

EXHIBIT 6

IN THE UNITED STATES DISTRICT COURT
NORTHERN DISTRICT OF ILLINOIS
EASTERN DIVISION

ENTERTAINMENT SOFTWARE)
ASSOCIATION, VIDEO SOFTWARE)
DEALERS ASSOCIATION, and)
ILLINOIS RETAIL MERCHANTS)
ASSOCIATION,)

Plaintiffs,)

vs.)

NO. 05C 4265

ROD BLAGOJEVICH, in his official)
capacity as Governor of the State of)
Illinois; LISA MADIGAN, in her official)
capacity as Attorney General of the State)
of Illinois; and RICHARD A. DEVINE,)
in his official capacity as State's)
Attorney of Cook County,)

Defendants.)

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. 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.

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*, *Communication Monographs*, the *International Journal on Media Management*, and more recently 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. In it, 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 helpful and 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 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. This should signpost 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, as Anderson states, 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 Anderson and every other 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.

9. When these three conditions have all been met, we typically accept statements about causality. Where Anderson and I will 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. The three methods outlined by Anderson—experimental designs, cross-sectional designs and longitudinal designs—are all appropriate for the study of video games and aggression. 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 attempts to address these

shortcomings. 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 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.

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, 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

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.

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-paced, the difference between the two is only the violent content and the effects test is a strong one. The problem is that the researchers in this best short-term experiment to date picked two games which did not meet this test, but they were apparently unaware of this. In the study cited here, the hyperkinetic violent game *Wolfenstein 3D* was paired with the non-violent game *Myst*, and the researchers prudently pre-tested them to make sure that they were equivalent on the dimensions cited above. There are two problems here. The first is that simply on their face, these two games are radically different in terms of excitement. I have played both many times and am confident in making this claim. *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. The music builds anxiety and the sense of imminent threat is palpable. In contrast, *Myst* is a deliberate, slow-paced cerebral puzzle and logic game set in an ethereal, beautiful locale with no motion. The music is symphonic and relaxing. The player does not run or experience speed. As the player moves from area to area, the screen loads the new image without the sensation of the most basic motion. It can safely be described as tranquil. Yet Anderson's test found it equivalent with *Wolfenstein 3D* in "action speed." This is a problem. On simple face validity, 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 second issue is that a pre-test that found them equivalent must have some significant validity problems. The researchers simply picked the wrong games, and unfortunately also demonstrated to game-specific researchers that they could not have been particularly familiar with general game content. Moreover, the peer-reviewers who approved the paper could not have been familiar with game content either or red flags would have been raised about the choice of the control game.

14. This is no small point. Many researchers outside of communication appear to be unfamiliar with gamers, game culture and game content. I note that new techniques in game creation and modification allow for more precise control of violent stimuli which could be very useful to social scientists. As all gamers know, a "mod" allows the creation of a game scenario with control over the content. These tools could easily be adapted to perform a more rigorous controlled test without delving into the vagaries of titles across genres.

15. The third problem 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 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 and measured at multiple intervals, ending at 75 minutes. He found that the effect occurred in the short term, but then

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

16. This idea of duration is an important one, and a place 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 own words "longitudinal research is badly needed" (Anderson & Bushman, 2001) (p. 359). 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.

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 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 30- and 45-minute studies which preceded it (75 minutes was the previous longest exposure time). 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. There were no aggression effects in the data.

19. 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. As Anderson correctly notes, one month isn't a truly longitudinal design, at least compared to the Huesmann work, although my own has the important advantage of a control group. 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. But if, as Anderson states, one month is not long enough to make longitudinal claims, how can 30 minutes be?

20. 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 of 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). 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 found in my own statistically based work (Williams, 2004).

21. Anderson et al’s unpublished longitudinal study represents something new in the research area. He and his colleagues have tested 3rd, 4th and 5th graders to see how much playing violent video games impacts aggression over the course of a two- to six-month period. And the findings as presented appear to support the hypotheses that more violent game play at time one leads to more aggressive behavior at time two. There are a few potential problems with the design, but we must keep in mind that this is an unpublished, non-peer-reviewed paper and isn’t ready for publication yet. By the same token, it is not ready to inform policy at this point. As it stands, I do not believe that it supports the findings at issue in this case. One issue is that the measure of violent media at time one seems to have a lot of noise because it was collected by asking non-experts (the students themselves) to evaluate the level of violence in their favorite games. Students might systematically under- or over-estimate violent content. If I were a reviewer, I would question that measure because of how it was collected and would not be surprised by its low reliability score of .68. My guess is that there is a fair amount of statistical noise in that measure in addition to the possible systematic error from the self-reports. Generally, scores over .80 are deemed acceptable, although scores as low as .70 are sometimes allowed with a compelling reason. Variables with scores under .70 are usually dropped from analysis as unreliable. (Frey, Botan, & Kreps, 2000)(p. 112-115). Given that it is the study’s key variable, this is cause for concern in the manuscript’s current state. In my one-month study, I tested only one game precisely because I was concerned that the noise and variety among a diverse group of complex and different games would prevent rigorous testing. This approach gave me less ability to talk about the effect of “games,” but far more confidence in knowing exactly what game had been

played, precisely what was in it, and how the players thought about that content (by interviewing them).

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 (which was unfortunately within the study's timeframe). This is a particular issue with young children, who experience so much developmental change. If I were a reviewer, I would like to hear how that might impact the findings 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 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 18-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 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. This is also the same as saying that 98%, 99% and 96.4% of that change can be explained by something else. It is impossible to know how big this impact is without means and standard deviations for the overall changes, which are not given. 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, which to the untrained observer might imply that there is a 4.8% increase in aggression, which is not the case. The authors never present the actual 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.

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, in press, 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. 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. Given that this finding is currently in press (i.e. peer reviewed and accepted, but not yet in print), Anderson’s team is probably unaware of it, although it was presented at an international conference last year.

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. there is a key finding of a “negative effect of video game violence on pro-social 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.

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 effects 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. As Anderson notes, these studies are important for theory building and for establishing the need for future research. Since correlations are 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, my own separation technique would not have left enough to do a meta-analysis. Two other researchers agree with this criticism. Dill and Dill sought to engage in a meta-analysis but chose a narrative review instead “because of the dearth of experimental findings” (Dill & Dill, 1998)(p. 407). Their narrative review of the literature concluded that there was reason for concern, while another literature review in the same journal stated that “the question of whether video games promote aggressiveness cannot be answered at present because the available

literature is relatively sparse and conflicting, and there are many types of video games which probably have different effects” (Griffiths, 1999)(p. 211). Lastly, Sherry’s more recent meta-analysis found small overall effects, but also found a negative relationship between the amount of play time and aggression, i.e. 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

31. Lastly, and on the same task of examining the plausible alternative hypotheses, I would like to review the GAM model that guides the bulk of the psychologists’ experimentation. 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.

32. 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 this is no defense of gaming; it could equally mean that effects are not present or are even worse than Anderson thinks. The problem is that we

simply don't know and it is inappropriate to make strong claims in the face of this potential issue.

33. 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. 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

34. I would like to end by referring to the statements made by the APA and other groups on both sides of the violent games issue. The APA has concluded that the evidence on games is sufficient to make strong causal claims. Meanwhile, other academic organizations take different stands. I attended the Digital Games Research Association (DiGRA) conference earlier this summer 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. I heard this very clearly in the general assembly and see this consistently on the research listserv discussions. Professor Anderson has stated in several briefs that the evidence is now conclusive and that there is no longer any serious debate about these issues by "true experts." With due respect, that is both dismissive and inaccurate. It ignores entire branches of the academy.

35. This brings me to a note about consensus. If the APA says something, that doesn't make it true. If DiGRA says something (e.g. games never cause effects), it doesn't make that true either. The fact is that we simply do not have the data to support any of the several strong claims I regularly hear about this issue. This is not a case of an ideologue denying the existence of global warming. It is a case of a social scientist being appropriately conservative in the face of scarce data.

36. 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 communications research. Many communications researchers are trained as social psychologists, but we all specialized in understanding the use, content and reception of media. Unlike some disciplines, this is not a facet of our research. It is our only research focus. Communication researchers are well-positioned to understand the content and context, use and effects of media simply because it *is* our entire field. 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 is a proposal for a debate on the video game aggression issue for next year's conference. 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, including any kind of "defense." Yet the notable outcome was that no one (out of 50 social scientists doing games-related work in communication) has 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.

37. 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. Video games are simply the latest in a long series of contested media, an old wine in a new bottle.

38. My own 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

39. 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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- Williams, D. (in press, 2006). Virtual cultivation: Online worlds, offline perceptions. *Journal of Communication*.
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I declare under penalty of perjury that the foregoing is true and correct.

EXECUTED on October 14, 2005.



Dmitri Williams

EXHIBIT A

Dmitri Williams Curriculum Vitae

**Assistant Professor, Speech Communication
University of Illinois at Urbana-Champaign**

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Degrees

Ph.D., Department of Communication Studies,
University of Michigan, 2004.

M.A., Communication Management, Annenberg School.
University of Southern California, 1996

B.A., English (Creative Writing), Minor in Business Administration.
University of Southern California, 1993

Publications

Journal Articles

- Williams, D. (in press). "Why Game Studies Now?" *Games & Culture: A Journal of Interactive Media*.
- Williams, D. (in press). Bridging the Methodological Divide in Game Research. *Simulation & Gaming*.
- Williams, D. (2006, in press). Virtual Cultivation: Online Worlds, Offline Perceptions. *Journal of Communication*.
- Williams, D. & M. Skoric (2005). Internet Fantasy Violence: Aggression in an Online Game. *Communication Monographs*, 72(2), p. 217-233.
- Valentino, N., Hutchings, V., & Williams, D. (2004). Advertising and the Architecture of Political Judgment: Consequences for Knowledge, Information Seeking, and Candidate Preference. *Journal of Communication*, 54(2), p. 337-354.
- Williams, D. (2003). The Video Game Lightning Rod: Constructions of a New Media Technology, 1970-2000. *Information, Communication and Society*, 6(4), p. 523-550.
- Williams, D. (2002). A Structural Analysis of Market Competition in the U.S. Home Video Game Industry. *International Journal on Media Management*, 4(1), p. 41-54.
- Williams, D. (2002). Synergy Bias: Conglomerates and Promotion in the News. *Journal of Broadcasting and Electronic Media*, 46(3), p.453-472.

Book Chapter

- Williams, D. (2005/2006, in press) A (Short) Social History of Electronic Games. In Vorderer, P & Bryant, J. (Eds.) *Playing Computer Games: Motives, Responses, and Consequences*. Mahwah, NJ: Lawrence Erlbaum.

Book Review

- Williams, D. (2003). The Medium of the Video Game by Mark J.P. Wolf. *Popular Communication*. 1(4), p. 251-253.

Currently Under Review

- Williams, D. "The Social and Civic Impact of Online Gaming."
- Williams, D. "Measuring Bridging and Bonding Online and Off: The Development and Validation of a Social Capital Instrument."
- Williams, D. "The Impact of Time Online: Social Capital and Cyberbalkanization."
- Steinkeuhler, C. & D. Williams "Where Everybody Knows Your (Screen) Name: Social Capital in Online Gaming."

Conference Papers & Presentations

- Williams, D. (2005, October 9). Usability/sociability In VoIP group systems. Association of Internet Researchers Meeting. Chicago, Illinois.
- Williams, D., Caplan, S. & Xiong, L. (2005, October 8). Can you hear me now? The social impact of voice on internet communities. Association of Internet Researchers Meeting 6.0. Chicago, Illinois.
- Williams, D. (2005, June 24). The Social Impact of MMOG Play. Games, Learning and Society Conference. Madison, Wisconsin.
- Williams, D. (2005, June 17). A Brief Social History of Game Play. Digital Games Research Association. Vancouver, Canada.
- Williams, D. (2005, May 27). Virtual Cultivation: Online Worlds, Offline Perceptions. International Communication Association Conference. New York City, New York.
- Steinkuehler, C. A. & Williams, D. (2005, April 15). Where Everybody Knows Your (Screen) Name: Online Games as 'Third Places.' Annual Meeting of the American Educational Research Association, Montreal, Canada. Also presented at the Digital Games Research Association. Vancouver, Canada (2005, June 19).
- Williams, D. (2003, November 22). "The Social Impact of an Online Game." National Communications Association Conference, Miami Beach, Florida.

- Williams, D. (2003, October 18). "Bridging, Bonding and Slaying the Evil Foo: Social Capital in an Online Game." Association of Internet Researchers Annual Conference 4.0, Toronto, Ontario.
- Williams, D. & Skorlic, M. (2003, July 30). "Massively Multiplayer Mayhem: Aggression in an Online Game." Association for Education in Journalism and Mass Communication Conference. Kansas City, Missouri.
- Knobloch, S., Keplinger, C. & Williams, D. (2003, July 30). "Thrilling News: Factors Generating Suspense During News Exposure." (With Silvia Knobloch and Caterina Keplinger) Association for Education in Journalism and Mass Communication Conference. Kansas City, Missouri.
- Williams, D. (2003, May 27). "Gains and Losses Online and Off: Bridging and Bonding Social Capital in Weak- and Strong-tie Networks." International Communication Association Conference. San Diego, California.
- Williams, D. (2002, July 17). "Structure and Competition in the U.S. Home Video Game Industry." Presented at the International Communications Association Conference. Seoul, South Korea.
Top Three Paper Award, Communication and Technology Division.
- Valentino, N. Hutchings, V. & Williams, D. (2002, April 26). "Advertising and the Architecture of Political Judgment: Consequences for Knowledge, Information Seeking, and Candidate Preference." Presented at the Midwest Political Science Association Conference. Chicago, Illinois.
- Williams, D. (2002, April 4). "Media Constructions of Video Games, 1970-2000." Presented at the ESRC Playing With the Future Conference. Manchester, England.
- Valentino, N., Hutchings, V., Philpot, T., & Williams, D. (2001, August 30). "When Enough is Enough, and When It Isn't: Political Advertising, Partisan Threat, and Selective Information Seeking on the Internet." Presented at the American Political Science Association. San Francisco, California.
- Williams, D. (2001, May 27). "Bots and e-commerce: Fluid Markets and the Death of Retail?" Presented at the International Communications Association Conference. Washington, D.C..
- Williams, D. (1999, October). "Synergy Bias: A Content Analysis of Evening Network News Examining Parent-Company Influence." Presented at the Midwest Association for Public Opinion Research. Chicago, Illinois.
Top Three Student Paper Award.
(Revision presented at the Association for Education in Journalism and Mass Communication, Phoenix, Arizona, August, 2000)

Invited Lectures

- "Academic Insights: What Researchers Can and Can't Tell You About Your Games." Games Developers Conference, San Francisco, California. March 10, 2005

- "What You Need to Know as a Game Maker." Presented to Wolverine Soft, Ann Arbor, Michigan. April 3, 2003.
- "Research Methods for the Online World." Presented at the Undergraduate Research Opportunity Program, Ann Arbor, Michigan. February 20, 2003.

Service

Journal Reviews

Aggressive Behavior, Journal of Broadcasting and Electronic Media, Communication Theory, Critical Studies in Mass Communication, Journal of Computer Mediated Communication, Human Communication Research.

Conference Paper Reviews

Association for Education in Journalism and Mass Communication (AEJMC), Association of Internet Researchers (AoIR), International Communication Association (ICA).

Blogs

Author, terranova.blogs.com. Virtual worlds academic and industry collaborative blog. Approximately 2,000 hits per day.

Education

Dissertation Committee

Susan Douglas, W. Russell Neuman (co-chairs), Paul Resnick, Nojin Kwak, Nicholas Valentino

Dissertation: "Trouble in River City: The Social Life of Video Games."

- Field experiment, content analysis, participant observation and historical study of game effects and sociopolitical factors.
- Defended, December 11, 2003.

Preliminary Exams and Committee Members

Methodology: Michael Traugott & Nicholas Valentino, August 2000

Communication Theory: Susan Douglas & Kristin Harrison (University of Illinois), March 2001

Economics and Policy: Barry Litman (Michigan State University) & W. Russell Neuman, May 2001

Fellowships

Dissertation Fellowship, Fall 2003

ISR Summer Research Assistant Fellowship, Summer 2002

ISR Summer Research Assistant Fellowship, Summer 2000

Jay H. Payne Summer Research Fellowship, Summer 1999.

University of Michigan Regent's Fellowship, awarded on entrance, 1998.

Awards

Mark Foote Distinguished Dissertation Award, 2004

Outstanding Graduate Student Teaching Award, Instructional and Developmental Division, International Communications Association, 2003.

Top Paper Award, Communication and Technology Division, International

Communications Association Conference, 2002.

Top Three Student Paper Award, Midwest Association for Public Opinion Research, 1999.

Academic Work Experience

Research Assistantships

- 2002 Internet and Political Activity Pilot (NSF Grant), PIs: Nicholas Valentino, Vincent Hutchings. Experimental design, web page survey construction, computer programming, staff training and supervision.
- Vote 2000 Priming Experiment Series (3 studies), PIs: Nicholas Valentino, Mike Traugott, Vincent Hutchings. Experimental design, survey construction, computer programming, multimedia production (creation of 17 political advertisements), staff supervision and running subjects.
- Research on Satellite Communications and CB Radio systems, Professor Susan Douglas, Summer 1999. Archival research.

Courses Developed and Taught

- Communications 479: Video games: Content, Industry and Policy, Spring 2002.

Graduate Student Instructorships (T.A.)

- Communication Studies 111: Introduction to Computers and Networks, Lab Instructor. Responsible for 60 students. Fall 2002.
- Film and Video Studies 370: Television History, Fall 2001, Professor Bambi Haggins. Responsible for 50 students.
- Communication Studies 111: Introduction to Computers and Networks, Lab Instructor. Responsible for 60 students. Winter 2001.
- Film and Video Studies 370: Television History, Fall 2000, Professor Bambi Haggins. Responsible for 50 students.
- Communication Studies 101: Introduction to the Mass Media, Winter 2000, Professor Susan Douglas. Responsible for 90 students.
- Communication Studies 101: Introduction to the Mass Media, Fall 1999, Professor Susan Douglas. Responsible for 90 students

Invited Class Lectures

- "Social Capital in Online Game Communities." and "Video Games, Politics, and the Beltway." Presented in Electrical Engineering and Computer Science 494: Computer Game Design and Implementation. Professor John Laird. 12/2/02 & 12/9/02
- "Media Conglomerates and Communication Systems." Presented in Comm 361: Media Processes and Effects. Professor Nojin Kwak. 10/4/00

- "Technological Determinism in Broadcasting? Satellites, Innovators and Policy in the Cable Revolution." Presented in Film and Video 370: Television History. Professor Bambi Haggins. 11/24/00

- "Three Ways to Change: An Industrial History of Television." Presented in Film and Video 370: Television History. Professor Bambi Haggins. 11/22/01

Graduate Student Mentor

Responsible for training and coordination of teaching assistants for the departments of Communication Studies and Film and Video Studies. Included leading a two-term seminar for new instructors, Comm 993. Fall 2000 and Winter 2001. Lead in conjunction with Susan Douglas.

Orientation Mentor/Advisor

The Center for Research and Learning on Teaching. 2000 GSI Orientation.

Statistical and Computing Consultant

Summer Program Computer Staff. ICPSR (Inter-University Consortium for Political and Social Research), Summer 1999. Statistical consulting and technical support for visiting scholars.

Departmental Involvement

- Department of Communication Graduate Student Representative, Rackham Graduate Student Ongoing Forum.

- Graduate Communication Association, founding member and faculty representative.

- Department of Communication, Student-Faculty representative, 1999-2000.

Professional Memberships

- National Communication Association (NCA)
- International Communication Association (ICA)
- Association for Education In Journalism and Mass Communication (AEJMC)
- Association of Internet Researchers (AoIR)
- Digital Games Research Association (DiGRA)

Prior Work Experience

Freelance Graphic Design, 8/96 to 10/2003. Graphic design, writing and editing, planning, consulting, digital manipulation, pre-press.

Director of Communication, USC Admission Office, 6/95 to 6/98.

Writing and editing, budget and planning, management, graphic design, digital manipulation, multimedia design, web site design, mailing systems, database design, photography.

Publications Assistant, USC Admission Office, 10/93 to 6/95.

Copywriting, layout and editing. Professional Honors

- **University College Designers Association's Gold Award, 1996.**
- **Admissions Marketing Report's Best of Show, 1995.**
- **CASE Award of Excellence, 1995.**

EXHIBIT 7

1 IN THE UNITED STATES DISTRICT COURT
2 FOR THE NORTHERN DISTRICT OF ILLINOIS
3 EASTERN DIVISION

3	ENTERTAINMENT SOFTWARE)	Docket No. 05 C 4265
	ASSOCIATION; VIDEO SOFTWARE)	
4	DEALERS ASSOCIATION; and)	Chicago, Illinois
	ILLINOIS RETAIL MERCHANTS)	Wednesday, November 16, 2005
5	ASSOCIATION,)	10:00 o'clock a.m.
)	
6	Plaintiffs,)	
)	
7	v.)	
)	
8	ROD BLAGOJEVICH, in his)	
	official capacity as Governor)	
9	of the State of Illinois;)	
	LISA MADIGAN, in her official)	
10	capacity as Attorney General)	
	of the State of Illinois; and)	
11	RICHARD A. DEVINE, in his)	
	official capacity as State's)	
12	Attorney of Cook County,)	
)	
13	Defendants.)	

14 VOLUME 3
15 TRANSCRIPT OF PROCEEDINGS
16 BEFORE THE HONORABLE MATTHEW F. KENNELLY

17 APPEARANCES:

17	For the Plaintiffs:	JENNER & BLOCK LLP
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19		MR. PAUL M. SMITH
		MS. KATHERINE R. HARTNETT
20		
		JENNER & BLOCK LLP
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22		MR. DAVID P. SANDERS
23	For the Defendants:	OFFICE OF THE ATTORNEY GENERAL
		(100 W. Randolph Street, 13th Floor,
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		MR. ANDREW L. DRYJANSKI
25		MS. ELLECIA L. PARSELL-BURKE

1 For the Defendants (cont.): HOGAN MARREN, LTD.
2 (180 N. Wacker Drive, Suite 600,
Chicago, IL 60606) by
MR. PATRICK E. DEADY

3
4 COOK COUNTY STATE'S ATTORNEY
5 (500 Richard J. Daley Center,
6 50 W. Washington Street,
Chicago, IL 60602) by
MR. STEPHEN L. GARCIA

7 Court Reporter: Laura M. Brennan
8 Room 2102
219 S. Dearborn Street
Chicago, IL 60604
(312) 427-4393

* * * * *

1 THE COURT: Fifteen may actually be a little on the
2 high end of some of these things in terms of skill.

3 MR. SMITH: Your Honor, on the question of whether
4 you owe deference to the findings made by the legislature, I
5 would point you to the Indianapolis case where there were
6 extensive findings made by the Indianapolis city council,
7 and what the Seventh Circuit said --

8 THE COURT: That's the Hudnut case?

9 MR. SMITH: Yes. No. This is the AAMA case.

10 THE COURT: Oh, the AAMA case.

11 MR. SMITH: And what Judge Posner said there was
12 that we would require a compelling showing of evidence in
13 court to justify this law. The findings don't change the
14 underlying obligation where you have strict scrutiny.

15 And the language in Turner that we quote is also
16 apposite there. It says the government can't just posit
17 some harm; it has to demonstrate it in court.

18 So, the findings, I think, are kind of a red
19 herring. You can't get around strict scrutiny by putting
20 some language in a statute that makes factual findings that
21 aren't supported.

22 THE COURT: You don't need to convince me of that.
23 I think that's right.

24 MR. SMITH: Now, I didn't really address the
25 frontal lobe evidence, but I think it's pretty clear that

1 whatever Dr. Kronenberger tried to prove, it didn't show
2 that there was any actual harm to anybody. First of all, he
3 didn't focus on video game play; they were using all media.
4 And second of all, what they didn't --

5 THE COURT: Well, and I guess the other thing is is
6 that the testimony of your guy, whose name escapes me right
7 now.

8 MR. SMITH: Dr. Nusbaum.

9 THE COURT: Dr. Nusbaum, which I think in this
10 respect, at least, is unrebutted, is that decreased activity
11 in a lobe doesn't necessarily mean it's damaged. It may
12 mean that you're just better at whatever activity it is.

13 MR. SMITH: Right. And, actually, there's pretty
14 good evidence out there that video game play can give you
15 some expertise and attention and --

16 THE COURT: The point being that decreased frontal
17 lobe activity does not translate into quote, unquote brain
18 damage.

19 MR. SMITH: That's correct, your Honor.

20 THE COURT: You don't need to address that further.

21 MR. SMITH: Okay. On the question that came out
22 before about discrimination among media, we have discussed
23 that in our brief under Footnote 8 where we cite -- this is
24 in our main brief, Footnote 8. We quote some language that
25 says when the law discriminates among media -- this is in

1 the Turner case -- that often presents serious First
2 Amendment concerns.

3 THE COURT: Which footnote you said?

4 MR. SMITH: This is Footnote 8 in our memorandum in
5 support of the motion for a preliminary injunction.

6 THE COURT: Okay.

7 MR. SMITH: And I think that's all the points I
8 needed to make.

9 THE COURT: Now, let's talk about what I'm going to
10 get. Somebody's going to get me the depositions. Were
11 there depositions taken of all these other people?

12 MR. SMITH: Those three factual declarants were all
13 deposed, your Honor.

14 THE COURT: Okay. Well, get me their depositions.
15 I mean, I'm assuming that somebody can do that within the
16 next day or two.

17 MR. SMITH: We can do that this afternoon, I think,
18 your Honor. And we could get you a brief within a day or so
19 that just points to -- the question was what is there in the
20 record about the existing signage that's already out there
21 in the world and whether or not it needs to be fixed.

22 THE COURT: Oh. In other words, pointing me to
23 where that is in the depositions?

24 MR. SMITH: Yes.

25 THE COURT: I'm going to read the depositions. You

1 don't have to give me that.

2 MR. SMITH: All right. And so, between the
3 declarations and the depositions, we think that issue is
4 covered. So, that would be --

5 THE COURT: All right. My time, just so everybody
6 knows here -- I know that this law is supposed to take place
7 January 1st, right? My timetable on this is to have a
8 ruling done by the end of this month. I mean, that's not a
9 ruling date because sometimes there's slippage in these
10 things, but that's my goal. And we'll let both sides know
11 by phone. What I typically -- what we do is we post it on
12 the court website once it's issued. And so, we'll let you
13 both know, and you can get it that way.

14 MR. SMITH: Thank you for doing this on such an
15 expedited basis. We appreciate it.

16 THE COURT: Happy to do it. Thanks very much to
17 both sides.

18 (Which were all the proceedings had in the
19 above-entitled hearing on the days and dates aforesaid.)

20 I certify that the foregoing is a correct transcript
21 from the record of proceedings in the above-entitled matter.

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24 Laura M. Brennan

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