SOMETHING IS ROTTEN IN THE STATE OF AGGRESSION RESEARCH:

NOVEL METHODOLOGICAL AND THEORETICAL APPROACHES

TO RESEARCH ON DIGITAL GAMES AND HUMAN AGGRESSION

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I can't believe schools are still teaching kids about the null hypothesis.

I remember reading a big study that conclusively disproved it years ago.

- Randall Munroe, xkcd

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Summary

This dissertation offers a comprehensive critique of the current state of research on violent game playing and aggressive outcomes. It discusses twenty-five years of research on violence in digital games and aggression, including empirical evidence, theoretical perspectives, and the heated debates in both the public and academia. The main focus here is on methodological issues limiting the conclusiveness of the research, particularly experiments conducted in psychological laboratories. By suggesting methodological advancements in the study of game violence effects, the thesis wants to offer new perspectives on digital games and aggression to move forward the field and the ideological debates that surround it. The thesis comprises a total of 5 peer-reviewed journal articles (of which 3 are published, one is accepted and in press, and one is under review) that include data from one original study and a secondary analyses of 3 further studies.

The first part of the thesis consists of a detailed review of the current scientific literature on violent game effects with a focus on the theories that have been developed to explain the relationship between the use of digital games and aggression. Important theoretical shortcomings and fallacies of social-cognitive perspectives on how aggression is acquired through violent media contents are identified and discussed.

The second part is a methodological critique of laboratory experiments in research on the effect of violent games. First, common problems and pitfalls in the manipulation of violence as an independent variable and improper control of relevant confounding factors are discussed. The modification of game content ("modding") is suggested as a novel method to meet the requirements of rigorous internal validity and sufficient external validity in psychological laboratory experiments. The advantages of this method are illustrated by the results of an experiment in which it was used. This is followed by an examination of one of the most popular laboratory measures of aggressive behavior (the Competitive Reaction

Time Task), providing evidence from three studies that the unstandardized use in the scholarly literature poses a threat to its interpretability and generalizability.

The dissertation concludes with an analysis of the scientific discourse on the game violence-aggression link, and the ways in which it is shaped by ideological convictions that affect both the theoretical assumptions and the methodological procedures. This duality of ideologies present in theory and methods constitutes a threat to violent game effects research, as it causes the field to stagnate. It is argued that this stagnancy can only be resolved through methodological rigor that will, ultimately, advance inadequate theories of media effects.

Introduction

The debate about harmful effects of popular media began long before the television or the personal computer entered our everyday life. During the first half of the 20th century it were mostly the radio and comic books that came under criticism (Ferguson, 2013a), but already in the late 18th century the so-called "reading mania", particularly regarding the excessive consumption of fiction novels by females, caused a heated dispute. Now, as then, there are two sides arguing whether and how morally objectionable media content (such as displays of violence) can affect its users. While the link between exposure to violent media and aggression has been debated in academia for decades (J. Anderson, 2008), the rise of the medium of digital games as a means for media users to *cause* (and enjoy) on-screen violence with a simple press of a button sparked a new controversy in science and the public, that has been ongoing for more than twenty-five years now.

Researchers on both sides of this debate (sometimes dubbed "believers" and "skeptics") have a great interest in understanding the antecedents and underlying mechanisms of aggression, as well as in reducing violence in society. Many scholars are convinced that a strict regulation of violence in games would lead directly to a reduction of problematic behaviors and crime rates, and that those opposing regulation induce a societal risk (e.g., Huesmann & Skoric, 2003). Others worry that the debate on media violence could distract from societal issues they consider more relevant to the etiology of aggression, such as poverty or inequality, and ultimately cause harm as these factors get ignored (Ferguson, 2013b). This tension between groups of scholars who are, naturally, convinced of the validity of their scientific opinion, but who also consider the behavior of the 'other side' to be harmful or dangerous, is a breeding ground for a heated debate often torn by ideological convictions (Grimes, J. Anderson, & Bergen, 2008). Yet both sides agree that science can only

provide an answer to the question whether violence in digital games can cause aggression on two conditions:

1. Theories and models must be able to accurately describe the effect mechanisms of violent games and offer predictions from which testable, falsifiable hypotheses can be derived.

2. Empirical operationalizations of these mechanisms and the measurement of aggression need to be objective, reliable, and valid, so that they can be properly interpreted and generalized.

One major reason for disagreement among scholars, however, is a dissent whether (or to what extent) these two conditions have been met.

The belief that exposure to violence in games increases aggression has been closely tied to a social-cognitive perspective on media effects and aggression, specifically Bandura's (1978) observational social learning theory. The underlying assumption is, in essence, that media characters function as models for behavior and that humans learn through the observation of game avatars just like they learn through the observation of others in their physical environment. Thus, observing on-screen violent behaviors being rewarded or punished would respectively increase or decrease aggressive tendencies in game players. Given that the playing of many games, e.g. the notorious first-person shooters, is somehow tied to the killing of opponent avatars, one might argue that, theoretically, the conditions for this mechanism are met. A popular formula for testing these assumptions empirically in laboratory experiments is to have study participants play one of two games (a violent and a nonviolent one), after which they partake in a laboratory procedure intended to measure aggressive behaviors. This "boilerplate" for experiments has been replicated dozens, if not hundreds of times, and makes up large parts of the foundation of empirical evidence on digital game violence effects (and media violence effects in general).

However, some media effects researchers have expressed doubts regarding the usefulness of conventional social-cognitive theories in predicting media violence effects, and criticized them for offering too simplistic views on the etiology of human aggression (Ferguson & Dyck, 2012). There are also profound concerns whether common empirical approaches to studying these effects in psychological laboratories, particularly regarding the methodology of measuring aggression, allow drawing meaningful conclusions about media violence (Ritter & Eslea, 2005; Tedeschi & Quigley, 1996).

This dissertation critically examines four theoretical and methodological aspects in game violence effects research: (1) extant models and perspectives predicting relationships between violent games and aggression, (2) the manipulation of independent variables and control of confounds in experiments on game effects, (3) the measurement of aggression in psychological laboratories, and (4) ideological biases that shape both the research as well as the academic debate that surrounds it.

The first paper (Elson & Ferguson, 2014a) included in this dissertation was published in *European Psychologist* and presents an overview of the theoretical and empirical literature on the effects of displayed violence in digital games on aggressive cognitions, emotions, and behaviors. It focuses on theoretical shortcomings of conventional, social-cognitive views on aggression and media violence effects, and discusses relevant gaps in the empirical scholarship. This review offers some new perspectives on potential mechanisms of media effects that could guide future empirical research in this area. Four scholars with opposing or diverging views were invited to comment on this article (Bushman & Huesmann, 2014; Krahé, 2014; Warburton, 2014) and offer a rebuttal to the issues that were raised.

The second paper (Elson & Ferguson, 2014b) is a response to these comments and was, hence, also published in *European Psychologist.* Due to the nature of the three

comments, this article reiterates some of the theoretical concerns in greater detail, but largely constitutes a discussion of the scientific discourse around violent digital games research rather than the science itself. It offers explanations why there is such a heated debate around violent game effects in the scientific literature, how this might pose a threat to the credibility of media effects research in general and its ability to inform society about a topic of public interest in particular.

The third article (Elson & Quandt, in press), to be published in *Psychology of Popular Media Culture*, constitutes a transition from theoretical considerations to novel methodological approaches to studying game effects. The article presents a rationale why using multiple games (violent and nonviolent ones) to create different conditions in laboratory experiments in order to study the effects of one specific variable (violence) violates fundamental assumptions of experiments as a scientific method. Put briefly, by using different games to manipulate the target variable of violent content, one is likely to accidentally manipulate other variables in this process that could easily conflate or confound any findings on dependent variables, if not properly controlled for. Next to a thorough examination of this problem and its prevalence in experiments with games, the article offers game modifications ("modding") as a viable solution readily available to all game researchers.

The fourth article (Kneer, Elson, & Knapp, under review) corroborates these methodological considerations with empirical data from a 2x2 experiment in which the violent content and difficulty of one game were manipulated (while holding all other game characteristics constant) instead of using different games for each condition. The data confirm the assumption that game difficulty is a key variable when studying the effects of game violence, particularly regarding emotional responses, and that modifying games constitutes a useful approach to studying the effects of individual game variables.

The fifth paper (Elson, Mohseni, Breuer, Scharkow, & Quandt, 2014), published in *Psychological Assessment*, concludes this dissertation by raising methodological concerns regarding the measurement of aggression in laboratory experiments. The paper discusses psychometric properties of one of the most widely used paradigms to measure aggression, the Competitive Reaction Time Task, and focuses specifically on objectivity and standardization issues. These concerns are confirmed by data from three studies that show that analyzing the same data with different variants of this test leads to large differences in significance levels, effect sizes, and even the direction of effects. Implications for the empirical literature on violent digital games and aggression research in general are discussed and practical suggestions on how this test should be used in order to arrive at more objective results are provided.

Learning to be Aggressive from Violent Games

Given the large popularity of digital games, the implications of their presumed effects on aggression would be unsettling. For example, at least one quarter of the German population plays digital games (Quandt, Breuer, Festl, & Scharkow, 2013). If the use of digital games did, indeed, have an effect on aggression, this would imply a substantial societal problem. But what exactly are the purported psychological mechanisms that would make people more aggressive from their exposure to violent digital games? Historically, many researchers have defaulted to a socio-cognitive perspective on the etiology of aggression in general and specific to the role of media violence. The following sections present an overview of proposed socio-cognitive mechanisms and their shortcomings, while arguing for the adoption of theories focused on biological determinants and influences from the social environment (such as family and peer groups).

Definitions of Aggression and Media Violence

The disagreement whether violent games cause aggression begins in basic scientific questions, such as the definitions of violence and aggression. The field of media violence effects research started out with operational definitions of aggression focused purely on the outcome of behaviors. Buss (1961), for example, defined aggression as one organism presenting painful stimulation to another organism. While this behavioristic approach to aggression had great merits for researchers in practice, it was eventually considered insufficient as it could not distinguish between accidental and intentional behaviors causing harm (which of course can be crucial, for example, in court decisions on crimes). A commonly used definition of aggression is the one by Baron and Richardson (1994), who defined aggression as any behavior that is intended to cause harm to another person who intends to avoid this harm. However, Grimes et al. (2008) cautioned against defining aggressive behaviors through preceding intentions, as it might entice psychologists to

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measure cognitive processes underlying aggression rather than whether any harm or injury has actually been inflicted. Grimes et al. (2008) criticize that, as violent behaviors are simply considered extreme forms of aggression (C. Anderson & Bushman, 2002), measuring these cognitive variables is, accordingly, also a sufficient proxy for violent behaviors, or even violent crimes. To address this problem, van der Dennen (1980) suggested to separate aggression and violence as completely distinct categories. According to this definition, as long as a drive, impulse, or desire to inflict pain is operative, a behavior should be considered aggressive (motivational component). Violence, on the other hand, describes a category of behaviors involving harm, elimination, or destruction, which can be direct or indirect, and physical or mental (behavioral component). As such, there are four types of behaviors: Aggressive violent behaviors (e.g., a crime of passion), aggressive nonviolent behaviors (e.g., gossip), nonaggressive violent behaviors (e.g., executions), and, of course, non-aggressive non-violent behaviors (the residual category).

Defining media violence or determining how violent one game is compared to another seems even more intricate, and varies substantially. The reason why psychological definitions of aggression or violent behaviors, such as the ones discussed above, might not be applicable to games is that a) since all game violence is virtual, and not physical, it is questionable whether any harm is actually being inflicted, and b) the narrative intentions of avatars might not be congruent with the intentions of players. Usually, empirical publications do not provide a definition at all since the difference in violent contents between games selected for experiments often has a high "face validity" (e.g., when one involves a considerable amount of combat, and the other is an abstract puzzle game). Of course, this theoretical gap becomes apparent when the public turns to psychologists and asks to apply their research to practical decisions. For example, a proper definition of media violence becomes necessary when lawmakers or judges decide whether specific violent contents are harmful enough to warrant censorship or legislation that limits access to games

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(Brown v. Entertainment Merchants Association, 2011), or simply when concerned parents wonder which games are and which are not suitable for their children. The increased graphicness of violence through technological advancements has alerted researchers to study potential increases in their effects (e.g., Ivory & Kalyanaraman, 2007), yet in most cases it does not seem to be a relevant factor when it comes to defining what makes a game violent. At least historically, researchers seemed more concerned with whether any violence is being rewarded in games rather than the magnitude of violent content itself, and considered seemingly innocuous titles like *Super Mario* violent as well (C. Anderson & Dill, 2000). According to Ferguson (2014), however, this rather vague perspective could render the category "violent games" useless. Recently, there have been more sophisticated attempts, and scholars suggested describing game violence through multiple technological and narrative components, such as graphicness, realism, and justification (Tamborini, Weber, Bowman, Eden, & Skalski, 2013).

The General Aggression Model

In 2002, C. Anderson and Bushman published a revised version of the General Aggression Model (GAM), a synthesis of several social-cognitive and neoassociative theories, that has become the default model for many game violence researchers, particularly for those who believe games to be a strong cause for aggressiveness. The GAM is strongly rooted in Bandura's (1978) social learning theory (SLT) of aggression, which predicts that aggressive behaviors can be reinforced either through direct experience or vicarious observation of aggressive acts being rewarded. The greater the rewards, the greater the reinforcement and, consequently, the likelier the chance of imitating what has been observed. According to SLT, a repeated experience or observation of aggressive behavior being rewarded results not only in a higher frequency of aggressiveness as the reward expectation increases, but also alters concepts regarding the appropriateness of aggressive behaviors in a wide range of situations. Thus, these rewarded models cause a greater

preference for aggression as instrumental to reaching goals, shape aggressiveness as a general response class, and ultimately consolidate it as a social norm.

Through repeated exposure to aggression, the GAM also predicts changes in knowledge structures, such a perceptual and behavioral schemata. As such, aggressive behaviors are also accompanied by an increasingly hostile perception of the world and the presumed intents of other persons. Whether a person responds aggressively or not to a particular event is determined in the GAM's tripartite process model. Situational characteristics (e.g., aggressive cues) and personality variables (e.g., traits, learned scripts) are located on the input side. The interpretation of the environmental input depends on internal states of cognitions (e.g., hostile thoughts), affect (e.g., mood), and psychophysiological arousal. This can be a relatively effortless, impulsive, and automatic process. However, when the immediate appraisal is not satisfactory and resources are not limited (usually time and capacity), any given information can be re-evaluated numerous times. Either way, this immediate or thorough evaluation determines a behavioral response as the outcome. The response to this outcome, again, becomes part of the information for the next episode. Ultimately, repeated episodes of actions and reactions result in more permanent perceptual, attitudinal, and behavioral patterns. And, in accordance with SLT, this mechanism also works when such action-reaction-chains are observed in the behavior of others.

Theoretical Shortcomings of the GAM

The GAM does not differentiate sufficiently between observations in physical and digital environments. The way violence is being rewarded in digital games, e.g. as a necessary condition to win the game, in-game benefits (e.g., better equipment), or through scores on leaderboards, is considered to be sufficient as a reinforcement of aggressive behaviors for game players. Accordingly, repeated exposure to games with such contents

would be considered a risk factor in the etiology of aggression. The first publication included in this thesis (Elson & Ferguson, 2014a) discusses at least 5 major weaknesses of the GAM perspective on the development of aggression in general and its specific assumptions about effects of game violence.

1. While offering simple and testable predictions about the antecedents and consequences of human aggression, the GAM is heavily focused on cognitive scripts and does little to elucidate motivational and personological variables that may influence aggressive behaviors. Within the GAM's line of argument, personality characteristics and motivations for behaviors are, in essence, nothing more than strongly and repeatedly reinforced cognitive scripts, thereby rendering it a "tabula rasa" theory (Pinker, 2002).

2. Particularly biological and genetic factors are neglected despite their importance in predicting aggression and even crime in individuals (Ferguson, Ivory, & Beaver, 2013). It remains especially unclear how they interact with supposedly acquired aggressive scripts. There is also a lack of specific variables explaining individual susceptibility or immunity to potential effects of violent games.

3. Despite its popularity in psychological media effects research, the GAM is not actually used by clinicians or other professionals in the field dealing with pathological forms of aggression (Ferguson & Dyck, 2012). Neither are there clinical diagnostic instruments based on the GAM, nor is the GAM being used to inform programs aimed at reducing pathological aggression, as opposed to biopsychosocial models of aggression that dominate clinical psychology.

4. The GAM does little to account for competing schemata and scripts. Even assuming that violent games are able to model aggression in their players, these models will sooner or later either be contradicted by punishments for aggressive behaviors, or contested by rewards for prosocial models, for example through

parents or peers. The GAM does neither predict the outcome of competing

models, nor which kind of rewards for aggressive scripts might supersede

nonaggressive scripts (and vice versa).

5. The GAM equates the effects of observations of rewarded aggression in virtual

and physical environments, and predicts that it makes no difference for the

underlying mechanisms to work whether the observed violence is fictional or

real.

The fifth point deserves some further attention, since Bushman and Huesmann

(2014) responded to it specifically by arguing that the assumed equality of learning

opportunities constitutes a theoretical advantage of the GAM, not a limitation. Bushman and

Huesman explicitly ask for a theory that would explain how viewing violence mass media

could be different from, for example, observing violence in war-torn countries (Boxer et al.,

2013). Therefore, the second publication (Elson & Ferguson, 2014b) expatiates on this

argument further. Briefly, assuming that these two experiences could be similar is faulty on

three grounds:

5a. The experience or observation of fictional acts of violence (e.g., a knight killing a

dragon) is not similar in its qualities to the experience and observation of real

violence (e.g., a news report on an ongoing war), even when both are presented

on screen. This is corroborated by evidence showing that children at the age of

five (or younger) are already able to distinguish between real and fictional

television (Wright, Huston, Reitz, & Piemyat, 1994).

5b. Observing violence in digital games does not have similar psychological effects

as observing proximal acts of violence (e.g., in the family), even when both acts

would be similar. This is substantiated by a large body of research findings (e.g.,

Ferguson, San Miguel, & Hartley, 2009) as well as decades of clinical practice and

psychological assessments of children witnessing domestic violence in their families (Levendosky, Huth-Bocks, Semel, & Shapiro, 2002).

5c. There is no theoretical explanation how observations of violence in virtual worlds (e.g., soldiers fighting) generalize to actual behaviors that are completely different in the real world (e.g, domestic abuse). While one could make a strong case for how digital games teach that violence is a promising and successful measure in other similar games, the transfer from behaviors in digital environments to other behaviors in physical environments (that are different in many aspects from the virtual environments) is at least not a natural given.

Finally, even ignoring all the issues raised above and assuming that repeated exposure to violence games would incrementally make players more aggressive, it must be taken into account how others would react to these changes in behaviors. A progressive increase in aggressive behaviors would usually get punished by peers or the family, thus decreasing them through undesired consequences. If, however, aggressive or antisocial behaviors are tolerated or even rewarded, does not the real issue lie within an unhealthy environment that fosters aggressiveness rather than peacefulness?

Violent Digital Games: Manipulation and Control of a Multifaceted Stimulus

Taken together, the first two publications (Elson & Ferguson, 2014a, 2014b) offer an overview of the results obtained in laboratories and the field with a strong focus on methodological rigor, and integrate different perspectives and interpretations to explain their relevance to the understanding of media effects. The main body of psychological research on the effects of digital games consists of laboratory experiments. Many of these studies share a certain design: Study participants (typically college students, most of them psychology or communication majors) either play a violent game (mostly a first-person shooter) or a nonviolent game. Psychophysiological arousal (heart rate, skin conductance level) is sometimes measured during, or before and after play. After playing one of the games, participants are subjected to a test or fill out a questionnaire to assess aggressive cognitions, emotions, or behaviors, which are then compared for the two groups. Any observed differences between those groups are then usually explained with the manipulation of violent content. An example for this kind of research design can be found in the study by K. Williams (2009) in which participants played either *Mortal Kombat: Deception* (Midway, 2004) or *Dance Dance Revolution Max 2* (Konami, 2003). *Mortal Kombat* is a fighting game in which players control a character engaged in close combat with an opponent. One match usually involves several rounds of fighting in an arena. *Dance Dance Revolution*, on the other hand, is a rhythm game in which players typically have to mimic dancing instructions to pop songs on special dance mats that serve as input devices. In this study, participants completed a hostility scale after playing and the results show that those who played *Mortal Kombat* reported significantly stronger feelings of hostility. K. Williams (2009) concludes that "[t]his supports past evidence that exposure to violent video games, when compared to nonviolent video games, results in aggressive affect" (p. 303).

However, this way of manipulating game contents as independent variables could violate fundamental assumptions of experiments as a scientific method. In his classic *Experimental Psychology*, Woodworth (1938) describes the defining elements of the scientific method of experiments in psychological science as we still know it today: For a study to qualify as an experiment, the researcher "holds all the conditions constant except for one factor which is his 'experimental factor' or his 'independent variable.' The observed effect is the 'dependent variable' which in a psychological experiment is some characteristic of behavior or reported experience'' (p. 2). And while this assertion is being taught in any ordinary introductory psychology class, it has serious consequences for research on and with digital games.

Arguably, it is very convenient and certainly bears convincing face validity to divide games into two groups according to the current variable of interest (e.g., violent and nonviolent games). Yet with a complex stimulus like games it should be considered that violence is unlikely to be the only difference between two games that have been selected for research purposes. Any of those additional differences constitutes a potentially confounding factor that might bias results if not controlled for. This problem is particularly intricate as there are common cooccurrences of themes, contents, and mechanisms in certain game genres (Apperley, 2006) that could lead to a systematic conflation in larger bodies of research. Although not a genre in itself, it is certainly possible that violent games commonly share some other characteristics that could be relevant for aggression research.

Adachi and Willoughby (2011b) argue that when investigating effects on aggressiveness, scholars should consider the *difficulty*, *pace of action*, and *competitiveness* of a game as possibly relevant variables besides violent content. A genre that most often features displays of violence is the first-person shooter. These games are usually also fast-paced, likely to be played competitively against other human players, and highly demanding in terms of perception and motor abilities – not to mention that first-person shooters are

always played from the first-person perspective. By contrast, the puzzle games (e.g., Tetris) that are popular stimuli for "control groups" are typically nonviolent, but also rather slow-paced, usually played alone, and require cognitive efforts, such as problem-solving abilities and mental rotation. So when observing differences in measurements between those groups after playing, does it mean that one particular game characteristic, such as violence, affected human behavior? This example illustrates that studies attributing changes or group differences in aggression to violent contents specifically might be severely confounded by other contents that were not properly controlled for, or even accidentally manipulated by using different games that varied on multiple dimensions.

The third publication (Elson & Quandt, in press) provides a detailed discussion on the problem of stimulus control in research with digital games. First, advantages and disadvantages of previous approaches to this problem are considered (e.g., using Likert scales to rate and control for relevant third variables), followed by the introduction of game modifications (or "mods") as a viable alternative to manipulate independent variables and control confounding factors. Of the different types of mods Scacchi (2010) identifies, the most relevant to psychological researchers are so-called *partial conversions,* which are smaller alterations or additions to an existing commercial game. These mods range from relatively small and cosmetic additions, such as new textures for existing objects and clothes for characters, to entirely new environments the game can be played in. While mods are usually being created for entertainment purposes, they could arguably be used as powerful manipulations of independent variables, while at the same no other aspects of a game would be changed and thus exerting meticulous control over confounding variables. To assess the relevance of modding for digital games effects research, Elson and Quandt (in press) conducted a three-step systematic literature review with the aim to estimate to what extent researchers already used modding techniques, but also which proportion of the experimental work on games could potentially benefit from it.

First, several academic databases were searched for all peer-reviewed entries using the following three terms in the field *All Text* (TX): *modding*; *video game** and *mods*; *video game** and *modif**. This resulted in a total of 52 publications that either dealt with the topic of modding specifically or utilized modding techniques for research purposes. Naturally, this type of literature search was unable to retrieve publications in which scholars make use of modding techniques without referring to them as such or using the terminology more common among game designers than social scientists. Therefore, the literature review was extended in a second step. The last ten volumes (total number of articles N = 4,160) of the journals *Communication Research* (Sage), *Computers in Human Behavior* (Elsevier), *Cyberpsychology, Behavior, and Social Networking* (Mary Ann Liebert), *Human Communication Research* (Wiley), *Journal of Communication* (Wiley), and *Media Psychology* (Taylor & Francis) were searched for games-related articles to which modding could, in theory, be applied as a means of stimulus creation, manipulation, or control. Of the *n* = 145 studies that employed digital games as stimuli, 26 (18%) used materials that were modded by manipulating contents so that the games would be more suited for their research questions (e.g., varying contents to create conditions, or removing unwanted contents to exert greater stimulus control). In 42 studies (29%) at least one independent variable was manipulated by using two or more completely different commercial off-the-shelf titles (potentially diminishing internal validity), and for 28 studies (19%) entirely new games were created as stimulus materials (potentially diminishing external validity). At least in these cases, modding one game instead, or using different playing modes of the same game (as suggested by McMahan, Ragan, Leal, Beaton, & Bowman, 2011) might have been viable alternatives.

Psychologists interested in game violence effects have benefited from utilizing mods in laboratory studies in the past. The earliest example is identified is the study by Staude-Müller, Bliesener, and Luthman (2008), whose participants either played a conventional

first-person shooter (FPS), or a mod in which avatars are being frozen instead of killed. Of course, this sophisticated mod might not fully solve problems of stimulus control, as it could be questioned whether freezing someone should be considered truly "nonviolent" (after all, like dying, freezing is not a very desirable experience). However, it still constitutes a highly functional approximation of a clear *relative* difference in degrees of violence between conditions. Any outcome variable that differs between the "kill" and the "freeze" version could be attributed to the degrees of manipulation, even when the latter version does not remove *all* violence from the former. Following the suggestions by Adachi and Willoughby (2011b), Elson, Breuer, Van Looy, Kneer, and Quandt (2013) studied the isolated and interaction effects of violence and pace of action in digital games on cardiovascular responses and aggressive behavior, They assigned their participants to play one of four versions of a FPS: normal-paced (default speed level) vs. fast-paced (speed level at 140%), violent (wielding a grenade launcher) vs. nonviolent (wielding a toy nerf gun). Hartmann, Toz, and Brandon (2010) created two mods to assess the effects of (un)justified violence on feelings of guilt: Their participants were either playing UN soldiers attempting to shut down a torture camp, or paramilitary forces defending the camp and continuing the cruelty.

In order to offer researchers a good rationale where to start when considering modding for an upcoming experiment, the third publication (Elson & Quandt, in press) also includes a brief overview of the modding tools currently available. While it cannot replace the study of elaborate tutorials, it aims to provide a rough idea of which tools might and might not suit researchers' needs.

Game Difficulty as a Relevant Confound in Game Violence Research

Further corroborating the theoretical considerations by Adachi and Willoughby's (2011b) about the relevance of other game characteristics, the fourth publication (Kneer et al., under review) presents an experiment examining the effects of violence and difficulty in

digital games. Difficulty is a particularly important variable as an unsatisfactory in-game performance might be frustrating to players. In line with the classic frustration-aggression hypothesis (Berkowitz, 1989; Dollard, Miller, Doob, Mowrer, & Sears, 1939), frustration resulting from a mismatch between game difficulty and player skills could lead to increases in aggression. Conversely, a game being too easy might be quite boring to players, particularly highly skilled ones. Past studies, however, have rarely controlled for game difficulty. When using different games to manipulate violent contents this could present a considerable problem as these games might also differ on their difficulty levels. In these cases, it would be quite problematic to determine whether increases in aggression can be traced back to the level of displayed violence or occur as a negative response to unattainable in-game challenges.

In the study by Kneer et al. (under review), N = 90 participants played the first-person shooter *Team Fortress 2* (Valve, 2007) in which two teams both try to capture and hold a control point while preventing the other team from doing so (in this study, teammates and the opponent team were controlled by the computer). Participants were assigned to one of four conditions with either high or low difficulty settings and a high or low amount of violent content. These conditions were created with in-game options and through publicly available modding tools. In the high violent conditions, the player and all bots wielded flamethrowers, and the portrayed deaths of characters in the game were rather bloody and graphic. In the low violent conditions, everyone was equipped with a 'rainbowblower' that blasted rainbows instead of fire while playing bubbling sounds, and instead of dying, this weapon incapacitated characters by making them drop to the ground convulsing with laughter. Difficulty was manipulated by altering the weapon's damage output, the player's resistance to enemy damage, and the speed at which the control point could be captured. Thus, instead of four different games, participants played one of four versions of the same game only differing with regard to the independent variables while

holding all other variables constant. The dependent measures were psychophysiological arousal (interbeat intervals and electrodermal activity measured continuously during play), aggression-related associations (measured with a lexical-decision task after play), aggressive behaviors (measured with the standardized Competitive Reaction Time Task after play), as well as positive and negative emotions (measured with Renaud and Unz's [2006] affect scale).

Results show that there was no influence of violent content on psychophysiological arousal, aggression-related associations and aggressive behavior, or positive and negative affect. Difficulty did not have any appreciable effect on psychophysiological arousal, aggressive behavior, and positive or negative affect. However, a higher difficulty was significantly associated with higher response latencies for aggressive words in the lexical decision task. Thus, a higher difficulty inhibited aggression-related associations. This difference was not significant for neutral words but the trend was similar, showing higher response latencies when the difficulty was increased. The reason for this finding might be that a higher difficulty of a game leads to exhaustion, resulting in slower responses in the lexical decision task in general. The interaction of violent content and difficulty did not produce any significant changes in any of the dependent variables. However, the results provide strong evidence that in-game performance (measured through the total number of opponents killed by the participant) predicts both positive and negative affect after play.

This study adds to the emerging literature on game characteristics particularly relevant to violent game effects research, such as pace of action (Elson et al., 2013), competitiveness (Adachi & Willoughby, 2011b), or technological advancement (Ivory & Kalyanaraman, 2007). While this study provides no evidence that game difficulty confounds measures of aggressive behaviors, its results suggest that difficulty and in-game performance should be taken into account when studying cognitive and affective processes during and after the exposure to violent game contents. This is further supported by studies

investigating other factors than difficulty that might elicit frustration in game players. For example, the studies by Breuer, Scharkow, and Quandt (2013) and Elson, Breuer, Scharkow, and Quandt (2014) show that, next to the outcome of the game (winning or losing), the behavior of others (e.g., their playing abilities, or their friendliness) can significantly frustrate players, which, in turn, predicts aggressive and cooperative behaviors towards their coplayers. These are, of course, variables related to the playing situation rather than the game itself. Finally, the study by Kneer et al. (under review) can be considered an example of how using options provided by games and modding tools can help psychologists to carefully design experiments that meet the requirements of clean variable manipulation and rigorous variable control.

Measuring Aggression in Laboratories: A Cautionary Tale

After presenting approaches to the precise manipulation of independent variables in violent game effects research, the following section is concerned with methodological concerns regarding the dependent measures, i.e. aggression. A large number of studies investigated the facilitation of aggressive cognitions (e.g., thoughts) through violent digital game playing. Aggressive cognitions themselves, or even simple aggressive thoughts are, however, quite difficult to assess as they cannot be observed directly and would need to be verbalized – or expressed in a different manner – by study participants. Instead, psychologists usually measure superficial correlates of aggressive thoughts, such as automatic semantic activations or the accessibility of words and concepts related to aggression. Popular measures of these associations are, for example, lexical decision tasks (see above) or the word stem completion task, in which participants are presented with a series of ambiguous items that can make more than one word by filling in the respective letters. Depending on which letters are inserted, the meaning of the word can either be related to aggression or to something else (e.g., "k i _ _" having the two possible completions "kill" and "kiss"). The underlying idea is, in essence, that a higher number of aggression-related completions indicates a greater accessibility of aggressive cognitions.

Arguably, these types of measures severely limit the real-world relevance of the results, as they cannot be generalized to actual aggressive thoughts, let alone aggressive behavioral tendencies. While these measures might be helpful when investigating which concepts (including aggression-related ones) are being primed by specific types of games, and which concepts might be suppressed (e.g., Kneer, Glock, Beskes, & Bente, 2012), they do not allow inferring any intent to commit aggression or violent crimes. Unsurprisingly, the majority of studies using these and similar measures do find that games with violent contents increase the accessibility of aggression-related concepts (Barlett, Branch,

Rodeheffer, & Harris, 2009; Sestir & Bartholow, 2010) compared to nonviolent games. To what extent these studies contribute to the understanding of violent game effects, however, remains debatable. They certainly do no warrant an alarmist warning of hazardous effects of digital games on the way we think, or even react to stimuli from our environment.

The Unstandardized Use of the Competitive Reaction Time Task

Most of the discussion about the potential harm of violent games within the scientific community, news media, and the general public has focused on the issue of whether violent digital game exposure results in aggressive or violent actions. However, this has been a difficult question to answer. Legal and ethical restrictions make measuring aggressive behavior in a laboratory a difficult enterprise. As can be imagined, it is generally not possible to create a scenario in which individuals will attack each other in the laboratory environment. Unfortunately, this means that most experiments must rely on instruments that do not measure aggression or violence directly, but vaguely approximate it in some way. Notable examples of these measures are the amount of hot sauce used by the participant to spice bowl of chili for someone else (Lieberman, Solomon, Greenberg, & McGregor, 1999), the number of needles used to pierce a voodoo doll (DeWall et al., 2013), or the accuracy of darts thrown at pictures of human faces (Mussweiler & Förster, 2000). An instrument used in many experimental studies (not only in media violence effects research) is the Competitive Reaction Time Task (CRTT). In the original version of the CRTT by Taylor (1967), the Taylor Aggression Paradigm, participants were led to believe that they would be playing 25 consecutive rounds of a reaction time game against another participant in which the winner of a round would punish the loser with an electric shock. Participants who lost a round would receive shocks of varying intensity and when participants won a round they could adjust the shock levels for their alleged opponents. The intensity level of the shock was used as the measure for aggressiveness. Recent adaptations of the CRTT allow participants to set the intensity (usually volume and/or duration) of a noise blast instead of

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an electric shock, as they are easier to use and bring up fewer ethical issues (Ferguson & Rueda, 2009). As there is no real opponent, the sequence of wins and losses, as well as the settings "chosen" by the opponent, are typically randomized and preset. Generally, louder and longer noise blasts are considered indicators of higher levels of aggressiveness.

However, the CRTT has been used in many different versions in the past. Inconsistencies are found in the procedure of the CRTT, as well as in the ways in which the CRTT data are analyzed by different (and sometimes even the same) authors. While the procedural aspects refer to the setup of the test, i.e., how the raw data are generated, the statistical differences refer to how the data are analyzed. Of course, the procedural decisions also affect the options for statistical analyses. At least 13 different variants to calculate a score for aggressive behavior can be found in the literature: Multiplication of each trial's volume and duration (Bartholow, Sestir, & Davis, 2005), volume and square root of duration (Carnagey & C. Anderson, 2005), or volume and log-transformed duration (Lindsay & C. Anderson, 2000); standardized and summed volume and duration (Bartholow, Bushman, & Sestir, 2006); separate average volume and log-transformed duration settings for each outcome (wins and losses) (C. Anderson & Dill, 2000); average volume, not allowing any duration settings at all (Sestir & Bartholow, 2010); sum of high volume settings, i.e. 8 to 10 on a scale from 1 to 10 (C. Anderson & Carnagey, 2009); separate volume and duration setting of only the first trials (Bushman & Baumeister, 1998); the setting of the first trial and the means of trials 2-9, 10-17, and 18-25 (C. Anderson et al., 2004); volume and duration in a two-phase version of 25 trials each, in which the participant can retaliate in the second phase for the punishment received during the first (Bartholow & C. Anderson, 2002).

From a methodological point of view, inconsistent procedures and analyses are highly problematic because they infringe upon the objectivity criterion of psychological test theory (Kaplan & Saccuzzo, 2009). Simmons, Nelson, and Simonsohn (2011) pointed out that flexibility in data collection, analysis, and reporting in psychological research

dramatically increases actual rates of false-positive findings. Moreover, if there is no standardized procedure for a test and no standardized way to process the raw data into a meaningful score, the question remains whether the unstandardized value really approximates the true value of the construct. Aggression scores that are calculated with different procedural versions of the same test become very difficult to compare. Under the assumption that all these different procedures and analyses are equally capable of measuring the construct of aggressiveness, it is unclear why so many versions exist. Without a doubt, theory-driven modifications of a method such as the CRTT, with the aim of answering specific research questions, can contribute to the understanding of psychological processes and extend the area in which a certain test can be applied. However, many authors do not explain in detail why they decided on a specific test procedure or on the aggression score they calculated from the raw data. In many cases, it is not clear why a particular score should be more suitable than others to address the respective research questions. Sometimes, the decision to focus on one of many possible scores seems to have been made post hoc, not prior to data collection.

While there have been several studies that examined at the validity of the test (Ferguson, Smith, Miller-Stratton, Fritz, & Heinrich, 2008; Suris et al., 2004; Tedeschi & Quigley, 1996), until now, there has been no study that addresses the aforementioned objectivity issues by systematically comparing the different analysis procedures for the CRTT. The fifth publication (Elson, Mohseni, et al., 2014) presents data from three studies that were conducted to investigate the effects of digital games on aggressive behavior (measured with the CRTT). All analysis procedures that could be identified in the literature were applied to the three datasets with the aim to investigate whether there would be any variability of results when using different CRTT scores within each study, and whether this variability could be replicated across studies. The analyses showed that there was a considerable range of significance levels (from $p = .070$ to $.934$ in study 1; $p = < .001$ to $.959$

in study 2; $p = .096$ to $.212$ in study 3) and effect sizes (from $\omega = .0$ to $.10$ in study 1; $\omega = .0$ to $.39$ in study 2; $\omega = .09$ to $.20$ in study 3). Thus, it seems that the calculation of different aggression scores can lead to results that are substantially different from each other; in one case, even diametrically opposed. Depending on which aggression score was calculated (and reported) with the data from study 2, results could provide evidence that playing a violent digital game increases aggressive behavior, decreases it, or has no effect at all. The findings also suggest that volume and duration do not measure the same construct, although they clearly seem to be related. This does not necessarily constitute a problem with the CRTT. In fact, it could be considered a benefit if the CRTT was capable of capturing different (sub-) dimensions of aggressive behavior. However, no attempts to systematically disambiguate the latent variables supposedly measured by volume and duration have been made thus far.

These findings suggest that concerns about the CRTT'S standardization issues were justified. Of course, as the CRTT is the most common measure for aggressive behavior in the scholarly literature on violent game effects (C. Anderson et al., 2010), this has considerable implications. The results of studies that use the CRTT and meta-analyses that include these have to be interpreted with great caution. Moreover, given the questionable external validity of the test (Mitchell, 2012; Ritter & Eslea, 2005; Suris et al., 2004), researchers should be careful when they generalize results to situations outside the lab or make inferences about potential long-term effects to the point of public health issues. Of course, this issue is not limited to media effects research, as the CRTT is being used in a large variety of fields. This includes investigations of social and cerebral response in criminal psychopaths (Veit et al., 2010); effectiveness of prescription drugs in reducing hostility in panic disorders (Bond, Curran, Bruce, O'Sullivan, & Shine, 1995); and the facilitation of aggression through various substances, such as alcohol (Pihl et al., 1995). In some cases, practical recommendations for clinicians regarding the diagnosis (McCloskey, Berman, Noblett, & Coccaro, 2006) and treatment (Ben-Porath & Taylor, 2002) of patients are made based on results obtained with

the CRTT. Given the impact of clinical research on the definition, assessment, diagnosis, and treatment of disorders in clinical practice, the importance of using objective, reliable, and valid measures cannot be overstated. The unstandardized use of the CRTT violates these requirements and, thus, poses a potential threat to the credibility of all laboratory research on aggressive behavior.

How to Advance a Field that is Loaded with Ideology

In view of the presented issues in theoretical conceptualizations, in manipulation and control of independent variables, and in the operationalization and measurement of dependent variables, it would not be sound to make any claims about conclusive evidence based on the available research. The conclusiveness of existing research on violent game effects is frequently overstated, and indulgence in ideological claims commonly go beyond what scientific evidence supports (Grimes et al., 2008). There appears to be a discrepancy between what media effects scholars find, and what some proclaim it means. Scholars have conjured violent games (and violent media in general) as a public health crisis, and claimed that it accounts for up to 30% of all violence in society (Strasburger, 2007), or that a strict ban of media violence would lead to an decrease of 10,000 homicides, 70,000 rapes, and 700,000 injurious assaults each year in the US alone (Centerwall, 1992). C. Anderson, Gentile, and Buckley (2007) consider violent video games as one of several risk factors that may cause aggressive, violent behavior and, in highly extreme and rare cases, even school shootings. Others draw rather curious comparisons, such as that the link between violent game use and aggression is as powerful as the link between condom use and prevention of HIV transmission, or as hazardous as smoking effects on lung cancer (C. Anderson et al., 2003). Not only does the alarmist manner in which a diffuse concept, such as aggression, is compared to a serious medical condition, such as cancer, unnecessarily heat the debate, it is

also faulty on methodological grounds as the methodologies of media effects research and oncology are so drastically different that a comparison of the resulting effect sizes is invalid. If cancer studies would consist of participants smoking cigarettes for 5–10 min and then rating their cancer severity on a 5-point Likert scale or pushing a button when they recognize cancer-related words, then yes, such analogies would be eligible. But fortunately, cancer research does not have the methodology or validity issues that media effects studies do. Ironically, tests for cancer have everything that currently employed aggression tests do not. They are standardized, they are clinically validated (according to the results, one either has cancer or not), and they have a high reliability and external validity (someone who has cancer in a laboratory also has it outside the laboratory). Unfortunately, the same cannot be said about measurements of aggression.

There appears to be a discrepancy between what social scientists commonly measure in their laboratories and the behaviors that the public (or policy makers) are concerned about. Past research has usually not been conducted to inform public policy directly, but to advance academic knowledge of fundamental cognitive and behavioral processes in controlled laboratory environments. Consequently, when policy makers (e.g., Brown v. Entertainment Merchants Association, 2011) evaluated the empirical evidence, they did not find compelling proof of a link between media use and real-world violent behaviors – they could not, simply because the academic research, with few exceptions, has little bearing on societal violence. Unfortunately, scholars themselves are not always cautious, generalizing findings from weak laboratory studies to societal violence in ways that are inappropriate. The rhetoric to characterize these measures is exaggerated in the same way as the effects they ostensibly provide evidence for. Bushman and Gibson (2011), for example, describe the CRTT as "a weapon that could be used [by the participants] to blast their partner" (p. 30). Bushman and Huesmann (2014) compare the CRTT's noise blasts to the rock music played at excruciating volumes prisoners in Guantanamo Bay have

been tortured with. To what extent this torture scenario, involving nonconsenting prisoners exposed to hours upon hours of sleep depriving noise, resembles the CRTT with its brief exposure and ostensibly consenting opponents in university laboratories remains unclear. Equating the CRTT to torture seems to be one more example of the irresponsible overreach to which this field has become accustomed.

The Disease of Moral Panic in Violent Games Research

But why is the public and scientific debate on violent games riddled with such a heated rhetoric? Offering one potential explanation, Gauntlett (2005) describes a phenomenon called moral panic. In a moral panic, a part of society considers certain behaviors or lifestyle choices of another part to be a significant threat to society as a whole, particularly when an older generation is not familiar with the behaviors of a younger generation (Kneer et al., 2012; Przybylski, 2014). In this environment, moral beliefs can substantially influence scientific research, and its results are readily used as confirmation for what has been suspected. Game researchers involved have a great interest in understanding the mechanisms of aggression to inform efforts at reduction of violent crime in society. Tackling an overt, proximal behavior, such as media use, has great merits: Attributing violence to manifest displays of media content that are considered immoral has convincing face validity. Moreover, media production and distribution could, in theory, be easily policed and regulated by state agencies. If media were causing harm in society, regulating them would be a fairly easy way of taking action against violent crime.

However, particularly when exaggerated, the danger of alarmist warnings about an overt, proximal behavior such as violent game use is a potential distraction from covert, distal issues rooted deep within society, such as poverty or inequality. Those problems are major sources of various societal issues, including violent crime, and are usually intangible, providing no ready 'bogeyman' in the parlance of moral panic theory – and are difficult

issues to address. Just as testimony regarding the 'harmfulness' of comic books given to governments by mental health professionals in the 1950s now looks to be an example of a nannying excess on the part of the scientific community, so too, as Hall, Day, and Hall (2011) argue, will the extreme statements about effects of violent games do little other than to damage the credibility of the field. More than ten years ago, the journal *Nature* (2003) called on media violence researchers to "tone down the crusading rhetoric until we know more" (p. 355). Ten years later, we do know more, and what we know now does not suggest that it is time to return to crusading rhetoric. Far from it, it is increasingly time for the scientific community to employ a more cautious language and act as a voice of reason in the face of societal moral panics. It is imperative that the scientific community remains alert to these issues moving forward.

The Future of Game Violence Effects Research

Revisiting the four major issues this dissertation addresses, there is evidence for problems in (1) extant theories on the relationships between violent games and aggression, (2) the manipulation of independent variables and control of confounds as well as (3) the measurement of aggression in game violence experiments, and (4) ideological biases that shape both the research as well as the academic debate that surrounds it. Future research must tackle each of these problems to be able to determine whether a link between violent games and aggressive behaviors exist, and to inform the public about these results.

The future of violent game effects research needs testable theories predicting the role of game violence in aggressive behaviors. Biopsychosocial diathesis-stress approaches, such as the Catalyst Model (Ferguson, Rueda, et al., 2008), already account for how exposure to games might shape the individual ways violent crimes are ultimately committed. Given the relatively small role games play in the etiology of criminal behavior in this model, however, the psychological functions of game use are rather underdeveloped. Integrating

these approaches to criminal behavior with motivational models of game use, such as mood management (Bowman & Tamborini, 2012; Zillmann, 1988) or uses-and-gratifications (Sherry, Lucas, Greenberg, & Lachlan, 2006), might be a viable solution. In contrast to socio-cognitive theories of aggression and media effects, these approaches are usually less concerned with passive learning through media contents, and more user-centric in explaining the functional link between psychological states and media exposure (Przybylski, Rigby, & Ryan, 2010). Such considerations are necessary when trying to explain why individuals may use the same media in very different ways, with very different outcomes (both intra- and interindividually). Thus, they could provide useful guidance when investigating whether specific use patterns of games, and not their contents, could be potentially detrimental to psychological well-being (including aggression).

On the empirical side, in light of the concerns about aggressive behavior or violent crimes precipitated by violent games, future studies should consider discontinuing investigations of game uses and effects in samples mostly consisting of college students. Studying game use patterns of offenders and those who have committed acts of violence against people or property instead could potentially yield highly interesting insights to our understanding of how and when violent media pose a risk. In addition, the identification of specific risk (and resilience) factors, such as an unfavorable family environment or mental health issues, preferably in prospective studies with actual control groups, might be an important future tasks for game violence researchers. Naturally, to be able to conduct these studies, the discussed problems in game effects research methodology have to be addressed first.

Methodological Rigor: A Potential Cure

While, as pointed out earlier, the propagation of extreme statements not supported by the available evidence is a problem of ideological convictions, the key condition enabling

this current state of affairs are the insufficient or ambiguous methods employed to measure

human aggression (Elson, Mohseni, et al., 2014; Ritter & Eslea, 2005), improper

manipulations of conditions to test effects of game violence (Elson & Quandt, in press), or

artificial situations under which games are studied (Elson & Breuer, 2014; D. Williams,

2005). With a corpus of precise and valid measurements for the different aspects of

aggressiveness (thoughts, emotions, and behaviors), study results could no longer be

subjected to interpretations from drastically different perspectives. In the case of research

on the effects of violent digital games, the value of empirical evidence suffers greatly from

the improper conclusions drawn based on results obtained through questionable methods.

Accordingly, Elson and Ferguson (2014b) recommend scholars to adhere to two steps: First,

not to generalize important findings further than the employed methods would allow (e.g.,

to consider aggression-related semantic activations simply as associations and not as

''aggressive thoughts''). Second, to overcome these limitations by developing standards to

ensure objectivity and focus research on the proper validation of key measurements. There

are current attempts to implement this, for example for the Hot Sauce Paradigm (Beier &

Kutzner, 2012), and further investments in these directions should be encouraged.

In light of the recent replication crisis shaking up psychological science (Pashler &

Harris, 2012), primarily research linking effects of cognitive priming with behavioral

outcomes (Pashler, Coburn, & Harris, 2012), it became evident (more than ever) that

ensuring the objectivity, reliability, and validity of research designs and key measures is

paramount. This crisis has increased the awareness of scientific misconduct in relatively

common questionable research practices (John, Loewenstein, & Prelec, 2012) and issues of

undisclosed "methodological flexibility" (Simmons et al., 2011). It seems that "hot-button

issues" in science are even more susceptible to these problems (Ioannidis, 2005), such as

research on behaviors that large parts of the population engage in (e.g., playing digital

games) or those that present a threat to societal values and norms (e.g., violence). Scientists

are entrusted by the public to act as advocates when accumulated evidence is compelling. Yet, at the same time, they are obliged to be rather conservative and acknowledge the gaps and boundaries of scientific knowledge at any given point in time. The debate on whether or not playing violent games causes aggression or crime cannot be resolved simply because a large number of prominent scholars believe they do (as suggested by Bushman & Huesmann, 2014). One can clearly make an argument that games feature a large amount of morally objectionable content, and be offended by the excessive displays of violence in them. Others may object that whether or not violent content in games is repulsive, objectionable, or immoral might be relevant to policy makers, but not to researchers (Grimes et al., 2008).

But the scientific opinion of whether these contents lead to problematic behaviors in game players can and should only be formed through compelling methodologies that are able to produce a corpus of unambiguous findings. For the effects of violent games, however, this corpus can currently only be described as fragmentary, at best. Neither does the current state of research allow drawing the conclusion that violent games are *harmful*, nor does it allow inferring that they are completely *harmless* – simply because it is doubtful whether *harm* is actually being measured. From a scientific perspective, the development of improved methods and measures to close this gap is the key to overcome this problem. Yet what can be observed instead is that, as the results of certain studies reinforce the belief in harmful effects of violent games, some scholars have developed an ideological belief in the validity of the methods repeatedly employed in those studies as well. When it is argued that the empirical evidence on the link between game violence and aggression is not substantial enough to warrant definitive conclusions, responses usually point to the large number of experiments (in which the CRTT is very common) allegedly proving a causal relationship. The issue of lacking evidence for the external validity of those measures, which is necessary to make such a claim, is then refuted by claiming that the convergence of studies on media violence and aggression substantiates the validity of the measures commonly used (an

example of this chain of arguments can be found in Bushman and Huesmann, 2014). This way, scholars develop a recursive argumentation in which a theoretical consideration demonstrates validity for methodological approach derived from it, and vice-versa.

This duality of ideologies, both on a theoretical and a methodological level, creates a vacuum in which science must necessarily stagnate. As Greenwald (2012) observed so keenly, "there is nothing so theoretical as a good method", by which he argued that the multidecade durability of theory controversies in psychology can often be resolved through methodological advancements generating new data, which, in turn, can inspire novel theoretical considerations. The ideological rigidity in theory and methods that can be observed in violent game effects research, however, could stifle this synergy, as scholars try to find theoretical arguments why their methods are sufficient, and use the same methods to prove their theories were veritable in the first place. Whether or not the methodological insights presented in this dissertation (Elson, Mohseni, et al., 2014; Elson & Quandt, in press; Kneer et al., under review) and elsewhere (Adachi & Willoughby, 2011a; Ferguson & Savage, 2012; Järvelä, Ekman, Kivikangas, & Ravaja, 2014) will be able to ultimately overcome this impasse, however, remains speculative at this point. But as social-cognitive theories on violent games and aggression appear to be growing in their rigidity (Elson & Ferguson, 2014a, 2014b), especially in the face of an increasing number of failed replications, only methodological innovations can enable researchers to inform the public debate in a meaningful way, and enable the scientific field as a whole to advance as it should.

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Appendix A

Publications within this cumulative dissertation and description of the doctoral candidate's contribution to each one.

1. Elson, M., & Ferguson, C. J. (2014). Twenty-five years of research on violence in digital games and aggression: Empirical evidence, perspectives, and a debate gone astray. *European Psychologist, 19*(1), 33–46. doi:10.1027/1016-9040/a000147

First author's contributions: Conceptualization of the article, composing theoretical considerations, review of the empirical literature, writing of the first draft, revising the manuscript after reviews.

Co-author's contributions: Assistance with the conceptualization, extension of theoretical review, review of meta-analytical work, writing of the first draft, assistance with revising the manuscript after reviews.

2. Elson, M., & Ferguson, C. J. (2014). Does doing media violence research make one aggressive? The ideological rigidity of social cognitive theories of media violence and response to Bushman and Huesmann (2013), Krahé (2013), and Warburton (2013). *European Psychologist, 19*(1), 68–75. doi:10.1027/1016-9040/a000185

First author's contributions: Conceptualization of the response, writing of the first draft, revising the manuscript after reviews.

Co-author's contributions: Assistance with the conceptualization, assistance with writing of the first draft, assistance with revising the manuscript after reviews.

3. Elson, M., & Quandt, T. (in press). Digital games in laboratory experiments: Controlling a complex stimulus through modding. *Psychology of Popular Media Culture*. doi: 10.1037/ppm0000033

First author's contributions: Conceptualization of the article, review of literature, data analysis, writing of the first draft, revising the manuscript after reviews.

Co-author's contributions: Proofreading and editing of the first draft and revised manuscript after reviews.

4. Kneer, J., Elson, M., & Knapp, F. F. (under review). Fight fire with rainbows: The effects of displayed violence, difficulty, and performance in digital games on affect, aggression, and physiological arousal. *Journal of Media Psychology: Theories, Methods, and Applications.*

Second author's contributions: Planning of the study, assistance in selection of methods and measures, assistance in data analysis, assistance in writing of the first draft.

Co-authors' contributions: Conceptualization of the article, data collection and analysis, writing of the first draft.

5. Elson, M., Mohseni, M. R., Breuer, J., Scharkow, M., & Quandt, T. (2014). Press CRTT to measure aggressive behavior: The unstandardized use of the Competitive Reaction Time Task in aggression research. *Psychological Assessment.* Advance online publication. doi:10.1037/a0035569

First author's contributions: Conceptualization of the article, literature review, data collection (study 2), standardization analyses for all three studies, writing of the first draft, revising the manuscript after reviews.

Co-authors' contributions: Assistance with the conceptualization, data collection (studies 1 and 3), reactivity analyses for studies 1 and 2, assistance with writing of the first draft, assistance with revising the manuscript after reviews.