Abstract

Previous research suggests that violent video games are a cause of aggressive behavior. We provide a strong test of this hypothesis by using violent and nonviolent games that are closely matched, collecting a large sample, and using a single preregistered outcome. 275 male undergraduates were randomly assigned to play a first-person shooter game, modified to be either violent or non-violent, difficult or easy. Following the gameplay session, participants were provoked by a confederate and given an opportunity to aggress. Neither game violence nor game difficulty predicted aggressive behavior. Incidentally, we found that 2D:4D ratio, thought to index prenatal testosterone exposure, did not predict aggressive behavior. Results cast doubt on violent video games and low 2D:4D as causes of aggressive behavior.

Violence is common in video games, and many are concerned about the effects of such games on their players. Psychological research reports that violent games may increase aggressive behavior, and academic societies have made public statements on the harmful effects of violent media (American Psychological Association Task Force on Violent Media, 2005). However, this research has been criticized on the grounds that violent and nonviolent games used in experiments may feature other confounding differences (Adachi & Willoughby, 2011), that outcomes are flexibly analysed post-hoc to find a significant result (Elson, Mohseni, Breuer, Scharkow, & Quandt, 2014), and that the published literature favors statistically significant results (Hilgard, Engelhardt, & Rouder, 2017).

In this study, we address these challenges. To address potential confounds, we controlled our stimuli by customizing video games. In one version of our game, participants had to kill invading aliens; in the other, participants had to save aliens that happened to be lost. Saving an alien required the participant to transport it back by aiming a remote controller at it, reproducing first-person-shooter gameplay but without the violence. The gameplay in both games was exactly the same; they differed only in graphics, sounds, and cover story. To address the critique that previous studies were underpowered and may have involved post-hoc selection of outcome variables, we collected 446 participants, preregistering our sample size, manipulation, hypotheses, and outcome. As a secondary goal, we test whether the ratio of the lengths of the index and ring fingers (2D:4D ratio), believed to measure prenatal testosterone, predicts aggressive behavior as theorized.

**Violent Video Games**

Evidence for causal effects of violent video games come from laboratory experiments. In such experiments, researchers randomly assign some participants to play a commercially-available violent video game, say *Doom*, and others to play a commercially-available nonviolent video game, say *Myst* (Anderson & Dill, 2000). Following game play, there is some measure of aggressive thoughts, feelings, or behavior. Dozens of such studies have been performed, and meta-analyses of these experiments reveal greater levels of aggression following violent-video game play than following nonviolent-video game play (Anderson et al., 2010; Greitemeyer & Mügge, 2014).

This evidence is controversial for two reasons: First, it is often unclear whether the effects are caused by video games’ violent content in specific. An alternative explanation is that these effects may reflect confounded characteristics of violent video games such as competition or frustration rather than violent content per se (Adachi & Willoughby, 2011; Przybylski, Deci, Rigby, & Ryan, 2014). Second, evidence for violent-video-game effects may be overstated through publication bias (Ferguson & Kilburn, 2010; Hilgard, Engelhardt, & Rouder, 2017).

Violent video games are hypothesized to cause increases in aggression through a number of causal pathways. These include the activation of aggressive thoughts, the operant and observational learning of aggressive scripts, increased processing of ambiguous cues as hostile, desensitization to suffering through repeated exposure to violence, increased arousal, and activation of hostile affect (Bushman & Anderson, 2002). Effect sizes have been reported as being consistent with typical effect sizes in social psychology (*r* = .21, Anderson et al., 2010; *r* = .19, Greitemeyer & Mügge, 2014) and practically meaningful based on their putative implications for public health.

**Difficult Video Games**

Researchers have attempted to test the specific effects of violent game content without confounding by other game features. Some have suggested that, despite these efforts, differences in violent content between games remain confounded by differences in competitiveness or frustration. One small-sample study suggests that differences in aggression may be attributable to competitive, rather than violent, content (Adachi & Willoughby, 2011; but see Anderson & Carnagey, 2009). Another series of studies reports that frustration with controls, but not game violence, may cause aggressive behavior (Przybylski et al., 2014). These confounds, rather than the violent content, may cause increases in aggression.

**Manipulating Game Content Without Confounds**

Most research manipulates violent content by assigning participants to play a violent or nonviolent game. However, violent and nonviolent games are often very different, usually belonging to very different genres with very different rules of play. For example, violent games are often shooter or fighting games, while nonviolent games are often racing, puzzle, or sports games. Therefore, while tested games do differ in their *violent content,* they are also different in their gameplay, creating a possible confound*.*

Researchers have attempted several ways to account for these potential differences. One approach is to collect a small pilot sample and show that there is no significant confound between games; this approach is flawed in that small pilots do not provide strong evidence of equivalence (Hilgard, Engelhardt, Bartholow, & Rouder, 2017). Another approach is to adjust for potential confounds as covariates. This approach may underadjust if the confounds are measured with error or overadjust if the “confounds” are themselves consequences of violent game play.

To balance our stimuli, we take a more direct approach by modifying the content of a single video game. Rather than comparing two separate games, game modification allows the researcher to exercise control over the game contents. For example, a game can be modified so that the same level is played either with violent or nonviolent contents, but all other game parameters are held constant (as suggested by Elson & Quandt, 2016 and demonstrated in Carnagey & Anderson, 2005; Elson et al., 2015; Przybylski et al., 2014). This approach allows manipulation of specific game features in much the same way that a researcher would manipulate features of a laboratory paradigm between conditions, permitting clearer inferences concerning the effects of the manipulated game feature.

**2D:4D Ratio**

While running the experiment, we took the opportunity to test a related hypothesis about hormones and aggression. The male sex hormone testosterone is theorized to be one cause of aggression (see Carré, McCormick, & Hariri, 2011 for a review), and it is hypothesized that development of aggressive tendencies may be caused, in part, by prenatal testosterone exposure (see, e.g., Cohen-Bendahan, Buitelaar, van Goozen, Orlebeke, & Cohen-Kettenis, 2005). One supposed index of this prenatal exposure is the ratio of the lengths of the index and ring fingers (2D:4D ratio); this ratio is thought to be related with both prenatal testosterone exposure and aggressive behavior (see Lutchmaya, Baron-Cohen, Raggatt, Knickmeyer, & Manning, 2004; Manning, Scutt, Wilson, & Lewis-Jones, 1998). There is cause for skepticism, however; meta-analysis indicates that gene polymorphisms that reduce androgen sensitivity do not predict higher 2D:4D as theorized (Voracek, 2014).

Because the overall correlation between 2D:4D ratio and aggression is small (*r* = -.06 among males and no effect among females, Hönekopp & Watson, 2011), proponents of the 2D:4D ratio hypothesis of aggression have suggested the effects of 2D:4D may be moderated by context, only predicting aggressive behavior in aggressive situations (Millet, 2011). For example, previous experiments report prediction of aggression by 2D:4D following an aggressive (but not a non-aggressive) music video (Millet & Dewitte, 2007) or priming by aggressive (but not neutral) words (Millet & Dewitte, 2009). Our experiment features a provocation, aggressive primes, and an opportunity to aggress, providing an ideal test of the correlation between 2D:4D ratio and aggression.

**Superadditive Causes of Aggressive Behavior**

Several models of aggressive behavior suggest that multiple coincident causes of aggression should have superadditive interactions (e.g., I3 Theory, Slotter & Finkel, 2011; the General Aggression Model, Anderson & Dill, 2000). For example, one might expect a violent prime would be most influential on those already temperamentally disposed towards aggression. In this study, we test for interactions between violent content, difficult content, and 2D:4D ratio in predicting aggressive behavior.

**Purpose**

The proposed study examines the effects of game violence, game difficulty, and 2D:4D ratio on aggressive behavior among college-aged males. These can be summarized as four hypotheses. H1: Violent video game content will increase aggressive behavior. H2: Video game difficulty will increase aggressive behavior. H3: More masculine 2D:4D ratios will be associated with more aggressive behavior. H4: These effects will yield superadditive interactions, such that the presence of multiple of these factors will produce more still more aggression.

**Method**

**Participants**

Participants were 446 male undergraduate students at a state university in the American Midwest. Our sample size was planned around a power analysis of the violent-game effect reported in previous meta-analysis, δ = 0.43 (see Anderson et al., 2010). To test this effect with 99% two-tailed power would require 400 subjects. We anticipated a loss of about 50 subjects to failures of methods or deception and set our sample size at 450. Power to detect higher-order interactions is harder to estimate, as it is unclear what effect size to expect. Our planned sample size of 400 would have been sufficient to detect effects as small as |ρ| = .12, two-tailed, with 80% power.

Participation was restricted to males because 2D:4D effects are thought to apply only to males (McIntyre et al., 2007, but see Millet & Dewitte, 2007). This removes gender as a potential source of variance. Participants were primarily Caucasian (79.7%), with some African-American (8.6%), Asian (4.6%), and Latino (3.3%), and 3.8% identified as another race. On average, participants were 19.0 (SD: 1.7, range 18-31) years old.

The semester ended before the last four experimental sessions could be conducted. Many subjects had to be excluded: 41 subjects were excluded because the RA marked the session as having some error, 3 were excluded for gameplay data that indicated an error of game assignment (e.g., dying in the easy game), 13 were excluded for missing data on the primary DV, and 114 were excluded for indicating awareness of the hypothesis. After exclusions, the final effective sample size was 275.

**Disclosures**

We report how we determined our sample size, all data exclusions, all manipulations, and all measures in the study. Hypotheses and sample size were preregistered at https://osf.io/cwenz/. All measures, materials, data, and analytic code are also available at that URL.

**Measures**

**2D:4D ratio.** Participants placed their hands on a flatbed scanner, fingers held together and fully extended. The distance from tip to basal crease of each index and ring finger was measured using the caliper tool in the GNU Image Manipulation Program (The GIMP Team, n.d.), a freeware Photoshop-like tool. 2D:4D ratios were created for each hand by taking the ratio of lengths of the index and ring fingers. Five coders provided measurements in this fashion, with each scan coded by at least two coders. Inter-rater reliability was assessed using a one-way, mixed, consistency, average-measures intra-class correlation (Hallgren, 2012; McGraw & Wong, 1996) using the psych package for R (Revelle, 2017). The resulting ICCs were excellent (ICC3k = .94 for left 2D:4D, .88 for right 2D:4D), indicating high agreement across coders and minimal loss of power due to measurement error.

**Cold pressor task.** Participants had an opportunity to aggress against their partner by assigning the partner to immerse his fist in a bucket of painfully-cold water for an amount of time (Pedersen, Bushman, Vasquez, & Miller, 2008). The cover story described the cold pressor as a form of distraction used in a subsequent task measuring decision-making under distraction. Before making the assignment, the participant first sampled the cold water himself for five seconds to learn that cold-water immersion is unpleasant. The participant then assigned the partner to a duration of cold-water immersion on a 9 point scale, ranging from 0 to 80 seconds in 10-second intervals. This measure has the benefit of being quantified only in one way (e.g. 1-9 rating), eliminating the concerns about flexible quantification methods associated with the competitive reaction time measure of aggression (Elson et al., 2014).

**Manipulation checks.** Participants completed a questionnaire assessing the efficacy of the various parts of the experimental manipulation. First, participants rated their partner’s feedback as pleasant or irritating (6 items). Then, participants rated the video game they played on a number of dimensions, including how violent, exciting, and challenging it was (18 items). All items were rated on a 1 (strongly disagree) to 7 (strongly agree) Likert scale. Participants then rated their degree of experience with video games, first-person shooter video games, and playing video games with a keyboard and mouse. Finally, participants provided demographic information about themselves.

**Probe for suspicion.** Research assistants attempted an oral funneled debriefing. Following this oral debriefing, participants completed a questionnaire intended to imitate a funneled debriefing. This debriefing questionnaire started with broad questions about the study and then grew increasingly specific, asking whether anything seemed strange about the study, the aggression measure, or the other participant in the study.

**Materials**

**Modified video games.** Four modified versions of the video game *Doom II* (iD Software, 1994) were created using software modification tools. These four versions were designed to create a 2 (Difficulty: Easy, Difficult) x 2 (Violence: Nonviolent, Violent) design.

Violent content was manipulated by changing the graphics, sounds, and story of the game while leaving the controls and enemy behavior constant. In the violent version, enemy graphics and sounds were borrowed from *Brutal Doom* (Abenante, 2012), a modified form of *Doom II* that makes the game more violent. In this game, defeated enemies exploded into fountains of gore and severed limbs. Participants in this condition were told that they must kill all the aliens. In the nonviolent version, enemy graphics and sounds were borrowed from *Chex Quest* (Digital Café, 1996), a modified version of *Doom II* that replaces the enemies with silly-looking booger aliens. Participants in this condition were told that the aliens are lost and confused and need to be sent home with the “zorcher”, a tool resembling a remote controller.

The difficulty of the games was manipulated by changing the enemies’ artificial intelligence. In the difficult version of the game, the enemies fought back, using weapons in the violent game and throwing boogers in the nonviolent game. In the easy version of the game, enemies would not attack the player, instead walking slowly towards the player and waiting to be killed or zorched.

The games were also programmed to track some statistics about the player’s performance. These variables included player deaths, player kills, wounds received, bullets fired, shotgun shells fired, and distance progressed. These were chiefly collected for quality control purposes to make sure that players assigned to the easy condition did not receive wounds and that all players fired their weapons and accumulated some kills.

**Procedure**

Participants arrived at the lab in pairs and were immediately escorted to separate adjacent rooms. In the case that only one participant was present, a male research assistant or graduate student would pretend to be the other participant. Following consent, their hands were scanned. Participants were able to see each other as scans were taken, demonstrating the presence of another participant in the study. After scanning, participants returned to their desks.

Participants were provoked by their partner in a procedure adapted from Bushman and Baumeister (1998). Participants were given an envelope, a sheet of loose-leaf paper, and a printed essay prompt. They were informed that the first task was to write a five-minute persuasive essay of their personal views on abortion which would later be judged by the other participant. At the end of these five minutes, the essays were collected so that they purportedly could be exchanged with the other participant. All participants wrote an essay that established an unambiguous view on abortion.

During the exchange, each participant received a fake, premade essay designed to oppose their beliefs (pro-life for pro-choice and vice-versa). Participants rated the essay, then put essay and evaluation in the partner’s envelope, which was then taken from the room, ostensibly for data entry.

Participants then played their assigned version of the video game for 15 minutes. Each received a cover story that explained the story and controls of the game.

The cold pressor consisted of a pitcher of water kept in the laboratory refrigerator. Five minutes before the end of the gameplay session, the research assistant added a dozen ice cubes to the pitcher. Unfortunately, we did not take measurements of the pitcher’s temperature.

When the game session ended, the research assistant brought the cold pressor pitcher and a towel into the room, recorded the gameplay variables, and quit the game. The assistant then prepared to open an E-Prime script ostensibly containing the next task. Participants were told that the next portion of the experiment involved performing a computer task while distracted by cold water.

The research assistant then provoked the participant by bringing the participant’s original envelope into the room and showing him the partner’s rating of his essay. The partner had rated all dimensions as between -8 and -10 in quality and commented “This is the stupidest thing I’ve ever read.”[[1]](#footnote-2) The participant was then asked to assign their partner to an amount of distraction (that is, cold pressor exposure). The researcher explained that, to avoid experimenter bias, participants were being asked to randomly assign each other to the various levels of distraction.

Finally, participants were told that the experiment was running out of time and that the distraction task would be skipped. Participants completed post-questionnaires asking them to rate the games, their partner’s feedback, and what they suspected was the purpose of the study. Participants were then fully debriefed and dismissed.

**Results**

**Manipulation Checks**

Participant responses on the post-questionnaire were submitted to exploratory factor analysis. Parallel analysis suggested four factors. EFA with the psych package (Revelle, CITE) extracted four factors. These factors represented 1) excitement and enjoyment, 2) challenge, stress, mental effort, 3) difficulty with the controls, and 4) violence and aggression, but also how satisfying the guns were… this is messy.

**Game manipulation.** Participant ratings on the post-questionnaires were submitted to 2 (Violence) × 2 (Difficulty) ANOVA. The manipulation was highly effective: participants indicated that the violent game (*M* = 5.3, *SD* = 1.6) was much more violent than the nonviolent game (*M* = 2.2, *SD* = 1.3; *d* = 2.1, [1.8, 2.4]). Exploratory factor analysis suggested a latent factor of how challenging, stressful, difficult, exhausting, and requiring of mental effort the game was. Participants also rated the difficult game (*M* = 0.33, *SD* = 0.94) higher on this factor than the easy game (*M* = -0.34*, SD* = 0.73, *d* = 0.74 [0.50, 0.99]).

**Provocation.** Mean evaluations of the participants’ interactions with the partner were also assessed. Participants generally indicated that they were irritated (*M* = 5, *SD* = 1.7), angered (*M* = 4.2, *SD* = 1.8), and annoyed (*M* = 4.9, *SD* = 1.8) by their partner. Furthermore, they were neither happy (*M* = 2.4, *SD* = 1.4) nor pleased (*M* = 2.2, *SD* = 1.4) with their partner and found the feedback unhelpful (*M* = 1.7, *SD* = 1.3).

To determine whether the cold pressor dependent variable was a sensitive measure of aggression, we tested whether these participants more provoked by the feedback gave higher cold pressor assignments. Parallel analysis suggested a two-factor solution for participants' ratings of their interaction with their partner. Factors were extracted using oblimin rotation. The first factor accounted for 52% of the variance and had the expected pattern of loadings: .77, .76, and .67 for irritation, anger, and annoyance, -.25, .02, and .02 for happiness, helpfulness, and pleasure. This provocation factor was then used as a linear predictor of cold pressor assignment. The relationship was moderately strong, *t*(249) = 5.73, *p* < .001, *r* = .33 [.22, .43], suggesting that the cold pressor measure was sensitive to participants’ intent to aggress. A scatterplot and loess regression line are provided in Figure 1.

Figure 1. Scatterplot of cold pressor sensitivity to composite irritation.



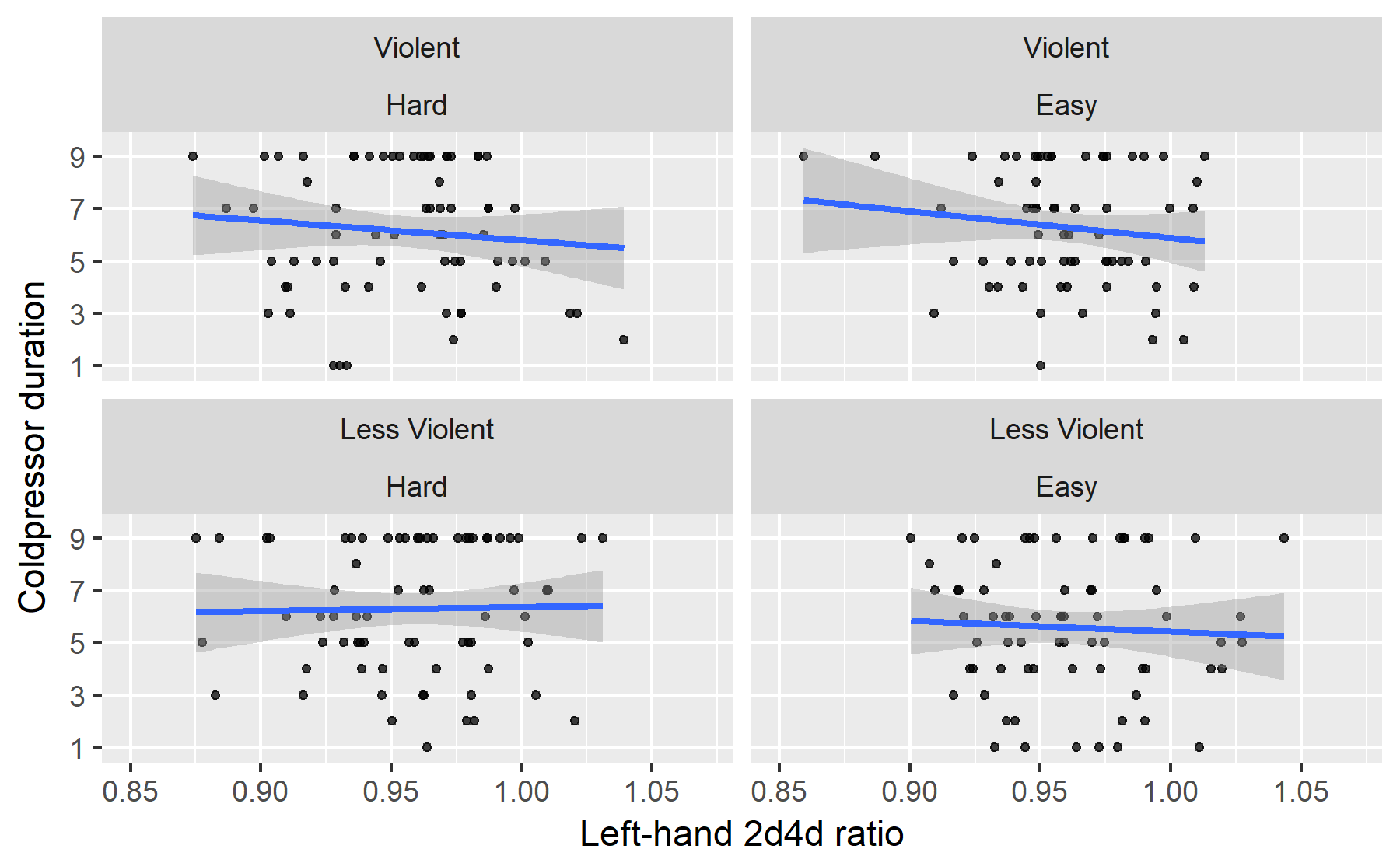
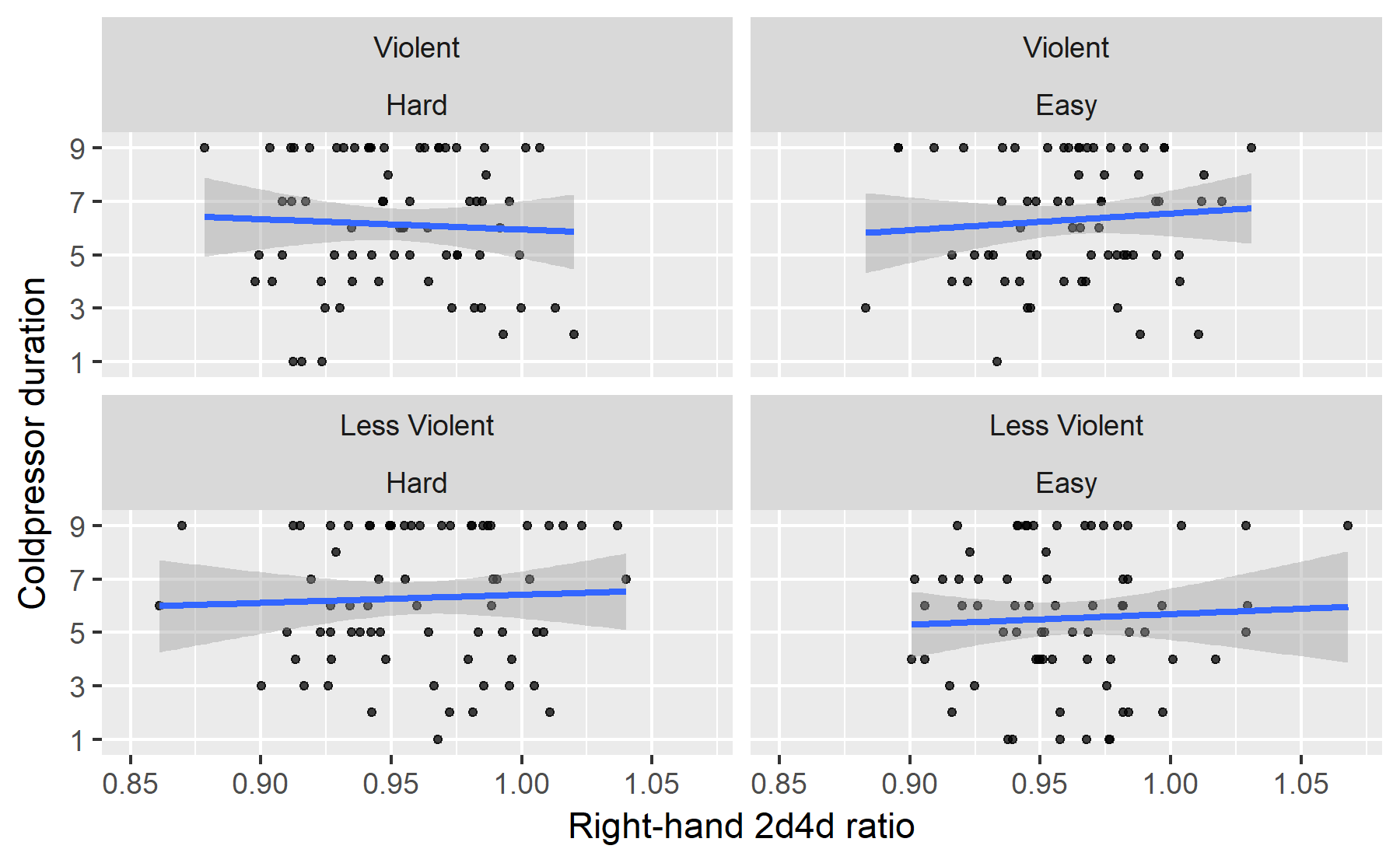
Scatterplot of participants’ experienced provocation and cold pressor assignment. A locally-weighted regression curve (LOESS) with shaded standard error region is overlaid.

A 2 (Violence) × 2 (Difficulty) ANOVA was conducted to determine whether the game played influenced participants’ ratings of the interaction. Effects were small and not statistically significant (violence, *t*(247) = -0.28, *p* = .777, *d* = -0.03 [-0.27, 0.2]; difficulty, *t*(247) = -0.17, *p* = .867, *d* = -0.02 [-0.26, 0.22]; Violence × Difficulty, *t*(247) = -0.86, *p* = .392, *d* = -0.1 [-0.34, 0.13]), suggesting that the game played had a minimal influence on participants’ reaction to the feedback.

**Conventional General Linear Models**

General linear models were used to look for main effects and interactions of game difficulty, game violence, and 2D:4D ratio. These tests were preregistered. Two models were used to look for effects of left and right 2D:4D ratio separately. Factors were contrast-coded and 2D:4D ratios were standardized to preserve orthogonality of parameter estimates. Cell means and SDs are provided in Table 1.

Neither model found any significant effects. Neither game violence (*t*(265) = 0.9, *p* = .371, *d* = 0.11 [-0.13, 0.35]), game difficulty (*t*(265) = 0.85, *p* = .395, *d* = 0.1 [-0.13, 0.34]), nor their interaction (*t*(265) = -1.52, *p* = .129, *d* = -0.18 [-0.42, 0.05]) significantly predicted aggression. Additionally, neither left-hand 2D:4D (*t*(265) = -1.11, *p* = .266, *r* = -.07 [-.18, .05]) nor right-hand 2D:4D (*t*(266) = 0.52, *p* = .602, *r* = .03 [-.09, .15]) had a significant main effect on aggressive behavior. See Figure 2. No higher-order interactions involving 2D:4D ratio of either hand were statistically significant. Full model output is summarized in Tables 2 and 3.

Figure 2. Null relationship between 2D:4D and aggression

Scatterplots illustrating the relationship between 2D:4D and aggression in each condition. Relationships are consistently near zero.

The earlier manipulation and sensitivity check indicated that much of the variance in aggression could be predicted by experienced provocation. Because this provocation was generally independent of the experimental condition, its inclusion as a covariate in analysis might increase statistical power. However, adding provocation as a covariate did not reveal significant effects. The effect of violence was *t*(246) = 0.78, *p* = .434, *d* = 0.09 [-0.14, 0.33], the effect of difficulty was *t*(246) = 1.08, *p* = .283, *d* = 0.13 [-0.11, 0.37], and their interaction was *t*(246) = -1, *p* = .318, *d* = -0.12 [-0.36, 0.12]. Effects of left-hand and right-hand 2D:4D remained nonsignificant (*t*(246) = -1.86, *p* = .065, *r* = -.12 [-.24, .01] and *t*(248) = -0.31, *p* = .755, *r* = -.02 [-.14, .11], respectively).

**Bayesian ANOVA**

Models were compared using the BayesFactor package for R (Morey & Rouder, 2015). The scale of the effect size under the alternative hypothesis was specified as H1: *δ* ~ Cauchy(.4), consistent with the effect size reported in meta-analysis (Anderson et al., 2010). Models were generated to represent all possible combinations of main effects and/or interactions. Models including interactions were required to include all lower-order interactions and main effects. All models were compared to a null-hypothesis model including no effects, H0: *δ* = 0. This is the same prior that describes the nil-null hypothesis used in frequentist testing.

Of all the models, the null-hypothesis model was best supported by the data. Models of main effects of Violence, Difficulty, left-hand 2D:4D, or right-hand 2D:4D were each outperformed by the null model (Bayes factors = 3.61, 3.81, 4.4, and 6.53 in favor of the null, respectively). Higher-order interactions were not supported by the data, either. Evidence was ambiguous regarding a Violence × Difficulty interaction (BF01 = 1.25 favoring the null). Neither violence nor difficulty interacted with 2D:4D of the left hand (BF01 = 3.6, 4.13, respectively) or 2D:4D of the right hand (BF01 = 4.84, 4.32). The Violence × Difficulty × 2D:4D interaction was not supported (left-hand BF01 = 3.38, right-hand BF01 = 3.04).

Experienced provocation was added to the model as a predictor. An effect of provocation was strongly supported by the evidence (BF = 1.04×106). However, addition of this covariate did not improve the strength of evidence for main effects of violence (BF01 = 4.92), difficulty (BF01 = 3.63), or 2D:4D (BF01 = 1.26, left hand; BF01 = 6.13, right hand). Taken together, these results indicate that aggression could be predicted by experienced provocation but not by game condition.

**Non-local Bayesian prior.** In the Bayesian hypothesis tests provided above, we use a non-directional, non-specific alternative hypothesis scaled roughly to the magnitude of the expected effect. While this is a useful hypothesis to test, it would also be useful to compare the obtained results against a more specific alternative hypothesis representing the effect as estimated from previous meta-analysis, δ = .43 (.35, .52) (Anderson et al., 2010).

The main effect of violence was *d* = 0.11 [-0.13, 0.35]. An online Bayes factor calculator (Dienes, 2008) was used to compare the evidence for H0: *δ* = 0 relative to H1: *δ* = .43 [.35, .52]. The obtained Bayes factor substantially preferred the null, BF01 = 14.2.

Proponents have suggested that the Anderson et al. (2010) estimate may be an overestimate due to publication bias, but that after adjustment for publication bias the effect is still approximately *d* = .30 (Kepes, Bushman, & Anderson, 2017). The Bayes factor calculator was used to compare the evidence for H0: *δ* = 0 relative to H2: *δ* = .30 [.20, .40]. The obtained Bayes factor still preferred the null, but less so relative to this more modest estimate, BF02 = 2.0.

**Supplementary methods**

Cold pressor assignments were non-normally distributed. Alternative statistical approaches were attempted to handle this. These methods did not yield substantively different conclusions. See the supplement for details.

**Exploratory analyses**

A number of exploratory analyses were conducted. These examined whether aggression was predicted by participants' experience of difficulty during the game, participants' self-reported history of video games, and participants' in-game behaviors. Participants who enjoyed the game more were more aggressive. However, neither experienced challenge, nor discomfort with the game controls, nor history of first-person shooter play were correlated with aggression, in contrast to previous research (Adachi & Willoughby, 2011; Anderson et al., 2010; Przybylski et al., 2014). See the supplement for details.

**Discussion**

Results indicate that when game stimuli are carefully controlled, the effects of fifteen minutes of violent and/or difficult gameplay on aggressive behavior may be small and indistinguishable from zero. This suggests that the effects of brief violent video game play on aggressive outcomes may be smaller and less robust than the published research literature would indicate (see also Engelhardt, Mazurek, Hilgard, Rouder, & Bartholow, 2015; Hilgard, Engelhardt, & Rouder, 2017; McCarthy, Coley, Wagner, Zengel, & Basham, 2016). Researchers may need to reevaluate whether violent game manipulations are useful for revealing the causes and mechanisms of aggression. Further research will also be necessary to determine whether, and under which conditions, competitive or frustrating gameplay causes aggression (Adachi & Willoughby, 2011; Przybylski et al., 2014).

2D:4D digit ratio also failed to predict aggressive behavior among participants. The current results cast doubt on 2D:4D as an index of prenatal testosterone and a predictor of aggressive behavior (see also Hönekopp & Watson, 2011; Voracek, 2014). The sample size of the current research is considerably larger than many other studies reporting significant associations between 2D:4D ratio and aggression (e.g., Millet & Dewitte, 2007, 2009).

The presented manipulation and sensitivity checks suggest that the null results are not due to failures of the methodology. First, participants indicated that the violent game was much more violent than the nonviolent game. Second, participants were generally irritated with their essay feedback. These indicate that both the game manipulation and the essay provocation were effective. Third, the cold pressor measure of aggression was moderately sensitive (*r* = .33) to participants’ irritation with their partners. This sensitivity suggests that the null result is not due simply to the unusual distribution of the data or an overall invalidity of the cold pressor measure (see also Pedersen et al., 2008).

**Limitations**

Two factors may have reduced the effect size relative to previous research. First, it is possible that the nonviolent *Chex Quest* game involves sufficient violence to cause an increase in aggression, eliminating the difference between conditions. One study has claimed that the effect of cartoon E-rated violence is as strong as that of explicit M-rated violence (Anderson, Gentile, & Buckley, 2007). This seems unusual; compared to mild violent content, exposure to more extreme violent content should be more desensitizing, activate more aggressive thoughts, and stimulate more aggressive feelings. Still, it is possible that an effect was not found in the present study because *Chex Quest* causes some increase in aggression, reducing the effect size when compared against *Brutal Doom*. Future research may test the dose-response curve of violent content and aggressive behavior.

Second, because a plurality of participants (29%) gave the maximum possible aggressive response, it is possible that our measure is not sensitive to the influence of violent games. In the context of provocation, there may be a ceiling effect that compresses scores and reduces the sensitivity of the measure. On the other hand, the measure was sensitive to participants’ irritation with their partner, suggesting that it remained a valid and sensitive measure of aggression. Since all participants were provoked, provocation is not a source of noise variance; at most, effect sizes might be reduced by restriction of range. Numerous other studies have found violent-game effects on aggression in the context of provocation (Carnagey & Anderson, 2005 use the same essay provocation and report significant effects on a noise blast task). We encourage researchers to report the properties and test the validity of measures of aggressive behavior.

Finally, many participants indicated awareness of the research hypothesis and were discarded. This may have been due, in part, to the redundant process of oral funneled debriefing and questionnaire funneled debriefing, which may have increased awareness of the hypothesis following collection of the primary outcome. We chose to be conservative in our quality checks so as not to overstate the evidence for the null hypothesis. Nevertheless, one might be concerned that still more participants were hypothesis-aware, reducing the observed effect size through reduction of internal validity or through reactance (Bender, Rothmund, & Gollwitzer, 2013). Researchers may find value in establishing standardized practices in deception and debriefing.

**Summary**

We found evidence that brief exposure to violent games does not cause aggressive behavior. It is uncertain whether laboratory paradigms involving brief exposure to violent video games can reveal the causes of aggression. 2D:4D similarly does little to predict aggression in a laboratory experiment. One might question the validity of 2D:4D as an index of prenatal testosterone or whether prenatal testosterone predicts aggression. Research may benefit from exploring other causes of aggression and addressing sources of irreplicabilty.

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Table 1.

|  |  |  |  |  |
| --- | --- | --- | --- | --- |
| Violence | Difficulty | n | M | SD |
| Brutal Doom | Hard | 69 | 6.13 | 2.46 |
| Brutal Doom | Easy | 67 | 6.31 | 2.24 |
| Chex Quest | Hard | 69 | 6.29 | 2.47 |
| Chex Quest | Easy | 70 | 5.53 | 2.54 |

Cell means of cold pressor assignments per condition.

Table 2

|  |  |  |  |
| --- | --- | --- | --- |
|  | estimate | *t* | *p* |
| Violence | 0.13 | 0.90 | 0.371 |
| Difficulty | 0.13 | 0.85 | 0.395 |
| Left 2D:4D | -0.17 | -1.11 | 0.266 |
| Vio × Diff | -0.22 | -1.52 | 0.129 |
| Vio × Left 2D:4D | -0.12 | -0.83 | 0.406 |
| Diff × Left 2D:4D | 0.07 | 0.46 | 0.646 |
| Vio × Diff × Left 2D:4D | -0.02 | -0.17 | 0.869 |

ANOVA model testing effects of difficulty, violence, and left-hand 2D:4D on cold pressor assignments to partner. All model terms have standard error 0.15.

Table 3

|  |  |  |  |
| --- | --- | --- | --- |
|  | estimate | *t* | *p* |
| Violence | 0.15 | 0.97 | .332 |
| Difficulty | 0.14 | 0.97 | .333 |
| Right 2D:4D | 0.08 | 0.52 | .602 |
| Vio × Diff | -0.24 | -1.59 | .113 |
| Vio × Right 2D:4D | -0.04 | -0.26 | .793 |
| Diff × Right 2D:4D | -0.09 | -0.62 | .537 |
| Vio × Diff × Right 2D:4D | -0.08 | -0.51 | .608 |

ANOVA model testing effects of difficulty, violence, and right-hand 2D:4D on cold pressor assignments to partner. All model terms have standard error 0.15.

1. Originally, the comment read, “This is one of the worst essays I have ever read!” consistent with previous research. Participants generally found this to be suspicious and unbelievable, so we changed it to a more flippant and more credible insult. [↑](#footnote-ref-2)