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An Experimental Examination of the Content of Persuasion Letters on Nonresponse Rates and Survey Estimates in a Nonresponse Follow-Up Study

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Nonresponse follow-up studies are often conducted to understand whether respondents and nonrespondents differ on survey variables of interest in sample surveys. Methods used to recruit respondents often differ between nonresponse follow-up studies and main studies. One method is persuasion letters sent from study staff to nonrespondents that are tailored to the types of concerns raised by the respondent and recorded in paradata about the survey recruitment process. This study examined whether tailored persuasion letters yield higher response rates in nonresponse follow-up and whether respondents to a nonresponse follow-up differ depending on the content of the persuasion letter. Nonrespondents to the University of Michigan Dioxin Exposure Study, a survey of adults conducted in 2004 and 2005 in five counties in Michigan, were randomly assigned to either an appeal to help the community or tailored types of persuasion letters. No difference in response rates to the letter types was found, but meaningful differences in the survey variables appeared between nonrespondents responding to the follow-up and respondents to the main interview. These differences also occurred between the two groups receiving different types of letters. The community appeal letter appeared to address unvoiced concerns and brought a different sample of nonrespondents compared to the tailored letters.

Keywords: survey research, nonresponse, nonresponse bias, persuasion letters

Introduction

Response rates to interviewer-administered sample surveys have fallen over the last decades (Steeh 1981; de Leeuw and de Heer 2002). In reaction, follow-up surveys have increasingly been used to obtain information on whether nonrespondents differ from respondents on key variables of interest (Groves 2006; Brogger et al. 2003). In these studies, a different recruitment protocol – such as additional incentives or shortened questionnaires – is used to recruit those who did not participate initially. These studies yield information on the difference between those who participated in the survey and those who did not participate.

Many survey organizations send *persuasion letters* to selected reluctant sample units in an attempt to convince them to participate (e.g., the National Survey of Drug Use and Health (Bowman et al. 2004), the American National Election Studies (NES 2004), the National Immunization Survey (Smith et al. 2005)). In interviewer-administered surveys, these letters, sometimes called "refusal conversion letters", can be tailored to concerns voiced by an informant as recorded in call records or paradata (Couper 1998) about the recruitment process. Study staff and interviewers make decisions about the content of letters based on broad guidelines established by organizational policy and approved by institutional review. Some survey organizations do not use any particular theoretical framework or body of empirical evidence to determine content. Others draw on theoretical paradigms for survey participation, such as social exchange theory (Dillman, Smyth and Christian 2009; Goyder 1987), compliance heuristics (Groves, Cialdini and Couper 1992), or leverage-saliency theory (Groves, Singer and Corning 2000).

There is little published experimental research about the effectiveness of particular appeals in persuasion letters. Couper and Groves (1995) showed that cooperation rates for those receiving persuasion letters during data collection are significantly lower than rates for those not receiving them, but this finding was because those with lower response propensities receive persuasion letters. In fact, to our knowledge, the effectiveness of different content of persuasion letters for increasing response rates has not been experimentally evaluated. In addition, it is not known whether particular features of a survey protocol affect the set of "nonrespondents" that are recruited into the nonresponse follow-up.

Persuasion letters may have different effects than ad-

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vance letters. *Advance letters* are generally effective at increasing initial response rates in general population surveys (de Leeuw et al. 2007). Although advance letters are not tailored to particular sample units, themes in the text of the letter (e.g., reciprocation) increase response rates (de Leeuw et al. 2007). Persuasion letters are sent to a subset of nonrandomly selected households that resisted prior survey recruitment efforts. Reluctant survey respondents differ from the not reluctant on willingness to participate and potentially on substantively important characteristics.

Many theories for nonresponse suggest that persuasion letters may have an effect on survey participation among reluctant sample members. Leverage-saliency theory posits that emphasizing survey design features individual sampled persons value and deemphasizing survey design features sampled persons do not value increases survey participation rates. Persuasion letters tailored to concerns voiced by the sample unit can thus be viewed as an attempt to deemphasize an individual's negative concerns and emphasize positive ones (Groves and Couper 1998; Groves and McGonagle 2001).

On the other hand, for persons previously resistant to arguments specific to their personally-voiced concerns, or whose actual concerns differ from those given to the interviewer, other theoretical frameworks may better explain how to reduce resistance to a survey request. For example, the scarcity principle (Groves, Cialdini and Couper 1992) suggests a letter would be more effective that emphasizes how individual participation cannot be replaced by another person. Alternatively, social exchange theory (Dillman 1978; Goyder 1987; Dillman, Smyth and Christian 2009) suggests that letters emphasizing that the community will benefit from their participation will yield higher participation rates. These alternative theories suggest that non-tailored appeals, such as an appeal to help the community or emphasizing the sampled person's unique contribution, may be more effective than addressing individual concerns.

The empirical evidence is limited about which theoretical framework operates with respect to persuasion letters. There is no published research on whether tailored persuasion letters accomplish the goal of increased response rates relative to letters emphasizing the social benefits of participating.

There is also no theoretical expectation or empirical evidence on the effect of persuasion letters on nonresponse bias. If concerns addressed in a letter are related to constructs measured in the questionnaire, such as trust in a sponsor, time availability, or community involvement, one might expect that persuasion letters with different content would draw responses from sampled persons with different characteristics. Such a 'selection' process could result in increased or similar response rates but different values for estimates. Findings on advance letters indicate that nonresponse bias is no different between those receiving advance letters and those who do not (Groves and Peytcheva 2008). But whether persuasion letter content affects the characteristics of respondents to nonresponse follow-ups and changes survey estimates has not been examined.

The study reported here tailored persuasion letter content in a face-to-face survey conducted in five counties in Michigan. Two types of letters, a general appeal to help the community and a set of tailored persuasion letters, were randomly assigned to sample units that had not cooperated in a main survey which had repeated visits during which statements made by respondents during doorstep conversations were recorded in paradata about the survey effort. Study staff then sent alternative versions of a persuasion letter prior to beginning follow up contact attempts in a nonresponse follow-up.

We address three research questions:

1. In a nonresponse follow-up, do tailored persuasion letters yield higher cooperation rates than a community appeal letter?
2. Are community appeal letter respondents different from tailored letter respondents on important survey characteristics?
3. How do the respondents to each condition compare to the respondents in the main study?

Study Design

The University of Michigan Dioxin Exposure Study (UMDES) examined the relationship between levels of dioxins (a family of toxic chemicals) in the environment and the levels in people's bodies. Study staff asked adults in five counties in Michigan who had lived in their current residence for at least five consecutive years to complete an hour long interview, to provide a blood sample, to permit a survey team to collect household dust, and to permit another survey team to collect soil samples from the property surrounding the housing unit (see Garabrant et al. 2009a). The main study achieved a 74.3% response rate (AAPOR RR3) to the full interview and blood, dust, and soil sample protocol. The study design, field and laboratory methods, and findings are reported elsewhere (Hedgeman et al. 2009; Garabrant et al. 2009b; Olson et al. 2006; Lepkowski et al. 2006; Franzblau et al. 2006).

Approximately two months following main data collection, study staff conducted a nonresponse follow-up study (NRFU) to investigate potential nonresponse bias. Staff recontacted all main survey nonresponding households, except those who gave firm refusals or indicated a severe family illness (150 households). The NRFU sample consisted of 417 sample households.[1]

Several design features differed between the main and NRFU studies. The main study included a 60 minute face-to-face interview, an 80 mL blood draw, a household dust sample collection, and multiple soil sample collections around the housing unit. The questionnaire covered recreational and occupational activities, food consumption, residential history, health status, and demographic characteristics, using

[1] In addition, 10 housing units assigned a final "no eligible respondent" code were included in the NRFU to rescreen for eligibility. One of these households completed an interview, but is not included in the analyses.

an event history calendar to facilitate retrospective recall. The main study protocol included unlimited potential contacts with the sample units and tailored persuasion letters for non-cooperating households. Respondents received a $60 interview and blood sample incentive paid prior to starting the interview, and separate $20 payments for the household dust and the soil sample collections.

The NRFU study interview was face-to-face or by telephone, and used a 15 minute version of the main study interview without event history questions, largely identical to the main interview questions. Study staff did not request blood, dust, or soil samples in the NRFU. The NRFU had a two contact limit and did not use a persuasion letter after the initial mailing.

Persuasion letter development started with staff review of main study persuasion letters and focus groups with survey methodology graduate students. Focus group findings led to a shortening of main study persuasion letters, use of larger font sizes, and changes to the text. The NRFU study design team and survey managers subsequently drafted seven tailored persuasion letters and a more general community appeal letter.[2]

The NRFU design team reviewed paradata in the form of contact observations recorded by the interviewer at each contact and interviewer call notes, and then determined a reason for nonresponse among the 417 NRFU households. The design team review identified five main themes: 1) 'Not Interested,' 2) 'Too Busy,' 3) concerned about 'Confidentiality,' 4) had 'No Trust in Surveys,' or 5) survey teams had 'No Contact' with the study household. The first author assigned each of the NRFU households to one theme for a tailored persuasion letter. Almost all households had only one concern recorded; those with more than one concern were assigned the most specific (e.g., no trust, confidentiality, or too busy). A total of 174 households were assigned to the "not interested" letter group, 120 households to the "too busy," and 39 households to either "confidentiality" or "not trusting surveys" letter group. The 63 no contact NRFU households were assigned to the "no contact" letter. A total of 21 contacted households had no contact observations recorded and had limited information available in the interviewer call notes. These households were assigned to the "community appeal" letter.

The first sentence and the last two paragraphs were identical in all persuasion letters. Only the end of the first paragraph differed across letters. For example, the end of the first paragraph in the community appeal letter stated:

> "I want to assure you that your participation is important. Each person in our study is unique. You have been carefully selected to represent a part of your community. Without talking to you, we will not be able to collect data needed to inform this important environmental contamination issue for Midland, Saginaw, and Bay Counties. Your community will benefit if you give us a few minutes of your time."

In contrast, the end of the first paragraph of the 'Too Busy' letter stated:

> "Your contribution to this project is extremely important. Busy schedules may make it difficult for you to fit in an interview with us. But without interviews from busy people such as yourself, we would end up with findings biased towards people who have different schedules. If that happened, our sample would not represent an important portion of the Midland, Saginaw and Bay County population."

Each community appeal or tailored letter contained a $20 bill, indicated that an interviewer would contact the household for a shortened 15 minute interview, and stated that no blood, dust or soil samples were being requested. The letters also stated that the respondent would receive $40 after completion of the 15 minute interview.

NRFU staff randomly assigned one half of each letter group to receive a community appeal letter (n=206) or a tailored letter (n=211). Although the interviewers were aware of the presence of an experiment, they did not know the content of the letter sent to each household. Checks of the randomization showed there was no statistically significant difference in the distribution of the letter themes across the experimental conditions (Chi-square = 2.94, 4 d.f., $p = 0.55$) and the distribution of household sampling weights (which varied across geographic areas) were not statistically different ($p = 0.80$). Subsequent checking revealed no difference between the community appeal and tailored nonrespondents in reports of receiving the letter (community 87.3 percent, tailored 84.9 percent, $p = 0.77$) or in reading the letter (community 70.1 percent, tailored 69.9 percent, $p = 0.94$). Thus, the treatment tailored letter and the control community appeal letter groups appear to be equivalent.

Four comparisons examine means and proportions across 84 NRFU questions: 1) main sample versus NRFU sample (community appeal and tailored letter combined), 2) community appeal versus tailored letter samples, 3) community appeal versus main sample, and 4) tailored versus main sample. Due to small sample sizes, data from tailored letter respondents are combined in one group for comparisons. Given small numbers in groups, and losses in precision due to a complex survey design, we use a $p < 0.10$ significance level to indicate a finding of statistical importance. One would expect eight or nine questions to show statistically significant differences between any two groups by chance alone. With the four comparisons each across 84 questions, one could expect 34 comparisons to be statistically significant. All t-test comparisons were conducted using SAS-callable IVEWare (Raghunathan et al., 2001) to reflect the complex sample design (weights, clustering, and stratification) and multiply imputed data sets.

[2] Two of the tailored letters relating to eligibility concerns (not living in the affected area) and sponsorship concerns were not used because no nonresponding households had expressed these concerns.

Results

There was no response rate difference between the community appeal and tailored letter groups overall (see Table 1), with the two rates close to the overall NRFU RR1 rate of 47.6% (AAPOR 2008). There was no response rate difference between community appeal and tailored letter groups within reason for nonresponse groups either.

Refusal rates (AAPOR REF1 definition) overall did not differ between the community appeal and tailored letter. There was variation in refusal rates within letter type across reason for nonresponse groups ($p = 0.004$ within the community appeal letters, and $p = 0.026$ within the tailored letters). The lowest refusal rates for both treatment groups (community appeal and tailored) were among those who received the no contact letter, but there is no clear explanation for this finding.

Table 2 presents counts of the number of statistically significant differences in means between main and NRFU respondents. In the main vs. NRFU comparison, 18 of the 84 variables had statistically significant differences, including race (the demographic variable), body mass index and weight (two of the health characteristics), living with someone who worked at Dow Chemical (an occupational variable), and eating deer liver during the last five years (a wild game consumption variable). Main study respondents were more likely to have the characteristic or to have eaten more of a food than NRFU respondents for all 18 variables.

Although there was no difference in the response rates between the community appeal and tailored experimental groups, the characteristics of the NRFU respondents did differ between the groups. Nine of 84 questions showed significant NRFU community appeal vs. NRFU tailored letter differences, including sex, height, weight, and smoking status (four of five demographic and health characteristics with differences) and eating wild game birds, pan-fish, and walleye or perch during the last five years (wild game and fish consumption differences). Considering all 84 comparisons, and not just those that were statistically significant, nearly 75% of the comparisons had means or proportions for the NRFU community appeal letter group that were smaller than those for the NRFU tailored letter respondents.

Many more differences occurred for the NRFU community appeal to main survey comparison (Table 2). Thirty-three of the 84 comparisons were statistically significant, and in every instance of the significant differences, the main survey respondents had higher means or proportions than the NRFU community appeal respondents. This pattern held even in the differences that were not statistically different from zero. Two-thirds of the significant differences are in the occupational history and wild game and fish consumption sections. Main respondents were more likely to be exposed to the chemicals in question, which occur in industrial settings and in wild game and fish, than the NRFU community appeal letter respondents.

Table 2 also shows in the last column that there were few significant differences between the NRFU tailored letter and the main respondents. The fifteen comparisons that were significantly different barely exceeded what would be expected by chance among 84 independent tests. Where significant, the direction of the differences between tailored letter and main respondents tended to be consistent.

Discussion

There four main study findings are:

1. NRFU persons who received a letter with a community appeal had response rates that were not statistically different from those who received a letter tailored to their concerns.
2. Although there was no difference in response rates, the community appeal letter respondents differed on important characteristics from the tailored letter respondents.
3. NRFU respondents tended to differ from main survey respondents by being less likely to engage in behaviors that would expose them to dioxins.
4. The difference between main study respondents and the NRFU respondents was largely due to the response to the community appeal letter.

These findings suggest that the amount of time and money spent by survey organizations in tailoring persuasion letters to householders' individual concerns does not yield higher response rates or recruit different sampled persons. Changing persuasion efforts to ones that emphasize how survey findings can help the common good was more effective for recruiting different types of resistant respondents later in a field period. Those who are less affected by or interested in a survey topic were the least likely to participate. But they were particularly persuaded by appeals to help their community, consistent with predictions from social exchange theory.

These findings indicate that estimates from the main respondents are biased due to nonresponse. The NRFU community appeal letter brought in respondents who were different on many key characteristics. Leverage-saliency theory predictions that tailored letters would raise participation relative to a community appeal letter do not hold. Further, using the tailored letter results alone would indicate that there was low risk of nonresponse bias in the main study.

This experiment has produced an example of an unusual finding: even when response rates are the same between experimental groups overall there can be differences in estimates on important survey variables. That is, two surveys from the sample population, on the same topic, and with the same response rate, but different survey nonresponse protocols, do not have to have the same estimates, demographically, or otherwise. The finding reinforces the lesson that nonresponse rates are not measures of nonresponse bias (e.g., Groves 2006; Groves and Peytcheva 2008), and that the effectiveness of different protocols to recruit nonrespondents varies for sampled persons with different characteristics.

As is the case with many field evaluations, this experiment was embedded in an effort to obtain as high a response as possible to the NRFU. The efforts included a $20 incentive in the letter, reduced questionnaire length, and elimination of requests for biomarker and other physical samples. Ad-

Table 1: Response Rates and Refusal Rates by Treatment Group (Community Appeal vs. Tailored Letter) Overall and by Reason for Nonresponse, University of Michigan Dioxin Exposure Study, 2004-2005.

	n		AAPOR Response Rate 1 (%)		AAPOR Refusal Rate 1 (%)	
	Community	Tailored	Community	Tailored	Community	Tailored
Overall	206	211	47.9	47.3	40.0	43.0
Reason for Nonresponse						
Not Interested	85	89	46.8	48.2	51.9	51.8
Too Busy	64	56	55.7	54.9	29.5	33.3
Confidentiality/ No Trust	19	20	44.4	50.0	55.6	50.0
No Contact	26	37	36.4	29.2	13.6	20.8
Reason not determined[a]	12	9	40.0	37.5	40.0	62.5

[a]Assigned to community appeal letter group.

Table 2: Number of statistically significant comparisons ($p < 0.10$) by section of the questionnaire for four comparisons, UMDES 2004-2005.

	# Questions	Main vs. NRFU	Tailored vs. Community	Community vs. Main	Tailored vs. Main
Demographics	6	1	1	0	4
General Health characteristics	13	3	4	4	2
Residential History	3	0	0	0	0
Property use	4	1	0	2	1
Occupational questions	19	4	0	7	2
Veteran status	4	2	0	3	0
Recreational hunting and fishing	5	0	0	0	2
Wild game consumption	12	2	2	8	0
Fish consumption	18	5	2	9	4
Total	84	18	9	33	15
% Significant comparisons		21%	11%	39%	18%

ditionally, all of the participants had received some sort of persuasion effort prior to the nonresponse follow-up. These design features – and especially the prepaid incentive (Singer 2002) – have been shown to have significant effects on survey participation. Of course, how the persuasion letters, tailored or community appeal, would have behaved in absence of these design features is unknown. Yet all of the letters – tailored and community appeal contained identical information about these protocol changes. Any refusal conversion effort embedded in the context of an existing study should replicate the experiment in future research under other survey protocols.

This experiment suggests that had the entire NRFU sample received a community appeal letter, very different conclusions about nonresponse bias would have been reached. Those who responded to the tailored letter were no different than the main study respondents, but those who responded to the community letter were. Which group is most 'representative' of the total pool of nonrespondents, however, is unknown. With the limited frame data available for the NRFU sample, we have no evidence about which approach reduces nonresponse bias the most.

These findings suggest three areas for future research. First, using paradata collected by the interviewers on the doorstep to tailor letters was not an effective approach for gaining cooperation late in the field period or obtaining sampled persons who differed from the main respondent pool in this study. Thus, more research is needed on which paradata on the recruitment process recorded by interviewers can or should be used to change field efforts. Although it is not known exactly what messages were delivered by the interviewers on the doorstep, interviewers were trained to deliver tailored responses to the concerns voiced by householders on the doorstep. These results suggest that tailored persuasion efforts may lose effectiveness with repeated applications. As such, different paradata may need to be collected for effectively guiding end of the field period implementation decisions.

Although our letters were written with the intention to address householder concerns, we do not know whether they effectively did so. A second area for future research would use cognitive interviews or focus groups with potential sampled householders to examine the content of persuasion letters. These qualitative methods could yield insights that lead to improvements in persuasion letters, tailored or not.

Finally, rarely are differences in sample composition examined between experimental groups that have equivalent response rates. Yet nonresponse rates are not a good indication of nonresponse bias (Groves and Peytcheva 2008). Reexamination of studies with no differences in response rates be-

tween experimental conditions may yield insights into differential attractiveness of particular design features to subgroups.

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