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Violent video games and social behaviour

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Abstract

There is public and scientific concern surrounding violent content in interactive video games. After several decades of experimental research, there is some evidence that violent content in media can influence people to behave in predictable, anti-social ways. These include increased hostile attributions, aggressive cognitions, and aggressive behaviour. There has been comparatively little research, however, on how prosocial behaviour (e.g. helping behaviour) is affected by violent media. Thus, the present thesis examined the hypothesis that, to the extent that violent video games can increase anti-social behaviour, they should similarly decrease prosocial behaviour. The investigation is grounded in contemporary theories of aggression, and draws from literature on social priming, economic games, and dehumanisation.

The experiments presented in Chapter 2 explore reasons for why a recent study found, un-intuitively, that violent video games have no effect on prosocial behaviour. I tried to demonstrate a violent video game effect using a traditional media exposure paradigm. I also report on the role of context in these paradigms, and the subsequent effect that context has on behaviour measures. Finally, I report a direct replication attempt of past research that suggested video game content can influence prosocial behaviour.

In Chapter 3, I tested the assumption that increasing violent content has an incremental effect on social behaviour. I used games with differing levels of violent content (non-violent, violent, and ultra-violent) and examined their impact on prosocial behaviour. This experiment extended on those reported in Chapter 2 by several means: (1) I increased the strength of violent content manipulation; (2) I used multiple measures of prosocial behaviour; and (3) I recruited a larger sample.

Finally, Chapter 4 tests whether certain preconditions need to be met for violent video games to affect prosocial behaviour. In this experiment, I tested whether participants need to be in a hostile cognitive state in order for violent video games to influence their behaviour. I paired violent video gameplay with a hostile semantic prime and observed whether participants were more or less likely to help another.

The results of these experiments suggest that the effect of violent video games on prosocial behaviour is, at most, a small effect, and that corresponding public concern should be minimal. These findings are, unfortunately, at odds with most prevalent theories of media effects. A key issue with prevalent media effects theories, however, is that they assume a content-driven view of media influence: that is, that media affect users in reliable and

predictable ways. What seems more likely is that, while media *can* influence behaviour, users are active shapers of their media experience. A user-driven view of media influence would examine motivations for using media (violent content or not), the social benefits of gaming with others (through violent media or not), and the satisfaction of needs for competence and autonomy (via skill development in violent games or not).

Declaration by author

This thesis is composed of my original work, and contains no material previously published or written by another person except where due reference has been made in the text. I have clearly stated the contribution by others to jointly-authored works that I have included in my thesis.

I have clearly stated the contribution of others to my thesis as a whole, including statistical assistance, survey design, data analysis, significant technical procedures, professional editorial advice, and any other original research work used or reported in my thesis. The content of my thesis is the result of work I have carried out since the commencement of my research higher degree candidature and does not include a substantial part of work that has been submitted to qualify for the award of any other degree or diploma in any university or other tertiary institution. I have clearly stated which parts of my thesis, if any, have been submitted to qualify for another award.

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Publications during candidature

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Conference abstracts

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Contributor	Statement of contribution
Author 1 (Candidate)	Designed experiments (60%) Collected data (100%) Conducted data analysis (100%) Wrote the paper (80%)
Author 2	Designed experiments (40%) Wrote and edited paper (20%)

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Author 2	Designed experiments (40%) Wrote and edited paper (20%)

Contributions by others to the thesis

My primary advisor, Associate Professor Mark Nielsen, provided guidance on theory, study design, and data analysis, and reviewed and commented on the thesis and associated manuscript.

Statement of parts of the thesis submitted to qualify for the award of another degree

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List of Abbreviations Used in the Thesis

ANOVA – Analysis of variance
CRTT – Competitive Reaction Time Task
GAM – General Aggression Model
NHST – Null Hypothesis Significance Testing

Chapter 1 – Introduction

Video games and social behaviour

> "You cannot tell me -- common sense tells you that if these kids are playing video games, where they're on a mass killing spree in a video game, it's glamorized on the big screen, it's become part of the fiber of our society…We're going to have to start addressing those issues and recognizing that the mass murders of tomorrow are the children of today that are being programmed with this massive violence overdose."
>
> - Dr. Phil McGraw (King & McGraw, 2007)

> "Over the centuries, mankind has tried many ways of combating the forces of evil...prayer, fasting, good works and so on. Up until Doom, no one seemed to have thought about the double-barrel shotgun. Eat leaden death, demon..."
>
> - Terry Pratchett (1998)

In Western culture, video games have become a ubiquitous form of entertainment, recreation, and education for people across a wide age span. Indeed, the old stereotype of a gamer as an introverted young boy is no longer true. The average age of someone who self-identifies as a gamer in the U.S. is 31 (ESA, 2014) and 32 in Australia (IGEA, 2014), and in Europe, almost 50% of all gamers are aged 35 and over (ISFE, 2012). Gamers are also no longer necessarily male, with U.S. and Australian consumer surveys reporting 47-48% of respondents as female (ESA, 2014; IGEA, 2014). And in Europe, 43% of women report playing video games, averaged across 15 European nations (ISFE, 2012). Those who do play video games play them often, with 37% of the U.S. population age 9 and older playing video games for an average of 6.4 hours per week (The NPD Group, 2014). In Australia, most gamers report playing video games daily, and typically play for an hour at a time (IGEA, 2014). Given that between 15-35% of the top selling games in 2013 (by units sold) were rated as suitable for mature audiences (ESA, 2014), there is seemingly justifiable concern over the effect violent and adult-themed games may have on those who play them.

It is the aforementioned ubiquity of video games, specifically of violent video games, that drives public concern. There are countless opinion pieces (Schlafly, 2011), petitions (Caissie, 2014), and discussion posts (Confucius, 2006) presuming that merely observing and interacting with violent content is enough to turn children into merciless killers. Indeed, violent video games are inevitably found in the bedrooms and homes of young male perpetrators of mass violence, including the shooters at Columbine High School in Littleton, Colorado, the Aurora shooter in Colorado, and the Sandy Hook shooter in Newtown, Connecticut. Of course, one should only be surprised had violent video games *not* been found, given that playing violent video games is a very typical thing of that demographic (ESA, 2014; IGEA, 2014; ISFE, 2012). It is in the aftermath of such terrible events that

parents, officials, politicians, and scientists seek to explain what led to such a tragic set of circumstances. As it was in past generations, credence is given to the conventional wisdom, or untested intuition, that exposure and consumption of 'immoral' media is in some part responsible for the current atrocity. Of course, failure to consider instances where 'immoral' media are present in the bedrooms of young males who are *not* perpetrators of mass violence, that is, failure to consider data that disconfirms the intuition about violent media, constitutes 'confirmation bias' and should be avoided by pundits on both sides.

Public concern and media in the 20th century

In what would become a typical reaction of the 20th century, the arrival and diffusion of each new form of media was met with public concerns about moral depravity in society (not from all 20th century citizens, but a vocal portion). These concerns included (but were not limited to) new media such as newspapers, motion pictures, comic books, radio, and television.

Public concern likely fed scientific concern and so scholars investigated the effect of media consumption on the character of individuals and changing moral sphere of society. Joseph Holmes, writing in the Journal of the American Institute of Criminal Law and Criminology, unequivocally concluded that "Newspapers are guilty of inciting to crime" (1929, p. 52). In his collation of professional opinions, Holmes explained that newspaper crime reports give precise information for *how* to commit crimes and escape apprehension following the crime. Charles Peters, writing in the Journal of Education Sociology, described a debate on whether motion films conflicted with societal standards of morality (Peters, 1933). He lamented: "In this agitation neither side has been able to appeal to objective evidence, either as to what constitutes morality or as to the amount of conflict by motion pictures with it if defined" (p. 251).

Radio was also met with similar concerns. Howard Rowland, speaking in Educational Research Bulletin, was skeptical that listening to radio crime dramas could have cathartic benefits (Rowland, 1944):

> Crime and violence in drama lose their cathartic value when there is a constant habituation to overdoses of these ingredients which not only results in jaded taste in children but may contribute to those frustrations which bring about aggressive behaviour. If this premise is correct, it follows that the producers of crime dramas help bring about some of the aggression which these dramas are supposed to relieve. (p. 214)

The explosion of comic books in the 1930s and 1940s was similarly concerning. With monthly circulation estimated at over 40 million (Cavanagh, 1949), it is not surprising to hear the now familiar invocation "Every now and then, when tragedy enters into the life of some boy or girl, investigation leads back to the youth's reading of the comic books and their incitement to crime" (Shea, 1948, p. 163).

It appears that whenever a new media is introduced and proliferates, then it is usually accompanied with public, and sometimes subsequently scientific, concern. Some further examples include concerns over satanic themes in table-top games (Martin & Fine, 1991), sexual norms in pornography (Malamuth & Briere, 1986), and violence in television (Eron, Huesmann, Lefkowitz, & Walder, 1972). With the advent of personal computers and video game consoles, and their subsequent proliferation, much of the media effects research turned its focus toward violent video games, with many early experiments (Anderson & Dill, 2000) and reviews (Dill & Dill, 1999; Griffiths, 1999) finding detrimental effects of violent video game exposure.

Epistemology of media effects research

Media effects research has been very responsive to changes in the learning psychology zeitgeist. The stimulus-response-reinforcement model of 1950s behaviourism provided a useful framework for beginning to think about how external stimuli might affect the individual. A behaviourist argues that behaviour is the product of a stimulus acting on an organism, the organism consciously or unconsciously selecting a response to the stimulus, and an external agent providing reinforcement for that response in the form of a punishment or reward: stimulus-response-reinforcement.

While a fine model for explaining how rats learn that a flashing light means they can push a button to receive some food, behaviourism begins to fall down once you change the stimulus from a flashing light to a stimulus that requires interpretation, such as any form of media. Indeed, the gaps left by behaviourism were filled by cognitivism in 1960s and 1970s, where the model was adapted to include interpretations made by the organism, i.e. stimulus-*organism*-response-reinforcement. This extra step is where concepts like personality, attitudes, values, and schemas provide input into response selection. These cognitive structures are thought to mediate external stimuli, maintain and moderate the behavioural response of the organism, and designate the value of reinforcement of the response.

Once the cognitive revolution displaced strict stimulus-response-reinforcement schedules of behaviourism, cognitivism afforded Albert Bandura the tools for which to develop his theory on social learning. Social learning theory posits that organisms do not need reinforcement in order to learn a particular behavioural strategy - behaviours can be learned purely through observation or direct instruction, that is, without the need for motor reproduction or direct reinforcement. According to Bandura (1963), a key problem with Skinnerian behaviourism was that it could not adequately account for the acquisition of novel responses. Bandura and colleagues (1961) were able to demonstrate, via their now famous Bobo doll experiments, that children could rapidly acquire novel behaviours through the process of observation and imitation. In the experiments, children were randomly allocated to one of three groups: (1) observation of an adult acting aggressively towards the Bobo doll (a child-sized inflatable doll that will always return to an upright position after being knocked

down), (2) observation of an adult acting peacefully towards the Bobo doll, and (3) a control group. Those children who observed the aggressive model were more likely to imitate aggressive actions toward the Bobo doll when given the chance to play independently with the doll. This rapid acquisition of behaviour occurred even in the absence of any direct reinforcement (although the adult may implicitly serve as an implicit reinforcement - "if an adult is doing it, then I, as a child, ought to be doing it too"). The work of Bandura and his colleagues had significant implications for the concern of the day: media violence (Miller, 2004). If children could learn aggressive behaviour by simply watching an aggressive model, then the surge of violent content on television screen around the world might have profound effects on the behaviour of child viewers (Miller, 2004).

Indeed, this viewpoint (that violence can be learned through observation) remained largely unchanged for several decades, and formed the basis for several offshoot theories of media violence and child development during the 1980s, all of which take a strong cognitivist account. Leonard Berkowitz took the cognitive structures of associations and developed his cognitive neoassociationist model of aggression (Berkowitz, 1989; 1990). The core notion of Berkowitz's model is that negative affect is the basic source of anger and angry aggression. Negative affect produced by unpleasant experiences automatically stimulates various thoughts, memories, expressive motor reactions, and physiological responses associated with both fight and flight tendencies. Furthermore, cognitive neoassociation theory assumes that cues present during an aversive event become associated with the event and with the cognitive and emotional responses triggered by the event.

A second account for media violence effects posits that the interpretation component of the stimulus-organism-response-reinforcement model has a moderating role to play in the relationship between excitation and aggression. According to Zillmann's excitation-transfer theory (Zillmann, 1983), excitation's role in aggression is partly moderated by the organism's appraisal of the source of excitation/threat. Thus, violent media could potentially make someone behave aggressively by increasing excitation in response to violent content. Residual excitation that amplifies excitation to a subsequent stimulus, paired with a faulty appraisal, could lead someone to incorrectly interpret an ambiguous gesture as hostile.

Finally, L. Rowell Huesmann (1986) proposed that social behaviours, including aggression, are influenced by 'scripts' or 'programs' for behaviour learnt early in a child's development - a script being an easily retrieved set way of behaving, given a particular scenario. Media violence thus affects children in the long- and short-terms: (1) acquiring a script is a cumulative learning process in which the child's observation of violence eventually leads the child to employing aggressive scripts, and (2) short-term increases in aggression are cued by the retrieval of already learnt aggressive scripts.

Each of these theories relies on the strength of cognitivist explanations for influencing behaviour. Cognitive neoassociationism relies on associations between feelings of anger and cues present during that angry episode. Excitation-transfer relies on appraisals of excitation

(or arousal) and the source of this excitation. Script theory relies on the association of a behavioural response selected in response to an event and positive outcomes of that response. In the next section, I present the General Aggression Model, which seeks to combine these theories into one unified theory of aggression and media effects.

The General Aggression Model

The General Aggression Model (GAM; previously known as the General Affective Aggression Model, Anderson & Dill, 2000; Lindsay & Anderson, 2000; sometimes referred to as the General Learning Model, Greitemeyer & Osswald, 2009; 2010) is a theoretical model for describing both short- and long-term effects of violent media exposure. The model is strongly influenced by the cognitivist framework of the 1970s. Indeed, the key proposition made by the GAM (Anderson & Bushman, 2002; Anderson, Deuser, & DeNeve, 1995; Lindsay & Anderson, 2000) is that aggression (or more generally, behaviour) is largely based on learnt scripts and associations that are driven by social learning.

The GAM consists of three broad components in selecting behavioural responses to events: (1) person and situation inputs; (2) present internal state routes; and (3) appraisal outcomes (DeWall, Anderson, & Bushman, 2011). The model begins with basic person and situation inputs, which describe anything in the current situation that could influence the person's current state. This stage includes situation variables, such as pain and discomfort, frustration, or attacks and threats, and person variables, such as trait aggressiveness or holding antisocial attitudes. These variables affect the current state of the person by influencing the interpretation and understanding of incoming information. For example, people who are uncomfortably hot, or have been insulted, are more likely to access hostility-related schema that a person who is comfortably cool or experiencing pleasure (Anderson, et al., 1995). The basic input also considers individual characteristic differences, such as traits, attitudes or beliefs about aggression, and skills (e.g. fighting ability).

Primary appraisals of events are influenced by the present internal state, which is discussed via three different routes: available cognitive schemata to process incoming information, the affective state through which incoming information is filtered, and the state of arousal. In the context of explaining aggression, the model proposes that aggressive behaviour is produced by input variables that activate aggression-related cognitions, inducing an anger-related affective state, and/or by increasing arousal. To describe how each of these states play, Anderson and colleagues (Anderson et al., 1995) describe situations involving your hypothetical nemesis, Joe Snerd. Imagine Joe Snerd apologises for previously disparaging your work. Your interpretation of the apology, your affective reaction, and, ultimately, your behavioural response may depend on whether you have a hostile schema active or not. Similarly, your interpretation of the apology, etc., may also depend on your on whether you are in an anger-related affective state. Finally, if you are in a highly aroused state, your ability to control impulsive behaviour is limited and you may impulsively act

aggressively. When time and cognitive resources are available, the results of the primary appraisal processes are further evaluated in a more thoughtful, effortful, and conscious secondary appraisal process before selecting a behavioural response, thus comprising the output component of the GAM.

Violent video games and aggression

The GAM has been predominately used to explain aggression arising from violent media exposure. A relatively new media, video games, are sometimes thought to be categorically different from other media, further fuelling concern about possible negative effects. Differences between violent video games and other violent media include the saturation of violent content and interactivity (Malamuth, Linz, & Yao, 2005). Violence on television is thought to happen intermittently and to be dispersed throughout a larger narrative structure. For example, in a television police procedural drama, the episode might open with a violent murder but there might not be any more violent content until the police capture a fleeing suspect. In a violent video game, however, should the player 'die', he simply restarts the level and reattempts the violent content again until he successfully completes the objective. It may thus be that the proportion of violent content in violent video games is much more than in other violent media, which some have argued may accelerate the effect of the violent content (Malamuth et al., 2005).

Additionally, video games are fundamentally different from other media in that they require the user to actively interact with the medium, rather than passively observe, which may be important for understanding violent video game effects (Malamuth et al., 2005). For example, those who watch a violent television program merely watch others behave aggressively. In a violent video game, players actively decide to punch, shoot, or harm others. That actively committing violence might have implications for the development of aggressive scripts makes intuitive sense, and there is preliminary behavioural evidence confirming the idea (Polman, Orobio de Castro, & van Aken, 2008) (although see the literature on in-game advertising showing that the more intently the user interacts with the game, the weaker the cognitive associations with brands from their virtual environment are; Chaney, Lin & Chaney, 2004; Herrewikm & Poels, 2013; Lee & Faber, 2007).

As mentioned previously, the GAM is almost exclusively used to predict aggression in the context of violent video game exposure. Of course, it is legally and ethically difficult to insert participants into situations where there are possibilities of real-world aggression. Thus, experimenters have needed to devise novel and creative means for inferring aggressive behaviour. One commonly used paradigm is the Competitive Reaction Time Task (CRTT; Taylor, 1967), in which participants compete against a fictitious other opponent to have the fastest reaction speed. The winner can punish the loser by setting the intensity of a noxious stimulus, usually a noise blast (although experimenters have also used electric shocks; Taylor, 1967). Using this approach, numerous studies have shown that playing violent video

games can lead to participants selecting higher intensity noise blasts than participants who play non-violent games (Anderson & Carnagey, 2009; Anderson et al., 2004; Bartholow & Anderson, 2002; Bartholow, Bushman, & Sestir, 2006; Carnagey & Anderson, 2005; Sestir & Bartholow, 2010). Importantly, however, other researchers find no effect of violent games on CRTT score (Elson, Breur, Van Looy, Kneer, & Quandt, 2013; Ferguson & Rueda, 2010).

There are other ways researchers have measured aggression. The hot sauce paradigm, developed by Lieberman and colleagues (Lieberman, Solomon, Greenberg, & McGregor, 1999), allows participants to aggress against another by selecting how much hot sauce the other must consume. Some studies have found that playing a violent video game leads participants to selecting larger portions of hot sauce (Barlett, Branch, Rodeheffer & Harris, , 2009; Fischer, Kastenmüller, & Greitemeyer, 2010), although others have suggested that the effect is driven by competitiveness, not violent content (Adachi & Willoughby, 2011b). Another study measured how likely participants were to steal lollies after playing a delinquency-reinforcing game compared to a neutral game (Fischer, Aydin, Kastenmüller, Frey, & Fischer, 2012). It is important to remember that each of these behavioural measures is only a vague analogue of aggressive behaviour, and there is evidence that standardised and validated measures of aggression are less likely to reveal an effect (Ferguson & Kilburn, 2009). Overall, evidence for behavioural effects of violent video games is mixed.

Early studies that documented behavioural differences between participants who play violent vs. non-violent video games were followed by attempts at delineating potential mechanisms. The GAM predicts that differences in the cognition of those who play violent or non-violent video games. Aggressive cognitions are often measured by word completion tasks. In these tasks, participants are shown words with one or two critical letters missing, for example, 's _ a b'. There are at least two possible letters that could fill the empty space to complete the word: 'l' to complete the word as 'slab', or 't' to complete the word as 'stab'. If aggressive cognitions are salient, then participants are more likely to complete the word as the aggressive form ('stab') over the non-aggressive form ('slab'). A number of studies reported that participants who played a violent video game made more aggressive completions than participants who played a non-violent game (Anderson et al., 2004; Barlett et al., 2009; Barlett & Rodeheffer, 2009; Carnagey & Anderson, 2005; Sestir & Bartholow, 2010). Other studies have shown that participants who play a violent game, compared to those who play a non-violent game, are quicker to identify aggressive words (Anderson & Carnagey, 2009), and have stronger associations between aggression and their self-concept (Bluemke, Friedrich, and Zumbach, 2010; Uhlmann & Swanson, 2004). As with aggressive behavioural studies described above, there are also some studies that show no effect of violent video games on aggressive cognition (e.g. Cicchirillo & Chory-Assad, 2005). Ivory and Kalyanaraman (2007) measured the similarity between aggressive and ambiguous word-pairs. If violent video games increase the salience of aggressive cognitions, then participants

should have interpreted the ambiguous words as more aggressive. However, the test did not yield any significant results between the experimental groups.

Affect is another important component of the GAM, usually measured via the self-report State Hostility Scale (Anderson, et al., 1995). Evidence for the effect of violent video games on affect and state hostility is mixed: some research has shown evidence for the effect (Barlett et al., 2009; Carnagey & Anderson, 2005; Saleem, Anderson, & Gentile, 2012; Sestir & Bartholow, 2010), whereas others find mixed (Anderson & Carnagey, 2009), or no evidence for the effect (Ballard, Hamby, Panee, & Nivens, 2006; Ferguson & Rueda, 2010; Ivory & Kalyanaraman, 2007; Valadez & Ferguson, 2012).

Several meta-analyses have been conducted to elucidate an overall effect of violent video games. Unfortunately, it is still not clear what effect violent video games have on behaviour. Anderson and colleagues (2010) conducted the largest meta-analysis to date (n = 130,296) and concluded that violent video games have small to medium effects on behaviour. There are, however, two major criticisms of the Anderson et al. (2010) meta-analysis. First, the authors report a stronger effect size estimate when they include in the analysis only the studies that use best practice methodology. Unfortunately, the criteria for what constitutes best practice is vague and subjective (e.g. violent games were determined as those containing "considerable violence"), and the best practice criteria may in fact reflect a biased selection (Hilgard, Engelhardt, Bartholow, 2015). Second, many of the unpublished data solicited for the meta-analysis do not appear to have come from researchers who often find differing results (Ferguson & Kilburn, 2010). Other meta-analyses (Ferguson & Killburn, 2009; Sherry, 2001; 2007) have thus estimated the effect of violent video games to be smaller than that reported by Anderson and colleagues (2010). Whether or not exposure to violent video games leads to increases in aggressive and/or violent behaviour thus remains a topic of much debate and one that continues to elude consensus.

Violent video games and prosocial behaviour

Sitting alongside the video game-violent behaviour debate are more recent attempts at charting the impact of game exposure on other behavioural domains. For example, if the content of video games affects behaviour via associations and scripts, then video games with prosocial content should increase prosocial behaviour (Gentile, et al., 2009). There have been several other experimental contexts where priming by prosocial content has increased prosocial behaviour. Macrae and Johnston (1998) showed participants prosocial words and those participants were subsequently more likely to help a confederate after a spontaneous mishap. Participants primed with the concept of ‘superhero’ were more likely to volunteer at a later date (Nelson & Norton, 2005).

In line with this research, prosocial content presented in video games has indeed been shown to increase prosocial behaviour. A comprehensive review conducted by Lenhart and colleagues (2008) examined the impact of civic gaming experiences on engagement of civic

and political activity. In this study, in-game civic gaming experiences, such as helping or guiding other players, playing games to learn and make decisions about social and ethical issues, or organising game groups or guilds, were significantly related to an increased interest in a variety of real-life instances of civic behaviour, including raising money for charity and civic engagement. After playing video games with prosocial aims and objectives, participants are more likely to make it easier for a partner to win a reward (Gentile et al., 2009), to spontaneously help an experimenter after a mishap (Greitemeyer & Osswald, 2010; Experiment 1), to volunteer more time to help an experimenter with further studies (Greitemeyer & Osswald, 2010; Experiment 2), and to intervene in a potentially costly harassment situation (Greitemeyer & Osswald, 2010; Experiment 3). In a fourth experiment, Greitemeyer & Osswald (2010) reported, partially consistent with the violent video game literature, that the accessibility of prosocial cognitions mediated the effect of video game type on prosocial behaviour. Cognitive routes, such as the accessibility of prosocial thoughts, are a hallmark of the GAM, as discussed earlier.

Following the GAM, if violent video games increase anti-social behaviour, it is reasonable to assume that playing violent video games would similarly decrease prosocial behaviour. Unfortunately, as with aggression, the effect of violent content on prosocial behaviour is limited and somewhat contradictory. Seminal work by Chambers and Ascione (1987) was among the first to demonstrate that violent video games can reduce prosocial behaviour by showing that children who played a violent video game donated less to charity than those who played a prosocial game (although results for another behavioural helping measure were not significant and, sadly, not reported). Subsequent experiments found that participants who played a violent game, compared to a non-violent game, were less likely to reward a confederate (Ballard & Lineberger, 1999), less likely to cooperate (Brady & Matthews, 2006; Sheese & Graziano, 2005), less willing to help an experimenter with other research (Happ, Melzer, & Steffgen, 2011), and less likely to intervene in a serious altercation (Bushman & Anderson, 2009). Other studies, however, have shown that playing a violent video game cooperatively can militate any negative effect of violent video games on prosocial behaviour, or even increase prosocial behaviour (Bennerstedt, Ivarsson, & Linderoth, 2012; Ewoldsen et al., 2012; Greitemeyer, Traut-Mattausch, & Osswald, 2012; Velez, Mahood, Ewoldsen, & Moyer-Gusé, 2012), suggesting that the effect of violent content on prosocial behaviour, as with aggression, is not clearly understood.

Indeed, a recent experiment conducted by Greitemeyer and Osswald (2010) failed to reveal an expected detrimental effect of violent video games on prosocial behaviour. In their experiment, the researcher asked participants to play either a violent (Lamers), non-violent (Tetris), or prosocial (Lemmings) video game for 12 minutes, before pretending to spill a handful of pens on the ground. Participants were deemed to have acted prosocially if they helped the experimenter gather the spilt pens. While Greitemeyer and Osswald found that participants who played prosocial video games were more inclined to help, frequency of

helping behaviour was approximately equal between participants who played violent and non-violent video games. The researchers were surprised at this finding and suspected two reasons for the failure to demonstrate a negative effect of violent video games: (1) the exposure times (12 minutes) were too short, and (2) the games were too old and lacked sufficient realism.

Rationale for the present thesis

It is important to test intuitions such as these, especially when evidence for the expected effect (a reduction in prosocial behaviour after playing violent video games) is mixed. We must, therefore, establish a reliable behavioural effect of violent video games on prosocial behaviour, that is, do violent video games, in fact, decrease prosocial behaviour? Answering this question will help determine whether public concern is warranted. There are also practical reasons for studying violent video game effects on prosocial behaviour. As discussed earlier, legal and ethical reasons make it practically impossible for experimenters to measure serious aggression or violence in the lab. Researchers must settle for analogues of aggressive behaviour as their dependent measures. Unfortunately, these lab-based measures of aggression have come under scrutiny for being poor approximations of serious aggression or violence (Ferguson & Rueda, 2009; Tedeschi & Quigley, 1996; 2000). Behavioural measures of helpfulness, however, are not constrained by the same legal and ethical concerns - participants are not at risk of harm if they behave prosocially in a lab-based experiment. Furthermore, prosocial measures can be easier to implement than lab-based aggression measures such as the CRTT, which require an elaborate ruse about another participant receiving noxious stimuli.

There are issues with the existing violent video game literature and prosocial behaviour. First, the violent video game effect on behaviour appears to be unreliable and researchers need to explore reasons for why that is. Second, there are a number of prevailing expectations about violent video game effects that remain untested. If the field is to move forward, it is important to experimentally test these intuitions and unstated assumptions. Finally, there are several reasons for investigating video game effects on prosocial behaviour over aggression, namely, the measures are easier to implement from both a legal and ethical standpoint, and from an ease of implementation standpoint. The results of these experiments will also help to address whether public concern for violent video games is warranted.

Overview and predictions

I present the following experiments on the relationship between violent video game exposure and prosocial behaviour. In Chapter 2, I test whether an effect of violent video games can be found on prosocial behaviour, such that those who play a violent game are less likely to help someone else than those who play another type of game. Across several experiments, I test variations of a standard video game effect procedure. Only if the effect of violent video games on prosocial behaviour is consistent across variations would public

concern be warranted. In Chapter 3, I test whether violent video game effects are due to idiosyncrasies in measures and stimuli. I use multiple indices of prosocial behaviour and video games of increasing violence to try and reveal a violent video game effect. Finally, Chapter 4 discusses the 'tipping point' hypothesis of violent video game effects, that is, violent media effects only manifest when participants are in a hostile cognitive state. If the hypothesis is correct, then participants who are exposed to a hostile semantic prime and then asked to play a violent video game ought to be the least prosocial.

Each chapter asks and addresses a particular research question: Chapter 2 asks how resilient is the violent video game effect to changes in experimental procedure; Chapter 3 asks if the violent video game effect is linked to particular idiosyncrasies of measures and stimuli; and Chapter 4 asks whether people are more susceptible to violent video game effects when they are primed with hostility. Each experiment tests the general hypothesis that where there is a violent and non-violent video game comparison, participants who play a violent video game will show reduced or less frequent helping behaviour, compared to participants who play a non-violent video game.

Chapter 2 – Failure to demonstrate that violent video games diminishes prosocial behaviour

Tear, M. J., & Nielsen, M. (2013). Failure to demonstrate that playing violent video games diminishes prosocial behaviour. *PLoS One, 8*(7). doi:10.1371/ journal.pone.0068382

Preface

This chapter is extracted from a published article in the generalist journal *PLoS One*. The motivation for these experiments was driven by a recent failure to demonstrate a negative effect of violent video games on prosocial behaviour, and the belief that there should be an effect to reveal (Greitemeyer & Osswald, 2010). The chapter asks how resilient the violent video game effect is to changes in experimental procedure. The research presented in this chapter explores potential methodological reasons for the effect failing to manifest, including manipulations of video game modernity, exposure times, and experimental context. None of these variations were sufficient for revealing a violent video game effect on prosocial behaviour. The final experiment represents a direct replication attempt of the original experiment, which similarly fails to reveal the effect.

Abstract

Background: Past research has found that playing a classic prosocial video game resulted in heightened prosocial behaviour when compared to a control group, whereas playing a classic violent video game had no effect. Given purported links between violent video games and poor social behaviour, this result is surprising. Here our aim was to assess whether this finding may be due to the specific games used. That is, modern games are experienced differently from classic games (more immersion in virtual environments, more connection with characters, etc.) and it may be that playing violent video games impacts prosocial behaviour only when contemporary versions are used.

Methods and Findings: Experiments 1 and 2 explored the effects of playing contemporary violent, non-violent, and prosocial video games on prosocial behaviour, as measured by the pen-drop task. We found that slight contextual changes in the delivery of the pen-drop task led to different rates of helping but that the type of game played had little effect. Experiment 3 explored this further by using classic games. Again, we found no effect.

Conclusions: We failed to find evidence that playing video games affects prosocial behaviour. Research on the effects of video game play is of significant public interest. It is therefore important that speculation be rigorously tested and findings replicated. Here we fail to substantiate conjecture that playing contemporary violent video games will lead to diminished prosocial behaviour.

Keywords: violent video games, prosocial behaviour, pen-drop task

Introduction

Video games proliferate most contemporary Western cultures and are one of the most commercially consumed forms of media. Indeed, major titles in the video game category regularly outsell the most successful titles in other media formats. For example, according to US and UK sales, the highest grossing video game over five days through to the end of 2011 was Call of Duty: Modern Warfare 3 ($775m). This eclipses the highest grossing movie over a similar period in 2010, The Dark Knight ($203m). However, not only are video games pervasive throughout contemporary culture, they are typically violent in nature and have thus become a target of public concern. Indeed, it is common for the behaviour of those perpetuating extreme acts of violence, such as that by Anders Breivik, James Holmes, and Adam Lanza, to be linked to video game play (though this link is often one of public perception: video games have been falsely attributed in some similar cases, see Yam, 2007). Spurred by this public concern, the past two decades have seen a concerted effort devoted to understanding whether a link between violent video games and real-world behaviour exists.

Because of their violent nature, the vast majority of research into video games has focused on the way game play impacts anti-social behaviour. A recent meta-analysis conducted by Anderson and his colleagues suggested that violent video games increase anti-social behaviour (Anderson et al., 2010). However, the value of that meta-analysis is debated (Ferguson & Kilburn, 2010; Huesmann, 2010), reflecting a wider debate in the literature (see Ferguson, 2013 for a summary). Regardless of which theoretical camp is right, comparatively little research has explored the effects of video games on other outcomes. Prosocial behaviour is one such example. If playing violent video games increases anti-social behaviour it seems reasonable to expect playing will also diminish prosocial behaviour. There is some evidence to support this. Participants who played a violent game, compared to a non-violent game, have been reported to be less likely to cooperate (Sheese & Graziano, 2005), and less likely to reward a confederate (Ballard & Lineberger, 1999). Conversely, studies from two camps of researchers demonstrated that violent video games can even *increase* prosocial behaviour (Bennerstedt, Ivarsson, & Linderoth, 2012; Ferguson & Garza, 2011). Moreover, the impact of playing violent video games is highlighted by findings that playing prosocial games can increase helping behaviour and decrease aggressive outcomes (Greitemeyer & Osswald, 2009). Few studies, however, have directly contrasted the effects of violent and prosocial video games on prosocial behaviour.

In a recent noteworthy article, Greitemeyer and Osswald (2010) demonstrated that video games can have beneficial effects on behaviour, provided the games have prosocial content. Participants played a classic prosocial game (Lemmings, where players must save as many game characters as possible), a classic violent game (Lamers, where players must kill all the characters as quickly as possible), or a classic neutral game (Tetris, where players must arrange shapes to fit together) for 8 minutes and then rated the game on measures of

enjoyment. Following gameplay, participants were presented with the pen-drop test (van Baaren, Holland, Kawakami, & van Knippenberg, 2004; Macrae & Johnston, 1998), where the experimenter accidentally spills some pens onto the floor. Whether the participant helps gather the pens or not is taken as a measure of spontaneous, unrequested assistance. Significantly more participants who played the prosocial game helped gather the pens (67%) than participants who played the violent game (28%) or the neutral game (33%). That is, those who played the prosocial game were more inclined to help pick up the pens. Notably, there was no significant difference between participants who played the violent and neutral games.

There are several explanations for Greitemeyer and Osswald's (2010) failure to find an effect of playing violent video games on prosocial behaviour. One is that participants played games for 8 minutes. This interval may not be long enough to elicit an effect. Indeed, most research with violent video games uses a playing time of 15 minutes or more. Greitemeyer and Osswald themselves comment that longer exposure times should reveal significant differences (p. 215). Furthermore, the 'classic' video games they used may not have been strong enough. Contemporary games are demonstrably more immersive (Schneider, 2004), realistic (Bensley, & van Eenwyk, 2001), and violent (Sherry, 2001), and subsequently require more emotional investment. Modern video game stimuli also vary in terms of competitiveness and difficulty (Adachi & Willoughby, 2011a), and the underlying intentions motivating game play (Przybylski, Rigby, & Ryan, 2010). Indeed, Greitemeyer (2011) speculated that "Modern, graphically sophisticated games may be more involving and thus should affect helping behaviour to a greater extent" (p. 252). Moreover, given public concern, the applied value of using contemporary and, importantly, commercially available video games is potentially more informative and valuable. If violent video games impact on prosocial tendencies, then we need to know if the games people currently play have this effect.

The current set of experiments was designed to explore whether contemporary violent video games lead to decreases in prosocial behaviour. Thus, the aim in Experiment 1 was to extend Greitemeyer and Osswald (2010) using longer exposure times and contemporary video games as stimuli. We included the anti-social video game Grand Theft Auto as our main game of interest, but to assess whether the anti-social nature of the game or the portrayed violence is more important for reducing prosocial behaviour, we included Call of Duty as a violent control. We compared these two violent games to a non-violent and a prosocial video game.

Experiment 1

Participants were exposed to one of four different types of video games: anti-social, violent, non-violent, or prosocial. It was hypothesised that, using contemporary exemplars of

video games, prosocial behaviour would be higher in participants who played a prosocial video game and lower in participants who played the anti-social or violent video game.

Method

Participants

Sixty-four undergraduate students (56% male) at a large metropolitan university (age range 17-33, *M*=20.30, *SD*=3.61) took part in Experiment 1 for course credit. The sample size was determined by a priori power analysis on the basis of both the cell (*n*=18) and effect sizes (*w*=.35) reported by Greitemeyer and Osswald (Experiment 1; 2010). All experiments in this chapter use this 'cell size equals 18' rule-of-thumb. Participants were mostly Caucasian (88%) with a minority reporting Asian ethnicity (12%). Participants gave written informed consent to participate in the experiment. Ethical clearance was granted by the Behavioural & Social Sciences Ethical Review Committee at the University of Queensland.

Video games

We note here that it is difficult to dichotomise games as either solely violent or prosocial since many violent games include prosocial themes (e.g. killing villains to save the world, see Ferguson & Garza, 2011). We tried to circumvent this by including games where players could engage in only violent or prosocial actions (e.g. killing zombies/attacking police vs. taking care of an animal). Participants played one of the following four video games:

1) Anti-social (Grand Theft Auto IV). Grand Theft Auto is an open-world sandbox game, meaning participants can adopt a non-linear style of playing and explore their environment. To ensure participants engaged in aggressive behaviours in the game we made all the in-game weapons (e.g. handguns, rifles, rocket launchers, etc.) available at the start of the session. Grand Theft Auto was included as an exemplar of an anti-social game. Since, intent is an important component of the standard definition of aggression (Baron & Richardson, 1994; Tedeschi & Quigley, 1996), we distinguish between styles of violence; morally defensible and indefensible violence. When playing the violent game exemplar described below (Call of Duty), players engaged in violent acts to preserve the lives of themselves and others (morally defensible). In Grand Theft Auto, however, players engaged in violence towards other members of a society often for no defensible reason, for example, stealing cars, damaging property, running over innocent civilians, running away from and killing police. The intent of this violence is not related to self-preservation or any other in-game objectives, it is entirely for the sake of being violent (morally-indefensible).

2) Violent (Call of Duty: Black Ops). Call of Duty was selected as a violent control game. In Call of Duty, which is a first-person shooter game, players assumed the role of various soldiers who wield firearms and explosives, and can engage in close quarters combat. Participants played the 'zombie' mode, where they needed to simultaneously solve puzzles to progress through a series of rooms, while also killing zombies with a variety of guns and

weapons. As previously mentioned, the violence in Call of Duty reflects a morally defensible intent to survive, or avoid death. Many games that could be considered violent employ a similar style of 'self-defence violence' (killing others to avoid being killed). To this end, Call of Duty served as a violent control to the deliberately anti-social content of Grand Theft Auto. Call of Duty qualified as a violent game because the zombie deaths were often quite extreme and grotesque (e.g. zombie corpses could be blown apart). We also selected the zombie mode because the gameplay was reasonably linear, meaning that each participant had a similar experience while playing the game.

3) Prosocial (World of Zoo). In World of Zoo players needed to create a successful zoo exhibit, which was achieved by taking care of animals by feeding, cleaning, and playing with them. Unlike the other games described here, World of Zoo is not explicitly marketed towards adults. It is, however, one of the few commercially available games that requires prosocial behaviour and does not contain violent or adult themes.

4) Non-violent (Portal 2). This is a non-violent puzzle game where the player used a gun that shoots entry and exit points of a portal, allowing them to access areas they would not normally be able to. This game acted as a non-violent control condition because, like Call of Duty, the player uses a gun-shaped tool to interact with the virtual world. The gun in Portal 2, however, shoots portals instead of bullets.

Procedure and design

The experimental procedure is described in text below and visually represented in Figure 1. Participants were informed that the session would comprise two unrelated studies that were bundled together in the interest of time. The first study was described as a pilot study that asked for participants' reactions to pre-selected games so we could assess their suitability as stimuli for future experiments. Games were played on a Sony Playstation 3 console, using a Sony 40" LCD TV. Participants were randomly assigned to play one of the four previously outlined games for 20 minutes and then asked to fill out a questionnaire ostensibly aimed at assessing their experience of playing the game. This questionnaire contained questions regarding the participant's levels of interest, frustration, and arousal experienced, and was used to test whether the games differed on factors other than the presence or absence of violent content. The questionnaire contained 12 Likert items (1: strongly disagree, to 9: strongly agree, example items: the game was too hard, the game got my heart racing, the game kept my attention). The second study was described as a distinct investigation of social attitudes. To this end, participants were asked to complete a series of questionnaires unrelated to the current study to reinforce the ostensible reason for the second study and to minimize suspicion about the true hypothesis. Once participants finished filling out these questionnaires the experimenter said he had to rush to the other side of campus for an ostensible appointment he had forgotten, and that he would debrief the participant via email. The experimenter gathered his belongings, namely some folders, some pens balanced on the

folders, and a cup of coffee, all to emphasise that he was fully laden. He then opened the door and, as the participant moved past the experimenter, 'accidentally' tipped the folders such that the pens fell to the floor, muttering under his breath as he did so. As per Greitemeyer and Osswald (2010), the experimenter waited five seconds for the participants to help pick up the pens. The participant was considered to have acted prosocially if he/she helped pick up at least one pen (some pens would land in a way that would not make sense for the participant to pick up). Once the pens had been gathered and the participant was on his/her way, the experimenter called the participant back into the room where he/she was probed for suspicion and debriefed. Neither in this experiment nor any of the subsequent experiments did any participant report suspecting the true aim of the experiment. Similarly, here, and in all subsequent experiments, the experimenter was not blind to experimental conditions. For the sake of brevity we do not mention this again.

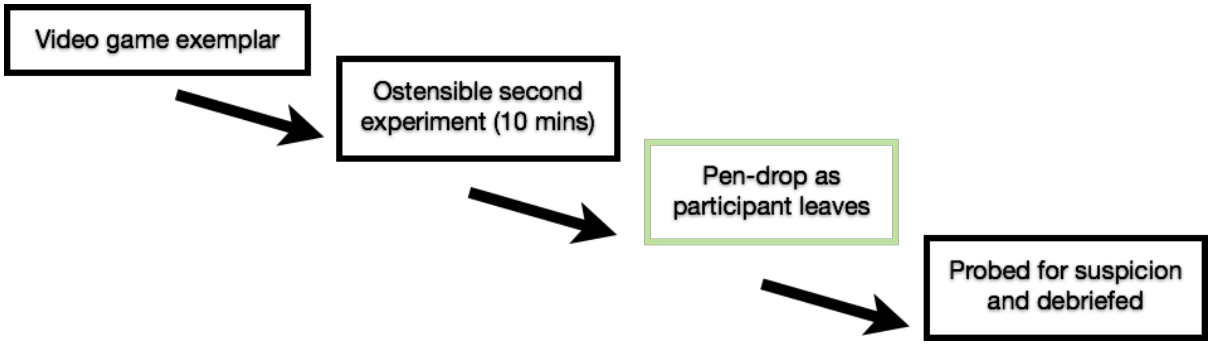


Figure 1. Experimental procedure for testing the effects of video game content on participants' propensity help gather spilt pens.

Results and Discussion

Table 1 shows the number of participants who helped (and did not help) by condition. Here it can be seen that there are small differences in prosocial behaviour between conditions. A Chi-Square analysis was unfeasible given that some cells contained less than 5 cases. We opted to use Fisher's Exact Tests to determine the feasibility of collapsing across similar conditions. First, we compared the games with violence (anti-social vs. violent, p=.220, two-tailed) and the games without violence (prosocial vs. non-violent, p=.685, two-tailed). Since there was no difference between similar games, and in order to increase power, we then collapsed the conditions into two broad categories (Violence-Present vs. Violence-Absent) and calculated a Chi-Square. The test on the collapsed data found no difference in prosocial behaviour between Violence-Present games and the Violence-Absent games, χ^2 (1, N=64)=0.00, p=>.999, V=0.00.

Table 1.

Frequency of prosocial behaviour across video game conditions in Experiment 1. Percentages in parentheses.

Video game condition	Behvioural outcome	
	Help	No help
Anti-social	2 (12.5)	14 (87.5)
Violent	6 (37.5)	10 (62.5)
Prosocial	3 (18.8)	13 (81.3)
Non-violent	5 (31.3)	11 (68.8)

For all experiments we report whether the self-report measures differed according to levels of the video game type variable, and follow up significant differences with hierarchical logistic regressions to determine if the self-report measures predict variance above and beyond video game type.

In experiment 1, a series of one-way ANOVAs established that our video game stimuli differed beyond presence or absence of violence. Analyses revealed a main effect of self-reported frustration, $F(1, 3)=3.72$, $p=.016$. Follow-up analyses showed that the violent game was significantly more frustrating ($M=5.13$, $SD=1.60$) than the non-violent game ($M=3.52$, $SD=1.47$) and the prosocial game ($M=3.69$, $SD=1.05$), ps$<.010$, but not the anti-social game ($M=4.30$, $SD=1.81$), $p=.126$.

There was also a main effect of self-reported arousal, $F(1, 3)=13.23$, $p<.001$. The prosocial game was significantly less arousing ($M=3.30$, $SD=1.40$) than all of the other games; violent ($M=6.03$, $SD=1.17$), anti-social ($M=5.36$, $SD=1.64$), and non-violent ($M=5.63$, $SD=1.09$), ps$<.001$.

Finally, the games differed on self-reported interest, $F(1, 3)=14.96$, $p<.001$. Participants found the non-violent game significantly more interesting ($M=7.59$, $SD=0.92$) than all of the other games; violent ($M=6.09$, $SD=1.66$), anti-social ($M=6.15$, $SD=1.02$), and pro-social ($M=5.26$, $SD=1.53$), ps$<.003$.

We conducted a hierarchical logistic regression to determine if the self-report variables could account for additional variance in prosocial behaviour above and beyond the video game manipulation. We entered the video game variable at Step 1 and the three self-report measures at Step 2. Maximum likelihood estimates are represented by pseudo R^2 statistics, such as Nagelkerke R^2 – 'pseudo' in that they are designed to mimic the goodness-of-fit interpretation of R^2 in ordinary least squares regression. As a set of predictors, the self-report measures do not account for additional variance above and beyond the video game

manipulation, Nagelkerke R^2=.13, $\chi^2(3, N=64)=2.42$, p=.491. None of the three self report measures were significantly linked to prosocial behaviour, Wald tests<.88, ps>.348.

Greitemeyer and Osswald (2010) previously demonstrated that playing a prosocial video game led participants to be more likely to engage in spontaneous, unrequested helping behaviour whereas playing a violent game showed no impact. Here we were unable to replicate this finding of improved performance for participants in the prosocial game condition. Moreover, despite extending the playing time to 20mins and using commercially available, contemporary games, we also failed to show a reduction in prosocial behaviour from playing violent games. An initial interpretation of our results might suggest we have simply found a baseline rate of helping in our population. Studies using the pen-drop task report baseline rates of around 30% of participants helping to pick up the pens (van Baaren et al., 2004; Greitemeyer & Osswald, 2010). It is thus possible that our stimuli were not potent enough to elicit a primed response. We find this unlikely given that effects of violent games have been shown with much simpler games (Anderson & Dill, 2000; Bushman & Anderson, 2002; Carnagey, Anderson, & Bushman, 2007; Greitemeyer & Osswald, 2010). Furthermore, the entire basis for using contemporary games over classic games is that they typically offer a much more enriched experience so intuition would posit the effect should be stronger for contemporary games. An alternative explanation is the timing of the pen-drop task. In order to avoid arousing suspicion from participants we inserted other tasks between game play and the test for them to complete. This may have inadvertently biased our protocol against revealing an effect by diluting the impact of the games (or by removing blatant demand characteristics). We attempted to remedy this in Experiment 2.

The prosocial game World of Zoo was marketed primarily as a children's game and, thus, inherently differed from the other games in more ways than just the presence of violent/prosocial content (e.g. significantly less arousing), a problem acknowledged in other research (Adachi & Willoughby, 2011a; Przybylski et al., 2010). Since we are interested in why Greitemeyer and Osswald (2010) could not show a detrimental effect of violent games beyond non-violent controls, we decided to omit World of Zoo (the prosocial game) from subsequent testing. Further, in Experiment 1 we included Call of Duty to control for the type of violent content (morally defensible vs. morally indefensible). As there was no statistical difference in performance between Call of Duty and Grand Theft Auto we omitted the former in subsequent testing.

Experiment 2

In Experiment 1, participants filled out questionnaires directly after playing the game, whereas in Greitemeyer and Osswald (2010) the pen-drop task happened immediately following play. The filler questionnaires in Experiment 1 took anywhere between five and ten minutes, and past literature has shown that filler tasks can nullify the violent video game effect (Sestir & Bartholow, 2010), though it could also be argued that filler tasks remove

blatant demand characteristics in violent video game studies. Further, the pens were dropped as participants left the room, whereas Greitemeyer and Osswald did so halfway through the experimental session, necessitating further interaction between the participant and experimenter. In Experiment 2 we, therefore, manipulated the administration of the pen-drop task to bring the test phase closer to the video game prime by either feigning the end of the session or by administering the pen-drop during the middle of the session.

Method

Participants

We recruited 64 undergraduate participants (55% male) from the first-year participant pool at a large metropolitan university. Participant ages ranged from 17-43 (M=21.63, SD=5.50). Most were Caucasian (77%), though some reported Asian ethnicity (14%), or other (9%). Participants provided written informed consent and either received course credit for participating in the experiment or a small monetary reimbursement. Ethical clearance was granted by the Behavioural & Social Sciences Ethical Review Committee at the University of Queensland.

Video games

Given our continued focus on using contemporary games to demonstrate the violent video game effect and the difficulty we had in procuring an adult-oriented prosocial video game, we only used two games in Experiment 2; Grand Theft Auto (anti-social) and Portal 2 (non-violent).

Procedure and Design

As per Experiment 1, participants were instructed that the experimental session comprised two ostensibly unrelated studies; the first to gather participants' opinions of pre-selected games to determine whether they would be suitable stimuli for a future experiment, and the second to pilot test various measures of social attitude. Games were played on a Sony Playstation 3 console, using a Sony 40" LCD TV. Participants were randomly allocated to play either Grand Theft Auto or Portal 2 for 20 minutes. Following the game, and to keep the story of the first ostensible study believable, the participant rated the games on the same measures as in Experiment 1. This was the only task the participant completed between the video game and pen-drop task, taking no more than 60 seconds (12 items). We deemed this short questionnaire necessary for the cover story of the first study.

We then manipulated the context in which the pen-drop task was administered; having it follow the video game prime and ending the session there (Session-Ends), or following the video game prime but with the session continuing into the second ostensible experiment (Session-Continues).

In the Session-Ends condition, following the participant completing the ostensible first study, the experimenter 'realised' that he forgot to bring the materials for the second study

and would have to end the experimental session early. As in Experiment 1, the experimenter gathered his belongings, making him appear sufficiently laden, and opened the door for the participant. The experimenter dropped the pens as the participant moved past him, waiting 5s for the participant to help gather the pens. Participants were then probed for suspicion and debriefed.

In the Session-Continues condition, participants finished the questionnaire for the ostensible first study and handed it to the experimenter. The experimenter then reached for the materials for the ostensible second study, knocking over a tin of pens placed at the end of a table, equidistant from both the experimenter and participant. The experimenter waited 5s for the participant's reaction; did they pick up the pens or not? Once the pens had been gathered the experimenter began the second study. Participants completed the second study, were probed for suspicion, and then debriefed. The full experimental procedure is visually represented in Figure 2.

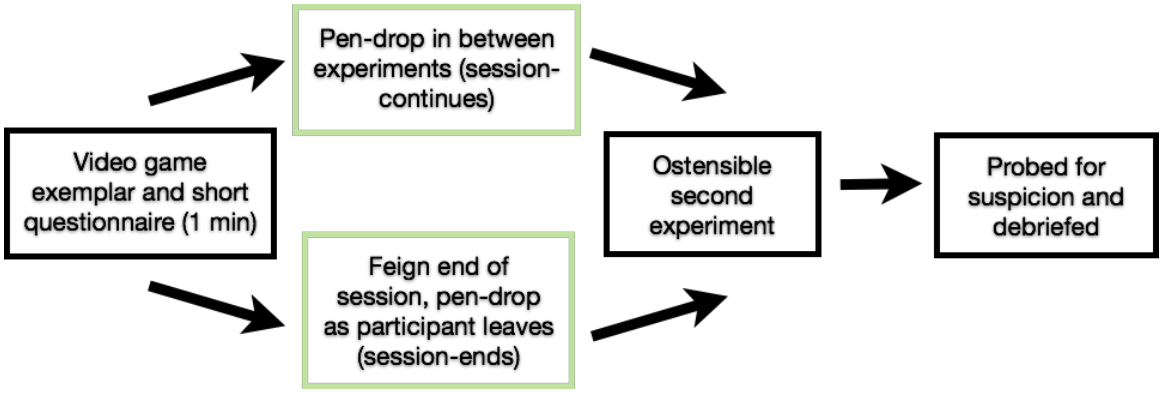


Figure 2. Experimental procedure for testing the effects of video game content and timing of the test-phase on participants' propensity to help gather spilt pens.

Results and Discussion

Table 2 shows the rates of helping (or not) for both games across both timing conditions. To investigate the effect of the relative timing of the pen-drop we conducted a Chi-Square analysis to assess whether there was a main effect of timing (Session-Ends vs. Session-Continues). The test showed that a greater proportion of participants helped when the pens spilled in the middle of the experimental session compared to the proportion who helped when the experimental session was thought to end, χ^2 (1, N=64)=12.30, p<.001, V=.438.

Table 2.

Frequency of prosocial behaviour across video game and timing conditions in Experiment 2. Percentages in parentheses.

Timing condition	Video game condition	Behvioural outcome	
		Help	No help
Session-Ends	Anti-social	6 (37.5)	10 (62.5)
	Non-violent	4 (25.0)	12 (75.0)
Session-Continues	Anti-social	14 (87.5)	2 (12.5)
	Non-violent	10 (62.5)	6 (37.5)

Within each level of the timing variable we conducted logistic regressions to see whether the rate of helping differed according to the game played. The type of video game played did not have an effect in either the Session-Ends condition (anti-social: 38%, non-violent: 25%), Nagelkerke R^2=.03, $\chi^2(1, N=32)=0.59$, p=.444 or the Session-Continues condition (anti-social: 87%, non-violent: 63%), Nagelkerke R^2=.12, χ^2 $(1, N=32)=2.76$, p=.096.

We ran t-tests to determine if the video games were experienced differently. Between the two games, we found no differences in terms of frustration ($t(62)=0.13$, p=.895), arousal ($t(62)=1.04$, p=.300), or interest ($t(62)=0.50$, p=.617).

It appears that neither arrangement of the pen-drop task in Experiment 2 was sufficient to elicit the violent video game effect. There was no main effect of game type: the number of participants helping was statistically equal across the two games (anti-social: 63%; non-violent: 44%). There was, however, a main effect of the context in which the pen-drop was administered: rate of helping was significantly higher in the Session-Continues condition (75%) than in the Session-Ends condition (31%).

In order to account for possible lack of power given the relatively small cell sizes used here and in Experiment 1, we collapsed the Session-Ends data with the anti-social and non-violent data from Experiment 1. Despite doubling participant numbers, differences in rates of helping between anti-social and non-violent conditions remained non-significant, χ^2 $(1, N=64)=0.80$, p=.777, V=.035.

Experiment 2 was designed to evaluate whether manipulating the contextual administration of the pen-drop task would help reveal an effect of playing an anti-social game. While subtle contextual differences in the administration of the pen-drop task are able to move base-rates of helping behaviour, they were not sufficient for revealing the anticipated

violent video game effect. Given the failure to show an effect of violent video games, it was necessary to attempt a procedural replication of Greitemeyer and Osswald (2010).

Experiment 3

In Experiments 1 and 2 we failed to find any effect of playing a violent video game on prosocial behaviour. It is conceivable that the motive for violence in classic games is much less ambiguous than contemporary games (e.g. killing creatures to prevent them for winning is more obviously violent than killing enemies to win a politicised and emotional war). Thus, we decided to replicate Greitemeyer and Osswald (2010), using classic games (Lamers and Lemmings), decreasing our exposure time to 8 minutes, and administering the pen-drop in the Session-Continues style of Experiment 2.

Method

Participants

We recruited 32 undergraduate participants (66% male) from the first-year participant pool at a large metropolitan university. Participant ages ranged from 17-26 (M=19.5, SD=2.29). Again, most were Caucasian (81%), with small minorities reporting Asian ethnicity (16%), or other (3%). Participants provided written informed consent and received course credit for participating in the experiment. Ethical clearance was granted by the Behavioural & Social Sciences Ethical Review Committee at the University of Queensland.

Video games

Following Greitemeyer and Osswald (2010), participants in Experiment 3 were randomly assigned to play one of the following two games:

1) Prosocial (Lemmings). The general prosocial aim of this game is to prevent a colony of lemmings from mindlessly marching over cliff edges or into hazards by assigning them useful roles (e.g. assigning a parachute role so they do not fall to their death). This gets progressively more taxing as players proceed through the increasingly difficult levels. A player's score is determined by how many lemmings they save from mindless self-death.

2) Violent (Lamers). Lamers is a violent parody of Lemmings, with the goal being to kill as many of the characters as possible before they reach their goal. Players have various weapons at their disposal, including guns and explosives.

Procedure and design

Following Greitemeyer and Osswald (2010), and as per Experiment 2, we administered the pen-drop in the Session-Continues form. Again, this experiment was run as two ostensibly unrelated studies bundled together to make best use of time. Games were played on a Sony Playstation 3 console, using a Sony 40" LCD TV. Participants were randomly assigned to play the prosocial (Lemmings) or violent (Lamers) game. After participants played the video game they were asked to fill out a questionnaire gauging their reactions to

the game and, once they finished, the experimenter reached for the materials for the second study before knocking over a tin of pens placed equidistant from both the experimenter and participant. As with the previous experiments, the experimenter waited 5s to see if the participant would help gather the spilt pens. Once the pens had been gathered the experimenter began the second study, after which participants were probed for suspicion and debriefed.

Results and Discussion

Here we adopted the exact protocol reported by Greitemeyer and Osswald (2010), that is, we used classic exemplars of violent and prosocial games, decreased the exposure time to 8 mins, and adopted the Session-Continues form of the pen-drop task, yet we were still unable to show any detrimental effect of violent games on prosocial behaviour, χ^2 (1, N=32)=0.53, p=.716, V=.129.

Again, we conducted a series of t-tests to determine whether our video game stimuli differed on potential variables of interest. As with Experiment 2, we found no difference between the video game stimuli on frustration (t(30)=0.88, p=.387), arousal (t(30)=1.10, p=.282), or interest (t(30)=0.80, p=.430).

The descriptive statistics in Table 3 show that the helping rates were very similar across game conditions. Even if the motives for violence in classic video games are less ambiguous than the motives for violence in contemporary video games, then it appears to have no bearing on prosocial behaviour. We were unable to demonstrate that classic violent and prosocial games prime different rates of prosocial behaviour, measured using the pen-drop task.

Table 3.

Frequency of prosocial behaviour across video game conditions in Experiment 3. Percentages in parentheses.

Video game condition	Behavioural outcome	
	Help	No help
Violent	11 (68.8)	5 (31.2)
Prosocial	9 (56.3)	7 (43.7)

General Discussion

Three experiments failed to find a detrimental effect of violent video games on prosocial behaviour, despite using contemporary and classic games, delayed and immediate test-

phases, and short and long exposures (results summarised in Table 4). While this study is not definitive evidence that violent video games have no detrimental effect on prosocial behaviour, it might be that previously raised concerns regarding the impact of violent games on prosocial behaviour may be mismatched or disproportionate. In this study, the context in which the prosocial task was administered had more influence over whether participants helped or not than did the type of video game they played. These findings may be viewed as being in line with previous research that has similarly failed to demonstrate a detrimental effect of violent video games on prosocial behaviour (e.g. Colwell & Kato, 2003).

Table 4.

Summary statistics from Chapter 1 experiments.

Experiment	Comparison	Behavioural outcome	χ^2	N (total)	*p*
1 – Contemporary games, long exposure	Violence-Present vs. Violence-Absent	Help vs. no help	< 0.01	64	>.999
2 – Contemporary games, long exposure, *delayed test-phase*	Anti-social vs. Non-violent	Help vs. no help	0.59	32	.444
2 – Contemporary games, long exposure, *immediate test-phase*	Anti-social vs. Non-violent	Help vs. no help	2.76	32	.096
3 – Replication; classic games, short exposure	Violent vs. Prosocial	Help vs. no help	0.53	32	.716

Experiments 1 and 2 were conceptual replications, designed to extend the basic finding reported by Greitemeyer and Osswald (2010) using contemporary video games, while Experiment 3 was a more precise replication using classic games. Across all three experiments we could not find a decrease in prosocial behaviour. We followed suggestions by Greitemeyer and Osswald but it seems that previously intuitive, yet untested, ideas that longer exposures and contemporary games should elicit stronger effects on behaviour do not hold.

We concede that our failure to find an effect may be due to the relatively small cell sizes reported in each experiment (16 participants per cell). We find this an unlikely reason for our failure to replicate, however, given that past research, including Greitemeyer and Osswald

(2010), used similar cell sizes (van Baaren et al., 2004; Macrae & Johnston, 1998). We further reject the poor power criticism of these experiments because in each the effect sizes are very small, ranging from essentially 0 to .188. Indeed, in order to have sufficient cell sizes to reject any of the null hypotheses reported here, the number of participants needed in each experiment would range between 223 (for an effect size of .188, reported in Experiment 2) and 12 559 (for an effect size of .025, reported in Experiment 2). Not only would this be impractical but, given the small effect sizes, the applied value of any significant result would need to be called into question.

In order to further address criticisms of power, one can conduct a meta-analysis. Meta-analysis creates an estimate of effect size across several samples by combining samples to increase sample size and, thus, arrive at a more accurate estimate of the true effect size. Thus, we ran a simple meta-analysis on the main 'violent vs. non-violent' comparisons in Chapter 2. Specifically, the 'Violence-Present' vs. 'Violence-Absent' comparison in the first experiment, the 'Anti-social' vs. 'Non-violent' comparisons for both Session-Ends and Session-Continues scenarios in the second experiment, and 'Violent' vs. 'Prosocial' comparison in the final experiment. There are multiple types of effect size, but *r* is generally considered the easiest to interpret (Rosenthal & DiMatteo, 2001). Thus, χ^2 statistics were converted to *r* (Lyons, 1998) and effects were pooled to derive an estimate of the meta-analytic effect size. Meta-analysis of the four samples, with a combined *N* of 160, suggests a population *r* of .150, $p = .290$. The result confirms a small and non-significant meta-analytic effect size across the four samples.

Other criticisms could be aimed at our choice of stimuli. First, we only selected single exemplars of contemporary video games to represent each game category. Other studies use multiple exemplars or even pilot test multiple exemplars, finally settling on two that score similarly on experience measures (e.g. interest, frustration, or arousal, see Anderson & Dill, 2000). We defend our decision to use single exemplars by highlighting that the games we chose were both popular and commercially available at the time of data collection, which is where the value of this research lies. It remains possible, however, that unexamined unique characteristics of the games may have inhibited an effect of violent games on prosocial behaviour (Adachi & Willoughby, 2011a; Ferguson & Rueda, 2010; Przybylski et al., 2010). Second, it is possible that our failure to find an effect of violent games is due to participants not recognising the violent nature of the games. However, this seems highly unlikely. Grand Theft Auto IV is rated MA15+ according to the Australian Classification Board because it contains strong violence that is relatively frequent and strong in playing impact. Moreover, this game is used as a violent exemplar in other research (Bowen & Spaniol, 2011; Chong, Teng, Siew, & Skoric, 2012) with Chong and colleagues stating that the game world of Grand Theft Auto IV is "fraught with violence and players are rewarded and reinforced for their use of violence in order to advance in the game" (p.962). It remains possible, however, that the failure to demonstrate a benefit of playing prosocial games may be due to ambiguity

of prosocial behaviour in the selected prosocial game. It is not entirely clear whether World of Zoo is perceived as having prosocial content.

Further, we believe that the reported null findings are important, given that the current climate in social psychology is geared towards replication of classic findings (for a wide review of the current climate see Nosek & Bar-Anan, 2012 and associated commentaries, as well as Ferguson & Heene, 2012), with recent failures to replicate calling into question the legitimacy of widely regarded effects in social psychology (Doyen, Klein, Pichon, & Cleeremans, 2012; Pashler, Coburn, & Harris, 2012; Shanks, et al., 2013). It is well known that novel and surprising findings are more prone to publication bias (Ioannidis, 2005) and likely to be false-positives (Simmons, Nelson, & Simonsohn, 2011). Unfortunately, this leads to null results being viewed as less interesting because they are often unfairly labelled as "difficult to interpret" (Ferguson & Heene, 2012). Of course, if null results are never reported, then we are only seeing a partial account of the true nature of any given effect. The Australian Government has even criticised research practices in the field for failing to include null effects in meta-analyses (Australian Government, Attorney General's Department, 2010). Given these pitfalls of scientific communication, methodical replication is paramount to the academic integrity of the field. We believe that our findings are a step in the right direction towards rebuilding that integrity.

Finally, there is some recent evidence showing that prosocial behaviour towards strangers (compared to friends or family) is most strongly affected by violent video game habits (mediated by decreased empathic concern) (Fraser, Padilla-Walker, Coyne, Nelson, & Stockdale, 2012). Given that the experimenter had never met any participants prior to test, we can speculate that prosocial behaviour should have been lower after playing a violent game. Considering this and our other attempts at creating optimal circumstances for the effect to reveal itself, we further speculate that the concern over the effect of violent video games is mismatched. Of course, Greitemeyer and Osswald (2010) used multiple measures of prosocial behaviour and the failure to replicate with one measure should not discredit their work. To this end, it is important that further work attempts to explore the effect that violent video games might have on prosocial behaviour with multiple measures and different stimuli. However, it remains possible that, in terms of impact on prosocial behaviour, public concern over violent video game play should be minimal.

Chapter 3 – Violent video games and prosocial behaviour: A study of the effects of non-violent, violent, and ultra-violent gameplay

Tear, M. J., & Nielsen, M. (2014). Video games and prosocial behaviour: A study of the effects of non-violent, violent and ultra-violent gameplay. *Computers in Human Behaviour*, *41*, 8-13. doi:10.1016/j.chb.2014.09.002

Preface

This chapter is an extracted article in the journal *Computers in Human Behaviour*. In this experiment I further explored the inconsistency in the literature regarding violent video game effects on prosocial behaviour. This chapter asks if the violent video game effect is linked to particular idiosyncrasies of measures and stimuli. To rule out any idiosyncratic effects of prosocial measures I collected data using several indices of prosocial behaviour. There are also possible concerns regarding the use of single exemplars to represent video game categories, so I used multiple games within each video game category. The resulting data did not show any pattern to suggest that playing violent or ultra-violent video games affected participants' behaviour any differently than non-violent video games. Thus, on the basis of the results reported here and in Chapter 2, it appears that any violent video game effect is fleeting.

Abstract

Experimental evidence has pointed towards a negative effect of violent video games on social behaviour. Given that the availability and presence of video games is pervasive, negative effects from playing them have potentially large implications for public policy. It is, therefore, important that violent video game effects are thoroughly and experimentally explored, with the current experiment focusing on prosocial behaviour. 120 undergraduate volunteers (M_{age}=19.01, 87.5% male) played an ultra-violent, violent, or non-violent video game and were then assessed on two distinct measures of prosocial behaviour: how much they donated to a charity and how difficult they set a task for an ostensible participant. It was hypothesised that participants playing the ultra-violent games would show the least prosocial behaviour and those playing the non-violent game would show the most. These hypotheses were not supported, with participants responding in similar ways, regardless of the type of game played. While null effects are difficult to interpret, samples of this nature (undergraduate volunteers, high male skew) may be problematic, and participants were possibly sensitive to the hypothesis at some level, this experiment adds to the growing body of evidence suggesting that violent video game effects are less clear than initially thought.

Keywords: video game violence, prosocial, social behaviour, behavioural priming

Introduction

Contemporary Western culture is saturated by multiple forms of media and their concomitant impact on society has hence been a popular topic of research for several decades. As a result of this research endeavour, few would question that the effect of media is a profound one. At the societal level, media campaigns are used to influence behaviour in a number of ways, from purchase decisions (Milner & Rosenstreich, 2013) to health behaviour (Wakefield, Loken, & Hornick, 2010). Indeed media, in one form or another, can be used for benefit and can cause harm. It is the latter, and particularly the influence of media violence, which has dominated much of the experimental research on media effects (Anderson et al., 2003).

Concern with violent media arose from the mass media explosion of the 20th century (Bushman & Anderson, 2001), leading researchers to investigate the impact of violence in a variety of media forms, including table-top games (Martin & Fine, 1991), pornography (Malamuth & Briere, 1986), and television (Eron, Huesmann, Lefkowitz, & Walder, 1972). With the advent of personal computers and video game consoles and their subsequent proliferation, much of the violent media research turned its focus toward video games, with many early experiments (Anderson & Dill, 2000) and reviews (Dill & Dill, 1999; Griffiths, 1999) finding detrimental effects. For example, participants who played a violent video game were more likely to deliver noxious stimuli to a (fictitious) partner than a participant who played a non-violent video game (Anderson & Dill, 2000). Subsequent studies have shown that after playing violent video games, participants expect greater hostility from characters in a vignette (Bushman & Anderson, 2002), have greater access to aggressive cognitions (Anderson et al., 2004), and are quicker to associate their self-concept with aggression (Uhlmann & Swanson, 2004). Given consistent reports that violent video games increase anti-social behaviour (aggression included) (although see Ferguson, 2013), it seems reasonable to expect that violent video games will decrease prosocial behaviour, that is, behaviour intended to help others (Gentile, et al., 2009).

Unfortunately, the violent video game literature on prosocial behaviour is limited and somewhat contradictory. Seminal work by Chambers and Ascione (1987) was among the first to demonstrate that violent video games can reduce prosocial behaviour by showing that children who played a violent video game donated less to charity than those who played a prosocial game (although results for another behavioural helping measure were not significant and, sadly, not reported). Subsequent experiments found that participants who played a violent game, compared to a non-violent game, were less likely to reward a confederate (Ballard & Lineberger, 1999), and less likely to cooperate (Sheese & Graziano, 2005). However, an experiment conducted by Greitemeyer and Osswald (2010) failed to reveal an expected detrimental effect of violent video games on prosocial behaviour (indeed, the strength and direction of this relationship is empirically contested see Ferguson & Garza,

2011; Valadez & Ferguson, 2012). In Greitemeyer and Osswald's experiment, the researcher asked participants to play either a violent, non-violent, or prosocial video game, and then surreptitiously spilled a handful of pens on the ground, before observing whether the participants helped gather the spilt pens. Participants who had played the prosocial video game picked up more pens than those who played a violent or non-violent video game. Importantly, there was no difference in frequency of helping between the violent and non-violent games.

Tear and Nielsen (2013) explored potential reasons for this failure to demonstrate that violent games decrease prosocial behaviour, compared to non-violent games. Adapting the pen-drop task used by Greitemeyer and Osswald (2010), Tear and Nielsen used contemporary and classic games, delayed and immediate test-phases, and short and long exposures in order to create optimal conditions for revealing a decrease in prosocial behaviour following violent video game play. In none of their experiments, were Tear and Nielsen able to show that playing violent video games diminished prosocial behaviour. While these findings, coupled with those of Greitemeyer and Osswald, suggest playing violent video games does not impact prosocial behaviour, they are based on participants' responses to just one task (the pen-drop task). Since the notion that violent video games should decrease prosocial behaviour seems intuitive, and persistent (Greitemeyer, 2011, p.252; Greitemeyer & Osswald, 2010, p.215), we sought to run an experiment using other measures of prosocial behaviour and observe whether the expected video game effect would reveal itself.

Here, we used two established measures of prosocial behaviour: (1) a charity donation task, used in several other domains, such as behavioural mimicry (van Baaren, Holland, Kawakami, & van Knippenberg, 2004), social priming (Garcia, Weaver, Moskowitz, & Darley, 2002), and social preferences (Levitt & List, 2007); and (2) the tangram task, used in past violent video game research (Gentile et al., 2009; Saleem, Anderson, & Gentile, 2012). To militate against any potential findings being attributable to the idiosyncratic influence of one game it is also important that research does not rely on only one exemplar per game category. Moreover, if violent video games impact on prosocial behaviour it would be reasonable to expect that stronger effects would be found with more violent games. We thus employed two distinct violent video game conditions: One in which participants played games rated as suitable for and legally saleable only to those aged 15 years and older and more graphic versions of the same games rated as suitable for and legally saleable only to those aged 18 years or older. Assuming an incremental effect of violence, that is the more violent the game the stronger the effect (Barlett, Harris, & Bruey, 2008; Farrar, Krcmar, & Nowak, 2006), it was hypothesised that participants who played the ultra-violent games would donate least to charity, and assign the most difficult tangrams, while those who played a non-violent game would donate most to charity, and assign the least difficult tangrams.

Method

Participants

Participants were 120 undergraduate students from a first-year introductory psychology course at a large metropolitan university (87.5% male, M_{age}=19.01, SD_{age}=2.72). Most participants self-reported as Caucasian (67.5%), with a minority reporting as Asian (25%) or Other (7.5%). In past experiments we found that participants with no video game experience often struggled to grasp basic mechanics essential for playing the games. To overcome this problem we recruited participants who played games at least once a week, which may explain the skew towards male participants. In this context, it is notable that Gentile and colleagues (2014) suggest it remains unclear what role gender has in violent video game effects. Ethical clearance was granted by the Behavioural & Social Sciences Ethical Review Committee at the University of Queensland.

Video game stimuli

Games were played on a Sony Playstation 3 console, using a Sony 40" LCD TV. Two games were chosen for each category in order to avoid the possibility of an effect being tied to the idiosyncrasies of one particular game. For the violent games (violent and ultra-violent) we elected to use games from the same franchise in order to evaluate the relative impact of increased violence while keeping other factors (e.g. game mechanics, pace, characterisations) relatively constant. In terms of varying the amount of violence, video game classification provides a useful, although imperfect, distinction between levels of violence. To this end, we chose a violent game franchise (e.g. Mortal Kombat) and picked exemplars from that franchise (e.g. Mortal Kombat vs. DC Universe, rated as suitable for those over 15 years, and Mortal Kombat: Komplete Edition, restricted to adults). Descriptions of the games we used follow.

Non-violent games (Portal 2 and Modnation Racers). Portal 2 is a non-violent puzzle game, where players use a gun that shoots entry and exit points of a portal, allowing them to access areas they would not normally be able to. Thus, as is common in violent games, the player uses a gun-shaped tool to interact with the virtual world. Except, instead of firing bullets, the gun in Portal 2 shoots portals. Modnation Racers is a non-violent racing game, where the player competes against several other computer controlled characters in a race around a circuit. Players can earn boosts by performing tricks while racing. They can also interfere with their opponents' race by picking up items (e.g. a green beam that slows their nearest opponent).

Violent games (God of War 3, Mortal Kombat vs. DC Universe). God of War 3 is a violent combat game set in the mythology of ancient Greece, where the player uses a variety of hand-weapons (blades, knives, mauls) to slay large quantities of non-human enemies. Participants played the combat arena mode, which pits him or her against a selected enemy

type. Players must utilise a variety of weapons and techniques to slay their enemies and avoid death. Mortal Kombat vs. DC Universe is a fighting game where players select a fighter and engage in a series of one-to-one matches against computer-controlled opponents. Players win these fights by using punches, kicks, and special moves involving projectiles to reduce their opponents' 'life' to nil. Mortal Kombat received much criticism in the early 90s for the inclusion of 'fatalities', hyper-violent finishing moves than often involve the dismemberment of the loser. It is important to note here that the fatalities in Mortal Kombat vs. DC Universe were a minor component of the participants' full experience. That is, fatalities were rarely executed by participants and were only ever experienced when the participant lost a round. Further, the extremity of fatalities in the version of Mortal Kombat we used were significantly reduced (in order to achieve a M 15+ rating).

Ultra-violent games (God of War: Ascension, Mortal Kombat: Komplete Edition). At the time of writing, God of War: Ascension was the latest version of the God of War franchise. Participants played a game mode where they must kill as many enemies as they could with a variety of weapons and combat techniques. While the differences in gameplay between the two God of War games were minimal, the important difference was the level of violence depicted in each game. Indeed, God of War: Ascension was restricted to adults aged 18 or older, as it was deemed to have "high impact violence", compared to the "moderate impact violence" of God of War 3. Again, at the time of writing, Mortal Kombat: Komplete Edition was the latest version of the Mortal Kombat franchise. Participants used a variety of techniques to defeat successive computer-controlled characters. This version of Mortal Kombat was advertised as the most violent in the series, with comments like "characters, environments and fatalities have never been presented with as much gory detail as in this next generation Mortal Kombat". Consequently, Mortal Kombat: Komplete Edition was also restricted to adults aged 18 or older, as it was deemed to have "high impact violence, blood and gore", compared to the "moderate impact violence" of Mortal Kombat vs. DC Universe.

Questionnaire measures

Because video game stimuli may vary considerably on a number of factors (e.g. game mechanics, pace, competitiveness), it is important to evaluate participants' impressions of important experiential factors. For each video game the participant played, we thus collected self-report measures of frustration, arousal, and interest. Example items included "the game was too hard" (frustration), "the game got my heart racing" (arousal), "the game kept my attention" (interest). Participants responded to the 12-item questionnaire (four items per measure) via Likert scale (1: strongly disagree, to 9: strongly agree). The participants also recorded their video game habits by listing their three most played games, whether those games were violent or not, and the category their most played games belonged to (e.g. fighting, sports, etc). A secondary aim of the video game questionnaires was to uphold our cover story for playing the video games, as discussed in the procedure section below. We also

included a single item manipulation check to assess how violent participants perceived the games they played, measured via the above Likert scale: "I felt violent while playing the game".

At the end of each session participants were debriefed and probed for suspicion via a four question funnelled debrief interview. Questions included: (1) what did you think the purpose of the session was; (2) have you ever completed tasks like these before; (3) was there anything strange about the experiment, or anything that didn't seem quite right; and (4) do you think the way you responded to any of the tasks was affected by how you responded on an earlier task. A composite score was created by summing the number of questions that participants indicated suspicion for.

Behavioural measures

Tangram task. The tangram task is a measure of prosocial behaviour in absence of personal cost. Tangrams are puzzles that are completed by using small shapes (e.g. squares, diamonds, triangles) to form larger, and more complex, patterns. Participants completed the tangram task under the guise that they were selecting tangram stimuli for another participant. Experimenters told the participants that an ostensible other participant would have to complete a series of tangrams under time pressure, winning a cash prize if they could complete the selected tangrams. The current participant was further told that the experimenters could not ethically select the tangrams because they varied in difficulty, meaning the cash prize created a conflict of interest. The experimenter then showed the participant the tangrams on a piece of paper, which were categorised by easy, medium, or hard difficulty (10 tangrams per difficulty category), and then asked the current participant to set the difficulty of the task by selecting 11 tangrams. Participants were reminded that the other participant would win a cash prize if they completed the selected tangrams under time pressure. Because the current participant loses nothing and ostensible future participants have everything to gain, this task provides a measure of prosocial behaviour without personal cost. Therefore, the tangram task can be used to measure both helping and hurting behaviour: helping behaviour operationalised as the number of easy tangrams assigned, and hurting behaviour as the number of difficult tangrams assigned (Gentile et al., 2009).

Charity donation. We recorded participants' donations to a real charity as a measure of prosocial behaviour that has a personal cost. Participants were paid $5 in $1 coins at the apparent end of the experimental session, before having their attention directed towards a donation box and a pile of questionnaires. The experimenter explained that the lab works closely with a charity at a local children's hospital (Children's Health Foundation) and that the charity often asks if the lab can help with some data collection. Participants were asked if they would volunteer their time to fill out a short questionnaire (4 items about public awareness of the charity, e.g. "I have heard of the Children's Health Foundation", "I know people who donate to the Children's Health Foundation"). If the participant was willing (none

were not), then the experimenter directed the participant's attention towards two metal boxes with the Children's Health Foundation logo, explaining that the participant should fold up their questionnaire upon completion and put into the box marked 'questionnaires'. The other box was marked 'donations' and the experimenter said that if the participant wanted to, they could make a contribution to the Children's Health Foundation by depositing money into that box. After full explanation, the experimenter immediately said that he would leave the room to give the participant privacy and to ensure anonymity. Prosocial behaviour measured via the donation task was operationalised as the number of $1 coins the participant placed into the donations box.

Procedure and design

The experimental procedure is described in text below and visually represented in Figure 3. Participants were informed that they were to take part in two pilot studies. The first, evaluations of different video games to assess their suitability as stimuli for future experiments, and the second, a stimuli selection task, where participants decide what stimuli future participants were exposed to (the tangrams). In reality, these two tasks were separate components of the same experiment; the first task comprising the video game measures, the second task comprising the tangram task, with the charity donation collected afterwards. Participants were randomly allocated to one of the three video game conditions where they played two exemplars of that category (i.e. two non-violent games, two violent games, or two ultra-violent games) in separate 15min blocks. It was explained to participants that they would play one game for 15mins, record their reactions and experience via questionnaires (which served to uphold the cover story), then play the other game for 15mins and complete a second set of questionnaires. Participants were then told that their final task would be the stimuli selection task, thereby completing the experimental session.

In actual fact, the order that participants experienced the tasks was never as described. A recent review has suggested the timing of behavioural measurements in violent video game effects research is important (Engelhardt & Bartholow, 2013). We were, therefore, conscious of minimising the time between playing a video game and our behavioural measurements (the tangram task and charity donation), while still maintaining a brief delay. This was achieved in the first case by pretending to have left the video game questionnaires in another lab, and having the participant complete the tangram task ahead of schedule, while the experimenter retrieved the missing questionnaires. The video game questionnaires were completed once the experimenter returned. After the second block, the participant completed a shortened version of the video game questionnaires (minus the video game habits questions) to keep the time between the video game and the charity donation to a minimum. Once the second set of video game questionnaires were completed, the participant was given $5, thanked for participating in the experiment then had their attention directed towards the charity questionnaires and

donation box. The experimenter left the room so that participants did not feel pressured to make a donation. Before participants left, they were probed for suspicion and debriefed.

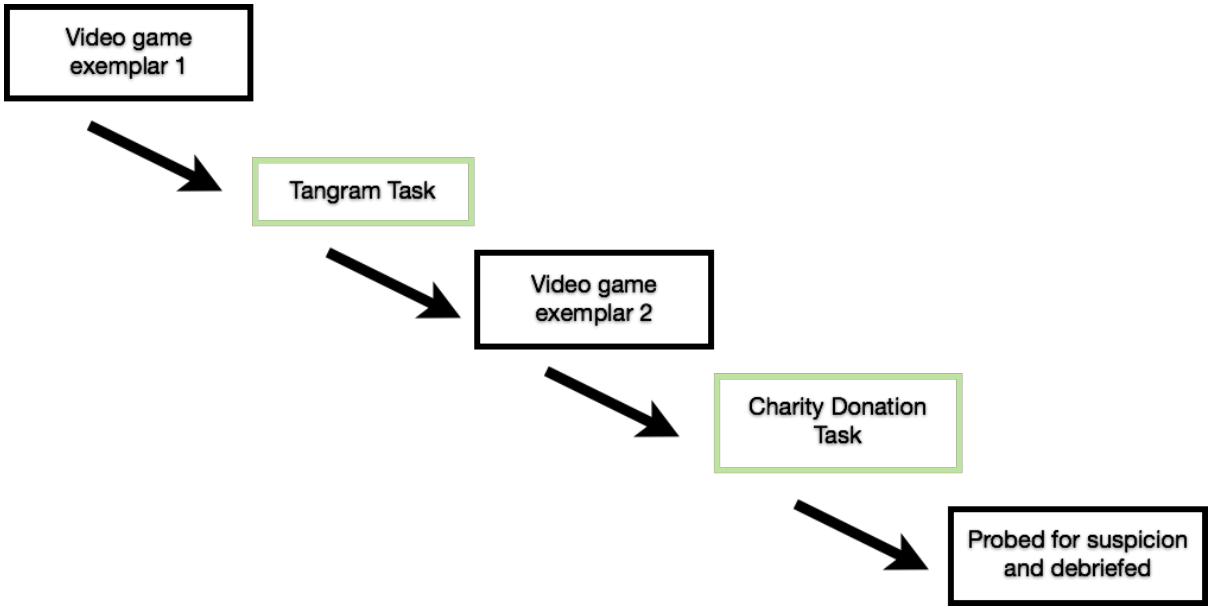


Figure 3. Experimental procedure for testing the effects of video game content on helping and hurting behaviour, and charity donation.

Results

Differences between games within a category

First, we conducted *t*-tests on the self-report experience measures to determine how the games within categories differed from each other. In the non-violent game condition, the games did not differ on measures of frustration, $t(39)=.22$, $p=.826$, $D=0.04$, arousal, $t(39)=1.35$, $p=.187$, $D=0.23$, or interest, $t(39)=.73$, $p=.471$, $D=0.04$. In the violent game condition, the games did not differ on measures of arousal, $t(39)=.18$, $p=.856$, $D=0.03$, or interest, $t(39)=.23$, $p=.817$, $D=0.13$. However, God of War 3 was rated as less frustrating ($M=2.74$, $SD=1.05$) than Mortal Kombat vs. DC Universe ($M=4.32$, $SD=1.72$), $t(39)=5.23$, $p<.001$, $D=0.86$. In the ultra-violent game condition, the games did not differ on measures of frustration, $t(39)=1.33$, $p=.192$, $D=0.21$, arousal, $t(39)=1.27$, $p=.211$, $D=0.20$, or interest, $t(39)=.79$, $p=.436$, $D=0.13$. Given these minimal differences all subsequent analysis was conducted with the two games used within each condition combined.

Manipulation checks

We next conducted manipulation checks to check that the violent games were perceived as more violent than the non-violent games. Indeed, the omnibus test was significant, $F(1,117)=40.03$, $p<.001$, violent games ($M=4.35$, $SD=2.18$) and ultra-violent games ($M=4.81$, $SD=1.89$) were perceived as more violent than non-violent games ($M=1.55$, $SD=0.99$), $ps<.001$, $Ds>1.76$.

We conducted a series of one-way ANOVAs to evaluate whether the categories of games (non-violent, violent, ultra-violent) were experienced differently, beyond violent content. Omnibus tests revealed differences between conditions on frustration, $F(2,117)=3.71$, $p=.028$, and arousal $F(2,117)=5.75$, $p=.004$, but not interest, $F(2,117)=1.36$, $p=.175$. Tukey follow-up tests revealed the difference in frustration was between the violent ($M=3.53$, $SD=1.06$) and ultra-violent games ($M=4.13$, $SD=1.04$), $p=.043$, $D=0.57$. For the arousal measure, the non-violent games were significantly less arousing ($M=4.50$, $SD=0.97$) than the violent ($M=5.09$, $SD=0.97$) and ultra-violent games ($M=5.16$, $SD=0.92$), ps$<.018$, Ds>0.61.

Behavioural measures

To evaluate whether there was an effect of video game category on observed behaviour, we ran one-way ANOVAs on our behavioural measures (charity donation, helping and hurting measures via tangram task). Means and standard deviations are summarised in Table 5.

Tangram task. The tangram task was used to measure both helping and hurting behaviour: (1) helping was measured by counting the number of easy tangrams participants assigned to an ostensible other participant; and (2) hurting was measured by counting the number of difficult tangrams assigned. We first analysed the helping behaviour DV and found no difference between game categories, $F(2,117)=0.19$, $p=.830$, $\omega^2=.014$. That is, it did not matter whether participants played a non-violent ($M= 3.20$, $SD=1.92$), violent ($M=3.20$, $SD=1.34$), or ultra-violent ($M=3.02$, $SD=1.03$) game, participants assigned an equal number of easy tangrams to an ostensible other participant. While the primary focus of our experiment was the effect of violent games on prosocial behaviour, the tangram task allows us to also examine hurting behaviour. Given that violent games have previously been associated with anti-social behaviour, we also analysed the hurting behaviour data. Again, we found no effect of our video game condition on hurting behaviour, $F(2,117)=0.01$, $p=.987$, $\omega^2=.017$. Participants assigned equal numbers of difficult tangrams across the non-violent ($M=3.35$, $SD=1.97$), non-violent ($M=3.30$, $SD=1.52$), and ultra-violent ($M=3.35$, $SD=1.21$) conditions.

Charity donation. We conducted a further ANOVA on the charity donation measure to see whether the type of game that participants played influenced donation tendencies. Omnibus tests revealed no differences between conditions on charity donation, $F(2,117)=2.54$, $p=.083$, $\omega^2=.025$, that is, participants donated equal amounts across the non-violent ($M=2.00$, $SD=1.94$), violent ($M=2.33$, $SD=2.03$), and ultra-violent ($M=3.00$, $SD=2.10$) conditions.

Table 5.

Means and standard deviations for focal DVs

DV	Non-violent	Violent	Ultra-violent
Helping Behaviour	3.20 (1.92)	3.20 (1.34)	3.02 (1.03)
Hurting Behaviour	3.35 (1.97)	3.30 (1.52)	3.35 (1.21)
Charity Donation	2.00 (1.94)	2.33 (2.03)	3.00 (2.10)

Suspicion measures

We created a summary suspicion score by recoding participant responses to the four debrief questions. Participants were coded as suspicious if they indicated suspicion on any of the debrief questions. We then conducted an ANOVA to determine if this suspicion varied between video game conditions, revealing no difference in suspicion between conditions, $F(2,117)=.60$, $p=.553$ $\omega^2=.007$. Because the afore-reported pattern of results remained after excluding suspicious participants (non-violent: 3; violent: 4; ultra-violent: 6), we elected to report the total sample.

Learning effects

Because participants played two exemplar games within a category (the first game at the beginning of the session and the second towards the middle), it's possible that participants became more sensitive to the experiment's hypotheses after playing the second game. Indeed, we did find that participants in the violent and ultra-violent conditions rated the second game as more violent (violent: $t(39)=3.29$, $p=.002$, $D=0.52$; ultra-violent: $t(39)=4.28$, $p<.001$, $D=0.68$), despite the presentation order of the games being counter-balanced. While participants in the violent and ultra-violent conditions reported a greater sensitivity to the violent content, they were no more suspicious of the experiment's true hypothesis than participants in the other conditions (see suspicion measures above).

Discussion

A growing literature has reported on the negative effects on behaviour of playing violent video games (Anderson et al., 2010). This literature has been met with increasingly concerted efforts at delineating the flaws in this research (Adachi & Willoughby, 2011a), with calls for replication and the dissemination of null result findings (Australian Government, Attorney General's Department, 2010), the absence of which risks presenting a skewed view of the real effect (Ferguson, 2013). Regardless of the merits of the perspectives proposed by both sides of this debate, it stands to reason that if playing violent video games leads to increases in

antisocial behaviour it should similarly lead to decreases in prosocial behaviour. The current experiment adds weight to the argument that it does not.

Previous research failed to find a negative effect of playing violent video games on prosocial behaviour. Greitemeyer and Osswald (2010) reported that playing an arcade-style violent video game did not diminish prosocial behaviour as measured using the classic pen drop task. Extending this to modern, graphic games, and expanding exposure time similarly failed to find any impact (Tear & Nielsen, 2013). In the current study we sought an effect using two new measures of prosocial behaviour and games so extreme in their violence that it is illegal to sell them to minors. Yet we too failed. Those playing an ultra-violent video game were as likely to set an easy tangram task for an ostensible future participant as those playing a non-violent game. They also donated similar amounts of money to charity.

It is standard practice to criticise null result studies for lacking sufficient power. We conducted an a priori power analysis to determine how many participants would be needed to reveal the observed effects. Using G*Power (version 3.1, Faul, Erdfelder, Buchner, & Lang, 2009), we calculated effect sizes by inputting the variance explained by the independent variable and the error variance. Then we specified an alpha level of .05, power of .80, a numerator *df* of 2, and the number of groups as 3. The power analysis revealed that for the observed effects of video games on helping (effect size $f = 0.06$) and hurting (effect size $f = 0.01$), we would have required 3,021 and 42,945 participants, respectively. Moreover, to the extent that any effect might be found, the only discernible trend was for participants who played the ultra-violent games to donate more to charity. There are many explanations for why this may happen, such as moral cleansing (Bastian, Jetten, & Fasoli, 2011) or as a response to perceived demand characteristics (Bender, Rothmund, & Gollwitzer, 2013). Regardless of the reasons, testing sufficient numbers of participants to get this trend to significance would still render as unfounded because the hypothesised direction was that playing violent video games would decrease prosocial proclivities, not increase.

It is notable that each of the studies failing to find an effect of playing violent video games on prosocial behaviour has been conducted with adults (Greitemeyer & Osswald, 2010; Tear & Nielsen, 2013). It is possible that by the onset of early adulthood the factors contributing to prosocial inclinations have been well established and are not easily shifted by playing a video game for half an hour. Indeed, Griffiths (1999) found that young children are more aggressive after a violent game than young adults. Furthermore, given the proliferation of video games within contemporary culture, older participants have likely had more opportunity to habituate to video games. Experimental research is now needed to explore the issues raised here in younger populations, where prosocial inclinations are more flexible and where habituation to video games has not yet been established. Our sample was also disproportionately male. Indeed, only 12.5% of our sample was female. Given purported gender differences in aggression (Archer, 2004; Bettencourt & Miller, 1996) and prejudice (Ekehammar, Akrami, & Araya, 2003), it seems likely that a gender difference might exist

for prosocial tendencies, although it is unclear what this difference might look like. Interestingly, past research has been able to reveal violent video game effects on prosocial behaviour with similar gender imbalances. The experiment conducted by Sheese and Graziano (2005) found, with an identical gender skew (87.5% male), that participants who played a violent game were more likely to exploit their partners than cooperate with them. A further concern with our sample is its cultural heterogeneity. Attitudes towards acting prosocially could conceivably differ between cultures.

It remains possible that participants suspected the hypothesis, despite reporting no suspicion, and that the marginally significant trend for ultra-violent video games to lead to more donations was driven by their suspicions or sensitivity to the hypothesis. Indeed, participants who identify strongly as gamers often modify their responses to transparent dependent variables in order to manage representations of gamers in the literature (Bender, Rothmund, & Gollwitzer, 2013). Any future work using this methodology should counter-balance the order of the prosocial dependent variables to address possible learning effects.

There is now growing reason to suspect that playing violent video games does not meaningfully impact prosocial behaviour in a normal population. However, it should be acknowledged that the effects explored, both here and in previous studies, are short-term and do not account for the possible effect of sustained playing. It remains to be established whether or not playing for long hours over an extended period of time will have detrimental effects. Recent research has concluded that long term effects of video games, as a superordinate category, on behaviour are minimal (Parkes, Sweeting, Wight, & Henderson, 2013). It then seems reasonable to expect that violent video games would similarly show no effect, given that 89% of video games contain violence (Glaubke, Miller, Parker, & Espejo, 2001).

However, it is worth considering that the debate about violent video games has been focused on showing that violent games have negative effects on behaviour, usually in simple exposure paradigms (participants play a game then respond to a dependent measure). We believe that mere exposure is not a sufficient framework for discussing violent video game effects. Indeed, if that were true, then our experiment should have shown conditional effects on prosocial behaviour. Furthermore, research in the field is increasingly considering mechanisms beyond mere exposure to violence, and towards pinning down the exact mechanisms by which video games may influence behaviour. For example, violent games are both more competitive (Adachi & Willoughby, 2011a) and potentially more difficult (Przybylski, Deci, Rigby, & Ryan, 2014) than non-violent games, and it may be these features that drive previous demonstrations of violent video game effects rather than the violent content per se. The results of the experiment we report here challenge the belief that these exposure effects exist at all.

Chapter 4 – A test of the ‘tipping point’ hypothesis of violent video game effects

Preface

Chapter 4 explores potential reasons for why the previous chapters failed to report any violent video game effects. In this chapter I tested the logic of a 'tipping point' hypothesis, where violent video game effects only manifest when appropriate psychological preconditions are present. Previous work tested this idea at the trait level and here I test whether a similar effect exists at the state level. The General Aggression Model highlights the importance of cognition in violent video game effects. So, in this experiment, participants were exposed to either a hostile or neutral semantic priming task to manipulate hostile cognitive states, and then were exposed to either an ultra-violent or a non-violent video game. I hypothesised that those in the hostile-prime and ultra-violent video game condition would demonstrate the least prosocial behaviour, whereas those participants in the neutral-prime and non-violent video game would report the most. The data revealed no differences between conditions, so it appears that priming hostility and playing an ultra-violent video is insufficient for revealing hypothesised violent video game effects.

Abstract

The effect of violent video games on behaviour is unclear – some researchers report consistent effects whereas others do not. One possibility for such discrepant reports is that violent video game effects are context dependent and rely on important psychological preconditions in order to manifest. For example, it is possible that certain trait variables can put individuals at risk of negative violent video game effects. Here, we investigate whether particular *state* variables result in individuals' behaviour being affected by playing violent video games. Specifically, using semantic tasks, we primed participants with the concept of hostility and examined whether the combination of a hostile cognitive state and exposure to a violent video game leads to differential behavioural patterns. We found that they did not. The results suggest that state-based accounts of violent media effects are insufficient, and we suggest future research focus on the role of the user in shaping their media experience.

Keywords: prosocial, violent video games, semantic priming

Introduction

Video games are near ubiquitous among modern Western societies. Indeed consumption has reached a point where consumers spend similar amounts of money on video games as they do movies (ESA, 2014; Gruenwedel, 2013). In the US, the average age of someone who identifies as a gamer is 31 (ESA, 2014) and in Europe, almost 50% of all gamers are aged 35 and over (ISFE, 2012). Thirty-seven per cent of the U.S. population age 9 and older currently plays PC games for an average of 6.4 hours per week (The NPD Group, 2014). Given that between 15-35% of the top selling games in 2013 (by units sold) were rated as suitable for mature audiences only (ESA, 2014), there is justifiable concern over the effect violent and adult-themed games may have on those who play them.

One of the most prominent concerns about the effect of violent and adult-themed games is the intuition that exposure with media violence can negatively influence subsequent behaviour. The dominant theory for testing such a relationship is the General Aggression Model (GAM, Anderson & Bushman, 2002; or General Learning Model, Greitemeyer, 2011). According to the GAM, exposure to violent stimuli primes aggressive behaviour by influencing the thoughts, feelings, and physical arousal of an individual. Some meta-analyses put the effect of violent video games at around r=.20 (Anderson et al., 2010; Greitemeyer & Mügge, 2014), although others suggest the effect is much smaller, around r=.04 (Ferguson, in press). These conflicting estimates reflect the, at times, extremely heated conflict in the literature over whether there is indeed any real impact of playing violent video games, and if there is an impact, what are the mechanisms and boundary conditions by which it manifests?

While there are several important sociological predictors of societal violence, there is yet to be a demonstrable link between violent video games and societal violence (see youth crime statistics discussed in Ferguson, 2013; in press; Markey, French, & Markey, 2014; Markey, Markey, & French, 2014). Yet, violent video games are still deemed by some researchers to be a significant risk factor for aggression (Bushman & Anderson, 2001; Bushman & Huessman, 2013). Regardless, while there are certainly proponents of the 'video games as harm' position, there are to the best of our knowledge no published claims that violent video game exposure is the only factor that leads to anti-social behavior. In this context, exposure to violent video games may be seen as a 'tipping point' for at-risk individuals. Giumetti and Markey (2007) demonstrated that dispositional anger moderated a violent video game effect, such that violent video games only affected the behaviour of those who rated themselves as moderate or high in dispositional anger. Advancing this work, Markey and Markey (2010) reviewed past research and integrated the findings into the Five-Factor Model taxonomy. Their analyses confirmed that individuals who were most responsive to the negative effect of violent video games, that is, those who were more likely to behave aggressively after playing a violent video game, scored high on neuroticism and low on agreeableness and conscientiousness. In the current research our aim was to investigate if a similar effect exists

at the state level of experience. That is, we wanted to see if participants could be primed in such a way as to be more responsive to the negative effects of violent video games. Indeed, this fits within the GAM framework, where a person's internal state (cognition, affect, arousal) dictates their behavioral outcomes (Bushman & Anderson, 2002). The literature on semantic priming provides a foundation from which to test this question.

Semantic primes can increase the accessibility of aggressive or hostile cognitions. A semantic prime is an exemplar of a category that can cue other exemplars in the category. For example, seeing the word 'dog' could make it easier to think of the word 'budgie' because they both belong to the semantic category 'domestic pets'. In this case, 'dog' would qualify as a semantic prime (although see Lucas, 2000 for a discussion of the differences between semantic and associative priming). With respect to hostility and aggression, Anderson, Benjamin and Bartholow (1998) demonstrated that weapons may operate as semantic primes. In two studies they showed that when participants merely identified a weapon, they were quicker to read a word related to aggression than on trials that required identifying non-weapons. That is, cognitively processing weapons made it easier for participants to have aggressive cognitions.

While the ability for a set of stimuli to prime aggressive thoughts is certainly interesting from an academic perspective, policymakers and parents are more concerned with aggressive *behaviour*. Turning to whether simple semantic primes can influence behaviour, several studies have demonstrated that semantic primes can influence myriad behaviours, despite not being processed consciously. For example, Bargh and colleagues (2001) demonstrated that semantic primes can activate behavioural goals without ever being consciously processed. These primes promoted goal-directed behaviour, such that participants who completed a word-task containing performance words like 'succeed', 'achieve' and 'win' performed better on a word search task than participants who saw neutral words like 'ranch', 'carpet', and 'river'. Other examples include priming participants with the concept of 'professor' to increase performance on a knowledge task (Dijksterhuis & van Knippenberg, 1998), and priming participants with the concept of 'warm' to promote interpersonal warmth (Williams & Bargh, 2008).

It is tempting to expect that violent video games could influence behaviour by operating as semantic primes. Unfortunately, the literature on video games as primers of behaviour is mixed. There are at least two possible reasons for this uncertainty. First, video games as primes are on an order of magnitude more complex than traditional semantic primes. Semantic priming is thought to work via spreading activation of representations, such that closely linked concepts should have strong connections (although see Frenck-Mestre & Bueno, 1999 for data disputing this). For a concept to be activated by a semantic prime, the prime must have high associative strength and featural overlap with the target concept (Hutchinson, 2003). This calls into question the ability of video games to reliably prime particular concepts, given that they are necessarily complex and dynamic, and comprise many

elements that are likely to corrupt or dilute the central target concept for priming. These elements may include dramatic plot elements, decision-making tasks, rewarding game mechanics, achieving goals, and evocative art styles. This is in stark comparison to traditional semantic prime stimuli, which are often merely a static list of words that the participant must process.

Further, traditional theories of video game effects, such as the GAM, are criticised for implying that violent media is injected into passive viewers who automatically absorb aggressive scripts without awareness or effort. These theories have hence been labelled as 'hypodermic needle' or 'tabula rasa' metaphors (Davis & Baron, 1981; Ferguson, in press; Pinker, 2002). According to proponents of this view, the traditional social learning accounts of violent media effects (e.g. the GAM) over-simplify the relationship between violent media consumption and behaviour into a simple stimulus-response-reinforcement model. But 'video games as stimuli' is only one way in which users interact with the medium. Ivory (2013) discusses other ways in which users consume media: as avocation, as skill development, and as a social environment. Indeed, others have discussed violent video game effects in the context of goal attainment and motivation satisfaction (Przybylski, Deci, Rigby, & Ryan, 2014; Ryan, Rigby, & Przybylski, 2006). If users are consuming violent video games for motivationally different reasons, then it stands to reason that the ability for those games to consistently prime aggressive behaviour will be highly variable.

The aim in the current research was test the idea that exposure to violent video games is not enough, in and of itself, to influence social behaviour. Rather, exposure to violent video games might serve as a tipping point when participants are already in a hostile cognitive state, as primed by using a semantic priming task for the concept of 'hostility'. We manipulated the content of the semantic prime (neutral vs. hostile) and the content of video games (non-violent vs. ultra-violent), and examined the effect of these variables on prosocial behaviour, which is thought to be affected by both semantic primes (Nelson & Norton, 2005; Shariff & Norenzayan, 2007) and violent video games (Anderson et al., 2010). One often-used method for inducing behavioural change via semantic primes is the scrambled sentence task (Bargh, Chen, & Burrows, 1996; Dijksterhuis et al., 1998; Shariff & Norenzayan, 2007; Srull & Wyer, 1979). Each item in the scrambled sentence task consists of five words, of which four can be rearranged into a coherent, grammatical sentence. Participants need to cross out the odd word so the remaining four make a coherent sentence. Importantly a portion of these items contain words that make a target concept salient, for example, Bargh and colleagues (1996) primed the construct 'rude' with items like 'they her bother see usually', which can be solved as 'they usually bother her ~~see~~'. For the behavioural measure, we will use the tangram task developed by Gentile and colleagues (Gentile et al., 2009; Saleem, Anderson, & Gentile, 2012), in which participants allocate tangrams of varying difficulties to a future partner. The number of easy tangrams selected is taken as a measure of prosocial (helping) behaviour,

whereas the number of difficult tangrams selected is a measure of aggressive (hurting) behaviour.

An additional aim of the current experiment was to provide preliminary data in order to test the exploratory hypothesis that playing an ultra-violent video game may actually lead to increases in prosocial behaviour. Participants may feel that engaging in, or enjoying, a very violent video game may be at odds with what it means to feel human. People who want to restore their humanity may try and do something good at the next possible instance, such as donating to a charity. Bastian and colleagues (2011) demonstrated preliminary evidence that violent video games can reduce feelings of humanity in participants. We included measures of humanity as a replication attempt of whether people who play violent video games rate themselves as less human afterwards.

In this experiment there were four conditions: (1) hostile prime and ultra-violent video game; (2) hostile prime and non-violent video game; (3) neutral prime and ultra-violent video game; and (4) neutral prime and non-violent video game. Consistent with a 'tipping point' hypothesis, we expected that participants in the hostile prime and ultra-violent video game condition (condition 1) will show the largest effect on prosocial behaviour. That is, we hypothesised that participants in condition 1 would select the fewest easy tangrams and most hard tangrams. The hostile prime and non-violent video game condition (condition 2) would show the next largest effect, followed by the neutral prime and ultra-violent video game condition (condition 3). The neutral prime and non-violent video game condition (condition 4) should show the smallest effect. Regarding the dehumanisation measure, consistent with Bastian and colleagues (2011), we hypothesised that participants who played a violent video game would rate themselves as less human than those who played a non-violent game.

Finally, we analysed our results using traditional null hypothesis significance testing (NHST) and via calculation of Bayes factors. It is often unclear about how to interpret null results in the violent video game literature. Some authors have declared that recent meta-analyses should "nail the coffin shut on doubts that violent video games stimulate aggression" (Huesmann, 2010). With this level of certainty that the violent video game effect is real, how are we to interpret experiments failing to show such an effect? In a field with documented cases of publication bias (Ferguson, 2007; 2012), null results should not be easily dismissed.

Calculating Bayes factors allows for clearer interpretation of null results. With traditional NHST, a *p*-value gives the probability of the data occurring if the null hypothesis was true; that is, how often the data occur by chance if there was no effect in the first place. We should not, however, assume a non-significant result counts as evidence for the null (as much as we sometimes like to). That is, we should not confuse 'absence of evidence' with 'evidence of absence' (Heather, 2014). A Bayes factor, however, represents the weight of evidence for the alternative hypothesis vs. the null hypothesis. Thus, while NHST allows two possible conclusions (reject the null or there is insufficient evidence to reject the null), a Bayesian

perspective allows three: 1) there is strong evidence for the alternative; 2) there is strong evidence for the null; 3) the data are insensitive with respect to the null and alternate hypotheses. Frequentist and Bayesian analyses will often show considerable agreement in interpretation, except in the case of null results - frequentist statisticians cannot interpret null results, Bayesians can. Dienes (2014) thus makes the case that researchers ought to adopt Bayesian approaches into their research workflow.

Method

Participants

We recruited 123 undergraduate students from a first-year introductory psychology course at a large metropolitan university (69% male, M_{age}=19.97, SD_{age}=3.17). Most participants self-reported as Caucasian (52%), with a minority reporting as Asian (37%) or Other (11%). Ethical clearance was granted by the Behavioural & Social Sciences Ethical Review Committee at the University of Queensland.

Video game stimuli

Off-the-shelf games provide media researchers ecologically valid and representative stimuli. In this experiment we selected two games that have been restricted to sale for person over 18 years; God of War: Ascension and Mortal Kombat: Komplete Edition. We also selected two non-violent games to serve as control games; Portal 2 and Modnation Racers. Video games were played on a Sony Playstation 3.

Ultra-violent games (God of War: Ascension and Mortal Kombat: Komplete Edition). God of War: Ascension is a violent combat game where the player uses a variety of weapons to destroy as many enemies as possible in order to progress to later stages in the game. Participants in this experiment played the 'story' mode from the first stage, meaning more competent players saw more of the game. Mortal Kombat is a violent game where players select a fighter and attempt to beat an opponent by reducing his or her 'life' to nil. Participants played the arcade mode where they were paired against a series of fighters, the difficulty of which increased with each subsequent opponent. Both these games were deemed to have high impact violence and as such their sale was restricted to persons aged 18 or older.

Non-violent games (Portal 2 and Modnation Racers). Portal 2 is a non-violent puzzle game where players need to use portals in order to navigate around a series of otherwise impassable obstacles. Participants began from the first playable stage, known as a 'test chamber'. Modnation Racers is a non-violent racing game where players compete against other computer-controlled characters in a race around brightly coloured locations. Players can earn boosts by showing high racing skill. Participants entered the 'Quick Race' mode where they were allowed to select what race they drove in, as well as setting any parameters they liked (e.g. number of opponents, number of laps).

Questionnaire measures

We asked participants to report the experience they had while playing each of the video games. For each video game played, we collected self-report measures for frustration, arousal, and interest (see Appendix A for full questionnaire). Example items included "the game was too hard" (frustration), "I found the game stimulating" (arousal), and "I found myself thinking about other things while playing the game" (interest, reverse scored). Participants responded to 12 items (four per measure) via a 9-point Likert scale (1: strongly disagree, to 9: strongly agree). A final question served as our violent content manipulation check. We asked participants to indicate using the above scale how violent they felt while playing the selected games. Participants completed these questions after playing the first game and again after the second game. Finally, participants had an opportunity to indicate what they thought the experiment was testing, as well as whether they thought any aspect of the experiment seemed unusual or unexplained. Participants were then debriefed (see Appendix E) and thanked.

Semantic prime

The hostile and neutral primes were adapted from the scrambled sentence task used by Srull and Wyer (1979). Different versions of the scrambled sentence task primed participants by activating either a hostile concept or no concept in particular. Participants saw 30 groups of five words and needed to cross out one word such that the remaining four words could be rearranged into a coherent sentence (e.g. "~~friend~~ went town to Gary" = "Gary went to town"). In the hostile versions, nine of the scrambled sentences had words related to violence or hostility (e.g. "can they hostile polite be"). Participants were exposed to a prime before each video game, so we created two versions of each prime type (see Appendix B).

Tangram task

The tangram task is a simple pen and paper measure of prosocial behaviour in the absence of personal costs. Tangrams are puzzles that require arranging small shapes (e.g. squares, diamonds, triangles) into larger and more complex patterns and shapes. Participants needed to select tangrams for ostensible future participants in an ostensible future experiment (see Appendix C). We told participants that we wanted to have participants in our next experiment complete a set of tangrams. We then said that, in order to make sure they were motivated to seriously attempt the tangrams, we were going to offer participants in the 'experiment' a $10 prize if they completed the set of tangrams in less than 10 minutes. To uphold the cover story, we explained that the experimenters could not ethically select the tangrams because they varied in difficulty, meaning the cash prize created a conflict of interest, but that the conflict of interest did not exist if someone not involved with the research selected the tangrams. Participants then saw a sheet of 30 tangrams of varying difficulty (10 easy, 10 medium, 10 hard) and were asked to set the difficulty of the task for a future participant by selecting a set

of 11 from the 30 options. At this point we reminded current participants that the ostensible future participants would win $10 if they completed the selected set within 10 minutes. The number of easy tangrams selected served as a measure of helping behaviour, whereas the number of hard tangrams selected corresponded to hurting behaviour (Gentile et al., 2009).

Dehumanisation

Our measures of humanness were adapted from those used by Bastian and colleagues (2011). In the final set of questionnaires, participants rated their humanness on 8 items (items 2, 5, 7 and 8 reverse-scored) that assess both human-nature and human-uniqueness constructs (see appendix D). Participants responded using a 7-point Likert scale (1: not at all, to 7: very much so). We asked participants to reflect on their experience of playing the video games and the extent to which they saw themselves as having the characteristics in the questionnaire. Thus, participants who scored high on this measure were those who believed they possessed many of the traits aligned with those that are innate and unique to humans. Conversely, those who scored low were those who believed they possessed few of the traits aligned with humanness.

Procedure

The experimental procedure is described in text below and visually represented in Figure 4. Participants were brought into the testing lab where they were provided with instructions on what to expect and their informed consent was gained. They then completed the scrambled sentence task, which comprised either 30% hostile sentences or was completely neutral. Participants then played a pre-selected video game for 15 minutes that was either a non-violent video game (Portal 2 or Modnation Racers, counterbalanced) or an ultra-violent video game (God of War: Ascension or Mortal Kombat: Komplete Edition, counterbalanced). The cover story for completing the scrambled sentence task and then playing the game in this order was that we wanted to see what effect a language processing task had on video game experience. Now primed via both a classic semantic priming task and a video game, participants were then asked to assign tangrams for a future participant to attempt to solve. It was explained to participants that a future experiment involved solving a set of tangrams under time pressure with the chance of a reward if successful and that we needed the current participant to select which tangrams to use because of a potential conflict of interest. Then the participants filled out one copy of the questionnaire measures, before moving onto a second version of the scrambled sentence task (of which the content was semantically consistent with the first version). As before, participants then played a second video game (content consistent with the first game), assigned a second set of tangrams for another future participant, and completed a second round of questionnaire measures. Finally, participants rated themselves on personality questions so we could determine their level of self-dehumanisation and indicated their video game habits. Participants were debriefed and given an opportunity to

voice their suspicions about the hypotheses if they had any. The combination of variables led to a 2 (prime: neutral vs. hostile) x 2 (video game: non-violent vs. ultra-violent) design.

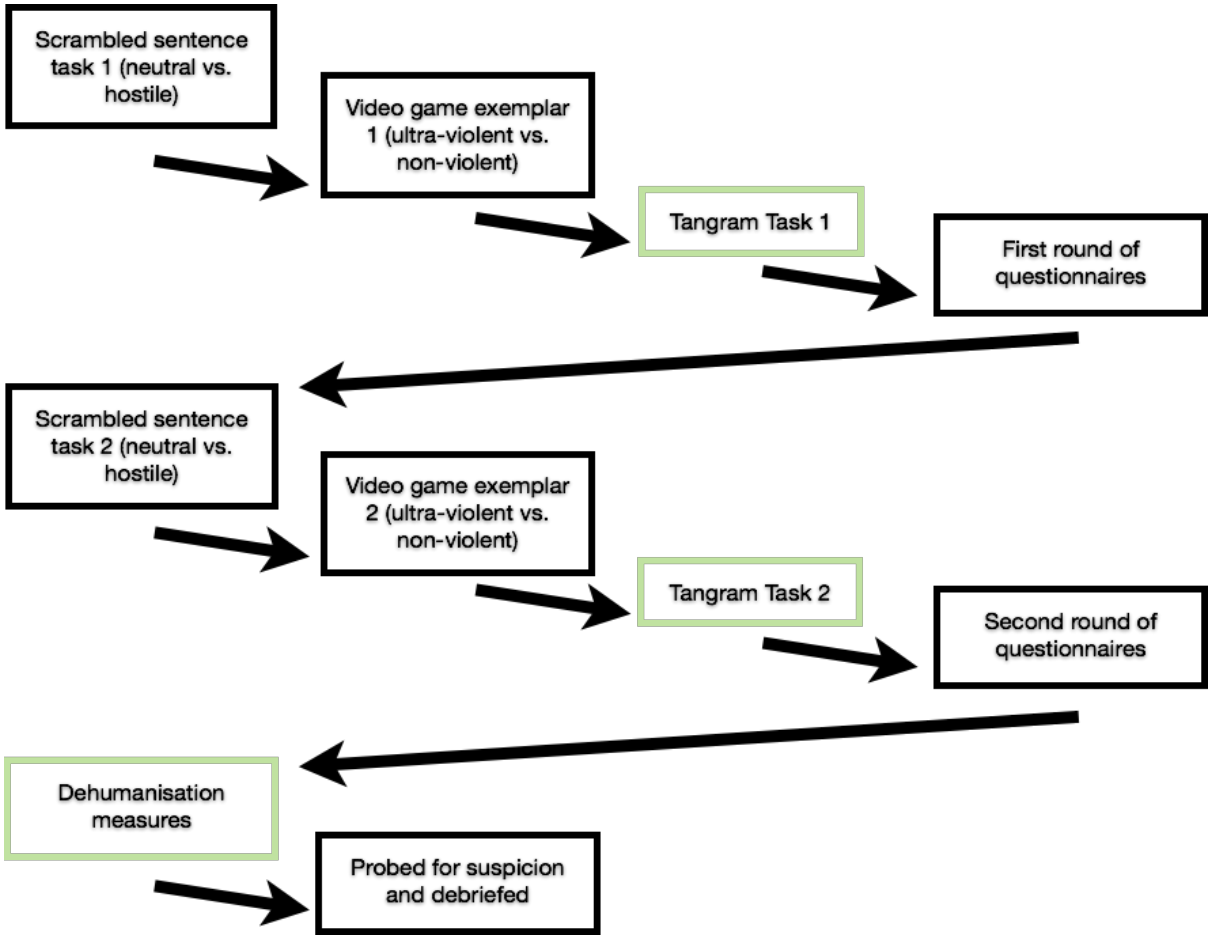


Figure 4. Experimental procedure for testing the effects of semantic primes and video game content on helping and hurting behaviour, and self-dehumanisation.

Results

Manipulation checks

We asked participants to indicate how violent they felt while playing the games and compared the responses from participants who played the ultra-violent games with those who played the non-violent games using an independent-groups *t*-test. The variances from the two samples were deemed significantly different, via Levene's test for equality of variances, $F=19.27$, $p<.001$, so we used the adjusted values for the subsequent *t*-test. Participants who played the ultra-violent games self-reported as feeling more violent ($M=5.27$, $SD=2.31$) than those participants who played the non-violent games ($M=2.05$, $SD=1.31$), $t(119.98)=9.481$, $p<.001$, $D=1.71$.We then conducted a series of independent-groups *t*-tests to determine whether the video game conditions differed on measures on experience. The games were rated by participants as being equally frustrating, arousing and interesting ($ts<1.04$, $ps>.299$).

Behavioural measures

We conducted a series of 2 (video game condition: non-violent vs. ultra-violent) x 2 (prime condition: neutral vs. hostile) ANOVAs to determine if there was an effect of video game or prime type on measures of helping and hurting behaviour, and on measures of self-dehumanisation (see Figure 5).

Tangram task. There was no main effect of video game condition on the number of easy tangrams selected, $F(1,118)=0.56$, $p=.455$, $\eta^2=.005$, nor on the number of hard tangrams, $F(1,118)=0.87$, $p=.353$, $\eta^2=.007$. Similarly, there was no main effect of prime condition on the number of easy tangrams selected, $F(1,118)=0.91$, $p=.343$, $\eta^2=.008$, nor on the number of hard tangrams, $F(1,118)=0.62$, $p=.434$, $\eta^2=.005$. Furthermore, there was no interaction between game type and priming on the number of easy, $F(1,118)=0.76$, $p=.387$, $\eta^2=.006$, or hard tangrams selected, $F(1,118)=0.29$, $p=.589$, $\eta^2=.002$.

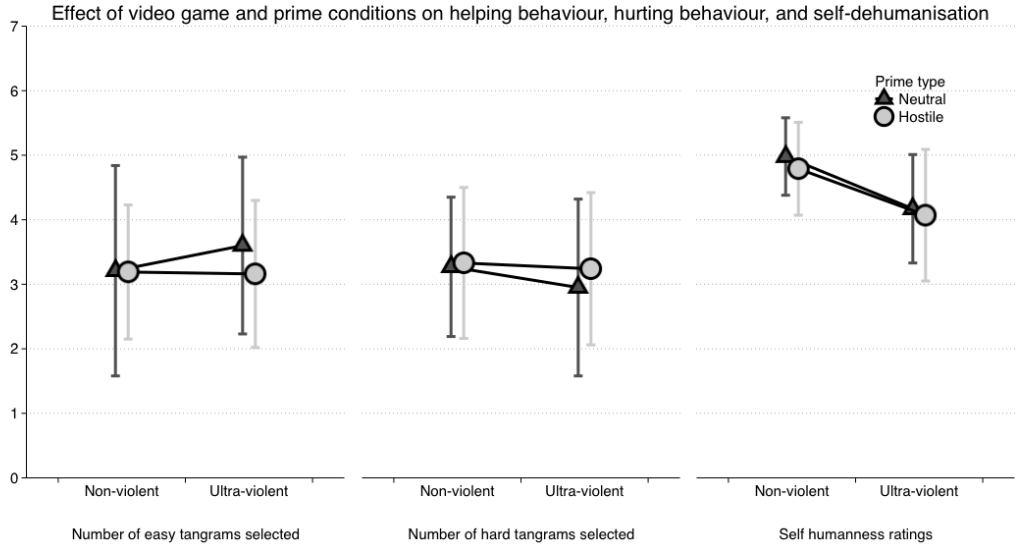


Figure 5. Cell means and standard deviations for helping behaviour (easy tangrams), hurting behaviour (hard tangrams), and self-dehumanisation.

Self-dehumanisation. Preliminary analysis revealed that the four items representing the human nature construct and the four items representing the human uniqueness construct showed acceptable reliability for scale creation (α =.704), and were combined into a single variable representing self-dehumanisation. Analyses revealed that participants who played the ultra-violent games rated themselves as less human (M=4.12, SD=0.93) than those who played non-violent games (M=4.89, SD=0.66), $F(1,118)=27.34$, $p<.001$, $\eta^2=.188$. Prime condition had no effect on self-dehumanisation, $F(1,118)=0.90$, $p=.344$, $\eta^2=.008$, nor was there an interactive effect of video game condition and prime condition, $F(1,118)=0.08$, $p=.771$, $\eta^2=.001$

Bayes factors

Null results are often criticized for being uninformative. When the result of a statistical test shows a *p*-value of >.05, we can only say that we have the probability of collecting those data given H_0. There are numerous reasons why a test might return a non-significant result; the tested effect is not real, the materials or method are insensitive to the effect, or even regular statistical variance. Making inferences about *p*s>.05 is difficult. Bayes factors, on the other hand, can tell us the probability that H_1 or H_0 are true given the data. In this context, according to Jeffreys (1967), Bayes Factors (BF_{10}) can be used to interpret whether *p*>.05 counts as evidence for the H_0 (when the Bayes factor is less than 0.3), evidence for H_1 (when the Bayes Factor is greater than 3), or that the data are insensitive (Bayes factor between 0.3 and 3) (at least for the Rouder and Morey Bayesians, see Schönbrodt, 2014 for a discussion of the different flavours of Bayes factors). The calculated Bayes factors are discussed below and also presented in Table 6. I have also provided a reanalysis of the data from Chapter 3 (but not Chapter 2 as Bayes factors cannot yet be calculated for categorical data such as those).

Tangram task. The above-discussed frequentist ANOVA analyses are entirely consistent with Bayes factors we calculated using the BayesFactor package for R (Morey & Rouder, 2014). The data for the number of tangrams assigned reflected greater support for the null, compared to the hypothesis that video games should affect easy tangram selection BF_{10}=0.25. Similarly, evidence for the null was greater than evidence that video games affect hard tangram selection, BF_{10}=0.29. We also predicted that those participants exposed to a hostile prime would select less easy tangrams compared to those exposed to a neutral prime. There was substantially greater evidence for the null compared to our hypothesis, BF_{10}=0.29. We made a similar prediction that participants exposed to a hostile prime would select more hard tangrams than those exposed to a neutral prime. Again, we found greater evidence for the null, BF_{10}=0.25. The full model (both main effects and the interaction) did not provide greater support for the theory with respect to easy tangram selection, BF_{10}=0.03, nor with respect to hard tangram selection, BF_{10}=0.02.

Self-dehumanisation. The orthodox statistical analyses are consistent with our calculated Bayes factors. We predicted that violent video games would lead participants to report feeling less human than those who played non-violent games. The Bayes factor we calculated to test our theory that violent games affect self-dehumanisation showed very strong evidence for our theory over the null, BF_{10}=22535.56. We made no specific predictions about the effect of prime condition on self-dehumanisation and the associated Bayes factor demonstrated evidence for the null that our prime showed no effect on self-dehumanisation, BF_{10}=0.29. The Bayes factor for the full model, including the interaction, showed greater evidence for the model over the null, BF_{10}=1771.23.

Table 6.

Bayes factors and F-test statistics calculated for each variable.

Dependent variable	Effect	BF_{10}	*F*	*p*
Helping behaviour	Main effect of video game	0.25	0.56	.455
	Main effect of prime type	0.29	0.91	.343
	Interaction	0.02	0.76	.387
Hurting behaviour	Main effect of video game	0.29	0.87	.353
	Main effect of prime type	0.25	0.62	.434
	Interaction	0.02	0.29	.589
Self-dehumanisation	Main effect of video game	**22535.56**	**27.34**	**<.001**
	Main effect of prime type	0.29	0.90	.344
	Interaction	1729.06	0.08	.771

Reanalysis of Chapter 3 data

Using this Bayesian technique, we can re-analyse the similar data in Chapter 3, which also used the tangram task (results shown in Table 7). In Chapter 3, the data for both the easy and hard tangram selection (BF_{10}=0.09 and BF_{10}=0.08, respectively) provided support for the null over the alternative that there would be a difference. The data for the charity donation variable was less clear. When participants played a non-violent, violent, or ultra-violent video game and were asked to make a donation to a local charity, the observed data provided more evidence for the null over the alternative hypothesis but not definitively so, BF_{10}=0.64. Bayes factors that hover around 1, which one could argue that BF_{10}=0.64 does, show similar amounts of evidence for both null and alternative hypotheses, thus failing to distinguish one having more evidentiary value over the other.

Table 7.

Summary statistics for Bayesian reanalysis of Chapter 3 experiment (F- and p-values included for comparison).

Dependent variable	Effect	BF_{10}	*F*	*p*
Helping behaviour	Main effect of video game	0.09	0.19	.830
Hurting behaviour	Main effect of video game	0.08	0.01	.987
Charity donation	Main effect of video game	0.64	2.54	.083

Discussion

For many researchers and theorists, violent video games have unequivocally negative effects on social behaviour, including the reduction of prosocial behaviour (Anderson et al., 2010; Greitemeyer & Osswald, 2009). Conversely, others report instances where no such relationship was revealed (Greitemeyer & Osswald, 2010; Tear & Nielsen, 2013; 2014). It may be that violent video game exposure requires several psychological preconditions in order to affect behaviour, effectively serving as a 'tipping point'. In this experiment we tested whether an existing hostile cognitive state qualified as such a precondition. To test this, we expected there to be a significant interaction between the prime and video game conditions, such that being exposed to both a hostile prime and an ultra-violent video game would have the largest effects on our helping and hurting measures, compared to other combinations of conditions. We observed no such relationship. Moreover, calculating the Bayes factors for these tests reveals considerable evidence for the null hypotheses over the proposed alternative hypothesis. We did, however, replicate the work of Bastian and colleagues (2011). Based on their work we hypothesised that playing a violent video game should increase self-dehumanisation. We supported this hypothesis by showing that playing an ultra-violent video game led participants to rate themselves lower on measures typically associated with human nature or human uniqueness. The accompanying Bayes factor showed very substantial evidence for the hypothesis.

We thus failed to demonstrate that exposure to violent video games can serve as a tipping point for negative social behaviour, which adds to the growing body of literature suggesting small-to-non-existent effects of violent video games (Ferguson, in press; Tear & Nielsen, 2013; 2014). We also failed to show that hostile semantic primes can influence behaviour, echoing current skepticism of the replicability of priming effects more generally (Doyen, Klein, Pichon, & Cleeremans, 2012; Pashler, Coburn, & Harris, 2012; Shanks et al., 2012). On first glance, our failure to support the 'tipping point' hypothesis may seem at odds with the result reported by Markey and Markey (2010), who showed that violent video games can

have negative effects on individuals, provided the individual is high in neuroticism and low in agreeableness and conscientiousness (this combination of traits is not necessarily the same as hostility but is indicative of being temperamental or volatile). Their results, however, relate to personality characteristics that put individuals at risk, that is, trait characteristics. We specifically tested whether the same argument could be made for state characteristics, and it appears that it can not.

Further, our results are at odds with the GAM. According to the GAM, video games influence behaviour by making hostile cognitions more salient. Here, we used a more direct semantic prime, specifically designed to make hostile cognitions salient, and paired that presentation with exposure to violent video games. Despite these efforts, we were not able to show behavioural differences between participants in this condition and the three other, control conditions. To the extent that hostile cognitions were actually present calls into question that ability for priming and cognition to influence behaviour. Unfortunately, we did not measure the salience of hostile cognitions in this experiment and so can't presume that the priming task worked as expected.

Another concern with this study is the small sample size. We decided a priori to collect data from as many participants as possible over the course of the semester, leading to us to collect 123 participants. The fact that this may have not been a large enough sample represents an inherent issue with experimental studies investigating stimulus-response effects of video games on behaviour. The number of participants you can test at once is contingent on the number of machines you can afford. In our case this led to testing one participant per hour. Given the small effect sizes reported in the thesis (and indeed in some meta-analyses, see Ferguson, in press), this would have required several years of testing.

Bastian and colleagues (2011) showed that those who played an ultra-violent video game felt more dehumanised than those who played a non-violent video game. The current study replicated this self-dehumanisation effect. This provides a potentially interesting mechanism that may explain why we were unable to show behavioural effects of media violence. If violent video games do reduce prosocial behaviour, that reduction may be masked by deliberate attempts to restore feelings of humanity. That is, participants who reported feeling less human after playing a violent video game may have taken deliberate action (helping behaviour) to re-establish feelings of humanness, thus negating any negative effect of the violent video games. Concepts of humanness and morality are tightly bound (Bastian, Laham, Wilson, Haslam, & Koval, 2011; Brandt & Reyna, 2011; Haslam, Bastian, Laham, & Loughnan, 2011), and there is already data to suggest that those who feel moral guilt are motivated to behave prosocially (Baumeister, Stillwell, & Heatherton, 1994; Freedman, Wallington, & Bless, 1967; Haidt, 2003; Ketelaar & Au, 2003). Future research ought to consider the role that self-dehumanisation may play in masking potential video game effects.

The possibility that participants' feelings of their own humanity may have masked a violent video game effect only highlights the importance of moving away from content-

focused theories of media effects, towards user-focused ones. The GAM prioritises the effect of content and cognitions on behaviour, but here we demonstrated that manipulating these constructs had no statistical impact on behaviour. Where the GAM focuses on content, other researchers have emphasised the significance of users shaping their experience with media Przybylski, Deci, Rigby, & Ryan, 2014). Theories of media effects need to move away from the content of games and start to consider *how* people play video games. As can be seen in these data, video game content had no impact on participants' behaviour.

Chapter 5 – General Discussion

While the work on violent video game effects on prosocial behaviour is younger and less developed than that on aggression, I was able to draw on literatures related to prosocial behaviour, such as moral licensing, economic game theory, and social priming, to test the robustness of the violent content hypothesis. What follows is a detailed summary of how the data presented in this thesis address the following research questions: (1) how resilient is the violent video game effect to changes in experimental procedure, (2) is the violent video game linked to particular measures, and (3) are people more susceptible to violent video game effects when they are primed with hostility?

Chapter 2 - **Failure to demonstrate that playing violent video games decreases prosocial behaviour** - addressed how resilient the violent video game effect is to changes in experimental procedure. I explored several intuitions for why a recent study (Greitemeyer & Osswald, 2010) did not show an expected violent video game effect. I tested whether their exposure times were too short by including both long (20min) and short (7min) exposures. I tested the intuition that their manipulation was not strong enough, that is, that their violent games were not violent enough, by using both modern and classic games. By making it easier for participants to help, I shifted the frequency of prosocial responses upward in order to negate any possible floor effects. Finally, I attempted a direct replication of the original study by following the methodology as close as possible. In none of these iterations was I able to reveal that playing violent video games leads to more or less prosocial behaviour than playing a non-violent or prosocial video game.

Chapter 3 - **Video games and prosocial behaviour: A study of the effects of non-violent, violent, and ultra-violent gameplay** - tested whether the violent video game effect is linked to particular measures. The preceding chapter documented a failure to show a violent video game effect on prosocial behaviour. In Chapter 3, I tested whether this failure was due to an idiosyncratic measure of prosocial behaviour. For this experiment I used two different measures of prosocial behaviour: a charity donation task and a tangram task. Again, in the event that the violent video game manipulation was not strong enough, I used the most violent video games legally available for purchase in Australia. I also tested whether the failure to demonstrate the violent video game effect was due to idiosyncrasies associated with using single exemplars of video game categories, e.g. one violent game, one non-violent game. Video games are, by their nature, incredibly complex stimuli, oftentimes with deep narratives and themes. Any effect, either increasing or decreasing prosocial behaviour, may be due to the specific idiosyncrasies of the game. To avoid this problem, I used multiple exemplars within the same video game category, e.g. two non-violent games, two violent games, two ultra-violent games. The data from this research again failed to reject the null hypothesis that video game effects on social behaviour should do not differ between violent and non-violent games.

Chapter 4 - **A test of the 'tipping point' hypothesis of violent video game effects** - examined whether violent video games can influence prosocial behaviour in the presence of a

hostile prime. I tested whether violent video games themselves are not enough to influence behaviour, but might if coupled with a hostile cognitive state. Participants played either a non-violent or ultra-violent game, and were exposed to either a neutral or hostile semantic prime. Consistent with the preceding experiments, I was again unable to show that violent video games have any negative effect on social behaviour. Perhaps most interesting was that the pairing of the violent video game and the hostile semantic prime was not enough to change participants' social behaviour. That is, when participants were primed with hostility, and then played a violent video game, they were no more or less prosocial than participants in any other combination of conditions.

In summary, using the standard methodology for demonstrating violent video game effects, I was unable to reject the null hypothesis that those who played a violent game would be less helpful than those who played a non-violent game. I tested several intuitions about possible changes to the methodology that would be more likely to reveal the effect, including increasing the exposure time, using modern games, manipulating the context in which the participant could help, using multiple dependent measures and video game exemplars, and pairing video games with a hostile prime (see Figure 6). It appears that the described methodological variations were still not sensitive enough to detect an effect of violent video games on prosocial behaviour (although the effect of video game content on self-perception was moderate-to-large).

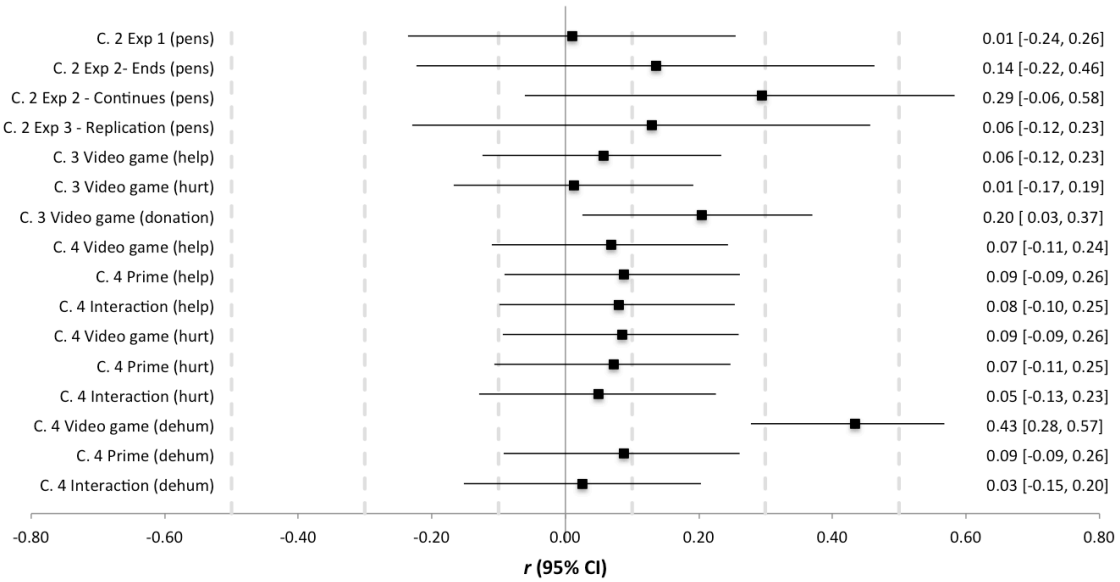


Figure 6. Forest plot demonstrating effect sizes for all effects of interest. Squares represent *r* point estimates with horizontal lines showing 95% CIs. Effects listed on left, with chapter number and DVs. Point estimates and 95% CIs listed in numerical form on right. Dashed grey lines represent effect size conventions (*r*=.10 as 'small', *r*=.30 as 'moderate', and *r*=.50 as 'large').

Relating to the literature

It's somewhat difficult to relate these null results to theory, given that null results are not often useful for confirming or denying theoretical accuracy (although Bayesian analyses, employed in Chapter 4, *can* test the evidential value in favour of null hypotheses). The experiments reported in this thesis draw heavily from the General Aggression Model (GAM; Anderson & Bushman, 2002) for making testable hypotheses. The GAM posits that repeated exposure to violent media creates, and subsequently reinforces the retrieval of, aggressive and antisocial cognitions, affect, and increases arousal. One example is the increase in what is known as the hostile attribution bias, where individuals exposed to violent video games are more likely to interpret ambiguous situations as hostile (Bushman & Anderson, 2002).

While the GAM is usually cited for work focussing on aggression, there have been some published studies that use the GAM to derive hypotheses testing violent video game effects on prosocial behaviour (e.g. Ballard & Lineberger, 1999; Greitemeyer & McLatchie, 2011; Greitemeyer & Osswald, 2010; Sheese & Graziano, 2005). Each of these studies makes the prediction that playing violent video games, compared to playing non-violent video games, should decrease subsequent prosocial behaviour, consistent with the GAM. Each of these studies subsequently reports evidence supporting the general 'violent video games decrease prosocial behaviour' prediction. Using these studies as a guide, each experiment reported in this thesis consequently adopted a similar hypothesis, that playing a violent video game would reduce prosocial behaviour. In none of my experiments was I able to find evidence to support this.

The GAM emphasises three critical stages that are important for understanding violent video game effects: (1) person and situation inputs; (2) present internal state routes; and (3) appraisal outcomes (DeWall, Anderson, & Bushman, 2011). An interruption in the process at any one of these three steps may be enough to explain my failures to demonstrate a violent video game effect. The first component relates to individual differences in personality and situational context (biological modifiers, environmental modifiers). While unlikely, it is possible that participants allocated to violent video game conditions had passive, or non-aggressive, personalities. For example, a review by Crick and Dodge (1994) suggests that when presented with ambiguous social events, children with aggressive personalities revert to a hostile interpretation. Presumably children with non-aggressive personalities would not automatically revert to a hostile interpretation, although it is important to point out that Crick and Dodge do not explicitly state so. It does seem unlikely, however, that each of my samples consisted entirely of non-aggressive, passive, or prosocial personalities, given that over 350 individuals participated in my experiments. Furthermore, to the best of my knowledge there is no evidence to suggest university students are more passive than the general population. Unfortunately, I did not include measures of personality and so cannot confirm the personality characteristics of my samples.

Another possibility is that participants' internal states, that is, their cognitions, affect, and arousal, were not meaningfully affected. The GAM posits that violent media increase the salience of aggressive cognitions or hostile attributions, such that, when faced with an event that demands a corresponding action, playing a violent game will lead to antisocial behavioural responses. None of the experiments in this thesis measure changes in cognitive, affective, or physiological experience, so a thorough examination of those mechanisms is beyond its scope. It may be possible, therefore, that the reason I was unable to show differential effects of video games is because the violent video games did not actually change participants' cognitions. This seems unlikely, as merely seeing a gun makes it easier to process a violent word (Anderson, Benjamin, & Bartholow, 1998). Couple this with the fact that in later experiments I selected the most violent games available to Australian consumers. It seems unlikely that these vivid and violent stimuli did not increase the salience of negative social scripts.

A final and, to my mind more likely, possibility is that any hostile or antisocial script that activated during my experiments was outcompeted by other scripts during the appraisal stage of the GAM. For every problem that can be solved with aggressive behaviour, there are likely many more nonaggressive options that contradict or compete with aggressive scripts. Indeed, the cognitive neoassociationist model (which forms the foundation of the GAM) states that concepts can exist next to each other despite being contradictory or competing. The GAM assumes that media consumers deliberately or automatically select the behavioural strategy that is most congruent with the media they are consuming. This ties into the single largest criticism of the GAM: its key assumption is that media users are passive consumers of media that are perfectly susceptible to its effects. Ivory (2013) categorises research making this assumption under the 'video games as stimuli' perspective of media research (although this is but one characteristic of the perspective). But other approaches to video game research place more credence on the idea that media consumers are not passive absorbers of media, and that they actively shape their experience with the game. Ivory lists three other research approaches that treat the media consumer as an active shaper of their experience: 'video games as avocation', 'video games as skill', and 'video games as social environment'. Przybylski and colleagues discuss video game violence in the context of goal attainment and motivation satisfaction, that is, players' aggression increases when they are prevented from attaining goals or satisfying motivations (Przybylski, Deci, Rigby, & Ryan, 2014; Ryan, Rigby, & Przybylski, 2006). Those who participated in my experiments may have held several motivations that were unaffected by the game content, for example, "play video games", or "complete an experiment for course credit".

Alternative mechanisms

Age of participants

A key issue with the data presented in this thesis is that all participants were mostly mature adults, ranging in age from 17-43. If the Catalyst Model (Ferguson et al., 2008) is correct, then exposing these participants to a violent video game for 20 minutes should have very little influence on their behaviour (and this is precisely what the data show). Indeed, another failure to demonstrate an effect of violent video games on prosocial behaviour used adult samples (Greitemeyer & Osswald, 2010). Studies using child samples, however, show that children are more aggressive after playing a violent game than young adults (Griffiths, 1999).

There are several possible interpretations for this result. First, older participants may have habituated to video game violence and hence show diminished response. Second, older participants are perhaps more familiar with the debate on video game violence and adjust their behaviour, possibly to disrupt an experiment seeking to confirm violent video game effects (Bender, Rothmund, & Gollwitzer, 2013). A final explanation is that young children are more malleable and that by the onset of early adulthood the factors contributing to prosocial inclinations have been well established and are not easily shifted by playing violent video games. Adapting the experiments introduced here for assessment with children will shed much needed light on this issue (fortunately, prosocial measures are ethically permissible for very young participants, unlike traditional measures of aggression).

Stimuli matching

There are other factors that may influence behaviour in violent video game studies that are not considered in this thesis. Pace of action is one variable that has been described as a potentially important confound of video game violence research (Adachi & Willoughby, 2011a). Elson and colleagues showed that pace of action can sometimes interact with violent content in strange ways to affect behavioural arousal (Elson, Breuer, Van Looy, Kneer, & Quandt, 2013). For example, while pace of action had no effect on aggression, fast-paced video games did inhibit participants' body movement, especially when participants played a non-violent game. This counter-intuitive finding requires more research to clarify the role of pace of action in video game research. While I did not record any physiological measurements in my experiments, I did include a crude measure of self-reported arousal. There were no systematic differences of self-reported arousal between violent and non-violent video game conditions that would explain my null results. In my experiments, non-violent video games may have moved participants' arousal from baseline to a level comparable to that which would be expected for violent video games, thus reducing any possible mean difference to effectively zero. It will thus be important in future research to

include pre- and post-measures of arousal to track changes in arousal during the experimental session.

Another factor that is not accounted for in my experiments, and that has the potential to influence behaviour is competitiveness. Adachi & Willoughby (2011a; 2011b) argue that violent video games not only differ from non-violent games in the amount of violent content they show, but they also tend to be more competitive. That is, those who play a violent video game are also playing a competitive video game. Additionally, traditional measures of lab-based aggression (Taylor Competitive Reaction time Task, see Anderson & Dill, 2000; Anderson & Murphy, 2003) are said to also measure competitiveness. Thus, any experiment that does not match stimuli on competitiveness, while also using a measure of aggression distinct from competitiveness, must only be limited in its value. To address these issues, Adachi & Willoughby (2011b) isolated video game competitiveness by matching violent games and non-violent games on difficulty and pace of action, but varying the level of competitiveness. They also included a measure of aggression that is not related to competitiveness (the Hot Sauce Paradigm). For example, the violent-competitive game was a fighting game where players compete against a computer controlled character, whereas the violent-noncompetitive game was a first-person shooter where players need to survive for as long as possible against an "undead horde". They found that video game competitiveness elevated aggressive behaviour in the short-term, regardless of the level of violent content. In other words, both violent and non-violent games increased aggression so long as they were also competitive. It remains possible that my video game stimuli were unintentionally matched for competitiveness across all my experiments, thus potentially explaining the repeated null effects. However, if this were the case, it would suggest that concern over the impact of violent video games on behaviour needs to be directed towards competitiveness in content and away from violence.

A final confound that has only very recently been systematically evaluated in video game research is competence-impedance: that is, frustration arising from difficulty. Przybylski and colleagues (Przybylski, et al., 2014; Ryan, et al., 2006) describe motivational models for how video game engagement shapes psychological processes. Specifically, they discuss the role that video games play in fulfilling basic psychological needs for competence, autonomy, and relatedness. In a clever series of studies, Przybylski and colleagues show that aggression can be increased by interactive elements of games that serve to impede players' fundamental psychological need for competence (Przybylski et al., 2014). That is, players who are asked to play video games with difficult-to-use controls showed more aggression than those who are given intuitive controls, irrespective of violent content. While, I did not consider the role of competence-impedance when designing my studies, I did include a crude self-report measure of frustration. As with self-reported arousal above, while there were occasional differences between games on this measure, there was no systematic pattern that would consistently explain several null results.

While I did not control for the specific confounds discussed above, I did attempt to address the issue of idiosyncratic differences between video game stimuli within video game categories. For example, in Chapter 3, by using games of *differing* violence levels within the *same* franchise I hoped to keep any other differences, such as pace of action, competitiveness, and competence-impedance, to a minimum. Nonetheless, the complex nature of video games as stimuli means that there are considerable obstacles to attempts at fully controlling for differences within experimental video game categories and doing so presents itself as an impediment to progress that researchers need to confront.

I did not use the above-described method for all my studies, however, and relied on stimulus matching for traditional matching variables (e.g. frustration, interest, and arousal, see Anderson & Dill, 2000). Using this method, researchers ask participants to rate the video game stimuli on measures deemed important for matching (e.g. frustration, interest, arousal), concluding the games to be matched if there is no statistical difference between the games on those measures. There were rarely statistically significant differences between my video game stimuli on measures of frustration, interest, or arousal (although, in Experiment 1 of Chapter 2, the violent was more frustrating than the non-violent game and the prosocial game, but not the anti-social game), but researchers have warned about using null results to infer stimuli are matched, because traditional null hypothesis significance testing (NHST) statistical procedures cannot be used to support the null (Hilgard, Engelhardt, Bartholow, & Rouder, 2015). Bayesian model comparison is more appropriate for assessing whether stimuli match because it allows for principled tests between the null hypothesis and the alternative hypothesis. Indeed, when researchers apply a Bayesian approach to check for confounding differences between video game stimuli, they often find examples of NHST comparisons that fail to reject the null, but Bayesian analyses for those same data showing evidence for a difference in comparisons (Hilgard, Engelhardt, Bartholow, & Rouder, 2015).

In what appears to be the best method for manipulating video game stimuli, other researchers (Elson et al., 2013; Hilgard, Engelhardt, Bartholow, & Rouder, 2015) recommend using video game modification tools (or 'modding'). With these methods, researchers take an existing game (or create their own) and 'mod' particular aspects of the game, such as character appearance, in-game items, or the goals of missions or quests. This way, researchers can manipulate the game to show violent or non-violent (or prosocial) content, while keeping everything else in the games identical. A crude example would be to take a first-person shooter game, such as Doom, and mod the game so that instead of killing as many monsters as possible with a rocket launcher, players could use a happiness ray to turn sad people happy. The one downside of this method is that the quality of the mod is dependent on the researcher's expertise. Unless the researcher has considerable experience and knowledge, the quality of the modified content may be poor, such that it would be easy to distinguish a "modded" game from a typical best-seller.

Strengths and limitations

The key strengths of the experiments in this thesis lie in its 'back to basics' approach to violent video game effects. Before I started this work, the prevailing wisdom was that violent video game effects are strong (Bushman & Huesmann, 2006) and violent video games are detrimental for society, for some authors on par with cigarette smoking (Bushman & Anderson, 2001). I have rigorously and methodically dissected the claim that violent video games negatively influence social behaviour, leading me to question the size and practical significance of previously reported effects. The effect of violent video games on social behaviour was elusive across all experiments presented in this thesis, while other factors showed demonstrable effects on behaviour. For example, changing the context of the helping situation presented to participants from a one-shot experience to a continued interaction experience served to increase rates of helping by a factor of two (Experiment 2, Chapter 2).

I tested previously untested intuitions about violent video game effects. For example, Greitemeyer and Osswald (2010, p. 215) make the following observation after their failure to show an expected effect of violent video games:

> "Note, however, that participants in the present study played the video games for only 8 min, whereas most previous experimental video game studies used considerably longer exposure times. Thus, it is conceivable that longer exposure to the video games would have yielded significant differences among the neutral and the aggressive video game condition."

Greitemeyer (2011) also speculated that "modern, graphically sophisticated games may be more involving and thus should affect helping behaviour to a greater extent'' (p.252). Despite implementing these suggestions, I was still unable to show the predicted negative effect of violent video games on prosocial behaviour.

That Greitemeyer's (Greitemeyer, 2011; Greitemeyer & Osswald, 2010) intuitions do not bear out in the data is something that speaks to the important distinction between direct and conceptual replications. I speculate that until researchers sufficiently catalogue an effect and its moderators they will overestimate the transferability of an effect from one context to another (an intuition that itself may not hold under scientific scrutiny). The value of replication is a topic currently under scrutiny in psychology (see the Perspectives on Psychology Special Section on Replicability, Pashler & Wagenmakers, 2012). Direct replications, that is, those replication attempts that mirror the original methodology as closely as possible, undertaken by both the original and other laboratories, are needed to verify the reliability of an effect (Simons, 2014).

Are prosocial behaviour and aggression necessarily related?

One glaring issue with this work is how I have considered prosocial behaviour. To date, there is very little work discussing video game effects on prosocial behaviour. I turned to work on aggression in order to derive my hypotheses, given that presumably similar, but

reversed, mechanisms are at play. That is, where violent games are predicted to increase aggression, others and myself have predicted that violent games should decrease prosocial behaviour (Ballard & Lineberger, 1999; Chambers & Ascione, 1987; Greitemeyer & Osswald, 2010; Sheese & Graziano, 2005). The issue is that I have treated aggression and prosocial behaviour as if they were two sides of the same coin. That which applies to aggression, however, may not necessarily apply to prosocial behaviour.

To support my treatment of aggression and prosocial behaviour I turn to work showing that violent video games can increase aggression and decrease prosocial behaviour simultaneously. Silvern and Williamson (1987) showed tentative evidence that aggression and prosocial behaviour are linked. After viewing a violent TV show or playing video game aggressive play in children increased and prosocial play decreased. The effect, however, was not as strong for prosocial behaviour as it was for aggression, possibly because of extreme values in one cell (baseline prosocial behaviour in girls). Crick (1996) demonstrated that teacher ratings of overt and relational aggression were statistically negatively correlated with prosocial behaviour. This work was expanded by Gentile and collages (2011) to include more types of media. They found that media violence exposure statistically predicted subsequent aggression and prosocial behaviour five months apart.

Not all research suggests an inverse relationship, however. Hinshaw and colleagues (1989) tested the effect of methylphenidate (a central nervous stimulant used in the treatment of ADHD) on aggressive and prosocial behaviours in hyperactive boys. They found that aggression decreased after treatment, whereas prosocial behaviour remained unchanged at the group level (although there were strong effects on prosocial behaviour for some individuals), suggesting potentially different mechanisms at work. Indeed, further work on this topic ought to consider whether models used to predict changes in antisocial behaviour can be used to predict similar changes in prosocial behaviour.

The issue of power

One limitation that is persistent across all studies in this thesis is the issue of power. This is a legitimate concern, and a frequent one when null results present. While underpowered studies can be responsible for the null results, the cells sizes reported in these chapters are comparable to the cell sizes in the published literature. And to the extent that the effect sizes reported in this thesis are accurate, the sample sizes that would be needed to render the effect significant would be impractical and largely not meaningful. Indeed, meta-analysis of the first experiments in the thesis confirm a small and non-significant effect of violent video games on prosocial behaviour (r=.11, p=.290).

To further address the issue of underpowered studies and small sample sizes, I conducted Bayesian analyses in conjunction with traditional NHST. No matter how much we may like it to be, traditional NHST analyses cannot provide meaningful interpretations from null results - they cannot distinguish between data providing evidence supporting the null and data that are

insensitive to it. Bayesian analyses provide a valuable tool in Bayes factors, which can distinguish between data showing evidence for the alternative hypothesis, data showing evidence for the null, and data that are insensitive to either hypothesis. Setting up the spectrum of evidence this way means that Bayes factors are quite good at indicating when insufficient data have been collected. If the sample size is too small, then the Bayes factors will hover around a value of 1, representing no change in beliefs. Bayes factors only become substantially larger (supporting the alternative hypothesis) or smaller (supporting the null hypothesis) when the sample size becomes large. If the obtained Bayes factor is not sufficiently large, more data can be collected. While optional or conditional stopping is a serious and dangerous form of research flexibility in NHST (Simmons, Nelson, & Simonsohn, 2011), it is not a problem for Bayes factors (Dienes, 2011; Rouder, 2014). Indeed, this data could be freely collected until the obtained Bayes factor is satisfyingly convincing. In a field where there is so much contention around null results (Hilgard, Engelhardt, Bartholow, & Rouder, 2015), Bayes factors serve as a useful tool for interpreting evidence for the null.

Video games studied in isolation

A broad issue with violent video game research (the present thesis included) is that video games are often only considered in isolation. Originally a valid platform for new research, the question "do violent video games make people aggressive?" now only provides a narrow insight to what is part of a broader question: Do video games influence people in ways that are categorically different from other activities? As it stands, there have been few studies comparing the effect of violent video games to other types of games and leisure activities on aggression and prosocial behaviour (i.e. their ability to cause short-term changes in social cognitions and behaviours).

According to the GAM, any stimulus (including video games) should be capable of having short- and long-term effects on behaviour (Buckley & Anderson, 2006; Swing, Gentile, & Anderson, 2008). It can therefore be expected that a variety of activities aside from video games will have some measurable effect on behaviour. Competitive sport, for example, commonly rewards aggressive behaviour that is simultaneously contrary to normal societal norms (Coulomb-Cobagno & Rascle, 2006), yet is perceived as legitimate (Conroy, Silva, Newcomer, Walker, & Johnson, 2001). For example, Wann and colleagues (2003) reported that players who identify strongly with a team were more likely to engage in anonymous hostile acts towards members of a rival team even though those hostile acts did not provide a competitive advantage for their own team. According to the script theory component of GAM, if players are constantly engaging in hostile or aggressive behaviour and are rewarded for it, then playing contact sport should create well-rehearsed, aggressive scripts that are easily accessed in response to social events. Script theory would also posit that once these aggressive scripts are a mainstay in a person's repertoire, then playing a one-off game

of sport should also prime short-term increases in aggression. As with violent video games, competitive sport is related to aggressive behaviour (Wann, Haynes, McLean, & Pullen, 2003). Remembering that if we want to determine the relevant societal cost of violent video games, then we need to compare them against other potentially aggression-inducing activities. For example, are violent video games a stronger prime for aggressive behaviour than competitive sports? As discussed in the introduction, there is some speculation that the interactive component of video games compared to other violent media makes them a candidate for societal concern (Anderson et al., 2003). It may be, however, that competitive sports are more interactive and are actually stronger primes of aggressive behaviour than violent video games. Future research on violent video game effects ought to test the ability of theoretical models to make predictions about other leisure activities.

Suggestions for future research

Based on the above, there are four primary areas in which continuing research is needed. First, there remains the need for large-scale, collaborative, direct replication attempts. This recommendation has the dual effect of testing the reliability of purported effects, while also collecting large samples to obtain satisfactory power. Second, further consideration of the relationship between aggression and prosocial behaviour and, potentially, the formulation of models specific to prosocial behaviour, would allow for more precise predictions of video game effects on prosocial behaviour. Third, researchers could calculate Bayes factors in conjunction with traditional NHST to squeeze out the last interpretational drip from null results. This would be particularly useful for alleviating concern of publication bias in the field (Ferguson, 2012). Finally, I suggest that violent media researchers could take models for predicting video game-influenced behaviour change and apply those models to other leisure activities to determine whether violent video games are necessarily a special class of stimuli. Several everyday scenarios can contribute to changes in social behaviour (e.g. being stuck in traffic). If violent media researchers want to suggest changes to public policy based on their research, then violent video games need to be tested for their relative ability to influence people first.

Implications for the field

The experiments in this thesis, and the conclusions arising from them, have important implications for how to conduct research on violent video game effects. Researchers are presently debating the size of the violent video game effect, with estimates ranging from medium (r=.24, Anderson et al., 2010) to near zero (r=.04, Ferguson, in press). Using these effect size estimates, a simple two sample independent groups analysis would need between 100 participants per cell (as per Anderson et al., 2010 estimate) and 2500 participants per cell (as per Ferguson, in press estimate) in order to be appropriately powered. In this context consider the typical method for testing violent video game effects. Researchers usually want to compare data from at least two groups of participants. Independent groups designs such as

these require larger sample sizes than repeated measures designs. Also recall that video game stimuli need to be presented on a video game device. This means that the rate of testing participants is dependent on the number of consoles the researcher has access to (in my case this was one participant per hour). As can be seen, collecting data this way is inherently time consuming. But from the power calculations above, it is clear that the traditional method for investigating is woefully inefficient and logistically very difficult.

In order for accurate statistical interpretation, that is, no false positives or false negatives, researchers need to run appropriately powered studies. Given the pragmatically difficult nature of collecting upwards of 200 participants (100 per cell), exacerbated by testing one participant per session, running appropriately powered studies will remain out of reach. To overcome this, Hilgard and his colleagues (2015) make the practical recommendation to consider multi-site investigations, where data are collected across several sites and several research groups. A multi-site strategy nullifies the pragmatic difficulties of collecting large samples by dividing the effort between labs, thus allowing for large sample sizes to be collected. Additionally, multi-site strategies could also be adversarial in order to alleviate concerns of bias or competence across research teams.

In closing, the data reported in this thesis speak to the issue of violent video games in our society. Some researchers find small but statistical relationships between violent video games and negative social behaviour, and on that basis suggest that violent video games put individuals at risk of committing real-world violence (Rushton, 2013). There are even some authors that suggest the effect of violent media on aggression is similar in size to the effect of cigarette smoking on lung cancer (Anderson et al., 2003). Yet, as documented in this thesis, the literature is still struggling to demonstrate reliable and robust effects of violent media. It may be that violent media does influence us, but any effect seems idiosyncratic and user-dependent, rather than generalised. Thus, recommendations for public policy must take a more nuanced and metered stance, lest the violent media field suffer a crisis of credibility (Hall, Day, & Hall, 2011; Ferguson, 2014). It is alarming that some are moving to end-discussion when any violent video game effect is difficult to tie down. Perhaps more alarming is the suggestion by key authors in the field that those who disagree with the 'violent media contributes to aggressive behaviour' hypothesis do so for their own personal gain (Huesmann, 2010) or for conspiratorial reasons (Strasburger, Donnerstein, & Bushman, 2014). Clearly, there are issues in the field beyond the scope of this thesis. What this thesis can speak to, however, is the generaliseability of violent media effects - the experiments reported here suggest that violent media effects are fleeting.

Appendices

Appendix A: Video-game Questionnaire

Video-game Questionnaire – Session 1

Using the scale below, indicate the extent to which you agree with the following statements.

1	2	3	4	5	6	7	8	9
Strongly Disagree								**Strongly Agree**

1. I found the game frustrating.	1 2 3 4 5 6 7 8 9
2. The game got my heart racing.	1 2 3 4 5 6 7 8 9
3. I found myself thinking about other things while playing the game.	1 2 3 4 5 6 7 8 9
4. The game was too hard.	1 2 3 4 5 6 7 8 9
5. I found the game stimulating.	1 2 3 4 5 6 7 8 9
6. I didn't care about what was happening in the game.	1 2 3 4 5 6 7 8 9
7. I couldn't get the game to do what I wanted.	1 2 3 4 5 6 7 8 9
8. I have played other games that are more exciting.	1 2 3 4 5 6 7 8 9
9. The game kept my attention.	1 2 3 4 5 6 7 8 9
10. The game was easy to control.	1 2 3 4 5 6 7 8 9
11. The game wasn't very exciting.	1 2 3 4 5 6 7 8 9
12. The game was interesting.	1 2 3 4 5 6 7 8 9
13. I felt violent while playing the game	1 2 3 4 5 6 7 8 9
14. My actions in the game helped other characters	1 2 3 4 5 6 7 8 9

What is your age? _____

What is your gender? _____

What is your nationality? _____
(e.g., Australian, Chinese, etc)

What is your ethnicity? _____
(e.g., Caucasian, Asian, etc)

Appendix B: Priming materials

Hostile prime - Version 1

1.	him was present she always	1.	She was always present/him
2.	from are *Spain* oranges temperature	2.	Oranges are from Spain /temperature
3.	ball the throw toss silently	3.	Throw the ball silently /toss
4.	**shoes give replace *old* the**	4.	Replace the old shoes /give
5.	he observes occasionally people watches	5.	He occasionally observes people /watches
6.	**be will swear *angry* they**	6.	They will be angry /swear
7.	**sky the seamless *aggressive* is**	7.	The sky is aggressive/seamless
8.	ate she it selfishly all	8.	She ate it all /selfishly
9.	be to back *true* better	9.	Better to be true /back
10.	prepare the gift wrap neatly	10.	Wrap the gift neatly /prepare
11.	sew *expensive* buy item the	11.	Buy the expensive item /sew
12.	he *clever* drops can seem	12.	He can seem clever /drops
13.	**are we *confrontational* courteous sometimes**	13.	We are sometimes courteous /confrontational
14.	the push wash frequently clothes	14.	Wash frequently the clothes /push
15.	us *tennis* sing play let	15.	Let us play tennis /sing
16.	should now withdraw charming we	16.	We should now withdraw /charming
17.	**somewhat prepared I was *combative***	17.	I was somewhat prepared /combative
18.	**sunlight makes temperature *pushy* people**	18.	Sunlight makes people pushy /temperature
19.	is *tall* he usually studying	19.	He is usually studying /tall
20.	**a have *spiteful* party holiday**	20.	Have a spiteful party /holiday
21.	picked throw apples hardly the	21.	The hardly picked apples /throw
22.	drink this looks seems *cold*	22.	This drink seems cold/looks
23.	they obedient him often meet	23.	They meet him often /obedient
24.	**there are they *nasty* going**	24.	They are going there /nasty
25.	knits *occasionally* he occasionally them	25.	He occasionally knits them /dependent
26.	**studies she texts *belligerent* him**	26.	She studies beligerent texts /him
27.	*constant* it hides there over	27.	It hides over there /constant
28.	is he *present* plant so	28.	He is so present /plant
29.	***malicious* alone very are they**	29.	They are very malicious /alone
30.	send I mail it over	30.	I send it over /mail

Hostile prime - Version 2

31.	that sometimes there were they	31. They were sometimes there/ that
32.	from are *mars* men temperature	32. Men are from Mars/ state
33.	cup the place set carefully	33. Place the cup carefully/ set
34.	**you collect gather *new* the**	34. Collect the new toy/ gather
35.	he observes occasionally people watches	35. She sometimes cooks food/ bakes
36.	**be can vow *unfriendly* it**	36. It can be unfriendly/ vow
37.	**animal the eating *killing* is**	37. The animal is killing/ eating
38.	took he the thoughtlessly most	38. He took the most/ thoughtlessly
39.	be to back *true* preferable	39. Preferable to be right/ behind
40.	make the laundry fold carefully	40. Fold the laundry carefully/ make
41.	stitch *rare* collect thing the	41. Collect the rare thing/ stitch
42.	She *alert* falls be might	42. She might be alert/ falls
43.	**can they *hostile* polite be**	43. They can be hostile/ polite
44.	the thrust clean regularly room	44. Regularly clean the room/ thrust
45.	we *research* eat conduct can	45. We can conduct research/ eat
46.	ought to return endearing they	46. They ought to return/ endearing
47.	**quite ready they were *quarrelsome***	47. They are quite quarrelsome/ ready
48.	**noise makes noise *violent* dogs**	48. Noise makes dogs violent/ sound
49.	is *heavy* she usually sleeping	49. She is often sleeping/ heavy
50.	**a have *mean* affair event**	50. Have a mean affair/ event
51.	harvested toss corn rarely the	51. The rarely harvested corn/ toss
52.	dog this tired seems *freezing*	52. This dog seems tired/ freezing
53.	we compliant there regularly go	53. We regularly go there/ compliant
54.	**this are they *offensive* doing**	54. They are doing this/ offensive
55.	makes dependent she sometimes them	55. She sometimes makes them/ depende
56.	**writes he words *militant* her**	56. He writes militant words/ Her
57.	*sustained* it ran there over	57. It ran over here/ sustained
58.	is he *absent* shrub very	58. He is very absent/ shrub
59.	***cruel* singular very was it**	59. It was very cruel/ singlular
60.	pass I give it forward	60. I pass it forward/ give

Neutral prime - Version 1

1.	him was present she always	1.	She was always present/him
2.	from are *Spain* oranges temperature	2.	Oranges are from Spain /temperature
3.	ball the throw toss silently	3.	Throw the ball silently /toss
4.	**shoes give replace *red* the**	4.	Replace the red shoes /give
5.	he observes occasionally people watches	5.	He occasionally observes people /watches
6.	**be will swear *happy* they**	6.	They will be happy/swear
7.	**sky the seamless *blue* is**	7.	The sky is blue/seamless
8.	ate she it selfishly all	8.	She ate it all /selfishly
9.	be to back *true* better	9.	Better to be true /back
10.	prepare the gift wrap neatly	10.	Wrap the gift neatly /prepare
11.	sew *expensive* buy item the	11.	Buy the expensive item /sew
12.	he *clever* drops on seems	12.	He seems clever /drops on
13.	**are we *true* courteous sometimes**	13.	We are sometimes courteous /true
14.	the push wash frequently clothes	14.	Wash frequently the clothes /push
15.	us *tennis* sing play let	15.	Let us play tennis /sing
16.	should now withdraw charming we	16.	We should now withdraw /charming
17.	**somewhat prepared I was *rested***	17.	I was somewhat prepared /rested
18.	**sunlight makes temperature *tanned* people**	18.	Sunlight makes people tanned/temperature
19.	is *tall* he usually studying	19.	He is usually studying /tall
20.	**a have *sumptuous* party holiday**	20.	Have a sumptuous party /holiday
21.	picked throw apples hardly the	21.	The hardly picked apples /throw
22.	drink this looks seems *cold*	22.	This drink seems cold/looks
23.	they obedient him often meet	23.	They meet him often /obedient
24.	**there are they *balanced* going**	24.	They are going there /balanced
25.	knits *occasionally* he occasionally them	25.	He occasionally knits them /dependent
26.	**studies she texts *complicated* him**	26.	She studies complicated texts /him
27.	*constant* it hides there over	27.	It hides over there /constant
28.	is he *present* plant so	28.	He is so present /plant
29.	***cautious* alone very are they**	29.	They are very cautious /alone
30.	send I mail it over	30.	I send it over /mail

Neutral prime - Version 2

31.	that sometimes there were they	31.	They were sometimes there/ that
32.	from are *mars* men temperature	32.	Men are from Mars/ state
33.	cup the place set carefully	33.	Place the cup carefully/ set
34.	**you collect gather *new* the**	34.	Collect the new toy/ gather
35.	he observes occasionally people watches	35.	She sometimes cooks food/ bakes
36.	**be can vow *ready* it**	36.	It can be ready/ vow
37.	**animal the eating *running* is**	37.	The animal is running/ eating
38.	took he the thoughtlessly most	38.	He took the most/ thoughtlessly
39.	be to back *true* preferable	39.	Preferable to be right/ behind
40.	make the laundry fold carefully	40.	Fold the laundry carefully/ make
41.	stitch *rare* collect thing the	41.	Collect the rare thing/ stitch
42.	She *alert* falls be might	42.	She might be alert/ falls
43.	**can they *near* polite be**	43.	They can be near/ polite
44.	the thrust clean regularly room	44.	Regularly clean the room/ thrust
45.	we *research* eat conduct can	45.	We can conduct research/ eat
46.	ought to return endearing they	46.	They ought to return/ endearing
47.	**quite ready they were *content***	47.	They are quite content/ ready
48.	**noise makes noise *jumpy* dogs**	48.	Noise makes dogs jumpy/ sound
49.	is *heavy* she usually sleeping	49.	She is often sleeping/ heavy
50.	**a have *lavish* affair event**	50.	Have a lavish affair/ event
51.	harvested toss corn rarely the	51.	The rarely harvested corn/ toss
52.	dog this tired seems *freezing*	52.	This dog seems tired/ freezing
53.	we compliant there regularly go	53.	We regularly go there/ compliant
54.	**this are they *fair* doing**	54.	They are doing this/ fair
55.	makes dependent she sometimes them	55.	She sometimes makes them/ dependent
56.	**writes he words *intricate* her**	56.	He writes intricate words/ Her
57.	*sustained* it ran there over	57.	It ran over here/ sustained
58.	is he *absent* shrub very	58.	He is very absent/ shrub
59.	***vigilant* singular very was it**	59.	It was very vigilant/ singlular
60.	pass I give it forward	60.	I pass it forward/ give

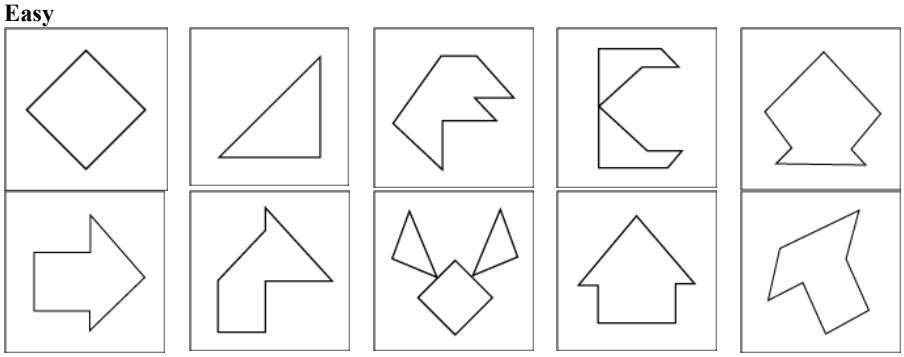
Appendix C: Tangram task

Participant ID:

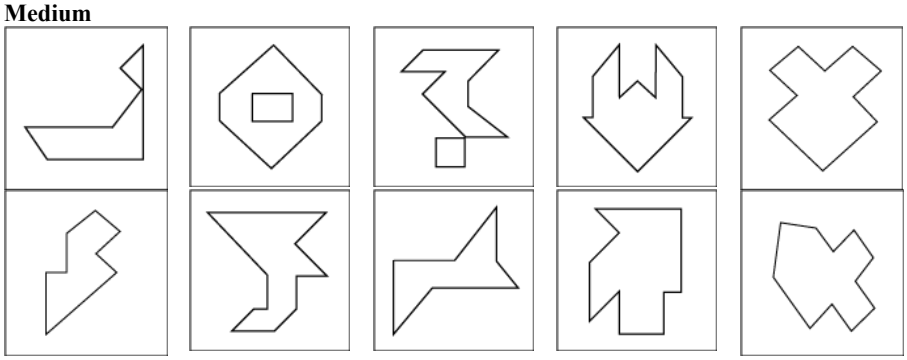
_____

Please choose 11 of the following tangram puzzles for your partner to complete. Remember that if your partner completes 10 out of the 11 tangrams you selected in ten minutes they will win the gift certificate. Although most people pick from the medium category, we encourage you to select tangrams from different categories. Circle the tangram you want to assign to your partner. Once you have selected all 11 tangrams, please inform the experimenter.

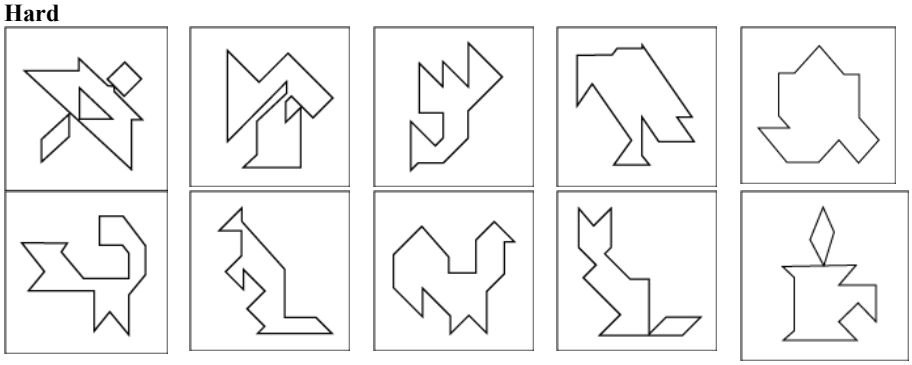
Easy



Medium



Hard



Appendix D: Self-dehumanisation questionnaire

Thinking about myself

Please think about the experiences you had while playing the video games and answer the following questions:

1	2	3	4	5	6	7
Not at all						**Very much so**

1. I felt like I was refined and cultured	1 2 3 4 5 6 7
2. I felt like I lacked self-restraint, like an animal	1 2 3 4 5 6 7
3. I felt like I was rational and logical, like I was intelligent.	1 2 3 4 5 6 7
4. I felt like I was open minded, like I could think openly about things	1 2 3 4 5 6 7
5. I felt like I was unsophisticated.	1 2 3 4 5 6 7
6. I felt like I was emotional, like I was responsive and warm.	1 2 3 4 5 6 7
7. I felt superficial, like I had no depth.	1 2 3 4 5 6 7
8. I felt like I was mechanical and cold, like a robot.	1 2 3 4 5 6 7

Human Nature (4, 6, 7r, 8r)
Human Uniqueness (1, 2r, 3, 5r)

Appendix E: Debriefing interview

Debrief Interview

1. What did you think the purpose of the session was?

_____

_____

_____

_____

2. Have you ever completed tasks, or undergone experimental protocol, like those in this experiment before?

_____

_____

_____

_____

3. Was there anything strange about the experiment or anything that didn't seem quite right?

_____

_____

_____

_____

4. Do you think your response to any task affected how you responded on another task?

_____

_____

_____

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