

Theoretical Motivation for Post-Survey Nonresponse Adjustment in Household Surveys

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In this paper we argue that statistical adjustment for nonresponse should be guided by an explicit theory of survey participation. We offer a framework for such a theory and discuss some of the factors that should be considered in gaining an understanding of why people agree to participate in surveys. Using data from a match of survey data to decennial census records, we illustrate the importance of considering different aspects of nonresponse (e.g., refusals and noncontacts) in informing adjustment models and discuss the implications of these findings for nonresponse adjustment.

1. Introduction

In the last ten years the survey literature has witnessed a significant increase in attention to nonresponse error in surveys. In many ways, the publication of the three volume set of the U.S. National Academy of Sciences Panel on Missing and Incomplete Data (Madow, Nisselson, and Olkin 1983) marks the commencement of new interest in nonresponse errors. Since that time the statistical procedures available to survey researchers have grown to include multiple imputation (Rubin 1987; Little and Rubin 1987), response propensity modeling and unit weighting adjustments (Ekholm and Laaksonen 1991), and combinations of newer techniques with the classic use of sample-based weighting class adjustments (Little 1982). From the perspective of attempts to reduce nonresponse rates there has been a continuation of developments in use of incentives (Kulka 1994), studies of advance letters (Traugott, Groves, and Lepkowski 1987), and evaluation of Dillman's total design method (1978). The *Journal of Official Statistics* has consistently been a forum for the presentation of new work in this field (e.g., Greenberg and Stokes 1990; Hinde and Chambers 1991; Göksel, Judkins, and Mosher 1992).

Finally, these years have seen the development of a series of international workshops on household survey nonresponse (see IASS 1994). These have documented response rate trends over different countries (de Heer and Israëls 1992; Gonzalez 1992) and acted as a forum for cross-national collaboration.

Within the research of the past few years there appears to be growing acceptance of the perspective that nonresponse is a phenomenon subject to stochastic properties;

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that is, all persons have some tendencies to participate and to fail to participate in surveys. This is in contrast to earlier perspectives (see the review by Holt and Elliot 1991) that viewed nonresponse as a fixed attribute of a sampled unit (e.g., person). The view that the decision to participate or not to participate is a process that has random components has led even those most attached to design-based estimation to contemplate the use of models for the adjustment process.

It is, however, at the model specification level that the field currently has its limitations. For the most part the practical use of adjustment procedures have been post hoc to survey design, the application of a statistical adjustment using whatever information happens to be available at the estimation stage. There has been very little discussion in the literature about the proper specification of postsurvey adjustment models, in contrast to analytic and empirical demonstrations of adjustment effects. There has been little attention to how survey designs might be altered to improve the quality of postsurvey adjustments.

Survey participation is a social behavioral phenomenon. Individuals, when presented with a survey request from an interviewer, engage in some sort of decision calculus prior to announcing their decision. Thus, appropriate models for postsurvey adjustment require the specification of a theory of these behaviors. This is inherently *not* statistical theory, although the theory must be expressed in statistical terms to be used in the statistical adjustment vehicles proposed. To be useful the theory must be one about human behavior, relevant to the survey decision.

Statistical commentary on nonresponse error can inform us about what properties of behavior are most important to such a theory. For example, the simple expression for the nonresponse error in a linear statistic, \bar{y} is

$$\bar{y}_r = \bar{y}_n + \left(\frac{m}{n}\right)(\bar{y}_r - \bar{y}_m) \quad (1)$$

where n is the total sample size,

m is the number of nonrespondents, and

r is the number of respondents, $n = m + r$.

This notes that total error in a linear statistic is a function of nonresponse rates and differences between the nonrespondents and the respondents on the survey variable in question. The task of the nonresponse adjustor, therefore, is one of predicting both response propensity and the survey variables jointly. The literature, however, has primarily demonstrated the effect of defining groups homogeneous on overall response propensities, ignoring explicit ties to the survey variables.

It is noteworthy that this expression generalizes to

$$\bar{y}_r = \bar{y}_n + \left(\frac{m_{ref}}{n}\right)(\bar{y}_r - \bar{y}_{m_{ref}}) + \left(\frac{m_{nc}}{n}\right)(\bar{y}_r - \bar{y}_{m_{nc}}) + \left(\frac{m_{ni}}{n}\right)(\bar{y}_r - \bar{y}_{m_{ni}}) \quad (2)$$

where the “*ref*” subscripts refer to refusal cases, the “*nc*” subscripts refer to non-contacted cases, and the “*ni*” subscripts refer to other noninterviewed cases.

Equation (2) underscores the fact that both the relative number and the characteristics of refusers, noncontact, and noninterview cases affect the overall nonresponse

error in survey statistics. Treating unit nonresponse as a homogeneous phenomenon (as suggested in equation (1)) may lead to harmful postsurvey adjustments, if causes of the three types of nonresponse vary.

This paper is guided by attempts to specify a mid-level theory of survey participation. The work has the long-run goal of providing practical tools for the design of surveys that have richer postsurvey adjustment possibilities for nonresponse. In essence the work has four steps:

1. building and testing theories of survey participation, with special attention to separate modeling for different types of nonresponse (e.g., refusal and non-contact);
2. inventing and evaluating observations for causes of survey participation, suitable for use on respondent *and* nonrespondent cases;
3. constructing similar measurements for survey-specific causes, with special attention to correlates of survey variables; and
4. empirically estimating and evaluating adjustment models.

This paper illustrates differences between the refusal and noncontact phenomena and points to the need to provide separate attention to each in specifying postsurvey adjustments. The main focus of this work is thus on steps 1 and 2 above. Section 2 reviews the theoretical perspective on survey participation that we take; Section 3 reviews the research design of a project that matched survey cases to their decennial census records; Section 4 motivates an operational model from the theory; Section 5 shows how the influences on cooperation appear to be quite different from those on contact; Section 6 comments on the import of these findings for propensity model adjustment schemes.

2. Theoretical Perspective

A full understanding of decisions to participate in surveys would guide the choice of procedures to keep nonresponse at acceptably low levels, and would help in the allocation of resources for such efforts. Such a theory would also guide assumptions inherent in statistical adjustment of survey data. In this paper we focus specifically on the refusal component of nonresponse in interviewer-administered (primary face-to-face) surveys. Figure 1 represents an overview of the factors shaping the decision to participate in a survey.

Our theoretical perspective includes four sets of influences that form the foundation of the householder's reaction to a survey request. Those on the left of the figure, "Social Context" and "Respondent" describe factors that are out of the control of the research design. Those on the right side of the figure are determined in large part by the survey design.

Some of the influences arise from relatively stable characteristics of the social environment or social context of the householder. These include both current social and economic conditions that may affect perceived needs for information, as well as public opinion and visibility of debate on issues of privacy and confidentiality of information provided for research and administrative uses. These combine to form

the “survey-taking climate” (Lyberg and Dean 1992). In addition, attributes of the neighborhood and the urbanicity of the residential location of the householder can influence reactions to survey requests from strangers.

The likelihood of survey participation is also directly influenced by various attributes of the persons sampled. These include knowledge of the survey topic, which may determine the cognitive burden of answering survey questions; prior experience as a survey respondent; and affective states extant at the time of the survey request.

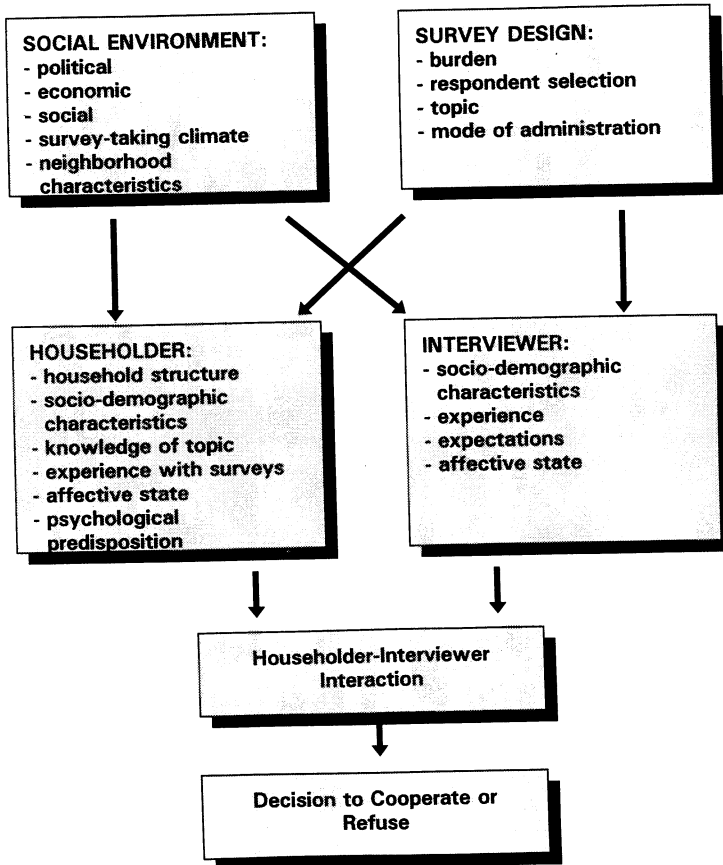
Surveys with high cooperation generally manipulate the characteristics on the right side of Figure 1 to minimize nonresponse rates. Surveys with different length of interviews, respondent selection procedure, and mode of data collection tend to vary in cooperation rates, depending on the characteristics of the population studied. Thus, “Survey Design” forms a block of influences on cooperation. Similarly, the survey design, through recruiting, training, and supervision of interviewers leads to a set of interviewers whose socio-demographic and attitudinal characteristics can influence the likelihood of cooperation of the householders they contact.

The influences of survey design and interviewer characteristics may or may not manifest themselves in the interaction between interviewer and householder, depending on the nature of the conversation between the two. Whatever happens, however, we believe, is seen through the lens of the social context of the householder and the psychological states relevant to a survey request. In the brief contacts that characterize interactions between interviewers and householders, that subset of factors deemed by the householder most relevant to the decision to participate is evoked and forms the basis of the judgement to accept or refuse the survey request.

Figure 1 focuses on refusals as a source of nonresponse, not noncontacts. We believe the noncontact phenomenon is somewhat simpler theoretically. Noncontacts arise as a logical function of the frequency and times that interviewers choose to call on sample households *and* the times that at least one person is at home in the sample household. Surveys with long data collection periods permit more callbacks. Surveys varying the time of interviewers’ contact attempts have lower noncontact rates, controlling on data collection length. Household “at-home” patterns are probably a function of socio-demographic attributes (e.g., number of persons in the household) and lifestyle (e.g., through working hours, social activities).

We thus expect that well-specified response propensity models must have different functional forms for noncontact and refusal components of nonresponse. The tendency of the literature to lump these phenomena together is no doubt one reason for the failure to replicate findings across studies. That is, a survey with a high noncontact and low refusal rate should theoretically have a different set of correlates of overall nonresponse than one with a low noncontact rate and high refusal rate.

A large literature exists on household-level correlates of survey participation (see Groves 1989, for a review). However, this literature has a number of shortcomings. First, most of the work is descriptive, addressing the question of what kinds of people tend to be nonrespondent. Second, these descriptions have focussed largely on individual attributes of nonrespondents, ignoring multivariate relationships.

Factors Affecting Survey Participation*Fig. 1.*

Third, the literature is characterized by an over-emphasis on case studies, with few (if any) cross-survey comparisons of nonresponse. Fourth, many of the studies have failed to distinguish between varieties of nonresponse (noncontact, refusal, etc.). This has led to a number of inconsistent findings in the literature (see Goyder 1987). Finally, the literature's collective results are largely the result of data dredging with conveniently available data, without use of theoretically motivated concepts.

The work described here is aimed at addressing each of these issues, with a special emphasis on contrasting refusal and noncontacts. The analyses, however, are focussed on only two set of theoretical factors: social context and household characteristics. Respondent-interviewer interactions are examined elsewhere (Groves and Couper 1993b), as are interviewer characteristics (Couper and Groves 1992a). The social context is also examined more fully in Couper and Groves (1995). Space does not permit a thorough review of the theoretical arguments for, and empirical evidence of, the effects of various socio-demographic effects on survey participation (see Groves, Cialdini, and Couper 1992).

3. Research Design

The 1990 U.S. decennial census provided us with a rare opportunity to obtain information on survey nonrespondents from their decennial census records. We matched all nonrespondent and a sample of respondent cases from each of the U.S. Consumer Expenditure Survey-Quarterly (CEQ); the U.S. Current Population Survey (CPS); the U.S. National Health Interview Survey (NHIS); the U.S. National Crime Survey (NCS); the 1990 National Household Survey on Drug Abuse (NHSDA); and the 1990 Survey of Census Participation (SCP) (see Groves and Couper 1993a).

These surveys were included because they were large national surveys being conducted at the time of the 1990 decennial census, and because funds were forthcoming from each of the sponsoring agencies of the surveys to conduct the research. The Census Bureau administered surveys are all ongoing data collections, and sample cases from a three-month period around census day (April 1, 1990) were selected. A subset of cases was selected from the NHSDA, while for the SCP the full sample was included in the match study.

Sample address information for each of the selected cases was used to match addresses to the decennial census, and thus household not person level attributes are available for study. Where available (e.g., the survey respondents), information on persons in households (surname, household composition, etc.) was used to check the veracity of the address-level match (Couper and Groves 1992b).

A total of 17,527 eligible sample cases from the six surveys were included in the match study, of which 7,167 were nonresponding units, and the balance were interviewed cases. An overall match rate of 96.3% was achieved, with a slightly higher rate for responding units (97.2%) than for nonresponding units (95.0%). The analyses reported here are based on the cases successfully matched for each survey. In addition, a small number of group quarters were excluded.

From the match to the decennial census, census household and person level data were assembled for all chosen respondent and nonrespondent households. Aggregate block-level census data corresponding to each sample address was also appended, to provide measures of the residential context of the sample household.

The data at our disposal have a number of limitations as tests of the theoretical notions discussed above. First, we do not possess indicators of all concepts in the theory. We have more data for household level attributes, but can conduct no useful analysis of psychological attributes, the effect of varying survey climates, influences of the interaction between interviewers and householders, or the number of contact attempts. Second, the overall response rates for these surveys are almost all above 90% of the sample households, making the detection of effects on response rates difficult. Third, these surveys are by no means representative of all household surveys conducted in the U.S., not even of all conducted by the U.S. Federal government. Thus, generalizability to other surveys and organizations is limited. Nevertheless, the data are unprecedented in their ability to test multivariate response propensity models on household surveys.

Of all the analytic questions that could be addressed with such data, we concentrate

on a key issue for model specification in unit nonresponse adjustment – do the data support different response propensity model specifications for refusal vs. noncontact sources of nonresponse? If so, nonresponse adjustment procedures need to model the two phenomena separately.

In this paper we have pooled the six surveys into a single data set, containing first-wave cases only from panel surveys. This meant dropping a number of cases from CPS and NCS. (We verified that conclusions of the paper are not affected by analyzing the surveys separately.) For our purposes, a cooperation rate is defined as: interviews/(interviews + refusals). Cooperation is thus defined as response, given contact, excluding “other noninterview” cases (e.g., those physically or mentally unable to respond). Eligible sample units that were not contacted or that yielded respondents who for a variety of reasons (illness, language barriers, mental incapacity, etc.) could not be interviewed were excluded from the denominator. The contact rate is simply the proportion of all eligible sample households which were actually contacted, regardless of the outcome of the contact, thus contacted/eligible.

The multivariate modeling is done using logistic regression, with standard error estimates and statistical tests calculated using Taylor series approximation, reflecting stratification and clustering of the survey designs (using SUDAAN; Shah, Barnwell, Hunt, and La Vange 1993). Data are weighted for all analyses to reflect different probabilities of selection, both in the original studies, and in the survey-census match sample. Dummy variables for each of the surveys are included (CEQ being the omitted category) to reflect response rate differences across surveys.

4. Motivating Socio-Demographic Models from the Theory

Although the theoretical concepts in Figure 1 are dominantly social psychological in character, only demographic and socioeconomic attributes of households are present in the data. Therefore, the empirical analyses rest on some logical assertions about the tendency of different subgroups in the population to exhibit the social psychological attributes described above.

The theory in Figure 1 suggests that the environment of large urban areas would yield lower survey contact and cooperation rates, because they foster dense, but superficial interactions with thousands of strangers, higher fear of criminal victimization, longer hours away from home, and reduced ties to community organization. We thus suspect that urbanicity influences both contact and cooperation.

Different types of housing structures may indicate lifestyles that are differentially integrated with the community. Large multi-unit structures increase the quantity of interaction with others but can act to inhibit rich relationships because of the relative infrequency of repeated interactions with the same persons. The negative effects of crowding on personal interaction come to the fore in such situations. Our qualitative investigations with field interviewing staffs lead us to believe that access to households in such structures is difficult, but there may also be reduced cooperation, given contact, based on the effects of social isolation of these groups. The short form census data provide an indicator for single detached housing units and for categories of multi-unit structures.

We further suspect that certain life statuses lead to more or less social isolation, to the limitation of rich relationships with others in the community. Such persons derive less support from the larger community and thus may feel less obligated to contribute to it. A common finding in the past literature is that persons who live alone tend to be nonrespondents, and conversely, that households with children tend to participate in surveys (Lievesley 1988).

Ceteris paribus, households with more members should have at least one person at home more often than those with fewer members. This would suggest higher non-contact rates for small households, but not necessarily higher refusal rates. However, the fact that elderly persons disproportionately live alone demonstrates that all things may not be equal. The other hypothesis offered for this correlation reflects speculation on how rich social interaction between the household and others affects response to requests from strangers. The larger the number of household members, the greater may be the tendency to have many persons interact with the household. The ties such a household has with many persons make its reaction to a stranger's request a more common event, received with less resistance than one rarely in contact with persons outside the household. This suggests greater social isolation among smaller households, leading to lower cooperation rates.

The notion of "civic duty" as a set of norms dictating certain behaviors for the benefit of the common good is closely related to these attributes. Although we suspect that elderly persons will disproportionately carry with them these norms, the literature on response suggests that other forces tend to depress both contact and cooperation rates among the elderly (Herzog and Rodgers 1988).

For fear of crime indicators at the household level the data set is unusually poor, although we can identify households with exclusively elderly respondents and rely on the findings of Miethe and Lee (1984) and others (see Rucker 1990) to note that elderly persons are disproportionately fearful of crime. Another hypothesis is that there is an increasing tendency toward "social disengagement" by persons as they age. Their network of friends reduces in size, their activities with those friends become more infrequent. In contrast to the elderly, young households (say, those with all persons less than 30 years of age) might be difficult to contact because of active out-of-home obligations, but willing survey participants once contacted.

Another factor operating at the household level that is hypothesized to affect survey participation is the concept of social location. Goyder's (1987) use of the term appears to refer more to status location within society (along the lines of Galtung's (1967) center versus periphery) rather than spatial location (e.g., urban versus rural). Two key indicators of social location in American society are socio-economic status (SES) and race, both defining the location of groups and persons in the power structure of society. We would expect from theory to see effects at both extremes of the socio-economic status scale, for persons at either end might feel less attached to the central institutions of the society.

The available indicators of socio-economic status on short form census data are whether the household owns or rents the housing unit; the value of the monthly rent, if it rents; and the estimated house value, if it owns. We hypothesize a curvilinear

relationship, with lower cooperation among the higher *and* lower values of rent and housing costs. However, little of these differences should apply to noncontact rates.

We also note the very mixed evidence of racial and ethnic groups differing on survey response rates (see Groves 1989, chap. 5). There is evidence of consistently lower response among Blacks in surveys of political participation. Some analysts believe that lower political efficacy and political participation among Blacks lead to this reaction. For other surveys the evidence is mixed. We believe that the undercoverage of Blacks in the U.S. decennial census also appears in household surveys, but that refusal levels are best explained by other variables. Nonetheless, in order to test these notions, our empirical analyses include race and ethnicity. Finally, noncontact rates among racial and ethnic minorities can be depressed by failure to make as many calls in evenings, based on safety concerns of interviewers.

5. Multivariate Analysis of Cooperation and Contact

It is clear that many of the variables above are themselves correlated with one another. For example, those in large multi-unit structures may be more likely to be renters than owners, households with all people under 30 may be more likely to have minor children, and so on. Multivariate analysis is needed to disentangle the separate effects of these variables.

An important question for postsurvey nonresponse adjustment is whether the same influences on refusal propensity apply to noncontact propensity. We note that increasing overall response rate by efforts in the field usually acts to reduce the noncontact rate of a survey more than the refusal rate. This is because increasing the number of callbacks is a more cost efficient way to reduce nonresponse in contrast to refusal conversion. However, if different influences apply to noncontact and refusals, different optimal postsurvey adjustment models should often apply for high and low response rate surveys.

Table 1 is a simple test of whether the same model applies to both dependent variables. After the cooperation model was constructed consistent with the theoretical structure in Figure 1, we used the same model specification for the dependent variable measuring contact propensity.

Urbanicity is one of the most consistent correlates of survey participation in the literature (see Couper and Groves 1994), and we find this is true both for cooperation and for contact in these data, even controlling for a variety of household-level variables. This supports the notion of contextual or environmental factors playing a role in survey participation (see Figure 1).

The cooperation model shows only minor tendencies for higher status households to refuse (there was no evidence of curvilinear effects for tenure and housing costs), that both "young" households and "old" households tend to cooperate, that households with children tend to be cooperative, that Blacks and Hispanics tend to be cooperative, that single person households do not, and that housing structure type is not relevant to cooperation.

It is interesting to note that we find no effect for type of housing structure on cooperation in these surveys. However, examining the contact-rate model in Table 1,

we find that large multi-unit structures (those with 10 or more apartments) have a significant negative effect on contact. This supports our interviewer reports that gaining access to such structures and finding their residents at home are the biggest problems; once such persons are contacted, they are no less likely to cooperate with the survey request than other households.

We also note that whereas the effects for house value and monthly rent are relatively small in the cooperation rate model, these coefficients are both significantly negative in the contact rate model. This suggests lifestyle differences across socioeconomic status (as measured by these two variables). Greater difficulty is experienced finding people in higher value homes and apartments at home, but once contacted they show little difference in cooperation.

The finding for household age composition is somewhat of a surprise. The marginal effect for households with all persons under 30 is significant and positive, even after controlling for presence of children in the household. Similarly, households where all members are over 70 are also significantly *more* likely to cooperate with a survey request, relative to those households where one or more members are between the ages of 30 and 70. As these are all surveys conducted or sponsored by the federal government, one explanation may be that government sponsorship might simultaneously increase the positive effects of civic duty among the elderly and decrease the fear of victimization.

In short, Table 1 shows differences in influences on contact and cooperation, which, if ignored, would produce a different suggested response propensity model. If adjusters used a response propensity model predicting 1 = interview and 0 = other (see column 3 of Table 1), they would ignore the fact that Blacks and Hispanics are mildly more difficult to contact, but quite cooperative when contacted; that large structures are difficult to enter, but their residents are not distinctive in cooperation with the survey request; that the age of residents has different effects on contact and cooperation.

6. Using Empirical Models for Postsurvey Adjustment

There is an important implication of the empirical results above – the specification of the response propensity part of a postsurvey adjustment scheme must reflect the two-step nature of survey participation. New model specifications are required to reflect the fact that the influences on the accessibility of sample households differ from those on the willingness to participate once contacted.

The two-step process of survey participation also has potential effects on the nature of the fit of response propensity models with dependent variable of 1 = interview; 0 = otherwise. Such models do not reflect the fact that predictor variables for contact are different from predictor variables for cooperation, given contact. We would expect, therefore, in the absence of theoretically derived specifications, that the predictors of overall response propensity found empirically important would be a function of the composition of nonresponse. For example, with large noncontact rates a predictor like the type of structure might be important; with low noncontact rates, unimportant. Similarly, in surveys with high refusal rates the age of household

Table 1. Effect of household variables and urbanicity on cooperation, contact and response

Independent variables	Cooperation		Contact		Response	
	Coeff.	(SE)	Coeff.	(SE)	Coeff.	(SE)
Constant	2.68**	(0.25)	4.73**	(0.34)	2.61**	(0.21)
Urbanicity: central city	-0.50**	(0.14)	-0.44**	(0.14)	-0.57**	(0.11)
Urbanicity: balance of CMSA	-0.20	(0.14)	-0.10	(0.14)	-0.18	(0.11)
Reference person						
Black nonHispanic	0.34*	(0.15)	-0.22	(0.16)	0.17	(0.12)
Reference person Hispanic	0.55*	(0.23)	-0.11	(0.21)	0.29	(0.15)
Single person household	-0.33*	(0.14)	-0.59**	(0.13)	-0.41**	(0.11)
Single family unit/ mobile home	0.33	(0.18)	0.39**	(0.15)	0.28*	(0.12)
Large multi-unit structure	0.035	(0.18)	-0.39*	(0.17)	-0.24	(0.14)
Children in household	0.23*	(0.11)	0.36**	(0.11)	0.30**	(0.087)
All persons under 30	0.82**	(0.15)	-0.033	(0.15)	0.42**	(0.11)
All persons over 70	0.38*	(0.19)	0.61**	(0.19)	0.29	(0.15)
Owner occupied	-0.29	(0.21)	-0.21	(0.18)	-0.25	(0.16)
Monthly rent for renters ¹	-0.051	(0.032)	-0.091**	(0.029)	-0.071**	(0.025)
House value for owners ²	-0.013*	(0.0057)	-0.029**	(0.0050)	-0.015**	(0.0044)
Survey indicators:						
CPS	1.42**	(0.18)	-0.66**	(0.29)	0.72**	(0.17)
NHIS	1.14**	(0.18)	0.13	(0.30)	0.93**	(0.17)
NCS	1.63**	(0.21)	-0.24	(0.31)	1.04**	(0.18)
NHSDA	-0.60**	(0.17)	-1.30**	(0.30)	-0.88**	(0.16)
SCP	0.36	(0.19)	-0.80*	(0.31)	0.0014	(0.17)

¹ Measured in units of \$100² Measured in units of \$10,000* $p < .05$ *** $p < .01$

members may be an important predictor, but in surveys with low refusal rates, an unimportant predictor. (Note that these examples assume that the cooperation model is itself fairly robust to different levels of refusals.) This could be removed as a source of confusion, if the two-step process were modeled directly.

7. Summary

The recent decades of statistical developments in postsurvey adjustment for non-response suffer from lack of commensurate development of theoretical motivations for adjustment models. The theoretical bases must lie in social psychological theories that specify influences on human behavior that affects survey participation. We have sketched the ingredients of such a theory, attempted to link socio-demographic indicators to the social psychological concepts, and utilized a unique data set with identical measures on respondents and nonrespondents to test elements of the theory.

To illustrate the importance of model specification, we chose to illustrate how the set of predictors appropriate to cooperation is not the same as the set for contact. Although we do not claim that either model is completely well-specified, the discrepancies between the two sets of coefficients are sufficient to demonstrate that different models apply, as asserted by the theory.

The conclusion of the paper is thus that the adjustor must devote separate attention to the process of contact and cooperation, at the time of survey design, in order to assemble indicators that might be useful in more fully specified response propensity models. From the model in Table 1 alone, we would suggest that efforts at interviewer observation of housing structure, size of household, presence of children, age of household members, and race/ethnicity, would be useful.

One way to proceed analytically may be to fit multinomial or nested multinomial logistic models, reflecting all of the relevant nonresponse categories. The predictor variables in those models would be the union of those found theoretically important for each of the outcome categories. The record-level weight would be a function of the sum of the estimated probabilities of falling in each of the nonresponse categories. To the extent that the different nonresponse categories had different influences, these weights would differ from those arising from the simple logistic regression predicting overall likelihood of participation.

We believe that it will become increasingly difficult to maintain high response rates while containing costs over the next few years. This will place greater emphasis on adjustment strategies rather than reduction of nonresponse rates for reducing non-response error. We hope to see more papers in the *Journal of Official Statistics* and elsewhere explicitly acknowledging nonresponse not only in the estimation phase, but also in the design phase. By increasing our understanding of the factors associated with survey participation and by attempting to embed measures of these concepts in the survey designs, we can improve the nonresponse adjustment process.

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