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## Abstract

According to the influence of presumed media influence hypothesis, people estimate the potential effects of media on other people and change their attitudes or behaviors as a consequence. In recent years, many studies offered some support for this idea. However, a central limitation of these studies is that all of them utilized correlational methodology and thus do not offer a valid way to infer causality. The current research examined the causal direction in the influence of presumed media influence using experimental methodology. In Study 1, the authors manipulated the perceived influence of watching pornography and measured the effects of this manipulation on support for censorship. In Study 2, perceptions regarding the influence of a news story about an expected shortage in sugar were manipulated indirectly, by manipulating the perceived exposure to the news story, and behavioral intentions resulting from the story were consequently measured. In both studies, results supported the causal direction postulated by the “presumed influence” hypothesis.

## Keywords

presumed media influence, third-person perception

For decades, media scholars have expended great efforts in an attempt to answer the question of whether and how, if at all, mass media affect their audiences. One interesting and increasingly popular hypothesis—the influence of presumed media influence (Gunther & Storey, 2003; Tsfati & Cohen, 2005)—claims that media effects are indirect. Stemming

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from Davison's (1983) notion of the third-person effect, this hypothesis claims that people estimate the potential effects of any communicative content on other people and change their attitudes or behaviors accordingly. For example, a shopkeeper sees a massive advertising campaign on TV for a certain snack. Expecting his clients to be persuaded to buy this snack, he places it in a prominent position in his store. A parent, expecting his or her kids to be influenced by violent cartoons on TV, gets a TV set with a V-chip. These examples convey indirect but very real effects of media that take place even if people's expectations regarding the impact of communication are exaggerated or erroneous.

In recent years, Davison's (1983) ideas have been extensively revisited in areas ranging from health communication (Gunther, Bolt, Borzekowski, Liebhart, & Dillard, 2006), through parental mediation of violent televised content (Tsftati, Ribak, & Cohen, 2005) to audience reaction to media panics (Tewksbury, Moy, & Weis, 2004). While all of these studies offer some support for the idea that people change their attitudes and behaviors in accordance with their perceptions of media impact on others, a central limitation of these studies is that all of them utilized correlational methodology. This is a major problem because correlations, while providing essential evidence of a relationship between variables, are theoretically handicapped because they do not offer a valid way to infer causal direction. Correlations alone also offer little defense against criticisms regarding potential spuriousness, which, as we shall demonstrate later, make much sense in some studies of the presumed influence hypothesis. In many of these studies, it is unclear whether people change their behaviors or attitudes because they think media may impact others or, on the contrary, change their perceptions of media influence as an explanation or excuse for why they act or think in certain ways.

Thus, to establish the theoretical validity of the "presumed influence" hypothesis as posited, it is critical to establish time order via an experimental test. If people's perceptions of media influence can be experimentally manipulated, consequent changes in attitudes or behaviors, if in fact they exist, would be a much stronger test of the behavioral component of Davison's (1983) third-person effect. The internal validity inherent in experimental methodology offers the best evidence for an "effect," that is, for the claim that some phenomenon is genuinely caused by another. This study reports on two such experimental investigations: In Study 1, we aimed to manipulate participants' perceptions that pornography has either positive, negative, or inconclusive effects and subsequently measured their attitudes toward censorship of pornographic content. In Study 2, perceptions regarding the influence of a news story about a shortage in sugar were manipulated indirectly, by manipulating perceived exposure to the news story, and behavioral intentions resulting from the story were consequently measured.

## The Influence of Presumed Media Influence

In 1983, Columbia University sociologist W. Phillips Davison published a seminal article titled "The Third Person Effect in Communication" in *Public Opinion Quarterly*. In this article, Davison argued that some of the mass media's most interesting and compelling effects on society take place because people think media are influential. Davison suggested

that people exposed to persuasive communication in the mass media typically think that such communication has much greater influence on other people than on themselves. This third-person perception (TPP) is oftentimes consequential, Davison argued, because it “leads to action” (p. 1).

While the article was full of convincing anecdotes and examples, it contained little empirical evidence, mainly documenting the perceptual component of the TPP. Despite this lack of evidence, Davison’s (1983) article has had a remarkable impact on communication research. A recent survey ranked the third-person effect fifth on a list of “most popular theories” in contemporary communication research (Bryant & Miron, 2004). A meta-analysis (Sun, Pan, & Shen, 2008) covering 60 articles, 106 studies, and 372 effect sizes found robust support for the perceptual component of the third-person effect (the average effect size was  $r = .307$ ).

Most studies that followed Davison (1983), especially in the early years, replicated his findings and tried to decipher the mechanism underlying the perceptual phenomenon. Indeed, our understanding of why people think others are more influenced than themselves has considerably improved (see Andsager & White, 2007, for a recent review). Yet the reason for Davison’s enthusiasm about TPPs was that he recognized the ways in which these perceptions might impact the real world. However, only in recent years have scholars turned their attention to the behavioral consequences of people’s biased perceptions of media effects.

These studies that focus on the behavioral component of the third-person effect (Davison, 1983) are generally called “the influence of presumed media influence” research (Gunther & Storey, 2003). Although this focus is relatively new, we were able to locate 30 published studies, all of them utilizing correlational designs, documenting an association between presumed media impact and some dependent variable.

The dependent variable that has attracted the most attention as a possible outcome of people’s perceptions of media influence is support for message restrictions. Research has documented that the more people perceive strong negative influence of harmful media content on others, the more they support censorship. Support for message restrictions as a result of perceived negative effects has been documented in various contexts, including pornography (Gunther, 1995; Lee & Tamborini, 2005; Lo & Wei, 2002), other sexual or immoral content (Gunther & Hwa, 1996), violence (Rojas, Shah, & Faber, 1996; Salwen & Dupagne, 1999), negative political advertising (Rucinski & Salmon, 1990), reality television (Sun, Shen, & Pan, 2008), election polling (de Vreese & Semetko, 2002), misogynic rap music (McLeod, Eveland, & Nathanson, 1997), and additional types of media content. Support for restrictions was stronger for more socially undesirable effects (Sun et al., 2008), in particular when behavioral effects on others were considered by the estimators (Hoffner et al., 1999) and the more concerned they were about these effects (Cohen & Weimann, 2008). As Gunther, Perloff, and Tsfati (2008) noted, this set of findings has socially important practical implications, as it demonstrates that people may form their attitudes toward censorship and freedom of speech based on biased perceptions of negative media impact on others.

But support for censorship is only one of the contexts in which the presumed influence hypothesis has been examined. Findings confirming the presumed influence hypothesis in different contexts have proliferated in recent years. It was found, for example, that adolescents perceiving that sexual messages on TV make their peers more permissive toward sex are significantly more likely to hold more permissive sexual attitudes themselves (presumably to comply with the perceived social norm), with these attitudes affecting their intentions to engage in sexual activities (Chia, 2006). It was also found that doctors were more likely to refuse to prescribe direct-to-consumer (DTC) drugs when they perceived that DTC prescription drug advertising had negative effects on their clients (Huh & Langteau, 2007). In other studies, minority members were found more likely to feel alienated when they felt mainstream news coverage stigmatized their group and had an impact on majority members (Tsati, 2007), and teenagers were more susceptible to cigarette smoking when they felt prosmoking media messages influenced their peers (Gunther et al., 2006).

A recent meta-analysis (Xu & Gonzenbach, 2008) examined 25 studies of the "behavioral component of the third-person effect" or "influence of perceived media influence" and found evidence for a relatively small, but significant, effect across studies (mean Pearson  $r = .054$ , median Pearson  $r = .15$ ,  $SD = 0.010$ , with a 95% confidence interval from 0.12 to 0.20). Twenty four of the 25 studies in the meta-analysis were correlational, and in the 25th (Golan, Banning, & Lundy, 2008), perceptions of influence were not manipulated, and thus it does not offer evidence confirming the causal mechanism behind the correlation between perceptions of media impact and their alleged consequences.

A major limitation of this set of findings is that the correlational design they all utilized leaves open the possibility of reverse causality, which is equally plausible in many of the studies: It could very well be, for example, that people support censorship of pornographic materials for ideological reasons (believing that people should engage in sexual activities only in private), and they form their perceptions of the negative impact of exposure to such materials as an excuse for their support for censorship. This hypothetical reasoning rests on the assumption that the conservative ideologies underlying support for censorship are deeply rooted and well-formed beliefs, whereas perceptions of media influence are tentative, short-term perceptions (Tal-Or & Tsati, 2007). This distinction seems plausible because research has shown that like other perceptions, perceptions of media influence are subject to a variety of biases in which people form their perception of reality to accommodate their underlying set of beliefs (e.g., Fields & Schuman, 1976). For example, when media messages are perceived as positive, people tend to see themselves as more, not less, affected by them (e.g., Gunther & Hwa, 1996). Does it not make sense, then, that perceptions of media influence could be formed in order to justify attitudes toward censorship in a general social environment that values free speech? It seems logical that relatively stable and well-ingrained attitudes would influence more tentative perceptions and not the other way around.

Similar "reverse causality" criticisms could be directed at "presumed influence" studies in other domains as well: It is possible to argue that minority members explain their alienation from society (that has to do with ethnic or socioeconomic conditions) by the influence of their stigmatizing media coverage on majority members. That is, the alienation

causes the perceived media impact and not the other way around, as suggested by the presumed influence hypothesis (Tsfati, 2007). It is likewise possible to argue, in contrast to presumed influence research (Chia, 2006), that a teenager who behaves in a sexually permissive manner may justify this nonnormative behavior by claiming that sexual TV materials cause her peers to be more permissive toward sex, which in turn creates normative pressures on her to behave the way she does. Moreover, it is possible to argue that a teenager on an unnecessary diet may report that media messages cause her peers to equate attractiveness with thinness, as an *excuse* or an *explanation* for her unhealthy dieting behavior and not the other way around (as argued by Milkie, 1999).

However, reverse causation is not the only possible criticism that could be aimed at studies of the influence of presumed media influence. Despite the fact that many studies employ a rather extensive set of controls in their statistical analysis, it is always possible that some uncontrolled third variable may underlie the reported associations between presumed influence and the dependent variables, and that these associations are in fact spurious. In the case of attitudes toward message restrictions, for example, a variety of individual traits (such as paternalism, conservatism, authoritarianism) that may explain support for censorship may also underlie the perceptual component of the third-person effect (Andsager & White, 2007). That is, it could be that one's paternalism (or maternalism) is the reason for her perception that others are influenced by violent content and, independently, account for her support for censorship. Similarly, it could be that doctors' attitudes toward DTC drugs are the cause for both the perceptions that DTC drug advertising has a negative influence on the audience and for their refusal to prescribe these drugs. Finally, high self-monitoring (a tendency to be preoccupied with how one is viewed by others and to behave, accordingly, in a manner that aims to please others; Snyder, 1987) may cause parents to report that their child is unaffected, whereas other children are affected, by potentially harmful violent or sexual media content. Independently, high self-monitoring may cause parents to report monitoring of one's children's media and social activities in order to be perceived as a responsible parent.

In sum, several potential criticisms can be aimed at correlational studies of the influence of presumed influence. Taken together, these criticisms suggest that possibly the effect of Davison's (1983) third-person effect is not an effect of perceptions of media influence on attitudes or behavior, but it rather is only a statistical artifact. These potential criticisms can only be ruled out using experimental investigations that would substantiate the causal mechanism implied by the presumed influence hypothesis, but such studies are lacking.

The only exception we were able to locate comes from a persuasion study in which Dillard, Shen, and Vail (2007; in Study 5) manipulated perceived message effectiveness (by telling their participants that the public service announcements [PSAs] they are about to watch have been either successfully or unsuccessfully pretested on other participants) and measured the effects of this manipulation on participants' behavioral intentions. Although their results are clearly relevant and important, they do not offer general support for the influence of presumed media influence, as they are limited to persuasive situations. It is thus impossible to generalize from Dillard et al.'s (Study 5) results to more wide-ranging scenarios in which people react more broadly to their expectations of how *other* people will operate regardless of the persuasive intent of the text.

In sum, the current literature does not offer clear evidence of the causal mechanism underlying the correlations reported in studies of the influence of presumed media influence. To make sure the presumed influence hypothesis holds as posited, it is necessary to put it to an experimental test. This article reports two such experimental investigations. In both cases, we hypothesize, given the notion of the influence of presumed influence (Gunther & Storey, 2003) stemming from Davison's (1983) idea of the third-person effect, that perceptions of media influence will influence participants' subsequent attitudes or behavioral intentions.

## Study I

In this study, we set out to manipulate participants' perceptions that pornography has either positive or negative or inconclusive social effects and subsequently measured their attitudes toward censorship of pornographic content. Given that censorship has been the most studied behavioral consequence of presumed media influence, we thought it would be essential to test the causal direction of these two variables. Based on previous correlational studies in the domain of message restrictions (e.g., Gunther, 1995), we hypothesize that

*Hypothesis 1:* Participants who are manipulated to think pornography has negative influences on its audience will become more supportive of censorship than those receiving inconclusive evidence and that participants who are manipulated to think that pornography has positive influences will become less supportive of censorship than the inconclusive group.

## Method

**Participants.** Participants were 127 undergraduate students enrolled in political science and law classes at a large university in Israel. The experiment was conducted in class during class hours. Seventy-nine females and 48 males participated in the experiment and their age ranged from 19 to 30 with a median age of 24. Eighty-seven percent were Jewish and the rest were Arab (11%) or others; 75% considered themselves to be secular, 22% traditional or religious, and the rest were coded as missing or "other."

**Procedure.** After receiving permission from instructors, experimenters entered the classrooms and handed out randomly three versions of a four-page pamphlet. In the first page of the pamphlet, there was an introduction explaining that participants were to answer a short survey about attitudes toward censorship. In the next page, the participants were asked to read a short paragraph about the effects of pornography and then answer a questionnaire concerning censorship. The paragraph either claimed positive effects of pornography, negative effects, or inconclusive evidence regarding the effects of pornography. In the third page, there were five questions that serve as a manipulation check and several demographic questions. The last page included a written statement debriefing the participants as to the true object of the study and to the fact that the information they received about the effects of pornography was contrived as a manipulation. They were informed that in fact research has shown negative effects under specific conditions.



**Stimulus materials.** The three manipulation paragraphs, originally in Hebrew, are translated below. We did not attribute the manipulation paragraphs to a specific source in order to minimize demand characteristic (potentially mentioning a powerful source would increase the desire to comply) and in order to avoid unnecessary ethical issues (of wrongly attributing false information to a specific source).<sup>1</sup>

*Negative:* In recent years many studies have examined the question of the impact of sexually explicit media content. These studies found that exposure to such content creates negative attitudes toward women, more sexual violence, and unrealistic expectations in sexual relationships. However, no evidence was found that exposure to sexually explicit content helps diffuse sexual tensions or enhances sexual performance. There is general agreement among scientists that exposure to sexually explicit materials creates real social and psychological harm whereas there are no social benefits from such exposure.

*Positive:* In recent years, many studies have examined the question of the impact of sexually explicit media content. These studies found that exposure to such content helps diffuse sexual tensions, reduces sexual violence, and enhances sexual performance. On the other hand, no evidence was found that exposure to sexually explicit content creates more negative attitudes toward women or unrealistic expectations in sexual relationships. There is general agreement among scientists that exposure to sexually explicit materials is socially beneficial and is not associated with any real social and psychological harm.

*Inconclusive:* In recent years, many studies have examined the question of the impact of sexually explicit media content. These studies did not arrive at any consistent findings about the influence of exposure to such content on diffusing sexual tensions, reducing sexual violence, and enhancing sexual performance. Inconsistent findings were also found regarding the impact of exposure to sexually explicit content on creating negative attitudes toward women or unrealistic expectations in sexual relationships. The contradiction in the findings of these studies have led scientist to agree that we do not have a reliable answer as to the harm or benefits of exposure to sexually explicit materials.

**Dependent measure.** Participants were presented with 11 statements about endorsement of censorship (see Table 1). Respondents rated their agreement to these statements on 7-point scales ranging from 1 (*strongly disagree*) to 7 (*strongly agree*). Items included, for example, "I support laws that would prohibit the distribution of sexually explicit materials," "There should be a body of experts in psychology and education that will have sole discretion as to what sexually explicit materials can be distributed," and "One of the responsibilities of the government is to limit the distribution of materials that are harmful to the public." All items were scored so that higher scores indicated a stronger belief in

**Table 1.** Factor Analysis on Censorship Items

Item	M	SD	Factor 1	Factor 2
Though there are people who want to watch sexually explicit materials, it is appropriate to limit such content	4.26	1.89	.85	.04
There ought to be laws punishing anyone who produces and disseminates sexually explicit materials	3.24	1.92	.81	.34
I support laws that would prohibit the distribution of sexually explicit materials	3.34	1.96	.76	.35
There is a need to limit the broadcast of sexually explicit materials to certain channels and times of day	5.31	1.71	.67	.07
There should be a body of experts in psychology and education that will have sole discretion as to what sexually explicit materials can be distributed	3.55	1.98	.62	.25
Anyone distributing harmful materials should bear the legal responsibility for any damage that may result from exposure to such materials	3.81	1.92	.62	.35
One of the responsibilities of the government is to limit the distribution of materials that are harmful to the public	4.44	1.92	.59	.33
The government should ensure that anyone who wants to watch TV and surf the Internet without encountering sexually explicit materials should be able to do so	4.63	1.81	.07	.75
Freedom of expression should end where it hurts others	4.40	1.83	.20	.72
Protecting society is more important than the right of people to express or consume anything they feel like	3.64	1.73	.58	.62
In a moral society there is no room for sexually explicit materials in the media	2.72	1.60	.53	.57

censorship. These items were based on those used by Gunther (1995) and were adapted to fit the Israeli context.

The 11 items measuring support for censorship of pornography were subjected to an exploratory factor analysis (Principal components, Varimax). Seven items loaded on one factor, two others loaded on a second factor, and the final two loaded on both factors (see Table 1). The four items that loaded on the separate factor did not seem to cohere in a way that offered a theoretically valuable addition to the first factor. The seven-item scale provided good reliability (Cronbach's  $\alpha = .87$ ) and was used as the dependent variable in

further analysis. Despite the fact that all items were directional (i.e., all of them measured agreement with statements supporting censorship, which may bias responses), the mean support for censorship across groups was at the midpoint of the scale ( $M = 4.01$ ,  $SD = 1.42$ ).

**Manipulation check items.** The manipulation check included five items regarding respondents' attitudes about the effects of pornography. These items employed a similar seven-point scale ranging from 1 (*strongly disagree*) to 7 (*strongly agree*). The five items were as follows: "Sexually explicit materials tend to increase viewers' chauvinist attitudes toward women," "Exposure to sexually explicit materials may sometimes encourage more aggressive sexual behavior," "Exposure to sexually explicit materials can help in overcoming sexual pressures," "Exposure to sexually explicit materials can enhance sexual performance," and "Exposure to sexually explicit materials can create unrealistic sexual expectations among viewers." All of these items were subjected to an exploratory factor analysis that revealed two factors. The first factor included the three items relating to negative influence. After removing the item concerning unrealistic sexual expectations, which reduced the reliability, the Cronbach's alpha reliability reached .82. An index was created based on the mean of the remaining two items (about chauvinist attitudes and aggressive sexual behavior;  $M = 4.04$ ,  $SD = 1.71$ ). The second factor included the two items relating to positive influence. An index was created based on the mean of these two items (about overcoming sexual pressures and enhancing sexual performance;  $M = 3.54$ ,  $SD = 1.34$ , Cronbach's  $\alpha = .76$ ).

**Demographics.** The demographic information included sex, age, years of education, ethnicity (Arab, Jew, other), level of religiosity (secular, traditional, religious, other), and political orientation (1 = *extreme left* to 7 = *extreme right*).

We examined the effectiveness of the random assignment by comparing the three experimental groups on the control variables. There were no significant differences between the groups on age, years in school, or political leanings (all  $ps > .39$ ). Chi-square tests on sex, ethnicity, and religiosity showed a marginally significant association between sex and group ( $p < .10$ ) due to a higher relative frequency of males in the inconclusive group than in the positive or negative groups. Ethnicity and group assignment were not statistically related. However, a significant association was found between condition and religiosity such that in the positive condition there were more secular and less traditional/religious participants. Thus, both sex and religiosity were controlled in the main analysis.

## Results

**Preliminary analysis.** Arabs were found to be more pro-censorship ( $M = 4.9$ ,  $SD = 1.1$ ) than Jews ( $M = 3.9$ ,  $SD = 1.4$ ),  $t(122) = -2.6$ ,  $p < .01$ . Similarly, religious and traditional students held more pro-censorship views ( $M = 4.9$ ,  $SD = 1.0$ ) than secular students ( $M = 3.8$ ,  $SD = 1.4$ ),  $t(119) = -4.5$ ,  $p < .001$ . Finally, females were more in favor of censoring pornography ( $M = 4.3$ ,  $SD = 1.4$ ) than males ( $M = 3.6$ ,  $SD = 1.3$ ),  $t(125) = -2.7$ ,  $p < .01$ .

**Manipulation check.** A one-way ANOVA comparing the means of the different groups on the negative influence index found significant between-group differences,  $F(2, 123) = 3.09$ ,  $p < .05$ . A significant planned contrast was found between the group receiving

information about positive effects ( $M = 3.51$ ,  $SD = 1.57$ ) and the negative-effects group ( $M = 4.40$ ,  $SD = 1.72$ ),  $t(123) = 2.38$ ,  $p < .05$ , in their belief that pornography increased violence and chauvinism. Likewise, a marginally significant contrast was found between the group receiving information about positive effects and the inconclusive evidence group ( $M = 4.19$ ,  $SD = 1.75$ ),  $t(123) = 1.85$ ,  $p = .07$ . However, the group receiving information about inconclusive evidence was not different from the negative effects group in their endorsement of the negative effects of pornography,  $t(123) = .59$ ,  $p > .57$ . Thus, the inconclusive and negative groups were merged. A one-way ANOVA comparing the means of the different groups on the positive-influence index did not find significant between-group differences,  $F(2, 123) = 1.03$ ,  $p > .10$ . Thus, our manipulation indeed affected respondents' perceptions about pornography's negative effects, but it failed to influence their perceptions that pornography has desirable or positive effects.

**Hypothesis testing.** A univariate ANOVA was conducted to compare the means of attitudes about censorship between the two experimental groups (i.e., the positive group compared with the inconclusive + negative groups together). The analysis used censorship as the dependent variable, the valence of pornography effects as an independent variable, and sex and religiosity (the two control variables that were not equally distributed among groups) as covariates. The results revealed a significant main effect for the valence manipulation,  $F(1, 117) = 4.74$ ,  $p < .05$ , such that, as expected, participants receiving information about positive effects of pornography were less supportive of censorship ( $M = 3.57$ ,  $SD = 1.29$ ) than those receiving either negative or inconclusive information ( $M = 4.27$ ,  $SD = 1.42$ ).<sup>2</sup>

**Mediation analysis.** If the process we depict holds, then our manipulation's effect on attitudes toward censorship should be mediated through participants' perceptions regarding pornography's impact. This is consistent with O'Keefe's (2003) claim that when manipulations are defined in terms of intrinsic features, manipulation checks are unnecessary, but similar measures may usefully be understood and analyzed as potential mediators between the manipulations and the dependent variable. To test for this mediated effect, we first used Baron and Kenny's (1986) causal steps strategy and then examined the statistical significance of the mediated path as recommended by Preacher and Hayes (2008).

Baron and Kenny (1986) claimed that mediation occurs when the independent variable significantly affects (a) the dependent variable and (b) the proposed mediator, (c) the proposed mediator significantly affects the dependent variable, controlling for the independent variable, and (d) the effect of the independent variable on the dependent variable decreases substantially when controlling for the mediator.

These conditions were tested using ordinary least squares (OLS) regression models, controlling for sex and religiosity (the two control variables that were not equally distributed among groups). Results are reported in Table 2. The first column on the left tests the first condition. This coefficient was obtained when regressing attitudes toward censorship on the independent (manipulation) variable. As reported above, the manipulation had a significant effect on attitudes toward censorship (in OLS regression terms,  $b = .57$ ,  $p < .05$ ). The second column tests Baron and Kenny's (1986) second condition. This coefficient was obtained when regressing presumed negative influence (the manipulation-check construct) on the independent (manipulation) variable. As reported in the manipulation

**Table 2.** Mediation Testing for Study 1: OLS regression coefficients ( $n = 119$ )

	Effect of Manipulation on Reactions	Effect of Manipulation on Mediator	Effect of Mediator on Reactions	Effect of Manipulation on Reactions, Controlling for Mediators
Baron and Kenny's (1986) notation	C path	A path	B path	C' path
	.57** (.26)	.87*** (.31)	.50*** (.06)	.13 (.21)

Notes: OLS = ordinary least squares.

All models controlled for sex and religiosity (that were not equally divided between experimental conditions).

\* $p < .1$ . \*\* $p < .05$ . \*\*\* $p < .01$ . \*\*\*\* $p < .001$ .

check, presumed influence was significantly affected by the manipulation. The third column tests Baron and Kenny's third condition. This coefficient was obtained when regressing attitudes toward censorship on presumed influence. Presumed influence significantly affected the dependent variable (attitudes toward censorship). Finally, the fourth column reports Baron and Kenny's C' path: The effect of the manipulation on the dependent variable, after controlling for the potential mediator. This coefficient was obtained when regressing attitudes toward censorship on the manipulation variable, controlling for the presumed influence of pornography (and for the covariates).  $R^2$  for this equation was .47. Results demonstrated that the effect of the manipulation decreased approximately by one fourth and became insignificant after controlling for presumed influence ( $b = .13$ ,  $SE = .21$ ,  $p = .53$ , compared to  $b = .57$ ,  $SE = .26$ ,  $p < .05$ ). Thus, the evidence suggested that the presumed influence of pornography mediates the effect of the manipulation on the dependent variable.

Finally, we tested whether the mediated effect of the manipulation through presumed influence was statistically significant, using Preacher and Hayes' (2008) indirect SPSS macro. Again, we used sex and religiosity as covariates. The bootstrap result for the mediated effect was significant ( $b = .43$ ,  $SE = .15$ ,  $p < .05$ ). Thus, we can conclude that our manipulation affected respondents' perceptions regarding pornography's impact, and these perceptions, in turn, affected their attitudes toward pornography.

**Interaction with sex.** In a further analysis testing all possible interactions, we found evidence for a significant sex  $\times$  manipulation interaction,  $F(1, 117) = 7.73$ ,  $p < .05$ . In order to understand this interaction, we conducted 2 one-way ANOVA's on each sex separately. This analysis showed that for males, the positive group ( $M = 3.68$ ,  $SD = 1.16$ ) was not significantly lower than the negative + inconclusive group ( $M = 3.56$ ,  $SD = 1.33$ ),  $F(1, 45) = 0.84$ ,  $p > .1$ . However, females receiving information that pornography has positive effects tended to support censorship less ( $M = 3.61$ ,  $SD = 1.37$ ) than the negative + inconclusive group ( $M = 4.61$ ,  $SD = 1.37$ ),  $F(1, 76) = 7.69$ ,  $p < .01$ . Thus, it seems that only women's attitudes about censorship were affected by the experimental manipulation.

To investigate why only women's attitudes toward censorship were affected, we explored whether the manipulations were equally effective for both sexes. Analyzing by

sex, a one-way ANOVA comparing the means of the negative-effects-manipulation-check scale across experimental groups indicated that for males, the positive group ( $M = 2.88$ ,  $SD = 1.53$ ) was not significantly lower than the negative + inconclusive group ( $M = 3.28$ ,  $SD = 1.57$ ),  $F(1, 45) = 0.60$ ,  $p > .1$ . However, females receiving information that pornography has positive effects tended to report weaker negative beliefs about the effects of pornography ( $M = 3.81$ ,  $SD = 1.53$ ) than the negative + inconclusive group ( $M = 4.95$ ,  $SD = 1.50$ ),  $F(1, 77) = 10.10$ ,  $p < .01$ . Thus, it seems that only women's attitudes about censorship were affected by the experimental manipulation. In sum, our post hoc analysis demonstrated that both the manipulation check and the main effect on support for censorship were limited to female respondents.

## Discussion

Our goal in the current research was to manipulate perceived media influence in order to ascertain its causal relationship with support for censorship, thereby confirming Davison's (1983) original hypothesis. Essentially, the results confirmed our expectations: We found that providing information about positive effects of pornography indeed weakened the support for censorship. Thus, this study provides initial experimental evidence of the causal direction of the influence of presumed media influence hypothesis suggesting that belief that the media can affect others is responsible for the consequent changes in the outcome variables in presumed influence studies.

Our experiment demonstrated that it is possible to manipulate experimental participants' perceptions of media influence. Specifically, we succeeded in altering participants' opinions about the negative effects of pornography on sexual aggression and chauvinism, making them less extreme. However, the component of our manipulation relating to positive pornographic influence (of easing sexual tensions and enhancing sexual performance) did not work as expected. This was the case despite using unambiguous information, ostensibly from scientific studies about pornography's effects. Perhaps the selective effectiveness of our manipulation is due to the fact that people's negative perceptions of pornography may be too deeply rooted for them to accept any possible positive influence of pornography.

Moreover, both the manipulation check and the main effect on support for censorship were limited to female respondents. Despite the fact that this pattern of findings was unexpected, the results of the experiment show that where we succeeded in manipulating beliefs about media influence, this manipulation significantly impacted attitudes toward censorship, and in the expected direction. It is no surprise that men, who were not affected by the manipulation, did not show any difference in attitudes toward censorship between experimental groups. The fact that our manipulation was effective only for female respondents may have to do with women's general aversion to risks (Byrnes, Miller, & Schafer, 1999), which may make them more likely to accept information on the possibility of negative outcomes as valid. This is the case in particular when the risks at hand are especially relevant for females (being the victim of sexual violence and chauvinist attitudes).

The main limitation of the study, in addition to the fact that the manipulation did not affect perceptions regarding positive effects and that it affected women only, was that it focused on the most studied influence of presumed media influence, that is, support for censorship (e.g., Gunther, 1995; Rucinski & Salmon, 1990). Censorship of pornography is a topic that is so often discussed that participants may have very firm opinions about it. Possibly, censorship of pornography has become the “gold standard” of third-person-effect studies because of the immense public interest and the active public discourse about it. Set in this context, our results are centrally relevant to the literature, but they may also be an overly conservative estimate of these effects because of firmly established attitudes. Another possible limitation is the fact that our manipulation may have been too overtly didactic and direct, and thus that participants’ reactions might have been influenced by demand characteristics. In other words, our participants read an excerpt saying that pornography has either harmful or beneficial effects and were expected to state their attitude toward censorship of pornography directly afterwards. In this context, it is arguable that participants could discern the experiment’s intention and respond accordingly. Although we found no patent evidence of this potential problem, we designed a second study to guard against it.

## Study 2

In his seminal article, Davison (1983) asserted that when media report about irregular supplies of consumer goods, people rush to the stores to get those goods the moment they hear about the anticipated shortage. According to Davison, as a result of assumed media impact, these people “want to stock up before the hoarders remove all goods from the shelves” (p. 13). Following his logic, in Study 2, we indirectly manipulated participants’ perceptions that a newspaper story about shortage in sugar would either have a strong or weak impact on other people’s intention to purchase sugar. We consequently measured respondents’ own intention to react to the publication of the story by buying sugar or otherwise. We hypothesize, in line with the presumed influence hypothesis, that

*Hypothesis 2:* Participants will be more likely to react to the sugar shortage report when they will be manipulated to think that others are more affected by the report.

The main objective of Study 2 was to replicate the findings of Study 1 in another context, one in which participants do not have firm beliefs and attitudes. In such a context, we speculated it should be easier to manipulate perceived media influence. Sugar shortage was selected as we expected our respondents not to hold such strong opinion about buying sugar (although this product is considered a necessity for everybody for everyday use). An additional aim of Study 2, in response to the potential limitation that the Study 1 manipulation was too direct (and thus, that participants’ reactions might have been influenced by demand characteristics), was to manipulate presumed media influence in a less obtrusive manner.



## Method

**Participants.** Participants were 134 undergraduate students enrolled in communication and political science classes at a large university in Israel. The experiment was conducted in class during class hours. Eleven students reported they never purchase sugar and it did not make much sense to include them in a study in which intentions to purchase sugar is the dependent variable. The remaining 123 participants were 43 males and 79 females (one respondent did not report his or her sex). Their age ranged from 18 to 61 with a median age of 24.

**Procedure.** Experimenters explained that participants were to answer a short survey about media impact on purchase decisions and handed out randomly two versions of a two-page pamphlet. In the first page, participants were asked to read a newspaper article about an expected shortage in sugar that is about to be published in the coming days. In the second page, the participants were asked to answer three questionnaires, one measuring reactions to the publication of the shortage in sugar story, the second served as a manipulation check, and the final set of questions were demographic. After respondents completed the questionnaires, they were debriefed as to the true object of the study and about the fact that the information they received about shortage in sugar was false.

**Stimulus materials.** Participants were instructed to read a news story paragraph, originally in Hebrew translated below:

### *Following an increase in demand for biodiesel*

#### *Shortage in sugar expected*

Financial and retail markets worldwide registered a substantial increase in the price of white sugar in recent weeks. This trend has also been felt here in Israel. Mr. Nir Edry, commercial VP of the Supersol supermarket chain, commented that if this shortage continues, the price of sugar may rise as early as next week, and perhaps an actual shortage will follow closely.

The reason for the shortage is related to the increasing use of biodiesel—a fuel made from organic materials including sugar cane—in the U.S. auto industry. Following the energy crisis last year, the use of sugar based biodiesel became an attractive alternative and many sugar farmers started marketing their produce for producing biodiesel rather than sugar.

A spokesperson for the ministry of commerce and industry commented that there was no reason for concern. According to the ministry's calculation, current supplies should be sufficient to last through the present crisis. The spokesperson was quoted as saying, "There is no need for panic. If households and the food industry will continue consuming sugar at the level they have been consuming in recent months, we should have no shortage at all and the sugar supplies should be enough for everyone."



Our manipulation of perceived influence followed research demonstrating that perceived exposure is a strong predictor of the perception that others are affected by media, such that the more people perceive the stimulus is watched or read by many, the more they think it is impactful (Eveland, Nathanson, Detenber, & McLeod, 1999) and will lead to changes in public attitudes (Gunther & Christen, 1999). In line with this logic, we manipulated perceived impact by randomly assigning participants to two different versions of the questionnaire. In the first version, the introduction to the news story said it was about to be published in the coming days on the front page of *Yediot Achronot* (Israel's largest circulation daily); in the second condition, the introduction said that the story is about to be published in an internal page of the economic supplement of the same newspaper. Following the perceived exposure explanation of the TPP, we expected that respondents in the "front page" condition would perceive more impact than respondents in the "internal page of the economic supplement" condition.

**Dependent measure.** Participants were presented with four statements about possible reactions to the publication of the shortage in sugar story. Respondents rated their agreement to these statements on scales ranging from 1 (*strongly disagree*) to 7 (*strongly agree*). The items included "publishing the article might affect my decisions about buying sugar," "I will buy sugar next time I go to the store," "I will buy more sugar than usual next time I go to the store," and "I will try to reduce my sugar consumption." The items were subjected to a factor analysis (Principal components, Varimax) and loaded on a single factor explaining 72.10% of the variance. Despite the fact that the first item was unidirectional (not referring to an increase but rather to a change in sugar consumption), interitem reliability was appropriate ( $\alpha = .84$ ;  $M = 3.48$ ,  $SD = 1.55$ ). This indicates that our participants perceived the nondirectional item consistently with the directional ones.

**Manipulation check.** We first measured the perceived influence of the publication of the story. The two items measuring this construct were worded "publishing this article will affect the public's decision to buy sugar" and "publishing this article will cause an increase in sugar consumption." The items employed a similar 7-point scale ranging from 1 (*strongly disagree*) to 7 (*strongly agree*). As the items were highly correlated (despite the fact that the second is directional and the first is not;  $r = .77$ ,  $p < .001$ ;  $\alpha = .87$ ), they were averaged to form a presumed influence scale ( $M = 5.60$ ;  $SD = 1.32$ ). In addition, we wanted to make sure that people react to perceived influence and not to the perceived importance of the topic (because perceived importance is confounded in the front page / back page manipulation), so we included two items asking about perceived importance: "The article is about an important topic concerning the global economic crisis" ( $M = 4.20$ ,  $SD = 1.74$ ) and "the decision to publish the article on the front/back page is an indication of the topic's importance" ( $M = 4.25$ ,  $SD = 1.73$ ; again, we used similar 1-7 "strongly disagree" to "strongly agree" scales as response categories). As the correlations between the two items were much lower ( $r = .39$ ,  $p < .001$ ;  $\alpha = .57$ ), they were analyzed separately, though averaging them and working with a scale did not impact the results reported below.

There were no significant differences between the experimental conditions on age and sex. In addition, we made sure that the groups did not differ in terms of the frequency of sugar use. Thus, the random assignment this time produced demographically equal groups.

## Results

**Manipulation check.** As expected, and in line with the perceived exposure argument (Eveland et al., 1999), participants in the “front page” condition were more likely to perceive that the publication of the story will affect the public’s decision to buy sugar ( $M = 5.85$ ,  $SD = 1.27$ ) compared to participants in the “internal page of the economic supplement” condition ( $M = 5.37$ ,  $SD = 1.33$ ),  $t(121) = 2.02$ ,  $p < .05$ . However, participants in the “front page” condition were also more likely to agree that “the article is about an important topic concerning the global economic crisis” ( $M = 4.53$ ,  $SD = 1.77$ ) compared to participants in the “back page of the economic supplement” condition ( $M = 3.91$ ,  $SD = 1.66$ ),  $t(121) = 2.02$ ,  $p < .05$ . There were no significant differences between the experimental conditions on the item worded “the decision to publish the article on the front/back page is an indication of the topic’s importance,”  $t(121) = 1.51$ ,  $p > .10$ . In sum, the manipulation indeed affected perceptions of media influence, but some of the evidence suggested that it was confounded with issue importance.

**Hypothesis testing.** Before examining the issue importance confound, we tested the hypothesis directly. As expected by Hypothesis 2, participants in the “front page” condition scored higher on the behavioral reaction scale ( $M = 3.74$ ,  $SD = 1.45$ ), compared to participants in the “back page” condition ( $M = 3.25$ ,  $SD = 1.60$ ). However, this difference was only of borderline statistical significance,  $t(121) = 1.79$ ,  $p = .07$ . Interestingly, logically, and theoretically consistent, these differences become more robust and statistically significant when excluding participants who reported they “seldom” buy sugar ( $n = 67$ ) and analyzing only participants reporting they buy sugar “sometimes” or “regularly” ( $M_{\text{front page}} = 4.38$ ,  $SD = 1.41$ ;  $M_{\text{back page}} = 3.48$ ,  $SD = 1.74$ ),  $t(54) = 2.08$ ,  $p < .05$ .

**Mediation analysis.** Given that the front page/back page manipulation had an impact not only on participants’ perceptions regarding the influence of the news story but also on their perceptions regarding the issue’s importance, it is necessary to demonstrate that the manipulation’s impact on the dependent variable is mediated through perceptions of media influence. Given that issue importance was also affected by the manipulation, it makes sense to examine whether this factor mediated the effect of the manipulation on the dependent variable as well. To test for these mediation effects, we again used Baron and Kenny’s (1986) causal steps strategy and then examined the statistical significance of the mediated paths as recommended by Preacher and Hayes (2008).

Baron and Kenny’s (1986) conditions were again tested using OLS regression models. The procedure they suggest was first conducted separately for each of the mediators. Results are reported in Table 3. The first column on the left again tests the first condition. As reported above, the manipulation had a borderline significant effect on reactions (in OLS regression terms,  $b = .50$ ,  $p = .07$ ). The second column tests Baron and Kenny’s second condition: As reported in the manipulation checks, both presumed influence and perceived importance were significantly affected by the front page/back page manipulation. The third column tests Baron and Kenny’s third condition: Both presumed influence and perceived importance significantly affected the dependent variable (reactions). Finally, the fourth column reports the effect of the manipulation on the dependent variable, after controlling for the potential mediators separately. In both cases, the effect of the manipulation

**Table 3.** Mediation Testing for Study 2: OLS Regression Coefficients ( $n = 121$ )

	Effect of Manipulation on Reactions	Effect of Manipulation on Mediator	Effect of Mediator on Reactions	Effect of Manipulation on Reactions, Controlling for Mediators
Baron and Kenny's (1986) notation	C Path	A Path	B Path	C' Path
Presumed media influence	.50* (.28)	.48** (.24)	.50**** (.09)	.25 (.25)
Issue importance	.50* (.28)	.62** (.31)	.40**** (.07)	.24 (.25)

Notes: Table entries are unstandardized coefficients. Standard errors are reported in parentheses.

\* $p < .1$ . \*\* $p < .05$ . \*\*\* $p < .01$ . \*\*\*\* $p < .001$ .

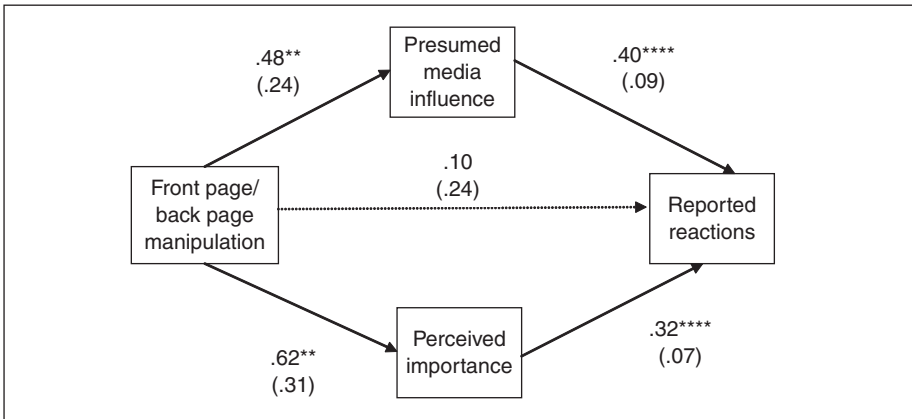
decreased approximately by half and became nonsignificant after controlling for each of the mediators. Thus, the evidence suggested that both presumed influence and issue importance mediate the effect of the manipulation on the dependent variable.

The next stage was to run a “multiple mediators” model to test whether the mediated effect of presumed media influence would remain significant when controlling for the other mediating factor—issue importance. In practice, this stage included running a fifth OLS regression model with the front page/back page manipulation, presumed media influence, and perceived importance as independent variables and reactions as the dependent variable ( $n = 121$ ,  $R^2 = .33$ ). The results of this model were incorporated with the results of the models predicting the mediators to form a path diagram, reported in Figure 1. As the figure shows, both presumed media influence and perceived importance had a significant effect on the reactions scale, even when controlling for each other. The effect of the manipulation on the dependent variable became even smaller ( $b = .10$ ,  $SE = .24$ ,  $p > .10$ ) when controlling for both mediators, clearly meeting Baron and Kenny’s (1986) fourth condition of a substantial reduction in the effect of the independent variables on the dependent variables when controlling for the mediators.

Finally, we tested whether the mediated effects of the manipulation through presumed influence and perceived importance were statistically significant, using Preacher and Hayes’ (2008) indirect SPSS macro. The bootstrap results for both mediated effects were significant ( $p < .05$ ), controlling for each other (for presumed media influence  $b = .19$ ,  $SE = .10$ ; for perceived importance  $b = .21$ ,  $SE = .11$ ). Thus, while part of the effect of the manipulation on the dependent variable was mediated through perceived importance, we were able to demonstrate that over and above this effect, the manipulation affected participants’ perceptions of the story’s influence and these perceptions in turn made people report that they intend to react to the story by buying sugar or otherwise.

## Discussion

As we expected, participants who were led to think that a news report about imminent shortage in sugar will have a strong impact on the general public (via a front page/back page



**Figure 1.** Path analysis of mediation effects—Study 2.

\*\* $p < .05$ . \*\*\* $p < .01$ . \*\*\*\* $p < .001$ .

manipulation) were more likely to report that they will purchase sugar or try to use less sugar than participants who were led to think that the news report will be somewhat less impactful. Logically, this finding was stronger for participants who reported buying sugar regularly.

Interestingly, the manipulation's effect on the dependent variable was mediated not only through perceptions of media influence but also through perceptions of the importance of the issue. Possibly, the interpretation of this path is that people buy more sugar when they think sugar shortage is an important issue (which could be interpreted as a direct effect of media coverage) and not just because the publication of the story will make others rush to the stores to buy sugar. Of course, our focus on the indirect effects of presumed influence does not rule out more direct effects of news stories. But, most importantly, the mediated effect of presumed media influence remained significant when controlling for issue importance. Thus, the results conform to our main argument regarding the causal relationship between presumed media influence and behavioral intentions.

This study complements Study 1 in several important ways. First, the context was one in which participants do not hold strong opinions. Second, media influence was not directly stated but instead was implied in the front-page/back-page manipulation, in a way that reduces the likelihood of demand characteristics. Third, interestingly, the results demonstrate that experimental investigations can not only be used to replicate and better understand the causal mechanism behind correlational findings in the direction postulated by Davison (1983), but they can also be used in order to shed light on previous unexpected findings demonstrating correlations in the reverse direction.

Davison's (1983) hypothesis that coverage of pending shortage leads to public panic through perceptions of media influence was put to an empirical test twice in the past. One study tested it in the context of media reports of the millennium bug (Y2K) panic (Tewksbury et al., 2004). This research found, in contrast to Davison's expectation, that respondents

who perceived larger effects of media coverage on others compared to self were *less* (not more) likely to stockpile supplies of food, water, gasoline, and cash. Another correlational investigation found that people who believed others to be more affected by newspaper coverage of earthquake predictions were somewhat *less* likely to report having taken steps to prepare their homes for the earthquake (Atwood, 1994).

Although several explanations for the contradictory findings were offered in each of the studies, it is possible that some mechanism, such as biased optimism, impression management, or some other self-serving mechanism, underlies both respondents' TPPs and their reports that they were not preparing for Y2K or the earthquake. Our results strengthen this suspicion, as using the experimental procedure, and thus eliminating the possibility of spuriousness, supported Davison's (1983) original hypothesis.

## General discussion

Media effects in their narrowest sense are social or psychological changes that occur in consumers of media messages *as a result* of being exposed to or processing or otherwise acting on those mediated messages (Bryant & Zillmann, 2009). The notion of media effects is thus intertwined with notion of causality (Perry 1996). As Beaudoin (2007) noted, "Anything short of a laboratory experiment cannot conclusively demonstrate causation" (p. 658; see also Iyengar & Kinder, 1987, pp. 6-8). This advantage explains why experiments are one of the most popular methodologies in quantitative investigations of media effects (Potter & Riddle, 2007). Given this recognition of the importance of experiments in substantiating media effects, experimental investigations of the increasingly important presumed influence hypothesis are notoriously absent (Tal-Or, Tsfat, & Gunther, 2009).

The current investigation thus presents a particularly important theoretical contribution to the literature on the influence of presumed media influence and the third-person effect. As a model of media effects, the presumed influence model makes a causal argument in which perceptions of media influence are the causes of various attitudinal and behavioral phenomena. But the current investigation is the first to test this causal argument, and thus it is actually the first to test the theory in its entirety.

Results from two experimental investigations, coming from two different contexts and utilizing different approaches for manipulating presumed media influence, consistently demonstrate that those participants who were affected by the manipulation and perceived stronger media impact were more likely to support censorship (in Study 1) or report intentions to buy a product in response to a media report on a pending shortage (in Study 2). The studies complement each other in several ways. The first revisits the most frequently studied context of the behavioral component of the TPP in which results supporting Davison's (1983) expectations were consistently obtained, and the second takes place in a much less-studied context, in which results contradicting Davison's hypothesis were hitherto published. The first manipulates perceptions of influence directly, by telling people that pornography has either positive, negative, or neutral effects, whereas the second utilizes our previous knowledge on what shapes perceptions of media impact and manipulates perceived media influence accordingly. The first utilizes an attitudinal dependent variable, and in the second, the dependent variable is more behavioral in nature.

Our results may have important implications. The successful manipulations of presumed media influence that in turn impacted behavioral intentions might serve as a persuasion tool. Persuaders may change attitudes and behaviors by providing evidence attesting to media influence on other people. This idea proved to be effective in the context of PSAs when information regarding the level of influence on other people by various PSAs affected the actual persuasiveness of these messages (Dillard et al., 2007), and our data suggest it is also true in the more complex general context of presumed media influence. For example, young women who receive evidence regarding the lack of media influence on the ideal women's body type held by young men may be less inclined to adopt severe diets. In addition, citizens of towns that are stigmatized by the media may be less inclined to consider relocation if they receive evidence regarding the lack of media impact on the perceived image of their place of residence. In this latter context, our evidence is especially important because mayors often complain about the "bad rap" their towns receive in the media (Avraham, 2000) and by doing so may actually be making the situation worse. This persuasion tactic might be particularly powerful as previous research showed that indirect strategies create less reactance than direct ones (Knowles & Linn, 2004).

Despite successfully demonstrating that people's perceptions of media influence shape their consequent attitudes, our two studies did not examine the possibility that this effect takes place in addition to a reverse effect of attitudes on perceptions of media influence. We do not rule out this possibility, and future studies will have to examine possible reciprocal effects in the domain of presumed media influence. Future research might also examine the possible interaction between preexisting perceptions concerning media influence and the manipulated perception of media influence on various behavioral intentions. Examining this impact of preexisting attitudes, although interesting, goes beyond the scope of the current study that aimed to establish the causal relationship between perceived media impact and behavioral intentions.

In recent years, communication researchers have discovered various other ways in which perceived media impact influences intention to behave. These behavioral intentions include the desire of peripheral towns' residents to relocate (Tsati & Cohen, 2003), medical doctors' resistance to prescribe widely advertised prescription drugs (Huh & Langteau, 2007), and youngsters' intention to smoke (Gunther et al., 2006). Future research might attempt manipulating perceived media impact in these other contexts and measuring their effects. Examining the impact of perceived media influence in contexts in which people's attitudes are weaker, and that manipulating them may be easier, should result in even stronger effects.

Future research will also benefit from examining effects on actual behavior, not just reported attitudes or behavioral intentions. Accomplishing this endeavor will bring the research of presumed media influence closer to Davison's (1983) original vision. Davison's conception of the third-person effect has yielded scores of studies that focused on documenting perceptual differences in various contexts and diverse populations. Only recently have researchers shifted their attention to the behavioral consequences of this perception. However, this research, being correlational in nature, does not emphatically, irrefutably, verify Davison's claim regarding the ability of perceived media impact to influence behavior, a

verification that was sorely needed given the abundant alternative explanations for this relationship. The current research provides a step in this direction by presenting clear experimental evidence that changing people's beliefs about the influence of media can affect their consequent beliefs and behavioral intentions. We hope that future research will continue our effort to substantiate the understanding of the power of perceived media influence.

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### Notes

1. It is important to note that the only possible change resulting from attributing the findings to a reputable source is that it would make the manipulation stronger. Our decision not to attribute the findings to a particular source only strengthens the validity of our findings, given that they were obtained despite the use of a relatively weak manipulation.
2. An ANOVA conducted separately on each of the remaining four items that, as explained above, were not incorporated into the dependent variable did not reveal any significant effects ( $p > .10$ ). We also conducted the hypothesis tests comparing only the positive to the negative groups (omitting the inconclusive condition). A univariate ANOVA was conducted to compare the means of attitudes about censorship between the two experimental groups. The analysis used censorship as the dependent variable and the valence of pornography effects as an independent variable. The results revealed a marginally significant main effect for the valence manipulation,  $F(1, 79) = 3.61, p = .06$ , such that, as expected, participants receiving information about positive effects of pornography were less supportive of censorship ( $M = 3.63, SD = 1.29$ ) than those receiving negative information ( $M = 4.23, SD = 1.52$ ).

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