



## Violent and nonviolent video games produce opposing effects on aggressive and prosocial outcomes

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### ABSTRACT

Experimental studies routinely show that participants who play a violent game are more aggressive immediately following game play than participants who play a nonviolent game. The underlying assumption is that nonviolent games have no effect on aggression, whereas violent games increase it. The current studies demonstrate that, although violent game exposure increases aggression, nonviolent video game exposure decreases aggressive thoughts and feelings (Exp 1) and aggressive behavior (Exp 2). When participants assessed after a delay were compared to those measured immediately following game play, violent game players showed decreased aggressive thoughts, feelings and behavior, whereas nonviolent game players showed increases in these outcomes. Experiment 3 extended these findings by showing that exposure to nonviolent puzzle-solving games with no expressly prosocial content increases prosocial thoughts, relative to both violent game exposure and, on some measures, a no-game control condition. Implications of these findings for models of media effects are discussed.

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A major development in mass media over the last 25 years has been the advent and rapid growth of the video game industry. From the earliest arcade-based console games, video games have been immediately and immensely popular, particularly among young people and their subsequent introduction to the home market only served to further elevate their prevalence (Gentile, 2009). Given their popularity, social scientists have been concerned with the potential effects of video games on those who play them, focusing particularly on games with violent content. While a large percentage of games have always involved the destruction of enemies, recent advances in technology have enabled games to become steadily more realistic. Coupled with an increase in the number of adult players, these advances have enabled the development of games involving more and more graphic violence. Over the past several years, the majority of best-selling games have involved frequent and explicit acts of violence as a central gameplay theme (Smith, Lachlan, & Tamborini, 2003).

A video game is essentially a simulated experience. Virtually every major theory of human aggression, including social learning theory (Bandura, 1977), cognitive neoassociation theory (Berkowitz, 1990), script theory (Huesmann, 1998), and the General Aggression Model (GAM; Anderson & Bushman, 2002), predicts that repeated simulation of antisocial behavior will produce an increase in antisocial behavior (e.g., aggression) and a decrease in prosocial behavior (e.g., helping) outside the simulated environment (i.e., in “real life”). In addition, an

increase in the perceived realism of the simulation is posited to increase the strength of negative effects (Gentile & Anderson, 2003). Meta-analyses of existing empirical studies generally support these predictions, in that exposure to violent video games, relative to nonviolent game exposure, causes increases in aggressive behavior, cognition, and affect, and decreases in prosocial behaviors and attitudes, (e.g., Anderson, 2004; Anderson & Bushman, 2001).

Although the effects of violent video game exposure on aggression are generally accepted, a common criticism of experimental research on this topic is that the negative effects of exposure to violent games are likely short-lived (see Freedman, 2001). In the experimental literature, researchers have uniformly measured aggression immediately or within a few minutes after video game play. Thus, little is known about the temporal stability of acute exposure effects. Testing the duration of such effects has important implications for understanding their nature. For example, if violent game effects on aggression are due largely to priming (e.g., Huesmann, 1998), the effects should decay rapidly in the absence of an opportunity to aggress, as aggressive cognitions and behavioral scripts recede from temporarily heightened accessibility (c.f., Higgins, 1996). In contrast, if exposure to a violent game creates a heightened state of aggression-related goal activation (e.g., Bargh, Gollwitzer, Lee-Chai, Barndollar & Troetschel, 2001; see also, Forster, Liberman & Higgins, 2005), the goal should remain active, and the likelihood of aggression higher, until an opportunity to aggress is presented, presumably for a longer period of time than if effects were due primarily to heightened accessibility of aggressive cognitions.

In addition to assessment of temporal stability, another important consideration is the choice of control condition. To establish baseline

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levels of aggression, studies testing violent game effects typically use nonviolent games as a control group (e.g. Anderson & Dill, 2000; Carnagey, Anderson, & Bushman, 2007). Thus, any post-game increase in aggression-related outcomes in the violent game condition is attributed solely to the violent game. This strategy is potentially problematic for several reasons, primarily that nonviolent games are likely not a “pure” control. While efforts often are made to match games on aggression-relevant factors such as arousal, frustration and interest (see Anderson & Dill, 2000), nonviolent games still tend to differ from violent games in qualities other than the absence of violent content. Thus, it is possible that violent and nonviolent games simply have divergent effects on outcomes of interest. In other words, while violent game exposure might increase aggressiveness, nonviolent game exposure might also decrease aggressiveness below some pre-exposure baseline.

This could occur for a number of reasons. Many nonviolent games, while still fast-paced, tend to emphasize problem-solving as a primary strategy for success, whereas the majority of violent games, while often incorporating problem-solving elements, primarily center on impulsive, “see-and-shoot” strategies. It is theoretically quite feasible that focusing on logical problem-solving in nonviolent games could make constructs associated with impulsive, hostile actions less accessible in memory. Alternately, or in addition, rehearsal of reasoned problem-solving strategies could temporarily reinforce tendencies toward more controlled evaluation of initial behavioral inclinations, which, according to the General Aggression Model (GAM), will decrease the likelihood of hostile aggression (Anderson & Bushman, 2002).

Finally, recent evidence (Gentile et al., 2009; Mares, Palmer, & Sullivan, 2008; Narvaez, Mattan, MacMichael, & Squillace, 2008) indicates that, just as violent game play can reduce prosocial tendencies, exposure to certain nonviolent games may elicit increased accessibility of positive, prosocial constructs. To date, however, such findings have been restricted to games with explicitly prosocial themes and content. However, to the extent that other types of nonviolent games lead to a focus on logical, reasoned problem-solving, it could be that similar effects would emerge even if a player’s aim is not expressly prosocial, which could contribute to potential decreases in the accessibility of aggression-related constructs. Thus, testing game effects on prosocial outcomes could provide information relevant to understanding effects on aggressive outcomes. A secondary aim of the current study was to test these possibilities.

In sum, the primary purpose of this research was to test the temporal stability of video game effects on aggression-related outcomes, and to test whether violent and nonviolent games have divergent effects on aggression-related and prosocial-related outcomes. In two experiments, participants were randomly assigned to play either a violent or nonviolent video game, or to play no video game, after which aggression-related outcomes were assessed either immediately or after a brief delay. In a third experiment, we expanded the design to test for potential effects of violent and nonviolent game exposure on accessibility of prosocial constructs. The delay manipulation served two related purposes pertinent to the aims of this research. First, assessing aggressive outcomes both immediately and following a brief delay helps to establish the temporal stability of acute game exposure effects. Second, use of a delay manipulation provides a means of testing whether effects of violent and nonviolent games produce divergent outcomes on measures of aggression. If so, measures of aggression-related outcomes should change significantly in opposing directions in the two game conditions between the no-delay and the 15-min delay conditions. In other words, compared to the no-delay assessments, aggression should decrease following delay for violent game participants and increase following the delay for nonviolent game participants.

With these issues in mind, 3 primary hypotheses were advanced for this study: (1) Playing a violent game will cause increases in aggressive cognitions, affect and behavior, relative to playing a nonviolent game, but only when outcomes are assessed shortly following game play; (2) delaying the assessment of aggression following game play will decrease aggression in the violent game condition and increase

aggression in the nonviolent game condition; and (3) compared to playing violent games, playing nonviolent games lacking specifically prosocial content will increase accessibility of prosocial constructs.

## Experiment 1

### Method

#### Participants

One hundred eighty-eight undergraduates (131 women) at the University of North Carolina at Chapel Hill, recruited using a Psychology Department website, participated in exchange for course credit.

#### Materials

**Video games.** Two violent games (Quake 3 and Unreal Tournament: Game of the Year Edition) and two nonviolent games (Zuma and The Next Tetris) were used in this study. Both violent games were first-person shooter games, where players run through a futuristic gladiator arena and must kill multiple enemies with a variety of weapons to succeed. Both nonviolent games were visual puzzle games, in which players must manipulate objects into various patterns within a time limit in order to succeed.

**Questionnaire measures.** Participants completed a number of measures to control for individual differences that could potentially impact aggression-related outcomes. These included the Interpersonal Reactivity Index (IRI, Davis, 1980), designed to measure trait empathy; the Caprara Irritability Scale (CIS, Caprara, 1982), designed to measure aggressive impulsiveness; and the Aggression Questionnaire (AQ, Buss & Perry, 1992), designed to measure trait aggression. Differences in other factors that could contribute to aggression, including frustration, arousal, and interest levels, were assessed using Likert-type scales anchored at 1 (*Not at all*) and 7 (*Extremely*), and were administered immediately after video game play.

#### Aggression-related dependent measures

Order of administration of the two dependent measures was counterbalanced within each condition.

**Aggressive affect.** Participants completed a measure of state aggressive affect consisting of 32 state mood statements (e.g., “I feel mad”), rated on a Likert-type scale anchored at 1 (*Strongly disagree*) and 5 (*Strongly agree*). This measure has been used in prior studies (Anderson, Deuser, & DeNeve, 1995) and has good reliability ( $\alpha = .93$ ).

**Aggressive cognitions.** Anderson, Carnagey, and Eubanks (2003) developed a word completion task designed to measure accessibility of aggressive cognitions. Participants are given a booklet containing 98 word fragments, each with one or more missing letters. Participants are given 5 min to finish as many items as possible. Completed words are then coded as nonviolent, violent, ambiguous, or non-words using an included coding guide (available from Craig A. Anderson), and the percentage of violent words (ambiguous words are counted  $\frac{1}{2}$ ) compared to the total number of words constitutes the aggressive cognition score.

#### Delay task

Participants were asked to spend 15 min drawing from memory “as complete, detailed, and accurate a map as possible” of their campus. Similar map-drawing tasks have been used as neutral time fillers in previous studies (Martin, Ward, Achee, & Wyer, 1993).

#### Procedure

Prior to each session, the experimenter determined condition assignments by two coin flips: one for video game (violent or nonviolent game), and one for delay (0 min or 15 min). Upon arrival at the laboratory, each

participant completed an informed consent form and was escorted into an individual room. Participants then completed all self-report measures, after which they were introduced to their assigned video game through an overview of controls and objectives and a brief practice level. Participants were instructed to play the game for 30 min with the door closed. Participants rated their frustration, arousal, and interest levels following the gameplay period. After this, participants completed the delay task and aggression measures, the order of which was determined by condition. Finally, participants were probed for suspicion, debriefed, and excused.

## Results

Data from seven participants were excluded from analyses: 2 began playing a video game before instructions were read, 3 experienced computer failures, 1 left the experiment early, and 1 failed to complete either dependent measure. Excluded participants were slightly more likely to have been in the violent game conditions (5 violent, 2 non-violent). Thus, analyses were based on data from 181 participants (129 women).

### Preliminary analyses

**Video game ratings.** Compared to nonviolent games, violent games were rated as significantly more frustrating, less exciting, and less interesting,  $t(179) > 2.83$ ,  $ps < .01$  (see Table 1). These effects were due largely to gender differences: women rated the violent games as more frustrating ( $M_s = 3.55$  and  $2.47$ ),  $t(127) = 3.83$ ,  $p < .001$ , less exciting ( $M_s = 2.84$  and  $4.32$ ),  $t(127) = -5.74$ ,  $p < .001$ , and less interesting ( $M_s = 2.37$  and  $4.10$ ),  $t(127) = -6.35$ ,  $p < .001$ . In contrast, men rated the games similarly on these qualities,  $t(50) < 1.3$ ,  $ps > .20$ .

**Individual difference measures.** None of the individual difference measures (CIS, BPAQ, IRI) correlated significantly with either dependent measure, and thus will not be discussed further.

### Analyses of aggressive outcomes

Aggressive cognition and aggressive affect scores were examined using separate 2 (Game; violent, nonviolent)  $\times$  2 (Delay; 0-min, 15-min)  $\times$  2 (Gender; male, female) factorial analyses of variance (ANOVAs).

**Aggressive cognition.** Mean aggressive cognition scores as a function of game and delay conditions are given in Table 2. The Game main effect was not significant,  $F(1, 177) = 2.10$ ,  $p = .149$ . However, the predicted Game  $\times$  Delay interaction was significant,  $F(1, 177) = 9.98$ ,  $p = .002$ . Follow-up contrasts comparing the effects of Game separately as a function of delay showed that, in the 0-min delay condition, participants who played a violent game used significantly more aggressive word completions than participants who played a nonviolent game,  $F(1, 88) = 10.01$ ,  $p = .002$ . In contrast, aggressive thought accessibility did not differ as a function of video game in the

**Table 1**  
Mean ratings of violent and nonviolent games on arousal, frustration, and interest in Experiment 1 (top section) and Experiment 2 (bottom section).

Games	Experiment 1		
	Arousal	Frustration	Interest
Nonviolent	4.32 <sub>a</sub>	2.38 <sub>c</sub>	4.20 <sub>e</sub>
Violent	3.46 <sub>b</sub>	3.04 <sub>d</sub>	3.04 <sub>f</sub>
Games	Experiment 2		
	Arousal	Frustration	Interest
Nonviolent	5.00 <sub>a</sub>	2.47 <sub>c</sub>	3.90 <sub>d</sub>
Violent	3.72 <sub>b</sub>	2.41 <sub>c</sub>	4.48 <sub>e</sub>

Note. For each experiment, means within each column that do not share a subscript are significantly different,  $p < .05$ .

**Table 2**  
Mean percentage of aggressive word completions as a function of video game and time delay: Experiment 1.

	0 min Delay	15 min Delay
Violent game	22.2% <sub>a</sub>	19.7% <sub>b,c</sub>
Nonviolent game	18.5% <sub>b</sub>	21.1% <sub>c</sub>

Note. Means that do not share a subscript are significantly different,  $p < .05$ .

15-min delay condition,  $F(1, 90) = 1.47$ ,  $p = .226$ . Additional contrasts showed that, within the violent game condition, aggressive cognitions decreased after a 15-min time delay,  $F(1, 83) = 5.44$ ,  $p = .021$ . Importantly, within the nonviolent game condition, aggressive cognitions increased after time delay,  $F(1, 94) = 6.78$ ,  $p = .011$ .

The Game  $\times$  Delay  $\times$  Gender interaction also was significant,  $F(1, 173) = 5.68$ ,  $p = .018$ . Although the patterns of means were similar for men and women, the effect sizes for the delay manipulation were larger for men within both the violent ( $d = .80$ ) and nonviolent game conditions ( $d = 1.21$ ) than for women (violent game  $d = .021$ ; nonviolent game  $d = .38$ ).

**Aggressive affect.** Mean levels of aggressive affect as a function of game and delay conditions are given in Table 3. The ANOVA showed a main effect for Game,  $F(1, 177) = 8.82$ ,  $p = .003$ , and a significant Game  $\times$  Delay interaction,  $F(1, 177) = 8.51$ ,  $p = .004$ . A planned contrast of the means in the 0-min delay condition replicated the typical violent game effect: participants who played a violent game reported significantly more aggressive affect than those who played a nonviolent game,  $F(1, 89) = 17.17$ ,  $p < .001$ . In contrast, in the 15-min delay condition hostile affect did not differ as a function of game,  $F(1, 90) = 0.00$ ,  $p = .970$ . Additional planned contrasts showed that aggressive affect decreased with delay in the violent game condition,  $F(1, 83) = 4.48$ ,  $p = .036$ , and increased with delay in the nonviolent game condition,  $F(1, 94) = 4.79$ ,  $p = .031$ . The Game  $\times$  Delay  $\times$  Gender interaction was not significant,  $F(1, 173) = 2.90$ ,  $p = .091$ .

**Covariates.** To ensure results were not affected by differential game experiences, additional analyses were conducted using ratings of the video game on frustration, excitement, and interest as covariates (i.e., ANCOVAs). In each case, inclusion of the covariates did not significantly change the patterns of means or  $p$ -values associated with effects of interest.

### Discussion

Findings from this experiment provided initial support for two of our primary hypotheses. When outcomes were assessed immediately following game play, participants who had played violent games displayed increased accessibility of aggressive thoughts and increased hostile affect relative to participants who had played a nonviolent game, consistent with a large number of previous studies (Anderson & Dill, 2000; Anderson & Bushman, 2001; Carnagey & Anderson, 2005; Bartholow, Sestir, & Davis, 2005). However, when assessment was delayed by 15 min, game effects were nonexistent. Moreover, and consistent with hypothesis 2, delaying assessment led to significant decreases in aggressive outcomes in the violent game condition and significant increases in the nonviolent game condition. Since aggression was not assessed prior to gameplay, it is not possible to conclusively say

**Table 3**  
Mean state hostile affect as a function of video game and time delay: Experiment 1.

	0 min delay	15 min delay
Violent game	72.41 <sub>a</sub>	64.73 <sub>c</sub>
Nonviolent game	57.73 <sub>b</sub>	64.60 <sub>c</sub>

Note. Means that do not share a subscript are significantly different,  $p < .05$ .

that nonviolent game exposure reduces aggression relative to baseline; however, it is widely considered impossible to accurately assess aggression multiple times within the same paradigm (see Lindsay & Anderson, 2000; Bartholow, Anderson, Carnagey, & Benjamin, 2005), making a true pre–post design unfeasible. Therefore, these data suggest, but do not confirm, that nonviolent game exposure leads to short-term decreases in both aggressive affect and aggressive cognition.

These findings further suggest that the typical violent video game effect reported in numerous experiments (see Anderson, 2004; Anderson & Bushman, 2001) might represent a combined increase in aggressiveness following exposure to violent games coupled with a decrease in aggressiveness following nonviolent game exposure. These data also are consistent with idea that acute exposure effects are due to priming of aggressive scripts (see Huesmann, 1998) and/or hostile affect, rather than activation of aggression-related goals (cf., Bargh et al., 2001; Forster et al., 2005).

The effects for aggressive cognitions appear to have been much larger in men than women. Similar gender differences were reported in a previous video game study (Bartholow & Anderson, 2002). This difference may be attributable in part to a sex difference in prior video game experience: in the current study, many more men (98.1%) than women (65.9%) reported having at least some recent video game experience. Additionally, male subjects reported higher levels of violent game exposure ( $M = 19.7$ ) relative to females ( $M = 5.1$ ),  $F(1,179) = 96.3$ ,  $p < .001$ . This difference in prior exposure could have affected how men and women experienced the games in the lab (cf., Bartholow, Sestir, et al., 2005).

It is possible, however, that the increase in aggressive tendencies in the nonviolent game condition could have been due to frustration attributable to the delay task; however, this would not explain the concomitant decrease in aggressiveness in the violent game condition. If the delay task was especially frustrating to participants, violent game players would be predicted to plateau or even increase in aggression after the delay period. We expect, then, that these effects are not attributable to an idiosyncratic response to the delay task used. However, Study 2 utilized a different delay task to further address this potential criticism.

Experiment 1 had two additional limitations. First, although hostile affect and aggressive cognitions both were affected by a delay, most researchers and policy officials are concerned with these outcomes only to the extent that they have implications for aggressive behavior, a relationship that the GAM (Anderson & Bushman, 2002) models explicitly. Therefore, Experiment 2 utilized an aggressive behavior measure. A second limitation of Experiment 1 was the lack of a control condition in which no game was played. Presumably, if nonviolent games are in fact suppressing aggressive tendencies, then aggressive tendencies among participants who played no video game should consistently remain between the means for violent and nonviolent game players. Without such a condition, it is difficult to know whether the apparent decrease in aggressive outcomes following nonviolent game exposure is any different than what would be observed with no game at all. Experiment 2 included a no-game control condition to address this issue. We predicted that participants who played no video game would display intermediate levels of aggression (i.e., in-between violent and nonviolent game players), and that the delay period would have no effect on their level of aggression.

## Experiment 2

### Method and procedures

#### Participants

Three hundred eighty-nine undergraduate men at the University of North Carolina participated in the study for credit toward a course requirement. Participants were recruited through a university website.

### Materials

All video games and questionnaires were identical to those used in Experiment 1.

### Delay task

To test the generality of the time delay effect, a different delay task was used in Experiment 2. Participants in the time-delay conditions completed a picture rating task for 15 min. In this task, participants viewed pictures on a computer screen, each displayed for 1000 ms and separated by a 1000-ms inter-stimulus interval. They were asked to rate the “colorfulness” of each picture by pressing one of four keys on the keyboard as the picture appeared. Each participant viewed 120 pictures. All pictures were taken from the International Affective Picture System (IAPS; Lang, Bradley, & Cuthbert, 2001), and were selected based on prior ratings of neutral valence ( $M = 5.46$  on a 9-point scale) and low arousal ( $M = 3.36$  on a 9-point scale).

### Aggressive behavior

Aggressive behavior was measured using a version of the Competitive Reaction Time task (see Taylor, 1967). Participants are told they are competing against another participant in a reaction time test, in which they must click their computer mouse button as quickly as possible following the onset of a visual stimulus. Prior to each of 25 trials, the participant sets the intensity (on a 1–10 scale ranging from 60 to 105 dB, with a 0 dB non-aggressive option) of a noise burst to be delivered to his opponent if the participant were to win that trial. The average noise intensity across all 25 trials constituted the measure of aggressive behavior. The participant also receives noise bursts set by his ostensible opponent on losing trials (12 out of 25).

In actuality, there was no opponent; a computer program determined all wins and losses as well as noise levels received by participants on losing trials. The “opponent” delivered an ambiguous pattern of noise blasts to the participant; this was chosen because prior research suggests that this pattern makes the CRT task more sensitive to effects of other predictor variables. An ambiguous pattern is one in which the first trial is of intensity level 5, the final 24 trials contain three blasts each at intensity levels 2–9, and there is a near-zero correlation between trial number and intensity. However, as a safeguard against suspicion, participants also automatically lost any trial in which response time was longer than 750 ms. This task is a widely used (e.g., see Bartholow & Anderson, 2002; Bushman, 1995; Dorr & Anderson, 1995) and externally valid measure of aggressive behavior (Anderson and Bushman, 1997; Anderson, Lindsay, & Bushman, 1999; Carlson, Marcus-Newhall, & Miller, 1989; Giancola & Chermack, 1998).

### Procedure

The basic procedure for Experiment 2 was identical to that of Experiment 1, with the exception of a die roll that determined if the session involved gameplay. For the no-game control condition, the 30-min gameplay period was omitted; all other procedures were identical.

### Results

Forty-three participants (11%) were excluded from analyses. This was due to equipment failure or failure to follow experimental procedures correctly ( $n = 24$ ), showing no variability in responses (choosing the same noise for each trial;  $n = 11$ ) or suspicion concerning the reaction time task (e.g., that there was no opponent;  $n = 8$ ). Therefore, data from 347 participants were used for analyses.

### Manipulation checks

Post-game questionnaire ratings are given in Table 1. Scores on the laboratory aggression measure were not correlated with any of the individual difference measures. Thus, no questionnaire scores were covaried in the main analyses.

### Aggressive behavior

The noise intensity data were analyzed using a 3 (Condition; violent game, nonviolent game, no-game)  $\times$  2 (Delay; 0-min, 15-min) factorial ANOVA. The predicted Condition  $\times$  Delay interaction was significant,  $F(2, 347) = 5.52, p = .004$  (see Fig. 1). This interaction was investigated using a series of planned contrasts. In the 0-min delay condition, participants who played the violent game were significantly more aggressive ( $M = 6.26, SD = 1.43$ ) than participants who played the nonviolent game ( $M = 5.66, SD = 1.50$ ),  $F(1, 113) = 7.28, p = .008$ . Violent game players displayed marginally more aggression relative to the control group ( $M = 5.94$ ),  $F(1, 103) = 3.35, p = .07$ ; however, aggression levels did not differ significantly between the nonviolent game and control conditions,  $F(1, 115) = .89, p = .35$ . Aggression levels did not differ significantly in the 15-min delay condition ( $M_s = 5.8, 6.03, \text{ and } 5.68$  in the violent, nonviolent, and no-game conditions),  $F(2, 183) = 0.82, p = .442$ .

Additional contrasts examined the effects of delay within levels of the Game variable. The 15-min delay, relative to the 0-min delay, resulted in significantly less aggressive responses in the violent game condition,  $F(1, 101) = 6.31, p = .014$ , and significantly more aggressive responses in the nonviolent game condition,  $F(1, 132) = 4.90, p = .029$ . Delay had no effect on aggression levels in the control condition,  $F(1, 108) = 0.27, p = .604$ .

**Covariates.** To ensure results were not affected by differential game experiences, ratings of the video game on frustration, excitement, and interest were again covaried out of each measured comparison. As in Experiment 1, patterns of means and  $p$ -values for effects of interest were not significantly altered by inclusion of the covariates.

### Discussion

The data from Experiment 2 again supported our primary hypotheses, this time using an aggressive behavior measure. As with measures of aggressive cognition and hostile affect used in Experiment 1, assessment of aggressive behavior immediately following game play showed the typical violent video game effect, with significantly more aggression displayed by those who played a violent compared to a nonviolent game (see Anderson & Bushman, 2001; Anderson & Bushman, 2002). When assessment was delayed 15 min, this time using a picture viewing task, this difference in aggression disappeared. Most importantly, whereas the delay significantly changed aggression levels in the predicted directions for both game-play conditions, as in Experiment 1, aggression levels in the no-game control condition were not affected by the delay. As in Experiment 1, with no pre-assessment of aggression, and no significant difference between the nonviolent and control condition, it is not possible to conclusively state that nonviolent game exposure is reducing aggression relative to baseline. However, as in Experiment 1, this pattern

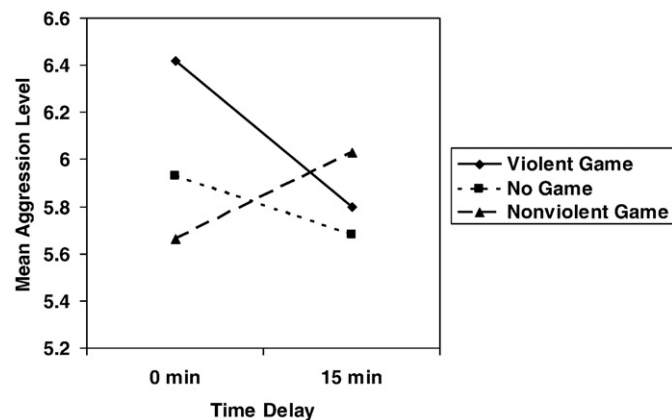


Fig. 1. Mean aggression levels as a function of game play and measurement delay conditions, Experiment 2.

suggests that both types of games—violent and nonviolent—have unique and opposing effects on aggression-related outcomes.

The fact that responses in the two game-play conditions did not differ significantly from responses in the control condition further suggests that interpretation of the violent game effect depends on the choice of a comparison group. However, it is also important to consider that the effect of the violent game (compared to the no-game control) on aggression was not negligible ( $d = .34; p = .07$ ), suggesting that this effect might have reached statistical significance with a larger sample.

Findings from the first two experiments converge on the idea that although violent games appear to increase aggressive tendencies, nonviolent games seem to actually reduce aggressive outcomes. However, the mechanism for these seemingly opposing effects is unclear. Based on predictions derived from the GAM (see Anderson & Bushman, 2002), it could be that exposure to nonviolent games decreases accessibility of hostile cognitions, hostile affect and aggressive behavioral scripts, increases the accessibility of opposing, prosocial constructs, or both. Although the data from Experiment 1 provided some evidence in support of the former possibility, it is not clear whether exposure to nonviolent games that lack explicitly prosocial content have any effects on the accessibility of prosocial cognitions. Previous research has shown that exposure to explicitly prosocial video games can increase prosocial script accessibility (Narvaez et al., 2008) and prosocial behavior (Mares, Palmer, & Sullivan, 2008). Past research (e.g. Anderson & Bushman, 2001) also has shown that violent game play not only increases aggression but also decreases prosocial tendencies. If nonviolent game play essentially counters effects of violent game play, it may serve to increase activation of prosocial constructs as well.

On the other hand, it could be that whereas exposure to violent games decreases accessibility of prosocial constructs, nonviolent games lacking prosocial content do not appreciably affect prosocial outcomes relative to a control condition. Such a pattern would suggest that the findings from the first two experiments reflect effects on aggression-related constructs only, and not a combination of effects on both aggressive and prosocial processes. Experiment 3 was designed to test these possibilities.

### Experiment 3

#### Method and procedures

#### Participants

One hundred and eleven undergraduate students (68 female) at the University of North Carolina and Gettysburg College participated in the study for credit toward a course requirement. Participants were recruited through websites at each University.

#### Materials

Games and initial questionnaires were identical to those utilized in Studies 1 and 2.

#### Dependent measures

Order of administration of the two dependent measures was counterbalanced within each condition.

**Story completion task.** In order to assess the accessibility of prosocial and aggressive cognitions, participants were given three ambiguous unfinished short stories, each detailing a potentially stressful incident (a car accident, a disagreement with a friend, not being served quickly in a restaurant), and asked to provide 15 items detailing “what could have happened next” for each. This measure has been used in several previous studies (e.g. Rule, Taylor & Dobbs, 1987; Bushman & Anderson, 2002; Narvaez et al., 2008). Each response was coded for aggressive, prosocial, or neutral content by two independent judges; the

proportions of prosocial and aggressive responses were the variables of interest. Prosocial responses were behaviors, thoughts, or emotions that supported the well-being of another person (e.g. helping a friend purchase a stereo, concern for the other driver in an accident). Aggressive responses were those that involved behaviors, thoughts or emotions that could do harm to another person (e.g. punching the other driver, the desire to have a waiter fired). Initial agreement was very high ( $ICC = .88$ ), and judges conferred to agree on all discrepant items.

**Word completion task.** The aggressive cognition task (Anderson, Carnagey, & Eubanks, 2003) used in Experiment 1 was modified to include a prosocial subscale. The majority of the scale was identical to the original, but 30 items were also created where the word fragment could be completed as a neutral or prosocial word; for example, A\_D could be completed as AND (neutral) or AID (prosocial). The final measure consisted of 90 items—30 that could be completed as aggressive or neutral, 30 that could only be completed as neutral, and 30 that could be prosocial or neutral. The first two groups were taken from the original measure, and the third was developed for this study. The percentage of completed words that were aggressive and prosocial was the variable of interest.

#### Procedure

The basic procedure for Experiment 3 was similar to that of Experiments 1 and 2, with two exceptions: all participants completed the dependent measures immediately (i.e., there was no delay condition), and a die roll determined if the session involved gameplay. For the no-game control condition, the 30-min gameplay period was omitted; all other procedures were identical. Once gameplay was completed, all participants were administered the Story Completion Task and Word Completion Task in counterbalanced order.

#### Results

Preliminary analyses indicated that participant gender did not significantly influence any of the effects of interest. Thus, gender was not included in the primary analyses. Percentage of aggressive and prosocial words used in the Word Completion Task and percentage of aggressive and prosocial story completions generated in the Story Completion Task were subjected to separate univariate ANOVAs. Means for both measures as a function of Condition are presented in Table 4.

#### Word completion task

There was a significant main effect for Game on both aggressive words,  $F(2, 108) = 6.72, p < .01$ , and prosocial words,  $F(2, 108) = 3.19, p = .045$ . Planned contrasts found that violent game players chose significantly more aggressive words ( $M = 16.9\%$ ) than nonviolent game players ( $M = 13.3\%$ ),  $F(1, 66) = 7.38, p < .01$ . Additionally, violent game players used significantly more aggressive words than control participants ( $M = 12.9\%$ ),  $F(1, 74) = 11.97, p < .001$ ; however, there was no significant difference between nonviolent and control participants,  $F(1, 76) = .13, p = .72$ .

Violent game players also chose significantly fewer prosocial words ( $M = 11.4\%$ ) than nonviolent players ( $M = 13.6\%$ ),  $F(1, 66) = 4.21, p = .044$ . Nonviolent game players chose significantly more prosocial words than control participants ( $M = 11.3\%$ ),  $F(1, 76) = 5.10, p = .027$ ; but there was no significant difference between violent game players and control participants,  $F(1, 76) = .01, p = .92$ .

**Story completion task.** There was a significant main effect for Game on aggressive items,  $F(2, 108) = 6.57, p < .01$ , and prosocial items,  $F(2, 108) = 5.28, p < .01$ . Violent game players again chose significantly more aggressive story completions ( $M = 19.7\%$ ) than nonviolent game players ( $M = 12.9\%$ ),  $F(1, 66) = 10.3, p < .01$ . There was a marginally significant effect when comparing violent game players to control

**Table 4**

Mean proportion of aggressive and prosocial word completions (top) and story completions (bottom): Experiment 3.

	Word completion		
	Violent game	No-game control	Nonviolent game
Aggressive words	16.9 <sub>a</sub>	12.9 <sub>b</sub>	13.3 <sub>b</sub>
Prosocial words	11.4 <sub>a</sub>	11.3 <sub>a</sub>	13.6 <sub>b</sub>
	Story completion		
	Violent game	No-game control	Nonviolent game
Aggressive completions	19.7 <sub>a</sub>	16.0 <sub>a</sub> *	12.9 <sub>b</sub>
Prosocial completions	12.1 <sub>a</sub>	14.1 <sub>a</sub>	16.7 <sub>b</sub>

Note. Within each section, means within each row that do not share a subscript differ,  $p < .05$ . \* Aggressive completion means in the Violent game and No-game control condition differed at  $p = .06$ .

participants ( $M = 16.0\%$ ),  $F(1, 74) = 3.62, p = .061$ ; the nonviolent condition chose significantly fewer aggressive responses than control,  $F(1, 76) = 5.26, p = .025$ .

Violent game players also chose significantly fewer prosocial story completions ( $M = 12.1\%$ ), relative to nonviolent players ( $M = 16.7\%$ ),  $F(1, 66) = 9.73, p < .01$ . Additionally, nonviolent game players generated significantly more prosocial items than the control participants ( $M = 14.1\%$ ),  $F(1, 76) = 4.62, p = .035$ , but violent players showed no significant difference relative to control ( $1, 74) = 2.13, p = .149$ .

#### Discussion

The results from Experiment 3 largely supported our hypotheses. Violent game players again showed significantly more aggressive responses on both dependent measures, relative to both nonviolent game players and no-game control participants. Nonviolent game players provided significantly more prosocial responses on both dependent measures relative to violent game players, and relative to control participants on the word completion task. Nonviolent game players also showed fewer aggressive responses than the control group on the story completion task, although this was not true of the word completion task. More importantly, nonviolent game players gave more prosocial responses than either no-game control or violent game players on both measures (see Table 4).

First, supportive evidence was found for nonviolent games serving as suppressors of aggressive tendencies. On the story completion measure, participants who played no game at all generated significantly more aggressive responses to the story stems than nonviolent game players. This would seem to indicate that nonviolent game play lowered aggressive tendencies. However, this pattern was not seen for the word completion task, where the nonviolent and control conditions were nearly identical. It is possible that these conflicting results are due to the more open-ended nature of the story completion task—partially completed words may have still elicited aggressive responses from nonviolent game players given the limited set of potential completions for each item, but when asked to generate answers to a more open-ended question, aggressive scripts may have been less accessible relative to control. However, it is difficult to be certain as to the cause of these results.

Additionally, this study provided indications that prosocial cognitions can be elicited when participants played games that were not explicitly prosocial. Nonviolent game players generated significantly more prosocial responses on both tasks relative to the violent game and control conditions. This is a novel finding, and is explored further in General discussion.

#### General discussion

The current studies had two primary aims: to investigate the persistence of the well-known violent video game effect, and to

determine whether that effect could be attributable, in part, to suppression of aggression by nonviolent game play. Addressing the first of these aims had implications for understanding the theoretical mechanism(s) responsible for violent game effects. Specifically, the decrease in aggressive outcomes in the violent game condition following a delay suggests that acute exposure effects are due primarily to short-term increases in accessibility of aggression-related constructs in memory (i.e., a priming effect; see Huesmann, 1998; see also Higgins, 1996), and probably do not reflect activation of aggression-related goal structures (Bushman & Anderson, 2001).

Addressing the second aim of this research also has important implications for understanding video game effects typically reported in the experimental literature. Whereas delaying assessment by 15 min led to significant decreases in aggression-related outcomes for participants who played violent games, the delay also led to significant increases in those outcomes among nonviolent game participants. Delay did not significantly affect aggressive behavior in the no-game control condition (Experiment 2). This pattern indicates not only that acute video game exposure effects are relatively short-lived, but also that nonviolent games appear to reduce aggression-related outcomes, and therefore provide a false baseline for assessing effects of violent games on aggressive behavior.

One implication of this finding is that researchers interested in understanding the effects of violent games on aggression-related outcomes would do well to include both nonviolent game and no-game control conditions. A broader implication is that video game exposure effects should be interpreted in terms of the combined effects of both violent games and nonviolent games. The current findings show that even when games are matched on major aggression-relevant variables, it is possible that nonviolent games may have independent effects on aggressive tendencies. Thus, it seems inappropriate to attribute differences between gaming conditions on outcomes of interest solely to effects of violent games.

The present results also have implications for theoretical models of aggression. For example, the GAM (Anderson & Bushman, 2002) does not explicitly outline a mechanism for the reduction of aggression, but the factors in the model depicted as increasing aggression (increased aggressive script accessibility, aggressive affect, hostile schemas) would seem to have nonviolent converses (explicitly pacifistic script accessibility, non-aggressive affect, forgiving schemas). If exposure to typical nonviolent games elicited these responses, then a reduction in state aggression would be consistent with the GAM. All nonviolent games utilized in this study focused on puzzle solving within a cartoonish environment. It is possible that a focus on logical, reasoned thinking—as opposed to the frenzied, see-and-shoot environment of violent games—might serve to activate cognitive constructs and affect that could effectively reduce aggressive inclinations below an individual's usual baseline. Moreover, just as repeated exposure to violent games is hypothesized to have a lasting impact on aggressive outcomes via chronically heightened accessibility of aggressive scripts and hostile affect (Anderson & Dill, 2000; Carnagey & Anderson, 2005) and/or desensitization (see Bartholow, Sestir, & Davis, 2005; Carnagey, Anderson, & Bushman, 2007), repeated exposure to nonviolent games could have the opposite effect. Moreover, from a behavioral engineering perspective, time spent on nonviolent games will reduce exposure to violent games, thereby alleviating some concerns about long-term exposure effects (e.g., see Anderson et al., 2008).

The findings from Experiment 3 provide clarification as to the potential mechanisms of the effect of nonviolent game play on reducing aggression. Specifically, nonviolent games seem to not only decrease the accessibility of hostile cognitions and affect, but such games—even those lacking explicitly prosocial content—also appear to

increase the accessibility of prosocial cognitions. Thus, just as violent and nonviolent games have opposing effects on aggressive outcomes their effects also oppose one another with respect to prosocial outcomes. This pattern has implications for understanding both the findings of previous research testing video game effects on prosocial outcomes (e.g., Gentile et al., 2009; Narvaez et al., 2008) and the psychological mechanisms that contribute to video game effects on aggression. First, with respect to previous studies of video games and prosocial outcomes, the current data suggest that explicitly prosocial content in video games is sufficient but not necessary to increase prosocial responses. Rather, our findings suggest that even games that focus the player on logical puzzle-solving have similar effects. Prosocial constructs should, at least, be primed by modeling and observing instances of similar behavior within a game. However, actual prosocial content does not appear to be necessary to elicit such an effect. Future work should compare the effects of explicitly prosocial games with the type of nonviolent games used here to determine whether they produce any differences in prosocial outcomes.

Second, when considered as a set, the findings from all three of the current studies suggest that the contribution of nonviolent game exposure to the typical “violent video game effect” reflects the combined influence of decreases in the accessibility of aggressive constructs and increases in the accessibility of opposing, prosocial constructs. Experiment 1 demonstrated that nonviolent game exposure produces short-term decreases in accessibility of aggressive cognitions and hostile affect, Experiment 2 produced an analogous set of findings for aggressive behavior, and Experiment 3 showed that playing nonviolent games increases prosocial construct accessibility.

It is important to note that none of the substantive findings of these studies seem to directly contradict the general implications of prior violent game research: namely, that violent game play does increase aggressive outcomes. The current work shows that due to nonviolent games providing a false baseline, these effects may not be as large as previously assumed, but the tendency toward increased aggression was consistently replicated. While prior work directly comparing violent gameplay to a no-game control is rare, findings have supported an independent effect of violent play (e.g. Calvert & Tan, 1994).

Although in theory it would be desirable to specify the nature of the associations among these outcomes by assessing them all in the same participants, thereby allowing a statistical test of mediation (e.g., Baron & Kenny, 1986; MacKinnon, Lockwood, Hoffman, West, & Sheets, 2002), such tests rely on the assumption that the act of measuring more proximal variables (e.g., accessibility of aggressive cognitions) not interfere with or influence more distal variables (e.g., aggressive behavior). Unfortunately, this assumption often is violated in the aggression laboratory (see Lindsay & Anderson, 2000), for at least two reasons. First, as underscored by the current results, effects of manipulated variables on aggression in the lab tend to be fleeting, and thus effects tend to be more pronounced on outcomes measured first compared to those measured later (see Lindsay & Anderson, 2000). Second, measuring proximal variables (e.g., accessibility of aggressive thoughts) can alter subsequent behaviors in important ways, such as by inadvertently revealing the true purpose of the competitive reaction time task (i.e., measuring aggressive behavior). As discussed in detail elsewhere (see Bartholow, Anderson, et al., 2005; Bartholow, Sestir, et al., 2005; Lindsay & Anderson, 2000), this problem is akin to the Heisenberg uncertainty principle in quantum mechanics, which generally holds that measuring one observable quantity increases the uncertainty with which other quantities can be known, because measurement of one variable influences the values of other related variables. However, the symmetry in the patterns of outcomes across experiments strongly suggests that nonviolent game exposure

reduces aggressive responding in the short-term via concurrently suppressing accessibility of aggression-related constructs and enhancing accessibility of prosocial constructs. Although we have speculated that these effects could occur because nonviolent games induce a more reasoned, less impulsive mind-set in players, the mechanism for these effects remains to be specified in future research.

A number of shortcomings of the current research could limit the generalizability of these findings. First, it is possible that the nonviolent game effect could be restricted to the specific subset of games used here. One caveat to this potential limitation is that many games that are completely lacking in violent content tend to have strong puzzle-solving or logical components, and thus these findings could apply to a broad range of nonviolent games. Future work should test this hypothesis with different types of nonviolent games. It could also be the case that the effects seen after the delay manipulation are specific to a 15-min delay. It is difficult to say where exactly the “tipping point” might be in terms of the post-delay shift in means seen in Experiments 1 and 2; use of varying delay manipulations in future studies could help address this. Finally, it is possible that the specific nature of the delay task could have impacted the experimental outcomes. Different tasks were used for Experiments 1 and 2 to help address this possibility, but it cannot be conclusively stated that the delay tasks had no independent effect on the dependent measures.

In addition, generalizability of the effects across gender groups has yet to be strongly established. While there were no significant gender effects in Experiment 3, Experiment 1 suggested that the effects occurred primarily in men, and Experiment 2 did not include women. Future research should attempt to reconcile these discrepancies and arrive at a clearer picture of potential gender differences in gameplay effects.

In sum, the current findings suggest that, in addition to the well-documented deleterious effects of violent video games on antisocial feelings, thoughts, and behaviors, nonviolent video games might also have positive, prosocial effects. Although the current findings suggest that these effects stem from both a “damping down” of aggressive inclinations and the activation of opposing, prosocial constructs, confirmation of this hypothesis awaits future research. In any case, it appears clear that media of varying content have the potential to change patterns of social interaction. To paraphrase an old saying, you are what you consume; this research indicates that in the case of media, this can be a blessing as well as a curse.

**Appendix A**

Prosocial word completion items.

Word Fragment	Prosocial	Neutral
H _ _ p	Help	Heap, Harp
S h _ r _	Share	Shard, Short
P _ _ c e	Peace	Place, Piece
S _ _ e	Save	Some, sore
A _ d	Aid	Add, and
S _ i l _	Smile	Still, Spill
T _ _ c e	Truce	Twice, Trace
R e _ e _ _	Redeem	Revert, reheat
U n _ _ e	Unite	Untie, Uncle
P r _ _ e _ t	Protect	Protest, Present
H _ g	Hug	Hag, Hog
F r _ _	Free	Fray, Fret
H _ _ l	Heal	Hell, Howl
_ i v e	Give	Live, Hive

**Appendix A (continued)**

Word Fragment	Prosocial	Neutral
A _ _ _ s t	Assist	Attest, Artist
T _ _ _ s t	Trust	Toast, Twist
R _ _ s _ _	Rescue	Rascal, Rescan
P r o _ _ _ e	Promote	Produce, Prorate
A g _ _ e	Agree	Agate, Agape
C o _ _ o r t	Comfort	Contort, Comport
L _ v _	Love	Live, Lava
B e n _ _ _ _	Benefit	Bending, Benzene
S u _ _ o r _	Support	Suitors, Succors
C o n _ _ _ e	Console	Convene, Concede
F o r _ _ _ _	Forgive	Forests, Forgets
S _ _ t h e	Soothe	Seethe
P a _ d _ _	Pardon	Payday, Pandas
R _ s t _ _ _	Restore	Resting, Rusting
_ _ _ t u r e	Nurture	Picture, Venture
R _ v i _ e	Revive	Revise, Ravine

Note: List of potential neutral completions is not comprehensive.

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