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Do Surveys Provide Representative or Whimsical Assessments of the Economy?

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We argue that survey responses to economic evaluation questions exhibit instability and can be affected by fairly trivial changes in questionnaire wording. Our analyses make three empirical contributions to this area of survey research. First, we demonstrate that within the course of the interview there is considerable instability in economic evaluations. Second, one source of this instability is cues regarding economic performance, such as those provided by the media. We find that respondents can be persuaded to change their economic evaluations if they receive contradictory cues. Finally, we demonstrate that question placement can affect economic evaluations. More specifically, we demonstrate that proximity to political questions can contaminate economic evaluations. If economic evaluations closely follow political preference questions, respondents have a tendency to give economic responses that are "consistent" with their political responses. Our empirical analysis is based on economic evaluations of respondents to the Hungarian Markets and Democracy Survey administered during December 1997.

1 Introduction

SIDNEY VERBA (1996, p. 1) emphasizes the central role of survey research in the study of political participation: ". . . surveys give the researcher access to the 'public,' an otherwise broad, amorphous, and hard-to-deal-with phenomenon." Much of the empirical work to model the link between economic evaluations and political preferences relies on survey research. In this article we assess the extent to which we can have confidence in the subjective measures of economic performance typically employed in economic voting models. Whether surveys provide more representative or whimsical assessments of the economy depends in part on how sensitive citizens' economic perceptions are to survey design.

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An extensive literature addresses the issues of questionnaire design and the sources of measurement error in survey research (e.g., Weisberg et al. 1996; Krosnick and Fabrigar 2000). Our concern here is survey design effects that have consequences for subjective economic evaluations. Survey design effects represent a potential source of systematic measurement error, which of course can bias coefficient estimates. Moreover, the theoretical implications of this bias can be serious. A case in point is the current debate about the exogeneity of economic evaluations in models of political behavior. Recent research has demonstrated that media exposure, information, and political preferences shape citizens' perceptions of national economic performance, thereby indicating that these perceptions are not entirely exogenous (Hetherington 1996; Duch et al. 2000). Similarly, question ordering and wording might contaminate economic evaluations so as to undermine further their exogeneity and produce misleading conclusions regarding their political impact.

Concern about the exogeneity of economic evaluations has focused primarily on prospective economic evaluations. This focus is due largely to the growing literature emphasizing the importance of prospective (as opposed to retrospective) economic assessments in shaping political attitudes and behavior (e.g., MacKuen et al. 1992). And while prospective economic evaluations have assumed considerable importance in the literature, their hypothetical, forward-looking nature makes them particularly susceptible to manipulation by survey design. In this context, the empirical analysis presented here provides an assessment of how sensitive prospective evaluations are to design effects.

We contend that the impact of survey design on the reporting of economic attitudes resembles the information and activation effects of media and political campaigns on public opinion (e.g., Zaller 1992; Lodge et al. 1995). Survey responses to economic evaluation questions reflect the common pathologies Zaller attributes to opinion research: responses are unstable and can be affected by fairly trivial changes in questionnaire wording. Our analyses make three empirical contributions to this literature. First, we demonstrate that question placement matters. If economic evaluation questions closely follow political questions, respondents tend to give economic responses that are "consistent" with their stated political preferences. Second, we show that responses to economic evaluation questions exhibit considerable instability *during the course of the survey interview*. Finally, one source of this instability is cues regarding economic performance, such as those provided by the media. We find that respondents can be persuaded to change their economic evaluations if they receive contradictory cues.

The empirical analyses reported in this article provide particularly appropriate and conservative tests of our measurement hypotheses. Our study considers the economic evaluations of respondents to the Hungarian Markets and Democracy Survey administered during December 1997. In general, economic evaluations represent an excellent set of attitudes on which to evaluate questionnaire design effects because individuals are generally more cognizant of economic conditions than other policy outcomes and hence should have relatively stable attitudes regarding the state of the economy. This contextual advantage is even greater for the Hungarian survey data that we analyze since the period prior to December 1997 was marked by relatively serious economic dislocation in which economic issues were highly salient (Duch 1999). We believe that respondents in such an environment are reasonably attentive to the economy and hence their economic evaluations are more stable and less "top of the head" than if they resided in a country with more stable and positive economic conditions.

2 Theoretical Argument

The Hungarian survey included an experiment designed to test hypotheses about the formation of economic evaluations, specifically, and mass opinion, more generally. Previous

survey research on mass opinion has revealed evidence of over-time instability, response (question-order) effects, and question-wording effects [for an excellent review of this literature, see Zaller (1992, pp. 28–37)].

The theory of survey response proposed by Zaller and Feldman (1992) raises an important and potentially troubling question about the relationship between economic evaluations and political responses. Zaller and Feldman argue that most people respond to survey questions on the basis of whatever ideas happen to be salient and accessible to them (i.e., at the "top of their heads") at the moment of answering. This theory, labeled the Response Axiom by Zaller (1992, p. 49), raises the possibility that responses to economic evaluation questions contain measurement error, which makes them unstable. Our essay explores three possible sources of contamination or measurement bias.

2.1 *Question Order*

To the extent that answers to survey questions about the economy are "top of the head" reactions, they should be highly sensitive to the stimuli that precede the economic evaluation questions. This expectation assumes that certain types of questions systematically stimulate respondents to consider particular ideas, which are then salient when the respondent answers subsequent questions about the economy. This systematic stimulation due to question order biases economic evaluations. Survey questions that measure respondents' political attitudes are one possible source of contamination. The principal concern here is that the placement of political questions prior to economic evaluation questions artificially inflates the correlation between responses to these questions. This hypothesized effect builds on an important body of evidence suggesting that respondents prefer giving consistent responses (Sudman and Schwarz 1996; Schwarz and Sudman 1992). In other words, respondents engage in a certain degree of retrospection, conditioning their answers on responses given to previous questions. To the extent that this holds true, asking political questions prior to economic evaluation questions produces endogenous consistency between respondents' subjective economic evaluations and partisan preferences.

The general literature on question placement indicates that priming effects exist (e.g., Schwarz and Hippler 1995) but are not "sizable" (Schuman and Presser 1981; Sigelman 1981). Research focusing specifically on economic voting, however, suggests that the positioning of questions on political attitudes immediately prior to economic evaluation questions does influence respondents' assessments of the national economy and their personal financial situation (Sears and Lau 1983; Lau et al. 1990). Lewis-Beck (1985) has challenged Sears and Lau by demonstrating that the distance between political and economic questions is not correlated with the magnitude of the statistical relationship between responses to these questions. However, Wilcox and Wlezien (1996) conduct a set of survey experiments in which they confirm that both political responses and subjective economic evaluations are subject to contamination when related questions are placed in close proximity to each other. Our empirical analysis contributes to this literature by investigating whether question placement contaminates Hungarian responses to economic evaluation questions.

2.2 *Response Stability*

A second concern explored here is the temporal stability of responses to economic evaluation questions. Unlike previous research, we investigate *within-survey* response stability by repeating an economic evaluation question near the end of the interview. There is considerable evidence indicating that the interview context matters (Abramson and Ostrom 1994; Johnston 1992; Schwarz and Sudman 1992). In our analysis, we consider whether respondents over the course of an interview incorporate information and cues from the

survey instrument into their economic evaluations. To the extent that the economy is a reasonably salient issue, as it was in Hungary during 1997, most respondents should not "update" their evaluations of economic performance based on information and cues gleaned from the survey instrument (i.e., the intervening battery of economic and political questions). Hence, evidence that evaluations in Hungary are highly unstable would provide particularly strong evidence that the survey instrument induces respondents to subjectively reevaluate economic performance.

We are also interested in modeling individual-level variation in within-survey response stability. Three theories provide insights into which respondents are most likely to use contextual information to update their subjective assessments of economic performance. First, as stated earlier, survey research finds that respondents prefer to provide consistent responses over the course of an interview (Schwartz and Sudman 1992). Consequently, respondents who express inconsistent economic assessments are more likely to change their assessments during the interview if given the opportunity. This psychological preference for response consistency provides a theoretical rationale for the occurrence of within-survey response instability.

But what factors distinguish among respondents in terms of their likelihood of initially expressing inconsistent attitudes? Zaller's Resistance Axiom (1992, p. 44) implies that respondents who have stronger predispositions should exhibit more response stability. According to this theoretical perspective, strong partisans should be less likely than weak partisans to update their initial evaluation of the economy in reaction to information and cues obtained from the survey instrument. The same expectation holds for those respondents who are well informed about the economy before the interview starts.

Alvarez and Brehm (1995, 1998) provide a third insight into response stability. They posit that ("ambivalent") survey respondents with conflicting values relating to a particular policy domain exhibit higher variance in their policy preferences. For example, Alvarez and Brehm find that many respondents have conflicting grounds for formulating their position on abortion policy because they strongly support both individual liberties and the notion of respect for human life. Granted, we are not modeling policy choice here. Nevertheless, the notion of conflicting values is analogous to the activation of "conflicting" predispositions over the course of an interview.

Additionally, Alvarez and Brehm posit that the policy attitudes of better-informed respondents have greater error variances. In our experiment, respondents become better informed between the first and the second appearances of the economic evaluation question if they actually obtain information from the survey instrument. In sum, Alvarez and Brehm's theory implies that within-survey response instability could occur due to the activation of conflicting predispositions and the dissemination of new information during the course of the interview.

However, the survey instrument does not necessarily activate predispositions and impart new information that contradict rather than reinforce the respondent's initial evaluation of the economy. Hence, a crucial issue here is distinguishing among respondents in terms of the likelihood that the survey instrument stimulates conflicting considerations. We expect respondents to react less to activation cues if their initial economic evaluations are consistent with their other economic and political attitudes. More specifically, for respondents whose initial economic evaluations are consistent with their partisanship, we expect response stability to increase with strength of partisanship due to greater *resistance* to arguments implied by the survey that are *inconsistent* with the respondent's political predispositions. In contrast, if the initial evaluation is inconsistent with the respondent's partisanship, response stability decreases with strength of partisanship due to greater *receptiveness* to arguments

implied by the survey that are *consistent* with the respondent's political views. We hold similar expectations about how personal financial situation affects the stability of national economic evaluations. For respondents whose initial national economic evaluations are positive, we expect response stability to increase as the respondent's personal financial situation becomes more positive due to greater resistance to contrary arguments implied by the survey. For respondents whose initial national economic evaluations are negative, we expect response stability to decrease as the respondent's personal financial situation becomes more positive due to greater receptiveness to contrary arguments implied by the survey.

2.3 *Persuasive Cues*

The final part of our survey design analysis evaluates in a more direct fashion the notion that cues associated with question wording shape (or "prime") economic evaluations. In particular, we test whether cues associated with the media affect prospective evaluations of the economy. Zaller and Feldman (1992) demonstrate that neutral question wording produces significantly different responses than question wording that prompts a "memory dump" of considerations immediately salient in the respondent's mind. An important implication of their theoretical argument for our analysis is that the context in which the question provides information shapes the impact of that information on the respondent's economic assessments. Hence, certain "trusted" cue givers might influence subjective evaluations of economic performance more than others (see Lupia and McCubbins 1996).

In our experiment, the media serves as a cue giver who provides explicit, objective information about the economy in the context of a survey question. There is considerable evidence to suggest that the media is an important source of cues regarding the economy (e.g., Mutz 1992, 1994; Hetherington 1996). We propose to evaluate the extent to which media cues can persuade individuals to rethink their assessments of economic conditions. Building on our earlier discussion of within-survey response stability, we expect media cues to matter most for respondents whose evaluations of the national economy are inconsistent with their partisanship and personal financial experiences. Hence, the consistency of economic responses with other economic and political attitudes should account for heterogeneity in the willingness of citizens to change their prospective evaluations in response to contrary media information about future economic conditions. An alternative expectation here, which more directly applies Zaller's Resistance Axiom, predicts that the influence of media cues decreases with the strength of political predispositions (partisanship) regardless of the consistency of the initial evaluation. The design of the Hungarian Markets and Democracy Survey allows us to directly test hypotheses about the influence of media cues, embedded within the survey instrument, on respondents' answers.

Finally, we expect the substantive case analyzed here—prospective evaluations in Hungary in 1997—to influence the magnitudes of the survey design effects discussed in this section. We suspect that prospective evaluations are more "top of the head" than are retrospective evaluations since they require the individual to assess the future rather than reflect on the past. In turn, we believe that respondents are less familiar with and less likely to have thought about the state of the economy in the next year than how the economy has performed over the past year (see Haller and Norpoth 1994). Hence, considerations about the economic future are less easily retrieved from memory, thereby increasing the potential influence of priming by the survey instrument and persuasion by media cues.

In addition, we believe that the institutional and political context influences the extent to which question ordering contaminates economic evaluations. We theorize that the clarity of institutional responsibility for economic policy outcomes affects the importance of question

ordering just as it conditions the extent of economic voting (Powell and Whitten 1993). The Hungarian case is one in which institutional responsibility for economic outcomes was relatively unambiguous: first, the coalition government in 1997 consisted of only two parties; and, second, executive power is clearly concentrated in the hands of the Prime Minister and his cabinet. Hence, question ordering is more likely to matter in the Hungarian case than in political systems where institutional responsibility is less clear, such as Italy. We also suspect that the likelihood of political cues stimulating economic assessments varies according to the relative saliency of economic and noneconomic issues. Clearly the economy was a salient political issue for the Hungarian electorate in 1997. In the early 1990s Hungary experienced substantial economic shocks marked by declining real GDP, high inflation, and severe unemployment. Under the Horn government the economy clearly recovered, attaining respectable rates of real GDP growth and declining levels of inflation and unemployment. But significant political disagreements still existed regarding the financial situation of the average Hungarian, particularly given the draconian cuts in social expenditures that had been enacted and were being proposed. Hence the Hungarian political context was one in which we should expect economic assessments to be particularly sensitive to stimulation by political cues.

3 Research Design

Our goal in this article is to establish the extent to which survey responses to economic evaluation questions are stable and uncontaminated by other survey items as a function of questionnaire design. Details about the sampling strategy of the Hungarian Markets and Democracy Survey are discussed in Appendix I of the web version of this article. The questionnaire incorporated three features designed to evaluate the nature of responses to economic evaluation questions. These features constitute an experiment that provides a direct means of analyzing the effects of survey design.

First, the Hungarian survey had a split ballot format. This was implemented to test question-placement hypotheses. In version 1 of the survey, the economic evaluation questions precede the party attachment questions (i.e., vote intention and strength of attachment), while in version 2 of the survey, they follow the party attachment questions. If question order matters, partisan attachment should have a stronger effect on economic assessments in the second version than in the first version.

Second, the Hungarian survey twice asked respondents to make a prospective assessment of national economic conditions during the next year. This design feature enabled us to investigate response instability over the course of a single interview, rather than across waves of a panel study (as done in previous research). Given the questionnaire's split ballot format, the distance between these two questions depended on the format. In version 1, the prospective assessment was first asked in question 26 and subsequently in question 90 (64 questions apart). In the second format, the prospective assessment was asked in questions 60 and 90 (30 questions apart). If these questions tap a stable meaningful opinion, prospective assessments of the economy should not change during the interview.

Third, the survey included a "persuasion scenario." Respondents who provided a positive or negative assessment of the national economy were told that recent newspaper articles had reported economic prospects contrary to their evaluations (as measured by the second prospective evaluation—question 90). Respondents were then asked to reconsider their evaluations of the economy taking into account the contrary information "reported" in the media. This "persuasion scenario" provides a means of evaluating the extent to which media cues can alter respondents' economic evaluations.

As stated earlier, the substantive context of our analysis also has advantages for investigating survey design effects on the measurement of public opinion. First, the analysis of national economic evaluations provides a conservative test of Zaller's model since such assessments, if truly objective, should be more stable than opinions on public policy issues and hence exhibit less evidence of contamination due to question order and wording. This expectation seems particularly valid when the economy is a highly salient issue, as it was in Hungary at the end of 1997. Second, unlike Wilcox and Wlezien (1996), we have little reason to question the relevance of Zaller's model on the grounds that citizens have more permanent attitudes on policy issues with which they are very familiar due to those issues' frequent consideration in polls reported by the mass media. This "familiarity" argument might hold for American citizens asked to evaluate national economic performance but not for citizens in Hungary where the democratic process is relatively new and polling of public opinion is still novel. In sum, the substantive context that we investigate is biased in favor of the null hypotheses, so evidence of survey design effects revealed by our analysis has greater import.

4 Results

We conducted our empirical analysis in three stages corresponding to the three separate theoretical concerns we address in this article: question placement, within-survey response stability, and persuasion by media cues. We discuss these methods as well as the model specifications in the context of reporting the results. The dependent variables are standard measures of retrospective sociotropic, retrospective pocketbook, prospective sociotropic, and prospective pocketbook evaluations, ranging from 0 for "worsen(ed) a lot" to 4 for "improve(d) a lot." Detailed definitions of the dependent and explanatory variables employed in the analysis are provided in Appendix II of the web version of this article.

4.1 *Question Placement*

In the first stage of our econometric analysis we evaluate two hypotheses about question placement. First, we consider whether question order alters the relationship between economic assessments and political attitudes toward the government. The economic evaluation questions precede the party attachment questions in version 1 of the survey but follow them in version 2. Hence, if question order matters, *Government Party Attachment* should have a stronger impact on economic assessments among version 2 respondents. Second, a related hypothesis is that the placement of economic evaluation questions later in the survey after an extensive series of political questions might have a general impact on economic evaluations. The economic evaluation questions begin 34 questions later in version 2 than in version 1. If the survey activates political and economic attitudes in general and those attitudes systematically influence economic assessments, responses to the economic evaluation questions should differ depending on how late these questions appear in the survey.

Table 1 presents ordered probit models of economic evaluations. Following Herron (2000), we report the expected percentage predicted correctly as well as the percentage error reduction as measures of model fit. For the purpose of simulating the effects of independent variables, we define the "typical" respondent by setting ordinal measures to the sample mean and binary variables to the sample median—standard errors on the changes in marginal probability effects are estimated using *Clarify* (King et al. 2000). The principal explanatory variables in Table 1 are *Version 2* and *Government Party Attachment* $*$ *Version 2*. Inclusion of *Version 2*—a binary indicator of the second questionnaire format—allows us to evaluate whether placement of the economic evaluation questions later in the survey causes

Table 1 Ordered probit models of personal and national economic evaluations[a]

Explanatory variable	*Retrospective sociotropic*	*Prospective sociotropic*	*Retrospective pocketbook*	*Prospective pocketbook*
Version 2	.14*	.11	.06	.07
	(.06)	(.06)	(.06)	(.06)
Government Party Attachment	.123**	.097**	.034*	.038*
	(.017)	(.017)	(.015)	(.016)
Govt Party Attachment ∗ Version 2	.016	.018	.070**	.040
	(.024)	(.024)	(.023)	(.024)
Economic sophistication	.255**	.174**	.098**	.085*
	(.039)	(.039)	(.038)	(.038)
Economic Media Usage	.088*	.083*	.012	.059
	(.038)	(.039)	(.038)	(.040)
Unemployed	−.19	−.05	−.33**	−.07
	(.12)	(.13)	(.12)	(.12)
Pensioner	.14	.20*	.04	−.03
	(.09)	(.10)	(.09)	(.09)
Family income	.054*	.100**	.158**	.102**
	(.024)	(.024)	(.023)	(.023)
Education	.037	.019	−.013	−.032
	(.028)	(.029)	(.027)	(.028)
Age	−.0055*	−.0027	−.0035	−.0085**
	(.0025)	(.0026)	(.0024)	(.0025)
Female	−.23**	−.09	−.09	−.06
	(.06)	(.06)	(.06)	(.06)
Constant	.82**	1.31**	.85**	1.63**
	(.14)	(.15)	(.13)	(.14)
Mu1	.90**	.97**	1.05**	.96**
	(.04)	(.06)	(.04)	(.05)
Mu2	1.75**	2.08**	2.37**	2.14**
	(.05)	(.07)	(.06)	(.06)
Mu3	4.13**	4.49**	3.88**	3.77**
	(.15)	(.14)	(.14)	(.10)
Expected % predicted correctly	42.1	44.9	41.6	41.2
% error reduction	11.6	9.8	0.7	0
N	1482	1417	1530	1369

[a]Dependent variables are standard retrospective and prospective measures of national economic evaluation and personal financial situation, ranging from 0 for "worsen(ed) a lot" to 4 for "improve(d) a lot." Coefficient standard errors are reported in parentheses. $^{*}p < .05$, $^{**}p < .01$ (two-tailed).

a shift in the mean evaluations. The interaction term evaluates whether the placement of political questions prior to economic evaluation questions contaminates (inflates or depresses) economic assessments. The models also include control variables that have been applied in previous research on economic evaluations (e.g., Markus 1988; Duch et al. 2000).

The results in Table 1 support the conclusion that question placement matters. With respect to the "contamination" hypothesis, there is evidence that the placement of partisanship questions prior to the economic evaluation questions has a "politicizing" effect. For respondents administered the second questionnaire format, the statistical relationships between partisanship and pocketbook evaluations are significantly higher than they are for version 1 respondents. Among typical, "version 1" respondents, a two-category increase

in the strength of their attachment to a government party (e.g., from "moderately close" to "very close") produces 1.4 and 2.4 percentage-point increases in their predicted probabilities of giving positive retrospective and prospective evaluations of their personal finances (with respective standard errors of 0.7 and 1.0). In contrast, 5.4 and 4.9 percentage-point increases occur for typical respondents to the version 2 survey given the same magnitude change in their partisanship (with respective standard errors of 1.1 and 1.0). Hence, the political contamination of question placement can more than double the marginal effect of partisanship on pocketbook evaluations. While question order does not produce similar contamination of national-level evaluations, assessments of the national economy are significantly higher in the second questionnaire format. According to the ordered probit estimates, positive retrospective and prospective evaluations among typical respondents were 5.2 and 4.3 percentage points more likely in the version 2 survey than in the version 1 survey.

The principal finding here is that the placement of partisanship questions prior to questions about personal finances can contaminate the latter, thereby inflating the correlation between the variables. This finding is consistent with the Sears and Lau (1983) and Lau et al. (1990) evidence regarding the "politicizing" of personal financial assessments. Our results suggest that there is no similar effect for sociotropic evaluations. But national economic evaluations are not entirely unaffected by question placement. As the first row in Table 1 indicates, the mean levels of retrospective and prospective assessments of the national economy are both higher in version 2 of the questionnaire than in version 1. This finding suggests that the survey interview can activate political and economic attitudes that systematically affect national economic evaluations. In the present case, administering more opinion questions prior to posing the economic evaluation questions triggered positive assessments of the national economy for both supporters and opponents of the incumbent government. One possible explanation for the heightened positive evaluation of the national economy is that the battery of additional questions in version 2 reminded respondents that national economic indicators (such as real GDP growth and inflation levels) had experienced significant improvements at the time of the survey (Duch 1999). This battery of questions may have simply reminded respondents of the national political debate that tended to concede that the overall economy had recently improved. In fact, the positive and significant coefficients on the economic media usage variable in the two sociotropic equations indicate that those most attentive to the news about the economy were more likely to have a positive assessment of economic performance.

4.2 *Within-Survey Response Stability*

In the second stage of our econometric analysis we investigate response instability during the administering of a survey. In the Hungarian Markets and Democracy Survey, respondents were twice asked to make a prospective assessment of national economic conditions during the next year. Note that the wording of these two questions was identical. To the extent that these questions tapped a stable meaningful opinion, prospective assessments of the economy should not have changed over the course of the interview. Table 2 presents a cross-tabulation that characterizes the extent to which prospective assessments of the national economy did change. Altogether 41.7% of the respondents changed their opinion over the course of the 30 or 64 questions (depending upon the questionnaire version) that separated these evaluations. The most stable responses were from those who expected the economy to remain the same (59%) or to improve somewhat (72%). Among the other categories, stability was quite low, with only 25–35% of the responses remaining unchanged. In general, respondents' evaluations tended to moderate: those changing from "worsen somewhat" and "improve

Table 2 Comparison of prospective evaluations of the national economy[a]

Second prospective evaluation of national economy	*First prospective evaluation of national economy*					
	Worsen a lot	*Worsen somewhat*	*Same*	*Improve somewhat*	*Improve a lot*	*Don't know*
Worsen a lot	31.6	7.1	2.5	1.2	0	0
Worsen somewhat	36.8	35.4	10.6	4.2	0	7.1
Same	17.5	36.7	58.7	18.9	8.3	19.0
Improve somewhat	5.3	14.2	19.8	72.0	66.7	24.6
Improve a lot	0	0	0.2	0.3	25.0	0.8
Don't know	8.8	6.6	8.3	3.4	0	48.4
N	57	226	530	592	12	126

[a]The table compares responses to two identical questions measuring prospective assessments of national economic conditions over the next year. In version 1 of the survey, these two questions were asked 64 questions apart, while in version 2 they were asked only 30 questions apart. Cell entries are percentages calculated by column.

somewhat" to "same" outnumbered those changing in the opposite directions. Hence, there is support for the hypothesis that survey responses, even those regarding fairly immediate concerns such as the national economy, are somewhat "top of the head" and hence subject to reconsideration during the interview.

Theories of response instability, such as those proposed by Zaller and Feldman (1992), characterize the average survey respondent as having little interest in the topics and limited information about the opposing considerations of most opinion questions. Hence in many cases the content and design of the survey can provide cues or "facts" that might shape responses. The high degree of instability identified in Table 2 suggests that economic evaluations fit this characterization. Instability over the 30- or 64-question interval in the Hungarian survey likely resulted from respondents' exposure to a variety of stimuli or cues associated with the intervening questions, which caused respondents to reconsider their initial economic evaluation. We theorize that certain respondents are more prone than others are to this "priming" by information inferred from the survey. In particular, we hypothesize that response instability is greatest among respondents who answered questions about their party attachment and personal financial situation in a manner inconsistent with their initial evaluation of the economy.

Table 3 presents two ordered probit models of within-survey instability in prospective evaluations of the economy. The dependent variable is a categorical measure of response change ranging from 0 for no change to 3 for a three-category change between the two prospective questions. Since our focus is on response instability, we do not model the direction of the change. The ordered probit models include *Consistency of Government Party Attachment*, *Consistency of Prospective PFS*, and *Strength of Government Party Attachment* to evaluate the resistance and activation hypotheses, which posit different forms of priming by the survey instrument. *Consistency of Government Party Attachment* equals *Government Party Attachment* if the respondent's initial prospective evaluation of the economy is positive, *Government Party Attachment* multiplied by -1 if it is negative and 0 otherwise. *Consistency of Prospective PFS* equals *Prospective PFS Evaluation* if the respondent's initial prospective evaluation is positive, *Prospective PFS Evaluation* multiplied by -1 if it is negative, and 0 otherwise. The models also control for other possible determinants of response stability relating to sophistication, media usage, social status, and demographic characteristics.

Table 3 Ordered probit models of change in prospective national economic evaluations during the survey[a]

Explanatory variable	*Model 1*	*Model 2*
Version 2	−.05	−.05
	(.06)	(.06)
Consistency of Government Party Attachment	−.083**	−.082**
	(.016)	(.016)
Consistency of Prospective PFS	−.183**	−.182**
	(.020)	(.020)
Strength of Government Party Attachment	—	−.009
		(.020)
Economic Sophistication	−.131**	−.126**
	(.044)	(.045)
Economic Media Usage	.090*	.092*
	(.042)	(.042)
Family Income	−.017	−.017
	(.026)	(.026)
Education	−.021	−.022
	(.030)	(.030)
Age	.0014	.0014
	(.0019)	(.0019)
Female	.01	.01
	(.06)	(.06)
Constant	.06	.07
	(.14)	(.14)
Mu1	1.44**	1.44**
	(.05)	(.05)
Mu2	2.40**	2.40**
	(.12)	(.12)
Expected % predicted correctly	53.3	53.4
% error reduction	4.5	5.1
N	1543	1543

[a]Dependent variable measures within-survey instability in the respondents' prospective evaluation of the national economy, ranging from 0 for no change to 3 for a three-category change. Individuals who responded "don't know" to both prospective questions are coded 0, while those giving a "don't know" response to only one of the questions are coded 1. Coefficient standard errors are reported in parentheses. $^{*}p < .05$, $^{**}p < .01$ (two-tailed).

According to our theoretical argument, more "opinion" change should occur among respondents whose government party attachment and prospective assessment of personal finances are inconsistent with their initial (first) prospective evaluation of the national economy. If the initial evaluation is positive, stronger attachment to a government party reflects greater consistency, which we posit increases resistance to contrary priming from the survey and hence decreases response instability. The logic of this part of the argument follows Zaller's Resistance Axiom. In contrast, if the initial evaluation is negative, stronger attachment to a government party reflects greater inconsistency, which we posit increases receptiveness to contrary priming from the survey and hence increases response instability. Hence, we expect *Consistency of Government Party Attachment* to have a negative coefficient (i.e., a positive effect on response stability). The same expectation and intuition holds for *Consistency of Prospective PFS*.

The results in Table 3 clearly support the notion that priming effects are most likely to occur among respondents with inconsistent attitudes. The significant negative coefficients for *Consistency of Government Party Attachment* indicate that response instability was higher among respondents whose initial evaluation of the economy was inconsistent with their party attachment. Among supporters of the governing coalition parties, for example, response instability was highest for those who initially gave a negative prospective evaluation (everything else being constant). Moreover, response instability for these respondents increased with the strength of their party attachments. In contrast, response instability decreased with strength of party attachment for government supporters whose initial prospective evaluations were positive. To illustrate these divergent effects consider how a two-category increase in strength of party attachment (e.g., a change from "moderately close" to "very close") influences response stability for typical respondents whose initial evaluations were positive.[1] This increase in strength of party attachment would reduce the predicted probability of a response change by approximately 4.7 percentage points (with a standard error of 0.9) for government supporters and increase the predicted probability of a response change by 3.5 percentage points (with a standard error of 0.5) for opposition supporters (where response change is defined as a one-category change in prospective national economic evaluations).

Similarly, *Consistency of Prospective PFS* proves significant in both models, providing further evidence supporting the activation hypothesis.[2] As with government party attachment, whether respondents are receptive or resistant to priming from the survey depends on the consistency between their personal financial situation and initial prospective assessment of the economy. For typical respondents whose initial prospective evaluation of the *national economy* was negative, a two-category increase in the prospective evaluation of their *personal finances* (e.g., from "remain the same" to "improve a lot") made them 3.7 percentage points (with standard error of 1.0) more likely to evaluate prospectively the *national economy* differently the second time. In contrast, for typical respondents with positive *national evaluations* initially, the same increase in *personal finances* made them 10.1 percentage points (with a standard error of 1.0) less likely to change their *national evaluation*. In the former case, the increase (improvement) in the prospective evaluation of personal finances made the respondent's economic attitudes more inconsistent, while in the latter case, the increase made those attitudes less inconsistent.

Is the effect of partisanship on the stability of economic evaluations contingent upon whether economic evaluations are initially consistent with the respondent's partisanship? As stated earlier, the logic of the activation hypothesis follows Zaller's Resistance Axiom when the initial prospective evaluation is consistent with the respondent's partisanship. More specifically, both hypotheses posit that response stability increases with strength of party attachment when the initial evaluation is positive for government supporters or negative for opposition supporters. Given this theoretical connection, the statistical significance of *Consistency of Government Party Attachment* lends support to the validity of Zaller's

[1] Here we define typical respondents as above: having mean (median) values of the ordinal (binary) independent variables.

[2] The models in Table 3 also test the hypothesis that question placement influences the magnitude of priming effects. The first prospective evaluation question was posed earlier in questionnaire version 1 than in version 2 (question 26 rather than 60). Hence, more opinion questions were asked between the two prospective questions in version 2. The greater number of intervening questions implies greater activation effects. The insignificant coefficient on *Version 2*, however, contradicts this expectation by indicating that question placement has no impact on the magnitude of priming effects. In auxiliary models not presented here, we also tried including interactions between *Version 2* and the *Consistency* variables, but these also proved insignificant.

theory. Note, though, that the two hypotheses make opposite predictions about the effect of strength of partisanship when the respondent's initial prospective evaluation is politically inconsistent—negative for government supporters or positive for opposition supporters. Even so, one could reasonably speculate that the significance of *Consistency of Government Party Attachment* is due largely to the Resistance Axiom on the grounds that initial prospective evaluations of the economy are generally consistent with the respondent's partisanship. Therefore, to clarify the implications of the results, we estimated Model 2 in Table 3, which adds *Strength of Government Party Attachment* to the specification in Model 1. The insignificance of *Strength of Government Party Attachment* verifies the merit of the activation hypothesis and indicates that for prospective evaluations of the economy, at least, resistance to priming does not always increase with strength of partisanship.

The models in Table 3 also control for other possible determinants of response instability. Not surprisingly, we find that response stability increases with the respondent's economic sophistication. Compared with their "average" counterparts, typical respondents with "very high" levels of knowledge about economic issues were 9.2 percentage points (with a standard error of 2.8) less likely to make a 1-unit change in their prospective evaluation of the economy. Somewhat surprisingly, though, the estimated likelihood of a response change increases with the respondent's level of economic media usage. One possible explanation for this finding is that greater exposure to media information on economic issues, controlling for economic sophistication, reflects greater receptiveness to cues and priming by the survey. Finally, there is no evidence that social status, age or gender affect the stability of prospective economic assessments.

4.3 *Persuasion by Media Cues*

In the third stage of our econometric analysis we investigate the impact of contrary media information on prospective evaluations of the national economy. Consistent with Zaller's Resistance and Accessibility Axioms, respondents' economic evaluations should change due to exposure to new media information, but the likelihood of evaluation change should be greater among those with weaker political predispositions. Consistent with the activation hypothesis, contrary media information should have a larger impact on respondents whose personal financial experiences and political attitudes are inconsistent with their initial prospective evaluation of the national economy. In other words, the impact of media information depends on whether it activates, rather than counters, preferences based on personal experiences and partisan predispositions.

Table 4 presents a cross-tabulation that characterizes the extent to which prospective assessments of the economy changed in response to contrary media information. Approximately 23% of the respondents changed their economic evaluation in response to the "media persuasion" question. Note that this percentage is only slightly greater than half the percentage of respondents who changed their evaluation between the first and second administration of the economic evaluation question. Hence, the contrary media cue produced less response instability than the survey instrument. Nevertheless, a significant number of respondents were persuaded to change their assessment of the economy when confronted with contradictory cues from the media.

Table 5 presents two bivariate probit models of information-induced change in prospective evaluations of the economy. As stated earlier, the Hungarian Markets and Democracy Survey includes a "persuasion scenario" following the second question asking respondents to evaluate national economic performance over the next year. The persuasion scenario presents respondents with contrary media information about future economic conditions

Table 4 Influence of media information on prospective evaluations of the national economy[a]

Post information prospective evaluation of national economy	*Second prospective evaluation of national economy*			
	Worsen a lot	*Worsen somewhat*	*Improve somewhat*	*Improve a lot*
Worsen a lot	**75.0**	4.2	0.8	0
Worsen somewhat	13.5	**67.2**	3.0	0
Same	7.7	13.0	11.8	0
Improve somewhat	0	9.9	**81.1**	14.3
Improve a lot	0	0	0.5	**71.4**
Don't know	3.8	5.7	2.8	14.3
N	52	192	602	7

[a]After evaluating future economic conditions for a second time, respondents who gave a positive or negative assessment were told that recent newspapers articles have reported economic prospects contrary to their evaluation and then asked to reevaluate the future state of the national economy. The table compares respondents' second prospective evaluation of the national economy with their postinformation prospective evaluation. Cell entries are percentages calculated by column.

and then asks them to reevaluate the economy taking into account this information. The persuasion scenario is only administered to those respondents who gave a nonneutral response (positive or negative evaluation) to the second prospective question. Hence, the second prospective economic evaluation constitutes a selection process that determines the subsample of respondents who are exposed to the contrary media information and presented with the opportunity to revise their evaluation. Respondents who gave a neutral or "don't know" response to the second prospective question are excluded from (selected out of) the sample even though the contrary media cues might have greater influence on their responses.

Proper estimation of models of information-induced response instability must take into account this sample selection process. In particular, it must allow for possible (negative) correlation between the probability of a nonneutral prospective evaluation and the probability of information-induced change in the prospective evaluation. Hence, bivariate probit with sample selection represents the most appropriate method here (see Greene 1997, pp. 906–911). Note that the specifications of the *Evaluation Change* equations are identical to those employed in Table 3. As in Table 3, we included *Consistency of Government Party Attachment*, *Consistency of Prospective PFS*, and *Strength of Government Party Attachment* to evaluate the resistance and activation hypotheses. The models also control for sophistication, media usage, social status, and demographic characteristics.[3]

The explanatory variables seek to account for heterogeneity in information-induced change in prospective economic evaluations. While the overall performance of the models is modest, several interesting results emerge. First, the significant negative correlation between the error terms in the two equations (i.e., the rho coefficient) suggests that Table 4 understates the extent to which contrary media cues can induce changes in economic evaluations. The negative correlation implies that respondents who were less likely to volunteer a nonneutral response to the prepersuasion evaluation question were also more likely to change their

[3]The effects of these explanatory variables are largely insignificant in the *Evaluation Change* equations. The notable exception is *Economic Media Usage*, which has an estimated effect in the opposite direction of that in Table 3. This difference is probably attributable to the nature of the persuasion scenario. We speculate that respondents with greater exposure to economic media were more likely to discount the contrary media information "reported" to them in the persuasion scenario.

Table 5 Bivariate probit models of change in prospective national economic evaluations in reaction to contrary media information[a]

	Model 1		*Model 2*	
Explanatory variable	*Evaluation change*	*Nonneutral response*	*Evaluation change*	*Nonneutral response*
Consistency of Government Party Attachment	−.005	—	−.002	—
	(.019)		(.020)	
Consistency of Prospective PFS	−.057*	—	−.054*	—
	(.024)		(.024)	
Strength of Government Party Attachment	—	.062**	−.019	.064**
		(.021)	(.032)	(.022)
Strength of Prospective PFS Evaluation	—	.390**	—	.387**
		(.056)		(.056)
Economic Sophistication	−.083	.016	−.075	.015
	(.059)	(.049)	(.060)	(.049)
Economic Media Usage	−.196**	.114*	−.195**	.114*
	(.066)	(.047)	(.065)	(.047)
Family Income	−.054	.085**	−.056	.085**
	(.037)	(.028)	(.037)	(.028)
Education	−.003	−.077*	−.001	−.077*
	(.049)	(.034)	(.049)	(.034)
Age	−.0055	−.0012	−.0052	−.0012
	(.0030)	(.0022)	(.0030)	(.0022)
Female	.11	−.05	.11	−.05
	(.10)	(.07)	(.09)	(.07)
Constant	.34	−.15	.38	−.15
	(.24)	(.16)	(.24)	(.16)
Rho	−.49*		−.53*	
	(.23)		(.23)	
Expected % predicted correctly	42.2		42.2	
% error reduction	4.8		4.5	
N	853/1394		853/1394	

[a]*Evaluation Change* is a binary variable denoting those respondents who changed their prospective evaluation of the national economy when told that recent newspapers articles have reported economic prospects contrary to their evaluations (as measured by the second prospective question). *Nonneutral Response* is a binary variable denoting those respondents who gave an initial positive or negative prospective evaluation and thus were asked the follow-up question that reported contrary media information. "Don't know" responses to the initial prospective question are excluded from the analysis, while "don't know" responses to the follow-up question are coded as an evaluation change. Coefficient standard errors are reported in parentheses. $^{*}p < .05$. $^{**}p < .01$ (two-tailed).

evaluation when exposed to the contrary media information. Given that respondents who actually gave neutral evaluations were screened from the persuasion scenario, the level of response instability in Table 4 is probably lower than if all respondents were exposed to media cues and asked to reevaluate future economic conditions.

Second, consistency between partisanship and the prepersuasion economic assessment does not affect the likelihood that contradictory media cues induce a response change. On the other hand, respondents with personal financial situations that are inconsistent with their prepersuasion economic assessments are significantly more likely to change their evaluation in reaction to the "media persuasion" stimulus. This finding suggests that the media is particularly effective at activating personal experiences, which cause citizens to

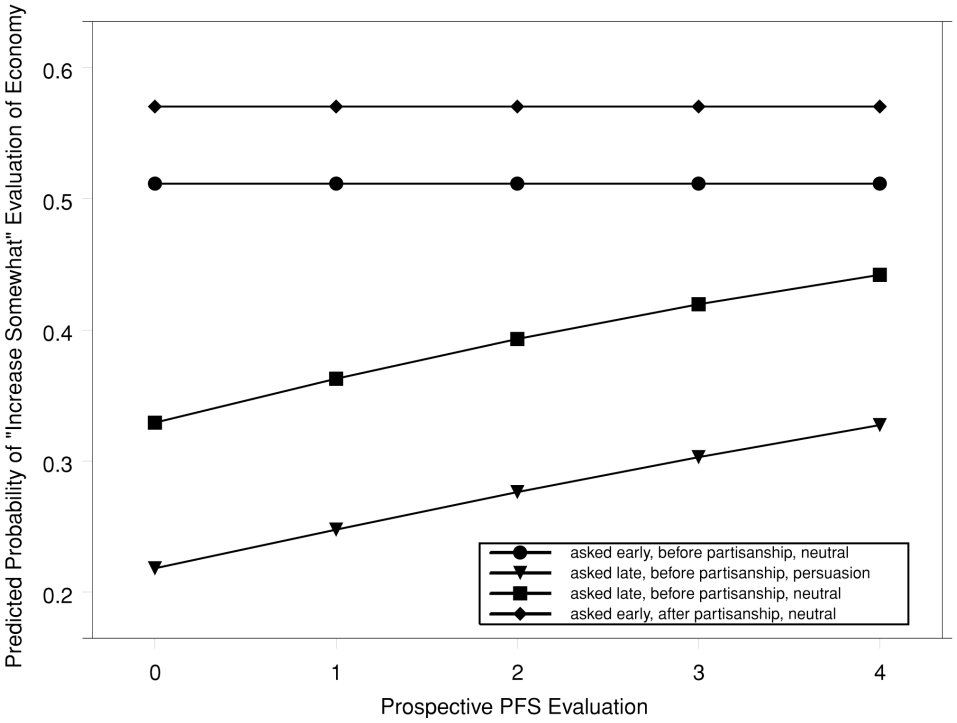

Fig. 1 Effect of design on government partisans' likelihood of positive prospective economic evaluation.

project their personal financial situation onto that of the national economy (Mutz 1994). To some extent, Ronald Reagan's successful effort during his 1984 reelection bid to exploit improving economic conditions can be linked to his effective media campaign, which encouraged voters to evaluate how much their personal financial situation had improved over his 4-year term (Lau et al. 1990). This finding suggests that it might not be appropriate to place survey questions "in context" by reference to media reports. Questions phrased in the following manner should clearly be avoided: "There has been considerable talk in the media lately about rising unemployment."

While the effect of the "media persuasion" stimulus is relatively modest, it can further exacerbate the contamination of poor survey design. Figure 1 illustrates the cumulative impact of survey design flaws, whose separate effects are modeled in Tables 1, 3, and 5. The figure also demonstrates how the cumulative impact of survey design on the likelihood of a positive prospective evaluation of the economy varies with respondents' evaluations of their personal financial situation. The top horizontal line in Fig. 1 is a baseline probability for comparison, derived from Table 1, corresponding to a survey design that maximizes the likelihood of a positive evaluation. This predicted probability of .570 represents the likelihood of an "increase somewhat" evaluation for typical respondents who feel "moderately close" to a government party and were asked to evaluate the economy later in the survey after questions about their partisanship and political attitudes.[4] Hence, while this baseline prediction assumes that respondents were asked an economic evaluation question that had

[4]We define typical respondents here as having mean (median) values of the other (binary) independent variables.

neutral wording, it does incorporate the effect of question order. Since the *Prospective Sociotropic* equation in Table 1 does not include prospective personal financial situation, the probability is constant across all values of the horizontal axis in Fig. 1.

The second horizontal line in Fig. 1, at .512, constitutes the predicted probability that typical government partisans would positively evaluate the economy if the prospective evaluation question were placed before political questions on partisanship. The difference between the two horizontal lines in Fig. 1 illustrates the effect of question order. The standard error for the second-line probability is .039 so it is relatively precise though not quite distinguishable from the top-line probability at the 5% level.[5]

The third and next-lowest line in Fig. 1 reflects the additional effect of response instability due to activation and priming by the survey instrument. The slope of the third line captures the consistency effect—respondents with more negative (inconsistent) attitudes about their personal finances are more likely to revise their national economic evaluations so that they match their personal financial circumstances. The third-line probabilities are products of the probabilities from the second horizontal line and predicted probabilities (from model 1 in Table 3) that within-survey response change *does not* occur. For the purpose of constructing Fig. 1, we interpret the response instability modeled in Table 3 as indicative of the effect of posing the economic evaluation question near the end of the survey. As shown in Fig. 1, the estimated probability of a positive prospective evaluation of the economy declines considerably due to question placement: from .512 to .329 for those with the most negative personal financial situation and from .512 to .442 for those with the most positive situation. The standard errors for the third-line probabilities range from .029 to .036, respectively.

The fourth-line probabilities are products of the third-line probabilities and predicted probabilities (from model 1 in Table 5) that response *change* due to media persuasion *does not* occur. The difference between the fourth line and the top horizontal line represents the cumulative effect of survey design resulting from question order, placement, and wording. This difference is greatest for respondents with the most negative (inconsistent) evaluations of their personal financial situation. The fourth-line probabilities range from .218 for respondents who believe that their future financial situation will "worsen a lot" to .327 for those who believe that their future finances will "improve a lot." The standard errors for these probabilities range from .032 to .042, respectively, so the overall probability changes induced by survey design are significantly different from zero at better than the 1% level.

To characterize the broader substantive implications of changes in the likelihood of a positive prospective assessment of the economy, we used the Hungarian survey data to estimate a probit model of government party support. This model included the prospective evaluation as an explanatory variable as well as controlling for other relevant factors.[6] According to

[5] We applied the delta method (Greene 1997, pp. 278–279, 884–845) to derive standard errors for the lines in Fig. 1. This derivation was straightforward for the second horizontal line since it is simply a predicted probability from an ordered probit model. For the lowest two lines, however, the derivation of standard errors was more complex since the lines are products of predicted probabilities from different models in Tables 1, 3 and 5. To derive these standard errors, we imposed the assumption that the probabilities from the different models are independent. Then for the lowest line, we applied the following equation: $\mathrm{Var}[abc] = (bc)^2\mathrm{Var}[a] + (ac)^2\mathrm{Var}[b] + (ab)^2\mathrm{Var}[c]$, where a, b, and c are the predicted probabilities from Tables 1, 3 and 5.

[6] Respondents who stated no vote intention were excluded from the estimation ($N = 901$). The control variables were measures of the respondent's education, family income, age, employment status, and gender. The probit coefficient for *Prospective Sociotropic* was .048, with a standard error of .024. Education proved to be the most significant determinant with a one-category decrease in education level decreasing the predicted probability of a government vote among typical respondents by almost 4 percentage points. The full probit results are reported in the web version of this article.

these probit results, an evaluation change from "improve somewhat" to "remain the same" decreases the probability of a government party vote among typical respondents by approximately two percentage points. As shown in Fig. 1, the cumulative effect of survey design reduces the probability of an "improve somewhat" prospective evaluation by 24.3 to 35.2 percentage points. Given the sample distribution of prospective personal financial situations, these probability changes imply that survey design could produce a one-category decrease in national economic evaluation for 29.6% of the respondents, which roughly translates into a 1 percentage-point swing in aggregate political support for the government. Hence, survey designs with multiple flaws can significantly contaminate economic evaluations, which in turn can bias the estimation results of models that incorporate these assessments as independent variables.

5 Conclusion

We argue that survey responses to economic evaluation questions reflect the common pathologies Zaller (1992) attributes to opinion research: responses are unstable and can be affected by fairly trivial changes in survey design. Our analyses make three empirical contributions to this literature and have specific implications for the design of economic voting survey instruments. First, we demonstrate that economic evaluations are subject to the "priming" effects associated with partisanship questions. As expected, priming effects are most likely to shape the responses of individuals with "inconsistent" political and economic attitudes. As others have argued (e.g., Lau et al. 1990), these priming effects have serious implications for economic voting models because the correlations between economic evaluations and political preferences are artificially inflated by question placement. Accordingly, economic evaluation questions should ideally precede questions about partisan preferences.

Second, our findings suggest that survey responses to economic items build on various considerations that are salient in the respondent's mind when the questions are posed. Consequently, we can expect respondents' economic evaluations to evolve over the course of administering the questionnaire. In the case of our experiment, over 40% of the respondents changed their prospective assessment of overall economic performance. Yet some respondents were more likely than others to engage in attitude updating. Response instability was higher among those respondents who gave a negative evaluation of the national economy but were supporters of the governing coalition parties. Moreover, this instability was highest among those with the strongest party attachments. Similarly, respondents who had personal financial experiences that were inconsistent with their assessments of the national economy were also more likely to exhibit response instability.

We cannot avoid attitude updating during the administration of a questionnaire. But we can be cognizant of this fact in the design and ordering of questions. Recognizing that the questions preceding a battery of economic evaluation items might serve as cues for some respondents, care should be taken against including questions that might stimulate an unbalanced or biased perspective on the economy. For example, it might be inappropriate to precede the economic evaluation items with a series of questions concerning attitudes toward poverty.

Finally, our results indicate that one source of response instability is cues regarding economic performance, such as those provided by the media. Respondents can be persuaded to change their economic evaluations if they receive contradictory cues. Once again, those who volunteer economic evaluations that conflict with their own partisanship and personal financial situation are more likely to be persuaded by cues from the media. In an interview in which many respondents are engaging in attitude updating, this suggests against using

questions that attribute information about the economy to salient and "trust worthy" cue givers—such as the media, educational institutes, or certain political institutions.

Our essay demonstrates that the interview situation is one in which many (not all) respondents have fairly vague ideas about how the economy has performed. For those with relatively "top of the head" attitudes regarding the economy, the interview experience represents an opportunity to update their assessments of economic performance. Over the course of the interview, respondents receive cues that they can exploit in forming and possibly re-forming economic evaluations. None of this is catastrophic for survey research on economic voting. Rather, these results simply provide insights into the design of questionnaires dealing with economic attitudes. Our design of questionnaires should leverage findings such as these and those reported in other studies (e.g., Zaller 1992; Lau et al. 1990) to ensure that question wording and placement do not prime respondents' attitudes. At a very minimum, we should avoid unbalanced cues in question wording and the placement of partisanship questions close to questions regarding the evaluation of policy outcomes. Recognizing that the interview experience triggers the formation of attitudes for some respondents, it also seems appropriate to design questions that encourage respondents to engage in balanced reflection of relevant economic considerations before asking them to evaluate the economy.

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