

Effects of Violent-Video-Game Exposure on Aggressive Behavior, Aggressive-Thought Accessibility, and Aggressive Affect Among Adults With and Without Autism Spectrum Disorder



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Abstract

Recent mass shootings have prompted the idea among some members of the public that exposure to violent video games can have a pronounced effect on individuals with autism spectrum disorder (ASD). Empirical evidence for or against this claim has been missing, however. To address this issue, we assigned adults with and without ASD to play a violent or nonviolent version of a customized first-person shooter video game. After they played the game, we assessed three aggression-related outcome variables (aggressive behavior, aggressive-thought accessibility, and aggressive affect). Results showed strong evidence that adults with ASD, compared with typically developing adults, are not differentially affected by acute exposure to violent video games. Moreover, model comparisons provided modest evidence against any effect of violent game content whatsoever. Findings from this experiment suggest that societal concerns that exposure to violent games may have a unique effect on adults with autism are not supported by evidence.

Keywords

violent video games, aggressive behavior, aggressive-thought accessibility, aggressive affect, autism spectrum disorder, open data, open materials

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In the aftermath of two widely publicized mass shootings, some media pundits and laypeople have speculated that violent video games (VVGs) cause adults with autism spectrum disorder (ASD) to commit violent crimes (e.g., RWW Blog, 2013; also see Harmon, 2012, for responses from experts and members of the ASD community urging caution in interpreting these isolated events as evidence for a link between ASD and planned violence). This speculation is amplified by evidence indicating that people with ASD spend more time playing video games than typically developing (TD) people do (Engelhardt, Mazurek, & Sohl, 2013), are more likely to become preoccupied with

video games than TD people are (Mazurek & Engelhardt, 2013), and often prefer game genres that contain large amounts of violent content (Mazurek & Engelhardt, 2013). However, no study to date has examined whether VVGs influence aggressive behavior among adults with ASD or whether they place adults with ASD at an elevated risk for

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aggression. The current study provides the first empirical evidence bearing on these clinical and societal concerns.

ASD is characterized primarily by difficulties in social communication and interaction and by restricted and repetitive behavior; however, these core symptoms often are accompanied by difficulties with emotional and behavioral regulation (American Psychiatric Association, 2013). One reason that VVGs might differentially affect adults with ASD is that their ability to downregulate arousal is impaired. Some meta-analyses have suggested that acute exposure to VVGs causes increases in physiological arousal relative to exposure to nonviolent video games (NVVGs; see Anderson et al., 2010; but see Adachi & Willoughby, 2011, for an argument that such differences may be caused by confounding game contents). Because individuals with ASD are at increased risk for situational hyperarousal compared with their TD peers (see Ben-Sasson et al., 2009), they might find it difficult to downregulate this proposed mechanism of aggression following exposure to VVGs (see Anderson & Bushman, 2002). Another reason to suspect that VVGs may differentially affect people with ASD is that they have a decreased ability to inhibit prepotent responses (for a meta-analytic review, see Geurts, Bergh, & Ruzzano, 2014). Research indicating that acute exposure to VVGs can undermine the neural correlates of response inhibition (e.g., Hummer et al., 2010) suggests that adults with ASD might behave aggressively following exposure to VVGs in part because they are unable to override the prepotent responses activated by VVGs—such as “attack” or “harm”—when given a chance to aggress in real life.

However, these hypotheses are offset by alternative possibilities suggesting that aggression among adults with ASD might not be affected by VVG exposure. For example, people with ASD are sometimes unable to correctly project mental states onto themselves and others (see Baron-Cohen, 1997). Thus, although adults with ASD might be able to successfully report on specific violent in-game behaviors, their ability to ascribe social attributions to such behaviors (e.g., “I am behaving aggressively”) might be impaired. The idea that people with ASD sometimes struggle with social attributions may also have implications for reactive aggression, because adults with ASD might not exhibit hostile expectations and attributional biases following exposure to VVGs in the same way that TD people do (see Hasan, Bègue, Scharkow, & Bushman, 2013).

The Current Study

Despite a large corpus of studies, meta-analyses of them (Anderson et al., 2010; Ferguson & Kilburn, 2009; Greitemeyer & Mùgge, 2014; Sherry, 2007) have reached differing conclusions about the degree to which

experimental research has demonstrated links between VVGs and aggression. Although some meta-analytic scholars have found the evidence convincing (Anderson et al., 2010; Greitemeyer & Mùgge, 2014), others have concluded that the effect sizes are small and likely explained by publication bias or the use of unstandardized outcome measures (e.g., Ferguson & Kilburn, 2010). In the study reported here, we tested whether there is an effect of violent game content on aggressive behavior and on two proposed mechanisms of such behavior—aggressive affect and accessibility of aggressive thoughts (see Anderson & Bushman, 2002)—and whether any such effects are more pronounced for adults with ASD compared with TD adults. These tests included participants from both diagnostic groups. We also tested whether there is an effect of violent game content on these outcomes separately by diagnostic group. This study is the first to use an experimental paradigm to test the effects of VVGs on aggression in adults with ASD.

Recently, there have been a number of critiques of the usual practices in reporting experiments and interpreting experimental results. In response, we provide detailed coverage of our methods, including our measures, how we decided on sample sizes, and data exclusions. In the Results section, we report Bayes factors, standardized effect-size estimates (Cohen's d and r), and 95% confidence intervals (CIs).

Method

All raw data, verifiable analysis code, measures, video-game manipulations, confederate videos, and study scripts relevant to this report are hosted on the Open Science Framework and can be accessed at osf.io/84xut.

Participants

One hundred twenty adults (60 adults with ASD, 60 TD adults) ranging in age from 17 to 25 years ($M = 20.48$, $SD = 1.71$) participated in this experiment in exchange for \$20. Participants in the ASD group were recruited from an interdisciplinary treatment and research center specializing in ASD. They had been previously diagnosed with ASD according to the center's clinical care model. The diagnostic procedure generally included interviews and behavioral observation focused on the criteria in the fourth edition of the *Diagnostic and Statistical Manual of Mental Disorders* (American Psychiatric Association, 1994), as well as evaluations conducted by a physician or psychologist (or both) using standardized tools, such as the Autism Diagnostic Observation Schedule (Lord, DiLavorne, & Risi, 2002) or the Autism Diagnostic Interview—Revised (Lord, Rutter, & Le Couteur, 1994). To be eligible for the ASD group, individuals had to be

verbally fluent, have an estimated IQ greater than 85, be able to read and write, and be able to tolerate loud noises for periods of about 5 s; they also could not be prone to seizures or prone to vertigo while playing video games. Participants in the TD group were recruited through undergraduate psychology courses, university mass e-mails, and campus flyers. To be eligible for this group, individuals could not have a history of neurological or developmental disorders.

Sample size in the ASD group was set to 60; sample size in the TD group was determined by the sample size of the ASD group. This stopping rule (60 per group) was determined prior to recruiting any participants and was based on funding constraints. The study was approved by our institutional review board.

Video-game manipulation

As other scholars have pointed out, VVGs and NVVGs often differ on numerous dimensions other than violence, such as competitiveness (Adachi & Willoughby, 2011), pace of action (Elson, Breuer, Van Looy, Kneer, & Quandt, 2015), and the extent to which in-game needs are thwarted (Przybylski, Deci, Rigby, & Ryan, 2014). These factors can also affect outcomes related to aggression. An ideal manipulation of violent video-game content, then, would involve two video games that differ in violent content but are matched on all other game features. Such control would permit cleaner inferences concerning effects of game content on outcomes of interest.

To create such an ideal manipulation, we modified two versions of the classic video game *Doom II* using freely available online software (e.g., vd Heiden, 2012). The versions we created and used are hosted and available at Open Science Framework (see Hilgard, 2014). In the classic first-person shooter version of *Doom II*, players adopt a first-person perspective as they shoot and kill demons and zombies in order to progress through the game. We created our violent version of the game from the *Doom II* modification *Brutal Doom*, which increases the amount of violent content and its graphic precision; we created our nonviolent version of the game from the *Doom II* modification *Chex Quest*, which changes the game's graphics and narrative to be nonviolent. We designed these two game versions to be identical in all ways other than violence; computer code that determined game mechanics, controls, level design, enemy locations, and enemy behavior were identical in the two versions. In other words, these games are not off-the-shelf retail games. We modified the games from the ground up using modification software tools and substantial amounts of computer programming. Because of this level of control, if we found that players' experiences and behaviors differed between the two game

conditions, we could infer that these differences were due to the presence versus absence of violent content specifically and not to other structural features of the games (but we did not find evidence for these differences; see Supplemental Results in the Supplemental Material available online). Similar game modifications have been used in previous research (see Elson et al., 2015; Przybylski et al., 2014).

A separate description of the story line, in-game characters, power-ups (e.g., kits that would replenish the player's health), controls, and in-game items (e.g., boxes of bullets) was also crafted for each game version. For example, the description of the VVG indicated that the player assumed the role of a space marine tasked with shooting and killing demons with a Gatling gun or a shotgun on a military base on Mars's moon, Phobos, and that the monsters would shoot the player with bullets or fireballs. In contrast, the NVVG description indicated that the player assumed the role of a hero tasked with helping frightened and confused aliens return to their home planet by using two teleporters, and that the aliens would fling green gobs at the player because they were confused and upset, not because they meant to harm the player.

We manipulated violent content across the game versions by changing the appearance and behavior of in-game characters, the cues associated with hitting game characters, the devices controlled by the player, and the depictions of ammunition and character health. Specifically, in the VVG, (a) monsters attempted to shoot, bite, and claw the player, (b) demons exploded into graphic gore upon being shot (blood splattered on the floor and walls, teeth and limbs were sent bouncing across the floor), (c) participants controlled a Gatling gun and a shotgun, and (d) participants picked up boxes of bullets, health packs, and armor vests. In the NVVG, (a) aliens attempted to hit the player with green gobs, (b) aliens bloodlessly disappeared with a gentle, twinkling sound, (c) participants controlled two teleporter tools (objects similar to large remote controls), and (d) participants picked up electrical teleporter fuel, food, and Chex cereal armor.

Measures

Demographics. Participants responded to questions about their age, gender, race, ethnicity, relationship status, current residence, highest level of education, employment status, academic status (student or nonstudent), current medications, and any previous chronic medical conditions or psychological or developmental disorders (see Table 1 for demographic information on the two groups).

Intelligence. IQ was measured using the Abbreviated Battery IQ (ABIQ) scale from the fifth edition of the

Table 1. Demographic Characteristics of the Two Diagnostic Groups

Characteristic	ASD group ($n = 60$)	TD group ($n = 60$)
Age (mean, in years)	20.42 (2.01)	20.54 (1.34)
IQ score (mean)	103.50 (10.75)	103.40 (8.86)
Autism-Spectrum Quotient score (mean)	70.24 (9.36)	57.17 (8.10)
Gender		
Female (n)	9	8
Male (n)	51	52
Race		
American Indian or Alaskan Native (n)	1	0
Asian (n)	3	2
Black, African American (n)	1	1
White (n)	50	56
Other (n)	4	1
Ethnicity		
Hispanic or Latino (n)	7	0
Non-Hispanic, non-Latino (n)	50	59
Previous diagnosis of a psychiatric or developmental disorder ^a		
No (n)	15	56
Yes (n)	44	4

Note: Standard deviations are given in parentheses. One individual in the autism spectrum disorder (ASD) group did not complete the demographic survey because her experimental session was terminated following the first game-play period; only the IQ score for this participant was collected during the study, but information on age and gender was obtained during a telephone interview that took place prior to the study. Three additional individuals, 2 in the ASD group and 1 in the typically developing (TD) group, did not report their ethnicity. The two groups did not differ in age, unbiased Cohen's d (d_{unb}) = -0.07 , 95% confidence interval (CI) = $[-0.43, 0.29]$, or IQ score, d_{unb} = 0.01 , 95% CI = $[-0.35, 0.37]$; as expected, their scores on the Autism Spectrum Quotient (Hoekstra et al., 2011) did differ, d_{unb} = 1.48 , 95% CI = $[1.07, 1.90]$.

^aIndividuals with ASD reported the following previous diagnoses: chromosome deletion disorder ($n = 1$), Tourette's disorder ($n = 1$), attention-deficit disorder ($n = 2$), bipolar disorder ($n = 2$), depression ($n = 2$), attention-deficit/hyperactivity disorder ($n = 3$), mood disorder ($n = 3$), obsessive-compulsive disorder ($n = 3$), and ASD ($n = 39$); 1 additional participant reported an unspecified previous diagnosis. TD individuals reported the following previous diagnoses: depression or anxiety ($n = 1$), panic disorder ($n = 1$), and anxiety disorder ($n = 2$).

Stanford-Binet Intelligence Scales (Roid, 2003). The ABIQ comprises two routing tests: the verbal (vocabulary) routing test and the nonverbal (object series, matrices) routing test. The ABIQ has demonstrated adequate reliability ($\alpha \geq .90$ for samples with ages similar to the age of the sample in this study) and validity (correlations between the ABIQ and other full-scale IQ measures $> .80$) in previous research (Roid, 2003).

Autism-Spectrum Quotient. ASD symptoms were assessed using the short form of the Autism-Spectrum Quotient (AQ-S; Hoekstra et al., 2011). This 28-item self-report questionnaire is intended to assess ASD symptoms along a continuum, with higher total scores indicating a greater degree of ASD symptomatology. Participants responded to items corresponding to social skills (e.g., "I find it hard to make new friends"), routine (e.g., "I prefer to do things the same way over and over again"), switching (e.g., "In a social group, I can easily keep track of

several different people's conversations"), imagination (e.g., "I find it very easy to play games with children that involve pretending"), and numbers and patterns (e.g., "I am fascinated by dates") on a 4-point Likert scale (1 = *definitely agree*, 2 = *slightly agree*, 3 = *slightly disagree*, 4 = *definitely disagree*). Following previous research (Hoekstra et al., 2011), we utilized all responses to calculate a total score, which had a possible range of 28 to 112. The AQ-S has shown adequate reliability and validity in the general population and in clinical samples (Hoekstra et al., 2011).

Aggressive behavior. Because extreme acts of violence cannot be studied ethically in the lab, researchers often test the causal effects of VVGs on aggression, defined as any behavior intended to cause harm to another individual who is motivated to avoid that harm (Anderson & Bushman, 2002). We measured aggressive behavior using a variant of the competitive reaction time task (CRTT),

which has been widely used in previous video-game research (see Elson et al., 2015; Engelhardt, Bartholow, & Saults, 2011; Hasan et al., 2013).

In the version used in this study, participants were led to believe that they were competing against another participant to determine who could react more quickly—by clicking a computer mouse—following the presentation of a colored square on a computer monitor. Before each of the nine experimental trials, a participant had the opportunity to set both the intensity and the duration of the noise blast that would be delivered to the opponent if the participant were to win that trial. Intensity was set on a scale from 1 (60 dB) to 10 (105 dB), with a 0-dB no-noise option, and duration was set on a scale from 1 (1 s) to 10 (5 s), with a 0-s option. Each participant received noise blasts set by the ostensible opponent on trials that the participant lost (five of the nine trials).

In reality, however, there was no opponent; a predetermined computer algorithm controlled the outcome of all trials. All participants lost the first trial and received a loud noise blast (intensity at Level 9 and duration at Level 8) intended to provoke them so that we could examine differences between unprovoked aggression (their initial noise setting) and reactive aggression (their subsequent noise setting). The intensity and duration of the remaining noise blasts set by the fictitious opponent were randomly determined by the computer algorithm. In order to prevent suspicion, we programmed the algorithm so that participants automatically lost any trial on which their response time was longer than 2 s.

Although the CRTT has been used extensively to measure aggressive behavior in previous game research, there seems to be little standardization regarding how to quantify the data obtained, which has perhaps led to inflated effect-size estimates in studies that have used the CRTT (for a review, see Elson, Mohseni, Breuer, Scharrow, & Quandt, 2014). With this issue in mind, we report the following CRTT aggression scores as our operationalization of aggressive behavior: noise intensity, noise duration, and a standardized and summed composite of noise intensity and duration for Trial 1 (unprovoked aggression), Trial 2 (reactive aggression), and Trials 3 to 9 (average aggression).

Aggressive-thought accessibility. Accessibility of aggressive thoughts was measured with a 98-item word-completion task (see Anderson et al., 2004). Participants were asked to complete partial words with the first word that came to mind. For example, “k i _ _” could be completed as *kind*, *kiss*, *kick*, or *kill*. Fifty of the items could be completed as aggression-related words; the remaining items could be completed only as non-aggression-related words. Aggressive-thought accessibility was calculated as the proportion of items attempted that were completed as aggression-related words.

Aggressive affect. Aggressive affect was measured with the 10-item short form of the State-Anger Scale (Spielberger, Jacobs, Russell, & Crane, 1983) and with 7 items from the State Hostility Scale (Anderson & Carnagey, 2009) plus 1 additional item (“I feel positive”). All 18 items were rated on a 5-point Likert-type scale (1 = *strongly disagree*, 2 = *disagree*, 3 = *neither agree nor disagree*, 4 = *agree*, 5 = *strongly agree*). Example items included “I feel like hitting someone” and “I feel like banging on the table.” The 18-item scale showed adequate reliability in the current study ($\alpha_s = .90$ and $.92$ for the ASD and TD groups, respectively). Although scale reliabilities were adequate, inspection of the item-level correlations indicated that 3 of the 18 items loaded poorly on the latent factor, particularly in the ASD group ($r_s < .20$). These items were “I feel tender,” “I feel tame,” and “I feel sympathetic.” Thus, analyses for this outcome variable were conducted on a 15-item composite score that excluded the 3 poorly loading items. Removing these items had no effect on the interpretation of the data.

Postgame rating measures (manipulation checks).

Following all other experimental procedures, participants rated their assigned video game on several dimensions (see Table 2). Frustration was rated on a 7-point scale with the following anchors: 1 = *not at all*, 4 = *moderately*, 7 = *extremely*. The other five items were rated on a 7-point Likert scale with the following anchors: 1 = *strongly disagree*, 4 = *neither agree nor disagree*, 7 = *strongly agree*.

Procedure

Prior to arriving at the lab, participants were assigned to play the VVG ($n = 59$) or the NVVG ($n = 61$) using a random number generator.¹ Among adults with ASD, 29 played the VVG and 31 played the NVVG; among TD adults, 30 played the VVG and 30 played the NVVG. Stratified random assignment was used to ensure that for each diagnostic group, the numbers of participants who were assigned to play the VVG and the NVVG were approximately equal within each gender.

At the lab, participants were greeted by an experimenter who was not blind to the experimental condition. After participants were escorted to the room where the experiment would take place, the experimenter explained that the purpose of the study was to understand how the reaction times of young adults with and without ASD were affected by screen-based media (the cover story). Informed consent was then obtained, after participants had an opportunity to ask questions about the study. Participants were informed that there would be a brief competition during the study, but that the bulk of the experiment, including the sessions of video-game play, would be completed individually.

Table 2. Comparison of the Postgame Ratings in the Two Game Conditions

	Violent game (<i>n</i> = 59)	Nonviolent game (<i>n</i> = 60)	Unbiased Cohen's <i>d</i>
I felt the video game featured a great amount of violence.	5.97 (1.36)	3.23 (1.81)	1.70 [1.27, 2.13]
To what extent were you <i>frustrated</i> by the video game you played?	3.19 (1.36)	3.02 (1.44)	0.12 [-0.25, 0.49]
I felt <i>excited</i> while playing the video game.	4.80 (1.62)	4.22 (1.55)	0.36 [-0.01, 0.73]
I felt <i>engaged</i> while playing the video game.	5.44 (1.58)	5.12 (1.35)	0.22 [-0.15, 0.58]
I found the game I played to be <i>interesting</i> .	4.81 (1.70)	4.48 (1.73)	0.19 [-0.18, 0.56]
I found the game I played to be <i>challenging</i> .	5.10 (1.57)	4.65 (1.77)	0.27 [-0.10, 0.63]

Note: For each game condition, the table presents means, with standard deviations in parentheses. All ratings were on scales from 1 to 7. Values in brackets are 95% confidence intervals.

Three activities were completed before participants were exposed to any stimulus or discussion related to their assigned video game. The verbal and nonverbal routing subtests from the ABIQ were administered first. Next, participants were told that they would be given a chance to practice two of the reaction time tasks to be completed later in the experiment. Participants were led to believe that these two tasks were designed to measure their reaction time abilities. The first practice task involved completing 10 of the items from the thought-accessibility measure. An experimenter explained that each item should be completed with the first word that came to mind, instructions consistent with the cover story. Five of the 10 items had a .25 probability of being completed as aggression-related words; the remaining items could be completed only as neutral (non-aggression-related) words. The practice task provided a baseline measure of the accessibility of aggressive thoughts. Illegible responses were clarified by asking participants nonleading questions (e.g., "What word did you intend to write here?").

The second practice task was the CRTT. An experimenter went over all CRTT instructions and showed participants examples of low-intensity (Level 1), medium-intensity (Level 5), and high-intensity (Level 10) noise blasts lasting 1 s each, in order to demonstrate that the noise blasts were aversive. Participants were then allowed to practice the task individually (they were told that they were indeed practicing, not competing), so that they would be comfortable with the instructions and controls. In other words, aggressive behavior was not assessed during this practice period. Participants were told that another participant, who was ostensibly completing a separate study, would be competing against them later in the experiment. A purported live video connection was then established with the opponent (matched for gender). In actuality, participants were shown a prerecorded video of a confederate, similar to one used in previous research (see Engelhardt et al., 2011). Prior to this

ostensible interaction, participants were informed that although they would be able to see their opponent, their opponent would not be able to see them. This statement was intended to provide participants with anonymity. The communication between the experimenter and the opponent during this staged interaction appeared to transpire in real time. For example, a timing cue (head scratch) in the video prompted the experimenter to pretend to communicate with the confederate over an intercom by stating, "There is a camera in the corner of the room. Can you turn around and give us a wave?" Upon purportedly hearing this request, the confederate, who was instructed to maintain a neutral facial expression throughout the video, briefly scanned the room prior to orienting to the corner with the camera and waving as requested.

Participants were then shown the video game they would be playing throughout the experiment. After they read the description for their assigned game, the experimenter unobtrusively watched them play the game until it was clear that they were comfortable with the controls. This generally occurred within 30 s.

Starting with this initial play period, participants alternated between playing their assigned video game for a period of time (15 min during the first play period, 10 min during the second and third play periods) and completing a measure of aggression. Specifically, they completed the CRTT (which measured unprovoked aggression, reactive aggression, and average aggressiveness), the measure of aggressive-thought accessibility, and the measure of aggressive affect approximately 1.5 min following the first, second, and third game-play periods, respectively.²

Once the experiment had ended, participants completed the AQ-S, the demographic questionnaire, and the postgame ratings. They were then probed for suspicion concerning the cover story and their alleged opponent. For example, they responded to open-ended questions about what they thought the study was designed to

investigate, whether any aspects of the study seemed confusing or strange, and their impression of the participant they competed against during the CRTT. If a participant reported any suspicions, the experimenter used a funneled questioning procedure to probe when and why these suspicions arose (e.g., during the staged video recording vs. during or after the CRTT). All participants were then thanked for their time. Debriefing occurred after data collection had been completed so that the cover story would not be undermined by word of mouth.

Results

Analytic strategy

As we mentioned earlier, there has been increased scrutiny in experimental psychology as to what constitutes evidence for effects. Given the numerous critiques of conventional null-hypothesis significance testing (e.g., Wagenmakers, 2007), we adopted the use of Bayes factors for quantifying the strength of evidence (see Edwards, Lindman, & Savage, 1963). Unlike point estimates and CIs, Bayes factors provide a direct measure of evidence for a null statement that there is no effect for a specific contrast relative to an alternative statement that there is such an effect (Rouder, Morey, Speckman, & Province, 2012).

Our experiment had a 2 (game condition: VVG or NVVG) \times 2 (diagnostic group: ASD or TD) design. We used Bayesian analyses to test for main effects and interactions. Bayesian analysis differs from traditional analysis of variance (ANOVA) in that models encompassing main effects and interactions, or their selective absence, are compared. Five models were considered: a null model in which there were no effects of diagnostic group or of game condition, a model in which there was an effect of diagnostic group but not of game condition, a model in which there was an effect of game condition but not of diagnostic group, an additive model in which there were effects of both variables but no interaction between them, and a full model in which there were not only effects of both variables but an interaction as well. Rouder et al. (2012) provided the models and computational algorithms for the Bayes factors reported here. Computations were performed in Morey and Rouder's (2014) BayesFactor package for R.

Bayes factors have a convenient interpretation. They are reported in ratios (e.g., 10 to 1) indicating the strength of evidence for a model with an effect relative to a model without an effect. Evidence in the Bayesian context refers to how data should normatively change beliefs. For example, if a Bayes factor is 10 to 1 in favor of an effect, then prior beliefs (e.g., that an effect is as likely as not)

need to be multiplied by a factor of 10. These evidence ratios are computed as the probability of the observed data under one model relative to the probability of the observed data under a different model, and they are a deep consequence of Bayes's rule.

Researchers using Bayes factors need to place prior distributions on the magnitude of effects. We used what Rouder et al. (2012) called a default prior. Accordingly, the effect size was distributed symmetrically around zero, with some spread, and smaller effects were more likely than larger ones. With this default, negative effects, in which VVGs led to a reduction in aggression, were a priori as likely as positive effects, in which VVGs led to an increase in aggression. We followed the advice of Rouder et al. and tuned the size of the expected effects to be in line with the effect sizes reported in previous research (see Anderson et al., 2010).

As recommended by the American Psychological Association (2010), we also report standardized effect-size estimates and CIs. To calculate unbiased Cohen's d s (d_{unb} s) for comparisons of independent means, we used the pooled within-groups standard deviation as the standardizer. The 95% CIs for d_{unb} s were derived using approximations for the noncentral t distribution (see Algina & Keselman, 2003). For all interaction terms with a single degree of freedom, we report effect-size r and its CI (Rosenthal & Rubin, 2003).

We sometimes refer to estimated effect sizes as tiny, small, or moderate in magnitude, but such labels do not necessarily indicate that results were statistically significant. Sufficient information (F statistics, sample sizes, and CIs) is provided, however, for readers to determine whether an effect was significant at the .05 level.

Demographics and manipulation checks

As expected, large group differences were observed on the AQ-S measure; adults with ASD had more autism-like traits than did TD adults. In contrast, group differences in IQ and age were estimated to be near zero (see Table 1). The manipulation-check items indicated that the VVG was perceived to be more violent than the NVVG, as intended, and that the games were comparable on dimensions other than perceived violence (see Table 2). The results summarized in Tables 1 and 2 suggest that the diagnostic groups were well matched and that the manipulation of violent content in the video games was powerful.

Aggressive behavior

All data from 27 participants were excluded from the CRTT analyses: 13 because of their level of suspicion regarding the opponent, 10 because of their response set

Table 3. Aggression Scores on the Competitive Reaction Time Task

Variable	Game condition		Diagnostic group		Cell			
	VVG (<i>n</i> = 48)	NVVG (<i>n</i> = 45)	ASD (<i>n</i> = 48)	TD (<i>n</i> = 45)	ASD, VVG (<i>n</i> = 25)	ASD, NVVG (<i>n</i> = 23)	TD, VVG (<i>n</i> = 23)	TD, NVVG (<i>n</i> = 22)
Trial 1: intensity	4.32 (2.26) ^a	4.22 (2.33)	4.72 (2.33) ^a	3.80 (2.16)	5.00 (2.45) ^a	4.43 (2.21)	3.61 (1.83)	4.00 (2.49)
Trial 1: duration	3.38 (2.22) ^a	2.73 (1.51)	3.38 (2.00) ^a	2.73 (1.81)	3.75 (2.25) ^a	3.00 (1.65)	3.00 (2.17)	2.45 (1.34)
Trial 1: composite	0.19 (1.92) ^a	-0.19 (1.54)	0.36 (1.90) ^a	-0.38 (1.50)	0.67 (2.11) ^a	0.04 (1.64)	-0.32 (1.61)	-0.44 (1.43)
Trial 2: intensity	6.46 (2.90)	6.89 (2.76) ^a	6.65 (2.71)	6.68 (2.98) ^a	6.36 (2.91)	6.96 (2.50)	6.57 (2.94)	6.81 (3.09) ^a
Trial 2: duration	6.46 (2.97)	5.43 (2.87) ^a	5.83 (2.97)	6.11 (2.96) ^a	6.44 (3.08)	5.17 (2.76)	6.48 (2.91)	5.71 (3.04) ^a
Trial 2: composite	0.09 (1.90)	-0.10 (1.78) ^a	-0.05 (1.76)	0.06 (1.93) ^a	0.05 (1.93)	-0.16 (1.59)	0.14 (1.90)	-0.03 (2.00) ^a
Trials 3–9: average intensity	5.69 (1.45)	5.73 (1.35) ^a	5.73 (1.49)	5.68 (1.30) ^a	5.66 (1.45)	5.81 (1.57)	5.71 (1.49)	5.64 (1.08) ^a
Trials 3–9: average duration	5.32 (1.57)	5.16 (1.66) ^a	5.42 (1.69)	5.05 (1.50) ^a	5.61 (1.58)	5.22 (1.80)	4.99 (1.52)	5.10 (1.52) ^a
Trials 3–9: average composite	0.03 (1.84)	-0.03 (1.84) ^a	0.13 (1.95)	-0.14 (1.70) ^a	0.20 (1.81)	0.06 (2.13)	-0.15 (1.90)	-0.14 (1.51) ^a

Note: The table presents means, with standard deviations in parentheses. The composite measure of aggression was created by standardizing and summing the scores for intensity and duration. ASD = autism spectrum disorder; TD = typically developing; VVG = violent video game; NVVG = nonviolent video game.

^aThe sample size was 1 smaller than indicated in the column heading.

(e.g., setting the noise and duration meters at the same level on all trials), 3 because of a failure to understand or follow task instructions, and 1 because of the discomfort associated with receiving loud noise blasts through a pair of headphones. Additionally, 1 participant believed that the opponent was real on the first trial only, and 1 participant believed that the opponent was real on every trial but the first trial. These 2 participants were included in analyses when appropriate. Approximately equal numbers of participants were excluded from each of the experimental conditions (see Table 3 for cell sizes). Because settings for noise intensity and noise duration were highly correlated on the first trial, $r = .53$, 95% CI = [.35, .71]; on the second trial, $r = .68$, 95% CI = [.53, .83]; and on Trials 3 through 9, $r = .67$, 95% CI = [.52, .83], we also quantified aggression by standardizing and summing the intensity and duration settings. Figure 1 depicts the means for this standardized and summed composite score for unprovoked, reactive, and average aggression, and this composite is the outcome measure discussed in the sections that follow. However, Table 3 also displays means and standard deviations separately for noise intensity and duration, in line with recommendations of other researchers (Elson et al., 2014).

Unprovoked aggression (Trial 1). Trial 1 noise blasts represent unprovoked aggression because they were not influenced by interactions with the perceived opponent over the remainder of the CRTT. Conventional analysis revealed a small effect of game condition, $F(1, 88) = 1.09$, $d_{\text{unb}} = 0.22$, 95% CI = [-0.20, 0.64]; some evidence that adults with ASD were more aggressive than TD adults, $F(1,$

88) = 4.21, $d_{\text{unb}} = 0.43$, 95% CI = [0.00, 0.85]; and virtually no evidence for a Game Condition \times Diagnostic Group interaction, $F(1, 88) = 0.54$, $r = .08$, 95% CI = [-.13, .28].

To test the effects of violent game content on aggressive behavior, and to test for differential effects in the two diagnostic groups, we set the scale parameter (r) of the prior in our Bayesian analysis to .43, commensurate with an expected effect size (ρ) of .21 (see Anderson et al., 2010). The Bayes-factor model comparison yielded modest evidence against any effect of game condition. The best model was the diagnostic-group model (with no effect of violent content). This model was preferred by a factor of 2.5 to 1 over the additive model (with main effects of both diagnostic group and game condition) and was preferred by a factor of 6.1 to 1 over the full model (with both effects and an interaction). These results indicate that aggressive behavior on Trial 1 seems to have been invariant to the degree of violence in the video game. Although the diagnostic-group model was preferred over the null model with no effects whatsoever, this preference was by a factor of only 1.5 to 1. To test for effects of violent game content separately for the two diagnostic groups, we performed follow-up contrasts. Results showed a slight preference for null effects among both adults with ASD (1.8 to 1) and TD adults (2.9 to 1).³

Reactive aggression (Trial 2). All participants lost the first CRTT trial and received a noise blast intended to provoke them (intensity at Level 9 and duration at Level 8). As a result, we consider the second trial of the CRTT to provide a measure of aggression immediately following provocation. A conventional ANOVA showed tiny

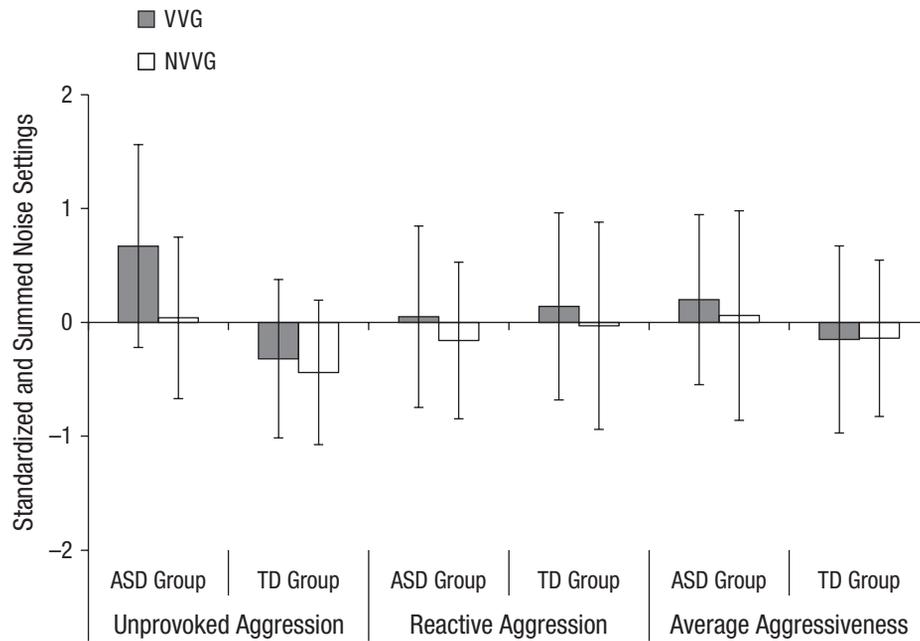


Fig. 1. Results from the competitive reaction time task: mean composite score (standardized and summed noise settings) for unprovoked, reactive, and average aggression as a function of game condition and diagnostic group. Error bars denote 95% confidence intervals. ASD = autism spectrum disorder; TD = typically developing; VVG = violent video game; NVVG = nonviolent video game.

effects of game condition, $F(1, 88) = 0.25$, $d_{\text{unb}} = 0.11$, 95% CI = [-0.31, 0.53], and diagnostic group, $F(1, 88) = 0.08$, $d_{\text{unb}} = -0.06$, 95% CI = [-0.48, 0.36], and a non-existent interaction between these two variables, $F(1, 88) = 0.00$, $r = .00$, 95% CI = [-.20, .20].

Bayes-factor model comparisons revealed that the null model was preferred to a model with game condition only (3.6 to 1), a model with diagnostic group only (3.9 to 1), a model with both variables (13.8 to 1), and the full model (43.5 to 1). Follow-up contrasts also showed a slight preference for a null effect for both adults with ASD (2.9 to 1) and TD adults (2.9 to 1). In sum, we obtained positive evidence that reactive aggression was invariant to diagnostic group and violent game content.

Average aggression. A conventional ANOVA on average aggression (CRTT Trials 3–9) revealed tiny effects for game condition, $F(1, 88) = 0.03$, $d_{\text{unb}} = 0.04$, 95% CI = [-0.38, 0.46]; diagnostic group, $F(1, 88) = 0.49$, $d_{\text{unb}} = 0.15$, 95% CI = [-0.27, 0.57]; and the interaction between these terms, $F(1, 88) = 0.04$, $r = .02$, 95% CI = [-.18, .22]. Bayes-factor model comparisons revealed support for the null model over the game-condition model (4.0 to 1), the diagnostic-group model (3.2 to 1), the additive model (13.0 to 1), and the full model (41.0 to 1). Follow-up contrasts also showed a slight preference for null effects among both adults with ASD (3.0 to 1) and TD adults (3.0 to 1).

Aggressive-thought accessibility

Data from 2 participants were removed from the analyses of aggressive-thought accessibility: 1 because the participant thought we were examining subliminal aggression, and 1 because the participant realized that the items could be completed as aggression-related and non-aggression-related words. Table 4 depicts the means and standard deviations for this measure, on both practice trials and postgame trials. The final row (also see Fig. 2) shows no evidence that the independent variables in the study had any appreciable effect on the accessibility of aggressive thoughts following game play.

This visual inspection is corroborated by the results of both conventional ANOVA and Bayesian analysis. The ANOVA showed a small effect for violent content, $F(1, 113) = 0.57$, $d_{\text{unb}} = 0.14$, 95% CI = [-0.23, 0.51]; diagnostic group, $F(1, 113) = 0.05$, $d_{\text{unb}} = -0.04$, 95% CI = [-0.41, 0.33]; and the two-way interaction between these terms, $F(1, 113) = 0.53$, $r = -.07$, 95% CI = [-.25, .12]. For the Bayesian analysis, we set the scale parameter (r) of the prior to .45, commensurate with an expected effect size (ρ) of .22 (see Anderson et al., 2010). The Bayes factors revealed support for the null model over the game-condition model (3.6 to 1), the diagnostic-group model (4.5 to 1), the additive model (16.1 to 1), and the full model with all terms (46.9 to 1). We also performed follow-up contrasts to assess the effect of violent content separately for adults with ASD and TD adults. Once

Table 4. Performance on the Aggressive-Thought-Accessibility Task

Variable	Game condition		Diagnostic group		Cell			
	VVG (<i>n</i> = 59)	NVVG (<i>n</i> = 61)	ASD (<i>n</i> = 60)	TD (<i>n</i> = 60)	ASD, VVG (<i>n</i> = 29)	ASD, NVVG (<i>n</i> = 31)	TD, VVG (<i>n</i> = 30)	TD, NVVG (<i>n</i> = 30)
Practice items (10 items)	.10 (.14)	.10 (.14)	.10 (.15)	.10 (.14)	.10 (.14)	.10 (.16)	.10 (.14)	.11 (.13)
Practice items following game play	.15 (.15) ^b	.16 (.17) ^a	.15 (.15) ^b	.17 (.17) ^a	.17 (.15) ^a	.13 (.16) ^a	.14 (.15) ^a	.19 (.18)
All items following game play	.18 (.05) ^b	.17 (.05) ^a	.17 (.06) ^b	.17 (.04) ^a	.17 (.06) ^a	.17 (.05) ^a	.18 (.04) ^a	.17 (.04)

Note: The table presents means, with standard deviations in parentheses. The measure for practice is the proportion of 10 items completed as aggression-related words before game play. The next row of the table shows the proportion of those same 10 items that were completed as aggression-related words on the full-scale (98-item) measure following game play. The final row shows the proportion of all items on the full-scale measure that were completed as aggression-related words. ASD = autism spectrum disorder; TD = typically developing; VVG = violent video game; NVVG = nonviolent video game.

^aThe sample size was 1 smaller than indicated in the column heading. ^bThe sample size was 2 smaller than indicated in the column heading.

again, these contrasts showed a slight preference for a null effect for both adults with ASD (3.4 to 1) and TD adults (1.8 to 1). Taken together, our analyses provide positive evidence that the accessibility of aggressive thoughts was invariant to diagnostic group and violent game content.

Aggressive affect

Means and standard deviations for this outcome variable are presented in Table 5 and are depicted in Figure 3. Results from a traditional ANOVA showed tiny effects for

game condition, $F(1, 115) = 1.11$, $d_{\text{unb}} = 0.19$, 95% CI = [-0.17, 0.56], and diagnostic group, $F(1, 115) = 0.29$, $d_{\text{unb}} = -0.09$, 95% CI = [-0.46, 0.27], but a small-to-moderate effect of the Game Condition \times Diagnostic Group interaction, $F(1, 115) = 3.73$, $r = -.18$, 95% CI = [-.34, .01].

For the Bayesian analyses, the scale parameter (r) of the prior was set to .61, commensurate with an expected effect size (ρ) of .29 (see Anderson et al., 2010). The Bayes factors demonstrated that the null model was preferred to the model with game condition (3.6 to 1), the model with diagnostic group (5.4 to 1), the additive model (19.9 to 1), and the model with the main effects and interaction term (16.6 to 1). Follow-up contrasts assessing the effect of violent content on aggressive affect separately for adults with ASD and TD adults were also conducted. These contrasts indicated that the null model was slightly preferred to a model with game condition among adults with ASD (3.8 to 1), but that the model with game condition was slightly preferred to the null model among TD adults (1.7 to 1).

Discussion

This experiment is the first to directly test whether VVGs affect adults with ASD and TD adults differently, as well as the first to test whether VVGs affect aggressive behavior among adults with ASD. We found strong evidence against the first possibility and modest evidence against the second. These results add important information to the growing understanding of the experiences of adults with ASD and provide the first experimental evidence countering media speculation about effects of video games in this population. Three model comparisons for the TD group indicated that there was modest evidence against an effect of violent content on aggressive behavior as large as that reported ($r = .21$) in one previous meta-analysis on this topic (see Anderson et al., 2010). Most previous research

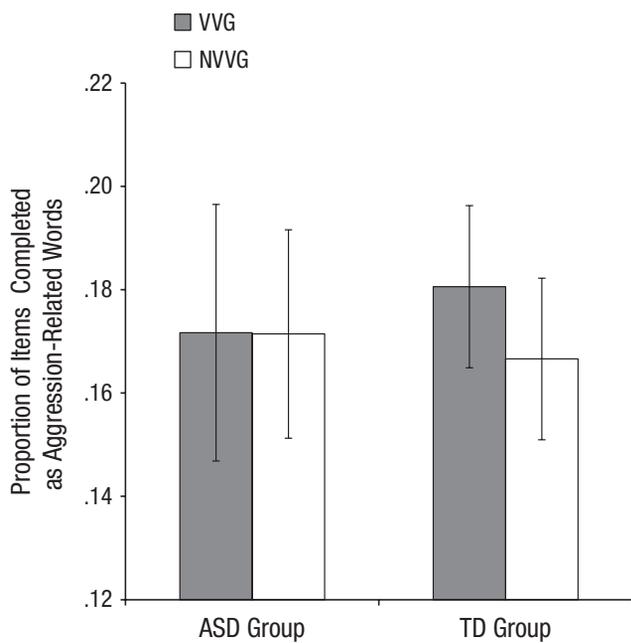


Fig. 2. Aggressive-thought accessibility following game play: proportion of items completed as aggression-related words as a function of game condition and diagnostic group. Error bars denote 95% confidence intervals. ASD = autism spectrum disorder; TD = typically developing; VVG = violent video game; NVVG = nonviolent video game.

Table 5. Aggressive-Affect Composite Scores

Game condition		Diagnostic group		Cell			
VVG (<i>n</i> = 59)	NVVG (<i>n</i> = 60)	ASD (<i>n</i> = 59)	TD (<i>n</i> = 60)	ASD, VVG (<i>n</i> = 29)	ASD, NVVG (<i>n</i> = 30)	TD, VVG (<i>n</i> = 30)	TD, NVVG (<i>n</i> = 30)
1.96 (0.61)	1.83 (0.67)	1.86 (0.65)	1.92 (0.64)	1.81 (0.57)	1.91 (0.72)	2.10 (0.62)	1.75 (0.61)

Note: The table presents means, with standard deviations in parentheses. ASD = autism spectrum disorder; TD = typically developing; VVG = violent video game; NVVG = nonviolent video game.

relied on contrasts between games that differed in violent content but may also have differed in other ways. As in a few other recent studies (e.g., Elson et al., 2015; Przybylski et al., 2014), the games we used were carefully matched on all dimensions but violent content to avoid confounding violent content with other game features. Thus, effects ascribed to violent content in previous video-game studies (see Anderson et al., 2010; Greitemeyer & Mügge, 2014) may actually have been due to the conflation of violent content with other game features that typically differ between VVGs and NVVGs and that influence aggressive behavior.

The ANOVAs involving the theoretical mediators of aggression—aggressive-thought accessibility and aggressive affect—also indicated that there was strong evidence against the hypothesis that VVGs affect adults with ASD and TD adults differently. The follow-up pairwise contrasts among adults with ASD provided slight evidence

that exposure to violent content did not affect these outcomes. However, the pairwise contrasts among TD adults suggested that there was slight evidence for an effect of video-game violence on aggressive affect.

Although the current study is the first empirical examination of video-game effects in adults with ASD, some limitations of the design should be noted. First, it is possible that the NVVG contained sufficient violence to have an effect on players, causing within-person increases on the aggression-related outcomes and reducing the apparent magnitude of the between-subjects effects of violent game content reported here. Although we cannot rule out this possibility, we think it is fairly remote. Recall that participants found the NVVG to be much less violent than the VVG (see Table 2). Additionally, the description of the scenario for the NVVG indicated that players were trying to help (not harm) confused and scared aliens by teleporting them to their home planet.

A second potential limitation is that we did not obtain baseline measures of aggressive behavior and aggressive affect, and therefore cannot determine whether playing the games caused differential increases or decreases on these measures relative to baseline. We intentionally did not administer these measures before game play because that could have alerted participants to the nature of the study, undermined our cover story, and introduced unwanted demand characteristics. Although we acknowledge that pre- and postgame measures can be useful in revealing how VVGs and NVVGs might cause differential changes in outcomes related to aggression, we urge researchers to exercise caution when using such methods.

A third limitation is that the sample size was relatively small. However, paradigms in which adults with ASD are randomly assigned to experimental conditions are exceedingly rare, as are sample sizes larger than ours in cross-sectional designs. More to the point, our sample was reasonably large, especially considering the challenges of recruiting members of a clinical population from a single geographic location. Consequently, the evidence provided here can be considered the best available to date for this special population (see Rosnow & Rosenthal, 1989). We acknowledge, though, that a larger sample size would be desirable and may provide stronger, more compelling Bayes factors.

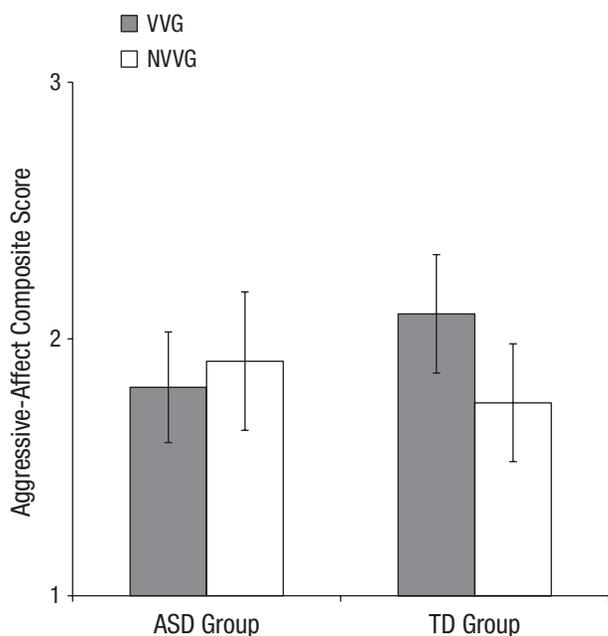


Fig. 3. Results for aggressive affect: composite score as a function of game condition and diagnostic group. Error bars denote 95% confidence intervals. ASD = autism spectrum disorder; TD = typically developing; VVG = violent video game; NVVG = nonviolent video game.

Two additional points are worthy of discussion. The first concerns our decision to use modifications of *Doom II*, which was originally released in 1994. We do not find the age of *Doom II* to be of much concern, because previous research has suggested that the age of a violent game has no effect on outcomes related to aggression (Ivory & Kalyanaraman, 2007), and because the age of *Doom II* is irrelevant to our primary research questions. We deliberately chose this game in order to operationalize the construct of violent content.

The second point concerns the trade-off between internal and ecological validity. We intended to manipulate violent game content. Therefore, to the extent that the games presented violent and nonviolent video-game content while holding all other game design elements constant, we believe that our design maximized the aspects of validity that were central to our research questions. As in most experimental research, there were compromises between internal and ecological validity. Had we wanted to maximize ecological validity, we could have purchased two modern video games, one exceedingly violent and one not, and used those in the current study. However, such a manipulation would lack internal validity, as contemporary games containing violent content generally differ substantially from contemporary nonviolent games in dimensions besides violent content. We believe that the current study therefore provides excellent internal validity and acceptable ecological validity. Even so, the results do not necessarily generalize to video games in general.

When events or behaviors seem inexplicable, people often generate causal attributions based on anecdotal or correlational evidence. Psychological science can help inform understanding of such events by directly testing these attributions. In the aftermath of widely publicized acts of violence, the purpose of this study was to address the controversial question of whether video-game violence differentially affects the behavior of adults with ASD (also see Ferguson & Olson, 2014, for complementary correlational findings among people with elevated levels of depression and attention-deficit/hyperactivity disorder). Although we did not investigate violent behavior, we did examine the extent to which adults with ASD and TD adults are willing to harm someone following brief exposure to a video game. The results of our study provide strong evidence against the hypothesis that VVGs affect adults with ASD differently than TD adults. Moreover, the results suggest that VVGs do not affect aggression in adults with ASD whatsoever. As is the case with a single study on any topic, the current findings should not be considered the final word on this subject, and we hope that other researchers will endeavor to replicate our approach using similar experimental methods. Although our findings are preliminary, we hope that they

will help to correct faulty assumptions among the general public and that they will inform understanding of a clinically relevant topic.

Author Contributions

C. R. Engelhardt, M. O. Mazurek, J. Hilgard, and B. D. Bartholow developed the study concept and contributed to the study design. C. R. Engelhardt, J. Hilgard, and J. N. Rouder performed the data analysis and interpretation. C. R. Engelhardt drafted the manuscript, and M. O. Mazurek, J. Hilgard, J. N. Rouder, and B. D. Bartholow provided critical revisions. All authors approved the final version of the manuscript for submission.

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Declaration of Conflicting Interests

The authors declared that they had no conflicts of interest with respect to their authorship or the publication of this article.

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Additional supporting information can be found at <http://pss.sagepub.com/content/by/supplemental-data>

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Notes

1. One parent indicated that her child with ASD would not be comfortable playing a VVG and would obsess about the violence for several weeks if asked to play it. Therefore, this participant was not randomly assigned to game condition but was intentionally assigned to play the NVVG during the study.
2. One adult with ASD elected to terminate the experiment immediately following the CRTT.

3. Tuning the prior of the expected effects to be medium or large in size would increase the apparent evidence for the null-effects models.

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