ARTICLES

MESSAGES RECEIVED: THE POLITICAL IMPACT OF MEDIA EXPOSURE

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Analyses of the persuasive effects of media exposure outside the laboratory have generally produced negative results. I attribute such nonfindings in part to carelessness regarding the inferential consequences of measurement error and in part to limitations of research design. In an analysis of opinion change during the 1980 presidential campaign, adjusting for measurement error produces several strong media exposure effects, especially for network television news. Adjusting for measurement error also makes preexisting opinions look much more stable, suggesting that the new information absorbed via media exposure must be about three times as distinctive as has generally been supposed in order to account for observed patterns of opinion change.

The state of research on media effects is one of the most notable embarrassments of modern social science. The pervasiveness of the mass media and their virtual monopoly over the presentation of many kinds of information must suggest to reasonable observers that what these media say and how they say it has enormous social and political consequences. Nevertheless, the scholarly literature has been much better at refuting, qualifying, and circumscribing the thesis of media impact than at supporting it. As Graber put it: "The findings that media effects were minimal were so pervasive in early research that after an initial flurry in the 1940s and 1950s, social science research into mass media effects fell to a low ebb. In study after study dealing with political socialization and learning, the mass media were hardly mentioned as an important factor" (1980, 10).

The field of electoral politics produced some of the most influential early findings of "minimal effects," especially in the classic Columbia studies of presidential campaigns in the 1940s (Berelson, Lazarsfeld, and McPhee 1954; Lazarsfeld, Berelson, and Gaudet 1948). Subsequent research, when it was conducted at all, generally produced similar results. Even the dramatic rise of television did little to alter the scholarly consensus regarding the role of the mass media. The thesis of a widely cited presidential campaign study conducted a generation after the Columbia studies (Patterson and McClure 1976) is nicely conveyed by its subtitle, *The Myth of Television Power in National Elections.*

Social scientists have occasionally acknowledged that the persuasive effects of the mass media may be more fugitive than minimal. For example, Arterton noted with apparent equanimity the disjuncture between what social scientists have demonstrated and what campaigners believe about media effects: "Political scientists studying the impact of listening to or reading reported news have been unable to document significant effects upon the attitudes, cognitions, or behavior of citizens. The effect may be there, but we have not been able to demonstrate it. In any case, political science findings notwithstanding, those who manage presidential campaigns operate on the conviction that what the media say about them will affect their candidates' votes on election day" (1978, 4).

The scholarly effort to document significant media effects has been bedeviled by a variety of methodological difficulties. In aggregate-level time-series analyses, it is usually impossible to distinguish the effects of the media themselves from the effects of the events they report. In individual-level cross-sectional studies, differences in opinions between those exposed to the media and those who remain unexposed may simply reflect preexisting differences between the two groups in political attitudes or characteristics. Self-reports of media exposure may be biased in ways that produce artificial correlations with political opinions, especially when the media exposure questions being used refer to specific candidates or issues. For all of these reasons, findings of significant impacts and minimal effects alike have seldom been wholly convincing.

Experimental research elegantly avoids many of these inferential pitfalls. Thus, it should not be surprising that the most convincing demonstrations of media exposure effects to date have come from laboratory settings (Iyengar 1991; Iyengar and Kinder 1987). Nevertheless, experimental methods have their own considerable limitations, primarily with respect to external validity. For this reason, if no other, it behooves us to explain—and to reassess—the pervasive pattern of negative findings and nonfindings in the nonexperimental literature on media effects.

The present work attributes this pervasive pattern of negative findings and nonfindings in part to limitations of research design and in part to carelessness regarding measurement. I do not intend to suggest that all of the findings of minimal effects in the existing scholarly literature are due simply to methodological limitations. Indeed, my own analysis of a varied collection of political opinions and perceptions in one eight-month presidential campaign season provides ample evidence that media exposure only occasionally produces strong, unidirectional opinion changes. Part of my aim in analyzing changes in a broad range of specific campaign opinions and per-

ceptions is to present a clearer sense of just how common such strong, unidirectional media exposure effects may be in a presidential campaign setting. But in addition, I hope to present a clearer sense of both why such effects are not more common than they are and why they are more common than previous analyses have generally been able to demonstrate. By reconceptualizing what sorts of media effects we might expect to find and how we might expect to find them given the nature of the available data, I attempt to cast both new and old evidence in a very different light.

A MODEL OF MEDIA EFFECTS

According to Graber, "People who are exposed to the mass media already possess a fund of knowledge and attitudes which they bring to bear on new information. Since we do not know precisely what this information is, nor the rules by which it is combined with incoming information, we cannot pinpoint the exact contribution which mass media make to the individual's cognitions, feelings, and actions" (1980, 11). I propose a model of opinion formation that can help to "pinpoint the exact contribution which mass media make to the individual's cognitions, feelings, and actions" by overcoming precisely the difficulties Graber identifies.

The "fund of knowledge and attitudes" respondents bring to bear as they are exposed to the media is addressed in the model by incorporating preexisting opinions as explanatory variables. Obviously, this approach requires repeated interviews with a panel of survey respondents.

Even with access to panel data, "we do not know precisely what this information is" because our measurements of preexisting knowledge and attitudes are usually quite imperfect. It should be well known (e.g., from Achen 1983) that measurement error in any explanatory variable will generally bias parameter estimates for every explanatory variable in a multiple regression model. Since analyses of media impact typically include explanatory variables subject to serious measurement error (including media exposure itself and, in panel studies, lagged values of the dependent variable), the biases introduced by treating all of these observed variables as error-free indicators of the underlying opinions and behaviors are likely to be both substantial in magnitude and unpredictable in direction. The analytic implications of measurement error are addressed here by distinguishing, both in the formulation of the model and in the subsequent data analysis, between the underlying variables of theoretical interest and observable indicators of those variables.

Finally, "the rules by which [preexisting information] is combined with incoming information" are modeled by the assumption that respondents use new information from the mass media and elsewhere to update their political opinions rationally in accordance with Bayes' Rule. Bayes' Rule may or may not be a realistic behavioral model; but it is certainly a useful accounting device—in particular because it provides a systematic way to characterize both the relative weight of old and new information in people's current opinions and the nature and sources of the new information they have absorbed between any two opinion readings.

My model represents an individual i's opinion about some political stimulus j at time t as a Normal probability distribution with mean θ_{ijt} and variance $1/\pi_{ijt}$. (It may be helpful to think of θ_{ijt} as representing the "location" of the opinion and π_{ijt} as representing the certainty or "precision" of the opinion.) Given Bayesian updating, the relationship between this opinion at time t and the corresponding opinion at any previous time s is

$$\theta_{ijt} = \theta_{ijs}\pi_{ijs}/(\pi_{ijs} + \omega_{ijt}) + \mu_{ijt}\omega_{ijt}/(\pi_{ijs} + \omega_{ijt})$$

$$\pi_{ijt} = \pi_{ijs} + \omega_{ijt},$$

where μ_{ijt} and ω_{ijt} represent the location and precision, respectively, of a Normal probability distribution representing new information (a "message") received between time s and time t. The precision of the opinion at time t, π_{ijt}, is equal to the sum of the prior precision (π_{ijs}) and the message precision (ω_{ijt}); the location of the opinion at time t, θ_{ijt}, is a weighted average of the prior location (θ_{ijs}) and the message location (μ_{ijt}), each weighted by its relative precision.

This model is obviously too general as it stands, since nothing in it is directly observable and everything varies both across individuals and over time. We might make some progress by introducing observable measures of subjective information corresponding to the unobserved variables π_{ijs} and π_{ijt}; but the resulting nonlinear model is difficult, in practice, to estimate. In view of this difficulty, a tempting alternative approach is to treat the ratio π_{ijs}/π_{ijt} as a constant parameter λ_{jt} for all i. This assumption implies that individuals vary in how much they know (or rather, in how much they think they know) at any given time, but that the amount of new (subjective) information acquired by any individual in a given time interval is proportional to the amount of (subjective) information that individual already possesses at the beginning of the interval.[1] Adopting this simplification gives

$$\theta_{ijt} = \theta_{ijs}\,\lambda_{jt} + \mu_{ijt}(1 - \lambda_{jt}), \qquad (1)$$

which represents each individual's opinion at time t (θ_{ijt}) as the same weighted average of prior opinions (θ_{ijs}) and new information (μ_{ijt}).

I assume that the relationships between the unobserved variables θ_{ijt} and μ_{ijt} and the observed data are of the forms

$$\theta_{ijt} = Y_{ijt} - \delta_{ijt}$$

and

$$\mu_{ijt} = (\mathbf{X_{it}} - \zeta_{it})\boldsymbol{\alpha_{jt}} + \varepsilon_{ijt}, \qquad (2)$$

where Y_{ijt} is an opinion reading (including measurement error); $\mathbf{X_{it}}$ is a vector of observations (including measurement error) of exogenous characteristics related to the message that individual i absorbs about stimulus j at time t; δ_{ijt} and $\boldsymbol{\zeta_{it}}$ are random variables representing the measurement error in the observed data Y_{ijt} and $\mathbf{X_{it}}$, respectively; ε_{ijt} is a random variable representing additional components of the message μ_{ijt} absorbed by individual i; and $\boldsymbol{\alpha_{jt}}$ is a vector of parameters to be estimated. Thus, the mean of the distribution representing opinion at time t is assumed to be measured directly but with error by the observed variable Y_{ijt}, whereas the mean of the distribution representing the new information received between time s and time t is assumed to be unobservable but linearly related to a vector of exogenous characteristics, which are in turn measured directly but with error by the observed variables $\mathbf{X_{it}}$.

Substituting these relationships into equation 1 and rearranging gives

$$\begin{aligned} Y_{ijt} &= (Y_{ijs} - \delta_{ijs})\lambda_{jt} + (\mathbf{X_{it}} - \boldsymbol{\zeta_{it}})\boldsymbol{\alpha_{jt}}(1 - \lambda_{jt}) \\ &\quad + \varepsilon_{ijt}(1 - \lambda_{jt}) + \delta_{ijt} \\ &= (Y_{ijs} - \delta_{ijs})\lambda_{jt} + (\mathbf{X_{it}} - \boldsymbol{\zeta_{it}})\boldsymbol{\beta_{jt}} + u_{ijt}. \qquad (3) \end{aligned}$$

This model takes the relatively simple form of an errors-in-variables regression model with dependent variable Y_{ijt}, explanatory variables Y_{ijs} (with associated parameter λ_{jt}) and $\mathbf{X_{it}}$ (with associated parameter vector $\boldsymbol{\beta_{jt}} = \boldsymbol{\alpha_{jt}}(1 - \lambda_{jt})$), and disturbance term $u_{ijt} = \varepsilon_{ijt}(1 - \lambda_{jt}) + \delta_{ijt}$.[2]

DATA

The data employed here to estimate the parameters of the model are from the 1980 American National Election Study (NES) panel.[3] The data consist of a variety of opinion readings at three time points for a national cross-section of 758 survey respondents (the survivors from a first-wave sample of 1,008). The first wave of interviews was conducted in late January and February (before the first primary voting in New Hampshire), the second wave in June (between the end of the primary season and the national nominating conventions), and the third wave in September (during the first month of the general election campaign).

My analysis utilizes two distinct measures of media exposure. The only relevant item included in the three waves of the 1980 NES survey employed here focuses specifically upon exposure to television network news: "How often do you watch the national network news on early evening TV—every evening, 3 or 4 times a week, once or twice a week, or less often?" The mean levels of network news exposure derived from translating responses to this question onto a 0 (minimum exposure) to 1 (maximum exposure) scale declined slightly over the course of the campaign season, from .71 in February to .66 in June to .64 in September.[4]

In the first two waves of the 1980 NES survey, respondents were also asked, "Do you read a *daily* newspaper regularly?" This question was omitted in the third wave of the survey but included again in a fourth (postelection) wave (albeit without the adverb), making it possible to exploit the availability of three-wave panel data for newspaper exposure, as well as television news exposure. Moreover, newspaper reading appears to be a sufficiently stable behavior to warrant using June exposure as a proxy for September exposure in the analysis that follows.[5]

In order to allow for the importance of partisan learning and reinforcement effects (Berelson, Lazarsfeld, and McPhee 1954; Conover and Feldman 1989; Finkel 1990), all of the analyses reported here incorporate party identification as an exogenous influence on the nature of the message μ_{ijt} received during each time period.[6] Partisan predispositions are measured quite reliably by the traditional party identification item in the NES survey[7] (recoded here to range from −1 for "strong Democrats" to +1 for "strong Republicans") and are exceedingly stable over the course of the campaign season.[8]

Most previous analyses of media exposure effects have been vulnerable to the argument that the apparent effects of media exposure actually reflect the impact of politically relevant social characteristics that happen to be correlated with media exposure. For example, since older people, the well-educated, and blacks are all disproportionately likely to watch television or read newspapers, any systematic opinion changes among these groups might easily be mistaken for effects of media exposure.[9] The argument is less compelling when party identification is already included as an explanatory variable, since the most likely source of systematic opinion change among particular groups in a political campaign is partisan "activation" (Berelson, Lazarsfeld, and McPhee 1954). Nevertheless, to further guard against the possibility of estimating spurious media effects, all of the analyses reported here include age, education, and race as additional exogenous control variables.[10]

The effects of media exposure were separately estimated for each half of the campaign year (February–June and June–September) for each of 37 distinct perceptions and opinions regarding the presidential candidates, their character traits, their issue positions, the respondents' own issue preferences, and (in the case of incumbent Jimmy Carter) various aspects of job performance. To facilitate comparison, all of the original responses were recoded to range from 0 to 100, with 0 denoting the most negative possible opinion and 100 the most positive possible opinion.[11]

The key to dealing with the biases created by measurement errors in explanatory variables is to obtain estimates of the magnitudes of those measurement errors. Repeated measurement of the same opinion or behavior at three or more time points provides leverage for distinguishing between change in underlying "true" opinions and random measurement error. Here, the magnitudes of measurement

errors are estimated using a variant of the Wiley and Wiley (1970) model.[12] The main assumptions underlying the model are that the measurement process produces constant error variance in each wave of the panel and that measurement errors for the same respondent in different waves of the panel are uncorrelated.[13]

Responses to the media exposure questions appear to contain a moderate amount of measurement error. Applying the modified Wiley–Wiley measurement model to the three available waves of panel data, the standard errors of measurement (the square roots of the estimated measurement error variances) on the 0–1 scale are .16 for television exposure and .23 for the dichotomous measure of newspaper exposure. The corresponding "reliabilities" of the exposure measurements (the ratios of "true" variance to total variance) are .75 for television exposure and .78 for newspaper exposure, which suggests that about a quarter of the observed variance in these variables represents random noise.

Descriptive statistics for all variables in each wave of the panel, as well as measurement error estimates calculated from the modified Wiley–Wiley model, are presented in Appendix A. The standard errors of measurement vary from 10 or 12 points on the 100-point scale (for overall "thermometer" ratings) to 20 or 25 points (for Carter job approval). The corresponding measurement reliabilities also vary widely, from a high of .84 (for Carter thermometer ratings) to a low of .29 (for placements of Carter on the "government spending vs. services" issues scale); average reliabilities are about .61 for Carter job approval, .58 for issue preferences, .57 for candidate traits, and .46 for candidate issue placements. These reliabilities are, in most cases, considerably lower than for media exposure and party identification, and suggest that with the notable exception of the thermometer ratings, about 40% of the observed variance in the political perceptions represents random noise.

ESTIMATED EFFECTS OF MEDIA EXPOSURE

Errors-in-variables parameter estimates for the model of opinion change I have proposed earlier are reported in Appendix B for both campaign periods.[14] Figures 1 through 4 present the average estimated effects of television news exposure, newspaper exposure, party identification, and prior opinions from Appendix B for seven subsets of dependent variables: thermometer ratings, character traits, leadership traits, competence traits, Carter job approval ratings, candidate issue positions, and respondents' own issue preferences.[15]

The average magnitudes of the television news exposure effects in Figure 1 range from about 3 points on the 100-point scale in the case of thermometer ratings and issue placements to about 6 points in the case of competence traits and Carter approval ratings.[16] The newspaper exposure effects in Figure 2 follow a similar pattern but are, on average, about half as large, ranging from 1.5 points for thermometer ratings and issue positions to less than 2.5 points for Carter approval ratings.[17]

Effects of the magnitude portrayed in Figure 1 are especially impressive when we bear in mind that even "full exposure" to network television news represents, at most, a few hours of relevant coverage of each candidate spread over the entire campaign year. Robinson and Sheehan's content analysis of the news media during the 1980 campaign suggests that a faithful viewer of *The CBS Evening News* from January through October was exposed to a total of about 15½ hours of presidential campaign coverage, of which about 10 hours were devoted to the "horse

FIGURE 1

Summary of Estimated Effects of Television News Exposure

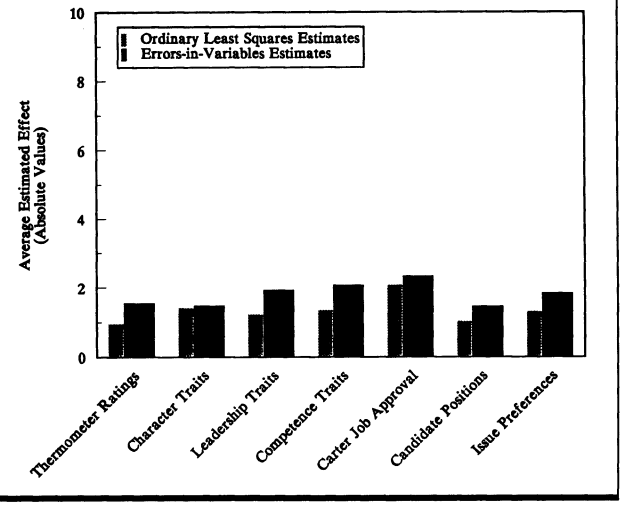


FIGURE 2

Summary of Estimated Effects of Newspaper Exposure

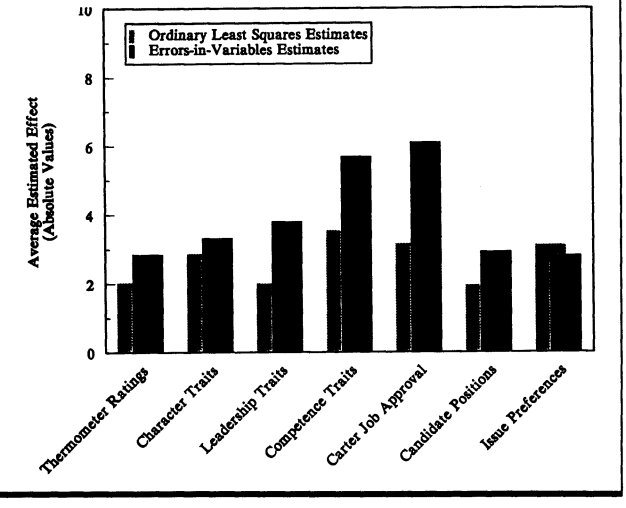
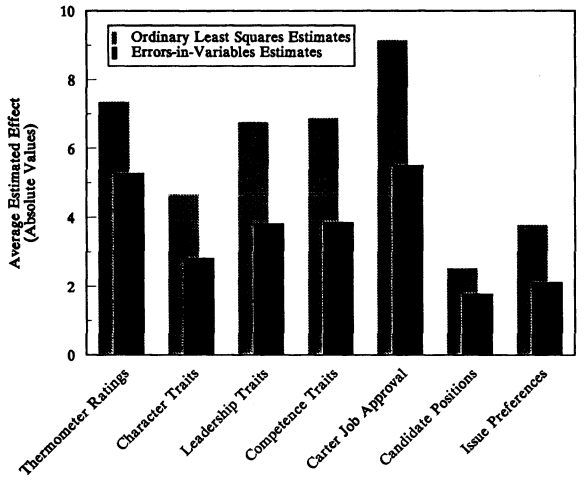


FIGURE 3

Summary of Estimated Effects of Party Identification

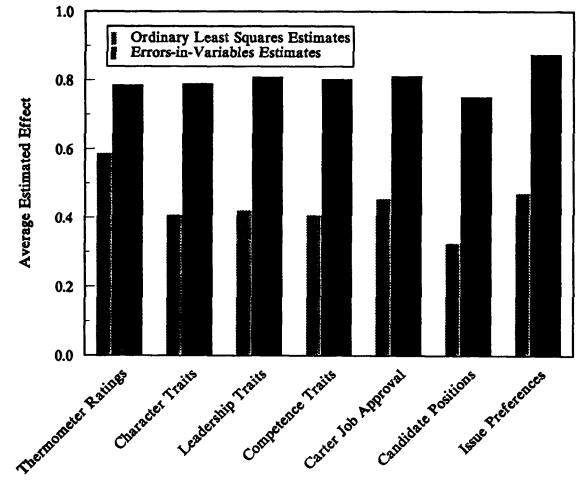


race" and about 5½ to "candidate information" and "policy issues" (1983, 149). Moreover, a significant fraction of that coverage occurred in the last month of the campaign, after the last interviews analyzed here. Even with some allowance for the effects of other sources of information that are likely to be correlated with exposure to television network news (most notably, debates and other public affairs broadcasts and advertising), the tendency of television viewers to make distinctive inferences about the candidates' personal traits, performance, and issue positions on the basis of relatively modest amounts of coverage is striking.

For purposes of comparison, Figures 1–4 also summarize the ordinary least squares estimates corresponding to the errors-in-variables estimates in Appendix B.[18] It is clear from the figures that the ordinary least squares estimates generally understate the effects of television news and newspaper exposure, in some cases by as much as 50%, while overstating the impact of party identification on opinion change and, most dramatically, understating the persistence of prior opinions. The magnitude and diversity of these divergences between ordinary least squares and errors-in-variables estimates highlight the inferential hazards of naive empirical analysis when explanatory variables are measured with significant error (Achen 1983).

FIGURE 4

Summary of Estimated Effects of Prior Opinions



The statistical fit of the model estimated in Appendix B (including prior opinions, party identification, demographic controls, and television news and newspaper exposure) is excellent.[19] But despite the overall goodness of fit of the model, none of the individual media exposure parameter estimates in Appendix B is very precise. More than half are smaller than their standard errors, since the standard errors themselves average almost 4 points on the 100-point scale for the television exposure effects and almost 2.5 points for the newspaper exposure effects.

However, it would be rash to infer from the prevalence of "insignificant" parameter estimates that there really are no underlying media exposure effects to be found. This is clear from further analysis of the candidate trait variables, where the availability of several measures of essentially similar traits can be exploited to refine the estimates of media exposure effects. For example, treating the "moral," "dishonest," and "power-hungry" traits explicitly as aspects of a more general *character* dimension makes it possible to estimate the effects of media exposure on perceptions of character more precisely than with the specific responses taken separately. The same is true for a *leadership* dimension made up of the "weak," "inspiring," and "strong leader" traits and for a *competence* dimension consisting of the "knowledgeable," "solve economic problems," and "develop good relations" traits.

Table 1 presents the results of covariance structure analyses for these three trait dimensions for each candidate. For each trait dimension, estimated effects of television news exposure, newspaper exposure, party identification, and prior opinion comparable to those reported in Appendix B are presented both for June (top) and September (bottom). Additional details of the covariance structure analysis, including the estimated relationships between each general trait dimension and its specific observable indicators, are reported in Appendix C.

Grouping the character traits into broader evaluative dimensions reduces the average standard error of the media exposure parameter estimates by almost a third. The result is to highlight the general pattern of media exposure effects already evident in Appendix B. Thus, for some trait dimensions, the additional precision of the parameter estimates makes it even clearer in Table 1 than in Appendix B that media exposure did, indeed, produce minimal opinion change. In other cases, the results in Table 1 make it even clearer that the observed effects of media expo-

TABLE 1

Covariance Structure Parameter Estimates for Carter and Reagan Trait Dimensions

TRAIT DIMENSIONS	TV NEWS EXPOSURE	NEWS EXPOSURE	PARTY ID	PRIOR OPINION	S.E. OF REGR.	$X^2_{(134)}$
			June			
Carter						
Character	−1.1	−.7	−1.6	.820		
	(2.8)	(1.7)	(1.2)	(.076)	10.5	297.0
Leadership	−2.6	−3.2	−4.1	.787		
	(2.7)	(1.7)	(1.2)	(.050)	9.1	272.7
Competence	−4.8	−3.5	−2.3	.955		
	(2.7)	(1.6)	(1.2)	(.069)	3.7	333.2
Reagan						
Character	−.2	.5	2.0	.774		
	(2.5)	(1.5)	(1.0)	(.064)	8.1	333.3
Leadership	.4	.7	5.1	.709		
	(2.5)	(1.5)	(1.0)	(.057)	9.8	295.9
Competence	4.1	1.8	5.1	.764		
	(2.5)	(1.5)	(1.1)	(.078)	8.0	282.3
			September			
Carter						
Character	3.8	1.6	−1.2	.802		
	(2.8)	(1.5)	(1.1)	(.059)	8.2	297.0
Leadership	.4	1.8	−1.9	.883		
	(2.8)	(1.6)	(1.2)	(.051)	7.0	272.7
Competence	4.2	1.0	−2.3	.995		
	(3.0)	(1.7)	(1.2)	(.069)	4.8	333.2
Reagan						
Character	.9	3.6	5.6	.872		
	(2.7)	(1.5)	(1.1)	(.065)	7.4	333.3
Leadership	−7.8	.1	3.3	.859		
	(2.8)	(1.6)	(1.2)	(.066)	10.4	295.9
Competence	−9.9	−.4	5.4	.817		
	(2.9)	(1.7)	(1.4)	(.078)	10.1	282.3

Note: Parameter estimates are analogous to those presented in Appendix B. Components of trait dimensions are specified in Appendix C. Intercepts and parameter estimates for age, education, and race are not shown. Asymptotic standard errors of parameter estimates in parentheses. Number of observations = 758.

sure are much too large to be attributed to mere sampling variability. Even in the case of newspaper exposure, where the effects are generally much smaller than for television news exposure, the three largest of the 12 t-statistics in Table 1 are 2.4, 2.2, and 1.9, much larger than could be expected on the basis of chance if all of the true effects were zero. It seems clear from this more refined analysis that the results in Appendix B should likewise be read as evidence of some very strong media exposure effects *and* some minimal effects, with a liberal admixture of pure noise due to the inherent imprecision of the individual parameter estimates.

MESSAGES RECEIVED

The results summarized in Figures 1 and 2 demonstrate that attention to measurement error is sufficient by itself to produce some upward revision in the apparent impact of media exposure. But the implications of the analysis presented here for our understanding of media effects are considerably greater than the comparisons in Figures 1 and 2 alone suggest. A more significant—but also more subtle—revision in our understanding of the role of the media is necessitated by the comparison in Figure 4, which indicates that allowing for the effects of measurement error produces a picture of much more stable political opinions over the course of a campaign season.

The comparison summarized graphically in Figure 4 is represented somewhat differently in the first two columns of Table 2, which compares the errors-in-variables and ordinary least squares estimates of the fraction of total information apparently gained during the campaign (i.e., between the first wave of NES interviews in February and the final preelection wave in September) for each of the 37 opinions and issue

TABLE 2

Campaign Information and Distinctive Campaign Messages, February–September 1980

	CAMPAIGN INFORMATION		TV NEWS EXPOSURE		NEWS. EXPOSURE		PARTY ID	
DEPENDENT VARIABLES	*EV*	*OLS*	*EV*	*OLS*	*EV*	*OLS*	*EV*	*OLS*
Thermometer ratings								
Carter	.372	.606	1.0	4.8	−5.5	−1.0	−25.1	−20.3
Reagan	.408	.704	14.4	5.0	7.2	2.0	23.3	15.4
Carter job approval								
Overall	.380	.727	−4.7	6.3	1.3	−3.0	−31.7	−23.2
Iran	.490	.809	−5.8	.0	−12.1	−4.0	−27.5	−18.5
Inflation	.192	.769	−16.2	.5	−29.1	−5.1	−44.4	−20.7
Unemployment	.455	.826	−11.1	−1.2	−8.7	−2.9	−27.2	−16.6
Energy	.261	.844	−2.2	1.6	−23.1	−5.9	−28.0	−15.7
Issue positions								
Lib./con. ideology	.073	.718	4.5	−.1	10.3	4.0	46.8	10.0
Spending vs. services	.306	.826	−2.3	−4.1	2.7	−1.2	7.8	9.8
Defense spending	.248	.741	22.8	11.9	−10.1	−1.7	12.3	5.3
Relations with Russia	.318	.819	−14.2	.4	5.7	2.3	1.2	2.7
				Carter				
Traits								
Moral	.218	.794	3.5	4.6	4.9	4.3	−7.5	−5.7
Dishonest	.536	.895	−19.5	−8.2	1.1	−.7	4.7	4.5
Power-hungry	.394	.802	.5	2.6	−4.1	−2.8	9.3	8.3
Weak	.388	.838	4.8	−1.6	.8	−1.9	12.7	11.1
Inspiring	.352	.826	.8	5.1	−4.5	−.9	−17.2	−12.1
Strong leader	.381	.747	−3.2	2.4	−2.8	.5	−18.7	−14.6
Knowledgeable	.449	.855	23.8	13.6	−2.4	.4	−8.5	−8.7
Solve economic problems	.282	.820	−14.5	.9	−12.2	−1.4	−10.4	−12.6
Develop good relations	.184	.828	−15.7	−.3	−7.8	−2.2	−28.5	−13.4
Positions								
Lib./cons. ideology	.206	.850	−.7	.6	4.8	.2	−23.5	−7.9
Spending vs. services	.363	.937	−16.5	−5.0	−2.9	−1.7	.7	−.2
Defense spending	.463	.916	−1.1	1.0	2.6	2.8	−4.8	−5.5
Relations with Russia	.804	.975	−5.6	−.2	2.3	2.2	−5.6	−5.1
				Reagan				
Traits								
Moral	.349	.832	−1.8	3.8	15.6	3.9	17.6	9.3
Dishonest	.522	.869	−11.0	−9.1	−8.8	−4.5	−11.5	−7.8
Power-hungry	.228	.796	3.8	−1.9	−4.9	−.5	−46.2	−17.0
Weak	.333	.942	25.3	3.6	−15.2	−4.8	−17.5	−8.1
Inspiring	.484	.745	−19.5	−5.2	1.0	−1.1	19.7	13.7
Strong leader	.191	.798	−35.7	−4.2	.4	−1.3	40.2	14.1
Knowledgeable	.460	.826	−12.1	−1.0	6.3	3.0	13.9	9.0
Solve economic problems	.460	.838	−19.8	−6.3	5.6	.3	21.9	13.8
Develop good relations	.327	.846	−16.8	−4.4	−1.5	−.8	34.4	16.5
Positions								
Lib./cons. idelogy	.195	.762	15.1	2.8	5.0	1.4	17.0	5.3
Spending vs. services	.458	.892	−8.8	−2.2	−3.5	−1.8	4.3	2.1
Defense spending	.558	.871	8.6	3.9	−.7	−.9	3.1	2.2
Relations with Russia	.438	.899	−12.2	−.2	−3.4	−.9	−3.9	−1.8

Note: "Campaign information" is the proportion of total September information received between February and September, estimated by $(1 - \hat{\lambda}_2 \hat{\lambda}_3)$, where $\hat{\lambda}_2$ and $\hat{\lambda}_3$ are the estimated effects of February opinions in June and of June opinions in September, respectively. "Distinctive messages" are differences in the content of messages received between February and September attributable to television news exposure, newspaper exposure, and party identification, estimated by $(\hat{\beta}_2 \hat{\lambda}_3 + \hat{\beta}_3)/(1 - \hat{\lambda}_2 \hat{\lambda}_3)$, where $\hat{\lambda}_2$ and $\hat{\lambda}_3$ are the estimated effects of February opinions in June and of June opinions in September, respectively, and $\hat{\beta}_2$ and $\hat{\beta}_3$ are the relevant estimated exposure effects in June and September, respectively. Errors-in-variables (EV) entries are based upon the parameter estimates in Appendix B. Ordinary least squares (OLS) entries are based upon analogous ordinary least squares regression estimates.

perceptions included in the analysis.[20] Since total information is, by definition, the sum of preexisting information and campaign information, the increased weight of preexisting information, once we take account of measurement error, necessarily produces a corresponding decrease in the relative weight of campaign information. The ordinary least squares estimates suggest that on average, more than 80% of the total information that respondents had at the time of the third NES interview in September had been gained since the first interview in February. The implication of these estimates is that campaign impressions dominate electoral politics, at least at the presidential level. By contrast, the errors-in-variables estimates suggest that on average, only a little more than one-third of the total information that respondents had in September had been gained since February. By these latter estimates, most of what people believed about both Carter and Reagan in the midst of the general election campaign was already fixed months earlier, before the public phase of the campaign had even begun.

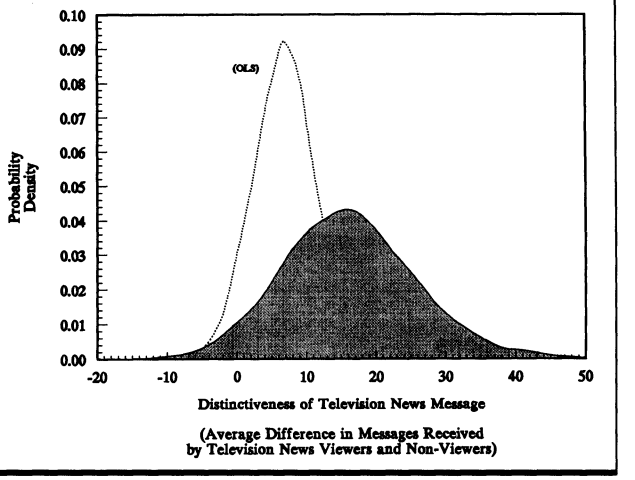
The difference between these two sets of estimates is of profound significance for any understanding of the electoral process. The tendency of measurement error to depress the apparent stability of political opinions has been widely recognized (Achen 1975; Feldman 1989). However, the implication of this tendency for analyses of opinion change has not generally been recognized. In terms of the Bayesian model of opinion change proposed earlier, new information must compete with a much greater mass of prior information than has generally been supposed and thus must itself be much more distinctive than has generally been supposed in order to produce the opinion changes that we actually observe. Thus (perhaps counterintuitively), evidence that preexisting opinions are very stable also suggests, albeit indirectly, that the political information required to produce systematic changes in those opinions must be very distinctive.

One advantage of the proposed Bayesian model is that it allows us to characterize precisely the new information, or "messages," required to account for observed opinion change during the course of a campaign. Moreover, the formulation in equation 2 makes it possible to assess how much these messages vary with media exposure, party identification, and other characteristics. When the (hypothetical) messages required to account for observed opinion changes vary systematically with media exposure, the implication is that media users were absorbing *distinctive* messages unavailable to those who remained unexposed.

This implication of the analysis is elaborated in the remaining columns of Table 2, which present estimates of the distinctiveness of the messages received during the 1980 campaign *from* television news and newspapers and *by* partisan identifiers, respectively.[21] For example, each entry in column 3 is an estimate of the difference between the messages received by a regular viewer of the network news and a nonviewer between February and September. In the notation of the model, these are estimates of the α_{jt} parameters for the effect of television news exposure on the location of the received message μ_{ijt} in equation 2.[22]

FIGURE 5

Estimated Distinctiveness of Television News Message for Reagan Thermometer Ratings, February–June 1980



The individual estimates vary a great deal and most are in any case quite imprecise. But the average difference in the messages received by regular television news viewers and nonviewers over this eight-month period is clearly on the order of 10 points on the 100-point scale. The corresponding average difference for regular newspaper readers and nonreaders is about half as large, while the average difference attributable to partisan predispositions is on the order of 15 or 20 points on the 100-point scale.[23]

The standard errors of these "distinctive message" estimates are impossible to calculate directly, since the estimates themselves are based on ratios of correlated parameter estimates from Appendix B. However, a good sense of the sampling variability of the estimates can be built up empirically by repeated sampling from the original correlated distributions. Figure 5 displays the estimated sampling distribution of message distinctiveness constructed in this way for a single, roughly typical case—the impact of television news exposure on changes in Reagan thermometer ratings between the February and June waves of the 1980 NES panel.[24]

For purposes of comparison, Figure 5 also displays the estimated sampling distribution of message distinctiveness based upon the ordinary least squares estimates of the same television news exposure effect. The specific example nicely illustrates the general tendency for ordinary least squares to underestimate the distinctiveness of the messages received by people regularly exposed to the mass media during the 1980 campaign. It was clear from Figures 1–4 that ordinary least squares significantly underestimates both the impact of media exposure and the stability of

preexisting opinions. The estimated message distinctiveness compounds these two underestimates, producing results that fall short by 57% in this case and by 67% on average. Thus, the messages conveyed by the media in the 1980 presidential campaign were about three times as distinctive as they appear from simple regression analysis.

CONCLUSION

Attention to the effects of measurement error significantly increases the apparent impact of media exposure on opinion change in a presidential campaign setting. Nevertheless, to the extent that analysts focus upon observable opinion change over relatively short periods of time, the apparent effects of media exposure will often be modest in magnitude even when adjusted for the effects of measurement error—not because the media cannot be persuasive but because opinions at the beginning of a typical presidential campaign are already strongly held and because media messages during the course of the campaign are, in any case, only occasionally sharply inconsistent with those preexisting opinions. By the logic of rational (Bayesian) opinion change, only discrepant messages can produce observable change—and then only in direct proportion to the subjective uncertainty of preexisting opinions.

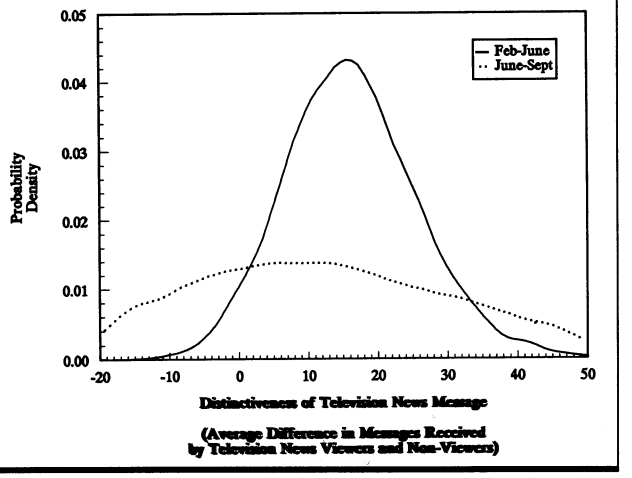
Media exposure is not directly implicated in the formation of these preexisting opinions themselves. But then, there is no way that it could be, given the nature of the available data.[25] A panel spanning eight months is far from a snapshot, but it is too truncated a moving picture to capture the full effect of media exposure on opinions and perceptions that were already well developed at the beginning of this eight-month period. In the absence of any direct access to the process of opinion formation prior to the campaign season, the most natural supposition is that it, too, was heavily dependent upon media exposure. However, the apparent stability of political opinions in a presidential campaign setting suggests that more direct and convincing demonstrations of significant opinion changes due to media exposure will require data collections spanning considerably longer time periods.

The logic of Bayesian opinion change pursued here also suggests that media exposure is most likely to be consequential (in the sense of producing large observable opinion changes) when prior opinions are weak, most notably for "new" candidates or issues. For this reason, simply as a matter of efficiency, analysts of media effects would do well to focus upon "new" or "uncrystallized" opinions, even if they are atypical or intrinsically less significant than opinions that are better established and more firmly held.

This point is illustrated in Figure 6, which compares the estimated sampling distribution of television news "message distinctiveness" for Reagan thermometer ratings between February and June 1980 (reproduced from Figure 5) with the corresponding

FIGURE 6

Estimated Distinctiveness of Television News Message for Reagan Ratings, February–June and June–September 1980



sampling distribution of message distinctiveness between June and September. The central tendencies of the two distributions suggest that the distinctive messages received from network television news were not wildly different for the two parts of the campaign; the medians for the two sampling distributions are 15.7 and 9.9, respectively. However, the sampling distribution for September is so diffuse that it provides little useful information about the real distinctiveness of the message received from television news during the latter part of the campaign. (More than 30% of the mass of the sampling distribution actually falls outside the range of values represented in Figure 6.) The main cause of this diffuseness is the significantly greater stability of prior opinions about Reagan in September than in June. As a result, the estimated impact of television exposure, which had a t-ratio of 1.8 in June, had a t-ratio of .4 by September—not because there was no impact to be measured, but because the stability of preexisting opinion by September reduced that impact to a level below the threshold of precision of the available data. Attempting to study media impact in settings with very stable prior opinions is a social scientist's equivalent of attempting to count galaxies through the wrong end of a telescope.

Finally, the analysis presented here highlights the political significance of distinctive, consistent media messages presented over relatively long periods of time. Many of the media exposure effects evident in the 1980 presidential campaign were not, ultimately, consequential in the final election outcome, either because positive effects in one period canceled out negative effects in another period (as with the effects of television news exposure on perceptions of Carter's competence and job performance) or because similar effects of roughly equal magnitude for both candidates canceled each other out. Consistent, distinctive media messages favoring one side or the

other in a political controversy are, by contrast, likely to produce sizable opinion changes over time (Zaller 1992). Thus, studies of when and why such consistent, distinctive media messages get produced should be among the highest priorities for research on the political impact of the mass media.

The distinctive campaign messages attributed to media exposure in Table 2 are doubly imprecise, since they compound the imprecision of the estimated media exposure and prior opinion effects in Appendix B. The imprecision of the individual estimates is mitigated, for present purposes, by the consistency of results across a quite varied collection of campaign opinions and perceptions. But for more detailed analyses of specific cases, more precise estimation of the messages received by different survey respondents will be of crucial importance.

Better data will also be required to analyze the distinctive effects of various sorts of direct and indirect media exposure. Obviously, not all of the effects portrayed here are specifically attributable to network news and newspapers, since respondents most exposed to these specific media were probably also most exposed to debates, candidate advertisements, and other sources of campaign information. More elaborate media exposure data will be required to sort out the effects of these various information sources more fully.[26] On the other hand, comparisons based upon direct media exposure probably understate to a considerable extent the total impact of the media, since they make no allowance for indirect exposure of the sort posited by Barr's Law.[27] Better data may eventually make it possible to trace these indirect effects of the media upon people who are not directly exposed to their distinctive messages.

Much remains to be done. Nevertheless, the view that media exposure has "minimal effects" in political campaigns appears, on the basis of the analysis presented here, to be due in significant part to inattention to the implications of measurement error, combined with the tendency of previous research designs to focus upon significant short-term opinion change in circumstances where such change is likely to be quite modest. More careful analysis, together with an alternative focus upon the distinctiveness of the messages received by people exposed to the media, casts the pervasive political impact of the modern mass media in a clearer light.

APPENDIX A

TABLE A-1

Descriptive Statistics and Measurement Error Estimates

VARIABLE	MEANS S.D. FEB.	JUNE	SEPT.	STD. ERROR OF MEASUREMENT	AVERAGE COEFFICIENT OF RELIABILITY
Television news exposure	.708	.662	.644	.158	.75
	.317	.316	.315		
Newspaper exposure	.621	.600	.623	.227	.78
	.485	.490	.476		
Party identification	−.124	−.099	−.116	.231	.88
	.657	.676	.678		
Carter overall job approval	55.3	36.9	39.2	19.7	.71
	36.6	35.6	36.7		
Carter approval					
Iran	57.2	33.6	35.5	24.8	.62
	42.1	39.8	39.0		
Inflation	34.1	26.2	32.9	21.3	.60
	34.3	32.9	34.0		
Unemployment	47.7	30.1	35.0	21.1	.59
	33.5	32.4	33.6		
Energy	34.6	27.1	40.2	24.4	.52
	36.7	34.0	35.2		
Issue Preferences					
Lib./con. ideology	54.4	53.7	55.2	11.0	.63
	17.6	18.5	18.5		
Spending/services	42.6	44.1	45.9	18.2	.57
	28.4	27.8	27.1		
Defense spending	69.1	65.6	68.6	14.5	.60
	23.0	23.3	22.7		
Relations with Russia	53.2	54.6	54.4	20.7	.53
	32.6	30.0	28.4		

TABLE A-1 continued

Descriptive statistics and Measurement Error Estimates

VARIABLE	MEANS S.D. FEB.	JUNE	SEPT.	STD. ERROR OF MEASUREMENT	AVERAGE COEFFICIENT OF RELIABILITY
Carter					
Thermometer rating	63.5 24.3	53.3 26.4	56.2 26.3	10.4	.84
Moral	74.1 23.5	70.8 24.4	70.8 24.5	15.9	.57
Dishonest	16.4 23.7	21.3 24.3	24.8 23.4	17.0	.49
Power-hungry	31.9 29.4	39.6 30.4	44.8 29.8	19.0	.60
Weak	40.7 30.1	49.7 29.7	44.9 26.9	19.7	.53
Inspiring	45.9 29.0	38.7 26.4	41.1 26.1	17.9	.56
Provide strong leadership	47.2 27.1	37.0 27.0	40.3 27.4	14.9	.70
Knowledgeable	65.3 25.2	59.8 26.4	61.1 23.8	17.4	.52
Solve economic problems	40.0 25.0	32.0 23.2	34.3 24.2	15.7	.58
Develop good relations	55.3 27.1	44.2 27.3	48.7 27.6	18.8	.53
Lib./cons. ideology	53.1 17.9	49.2 20.0	47.6 18.1	13.5	.47
Govt. spending/services	38.1 20.6	41.5 20.4	40.1 19.1	16.8	.29
Defense spending	59.2 21.4	51.0 20.7	50.8 19.9	15.9	.41
Relations with Russia	44.9 28.8	40.9 24.8	42.2 22.6	19.6	.39
Reagan					
Thermometer rating	52.5 23.6	58.4 23.3	56.2 25.0	11.7	.76
Moral	62.6 22.2	63.7 21.8	61.5 23.1	14.6	.57
Dishonest	27.5 23.7	26.5 22.7	25.6 24.0	15.2	.58
Power-hungry	48.9 28.2	49.1 28.0	53.2 30.4	18.8	.57
Weak	31.6 23.2	28.8 21.7	28.6 24.7	18.0	.39
Inspiring	45.9 25.7	48.1 25.8	44.8 27.6	13.5	.74
Provide strong leadership	52.5 23.9	55.5 24.5	51.7 27.3	16.3	.58
Knowledgeable	61.0 21.8	59.3 22.9	58.5 26.0	14.3	.63
Solve economic problems	46.3 22.5	49.4 22.5	44.2 24.2	14.4	.61
Develop good relations	49.0 23.3	51.2 22.9	46.3 26.0	15.7	.57
Lib./cons. ideology	60.4 21.3	63.3 21.8	62.6 20.8	13.6	.59
Govt. spending/services	53.7 19.8	54.7 19.8	55.5 21.3	14.4	.50
Defense spending	60.0 20.5	61.9 19.5	69.6 21.4	13.0	.59
Relations with Russia	55.4 22.6	54.2 20.8	57.1 21.6	15.7	.47

Note: All variables except network news exposure (0–1), newspaper exposure (0–1), and party identification (−1–1) are recoded to vary between 0 and 100. Number of observations = 758.

APPENDIX B

TABLE B-1

Errors-in-Variables Parameter Estimates, June 1980

DEPENDENT VARIABLES	TV NEWS EXPOS.	NEWSP. EXPOS.	PARTY ID	FEB. OPIN.	S.E. OF REGR.	$X^2_{(30)}$
			Thermometers			
Carter	−2.6	.5	−5.9	.810	13.3	61.1
	(3.1)	(1.9)	(1.4)	(.055)		
Reagan	5.2	3.0	7.8	.667	11.7	30.8
	(3.0)	(1.8)	(1.3)	(.051)		
			Carter Approval			
Overall	−9.5	1.1	−6.1	.683	17.9	56.6
	(4.8)	(3.0)	(2.1)	(.057)		
Iran	−4.9	−2.6	−7.1	.557	23.2	37.2
	(5.9)	(3.6)	(2.3)	(.053)		
Inflation	−3.4	−0.8	−2.2	.818	11.0	49.5
	(4.8)	(2.9)	(2.2)	(.071)		
Unemployment	−14.1	−0.6	−2.8	.594	18.0	48.9
	(5.0)	(3.0)	(2.0)	(.069)		
Energy	6.0	−3.7	−4.4	.739	11.2	55.8
	(5.3)	(3.2)	(2.3)	(.073)		
			Issue Positions			
Lib./cons. ideology	−.6	−1.7	4.1	.927	5.5	49.3
	(2.6)	(1.6)	(1.1)	(.070)		
Govt. spending vs. services	−5.6	−.1	.6	.781	11.6	44.6
	(4.1)	(2.6)	(2.0)	(.098)		
Defense spending	1.4	−1.0	2.8	.893	8.0	39.2
	(3.4)	(2.1)	(1.3)	(.083)		
Relations with Russia	.4	−2.7	3.4	.738	10.0	50.8
	(4.6)	(2.8)	(1.8)	(.065)		
			Carter Traits			
Moral	−3.3	.7	−2.7	.892	8.9	51.2
	(3.8)	(2.3)	(1.6)	(.121)		
Dishonest	−5.8	.3	2.8	.678	12.3	43.8
	(3.9)	(2.4)	(1.6)	(.141)		
Power-hungry	2.9	1.7	.1	.753	15.8	40.9
	(4.6)	(2.8)	(2.0)	(.095)		
Weak	5.0	2.5	6.5	.735	10.9	43.1
	(4.3)	(2.7)	(1.9)	(.074)		
Inspiring	−3.6	−.9	−5.7	.690	7.5	50.8
	(3.7)	(2.3)	(1.6)	(.063)		
Provide strong leadership	.6	−4.3	−4.5	.686	13.5	48.3
	(3.7)	(2.3)	(1.6)	(.063)		
Knowledgeable	3.4	.3	−.8	.877	10.4	38.2
	(4.1)	(2.5)	(2.1)	(.139)		
Solve economic problems	−5.1	−4.9	−4.4	.725	7.1	47.5
	(3.4)	(2.1)	(1.4)	(.074)		
Develop good relations	−7.0	−3.7	−2.1	.844	5.6	50.9
	(4.2)	(2.5)	(1.9)	(.104)		

TABLE B-1 continued

Errors-in-Variables Parameter Estimates, June 1980

DEPENDENT VARIABLES	TV NEWS EXPOS.	NEWSP. EXPOS.	PARTY ID	FEB. OPIN.	S.E. OF REGR.	$X^2_{(30)}$
			Carter Issue Positions			
Lib./cons. ideology	−4.8	.7	−2.9	1.000	6.2	42.1
	(3.2)	(2.0)	(1.3)	(.008)		
Govt. spending vs. services	−2.8	.1	−.9	.835	5.6	33.2
	(3.5)	(2.2)	(1.4)	(.165)		
Defense spending	−2.1	2.4	−1.3	.662	8.5	33.5
	(3.3)	(2.1)	(1.5)	(.115)		
Relations with Russia	−5.5	−1.4	−2.2	.267	12.8	48.6
	(4.0)	(2.4)	(1.6)	(.057)		
			Reagan Traits			
Moral	1.6	.4	1.4	.768	8.3	30.8
	(3.3)	(2.0)	(1.4)	(.092)		
Dishonest	−2.5	−.6	−1.9	.577	11.8	39.4
	(3.5)	(2.1)	(1.4)	(.082)		
Power-hungry	3.4	−.4	−3.7	.812	10.0	37.9
	(4.2)	(2.6)	(1.8)	(.089)		
Weak	−.4	.3	−2.9	.667	4.8	31.4
	(3.6)	(2.2)	(1.5)	(.127)		
Inspiring	−1.3	−.7	6.6	.668	14.3	39.8
	(3.5)	(2.2)	(1.4)	(.063)		
Provide strong leadership	1.5	1.4	5.3	.809	9.4	33.7
	(3.7)	(2.3)	(1.6)	(.098)		
Knowledgeable	3.8	1.4	2.5	.661	13.5	33.9
	(3.5)	(2.2)	(1.5)	(.110)		
Solve economic problems	.6	5.4	6.9	.654	10.8	31.6
	(3.2)	(2.0)	(1.5)	(.083)		
Develop good relations	6.0	−.5	5.8	.780	7.2	36.1
	(3.4)	(2.1)	(1.5)	(.099)		
			Reagan Issue Positions			
Lib./cons. ideology	−1.4	1.6	2.1	.908	6.2	30.1
	(3.1)	(1.9)	(1.2)	(.093)		
Govt. spending vs. services	−3.9	.1	−.4	.763	5.6	33.0
	(3.1)	(1.9)	(1.2)	(.148)		
Defense spending	3.4	−1.9	2.0	.630	8.5	36.0
	(2.9)	(1.7)	(1.1)	(.086)		
Relations with Russia	.2	−4.2	1.6	.562	12.8	39.4
	(3.3)	(2.0)	(1.3)	(.076)		

Note: Intercepts and parameter estimates for age, education, and race not shown. Asymptotic standard errors of parameter estimates in parentheses. Number of observations = 758.

TABLE B-2

Errors-in-Variables Parameter Estimates, September 1980

DEPENDENT VARIABLES	TV NEWS EXPOS.	NEWSP. EXPOS.	PARTY ID	FEB. OPIN.	S.E. OF REGR.	$X^2_{(30)}$
			Thermometers			
Carter	2.4 (2.9)	−2.4 (1.7)	−4.8 (1.3)	.776 (.045)	10.9	61.1
Reagan	1.2 (3.3)	.3 (1.9)	2.6 (1.6)	.887 (.067)	11.3	30.8
			Carter Approval			
Overall	6.8 (4.8)	−.5 (2.8)	−6.5 (2.2)	.908 (.068)	9.6	56.6
Iran	1.6 (5.7)	−3.5 (3.3)	−6.9 (2.2)	.915 (.049)	<1	37.2
Inflation	.2 (5.1)	−4.8 (2.9)	−6.4 (2.1)	.987 (.057)	<1	49.5
Unemployment	7.9 (5.2)	−3.4 (2.9)	−9.8 (2.0)	.917 (.099)	8.5	48.9
Energy	−6.6 (5.8)	−2.3 (3.4)	−2.9 (2.2)	1.000 (.018)	<1	55.8
			Issue Positions			
Lib./cons. ideology	.9 (2.7)	2.4 (1.6)	−.7 (1.0)	1.000 (.026)	<1	49.3
Govt. spending vs. services	4.2 (4.3)	.9 (2.5)	1.9 (1.9)	.888 (.092)	6.6	44.6
Defense spending	4.5 (3.5)	−1.6 (2.0)	.7 (1.3)	.842 (.064)	7.0	39.2
Relations with Russia	−4.9 (4.8)	4.3 (2.8)	−2.8 (1.8)	.923 (.061)	<1	50.8
			Carter Traits			
Moral	3.7 (4.0)	.4 (2.3)	.7 (1.6)	.877 (.090)	9.4	51.2
Dishonest	−6.5 (4.0)	.4 (2.2)	.6 (1.6)	.685 (.120)	9.9	43.8
Power-hungry	−2.1 (4.6)	−3.0 (2.7)	3.6 (1.8)	.805 (.082)	11.5	40.9
Weak	−2.3 (4.3)	−1.8 (2.5)	−.5 (1.8)	.833 (.061)	<1	43.1
Inspiring	3.7 (4.2)	−.8 (2.4)	−.8 (1.9)	.938 (.083)	1.4	50.8
Provide strong leadership	−1.7 (3.7)	2.8 (2.2)	−3.1 (1.7)	.903 (.073)	8.5	48.3
Knowledgeable	8.6 (3.8)	−1.2 (2.2)	−3.3 (1.6)	.628 (.078)	8.0	38.2
Solve economic problems	1.0 (3.8)	1.4 (2.2)	−2.2 (1.7)	.990 (.092)	2.8	47.5
Develop good relations	3.9 (4.7)	2.2 (2.7)	−3.2 (2.0)	.968 (.098)	7.1	50.9
			Carter Issue Positions			
Lib./cons. ideology	3.6 (3.0)	.4 (1.8)	−2.6 (1.3)	.794 (.085)	2.1	42.1
Govt. spending vs. services	−3.9 (3.5)	−1.1 (2.1)	.9 (1.3)	.763 (.098)	<1	33.2

TABLE B-2

Errors-in-Variables Parameter Estimates, September 1980

DEPENDENT VARIABLES	TV NEWS EXPOS.	NEWSP. EXPOS.	PARTY ID	FEB. OPIN.	S.E. OF REGR.	$X^2_{(30)}$
Carter Positions (continued)						
Defense spending	1.2	−.8	−1.2	.811	4.2	33.5
	(3.5)	(2.1)	(1.5)	(.132)		
Relations with Russia	−.5	2.9	−2.9	.735	<1	48.6
	(4.2)	(2.4)	(1.6)	(.095)		
			Reagan Traits			
Moral	−1.9	5.1	4.9	.848	8.3	30.8
	(3.6)	(2.1)	(1.4)	(.091)		
Dishonest	−3.7	−4.1	−4.4	.829	10.5	39.4
	(3.9)	(2.2)	(1.5)	(.118)		
Power-hungry	−2.4	−.7	−7.0	.951	10.6	37.9
	(4.7)	(2.8)	(2.0)	(.088)		
Weak	8.8	−5.3	−3.0	1.000	11.4	31.4
	(4.8)	(2.7)	(1.8)	(.007)		
Inspiring	−8.4	1.1	4.5	.772	15.1	39.8
	(3.9)	(2.3)	(1.6)	(.072)		
Provide strong leadership	−8.3	−1.3	2.4	1.000	10.3	33.7
	(4.3)	(2.5)	(1.6)	(.013)		
Knowledgeable	−8.7	1.8	4.4	.818	14.6	33.9
	(4.1)	(2.3)	(1.7)	(.112)		
Solve economic problems	−9.6	−1.9	4.4	.826	10.5	31.6
	(3.6)	(2.1)	(1.8)	(.100)		
Develop good relations	−10.6	−.1	6.3	.863	12.3	36.1
	(4.2)	(2.4)	(1.9)	(.102)		
			Reagan Issue Positions			
Lib./cons. ideology	4.2	−.4	1.5	.887	4.3	30.1
	(3.2)	(1.8)	(1.2)	(.070)		
Govt. spending vs. services	−1.3	−1.7	2.3	.710	11.6	33.0
	(3.6)	(2.1)	(1.3)	(.121)		
Defense spending	2.4	1.0	.3	.701	12.8	36.0
	(3.5)	(2.0)	(1.3)	(.107)		
Relations with Russia	−5.5	2.7	−3.3	1.000	5.2	39.4
	(3.9)	(2.3)	(1.4)	(.009)		

Note: Intercepts and parameter estimates for age, education, and race not shown. Asymptotic standard errors of parameter estimates in parentheses. Number of observations = 758.

APPENDIX C

Table 1 in the text reports parameter estimates from a covariance structure analysis of candidate traits. The aim of that analysis is to refine the estimated media exposure effects in Appendix B by exploiting the availability of multiple indicators of essentially similar trait dimensions.[28]

The estimated factor loadings for the individual trait items are shown in Table C-1. To facilitate comparison between the media exposure effects in Table 1 and those in Appendix B, the factor loadings for each trait dimension in Table C-1 are normalized to have an average absolute value of 1.0. It is clear from the results in Table C-1 that all of the individual trait items are strongly related to the corresponding dimensions in ways that make good substantive sense. Moreover, the patterns of factor loadings are quite similar for the separate analyses of Carter and Reagan traits, except that the leadership dimension had a somewhat stronger impact on responses to the "weak" item for Carter and the "inspiring" item for Reagan. Furthermore (as with the opinion-change models for the individual indicators in Appendix B), these more elaborate dimensional models appear to fit the data well.[29]

TABLE C-1

Measurement Model Parameter Estimates for Carter and Reagan Trait Dimensions

TRAIT DIMENSIONS AND COMPONENT ITEMS	CARTER[a]		REAGAN[b]	
	FACTOR LOADING	ITEM-SPECIFIC STD. DEV.	FACTOR LOADING	ITEM-SPECIFIC STD. DEV.
Character				
Moral	.876 (.035)	16.6	.951 (.030)	15.3
Dishonest	−.864 (.029)	18.2	−.899 (.029)	17.2
Power-hungry	−1.260 (.041)	20.1	−1.151 (.038)	19.7
Leadership				
Weak	−.938 (.026)	20.2	−.628 (.032)	18.8
Inspiring	.965 (.022)	17.6	1.245 (.031)	15.0
Provide strong leadership	1.097 (.020)	15.9	1.127 (.025)	15.7
Competence				
Knowledgeable	.787 (.033)	19.3	.825 (.031)	17.5
Solve economic problems	1.106 (.029)	16.0	1.056 (.027)	15.1
Develop good relations	1.107 (.030)	19.5	1.118 (.025)	15.7

Note: Estimated effects of television news exposure, newspaper exposure, party identification, and prior opinions on latent trait dimensions are reported in Table 1. Asymptotic standard errors of parameter estimates in parentheses. Number of observations = 758.
[a] $\chi^2_{(134)}$ = 297.0, 272.7, and 333.2 for character, leadership, and competence, respectively.
[b] $\chi^2_{(134)}$ = 333.3, 295.9, and 282.3 for character, leadership, and competence, respectively.

Notes

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1. This assumption is consistent with the empirical literature on learning from the media, which suggests that people who are most informed about public affairs are typically most likely to absorb any given piece of new information (Price and Zaller 1992; Robinson and Levy 1986). It is worth noting that if the assumption held exactly, the unconstrained nonlinear model in which π_{ijs} and π_{ijt} are separate variables with parameters to be estimated would be underidentified; indeed, difficulties encountered in estimating the parameters of the unconstrained nonlinear model may be attributable to the approximate correctness of this simplifying assumption for relatively short-term processes of opinion change. The same simplifying assumption would presumably be less adequate for long-term processes of opinion change, where prior certainty (but not receptivity to new information) may be strongly related to age or experience (Achen n.d.).

2. The most straightforward way to assure that this model is identified is to assume that all the stochastic terms (ζ_{it}, δ_{ijs}, δ_{ijt}, and ε_{ijt}) have mean zero and are uncorrelated with each other and that we have consistent estimates of the measurement error variances $\sigma^2_{\zeta t}$, $\sigma^2_{\delta js}$, and $\sigma^2_{\delta jt}$. These assumptions can be relaxed somewhat when the right sorts of additional data are available, as in the following empirical analysis.

3. The data were originally collected by the Center for Political Studies of the Institute for Social Research, University of Michigan, under the direction of Warren E. Miller. They are available through the Inter-University Consortium for Political and Social Research.

4. These values are too high to be consistent with independent estimates of the size of the audience for network news. Since measurement error models of the sort employed in the following analysis cannot distinguish between stable "true" responses and consistent overreporting of news exposure, the parameter estimates for news exposure effects (but not the estimates of aggregate exposure effects) will be artificially attenuated by the inclusion of some overreporters among those apparently exposed to the news.

5. The estimated effect of February newspaper exposure on June newspaper exposure (in an equation including age, education, race, and party identification as control variables and adjusting for measurement error) is .927 (with a standard error of .052). The estimated effect of June newspaper exposure on postelection newspaper exposure in an equation including the same control variables, in spite of the slight change in question wording and some panel attrition, is .769 (with a standard error of .041). The average effect of June newspaper exposure on September opinions in the analysis that follows is 20% greater than the average effect of June newspaper exposure on June opinions. This difference parallels the 18% increase in the average effect of television news exposure from June to September, suggesting again that June newspaper exposure is an adequate proxy for September newspaper exposure.

6. It would be desirable from a theoretical standpoint to

allow for the possibility that party identification conditions the impact of media exposure (via selective perception) in addition to mattering in its own right. I am convinced, on the basis of some exploratory analysis, that partisanship and media exposure do interact significantly; but it is impossible to pursue that interaction rigorously here, given the other complexities of the statistical model and the limitations of the available data.

7. The estimated reliability of measured party identification is .88.

8. Given the logic of Bayesian updating, the stability coefficients for all variables should be less than or equal to 1.000. That constraint is binding for the estimated stability coefficients for party identification in the following analysis, but only barely: the corresponding unconstrained estimates (from a model including age, education, and race as control variables) are 1.008 (with a standard error of .026) from February to June and 1.005 (with a standard error of .021) from June to September.

9. The estimated effects of these demographic variables on television news exposure in the first wave of the panel (with standard errors in parentheses) are as follows: age (in years), .0050 (.0007); education (in years), −.001 (.004); black, .052 (.039); party identification, −.035 (.019). The estimated effects of these variables on newspaper exposure in the first wave of the panel are as follows: age, .0067 (.0010); education, .049 (.006); black, .100 (.059); and party identification, .001 (.029).

10. The parameter estimates associated with these control variables in the opinion change equations are omitted from the tables due to space constraints but are available from the author. It is, of course, conceivable that media exposure could be correlated with unmeasured causes of opinion change even after controlling for party identification, age, race, and education. Unfortunately, there is no obvious way to address this remaining potential endogeneity, since any available instrument for media exposure might itself be a direct cause of opinion change. Fortunately, potential biases of this sort seem unlikely to be very important, at least if we interpret the estimated media exposure effects broadly (as including the effects of correlated exposure to other campaign media), rather than narrowly (as specific effects of exposure to network television news and daily newspapers).

11. Issue positions were recoded so that 0 denotes the liberal endpoint and 100 the conservative endpoint of each issue scale.

12. The standard Wiley–Wiley measurement model is augmented here to make ("true") television news exposure in each wave of the panel a function of age, education, race, and party identification in addition to previous television news exposure, and likewise for newspaper exposure. In addition, disturbances for the television news and newspaper exposure equations are allowed to be correlated (though the correlations turn out to be small—.03 in June and −.13 in September).

13. With three waves of panel data for a single variable, the basic Wiley–Wiley model is just identified. Hence it is impossible to test its goodness of fit. Here, the availability of additional data makes it possible to test the goodness of fit of the model; and in every case, the fit is quite good. It is also possible to relax the conventional assumptions somewhat—for example, by allowing measurement error variances to differ across panel waves or by allowing measurement errors for different responses by the same respondent to be correlated. Having explored several modifications of this sort, I found none that produced more than marginal improvements in the statistical fit of the model and none that appreciably changed the substantive results. For example, allowing measurement errors for thermometer ratings of Jimmy Carter and Ronald Reagan to be correlated produces an estimated format-induced correlation of .20; but the average difference in the eight estimated media exposure effects resulting from this generalization of the model is only .12. For additional examples of alternative model specifications (again, with no appreciable impact on the substantive results of the analysis), see nn. 12, 28. These results are consistent with those reported by Feldman, who applied the Wiley–Wiley model to a variety of items similar to those used here (party identification, issue positions, and candidate evaluations) using data from a five-wave panel (for which the model was overidentified) and concluded that "the simple measurement model fits very well" (1989, 33, 38).

14. All of the parameter estimates that I report were produced using the generalized least squares routine in the EQS software package (Bentler 1989).

15. This classification is imposed purely for descriptive purposes in Figures 1–4 but reappears as the basis for a more explicit dimensional analysis in Table 1.

16. Some perspective on the magnitude of these effects may be gained by noting that in a probit analysis of vote choices, a three-point change in the thermometer rating for either candidate in the last month of the campaign translated into a corresponding change of up to 5 or 6 percentage points in the probability of actually voting for that candidate. Vote choices are not analyzed directly here, because prospective voters were not asked about their vote intentions until the third (September) wave of the 1980 panel.

17. Any comparison of television news and newspaper exposure effects must recognize that many people may "read a daily newspaper regularly" for sports news, horoscopes, and want ads, without paying the least attention to news of national politics and public affairs of the sort primarily featured on the television network news programs. A more precise measure of exposure to a daily newspaper's *political* news would produce somewhat larger exposure effects but a correspondingly lower estimate of how many people are exposed. Thus, although the differences reported here in estimated exposure effects should not be taken as evidence that print news is inherently less persuasive than broadcast news, they do demonstrate that it is less influential in the aggregate.

18. The individual ordinary least squares parameter estimates summarized in Figures 1–4 are available from the author.

19. The average standard errors (estimates of σ_u from equation 3) for the 37 separate opinion regressions on the common 100-point scale are 10.5 in June and 6.9 in September. The average of the 37 distinct chi-squared values is 41.9—of which 19.0 is attributable to the basic exposure model relating media exposure in each wave of the panel to prior media exposure and demographic variables (with 12 degrees of freedom) and 22.9 is the average increment attributable to the model in equation 3 relating opinions in each wave of the panel to prior opinions, media exposure, and demographic variables (with 18 degrees of freedom). In each of the 37 models, the Bentler Comparative Fit Index is 1.000 (1989, 114–17).

20. From successive application of equation 1,

$$\theta_{ij3} = [\theta_{ij1}\lambda_{j2} + \mu_{ij2}(1 - \lambda_{j2})]\lambda_{j3} + \mu_{ij3}(1 - \lambda_{j3})$$

$$= \theta_{ij1}\lambda_{j2}\lambda_{j3} + \mu_{ij2}(1 - \lambda_{j2})\lambda_{j3} + \mu_{ij3}(1 - \lambda_{j3}).$$

Thus, the combined weight attached to the campaign information terms μ_{ij2} and μ_{ij3} is

$$(1 - \lambda_{j2})\lambda_{j3} + (1 - \lambda_{j3}) = 1 - \lambda_{j2}\lambda_{j3}.$$

The "campaign information" estimates in Table 2 were calculated by replacing λ_{j2} and λ_{j3} in this expression with the corresponding errors-in-variables and ordinary least squares parameter estimates.

21. In the case of party identification, the entries in Table 2 represent the differences between the messages received by Strong Republicans and Pure Independents (or between the messages received by Pure Independents and Strong Democrats).

22. Since the β_{jt} parameters in equation 3 are defined in terms of the α_{jt} and λ_{jt} parameters as $\beta_{jt} = \alpha_{jt}(1 - \lambda_{jt})$, estimates of the α_{jt} parameters for each variable in each panel wave can, in principle, be retrieved by substituting esti-

mates of the β_{jt} and λ_{jt} parameters into the equation $\alpha_{jt} = \beta_{jt}/(1 - \lambda_{jt})$. However, this approach fails for 6 of the 74 possible separate calculations—because the estimated lag parameter λ_{jt} equals the theoretical maximum value of 1.00 (so that the estimated denominator of the ratio $\beta_{jt}/(1 - \lambda_{jt})$ is zero)—and provides very untrustworthy estimates in several other cases because the estimated denominator is close to zero. This problem can be mitigated by cumulating over campaign periods, as with the estimated relative weight of campaign information reported in Table 2, col. 1. Since the cumulative campaign message is simply the precision-weighted average of the separate messages μ_{ij2} and μ_{ij3}—with precisions $(1 - \lambda_{j2})\lambda_{j3}$ and $(1 - \lambda_{j3})$, respectively—the contribution of characteristic j to the cumulative campaign message is the same precision-weighted average of the separate message parameters α_{j2} and α_{j3},

$$[\alpha_{j2}(1 - \lambda_{j2})\lambda_{j3} + \alpha_{j3}(1 - \lambda_{j3})]/[(1 - \lambda_{j2})\lambda_{j3} + (1 - \lambda_{j3})].$$

Substituting $\beta_{j2}/(1 - \lambda_{j2}) = \alpha_{j2}$ and $\beta_{j3}/(1 - \lambda_{j3}) = \alpha_{j3}$ into this expression, the contribution of characteristic j to the cumulative campaign message can be expressed in terms of the β_{jt} and λ_{jt} parameters as $(\beta_{j2}\lambda_{j3} + \beta_{j3})/(1 - \lambda_{j2}\lambda_{j3})$. Substituting the corresponding errors-in-variables or ordinary least squares parameter estimates for β_{j2}, λ_{j2}, β_{j3}, and λ_{j3} in this ratio produces the estimated contributions reported in Table 2 of television news exposure, newspaper exposure, and party identification to the cumulative campaign message for each of the 37 separate opinions.

23. Here, as elsewhere, it may be worth noting that the effects of party identification, though large for individual identifiers, are much smaller for the population as a whole, simply because effects on Republicans and opposing effects on Democrats tend to cancel out. Even if every strong partisan absorbed new information 15 or 20 points more favorable toward his or her own candidate than the new information absorbed by "pure" independents, as the results in Table 2 suggest, the corresponding average *aggregate* impact of this partisan reinforcement would have been only two points or so on the 100-point scale, given the distribution of party identification in 1980. By contrast, the average aggregate impact of television news exposure on the messages received by the population as a whole is on the order of 6 points, and the average aggregate impact of newspaper exposure is on the order of 3 points on the 100-point scale.

24. The distributions of $\alpha_{jt} = \beta_{jt}/(1 - \lambda_{jt})$ shown in Figures 5 and 6 are based on samples of 10,000 pseudo observations each for β_{jt} and λ_{jt}. The pseudo observations were sampled (from Normal distributions with means, variances, and covariances derived from the empirical analyses reported in Appendix B and, in Figure 5, from the corresponding ordinary least squares analysis) and the distributions constructed by Simon Jackman.

25. There are many cases in which February values of the variables examined here are significantly related to media exposure, but the difficulties involved in making causal inferences from such cross-sectional relationships are daunting.

26. Patterson's (1980) 1976 election year panel study may be helpful in this regard, since it includes a somewhat more extensive battery of media exposure questions than the National Election Study surveys used here. Patterson also had the good fortune to be surveying a public with very weak preexisting opinions about one of the eventual nominees, Jimmy Carter.

27. "That's how it goes: One person believes that I'm the ceremonial gypsy goddess human sacrifice of a Mexican death cult, and soon everyone else just knows it, too—even though they don't even go to the supermarket" (Barr 1989).

28. In addition, the availability of multiple indicators of each general dimension makes it possible to further relax the assumptions of the measurement model applied to the individual indicators. The estimates presented in Tables 1 and C-1 are from a model in which measurement errors for different measures of the same image dimension are allowed to be contemporaneously correlated. The 18 estimated measurement error correlations (three for each of the three-image dimensions for each of the two candidates) range from −.07 to .18 and average .06. These correlations are large enough to improve the fit of the model slightly but do not significantly affect the results. Additional experimentation with model specification suggests that further weakening the measurement assumptions (e.g., allowing measurement error variances and covariances to vary across waves of the panel) would also improve the fit of the model somewhat, without significantly affecting the results reported here.

29. The average value of the Bentler Comparative Fit Index for the six separate models (one for each candidate for each trait dimension) is .998, and the average chi-squared value is 302.4 (with 134 degrees of freedom). The average standard error for the 12 distinct opinion regressions is 8.0 on the 100-point scale, as compared with an average standard error of 9.6 for the 36 corresponding individual item regressions in Appendix B.

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