-six adult males with normal or corrected to normal visual acuity, color vision, and contrast sensitivity participated in the study. We recruited participants in the same manner as in Experiment 1. NAVGPs reported no hours of AVG play, LAVGPs reported between 1 and 6 h of play (M=3.00, SD=1.52), and HAVGPs reported between 8 and 23 h of game play (M=14.82, SD=7.70). Both LAVGPs and HAVG Ps stated experience with the same games as those in Experiment 1.

Procedure

The procedure was the same as in Experiment 1 except that after the informed consent we read all participants the same statement that we had recruited them for a study examining the effects of AVG play on driving performance and we reminded participants of their answer to the video game questionnaire they filled out previously.

Results and Discussion

As in Experiment 1, the dependent variables of interest were not uniformly correlated across conditions and measures. Thus, we conducted each analysis in a separate univariate ANOVA. We obtained the data for lane deviations using the same method as Experiment 1 by averaging ratings provided from two raters. An inter-rater reliability analysis, using the kappa statistic, revealed high rater agreement (Cohen 1960), κ=.83, p<.05, for ratings of lane deviations. As in Experiment 1, co-varying speed yielded the same pattern with and without the use of the covariate; therefore for simplicity we conducted the analysis using only the untransformed lane deviation values. A mixed factorial 3×3 ANOVA was conducted on lane deviations as a function of the three driving tasks (non-distraction, distraction, non-distraction) and AVG experience (NAVGP, LAVGP, and HAVGP). Unlike Experiment 1, there was no main effect of the driving condition, $F(2,66)=1.47$, $p=.239$, $\eta_p^2=.043$, $\eta^2=.01$, $\omega^2=.013$, $p_{\mathrm{BIC}}(\mathrm{H_0|D})=$ 0.943. Thus, the distraction manipulation did not create an overall increase in lane deviations. Participants had an average of 3.92 (SD=3.48) lane deviations during the first, control drive, 3.61 (SD=3.53) lane deviations during the distracted drive, and 4.39 (SD=4.19) lane deviations during the final control drive.

Again there was a significant main effect for AVG group, $F(2, 33)=10.62$, $p<.001$, $\eta_p^2=.39$, $\eta^2=.27$, $\omega^2=.38$, $p_{\mathrm{BIC}}(\mathrm{H_0|D})=0.004$. Replicating Experiment 1, NAVGPs had the highest number of lane deviations (M=6.33; SD=3.82), followed by LAVGPs (M=3.50; SD=2.91) and HAVGPs (M=1.00; SD=1.04). A post hoc Tukey HSD test indicated that only marginal differences existed between LAVGPs and HAVGPs (p=.094), the LAVGPs and NAVGPs (p=.051); however, NAVGPs were significantly different from HAVGPs (p<.001).

As in Experiment 1, there was no interaction between the distraction manipulation and AVG group, failing to support the hypothesis that the introduction of demand characteristics would create evidence more favorable to the AVG experience effect for this task, $F(4,66)=1.80$, $p=.139$, $\eta_p^2=.099$, $\eta^2=.03$, $\omega^2=.043$, $p_{\mathrm{BIC}}(\mathrm{H_0|D})=0.995$. We display the number of lane deviations for Experiment 2 in Fig. 2b.

Next, a mixed factorial 3×3 ANOVA was conducted on total workload ratings across drives and AVG groups. Results showed a main effect for workload across the driving conditions, consistent with Experiment 1, $F(3, 105)=74.13$, $p<.001$, $\eta_p^2=.70$, $\eta^2=.68$, $\omega^2=.68$, $p_{\mathrm{BIC}}(\mathrm{H_0|D})<0.01$. Tests of within-subjects contrasts indicated a significant difference among groups (p<.005). Participants rated higher workload in the distracted drive (M=51.30, SD=15.05) than baseline (M=12.22, SD=8.34), drive 1(M=40.83, SD=16.15) or drive 3(M=38.45, SD=20.58). Additionally baseline ratings of workload were lower than all other drives. The non-distraction drives did not significantly differ from each other. Unlike Experiment 1, there was a significant main effect for AVG group on workload, $F(2, 33)=9.24$, $p<.001$, $\eta_p^2=.34$, $\eta^2=.35$, $\omega^2=.31$, $p_{\mathrm{BIC}}(\mathrm{H_0|D})=0.020$. Tukey HSD post hoc tests showed that NAVGPs (M=43.92, SE=2.78) had significantly higher workload than HAVGPs (M=27.06, SE=2.78), but not LAVGPs (M=36.12, SD=2.78). Also, there was a small but significant interaction between drive and AVG group $F(6, 99)=2.48$, $p<.05$, $\eta_p^2=.13$, $\eta^2=.13$, $\omega^2=.08$, $p_{\mathrm{BIC}}(\mathrm{H_0|D})=0.99$. Importantly, we found that the interaction occurred because the differences between the AVG experience groups were less pronounced at baseline than after any of the three drives, as shown in Fig. 3, and not because the HAVGPs reported markedly lower workload during the distracted drive compared to the non-distracted drives. Thus, while the experienced game players reported an overall lower mental workload in Experiment 2, an effect, which we attributed to the demand characteristic, this effect, was not especially pronounced during the distracted drive (See Fig. 3b).

Finally, we compared the percentage of questions recalled following the distracted drive. The percent recall by NAVGPs (M=26.58, SD=11.65) was significantly less than LAVGPs (M=35.94, SD=6.52; p=.040) and HAVGPs (M=38.62, SD=7.91; p=.007), F(2, 33)=5.96 p=.006, η_p^2=.266, ω^2=.22, $p_{\text{BIC}}(\text{H}_0|\text{D})$=0.12. However, light and heavy players were not different from each other (p=.747). We show these data in Fig. 4b. As in Experiment 1, we asked participants questions continuously throughout the second drive and we allowed each participant to answer at their own pace. We used an ANOVA to determine if differences existed in the number of questions each participant received. Again a small, but significant difference in the number of questions answered was found, F(2,33)=4.18, p=.024, η_p^2=.20, ω^2=.15, $p_{\text{BIC}}(\text{H}_0|\text{D})$= 0.43 and again we feel that this difference was too small to affect the overall percentage of questions recalled (See Fig. 5b). NAVGPs answered on average 23.00 (SD=1.04), LAVGPs 24.08 (SD=1.31) and HAVGPs 24.33 (SD=1.30).

General Discussion

In the current work, we sought to evaluate the hypothesis that the AVG experience effect may be explained as a form of task-specific training, rather than a more general form of strategic development or cognitive skill training. The cognitive skill hypothesis would have predicted that experienced AVG players had improved cognitive resources that would have made them better at the distracted driving task compared to non-gamers. Such resources might have included executive control, working memory capacity, or divided attention skill. The strategic development hypothesis would have made a similar prediction that AVG players would have performed better than non-gamers in the distracted driving task, though for different reasons than cognitive skill. Here, the expectation was that gamers would have learned strategies in games that were applicable to the task. For example, in a change blindness task gamers used a broad search instead of a more elaborate search, used by non-gamers, which took more time (Clark et al. 2011). Hence, the gamers would not necessarily have had more cognitive resources to handle the demanding task of driving and talking on the cell phone, but rather would have had better strategies for allocating their attention or the secondary task demands during the drive.

The task-specific training hypothesis was distinguished from the predictions of these hypotheses in the sense that it predicts little or no relationship between AVG experience and performance on the real-world task of distracted driving. While our primary dependent variable of lane deviations may not have been sensitive enough to pick up all variability in lane maintenance behavior, we were limited in terms of the hardware employed in the current studies. Additionally, a stronger distraction manipulation may have improved our sensitivity group differences. Nevertheless, we feel that the lane deviation data provides a pattern that can be meaningfully interpreted. However, we note that future work in this domain, should consider both additional performance metrics as well as a stronger distraction manipulation (e.g. texting).

In Experiment 1, we found no evidence that gamers handled the introduction of the distraction task while driving any better than the non-gamers, which is thus inconsistent with both the ideas of cognitive skill and strategic development. Hence, this negative result could support the idea that AVG experience provided only task-specific training that was not sufficient to prepare the gamers for the more real-world task of distracted driving.

However, we should also note that during all conditions and both studies, whether distracted or not, the experienced AVG players tended to be better drivers in the simulator, exhibiting fewer overall lane deviations. How then, should we explain this finding? The challenge arises in that possibly all three listed hypothesis might account for it. First, we will reconsider the cognitive skill explanation. It is possible that driving in a simulator is a novel enough task that additional working memory and executive control abilities are required in relation to driving a real car, and that the experienced gamers had just enough improvements in those abilities to handle the demands of this task, but not of the more difficult distracted driving task. Additionally, it could also be the case that the AVG players experienced improvements only in terms of spatial ability as a result gameplay (e.g. Feng et al. 2007), which may account for their better lane maintenance performance, but might not necessarily be expected to improve their ability to do so while distracted.

In reconsideration of the strategic development hypothesis, one may claim that the experienced game players had learned strategies for navigating virtual environments that were relevant to the steering task, but had no strategies that enabled them to handle distraction. Finally, the task-specificity hypothesis would predict that the driving simulator is ultimately very game-like in nature, involving the control of a first-person viewpoint through a virtual environment using a control-display interface. It is, in essence, a driving game. Experienced gamers would thus be expected to perform better than non-gamers at a driving game, but would otherwise not have any particular capacity to do so while engaged in a distracting task. Evidence against the task-specificity hypothesis comes from work that argues that simulations are not just games, but are designed to simulate the real-world task and ensure optimal transfer from simulation to real-life; thus they are as similar as possible to the task they are trying to simulate (Aldrich 2009). Driving simulators, are, after all, meant to teach people how to drive real cars. Other studies have shown that simulated driving is comparable to real-world driving (De Winter et al. 2009).

In sum, it is difficult to select between these three explanations to account for the overall results of Experiment 1, except that we may note that the explanation in terms of task-specificity is the most parsimonious. However, the results of Experiment 2 may shed more light on the issue. In this experiment, we added a demand characteristic, in contrast to Experiment 1 in which the purpose of the experiment was disguised. This manipulation was included as a potential way to increase the motivation of experienced AVG players.

In terms of driving performance, the results of Experiment 2 were almost identical to Experiment 1. Overall, AVG players were better than non-gamers at lane maintenance, but did not show an inclination to handle the distraction task better than non-gamers. In and of itself, this finding mounts more evidence against the ideas of cognitive skill and strategic development. If the results of Experiment 1 might be explained by a lack of motivation on the part of the AVG players to apply their skills to the task, then Experiment 2 suggests that even when given such motivation, the gamers failed to show any ability to outperform non-gamers in terms of driving performance while distracted. This result would again support the notion of the task-specificity of AVG experience.

Even if the demand characteristic failed to increase motivation among the gamers, Experiment 2 could be seen as a replication of Experiment 1, leaving the same predicament of explaining why the AVG players were better at overall lane maintenance, though not at distracted lane maintenance—but a further result bears mentioning. In Experiment 2, the experienced AVG players were able to remember more of the distracting conversation than non-gamers. This suggests something beyond task-specific game training that may have involved improved working memory capacity (consistent with the cognitive skill hypothesis) or better allocation of cognitive resources during the distracted drive that preserved some memory capacity (consistent with the strategic development hypothesis). However, it is problematic to fully endorse these latter two ideas because we cannot show that the results were not due to a nocebo effect on the part of the non-gamers. Visual inspection of Fig. 2 reveals that in terms of driving performance between Experiments 1 and 2, the non-gamers of Experiment 2 were worse than the non-gamers of Experiment 1—in fact, the non-gamers performed as if they were distracted across all drives in experiment 2 since their performance matched the NAVGP distracted performance in experiment 1. More puzzling still is that the NAVGP group decreased with greater simulator experience (D3). While it could be possible that the participants in experiment 2 were worse drivers than in experiment 1, we feel that this is not the case. Only the NAVGP group had this extreme performance decrement and further all participants had comparable driving experience as the other groups in both experiments. Additionally, this finding may be important to explain the lack of distraction effect for experiment 2.

Thus, the difference in recall between the non-players and experienced players may possibly be attributed to a nocebo effect that caused the non-gamers to perform less well at all experimental tasks.

It is here that our measurement of subjective workload ratings after each drive may shed additional light on these interpretations. In Experiment 1, there was no difference in reported workload between gamers and non-gamers, but in Experiment 2, there were marked differences. Visual inspection of Fig. 3 suggests that the effect can be explained as a reduction in workload for the experienced AVG players, rather than an increased workload for the non-gamers. This effect provides evidence against the aforementioned nocebo effect, suggesting instead that the improved recall performance of the AVG players in Experiment 2 may be attributed also to a performance boost via the demand characteristic, and not just a performance decrement among the non-gamers. Why then, did the experienced AVG players not show a decrease in lane maintenance errors from Experiment 1 to Experiment 2? The answer may be that a floor effect prevented their improvement to anything less than the 1–2 errors exhibited on average. Another answer may be that the drivers were not instructed as to how to prioritize the two tasks of conversing and driving, and may have thus given the conversation a higher priority. If true, we might predict that the effect is malleable and through instructions could be shifted to measures of driving performance.

We are left then, to conclude that our findings suggest that the experienced AVG players have either improved cognitive skills or have developed strategies that enable them to outperform non-gamers on a real-world task. It remains difficult to test between these two hypotheses, as they both predict successful transfer from game play to real-world tasks. While our current design limits our ability to tease apart these hypotheses, future work can do so as long as elements of both game play and testing are carefully controlled. For example, if one were to accept the idea that the AVG players in the current studies had better spatial abilities, which accounted for their improved lane maintenance, then their performance should be evaluated in real-world tasks that involve spatial skill (e.g., tele-operation) compared to those that do not. Only when performance transfers to tasks appropriate to the cognitive skill being evaluated would there be evidence favoring this explanation, and only so long as the existence of game-play strategies could be ruled out. While there is some evidence in favor of the AVG experience effect in tele-operation tasks (e.g., Keebler et al. 2014; Wright et al. 2013), these studies were not designed to specifically control for the differences between cognitive skills and game-related strategies.

The finding that the influence of AVG experience was more pronounced in the presence of a demand characteristic does not necessarily mean that such effects are merely artifacts of experimental design to be disregarded. Rather, we suggest that the game players' use of their skill is somewhat motivational and possibly also informational in nature. That is, in order to apply one's skills to a task at hand, a game player must first recognize that they have skills relevant to the task. The explicit recruitment procedure employed in Experiment 2 provided just such information to the experienced gamers, while also possibly increasing their motivation to perform well, exhibiting a competitive gaming mentality (Carstens and Beck 2005). Moreover, previous research has indicated that motivational factors can influence direction of exogenous spatial attentional leading to improvements in cuing task performance (Engelmann and Pessoa 2014). The finding that motivational effects can modify spatial attention has direct implications for the current study as driving (especially lane maintenance) is reliant on spatial information.

Such motivational effects may be explained by self-determination theory (e.g. Ryan and Deci 2000). According to this theory, intrinsic motivation to achieve a goal is explained by how well the goal supports an individual's needs for autonomy, competence, and social connectedness (Przybylski et al. 2010). Individuals thus motivated will exhibit increased vitality, excitement, persistence, and confidence, all of which may lead to increased performance. Lazarus (1991) also linked motivational states to decreased perceived workload and stress. We suggest that experienced gamers were intrinsically motivated to perform better in Experiment 2 because the demand characteristic activated their need for competency by challenging them to do well, and their social connectedness need by challenging them to do outperform non-gamers.

While we reject the strong claim that all reported differences in the literature might be attributed to recruitment artifacts when blind recruitment is not employed, we have provided evidence that recruitment methods and demand characteristics can influence the outcomes of AVG experience studies. Further, the results of any study that failed to use blind recruitment must be evaluated in terms of both motivational effects on the part of the experienced game-players as well as possible nocebo effects on the part of the non-gamers. We argue that such effects are likely to account for the discrepant findings previously discussed in terms of distracted driving and AVG players (Donohue et al. 2012; Telner et al. 2009).

There are likely real differences between experienced AVG players and non-players but inconsistencies in recruitment methods across studies compromise our ability to discern exactly what the differences reported in the literature mean, and what the real magnitudes of these differences are. Additionally, while our study was not a game training study and utilized action game experience as a subject variable, we feel that these concerns are equally important for both studies of gamer individual differences and game training studies.We emphasize that replication with an eye toward the control of these factors is warranted as this research theme continues to grow and influence our understanding of cognitive function and skill acquisition. Some have argued, however, that such efforts would be inefficient and expensive, because blind or covert recruitment that entails the need to screen for video game experience after testing would require a very large sample size in order to obtain a sufficient number of experienced gamers (e.g. Green and Bavelier 2012). We note that the deceptive methods employed in our Experiment 1 may present a compromise that can reduce concern about demand characteristics without over-inflating sample size to an unmanageable level. To reiterate, we masked the purpose of the study by informing participants of an irrelevant hypothesis, masked video game experience questions under the pretense that they were part of an instrument used to screen for susceptibility to motion or simulator sickness, and introduced a 5 day delay between exposure to questions about video game experience and testing.

Many studies have sought to demonstrate the positive effects of action video game experience on task performance and cognitive ability, with claims that action games augment our basic cognitive abilities (e.g. attention, executive functioning, & memory; e.g. Green and Bavelier 2003; Bavelier 2011). The thought of action games as a panacea for debilitating illnesses such as dementia, or reducing the negative effects of the aging process, or to provide a benefit to those performing critical and complex tasks (e.g. TSA screeners, medical professionals, pilots, soldiers, distracted drivers), or even enhance our everyday lives in quite exciting. In fact, many individuals play so-called brain training games with the hopes that it will help them with activities of daily living. Unfortunately, this hope may be unfounded. The Max Plank Institute for Human Development and Stanford Center on Longevity (2014) recent hosted a conference of over 80 cognitive psychologists, which concluded that the current evidence does not support the strong claims of transfer effects present in the literature. We argue that our findings provide an important new way to think about the AVG experience effect in light of motivational and informational influences on performance, and thus additional studies should account for these effects for either accepting or rejecting any claims about game-based training for cognitive functioning.

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