

# Sexual and Violent Media's Inhibition of Advertisement Memory: Effect or Artifact?<sup>1</sup>

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Research (Bushman, 2005; Bushman & Bonacci, 2002) has claimed to demonstrate that sexual and violent content in television programs inhibits viewers' memory for advertisements. However, that research failed to adequately control other aspects of the programs' content, making interpretation problematic. The present paper attempts to correct these flaws. Studies 1 and 2 demonstrate that if other aspects of show content are held constant, sex and violence alone do not affect memory for advertisements. Study 3 provides evidence that while sex or violence does not affect memory, other aspects of program content (e.g., plot, humor) do have a significant influence on advertisement memory. Implications of this research on the interpretation of previous research are discussed.

Social scientists have spent decades examining the effects of television on viewers. The research has been diverse, spanning such topics as the portrayal of gender stereotypes, the value of television as an educational venue, and the influence of televised debates on presidential elections (for an overview, see Oskamp, 1988). Much of the research has focused on the potentially negative effects of violent or sexual content, and there have frequently been social, political, and policy implications drawn from such research (e.g., Federal Trade Commission, 2000; "Joint Statement," 2000; National Television Violence Study, 1996, 1997, 1998). Although some researchers have painted the negative effects of sex or violence as powerful and clear-cut (e.g., Anderson & Bushman, 2002; Court, 1984; Donnerstein, Linz, & Penrod, 1987), others have suggested that the evidence is neither strong nor straightforward (Freedman, 1988, 2002; Milavsky, 1988; Ward, 2003).

One recent topic of related research is whether sexual or violent program content affects memory of advertisements shown during the program. Even within this narrow research domain, there have been conflicting findings and a heavy dose of social and public policy spin. Research is often presented

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hand in hand with calls for corporations to limit spending on advertising in certain objectionable programs (Bushman, 1998; Prasad & Smith, 1994).

A meta-analysis by Bushman and Phillips (2001) examined the effects of violence in television programs on viewers' memory of advertisements within those programs. The authors concluded that viewers remember fewer advertisements from within violent television programs. However, a careful examination of the studies included in the meta-analysis raises some questions about those conclusions. Of the 11 published articles included in the meta-analysis, only two (Bushman, 1998; Prasad & Smith, 1994) specifically examined violent content as a primary independent variable. Bushman (1998) reported three studies with 520 total subjects making up the bulk of the subjects in the larger meta-analysis, which compared memory for advertisements placed in violent movies to those placed in nonviolent movies. The results showed memory impairment for both brand and message recall of advertisements within the violent movies. Prasad and Smith showed violent and nonviolent versions of the same "dramatic television program" to children and then measured their recall of and attitudes toward the product advertised. The results showed no main effect for violent content on either product recall or advertising copy recognition, but the results suggested that program content had an impact on emotional responses to the products advertised.

The remaining studies in the meta-analysis did not specifically examine the effects of violent content. The conceptual independent variables were either mood (Goldberg & Gorn, 1987; Mathur & Chattopadhyay, 1991; Mundorf, Zillmann, & Drew, 1991; Murphy, Cunningham, & Wilcox, 1979; Murry, Lastovicka, & Singh, 1992) or cognitive engagement/involvement (Bryant & Comisky, 1978; Kennedy, 1971; Soldow & Principe, 1981). In fact, only two of these studies included clearly violent media conditions. One (Mundorf et al., 1991) used a graphic video of a man shooting himself to provoke "disturbing" feelings; while the other (Bryant & Comisky, 1978) used a fight scene from the police show *Banacek*, but this violent content was held constant across all experimental conditions and the study varied advertisement placement. The remaining articles made no direct mention of violent content.

Other published research has suggested that a more complex relationship exists between program content and advertisement memory. Gunter and colleagues (Gunter, Tohala, & Furnham, 2001; Gunter, Furnham, & Pappa, 2005) placed violent and nonviolent advertisements in both violent and nonviolent programs. Neither study showed a main effect of violent program content, but both showed a significant interaction. In both studies, violent advertisements within violent programs were remembered better than were nonviolent advertisements similarly embedded in violent programs. In addi-

tion, violent advertisements evidenced stronger recall than did nonviolent advertisements, regardless of the program content.

Shen and Prinsen (1999) placed involving and uninvolved advertisements in violent and nonviolent programs, and again found no main effect for violent program content on memory for advertisements. They did find that recognition for high-involvement advertisements was better in nonviolent programs, but recognition for low-involvement advertisements did not differ between violent and nonviolent programs. This effect was not replicated for recall measures, which showed no difference between violent and nonviolent programs.

These studies suggest that violent content, in and of itself, does not necessarily impair memory of advertisements. Instead, they suggest that program content can influence viewers' mood or cognitive involvement, and these states may interact with advertisement content to inhibit memory for some advertisements.

When reviewing previous research on violent media content and memory, one methodological issue stands out. Very few studies use violent and nonviolent versions of the same programs. Exceptions include Prasad and Smith (1994), who edited an "action adventure" movie into violent and nonviolent versions and Bryant and Comisky (1978) who edited a police drama so that ads came before, during, or after violent conflict (plus a control condition that saw no program). Neither study demonstrated a significant main effect of violent content on memory of advertisements. More commonly, entirely different programs were used in different conditions. For example, Bushman (1998) compared violent movies (e.g., *Cobra*, *Die Hard*, *Single White Female*) to nonviolent movies (e.g., *Gorillas in the Mist*, *Awakenings*, *Chariots of Fire*, *Field of Dreams*). Although these movies vary in violent content, they also vary in genre and emotional tone, as well as other dimensions. Because variables such as mood, engagement, and excitement can have significant and complex effects on memory, such differences among the movies could easily have a confounding effect on advertisement memory.

Taken together, this research paints a blurry, complex picture of the potential negative impact of violent content on advertisement memory. There is rarely an unambiguous manipulation of violent content. Often, the programs used as stimuli confound violence with genre or other components of content, and there can be an interaction (as opposed to a main effect) between program content and advertisement content. Consequently, it seems that the conclusion of Bushman and Phillips' (2001) meta-analysis—that violent content inhibits memory for advertising—is tenuous.

More recently, researchers have studied the effect of sexual content on memory of advertisements. There is already a long line of research examining the effects of sexual content within advertising itself (for a review, see

Reichert, 2002). The pattern of results in this area is complex. Sexual content in advertisements does attract attention, and viewers find such advertisements more engaging (Belch, Holgerson, Belch, & Koppman, 1981; Dudley, 1999; Reichert, Heckler, & Jackson, 2001). Sexual content can lead to greater recognition of the ad (Reid & Soley, 1981) and to greater behavioral intentions to purchase the products (e.g., Grazer & Keesling, 1995; Reichert et al., 2001). However, there is strong evidence that sexual content is a distraction that interferes with a viewer's ability to process the advertisement content.

Sexual content inhibits viewers' ability to remember brand names (Judd & Alexander, 1983; Reichert & Alvaro, 2001) and specific information contained in the advertisements (Severn, Belch, & Belch, 1990). Although such research did not examine the effect of sexual program content on advertisement memory, it suggests that an effect likely exists. Unfortunately, an effect could be predicted in either direction. If sexual content captures the attention of the viewer, this increased attention may aid memory of the advertisements. On the other hand, sexual program content may draw on and deplete all of the viewer's attentional resources, essentially creating an attentional refractory period, thus inhibiting memory for the advertisements that follow.

Bushman and Bonacci (2002) directly examined the effects of both sexual and violent program content on advertisement memory. They hypothesized that either violent or sexual content would interfere with advertisement memory. Subjects viewed programs with sexual, violent, or neutral content and were given immediate and delayed tests of memory for the advertisements contained within the programs. The results indicated that, compared to viewers of neutral programs, those who saw either the violent or sexual programs demonstrated poorer memory of the advertisements.

A follow-up study (Bushman, 2005) expanded on this original research by adding a sex-and-violence condition, thus completing the factorial design, and added a measure of consumer behavior: choosing coupons. The results were similar to those of the earlier study: Violence, sex, or their combination lowered recognition of the ads, behavioral intention to purchase the products, and the likelihood that participants would take coupons for those products. Bushman concluded that advertisers should avoid buying time in programs that contain sex or violence because viewers do not remember or respond to the advertisements.

Methodological shortcomings in that research, however, raise questions about the conclusions. As with much of the previous research on violent content, the content of the programs used in both studies (Bushman, 2005; Bushman & Bonacci, 2002) was not well controlled. The programs varied substantially in ways other than merely their sexual or violent content. For example, programs used in the sexual-content condition were more likely to be comedies (e.g., *The Man Show*, *Sex in the City*); violent programs were

more likely to be dark dramas or adventures (e.g., *CSI*, 24); and control programs tended to be light family fare (e.g., *Touched by an Angel*, *America's Funniest Animals*). In addition to obvious differences of genre, some were plot-driven programs and others were based on skits, vignettes, or video clips. As stated earlier, variables such as mood, engagement, and excitement can have significant and complex effects on memory. Extraneous differences among the programs could easily have confounded the results.

It is also unclear how successfully Bushman and Bonacci (2002) or Bushman (2005) manipulated sexual content. Many of the programs used in the sexual-content conditions (e.g., *The Man Show*) were comedies with more sexual jokes and references than actual sexual scenes. Consequently, manipulation checks revealed that although such programs were rated significantly higher on sexual content than were other programs, the ratings did not pass the midpoint of the scale in either study. Bushman and Bonacci reported a mean of 3.58 on a 10-point scale; while Bushman reported means of 3.60 and 3.42 (also on a 10-point scale) for the sex and the sex-and-violence conditions, respectively. Although the midpoints on such scales are always somewhat arbitrary, it suggests that the manipulation of sexual content could be strengthened.

The studies reported herein began as an attempt to replicate the findings of Bushman (2005; Bushman & Bonacci, 2002), while correcting for the methodological limits of those studies by using more standardized content with stronger manipulations. In Study 1, a full factorial design using four edited versions of the same movie was used in order to control better for program content.

## Study 1

### *Method*

#### *Materials*

The movie *True Romance* was chosen for use in Study 1 because the first third of the movie, which was shown to participants, contains several superfluous, but graphic sexual and violent scenes. The movie was edited to create four conditions (a complete  $2 \times 2$  factorial): sex only; violence only; sex and violence; and no sex or violence.

All conditions contained a standardized 7-min introductory section, a 2.5-min commercial break, a middle segment of approximately 20 min, another 2.5-min commercial break, and a standardized 7-min final segment. The middle section was edited to contain two sex scenes (30–60 s), one violent scene about 2 min in length, all three scenes, or none, thus yielding the four

experimental conditions. None of these scenes were integral to the plot of the movie.

Each commercial break consisted of seven advertisements: three of them 30 s in length, and four of them 15 s in length. The order in which the ads appeared was held constant across all trials and conditions. Only certain types of product advertisements were selected for use in the research. Each advertisement was for a nationally available product or service. Only one advertisement per product category was used (i.e., only one soft drink advertisement, only one fast-food chain advertisement, etc.). Finally, only products with at least four recognizable competing brands available within the same category were used (e.g., Band-Aids<sup>®</sup> were not used, as there are not four other well-known brands of adhesive bandages available to serve as distracters). These guidelines were used to enable the development of better memory measures.

A questionnaire administered before the movie assessed viewers' attitudes and television viewing habits. This included items on attitudes toward sexual and violent media, religious beliefs, and demographics. A post-movie measure assessed reactions to the movie, including the manipulation checks, measures of familiarity with the movie, and measures of interest and involvement with the movie. Both of these measures were designed to replicate the measures used in Bushman's (2005; Bushman & Bonacci, 2002) research as closely as possible, with the exception that 7-point scales were used.

The dependent measures were also designed to replicate Bushman and Bonacci (2002). We used three measures of advertisement retention. Immediately following the post-movie measure, which included the manipulation checks, participants were asked to simply write as many products that they could remember seeing advertised. Following that, a multiple-choice test was administered to measure recognition of the advertised products. This measure asked participants to identify what product they saw advertised within a specific product category (e.g., "Which air freshener did you see advertised?") from among five brands: the correct brand and four distracter brands. The order of the questions in relation to the order of the ads was randomized, as was the order of the correct responses within the distracters.

A delayed-recognition test was distributed 24 hr later. This multiple-choice test was e-mailed to participants as an Excel<sup>™</sup> file with thumbnail pictures of product logos listed beside each correct and distracter brand name.

### *Participants and Procedure*

Study participants were 111 college students who received extra credit for their participation. Although Bushman and Bonacci (2002) made a point of

recruiting participants of ages not traditional for college students, we did not feel that this was vital. Young adults under the age of 30 are a key demographic group sought by advertisers and television programmers alike. Therefore, it is fitting that we used this sample in the present research.

Participants were run in groups of 5 to 15 at a time. Study participants first signed a standard informed consent form notifying them of the possibility of viewing sexual or violent material. They then completed the pre-movie questionnaire and watched the appropriately edited video. The video was projected on a large screen at the front of the room via an LCD projector. When the video ended, participants completed the post-movie questionnaire and the initial memory tests. Participants received the delayed-recognition task via e-mail 24 hr after their session.

## *Results*

### *Manipulation Checks*

The post-movie questionnaire asked participants to rate, on a 7-point scale, the sexual and violent content of the movie they just viewed. Means are reported in Table 1. As anticipated, mean ratings from viewers in the sex conditions were significantly higher than those in the no-sex conditions,  $t(109) = -10.50, p < .001, d = 2.01$ . Similarly, ratings of violent content were significantly higher among viewers in the violence conditions than in the no-violence conditions,  $t(109) = -8.85, p < .001, d = 1.70$ .

As a further manipulation check, we asked participants to rate how offended they were by the sexual content and the violent content of the movie. Those in the sex conditions reported a higher mean rating of sexual offensiveness than those in the no-sex conditions,  $t(109) = -2.91, p = .004, d = 0.56$ . Similarly, those in the violence conditions reported a higher mean rating of violence offensiveness than those in no-violence conditions,  $t(109) = -3.84, p < .001, d = 0.74$ .

To ensure that the selective editing of the movie did not disrupt the plot more in one condition than in another, an ANOVA was conducted on viewers' ratings of how difficult the plot was to follow. There was no main effect of sex,  $F(1, 107) = 0.94, p = .334$ ; or violence,  $F(1, 107) = 0.79, p = .377$ ; and there was no Sex  $\times$  Violence interaction,  $F(1, 107) = 0.00, p = .990$ .

### *Dependent Measures*

The key dependent measure was the number of advertisements correctly recalled or recognized (out of a possible 14). On the recall measure, the answer

Table 1

*Means of Manipulation Checks and Dependent Measures by Condition: Study 1*

	No sexual content				Sexual content			
	No violence		Violence		No violence		Violence	
	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>
Manipulation checks								
Sexual content <sup>a</sup>	2.71	1.30	3.08	1.09	5.15	1.03	5.04	0.98
Sexual offensiveness <sup>a</sup>	2.23	1.28	2.27	1.15	3.00	1.59	3.15	1.92
Violent content <sup>a</sup>	3.10	1.45	5.46	0.81	3.48	1.55	5.37	1.12
Violent offensiveness <sup>a</sup>	2.32	1.40	3.58	1.70	2.31	1.47	3.56	2.04
Dependent measures								
Immediate recall	2.37	1.81	2.88	1.54	2.04	1.54	2.30	1.46
Immediate recognition	9.37	2.66	9.76	2.62	9.68	2.81	9.09	2.19
Delayed recognition	8.96	1.74	10.44	2.10	9.96	2.35	8.39	2.74

<sup>a</sup>A significant main effect of the variable was demonstrated.

was counted as correct if the participant identified the product by name (e.g., Vive for Men), but not if the participant identified only the product type or category (e.g., shampoo). On the delayed-recognition measure, the response rate was 90.0%. All immediate measures were included in the analysis, even for participants who failed to complete the delayed measure.

A MANOVA was conducted to test for the effects of sex and violence on the three dependent variables: recall, immediate recognition, and delayed recognition.<sup>3</sup> Means of each dependent measure within each condition are reported in Table 1.

<sup>3</sup>The studies that we attempted to replicate and extend (i.e., Bushman, 2005; Bushman & Bonacci, 2002) reported ANCOVA as the primary means of analysis. We measured most or all of the same covariates. These include viewing habits, familiarity with the programs used, ratings of interest and involvement in the programs used, religious beliefs, and beliefs on sex and violence in general. We found that the inclusion of all these covariates did not change the pattern of results in any way. In the interest of clarity and brevity, therefore, we chose to present the results without any covariates.



This analysis yielded no significant effect of sex, Wilks'  $\Lambda$ ,  $F(3, 94) = 1.04$ ,  $p = .377$ , partial  $\eta^2 = .032$ ; and no significant effect of violence, Wilks'  $\Lambda$ ,  $F(3, 94) = 0.73$ ,  $p = .540$ , partial  $\eta^2 = .023$ . However, it did reveal a significant interaction between sex and violence, Wilks'  $\Lambda$ ,  $F(3, 94) = 3.68$ ,  $p = .015$ , partial  $\eta^2 = .105$ . Follow-up univariate ANOVAs on each of the three dependent variables, however, reveal that this interaction was not present among scores of either the recall measure,  $F(1, 96) = 0.15$ ,  $p = .702$ , partial  $\eta^2 = .002$ ; or the immediate-recognition measure,  $F(1, 96) = 0.90$ ,  $p = .346$ , partial  $\eta^2 = .009$ . This unanticipated interaction existed only among the scores of the delayed-recognition measure,  $F(1, 96) = 9.25$ ,  $p = .003$ , partial  $\eta^2 = .088$ . Delayed recognition was highest in the violence/no-sex condition; and it was lowest in the sex-and-violence condition.

### Discussion

Study 1 clearly did not replicate Bushman's (2005; Bushman & Bonacci, 2002) finding that sexual and violent content in programming inhibits viewers' memory of advertisements. The manipulation checks suggest that sexual and violent content were successfully manipulated, yet virtually no effect on memory of advertisements was detected. The only significant effect was an unexpected interaction between sexual and violent conditions only on the scores of the delayed-recognition test. The implications of this particular interaction are unclear, however, and the fact that it only occurred in one of the three dependent variables suggests that it may have simply been a Type I error.

### Study 2

It is possible that the lack of significant effects in Study 1 was a result of a lack of power in the manipulation. Although manipulation checks reveal significant differences in rated levels of sexual or violent content, the differences in ratings of sexual or violent offensiveness—although also significantly different—were not as large. The means in all conditions were below the midpoint of the scale.

If stronger and perhaps more offensive sexual or violent content is necessary to impede advertisement memory, stronger manipulations may be necessary to elicit this effect. Therefore, we chose a movie with stronger sexual and violent content (*Wild at Heart*) to serve as the stimulus in Study 2. Furthermore, we wanted to find if the unexpected interaction on the delayed-recognition measure would replicate or if it would disappear, suggesting that it was a merely a Type I error.

## Method

### Materials

The materials used were the same as in Study 1, with the exception that the movie *Wild at Heart* was edited to produce the four experimental conditions. The movie contains stronger sexual and violent scenes than the movie that we used in Study 1, and the middle segment containing the sexual and violent scenes was also longer. We used the same advertisements as in Study 1, in the same order and presented at roughly the same points in the program. The same pre-movie, post-movie, and memory measures were used as well.

### Participants and Procedure

Study participants were 104 college students who received extra credit for their participation. None of the participants had participated in Study 1. The procedure was identical to that of Study 1.

## Results

### Manipulation Checks

The means of all manipulation checks are reported in Table 2. As in Study 1, viewers' ratings of sexual content were higher in the sex conditions than in the no-sex conditions,  $t(102) = -16.37$ ,  $p < .001$ ,  $d = 3.24$ . Likewise, mean ratings of sexual offensiveness were significantly higher in the sex conditions than in no-sex conditions,  $t(102) = -6.56$ ,  $p < .001$ ,  $d = 1.30$ . Ratings of violent content were significantly higher in the violent conditions than in the no-violence conditions,  $t(102) = -12.47$ ,  $p < .001$ ,  $d = 2.47$ . Finally, ratings of violence offensiveness in the violence conditions were significantly higher than those in the no-violence conditions,  $t(102) = 4.41$ ,  $p < .001$ ,  $d = 0.87$ .

The purpose of Study 2 was not only to replicate Study 1, but to do so with stronger manipulations. To compare the ratings of sexual content and of violent content across both movies (*True Romance* from Study 1 and *Wild at Heart* from Study 2), we conducted a Movie  $\times$  Sex  $\times$  Violence ANOVA. The difference between mean ratings of sexual content (in the sex and no-sex conditions) was greater in *Wild at Heart* than in *True Romance*,  $F(1, 206) = 27.12$ ,  $p < .001$ , partial  $\eta^2 = .117$ . Similarly, the difference between mean ratings of violent content (in the violence and no-violence conditions) was also greater in *Wild at Heart* than in *True Romance*,  $F(1, 206) = 6.10$ ,

Table 2

*Means of Manipulation Checks and Dependent Measures by Condition: Study 2*

	No sexual content				Sexual content			
	No violence		Violence		No violence		Violence	
	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>
Manipulation checks								
Sexual content <sup>a</sup>	1.83	0.78	2.76	1.62	6.19	1.06	6.03	0.96
Sexual offensiveness <sup>a</sup>	1.57	0.95	2.24	1.62	4.54	1.88	3.57	1.68
Violent content <sup>a</sup>	2.61	1.27	6.24	1.09	3.96	1.22	6.20	0.81
Violent offensiveness <sup>a</sup>	1.91	1.38	4.12	2.11	3.08	1.50	3.97	1.73
Dependent measures								
Immediate recall	3.27	1.45	2.79	1.91	2.70	1.64	2.69	1.67
Immediate recognition	10.32	1.89	10.63	1.91	10.13	2.46	10.41	2.41
Delayed recognition	10.00	2.05	10.00	2.64	9.52	2.52	9.69	2.29

<sup>a</sup>A significant main effect of the variable was demonstrated.

$p = .014$ , partial  $\eta^2 = .029$ . Thus, the manipulations in Study 2 were successfully strengthened over those in Study 1.

As in Study 1, mean ratings of how hard the plot was to follow did not differ by either sex condition,  $F(1, 100) = 0.66$ ,  $p = .418$ , partial  $\eta^2 = .007$ ; or violence condition,  $F(1, 100) = 1.39$ ,  $p = .241$ , partial  $\eta^2 = .014$ . There was no Sex  $\times$  Violence interaction, either,  $F(1, 100) = 0.01$ ,  $p = .906$ , partial  $\eta^2 = .000$ .

*Dependent Measures*

The dependent measures were scored in a manner identical to Study 1. Response rate on the delayed recognition for Study 2 was 95.5%. A MANOVA was conducted to test for effects of sex by violence on the three dependent variables: recall, immediate recognition, and delayed recognition. Means of each dependent measure in each condition are reported in Table 2. This analysis reveals no effect of sex, Wilks'  $\Lambda$ ,  $F(3, 92) = 0.40$ ,  $p = .752$ ,

partial  $\eta^2 = .013$ ; or violence, Wilks'  $\Lambda$ ,  $F(3, 92) = 0.51$ ,  $p = .676$ , partial  $\eta^2 = .016$ . In addition, there was no interaction, Wilks'  $\Lambda$ ,  $F(3, 92) = 0.19$ ,  $p = .901$ , partial  $\eta^2 = .006$ .

### *Discussion*

Study 1 demonstrated very little effect of sexual or violent program content on viewers' memory of advertisements. Study 2, with demonstrably stronger manipulations, demonstrated no effect of sexual or violent program content on any of the measures of memory. Furthermore, the unanticipated Sex  $\times$  Violence interaction on the delayed-recognition measure that was found in Study 1 was not replicated in Study 2. This failure to replicate strengthens our interpretation of that interaction in Study 1 as merely a Type I error.

### Study 3

Together, the results of Study 1 and Study 2 strongly suggest that if content is otherwise held constant, the sexual or violent content in programs does not inhibit the memory of advertisements in those programs. These results suggest further that the effects reported by Bushman and colleagues (Bushman, 1998, 2005; Bushman & Bonacci, 2002) were likely not caused by the sexual and violent content of the programs that they used as stimuli, but rather by some concomitant differences between those programs. Extraneous differences among the programs they used as stimuli include whether the programs were comedies, as opposed to dramas or informational, and whether the programs used a plot format, as opposed to skit or video clip-based programs. As such, Study 3 is designed to test again for effects of sexual or violent content. It is also designed to determine if memory differences can be demonstrated between programs that differ on these other dimensions, such as genre.

In designing Study 3, we chose to use television shows (from network or basic cable), rather than movies. This provides a broader variation of programs used, increases the overall external validity of the research, and even more closely replicates the programs used by Bushman (2005; Bushman & Bonacci, 2002).

For Study 3, we created conditions that varied on sexual content and violent content, specifically including different edited episodes of the same program to control best for extraneous program content. We also chose to include programs that varied with regard to presence of plot and presence of

humor. These conditions would allow us to test our hypotheses by conducting a one-way ANOVA with planned comparisons. Given that constraint, we found it preferable to restrict the dependent measure to a single variable. We selected the psychometrically strongest measure—immediate recognition—to be the dependent variable. This immediate objective measure avoided any potential problems of scoring free-recall responses and avoided any potential problems with response rates inherent in delayed-recall measures.

### *Method*

#### *Materials*

The materials included 10 edited television program episodes. Two episodes of *Sex and the City*, a plot-based program with humor, were included: one edited to include sexual content and one edited to exclude it. Two episodes of *Law & Order*, also a plot-based program but without humor, were included: one edited to include violent content and one edited to exclude it. Two episodes of *Reno 911*, a non-plot-based program with humor, were included: one edited to include sexual content and one edited to exclude it. Two episodes of *Cops*, a non-plot-based program without humor, were included: one edited to include violence, and one edited to exclude it. Two other programs contributed one episode each as controls. An episode of *Judging Amy* provided a plot-based program without sex, violence, or humor. Finally, an episode of *Unsolved Mysteries* provided a non-plot-based program without sex, violence, or humor.

To ensure that the commercials included in the programs were current (roughly 1 year had passed since Study 1 was initiated), 14 new commercials were selected using the same guidelines as in Studies 1 and 2. They were again divided into two commercial breaks with seven advertisements each (three were 30 s long, and four were 15 s long). The commercials and their order were held constant across all conditions. The order of the questions in relation to the order of the ads was randomized, as was the order of the correct responses within the distracters.

#### *Participants and Procedure*

Study participants were 235 college students who received extra credit for their participation. None of the participants had participated in Study 1 or Study 2. The procedure and measures were identical to that of Study 1, except that only an immediate-recognition measure was collected.

Table 3

*Mean Number of Advertisements Correctly Recognized: Study 3*

Program (condition)	<i>M</i>	<i>SD</i>	<i>n</i>
1. <i>Unsolved Mysteries</i> (no plot, no humor, no sex, no violence)	9.43	2.39	28
2. <i>Judging Amy</i> (plot, no humor, no sex, no violence)	9.04	2.61	24
3. <i>Sex and the City A</i> (plot, humor, no sex, no violence)	9.73	2.55	22
4. <i>Sex and the City B</i> (plot, humor, sex, no violence)	10.77	2.43	22
5. <i>Reno 911 A</i> (no plot, humor, no sex, no violence)	9.19	2.29	27
6. <i>Reno 911 B</i> (no plot, humor, sex, no violence)	9.36	2.24	22
7. <i>Cops A</i> (no plot, no humor, no sex, no violence)	8.57	2.98	23
8. <i>Cops B</i> (no plot, no humor, no sex, violence)	8.82	1.87	22
9. <i>Law &amp; Order A</i> (plot, no humor, no sex, no violence)	9.00	2.93	22
10. <i>Law &amp; Order B</i> (plot, no humor, no sex, violence)	9.83	2.29	23

*Note.* Planned comparisons for sexual content compared Programs 4 and 6 against all others. Planned comparisons for violent content compared Programs 8 and 10 against all others. Planned comparisons for comedy compared Programs 3, 4, 5, and 6 against all others. Planned comparisons for plot compared Programs 2, 3, 4, 9, and 10 against all others.

*Results*

The mean number of advertisements remembered (out of 14) by participants in each condition is presented in Table 3. Means for all manipulation checks, as well as those of the specific planned comparisons used to test the hypotheses, are presented in Table 4.

*Manipulation Checks*

Ratings of sexual content were significantly higher in the sex conditions than in the no-sex conditions,  $t(231) = -17.58$ ,  $p < .001$ ,  $d = 2.31$ . Similarly,

Table 4

*Unadjusted Means of Manipulation Checks and Dependent Measures by Condition: Study 3*

Independent variable	Treatment conditions			Control conditions		
	<i>M</i>	<i>SD</i>	<i>N</i>	<i>M</i>	<i>SD</i>	<i>N</i>
Manipulation checks						
Sex <sup>a</sup>	4.27	0.87	44	1.81	0.83	189
Violence <sup>a</sup>	4.20	0.87	45	2.36	1.16	190
Plot <sup>a</sup>	3.96	0.92	133	2.58	1.20	122
Humor <sup>a</sup>	3.42	1.01	93	1.82	1.09	142
Dependent measures						
Sex	10.07	2.42	44	9.20	2.49	191
Violence	9.33	2.13	45	9.37	2.58	190
Plot <sup>a</sup>	9.66	2.60	133	9.09	2.37	122
Humor <sup>b</sup>	9.73	2.41	93	9.13	2.52	142

*Note.* The difference in dependent means between the sex conditions is large enough to be significant, but is the opposite direction as in previous research.

<sup>a</sup>Significant difference,  $p < .05$ . <sup>b</sup>Nearly significant difference,  $p < .06$ .

ratings of violent content were significantly higher in the violence conditions than in the no-violence conditions,  $t(233) = -10.01$ ,  $p < .001$ ,  $d = 1.31$ . Manipulations of plot and humor conditions were successful as well. Mean ratings for the presence of a plot were significantly higher in the plot conditions than in the no-plot conditions,  $t(233) = -9.80$ ,  $p < .001$ ,  $d = 1.28$ . When asked to rate the extent to which humor was present, ratings were significantly higher in the humor conditions than in the no-humor conditions,  $t(233) = -11.25$ ,  $p < .001$ ,  $d = 1.47$ .

### *Dependent Measures*

To examine the influence of sexual content, violent content, plot, and humor on subsequent advertisement recognition, we conducted a series of planned comparisons designed to test specific hypotheses. These contrasts were run through an ANCOVA with participant gender entered as the covariate. The results of these contrasts are presented in Table 4.

The first contrast compared conditions with sexual content against those without sexual content. There was a significant effect, but in the opposite direction reported by Bushman (1998; Bushman & Bonacci, 2002): Product recognition was greater for advertisements placed within programs with sexual content,  $F(1, 224) = 4.80, p = .030$ , partial  $\eta^2 = .021$ . A comparison of programs with and without violent content reveals no significant difference,  $F(1, 224) = 0.03, p = .864$ , partial  $\eta^2 = .000$ .

To determine if the presence of plot or humor in the program influenced subsequent advertisement recognition, we conducted planned comparisons specific to those conditions. There was a significant effect of plot: Mean advertisement recognition of programs with a plot was greater than those without a plot,  $F(1, 224) = 4.99, p = .026$ , partial  $\eta^2 = .022$ . Recognition was marginally significantly higher in humor conditions than in conditions without humor,  $F(1, 224) = 3.58, p = .060$ , partial  $\eta^2 = .016$ .

### *Discussion*

The purpose of Study 3 was to use a wider variety of television programs to replicate earlier research in the area more directly and to test the effects of sexual and violent media content on advertising retention. In addition, the design allowed us to test whether other differences in the types of programs (e.g., genre) previously used might account for differences in advertisement retention. The results show that violent content had no effect on advertisement retention, and sexual content had the opposite effect predicted by previous research (Bushman, 2005; Bushman & Bonacci, 2002). Both the presence of plot and the presence of humor led to greater advertisement retention. These findings suggest that in previous research, where sexual and violent content were confounded with variables such as plot or humor, these concomitant variables may be responsible for the results.

### General Discussion

As is typical of much of the research in the general area of media influence, the results of these studies contradict those of previous studies. Previous research (Bushman, 1998, 2005; Bushman & Bonacci, 2002) has concluded that sexual or violent content in television programs impairs viewers' memories of advertisements within that programming. Our studies, which we believe are methodologically superior, show that sexual and violent program content in and of itself does not impair viewers' memory of advertisements when other aspects of program content are held constant. In fact, the results



of Study 3 suggest that sexual content may be associated with improved advertisement recognition in some programming.

Obviously, drawing strong conclusions from null effects is inherently difficult. Several aspects of the current research make us confident that these results represent a true lack of effect, rather than Type II error. With the exception of the programs used, every effort was made to replicate the methods and measures used in the previous research (Bushman, 2005; Bushman & Bonacci, 2002) as closely as possible.

The results of the manipulation checks show that we were very successful in manipulating the key variables of sexual and violent content, with exceptionally large robust effect sizes (most Cohen's *ds* over 1.00). Sample sizes were more than adequate to capture even small effects, with no cell having fewer than 20 participants. We used three separate, but concurrent dependent measures of memory in two of the three studies; yet neither MANOVA nor exploratory univariate ANOVAs revealed any significant main effects. Finally, we had multiple studies, all of which failed to demonstrate any inhibitory effect for violent or sexual content on advertisement memory. In fact, in no cases were the means in the predicted direction.

Beyond merely demonstrating the absence of the effect reported by Bushman (1998, 2005; Bushman & Bonacci, 2002), however, the current studies demonstrate that differences between programs other than presence of sex and violence can influence viewers' recognition of advertisements. Plot-based programs were associated with significantly better advertisement recognition, and comedies were associated with nearly significant improvements in advertisement recognition.

We did not attempt to manipulate all of the possible combinations of genre, mood, sex, and violence. In this study, we were merely trying to demonstrate that some of the differences in the types of programs used in previous studies could ultimately have been responsible for the differences found. We are not suggesting that the presence of plot or of humor is the ultimate explanation for all differences in advertisement recognition, nor are we suggesting a mechanism by which those results occurred. We are concluding, however, that confounding differences between programs with or without sexual or violent content are likely the cause of the effects that Bushman (2005; Bushman & Bonacci, 2002) reported, not the sexual or violent content itself. As in any research of this type, we were limited in the number of programs we could use. It seems likely that the biggest determinant in the memory for advertisements is the individual program.

These findings, particularly those of Study 3, suggest several avenues for future research. It would be useful to try to identify the mechanism involved in the memory difference by incorporating mood measures, mood manipulations, or measures of cognitive load or cognitive interference in future

research. Finally, an ambitious researcher may attempt a full factorial design manipulating sex, violence, plot, humor, and drama, although it is difficult to imagine finding comparable programs that would fit neatly into each of these cells.

It is important to note that we did not undertake this research to defend sex or violence in the television media, nor are we calling for more sex and violence on television. Instead, we have demonstrated that the relationship between program content and advertisement memory is not so clear-cut as has been claimed. The broad claim that sex or violence necessarily impairs viewers' memories for advertisements is simply not supported by the data.

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