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Talking about Quitting: Interpersonal Communication as a Mediator of Campaign Effects on Smokers' Quit Behaviors

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Abstract

This study examined the role of interpersonal communication in the context of a mass media anti-smoking campaign. Specifically, it explored whether conversations about campaign ads and/or about quitting mediated campaign exposure effects on two quitting behaviors (sought help to quit and tried to quit smoking completely), as well as the relationship between ad-related and quitting-related conversations. Data were collected prior to the campaign and monthly for 16 months during the campaign through cross-sectional telephone surveys among a sample of 3277 adult Philadelphian smokers. Follow-up interviews were conducted among 877 participants three months after their first survey. Cross-sectional and longitudinal mediation models with bootstrap procedures assessed the indirect effects of campaign exposure on outcomes through conversations, and of conversations about ads on outcomes through conversations about quitting. In addition, lagged regression analyses tested the causal direction of associations between the variables of interest. The results partially support hypotheses that conversations about quitting mediate campaign effects on quitting-related behaviors, and, in line with previous research, that conversations about the ads have indirect effects on quitting-related behaviors by triggering conversations about quitting. These findings demonstrate the importance of considering interpersonal communication as a route of campaign exposure effects when evaluating and designing future public health campaigns.

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Keywords

Interpersonal communication; Mass media campaign; Smoking cessation; Antismoking campaign; Health communication

Tobacco use is the leading cause of preventable death in the United States, being responsible for 1 in every 5 deaths per year (U.S. Department of Health and Human Services, 2012). There have been numerous efforts, especially in the form of mass media campaigns, to prevent smoking initiation and encourage smoking cessation. As a result, there are now several reviews on the effectiveness of anti-smoking mass media campaigns (Durkin, Brennan, & Wakefield, 2012; National Cancer Institute, 2008; U.S. Department of Health and Human Services, 2012). Overall, studies have found that media campaigns have the potential to effectively change smoking-related outcomes, including attitudes, intentions, and actual behavior. Though there is need for more research into the mechanisms behind campaign effects, mass media campaigns are broadly thought to have both direct and indirect effects on behavior (Wakefield, Loken, & Hornik, 2010). Direct effects include possible influences on beliefs, perceived norms, self-efficacy and skills. Indirect effects of mass media campaigns include producing heightened public discussion and subsequent changes at the policy level, or encouraging increased interpersonal discussion within social networks that could potentially retransmit the message to unexposed individuals or reinforce the message for those who were directly exposed — all of which can affect an individual's decision to change behavior. In line with the latter, a particular focus of recent studies has been on the indirect effects through interpersonal communication (e.g., Dunlop, 2011; Dunlop, Wakefield, & Kashima, 2008; Durkin & Wakefield, 2006; Hwang, 2012; van den Putte, Yzer, Southwell, de Bruijn, & Willemsen, 2011). The aim of this study is to examine the role of interpersonal communication in the context of an anti-smoking campaign, and to provide further empirical support for a proposition that has only recently begun to gather evidence in the literature.

Relationship between Mass Media and Interpersonal Communication

From Katz and Lazarsfeld's two-step flow model (1955) to Rogers' diffusion of innovations theory (1962), communication scholars have long acknowledged the linked influence of mass media and social networks, and there are several explanations as to how that linked influence operates (Southwell & Yzer, 2007). Some have suggested that the two channels work independently to bring about effects in stages over time, such that mass media may be more suited to raise awareness of an issue, while interpersonal communication has greater potential to change attitudes toward a health behavior and ultimately, the actual behavior (Rogers, 2003; Schuster et al., 2006). Others have posited that the two channels are more interdependent. According to one explanation, social influencers who are directly exposed to mass media content could relay the information to individuals who are otherwise unexposed (Hornik, 2006; Hornik & Yanovitzky, 2003; Katz & Lazarsfeld, 1955). According to another explanation, mass media content and interpersonal communication could reinforce each other (Hornik, 1989; Rogers, 2003); for example, after individuals are directly exposed to a media message, interpersonal communication could lead to a discovery of social norms

surrounding that message, affecting how they interpret or act upon the message (Hornik, 2006; Hornik & Yanovitzky, 2003).

In line with these various explanations, recent research has explored a number of potential mechanisms that might explain the relationship between mass media and interpersonal communication. Some studies have placed interpersonal communication in the role of a moderator of mass media campaign effects, examining the potential amplifying or dampening effects of conversations on desired health outcomes (e.g., Dunlop, 2011; Durkin & Wakefield, 2006; Lee, 2009). Other studies have placed interpersonal communication in the role of a mediator of mass media campaign effects, examining the possibility that campaigns affect interpersonal communication, which then influence health outcomes (e.g., Hafstad & Aarø, 1997; Hwang, 2012; Schuster et al., 2006; van den Putte et al., 2011). Going one step further, some studies have begun examining the mediating role of interpersonal communication depending on the content of those conversations: whether they were about the campaign ads or about quitting smoking in general (Hendriks, van den Putte, de Bruijn, & de Vreese, 2014; van den Putte et al., 2011). In a recent longitudinal study examining the effects of an anti-smoking campaign implemented in The Netherlands, van den Putte and colleagues (2011) found that campaign exposure had an indirect effect on smokers' quit behaviors via conversations. More importantly, they also found that while conversations about quitting directly led to behavior change, conversations about the ads only had an indirect effect on quitting behavior by increasing the likelihood of conversations about quitting. In other words, conversations about quitting mediated the effects of conversations about the campaign ads on quitting behaviors.

The Present Study

As outlined above, there are multiple plausible explanations as to how mass media and interpersonal communication are linked in bringing about effects. The study presented here examines just one particular subset of these ideas: namely, the proposition that interpersonal communication mediates the effects of campaign exposure on behavior and, in line with the findings of van den Putte et al. (2011), the mediation process might vary depending on whether the conversations are about the campaign ads or about quitting in general.

The present study aims to move the empirical literature in this area forward in two ways. First, it moves away from simply inferring mediation from the positive effects of exposure on conversations, and that of conversations on health behavior; rather, it uses these analyses in conjunction with more formal mediation tests. Second, this study tests the mediation model in a unique campaign context, with a distinct target population and target behaviors specific to this particular campaign, thus extending the theory into additional contexts in an attempt to further validate the notion that conversations could mediate campaign effects on health behavior.

Specifically, this study attempts to test the mediating role of interpersonal communication in the context of a citywide anti-smoking mass media campaign in Philadelphia. Funded by the U.S. Centers for Disease Control and Prevention, the Philadelphia Department of Public Health launched this campaign aimed at increasing use of help when trying to quit, and

ultimately successful cessation, among Philadelphian adult smokers. It ran for 16 months (December 2010 through March 2012) on television, radio, and in print (i.e., ads displayed on buses, subway cars, and convenience stores). The campaign aired concurrently to other components of the overall intervention, including free nicotine replacement therapy giveaways. Evaluation of the campaign did not detect population effects, but did find that smokers reporting four exposures per week at first interview were 5% more likely than those with no exposure to seek help to quit, and 4% more likely to make a quit attempt (Gibson, Parvanta, Jeong, & Hornik, 2014). Recognizing that conversations related to a media campaign may influence its effectiveness (Hornik & Yanovitzky, 2003; Southwell & Yzer, 2007), the present study tests if conversations mediate the exposure-behavior associations observed in response to the Philadelphia "quit with help" campaign.

Hypotheses

The present study tested the role of interpersonal communication in campaign effects in three separate sets of hypotheses (see Figure 1 for the full model of proposed mediation pathways). We first tested whether exposure to the "quit with help" campaign generated conversations about the ads, and whether these conversations mediated the effects of exposure on the behavioral outcomes (see Figure 2, Hypothesis 1).

> Hypotheses 1a–b: Interpersonal communication about *campaign ads* mediates the relationship between exposure and (a) seeking help to quit and (b) making a quit attempt.

We then tested whether exposure to the campaign generated conversations about quitting smoking, and whether those conversations then led to the same two behavioral outcomes (see Figure 2, Hypothesis 2).

> Hypotheses 2a–b: Interpersonal communication about *quitting smoking* mediates the relationship between exposure and (a) seeking help to quit and (b) making a quit attempt.

The next set of hypotheses was motivated by van den Putte et al.'s findings (2011), testing whether the mediating effects of talking about campaign ads could be explained through an association between ad- and quitting-related talk, such that conversations about the ads naturally lead to conversations about the general topic of quitting smoking, which then lead to the two behavioral outcomes (see Figure 2, Hypothesis 3).

> Hypotheses 3a–b: Interpersonal communication about *quitting smoking* mediates the relationship between interpersonal communication about the *campaign ads* and (a) seeking help to quit and (b) making a quit attempt.

Method

Participants

This study used data drawn from a sample comprising current adult smokers in Philadelphia, originally collected to evaluate the "quit with help" campaign. Social Science Research Solutions (SSRS) recruited and interviewed participants who were either current smokers or

recent quitters via random digit dialing (RDD) of landline phones before the campaign began (in November/December 2010; n = 498) and for 16 months over the duration of the campaign (until March 2012; total campaign n = 2856). Recent quitters made up only 2% (n = 77) of the recruited sample and were removed from the analytic sample. The American Association of Public Opinion Research response rate 3 for the cross-sectional interviews was estimated at 27%. SSRS also re-contacted participants who completed their initial survey in months of high campaign activity (January-March and July-December 2011) for a three-month follow-up interview. Of the 1785 respondents eligible for follow-up, 877 (49%) were successfully interviewed. Sample demographics for the cross-sectional and longitudinal samples are shown in Table 1. There was little difference in demographic characteristics between the cross-sectional and longitudinal samples. Comparison of the current sample with population estimates of Philadelphia's smoker population is available elsewhere (see Gibson et al., 2014, Table 1).

Measures

Outcomes—We measured the two behavioral outcomes at two time points: in the past month (for all cross-sectional analyses) and in the past three months (for all longitudinal analyses). Of the two outcomes, we measured one directly: *tried to quit smoking completely* (*yes/no*). The second outcome variable, *sought help to quit*, was created by combining six individual measures that asked respondents if they had used the following six methods to help them quit: (1) seeking advice from a doctor/ another health care provider; (2) calling the telephone quitline (1–800-Quit-NOW); (3) going to the website (SmokeFreePhilly.org); (4) going to any [smoking cessation] programs; (5) using any nicotine replacement therapy; and (6) using any of the following prescription medications: Zyban, Wellbutrin, or Chantix. If a respondent used any of these methods, he/she was coded as having sought help to quit. Although these questions asked about seeking help to quit, they were asked of all respondents, irrespective of whether they had made a recent quit attempt. Therefore, it is important to note that seeking help did not necessarily indicate that the respondent had attempted to quit. In fact, of the 982 respondents who reported that they had sought help to quit in the past three months, only 55% also reported that they had made a quit attempt. In other words, some of the cessation aid behaviors such as going to a doctor or calling the quitline may happen independently of following through with an actual quit attempt — a surprising, yet important distinction.

Campaign exposure—The exposure measure used here was identical to that used in the main evaluation study (see Gibson et al., 2014). We measured campaign exposure using aided recall, which meant participants were given a brief description of each of the eight different television, radio, and print ads that comprised the campaign before being asked if they recalled hearing/seeing each ad during the past month. If they recalled hearing/seeing any ad, they were also asked how often they remembered hearing/seeing it. As it was done for Gibson et al. (2014), the frequency of exposure measure was recoded into numerically meaningful categories (*never=0; less than once a week=0.5; about once a week=1; several times a week=4; about every day=7*) for each ad.

When calculating total campaign recall, only recall for the two television ads (one aired on cable television for 27 weeks and the other on broadcast television for 10 weeks) and one radio ad (on air for 24 weeks) were considered. Of the eight campaign ads, only these three had more reported exposure during the campaign than before the campaign launch. Recall estimates for the other five ads had too much over-reporting at baseline to be considered credible (Gibson et al., 2014). Therefore, combining the individual exposure measures for these three ads led to a final continuous measure of exposure ranging from 0–21 exposures per week.

Interpersonal communication—We assessed interpersonal communication using two separate measures: (a) talked about any of the ads with anyone (*yes/no*); and (b) talked to a close other about quitting in the last one month (*yes/no*). The two measures had different numbers of responses because the *talked about ads* measure was asked only of respondents who recalled seeing at least one of the three ads that were used in the exposure measure in the past month (allowing us to infer that if they talked about the ad, it was also in the past month). Respondents who reported no campaign exposure or exposure only to the ads that weren't included in the final exposure measure were coded as missing. In contrast, the *talked about quitting* measure was asked of all respondents.

Confounders—All models were adjusted for potential confounders, including demographic variables (gender, race (*non-Hispanic white; non-Hispanic black; Hispanic; other*), age, education in years, marital status, children in household, home ownership), religious attendance, Medicaid insurance, perceived health status, health orientation, variables specific to smoking behavior (number of cigarettes smoked per day in the last week, percent of other people in the household who smoke, time to first cigarette in the morning), whether the survey occurred during the first two months of the nicotine replacement therapy give-away (coded as a dummy variable), and the first month in which each ad aired (coded as a dummy variable) in order to account for potential novelty effects (Dunlop, Cotter, Perez, & Wakefield, 2013).

Data Analyses

Cross-sectional mediation analyses—We tested each of the proposed pathways in Hypothesis 1–3 using three complementary analytic approaches. First, we tested whether mediation occurred at the cross-sectional level; that is, whether interpersonal communication about the ads and/or about quitting mediated the association between exposure and each of the two behavioral outcomes (Hypotheses 1–2), and whether interpersonal communication about quitting mediated the association between interpersonal communication about the ads and each of the two behavioral outcomes (Hypothesis 3), when all variables were measured at first interview. This step was carried out by running mediation models with bootstrapping procedures (with 500 replications). Bootstrapping is a method that has support in the statistical literature because it has high power and eliminates the assumption of a normal sampling distribution (Hayes, 2009; Hayes, Preacher, & Myers, 2011). To assess whether the indirect effects of campaign exposure on behavioral outcomes through interpersonal communication were nonzero, bias-corrected confidence intervals were constructed using bootstrapping.

Testing temporal order—It is difficult to establish the causal order of exposure, interpersonal communication and outcome(s) in a cross-sectional model that consists only of variables collected at first interview. Therefore, in order to more precisely establish the causal order of effects that would be further explored in the subsequent longitudinal mediation analyses, our second set of analyses tested pairs of variables in the mediation model with logistic regression, using two waves of panel data. First, we tested whether campaign exposure at first interview predicted behavioral outcomes measured at three-month follow-up interview (as reported in the main campaign evaluation; Gibson et al., 2014), adjusting for the corresponding behavior measured at first interview. Next, we tested whether campaign exposure at first interview predicted interpersonal communication measured at follow-up, adjusting for prior interpersonal communication. Third, we tested whether interpersonal communication at first interview predicted behavioral outcomes measured at follow-up, adjusting for the corresponding prior behavior. Lastly, we tested whether interpersonal communication about the ads at first interview predicted interpersonal communication about quitting at follow-up, adjusting for prior interpersonal communication about quitting.

Longitudinal mediation analyses—Lastly, in a full mediation model that employed two waves of data, we tested whether the causal order continued to hold true *only for the variables that showed significant lagged relationships in the predicted causal order* in the logistic regression analyses above. Again applying bootstrapping procedures, we tested whether mediation occurred at the longitudinal level, using exposure and interpersonal communication variables measured at first interview and behavioral outcomes measured at follow-up, adjusting for prior behaviors.

Sensitivity analyses—Furthermore, we recognized the possibility that all of the observed relationships between pairs of variables could potentially operate in the reverse direction. Therefore, we conducted reverse lagged regression analyses to test whether the causal direction of the effects ran both ways or only in the proposed direction. In particular, we tested whether there was evidence that interpersonal communication about *quitting* predicted interpersonal communication about the *campaign ads*.

Listwise deletion was used to deal with values missing on any variables in all analyses. This was deemed appropriate as it resulted in less than 11% of the cases being dropped. All tests were run on Stata 12.0 (StataCorp, 2011).

Results

Descriptive Data

Table 1 also shows descriptive statistics for the exposure, interpersonal communication, and quitting behavior variables, while Table 2 shows their zero-order correlations. Nearly all of these variables were significantly correlated at the bivariate level.

Testing Hypothesis 1

Hypothesis 1 predicted that interpersonal communication about campaign ads would mediate the relationship between campaign exposure and (a) seeking help to quit and (b) making a quit attempt. At the cross-sectional level, talk about the ads was a significant mediator between exposure to the campaign and both seeking help and quit attempts (supporting Hypothesis 1a–b) (Table 3). When we tested the temporal order of the variables that made up Hypothesis 1, we found that exposure did not predict talking about the ads at follow-up and talking about the ads at first interview did not predict seeking help or quit attempts at follow-up (failing to support Hypotheses 1a–b); however, exposure at first interview did significantly predict seeking help at follow-up (Table 4). Because talking about the ads was not significantly associated with either exposure or the behavioral outcomes, we did not test Hypotheses 1a–b using longitudinal mediation analyses. Overall, Hypothesis 1 was not supported.

Testing Hypothesis 2

Hypothesis 2 predicted that interpersonal communication about quitting would mediate the relationship between exposure and (a) seeking help to quit and (b) making a quit attempt. At the cross-sectional level, talk about quitting significantly mediated the association between exposure to the campaign and both seeking help and quit attempts (supporting Hypotheses 2a–b) (Table 3). When we tested the temporal order of the variables that made up Hypothesis 2, we found that 1) exposure at first interview significantly predicted talking about quitting at follow-up, 2) talking about quitting at first interview significantly predicted both seeking help and quit attempts at follow-up, and 3) exposure at first interview significantly predicted seeking help at follow-up but not quit attempts at follow-up (Table 4). However, the longitudinal mediation analyses failed to show any significant indirect effects for the two pathways proposed in Hypothesis 2, although both pathways trended towards nonzero indirect effects (Table 5). In sum, Hypothesis 2 was partially supported by the cross-sectional mediation analyses and the analyses testing temporal order.

Testing Hypothesis 3

Hypothesis 3 predicted that interpersonal communication about quitting would mediate the relationship between interpersonal communication about the campaign ads and (a) seeking help to quit and (b) making a quit attempt. At the cross-sectional level, talk about quitting significantly mediated the association between talk about the ads and both seeking help and quit attempts (supporting Hypotheses 3a–b) (Table 3). When we tested the temporal order of the variables that made up Hypothesis 3, we found that talking about the ads at first interview significantly predicted talking about quitting at follow-up, and that talking about quitting at first interview significantly predicted both seeking help and quit attempts at follow-up (supporting Hypothesis 3) (Table 4). The longitudinal mediation analyses showed that there were significant indirect effects for both pathways: talk about quitting at first interview mediated the relationship between talk about ads at first interview and 1) seeking help at follow-up and 2) quit attempts at follow-up (Table 5). Hypothesis 3 was fully supported by all three analytic steps.

Sensitivity analyses

Testing the reverse pathway between the two interpersonal communication variables showed that talking about quitting at first interview predicted talking about the ads at follow-up (*OR* = 1.77, *p* = .014).

Discussion

Consistent with previous studies (e.g., Schuster et al., 2006; van den Putte et al., 2011), the current study demonstrated that interpersonal communication can mediate the relationship between campaign exposure and smoking cessation-related behaviors. In particular, the findings of this study partially supported the hypothesis that conversations about quitting smoking mediate the relationship between campaign exposure and smoking cessation-related behaviors (i.e., Hypothesis 2). Not only did the cross-sectional mediation analyses show support for this indirect effect, but the analyses examining temporal order found that exposure predicts conversations about quitting and that conversations about quitting predict both seeking help and actual quit attempts. These results are somewhat parallel to van den Putte et al.'s (2011) findings: they also found that conversations about quitting had a significant direct effect on quit attempts, and though they didn't find a significant effect of campaign exposure on conversations about quitting, the effect was in the predicted direction.

Furthermore, this study strongly supported the notion that the content of the conversation matters: consistent with the findings from van den Putte et al. (2011), we found that conversations about quitting smoking mediated the relationship between conversations about ads and smoking cessation-related behaviors (i.e., Hypothesis 3). The analyses testing temporal order showed that while talking about the campaign ads did not significantly predict seeking help to quit and quit attempts three months later, it did predict talking about quitting three months later. In turn, talking about quitting predicted both behaviors three months later. Parallel to these results, both the cross-sectional and longitudinal mediation tests showed that talk about quitting mediated the association between talking about ads and both seeking help and quit attempts (at both first-interview and at follow-up). These findings suggest that campaign exposure can lead to conversations about the ad content but that these conversations may not be sufficient to lead to quitting-related behaviors, unless they are followed by or include additional talk about smoking cessation. Given that exposure predicts talk about quitting three months later but not talk about ads, it may be that talk about ads lasts for a relatively short time compared to talk about actual quitting, and ceases before having any direct effect on behavioral change. Nevertheless, the results show that ad-related conversations play a role in leading to quitting-related conversations, which can in turn impact actual behavioral change.

It is worth noting that the lack of convergence across the three analytic steps does not undermine any of the proposed pathways. In fact, the use of multiple steps to test our hypotheses allowed us to further explore the mediation hypothesis for relationships that did not hold true in a particular model. For instance, results from testing temporal order showed exposure was associated with a greater likelihood of having talked about quitting three months later, and talking about quitting was associated with both behaviors three months later, but the longitudinal mediation results did not show evidence for any of the pathways

involving talking about quitting as a mediator of campaign effects on behavior. One potential reason for this is that talking about quitting may not be an immediate outcome of campaign exposure, but rather may take some time to manifest. The longitudinal mediation models measured exposure and talk about quitting at first interview, and only the behaviors at follow-up, so it is possible that campaign effects on talk about quitting at a later time point would only appear in the tests of temporal order.

While our analyses examining temporal order found that talking about ads predicted later talking about quitting (as hypothesized), our sensitivity analyses showed that the reverse effect was also significant: that talking about quitting also predicted later talking about ads. In reality, it is possible to conceptualize talking about the ads and talking about quitting as causing each other and happening concurrently, rather than being unidirectional. But given that we also found a significant effect in the hypothesized direction, this finding does not necessarily refute our claim that talking about ads leads to talking about quitting.

One thing to keep in mind when interpreting these results is that the indirect effect sizes are small, albeit significant and in the expected direction. This was not unexpected, as the total effect sizes of mass media campaigns are typically small — especially for smoking cessation behaviors (Snyder et al., 2004). Furthermore, this study does not suggest that interpersonal communication fully mediates the effects of mass media campaigns on quitting behaviors. In other words, there are other processes that may also account for campaign effects, such as improvements in self-efficacy or increased awareness of the negative consequences of smoking and subsequent perceptions of one's susceptibility to these consequences. Though it is beyond the scope of this current study to explore these other mechanisms, they are worth addressing in future research.

Admittedly, there are multiple routes through which the observed mediation effects of interpersonal communication might come about, including whether the conversations were important because they led to changes in relevant beliefs or normative expectations, transmitted information about logistical steps to successful quitting, or influenced perceptions of self-efficacy for quitting. While it wasn't the objective of this study to examine all of the potential mechanisms through which conversations could mediate campaign effects, the current findings do make it clear that interpersonal conversations play an important role in determining the effectiveness of health promoting mass media campaigns.

The use of longitudinal data for most of our analyses is a major strength of our study. We recognize that cross-sectional analyses limit claims of causal order in mediation analyses; therefore, we relied on the findings from our lagged logistic regression analyses to test temporal order and longitudinal mediation models for stronger evidence supporting the proposed pathway of effects. This study can be read, in part, as a replication of the van de Putte et al. (2011) study, establishing that its findings held up with a different campaign addressing a different behavior in a quite different context with a different research design.

Limitations and Directions for Future Research

As with all studies that rely on self-reported measures, the actual amount of campaign exposure, interpersonal communication, and cessation-related behavior may be subject to recall bias. In an effort to ameliorate such effects, we used measures that included time-frame references (to lessen potential memory confusion) and our final exposure measure only included those respondents who recalled seeing one of the three ads for which we observed higher levels of recall once they had been on air than at baseline (i.e., increasing our confidence that there was true rather than misremembered recall). Relatedly, because the interpersonal communication and behavioral outcome measures were asked in reference to the past month or three months, we had more confidence that respondents could provide yes/no answers than that they could provide detailed frequency information, so we relied on simple dichotomous measures. Although these dichotomous measures restricted our ability to explore certain questions (e.g., whether participating in multiple conversations about the campaign or about smoking cessation would yield different effects on behavior change than simply having one conversation), they were potentially less vulnerable to recall bias.

Moreover, our survey did not differentiate whether the study participants disseminated the campaign message to others (i.e., respondent talking to someone about campaign ads) versus received information from others (i.e., someone talked to the respondent about the campaign ads). The possibility that people could either be the initiators or the recipients of campaign-related conversations suggests the need for future campaign evaluations to be more precise in differentiating between the two different ways in which conversation participation comes about. Respondents who reported zero campaign exposure were not given the chance to report any interpersonal communication about the ads, leading to potential underestimation of conversations triggered by someone else talking to the respondents about the ads. This also led to a systematic underestimation of the effects reported here; because respondents who reported no campaign exposure could not report talk about the ads, they were made missing for all analyses, but this meant a restriction in variance for both variables and thus an almost inevitable underestimation of their association with each other and with other variables.

The original response rate of 27%, and follow-up rate of 49%, are consistent with recruitment rates of other RDD samples for this population. However, there is some risk that these response rates produce some bias in the included sample. Another limitation was the use of a landline-only sample. At the time of data collection, the estimated proportion of Philadelphian smokers who only used cell phones was 18%. We accepted the tradeoff between some bias in eligibility and the increase in sample size that could be afforded with a landline-only sample.

Future studies would benefit from additional elaboration of other related issues. Southwell and Yzer (2007) express concern with measures of interpersonal communication that assess frequency but do not assess the nature of the conversations in terms of what exactly was being said, its emotional tone, and who was involved. That is a weakness of this study as well, reflecting the need to trade off assessment of conversation detail with a primary focus on assessing campaign outcomes on a large number of smokers over time. Thus, future research may wish to further explore how the characteristics of the people participating in

the conversations and the nature of the conversations could impact the effects of those conversations. Interpersonal communication can lead to undermining or heightened effects, contingent on the characteristics of the conversation (e.g., David, Cappella, & Fishbein, 2006; Hendriks et al., 2014). For example, depending on whether a conversation participant is low or high on the quitting contemplation ladder, he or she may transmit undermining or supportive interpersonal communication. Interpersonal communication can also have different effects depending on the people participating in the conversations, such as whether one has positive or negative responses to the campaign itself (Dunlop, 2011), and how motivated the conversation participants are to conform to the recommendations or opinions of their conversation partner (Fishbein & Ajzen, 1975). Following studies that have found the effectiveness of highly emotional messages (Dunlop, Cotter, & Perez, 2014) and loss-framed messages (Wong, Harvell, & Harrison, 2013) in stimulating interpersonal communication among the family and friends of smokers, future research studies should also explore other message themes that may provoke positive conversations about smoking cessation.

Conclusion

By demonstrating that the effectiveness of anti-smoking campaigns is at least partially explained by the conversations that smokers have after exposure, this study has several important implications for campaign evaluation. Campaign planners and evaluators should expect that campaign effects will be mediated through interpersonal processes, and at minimum, should incorporate measures of interpersonal communication into their evaluation designs; otherwise they risk underestimating campaign effects. Furthermore, it may be worth measuring not just whether conversations occurred, but also the actual content of the conversations, how the conversation was triggered, and the individual characteristics of the participants of those conversations. Future findings from both research and campaign evaluations may make a case for campaign developers to increasingly move beyond attending to the message characteristics that are most associated with direct persuasion, to also consider the features that most effectively stimulate interpersonal communication about either the specific ad and the target health behavior addressed by the campaign.

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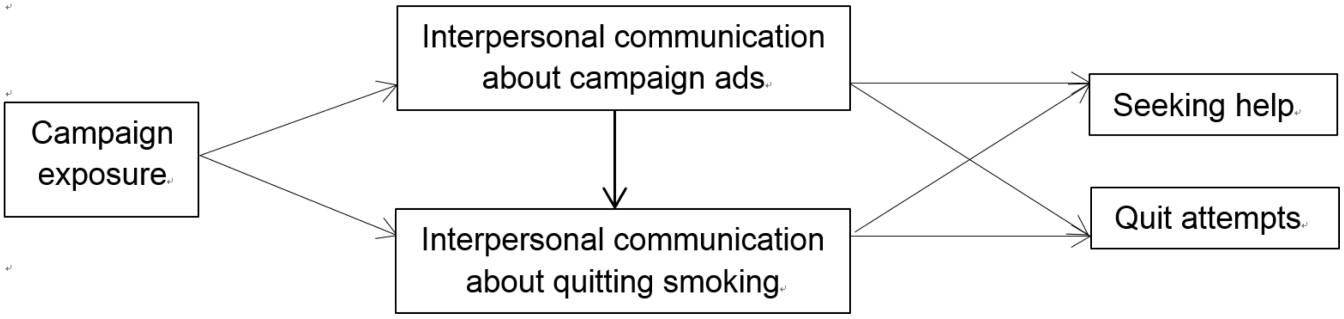

Figure 1.
Full model of proposed pathways.

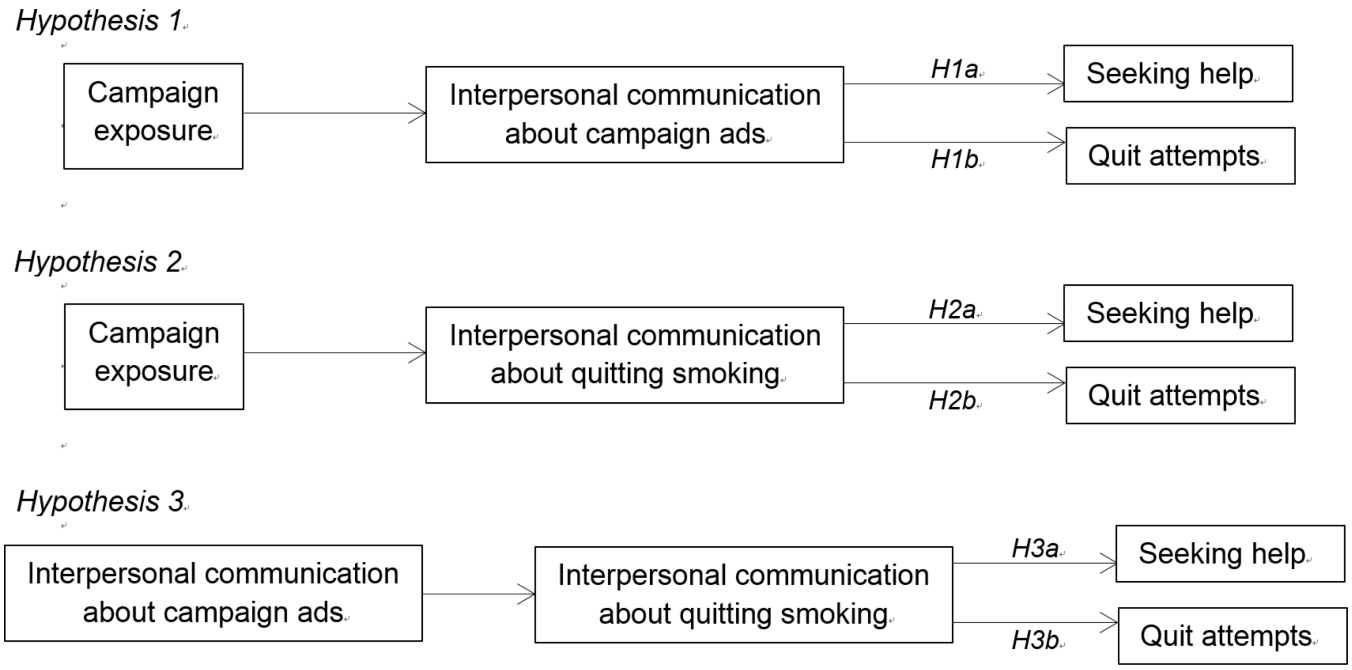

Figure 2.
Proposed model of mediation effects: by hypothesis.

Table 1

Descriptive Statistics of the Cross-Sectional and Longitudinal Samples

	Cross-sectional (*N* = 3010)	Longitudinal (*n* = 804)
Demographics and Smoking Behaviors	**Mean ± SD**	**Mean ± SD**
Age (years)	52 ± 14	54 ± 13
Education (years)	13 ± 2	13 ± 2
Cigarettes per day	13 ± 10	14 ± 10
	%	%
Female	64	66
Race/Ethnicity		
Non-Hispanic White	43	45
Non-Hispanic Black	47	49
Hispanic	6	4
Other	3	3
Marital status		
Married/Living as married	35	33
Divorced/Widowed/Separated	34	36
Never married	31	31
Children present in home	32	28
Own their home (vs. rent)	62	61
Income ($40,000 or more)	35	35
Employed	41	41
More than monthly religious attendance	44	43
Insurance		
Any insurance other than Medicaid	56	57
Medicaid	35	35
Not covered by insurance	9	8
Has a doctor	78	80
Health status		
Very good/Excellent	25	26
Good	39	37
Fair/Poor	37	37
Health orientation		
Strongly agree	70	72
Somewhat agree	26	25
Disagree	4	3
Less than 5 minutes to first cigarette	28	29
Living situation		
All smokers	19	19
Some smokers	10	11
All others non-smokers	35	33

	Cross-sectional (*N* = 3010)	Longitudinal (*n* = 804)
Demographics and Smoking Behaviors	**Mean ± SD**	**Mean ± SD**
Alone	36	37
Campaign-relevant Measures		
Campaign exposure in the past month	67.0	81.8
Talked about quitting in the past month	20.3	24.7
Talked about ads [a]	21.1	24.8
Sought help in the past month	13.5	16.7
Tried to quit completely in the past month	19.7	23.4
Sought help in the past 3 months	32.6	33.8
Tried to quit completely in the past 3 months	34.2	35.5

Note. Sample size reflects the final analytic sample, which does not include those missing on confounders.

[a] Cross-sectional *n* = 2014 and longitudinal *n* = 658 due to missing data on the *talked about ads* variable (participants who reported zero exposure to the campaign ads were not asked this measure).

Table 2

Zero-order Correlations for Campaign Exposure, Interpersonal Communication Variables, and Quit Behaviors at the Cross-sectional Level

		1	2	3	4	5	6	7
1	Campaign exposure in the past month	1.000						
2	Talked about quitting in the past month	**0.080**	1.000					
3	Talked about ads	**0.195**	**0.209**	1.000				
4	Sought help in the past month	**0.050**	**0.228**	**0.116**	1.000			
5	Tried to quit completely in the past month	0.008	**0.259**	**0.089**	**0.298**	1.000		
6	Sought help in the past 3 months	**0.061**	**0.231**	**0.167**	**0.571**	**0.261**	1.000	
7	Tried to quit completely in the past 3 months	0.013	**0.255**	**0.087**	**0.240**	**0.693**	**0.310**	1.000

Note. $N = 3277$. Pairwise Spearman's correlation coefficients (rho) are presented. All bolded results are significant at $p < .01$.

Table 3

Indirect Effects for Cross-sectional Mediation Analyses

				Indirect Effect		Total Effect
	(T1)	**(T1)**	**(T1)**	**Effect Size**	**BC Bootstrap CI**	**Effect Size**
H1a	Exposure	→ Talk about ads	→ Sought help	**.033**	.016 – .051	.064
H1b	Exposure	→ Talk about ads	→ Tried to quit	**.022**	.005 – .040	.052
H2a	Exposure	→ Talk about quitting	→ Sought help	**.024**	.012 – .037	.057
H2b	Exposure	→ Talk about quitting	→ Tried to quit	**.027**	.012 – .042	.004
H3a	Talk about ads	→ Talk about quitting	→ Sought help	**.050**	.034 – .068	.144
H3b	Talk about ads	→ Talk about quitting	→ Tried to quit	**.061**	.044 – .078	.096

Note. N = 2010–3007 (varies across analyses due to missing data on confounders and *talk about ads* variable). T1 = variable measured at first interview; BC Bootstrap CI = bias-corrected bootstrap confidence intervals. Indirect and total effect sizes are standardized. Non-zero indirect effects are bolded. These analyses report the effects of the compound path from the first named variable to the last named variable through the mediator, adjusting for confounders. Behavioral outcomes occurred within the past month.

Table 4

Odds Ratios for Lagged Regression Analyses that Test Proposed Causal Pathways

Pathway	*OR*	95% CI
Exposure (T1) → Behavior (T2)		
Exposure (T1) → Sought help (T2)	1.05 **	1.01 – 1.09
Exposure (T1) → Tried to quit (T2)	1.03	0.99 – 1.07
Exposure (T1) → Interpersonal Communication (T2)		
Exposure (T1) → Talk about ads (T2)	1.04	0.99 – 1.10
Exposure (T1) → Talk about quitting (T2)	1.05 *	1.01 – 1.10
Interpersonal Communication (T1) → Behavior (T2)		
Talk about ads (T1) → Sought help (T2)	1.49	0.96 – 2.30
Talk about ads (T1) → Tried to quit (T2)	1.33	0.85 – 2.08
Talk about quitting (T1) → Sought help (T2)	2.04 ***	1.41 – 2.94
Talk about quitting (T1) → Tried to quit (T2)	1.79 **	1.22 – 2.61
Interpersonal Communication (T1) → Interpersonal Communication (T2)		
Talk about ads (T1) → Talk about quitting (T2)	1.82 **	1.16 – 2.87

Note. n = 551–813 (varies across analyses due to missing data on confounders and *talk about ads* variable). OR = odds ratio; CI = confidence interval; T1 = variable measured at first interview; T2 = variable measured at three-month follow-up interview. These analyses report the coefficient of the first named variable on the outcome, adjusting for the effects of the prior level of the outcome and all confounders. Behavioral outcomes occurred within the past three months.

$^{\dagger}p < .10$.

$^{*}p < .05$.

$^{**}p < .01$.

$^{***}p < .001$.

Table 5

Indirect Effects for Longitudinal Mediation Analyses

	(T1)	(T1)	(T2)	Indirect Effect		Total Effect
				Effect Size	BC Bootstrap CI	Effect Size
H2a	Exposure	→ Talk about quitting	→ Sought help	.013	−.001 – .031	.122
H2b	Exposure	→ Talk about quitting	→ Tried to quit	.010	−.004 – .028	.077
H3a	Talk about ads	→ Talk about quitting	→ Sought help	**.037**	.013 – .070	.095
H3b	Talk about ads	→ Talk about quitting	→ Tried to quit	**.037**	.008 – .072	.074

Note. n = 608–802 (varies across analyses due to missing data on confounders and *talk about ads* variable). T1 = variable measured at first interview; T2 = variable measured at three-month follow-up interview; BC Bootstrap CI = bias-corrected bootstrap confidence intervals. Indirect and total effect sizes are standardized. Non-zero indirect effects are bolded. These analyses report the effects of the compound path from the first named variable to the last named variable through the mediator, adjusting for the effects of the prior level of the outcome and all confounders. Behavioral outcomes occurred within the past three months.