Understanding Nonresponse Mechanisms in Telephone Surveys

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Introduction

Response rates to social and behavioral surveys have been declining for several decades (Groves and Couper, 1998), increasing the likelihood that differences between respondents and nonrespondents may be sufficient to bias survey estimates (Caetano, 2001). There are several methodologies that have been employed to investigate the potential effects of nonresponse on survey estimates. These include (1) the use of follow-up surveys that attempt to conduct follow-up interviews with persons not responding to the initial survey, (2) comparisons of early vs. late responders to survey requests (under the assumption that late responders are more similar to nonresponders) (3) panel attrition studies that compare the baseline characteristics of respondents who are and are not lost to follow-up waves of interviewing, and (4) records match analyses that compare the personal and/or community characteristics of households that do and do not respond to survey requests. In this paper, we report a unique application of a records match methodology to estimate potential nonresponse bias in an RDD telephone survey. The study in question was concerned with the estimation of statewide treatment needs for alcohol and/or drug abuse. To place this study within an appropriate context, we review below previous attempts to evaluate nonresponse bias in epidemiologic studies concerned with substance use.

Follow-up surveys on nonrespondents have produced mixed findings, with some indicating less substance use among nonrespondents (Hill et al., 1997) and some indicating no relationship between substance use and survey nonresponse (Gmel, 2000). This research can be interpreted as evidence that persons with fewer substance use experiences, for whom the topic of the survey may be of less interest or saliency, may be less likely or willing to participate. There are several important limitations of follow-up surveys, however, that may call into question the quality of these findings and account for their empirical discrepancies. For example, follow-up surveys typically have low response rates and it is thus unclear whether follow-up survey respondents are actually representative of nonresponders. Available evidence from panel attrition studies (reviewed below) strongly suggests that substance users may in general be less available to be interviewed. If substance users are less available for participation in both surveys and follow-ups, the studies reviewed here may be vulnerable to correlation bias limitations. That is, substance users may be more likely to be excluded from both surveys and follow-up surveys, thus calling into question estimates obtained from each. In addition, differences in the mode of data collection between main and follow-up interviews are common (Gmel, 2000; Hill et al., 1997) and this may confound nonresponse with substance use measurement quality.

Other nonresponse research has also been conducted on the assumption that *difficult-to-reach respondents* are most similar to nonrespondents. Two studies that have examined these difficult-to-reach respondents have found them to consume more alcohol, compared to less difficult-to-reach respondents (Crawford, 1987; Wilson, 1981). A third study, reported by Cottler et al. (1987), found similar evidence. They reported that persons with current alcohol disorders required the greatest number of contact attempts, on average, in the St. Louis Epidemiologic Catchment Area study. Two other studies have found no differences in alcohol consumption among early vs. late respondents (Etter & Perneger, 1997; Voigt et al., 2003). These studies, unfortunately, are also vulnerable to the criticism that survey response rates are imperfect and that nonrespondents may be different from both early and late respondents across key variables of interest.

Panel attrition studies are less vulnerable to these limitations, as common baseline data are available for both respondents and nonrespondents to follow-up interview requests. In contrast to the findings of follow-up surveys reported above, most available panel attrition studies find evidence that substance use is positively associated with nonresponse to follow-up interviews (Bucholz et al., 1996; Caetano et al., 2003; Wild et al., 2001). One other study, though, found no differences in substance use behavior of those who did and did not complete a follow-up survey (Iversen and Klausen, 1986). An important limitation of these studies, however, is that they mostly fail to differentiate between respondents who refuse and those who cannot be contacted, making it difficult to determine if substance use is associated with availability or willingness to participate. One exception is a study

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reported by Eaton et al. (1992), which found drug and alcohol abuse/dependence to be associated with failure to contact for follow-up interview, but not refusal to participate once contacted.

Finally, *records match analyses* are another approach to investigating survey nonresponse (Groves and Couper, 1998). There are two examples available of applications of this technique to assessment of nonresponse bias in substance use research. Needle et al. (1985) examined the service utilization records of a health maintenance organization and found no differences between households that did and did not respond to a survey in terms of alcohol and drug treatment service utilization. More recently, Gfroerer and colleagues (1997) analyzed the nonresponse patterns of 5,030 households sampled to participate in the 1990 National Household Survey on Drug Abuse (NHSDA). Household and community level information from the 1990 Decennial Census for each sampled household was appended to the data set. Overall, they found that males and Latinos were less likely to participate in the NHSDA. At the household level, owner-occupied households, those with greater value, and single person households consisting of persons aged 65 and older were less likely to respond. At the block level, urbanicity was associated with lower participation. The degree to which these measures were associated with the substance use indictors being estimated by the survey, however, were not reported. Nonetheless, the key advantage of the records match approach is that information regarding all sampled households are available for analysis, providing researchers with the opportunity to make better generalizations from these data. We note, however, that this approach has not been employed to evaluate nonresponse bias in telephone surveys. In this paper, we present an effort to employ this methodology to investigate nonresponse bias in a statewide telephone survey concerned with estimated substance abuse treatment needs in Illinois.

Methods

Data for this study come from the 2003 Illinois Household Treatment Needs Assessment Survey (Johnson, Cho, Lerner et al., 2004). The University of Illinois at Chicago Survey Research Laboratory completed a total of 4,155 random telephone interviews between January 15 and August 15, 2003. The sample was stratified in order to produce reliable estimates for each of eight geographic areas. Wherever possible, sampled households were sent an advance letter introducing the survey.

Within each sampled household, one person aged 16 or older was randomly selected to be interviewed. Where persons aged 16-17 were selected, parental consent was obtained in advance. Interviews averaged 29.8 minutes in length (standard deviation = 12.7) and were primarily concerned with issues of substance use involvement, severity and treatment needs. Using AAPOR (2000) formula RR 3, the survey's response rate was estimated to be 32.7 percent. The parental approval rate for participation by their underage children was 68 percent.

To examine the potential effects of nonresponse error in this study, indicators of survey response, refusal, and noncontact were first constructed using the final sample dispositions assigned to each of the 20,774 telephone numbers included in the survey's sample frame. Several nonresponse measures were constructed from these data. Two general indicators of response were used to contrast those households completing telephone interviews with (1) all households identified as eligible but that did not participate, regardless of reason, and (2) all households identified as eligible plus all unscreened telephone numbers that likely included some eligible households. Additional measures were developed to contrast (1) households that refused vs. those that participated, and (2) households in which the randomly selected respondent could and could not be directly contacted.

The specific disposition codes employed to develop each of these measures are as follows. For the first general nonresponse indicator, households completing or partially completing interviews were coded 1, and households not completing interviews were coded 0. The second general nonresponse indicator was identical to the first with the exception of also including among the non-responding cases those telephone numbers coded as having never been answered. For the refusal indicator, households refused to participate were classified as refusals, and households completed all or part of the interview, or households contacted but respondents were not available were coded non-refusals. The noncontact indicator identifies households where the selected respondent was never directly reached. We note that households not speaking English or Spanish were excluded from these analyses.

Data from several other sources were next merged with this sample frame file for analysis. First, survey responses for key outcome measures of interest were appended to those phone numbers that yielded completed interviews. Three dependent variables of interest are examined: current alcohol use treatment needs, current drug use treatment needs, and current treatment needs for either alcohol or drug use. After applying stratification and poststratification weights, the estimated values of these three variables were 15.5% (SE = 0.9), 3.1% (SE = 0.4) and 16.4% (SE = 1.0), respectively. For details on the construction and weighting of these measures, see Johnson, Cho, Lerner & Pickup (2004).

Second, each telephone exchange/area code combination included in the survey's sample frame was linked with the predominant area code that they served. Data were available for a sample of 779 zip codes in Illinois. Next, a set of approximately 70 measures from the 2000 Census [http://factfinder. census.gov/home/saff/main.html?_lang=en] were aggregated at the zip code level and merged with the sample frame information. The Census variables included a variety of housing, income, occupation and sociodemographic dimensions that collectively represented broad social conditions within each local geographic area.

Using this merged data file, an exploratory ecological factor analysis was conducted to determine if a smaller set of underlying dimensions could be identified to represent these Census data. Ten orthogonal (i.e., uncorrelated) factors identified from these data, and factor scores for each zip code were constructed using these 10 factors. Hierarchical linear modeling (HLM) was next used to examine potential associations between each of the ten ecological factors and household nonresponse, survey refusal, and survey noncontact (Stephen et al., 2000). Since the outcomes are all binary (coded 0 for no and 1 for yes), the first-level equations for these outcomes use a logit link function (Hedeker & Gibbons, 1994) to estimate the log-odds of occurring 'yes'. State geographic region was included as a covariate in each of these analyses.

Findings

In Table 1, two equations are presented, one examining the zip-code level predictors of survey nonresponse *excluding* phone numbers with unknown eligibility (i.e., the 'ring, no answer' numbers; equation 1 in Table 1), and the second examining the same predictors of survey nonresponse *including* phone numbers with unknown eligibility (equation 2). Four factors were found to be associated with likelihood of survey response (excluding unknown eligibility cases): community income, community poverty, average housing size, and the number of working families in the community. As the first column of Table 1 indicates, survey response was lower among households in areas with higher incomes, with greater levels of poverty, with more crowded housing conditions and with greater numbers of working families. The second equation examined in Table 1, which includes unknown eligibility cases, also identified households in high-income areas and areas with more crowded housing conditions as having lower survey response rates. Households in areas with more white-collar employees were also less likely to participate in the survey. We also note that the average log-odds (estimated probability) of outcomes (=1) did not vary significantly between zip-code areas, as indicated at the bottom part of this and subsequent tables.

Analyses of ecological predictors of refusals and noncontacts are presented in Table 1. The third equation presented in this table examines the predictors of survey refusal among households determined to be eligible. None of the ten ecological measures examined were found to be associated with likelihood of refusing to participate. Equation 4 in Table 1 examines the predictors of survey noncontact. Five of the factors examined were found to be independently associated with likelihood of noncontact. Specifically, households in areas with higher incomes, more immigrants, more crowded housing conditions, those in more urban areas and those with more white collar employees were all less likely to have been successfully contacted as part of the survey.

A series of HLM models were next estimated to examine potential associations between the ecological measures and the alcohol and other drug use treatment needs of survey respondents, controlling for geographic region of the State. Table 2 presents these findings. The first equation in Table 2 indicates that respondents living in higher income areas and in areas with more new housing units were more likely to report alcohol treatment needs. In contrast, persons living in high poverty areas were less likely to report alcohol treatment needs. The second equation in Table 2 indicates that only one ecological measure was associated with drug use treatment needs. Respondents in more urban areas and in communities with higher proportions of government workers were more likely to report drug use treatment needs. Finally, equation 3 indicates that the likelihood of reporting treatment needs for either alcohol or drug use was greater among respondents living in higher income areas, lower among respondents living in higher poverty areas, and higher in areas containing more new housing units.

The findings reported in Tables 1 and 2 suggest that two community measures are associated with both likelihood of survey nonresponse and respondent treatment needs for alcohol and either alcohol or drug use (although these latter associations were only borderline significant, $p < .06$). The direction of these relationships indicate that persons in higher income areas are less likely to be included in the survey and more likely to have treatment needs. In addition, persons in high poverty areas also appear less likely to have responded to the survey but to be less likely to have alcohol and alcohol or drug treatment needs. The findings in regard to community income levels suggest that survey treatment need estimates may be underestimated as a consequence of nonresponse bias. With regards to community poverty levels, an opposite conclusion may be drawn: because households in higher poverty communities are less likely to respond, and because they also have lower rates of alcohol and alcohol/drug treatment needs, treatment need estimates may be somewhat overestimated.

There are a number of strong and well known sociodemographic correlates of substance use that were included in the survey's poststratification weights, however, that have yet to be examined, including gender, age and race/ethnicity. We next re-estimated the models presented in Table 2 to determine if community income level and poverty level remain associated with treatment needs once these measures are introduced as controls. Indeed, findings presented in Table 3 indicate that community income level is not predictive of alcohol and alcohol or other drug treatment need once these sociodemographic characteristics are introduced into the models. For both of these treatment need measures, gender, age and race/ethnicity were each found to be significantly associated with treatment need.

Discussion

This study identifies several ecological variables that are predictive of survey nonresponse in a statewide RDD telephone survey. Because nonresponse is greater in both high income and poverty areas, the relationship between survey nonresponse and community SES status would appear to be curvilinear. In addition, nonresponse is also greater in areas featuring crowded housing conditions. Non-contact, an element of overall nonresponse, was found to be greater in areas with higher immigrant populations, more crowded housing conditions, and more urban environments. These latter findings are consistent with prior research suggesting reduced contactability of respondents in densely populated urban environments (Groves and Couper, 1998). They also suggest that this phenomena appears to be applicable to telephone surveys in addition to those conducted face-to-face, which must contend with physical barriers to respondent contact such as restricted access apartment buildings. The lower contact rates found in areas that can be characterized as urban, however, should serve as a warning to researchers investigating attitudes and/or behaviors that are known to vary across urban vs. rural environments.

Additional analyses found that some of these ecological measures were also predictive of substance use treatment needs, community income and poverty levels in particular. These ecological indicators, however, did not remain predictive of treatment need once demographic measures are adjusted for. Each of these demographic variables, including gender, age and race/ethnicity, are well known correlates of substance use behaviors (Office of Applied Studies 2000). Because each of these measures were also included in the study's poststratification weights, the ecological variables identified as correlates of both nonresponse behavior and substance use treatment needs were adjusted for in this particular study. Consequently, in this case study, we are able to conclude that the community level nonresponse processes identified were likely compensated for by the studies poststratification weights and that nonresponse bias is hopefully minimalized. Of course, other features of this study, the sensitive nature of many of the substance use questions in particular, clearly suggest other sources of survey error, measurement problems in particular, that are likely to influence survey estimates.

In addition, community level dimensions other than those examined here may also be associated with survey nonresponse and respondent availability, as well as with key survey measures being estimated. Particular features of this survey covered in the introduction (i.e., sponsorship, subject topic, interview length) may also potentially be associated with nonresponse and/or refusals. Replication of this methodology in other studies should thus be encouraged to determine the degree to which the findings reported here are generalizable. Our initial speculation is that non-contact correlates are more likely to be applicable to surveys of varying topics conducted in varying environments, and that refusal rates may be more dependent on specific qualities of the survey being conducted.

Regarding this latter point, it is interesting to note that none of the community level measures examined here were predictive of refusal rates. In fact, these findings, when viewed in isolation, suggest that refusals may be randomly distributed across geographic areas (at least in this study). These findings thus tend to reinforce available evidence obtained via other methodologies that have reported few differences in substantive measures across surveys with varying response rates (Curtin et al., 2000; Keeter et al., 2000; Merkle and Edelman, 2002). Fortunately, we believe that our methodology can be replicated in many other RDD studies at little additional cost.

Based on these findings, we conclude that (1) RDD telephone survey nonresponse can be predicted from zip code level ecological measures, and (2) the independent effects of these forms of nonresponse bias on key survey measures can be evaluated.

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Table 1. Hierarchical model of the regional effects on the likelihood of survey response

	Survey response 1 (1=yes)		Survey response 2 (1=yes)		Refusals (1=yes)		Non-contacts (1=yes)	
Level 1 N	10997		13102		10753		10997	
Level 2 N	779		796		771		779	
Fixed Effects	**Coefficient**	**(S.E.)**	**Coefficient**	**(S.E.)**	**Coefficient**	**(S.E.)**	**Coefficient**	**(S.E.)**
Intercept	-0.441***	(0.059)	-0.640***	(0.056)	-0.319***	(0.052)	-0.413***	(0.061)
Zip-code level Factors								
Factor 1:High Income Areas	-0.059*	(0.024)	-0.095***	(0.025)	0.022	(0.023)	0.077**	(0.023)
Factor 2: High Poverty Areas	-0.047**	(0.016)	-0.028	(0.017)	0.023	(0.014)	0.014	(0.017)
Factor 3: Immigrant Populations	-0.026	(0.020)	-0.039	(0.020)	-0.024	(0.019)	0.063**	(0.019)
Factor 4: Smaller Housing Units	-0.042*	(0.018)	-0.076**	(0.021)	-0.003	(0.016)	0.065**	(0.019)
Factor 5: Urbanicity	-0.032	(0.019)	-0.018	(0.018)	0.003	(0.018)	0.039*	(0.018)
Factor 6: Working Families	0.083*	(0.042)	0.075	(0.042)	-0.077	(0.041)	-0.066	(0.042)
Factor 7:Poor Housing	0.086	(0.064)	0.060	(0.065)	-0.089	(0.066)	-0.127	(0.066)
Factor 8: Government Employees	0.011	(0.028)	0.016	(0.028)	0.005	(0.026)	-0.005	(0.030)
Factor 9: White Collar Employees	-0.066	(0.045)	-0.110*	(0.043)	0.020	(0.044)	0.091*	(0.046)
Factor 10: New Housing	-0.023	(0.017)	-0.032	(0.019)	-0.014	(0.016)	0.031	(0.019)
Individual Level Covariates								
Regions (Ref: Southern Illinois)								
Chicago	-0.126	(0.124)	-0.173	(0.120)	-0.210	(0.117)	0.304*	(0.117)
Suburban Cook	-0.125	(0.099)	-0.188	(0.101)	-0.136	(0.090)	0.196	(0.112)
Northwest Illinois	0.201*	(0.101)	0.160	(0.103)	-0.131	(0.081)	-0.095	(0.106)
South Collar Counties	-0.144	(0.089)	-0.151	(0.086)	-0.031	(0.084)	0.228*	(0.090)
North Collar Counties	-0.103	(0.094)	-0.202*	(0.095)	-0.092	(0.086)	0.153	(0.100)
West Central Illinois	0.269**	(0.086)	0.218**	(0.080)	-0.272**	(0.083)	-0.018	(0.092)
East Central Illinois	0.367***	(0.082)	0.349***	(0.078)	-0.276**	(0.084)	-0.274**	(0.083)
Random Effects	Variance Component *(df)*	χ^2	Variance Component *(df)*	χ^2	Variance Component *(df)*	χ^2	Variance Component *(df)*	χ^2
Residuals	0.008 (768)	771.352	0.013 (785)	807.859	0.000 (760)	724.190	0.014 (768)	800.967

* p<.05; **p<.01; ***p<.001

Note: Survey Response 1 *excludes* unknown eligibility cases; Survey Response 2 *includes* unknown eligibility cases.

Table 2. Hierarchical model of the regional effects on the likelihood of being diagnosed for substance abuse treatment: (N=Level 1:4038; Level 2:627)

	Alcohol Problem (1=yes)		Drug Problem (1=yes)		Alcohol or Drug Problem (1=yes)	
Fixed Effects	**Coefficient**	**(S.E.)**	**Coefficient**	**(S.E.)**	**Coefficient**	**(S.E.)**
Intercept	-1.902***	(0.145)	-3.935***	(0.327)	-1.911***	(0.142)
Zip-code level Factors						
Factor 1: High Income Areas	0.128*	(0.062)	-0.039	(0.105)	0.119*	(0.060)
Factor 2: High Poverty Areas	-0.100*	(0.046)	0.029	(0.072)	-0.089	(0.042)
Factor 3: Immigrant Populations	0.031	(0.045)	0.036	(0.086)	0.042	(0.044)
Factor 4: Smaller Housing Units	0.033	(0.040)	0.056	(0.071)	0.043	(0.039)
Factor 5: Urbanicity	0.046	(0.040)	0.146*	(0.063)	0.066	(0.038)
Factor 6: Working Families	0.107	(0.099)	0.079	(0.169)	0.117	(0.097)
Factor 7: Poor Housing	-0.060	(0.140)	0.199	(0.290)	0.000	(0.131)
Factor 8: Government Employees	0.085	(0.061)	0.336**	(0.116)	0.099	(0.058)
Factor 9: White Collar Employees	0.081	(0.100)	-0.134	(0.180)	0.120	(0.101)
Factor 10: Newer Housing Units	0.082*	(0.038)	0.070	(0.081)	0.079*	(0.036)
Individual Level Covariates						
Regions (Ref: Southern Illinois)						
Chicago	0.243	(0.288)	0.088	(0.447)	0.250	(0.276)
Suburban Cook	-0.097	(0.253)	0.335	(0.443)	-0.069	(0.245)
Northwest Illinois	0.216	(0.200)	-0.036	(0.469)	0.271	(0.198)
South Collar Counties	0.101	(0.201)	0.234	(0.455)	0.154	(0.191)
North Collar Counties	0.109	(0.236)	0.573	(0.438)	0.199	(0.226)
West Central Illinois	0.103	(0.188)	0.444	(0.383)	0.229	(0.180)
East Central Illinois	-0.064	(0.196)	0.289	(0.392)	-0.019	(0.192)
Random Effects	Variance Component *(df)*	χ^2	Variance Component *(df)*	χ^2	Variance Component *(df)*	χ^2
Residuals	0.058 (616)	639.406	0.180 (616)	517.829	0.042 (616)	636.637

†<.06; * p<.05; **p<.01; ***p<.001

Table 3. Hierarchical model of the regional effects on the likelihood of being diagnosed for substance abuse treatment (N=Level 1: 4038; Level 2: 627)

	Alcohol Treatment Need (1=yes)		**Drug Treatment Need (1=yes)**		**Alcohol or Drug Treatment Need (1=yes)**	
Fixed Effects	**Coefficient**	**(S.E.)**	**Coefficient**	**(S.E.)**	**Coefficient**	**(S.E.)**
Intercept	-1.570***	(0.176)	-2.125***	(0.379)	-2.125***	(0.379)
Zip-code level factors						
High Income Areas	0.046	(0.050)	-0.076	(0.080)	-0.076	(0.080)
High Poverty Areas	-0.012	(0.039)	0.051	(0.084)	0.051	(0.084)
Individual demographic factors						
Gender (Male)	0.946***	(0.087)	0.858***	(0.199)	0.858***	(0.199)
Age	-0.213***	(0.025)	-0.822***	(0.074)	-0.822***	(0.074)
Race/Ethnicity						
Black	-1.105***	(0.238)	-0.186	(0.319)	-0.186	(0.319)
Hispanic	-1.019***	(0.193)	-0.880***	(0.328)	-0.880***	(0.328)
Other	-1.002***	(0.265)	-0.261	(0.444)	-0.261	(0.444)
Regions (Ref: Southern Illinois)						
Chicago	0.743***	(0.199)	0.704	(0.375)	0.704	(0.375)
Suburban Cook	0.165	(0.224)	0.183	(0.434)	0.183	(0.434)
Northwest Illinois	0.213	(0.200)	-0.426	(0.484)	-0.426	(0.484)
South Collar Counties	0.285	(0.190)	0.097	(0.447)	0.097	(0.447)
North Collar Counties	0.238	(0.204)	0.334	(0.443)	0.334	(0.443)
West Central Illinois	0.137	(0.194)	0.415	(0.399)	0.415	(0.399)
East Central Illinois	0.002	(0.202)	0.341	(0.369)	0.341	(0.369)
Random Effects	Variance Component *(df)*	χ^2	Variance Component *(df)*	χ^2	Variance Component *(df)*	χ^2
Residuals	0.094 (624)	593.356	0.168 (624)	399.089	0.008(625)	582.377

* p<.05; **p<.01; ***p<.001