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Understanding the Relationship Between Communication and Political Knowledge: A Model Comparison Approach Using Panel Data

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*The purpose of this study was to examine more closely the assumptions of causality in research on communication and political knowledge. Although most communication theory suggests that communication causes learning, some have argued for the reverse causal direction or reciprocal causality. Others have confounded these concepts—in conjunction with political interest—in measures of political "sophistication" or "expertise." We collected panel data (*N *= 1,109) on a national sample in June and November 2000. We employed a model comparison approach to identify the best fitting model among alternatives that included models of unidirectional and reciprocal causality in both lagged and synchronous models, controlling for prior political interest and various demographic factors. The data are most consistent with a model of causality that is unidirectional running from Time 2 measures of news use and political discussion to Time 2 political knowledge.*

Keywords deliberation, discussion, expertise, learning, news media, sophistication

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Political communication researchers are often interested in studying the effects of various forms of political communication on important democratic outcomes such as political knowledge. Our theories are often very clear about the direction of causal influence among these variables, normally assuming that it runs from communication variables to political knowledge.

Unfortunately, our survey data are rarely capable of testing this causal ordering assumption. That is, few have been able to present survey data with repeated measurements across waves (for a rare exception, see Atkin, Galloway, & Nayman, 1976) to evaluate the common claims of causal influence while precluding the possibility of reverse causation. Without these data, researchers are often forced to make inferences about causal influence without the ability to rule out alternative explanations. For instance, do news use and political discussion produce political knowledge, or do the more politically knowledgeable use news more often and seek out political discussion more frequently? There are a number of plausible models of the relationship between these variables, but most survey research findings cannot distinguish between them.

The focus of the present study is to analyze panel data with measures of news media use, political discussion, and political knowledge in both waves. We employ a model comparison approach using combined structural and measurement models in SEM to fit various plausible theoretical models to the data. We develop models of unidirectional causation in each direction separately as well as models of reciprocal causation. We consider causation among both synchronous measures (i.e., within the second wave only but controlling for the lagged outcome measure) and lagged measures (predicting a Time 2 [T2] measure of the outcome with the Time 1 [T1] measure of the causal agent, controlling the T1 measure of the outcome). We evaluate the models on the basis of their empirical fit as well as parsimony. We conclude that the best fitting model is consistent with claims of a unidirectional influence of news media use and political discussion on political knowledge acquisition coming primarily through a synchronous path.

The Problem: Causal Ambiguity

A simple bivariate correlation between communication and political knowledge can, of course, have numerous interpretations. Below we briefly discuss some of the possible interpretations as they relate to existing theoretical arguments.

Communication and Political Knowledge Are Spuriously Related

One explanation for an empirical relationship between communication and political knowledge is that some third variable causes both of them, and thus their relationship is spurious due to that third variable. Some might provide a social theory to explain the relationship by claiming that communication and political knowledge are both simply a function of social status. This argument would claim that variables such as income or education contribute to political knowledge, discussion, and news use, eliminating the empirical relationships among these variables when controlled.

Another common critique of those who argue that there is a causal relationship between communication and political knowledge is that political interest—or some similar concept—can account for both high levels of communication and high levels of political knowledge. Some have implied that news use, political discussion, and political knowledge are simply indicators of a larger concept such as political involvement, sophistication, or expertise (Cassel & Lo, 1997; Fiske, Kinder, & Larter, 1983; Fiske, Lau, & Smith, 1990; McGraw & Pinney, 1990). For example, Cassel and Lo (1997) combine print media use and political interest measures together to form a measure of "political involvement." Fiske et al. (1983) set forth a "practical view of political expertise as including the interlocking set of knowledge, interest, and participation" (p. 385) but include measures of news media use (as well as political participation) as indicators for this concept. McGraw and Pinney (1990) construct a measure of "political sophistication" from subscales of political knowledge, media use, interest, and behaviors. Implied by these measures is that relationships among communication and knowledge are not causal but simply a function of them being themselves caused by the same overarching unobservable construct, whether it be labeled involvement, expertise, or sophistication. Despite criticisms we might make of the conceptual and measurement issues for such concepts, it is true that interest is often empirically related to political knowledge (Bennett, 1995; Marcus & MacKuen, 1993), news use (Lazarsfeld, Berelson, & Gaudet, 1948; Luskin, 1990; McLeod, Scheufele, & Moy, 1999; Myers, 1994), and political discussion (Bennett, Flickinger, & Rhine, 2000; Myers, 1994; Straits, 1991).

A first step, then, would be to account for variables that might cause both communication and political knowledge. We have selected political interest and four demographic variables—age, education, income, and gender—to address this concern. We consider these variables as exogenous predictors of both communication and political knowledge in the second wave of our panel.

Communication Causes Political Knowledge

For decades political communication researchers have devoted significant effort to demonstrating the role of news media use in producing political knowledge (Brians & Wattenberg, 1996; Chaffee, Zhao, & Leshner, 1994; McLeod, Daily, et al., 1996; McLeod & McDonald, 1985; Neuman, 1986; Neuman, Just, & Crigler, 1992; Palmgreen, 1979; Robinson & Levy, 1986b, 1996), although usually without the advantage of panel data. Even when panel data have been collected, typically only the knowledge measure has been repeated over time (e.g., Chaffee & Schleuder, 1986; McLeod, Guo, et al., 1996) to allow for control of prior knowledge in assessing effects of news use.

From a theoretical perspective, it almost goes without saying that news use can cause political knowledge. Delli Carpini and Keeter (1996, p. 185) express the common belief that "much of one's observed knowledge about politics must come, at least initially, from the mass media." Although individuals could learn basic civics knowledge in high school or college government classes, where else would individuals obtain information about current presidential candidates or the major political issues of the day other than news media? Given how far removed most individuals are from politics, few have direct personal experience from which to draw for their political information, particularly at the national level. Thus, in practice knowledge of politics is dependent on communication, and in particular mass communication through news media.

Nonetheless, the evidentiary base for a claimed relationship between political discussion and political knowledge has been growing rapidly in recent years (e.g., Bennett et al., 2000; Delli Carpini & Keeter, 1996; Kwak, Williams, Wang, & Lee, 2005; Robinson & Davis, 1990; Robinson & Levy, 1986a). Scholars have attempted to develop theoretical explanations for this relationship that move beyond simple two-step flow explanations that imply that interpersonal discussion is merely a conduit for gaining second-hand information from the news (e.g., Eveland, 2004; Lenart, 1994; Scheufele, 2002).

The decades of research on the role of communication in political learning have produced substantial information about the effects of motivations for news use (e.g., Gantz, 1978), differences in learning across media sources (e.g., Chaffee & Frank, 1996; Robinson & Levy, 1986b), and the role of attention to (e.g., Chaffee & Schleuder, 1986) and reliance on (e.g., Culbertson & Stempel, 1986) various forms of news in political learning. Researchers have also worked to uncover the processes behind political learning through variables such as information processing (e.g., Eveland, 2001; Kosicki & McLeod, 1990). However, issues of causality have typically been sidestepped, aside from obligatory comments in discussion sections regarding the limitations of a particular study in making causal inferences. Thus, although all of these issues are of great importance for the study of political learning, in this study we set aside for the moment issues of differences in medium (e.g., TV vs. newspapers), measurement (e.g., exposure vs. attention), and the process of learning (e.g., motivation and information processing) and focus more directly on issues of causation between basic communication behaviors and political knowledge.

Political Knowledge Causes Communication

Although most political communication researchers seem to agree that communication is the primary causal agent in the pair, it is relatively easy to argue the reverse—that political knowledge causes communication. For instance, Neuman (1986) suggests that having knowledge of politics makes individuals more likely to seek further information, and presumably this information could come from either mediated or unmediated sources. Luskin (1990) believes that political sophistication (essentially knowledge in his study) influences political print news media use through political interest. The logic may be that when individuals have a strong foundation of political knowledge, news media content is easier to understand and thus more enjoyable.

Reciprocal Causality Between Communication and Political Knowledge

The theoretical arguments above have been presented as either/or explanations for the relationship between communication and political knowledge. However, there is no reason that these explanations must be mutually exclusive. Indeed, it is possible that communication causes political knowledge *and* that political knowledge causes communication, and that some part but not all of their observed relationship is spurious.

The idea of reciprocal causality is what one prior study of learning from the news using repeated measurements across waves of a panel of college students concluded (Atkin et al., 1976). However, data from this study were not nationally representative, and findings across the two universities from which data were collected produced somewhat different conclusions. Moreover, tools for the analysis of panel data have surpassed the traditional cross-lagged correlation analysis employed in early studies such as those of Atkin et al. One benefit of more advanced methods is the ability to control for exogenous variables such as demographics and interest in assessing empirical relationships among waves of a panel. In essence, this is the ability to address both spuriousness and causality simultaneously. Another benefit is the ability to account for the error in measures of news media use (e.g., Bartels, 1993), which can lead to the attenuation of relationships between news use and other variables. Thus, a final step is testing the unidirectional causal models against a model of reciprocal causality while accounting for spuriousness as best as possible.

Method

Sample

This study relies on national survey data collected in February 1999, June 2000, and November 2000 from a single panel of respondents. The February 1999 data were collected as part of an annual mail survey—the "Life Style Study"—conducted by Market Facts on behalf of DDB-Chicago. Initially, Market Facts acquires the names and addresses of millions of Americans from commercial list brokers, who draw available information from various centralized sources. Via mail, large subsets of these people are asked to indicate whether they would be willing to participate periodically in surveys for small incentives. This produces a pool of roughly 500,000 people who have expressed a willingness to complete surveys.

In an effort to achieve a study sample that is representative of the population, stratified quota sampling procedures are then employed to select approximately 5,000 respondents to whom a survey is mailed. Consistent with past performance, 3,388 usable responses were received based on the February 1999 mailout, representing a response rate of 67.8%. This stratified quota sampling method differs markedly from more conventional probability sample procedures yet produces highly comparable data (see Eveland & Shah, 2003; Putnam, 2000; Putnam & Yonish, 1999).

For the June 2000 wave of the study (hereafter labeled "T1," even though it was technically the second survey in this panel series), we developed a custom questionnaire and then engaged Market Facts to recontact the individuals who completed the February 1999 Life Style Study. Due to some erosion in the panel, 2,737 questionnaires were mailed out. The attrition rate for this survey against the previous wave was 43.9%, with 1,902 respondents completing the questionnaire.

For the November 2000 wave of the study (hereafter labeled "T2," even though it was technically the third survey in this panel series), another custom questionnaire was developed and Market Facts again recontacted individuals who had completed both prior surveys. Due to some erosion in the panel, 1,850 questionnaires were mailed to June 2000 respondents. The attrition rate against the previous wave for this survey was 30.86%, with 1,315 respondents completing the questionnaire. For all analyses that follow, subjects with missing data on any particular item were deleted, leaving 1,109 cases with complete data for analysis.[1]

Measurement

Four demographic variables and political interest served as exogenous control variables. Education was measured as an ordinal variable on a 7-point scale ranging from attending but not graduating from elementary school through attending postgraduate school (mode and median = 5, attending but not graduating college). Age was a continuous measure ranging from 18 through 89 years ($M = 53.93$, $SD = 15.32$). Annual household income was measured on a 5-point ordinal scale from less than $15,000 through $100,000 or more (mode and median = 3, $30,000 to $59,999). Gender was measured as a dichotomy, with females (63%) being given the high value. Finally, political interest was measured using a single indicator at T1 based on responses to the statement "I am interested in politics" on a 6-point scale ranging from definitely disagree to definitely agree ($M = 3.27$, $SD = 1.54$).

News media use was measured with four identical indicators at T1 and T2. Two of these questions asked respondents how many days in the past week they watched stories about the (a) presidential campaign and (b) national government and politics on television. The other two indicators asked about reading the same sorts of articles in newspapers. The average of these items at T1 (M = 1.62, SD = 1.73) produced a reliable scale (α = .88). Similarly, at T2 the average of these items (M = 3.55, SD = 2.37) was reliable (α = .89). The use of these four indicators in each of the two waves will be discussed later as part of the measurement model incorporated into our structural equation models.

Political discussion was measured with the same five indicators in each of the two waves. At T1 the questions asked respondents to separately indicate their frequency of political discussions with (a) coworkers, (b) neighbors, (c) friends, (d) family, and (e) acquaintances during the past year on an ordinal scale ranging from 1 (none) to 8 (100 or more times). The average of these five items (M = 2.53, SD = 1.37) produced a reliable scale (α = .88). At T2 the same questions were asked, but they made reference to the prior 3 months and used a smaller frequency range, although commensurate with the reduction in the applicable time frame (from 0 [none] to 7 [25 or more times]). The average of these five items (M = 2.49, SD = 1.62) produced a reliable measure of political discussion (α = .84).[2]

Scholars interested in understanding levels of citizen competence or comparing changes in the political knowledge of the public over time often employ indicators of basic civics knowledge or measures of the names and party affiliations of public office holders (e.g., Bennett, 1989; Delli Carpini & Keeter, 1991). However, research on the effects of election campaign communication on political knowledge tends to focus on knowledge of the major presidential candidates' issue stances and other background information (Benoit & Hansen, 2004; Brians & Wattenberg, 1996; Eveland & Scheufele, 2000; Kwak, 1999). We follow the latter approach in part because it is a necessary condition for communication effects that the content of the knowledge questions was part of public discourse during the campaign. Moreover, knowledge of such information is important for citizens to make an informed decision.

Therefore, political knowledge was measured with four dichotomous indicators at T1 and eight dichotomous indicators at T2. At T1, respondents were asked which candidate (George W. Bush or Albert Gore) (a) favors a system of school vouchers (Bush), (b) was once a U.S. senator (Gore), (c) wrote a book called *Earth in the Balance* (Gore), and (d) supports the larger tax cut (Bush). Correct responses to these questions were scored as a "1," and incorrect and "don't know" responses were scored as "0." The mean score across the four questions was then computed and multiplied by 100 to produce a percentage correct value (M = 40.48, SD = 37.51). The internal consistency reliability of this measure of knowledge was acceptable by traditional standards ($KR\text{-}20$ = .77).

At T2, respondents were asked which candidate (Bush or Gore) (a) has a brother who is currently a state governor (Bush), (b) favors allowing young people to devote up to one sixth of their Social Security taxes to individually controlled investment accounts (Bush), (c) favors providing "targeted tax cuts" to particular groups (Gore), (d) gave a dramatic kiss to his wife at the national nominating convention (Gore), (e) favors drilling in Alaska's Arctic National Wildlife Refuge for oil (Bush), (f) used to be partial owner of a Major League Baseball team (Bush), (g) served as a journalist in Vietnam (Gore), and (h) favors a 72-hour waiting period for gun purchases at gun shows (Gore). Again, correct responses were scored "1," and incorrect and "don't know" responses were scored "0." The mean score across the eight questions was assessed and multiplied by 100 to represent a percentage correct value (M = 60.01, SD = 28.33). The reliability of this measure of knowledge was acceptable ($KR\text{-}20$ = .76).

Analytic Strategy

To test various theoretical arguments that have been proposed in the literature, we developed six combined structural and measurement models that examine the relationship between political knowledge and communication. These models are models of unidirectional and reciprocal causation between communication and knowledge, both lagged (theoretical predictors from T1 and outcomes from T2) and synchronous (both theoretical predictors and outcomes from T2). In the results section, we describe each of these models in detail and move deliberately through each successive model in order to draw a conclusion about which model best fits the data and the process underlying the data.

Consistent with the recommendations of Shoemaker and Lomax (1996, p. 72), we developed a best-fitting measurement model before estimating the structural model of the relationship between political knowledge and communication. Since our primary focus is on the structural models, and the measurement models are held constant across structural models, we describe the development of the measurement model in the appendix. The measurement and structural models were all estimated using Analysis of Moment Structures (AMOS) software.

In all models described below, age, education, income, gender, and political interest are included as exogenous variables that predict all T2 outcome measures. Moreover, they are expected to correlate among themselves as well as with the T1 theoretical measures of communication and political knowledge. It is also important to note that we assume that communication and political knowledge will be correlated at T1, but we do not specify a causal relationship between them at this prior time point. All of our tests of causal paths will be based on T2 outcomes. In addition, in all models we include a structural path between T1 and T2 news use, between T1 and T2 discussion, and between T1 and T2 knowledge. These paths represent temporal stability and effectively control for prior levels of the outcome, making other paths to these outcome variables interpretable as predicting "change" in the outcome variable compared to what would have been predicted from each individual's prior level of the outcome variable. (However, for simplicity in phrasing, we will continue to use the basic variable labels of news use, discussion, and knowledge in the results section.) Finally, we assume that political discussion and news media use will be correlated with one another at both T1 and T2, but we do not specify a causal relationship between them at either time point (see commentary article in this issue for more on this).

To compare the relative fit of the structural models, we used several measures: (a) the Akaiki information criterion (AIC), (b) the Bayesian information criterion (BIC), (c) the Brown-Cudeck criterion (BCC), (d) the consistent AIC (CAIC), (e) the root mean squared error of approximation (RMSEA), and (f) the ratio of the chi-square statistic to the degrees of freedom for the model (χ^2/df). The first four of these measures are derivatives of the model chi-square, but they "penalize" models that include more structural paths and thus are more complex and less parsimonious. AIC imposes the smallest penalty for model complexity, whereas CAIC imposes the harshest penalty. These measures also have the advantage that models that are non-nested can be compared, with the model with the lowest value being designated as best fitting. However, model comparisons using these first four measures often are sample size specific, with the more complex model being preferred with bigger samples and the less complex model being preferred in smaller samples (MacCallum, 2003). Although sample size was constant across all models we evaluated, there is still the possibility that the relative fit of the models would rank substantially differently if the sample size were larger or smaller.

For this reason, we also used RMSEA and χ^2/df as measures of relative fit in order to compare the orderings of the models using the first four measures to a different standard than how the models compare just to each other. As with the other four measures, lower values of RMSEA and χ^2/df indicate better fit. Finally, in some cases the models we compare are nested, and when appropriate nested models are compared using the standard chi-square test. When such a test is appropriate, we conduct that test and report it below. Discussions of the various measures of fit can be found in Arbuckle and Wothke (1999, pp. 404–405), Brown and Cudeck (1993), Raftery (1993), and Maruyama (1998, pp. 246–247).[3]

Results

Before addressing the focal causal effects, we will briefly describe the direct impact of the exogenous control variables on T2 communication and T2 knowledge (see Table 1 and Table 2).[4] What is most immediately apparent is the relatively small impact of these variables given the control for prior measures of the endogenous variables. First, there is a consistent pattern for older and richer respondents to be higher in news use that is replicated across all six models. Moreover, in five of the six models those with higher

Table 1
Influence of exogenous variables on T2 communication and T2 political knowledge: Models 1–3

	T2 news use	T2 discussion	T2 knowledge
	Model 1: reciprocal lagged		
Gender (female)	.05	.01	.01
Age	.08*	–.04	.05
Income	.11*	.11*	.09*
Education	.04	.02	.01
Political interest	.09*	.11*	.03
	Model 2: communication → knowledge lagged		
Gender (female)	.05	.01	.01
Age	.08*	–.04	.06
Income	.11*	.11*	.09*
Education	.05*	.03	.02
Political interest	.11*	.13*	.04
	Model 3: knowledge → communication lagged		
Gender (female)	.05	.01	.01
Age	.08*	–.04	.07*
Income	.11*	.11*	.09*
Education	.04	.02	.01
Political interest	.10*	.11*	.05

Note. Coefficients are standardized regression weights. When decisions of statistical significance using bootstrap and normal-theory derived p values conflicted, we report significance based on the bootstrapped p value. T2 = Time 2.
$^*p < .05$.

Table 2
Influence of exogenous variables on T2 communication and T2 political knowledge: Models 4–6

	T2 news use	T2 discussion	T2 knowledge
Model 4: reciprocal synchronous			
Gender (female)	.05*	.01	.01
Age	.08*	–.05	.05
Income	.10*	.10*	.06*
Education	.04	.01	.01
Political interest	.08*	.10*	.00
Model 5: communication → knowledge synchronous			
Gender (female)	.05	.01	.01
Age	.08*	–.04	.04
Income	.11*	.11*	.05
Education	.05*	.03	.00
Political interest	.11*	.13*	.01
Model 6: knowledge → communication synchronous			
Gender (female)	.06*	.02	.01
Age	.07*	–.06*	.07*
Income	.07*	.08*	.09*
Education	.04	.01	.02
Political interest	.05	.08*	.05

Note. Coefficients are standardized regression weights. When bootstrap and normal-theory-derived p values conflicted, we report significance based on the bootstrapped p value. T2 = Time 2.
*$p < .05$.

levels of political interest are also more likely to be exposed to news media, and in three of the six models women are more likely to use news than men. The pattern of prediction of political discussion is similar to that of news use, but there are some differences. Wealthier and more politically interested respondents are more likely to engage in political discussion in each of the six models, as for news use. However, there are no gender differences and, in one of the models, younger respondents are more likely to have discussions than older respondents. Level of formal education is completely unrelated to either form of communication.

The only consistent relationship between a control variable and political knowledge is the finding that those with higher incomes also have higher levels of knowledge, a finding replicated across five of the six models. Political interest has no direct effect on political knowledge despite a zero-order correlation of .36. The typical finding of a relationship between education and political knowledge is not supported in even a single model despite a zero-order correlation of .30. (See the appendix for a complete table of correlations between demographics and interest on the one hand and both T1 and T2 measures of news use, discussion, and knowledge on the other.) These unusual findings that generally preclude direct effects of education and interest are likely because of our control for prior knowledge in this study. Our findings actually make more sense than traditional findings, as it is hard to make a coherent theoretical argument for these variables having *direct* effects on measures of current political knowledge such as ours. As Luskin (1990, p. 349) points out, "Why, then, do so many cross-sectional analyses of adult samples show a relationship between education and sophistication [i.e., knowledge]? The simplest explanation is the paucity of controls."

We now move to the central causal relationships in this study. Details are presented in the corresponding figures—one for each model—and model fit statistics are reported in Table 3. Model 1 (see Figure 1) presents a modern alternative to the traditional cross-lagged correlation analysis that Campbell and Kenny (1999, p. 148) call the "regression model," initially proposed by Rogosa (1980). We specify cross-lagged paths such that T1 communication variables cause T2 knowledge and T1 knowledge causes T2 communication variables. We also include a correlation between the T2 error terms, implying that there is a synchronous correlation between the two forms of communication and knowledge at T2 but no causal effects within this time point. As is true for all of the models that follow, the strongest relationships tend to be those for a given measure over time. That is, there is considerable stability in both forms of communication and political knowledge over time, especially considering the 6-month time span between waves of data collection. There are no significant lagged paths in this model, although the correlations among the T2 error terms are all positive and statistically significant.

Model 2 (see Figure 2) simplifies Model 1 by addressing a unidirectional model of causality with communication variables at T1 causing knowledge at T2. This model produces the same basic findings as Model 1—no lagged causal effects of communication variables on knowledge but significant correlations among the disturbances of the T2 measures.

Model 3 (see Figure 3) assumes the alternate causal direction of influence compared to Model 2. Model 3 assumes that T1 knowledge causes T2 news use and T2 discussion. But as with Model 1, there are no significant paths between T1 knowledge and the two forms of communication in this model. However, the correlations among the T2 error terms all remain significant and positive.

An alternative model of causality to Models 1 through 3 is presented in Models 4 through 6. Model 4 assumes that there is no cross-lagged causality. Instead, it reverses assumptions compared to Model 1. Where Model 1 assumes that all causality is lagged but that it may be reciprocal, Model 4 assumes that all causality is synchronous, and it allows for reciprocal causality.[5] It also assumes a correlation between the disturbance terms of the two communication variables at T2. Keep in mind that because of the path from each T1 measure to its corresponding T2 measure, paths between the T2 measures can be interpreted as reflecting "change" in the outcome variable compared to what would have been predicted based on prior levels of the outcome variable.

With regard to significant causal paths among the T2 measures, Model 4 (see Figure 4) reveals that both T2 news use and T2 discussion significantly predict T2 knowledge, but that T2 knowledge does not predict T2 communication. Moreover, as prior models suggested, the two communication measures are significantly correlated at T2.

Just as Model 2 was a modification of Model 1, Model 5 (see Figure 5) presents a modification of Model 4 that assumes unidirectional causal influence of T2 communication on T2 knowledge, but no effect of T2 knowledge on T2 communication. In this model, the paths from T2 communication to T2 knowledge are both positive and significant. The T2 measures of communication are also significantly correlated. This is the first model for which all theoretical paths are statistically significant.

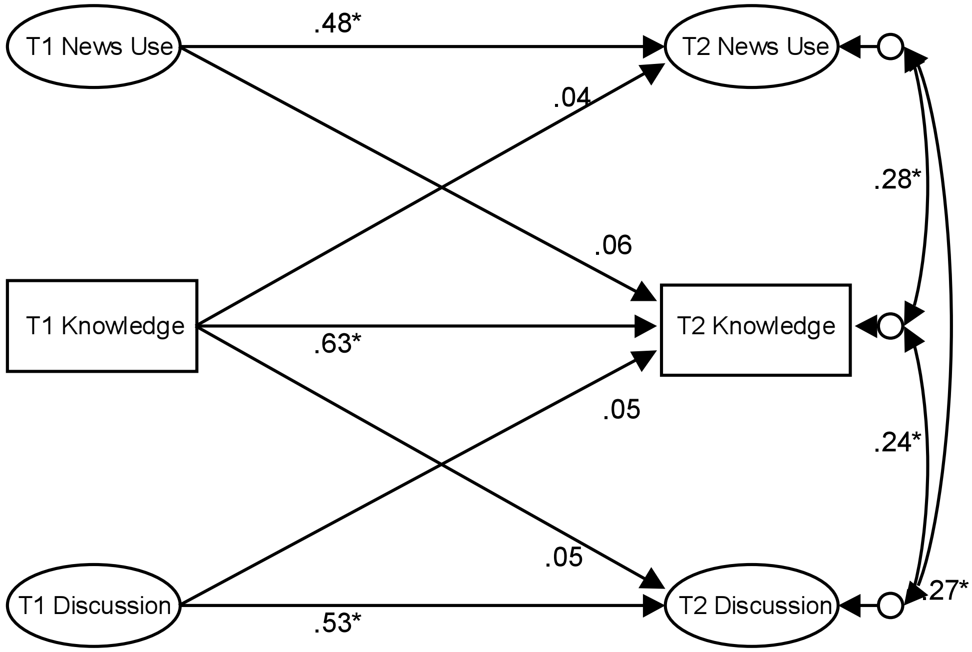

Figure 1. Reciprocal lagged model (Model 1). T1 = Time 1; T2 = Time 2; $*p < .05$.

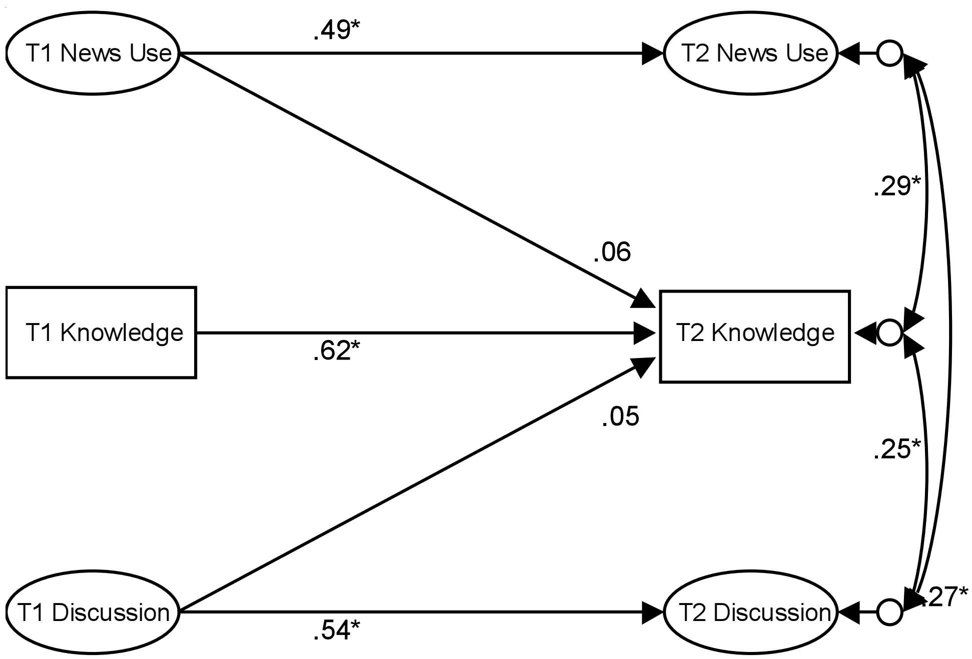

Figure 2. Communication causes knowledge lagged model (Model 2). T1 = Time 1; T2 = Time 2; $*p < .05$.

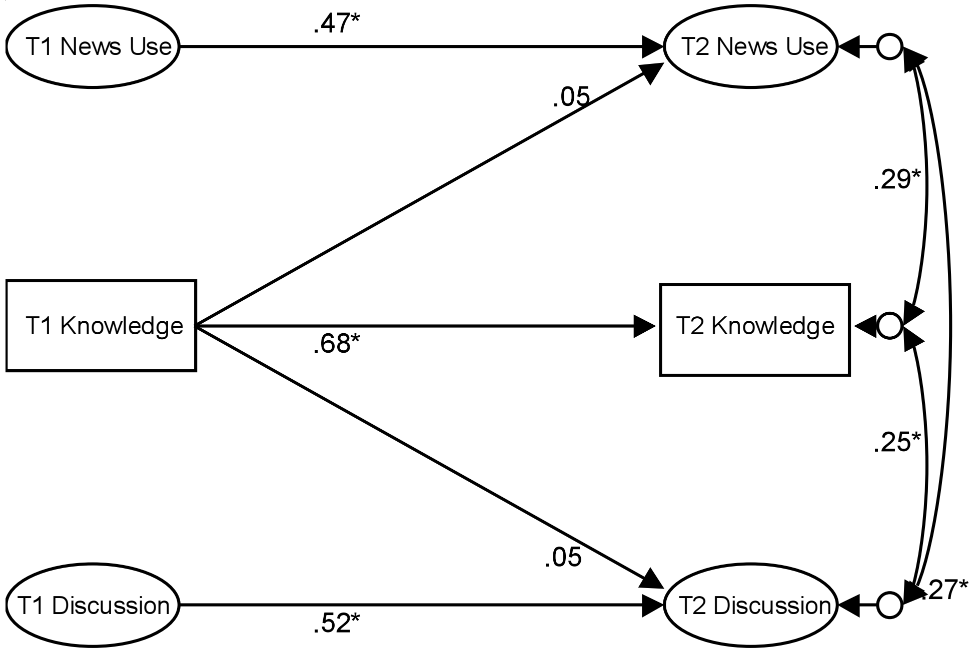

Figure 3. Knowledge causes communication lagged model (Model 3). T1 = Time 1; T2 = Time 2; $*p < .05$.

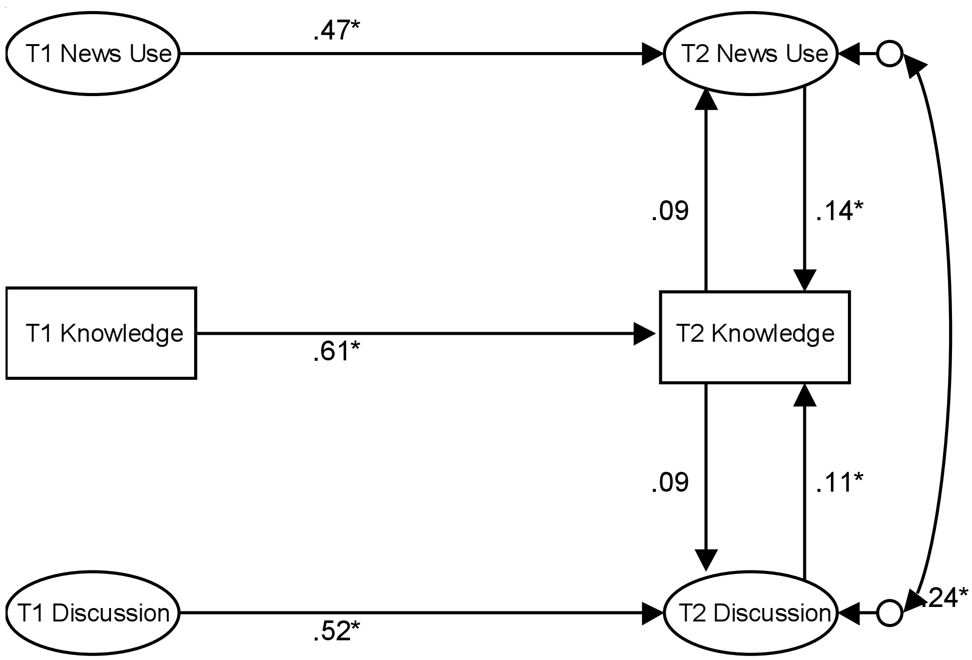

Figure 4. Reciprocal synchronous model (Model 4). T1 = Time 1; T2 = Time 2; $*p < .05$.

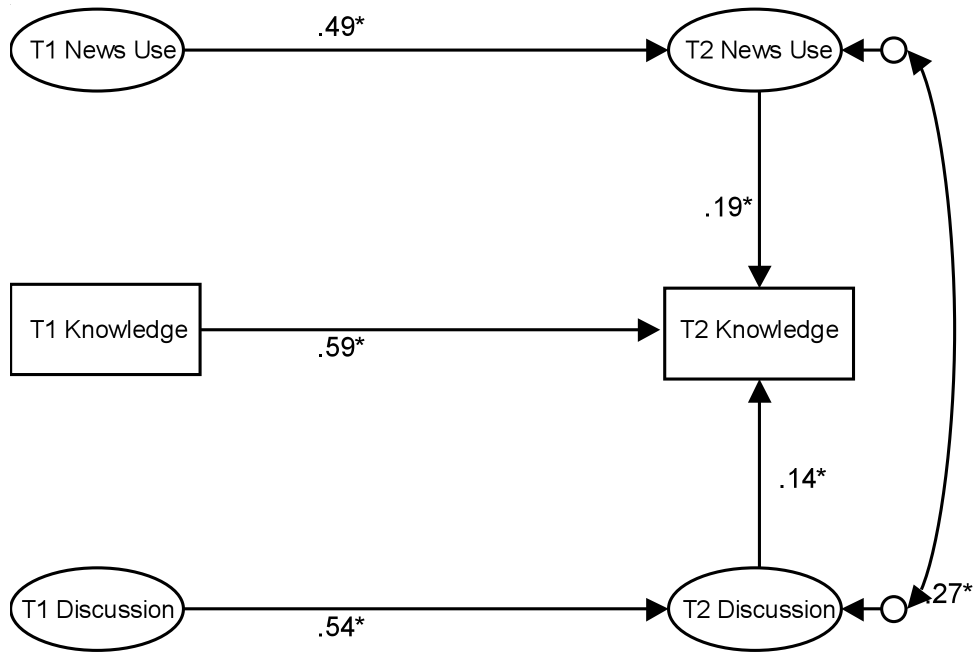

Figure 5. Communication causes knowledge synchronous model (Model 5). T1 = Time 1; T2 = Time 2; $*p < .05$.

Model 6 (see Figure 6) reverses the direction of causality between knowledge and communication but maintains the assumption of synchronicity. In this model, the two paths from T2 knowledge to T2 communication are positive and significant. In addition, the correlation between T2 discussion and T2 news use is positive and significant.

Up to this point, we have focused entirely on the significance of specific paths in the models but have not discussed the central issue, that of model fit. First we will address the relative fit of nested models by testing the differences in their chi-square fit statistics. There are four possible comparisons: the reciprocal lagged model separately with each of the unidirectional lagged models and the reciprocal synchronous model separately with each of the unidirectional synchronous models. Then the six relative model fit statistics described above (see Table 3) will be interpreted to compare nonnested models.

To compare nested models, we simply compare the value of $\Delta\chi^2$ between the models to the critical chi-square value. In our comparisons, the reciprocal models differ from the unidirectional models by 2 degrees of freedom, and thus the critical value for $\Delta\chi^2$ is 5.991. The reciprocal lagged model, $\chi^2(219) = 971.6845$, did not fit the data significantly better than the communication to knowledge lagged model, $\chi^2(221) = 973.9217$, $\Delta\chi^2(2) = 2.2372$. Moreover, the reciprocal lagged model did not fit the data significantly better than the knowledge to communication lagged model, $\chi^2(221) = 977.3428$, $\Delta\chi^2(2) = 5.6583$. Given that the two unidirectional lagged models are simpler than the reciprocal lagged model but do not fit significantly worse, the principle of parsimony suggests we should prefer one of the two unidirectional models over the reciprocal lagged model. However, since the two unidirectional models are not nested, we must wait to compare which fits the data better until we examine the relative fit indices for nonnested models.

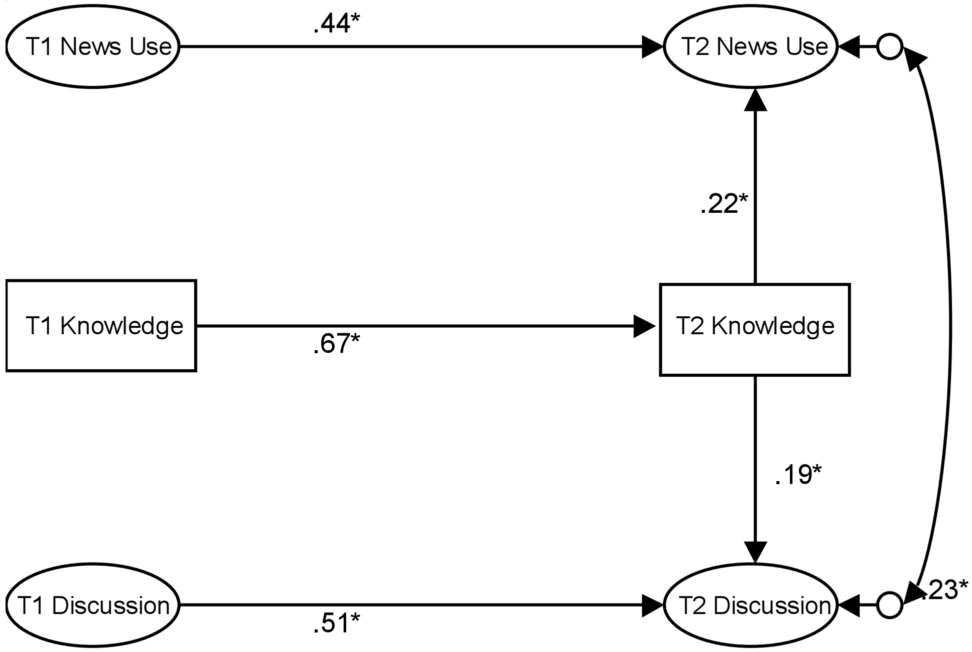

Figure 6. Knowledge causes communication synchronous model (Model 6). T1 = Time 1; T2 = Time 2; **p* < .05.

Regarding the nested synchronous models, the reciprocal synchronous model, χ^2 (221) = 971.0653, did not fit the data significantly better than the communication to knowledge synchronous model, $\chi^2(223) = 976.3192$, $\Delta\chi^2(2) = 5.2539$. Thus, as in the comparison of the lagged models, parsimony concerns suggest that we should prefer the communication to knowledge synchronous model over the reciprocal synchronous model. Moreover, since neither of the two paths that differed between these models—the two knowledge to communication paths—were significant in the reciprocal model, we lose no information about relationships by deleting them from the model. Furthermore, we find that the reciprocal synchronous model fits the data significantly better than the knowledge to communication synchronous model, $\chi^2(223) = 990.9445$, $\Delta\chi^2(2) = 19.8792$, $p < .05$.

Table 3 can be used to compare nonnested models.[6] Among the lagged models, the best fitting model employing each of the fit measures was the unidirectional communication to knowledge model. More broadly across all models, the communication to knowledge lagged model was ranked third overall. The best fitting among all models was always either the reciprocal synchronous model (based on AIC and BCC) or the communication to knowledge synchronous model (based on BIC, CAIC, RMSEA, and χ^2/df). In the two of six instances in which the communication to knowledge model was *not* the best-fitting model, it was the second best-fitting model, and the difference between it and the reciprocal synchronous model was very small. However, the reverse was not true, in that the reciprocal synchronous model was not always even the second-best model. And the differences between the two models were generally larger for those statistics in which the communication to knowledge synchronous model was the best model. The average ranking across all of the statistics suggests that the best-fitting model is Model 5, the

Table 3
Fit statistics for models (rit rank in parentheses)

Model	AIC	BIC	BCC	CAIC	RMSEA	χ^2/df Mean	χ^2/df Rank
1. Reciprocal lagged	1183.6845 (4)	2056.0740 (6)	1188.7787 (4)	1820.8731 (6)	.0557 (5.5)	4.4369 (5)	5.1
2. Communication $\rightarrow$ knowledge lagged	1181.9217 (3)	2037.8510 (4)	1186.9198 (3)	1807.0879 (3)	.0555 (3)	4.4069 (3)	3.2
3. Knowledge $\rightarrow$ communication lagged	1185.3428 (5)	2041.2722 (5)	1190.3410 (5)	1810.5091 (5)	.0556 (4)	4.4224 (4)	4.7
4. Reciprocal synchronous	1179.0653 (1)	2034.9947 (3)	1184.0635 (1)	1804.2316 (2)	.0553 (2)	4.3940 (2)	1.8
5. Communication $\rightarrow$ knowledge synchronous	1180.3192 (2)	2019.7884 (1)	1185.2212 (2)	1793.4630 (1)	.0552 (1)	4.3781 (1)	1.3
6. Knowledge $\rightarrow$ communication synchronous	1194.9445 (6)	2034.4137 (2)	1199.8466 (6)	1808.0884 (4)	.0557 (5.5)	4.4437 (6)	4.9
Best fitting model*	Model 4	Model 5	Model 4	Model 5	Model 5	Model 5	Model 5

*Best-fitting model based on the particular fit statistic employed. Lower values indicate better fit.

Table 4
Variance accounted for in endogenous variables

	T2 news use	T2 discussion	T2 knowledge
1. Reciprocal lagged	.378	.406	.554
2. Communication → knowledge lagged	.376	.404	.547
3. Knowledge → communication lagged	.373	.403	.559
4. Reciprocal synchronous	.401	.426	.596
5. Communication → knowledge synchronous	.376	.404	.592
6. Knowledge → communication synchronous	.408	.429	.549

Note. T2 = Time 2.

communication to knowledge synchronous model. And, given that this model does not fit worse than the more complex reciprocal synchronous model in which it is nested—and no significant paths in the reciprocal model are lost in the reduction to the unidirectional model—the weight of the evidence suggests that the preferred model should be the unidirectional communication to knowledge synchronous model.

By contrast, the two unidirectional models from knowledge to communication were regularly among the worst-fitting models in this group, and across all fit measures ranked fourth and fifth out of six models. The worst-fitting model of all was the reciprocal lagged model.[7]

The description of model fit above relates to how well our models fit the observed data but does not speak directly to the predictive strength of the models. Table 4 presents the squared multiple correlations for T2 news use, T2 discussion, and T2 knowledge for this purpose. Our models account for between 37% and 41% of variance in news use, between 40% and 43% of variance in political discussion, and between 55% and 60% of variance in political knowledge.

Discussion

The purpose of this study was to examine more closely the assumptions of causality in research on communication and political knowledge. This close empirical scrutiny of causal assumptions is rarely done by researchers in this area, most likely because of the lack of appropriate data available to address these issues. Our central conclusion is that the data are most consistent with the traditional assumption among political communication researchers that causality is unidirectional, running from communication—both mass and interpersonal—to political knowledge.[8] Moreover, any impact of prior communication appears to be indirect—because of strong stability in communication behaviors over time—through current levels of communication.

It is important to note that because we are able to control for prior levels of the T2 predictor *and* outcome variables, one interpretation of our findings is that "change" in the predictor produces "change" in the outcome. However, because we employed different indicators of knowledge in each wave (a discussion of this appears below), we cannot speak of "change" in a literal sense, but instead in the sense of variation from what would be expected. That is, our findings suggest that variations in communication from prior to the general election campaign to the general election campaign produce contemporaneous variations in political knowledge. This is a slightly different statement than just that communication causes knowledge, because we know from our models that there is tremendous stability in each of these variables over time. Our models permit us to discuss what predicts variations above and beyond that degree of temporal stability.

One might question the ultimate value of our endeavor given that we have merely upheld the common assumption implied in decades of research. Nonetheless, the value of this work should not be underestimated. Without recourse to analyses such as those reported here, there will always be some question as to the validity of the causal claims made by those analyzing effects of communication on political knowledge with cross-sectional data. Those researchers will always need to note the limitation of their causal inferences in discussion sections of articles. But given our findings, it may not be necessary for future work to always collect panel data or always be so cautious regarding the likelihood of reverse or reciprocal causation. Our results reveal that any model of unidirectional causality from knowledge to communication (Model 3 and Model 6) fits the data worse than any model of unidirectional causality from communication to knowledge (Model 2 and Model 5). Moreover, in neither model of reciprocal causality is there a significant causal path between knowledge and T2 communication measures. Thus, even if we were to prefer a more complex but equally well-fitting model to Model 5—thus ignoring the value of parsimony—we would come to the same conclusion of positive effects of communication on knowledge but no significant effects of knowledge on the two communication variables (Model 4). Although the findings of the present study will require replication with different time lags, different measures of news use and knowledge, and data collected in different political contexts (e.g., non-election, local knowledge), they can be used to buttress causal claims made by others in the future who do not have the luxury of testing causal assumptions in their own data.

In some ways, the findings for news use in our study represent a replication of the work of Atkin et al. (1976)—the only other such analysis of which we are aware of a panel of adults with repeated measures of news exposure and political knowledge across two or more waves. Atkin and colleagues employed panel data with a 1-month lag collected from two separate convenience samples of college undergraduates during the 1972 election campaign. They found that in their Michigan State sample there was reciprocal causality, but that the causality in their Colorado State sample was unidirectional from T1 knowledge to T2 news use.

Thus, these early findings contradict our own. However, our research has a number of advantages over this previous work, including a nationally representative sample and more advanced methods of analysis, such as an explicit SEM model comparison approach. Nonetheless, the inconsistencies in findings between our study and Atkin et al. (1976) suggest that more work needs to be done before a definitive conclusion about the causal assumptions regarding the relationship between communication and knowledge is in order. Potentially the most important avenue for further research is to vary the time lag across studies. Our conclusion of no meaningful causal effects from T1 communication to T2 knowledge (or the reverse) may be due to the long lag between waves of data collection, which itself may have served to reduce the possibility for lagged causal effects. Perhaps a lag of 1 or 2 months—or even just a few weeks—would be more appropriate. However, given the strong stability in both communication and political knowledge over our lengthy time lag, a shorter lag may produce stability estimates so strong as to preclude any causal effects over time.

One concern that might be raised about this study is that the knowledge questions at T1 were different from the knowledge questions at T2. This was a practical necessity.

Were a given knowledge question repeated across survey waves, we would have concerns that respondents would have been particularly sensitive to the correct answer if they encountered it between the first and second waves had they not known the answer at T1. Moreover, the content of news media coverage changes over time, and some topics lose attention whereas others gain attention, particularly over a 6-month time period. Thus, we made an effort to employ the same measurement approach at both time points, but the characteristics and issue stances we asked about varied over time based on what we judged to be relevant topics discussed in the media in the prior months. It should be of little surprise that the zero-order correlation of knowledge across waves (r = .58) was essentially the same as the zero-order correlations of news use (r = .56) and discussion (r = .60) across waves, and that after adjustment for unreliability the stability of political knowledge was even greater than for news use and discussion.

A concern also could be raised about the validity of our knowledge measure because we employed a mail survey instead of the traditional telephone or face-to-face survey. Since there was no interviewer present, it may be that some of our respondents sought out the answers to the knowledge questions before answering them. Although this is possible, it is important to note that these individuals were answering knowledge questions embedded in a lengthy survey, and that these respondents were part of a mail panel for which they regularly completed surveys. These two factors, in addition to the absence of an interviewer (thus reducing concerns about impressions an interviewer would have of them had they not known the answers), make this behavior unlikely for all but a very few respondents. In addition, in order to reduce any concerns among respondents who did not know the correct answers, we explicitly indicated to them that "Of course, there is so much going on these days that it's impossible to keep track of all of it. In any case, do you happen to know which presidential candidate. . . ?"

Finally, we think it is important to acknowledge that although the model of synchronous unidirectional causality from communication to knowledge fit best among the models we estimated, all of these models fit well by commonly accepted standards. This highlights two important points that political communication researchers using structural equation modeling too often forget. First, good model fit does not mean that the model is necessarily correct. All models are simplifications of a process probably too complicated to be accurately modeled (MacCallum, 2003). Our models have greatly oversimplified the complicated processes that occur between communication and knowledge acquisition, and, as such, even our best fitting model is probably wrong in an absolute sense. For instance, we assume no causal relationship between news use and political discussion, but this may be a flawed assumption. We address this issue in our commentary article later in this issue.

Second, the identification of a good fitting model must be interpreted in light of the fact that there often exist alternative models that fit just as well (MacCallum et al., 1993; see Note 7 for examples), or even better. The real power of structural equation modeling stems not so much from its ability to simultaneously estimate the coefficients in a model and assess fit, but to *compare* the fit of alternative theoretical models of the same process. Whereas Model 5 is no doubt literally wrong, it does fit as well as or better than alternative competing theoretical models of the process that utilize the same variables. As such, it is the least wrong of the alternatives we considered. We believe that this is the most important take-home message of this research.

In conclusion, this study suggests that the relationship between communication and political knowledge is best described as causal and not the result of spuriousness. Moreover, it appears that the data are most consistent with a unidirectional causal influence from communication to knowledge. However, limitations of the sort that plague most studies lead us to encourage replication of these findings in the near future.

Notes

1. One possible concern given our three waves of data collection is that of panel attrition. By T1 (the second wave of the survey) our remaining respondents were more likely to be female, highly educated, older, and of higher income compared to those who responded to the initial 1999 DDB survey. However, these respondents looked very similar demographically to the second wave of the 2000 American National Election Study respondents. Attrition between the T1 and T2 surveys again increased the proportion of females as well as the mean age, education, and income of our sample, making it less representative compared to the initial survey. However, since all analyses were conducted only with those who had completed all three waves of data collection, any bias is not so much based on attrition as on a lack of representativeness, which should be less likely to bias our estimates of relationships.

2. Of course, any self-report measure of communication behaviors such as news use and discussion is likely to have some degree of error, both error in recall and potential social desirability bias. Unfortunately, this is true of nearly every study that has examined the effects of news use and political discussion using survey data.

3. The CFI and related measures of fit are commonly used to assess the fit of a covariance structure model. For all structural models discussed here, CFI meets the .95 minimum criterion (Hu & Bentler, 1999). Although the CFI and statistically related measures are widely used as measures of *absolute* fit, they are not good measures for deciding between nonnested alternative models because they do not adjust for model complexity, so models with more estimated parameters will tend to fit better. That is, CFI will tend to increase as the number of structural paths increases, giving a fit advantage to models that include more structural paths (Mulaik et al., 1989). The solution to this problem is to use a measure of fit that allows models that differ in their complexity to be compared on a more equal footing. The problem is that just as for quantifying absolute fit, there are many proposed measures of relative fit. Our approach in the evaluation of the relative fit of the models examined was not to focus on a single measure of relative fit but instead to focus on consistency across several measures of relative fit and select as the best fitting model the one that emerges as fitting best across multiple measures.

4. The path estimates and tests of significance for the central parameters in the models described here are based on the assumption of multivariate normality. We also estimated the models and tests of significance using an empirical bootstrapping procedure based on 10,000 bootstrap samples. The resulting estimates and p values were similar and did not require us to modify our substantive interpretation of the results.

5. Some have expressed concern that the estimates of reciprocal synchronous causal paths in a model without cross-lagged effects are biased when the correlation between the disturbances of endogenous variables is excluded, as is the case in Model 4 (cf. Anderson & Williams, 1992; Wong & Law, 1999). We estimated the correlations between the T2 communication and knowledge disturbances and could not reject the null hypothesis of no correlation between these disturbances, as the fit of the version of Model 4 that included the correlation between the disturbances did not fit any better than the original version of Model 4, $\Delta\chi^2(2) = .0965$, $p > .50$. Furthermore, neither of the correlations were individually significant in this revised model. Thus, we assume that these correlations are zero and so excluding them should not substantially bias the estimation of the reciprocal paths.

6. There is no test of significance of the difference between these measures of relative fit.

7. It may seem as though we left out two potentially interesting models that combined the unidirectional lagged and synchronous models (Models 2 and 5 and Models 3 and 6). But such combined models cannot be estimated because they are not identified. However, they could be identified by fixing the correlation between the disturbances between T2 communication and knowledge to zero. Although identified, such combined models are equivalent to Models 2 and 3

by the Lee-Hershberger rules (MacCallum, Wegener, Uchino, & Fabrigar, 1993) and so fit exactly the same by all measures of fit. However, they do have different interpretations. We fit these equivalent models and found that the synchronous paths were positive and statistically significant, consistent with Models 5 and 6.

8. Based on a reviewer's suggestion, we reran all models with the addition of a measure of strength of partisanship as another exogenous variable. There were no substantive differences in terms of significant paths among the T2 measures or in terms of relative model fit.

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Appendix: Measurement Models for News Use, Political Discussion, and Political Knowledge

To develop the measurement model of news use, we fit a baseline measurement model that estimated (a) the factor loadings and indicator errors for the T1 and T2 news use factor, (b) the temporal stability in the news use factor between T1 and T2 (expressed as a regression path), and (c) measurement error correlations between the errors of the same indicator at different time points. We measure the fit of the measurement models using the confirmatory fit index (CFI). Hu and Bentler (1999) argue that good fitting models should have a CFI of at least .95 (in contrast to the more liberal .90 rule commonly used). We test the significance of alternative measurement models, all of which are nested, by comparing the chi-square statistics associated with each successive model (labeled here as "$\Delta\chi^2$").

In the baseline model for news use, we assumed no measurement error correlations within each time period and invariance of the factor loadings over time. Although all factor loadings, the temporal stability coefficient, and three of the four across-time measurement error correlations were statistically significant, this model did not fit well, CFI = .80. An examination of the modification indices showed that the model could be substantially improved by including four within-time measurement error correlations. These correlations were (a) between the errors of exposure to televised stories about the presidential campaign and national government/politics and (b) between TV and newspaper exposure to stories about national government/politics. Both of these error correlations were included for both T1 and T2 measures. Freely estimating these correlations substantially improved the fit of the model compared to the previous model, CFI = .98, $\Delta\chi^2(4) = 1{,}353.69$, $p < .05$. In addition, freely estimating the factor loadings at each time separately did significantly improve the fit of the model. However, the increase in fit was negligible and attributable to only a single indicator (exposure to televised stories about national government/politics), $\Delta\chi^2(1) = 4.858$, $p < .05$. We allowed this indicator loading to vary over time, yielding a final model with acceptable fit, CFI = .98.

The measurement model for discussion was constructed using an identical procedure and sequence. Unlike the measurement model for news use, the fit of the baseline model was acceptable, CFI = .97. Although the fit of this baseline model could have been improved slightly by adding error correlations within time, none of the modification indices stood out as especially large, so we chose not to estimate such correlations and instead constrained them to zero. However, relaxing the assumption of equality of indicator factor loadings over time did substantially improve the fit of the model. This improvement in fit was attributable to freely estimating the factor loadings at each time for discussion with neighbors, family, and acquaintances, CFI = .98, $\Delta\chi^2(3) = 50.023$, $p < .05$. Freely estimating the remaining factor loadings in each time period (discussion with friends, discussion with coworkers) did not significantly improve the model. Thus, the final measurement model allowed the factor loadings for discussion with neighbors, family, and acquaintances to vary over time. The fact that some of the factor loadings were invariant over time satisfies the important assumption of partial measurement invariance (Byrne, Shavelson, & Muthén, 1989).

Given that the knowledge questions in the survey were dichotomous, it was not

Table A1
Zero-order relationships between demographics/interest and communication and knowledge

	Gender	Age	Income	Education	Interest T1
Discussion T1	–.15**	.07*	.18**	.17**	.36**
Discussion T2	–.10**	.00	.23**	.17**	.33**
News T1	–.13**	.32**	.15**	.24**	.42**
News T2	–.04	.22**	.21**	.20**	.38**
Knowledge T1	–.15**	.12**	.26**	.35**	.43**
Knowledge T2	–.10**	.14**	.26**	.30**	.36**

Note. Variables represented in this matrix are based on simple additive indexes, and thus are not identical to the variables produced in the measurement models for the SEM analyses. T1 = Time 1; T2 = Time 2.

**p* < .05, ***p* < .01.

appropriate to formulate a linear measurement model predicting responses to each of the knowledge questions from a latent variable (Hoijtink, Rooks, & Wilmink, 1999). However, treating knowledge as a measured variable would ignore the fact that knowledge is measured with error. Thus, we used the percentage of questions correctly answered at each time period, as already described, as the measure of political knowledge. However, employing a method described by Kline (1998), we treated this observed knowledge measure as the single indicator of knowledge at that time period, predicted from a latent knowledge variable as well as a random error. The method requires that the variance of the random error be set to $(1 - r)V_o$, where r is an a priori estimate of reliability of measurement and V_o is the variance of the observed knowledge score. To identify these measurement models of knowledge, the paths from the latent knowledge variable and the error to the observed knowledge score must be set to 1. As an estimate of reliability, we used .80, reported by Atkin et al. (1976) in their study (which is also consistent with the internal consistency estimate of reliability of the sum of the knowledge questions reported above).