

# *Personal Influence and the Effects of the National Youth Anti-Drug Media Campaign*

*By*  
ROBERT HORNIK

*Personal Influence* (Katz and Lazarsfeld 1955/2006) put forward and tested a variety of hypotheses about how social contexts constrain media effects. Five such hypotheses are described: three about interactions of media exposure with social context (Stability, Conformity, and Instrumental) and two about two-step flow effects (Relay and Message Interpretation). Each is tested here with data from the evaluation of the National Youth Anti-Drug Media Campaign. The evaluation of the campaign has suggested boomerang outcomes—more exposure to the campaign led to more interest in marijuana use. This article examined whether those effects were magnified through interactions with siblings. In general, no evidence showed that older siblings' beliefs or behavior interacted with younger siblings' exposure to campaign messages in producing effects. However, evidence showed that the two-step flow did operate: older siblings were themselves affected by their own exposure to the campaign and, in turn, affected the beliefs and behaviors of their younger brothers and sisters.

*Keywords:* mass media; personal influence; drug use; two-step flow; sibling; antidrug campaign

*Personal Influence* (Katz and Lazarsfeld 1955/2006) outlines a set of hypotheses about how it is that the social context of an individual will affect the persuasive impact of mass media exposure. The first sections of that book describe the various and distinct mechanisms through which the social context might work. This article takes that list of mechanisms and

*Robert Hornik is Wilbur Schramm Professor of Communication and Health Policy at the Annenberg School for Communication, University of Pennsylvania. He has evaluated more than twenty-five large-scale public health communication programs including the National Youth Anti-Drug Media Campaign, and directs the Center of Excellence in Cancer Communication Research at Penn. He is author of Development Communication and editor of Public Health Communication: Evidence for Behavior Change.*

NOTE: I am grateful to Susana Ramirez and Shawnika Hull for help in preparing this article.

DOI: 10.1177/0002716206291972

applies it explicitly to data collected for the evaluation of the National Youth Anti-Drug Media Campaign. That evaluation (Hornik et al. 2003) concluded that the campaign, which cost the U.S. government more than \$1 billion, appeared to produce a boomerang effect; the more young people were exposed to the campaign advertisements, the more likely they were to report promarijuana cognitions and also to initiate marijuana use. This chapter begins with that result. It then elaborates the result following the hypothesized mechanisms of *Personal Influence*.

*Personal Influence* argues that the social context matters for media effects in a variety of distinct ways. It is useful to divide these hypotheses into two broad categories: hypotheses that suggest that the effects of individual exposure to messages will be contingent on their social context and hypotheses that suggest a two-step flow more directly: the campaign will affect the social influencers who will directly focus the individuals.

Social context moderates the effects of individual campaign exposure:

1. The stability hypothesis: The social network around an individual can be more or less consistent with regard to a particular issue or it can be more or less stable in general; insofar as the network is inconsistent or unstable, media effects from individual exposure to messages should be larger.
2. The conformity hypothesis: The social network around an individual provides its own persuasive influence; insofar as the persuasive ideas in media messages are accepted by the social network, those messages will be more persuasive; insofar as they contradict that network's opinions, they will be less persuasive. (Conformity may not be the best term for this influence; it is not the only mechanism through which social network opinions constrain media influence. For example, there may also be greater credibility associated with personally communicated messages.)
3. The instrumental support hypothesis: The social network provides more or less instrumental support for behavior; insofar as it provides material support for behaviors advocated in media messages, those messages will produce more behavior change.

Campaigns affect social influencers who, in turn, affect individuals:

4. The message interpretation hypothesis: Social networks provide meaning as people make sense of media messages. An individual's interpretation of media messages (and thus, presumably, their effects) will be affected by the way that social networks interpret those messages.
5. The relay hypothesis: Social influencers may be directly exposed to media messages. Network members may be persuaded by their own exposure and in turn persuade others in the network. This relay function of social networks is closest to the term most often associated with the argument of *Personal Influence*—the two-step flow.

In addition to these specific hypotheses about direct social network effects, the volume addresses the structure of the social networks themselves: their size; the strength of affiliation; the relationships between the members; the operational style—for example, democratic versus authoritarian—the likelihood of overlapping memberships, which can create cross pressures. Differently structured networks would be expected to produce varying effects on media persuasiveness.

However, these important arguments are, in most cases, not so easily tested with the drug campaign data, so they are not further discussed.

These ideas are elegantly elaborated in the original volume. They were a counterweight to the prevailing assumption that the influence of media is individual and direct. They recognized that individuals were embedded in social networks, which both influenced individuals directly and moderated and mediated the influences of the media on individuals. That focus was consistent with the other studies coming from Columbia (cf. Wright 1989) and independently from the rural sociologists concerned with diffusion of innovation (cf. Rogers 2003; Ryan and Gross 1943) although these diffusionists only later dealt with the intersection of media and interpersonal networks. They opened a powerful theoretical tradition and one that continues to compete with and complement more individual psychological perspectives. However, as intriguing as the ideas were, the actual empirical work explicitly testing these hypotheses about the intersection of media and interpersonal networks has been rare. The original volume depended mostly on claims by interviewees of contact with their social networks and use of media, but not as much on direct evidence of influence, establishing the flow from mass media to putative influencers and then to influencees. There are mountains of research about the operations of social networks; there are mountains of research about the effects of media and about the influence of social context and social networks on the interpretation of media content. But even now there is much less research documenting either the mediating or moderating role of social networks in the flow of *specific* mass communication influence. Indeed, in the specific area of examination of this study, the idea that siblings affect the effects of media exposure, not a single parallel study was unearthed.

---

*[T]he actual empirical work explicitly testing  
[Personal Influence's] hypotheses about the  
intersection of media and interpersonal  
networks has been rare.*

---

## Literature

There are many studies that address and some that establish the likely influence of social networks on individual behaviors of all sorts (e.g., Allen et al. 2003). Some of the results to be reported below support such claims of social network

influence on youth marijuana cognitions and use. However, they are not relevant for this study unless they address media persuasion effects as well. The personal influence hypotheses deal with media effects in the context of social networks. The relevant studies must include measures of at least three constructs: exposure to mass mediated persuasive messages, cognitions or behaviors of the target audience, and information about the social context in which possible media effects might occur. The analyses will focus on marijuana use by young people and will be limited to the social context defined by siblings; the search for relevant studies focuses in similar domains. It turns out that the relevant literature, despite fifty years of *Personal Influence* sitting on our shelves, is not so large.

A first search in the literature could find no studies that focused on siblings and media message effects. Most of the somewhat on-topic studies deal with parental mediation of television effects. By and large, they focus on alternative ways that parents interact with their children concerning television content, and sometimes they try to extend that to understand how the different forms of mediation affect substance use or aggression or other behaviors. They do show that parent or peer interaction with a youth around media can be associated with some outcomes (cf. Austin, Pinkleton, and Fujioka 2000; Austin and Yin 2003; Ennett et al. 2001; Nathanson 1999, 2001). None directly consider evidence for the argument that the effects of exposure to specific messages from a campaign or on a specific topic are mediated through a social context.

These data, in contrast, lend themselves particularly well to examining the social network effects of parents and of siblings. The current article deals only with sibling influence. The data set permits systematic examination of older siblings (and parent influences, although they are not examined here) because it includes data gathered directly from them as well as from the younger siblings.

Ideally, one would want also to examine the influence of other adults, of other institutions with which youth affiliate, and particularly of friends and peers. In fact, there is also a good deal of information about friends and peers of target youth in the data set. However, that material relies on self-reports by youth of their perceptions of others around them. As indirect reports, they have strength because they may represent what the youth may perceive and thus may represent what is affecting them; as indirect reports, however, they also have a weakness: they do not permit separation of the actual beliefs and behavior of others from what a youth projects as the opinion or behavior of others given his or her own beliefs. Because of that uncertainty, and because of an interest in focusing this article, the analyses below are limited to tests of the personal influence hypotheses related to siblings.

## The Data

The data collected for the evaluation are from a representative national sample of around eight thousand youth aged nine to eighteen who were followed

for up to three years and four rounds of interviews or until they were age nineteen. Each household could contribute up to two youth respondents—hence the availability of sibling data. Each of those youth interviewees had a corresponding parent (typically the same individual) also interviewed four times over three years. There were about six thousand parents interviewed. Given that the evaluation had found evidence for a persuasive effect (albeit an undesirable one) in this data and direct measures of the youth and a sibling and a parent, this evaluation promises an unusual opportunity to examine social context effects. While the Decatur study did include some independently collected data from putative influencers as well as influencees, that data made up a small part of the study. Most claims in the Decatur study depended on self-reports of influence by the primary interviewees.

The sample for the current article was selected from the broader sample. It includes only youth drawn from households where two youth were interviewed. Each sibling pair was included if there were data from at least two rounds of interviews for the younger sibling and data from the first round from the older sibling. There were 1,769 sibling pairs with at least one set of such two-round data. Each two-round set of interviews is considered one lagged case. Thus, individual sibling pairs can contribute up to three cases to this sample—if a youth was interviewed for four rounds of data and his or her sibling was interviewed for the first three rounds, the youth would contribute three lagged cases. In fact, the average sibling pair contributed 2.33 cases to the sample, with a total  $N$  of 4,130.

The overall sample for the evaluation was developed from a multistage cluster sample. In the overall evaluation, appropriate weights were developed to match the U.S. population, and analyses took direct account of the complex sample design in correcting standard errors. However, for this article such weights are not employed and standard error corrections are not introduced. Those were appropriately used for the full evaluation. However, given that the primary purpose of this analysis is to test theory rather than make claims about the national population, there is a justification for fully exploiting the available sample. Thus, the analyses that follow use the observed  $ns$  in statistical tests rather than the smaller effective sample size that would have been available after weighting and complex sample corrections. A separate sampling concern is raised by the inclusion of up to three cases per youth in each sample. There is some risk that claims of significance are exaggerated because the observed standard errors are smaller than the true standard errors. This bias may be present because some cases appear more than once in the data set, if a particular sibling pair is included at multiple ages. The statistical adjustments for nonindependence are not made formally here. However, the actual design effect associated with this repetition of cases for the four major outcome variables ranges from 1.16 to 1.39. Thus, the underestimation of standard errors is small, and it is unlikely that there are substantial effects on claims of significance. Still, this might mean that some claims of significant effects would be less strong if all appropriate corrections were included.

The  $ns$  in the different analyses vary sharply. In particular, the analyses using past year marijuana use and the nonintention to use marijuana tend to include

much of the sample, while the analyses that focus on marijuana social norms and attitudes-beliefs have smaller eligible samples. This is a reflection of the fact that the instruments used for youth younger than twelve were different from those used for older youth, and the “younger” instrument did not have identical measures of norms and attitudes to the “older” instrument. Many of the sibling pairs eligible for these analyses include youth who were younger than twelve when first measured, or even when measured for the first two rounds; they were not eligible for analyses requiring the social norm and attitude-belief baseline measures. This means that there were different *ns* for the four outcome variables with two implications: the power to find effects is less with the norm and attitude-belief outcomes; and that those analyses tended to include samples that were, on average, older than those in the past year or intentions analyses.

*Measures.* The primary outcome measures are past year marijuana use, intentions to not use marijuana in the next year, an index of normative beliefs about marijuana use, and an index of beliefs and attitudes about marijuana use assessed at the lagged round. The predictor variables include the same variables assessed at the prior round for the younger sibling and for the older sibling. In addition, there is a measure of exposure to the campaign, based on frequency of youth recall of specific television ads shown to them on laptop computers and recently broadcast by the campaign. Also, youth were asked to evaluate a subset of ads on three dimensions, and their average across all ads shown was considered the mean ad evaluation. One analysis makes use of a measure of perceived family togetherness reported by both siblings. Major control variables included parents’ reported level of education; youth age; gender; round of interview; and a risk of marijuana use score derived from a set of other variables shown to predict marijuana use, including youth prior tobacco and alcohol use; sensation seeking; urban residence; parent marijuana, tobacco, and alcohol use; religiosity; and whether the household included one versus two parents. (Measures are fully described in Hornik et al. 2003.)

*Analysis.* The boomerang effects have been previously established for the population of youth who were nonusers at the prior round of interviewing only. This was a focus of the evaluation on the grounds that prevention of initiation of use was a distinct goal for the national campaign. However, in the analyses presented here, we seek to exploit the full sample of youth. Rather than limiting the analysis to nonusing youth, we incorporate statistical controls for prior use in all appropriate analyses. However, the narrative begins with establishing that the previously reported boomerang effects continue to hold regardless of prior use status. However, this difference in approach, along with the less conservative statistical approach taken, leads to one important caveat: this analysis does not provide a substitute or replication of the formal campaign evaluation. It should not be used to make overall claims about the effects of the campaign on U.S. youth.

As already noted, most analyses address four outcome variables: marijuana use in the past year, definite nonintention to use marijuana in the next year, a scale

derived from multiple measures of antimarijuana beliefs and attitudes, and a scale derived from multiple measures of perceived social norms supporting nonuse of marijuana. Analyses with the first two measures use logistic regression, while those with the second two use ordinary least squares (OLS) regression. If there are promarijuana effects of media exposure, analysis will show a positive coefficient for the marijuana use measures but a negative coefficient for the other three outcomes.

Almost all analyses involve the use of the measure of exposure to specific television ads. This is a four-category ordered variable based on a continuous measure of exposure. However, we establish that the four-category measure has essentially a linear relationship with each of the outcomes, and so it is treated like an interval variable without transformation for all analyses.

Many of the analyses involve a test of the interaction between the exposure measure and some characteristic of the social context in their joint effects on an outcome variable. Simple multiplicative interaction terms are employed for this purpose, with statistical significance estimated on the basis of the additional variance accounted for by the interaction term.

The major threats to causal claims from observational studies are two: that an observed association is merely the result of the effects of some third variable on the two observed variables, or that the observed association is the result of reverse causation (not exposure causing marijuana use, but marijuana use causing recall of exposure). We address the first by implementing substantial statistical controls. Obviously, there may be other unmeasured variables that may still threaten claims of effects. We address the causal direction issue by focusing most analyses on lagged effects; the independent, control, and social context variables are assessed at the prior round and the outcome variables at the follow-up round. Most analyses exploit the two round nature of the data set.

All of the analyses focus on the influence of the older sibling on the younger sibling. It is, of course, possible that the influence flows the other way or is mutual. Also, given that there are sometimes more than two siblings in a household, but only two are included in the study, it is quite possible that a more complex dynamic in the family is lost. Nonetheless, to allow the analyses to proceed in a fairly straightforward manner, and because of the sensible idea that older siblings are influences on younger siblings, it was worth proceeding with this assumption. This leaves the opportunity for investigating more complex hypotheses to other investigators. The analyses are performed with SPSS.

## Analyses

### *The basic relationship of ad exposure and initiation of marijuana use*

Specific ad exposure at the prior round positively predicts use of marijuana by the subsequent measurement round. The effect is largely monotonic across the four categories of exposure and is statistically significant. A parallel relationship

(negative because of how the measures are ordered) is found when nonintention to use, and attitudes and beliefs, or social norms opposing marijuana use are the lagged outcome variables. The relationships are reduced when a variety of potential confounders are controlled, including parental education, youth age and gender, an overall estimate of risk for marijuana use, and round of baseline measurement. Table 1 presents the percentage using in past year, the proportion not declaring a firm nonintention to use, and the mean levels of antimarijuana beliefs and norms as a function of exposure category. The relative odds or unstandardized regression coefficient capturing the simple association and the associations after controls are also presented in the table. The apparent boomerang effects remain for marijuana use and social norms, are marginal for intentions ( $p = .054$ ), and are no longer significant for the attitude-belief index ( $p = .214$ ).

This analysis provides the foundation for the explicit tests of the social context hypotheses: is it the case that if we know something about the social context of these youth, we can establish the conditions under which these apparent effects have taken place, or, alternately, can we show that the effects reappear for the intention and attitude-belief measure under some social contingencies? The analysis begins with the hypotheses that suggest that the effects of exposure to the campaign are contingent on the social context.

### *The stability hypothesis*

The social network around an individual can be more or less stable or consistent; insofar as the network is unstable, media effects from individual exposure to messages should be larger. The network may be inconsistent because of disagreement about the specific issues at hand (marijuana use) or it may be unstable in a more general way—a family that is not “together.”

This hypothesis is tested with two approaches to measuring stability/inconsistency. The first focuses on evidence of discrepancy in cognitions about marijuana use between the target youth and his or her sibling. The degree to which they are different is assumed to represent greater inconsistency in the attitude environment. The absolute value of the discrepancy at the prior wave of measurement is used as the estimator of instability. Parallel inconsistency estimates are developed based on all four measures. The second approach relies on a direct measure of family stability—youth are asked to indicate their agreement with the statement, “There was a feeling of togetherness in our family.” We assume that youth who report lower levels of togetherness are indicating some family instability.

*Hypothesis 1a:* To the extent there is a discrepancy between youth and sibling in reported marijuana use, intentions, attitude-belief, and social norms, there will be a larger effect of campaign exposure on the lagged measure of the respective outcome.

The scores of the youth were subtracted from the scores of their siblings, and the absolute value was calculated on the observed difference. The test for the social context effect was whether there was additional variance accounted for in

TABLE 1  
MARIJUANA OUTCOMES AT FOLLOW-UP ROUND AS A FUNCTION OF SPECIFIC AD EXPOSURE  
AT PRIOR ROUND, WITH AND WITHOUT CONFOUNDER CONTROLS

Specific Ad Exposures Recalled per Month at Prior Round	Outcome Measures at Follow-Up Round			
	Percentage Using Marijuana in Past Year	Percentage Other Than "Definitely Not" on Intention to Use Marijuana	Mean Score on Antimarijuana Attitude- Belief Scale	Mean Score on Antimarijuana Social Norms Scale
Less than one time	3.4	89.6	109.1	119.6
One to three times	6.2	84.5	92.4	93.6
Four to eleven times	8.2	78.2	86.2	86.0
More than eleven times	9.1	78.7	88.1	84.5
Exposure effects				
Relative odds: per category (confidence interval [CI])	<b>1.46</b> <b>(1.26, 1.69)</b>	<b>0.74</b> <b>(0.68, 0.81)</b>		
Relative odds: with confounders controlled (CI)	<b>1.21</b> <b>(1.19, 1.65)</b>	<b>0.89</b> <b>(0.79, 1.00)</b>		
Unstandardized regression coefficient (CI)	—		<b>-6.9</b> <b>(-11.3, -2.5)</b>	<b>-12.0</b> <b>(-16.4, -7.7)</b>
Regression coefficient with confounders controlled (CI)	—		<b>-2.7</b> <b>(-6.8, 1.5)</b>	<b>-6.3</b> <b>(-10.4, -2.2)</b>
<i>n</i> for bivariate analysis	4,077	3,972	3,262	3,262
<i>n</i> for controlled analysis	3,529	2,915	2,819	2,819

NOTE: Controls include prior year marijuana use, age, risk score (and gender, parent education, race-ethnicity; measurement round). The last three were not significant and were removed from the final models. Significant results appear in bold.

TABLE 2  
 FOLLOW-UP ROUND ANTIMARIJUANA ATTITUDE-BELIEF INDEX  
 AS A FUNCTION OF PRIOR ROUND CONTROLS, EXPOSURE,  
 SIBLING-YOUTH DISCREPANCY ON THE ATTITUDE-BELIEF INDEX,  
 AND THEIR INTERACTION ( $n = 1,969$ )

	Constant	Age	Risk Score	Past Year Use	Attitude Discrepancy	Exposure	Exposure × Discrepancy
Mean		13.1	.04	.05	112.9	1.60	183.7
SD		0.97	0.07	.21	90.3	0.91	198.8
B. coefficient	<b>192.0</b>	<b>-5.7</b>	<b>-302.7</b>	<b>-92.0</b>	<b>-0.216</b>	<b>-11.0</b>	<b>0.103</b>
Confidence interval	<b>122, 261</b>	<b>-11.0, -0.4</b>	<b>-375, -230</b>	<b>-116, -68</b>	<b>-0.33, -0.10</b>	<b>-19.5, -2.6</b>	<b>0.04, 0.16</b>
<i>p</i> value	<b>&lt;.001</b>	<b>.034</b>	<b>&lt;.001</b>	<b>&lt;.001</b>	<b>&lt;.001</b>	<b>.011</b>	<b>.001</b>

NOTE: Significant results appear in bold.

the respective regression equation when the interaction of exposure and the discrepancy was added to an equation including control variables and main effects for exposure and the discrepancy.

In three of the four analyses (data not shown), there was no evidence for an interaction between the discrepancy score and the exposure measure in their joint effects on the respective outcome. In only one case was there an interaction, and it was somewhat difficult to interpret. In Table 1, it was noted that there was no significant association of exposure and the belief-attitude index when the control variables were introduced. However, when the discrepancy of the sibling and the youth was incorporated and the interaction calculated, both the main effect of exposure and the interaction become significant, as shown in Table 2.

The interaction is positive, suggesting that when the youth and sibling are more discrepant, the effects of exposure on the attitude-belief index are less negative. Specifically, among those with the least discrepant views, there is a significant bivariate negative relationship between exposure and antidrug beliefs/attitudes. Among the group with the highest discrepancy, this effect disappears and even turns significantly positive. This is not consistent with the social context hypothesis, which suggested that the smallest media exposure effects would be found among those with the smallest discrepancy.

*Hypothesis 1b:* Youth who come from a family perceived as feeling less together will show stronger media persuasive effects.

The second analysis done under the stability hypothesis uses the perception of the family as being together. This measure is scored 0 or 1 for both youth and sibling, with 1 given to youth who said that this was often or always true for their families (about 50 percent of youth said this). The average of the youth's and sibling's scores are used to characterize the family. Again, prediction of the four

outcomes was considered. None of the analyses with this measure showed a significant interaction of togetherness and exposure (data not shown).

Overall, of eight tests of the stability hypothesis (four with the discrepancy score and four with the togetherness score), only one showed evidence for an interaction with exposure, and that was in the wrong direction, with the youth with more consistency with siblings showing more effects of exposure.

### *The conformity hypothesis*

The social network around an individual provides its own persuasive influence; insofar as the persuasive ideas in media messages are accepted by the social network, they will be more persuasive; insofar as they contradict that network's opinions, they will be less persuasive.

The test of this hypothesis follows the same analytic approach as the stability analyses. It asks whether the effects of exposure are contingent on the cognitions or behavior of the sibling. Will a youth whose sibling expresses promarijuana views be more vulnerable to the promarijuana effects of the campaign than a youth whose sibling is less supportive of marijuana use?<sup>21</sup>

*Hypothesis 2:* The effect of direct exposure to persuasive messages is contingent on the nature of the siblings' attitudes—if they are supportive of the idea in the persuasive message, mass media messages will have a larger effect.

This hypothesis is tested by predicting each lagged youth outcome from prior round measures of controls, youth exposure and the older sibling scores on matched outcomes, as well as from the prior round interaction of youth exposure and sibling outcome measures. These analyses appear in Table 3.

None of the interaction terms in these four analyses were significant. There is no evidence consistent with the hypothesis that youth exposure effects are contingent on the siblings' prior round position on the outcome variables.

### *The instrumental support hypothesis*

Effects of the campaign on behavior may be contingent on opportunities to take action. If the social network provides an opportunity for action, mass media effects will be larger. An older sibling may not only provide supportive or non-supportive cognitive context for the younger sibling who is exposed to ad; he or she may provide concrete assistance to the younger sibling, making it easier to take an action consistent with a message.

*Hypothesis 3:* If a younger sibling has an older sibling who offers him or her marijuana, the promarijuana effects of the campaign on use will be larger.

This hypothesis turned out to be untestable with the sibling data.

Among the questions asked respondents was whether anyone had offered him or her marijuana and, if so, who made the offer. One of the options was a brother

TABLE 3  
 COEFFICIENTS FOR PREDICTION OF FOUR LAGGED YOUTH OUTCOMES FROM PRIOR  
 WAVE MEASURES OF EXPOSURE, SIBLING OUTCOME, AND THEIR INTERACTION,  
 ALONG WITH CONTROLS; ODDS RATIOS (MARIJUANA USE AND INTENTIONS)  
 OR UNSTANDARDIZED COEFFICIENTS (ATTITUDE-BELIEF AND SOCIAL NORMS) ARE REPORTED

	Constant	Age	Risk Score	Youth Past Year Use	Youth Prior Round Outcome	Sibling Prior Round Outcome	Exposure	Exposure × Sibling Prior Round Outcome
Marijuana use (relative odds)	<b>0.00</b>	<b>1.47</b>	<b>156.6</b>	<b>12.24</b>		<b>2.47</b>	1.13	1.13
Marijuana nonintention (relative odds)	<b>30.01</b>	<b>0.76</b>	<b>0.037</b>	<b>0.258</b>	<b>1.94</b>	0.247	<b>0.789</b>	1.25
Antimarijuana attitude-belief (unstandardized coefficient)	<b>45.1</b>	<b>-1.37</b>	<b>-179.9</b>	<b>-24.6</b>	<b>0.47</b>	<b>0.12</b>	2.42	0.002
Antimarijuana social norm (unstandardized coefficient)	<b>136.3</b>	<b>-8.3</b>	<b>-140.6</b>	<b>-24.1</b>	0.46	0.14	-1.22	0.001

NOTE: Significant results appear in bold. *ns* for the respective equations: use: 3,517; intentions: 2,907; attitude-belief: 1,969; social norms: 1,969. The *ns* are small for the attitude-belief and social norms analysis because many youth are missing for those variables on the prior round. These measures were only assessed for youth aged twelve to eighteen, and many of the younger siblings were younger than twelve the first time they were measured. A parallel analysis, eliminating the prior outcome measures for the youth for those two equations, moves the sample *ns* to about 2,800. The essential results, nonsignificant coefficients for the interaction terms, are replicated.

or sister. If there were enough siblings reported as offering marijuana, it would have been possible to test the interaction of sibling offers and campaign exposure. However only 1.3 percent of the respondents (53) claimed that a sibling offered marijuana. This was an insufficient number to permit testing whether the campaign had larger promarijuana effects if the sibling had offered marijuana. When youth reported that they had been offered marijuana, they almost always pointed to other youth, not family members, as the source.

The next two hypotheses turn from contingent effects of the social context to two-step flow effects: whether the campaign influenced the older sibling, who in turn influenced the younger sibling.

### *The relay (or two-step flow) hypothesis*

The relay hypothesis can be seen as a variant of the conformity hypothesis. Rather than focusing on whether the older siblings cognitions and behavior provide a context in which the younger siblings respond to the campaign, it asks whether the influence of the campaign on the older siblings is retransmitted to the younger siblings.

*Hypothesis 4:* The campaign affects the older siblings' marijuana cognitions and behavior, and the older siblings' cognitions and behaviors affect their younger brothers' or sisters' cognitions and behaviors.

The analysis for these effects require two stages: showing that the older siblings show the same effects as the target youth, as displayed in Table 1; and showing that the target youth lagged outcomes are affected by the older siblings' outcomes measured at the prior wave, controlling for the target youth outcomes at the prior wave (as well as the standard control variables).

The siblings analysis shows campaign effects substantially parallel to the target youth analysis in Table 1 (unsurprisingly). There are significant effects shown on lagged marijuana use (odds ratio [OR] = 1.17,  $p < .05$ ,  $n = 3,658$ ), on nonintentions (OR = 0.902,  $p < .05$ ,  $n = 3,644$ ), and on the social norms scale (coefficient = -4.7,  $p < .01$ ,  $n = 3,620$ ), but not on the attitude-belief scale (coefficient = -1.3,  $p = .44$ ,  $n = 3,620$ ), once control variables (age, risk score, prior round score on outcome variable, gender, and parent education) are entered. This satisfies stage 1 for testing this hypothesis.

Stage 2 requires showing an effect of prior round sibling scores on these cognitive and behavioral variables on lagged round scores of the target youth on parallel measures. To simplify this analysis, we examined whether the set of prior round sibling measures predicted each of the four outcomes, over and above the effects of control variables and the prior round measure of the target youth outcome.

There is evidence that older siblings' beliefs and behavior at the prior round are predictors for all four lagged youth outcomes. Table 4 displays results from each of the four equations. In the table, the individual significant coefficients are displayed, along with the information about the magnitude and significance of

TABLE 4  
 PREDICTION OF LAGGED YOUTH OUTCOMES FROM SIBLING  
 PRIOR ROUND OUTCOMES, CONTROLS, AND PRIOR  
 ROUND YOUTH OUTCOME

Predictor	Lagged Youth Outcomes			
	Past Year Use (Relative Odds)	Nonintention (Relative Odds)	Antimarijuana Attitude-Belief (Unst. Coeff.)	Antimarijuana Social Norms (Unst. Coeff.)
Youth variables				
Age	1.461	0.765	-1.22	-8.18
Risk score	209.7	0.029	-183.6	-141.2
Prior year use	12.38	0.237	-22.6	-20.5
Prior round score on outcome	—	6.61	0.471	0.457
Sibling prior round				
Use	1.882	—	—	-13.2
Nonintention	—	—	—	—
Antimarijuana attitude-belief	0.996	1.002	0.083	—
Antimarijuana social norms	—	1.002	0.067	0.122
R-squared or	30.0	28.7	28.5	27.0
Nagelkerke	chi-square =	chi-square =	$F = 194.2,$	$F = 180.1,$
R-squared— youth variables	47.57, $p < .001$	418, $p < .001$	$p < .001$	$p < .001$
Incremental	3.9	2.3	2.0	2.0
R-squared or	chi-square =	chi-square =	$F = 13.7,$	$F = 14.3,$
Nagelkerke	9.82, $p = .002$	8.42, $p = .004$	$p < .001$	$p < .001$
R-squared— sibling variables				
<i>n</i>	3,492	2,886	1,955	1,955

NOTE: Only significant results included.

the block of four older sibling predictors. The claim that the siblings' cognitions and behavior matter is based on the overall effect of all of the sibling measures from the prior round, captured in the incremental effects row in the table. The use of the overall effect reflects a judgment that the hypothesis of sibling influence on any one outcome is not limited to the sibling score on that outcome on the prior round; it is just as reasonable to argue that older sibling beliefs and attitudes affect the younger sibling's perceived social norms as that they influence the younger sibling's own belief-attitude score.

This version of the two-step flow hypothesis is then supported; we have evidence both that the campaign affected older siblings' behavior and cognitions

and that older sibling behavior and cognitions have an effect on their younger siblings' behaviors and cognitions. The effects are not large; they add about 2 to 4 percent in predictive power.

It is worth noting that while there is evidence for the two-step process outlined here, there is inadequate evidence for a direct effect of older sibling exposure on younger sibling lagged outcomes. There is a simple significant association of older sibling exposure and younger sibling lagged outcome. However, for each of the four lagged outcomes, when the control variables representing the youth's own characteristics (age, risk score, prior year use) are entered, any association of sibling exposure with youth lagged outcome is no longer significant. The product of the two effects (campaign on sibling outcomes; sibling outcomes on youth lagged outcomes) is not large enough to be detectable as a direct effect.

---

*[W]e have evidence both that the [National Youth Anti-Drug Media Campaign] affected older siblings' behavior and cognitions and that older sibling behavior and cognitions have an effect on their younger siblings' behaviors and cognitions.*

---

#### *The message interpretation hypothesis*

Social networks provide meanings for situations that do not explain themselves, including media messages. Those meanings influence how those messages are understood and thus what effects they are likely to have.

*Hypothesis 5:* Older siblings' interpretations of the ads affect their younger siblings' interpretations.

One of the ways that older siblings may affect younger siblings is by the way they interpret the advertisements broadcast by the campaign. The ads themselves are intended to convey various antidrug messages. However, given the evidence that they produce prodrug outcomes, it seems likely that the interpretation given to the ads is not the intended one. This raises the issue of where the counterinterpretation might come from. One possibility, consistent with the *Personal Influence* argument, is that it comes from the social context. In this case, this would mean that a youth's interpretation of the ads is influenced by the interpretations

offered by his or her older siblings. Since these data offer evidence about the evaluations of the ads offered by both members of sibling pairs, it is possible to test this hypothesis directly.

For as many as two ads that they had seen, youth were asked how they evaluated them on three criteria (there was a fourth, but it is not used for these analyses): whether they agreed or disagreed that this “ad got my attention,” “ad was convincing,” and “ad said something important to me.” The evaluation scores could vary from  $-2$  to  $+2$ , with  $+2$  equaling *strong agreement*,  $-2$  equaling *strong disagreement* and  $0$  equaling *neutrality* as to the value of an ad. The mean evaluation score among the younger siblings was  $0.93$ , while among older siblings it was a less positive  $0.78$ .

The test examined the association of the youths’ own lagged evaluation and their older siblings’ prior evaluation of the ads. The correlation was positive ( $r = .161$ ) and the beta remained positive ( $.094$ ) when the youth’s own prior year ad evaluation, age, risk score, and past year’s marijuana use were controlled. The unstandardized coefficient from the OLS multiple regression equation was  $.095$ —if the older sibling was one point higher on the evaluation scale, the younger sibling was about one-tenth of a point higher in the follow-up round. Interestingly, the lagged association was actually higher than the cross-sectional correlation between sibling and youth evaluations ( $r = .102$ ). This test of the social influence hypothesis is supported.

However, it is probably worth noting that the implied assumption of this analysis—that the effects of youth exposure are contingent on the ad evaluation—is not consistent with most of the evidence. The youth’s evaluation at the prior round does predict the outcomes at the lagged round even when the usual variables are controlled. However, the interaction of youth evaluation and exposure does not predict the lagged youth outcomes. Thus, while we can show that sibling evaluations affect youth evaluations, we cannot show that youth evaluations condition the effects of youth exposure on the outcomes. A possible explanation for that failure is presented in the discussion below.

## Discussion

The article presented two types of analysis to test the social context hypotheses described in *Personal Influence*: the first asked whether the social context affected individual exposure effects; the second asked whether the influence of the campaign on older siblings was passed through to younger siblings. In general, the evidence drawn from the first set of analyses was not supportive; most tellingly, whatever the sibling’s beliefs and behavior were concerning marijuana, the apparent effects of the campaign did not vary significantly. The evidence drawn from the second set of hypotheses was supportive. Most clearly, older siblings appeared to both be affected by the campaign and in turn seemed to affect the behaviors and cognitions of their younger brothers and sisters. Also, their

evaluations of the campaign ads appeared to affect the younger siblings ad evaluations. However, these pass-through or two-step flow effects were not so strong that they provided a significant correlation between older sibling exposure and younger sibling outcomes, once control variables were included.

How are these results to be interpreted? First, why is it that the social context did not seem to condition the response to the campaign? There are three competing explanations: the first is that the hypothesis was wrong for sibling effects. In other words, the younger sibling interpreted the campaign independently of the elder's beliefs and behaviors. The second explanation is that the measures of sibling cognitions and behavior, which were used as surrogates for the actual context in which the youth received the campaign, did not capture that context. That is, what older siblings reported as their cognitions about marijuana use did not define the context that they provided their younger siblings as they received the campaign. The sibling context, for example, contained more complex messages—even if the older sibling was positive about marijuana use, he or she may have tried to hide that from the younger sibling. Or similarly, it might be that there were sibling effects but they require further digging down—seeing whether siblings who are particularly close in age or of the same gender, or those who talk a lot with one another, show these interactions, while others do not. These analyses do not separate these subgroups of siblings. They are possible with this data set, but have not yet been undertaken. The third explanation is that the statistical analysis was insufficiently sensitive to capture interactions. Although these analyses included relatively large samples, interactions are always hard to detect. In most cases, the observed interaction coefficients were in the predicted direction, but they were not sufficiently far from zero to be significant. This limitation would also constrain optimism about finding statistically significant interactions among subgroups of siblings, even if theory would argue for digging down further.

However, if the interactions did not show evidence consistent with the social context hypotheses, the two-step flow analyses did show such effects. These are statistically easier analyses and depend on finding main effects rather than interactions, and thus they have greater power to find effects. Also, because they separate the analyses into two stages—showing campaign effects on older siblings and then showing cross-sibling effects on cognitions and behavior—they have a simpler task. Hence, we are willing to claim a two-step flow on the basis that there was evidence for an effect at each stage even though we could not show a direct effect of older sibling exposure on younger sibling outcomes. The greater power to detect main effects and the simpler criterion for success may explain why the effects appear more readily.

Clearly, these analyses and the claims made here do have some limitations. The statistics were not conservative insofar as they do not account for the complex sample design and the nonindependence of the cases; also, while the control variables were important ones, it is certainly possible that there are other, not included variables that might explain the apparent effects. As tests of the theory

of *Personal Influence*, they are only a start, even for these data: analyses with parents, and with the information about peers, would enrich the relevance of this as a test. It would seem quite likely that the effects of campaign exposure would be different depending on what parents and peers were saying about it.

One more result, not detailed here, bears on this idea. The evaluation report, in finding the apparent boomerang effect, considered a variety of possible mechanisms. The most persuasive of them was that the ads contained a meta-message. While the individual ads all had an explicit antidrug argument, the exposure to a large number of such ads might have contained an opposite message—that drugs were a big problem and that everyone was using them. Thus, the meta-message might have led youth to believe that many of their peers were using marijuana and that this, in turn, led them to consider drug use as an option for themselves. There was evidence consistent with this path of effect: the more exposure to the ads, the more youth believed that other kids were using marijuana; the more they believed that other kids were using marijuana, the more likely they were to initiate use themselves. This is partly directly linked to the hypotheses of personal influence. But it is also a different version of the social influence hypothesis: social influence may not only be conditioned on the actual social context but also on the virtual social context captured through mediated experience.

## Note

1. Whereas for the stability analyses we focused on the absolute value of the discrepancy, ignoring the direction of the difference, for this analysis we use the actual sibling score at the prior wave on each of the four outcome variables. Given that older siblings on average tend to be more promarijuana than younger ones, the level of absolute discrepancy tends to correlate with having siblings with relative promarijuana cognitions and behavior. Thus, the tests with the absolute values of discrepancy and those with the observed value of the sibling's score are not independent in practice, even if they are independent in underlying logic.

## References

- Allen, Mike, William A. Donohue, Amy Griffin, Dan Ryan, and Monique Mitchell Turner. 2003. Comparing the influence of parents and peers on the choice to use drugs. *Criminal Justice & Behavior*, 30 (2): 163-86.
- Austin, Erica W., Bruce E. Pinkleton, and Yuki Fujioka. 2000. The role of interpretation processes and parental discussion in the media's effects on adolescents' use of alcohol. *Pediatrics* 105:343-49.
- Austin, Erica W., and J. Yin. 2003. The relationship of parental reinforcement of media messages to college students' alcohol-related behaviors. *Journal of Health Communication* 8:157-69.
- Ennett, Susan T., Karl E. Baumann, Vangie E. Foshee, Michael Pemberton, and Katherine A. Hicks. 2001. Parent-child communication about adolescent tobacco and alcohol use: What do parents say and does it affect youth behavior? *Journal of Marriage and Family* 63:48-62.
- Hornik, Robert, David Maklan, Diane Cadell, Carlin Barmada, Lela Jacobsohn, Vani Henderson, Anca Romantan, Jeffrey Niederdeppe, Robert Orwin, Sanjeev Sridharan, Robert Baskin, Adam Chu, Carol Morin, Kristie Taylor, and Diane Steele. 2003. *Evaluation of the National Youth Anti-Drug Media Campaign: 2003 report of findings*. Washington, DC: Westat.
- Katz, Elihu, and Paul Lazarsfeld. 1955/2006. *Personal influence: The part played by people in the flow of mass communication*. 2nd ed. New Brunswick, NJ: Transaction Publishers.

- Nathanson, Amy I. 1999. Identifying and explaining the relationship between parental mediation and children's aggression. *Communication Research* 26 (2): 124-43.
- . 2001. Parents vs. peers: Exploring the significance of peer mediation of antisocial television. *Communication Research* 28 (3): 215-74.
- Rogers, Everett. 2003. *Diffusion of innovations*. 5th ed. New York: Free Press.
- Ryan, Bryce, and Neal C. Gross. 1943. The diffusion of hybrid seed corn in two Iowa communities. *Rural Sociology* 8 (1): 15-24.
- Wright, Charles R. 1989. *Mass communication: A sociological perspective*. 3rd ed. New York: Random House.