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Nailing the Coffin Shut on Doubts that Violent Video Games Stimulate Aggression ~Comment on Anderson et al. (2010).

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Abstract

Over the past half-century the mass media, including video games, have become important socializers of children. Observational learning theory has evolved into social-cognitive information processing models that explain that what a child observes in any venue has both short-term and long-term influences on the child's behaviors and cognitions. Anderson's (2010) extensive meta-analysis of the effects of violent video games confirms what these theories predict and what prior research about other violent mass media has found – that violent video games stimulate aggression in the players in the short run and increase the risk for aggressive behaviors by the players later in life. The effects occur for males and females and for children growing up in Eastern or Western cultures. The effects are strongest for the best studies. Contrary to some critics' assertions, the meta-analysis is methodologically sound and comprehensive. Yet the results are unlikely to change the critics' views or the public's perception that the issue is undecided because there are some studies that have yielded null effects, because many people are concerned that the implications of the research threaten freedom of expression, and because many people have their identities or self-interests closely tied to violent video games.

The emergence of the mass visual media as a fundamental element of most children's socialization experiences has been one of the most dramatic changes in child rearing that has occurred in the past 100 years. Children no longer “see” only those in their own family, neighborhood, community, and culture. They are exposed at a very young age to the looks, behaviors, and beliefs of a wide variety of others behaving in a wide variety of different manners. Inevitably, these mass media exposures contribute to a child's socialization (Dubow, Huesmann, & Greenwood, 2006) just as exposures to family, peers, and community contribute.

A primary process in such socialization is observational learning (Bandura, 1973) taken in its broadest sense. Children and adolescents mimic what they see in the short run (Meltzoff & Moore, 1977) and acquire complicated scripts for behaviors, beliefs about the world, and moral precepts about how to behave in the long run from what they observe (Huesmann, 1988, 1997; Huesmann & Kirwil, 2009). It requires a tortuous logic to believe that children and adolescents are affected by what they observe in their living room, through the front window of their house, in their classroom, in their neighborhood, and among their peers, but not affected by what they observe in movies, on television, or in the video games they play. Yet many have argued just such a view in opposition to researchers who conclude that media violence

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stimulates aggression. Furthermore, the most vociferous opposition has been expressed against conclusions that violent video games might be teaching youth to behave more aggressively.

The current meta-analysis by Anderson and his colleagues is the best yet in proving beyond a reasonable doubt that exposure to video game violence increases the risk that the observer will behave more aggressively and violently in the future. "Risk," of course, does not mean "determines." The probability of behaving aggressively is increased for individuals in the population exposed, but for many exposed individuals no detectable change in behavior will occur. This does not diminish the concern we should have about violent video games as a public health threat. The same statements can be made about most public health threats including exposure to cigarette smoke and lead based paint. The *probability* of lung cancer or intelligence deficits is increased by exposure, but not guaranteed.

Anderson's meta-analysis shows that significant increases in risk for behaving aggressively occur in the short run after playing a game once and in the long run after habitual playing of games. While in many laboratory studies the aggressive behaviors that become more likely in the short run were relatively mild, in the longitudinal studies the aggressive behaviors showing the greatest increase were the more physical. If anything, effects were stronger for more violent outcomes than for less violent. As social-cognitive observational-learning theory would predict, playing violent video games had a significant effect on increasing aggressive cognitions and aggressive affect as well as the risk for aggressive behavior. Significant increases in risk for aggression occurred in Western countries and in Eastern countries. Significant increases in risk occurred for males who play violent video games and for females who play violent video games. The meta-analysis only provides very weak evidence, as theory predicts, that effects are stronger for younger game players. However, the variance in ages within a design class (experiment vs. longitudinal) was quite limited (and the Johnson et al. study cited as an example of obtaining long-term effects among older subjects was actually not a study of media violence but only of media use). While predictions from theory and extrapolations from longitudinal research suggest that the effects of violent video games should be stronger for younger children, a definitive conclusion about how age moderates the effects of violent video games may need to wait until longer term studies of violent video games are conducted that follow children into adulthood as has been done with exposure to TV violence (Huesmann, Moise, Podolski, & Eron, 2003).

All of the results in the meta-analysis have more impact because of the high quality sampling strategy and analysis techniques employed by the authors. The authors include 136 studies in their meta-analysis using a sampling frame that is highly inclusive of both published and unpublished studies that have made it into computerized data bases. This is probably about as exhaustive a sampling of the pre-2009 research literature as one could obtain and far more than used in any other review of violent video game effects. Equally important instead of simply excluding studies of "poor quality," Anderson did one meta-analysis for the full sample and another for the best practices sample. Both show significant effect sizes for playing violent video games increasing the risk of behaving aggressively. Especially noteworthy is that the effect sizes were *greater* for the "best practice" sample as one would expect if the effects are real. One should understand that many studies were excluded from the "best practices" sample not simply because Anderson and his colleagues thought they were methodologically flawed, but because some of the studies purporting to study this topic do not even assess the playing of violent video games but use the playing of video games in general as a proxy measure for the playing of violent video games.

Despite the seeming conclusiveness of this meta-analysis, I expect that it will not change the expressed views of the many purveyors of violent video games or their ad hominem attacks on researchers like Anderson; will not change the minds of the many psychological

unsophisticated journalists who write glibly in the popular press about this topic; will not change the minds of the many psychologically unsophisticated popular culture scholars who write about this topic (Jenkins, 2006); and, most disturbingly, will not change the minds of the few psychologically sophisticated researchers who deny that media violence can have any important psychological effect on the risk for aggressive behavior (e.g., Ferguson & Kilburn, 2009; Freedman, 2002).

Over the course of several decades of debate on the topic of media violence, I have written two chapters and numerous essays to counter the arguments of these psychologist critics (Huesmann, Eron, Berkowitz, & Chaffee, 1991; Huesmann & Taylor, 2003). Generally, I would argue that they eliminate entire segments of research on false grounds (e.g., experiments are artificial and can never study “real aggression”); selectively examine the remaining literature; identify correctly small flaws in studies; magnify those flaws with false logic into indictments of most of the research; uncritically accept the few flawed studies or meta-analyses that show no effects as true indicators of the population; and cite other flawed reviews as facts. Most importantly they mostly ignore observational learning theory and the general research on imitation. Some have made valuable contributions in pointing out weaknesses in studies and exaggerations in statements of policy groups (Freedman, 2002). However, the intent has been to dismiss the whole body of research on media violence as incorrect. Of course, these critics have made similar arguments about the publications of the major researchers on video game violence and media violence.

Rather than engage in another round of similar debates, let me suggest that some important individual difference variables may explain a lot of the variance in the debaters' positions. Among those psychologists who have actually done empirical research on the topic of media violence or video game violence and who understand the theory of observational learning, there is great consensus (even before the current meta-analysis is published) that media violence increases the risk for aggressive behavior (Murray, 1984). Among those scholars with a vested interest in video games, either because playing them is an important part of their identity (e.g., Ferguson; Jenkins) or because they have been funded by the media industry (e.g., Freedman), there is a lasting expressed disbelief that media violence can cause aggressive behavior. Their disbelief seems to be compounded by their failure to grasp observational learning theory. Of course, such disbelief may also be indirectly fueled in all of us by our American distaste for anyone telling us what we should look at or play. Freedom of speech and publication is an essential element of our free society and any discussion of “inappropriate content” in the mass media inevitably primes our negative reactions to censorship or control on free speech.

While most scientists have seen the flaws in the critiques of psychologists and non-psychologists alike, the influence they have had on the general public has unfortunately been substantial. There is a general perception among journalists and the public that the issue of whether media violence causes aggressive behavior is undecided. I doubt that the Anderson meta-analysis will change that public perception much. The public perception that the issue is undecided is undoubtedly aided by disingenuous presentations by some writers of irrelevant data as if it were relevant. For example, in their comment in this issue Ferguson and Kilburn present a graph showing that sales of video games (violent and non-violent) have increased over the last 12 years while youth violence has decreased, as if that negative .95 correlation between these 12 time-series points was some evidence that video game violence was not related to youth violence. Of course, it is completely irrelevant data to the theory that kids who play violent video games more are more at risk for aggression; just as data showing that youth crime increased dramatically when TV sales increased is irrelevant to concluding that kids who watch more TV violence behave more aggressively.

Despite my pessimism about the prospects for this meta-analysis changing the “deniers” views, this meta-analysis represents an important step forward for our knowledge about the causes of aggressive behavior. Not only does it confirm that playing violent video games increases the risk for aggressive behavior in the short run and in the long run, it also adds credence to the social-cognitive theory that has emerged to explain the processes through which passive or interactive media violence causes aggression. About 38 years ago the then Surgeon General of the United States, Jesse Steinfeld, reviewed the research that had been conducted to date on the effects of TV violence on youth behavior, and stated in testimony before congress,

“... it is clear to me that the causal relationship between [exposure to] televised violence and antisocial behavior is sufficient to warrant appropriate and immediate remedial action... ... there comes a time when the data are sufficient to justify action. That time has come.”

With the evidence provided by Anderson's meta-analysis, it would now be fair to make the same statement about violent video games. It is time for the public health establishment to accept the fact that playing violent video games increases the “risk” that the player will behave more aggressively.

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