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12

13   UNITED STATES DISTRICT COURT

14   FOR THE NORTHERN DISTRICT OF CALIFORNIA

15

16 VIDEO SOFTWARE DEALERS
    ASSOCIATION and ENTERTAINMENT   CASE NO. C 05-4188 RMW (RS)
17 SOFTWARE ASSOCIATION,
                                           DECLARATION OF **DMITRI WILLIAMS**
18   Plaintiffs,

19   vs.

20
    ARNOLD SCHWARZENEGGER, in his official
21 capacity as Governor of the State of California;
    BILL LOCKYER, in his official capacity as
22 Attorney General of the State of California;
    GEORGE KENNEDY, in his official capacity as
23 Santa Clara County District Attorney, RICHARD
    DOYLE, in his official capacity as City Attorney
24 for the City of San Jose, and ANN MILLER
    RAVEL, in her official capacity as County
25 Counsel for the County of Santa Clara,

26   Defendants.

27

28   I, Dmitri Williams, hereby declare as follows:

                                1

DECLARATION OF DMITRI WILLIAMS

Pursuant to 28 U.S.C. § 1746, I, Dmitri Williams, under penalty of perjury state as follows:

Background

1.   This document is my expert opinion in this case, and involves my review of the scientific literature to date regarding media violence and that literature's more recent foray into video game violence, as well as my review of the subset of items that constitute the State's Legislative Bibliography. This report was requested by the law firm of Jenner and Block, Washington, D.C. My expert opinion is based on accepted principles in social psychology, communication and sociology, my understanding and use of the various standard research methods, and my time spent in contact with game players and game developers. My CV is attached to this Declaration (Exhibit A).

2.   I received my Ph.D. in Communication Studies from the University of Michigan where I trained in both qualitative and quantitative research methods. I consider myself a social psychology experimentalist, but also believe in the importance of understanding the social context of my subjects. Thus, I usually involve a series of interviews and participant observation steps. This approach is more time consuming and difficult, but is crucial to understanding the depth and setting of most communication related issues. 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 also served as an expert witness in the case of *Entertainment Software Association v. Blagojevich,* the Illinois video games case decided this past December.

3.   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. Like most social scientists, I am familiar with the tools: ANOVA, factor analysis, structural equations modeling, multiple regression, meta analysis, etc. My work has 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*

2

DECLARATION OF DMITRI WILLIAMS

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. 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 document 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.

4.   This document will outline the case that the research on video games and violence has not yet met the basic conditions for strong causal claims. I am aware of Professor Craig Anderson's work and will be commenting on his conclusions, as well as conclusions from my own research. This research is generally concerned with 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). Thus, research articles are typically framed around spectacular well-covered incidents of youth violence such as the Columbine massacre (Anderson, 2004; Anderson et al., 2004). I will agree with much of what Prof. Anderson suggests about the television literature, but disagree with many key premises and conclusions when he and his colleagues import their approach to the study of a new and much more complex medium like video games. The major argument herein is that the research to date has fulfilled necessary, but not sufficient conditions to warrant the strength of Anderson's conclusions. In layman's terms, the work so far is suggestive, but not enough to support such strong claims.

The Media Violence Issue and Causality

5.   Let me begin by laying out some of the common ground. Like Anderson, I am concerned with the potentially negative impacts of playing violent video games. This is an area worth studying, and Prof. Anderson is well respected in it. And there is indeed a long history of media effects work on 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

DECLARATION OF DMITRI WILLIAMS

watching television (Paik & Comstock, 1994). These effects are indeed most likely to materialize in the acquisition of scripts about violence, emotional desensitization and in potentially aggressive behaviors. I note with some irony that the evidence on television as creating a "mean world" effect has been almost entirely discredited (Gerbner, Gross, Morgan, & Signorielli, 1980, 1981; Hirsch, 1980, 1981), whereas I think this effect is much more likely to occur in games (Williams, in press, 2006). Yet my research strongly suggests that these effects are very specific and likely do not yield the kind of priming-based spreading activation that lies at the heart of the hostile attribution approach. 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 particularly interested in defending them. However, this declaration will show that my read of how games work is different than Anderson's.

6.   I further agree with Anderson that media is only one of several variables in the mix of risk factors for children.

7.   I agree that theoretically driven models are the best way to test for effects and to advance understanding. 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.

8.   Lastly, I would like to spell out exactly how causality works in the social sciences by stating 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."

DECLARATION OF DMITRI WILLIAMS

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.

9.   When these three conditions have all been met, we typically accept statements about causality. Where Anderson and I part ways is in our interpretation of the literature to date and how it meets these three conditions. It is clear to me that the literature 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

10.   There are three major methods appropriate for the study of video games and aggression: experimental designs, cross-sectional designs and longitudinal designs. Each 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, but I think that Anderson's most recent unpublished work and my own recent published paper on aggression attempt to address these shortcomings, albeit in different ways. Moreover, I also believe that the guiding theoretical model-the GAM proposed by Anderson, Bushman and Dill (Anderson & Bushman, 2001; Anderson & Dill, 2000)-needs further development before it can be properly operationalized for testing video games.

Experimental Evidence

11.   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.

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DECLARATION OF DMITRI WILLIAMS

12.   The main shortcomings of the experiments to date are threefold. Number one, 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 validity challenge for the game effects work to date (Sherry, 2001), and the most prominent names in aggression research (including Anderson) have noted that the research still needs these factors included 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 (see below for a theoretical impact of social play).

13.   The second problem is one advanced by a plausible alternative hypothesis: the effects derived were not a result of playing the game, but were simply the result of being excited, i.e. what was measured 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. It was Anderson who recognized this flaw and sought to address it by including a second video game as a control condition (Anderson & Dill, 2000). I will address this study because it is the most cited, and therefore the most influential in the literature.[1] As he correctly noted, the violent and non-violent video games under study "should be made as equivalent as possible on theoretically relevant characteristics." In this case, to defuse the argument that the effect is excitement and not aggression, the researcher would want a control game with a matching level of excitement-inducing characteristics. If the control game is equally exciting, frustrating and fast-

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[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 nextmost influential study, is the Anderson and Bushman 2001 study discussed on the next page.

6

DECLARATION OF DMITRI WILLIAMS

1   paced, the difference between the two is only the violent content and the effects test is a strong one.

2   The problem is that the researchers in this best short-term experiment to date picked two games

3   which did not meet this test, but they were apparently unaware of this. In the study cited here, the

4   hyperkinetic violent game *Wolfenstein 3D* was paired with the non-violent game *Myst,* and the

5   researchers prudently pre-tested them to make sure that they were equivalent on the dimensions cited

6   above. There are two problems here. The first is that simply on their face, these two games are

7   radically different in terms of excitement. I have played both many times and am confident in making

8   this claim. *Wolfenstein 3D* is an exciting, fastpaced, twitch-based shooter game in which the player is

9   hunter and hunted and usually feels intense fear and tension throughout play. The music builds

10   anxiety and the sense of imminent threat is palpable. In contrast, *Myst* is a deliberate, slow-paced

11   cerebral puzzle and logic game set in an ethereal, beautiful locale with no motion. The music is

12   symphonic and relaxing. The player does not run or experience speed. As the player moves from area

13   to area, the screen loads the new image without the sensation of the most basic motion. It can safely

14   be described as tranquil. Yet Anderson's test found it equivalent with *Wolfenstein 3D* in "action

15   speed." This is a problem. On simple face validity, these two games would not be described by any

16   game player or game researcher as equivalent in terms of action. They are, even to the untrained eye,

17   the equivalent of heavy metal and classical music. The second issue is that a pre-test that found them

18   equivalent must have some significant validity problems. The researchers simply picked the wrong

19   games, and unfortunately also demonstrated to game-specific researchers that they could not have

20   been particularly familiar with general game content. Moreover, the peer-reviewers who approved the

21   paper could not have been familiar with game content either or red flags would have been raised

22   about the choice of the control game. This is no small point. Many researchers outside of

23   communication appear to be unfamiliar with gamers, game culture and game content.

24   14.   The third problem relates to the duration of effects. Let us ignore the preceding issues

25   and assume for the moment that every test to date had occurred with perfect control and validity, and

26   that the evidence showed that there was aggressive behavior after and because of violent game play.

27   One question is whether these effects persist. Would the same players be aggressive an hour later, a

28   week later or five years later? The typical stimulus time for a game experiment is 10 to 30 minutes,

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1   often interrupted by questions. Two studies of the same game offer a test of this hypothesis. Both

2   Ballard & Weist (1995) and Hoffman (1995) ran studies of the aggression effects of *Mortal Kombat*

3   on the same type of subjects. Ballard and Weist tested for 10 minutes and concluded that there was an

4   aggression effect. Hoffman kept testing for 75 minutes. She found that the effect had dissipated

5   almost entirely by the end of the play session. This comparison lends strength to the explanation that

6   the effects are either short-term only, or are simply excitation and not true aggression, which is a

7   possibility raised by Sherry in his meta analysis (2001).

8   15.   This drop off in duration in the Sherry analysis was a point of contention in my

9   deposition in the Illinois case and during the hearing. The State implied that Sherry was wrong. This

10   left me wondering if Sherry had used the wrong data and if I was incorrect by extension. So, to be

11   safe, I contacted Professor Sherry and we walked through the Hoffman study page by page over two

12   hours to double check Sherry's numbers. When we went through it more carefully, Professor Sherry

13   found that he had actually included too many of the data points and had overestimated the effects

14   based on the correct criteria. Thus, the correct number should have been .04 rather than .05. This

15   would be an even smaller effect than he had reported in his article, making the timedissipation

16   argument actually stronger than before. At the same time, we noticed something incorrect in

17   Anderson's interpretation. In the trial transcript from the Illinois case, Professor Anderson stated that

18   Sherry had used the wrong data because he had included people watching games and not only playing

19   them. That is incorrect, and we checked Professor Sherry's notes carefully to be sure. Professor

20   Sherry used only data from those who played. Moreover, Professor Anderson stated at the hearing

21   that the actual numbers should have been .28 or .36, and not .05. When Professor Sherry and I

22   reviewed the study carefully, we found that there were several series of measures used, most of which

23   yielded 0 or tiny scores. However, there were two scores of .27 and .36 on two of the seven

24   "vignette" measures. But in order to use those numbers, the meta-analyst would have had to not only

25   ignore the other series of standard relevant measures (the Buss Durkee scale) that he cited at the

26   hearing (most of which were near zero), but would have had to ignore the other five vignettes as well,

27   all of which were 0. In other words, to use those higher numbers and argue for large effects, he would

28   have had to cherry pick only the high numbers and ignore the others altogether.

DECLARATION OF DMITRI WILLIAMS

16.   Incorrect numbers aside, this idea of duration is an important one. It is where I find myself most confused by Anderson's strong claims about long-term causal effects. 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? In Anderson's words "longitudinal research is badly needed" (Anderson & Bushman, 2001) (p. 359). Yet this begs the question: If the findings to date are so conclusive, why would we need long-term research?

17.   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

18.   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 gainers 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

DECLARATION OF DMITRI WILLIAMS

1   benefit of being conducted in people's homes (i.e., not in a lab) and, unlike most long-term research,

2   maintained a control group for the duration of the study. There were no aggression effects in the data.

3   19.   I will make a few observations about this study as it compares to prior studies. Given

4   that no effects materialized after 56 hours of play, it lends credibility to the hypothesis that the short-

5   term studies are either flawed in their settings or are subject to the excitement explanation.

6   Nevertheless, my single study does *not* disprove that games cause violence. As Anderson has noted,

7   one month isn't a very long design, at least compared to the Huesmann work, although my own has

8   the important advantage of a control group. Then again, I don't make claims for what will happen

9   after my study's time window, whereas many researchers do this regularly. I would also add that my

10   own study, like the others before it, was a study of only one game. I will not make the case that

11   studying one game proves what all games do. Games are simply more varied and complex than prior

12   broadcast media and the same rules of generalizability do not apply. The research community lacks

13   even a basic typology of content and play variables to aid such a claim. It is an error to collapse

14   multiple games into one variable and expect a coherent result. Nevertheless, reporters have pressed

15   me to state that my findings prove that "games" don't cause violence, but that strength of claim is not

16   warranted by my data. One game and one month is not sufficient to make that claim. But if, as

17   Anderson states, one month is not long enough to make longitudinal claims, how can 10 or 30

18   minutes be?

19   20.   Unbeknownst to most effects researchers, there actually *are* a handful of long-term

20   game effects papers out there. Indeed, there have been three very in-depth studies of arcades and

21   youth habits, and all of them concluded that games were not having negative impacts on children's

22   aggression (Garner, 1991; Meadows, 1985; Ofstein, 1991). Actually, the studies all concluded that

23   the social milieu of the arcade provided strong peer-based sanctions *against* physical violence and

24   aggressive behaviors. Why? One of the basic appeals of video games for youth is that they are

25   meritocratic: they are a safe play space independent of social status, physical strength, etc. (Herz,

26   1997). Indeed, many were havens from physical violence. This is an example of why social context,

27   typically missing in lab experiments, is so important. Additionally, there are two now-dated studies of

28   games, families and homes (Mitchell, 1985; Murphy, 1984), and these also concluded that games did

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DECLARATION OF DMITRI WILLIAMS

1   not lead to aggression. In all five studies, the researchers took pains to note that the likelihood of

2   aggressive behavior was inevitably related to parenting variables rather than the amount of game

3   play. Murphy and Mitchell also noted that game play typically lead to more active family time

4   because it tended to cut into television viewing, a finding I have also found in my own statistically

5   based work (Williams, 2004).

6   21.   As part of the Illinois case, I was given Anderson et al's unpublished longitudinal

7   study to review. I believe it may represent something new in the research area. He and his colleagues

8   have tested 3r a, 4th and 5th graders to see how much playing violent video games impacts aggression

9   over the course of a two- to six-month period. And the findings as presented appear to support the

10 hypotheses that more violent game play at time one leads to more aggressive behavior at time two.

11 There are a few potential problems with the design, but we must keep in mind that this is an

12 unpublished, nonpeer-reviewed paper and isn't ready for publication yet. By the same token, it is not

13 ready to inform policy at this point. As it stands, I do not believe that it supports the findings at issue

14 in this case. One issue is that the measure of violent media at time one seems to have a lot of noise

15 because it was collected by asking non-experts (the students themselves) to evaluate the level of

16 violence in their favorite games. Students might systematically under- or over-estimate violent

17 content. If I were a reviewer, I would question that measure because of how it was collected and

18 would not be surprised by its low reliability score of .68. My guess is that there is a fair amount of

19 statistical noise in that measure in addition to the possible systematic error from the self-reports.

20 Generally, scores over .80 are deemed acceptable, although scores as low as .70 are sometimes

21 allowed with a compelling reason. Variables in media research with scores under .70 are typically

22 dropped from analysis as unreliable (Frey, Botan, & Kreps, 2000) (p. 112-115). Given that it is the

23 study's key variable, this is cause for concern in the manuscript's current state. In my one-month

24 study, I tested only one game precisely because I was concerned that the noise and variety among a

25 diverse group of complex and different games would prevent rigorous testing. This approach gave me

26 less ability to talk about the effect of "games," but far more confidence in knowing exactly what

27 game had been played, precisely what was in it, and how the players thought about that content (by

28 interviewing them).

11

DECLARATION OF DMITRI WILLIAMS

22.   The large and flexible time window for data collection (the time window varies from two to six months) is also a potential problem in terms of data collection and control. This time window allows for the intrusion of life events and historical events, i.e. the same people might respond differently at different times of the year based on their personal lives, seasonal changes in schools and households, and impactful events like 9/11. 9/11 unfortunately occurred within the study's timeframe. This is notable because many of the measures involved perceptions of how threatened the adolescent felt by daily events, which could conceivably have been impacted by the terrorist attack. This open time window issue is a particularly important one with young children, who experience so much developmental change. If I were a reviewer, I would like to hear how that traumatic event might impact findings about threats other than simply adding in a variable for it. Another item that would normally be addressed is the 70% consent rate. As Huesmann has noted, it is crucial to show that those left out are no different than those left in so that the findings are representative. (I'd further want to know that if anyone dropped out, they weren't different either, but perhaps these authors had a 100% retention rate. This isn't clear.)

23.   Also, the test is of 3rd, 4th and 5th graders, yet we are not told how the effects might vary between grades (i.e. for age). The accepted theory in television effects work suggests that younger minds are more vulnerable, so I'd expect to see the 3rd graders more affected than the 5th graders. So, are the effects uniform across these three grades? Is there a drop off in effects between grades? Why wasn't age included in the crucial destructive tests in Table 13? Given that the law in dispute is about 16-year-olds and younger, it would be very important to know if the level of risk starts to tail off at say, age 8 or age 13, or if it defies our theories and remains constant. I'm left thinking that there might be an effect in there, but I'm not sure how large or for whom or with what trend.

24.   However, the main problem I have with the study is that the authors seem eager to present their data in a way to prove that there is a large effect rather than laying out a dispassionate empirical case for readers. Table 13 shows how the various outside explainers make the actual impact much smaller, i.e. they partial out things that help explain the effect like parental involvement and gender (yet they include total screen time but not a measure of violent TV content, which would be a

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DECLARATION OF DMITRI WILLIAMS

much better control variable). Table 13 is the key to the whole study and it is an important contribution to the literature that I will accept if the authors can address the criticisms I have raised-but even so, it's just not clear how big the results are. The controlled results make for a very small final set of outcomes from the noisy game violence variable: 2%, 1 % and 3.6% of the overall change that occurred for the three major variables (hostile attribution, verbal aggression and physical aggression respectively) can be explained by playing violent games. This is *not* the same thing as saying that aggression increased by those percentages. It is saying that 2%, 1 % and 3.6% of the overall changes can be attributed to violent game play. We are not given the actual totals, so we are left asking: *2% of what? 100* more fights on average? 2 fights? It is impossible to know how big this impact is without means and standard deviations for the overall changes. If there was a small change and this small percentage of it can be explained by violent play, the true impact may be negligible.

25.   Is this a case of something that is statistically significant but substantively negligible? In their conclusions (p. 114), they present the larger, uncontrolled number of 4.8% of variance explained, rather than the smaller, controlled, and more accurate number. To the untrained observer this might imply that there is a 4.8% increase in aggression, which is not the case. The authors never present the actual amount of aggression with means and standard deviations of the measures. This is a major omission. Presenting means and deviations is a standard practice, yet the authors give only correlations. Correlations are helpful, but they only describe relationship strength, not absolute size, which is what we are really interested in. A regression model with standardized (i.e. comparable) coefficients would be very helpful in answering these questions. Or a hierarchical regression would do the same and allow for both destructive tests and comparisons. They could also have presented the destructive models in Table 13 in different orders to see the comparative levels of effect for each remaining factor. For instance, the destructive analysis in Table 13 and the correlations presented in Table 14 suggest that parental involvement might be a large factor in the effects (this is what the communications research consistently shows and so is reasonable to consider). Yet this possible moderation is not presented in the path analyses in Figures 10 and 11. Instead, the hostile attribution path is modeled, likely because the GAM predicts it.

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26.   Yet my recent research suggests that this kind of broad "mean world" effect in fact does not work that way in long-term video game exposure (Williams, 2006). Anderson's model assumes that scary game content will lead students to think that people in the real world are out to get them, but my findings show that this is not how video games work. It is far more likely that this sort of cultivation "mean world" effect is specific to particular in-game incidents, and would not spread to unrelated "mean world" events. For example, if a student were to play a game in which people shot guns a lot, they will be more likely to think that more people in the real world use guns than actually do. Yet it does not mean that a student playing this game will now become suspicious of all others, or that they would spread that suspicion to people in other unrelated contexts like a school hallway. The effect I found is context-specific and did not spread when it easily could have. Again, this sort of spreading mean-world cultivation approach is discredited in media research circles because the spreading mechanism does not hold up under the noise of diverse media (Potter, 1994; Potter & Chang, 1990; Shrum, 2001, 2002). "TV" is not a good enough variable because of the variety in it, and it's not surprising that "violent games" would be a problem as well.

27.   In the end, I am left thinking that they might establish causal direction and violent game effects in their sample-which could be a new and important finding-but I am not sure if those effects are moderate or miniscule and how they truly compare with other factors, including other media. In the mean time, their conclusions (e.g. effects are "sizeable," p. 104) are too strong and in some cases do not match the data they themselves present (e.g. they report a "negative effect of video game violence on prosocial behavior," p. 108, despite the data in Table 13 showing that this effect disappears entirely with controls). Perhaps sensing this, they defend the notion that any effect, no matter how small, is crucial because there are so many possible things that affect our behavior. I find this logic unpersuasive considering how much time is devoted to comparing theorized game effects to well-known and accepted risk factors such as smoking for cancer, etc. In the end, there is still a need to do more research to answer the key questions. Even if this study eventually works out its problems and does establish effects, it will not be in the same state as the television literature, which includes measures of actual crime years down the road.

14

DECLARATION OF DMITRI WILLIAMS

Cross-sectional Studies

28.   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 used to "prove" 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.

29.   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

30.   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 game meta-analyses are not safe to use. Anderson has attempted to separate the wheat from the chaff in the literature, which is a welcome step. However, he has also possibly used high numbers from one study and ignored low ones, if his testimony about the Hoffman dissertation was correct. Using a number like .38 rather than a .06 is obviously going to

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1 make effects look larger than they are. Unlike Sherry's meta-analysis, Andersons' several versions

2 have not shown the actual numbers used, so his work cannot be reviewed.

3   31.   Dill and Dill sought to engage in a meta-analysis but chose a narrative review instead

4 "because of the dearth of experimental findings" (Dill & Dill, 1998)(p. 407). Their narrative review

5 of the literature concluded that there was reason for concern, while another literature review in the

6 same journal stated that "the question of whether video games promote aggressiveness cannot be

7 answered at present because the available literature is relatively sparse and conflicting, and there are

8 many types of video games which probably have different effects" (Griffiths, 1999)(p. 211). Lastly,

9 Sherry's more recent meta-analysis found small overall effects, but as noted above, found a negative

10 relationship between the amount of play time and aggression, i.e. across the various studies to date,

11 more playing time has lead to *less* aggression (Sherry, 2001). Taken together, the effects picture is

12 anything but clear right now.

13   *Theoretical Models*

14   32.   Lastly, and on the same task of examining the plausible alternative hypotheses, I

15 would like to review the GAM model that guides the bulk of the psychologists' experimentation. The

16 model was developed for testing the effects of watching violent television, but it is not clear that it

17 can be used on an entirely different medium without significant modification. The two basic

18 problems are the use of behavioral modeling and the level of active cognition that the model assumes.

19   33.   By behavioral modeling, I am referring to the foundational work by Bandura (1994),

20 in which children watching a violent act repeat that act after exposure, i.e. the children observe the

21 behavior and then copy it. For anyone with a child, this kind of mimicry is common sense, and it is

22 not a large leap to worry that a child watching TV will imitate an undesired behavior. Children

23 "model" behaviors and then consider trying them. The problem with exporting this approach to video

24 games is that it is not clear exactly what is being "modeled." With television, the experience is

25 generally assumed to be passive. The viewer on the couch is observing the characters on the screen

26 and is not thinking very actively. They have the potential to model the televised characters. Yet in

27 video games it is far more complex; there are several possible objects that might be modeled, rather

28 than assuming passive observation. First, the player's character on the screen might be mimicked,

16

DECLARATION OF DMITRI WILLIAMS

1 even though *it is not clear that this is truly mimicry if the player is the one directing the action.*

2 Secondly, the computer-directed characters might be the things observed and modeled. These are

3 sometimes aggressive and sometimes not. Third, the other player-controlled characters might be

4 being modeled. These are sometimes working against the player aggressively and sometimes are

5 helping the player. Fourth, the other people present live in the room might be modeled for behaviors.

6 This might include other players, other viewers or parents. Any one of these figures might be a source

7 of modeled behavior, and they might cause effects in different directions. For example, seeing a

8 fellow player on a couch become aggressive might help the first player become even more aggressive

9 than they would as compared to TV. Or, seeing a parent disapprove of some action might make the

10 player less likely to internalize the behavior or even to classify it as an unacceptable real-life choice.

11 There are a wide range of possibilities here and some might lead to better or worse outcomes. The

12 point is that the work to date either wholly ignores these possible sources of modeling by having

13 players play games by themselves (the problem noted above by Sherry), or simply collapses all of

14 these potentially different effects into one source. In social science, we say that the model is not

15 nuanced enough to account for the actual variables that exist in real-life settings. I would note here

16 that this is no defense of gaming; it could equally mean that effects are not present or are even worse

17 than Anderson thinks. The problem is that we simply don't know and it is inappropriate to make

18 strong claims in the face of this potential issue.

19   34.   Secondly, there is an issue with the level of active cognitions that occur during game

20 play. Our generally accepted models of cognition include one route for very active thinking ("central

21 processing") and another for relatively inattentive thinking ("peripheral processing") (Chaiken,

22 Liberman, & Eagly, 1989; Petty & Cacioppo, 1981). The television research has always assumed a

23 fairly inactive viewer, who is thought to use this more inattentive peripheral mode of thinking. Yet

24 the assumption has shifted with video games to move the viewer into the more active, centrally

25 processing group. It is not clear that this is the case, and it is even less clear when a game player

26 might be more active or more passive. Mood management theory (Zillmann, 1988) suggests that this

27 level of attention might vary between gamers, games or even play session. One hypothesis I have

28 been considering is the extent to which a truly active cognitive state might either lead to especially

DECLARATION OF DMITRI WILLIAMS

1   stronger or weaker aggression effects. Consider the youth playing a violent shooter game. Is that

2   youth actively considering the violent content? If so, is he/she going to be thinking "yes, this is

3   exactly how I want to behave" or is he/she going to be thinking "this is a game and this is not how I

4   behave when the game is turned off." This latter possibility is the one found by Holm Sorensen and

5   Jessen (2000), who, when studying very young children, found that they were highly aware of the

6   non-real nature of the games and made separate rule sets for behaviors inside and out of playmuch

7   like children do in nearly every other form of play. Yet this kind of filtering is not included in the

8   current approaches. Similarly, if the player is in a more passive mode, are they more or less likely to

9   acquire these negative scripts? This is a hypothesis that has not been incorporated into the research

10   and might make a tremendous difference. Given this possibility, I do not accept the simple statement

11   that game players are more likely to become violent because they are playing the game rather than

12   watching it. I find the medium more complicated than that and would need to see this hypothesis

13   systematically tested before accepting such a claim. I find it worrisome that some researchers accept

14   the claim without proof.

On Consensus

16   35.   I would like to end by referring to the statements made by the APA and other groups

17   (California Psychiatric Association, NAACP, Girl Scouts, etc.). I read through their support letters

18   and it is clear that they are all drawing their conclusions and talking points from the same body of

19   research that Anderson endorses and that I have taken issue with here. They repeat the

20   correlational/causal errors, and use the in-press study of 3^d-5th graders and the untested concept of

21   interactivity as a strengthener of effects. They conflate the television research with game research,

22   and they are clearly unaware of the arousal confound in the game research. These are all good

23   organizations (many of which I personally support), clearly trying to do the right thing, but they are

24   uninformed and should not be involved in the policy process until they are aware of the scientific

25   disputes. Meanwhile, other academic organizations take wholly different stands. I attended the

26   Digital Games Research Association (DiGRA) conference last year in Vancouver and the violence

27   issue was, as always, at hand. The difference is that that association, comprised of people who do

28   *only* games-related research, was virulently opposed to the APA statement. I heard this very clearly in

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DECLARATION OF DMITRI WILLIAMS

1   the general assembly and see this consistently on the research listserv discussions. Professor

2   Anderson has stated in several briefs that the evidence is now conclusive and that there is no longer

3   any serious debate about these issues by "true experts." With due respect, that is both dismissive and

4   inaccurate. It ignores entire branches of the academy.

5   36.   This brings me to a note about consensus. If the APA says something, that doesn't

6   make it true. If DiGRA says something (e.g. games never cause effects), it doesn't make that true

7   either. The fact is that we simply do not have the data to support any of the several strong claims I

8   regularly hear about this issue. This is not a case of an ideologue denying the existence of global

9   warming. It is a case of a social scientist being appropriately conservative in the face of scarce data.

10   37.   A more appropriate attitude can be found in communication research circles. I am a

11 member of the International Communication Association, the premier international body in

12 communications research. Many communications researchers are trained as social psychologists, but

13 we all specialized in understanding the use, content and reception of media. Unlike some disciplines,

14 this is not a facet of our research. It is our only research focus. Communication researchers are well-

15 positioned to understand the content and context, use and effects of media simply because it *is* our

16 entire field. This community has recently formed a games research interest group and is being lead

17 by our field's senior scholars, including people convinced of the link between television violence and

18 aggression. A recent event serves to show what kind of consensus there is about game effects: there

19 was a proposal for a debate on the video game aggression issue for this year's conference in June. I

20 was invited to take the "games do not cause aggression" approach, but declined because-even

21 including my own long-term study-I think that the evidence does not support any strong position yet,

22 including any kind of "defense." Yet the notable outcome was that no one (out of 50 social scientists

23 doing games-related work in communication) has volunteered to take the "games cause aggression"

24 position. Everyone who expressed an interest in the session wanted to take some more nuanced

25 approach because they did not feel that the data warrants strong claims on either side.

26   38.   This leads me to ask, Why are some people so *certain* then? The answer, I think, lies

27 in how we as a society react to new technologies. The history of communication shows quite clearly

28 that the advent of every major medium has been greeted with utopian dreams of democracy, but also

DECLARATION OF DMITRI WILLIAMS

1   with tales and visions of woe and social disorder (Czitrom, 1982; Neuman, 1991). The reactions

2   themselves even follow a set pattern in every case (Wartella & Reeves, 1985). This pattern has been

3   consistent and has maintained itself dating from the telegraph (Standage, 1999), and persisting

4   through nickelodeons (Gabler, 1999), the telephone (Fischer, 1992), newspapers, (Ray, 1999),

5   movies (Lowery & DeFluer, 1995), radio (Douglas, 1999), television (Schiffer, 1991), and now with

6   both video games and the Internet. As generations age, we tend to fear the things that are new and not

7   understood. Typically, this lets us avoid thinking about thornier issues that are personally

8   uncomfortable to us (Glassner, 1999). In the case of video games, much of the angst is related to how

9   well we treat our children, and we are looking for explanations or outlets for our concerns. In the case

10 of electronic media, the real source of tension comes from the dramatic shift in the role of women

11 over the past three decades. As more women have left the home to pursue jobs and income, our

12 society is grappling with a large shift toward child care rather than parental care. The unease is

13 palpable. We worry that electronic media are babysitters doing our jobs for us and we don't feel good

14 about it. Thus, there is a heady dose of collective guilt over how we treat children and we, and our

15 news media (Williams, 2003), are looking to external sources of blame rather than to look at

16 ourselves (Williams, 2006, in press). Thus, video games, the Internet, television and music have born

17 extra scrutiny over the past thirty years. In this sense, video games are simply the latest in a long

18 series of contested media, an old wine in a new bottle fulfilling the same social function.

19   39.   Lastly, I have reviewed the state's legislative bibliography of academic works and I'm

20 struck by the fact that they've excluded several major articles and points of view. It appears that they

21 have only included the papers that they might interpret to support the law. That is politics, not

22 science. In science we look specifically for the points of disagreement because we want to learn

23 more, even if it upends our starting position. If 10 papers say black and 10 papers say white, there's

24 usually a good reason why, and finding it is how we advance understanding. But if we ignore the

25 papers that don't support our presumptions, we are only working with half of the facts. This is a dumb

26 way to conduct science and a dangerous way to set policy, especially if it's a policy that purports to

27 be based on a comprehensive review of the literature.

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DECLARATION OF DMITRI WILLIAMS

40.   I have read the articles on both sides of the issue and considered them carefully. My conclusion, which I hope I have laid out here, is that there are many unanswered questions and that we should know the answers with certainty and precision before we make any strong claims about what games do or don't do.

Conclusion

41.   The findings and statements above are, in my opinion, based on a reasonable degree of scientific certainty. Therefore, I disagree with the claim that "minors who play violent video games are more likely to exhibit violent, asocial, or aggressive behavior and/or experience feelings of aggression." I do not think the claim is well supported by existing theory or data.

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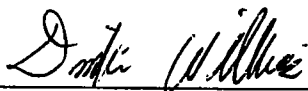
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DECLARATION OF DMITRI WILLIAMS

I declare under the penalty of perjury that the foregoing is true and correct.

Dated this _29_ day of March,

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DMITRI WILLIAMS