**Hero Violence**

The potential effects of violent video game exposure on gamers have received considerable attention in recent years. The published scientific literature generally supports the conclusion that violent video games increase aggressive behavior (Anderson et al., 2015; Bushman, Gollwitzer, & Cruz, 2015; but see McCarthy, Coley, Wagner, Zengel, and Basham, 2016, for a null result in a preregistered setting and Hilgard, Engelhardt, and Rouder, 2017, for indications that these effects may be substantially overestimated by publication bias). Violent video game effects have been found to increase aggression immediately after playing in a number of different ways using different measures of aggression (Anderson & Dill, 2000; Bartholow & Anderson, 2002; Bartholow, Bushman, & Sestir, 2006; Engelhardt, Bartholow, Kerr, & Bushman, 2011; Fischer, Kastenmuller, & Greitemeyer, 2010). These effects are not limited to the time period immediately following violent video game exposure, but can remain even 24 hours later (Bushman & Gibson, 2011), as evidenced by men who were more aggressive after returning the day after playing a violent video game with the instruction to ruminate about it. Most studies show that violent game exposures as brief as 20 minutes are sufficient to increase players’ aggression. When examining long-term exposure, evidence indicates that sustained violent video game exposure over time is related to increased aggressive behavior, with greater exposure relating to greater levels of aggression (Willoughby, Adachi, & Good, 2012). In addition, playing violent video games has been linked to desensitization toward arousing stimuli (Bailey & West, 2013; Bailey, West, & Anderson, 2011; Bartholow, Bushman, & Sestir, 2006; Carnagey, Anderson, & Bushman, 2007; Engelhardt, Bartholow, Kerr, & Bushman, 2011), perception of one’s own aggressive behavior as less aggressive (Greitemeyer, 2014), and increased interpretation of aggression in others (Bushman & Anderson, 2002).

Comprehensive meta-analytic reviews including various methodologies have supported the theory that violent video game playing causes negative effects in a number of domains related to aggression (Anderson et al., 2010; Greitemeyer & Mugge, 2014). Overall, it seems as though elevated aggressive behavior following violent video game play is likely the result of an increase in the activation of aggression related content, according to the current General Aggression Model (Anderson & Bushman, 2002b). Of particular importance to the current study is the negative effect violent video games and activated mental content appear to have on prosocial behavior.

Beyond effects on aggression, research has reported that playing violent video games also increases the latency with which one helps others (Bushman & Anderson, 2009), notably by reducing empathic concern for others (Fraser et al., 2012; You, Kim, & No, 2015). On the other hand, research with prosocial video games, in which the player does nice things (or life-saving things) to or for other game characters, indicates that playing these types of games actually increases prosocial behavior (Gentile et al., 2009; more), likely by increasing access to prosocial thoughts (Greitemeyer & Osswald, 2010; Greitemeyer & Osswald, 2011; but see Tear and Nielsen, 2013 for a failure to replicate). Thus, results indicate that the activation of prosocial or aggressive constructs through exposure to congruent content in video games influences future behavior*.* So, depending upon the type of behavior engaged in while playing the game, gamers will either have more prosocial constructs activated following gameplay, making them more inclined toward compassion and helping, or they will have more aggressive constructs activated following gameplay, making them more inclined toward anger and aggression.

Interestingly, there are certain games in which both constructs are intertwined within the game’s objectives. Some video games require violence in order to help save innocent people, or even the world, against those attempting to do harm. Many games require the player to save innocents from monsters, terrorists, or even Nazis. Thus, players must be violent, which may reduce prosocial behavior, but within the plot of the game the violence is in the service of helping others, which may increase prosocial behavior. Interestingly, research has shown that playing a violent video game cooperatively with other players increases prosocial behavior (Ewoldsen, Eno, Okdie, & Velez, 2012), so there is evidence to suggest that prosocial contexts within a violent video game can produce prosocial effects. However, the objective of cooperative violent game play is usually simply to win, not to act prosocially per se. To this point, no research has investigated the effects that violent games with prosocial objectives may have on prosocial behavior. If it were merely the case that violent video game content reduced prosocial behavior, and prosocial video game content increased it, then perhaps the two effects would generally cancel each other out, resulting in neither an increase or decrease in prosocial behavior. However, it is possible that prosocial violence in video games will activate a prosocial concept that would produce increased prosocial behavior when given the opportunity and context to do so.

We hypothesize that the prosocial nature of the violence in a video game scenario in which players must kill in order to save innocent hostages will increase the tendency to be prosocial when given the opportunity. This could be especially true when the nature of the helping opportunity is phrased in a way that matches the nature of the prosocial behavior in the violent video game, namely in order to save lives. Players who play this type of game will be significantly more prosocial after the game has ended than players of a video game in which violence is gratuitous, and also significantly more prosocial than players of a control game.

**Method**

We report how we determined our sample size, all data exclusions (if any), all manipulations, and all measures in the study (Simmons, Nelson, Simonsohn, 2012)

**Participants**

Participants were (189) college students (xx% male) from two large Midwestern universities. The study was advertised as an investigation as to how video games relate to both enjoyment and emotion, with the statement that participants would simply play a video game and then answer some questions about how much they enjoyed playing it and what kinds of emotions they experienced during their gameplay. All participants were traditional college aged students. A sample size of 40 participants per condition was judged sufficient to achieve adequate statistical power.

**Materials and Procedure**

Participants were randomized (by sampling to a quota per cell) to a 3 (Game: Gratuitous violence, Hero violence, No violence) × 2 (Help Red Cross, Save lives) between-subjects design.

When participants first entered the lab, one of three different video games was awaiting them. In the “Gratuitous Violence” condition, participants played a level in *Call of Duty: Modern Warfare* entitled “No Russian”, in which the participant controlled a character who is a US special ops member that is undercover as part of a terrorist organization that walks into an airport and has to kill innocent civilians and police officers. However, manipulation of the intro video to the game’s level made it appear that they were merely a part of the terrorist organization. Participants in the “Hero Violence” condition played a different level in Call of Duty: Modern Warfare entitled “The Only Easy Day Was Yesterday”. In this level, participants were first told in the intro video that some innocent crew members of an oil rig were being held captive by a terrorist organization, and it was the job of the participant to sneak onto the oil rig and save the hostages. Success in this level required killing many of the terrorist members while freeing the hostages. Before playing either of these Call of Duty levels, participants in both conditions warmed up to the controls during a 5-minute training level at the beginning of the Call of Duty campaign that systematically shows participants how the controller works and allows them time to practice with the controller. In the control condition, participants played *Gran Turismo*, in which they were required to race other computer controlled cars. Before they proceeded to race, participants received instruction on the use of the controller, and were allowed to practice for five minutes on a time trial track with no other cars. In all three of these conditions, participants played the actual level for 15 minutes.

After gameplay ended, participants completed questionnaires on a computer for approximately 10 min. They first completed the PANAS (citation), followed by several questions regarding their gameplay experience. These questions included experience of enjoyment playing the game, how often the participant played video games, and if the genre of game they played was the genre they normally would enjoy playing. Participants were then asked demographic questions and questions to probe for suspicion, before being told that they had completed the experiment. They were then given a fake debriefing statement. This statement told participants that we were interested in how video game experience affected enjoyment and mood. Participants then left the lab.

As participants walked down the hall to the exit, they were greeted by a female confederate posing as a member of the Red Cross looking for people to volunteer their time to help with a blood drive (Manucia, Baumann, & Cialdini, 1984). When she stopped the participant in the hall, she recited the following: “Hi – my name is Sara with the Red Cross. We are currently looking for volunteers to call previous blood drive donors to see if they would be willing to donate again at the present time. If you choose to volunteer, you would choose anywhere from one to 20 individuals to call, and we would provide you a list of individuals that you would contact and inquire into their willingness to donate again. Your participation could really help out our blood drive**.** Would you be willing to volunteer?” Depending upon which condition they were assigned, some participants had the confederate say “Your participation could really save some lives” instead of “Your participation could really help out our blood drive”. Everything else in the script was identical. If the participant responded “yes”, then the confederate asked how many people the participant would volunteer to call. After this point, or after the participant had declined to volunteer, the confederate told the participant that she was part of the experiment, and asked if the participant had any suspicions of this. Following this acknowledgment, the confederate then debriefed participants as to the real purpose of the experiment to see how types of video games may affect prosocial behavior and asked if the participant had any questions. At this point the experiment ended, and the participant was thanked and left.

**Results**

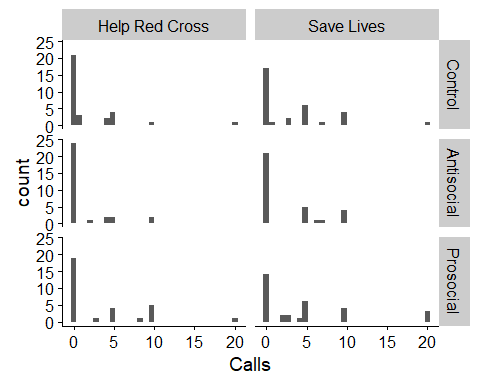
**Sample**

An initial data collection included 216 subjects, 191 of which were successfully intercepted by the confederate as they left the laboratory. During data collection, the Spring semester ended before data collection was completed. We looked at the data to examine whether it was trending in the hypothesized direction, with the idea that we would continue data collection as planned if it was, and would drop data collection if it was not. The data was indeed trending in the hypothesized direction, so we picked up data collection as originally planned in the following Fall semester. This natural pause in data collection and evaluation of whether or not to continue collecting data may or may not influence Type I error rates.. Additionally, we decided to exclude a semester's worth of participants from one study site (*n* = 31), as the confederate used for only that semester did not correctly collect data as was instructed. To support this exclusion of the data, we found the participants solicited by this confederate were statistically less likely to volunteer than other participants, chi-square(1) = 3.96, *p* = .047. This left us with a final sample of 204 subjects, 189 of which were successfully intercepted by the confederate.

Our primary analyses concern this final sample. However, to explore the robustness of our results, we also analyze the data including those 31 participants.

**Distribution**

Initial inspection of the data revealed pronounced non-normality (Figure 1). Many subjects did not offer to make any calls, and among those who did, the number of calls offered was strongly right-skewed and favored multiples of 5.



This non-normality creates ambiguity in what might be the most appropriate model to fit. Although ANOVA is commonly used and fairly robust to outliers, its basic assumptions are violated by the pronounced non-normality of the residuals (CITATION HERE). Therefore, we fit alternative models to explore the robustness of the results and to characterize their analytic ambiguity. In total, we fit: 1) ANOVA; 2) logistic regression (general linear model; GLM), testing whether condition affected the odds of volunteering to make any calls; 3) a chi-squared test, again testing whether condition affected the odds of volunteering to make any calls; 4) a zero-inflated negative binomial, which accounts for the frequent zero-responses and the strong right skew; and 5) a non-parametric Kruskal-Wallis test for differences in the median. Results for each model are reported separately, then synthesized and summarized at the end.

**ANOVA**

ANOVA indicated a significant effect of game, *F*(2, 184) = 3.23, *p* = .042. Neither the effect of request nor the Game × Request interaction was statistically significant, *F*(1, 184) = 1.42, *p* = .235 and *F*(2, 184) = 0.01, *p* = .992, respectively.

Collapsing across levels of request, a contrast between the prosocial-violence and antisocial-violence conditions was statistically significant, *t*(122) = 2.48, *p* = .015, *b* = 2.02 [.40, 3.63]. Neither game significantly differed from control.

**Logistic GLM**

To test whether the game influenced the odds of volunteering, we collapsed observations to a binomial outcome (0 = did not volunteer, 1 = volunteered). A logistic GLM was fit to test for effects.

The effect of game was not statistically significant, but not especially far from significance either, *chiq(2)* = 5.08, p = .079. Effects of request and the Game x Request interaction were not statistically significant, *chisq(1)* = .351, *p* = .358 and *chisq(2)* = .924, *p* = .948, respectively. Again, the contrast between the prosocial-violent and antisocial-violent games was statistically significant, *z* = 2, *p* = .029, *OR* = 2.31 [1.1, 4.96]. However, neither game significantly differed from the control condition.

**Chi-Square Analysis**

A chi-squared test was used to test whether the probability of volunteering at all differed across game conditions (collapsing across levels of request). The omnibus test for game type was not significant, but not far from significance, chi-square(2) = 4.92, *p* = .085. As in other analyses, the contrast between prosocial-violent and antisocial-violent was just significant, chi-square(1) = 4.07, *p* = .044, although neither game significantly differed from control.

**Zero-inflated negative binomial**

A zero-inflated negative binomial was then fit to the data. This model has two parts: The zero-inflation model, which estimates whether participants volunteer at all, and the count model, which estimates how many calls they offer when they do volunteer. Application of the zero-inflated negative binomial produced a nearly but not quite significant omnibus effect of game (*chisq*(2) = 5.09, *p* = .079). Effects of request (*chisq*(1) = 0.85, *p* = .358) and the Game × Request interaction (*chisq*(2) = 0.11, *p* = .948) were not statistically significant. The overdispersion parameter was highly significant (*z* = 4.66, *p* < .001), supporting the use of the negative binomial over a zero-inflated Poisson distribution.

A pairwise contrast between the prosocial-violent and antisocial-violent games did not yield a significant omnibus test for an effect of game, *chisq*(2) = 3.35, *p* = .187. Tests of individual parameters, however, found a significant difference on the zero-inflation parameter (*z* = -2.14, *p* = .032) but not on the count parameter (*z* = 1.46, *p* = .146). Again, neither game differed from the control condition.

**Kruskal-Wallis test**

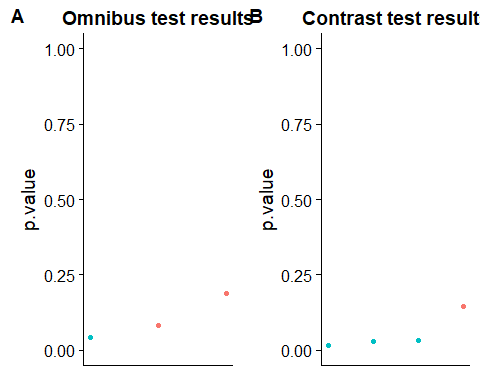
The Kruskal-Wallis test is a nonparametric test of group medians. Because the test is only applicable to one-way ANOVA designs and the primary hypothesis regarded effects of game, we collapsed across levels of request and tested for an effect of game. The omnibus test of different group means produced a marginal effect, *chisq*(2) = 5.09, *p* = .079.

The pairwise contrast between the antisocial and prosocial violent games was statistically significant, *chisq*(1) = 4.97, *p* = .026. However, neither game significantly differed from the control condition, *p*s = .224 and .262.

**Describing uncertainty**

**Uncertainty across models**

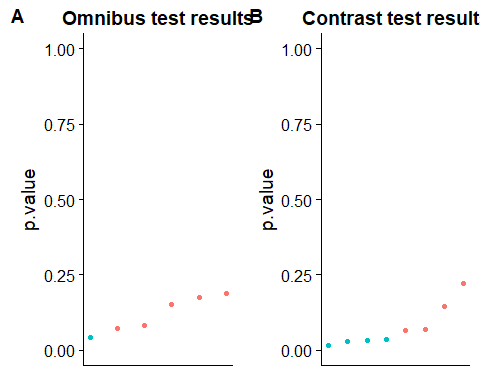
Figure 2A shows the distribution of omnibus test *p*-values across the five models. Figure 2B shows the distribution of pairwise prosocial/antisocial game contrast *p*-values across the five models. Blue dots indicate significant results, whereas red dots indicate non-significant results.



Across models, the omnibus test is generally not statistically significant (median *p* = 0.081), reaching a minimum p-value of .043 in the ANOVA model. The pairwise contrast, on the other hand, is generally statistically significant. Its *p*-values range from .015 (ANOVA) to .146 (zero-inflated negative binomial model's zero-inflation parameter), with median *p* = 0.031.

**Uncertainty across datasets**

In the analyses presented above, 31 participants were excluded after it was revealed that the confederate at one site during one semester was not collecting data as was instructed. One may wonder how robust the results are to the inclusion of these subjects. Figure 3 shows the variability in the *p*-value across this decision to include or exclude. Six parameters in two datasets yields twelve *p*-values. Figure 3A shows the distribution of omnibus test *p*-values, and Figure 3B shows the distribution of pairwise contrast *p*-values across the five models.



Together, these figures illustrate the uncertainty in the *p*-value across datasets. Again, we note that only one analysis yielded a significant omnibus test. Additionally, the pairwise contrast is no longer statistically significant when including the 31 subjects of questionable quality. Naturally, more data is better, and bad data should be excluded. Still, to the extent that this decision was made *ad hoc*, it is useful to note their influence on the *p*-value.

**Discussion**

We generally did not detect a significant overall effect of game on prosocial behavior. Still, pairwise contrasts suggested a significant difference in behavior following the prosocial-violent as compared to the antisocial-violent game. However, this contrast should be interpreted with caution given that neither violent game significantly differed from the control game.

Considerable uncertainty remains.. The decision to exclude subjects is justifiable, but not based on any *a priori* rule, creating another researcher degree of freedom. The prosocial-violence vs. antisocial-violence contrast is one of three potential contrasts that consider the effects of game. Finally, the results are a little sensitive to the particular model applied. Whereas a typical manuscript might conceal the ambiguity by presenting just one analysis, here we find it most helpful to show all possible analyses.

This uncertainty should not be mistaken for an argument that games do not influence prosocial behavior. Although statistical significance is sporadic and sensitive to choice of model, contrast, or dataset, it is generally not the case that the results constitute clear support for the null hypothesis.

The results of this study are nonetheless helpful in estimating the effects of antisocial-violent and prosocial-violent games. It is unclear whether these types of games are substantially different than a control game in terms of their effect on prosocial behavior. If such effects exist, they are likely not large and obvious, at least with measures of prosocial behavior that result in a floor effect, as our measure did. Furthermore, we are able to rule out some effects in the opposite direction. The data are inconsistent with antisocial-violent games *promoting* prosocial behavior or prosocial-violent games *inhibiting* prosocial behavior.

As mentioned above, the prosocial measure we selected ultimately was the greatest limitation of the current study. First of all, several of the participants that completed the lab portion of the study were missed by the confederate, so they were unable to encounter the prosocial portion of the experiment. Second, and perhaps more importantly, we chose a measure in which most of the participants declined to volunteer, which did not allow an adequate contrasts between our game conditions. It would have been much more useful to use a prosocial measure in which most participants are willing to help at least a little bit. This would be considerably more effective in determining the extent to which the violent game condition might decrease prosocial behavior (because the standard response is to help a little, thereby giving that response room to decrease). Likewise, a prosocial measure in which participants are generally willing to help a little would be a better measure in demonstrating how that tendency could potentially be increased by prosocial games that are violent.

In conclusion, despite collection of a moderately-sized sample, the results are ambiguous regarding whether prosocial-violent and antisocial-violent games affect prosocial behavior as compared to a nonviolent game. Some support for a difference between antisocial and prosocial violent games was found, but this difference was sensitive to the choice of model and dataset. Future research is encouraged to test the effects of prosocial, antisocial, violent, and nonviolent games on prosocial behavior. This research program would be facilitated by the development of more sensitive and model-friendly measures of prosocial behavior. In particular, with a better measure of prosocial behavior, the current ambiguous results that provide some very tentative support for the difference between prosocial-violent and antisocial-violent could be clarified. It is possible that violent video games reduce prosocial behavior, whereas the prosocial element of violent video games may counteract the violent nature of the game, which would result in similar prosocial behavior to a control game. On the other hand, it is possible that the prosocial element of a violent video game increases prosocial behavior above an antisocial violent game, which may or may not be different from a control game. Another possibility is that a prosocial violent video game encourages both prosocial behavior and aggression, but the context determines which of those responses would be elicited. For instance, after playing a prosocial violent game, the prosocial element of the game may influence players to help an elderly person across the street, while the violent element of the game may influence the player to yell at someone in a car that impatiently honks at the delay. Future research should work to disentangle all of these possibilities and further enhance our understanding of the potential different effects that antisocial violence and prosocial violence may have on the gamer.

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