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HEARING BEFORE THE SUBCOMMITTEE ON THE CONSTITUTION, CIVIL RIGHTS, AND PROPERTY RIGHTS

"WHAT'S IN A GAME? STATE REGULATION OF VIOLENT VIDEO GAMES AND THE FIRST AMENDMENT"

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I would like to thank Senator Brownback and Senator Feingold for the opportunity to testify here today. The purpose of my testimony is to describe the status of the current social science research concerning the effects of violent video games on those who play them. My remarks about the state of the research on video games are based on accepted principles in social psychology, communication and sociology, my understanding and use of the various standard research methods, my time spent in contact with game players and game developers and my experience as an active researcher of video games.

Background

I am currently an Assistant Professor at the University of Illinois at Urbana-Champaign in the Department of Speech Communication. My department is ranked in the top six nationally according to the National Communication Association Annual Survey, and number two in my research area of technology and communication. I teach courses in video games, virtual communities and the social impacts of new technology.

I have published several articles and book chapters on the topic of video game uses, effects, industrial practices, economics and social history. My work has used a wide range of research methods including content analysis, field and lab-based experimentation, interviews, industrial organization modeling and others. My papers have appeared in my field's top journals, including the *Journal of Communication,* the *Journal of Broadcasting and Electronic Media*, *Information, Communication & Society*, *Journal of Computer Mediated Communication*, *Communication Monographs,* the *International Journal on Media Management*, and in the game-specific journals *Games & Culture* and *Simulation and Gaming*. I regularly present on gaming research issues at the major communication and Internet research conferences, the game-specific research conferences, and at the Games Developer Conference.

With my co-author, I am the only person in the world to have published a field-based, i.e. non-laboratory and real-life, study of video game effects that tests the exposure of violent game imagery for longer than 75 minutes (Williams & Skoric, 2005). As someone who has completed a test with this method, I am in a relatively strong position to understand and comment on long-term effects in gaming. Yet, as this statement will illustrate, I have simply uncovered more that we have yet to learn about this medium before I or anyone else can make strong claims.

This document will outline my view that the research on video games and violence has not yet met the basic conditions for strong causal claims about the long-term effects of video games.

The research on the effects of video games is generally concerned with the potential for creating violent adolescents because of the harm they might conceivably inflict on others, and so touches on a number of cultural and social tensions (Williams, 2006, in press). It is my position that the research to date has not fulfilled sufficient conditions to establish a causal connection between exposure to violent video games and a general increase in aggression among minors or adults. In layman's terms, the work so far is suggestive, but not enough to support such strong claims.

The Media Violence Issue and Causality

There is a long history of studies on the effects of media violence, chiefly focused on television's effects. I believe that this research generally points to the susceptibility of children to experience effects at a greater rate than adults when watching television (Paik & Comstock, 1994). These effects are most likely to materialize in the acquisition of scripts about violence, emotional desensitization and in potentially aggressive behaviors. I have also found that some games can isolate players and potentially make them more lonely. This should signpost that I have found and published negative effects from gaming and am not interested in defending them for their own sake.

Like other social scientists who have studied video games, I agree that theoretically-driven models are the best way to test for effects and to advance understanding and that media is only one of several variables in the mix of risk factors for children. And I agree that experiments, cross-sectional studies, longitudinal studies and meta analyses are all important tools for advancing understanding. I have no issue with the standard measures used in the research, and have used many of them myself (e.g. scales, word-completion tasks, etc.). Our chief goal is to understand causation: what causes what. In this case, the hypothesis worth testing is that the use and observation of violent video games causes violent behaviors, feelings, beliefs and cognitions.

In assessing the state of the research concerning video games, it is important to keep in mind how causality works in the social sciences. Here, I reference a model that I know every responsible social scientist takes to heart. Causality is an extraordinarily difficult condition to prove (Popper, 1959). All of us who practice the social sciences hope to reach that level, but we are usually conservative in our claims because of the very difficult conditions which we much satisfy. Based on the generally accepted work of John Stuart Mill some 150 years ago, we all accept these three conditions for proving causality:

1) Concomitant variation, i.e. correlation, or "when one thing moves, the other also moves."
2) Time-order control, i.e. one thing must precede the other.
3) Elimination of plausible alternative hypotheses, i.e. every other reasonable explanation must be ruled out.

When these three conditions have all been met, we typically accept statements about causality. It is clear to me that the literature concerning video games to date satisfies the first two conditions. It is equally clear to me that the literature to date does *not* satisfy the third condition. There are a range of plausible, and some even likely, explanations for other causal models to be at work in the realm of video game violence.

Methods and Examples of Violent Video Game Research

There are three major methods appropriate for the study of video games and aggression: experimental designs, cross-sectional designs and longitudinal designs. Some video game researchers have also used meta-analyses guided by theoretical models to draw conclusions. Each type of method has a different set of strengths and weaknesses that address different portions of Mill's three conditions for causality. In reviewing the research, it is my opinion that the use of each method to date falls short of the three conditions.

Experimental Evidence

Experiments are the social scientist's best tool for establishing causality because, when they are designed well, they automatically address the first two conditions that Mill gave us. A well-run experiment can measure correlations through standard survey measures and observational data and can firmly establish time order because the experimenter controls the procedure. Experiments can also rule out the problem of a testing effect because the presence of a control group allows the examination of whether simply being tested causes an effect. Experiments can rarely address all possible alternative explanations, but they remain our best tool short of controlled longitudinal designs.

There have been a number of experimental studies attempting to measure the aggression effects of violent video games. The main shortcomings of these experiments are threefold.

First, they measure events that may not occur outside of a lab. Many critics decry the artificial setting of the laboratory, but I think that a control group at least partially addresses this when done well. Additionally, most well-trained researchers are careful to make the lab settings at least resemble a home environment. A more apparent problem is that experiments typically have people play alone when the majority of game play is a social experience. This presents a significant challenge to the validity of these experimental studies to date (Sherry, 2001), and the most prominent names in aggression research have noted that the research still needs to take social experience into account, but has yet to do so (Anderson et al., 2003). The prior literature on arcades, home settings and the opinion and survey data over the past 25 years shows that game players have played with other game players almost whenever possible (Williams, 2006, in press).

Thus, if experimenters measure people playing solo, it is not clear how useful any findings might be.

The second problem is one advanced by a plausible alternative hypothesis: namely, that the effects observed were not a result of playing the game, but were simply the result of being excited. In other words, it is possible that what was measured in a particular experiment was the result of excitement, not aggression. Critics can easily suggest that the same effects would occur if the subjects were running or playing Frisbee. Much of the early game research was subject to this flaw.

Professor Craig Anderson, who has done much of the research in this area, sought to address this weakness by including a second video game as a control condition (Anderson & Dill, 2000). But this study – which is the most cited in the research[1] – failed to account for the potential effects of excitement. In their study, Anderson & Dill attempted to use violent and non-violent video games that were as equivalent as possible, except for level of violence. But the researchers picked two games – the hyperkinetic violent game *Wolfenstein 3D* and the soothing game *Myst* – which cannot be considered equivalent. On their face, these two games are radically different in terms of excitement. *Wolfenstein 3D* is an exciting, fast-paced, twitch-based shooter game in which the player is hunter and hunted and usually feels intense fear and tension throughout play. In contrast, *Myst* is a deliberate, slow-paced cerebral puzzle and logic game set in an ethereal, beautiful locale with no motion. These two games would not be described by any game player or game researcher as equivalent in terms of action. They are, even to the untrained eye, the equivalent of heavy metal and classical music. The researchers' claim to have tested for equivalence by use of a pre-test raises significant validity problems, as the games are vastly different to even the most casual observer. This is no small point. Many researchers outside of the field of communication appear to be unfamiliar with gamers, game culture and game content, a fact that, as this example demonstrates, can affect the strength of their conclusions.

The third problem with the experimental research to date relates to the duration of effects. Let us ignore the preceding issues and assume for the moment that every test to date had occurred with perfect control and validity, and that the evidence showed that there was aggressive behavior after and because of violent game play. One question is whether these effects persist. Would the same players be aggressive an hour later, a week later or five years later? The typical stimulus time for a game experiment is 10 to 30 minutes, often interrupted by questions. Two studies of the same game offer a test of this hypothesis. Both Ballard & Weist (1995) and Hoffman (1995) ran studies of the aggression effects of *Mortal Kombat* on the same type of subjects. Ballard and Weist tested for 10 minutes and concluded that there was an aggression effect. Hoffman kept testing for 75 minutes. She found that the effect had dissipated almost entirely by the end

[1] This is based on use of the ISI Web of Knowledge, which tracks how many times a paper in a given topic area is cited. Based on the topic "video game" this paper is the most cited paper on effects, with 70 citations. The second-most cited, and therefore next-most influential study, is the Anderson and Bushman 2001 study discussed on the next page.

of the play session. This comparison lends strength to the explanation that the effects are either short-term only, or are simply excitation and not true aggression, which is a possibility raised by Sherry in his meta analysis (2001).

This idea of duration is an important one. It is where I find myself most confused by the frequently-made strong claims about long-term causal effects of video games. Since there are no truly long-term studies of game-based aggression, how can we take the short-term findings and make claims about what will happen in X weeks, months or years? What data are these claims based on?

The reason, as all of us know, is that if you want to make long-term claims, you need long-term studies. And unlike the television literature, these do not exist for games. A longitudinal design follows a group of people over a longer time period than a lab experiment will allow. The reason to do this is to provide a more realistic real-world exposure and to allow for long-term conclusions. If we truly want to know effects over a day, week, month or several years, then that is how long we must observe and measure. 30 minute studies cannot suffice to make lifespan-long claims. And given the two *Mortal Kombat* studies mentioned above, we have strong reasons to be suspicious of long-term claims of more than 30 minutes, let alone many years.

Longitudinal Designs

The television research has the benefit of having a well-known, truly longitudinal design, albeit one without a control condition (Huesmann, 1999). This research, although hotly disputed by some for a lack of rigor and unwarranted claims (Moeller, 2005), is generally accepted by most communication and psychology researchers. The central claims are that exposure to large amounts of televised violence causes short-term and probably long-term increases in aggressive behaviors, thoughts and cognitions.

The problem is that we do not have this kind of data for video game play. According to one well-respected game effects researcher in his meta analysis, longitudinal designs are "conspicuously absent" (Sherry, 2001) (p. 426). The longest published study to date is my own (Williams & Skoric, 2005), which followed gamers playing a violent game for one month. The average exposure time was 56 hours, which offers a much more powerful possible causal model than the typical 10 to 30 minute studies which preceded it (Hoffman's study, i.e. the one where the effects nearly disappeared, was the previous longest exposure time at 75 minutes). The study also had the benefit of being conducted in people's homes (i.e., not in a lab) and, unlike most long-term research, maintained a control group for the duration of the study. The data in my study revealed no statistically significant effects on aggression.

I will make a few observations about this study as it compares to prior studies. Given that no effects materialized after 56 hours of play, it lends credibility to the hypothesis that the short-term studies are either flawed in their settings or are subject to the excitement explanation. Nevertheless, my single study does *not* disprove that games cause violence. One month isn't a very long design, at least compared to the Huesmann work, although my own has the important advantage of a control group. Then again, I don't make claims for what will happen after my study's time window, whereas many researchers do this regularly.

I would also add that my own study, like the others before it, was a study of only one game. I will not make the case that studying one game proves what all games do. Games are simply more varied and complex than prior broadcast media and the same rules of generalizability do not apply. The research community lacks even a basic typology of content and play variables to aid such a claim. It is an error to collapse multiple games into one variable and expect a coherent result. Nevertheless, reporters have pressed me to state that my findings prove that "games" don't cause violence, but that strength of claim is not warranted by my data. One game and one month is not sufficient to make that claim. 10- and 30-minutes studies are even less able to support such claims.

Unbeknownst to most effects researchers, there actually *are* a handful of long-term game effects papers out there. Indeed, there have been three very in-depth studies of arcades and youth habits, and all of them concluded that games were not having negative impacts on children's aggression (Garner, 1991; Meadows, 1985; Ofstein, 1991). Actually, the studies all concluded that the social milieu of the arcade provided strong peer-based sanctions *against* physical violence and aggressive behaviors. Why? One of the basic appeals of video games for youth is that they are meritocratic: they are a safe play space independent of social status, physical strength, etc. (Herz, 1997). Indeed, many were havens from physical violence. This is an example of why social context, typically missing in lab experiments, is so important. Additionally, there are two now-dated studies of games, families and homes (Mitchell, 1985; Murphy, 1984), and these also concluded that games did not lead to aggression. In all five studies, the researchers took pains to note that the likelihood of aggressive behavior was inevitably related to parenting variables rather than the amount of game play. Murphy and Mitchell also noted that game play typically lead to more active family time because it tended to cut into television viewing, a finding I have also found in my own statistically-based work (Williams, 2004).

Cross-sectional Studies

There have been a number of cross-sectional studies on games and aggression, games and grades, truancy, etc. Many of these have been offered as proof of game effects, yet this is inappropriate. As every statistics student learns, correlation is not the same as causation. Showing that two things are related is very different than proving that one thing causes another. For example, the number of churches and liquor stores are nearly always correlated, but it would be incorrect to then state that going to church leads to drinking or vice-versa. Such thinking obfuscates the possibility that there is some actual third variable that drives both (population). Likewise, correlational video game studies have been offered as "proof" of the harmful effects of games since the early 1980s by showing relationships between games and poor grades, aggressive behavior, truancy, etc. Yet it is equally likely that students with poor grades and aggressive behavior are more likely to play (likely due to a lack of parental involvement and oversight) and that there is no causal relationship.

These studies are certainly important for theory-building and for establishing the need for future research. They are also useful for ruling out some alternative explanations. But since correlations are only one of the three conditions needed for causal proof, these studies provide necessary, but not sufficient evidence of a causal relationship. Thus, a cross-sectional survey can be used as an inexpensive tool to pave the way for a more involved and expensive experiment or longitudinal design. But they simply do not prove cause and should not take up space in any discussion of causal effects.

Meta-analyses

Meta-analyses are tests which use previous studies as individual data points to look at big-picture outcomes. They are important and useful tools for making sense of a large body of research, but they must be based on solid studies. Given the criticisms laid out in this document, it is my opinion that the source studies used in video game meta-analyses are not safe to use. Still, across the various studies to date, more playing time has lead to *less* aggression (Sherry, 2001). Taken together, the effects picture is anything but clear right now.

Theoretical Models

Lastly, and along the same lines of examining the plausible alternative hypotheses, I would like to review the "General Aggression Model", which guides the bulk of the research in this area. The "GAM" posits that media can affect people in several ways. The model was developed for testing the effects of watching violent television, but it is not clear that it can be used on an entirely different medium without significant modification. The two basic problems are the use of behavioral modeling and the level of active cognition that the model assumes.

By behavioral modeling, I am referring to the foundational work by Bandura (1994), in which children watching a violent act repeat that act after exposure, i.e. the children observe the behavior and then copy it. For anyone with a child, this kind of mimicry is common sense, and it is not a large leap to worry that a child watching TV will imitate an undesired behavior. Children "model" behaviors and then consider trying them.

The problem with exporting this approach to video games is that it is not clear exactly what is being "modeled." With television, the experience is generally assumed to be passive. The viewer on the couch is observing the characters on the screen and is not thinking very actively. They have the potential to model the televised characters. Yet in video games it is far more complex; there are several possible objects that might be modeled, rather than assuming passive observation. First, the player's character on the screen might be mimicked, even though *it is not clear that this is truly mimicry if the player is the one directing the action*. Secondly, the computer-directed characters might be the things observed and modeled. These are sometimes aggressive and sometimes not.

Third, the other player-controlled characters might be being modeled. These are sometimes working against the player aggressively and sometimes are helping the player. Fourth, the other people present live in the room might be modeled for behaviors. This might include other players, other viewers or parents. Any one of these figures might be a source of modeled behavior, and they might cause effects in different directions. For example, seeing a fellow player on a couch become aggressive might help the first player become even more aggressive than they would as compared to TV. Or, seeing a parent disapprove of some action might make the player less likely to internalize the behavior or even to classify it as an unacceptable real-life choice.

There are a wide range of possibilities here and some might lead to better or worse outcomes. The point is that the work to date either wholly ignores these possible sources of modeling by having players play games by themselves (the problem noted above by Sherry), or simply collapses all of these potentially different effects into one source. In social science, we say that the model is not nuanced enough to account for the actual variables that exist in real-life settings. I would note here that it is equally possible that effects are not present or are even worse than some think. The problem is that we simply don't know and it is thus inappropriate to make strong claims in the face of this potential issue.

Secondly, there is an issue with the level of "active cognitions" that occur during game play. Our generally accepted models of cognition include one route for very active thinking ("central processing") and another for relatively inattentive thinking ("peripheral processing") (Chaiken, Liberman, & Eagly, 1989; Petty & Cacioppo, 1981). The television research has always assumed a fairly inactive viewer, who is thought to use this more inattentive peripheral mode of thinking. Yet the assumption has shifted with video games to move the viewer into the more active, centrally processing group. It is not clear that this is the case, and it is even less clear when a game player might be more active or more passive. Mood management theory (Zillmann, 1988) suggests that this level of attention might vary between gamers, games or even play session. One hypothesis I have been considering is the extent to which a truly active cognitive state might either lead to especially stronger or weaker aggression effects. Consider the youth playing a violent shooter game. Is that youth actively considering the violent content? If so, is he/she going to be thinking "yes, this is exactly how I want to behave" or is he/she going to be thinking "this is a game and this is not how I behave when the game is turned off."

This latter possibility is the one found by Holm Sorensen and Jessen (2000), who, when studying very young children, found that they were highly aware of the non-real nature of the games and made separate rule sets for behaviors inside and out of play—much like children do in nearly every other form of play. Yet this kind of filtering is not included in the current approaches to video game research. Similarly, if the player is in a more passive mode, are they more or less likely to acquire these negative scripts? This is a hypothesis that has not been incorporated into the research and might make a tremendous difference. Given this possibility, I do not accept the simple statement that game players are more likely to become violent because they are playing the game rather than watching it. I find the medium more complicated than that and would need to see this hypothesis systematically tested before accepting such a claim. I find it worrisome that some researchers accept the claim without proof.

On Consensus

I would like to end by referring to the statements made by the APA and other groups (California Psychiatric Association, NAACP, Girl Scouts, etc.) in the various state cases. It is clear that they are all drawing their conclusions and talking points from the same body of research that I have taken issue with here. They repeat the correlational/causal errors and the untested concept of interactivity as a strengthener of effects. They conflate the television research with game research, and they are clearly unaware of the arousal confound in the game research. These are all good organizations (many of which I personally support), clearly trying to do the right thing, but they are uninformed and should not be involved in the policy process until they are aware of the scientific disputes. Meanwhile, other academic organizations take wholly different stands. For example, I attended the Digital Games Research Association (DiGRA) conference last year in Vancouver and the violence issue was, as always, at hand. The difference is that that association, comprised of people who do *only* games-related research, was virulently opposed to the APA statement.

A more appropriate attitude can be found in communication research circles. I am a member of the International Communication Association, the premier international body in mass communication research. This community has recently formed a games research interest group and is being lead by our field's senior scholars, including people convinced of the link between television violence and aggression. A recent event serves to show what kind of consensus there is about game effects: there was a proposal for a debate on the video game aggression issue for this year's conference in June. I was invited to take the "games do not cause aggression" approach, but declined because—even including my own long-term study—I think that the evidence does not support any strong position yet. Yet the notable outcome was that no one (out of 50 social scientists doing games-related work in communication) volunteered to take the "games cause aggression" position. Everyone who expressed an interest in the session wanted to take some more nuanced approach because they did not feel that the data warrants strong claims on either side.

This leads me to ask, Why are some people so *certain* then? The answer, I think, lies in how we as a society react to new technologies. The history of communication shows quite clearly that the advent of every major medium has been greeted with utopian dreams of democracy, but also with tales and visions of woe and social disorder (Czitrom, 1982; Neuman, 1991). The reactions themselves even follow a set pattern in every case (Wartella & Reeves, 1985). This pattern has been consistent and has maintained itself dating from the telegraph (Standage, 1999), and persisting through nickelodeons (Gabler, 1999), the telephone (Fischer, 1992), newspapers, (Ray, 1999), movies (Lowery & DeFluer, 1995), radio (Douglas, 1999), television (Schiffer, 1991), and now with both video games and the Internet. As generations age, we tend to fear the things that are new and not understood. Typically, this lets us avoid thinking about thornier issues that are personally uncomfortable to us (Glassner, 1999). In particular, we do not want to confront the reality that millions of children suffer real harm through sexual and physical abuse every year (data from the U.S. Department of Health and Human Services, 2003), and that this harm comes from within families, not outside them. About four children die every day from abuse and neglect from known people—not strangers, and not from video games.

In this sense, video games are simply the latest in a long series of contested media, an old wine in a new bottle fulfilling the same social function.

Lastly, I have reviewed the materials used by the state legislatures in Illinois and California, and I'm struck by the fact that they've excluded several major articles and points of view. It appears that they have only included the papers that they might interpret to support the law. That is politics, not science. In science we look specifically for the points of disagreement because we want to learn more, even if it upends our starting position. If 10 papers say black and 10 papers say white, there's usually a good reason why, and finding it is how we advance understanding. But if we ignore the papers that don't support our presumptions, we are only working with half of the facts. This is a poor way to conduct a review and a dangerous way to set policy, especially if it's a policy that purports to be based on a comprehensive review of the science to date.

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