CORRELATES OF NONRESPONSE IN PERSONAL VISIT SURVEYS[1]

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1. Introduction

Errors of nonobservation in sample surveys include those arising from coverage failures of the sample frame, sampling error, and nonresponse or failure to obtain information desired for the survey. Each of these errors can be conceptualized as having fixed and variable components over realizations of a survey design. For example, sampling variance measures how consistent are estimates of sample statistics with the corresponding population statistic, over realizations of the sample design. Sampling bias would measure the extent of deviation between the sample statistic and the population statistic common to all realizations of the sample design. Among the errors of nonobservation only sampling variance has yielded itself to routine measurement and control, chiefly through the application of probability sampling theory to survey practice.

There are two strains of literatures on nonresponse biases. First, there are many examples in self-administered survey research of nonresponse bias estimation, through the use of data on the sampling frame known for both respondent and nonrespondent cases (see for example Goyder, 1987). There are several attempts to gauge the effect of nonresponse on survey statistics by comparing earlier or cooperative respondent characteristics with those of late or reluctant respondents (see O'Neil, 1979; Smith, 1984). In Great Britain, over two decades Kemsley (1975, 1976) matched sample survey cases with their records from the decennial census to learn about socio-demographic correlates of survey nonresponse. Second, there are a wide variety of techniques that impose external model-based assertions about the characteristics of nonrespondents, for purposes of reducing nonresponse bias. These include use of imputation procedures (Rubin, 1986; Little and Rubin, 1987) and weighting of respondent cases (Kalton, 1983).

It is tempting to react to the two strains of literature by noting that the first attempts to understand the causes and effects of nonresponse, and the second attempts to reduce the effects of nonresponse. As with many areas of survey methodology the understanding of human behavioral phenomena underlying the statistical properties of survey estimates is often not used to construct statistical procedures to improve the estimates. We have noted elsewhere (Groves, Cialdini and Couper, 1992) that all postsurvey adjustments for nonresponse entail implicit or explicit theories of nonresponse behavior. We argue that combining the two perspectives (understanding causes of nonresponse and postsurvey adjustments to reduce the effects of nonresponse) may be fruitful.

2. Theoretical Perspective on Face to Face Survey Participation

We believe that full understanding of decisions to participate in a survey requires a theory that integrates the observed influences of socio-demographic and survey design factors, on one hand, with the less observable impact of the psychological components of the interaction between interviewer and respondent. We view the decision to participate on the part of the sample person to be the fusion of diverse influences on participation, shaped by the events of the relatively short interactions with the interviewer. Some of these factors are discussed in more detail in Groves, Cialdini and Couper (1992), and we will only list them briefly here. There are a set of societal-level factors that define the "survey climate" (e.g., degree of oversurveying), various attributes of the survey design (e.g., length of questionnaire), demographic characteristics of the sample person (e.g., gender), psychological predispositions of the sample person (e.g., fear of victimization, social connectedness (Goyder, 1987)), and attributes of the interviewer (e.g., years of experience).

3. Data Collection Design

The 1990 decennial census provided us with a rare opportunity to obtain information on survey nonrespondents from decennial census records. The data used here were produced by matching nonrespondent and respondent cases from seven different national face to face surveys to data from the decennial census.

The following seven surveys were included in the survey-census match project (followed by their household response rate):

Consumer Expenditure Survey, Diary (90%)
Consumer Expenditure Survey, Quarterly Interview (86%)
Current Population Survey (95%)
National Health Interview Survey (96%)
National Crime Survey (97%)
National Household Survey on Drug Abuse (82%)

Survey of Census Participation (90%)

The selection of these surveys was somewhat arbitrary. They were included because they represented large national surveys being conducted at the time of the 1990 decennial census, and because funds were forthcoming from each of the agencies represented by these surveys. All seven are personal visit surveys, although one involves a self-administered diary.

From each of these surveys a random selection of respondent and nonrespondent cases was made. The five Census Bureau-administered surveys are all ongoing, and cases from a three-month period around census day (April 1, 1990) were selected. From the other two surveys, a subset of all cases was made. In order to maximize the number of nonrespondent cases, these were disproportionately selected from each survey. In total the data set has about 10,400 interviewed cases and about 7200 nonrespondent cases. A match to decennial census records was attempted for all these cases, at the level of the housing unit (not the person). On average across the surveys 97% of the interviewed units were matched successfully, and 95% of the nonrespondent cases were matched. 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 respondent and nonrespondent households. Block-level census data corresponding to each sample address was also appended, to provide measures of the residential context of the sample household. Finally, in order to gather information concerning interviewers, self-administered questionnaires were distributed to all interviewers assigned to the surveys. These questionnaires measured experiences, data collection behaviors, attitudes, and demographic characteristics of the interviewers. Thus, the data set has measures at three levels of aggregation: sample household, neighborhood, and interviewer.

There are many factors that contribute to the variation over surveys in the response rates, including important survey design differences (e.g., respondent rule, mode of interview, length of survey period, length of interview, panel nature of design, etc.). It would be inappropriate to compare these rates to make inference about any single design feature without appropriate caution. Similar caution must be taken when examining other influences on response rates, using these seven surveys, because measured effects of one variable may be confounded with design differences. On the other hand, correlates of response rates that are present over all surveys merit attention.

4. Correlates of Survey Participation

A convenient expression for nonresponse bias in a linear statistic is

$$(n_{nr}/n)(y_r - y_{nr}) \quad (1)$$

where n_{nr} is the number of nonrespondent cases in the sample; n is the full sample size; y_{nr} is some linear statistic for variable y based on nonrespondent cases in the sample; and y_r is the same linear statistic for variable y based on the respondent cases in the sample.

The first term in the expression, (n_{nr}/n), is the nonresponse rate for the survey and the second term, $(y_r - y_{nr})$, is the difference between respondents and nonrespondents on some statistic of interest. These two components evoke two approaches to examining the data from the census-survey match, examining variation in response rates over different household-level characteristics and examining how respondents and nonrespondents different on those characteristics.

One approach to analyzing the census-survey match data is to seek replication of findings from the survey methodological literature concerning correlates of nonresponse. In a review of this literature Groves (1989) notes several consistent findings:

a. households in urban areas have higher response rates
b. single person households have lower response rates
c. older persons have higher refusal rates, but lower noncontact rates
d. younger persons have higher noncontact rates
e. black persons have lower response rates on political opinion surveys but not necessarily on surveys of other topics
f. persons with high educational attainment have higher response rates on self-administered questionnaires, but lower on face to face surveys
g. households with young children have higher response rates

Some consistently powerful correlates of nonresponse among the seven surveys are two attributes of the housing unit. Table 1 shows that higher response rates are indeed found in rural areas and in urban areas outside areas defined as urbanized by the Census Bureau. Rates in urban locations within urbanized areas tend to be lower by 2 - 6 percentage points. (It is important to note that only for three of the seven surveys would the difference be found statistically significant at traditional levels.)[2] Similarly, consistently across surveys there is evidence of lower response rates among single person households. This is one of the largest correlates consistently found across surveys, with single person households often producing rates 5 to 7 percentage points lower than other households. (In only two of the surveys would the differences be found statistically significant on separate tests.) In analyses

not presented in Table 1, there is inconsistent evidence of race/ethnicity differences, of educational differences, and of differences associated with the presence of children in the household.

In addition to attempting to replicate past correlates of response rates, there are several questions that require multivariate analysis to address. The match to census data permits such analysis:

a. Do surveys differ in the basic influences toward nonresponse?
b. Do the correlates of overall nonresponse mask different influences playing on noncontacts, on one hand, and refusals, on the other?
c. Do the influences on participation vary across subgroups?

4.1 Variation in Correlates over Surveys

The response rates for housing units in multi-unit structures tend to be lower, by about 5 to 10 percentage points for the extreme comparisons. In almost all surveys, there is a monotonic decline of response rates across structure size, single family structures tend to have higher response rates than units in larger structures. Traditionally, these differences were thought to be the combined effect of higher noncontact rates and higher refusal rates. This speculation seems to be true for most of the surveys examined here, with the possible exception that those conducted by nongovernment agencies show higher noncontact rates as a cause of lower overall response rates in large structures. This may relate to relatively lower perceived authority to collect data by nongovernment agencies by gatekeepers in large structures.

A similar variation over surveys may be present concerning the response rates for households with persons over 65 years. Here the government surveys show no consistently lower response rates in these units, but the two nongovernment surveys do. Common speculation about the behavior of elderly respondents who live alone is that they are often at home (i.e., noncontact rates are low) but that fear of crime, general reticence to interact with strangers, and physical disabilities produce higher nonresponse rates among them. In this data set, for example, The National Household Survey on Drug Abuse, shows that the largest differences in nonresponse lie in the "other noninterview" category, with only small increases in refusals.

4.2 Do Different Influences Apply to Refusals and Noncontacts?

A common analysis on respondent data is to compare respondents, noncontacts and refusals on various observable characteristics of the sample units. This often reveals that different correlates exist for the ability of the interviewer to contact the sample unit versus the ability to elicit the cooperation of the household.

Analysis of the seven surveys shows rather consistently higher noncontact rates in large, multi-unit structures (e.g., those with 10 or more housing units). Estimated standard errors suggest that most of the surveys individually would not yield rejection of the null hypothesis in separate tests, the trend is consistent over most of them. Field interviewers often complain about the difficulty of gaining access to housing units in large structures because of locked central entrances and other security features. In contrast, among those housing units where contact was made by the interviewer, there is little evidence of higher refusal rates in larger units. That is, there appears to be no reluctance to respond once contacted among residents of larger units consistent across all surveys.

Such variation in the characteristics of those left uncontacted and those refusing a survey request may have implications for resulting nonresponse bias. Expression (1) can be elaborated with separate terms for refusals and noncontacts, showing that the overall nonresponse bias can be increased or decreased by shifting nonrespondents from the noncontact portion to the refusal portion of nonresponse. A common strategy of surveys over the past years is to maintain response rates by increasing efforts to contact all the sample cases, while experiencing increasing refusal rates. The impact on nonresponse bias is a function of differences between those unlikely to be contact with mild efforts and those ultimately refusing. Further examination of the differences between those noncontacted and those refusing should guide speculations on the type of survey statistics most affected by this tradeoff.

4.3 Do Influences on Participation Vary Over Subgroups?

Having sample household, interviewer, and neighborhood characteristics available on the same data set permits the estimation of a variety of statistical interaction effects, all of which address whether the influence of one variable on the likelihood of refusal itself varies across subgroups.

For example, the interaction effect of interviewer race and respondent race has been a popular topic in research on survey measurement errors (Schaeffer, 1980; Schuman and Converse, 1971). The typical finding is that, for race-relevant topics, respondent answers vary depending on the race of the interviewer assigned to the respondent. With regard to compliance behavior in surveys, we suspect differences across like and different race pairings although their character might differ. The race of interviewer effects for responses were measured, by definition, among those willing to respond to interviewers of the given race.

Response rate differences by race pairings, in some sense, measure components of the selection bias of those results.

For example, The National Household Survey on Drug Abuse refusal rates show sensitivity to race of interviewer and race of reference person in the household (the first person listed on the Census form). First, White, Non-Hispanic interviewers in general obtained lower refusal rates than other interviewers (10.9% versus 13.8%). This confounds interviewer abilities to gain cooperation with differential difficulties of persuading the respondent to cooperate. The lowest panel of the table shows that White, Non-Hispanic households produce higher refusal rates than other households (12.6% versus 9.5%).

When race of interviewer and race of household is jointly examined, it appears that the highest refusal rates arise when "other" race interviewers are assigned to White, Non-Hispanic sample households (17.5%) and the lowest when White, Non-Hispanic interviewers are assigned to "Other" race households. A logit model for this analysis was specified as $g(y_i) = \beta_0 + \beta_1 x_{1i} + \beta_2 x_{2i} + \beta_3 x_{3i} + \epsilon_i$, where $g(y_i)$ is the logit link function; x_{1i} is 1, if the reference person is White, Non-Hispanic, and 0, otherwise; x_{2i} is 1, if the interviewer is White, Non-Hispanic, and 0, otherwise; x_{3i} is 1, if both the reference person and the interviewer are White, Non-Hispanic, and 0, otherwise.

When standard errors reflect the complexity of the sample design, the coefficient for race of reference person would be judged non-zero with conventional tests of significance. That is, White, Non-Hispanic respondents tend to have higher refusal rates, controlling on race of interviewer. The coefficient for race of interviewer is on the margin of the traditional .05 level of significance, and the interaction term is nonsignificant. In short, from this data set alone there is weak evidence of an interviewer race effect and no evidence of an interaction effect.

The *post hoc* hypothesis consistent with the data is the use of majority race interviewers for all assignments yields the lowest refusal rate. We caution against this interpretation because of the failure of this simple analysis to control for other characteristics of the sample households. That is, it is quite possible that the assignment patterns of the survey were such that "other" race households assigned to White non-Hispanic interviewers would agree to be interviewed at higher rates (regardless of race of interviewer) than "other" race households in areas interviewed by "other" race interviewers. Such refinement in these interaction effect measures will be the aim of further work on the data.

Another multi-level phenomenon for which interaction effects might be present are housing structure and household variables. For example, some surveys have found higher refusal rates among single person households. We know that large structures also cause interviewers difficulties.

Table 2. Percentage Refused By Household Size By Type of Structure: Percentage Refusal (Among Refusals and Interviews), 1990 Survey of Census Participation

	Type of Structure	
Size of Household	Single Detached	Multi-Unit
Single Person Households	5.0%	9.5%
Multiple Person Households	5.1%	3.8%
Total	5.1%	5.7%

Logit Model Parameter Estimates (predicting refusal)

Parameter	**Estimate (St. Err.)**
Intercept	-3.2 (0.26)*
Size of Household (1 = Single Person)	0.9 (0.32)
Type of Structure (1 = Multi-Unit)	0.2 (0.30)
Interaction (1 = Single Person in Multi-Unit)	0.9 (0.45)

* Standard error estimates using Taylor Series approximation, reflecting stratification and clustering of the design.

Table 2, for the Survey of Census Participation, shows the refusal rate of the cross-tabulation of household type by structure. Here we see that the tendency for single person households to be refusals appears within multi-unit structures only. There is over a six percentage point increase in refusals for single person households over larger households in

multi-unit structures but almost no difference for detached units. A logit model used to test for the interaction term is found to reject the null hypothesis of consistent effects of household size by structure type. Here too we believe that other variables lie behind this result. We suspect that single person households in detached units tend to be elderly persons and that the reduced time constraints and perhaps greater sense of civic duty of those households yield higher response rates. Some of these ideas are testable with the data set.

5. Differences Between Respondents and Nonrespondents on Survey Statistics

Another perspective on nonresponse error examines the second component of $(n_{nr}/n)(y_r - y_{nr})$, the term that reflects the deviation between respondents and nonrespondents on the survey statistic of interest. This component of nonresponse error is specific to the variables measured in a particular survey. We can simulate this perspective by treating the 1990 decennial data as a set of possible survey variables and estimating the expression above for different variables. The variables in the decennial data are generally sociodemographic on the person level and structural and economic variables on the housing unit level.

Table 3 simulates the measurement of several housing statistics and household statistics based on the sample surveys. For example, we simulate the results of using the seven surveys to estimate the mean house value among homes owned by their occupants. The most typical finding is that refusal cases have higher mean house values than do respondent cases. The result of this is that there is a downward bias in the estimated house value in survey statistics based only on respondent cases. In many cases the respondent mean house value is $15,000 - $20,000 below that of the refusal cases, with the full population mean about $110,000. Although this type of difference appears in six of the seven surveys, none of these differences is statistically significant in separate tests, at conventional levels.

A similar finding of refusal and noncontact cases being more costly housing units applies when using mean monthly rent as a statistic of interest. In addition, the mean persons per room of respondent cases is higher, reaching conventional levels of statistical significance in five of the seven surveys.

Some statistics concern the person reported to be the owner of the unit or the member in whose name the unit is rented. There is no consistent result over surveys on nonresponse characteristics of estimates of that person's employment status. For some surveys respondents tend to have employment rates lower than those for nonrespondents, but in some cases it is between those of refusals and noncontacts, and in one case even higher than for nonrespondent cases.

Nonrespondents tend to live in households without disabled persons (persons prevented from working because of a disability), but this is again not consistently so across all surveys. In some surveys, nonresponse bias may produce overestimates of the proportion of homes with disabled persons. Similarly, the percentage of households with all persons speaking English is sometimes higher and sometimes lower among respondents than among nonrespondents.

The comments above mainly concern the direction and magnitude of the term, $(y_r - y_{nr})$, as a way of describing what the nature of the nonresponse effect on survey statistics might be. The magnitude of overall nonresponse bias is a function of this difference and the nonresponse rate. Table 4 presents estimates of nonresponse bias ratios, defined as $(n_{nr}/n)(y_r - y_{nr})/y$ where n_{nr} is the number of nonrespondent cases in the sample; n is the full sample size; y_{nr} is linear statistic for variable y based on nonrespondent cases in the sample; y_r is the same linear statistic for variable y based on the respondent cases in the sample; and y is the same linear statistic for variable y based on all sample cases.

Other things being equal we expect the bias ratios of surveys with higher nonresponse rates to be higher. The lowest response rates apply to the two Consumer Expenditure Surveys (90% and 85%), the Drug Abuse Survey (82%), and the Survey of Census Participation (90%). In general, the bias ratios of these surveys are larger in absolute value, with an overall mean bias ratio ranging from .02 to .03 for those surveys versus ones of .003 to .01 for the other surveys. This means that nonresponse bias represents two to three percent of the statistic for these seven variables for the surveys with lower response rates.

6. Summary

Both efforts to increase response rates (to reduce nonresponse error) and to construct effective postsurvey adjustments require some speculation on predictors of survey participation. These predictors are best identified, we submit, through the construction of theories of survey participation. It is likely that useful theories will involve effects of interviewers, of the residential context, as well as of personal and household level attributes.

Obtaining demographic data from the 1990 decennial census permits only partial tests of some of these concepts but does allow examination of the

multivariate mix of influences on survey participation. The initial findings largely support the past literature on nonresponse correlates, with some evidence that government surveys may profit from relatively more legitimized authority that reduces the effect of age of sample person on cooperation and ability to enter large housing structures.

The design of the data set permits testing of a variety of interaction effects on response and refusal rates. Those of greatest interest involve cross-level correlations, across interviewer, neighborhood, and sample household levels. In this initial work we demonstrated how the higher refusal rates of single person households appears to be located in larger structures only, and that there is no evidence yet of race of interviewer-race of household interaction effects on refusals. When interactions do exist they can identify domains where simple "single rule" interviewer guidelines may not be effective in obtaining high response rates *and* where simple post-survey adjustments may be less efficient. If these cross-level interactions are prevalent in the further analysis of the data, then it is possible that post-survey weighting schemes that include interviewer-level and neighborhood level interactions may be desirable.

Finally, in order to place nonresponse error in an appropriate context the paper shows that bias ratios (on statistics computed based on 1990 census data) due to nonresponse tend to be small, often less than 1 percent of the statistic's value. Bias ratios for the surveys with higher nonresponse rates tend to lie between 2 and 3 percent of the estimate. Since nonresponse bias is a function of the relationship between the statistic in question and correlates of nonresponse, the inferential value of this exercise is limited to statistics of similar character to those examined. However, with surveys having such high response rates, such low bias ratios are to be expected.

This paper was a preliminary analysis of a large data set. Future analyses will elaborate cross-level models of survey participation and examine alternative postsurvey adjustment procedures.

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[2] Standard errors appearing in all tables were based on generalized variance estimators. First, intracluster correlations for the relevant variable were computed on two of the surveys. Then a design effect was estimated for each survey for the given statistic, reflecting different cluster sizes. It is expected that these are overestimates of the actual standard errors.

Table 1. Response Rate by Various Subgroups by Survey

	Consumer Expenditure Diary	Consumer Expenditure Quarterly	Current Population Survey	Health Interview Survey	National Crime Survey	Household Survey on Drug Abuse	Survey of Census Participation
Urbanicity							
Urban, urbanized	87.5%	84.1%	94.0%	95.0%	96.4%	77.4%	88.5%
	(1.9)	(1.3)	(0.6)	(0.6)	(0.5)	(1.0)	(0.9)
Urban, nonurbanized	92.9	90.0	96.2	98.3	97.3	86.6	94.2
	(4.8)	(2.6)	(1.2)	(0.8)	(1.2)	(2.3)	(1.5)
Rural	93.3	88.2	95.7	97.5	98.0	89.4	93.7
	(2.7)	(2.0)	(0.9)	(0.8)	(0.7)	(1.5)	(1.1)
Persons in Household							
One	85.3%	82.1%	91.6%	93.8%	95.1%	78.7%	84.6%
	(2.3)	(2.0)	(1.1)	(1.0)	(0.9)	(1.7)	(1.5)
Two	89.2	85.4	95.3	96.4	97.0	81.3	91.0
	(2.7)	(1.6)	(0.8)	(0.7)	(0.7)	(1.4)	(1.0)
Three	92.2	87.7	95.7	96.4	97.8	82.4	92.6
	(3.2)	(2.0)	(1.0)	(0.9)	(0.8)	(1.4)	(1.2)
Four	92.9	86.9	96.6	97.5	98.2	84.1	95.3
	(3.5)	(2.1)	(1.1)	(0.8)	(0.8)	(1.4)	(1.1)
Five or More	90.0	90.7	97.2	97.6	98.3	87.3	93.9
	(4.4)	(2.2)	(1.1)	(0.9)	(0.9)	(1.3)	(1.3)
Units in Structure							
Mobile Home	95.4%	90.1%	95.9%	97.2%	97.9%	89.0%	89.4%
	(5.4)	(3.4)	(1.8)	(1.6)	(1.3)	(2.5)	(2.3)
One	90.5	86.2	95.6	96.8	97.3	82.5	91.9
	(1.8)	(1.2)	(0.5)	(0.5)	(0.5)	(1.0)	(0.8)
2-9	89.4	83.1	92.8	95.2	96.5	78.9	90.5
	(4.2)	(2.6)	(1.3)	(1.1)	(1.0)	(1.9)	(1.4)
10-49	85.4	83.6	89.3	93.7	94.5	77.7	81.8
	(5.8)	(3.3)	(2.2)	(1.7)	(1.7)	(2.2)	(2.6)
50 or more	65.1	82.3	90.1	85.3	94.6	71.5	85.4
	(10.3)	(4.1)	(2.5)	(3.6)	(1.9)	(2.7)	(3.1)
Single Person Household, over 65 yrs.							
Yes	87.3%	85.5%	96.1%	96.5%	97.3%	77.7%	87.8%
	(5.3)	(2.9)	(1.4)	(1.3)	(1.2)	(3.2)	(2.2)
No	89.8	86.1	94.8	96.2	97.0	82.6	91.0
	(1.6)	(1.1)	(0.5)	(0.5)	(0.4)	(0.9)	(0.7)

Table 3. Survey Estimates for Nonrespondent and Respondent Cases for Various Housing Statistics, By Survey

	Consumer Expenditure Diary	Consumer Expenditure Quarterly	Current Population Survey	Health Interview Survey	National Crime Survey	Household Survey on Drug Abuse	Survey of Census Participation
Mean House Value							
Refusal	**$108,734**	**$123,637**	**$131,071**	**$124,322**	**$116,698**	**$119,852**	**$143,217**
	(10,294)	(8,436)	(8,147)	(7,760)	(7,653)	(6,842)	(18,989)
Noncontact	**143,131**	**145,844**	**143,439**	**101,052**	**140,267**	**155,125**	**114,754**
	(33,318)	(20,184)	(8,054)	(7,237)	(12,191)	(9,619)	(19,209)
Respondent	**109,745**	**100,190**	**111,928**	**115,152**	**101,244**	**103,471**	**105,349**
	(9,601)	(6,951)	(4,311)	(6,105)	(4,539)	(5,849)	(5,329)
Mean Monthly Rent							
Refusal	**$ 379**	**$ 458**	**$ 516**	**$ 463**	**$ 410**	**$ 481**	**$ 435**
	(47)	(23)	(21)	(23)	(16)	(18)	(37)
Noncontact	**402**	**467**	**433**	**436**	**435**	**563**	**426**
	(85)	(41)	(15)	(22)	(16)	(21)	(30)
Respondent	**387**	**402**	**439**	**412**	**417**	**394**	**394**
	(21)	(17)	(14)	(13)	(14)	(12)	(11)
Mean Persons Per Room							
Refusal	**0.44**	**0.47**	**0.50**	**0.50**	**0.48**	**0.48**	**0.49**
	(0.03)	(0.01)	(0.02)	(0.02)	(0.01)	(0.01)	(0.03)
Noncontact	**0.49**	**0.55**	**0.47**	**0.47**	**0.41**	**0.56**	**0.51**
	(0.07)	(0.06)	(0.02)	(0.03)	(0.03)	(0.02)	(0.04)
Respondent	**0.55**	**0.57**	**0.53**	**0.56**	**0.54**	**0.55**	**0.55**
	(0.03)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)

Table 4. Survey Estimates by Disposition Category, Nonresponse Bias Ratios, for Various Statistics, By Survey

	Consumer Expenditure Diary	Consumer Expenditure Quarterly	Current Population Survey	Health Interview Survey	National Crime Survey	Household Survey on Drug Abuse	Survey of Census Participation
Mean House Value							
Respondent	$109,745	$100,190	$111,928	$115,152	$101,244	$103,471	$105,349
	(9601)	(6951)	(4311)	(6105)	(4539)	(5849)	(5329)
Total	$109,828	$103,775	$112,809	$115,263	$101,786	$107,580	$107,085
	(8705)	(6527)	(3106)	(5124)	(4047)	(5756)	(5506)
Bias Ratio	-.00076	-.035	-.0078	-.00096	-.0053	-.038	-.016
Mean Monthly Rent							
Respondent	$387	$402	$439	$412	$417	$394	$394
	(21)	(17)	(14)	(13)	(14)	(12)	(11)
Total	$389	$409	$441	$414	$417	$415	$400
	(19)	(15)	(10)	(10)	(11)	(11)	(10)
Bias Ratio	-.0051	-.017	-.0045	-.0048	.00	-.051	-.015
Mean Persons Per Room							
Respondent	0.55	0.57	0.53	0.56	0.54	0.55	0.55
	(0.03)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Total	0.54	0.55	0.52	0.56	0.54	0.54	0.54
	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Bias Ratio	.019	.036	.019	.00	.00	.019	.019
Percent Units Boarded Up							
Respondent	0.72	0.35	0.19	0.27	0.30	0.22	0.29
	(0.44)	(0.09)	(0.04)	(0.05)	(0.07)	(0.04)	(0.06)
Total	0.67	0.33	0.20	0.27	0.30	0.23	0.30
	(0.36)	(0.07)	(0.03)	(0.04)	(0.06)	(0.04)	(0.06)
Bias Ratio	.075	.061	-.050	.00	.00	.043	.033

Table 4. Survey Estimates by Disposition Category, Nonresponse Bias Ratios, for Various Statistics, By Survey (contd.)

	Consumer Expenditure Diary	Consumer Expenditure Quarterly	Current Population Survey	Health Interview Survey	National Crime Survey	Household Survey on Drug Abuse	Survey of Census Participation
Percent Person 1 Employed For Pay							
Respondent	**70.8%**	**78.5%**	**79.2%**	**78.3%**	**79.1%**	**78.0%**	**79.9%**
	(6)	(4)	(3)	(3)	(3)	(3)	(2)
Total	**70.9**	**79.3**	**79.3**	**78.5**	**79.1**	**78.2**	**79.1**
	(5)	(3)	(2)	(2)	(2)	(2)	(2)
Bias Ratio	**-.0014**	**-.010**	**-.0013**	**-.0025**	**.00**	**-.0026**	**.010**
Percent with Household Member Prevented from Working Because of Disability							
Respondent	**17.4%**	**18.6%**	**15.0%**	**13.3%**	**16.1%**	**20.1%**	**17.1%**
	(5)	(4)	(2)	(2)	(3)	(3)	(2)
Total	**16.7**	**18.2**	**15.1**	**13.3**	**16.1**	**19.7**	**17.4**
	(4)	(3)	(2)	(2)	(2)	(2)	(2)
Bias Ratio	**.042**	**.022**	**-.0066**	**.00**	**.00**	**.020**	**-.017**
Percent All Members Speak English							
Respondent	**90.6%**	**85.8%**	**88.5%**	**89.1%**	**81.8%**	**87.8%**	**83.4%**
	(4)	(4)	(2)	(2)	(3)	(2)	(2)
Total	**90.0**	**86.5**	**88.6**	**88.9**	**81.7**	**88.0**	**84.2**
	(4)	(3)	(2)	(2)	(2)	(2)	(2)
Bias Ratio	**.0066**	**-.0081**	**-.0011**	**.0022**	**.0012**	**-.0023**	**-.010**