MESSAGES FORGOTTEN:
Misreporting in Surveys and the Bias Toward Minimal Effects

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I. Introduction

Anyone who has read the tea leaves of public opinion polls is struck by one recurring theme: when politicians and journalists speak, voters don't seem to listen. The evidence for this result is impressive. In fifty years of intense study, survey researchers have detected few discernable differences between those who report that they have and those who report that they have not seen an advertisement (Patterson and McClure, 1976; West, 1994), received congressional mail (Johannes and McAdams, 1979), had direct contact with or even heard something about their representatives (Cook, 1981), heard a speech (Berelson, et al, 1954), read or seen a news story (cites), watched or heard a political debate (cite), or followed the campaign or public affairs in the news (Patterson, 1980; Graber, 1984). Writers summarizing the volumes of research describe a virtual consensus that political communications have minimal effects on the public's beliefs and behavior.

Taken at face value, survey results suggest that politicians can win office and govern without constituent mailings, without hours of press conferences, without broadcast advertising, without coverage by the local media, and without the services of high-priced media consultants. Yet, over the last fifty years, American politicians have embraced

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the technologies of mass communications, as politicians in no other country have. U. S. elections are no longer won through the labor of party organizations. Today, campaigns are media-driven affairs in which candidates spend hundreds of millions of dollars to create and broadcast their messages. And once in office, legislators churn out tons of mail to their constituents and stay up to the wee hours of the morning performing for the unmoving eye of the C-SPAN cameras. Perhaps there is something perverse about the behavior of politicians. More likely there is something wrong with the survey research.

We demonstrate theoretically and empirically that the minimal effects conclusion can be attributed entirely to misreporting in survey questions. A survey cannot determine if a respondent indeed saw or heard a message. Instead, surveyors must rely on the respondent's own reported media exposure. Unfortunately, serious errors creep into even the most straightforward questions about recalled or reported exposure. Many people simply forget that they had seen or heard a message; others state that they received a message when in fact they did not, so as not to appear out-of-touch to the interviewer. As we demonstrate throughout this paper, such seemingly innocent measurement errors bias estimates of media effects toward zero, and are sufficient to explain why surveys repeatedly find minimal effects. Importantly, the source of the bias is not random measurement error, which is well understood (Achen, 1987; Goldberger, 1972), but systematic misclassification, which is poorly understood by methodologists in political science and much harder to correct.

A few examples give the reader a sense of the general format of questions that surveys have used to ascertain media exposure. A list of others can be found in Price and Zaller (1993). Each of these questions has produced minimal effects results.

1948 Voting Study. "Did you listen to or read any of Truman's speeches in the last few days before the election?"

"Did you hear the broadcast by the Republicans, with all the movie stars, on the night before the election?" (Berelson, et al, 1954)

1952 and 1978 National Election Study. "Do you happen to remember anything special that your U.S. representative [name] has done for this district or for the people in this district while [he/she] has been in Congress?" (Cain, et al., 1987)

1972 Presidential Advertising Study. To measure political advertising exposure: "Below is a list of nighttime television programs that are shown in this area once a week. You are to indicate your own viewing of each of these programs during the past four weeks. ... If you have actually watched the program in the last four weeks, then check the box that tells how many of the last four shows you have watched." (Patterson and McClure, 1976)

1992 Los Angeles County General Election Study "How many times in the past week do you recall seeing Senate campaign ads for [candidate name]? (1) Five days or more, (2) three or four days, (3) one or two days, or (4) not at any time." (West, 1994)

The responses to such questions are certainly related to actual exposure to the messages, but the correlation may not be very strong. For example, many more people know the names of their representatives than can recall them. Studies of Congress find that almost all survey respondents (85 percent), when presented with a list of names, recognize the names of their representatives in the U.S. House. However, without any prompting, only about half of the respondents can recall the names of their Members of Congress (Mann, 1978; Jacobson, 1991). The upshot is that recall questions may classify large numbers of people as not knowing their representatives, when, in fact, they do.

When reported media exposure is used in lieu of actual media exposure to explain voters' opinions, the estimated effect will invariably be biased toward zero. A simple example clarifies the nature of the bias due to misclassification. Suppose that there are six people, four of whom are actually exposed to a message and two are not. Of those actually exposed to the message two report that they did not receive it. I.e., half of those who could not recall seeing a message actual had. Assume that the dependent variable (say vote preference) equals 1 if a person is actually exposed to a message and 0 if not. The true effect, then, is 1. But the estimated effect using recall data is 1/2 since the mean among those who report that they were exposed is 1 and the mean among those who report that they were not exposed is 1/2 (= (1+1+0+0)/4).

Recall questions may be further biased toward minimal effects if some respondents falsely report that they had seen or heard a message. Continuing with the example above, if one of the two people who did not see the message reports that he or she did, then the estimated effect equals 0. The mean of those who report that they saw the message is 2/3, since it is the average of the response for the two people who correctly reported seeing the message and the one person who falsely recalled the message. The mean of those who report that they did not see the message is also 2/3, since it is the average of the response variable for the two people who forgot that they saw the message and the one person who correctly reported that he or she did not receive the message.

We are certainly not the first writers to doubt the minimal effects conclusions. Researchers looking at fluctuations in survey results over the course of campaigns (rather than at the relationship between two questions within a survey) find sizable shifts in public opinion associated with conventions, debates and other campaign events (Gelman and King, 1993; Holbrook, 1994). Iyengar and Kinder (1987) and many others demonstrate that in a laboratory setting broadcast and print messages strongly influence viewers' attitudes. Cain, Ferejohn, and Fiorina (1988) argue that strategic resource allocation biases estimates of the effectiveness of political communications toward zero. Correcting for the fact that vulnerable incumbents are more likely to engage in direct voter contacts, Cain, et al., show that congressional mail and other constituent communications strongly affect evaluations of the incumbents.

Nor are we the first to doubt the veracity of the survey instrument. Graber (1984), Neuman, Just, and Crigler (1993) and Price and Zaller (1993) critique reported media exposure variables as measures of actual information learned from television news. Bartels (1993) argues that it is impossible to isolate the effect of one message in a continual flow of information, and Zaller (1992) argues that different people react to information differently, so researchers are unlikely to find a media effect averaging over all persons. Each of these studies represents a serious challenge to the minimal effects hypothesis, but none has shown that survey findings are necessarily wrong. In fact, some writers go to great lengths to square their evidence of strong media effects with existing findings of minimal effects (see Bartels, 1993, page 275). No such apologies are necessary. Our evidence is unequivocal that misreporting is the root of minimal effects. Using experimental data, we contrast two estimates of the effects of political advertising on vote preferences: one estimate uses an indicator for those actual exposure to a broadcast message, the other uses an indicator for those who recalled seeing a message. When actual exposure is used, the effects are strong and statistically significant. When reported exposure is used, the effects are much smaller in magnitude and statistically insignificant.

While this problem with the media effects findings is certainly damning, it is also remediable. The experimental data allow us to evaluate various methods for adjusting survey data. A two-stage model similar to that developed by William McGuire and John Zaller can be used to correct much of the bias.

In section 2, we derive a formula that expresses the bias due to misreporting. In section 3, we present experimental results that show the extent of the bias in recall questions and, using the result from section 2, we calculate the sources of the bias. In section 4, we use the experimental data to evaluate a two-stage method for correct these biases in survey data.

II. Theoretical Results

We are interested in the effect of actual media exposure on a response variable, Y, such as level of information, sense of political efficacy, or voting preferences. Specifically, we would like to contrast the mean levels of Y among those people exposed to a message and among those people not exposed to a message.

If it were known who was actually exposed to a message, then an unbiased estimate of the effect of exposure on the dependent variable could be derived by regressing Y on a dummy variable that indicates media esposure, X, and other factors that affect the response variable.[2] Other factors are represented as a matrix Z or, for any individual observation i, a vector $\mathbf{z}_i$. That is,

$$y_i = \beta_0 + \beta_1 X_i + \mathbf{z}_i' \beta_{\mathbf{z}} + \epsilon_i. \tag{1}$$

Surveys cannot measure actual exposure to a message. Instead, they elicit whether someone recalls receiving a message. As the experimental data reported in section 3 show, many people simply forget that they saw a message, and, occasionally, people say they saw something that they did not. In determining the consequences of both forms of reporting error on regression estimates, we distinguish survey respondents according to four different indicators. The first two indicators distinguish actual and reported exposure. X denotes actual media exposure; it equals 1 if a respondent infact saw or heard a message, and 0 otherwise. R denotes reported media exposure; it equals 1 if a respon-

[2]We are ignoring further problems that may arise from selective perception and from strategic resource allocation. These problems are important in their own right and ignoring them might introduce further biases. Our point, however, is that the biases in surveys can be attributed almost entirely to faulty recall measures.

dent recalled seeing or hearing a message and 0 otherwise. The third and fourth indicators distinguish different types of people according to two different types of errors in memory that may be committed. M denotes those people who remember the messages that they see. M equals 1 if the respondent would remember the message and 0 otherwise. Importantly, this variable represents a characteristic of each survey respondent that is separate, though not necessarily independent, from actual exposure. Some people who would remember a message are not exposed to it, and some people who would forget it are exposed. Finally, F denotes those people who falsely report that they saw a message (regardless of whether they saw it). F equals 1 if the respondent would falsely report exposure, and 0 otherwise.

The indicators for people who remember and people who falsely recall connect actual and reported exposure in a straightforward way: $R = XM + F - XMF$. Table 1 shows the possible cases, and the relationship between the indicators for actual exposure, reported exposure, remembering, and false recall.

[Table 1]

In the experimental setting, X is a fixed number since exposure to the message is controlled. Whether someone remembers or falsely recalls a message, on the other hand, is not a fixed dichotomy. Both indicators for types of errors of memory are continuous, but latent variables. We assume that M and F have random components, u_1 and u_2, and they have systematic components, M^* and F^*, respectively, that are a function of various factors including age, education, and interest in the subject matter. Hence, M is a random number that equals 1 if $M^* + u_1 > 0$, and F is a random number that equals 1 if $F^* + u_2 > 0$. Since M and F are random, so too is R.[3] Survey researchers use the random variable R as a proxy for the actual, but unobservable media exposure

[3]This treatment differs from Zaller's (1992) treatment of the problem. He defines someone to be a forgetter only if the person has actually seen the message and then forgotten it.

variable, X. Inserting R in the place of X in equation (1) changes both the intercept and the slope. Using the definitions from Table 1,

$$\begin{aligned} y_i =& [\beta_0(1 - F_i)(1 - X_i) + \beta_1 X_i(1 - M_i)(1 - F_i)] \\ & + [\beta_0(1 - X_i)F_i + \beta_1 X_i(M_i + F_i - M_iF_i)] + \mathbf{z}_i'\beta_{\mathbf{z}} + \epsilon_i \\ (2) \qquad =& \alpha_0 + \alpha_1 R_i + \mathbf{z}_i'\beta_{\mathbf{z}} + \epsilon_i. \end{aligned}$$

Implicitly, this substitution assumes that errors in recall do not mediate the effect of actual exposure to a message. That is to say, the persuasive effects of a message are the same for those who forget and those who don't. If recall mediates or interacts with the effects of media exposure, then interaction terms between recall and M and F must be included in equation (2), as well. We assume that these interactions are zero, and the results in section 3 clearly confirm this assumption.

Using equation (2) in lieu of equation (1) will produce bias estimates of the effects of media exposure toward zero. Two problems arise. First, misclassification shrinks the size of the estimated effect downward in proportion to the true effect. The regression estimate of α_1 is the estimated difference between the mean of those who report that they were exposed and the mean of those who report that they were not exposed, holding all other factors constant. However, some people who report that they were not exposed infact were exposed to and persuaded by the message, and some people who report that they were exposed were infact not exposed. Under the assumption that recall does not mediate the effects of political advertising, these "accounting" errors shrink the means of the two groups of reported exposure ($R = 1$ and $R = 0$) toward one another in proportion to the fraction of those in each group who are actually misclassified.

Conceptually, this is problematic because the ability to remember is a characteristic of individuals regardless of whether they have seen a message. Defining M only for those who have $X = 1$ will, thus, impose severe restrictions on u_i, rendering it non-normal.

Second, selection bias in the errors may exist if the typical individual who recalls the message would have scored high (or low) on the dependent variable anyway. Correlation between the errors in the regression (ϵ) and the errors in the latent variables (u_1 and u_2) will mean that a disproportionate number of positive (negative) errors are attributed to those that could not recall a message or those that falsely remembered one, if the correlations between ϵ and u's are positive (negative). The sign of this bias term will depend on the sign of the correlation in the errors. If one assumes that the errors are jointly normal, it is possible to specify a correction term. We choose a second approach, which is to estimate the size of the selection and misclassification biases experimentally.

The specific formula for the bias due to misreporting follows:

RESULT:

$$\text{Bias}[a_1] = E[a_1 - \beta_1] = -(f_{10} + f_{01})\beta_1 + S$$

where $f_{10} = Pr(X = 1|R = 0)$, $f_{01} = Pr(X = 0|R = 1)$ and $S = E[\epsilon|R = 1, X = 0] - E[\epsilon|R = 0, X = 1]$.

PROOF: See Appendix.

Two factors drive this bias formula–the selection bias in the error, S, and the misclassification bias, $f_{10} + f_{01}$. The piece due to selection bias is the standard sort discussed in by statisticians and econometricians (Goldberger, 1972; Willis and Rosen, 1979; Achen, 1987; Madalla, 1983, surveys many applications). It is a function of the correlations between the measurement errors and the regression error. If those are zero it is zero.

The bias due to misclassification is not standard. As the formula indicates, faulty memories will shrink the regression estimates at the rate $f_{10} + f_{01}$. The fraction f_{10} is the fraction of those who report that they were not exposed but actually were. This is a function of the rate at which people forget. The fraction f_{01} is the percent of people who report that they were exposed but in fact were not, which is a function of the false memory rate. If people are apt to forget or falsely report a message, then the bias due to misclassification will be sizable. Since these terms are always positive, the bias is always toward minimal effects. Reported exposure can even produce negative estimates of media exposure when the true effect is positive, since the two conditional probabilities can sum to more than one.

III. Experimental Evidence of Bias

The magnitude of the bias produced by recall questions is ultimately an empirical matter. How frequently do people forget that they saw a message, how frequently do they report that they saw a message when infact they did not, and how much selection bias is contained in the regression errors?

We estimated the size and sources of the bias in recall questions experimentally. In our experiments, we showed one set of participants a political advertisement embedded in a videotape of the nightly news; we showed a second group the same video without the political ad. Comparing the opinions of these two groups provides an unbiased estimate of the effect of advertising exposure. Approximately one-half hour after they watched the videotape, we asked all participants what commercials they could recall seeing in the treatments. Hence, we measure directly the rate at which people who are actually exposed to messages forget or falsely recalled what they saw. Most importantly, we can estimate directly how misreporting biases estimates of media exposure by contrasting the unbiased estimate of advertising exposure with a biased estimate derived using recalled instead of actual exposure. Before presenting the findings, we briefly describe the experimental procedures. A complete description of the experiments is found in Ansolabehere, et al (1994).

Between and

~~From~~ 1990 ~~to~~ 1993 we conducted six different experiments in the Los Angeles area in which some viewers saw a videotape of a local newscast that contained a political adveritisement in the commercial break and other viewers saw the identical tape that contained no political advertisement. Each political advertisement was sponsored by one of the candidates engaged in an on-going campaign, and each discussed either an important issue in the election or the personalities of the candidates. The commercials were professionally produced expressly for use in our experiments. Identical messages were prepared for all candidates in a given race, so our estimates of the effects of the advertisements are not confounded by differences in the issues addressed, the tone of the messages, or the quality of the production. The local newscast, within which we nested the experimental treatment, had been broadcast no more than a week prior to the experiment and contained no stories about politics or the elections; also, the other advertisements in the commercial break were product ads.

When participants came to our laboratory they filled out a brief pre-test questionnaire that included questions about the demographic characteristics and political beliefs and behavior of the respondents. Each individual then watched a videotape, to which he or she had been randomly assigned. After the tape was completed, the participant filled out a questionnaire that inquired about the material in the newscast, the respondent's opinions about politics in general, and about the on-going elections. In the analysis here we contrast the estimated effects of actual and reported advertising exposure on voting preferences. The dependent variable is simply voting preference. Two-thirds through the post test questionnaire we asked: "How do you intend to vote in the coming elections?" Respondents could choose one of the candidates listed, write in another name, state that they were undecided, or state that they did not intend to vote. Actual exposure is dictated by the experimental manipulation. Reported exposure is measured at the end of the questionnaire (approximately one-half hour after watching the videotape):

"Now we'd like you to think back to the commercials you watched in the newscast. Please list the ads you can remember and say a little about their content."

We code the respondents as recalling the experimental advertisement if they say *anything* about a political advertisement, including simply stating that there was a political or campaign ad.

Recall rates in our experiments were shockingly low. Table 2 presents the reported exposure rates among those exposed to an experimental advertisement and those not exposed in all six experiments combined.[4] Forgetting is clearly a significant problem when recall questions are used to measure an actual event. Slightly more than half (56 percent) of those people who saw an experimental advertisement could recall having seen the commercial. Also, of all people who could not recall having seen a commercial in the videotape 54 percent had in fact seen one.[5] In other words, f_{10} is quite high, .54, and the forgetfulness of survey respondents is likely to be a very serious problem.

[Table 2]

To assess how misreporting influences estimates of media effects we contrast three regressions. The first regression estimates the effects of actual exposure on voting preferences, holding other factors constant. The second regression estimates the effect of re-

[4]In table 2 and in subsequent analyses we have pooled all six experiments. A simple F-test revealed that pooling was justified (e.g., there were no statitistically signficant differences in recall rates across experiments).

[5]False memories may bias recall questions, just as our results indicate that forgetting does, but our data indicate that false memories are extremely uncommon and are of little concern. Only 4 percent (18 people) of those who saw no political advertisement stated that they had seen one. These people may have merely guessed that one of the ads had something to do with the campaign, or they may have recalled an ad that they had seen outside of the laboratory setting.

ported exposure on voting preferences. The third regression contrasts the responses of those not exposed to an advertisement, those who were exposed and could not recall seeing the message, and those who were exposed and could recall the message. Since the rate of false recall is trivial, we focus only on the effects of forgetting in these regressions, and exclude the 18 individuals who falsely reported that they had seen a commercial. Table 3 presents the parameter estimates for all three specifications.

The dependent variable in each regression is a trichotomy that equals +1 if the respondent intends to vote for the Democratic candidate, 0 if the respondent is undecided, and -1 if the respondent intends to vote for the Republican. Since this variable is not continuous non-linearities might also be a problem. We estimated the regressions as ordered probits and found that the non-linearity created by the discrete dependent variable does not affect our estimates at all. Table 3 presents the linear regression estimates, since those are easier to interpret. While we discuss the probit estimates for completeness, the appendix contains the full results of the probits.

We code the actual and reported exposure measures similarly to the dependent variable. Actual or reported exposure equals +1 if the sponsor of the ad (either actual or recalled) is a Democrat, 0 if no political ad is shown, and -1 if the sponsor of the ad is a Republican.[6] The coefficient on this trichotomy measures the effect of exposure to an ad, regardless of the party of the sponsor. Specifically, it is the increase in vote support for the sponsor, averaging over the party of the sponsor, that is attributable to advertising exposure (or recall). A simple F-test revealed that we could not reject the hypothesis that the effects of the actual advertising exposure are symmetric. Symmetry is not justifed in the Recall equation. Both the intercepts and the effects differ across the Democratic and Republican experiments. To correct for this we include an additional variable to indicate whether the experiment involves a Republican or Democratic advertisement.

[6]We exclude the 18 cases of false recall.

In addition to the indicators for actual and reported exposure the regressions include several control variables that strongly affect voting preferences. The control variables are all measured in the pretest questionnaire, so answers to these questions are not affected by actual advertising exposure. "1988 Turnout" equals 1 if the respondent stated that he or she voted in 1988 and 0 otherwise. "1988 Vote" equals +1 if the respondent indicates a preference for Dukakis, -1 if the respondent indicates a preference for George Bush, and 0 if the respondent says that he or she did not vote, could not recall, or would not say. "Party Identification" is also coded as a trichotomy, and equals +1 if the respondent stated that he or she is a Democrat, -1 if he or she is a Republican, and 0 if neither. "Independent" is a dummy variable for non-partisan voters. "Follow" is a four point scale indicating the frequency with which the respondent "follows government and public affairs": lower values represent higher levels of interest. "White" and "Female" are indicators for respondents who identified themselves as caucasians and females, respectively. "Education" is an ordinal variable that equals 1 if the respondent did not finish high school, 2 if the respondent is a high school graduate, 3 if he or she went to but did not complete a college program, 4 if the respondent is a college graduate, and 5 if he or she had done some post-college study.[7] "Age" measures the age of the respondent in years, and ranged from 18 to 89. Descriptive statistics for these variables are contained in the appendix.

[Table 3]

The estimates in the first two columns of Table 3 reveal that misreporting of recall questions is sufficient to produce the minimal effects commonly found in survey data. When Actual Exposure is used in a linear regression, there is clear evidence against the hypothesis that advertising has minimal effects on voting preferences. The estimated

[7]We also estimated the regressions with a dummy variable for college graduates, instead of the 5-point scale. The dummy worked less well, so we used the original measure.

effect of exposure to a single ad is .078. In other words, exposure to an advertisement increases the sponsor's lead by 7.8 percentage. With a standard error of .018, this estimate is highly significant, with a p-value less than .01. When Reported Exposure is used instead of Actual Exposure a very different story emerges. The estimated effect of Reported Exposure in the linear regression is just 2.3, less than a third the size of the unbiased estimate above. With a standard error of 3.2, this effect has a t-statistic less than one and is statistically and substantively trivial.[8] Using Reported Exposure, then, would lead a researcher to conclude that advertising does not influence people's preferences, when in fact it does.

The seriousness of the errors caused by misreporting can be more fully appreciated by calculating the amount and sources of the bias. Our estimate of the bias is simply the difference between the estimated effects when actual exposure is used for the treatment variable and when reported exposure is used. We estimate the bias to be -.054 (=.023-.077) in the linear regressions and -.117 (=.046-.163) in the probits. In both cases the bias is more the double the estimated effect using reported exposure.

The estimates in Table 3 also allow us to separate the extent to which the bias stems from misclassification or selection. Consider the formula derived in section 2. The bias attributable to misclassification equals $-f_{10}\beta_1$. From Table 2 we calculate that f_{10} equals .54, and from Table 3 we estimate that β_1 equals .077 in the linear case and .163 in the probit case. Hence, the amount of bias due to misclassification is .042 in the linear case and .088 in the probit. The bias due to selection can be calculated as $S = a_1 - b_1 + f_{10}b_1$, which equals .013 in the regression and .039 in the probit. Using

[8]The probits, though somewhat harder to interpret substantively, show the same pattern. The coefficient on Actual Exposure was .163, which, with a standard error of .038, is highly significant. The coefficient on Reported Exposure is .046, which, with a standard error of .065, is statistically insignificant.

these figures we estimate that in the linear case 78 percent (.042/.054) of the bias and in the probit case 75 percent (.088/.117) of the bias comes solely from misclassification, with the remainder is due to selection. While neither bias term is trivial, the systematic errors caused by misclassification are clearly more important and deserve closer methodological attention from survey researchers.

Finally, the results in Table 3 verify the key assumption in our analysis is justified. In deriving the result of the previous section, we assume that recall does not mediate the persuasiveness of actual advertising exposure. Specifically, the effect of advertising exposure is β_1 for both those who recalled and those who forgot that they had seen an experimental advertisement. If recall is an important mediating step in the chain of reasoning, then those who could recall the message should exhibit different preference from those who were exposed to the message but could not recall it and from those who were not exposed to the message. Such a mediating effect would manifest itself in the regressions as an interaction between actual and reported recall.

We found no significant difference between the forgetters and the recallers. The third column of the table presents a regression in which we break the indicator for actual exposure into two. One of these variables indicates whether someone actually saw an ad and reported that they did; the other indicates whether someone saw an ad but reported that they did not. The estimated difference between these groups is -.009, with a standard error of .023.

One may put either a political or a methodological spin on the insignificance of the interaction between reported and actual exposure. Politically, this finding implies that people are influenced by political communications even if they cannot recall seeing the messages on which they based their opinions. Such a conclusion raises the specter that the mass media in politics produce subtle forms of political manipulation. Perhaps, some form of political consumer protection is in order. We do not want to push this wrinkle on the results too hard. Other aspects of voters decision making, such as their political predispositions, guards against voters choosing someone with whom they may fundamentally disagree. Instead, we think that the real lesson here is methodological. Survey questions that rely on self-reported media exposure perform very badly as measures of actual media exposure. Reported media exposure appears to contain little information about the effectiveness of political communication--not as a main effect and not as a mediator.

IV. Adjusting Surveys

Our findings on the biases contained in reported exposure are certainly discouraging. A straight-forward question inquiring what a person has seen can be so skewed as to produce no effects on the respondent's opinions. All is not lost. A handful of recent works have tried to adjust survey data to correct for misreporting, with varying degrees of success.

Two techniques have been proposed. First, one may use other proxy variables to measure media exposure instead of reported exposure. Bartels (1993), for example, employs frequency of television news viewing and newspaper reading as proxies for exposure to political information. Second, one may use a two-step model (akin to two- stage least squares or instrumental variables estimators) to estimate the extent of false reporting and to purge reported media exposure of measurement errors. Such an analysis requires variables that are correlated with exposure and with the memory process. Price and Zaller (1993), for example, argue that education is an instrument for exposure. In this section we use the insights from the experimental data to evaluate these two approaches.

Proxy Variables

Considering the degree of misreporting in recall questions, alternative measures of media exposure have great appeal. Media use variables are the leading competitors to recall questions. Since people forget what they saw, one way to measure exposure is to ascertain how often people use the media through which the messages travel. The more a person watches television news, reads newspapers, or talks about politics the more likely he or she is to be informed about politics (Robinson and Davis, 1990; Neumann, Just, and Crigler, 1992).

Such proxy variables must be handled with caution. Two sorts of errors in proxies can produce biases similar to those produced by reported exposure.

First, proxies contain random measurement error, which will tend to bias estimates of media exposure toward minimal effects. Even though frequency of media use may produce the correct average level of exposure, such questions do not produce the right measure of exposure for each case. Mathematically, these proxy variables equal the true variable plus random noise: $X^* = X + u_1$, where X is actual exposure and X^* is a proxy. It is well known that when a noisy variable is used in place of the true measure in a bivariate regression, the estimated effects will be biased toward zero (Achen, 1984). In a multivariate analysis, the sign of the bias cannot generally be determined (Greene, 1993). A number of corrections for this sort of error have been devised. To our knowledge, only Bartels (1993) has corrected for such errors in the independent variables, and his analysis shows strong media effects.

Second, proxy variables may be subject to systematic measurement errors similar to those that distort recall questions. One wants to know the effects <u>political</u> information on political opinions. Variables such as frequency of television watching and frequency of newspaper reading capture exposure to political as well as non-political information. A typical viewer, according to the Neilsen ratings, watches 4 hours of television a day. Very little of that programming covers politics: sitcoms, sporting events, and dramas draw much larger audiences than the evening news. Newspapers aren't much better. Daily papers are loaded with information about sports, entertainment, and department store sales.[9] Media use variables, thus, overstate the actual amount of political information to which a viewer or reader is exposed.

The result is further bias toward minimal effects. The intuition for this claim is simple. Regressions using proxies such as the number of hours of television watched measure the change in the dependent variable divided by the change in the media use variable, i.e., the marginal effect of an additional unit of newspaper reading or news watching. An additional hour of tv viewing, however, may contain only a small amount of political news, say 5 minutes worth. The denominator in the estimate of the marginal effect of media use is off by a factor equal to the number of units of media use (e.g., hours of television or days of paper reading) that one would have to endure to acquire one unit of political media use. If, for example, every hour of television contained 5 minutes of political information, then it would take 12 hours of television viewing to acquire one hour of political information, and the estimated effect equals one-twelfth the true effect. Mathematically, the observed media exposure is a multiple of actual exposure: $X^{*} = \lambda X$. A simple bivariate regression estimates the degree of bias. $b_1 = \frac{Cov(X^{*}, y)}{Var(X^{*})} =$

[9]The informational content of print and broadcast media is actually subject to some controversy. Neuman, Just, and Crigler (1992) and Price and Zaller (1993) find that newspaper exposure is unrelated to recall of information contained in print media. Jeffery Mondak (1995) concludes from a study of the Pittsburgh newspaper stike that "exposure to a major newspaper does not enhance knowledge of national or international politics." Robinson and Davis (1990), Berkowitz and Pritchard (1989), and Weaver and Drew (1993) all find that newspaper readers are better informed than the general public. Of course, the recall and media use measures employed in many of these studies are subject to the same criticisms raised here.

$\frac{Cov(\lambda X, \beta_0+\beta_1 X+\epsilon)}{Var(\lambda X)} = \beta_1/\lambda$.

This is essentially a scaling problem, and one possible correction involves contextual information. Researchers might want to estimate λ using content analysis of local newspapers and television programming, and then use the estimates of λ to rescale the media use variables so that they reflect a standard unit of political information per hour of television or days of newspaper reading per week. Alternatively, one might choose a more refined measure of media usage, such as frequency of viewing the local news. Such questions, however, sound strikingly similar to the reported exposure questions discussed earlier. Ultimately, it seems that researchers need to address the biases in reported media exposure directly.

Instrumental Variables

A second approach is to try to correct the errors in reported exposure, rather than to find a substitute measure. John Zaller (1992) has developed a model of opinion formation that applies directly to this problem. The idea is to derive a measure of predicted exposure for each individual based on variables that affecct the likelihood of actual exposure and the likelihood of forgetting. These factors enter interactively. The probability that a survey respondent reports exposure equals the probability that the respondent reports exposure and was exposed plus the probabilty that the respondent reports exposure and was not exposed. Under a variety of assumptions it is possible to estimate the different components of this model and adjust the survey data for misreporting (see Price and Zaller, 1993; Zaller, 1992, Chapter 7 for further details).

Here we assess the performance of this model directly using our experimental data. Are the assumptions behind the model justified? Does the model produce signficant improvements over estimates using reported exposure? While there we detect one potentially important misspecification in the Price and Zaller (1993) estimates, we find that, on the whole, the two-step model does quite well.

The two-step model assumes, first, that the probabilty that a respondent reports exposure and was not exposed equals zero. Second, assume that the process of exposure is statistically independent from the process of remembering. Under these assumptions, the model simplifies to $P(R = 1) = P(R = 1, X = 1) = P(M = 1, X = 1) = P(M = 1)P(X = 1)$. Third, Price and Zaller assume that specific variables affect the memory process and others affect exposure. Importantly, education is assumed to affect exposure but not memory.

Our experiments offer direct evidence about the first and third assumptions. The first assumption that no survey respondents report false memories is quite important. If a significant fraction does, then the model is not identifiable. Table 2 above reveals that the assumption is very reasonable. In our studies, only 4 percent of those not exposed to an experimental advertisement stated that they had seen one.

The fourth assumption raises a more general concern. What variables are good instruments for exposure and for memory? Obviously, the experiment has no direct information about the variables that effect exposure, because that is the treatment variable. The experiments do, however, provide direct information about what variables explain the ability to remember a message, independent of exposure.

The first column of Table 4 presents probit estimates of the probability that someone recalled a seeing an advertisement among those people who actually did. Under the independence assumption of Zaller's model, this is equivalent to estimating the probability that an individual has M equal to 1. As independent variables we have included Education, Age, Follow Politics, and 1988 Turnout, defined above. We have also included two media use variables: Read Paper, an indicator if the individual reads a daily newspaper, and Watch TV, a trichotomous variable indicating if the person watchs 1 or fewer hours, 1 to 3 hours, of 4 or more hours of television daily.

[Table 4]

Only education and age are strong predictors of memory. The better educated and the younger the individual is the more likely he or she is to remember a message. The implication is that education and age are good instruments–but not for exposure, as they are used in Price and Zaller. Media use, political participation, and interest in politics, on the other hand, are unrelated to memory. The insignificance of these factors in the recall equation suggests that they may make very good instruments for actual exposure in survey data. It is easy enough to remedy the error of using education as an instrument for exposure.

The larger question is how well does this model work in adjusting the survey data, and the answer is quite well. The two-step model is estimated using the following algorithm. (1) estimate the recall rate as a function of various control variables conditional on actually being exposed to the message. These estimates are presented in column (1) of Table 4 and were discussed earlier. Under the assumption that the ability to remember a message is indepedent of actual exposure, this regression estimates M^*. (2) generate predicted probabilities ($\hat{m}_i$) of remembering the message for all people who reported that they had not seen one. (3) construct the predicted exposure rate: $\hat{X} = 1$ if $R = 1$, and $\hat{X} = x(1 - \hat{m}_i)$ if $R = 0$, where x is the fraction actually exposed. In our experiments, x is known, but in survey data it must be estimated for each observation, just as we have estimated $\hat{m}_i$.[10]

The second column of Table 4 presents the revised regression estimates using predicted exposure instead of reported exposure. The technique certainly adjusted for most of the bias in the estimated coefficient. Adjusting the reported exposure question, we

[10] Of course, knowing x gives us a big headstart toward estimating this correction. To implement the two stage model we need only estimate the components of the memory equation.

now estimate the effect of advertising to be .069, which is notably close to the actual effect of .077. Unfortunately, the two-step estimate, at least as it is implemented here, produces a larger standard error (.043) compared to .018 for the estimate using actual advertising exposure. This may be an inefficiency in our algorithm, or it may be the cost of using the two-step method. Even with the rise in inefficiency, the two-step approach represents a considerable improvement over estimates using unadjusted measures of reported recall. In short, this method appears to be capable of correcting most of the bias contained in survey items that ascertain media exposure.

V. CONCLUSION

Survey researchers have long suspected that reporting errors in media exposure variables exist. Our experiments demonstrate that misreporting is sufficiently common as to make strong and significant media effects appear minimal. This is a discouraging conclusion to say the least. Several generations of survey research into the effects of political communications may be invalid simply because of poor question wording.

Though our main result is pessimistic about the capacity of surveys to measure the effects of advertisements, news stories, and the like, it is possible to improve survey methods for measuring the effects of political communications. One approach is to refine techniques for analyzing recall data. To this end, the two-step model of opinion formation and change developed by McGuire (1973) and Zaller (1992) holds considerable promise. Our experimental data suggest that this approach can be adapted as a model of the survey response and used to reduce much of the bias due to respondent's faulty memories.

A second approach is to ask better questions. Simple recall questions, we have found, work poorly. Perhaps some other question wordings–such as recognition of a message–would produce more accurate results. The accuracy of new question wordings can only be gauged if researchers can measure actual exposure to a political message, as well as the answer to the question. Experiments are ideally suited for that task. Survey researchers should make greater use of experiments as a means of testing and refining questions.

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Appendix

PROOF OF RESULT: The effect of actual exposure is $E[y|X = 1] - E[y|X = 0]$, and the estimated effect based on R is $E[y|R = 1] - E[y|R = 0]$. To derive the bias formula, consider each element of $E[y|R = 1]$ and $E[y|R = 0]$ separately.

$$
\begin{aligned}
E[y_i|R_i = 1] = &\sum_{j,k,l} E[y|R_i = 1, X_i = j, M_i = k, F_i = l]P(X_i = j, M_i = k, F_i = l|R_i = 1) \\
=&(\beta_0 + \beta_1 + \mathbf{z}_i'\beta_\mathbf{z} + E[\epsilon_i|M_i = 1, F_i = 1])P(X_i = 1, M_i = 1, F_i = 1|R_i = 1) \\
&+ (\beta_0 + \beta_1 + \mathbf{z}_i'\beta_\mathbf{z} + E[\epsilon_i|M_i = 1, F_i = 0])P(X_i = 1, M_i = 1, F_i = 0|R_i = 1) \\
&+ (\beta_0 + \beta_1 + \mathbf{z}_i'\beta_\mathbf{z} + E[\epsilon_i|M_i = 0, F_i = 1])P(X_i = 1, M_i = 0, F_i = 1|R_i = 1) \\
&+ (\beta_0 + \mathbf{z}_i'\beta_\mathbf{z} + E[\epsilon_i|M_i = 1, F_i = 1])P(X_i = 0, M_i = 1, F_i = 1|R_i = 1) \\
&+ (\beta_0 + \mathbf{z}_i'\beta_\mathbf{z} + E[\epsilon_i|M_i = 0, F_i = 1])P(X_i = 0, M_i = 0, F_i = 1|R_i = 1)
\end{aligned}
$$

Notice that not all possible probabilities are listed; the others equal zero. Since these probabilities sum to one we can collect terms and simplify the expression considerably. Define $f_{11} = P(X_i = 1, M_i = 1, F_i = 1|R_i = 1) + P(X_i = 1, M_i = 1, F_i = 0|R_i = 1) + P(X_i = 1, M_i = 0, F_i = 1|R_i = 1)$ and $f_{01} = P(X_i = 0, M_i = 1, F_i = 1|R_i = 1) + P(X_i = 0, M_i = 0, F_i = 1|R_i = 1)$, where $f_{11} + f_{01} = 1$. Then

$$E[y|R_i = 1] = \beta_0 + \mathbf{z}_i'\beta_\mathbf{z} + f_{11}\beta_2 + \lambda_1,$$

where λ_1 denotes the weighted average of the selection bias terms.

$$
\begin{aligned}
E[y_i|R_i = 0] = &\sum_{j,k} E[y|R_i = 0, X_i = j, M_i = k, F_i = l]P(X_i = j, M_i = k, F_i = l|R_i = 0) \\
=&(\beta_0 + \beta_1 + \mathbf{z}_i'\beta_\mathbf{z} + E[\epsilon_i|M_i = 0, F_i = 0])P(X_i = 1, M_i = 0, F_i = 0|R_i = 0) \\
&+ (\beta_0 + \mathbf{z}_i'\beta_\mathbf{z} + E[\epsilon_i|M_i = 1, F_i = 0])P(X_i = 0, M_i = 1, F_i = 0|R_i = 0) \\
&+ (\beta_0 + \mathbf{z}_i'\beta_\mathbf{z} + E[\epsilon_i|M_i = 0, F_i = 0])P(X_i = 0, M_i = 0, F_i = 0|R_i = 0)
\end{aligned}
$$

We can again simplify. Define $f_{10} = P(X_i = 1, M_i = 0, F_i = 0|R_i = 0)$ and $f_{00} = P(X_i = 0, M_i = 0, F_i = 0|R_i = 0) + P(X_i = 0, M_i = 1, F_i = 0|R_i = 0)$. Then

$$E[y|R_i = 0] = \beta_0 + \mathbf{z}_i'\beta_{\mathbf{z}} + f_{10}\beta_1 + \lambda_0,$$

where λ_0 denotes the weighted average of the selection bias terms. We can now calculate the expected value of the estimated effect when R is used instead of X.

$$E[y|R = 1] - E[y|R = 0] = (f_{11} - f_{10})\beta_1 + \lambda_1 - \lambda_0$$

Finally, the bias in the regression estimate is $E[a_1 - \beta_1] = (f_{11} - f_{10} - 1)\beta_1 + \lambda_1 - \lambda_0 = -(f_{01} + f_{10})\beta_1 + S$. QED.

Table 1. Indicators of Actual Exposure, Memory, False Recall, and Reported Exposure.

Actual Exposure (X)	Memory (M)	False Recall (F)	Reported Exposure (R)
1	1	1	1
1	1	0	1
1	0	1	1
1	0	0	0 (incorrect)
0	1	1	1 (incorrect)
0	1	0	0
0	0	1	1 (incorrect)
0	0	0	0

Table 2. Actual and Reported Exposure Rates in the Advertising Experiments.

	Actual Exposure		
Reported Exposure	Democratic Ad	Republican Ad	No Ad
Yes	.56	.55	.04
No	.44	.45	.96
Number of Cases	629	447	432

Table 3. Effects of Actual and Reported Advertising Exposure on Voting Preferences (standard errors in parentheses)

	Actual Exposure	Reported Exposure	Interaction
Constant	-.052 (.089)	-.065 (.088)	-.010 (.089)
Ad Exposure			
Actual	.078 (.018)	----	----
Reported	----	.023 (.024)	----
Actual & Report Yes	----	----	.079 (.025)
Actual & Report No	----	----	.077 (.028)
Control Variables			
1988 Vote	.324 (.024)	.327 (.024)	.321 (.025)
1988 Turnout	.116 (.035)	.111 (.035)	.110 (.035)
Follow	.019 (.022)	.017 (.022)	.006 (.020)
Independent	-.012 (.036)	-.009 (.037)	-.010 (.038)
Party Identification	.392 (.053)	.394 (.054)	.376 (.051)
Follow x PID	-.103 (.026)	-.105 (.026)	-.096 (.024)
Female	.116 (.032)	.117 (.032)	.118 (.033)
White	.029 (.034)	.026 (.034)	.027 (.034)
Age	.000 (.001)	.000 (.001)	.000 (.001)
Education	.030 (.017)	.028 (.017)	.028 (.017)
Sum of Sq Res.	492.65	498.53	492.64
R-squared	.32	.29	.32
N	1476	1476	1476

Table 4. Two-Stage Estimates of Advertising Effects on Voting Preferences (standard errors in parentheses)

	Prob(Recall | Exposure) [Probit]	Vote Preference [OLS]
Constant	.328 (.232)	-.079 (.110)
Predicted Exposure	----	.063 (.056)
1988 Vote	----	.320 (.029)
1988 Turnout	.190 (.091)	.119 (.041)
Follow	-.036 (.048)	.021 (.024)
Independent	-.044 (.093)	-.010 (.038)
Party Identification	----	.398 (.059)
Follow x PID	----	-.097 (.028)
Female	----	.112 (.038)
White	----	.027 (.034)
Age	-.022 (.003)	-.001 (.001)
Education	.182 (.044)	.031 (.020)
Watch TV	-.034 (.042)	----
Read Paper	.041 (.057)	----
Talk Politics	.026 (.043)	----
Log Likelihood	-658.74	
Sum of Sq Res.		494.80
Percent Correct	62.14	
R-squared		.31
N	1044	1476