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Bethany Albertson and Adria Lawrence
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After the Credits Roll

The Long-Term Effects of Educational Television on Public Knowledge and Attitudes

Bethany Albertson

University of Washington, Seattle

Adria Lawrence

Yale University, New Haven, Connecticut

Television has replaced the newspaper as the major source of news for most people, and thus has the potential to inform the public and influence attitudes. A growing literature has demonstrated the immediate effects of television viewing, but the ability of a particular program to have lasting effects is less clear. In this article, we report on two field experiments that provide a test of the durability of media effects by examining whether two television broadcasts had a lasting influence on viewers' knowledge and attitudes. Both television programs were designed to raise awareness about particular policy issues and to persuade. We find that one of the television programs affected viewer attitudes, even weeks after it aired, while the second television program did not. We consider why particular types of television programs and particular formats are better able to have a lasting impact on the public. Our findings suggest that message repetition is a crucial mechanism for influencing attitudes over the long-term.

Keywords: *public opinion; media effects; field experiments; drug policy; affirmative action*

In the last half century, television has become a primary source of news and information for the American public. As such, it has the potential to inform viewers and influence their attitudes and behavior. Political scientists,

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politicians, and pundits alike have all found fault with contemporary television programming for its lack of serious, public affairs content. But when stations do devote time to public affairs programming, how are viewers affected? Does civic-minded television have a lasting impact on those who watch? In this article, we report on two field experiments that provide a test of the durability of media effects. Specifically, we investigate whether two political information broadcasts influenced viewers' knowledge and attitudes, even weeks after the broadcast aired.

The first program was a Public Broadcasting Systems (PBS) special on drug addiction, hosted by Bill Moyers, which aired over 3 days. The second was a half-hour Fox News debate on affirmative action prior to the 1996 election in which Californians voted on Proposition 209, better known as the California Civil Rights Initiative. Though these programs varied in length, format, and topic salience, in both programs, speakers sought to inform and persuade viewers about public affairs issues. Field experiments were designed to study the effects of the two programs. For both studies, an area probability sample was selected for participation in a survey.¹ After an initial interview, participants were randomly assigned to a "treatment condition" in which they were asked to watch the television broadcast. They were told that they would be re-interviewed after the program aired. Participants in the remainder of the sample, the "control condition," were not told about the program, but told only that they would be re-interviewed later. Participants were not re-interviewed immediately, but in the days and weeks following the broadcasts. Postbroadcast interviews began 10 days after the PBS addiction program aired and continued for several weeks. For the Fox News Special, postbroadcast interviews began the day after the broadcast and continued for several weeks.

This research design offers a particularly tough test of media effects, since it measures knowledge and attitudes long after exposure. Laboratory studies often demonstrate the immediate effects of exposure to media. A television program could shift perceptions of issue importance or change attitudes temporarily, but over time, as people encounter numerous other stimuli, these effects might diminish. While understanding the short-term effects of television viewing is important, we ought also to investigate how and whether television affects viewers over longer periods of time. Our research design permits us to investigate the long-term effects of programs designed to inform viewers. We examine how watching programs affected viewers days and weeks after the program had aired.

The field experiment design allows us to benefit from some of the advantages associated with both observational studies and laboratory experiments. By using a random sample, we avoid the external validity problems associated

with samples of convenience that are generally used in laboratory experiments. In addition, by assigning respondents at random to viewing and nonviewing conditions, these studies benefit from experimental control. Yet unlike laboratory experiments, this design allows respondents to view programs in their own homes, thus more closely approximating regular viewing conditions.

Our findings suggest that particular kinds of programming have lasting effects on political attitudes. We find that the PBS program affected viewers' attitudes toward government policies on addiction. Specifically, watching the program made respondents more likely to favor increased government spending on programs to prevent and treat addiction and less likely to support prison sentences for drug addicts. The Fox special on affirmative action, however, had no effect on viewers' attitudes toward Proposition 209, although there is some evidence that the program made viewers feel more knowledgeable about the proposition. This finding is puzzling; we might have expected the effects of the PBS program to appear weaker than the effects of the Fox News debate, since participants in the Fox News study were interviewed sooner after the broadcast than participants in the PBS study were. What explains the discrepancy? We suggest that the format of the PBS program, which aired over multiple nights, advocated a clear position, and repeated its message, explains the effectiveness of the program in changing public opinion. When public affairs programming sends a clear, consistent message, it has the potential to persuade viewers over the long term.

We proceed as follows. We begin by drawing hypotheses from the main substantive findings in the literature on how the media affects viewers' attitudes and knowledge. We also consider how the passage of time changes the effects of television. Second, we formally demonstrate our approach for estimating the effects of the field experiments. Third, we report on each field experiment. We conclude with implications for the study of media effects.

Television's Informative and Persuasive Powers

Although television is often held responsible for a less informed American public, empirical work has shown that television journalism can educate viewers. Neuman, Just, and Crigler (1992) argue that the relationship between relying on television for news and lower levels of political information is spurious; people who rely on television have lower cognitive skills than those who rely on newspapers. They demonstrate that people do learn from television journalism, particularly for low-salience issues. While studies of the effects of public television in particular are scarce, Holtz-Bacha and Norris

(2001) found that viewers of public television were consistently more informed than viewers of commercial television in the European Union.² Strikingly, those who watched entertainment programs on public television had more political information than those who watched news on commercial television in many countries, although this finding suggests self-selection effects. A laboratory experiment on documentary viewing in the U.S. found that subjects who viewed the documentaries performed better on tests of relevant information. Subjects also reported that they felt more informed. These increases in both objective and subjective measures of knowledge persisted four weeks later (Fitzsimmons & Osburn, 1968). In our studies, we hypothesize that exposure to information via television leads to more informed citizens, based on both subjective and objective measures.

Media exposure may also affect political attitudes (e.g., see Page, Shapiro, & Dempsey 1987; Zaller, 1996). A television program with one main message may influence viewers to adopt that attitude. Our first program, *Moyers on Addiction*, had a clear agenda: It advocated the need for more public treatment programs and called for treatment instead of incarceration for addicts. Given the one-sided nature of the message, we expect to find attitude change toward the attitudes advocated by the program in the aggregate. Our second program featured speakers on both sides of the affirmative-action debate. For this study, we do not expect to see attitude change in the aggregate, since both sides were represented.³ However, we expect that those predisposed toward one side will be even more favorable toward that side after exposure to the program. Attitude polarization is expected when elites are fairly evenly divided on an issue, because people are more receptive to messages from their trusted elites (Zaller, 1992). For example, Krosnick, Holbrook, and Visser (2000) find attitude polarization based on partisanship in response to heightened media coverage on global warming. However, Gerber and Green (1999) argue that there is limited evidence for attitude polarization, and suggest that the effects of perceptual bias outside of the laboratory might be limited in duration and might only exist for a small subset of the population.

Both programs examined in this study were designed to inform viewers, and we expect that new information might lead to attitude change. However, when evaluating long-term effects of media exposure, attitude change and learning may not go hand in hand. Viewers might engage in on-line processing, in which they use new information to update their attitudes as they are watching the program. A week or two later, they may not remember that information, even though it changed their attitudes. This is in contrast to a memory-based model of processing, where people do not evaluate new information at the time of exposure. Information in the programs might

still affect their attitudes, but only if it is stored in memory and then retrieved at a later time (Lodge, Steenbergen, & Brau, 1995). If viewers are using on-line processing, then we may see attitude change without corresponding evidence of learning.

On-line processing is likely when people are motivated to evaluate, and this motivation seems likely in the case of informational programming. However, people also need to have sufficient ability. Ability can be affected by individual level factors, such as political sophistication (McGraw et al., 1990) but also by the manner in which new information is presented. Rahn, Aldrich, and Borgida (1994) find that on-line processing is less likely in a debate, where the back-and-forth makes evaluation more difficult. A one-sided, and in their case, person-centered appeal, is less taxing. This work suggests that the PBS special may have triggered attitude change without corresponding evidence of learning because people evaluated the information as they viewed. On the other hand, respondents in the affirmative-action debate study may have had less opportunity for on-line processing because of the back-and-forth nature of the debate.

We hypothesize that the media may affect attitudes by increasing issue salience. A major response to the *minimal effects paradigm* has been that while the media might not be able to tell the public what to think, it does tell the public what to think about. Cook et al. (1983) found that watching a news program about home health care increased viewers' sense of the importance of home health care, government aid, and fraud. Many other studies demonstrate that the media affects what people think is important (see Iyengar & Kinder 1987; Iyengar & Simon 1993; McCombs & Shaw 1972). Following these findings, we test whether exposure to our programs increased the salience of issues that they addressed.

Finally, we tested whether exposure to our second program influenced behavior. Our second program aired the night before an election in which viewers were asked to vote on the issue of affirmative action. We see if exposure to the program made people more likely to vote. The program on Proposition 209 was designed to inform viewers, and the link between political information and voting is well established (Delli Carpini & Keeter 1996; Verba, Schlozman, & Brady, 1995). Furthermore, Hofstetter and Buss (1980) demonstrated that "last minute political television" can boost vote turnout. Media coverage is also important for increasing awareness of propositions (Nicholson, 2003), though Proposition 209 was a particularly well-publicized proposition. We hypothesize that viewing the debate will increase turnout, either through informing viewers about the issue, or by reminding them that the proposition is on the ballot.

The existing literature supports the view that television can be expected to inform viewers, make issues more salient, change viewers' attitudes, and possibly even affect their behavior. Yet do these effects persist over time? Experimental work often measures variables of interest directly after media exposure. A meta-analysis of media effects research demonstrates that experimental work shows significantly greater media effects than survey research (Emmers-Sommer & Allen, 1999), probably due at least in part to how quickly postexposure effects are measured in laboratory settings. Some researchers have considered the longevity of media effects. Mutz and Reeves (2005) find that the effects of brief exposure to televised political disagreement on political trust disappeared 1 month later. Druckman and Nelson (2003) followed up their newspaper-based framing experiment 10 days later and found that initial framing effects had disappeared. These short-term effects are still important, however, in part because we assume that people are continually exposed to political disagreement and elite framing in the media. Yet it is also valuable to investigate the long-term impact of television viewing. Furthermore, short-term effects are less interesting in the current studies, because these political specials were isolated media events. In order for these programs to matter politically, we should see effects even after people have encountered numerous other sources of information.

Long-term effects are rare in the experimental literature, but not unprecedented. In a follow-up survey given 1 week after their agenda-setting experiments, Iyengar and Kinder (1987) find that the effects persist. Our studies offer an even tougher test: Because attitudes and knowledge were not measured directly after media exposure, there are no previous answers that subjects might feel pressured to match. In these studies, we test whether the programs affected viewers' knowledge, attitudes, and behavior after allowing time to elapse between watching the program and the follow-up survey. We are thus able to test the long-term effects of watching a particular program, both over time and on multiple dependent variables. If the passage of time erodes the effects of the programs, we would expect weak or nonexistent results.

Methodology

Field experiments offer a number of advantages, such as more realistic settings and random selection, but they also present some complications that must be addressed to obtain accurate estimates of the effects of treatment. In the laboratory, the investigator has complete control over administration of the treatment, while in the field it is often the case that the treatment is

administrated imperfectly. In our studies, not all respondents behaved according to the experimental design: Some respondents in the treatment group reported that they did not watch the program, and some respondents in the control group reported that they did. Self-selection was thus an issue, as some chose to watch the show of their own accord, while others who were asked to watch did not comply. This imperfect administration of the treatment does not, however, pose an insurmountable problem for estimating the effects of watching the program. There are well-established methods available to deal with this problem, which have been widely used to analyze field-experiment data.⁴ Even under conditions of self-selection, consistent estimates can be obtained using instrumental variable analysis.⁵

Since some of the respondents self-selected, we cannot simply compare watchers and nonwatchers using ordinary least squares regression. For instance, in the PBS study of addiction, if those who chose to watch the show were more knowledgeable about addiction to begin with, and held attitudes that were systematically different from nonwatchers, comparing the responses of watchers and nonwatchers would produce biased estimates of the effects of watching the show. Although we can attempt to control for these differences by introducing covariates into the regressions, it is not clear whether these covariates eliminate the problem (see Gerber & Green, 2000, p. 654). Moreover, it may be difficult to identify and measure all relevant differences between watchers and nonwatchers. If watchers are systematically different from nonwatchers in ways that are not included in the regression, the results will remain biased.⁶

To estimate correctly how the programs affected viewers, regardless of their prior knowledge and attitudes, we need to isolate the effects of watching the programs. Fortunately, experiments are well-suited for this task because they provide a valid instrument for the treatment (in this case, watching the show). As Sekhon (2008) points out, the fact that there was a manipulation can help to correct problems of noncompliance. The treatment group (i.e., the group that was prompted to watch) is a random subset of the entire sample, and therefore the proportion of those who would be willing to watch if asked should be the same in both the treatment and the control groups. A valid instrument for watching the show, therefore, is assignment to watch the show. Assignment to treatment and control groups meets the criteria for a valid instrument: It is correlated with our independent variable of interest, watching the show, but uncorrelated with the regression error term.⁷ By using assignment to treatment and control groups as an instrument for watching, we are able to obtain consistent estimates of the effects of watching these broadcasts.

The use of random assignment-to-treatment and control groups as an instrument is fairly common in analyses of field experiments. Following the approach of Angrist, Imbens, and Rubin (1996), we illustrate how the logic applies to our studies. In our postbroadcast survey, we asked respondents a number of questions that were answered in the broadcast. To find out whether watching the show increased the probability that a respondent answered the question correctly, we reason as follows. We reason that the population can be divided into three groups. The first are those who will watch the broadcast without any prompting: They are *self-motivated watchers*. This group undergoes the treatment t (watching the show), regardless of whether they are in the treatment or control group. Let α_1 be the proportion of the population that are self-motivated watchers. Let p_1 be the probability that a member of this group answers the test question correctly without watching the program. The second group consists of those who would watch the program if they are asked. Without any prompting, they would not watch, but as a result of the treatment, they watch. Let α_2 be the proportion of the population that would watch the program if prompted. Let p_2 be the probability that such a person would answer the test question correctly without watching. The third group is made up of nonwatchers: These people will not watch even if asked. Let p_3 be the probability that a nonwatcher answers the test question correctly. The probability that a randomly selected member of the control group will answer the question correctly equals

$$P_c = \alpha_1(p_1 + t) + \alpha_2 p_2 + (1 - \alpha_1 - \alpha_2)p_3, \quad (1)$$

This probability equals the probability that an individual member of the control group is a self-motivated watcher (α_1) times the probability that the person will answer correctly (p_1) after being exposed to the treatment t , plus the probability that the person would watch if asked (α_2) times the probability that this kind of person would answer correctly (p_2), plus the probability that the person is a nonwatcher ($1 - \alpha_1 - \alpha_2$) times the probability that a nonwatcher answers correctly (p_3).

The probability that a randomly selected member of the treatment group will answer the question correctly equals

$$P_t = \alpha_1(p_1 + t) + \alpha_2(p_2 + t) + (1 - \alpha_1 - \alpha_2)p_3, \quad (2)$$

where the difference between equations 1 and 2 is due to the effect of the experimental treatment on the group that watches only when asked. Thus, in both the treatment and control groups, there are individuals who undergo the treatment, but in the control group, only the self-motivated watchers

undergo the treatment, whereas in the treatment group, both the self-motivated watchers and those who watch when prompted undergo the treatment. This difference between P_t and P_c equals $\alpha_2 t$. Solving for t , we derive an equation for the treatment effect:

$$t = \frac{P_t - P_c}{\alpha_2} \tag{3}$$

Although the population probabilities are not observed, the law of large numbers can be used to estimate t from the sample data:

$$p \lim A_t = P_t \quad p \lim A_c = P_c, \tag{4}$$

where A_t is the percentage of the treatment group that answers correctly and A_c is the percentage of the control group that answers correctly. A similar calculation can be made to find the probability that an individual would watch if asked (α_2):

$$\alpha_2 = \frac{N_{tw}}{N_t} - \frac{N_{cw}}{N_c}, \tag{5}$$

where the first proportion is the number of people in the treatment group who watched (N_{tw}) divided by the number of people in the treatment group overall (N_t), and the second proportion is the number of people in the control group who watched (N_{cw}) divided by the total number of people in the control group (N_c). Subtracting the proportion of watchers in the control group is the equivalent of subtracting the self-motivated watchers from the treatment group in order to isolate the proportion of the population that watches only when asked.⁸

Using equations 3, 4, and 5, we obtain a consistent estimator of t :

$$t = p \lim \frac{A_t - A_c}{\frac{N_{tw}}{N_t} - \frac{N_{cw}}{N_c}} \tag{6}$$

In effect, we find the treatment effect by subtracting the correct response rate of the control group from the correct response rate in the treatment group and dividing this difference by the *contact rate*, the number of those who watched when and only when asked.

In our analyses, we implement instrumental variable analysis using two-stage least squares regression.⁹ We use assignment to treatment and control

as an instrument for watching. This approach provides consistent estimates of the effects of watching these shows and corrects for the self-selection and noncompliance issues that arise from the experimental design.

Study 1: *Moyers on Addiction: Close to Home*

In late March 1998, PBS broadcast a 5-part series on drug abuse treatment, relapse, and recovery, funded by the Robert Wood Johnson Foundation and produced by Bill Moyers. The National Opinion Research Center (NORC) at the University of Chicago conducted a survey evaluating the effects of the program in the nation's five largest demographic areas (New York, Los Angeles, Chicago, Houston, and Philadelphia). The goal of the program was to inform viewers about alcohol, drug, and tobacco addiction, and to change viewer attitudes about treatment. The program sought to convey the message that addiction, whether to drugs, alcohol, or cigarettes, is in large part due to physical factors, such as genetic predisposition and biological changes in the brain caused by repeated ingestion of addictive substances. The program attempted to show that addiction is not the result of moral weakness, but can affect anyone, and that relapse is a natural part of the recovery process. The program advocated the need for more public treatment programs and for diverse treatment methods.

The first wave of the study was conducted between March 12 and March 25, and 1,360 interviews were completed over this period. During the first wave, respondents were randomly assigned to treatment and control groups, and those in the treatment groups were asked to watch the program. The program aired on three consecutive evenings: Sunday, March 29, through Tuesday, March 31, with two, 1-hr segments on Sunday, a 1.5-hr segment on Monday, and two 1-hr segments on Tuesday. Approximately 80% (1,089) of the original respondents were re-interviewed between April 10 and April 28.¹⁰ Table 1 provides descriptive statistics for the sample and Table 2 provides treatment and control group characteristics.

Our experimental analysis addressed a number of different questions: How effective was this program in meeting its educational goals? Did viewers learn from the show? Did they come to regard the problem of addiction as more pressing than they previously thought? Did the show change public attitudes toward treatment and funding for treatment?

Effects of the Broadcast on Learning

Respondents who watched the broadcast stated that they found it informative. Of those who watched the first segment of the documentary,

Table 1
Summary Statistics for Both Studies

	Study 1: PBS, <i>Moyers on Addiction: Close to Home</i>	Study 2: <i>Fox News</i> , "Channel 11 Special on Proposition 209"
Format of broadcast	Documentary	Expert debate
Duration of broadcast	5.5 hr over 3 consecutive evenings	0.5 hr
Location of broadcast (sample)	National (5 metropolitan areas)	Orange County, California
Date of broadcast	March 29–March 31, 1998	Nov 4, 1996
Date of Round 2 survey	April 10–April 28	November 18–December 2
<i>N</i> in Round 2	1089	507
Percentage successfully contacted for Round 2	80	63
Number in treatment group	510	259
Number in treatment group who watched	244	117
Number in control group	579	248
Number in control group who watched	74	11

Note: PBS = Public Broadcasting Service.

74% said they learned either a lot or some, and the corresponding figures for segments 2, 3, 4, and 5 were 79%, 70%, 81%, and 81%, respectively. To gain a more objective measure of learning, the post-broadcast interview included a short test with 8 questions about addiction that were answered during the show (see Appendix for the test). These questions were designed to evaluate whether viewers learned the major lessons the program sought to impart. We scored each respondent according to the number of questions answered correctly to obtain an overall level of knowledge about addiction. We hypothesized that watching the program should increase respondents' knowledge about addiction.¹¹

Table 3 (column 2) presents the regression results of knowledge about addiction. Using two-stage least squares regression, we do not find any support for our hypothesis that people learned about addiction from watching the show. This was surprising, given our expectations from previous media studies and viewers' claims that they did learn from the show. These self-reports may be unreliable, as people might report learning for other reasons.

Table 2
Study 1—Treatment and Control Group Characteristics

	Treatment	Control
Mean years of education	>2 years of college	>2 years of college
Modal household income category	≤ \$30,000	≤ \$30,000
Mean age	55	54
Percentage identifying as Republican	32	30
Percentage identifying as conservative	33	31
Percentage White	76	74
Percentage female	54	52
Modal political interest	Somewhat interested	Somewhat interested
Percentage watching TV news several times a week or more	85	79
Percentage watching PBS several times a week or more	64	63
Percentage reading newspaper several times a week or more	73	71
Percentage with close friend or family member who has been addicted to drugs or alcohol	55	58
Average response to prebroadcast question asking if respondents agree that more treatment programs are needed (1 = <i>strongly agree</i> ; 2 = <i>agree</i> ; 3 = <i>disagree</i> ; 4 = <i>strongly disagree</i>)	2.06	2.02
Average response to prebroadcast question about the importance of alcoholism as a social problem (1 = <i>very important</i> ; 2 = <i>important</i> ; 3 = <i>unimportant</i> ; 4 = <i>very unimportant</i>)	1.51	1.57

Note: A chi-square test revealed that none of these differences between the treatment and control group were statistically significant at the $p < .05$ level, except for watching TV news; PBS = Public Broadcasting System.

After watching a long, multipart documentary, viewers may want to believe that they learned. Viewers may also want to report learning because they feel that this answer is more polite. Our finding, at a minimum, calls attention to the differences between subjective and objective measures of learning. The time lapse between the broadcast and the interview may also explain the absence of an effect; people may have forgotten the answers during the interval.¹²

Table 3
Two-Stage Least Squares Estimates of the Effects
of Watching *Moyers on Addiction*

Independent Variables	Dependent Variables		
	Knowledge of Addiction	Attitudes	Salience
Watching (experimental treatment)	.160 (.164)	.892* (.334)	.191 (.214)
Prebroadcast responses ^a	.450* (.028)	.964* (.042)	.460* (.024)
Rarely watches TV news	.045 (.033)	.122 (.064)	-.077 (.040)
Rarely watches PBS	.037 (.029)	-.006 (.056)	.029 (.035)
Rarely reads the news	.017 (.025)	.046 (.050)	.049 (.031)
Interest in politics and national affairs (low to high)	.149* (.038)	.001 (.074)	-.024 (.046)
Party (Republican to Democrat)	—	.128 (.051)	.060 (.032)
Income	.038* (.013)	.027 (.024)	.005 (.016)
Sex (Male = 0, Female = 1)	.212* (.063)	.238* (.119)	.157* (.076)
Education	.025* (.010)	.048* (.020)	.003 (.012)
Age	-.006* (.002)	.008* (.004)	.003 (.002)
Black	-.366* (.107)	.083 (.209)	.311* (.133)
Hispanic/Latino Chicano	-.584* (.144)	-.243 (.265)	.114 (.177)
Other non-White	-.472* (.097)	-.207 (.189)	-.038 (.119)
Close friend/family an addict	.029 (.063)	.281* (.121)	.048 (.076)
Los Angelus dummy	-.074 (.102)	-.493* (.200)	.061 (.127)
Chicago dummy	-.029 (.097)	-.261 (.185)	.035 (.118)
Houston dummy	-.021 (.101)	-.374* (.193)	-.044 (.122)
Philadelphia dummy	-.082 (.099)	-.421* (.192)	.082 (.122)
Constant	4.09	1.85	5.20
<i>F</i>	23.79	27.65	20.81
Number of observations	968	905	1,070

a. Controls for responses to knowledge, attitudes, and salience questions given during the first wave, prior to the broadcast. The dependent variables are coded as follows: "Knowledge of Addiction" is coded from 0 to 8, 0 = no correct responses on the test, 8 = all correct responses; "attitudes" are also coded from 0 to 8 (see Appendix); "salience" is coded from 1 to 12, a score of 1 means the respondent found tobacco, drug, and alcohol addiction *very unimportant*, a score of 12 means the respondent found all three to be *very important* social problems; see Appendix for further clarification on all dependent variables.

*Significant at $p < .05$. The first-stage equations include dummy variables for missing values of control variables.

Effects of the Broadcast on Public Attitudes

The PBS broadcast sought not only to provide information to viewers, but also to influence their attitudes toward public policies concerning

addiction treatment and funding for addiction. Throughout the broadcast, and particularly during the fifth segment, “The Politics of Addiction,” Moyers interviewed experts who argued in favor of building more treatment centers for addicts. The broadcast argued that funding should be shifted from incarcerating drug offenders to better treatment options for addicts and emphasized the need for more treatment centers, both for those in prison and for noncriminals.

How well did the program succeed in conveying this message? We hypothesized that watching the show would change viewers’ attitudes; specifically, it would make viewers more likely to support the creation of treatment centers and to approve of increases in government funding for treatment. We created a composite variable, made up of questions about increases in funding and the necessity for drug treatment programs, intended to measure respondents’ support for these policy measures.¹³

Table 3 (column 3) presents the results of our analysis. We find that watching had a significant effect on support for increasing the availability of treatment centers. Watching the show made viewers more likely to support public spending on treatment centers, treatment instead of incarceration, and the addition of treatment options for addicts. Thus despite the time lapse between the show and the interview, viewers’ attitudes were markedly different from nonviewers’ attitudes.

This finding is particularly interesting given that we did not find any objective evidence of learning from the broadcast. The absence of learning is consistent with on-line processing, in which people evaluate new information as it is encountered. This interpretation also might account for the discrepancy between the subjective and objective measures of learning. People felt that they acquired new information, and they did in a sense. They appear to have put that information to use in developing their attitudes. However, learning was not a durable effect. After time lapsed, people no longer remembered specific information about addiction, but they had become more sympathetic toward policies for treatment of addiction.

Effects of the Broadcast on Issue Salience

One of the main findings in the literature on media effects is that the media sets the public agenda, telling people which issues are important. Respondents were asked to evaluate the importance of tobacco, alcohol, and drug addiction. According to the agenda-setting argument, we should find a large effect on salience, particularly since the experimental design encouraged people to watch who would not otherwise have watched. Table 3

(column 4) presents our results. We find no association between watching the show and the salience of the topics covered. Viewers were no more likely than nonviewers to respond that addiction to drug, alcohol, and tobacco is an important social problem.

Discussion

Critics of experimental methods and experiment practitioners have pointed to the possibility that participating in an experiment can affect how subjects behave. Members of the treatment group were contacted by phone, asked to watch the program, and told that they would be surveyed after they watched it. This intervention could have affected the manner in which they viewed the program or their responses in the postprogram survey. In this section, we consider potential objections to our research design, and explain why we feel confident in the results presented above.

Two pieces of evidence suggest that in this case, the findings are a result of watching the program, and not of being asked to watch. First, if asking respondents to watch the program led them to pay more attention to it, we would expect that the effects of the program would be amplified among the treatment group. The fact that we found no effects for two of our dependent variables operates against this expectation. We found that the treatment group was no better able to provide correct answers to our knowledge test than the control group. The treatment group also did not think that the topic of the show was more important than the control group. The treatment group differed from the control group only on attitudes toward the provision of more options for addicts. It is unlikely that this one difference resulted from our intervention. Second, we might have expected that members of the treatment group, who were asked to watch the show, would feel a responsibility to answer more questions than members of the control group would. We compiled nonresponses on respondents' attitudes toward addicts and addiction and toward policies for addressing addiction, including attitudes toward public spending for treatment programs. We found that participation in the treatment group did not affect the nonresponse rate. This finding is encouraging, since it bolsters our belief that participation in the treatment group itself did not change the way in which people responded to questions.

Another concern with our approach is that respondents in the treatment group might have falsely reported watching the show. After being asked to watch, respondents might have felt pressure to report that they did. If respondents in the treatment group falsely reported watching the show, we would expect weakened effects. There are several reasons why we believe

that experimental demand is not a serious impediment for this study. First, if a substantial portion of the prompted watchers were lying due to social desirability pressures, we would expect either no significant findings on our dependent variables, or a significant finding only on issue salience, since the experimental demand that can cause falsely reported watching might also cause reports of heightened issue importance. Yet we do not see any effects on issue salience. Instead, there is a significant finding on attitude change. The causal link between experimental demand and attitude change is not clear, and it is difficult to explain why we would see a significant effect on attitudes if respondents in the treatment group were not truly watching.

Second, respondents were asked several questions about the quality of the programming. If prompted watchers did not actually watch the program, they may have been less likely to answer these questions than self-motivated watchers. We compared prompted watchers and self-motivated watchers to see if the prompted watchers were less likely to answer questions about the programs. There were very few nonresponses among either group, and the groups did not differ significantly, suggesting that the watchers actually watched. While these arguments do not fully remove the doubt cast by social desirability concerns, we believe that the external validity that is gained by moving outside of a laboratory setting outweighs the possible threats to our causal story introduced by experimental demand. The only sure way to know that members of the treatment group actually watch is to supervise their watching, as laboratory experiments are able to do, but this sacrifices the advantages of studying television viewing in the field.

Study 2: Fox News Special on Proposition 209 (Affirmative Action)

Different television shows can be expected to affect viewers in different ways. The Fox News Special on Proposition 209 differed from the PBS program, *Moyers on Addiction*, in a number of ways. Unlike the PBS program, the Fox News Special was short, consisting of the last half hour of an hour-long news program. While the PBS program sought to present factual information about addiction from a range of sources, the Fox News Special showed viewers a heated debate between four experts, two opponents and two proponents of Proposition 209, a proposition that would eliminate affirmative action in California.¹⁴ During the program, these experts took questions from undecided voters throughout Orange County. Rather than providing facts and information about Proposition 209, the four experts

primarily spent their time countering their opponents' statements. Opponents of Proposition 209 argued that it would take away opportunities for women and minorities. Proponents countered that affirmative action programs are discriminatory and unfair. The Fox News Special was specifically directed at potential voters; it aired the night before the 1996 presidential election, in which California voters would decide on the proposition. The goal of the program was to raise awareness about Proposition 209 and assist viewers in deciding how to vote on the issue.

The National Opinion Research Center designed a field experiment to evaluate the effects of the program. The study relied on a random-digit-dial, 2-wave panel survey of households in Orange County, California. The first wave of telephone interviews was conducted in late October 1996 with 805 randomly selected respondents (the response rate was 58%). Respondents were assigned to treatment and control groups during the first wave.¹⁵ Respondents in the treatment group were prompted to watch the program in the following manner: The interviewer told the respondent about the program, gave the broadcast time, and asked the respondent to watch the program as a part of the study. Each respondent was then told that he or she would be contacted for a re-interview after the election. Next, a letter was sent to each respondent repeating the information given by the interviewer. A refrigerator magnet with the time of the program was included with the letter, along with a token incentive (a \$2 bill). The control group did not receive the letter or the incentive, and was told only that they would be contacted for a follow-up interview after the election. The second wave began immediately after the election with 507 (63%) of the respondents from Wave 1. Table 1 provides summary statistics for the study.

Our experimental analysis of this broadcast addressed some questions similar to those in Study 1. Did the broadcast change viewers' attitudes toward Proposition 209? Did it make the issue more salient? Did viewers feel more informed about Proposition 209?¹⁶ The experiment also allowed us to estimate the effects of watching on voting behavior. We hypothesized that viewers would be more likely to vote than nonviewers.

Table 4 summarizes our results for these hypotheses. We found some support for the hypothesis that watching the program made viewers feel more knowledgeable about Proposition 209. The effect is significant at the .05 level with a one-tailed test. This study only included subjective measures of knowledge; no knowledge test was given, but like the PBS study, those who watched reported that they learned from the broadcast. We also tested whether watching the show influenced viewers' voting behavior and issue salience. Watching the program had no effects in any of these models.

In addition, watching the show did not affect viewers' support for Proposition 209. We also hypothesized that exposure to a program such as this with competing messages might result in polarization. We tested whether watching the program caused more extreme attitudes amongst Democrats and Republicans, but found no effect on partisan support for the proposition (results not shown).¹⁷ It is possible that by the night before the election, Californians had already made up their minds about the highly controversial proposition.¹⁸

Discussion and Conclusion

Our findings suggest that documentary viewing can affect viewers' attitudes, even long after the show has aired. The program *Moyers on Addiction* had a clear agenda it sought to convey to viewers. Although the program generally attempted to provide information from different points of view, it sought to change public attitudes about funding and treatment, and arguments were presented throughout the broadcast. We find that the program successfully conveyed these messages; viewing was associated with supporting more treatment centers and more funding for treatment. Viewers' attitudes were markedly different even though they were interviewed 10 to 28 days after the broadcast. We found that the Fox News special did not change attitudes, but this is less surprising because the program did not advocate one single message but presented both sides of an issue. Another reason for the ability of the first program to shape attitudes may be its length. The program in Study 1 lasted for 5.5 hours and was shown on multiple nights. Longer programs may be better able to alter public opinion over the long-term. Repeated exposure to the message may also have given it greater staying power.

Our studies provide support for the hypothesis that watching a program makes respondents feel more informed. In Study 2, we found that watching was associated with feeling more informed about affirmative action. In both studies, viewers reported that the shows were informative. However, in Study 1 we found that viewers were no more informed when it came to an objective knowledge test. This finding corresponds to a growing literature on the discrepancy between subjective and objective attitude measures (Holbrook & Krosnick, 2005; Visser, Bizer, & Krosnick, 2006). It also suggests that attitude change in response to *Moyers on Addiction* occurred via on-line processing. Viewers used what they learned from the special to inform their attitudes, but then forgot the information itself.

Table 4
Two-Stage Least Squares Estimates of the Effects of Watching Fox Special on Proposition 209

Independent Variable	Dependent Variables				
	Voting in the Election	Voting on Proposition 209	Support for Proposition 209	Information on Proposition 209	Salience
Watching (experimental treatment)	-.025 (.072)	-.022 (.068)	-.081 (.093)	.300 (.158)	.051 (.151)
Party identification (strong Republican to strong Democrat)	-.009 (.007)	-.008 (.007)	-.084* (.010)	-.019 (.016)	-.015 (.015)
Interest in politics (high to low)	-.101* (.022)	.022 (.024)	-.039 (.030)	.247* (.048)	.113* (.046)
Watch national news	.004 (.011)	-.004 (.011)	.010 (.014)	.001 (.024)	.001 (.023)
Read newspapers	.024* (.009)	.018 (.009)	-.008 (.012)	.111* (.019)	-.017 (.018)
Education	.012* (.006)	-.001 (.006)	-.013 (.008)	.003 (.013)	.000 (.012)
Income	.006 (.006)	.002 (.006)	.008 (.008)	-.009 (.013)	-.019 (.012)
Gender (0 = male; 1 = female)	.065* (.031)	-.008 (.032)	-.024 (.042)	-.074 (.069)	.075 (.066)
White	.118* (.040)	.051 (.043)	.192* (.053)	.032 (.086)	-.146 (.082)
Constant	0.115	0.726	0.999	1.826	3.370
F	7.10	1.11	7.52	8.01	1.30
Number of observations	499	409	441	498	492

Note: The dependent variables are coded as follows: For “voting in the election,” 1 = voted, 0 = did not vote; for “voting on Proposition 209,” 1 = voted, 0 = did not vote; for “support for Proposition 209,” 1 = voted against or opposed, 0 = voted for or favored; “information on Proposition 209,” is coded from 1 to 4, with 4 meaning respondents had a great deal of information about Proposition 209 prior to the election, and 1 meaning respondents reported no information about the proposition before the election; “salience” is coded from 1 to 4, with 1 indicating that the respondent thought Proposition 209 was *very unimportant*, and 4 indicating a response of *very important*.

*Significant at $p < .05$. The first-stage equations include dummy variables for missing values of control variables.

Although nonfindings are sometimes considered uninteresting and not worth reporting, the failure to support our hypothesis about salience is in fact substantively important because it contributes to our understanding of the media's agenda-setting ability. In both studies, watching the program was not associated with changes in viewers' evaluations of the salience of the issue presented. We expected these studies to show changes in viewers' perceptions of the importance of the issues covered in the broadcasts, particularly since our treatment condition succeeded in motivating people to watch who otherwise would not have watched. These people should be more open to changes in issue importance than those who would have watched anyway, since many of the self-motivated watchers probably already thought the issues were important. Instead, the studies suggest that the media is not always successful at raising issue importance. The boost in issue salience offered by television programming might be too short-lived to be detected in a survey given a week or two after exposure. Salience may be easier to change in the short-term than the long-term. In Zaller's (1992, p. 78) model of attitude change, media exposure might make an idea temporarily more accessible, affecting immediate attitude reports without altering underlying attitudes.

We analyzed these field experiments with two main goals in mind. First, we sought to contribute to ongoing debates about the durability of media effects. The duration of media effects is not well understood (Iyengar, Peters, & Kinder, 1982), and studies that measure attitudes weeks after media exposure are critical for understanding longer term effects. Our studies were designed to see whether two informational broadcasts were able to influence viewers' knowledge and attitudes days and weeks after the broadcasts aired. Our results suggest that educational broadcasts can have persistent effects on attitudes. Although respondents had viewed the PBS broadcast on addiction weeks before they were questioned, their responses suggest that watching the show influenced their attitudes, just as the program hoped. This finding encourages our belief that educational programs can have long-lasting effects on the public, and suggests that media can indeed persuade the public. Our second study suggests, however, that only particular kinds of programs can induce attitude change over the long term. Although respondents who viewed the Fox program were often interviewed within days, rather than weeks, of the broadcast, the show failed to influence attitudes or behavior. The short, debate format of the show, and the high saliency of affirmative action prior to the election, most likely prevented it from affecting viewers' attitudes.

Second, we sought to call attention to the need for ongoing research to investigate the mechanisms by which television affects viewers and point to an approach that can be used to gain further leverage over debates about

media effects. Field experiments provide a productive middle ground between surveys and laboratory experiments; they take place in more realistic settings than a laboratory, but they still benefit from random assignment. Field experiments therefore combine some of the advantages of observational and experimental approaches. Using field experiments and varying the type, content, length, and iteration of television programs can help disentangle what it is about television programming that makes it informative and persuasive.

Appendix

Study 1: Moyers on Addiction

Knowledge acquisition test: Subjects were asked if the following statements were true or false.

1. Because the causes of addiction are the same for everyone, all people who have addiction problems should benefit equally well from the same kind of treatment. (False)
2. Long-term use of drugs, alcohol, or tobacco causes physical changes to the brain that make it hard to stop using the substance. (True)
3. All addiction treatment programs are basically the same and will work equally well for anyone with an addiction problem. (False)
4. Children of alcoholics are not more likely than others to become alcoholics later in life. (False)
5. If a person wants to get over an addiction to drugs, alcohol, or cigarettes, the way is for that person to resolve NEVER to use the substance again. (False)
6. If a person who has been addicted to alcohol or drugs has been free of the addiction for 10 years, that person never has to worry about becoming addicted again. (False)
7. The frequent use of drugs, alcohol, or tobacco creates brain pathways that produce cravings. (True)
8. When people addicted to a substance are exposed to environmental cues associated with the substance they can experience powerful cravings. (True)

Issue salience: What about tobacco addiction (alcohol addiction, drug addiction). Would you say that it is a very important social problem, an important social problem, an unimportant social problem, or a very unimportant social problem?

Attitude index: The attitude index was compiled from the following questions, and coded to match our hypothesis about the direction the attitude should take if the program was effective.

1. Is the government spending too much, too little, or about the right amount on drug and alcohol rehabilitation programs?

(continued)

Appendix (continued)

2. Is the government spending too much, too little, or about the right amount on healthcare for drug, alcohol and tobacco-related health problems?
3. Is the government spending too much, too little, or about the right amount on drug, alcohol, and tobacco use prevention programs in schools?
4. There should be more treatment programs for people who have addiction problems.
5. Drug treatment programs are more effective than jail sentences in reducing drug addiction.
6. Recently, the state of Arizona passed Proposition 200 which sends drug users who commit nonviolent crimes to a drug rehabilitation program rather than to jail. Would you say you agree or disagree with this idea?
7. There should be more drug and alcohol treatment programs in prisons.
8. Would you support or oppose the mayor of your community if the mayor wanted to create more treatment programs for drug and alcohol addiction in your community?
9. Would you support or oppose the governor of your state if the governor wanted to create more treatment programs for drug and alcohol addiction in your state?

Study 2: Fox News Special on Proposition 209

Voting: In any election, some people are not able to vote because they are not registered, are sick, too busy, or simply did not want to vote. Did you vote in the presidential election this year?

Voting on Prop. 209: One of the propositions on the ballot this year was Proposition 209, also called the California Civil Rights Initiative. Proposition 209 would eliminate discrimination or preferential treatment for anyone on the basis of race, sex, color, ethnicity, or national origin in state and local government hiring, education, and contracting. Did you vote on Proposition 209?

Support for Proposition 209: Did you vote for or against Proposition 209? Or, did you favor or oppose this initiative, or have you not thought about it much (asked if respondent did not vote).

Information on Proposition 209: How much information did you have about Proposition 209 before the election? Would you say a great deal, some, a little, or none?

Salience: How important to you is the issue raised in Proposition 209? Is it very important, somewhat important, somewhat unimportant, or very unimportant?

Notes

1. In the first field experiment, a randomly selected half of a probability sample in each of the 5 largest cities in the United States was asked to view the PBS broadcast. In the second study, half of a probability sample of residents of Orange County, California, was asked to watch the Fox News debate.

2. The authors note that not enough Americans watch public television to allow for comparisons to the United States. Little has been written on the effect of public television programming in the United States; this study of the PBS program on addiction addresses this gap in the media literature.

3. Zaller (1996) attributes much of the minimal effects literature to the fact that the media often carries competing messages.

4. See, for example, Gerber and Green (2000) and Horiuchi, Imai, and Taniguchi (2007).

5. Instrumental variable analysis is by now a commonly used and well-accepted method for estimating effects in situations where ordinary least squares (OLS) would be biased (for examples see Angrist & Krueger, 2001). Specifically, instrumental variable analysis is used when OLS would be biased due to an endogenous independent variable, a mismeasured independent variable, or an omitted independent variable (Murray, 2006a). In our studies, the problem is one of endogeneity: Self-selection means that there may be an endogenous relationship between watching the program and prior beliefs about and knowledge of drug addiction and treatment. In this situation, instrumental variable estimation can consistently estimate coefficients. For recent introductory treatments of instrumental variable estimation in general, see Murray (2006b, ch. 13) and Stock and Watson (2003, ch. 10).

6. The use of propensity-matching scores would be similarly problematic because this approach also relies on observable measures of the differences between watchers and non-watchers so that watchers can be matched to similar non-watchers. Propensity matching can remove bias based on observable characteristics in observational studies (Rosenbaum & Rubin, 1983), but it fails to take advantage of the random assignment of field experiments. See Sekhon (2008) for a discussion of both matching and experiments.

7. For more on valid instruments, see Murray (2006b).

8. Angrist and Imbens (1999) correctly show that the model estimates the effect for the compliers (i.e., those who watched the program when asked). Thus, we are estimating the *local average treatment effects* (LATE) rather than the *average treatment effects*. The model does not estimate the effects of watching among the self-motivated watchers or the nonwatchers (i.e., those who do not comply). See Heckman (1997).

9. Data analysis was performed in Stata 10 (StataCorp, 2007), using the *ivreg* command.

10. The attrition rate between pretest and posttest could alter the effectiveness of randomization if the attrition rate is associated with particular values of our dependent variables. We tested the effectiveness of randomization by regressing random assignment on a number of control variables. We found a small effect for watching television news; regular news watchers were somewhat more likely to be in the treatment group than the control group (moving 1 unit up the 5-unit scale from never watching to daily watching made a respondent 3.5% more likely to be in the treatment group, with a standard error of 1.6.) This may be due to random chance or attrition. The likely effect of having more regular news watchers in the treatment group would be a slight upward bias; we would expect that the treatment group would appear to be more knowledgeable than the control group because they watch the news more often. Our results suggest that this was not the case. The appropriate way to manage the attrition problem

is to introduce control variables from the pre-broadcast survey (see Horiuchi et al., 2007). We therefore present our estimates with covariates. See Table 2 for mean and modal characteristics of the treatment and control groups.

11. Respondents were coded as watching the program if they reported watching any segment of the 5 half-hour segments. Each segment presented the main points the broadcast sought to impart, and we therefore expected to see effects from watching parts of the broadcast. We also carried out two robustness checks. We coded only those viewers who reported watching the entire show as watchers and found similar results to those presented here, although our significant finding disappeared due to the lowered number of watchers. Where relevant, we also coded watching according to those who viewed the segment most likely to affect the dependent variable, and again reached similar findings to those presented here.

12. Two-stage least squares estimators are less efficient than OLS, so some may suggest that weakened effects are due to a loss of efficiency. However our sample size is large enough for losses of efficiency to be inconsequential.

13. See appendix for the list of questions. An alpha test revealed a scale reliability coefficient of .78.

14. The speakers in favor of Proposition 209 were Ward Connerly and Erroll Smith. The speakers against the proposition were Katherine Spillar and Joe Hicks.

15. See Table 2 for characteristics of the treatment and control groups.

16. This study included subjective measures of knowledge only; no knowledge test was given.

17. We also tested nonresponse rates, as we did in our first study, to see if assignment to the treatment group led respondents to answer more questions. We found no effect of assignment on the response rate.

18. Neuman et al. (1992) find that television is more successful at shifting attitudes on low-salience issues.

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Bethany Albertson is an assistant professor of political science at the University of Washington, Seattle.

Adria Lawrence is an assistant professor of political science at Yale University and a research fellow at the Macmillan Center for International & Area Studies.