REVISION II

ANALYSIS OF NONRESPONSE EFFECTS IN THE 1995 SURVEY OF CONSUMER FINANCES

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data and has been an invaluable colleague on too many counts to describe. This paper is dedicated to the interviewers for the 1995 SCF and the other NORC staff, without whom there would be nothing to report here.

Unit nonresponse is a serious problem in the Survey of Consumer Finances (SCF). In the area-probability (AP) part of the SCF sample, only about 70 percent of the selected respondents agree to participate in the survey; in the relatively wealthy SCF list sample, the cooperation rate is much lower.[1] Unsurprisingly in this light, the study of nonresponse has long been a core area of research for the project.[2] The survey is fortunate in having extensive frame data on income and some other characteristics for the entire list sample, and this information has driven most of the project research on nonresponse. This work has contributed very substantially to our ability to measure the behavior of wealthy households. However, until the 1995 survey, the only systematic information available for the AP sample has been the identity of the primary sampling unit.

This paper uses information newly available for the 1995 SCF to look more broadly at the causes of unit nonresponse and the efforts expended to obtain completed interviews. The new data used here include information about characteristics and attitudes of the interviewers, descriptive material about the first contact with the respondent, characteristics of the respondent's neighborhood, and the administrative logs that interviewers keep to track actions for each case.

The paper focuses on two issues. First, following the research of Groves and Couper [1996] on "tailoring" behavior by interviewers, the paper develops a set of reduced form models describing the interaction of effects attributable to interviewers, respondents, and the contextual effects of neighborhoods. An innovation in the approach here is the use of a discrete time hazard model of the resolution of the sample cases into complete or refused final dispositions. Second, I present information on the informational gains from pursuing very difficult cases though a large number of attempts. Traditionally, it has been argued that the value of this approach lies in a belief that such difficult cases are more like the cases that ultimately refuse to participate than the cases that do participate. However,

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[1]See Kennickell and Woodburn [1997] for a description of the SCF sample and more detail on response rates. Other SCF information is available on the FRB webpages at http://www.bog.frb.fed.us/pubs/oss/oss2/scfindex.html .

[2]For example, see Woodburn [1991] and Kennickell and McManus [1993].

such cases also tend to be quite expensive. Here I look at the effects on some key wealth estimates of truncating the efforts expended on cases.

The paper has four sections. The first part gives some background on the design of the SCF and provides a basic description of unit nonresponse in the survey. The second section develops a multivariate model of nonresponse. The third section presents information on the effects on wealth estimates of truncating the set of completed cases based on the number of contacts required to complete them. The final section summarizes the findings of the research and outlines future work.

I. Background

Description of the Survey

The SCF is a triennial survey sponsored by the Board of Governors of the Federal Reserve System, with the cooperation of the Statistics of Income Division (SOI) at the IRS. Data for the 1995 survey, the basis of this paper, were collected by the National Opinion Research Center at the University of Chicago (NORC) between the months of June and December using computer-assisted personal interviewing. There were 246 final interviewers for the cases released to the field. The median interview required approximately 90 minutes, but some took as long as three hours. The questionnaire focuses on households' assets, liabilities, and financial relationships. Data are also obtained on employment history, pension rights, marital history, demographic characteristics, and various attitudes and expectations.[3]

The SCF employs a dual-frame sample design, including an area-probability (AP) sample and a list sample.[4] The AP sample is a multistage design with equal probabilities of selection for each household included.[5] The list sample is drawn from a special sample of tax returns selected and edited by SOI for research purposes, the Individual Tax File (ITF). These data are divided into seven strata for sampling. Empirically, the first three strata overlap strongly with the AP sample in terms of their

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[3]Kennickell, Starr-McCluer and Sundén [1997] provide an overview of the data.
[4]See Kennnickell and Woodburn [1997] for details.
[5]See Tourangeau et al. [1993].

Table 1: Response Rates as a Percent of Eligible Respondents, 1995 SCF, for Various Parts of the Sample

All AP sample cases	
66.3	
Northeast region	60.1
Northcentral region	70.9
Southern region	
67.2	
Western region	
65.3	
Largest urban areas	58.9
Other cities and towns	66.6
Non-urban areas	
77.6	
All list sample cases	30.4
Stratum 1	45.2
Stratum 2	39.5
Stratum 3	35.5
Stratum 4	35.0
Stratum 5	30.4
Stratum 6	23.9
Stratum 7	12.8
List sample participants as a % of those not refusing by postcard	
All list sample cases	38.7
Stratum 1	54.2
Stratum 2	54.7
Stratum 3	47.5
Stratum 4	45.4
Stratum 5	38.7
Stratum 6	29.9
Stratum 7	15.1

Table 2: Reasons for Noninterview, 1995 SCF, Percent of Eligible Sample Type

	AP	*List*
Postcard refusal	NA	30.7
No contact	2.1	0.0
Unlocatable	0.1	3.9
Unavailable	0.3	3.0
Language problem	3.3	0.7
Too ill	4.4	2.0
Refused by gatekeeper	2.8	3.6
Refused, too long	17.5	16.9
Refused, too personal		47.2
	18.7	
Refused, gov't involvement	7.6	2.8
Stopped work	5.4	15.2
Other incomplete	9.4	2.5

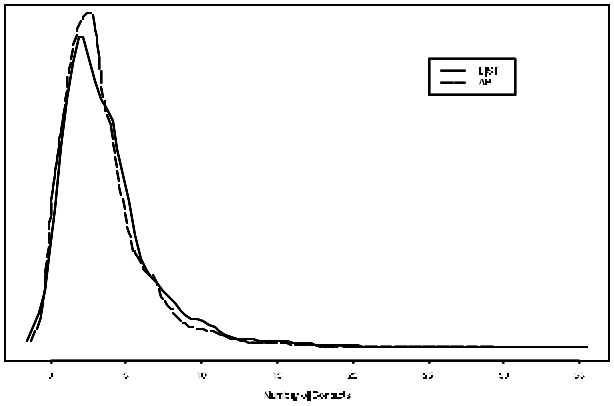
wealth and the top four strata are generally substantially wealthier. Cases in higher strata are sampled at increasingly higher rates. List respondents are treated somewhat differently from AP respondents: by agreement with SOI, list sample respondents are initially sent a postcard offering them a chance to refuse participation in the survey. All list cases not returning a postcard and all AP cases are to be pursued with equal vigor. The AP sample provided about 2,800 of the survey participants in 1995, and the list sample about 1,500.

Unit Nonresponse

The general experience over the history of the survey is that respondents feel that the survey is long and that it requests particularly sensitive information. Consequently, it is not surprising that response rates have been lower than those in many other U.S. government surveys. Table 1 provides information on nonresponse for different parts of the sample. For the AP sample, nonresponse is a particular problem in the northeast region and in more urban areas. For the list sample, response rates decline from the bottom stratum to the top stratum. Even removing the postcard refusals from the calculation, the response rates in the lower strata are still substantially below those for the AP sample. Thus, it seems that there may be some factors affecting response for the list sample that are not as strong for the AP sample. Perhaps it is the effect of

being contacted more times than AP cases or being contacted specifically by name, either of which might arouse suspicion.[6]

Figure 1: ASH Plot of Number of Contacts, AP Sample and List Sample, 1995 SCF



Based on the final case disposition codes, almost half of the final reasons entered for nonresponse in the AP sample indicate that the respondent thought the survey was "too personal" (table 2). The length of the survey is also an important factor for the group. For the list sample, the length of the survey is about as important as for the AP sample; the lower proportion coded "too personal" and "government involvement" may be explained by the elimination of the group that refused by postcard.

The data also show that a significant fraction of apparently eligible observations cannot be classified as either complete or refused. About 8 percent of AP cases and about 22 percent of list case have final completion codes of "no contact," "unlocatable," "unavailable," or "stopped work." Moreover, it appears that even these figures understate the number of such "censored" cases. If we take the set of incomplete cases and reclassify them as censored if the last recorded action in the record of calls indicated that the case had not been contacted on that attempt, the proportion of such cases rises to about 9 percent for the AP sample and 30 percent for the list sample. I suspect that the proportion of such observations in the SCF is high relative to what might be found in other surveys, but I know of no systematic investigation of such outcomes in other surveys.

Contacts

[6]Cartwight and Tucker [1967] discuss an example where advance contact has negative effects.

Figure 2: ASH Plot of Number of Contacts, by Final Disposition; AP Sample, 1995 SCF

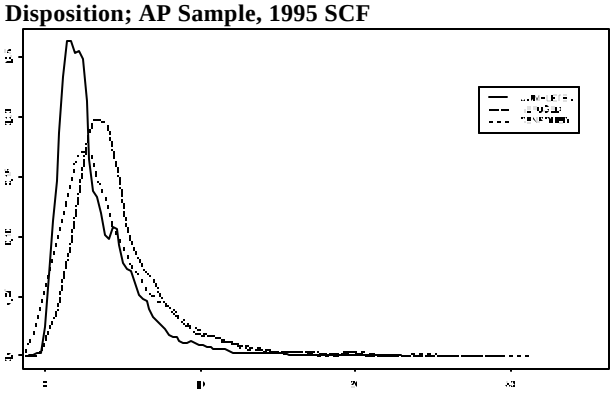
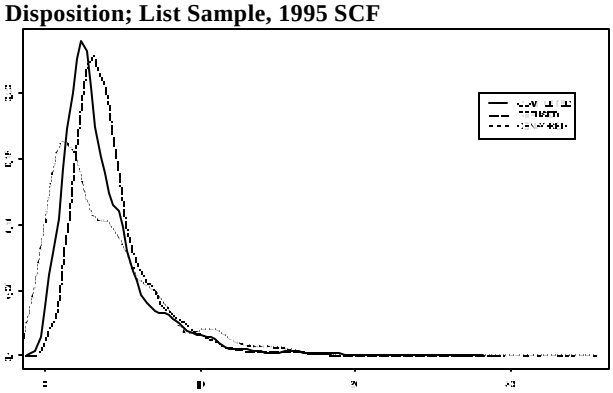


Figure 3: ASH Plot of Number of Contacts, by Final Disposition; List Sample, 1995 SCF



The project interviewers were diligent in pursuing the respondents. For each sample, figure 1 shows an average shifted histogram (ASH)—a type of kernel density estimate—plot of the distribution of the number of contacts at the end of the field period.[7] The results for both are remarkably similar. The overall median number of contacts was only 3 (mean of 4.1), but 10 percent of cases had eight or more contacts, and one case had 34 contacts.[8] As shown for the AP and list sample respondents respectively, the results differ surprisingly little when broken out by final disposition. The solid lines in the figures show the distribution for cases that were resolved as completed, the dashed line those that were resolved as refused, and the dotted line those that were unresolved at the end of the field period. For both samples, the distribution of contacts for the refusals is shifted to the right of that for the completed cases. This outcome is expected, since efforts are expected to be made to convert refused cases until it is judged that such efforts are no longer productive. It is also striking how much more alike the distributions are for completed and refused cases in the list sample than in the AP sample. For the cases that were unresolved at the end of the field period, it is interesting (the average of "a surprise" and "a relief") how similar the distributions are to those for the sample cases that had a final resolution:

[7]The number of contacts for the list sample excludes the initial postcard mailing. The interviewers reported that very many respondents had no recollection of having received any earlier survey materials.

[8]For a small number of cases, there were no recorded contacts: of the 385 cases with no recorded contacts, 13 were complete cases, 2 were refusals, 179 were censored cases, and 191 were ineligible.

both the median and mean number of contacts are virtually the same, and the distribution is no less skewed. Thus, there is no indication at this level that there was any different effort expended on cases that were never resolved.

Although contacts were monitored by the field supervisory staff, it was impossible to enforce a strict protocol without more precise information than was available without great effort. Extreme numbers of calls to resolve a case have a clear monetary cost. It is also possible that some interviewers could also have ceased trying to make contact with cases that might have been particularly stressful; the potential cost of such behavior is harder to evaluate.

New Data in the 1995 SCF

In the 1995 SCF, several new sources of information were added with the goal of furthering our understanding of unit nonresponse in the survey. First, new questions were added to the household enumeration folder (HEF), a paper document interviewers use to determine the respondent and record their actions on a case. The coded HEF data include a description of the first interaction with a person in the selected units, some characteristics of the informant for the initial household listing used to determine the eligible respondent, characteristics of the neighborhood surrounding the dwelling, and key items from the record of calls, a listing of all attempts to contact respondents. Second, interviewers completed a questionnaire about their own work and educational background and their attitudes. Third, ZIP code data were available for every case, and this information was used to link socio-demographic data derived from public files for the 1990 Census of Population.

There is no usable information on the record of calls for only 504 observations out of about 8,740.[9] The completion rate for the interviewer questionnaire was 100 percent, and missing information problems there are fairly small. There were minimal problems in matching the geographic variables by ZIP codes. Unfortunately, there are many cases with missing data among the variables in

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[9]The totals exclude the 1,070 list sample cases that refused participation by returning the postcard. The cases with missing data are nearly equally divided between the two samples; only about 30 were complete cases, about 70 were ineligible, about 100 are in the censored group, and the remainder were refusals. There are also more minor problems with missing data within the record of calls.

the HEF describing the structure of the sample household (about 4,100 cases) and those describing the first contact with the HEF respondent (about 1,500 cases).[10] Interestingly, these missing data problems were widely spread over the whole group of interviewers, rather than being concentrated in a smaller group. Logit modeling indicates interviews more likely to have substantial missing enumeration data were in ZIP codes with higher proportions of college educated adults, and less likely when the interviewer was older and more experienced and when the respondent was a list sample case selected from a stratum with high predicted wealth. Because of the severity of this missing data problem, the models reported in the next section that use the enumeration and contact variables should only be taken as suggestive.

II. Models of Unit Nonresponse

Background

The interactions between interviewers and respondents are at the heart of the survey process, but very many of the events that occur at that level either are unmeasurable without severe disruption of the interview, or are very difficult to define objectively. Most of the early research on these interactions examined behavior during an interview. Study in this area dates at least to Rice [1929] who studied the effects of interviewer beliefs (in his case about prohibition) on the answers respondents give. Hanson and Marks [1958] focused on the relationship between interviewer characteristics and data quality in an experiment using the 1950 Census of Population. Cannell et al. [1968] devised a sophisticated study merging data from the Health Interview Survey, a set of observations of interviewer behavior, a questionnaire administered to respondents about the original interview, and an interview with the interviewers. Their results indicated that attitudinal variables had at best a minimal influence on accuracy, but behavioral variables had strong effects.

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[10]The observations with missing enumeration data include about 1,300 completed cases, 1,700 refusals, and 1,100 cases that were neither complete nor refused at the end of the field period. Data on the first contact are missing in roughly equal numbers for completed, refused and censored cases.

More recently, there has been much discussion of unit nonresponse in the literature (for extensive citations see Bogeström et al. [1981] and Holt and Elliot [1991]). However, most of this research has dealt with the effects of nonresponse in estimation and possible remedies through weight adjustments (see, e.g., Little [1993]). Recent work by Groves and Couper [1993a, 1993b, 1996] has developed a theory of response and assembled a variety of types of information to test aspects of the theory. Because of the importance of their efforts for interpreting the research reported in paper, it is useful to describe their work briefly.

Groves and Couper hypothesize that two factors should figure prominently in interviewers' strategies as they approach a meeting with a respondent: keeping the respondent engaged, and tailoring their remarks throughout the interaction in a direction expected to avoid a permanent refusal. Interviewers may differ in their abilities to generate responses to the subjects' reactions that lead in a positive direction, and in their ability to decode the cues respondents provide. The authors assembled several sources of data. As a part of the National Survey of Health and Stress, a long interview on physical and mental health, interviewers obtained information describing the person with whom the negotiation for the interview took place and the events that occurred during the interaction. The interviewers also maintained a record of calls for each sample element, and they filled out a questionnaire about their own backgrounds and attitudes. These data are brought together in a series of models describing the success of each of the first through fourth contacts with the sample households as well as an overall model of response.

In their models, they find some strong effects, and a general weakening of effects with repeated contacts with a respondent. For example, barriers to entry have a negative effect on completing a case, but the effect fades with additional contacts. They find that success is less likely with one-person households and male respondents, and more likely where the interviewer is confident. Interestingly, interviewer experience has no effect. Initial negative statements and time delay statements made by a respondent have a persistent negative effect over repeated contacts. A measure of the degree to which interviewers tailored their interactions from one contact to the next has no significant effect, perhaps because of the crude nature of the measure. There are problems with their model. As in the SCF

study reported here, interviewer assignment was nonrandom, and many important variables are unobserved.

Prior SCF Work on Nonresponse

Most prior work on nonresponse in the SCF has focused on the list sample. Research reported in Woodburn [1991] investigated the effects of post-stratification for nonresponse adjustment in the list sample. Kennickell and McManus [1993] used more detailed information in the list sample frame to develop models of nonresponse for this group.[11] In these models, about three-quarters of the explanatory power came from a measure of financial income, with higher levels of this variable correlating with a lower response propensity.[12] Other important contributing factors were non-taxable income (positive effect on response); pension income (positive effect); real estate taxes (negative effect); wage and salary income (negative effect); estate, trust or royalty income (negative effect); age (negative effect), residence in the Western or Southern regions (positive effects); residence in California or any self-representing PSU (negative effect). The results of these models support the structure of the nonresponse adjustments applied to the SCF weights for the list sample. Unfortunately, because the variables available for modeling are so highly aggregated and abstract, it is difficult to extract much insight into the behavioral mechanisms that underlie the decision to participate in the survey.

Analysis Using the 1995 SCF Data

Respondents and interviewers come together usually with different information and perceptions about each other, and with very different incentives. Their interaction is a two-part behavioral game (or more-parted, if we allow for the effects induced by supervisors, survey organizations, and principal investigators).

The intended role of the interviewer during the negotiation stage is to communicate information to the respondent that will lead to an agreement to complete an interview. Interviewer behavior is influenced by a number of factors. As with other workers, it is important that they perform sufficiently

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[11]The variables constructed from the ITF include a number of income, tax, and other dollar values, the age of the filer, geographic information, and other variables related to the SCF sample design.

[12]Financial income is includes all types of interest and dividend income.

well to keep their jobs. There is monitoring of interviewers' performance along several axes, including the proportion of cases they complete, and some indications of the quality of the data collected. However, it seems likely that interviewers are driven by other less traditional incentives as well. It is striking how frequently the SCF interviewers talk about the importance of the research that gets done with the data they collect, the interest they have in the lives of other people, the adventure they find in visiting strangers in unusual places, and the appreciation they have of the independence of their work. While it is clear that they find most respondents enjoyable, there are sometimes very stressful and unpleasant interviews. Potential SCF interviewers are made aware of the nature of the survey, and they are selected based on their past performance and credentials, and at least implicitly on their ability to deal with strangers with a reasonable lack of fear. Because there is generally other work that competes in the same salary range as interviewing, experience is likely to weed out people who do not fit the desired profile. SCF interviewers are also extensively trained in order to minimize variations in technique. Nonetheless, many important variations likely remain in this group.

Randomization in the SCF sample designs virtually guarantees that respondents are more varied than interviewers. Respondents are taken to have a set of preconceptions and an internal structure that determines their responses to stimuli. Prominent among the factors that might influence respondents in their willingness to participate in an interview are: a desire for attention or company, a sense of the competing uses or value of their time, their past experience with surveys, their sense social integration and the value of public service, their faith in government, their sense of their physical security, and their feelings about privacy. Respondents' reactions to an interview may also be shaped by their education or sophistication. It may also be that respondents who understand a survey and who feel themselves to be particularly interesting in the context of the survey might also be made particularly suspicious. No doubt there are many other psychological and demographic considerations that also enter into a decision to cooperate in an interview.

Although it would be interesting to model separately the interviewers' efforts and the respondents' receptivity, our ability to monitor what actually happens during the negotiations between interviewers and respondents is very limited. This work reported here takes a reduced form approach,

focusing on the factors influencing the resolution of cases into "completes" and "refusals." These resolutions are taken to be indicators for a latent variable reflecting something one might call the respondents' "enthusiasm" — denoted **E**, where this variable is a function of respondents' pre-existing attributes and their cumulative reactions to the interviewer. If **E** rises above a certain upper level $\mathbf{E}^+$, the respondent completes the interview, and if it falls below a certain lower level $\mathbf{E}^-$, he refuses "permanently." Until a respondent passes up to or beyond either $\mathbf{E}^+$ or $\mathbf{E}^-$, he remains "at risk": for respondents at risk, all we know is that their level of **E** lies between $\mathbf{E}^+$ and $\mathbf{E}^-$.

We might approach modeling the outcomes in several ways. One might simply model overall response versus all other outcomes, as has been common in most of the literature, or the probability of response at a given contact, as in Groves and Couper [1996]. An appendix to this paper provides a set of estimates of a set of such models for comparison with such earlier results. This paper adopts a different approach.

A respondent's decision at each contact to participate, refuse participation, or to stop short of either fits within the framework of a discrete time hazard model.[13] A classic example of the application of a hazard model is in the biometric literature where there is the possibility that a person under study might die of a number of causes over the period of observation or might continue to live at risk of dying through the period of observation. For the model considered here, the exit states are completed status and refused status, and the population at risk at each contact consists of the cases that have not received a final disposition as of the previous contact. Cases that cease to be contacted before they achieve a final resolution into complete or refused cases are treated as censored. The time dimension is

[13]Another possibility might be to model the process incorporating the ordering implied by **E** using, for example, a version of the ordered probit for repeated events. Although, a simple two-state hazard model should be less efficient than a correctly specified model incorporating the ordering, it is also a more flexible form than the ordered probit: the ordered probit estimates one set of coefficients with an event-specific shift parameter, while the hazard model allows a full separate set of parameters for each outcome. Investigations not reported here suggest that the simple ordered probit model is insufficiently flexible to capture the effects captured by the hazard model.

taken to be indexed by contacts with the survey respondent.[14] The general form assumed for the model is a form of logit:

$$\log \frac{P_{ijt}}{P_{i0t}} = \beta_j X_{it}$$

where P_{ijt} is the probability of outcome j for case i at time t, P_{i0t} is the probability that case i remains at risk after period t, X_{it} is a vector of possibly time-varying covariates for case i at time t, and β_i is a vector of parameters conformable with X_{it}. Because the likelihood function is the product of the probabilities at each period observed and β_i is not time-varying, the model can be estimated using a standard multinomial logit procedure taking each period for each survey case that is still at risk at that point as a separate observation.[15]

Plots of the unconditional discrete time hazards of resolving a case as a complete interview or a final refusal are shown in figures 4 and 5 for the AP and list samples respectively.[16] The general shape of each of the plots is very similar: there is a sharp initial rise in the hazard, followed by a sharp decline and a trailing off of the rate. The fact that the hazard first rises and then declines most likely reflects two factors: First, many respondents express a desire to read the study materials, confirm the authenticity of the study, or simply to think over the decision to participate. Second, reluctant respondents (even quite reluctant ones) are given additional information in subsequent attempts or exposed to different interviewers until the respondent unequivocally refuses to participate. In practice, the two effects are entangled. As expected, the refusal hazard for the list sample cases is initially much greater than that for

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[14]There are other possible choices for the time dimension in the model, most notably attempts on a case. The general distribution of contacts and attempts is similar in shape. However, because the coding of case actions is insufficiently strong to distinguish trivial actions from serious actions, the variable attempts appears to be too noisy an indicator to use in modeling.

[15]See Allison [1984, 1995] for a discussion of the estimation of discrete time hazard models. The SAS procedure CATMOD was used for estimation.

[16]The hazard for refusal is computed for each contact as the number of cases that resolve at that point as refusals, divided by the number of cases still "at risk" just before that contact less the number of observations censored at that point. This calculation and those that follow exclude the list sample cases that refused participation by returning the postcard.

Figure 4: Hazard Rate over Contacts, by Final Disposition; AP Sample, 1995 SCF

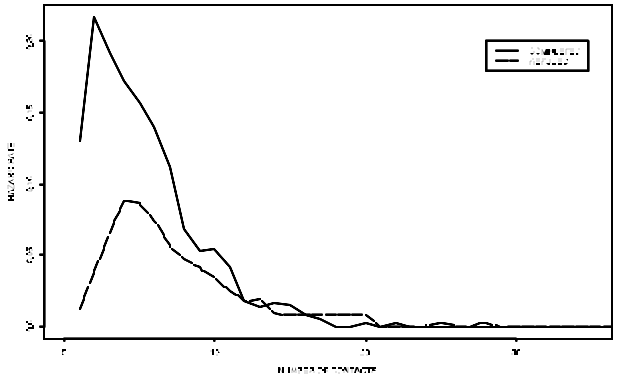
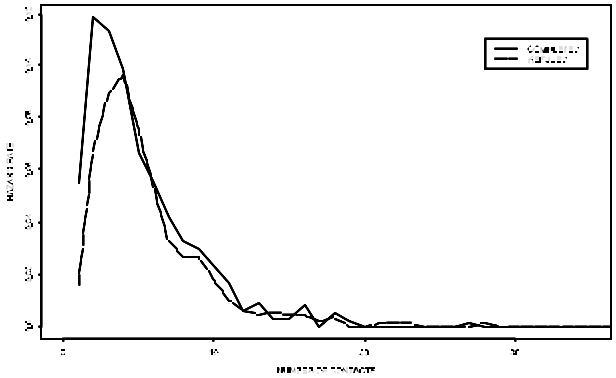


Figure 5: Hazard Rate over Contacts, by Final Disposition; List Sample, 1995 SCF



the AP cases. Overall, the shape of these plots reflects the simple intuition that "the easy cases resolve first." The important question is, what are the systematic components of this process?

Although there are interesting new data available for modeling the hazards, the information is still limited relative to the task. It is particularly problematic that to understand unit nonresponse more fully, we need information on the characteristics of the respondent, which are very likely to be unavailable from the respondent in cases where that person wishes strongly to avoid giving the interviewer information. The first column of table 3 shows the simplest model incorporating variables constructed by matching sample observations by ZIP code with characteristics measured in the 1990 Census, and some terms describing the sample design. The matched Census variables are available for almost all cases.[17] The variables selected for modeling here include the percent of non-whites in the neighborhood, the percent of residents older than age 65, the percent of adults who have at least some college education, the percent of adult males working, the percent of adult females working, the average household size, the median house value, the average commuting time, and the degree of

[17]The ZIP code information for AP cases is based on the actual sample address, but the code for list sample cases is taken from the original address from which their tax return was filed. Although tax filers are required by law to use their home address on their tax return, it is clear from interviewers' remarks that many list respondents were, in fact, interviewed elsewhere. Thus, neighborhood characteristics may be measured with error for such observations.

urbanicity of the neighborhood.[18] These variables reflect three effects: (1) the pure effects of neighborhood context, (2) indirect characteristics of respondents who choose to live in such areas, and (3) other unobserved characteristics of the respondent that may happen to be correlated with the variables. Some of these variables are included to allow for obvious demographic variation. The percent of workers, household size, and commuting time are intended largely to reflect characteristics related to the value of time. Such effects are also likely captured by the income and house value variables. To allow for some differences in the two samples, all the models shown also include dummy variables indicating whether an observation derived from the bottom three strata of the list sample or the higher strata of that sample.

In this model and those that follow, the cases included are those that had at least one contact and for which the variables in the models contain no missing data. The first line for each variable in the table shows the estimated marginal effect on the propensity to complete an interview compared to remaining unresolved, and the third line shows the effect on the propensity to refuse compared to remaining resolved. The second and fourth lines give standard errors for the coefficients above them. A "+" indicates that an estimate is significant at the 1 percent level, and a "*" indicates that it is significant at the 5 percent level.

The pure geographic effects are limited, but interesting. Respondents living in central areas of the largest cities are more likely to refuse than people living in non-urban (on average subjected to higher levels of stimuli?), but they are not different in their response propensity. Those in outside the central areas of the largest cities are not significantly different from those in non-urban areas. Respondents in other cities are less likely than those in non-urban areas to give a complete interview (smaller populations may make raise questions of privacy?), but are no different in their propensity to refuse.

Most of the neighborhood variables have strong effects: Cases in neighborhoods that are disproportionately white in their racial composition are more likely to be resolved overall, but refusals

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[18]Other variables, such as the median household income, are also available, but failed to account for significant additional variation in other exploratory modeling.

are the more likely outcome. Neighborhoods with greater concentrations of people over the age of 65 are less likely to give an interview (security issues or suspicion?), but are no different in their refusal propensities. Neighborhoods with higher proportions of college graduates are more likely to complete an interview, suggesting that respondents who are more educated may be more likely to understand and appreciate the purpose of the survey. Two of the variables expected to proxy for the value of the respondents' time have significant effects: neighborhoods with higher proportions of working males and neighborhoods where people have longer commuting times to work are less likely to complete interviews, though no they are different in terms of their refusal propensities. Consistent with earlier SCF findings of a wealth effect in nonresponse, cases in neighborhoods with higher housing values were significantly less likely to complete an interview. As expected, relative to AP cases the observations from the higher strata of the list sample are more likely to refuse and less likely to complete an interview; the cases from the lower strata are not significantly different from other cases in terms of their estimated hazards. Generally, these effects persist in the more complicated models below.

The hazard model offers a convenient way of including contact-varying characteristics. The model in the second column of table 3 adds a variable indicating whether the interviewer at a given contact is a different one from the one who started the interview, and variables intended to capture time effects, including the number of days elapsed from the first contact to the current contact, the total elapsed number of attempts (including contacts), and the elapsed number of contacts. Cases that have been taken over by a new interview are strongly more likely to be resolved overall, but such events are significantly more likely to be refusals; this outcome undoubtedly reflects the fact that most changes of interviewer take place when a case has already given a refusal just short of a final one and it is believed that a different interviewer might "convert" the case. Unsurprisingly, the greater the number of days a case has been "in play," the more likely it is to exit as a refusal and less likely as a complete case. The effect of "persistence" is shown in the coefficients on number of attempts: more attempts correlate with greater probability of exit in both states. Increasing numbers of contacts lessen the likelihood of exiting

as a refusal; this result could be taken to suggest that the personalization of the process over repeated contacts makes it harder for a respondent to make a firm refusal.[19]

The model in the third column of the table adds variables obtained from the questionnaire administered to the project interviewers. The values entered into the model are based on the responses of the interviewer who was working on each case at each contact. The variables included are selected from a much larger number through initial modeling with simpler estimation methods (e.g., probit models of overall response, or response given that a case was still at risk at a given contact).

Cases assigned to more experienced interviewers are more likely to resolve as refusals; this result likely reflects a tendency to assign more difficult cases to more experienced interviewers. Previous computer experience is associated with a higher completion propensity; perhaps such interviewers appear more "professional" to respondents. Cases administered by college educated interviewers do not differ significantly in their response hazards. Older interviewers are less likely to have refusals; this result accords with survey "folklore" that respondents find it harder to say "no" to older interviewers. However, the propensity for completing an interview is not significantly different for cases approached by older interviewers. Interviewers who are relatively confident that they can persuade reluctant respondents are actually less likely to obtain either final resolution, but refusals are relatively less likely than completions. Outgoing interviewers are more likely overall to resolve their cases. Interviewers who think of themselves as "hams" are significantly less likely to have refusals; this group may be particularly good at tailoring their remarks to deal with respondents' reservations. Those who favor a strategy to emphasize engagement with the respondent on the first contact do not have notably different outcomes. Interviewers who are relatively curious about other people are less likely to have lower completion rates. Curiously, interviewers who have relatively greater interest in the research are significantly more likely to have their cases resolve as refusals.

The fourth model adds variables based on data interviewers recorded about the respondents on the first contact and about respondents' neighborhoods on the first in-person attempt. Because, as

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[19]Alternatively, the result may simply reflect unobserved heterogeneity in the population modeled. See Allison [1995].

noted earlier, the missing data rate is very high for these variables, the model estimates should be taken as merely suggestive. Cases with barriers (either physical ones or gatekeepers) are not significantly different from other cases; perhaps barriers are more important in determining the possibility of contact at all. There is a counterintuitive lower propensity for cases in "rich" neighborhoods to refuse; because of the presence of the other economic controls, this may indirectly reflect characteristics of neighborhoods that have changed since the 1990 Census. Otherwise, the interviewers' perceptions of the relative prosperity of neighborhoods have little effect. Contrary to the customary presumption, male respondents appear less likely to refuse, though they are no different in their propensity to complete a case. Younger respondents tend to be less likely to achieve a final resolution of their interviews. Not surprisingly, single-person households were both more likely to refuse and less likely to complete an interview; security concerns are likely to be important for such cases. Respondents who asked informational questions or questions about possible incentives to participate do not appear to differ from other respondents. However, those who made negative comments at the time of the first contact were more likely to resolve as a refusals and less likely to resolve as a completed cases. Respondents who asked questions about the length of the interview were less likely to refuse, but those who made comments indicating that they wanted to delay the interview were less likely to resolve as completed cases.

The clearest problem in these models is the fact that cases are not randomly assigned to interviewers. Almost surely, there are also important dimensions of unobserved heterogeneity across the sample cases, though the expected effect of such omissions should be to bias coefficients toward zero.[20] However, there are also several noteworthy potential problems that entail the condition of "informative censoring," which occurs when censored cases would have been more likely to have exited in one state or another had they been contacted a sufficient number of additional times.

There are at least three mechanisms that might induce informative censoring in the SCF sample. First, unless a respondent refuses very strongly, he should be pursued until he does so or he agrees to

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[20]Estimated standard errors and significance tests are not affected by unobserved heterogeneity bias (Allison [1995]).

complete an interview. Given the tremendous pressures on interviewers to produce completed cases, it would be very surprising if they attempted to contact every case with equal vigor, particularly those they might have believed to have been more likely refusals. Second, during the field period, a concerted effort is made to avoid (to the degree feasible) large disparities in completion rates across PSUs, and there are fairly hard targets for numbers of cases in the various list sample strata. Although this balancing has some desirable effects (particularly on estimated variances), it may induce differences in effort since it is clear that cases are not equally difficult in all areas. Third, some respondents may make themselves hard to contact rather than have to deal with an interviewer, and such people may be more likely to have refused had they been contacted further.

Informative censoring may lead to complex biases, and there are no simple tests for bias. However, sensitivity tests excluding the censored cases from the modeling altogether suggest that informative censoring may not be a large problem. Moreover, the distribution of effort in figure 4 showing very similar patterns of effort across censored and fully resolved cases also offers some comfort.

Table 3: Discrete Time Hazard Models of Completion and Refusal, 1995 SCF

INTRCPT	1.53+	1.41+	1.59*	3.06+		.	.	0.03	0.10
	0.51	0.51	0.72	1.13	ITALK1	.	.	0.01	-0.01
	-5.21+	-3.82+	-0.55	-7.86*		.	.	0.02	0.02
	0.77	0.78	1.15	3.92		.	.	0.01	0.10
CCCMSA	0.05	0.04	-0.05	0.21		.	.	0.03	0.08
	0.06	0.06	0.07	0.11	BARR	.	.	.	0.04
	0.37+	0.23+	0.22*	0.17		.	.	.	0.11
	0.08	0.08	0.09	0.33		.	.	.	0.25
OCMSA	-0.09	-0.09	-0.06	-0.02		.	.	.	0.31
	0.05	0.05	0.06	0.09	RHRES	.	.	.	-0.08
	0.06	0.04	0.04	-0.03		.	.	.	0.11
	0.06	0.06	0.07	0.24		.	.	.	0.05
MSA	-0.40+	-0.42+	-0.38+	-0.48+		.	.	.	0.4
	0.05	0.05	0.06	0.08	POOR	.	.	.	0.18
	0.04	-0.26*	-0.16	-0.37		.	.	.	0.10
	0.1	0.11	0.12	0.30		.	.	.	0.00
PWHITE	0.37+	0.43+	0.33+	0.37		.	.	.	0.35
	0.12	0.12	0.13	0.20	RICH	.	.	.	-0.12
	0.54+	0.56+	0.80+	0.84		.	.	.	0.08
	0.17	0.19	0.21	0.66		.	.	.	-0.57*
PGT65	-2.39+	-2.54+	-2.44+	-2.14*		.	.	.	0.25
	0.54	0.54	0.61	1.00	MALE	.	.	.	0.02
	-0.23	-1.21	-0.85	0.70		.	.	.	0.06
	0.76	0.78	0.88	3.44		.	.	.	-0.43*
AHHSZ	0.13	0.14	0.19*	0.24		.	.	.	0.18
	0.08	0.08	0.08	0.14	ALE30	.	.	.	-0.33+
	0.11	0.02	0.07	0.35		.	.	.	0.09
	0.1	0.11	0.12	0.41		.	.	.	-0.85+
PCOLL	0.53*	0.51*	0.62*	1.22+		.	.	.	0.33
	0.25	0.25	0.27	0.41	A31_40	.	.	.	-0.31+
	-0.12	0.14	0.03	-0.67		.	.	.	0.08
	0.34	0.35	0.38	1.24		.	.	.	-0.25
PMWK	-1.22+	-1.41+	-1.83+	-1.31		.	.	.	0.25
	0.43	0.43	0.47	0.73	A41_50	.	.	.	-0.20+
	0.19	-0.84	-1.54*	-1.07		.	.	.	0.07
	0.65	0.66	0.72	2.51		.	.	.	-0.23
PFWK	0.66	0.76	0.92	-0.01		.	.	.	0.23
	0.43	0.44	0.47	0.70	ONEP	.	.	.	-0.77+
	0.30	1.00	1.50*	1.45		.	.	.	0.15
	0.61	0.64	0.69	2.34		.	.	.	1.14+
ATRAV	-0.21+	-0.22+	-0.19+	-0.02		.	.	.	0.23
	0.04	0.04	0.05	0.07	INFOQ	.	.	.	0.09
	0.09	0.01	0.07	0.11		.	.	.	0.06
	0.06	0.06	0.07	0.24		.	.	.	0.35
MHVAL	-0.22+	-0.21+	-0.22+	-0.26+		.	.	.	0.20
	0.04	0.04	0.05	0.08	TIMEQ	.	.	.	-0.10
	0.11	0.00	-0.02	0.26		.	.	.	0.06
	0.06	0.06	0.07	0.23		.	.	.	-0.56+
IEXP	.	.	-0.01	-0.01		.	.	.	0.19
	.	.	0.01	0.02	INCENQ	.	.	.	0.12
	.	.	0.04+	0.05		.	.	.	0.16
	.	.	0.01	0.05		.	.	.	-1.22
ICOMEX	.	.	0.09*	0.14*		.	.	.	0.75
	.	.	0.05	0.07	RNEG	.	.	.	-0.47+
	.	.	-0.05	0.00		.	.	.	0.07
	.	.	0.07	0.23		.	.	.	0.51+
ICOLL	.	.	-0.02	0.02		.	.	.	0.18
	.	.	0.06	0.08	RDELAY	.	.	.	-0.5+
	.	.	0.09	-0.30		.	.	.	0.06
	.	.	0.09	0.26		.	.	.	-0.05
IAGE	.	.	-0.01	-0.24		.	.	.	0.18
	.	.	0.10	0.16	DAYS	.	-0.03+	-0.03+	-0.01
	.	.	-0.82+	-0.40		.	0.01	0.01	0.01
	.	.	0.15	0.54		.	0.14+	0.14+	0.14+
ICONV	.	.	-0.05*	-0.03		.	0.01	0.01	0.02
	.	.	0.02	0.03	NATT	.	0.03+	0.03+	0.05+
	.	.	-0.28+	-0.20*		.	0.00	0.01	0.01
	.	.	0.03	0.10		.	0.02+	0.02*	0.04
IOUTGO	.	.	0.07*	-0.03		.	0.01	0.01	0.03
	.	.	0.03	0.04	NCON	.	0.01	0.01	0.00
	.	.	0.12+	0.24		.	0.01	0.01	0.01
	.	.	0.04	0.14		.	-0.06+	-0.04+	-0.01
ICURIO	.	.	-0.09+	-0.08*		.	0.01	0.01	0.04
	.	.	0.02	0.03	NEWI	.	0.18+	0.09	0.18*
	.	.	-0.02	-0.02		.	0.04	0.05	0.07
	.	.	0.03	0.09		.	1.09+	1.02+	0.86+
INEIGH	.	.	0.03	0.08		.	0.06	0.06	0.20
	.	.	0.03	0.04	LSSTGE4	-0.31+	-0.32+	-0.33+	-0.11
	.	.	-0.03	0.19		0.03	0.03	0.03	0.06
	.	.	0.04	0.13		0.10+	0.09*	0.11+	0.08
IRES	.	.	0.03	0.07		0.04	0.04	0.04	0.06
	.	.	0.04	0.06	LSSTLT4	0.04	0.10*	0.14+	0.31
	.	.	0.22+	-0.05		0.04	0.04	0.05	0.17
	.	.	0.07	0.28		-0.02	0.10	0.05	0.08
IHAM	.	.	-0.03	0.02		0.05	0.05	0.06	0.18
	.	.	0.02	0.03					
	.	.	-0.23+	0.19					

Variable definitions for table 3

INTRCPT: Model intercept.
CCCMSA: Dummy variable: R's residence in center city of a CMSA (1=included).
OCMSA: Dummy variable: R's residence is in a non-center-city CMSA (1=included).
MSA: Dummy variable: R's residence is in a MSA (excluding CMSAs) (1=included).
PWHITE: Fraction of residents of R's ZIP code who are white.
PGT65: Fraction of residents of R's ZIP code who are age 65 and older.
AHHSZ: Average number of people in households in R's ZIP code.
PCOLL: Fraction of adults in R's ZIP code with at least some college education.
PMWK: Fraction of adult males in R's ZIP code who are in the labor force.
PFWK: Fraction of adult females in R's ZIP code who are in the labor force.
ATRAV: Average number of minutes workers in R's ZIP code travel to get to work divided by 10.
MHVAL: Logarithm of the median dwelling value in R's ZIP code.
IEXP: Logarithm of years of interviewer's experience.
ICOMEX: Dummy variable: interviewer experienced with computers (1=experienced).
ICOLL: Dummy variable: interviewer has at least some college education (1=college).
IAGE: Logarithm of the age of the interviewer.
ICONV: Scale variable: interviewer believes every R can be converted with enough effort (1=strongly disagree, 5=strongly agree).
IOUTGO: Scale variable: interviewer considers self outgoing (1=strongly disagree, 5=strongly agree).
ICURIO: Scale variable: interviewer curious about other people and what they do (1=strongly disagree, 5=strongly agree).
INEIGH: Scale variable: interviewer enjoys challenge of unfamiliar neighborhoods (1=strongly disagree, 5=strongly agree).
IRES: Scale variable: interviewer likes being part of a research project (1=strongly disagree, 5=strongly agree).
IHAM: Scale variable: interviewer thinks of self as a bit of a "ham" (1=strongly disagree, 5=strongly agree).
ITALK1: Scale variable: interviewer believes it is better on the first contact to keep a conversation going rather than press for a quick decision (1=strongly disagree, 5=strongly agree).
BARR: Dummy variable: barriers (including physical barriers and gatekeepers) to contacting R (1=barriers).
RHRES: Dummy variable: by observation, R's neighborhood mostly residential (1=residential).
POOR: Dummy variable: by observation, R's neighborhood is poor (1=poor).
RICH: Dummy variable: by observation, R's neighborhood is rich (1=rich).
MALE: Dummy variable: R for listing was male (1=male).
ALE30: Dummy variable: R for listing was aged 30 or younger (1=<=30).
A31_40: Dummy variable: R for listing was aged 31 to 40 (1=31 to 40)
A41_50: Dummy variable: R for listing was aged 41 to 50 (1=41 to 50).
ONEP: Dummy variable for R lives alone (1=alone).
INFOQ: Dummy variable for R asked for information about the survey as the first contact (1=asked).
TIMEQ: Dummy variable: R asked about the length of the interview at the first contact (1=asked).
INCENQ: Dummy variable: at the first contact, R asked about the possibility of monetary incentives (1=asked).
RNEG: Dummy variable: at the first contact, R made negative comments about the survey (1=made comments).
RDELAY: Dummy variable: at the first contact, R made comments to delay interview (1=made comments).
NOREF: Dummy variable: at the first contact, R did not refuse to do interview on first contact (1=did not refuse).
DAYS: Number of days elapsed since first attempt on case, divided by 10.
NATT: Number of attempts made on case including current contact.
NCON: Number of contacts made on case including current contact.
NEWI: Dummy variable: interviewer changed since case originally fielded. (1=changed)
LSSTGE4: Dummy variable: case in list sample strata 4 or higher (1=included).
LSSTLT4: Dummy variable: case in list sample strata less than 4 (1=included).

III. Investigation of Some Costs of Unit Nonresponse

Contact-Level Variation in Net Worth

Although there is fairly strong evidence of systematic variation in unit nonresponse across the sample, it is very difficult to look at a set of coefficients and integrate though to a sense of the overall effects on a variety of estimates based on the set of final complete cases. Unfortunately, by the nature of the phenomenon, we cannot directly estimates the costs of unit nonresponse. Traditionally, one argument that is given for pursuing cases through many contacts is that the "difficult" cases are the ones that are most like the cases that are not eventually interviewed (e.g., see Holt and Eliot [1991]). If we take this conjecture at face value, we can use some information from the sample cases to draw inferences about the nonrespondents.

Although there are many important potential variables one might examine for bias, a key variable in the SCF is net worth. To get a sense of the variation in wealth data collected at each level of contact, figures 7 and 8 plot some key statistics of the distribution of net worth for cases completed from the two samples at different numbers of contacts.[21] For each contact, the central line in the figures give the median net worth, the shaded area indicates the inter-quartile range, and the outer lines show in the minimum and maximum values.[22] For the AP sample, the median and the interquartile range vary little, though one might argue that the distances of the extreme values from the median decline somewhat. For the list sample, between six and eleven contacts, the interquartile range bulges slightly; overall the median varies a little and the extreme values appear to move toward the center.

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[21]The plots are truncated at 13 contacts because the sample becomes too thin after that point to make reasonable estimates. Plots showing gross assets instead of net worth, and plots arraying the data by number of attempts rather than number of contacts, are extremely similar.

[22]To minimize the graphical weight of very large values, while preserving the sign of the data, the values plotted have been subjected to a transformation using the inverse hyperbolic sine with a scale parameter of 0.0001 (see Burbidge, Magee, and Rob [1988]).

Figure 7: Net Worth by Number of Contacts to Completion, AP Sample; Median, Inter-quartile Range, Minimum, and Maximum

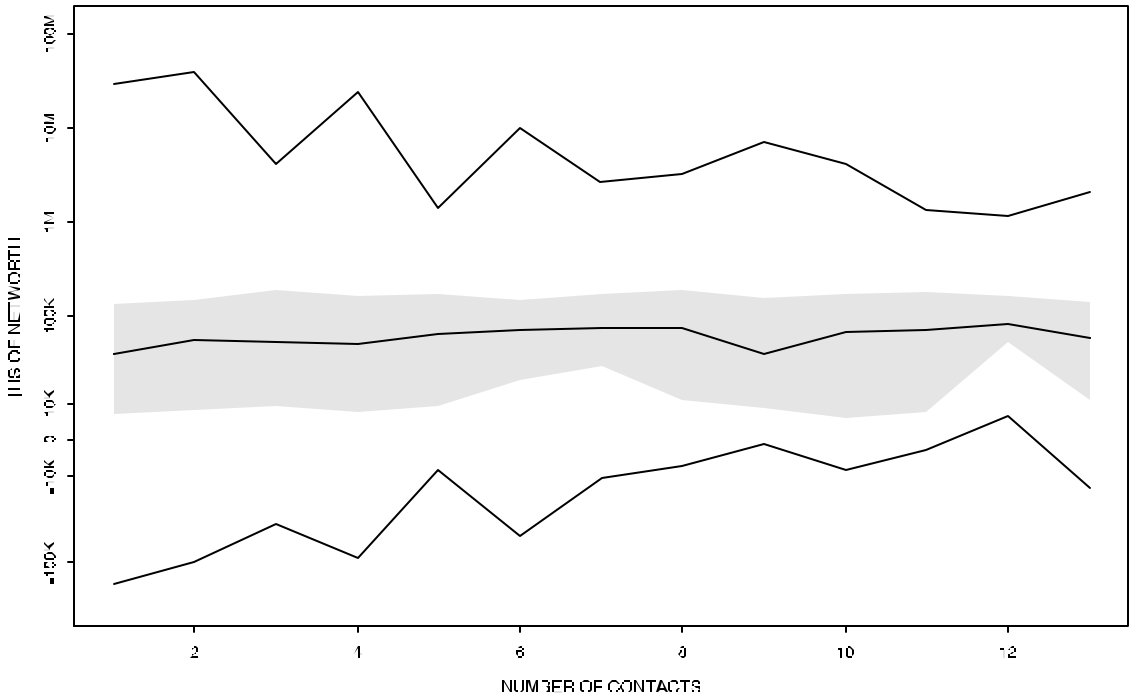
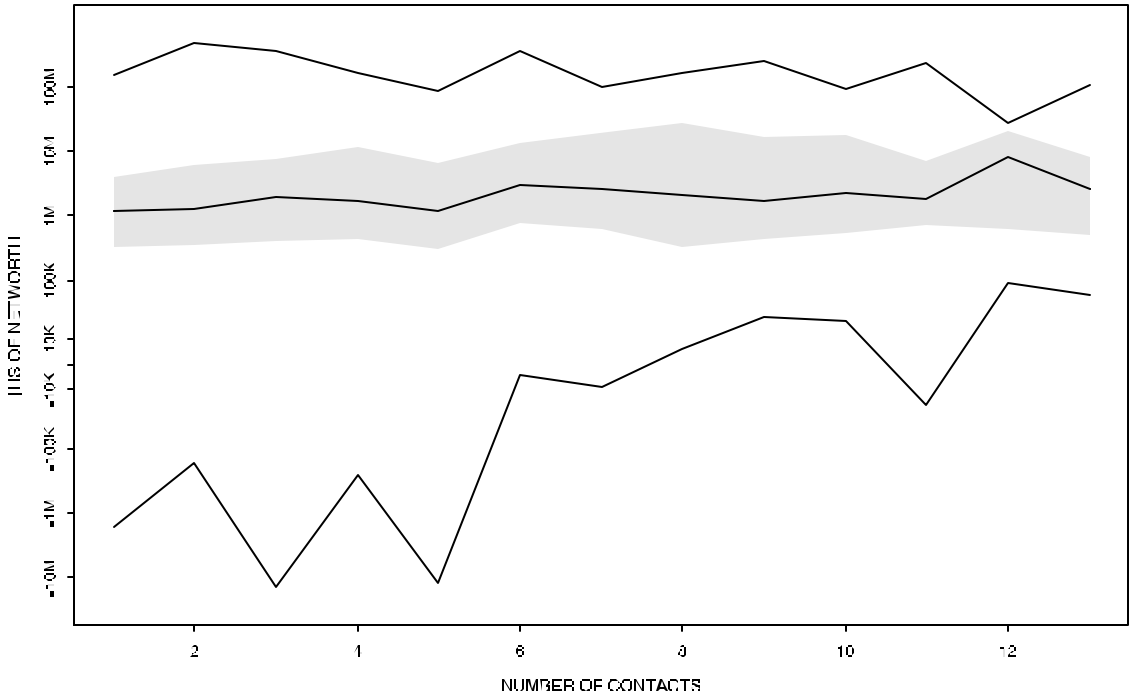


Figure 8: Net Worth by Number of Contacts to Completion, List Sample; Median, Inter-quartile Range, Minimum, and Maximum



Shifts in the Net Worth Distribution Over Contacts

To press the question further, I designed the following experiment. Artificial samples of respondents were generated by deleting cases with numbers of contacts exceeding various levels, and for each such sample, the nonresponse-adjusted weights were recomputed using only data from the survey and the frames for the samples.[23] Given these samples and weights, the distribution of household net worth was estimated. Figures 9-14 show quantile-quantile (Q-Q) plots of the distribution of net worth from the full sample against net worth from each of the reweighted subsamples deleting cases with more than eight contacts, five contacts, four contacts, three contacts, two contacts, and one contact, respectively.[24] Of the full sample of 4,299 completed cases, about 330 required more than 8 contacts, 910 more than 5 contacts, 1,280 more than 4 contacts, 1,820 more than 3 contacts, 2,570 more than 2 contacts, and 3,580 more than one contact. Deleting cases with more than 8 contacts has very little effect on the estimated distribution. Deleting cases with more than 5 or 4 contacts causes remarkably little additional distortion. When cases having more than three contacts are deleted, the estimated distribution begins to deviate a little more noticeably from the full sample estimate. However, even when the comparison is made between the full sample and those that were interviewed on the first contact, it is surprising how well the distributions match outside of the tails.

Given the nature of the information available in the list sample frame for use in nonresponse adjustments to the pooled sample weights, one might well wonder how this result would carry over to other surveys where such information is not available. To illuminate this point, figures 16-21 show corresponding plots for the area-probability sample alone, where the weights have been adjusted for nonresponse using only simple PSU-level ratio adjustments, and some post-stratification based on age, home ownership, and the regional distribution of households with control totals computed using data

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[23]Implicitly, some information from the full sample appears in the truncated samples because the data were imputed using information from the full sample (see Kennickell [1991]). It was much too expensive an exercise to create separately imputed datasets for each experiment.

[24]Again, the data are transformed using the inverse hyperbolic sine. The vertical lines in the plot denote (reading left to right) the 90th percentile of the distributions, the 99th percentile, and the 99.5th percentile.

from the Current Population Survey. Two points are worth noting. First, it is clear that the estimates of the percentile breaks shown are lower when the estimates are made for the AP sample alone; however, this results turns almost entirely on the difference between the AP and list samples, not the experimental truncations. Second, the deterioration of the correspondence of the full sample with the truncated distributions across the experiments is comparable to that for the full sample.

Figure 9: Q-Q Plot of Net Worth, Full Sample vs. Full Sample with 8 or Fewer Contacts

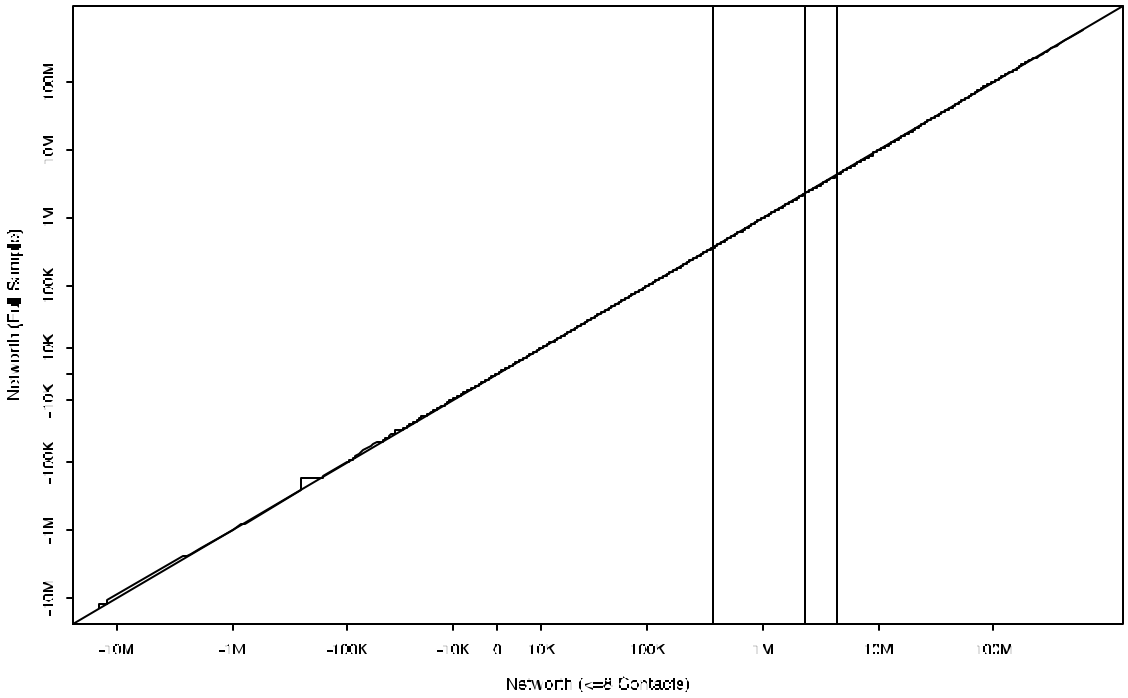


Figure 10: Q-Q Plot of Net Worth, Full Sample vs. Full Sample with 5 or Fewer Contacts

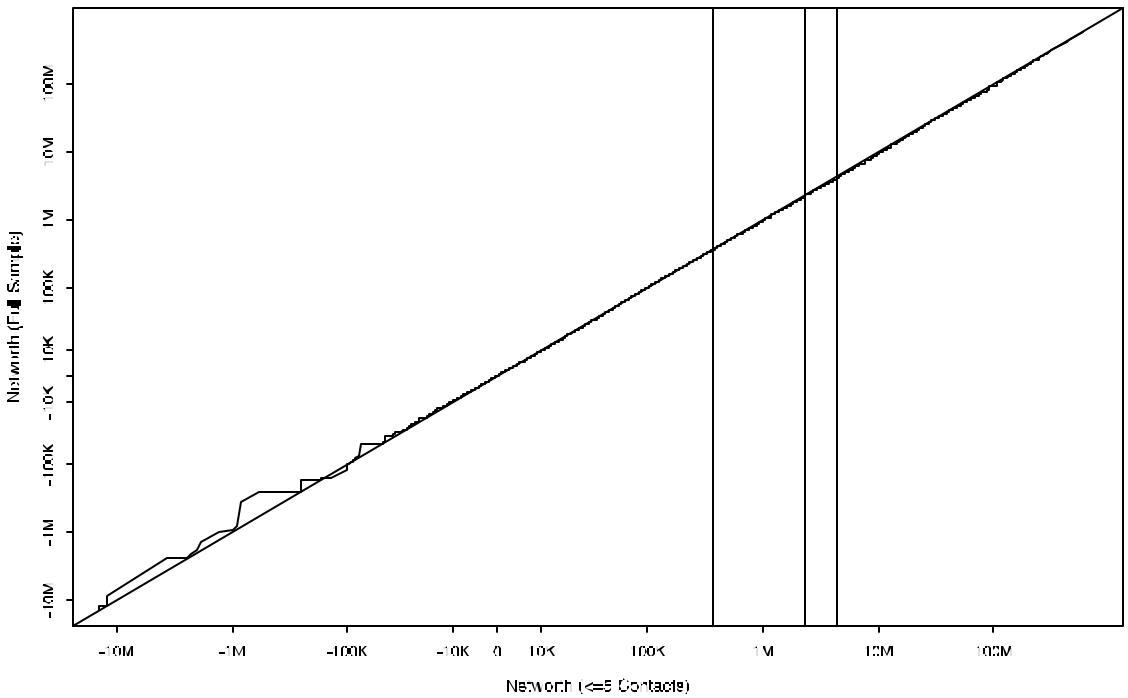


Figure 11: Q-Q Plot of Net Worth, Full Sample vs. Full Sample with 4 or Fewer Contacts

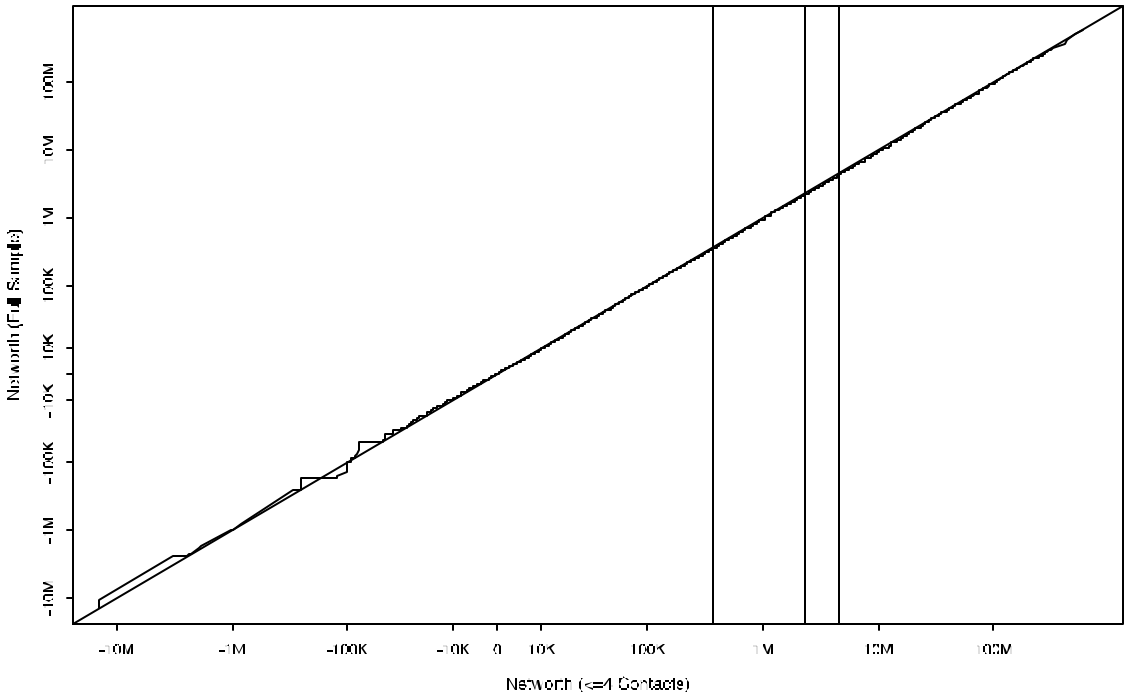


Figure 12: Q-Q Plot of Net Worth, Full Sample vs. Full Sample with 3 or Fewer Contacts

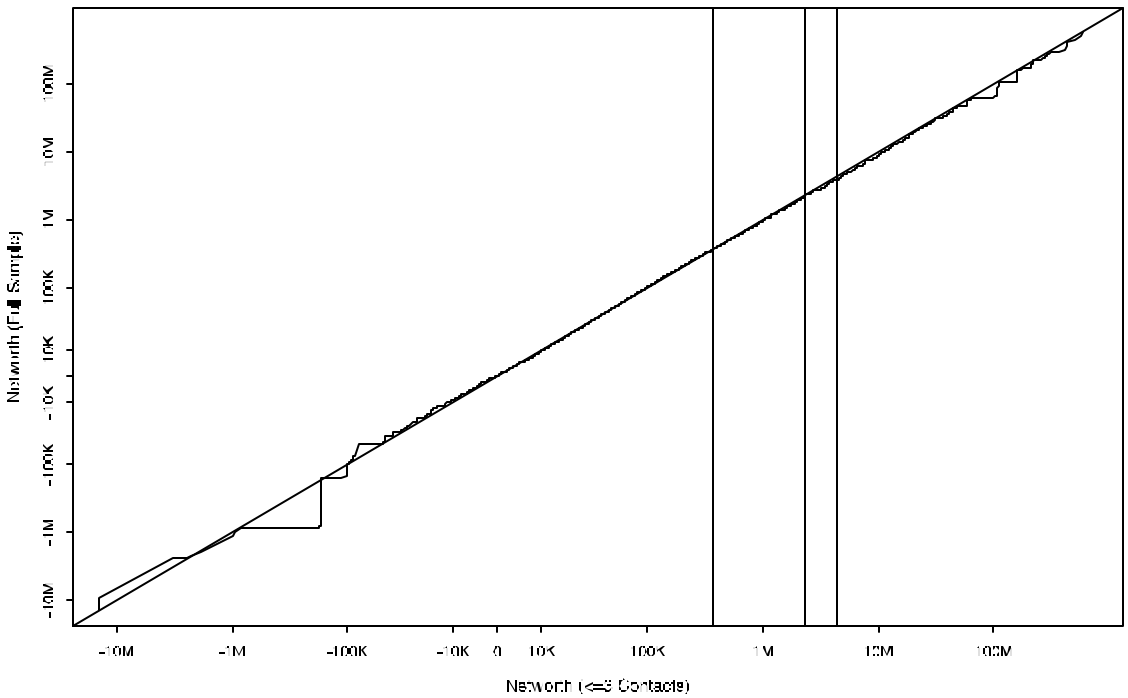


Figure 13: Q-Q Plot of Net Worth, Full Sample vs. Full Sample with 2 or Fewer Contacts

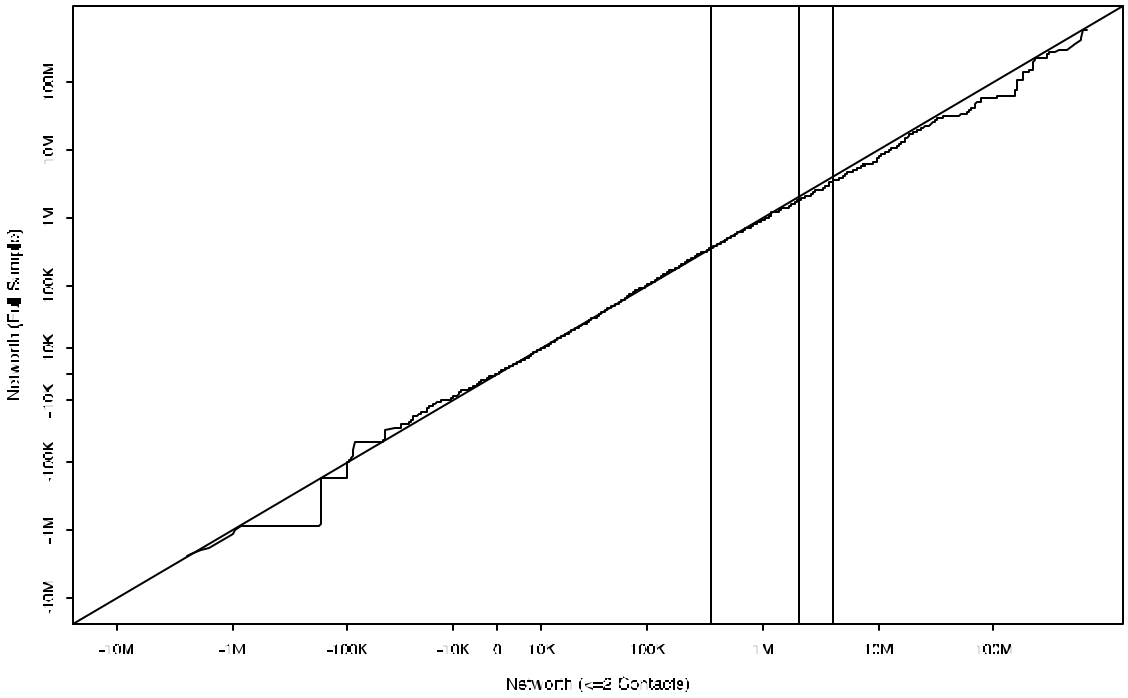


Figure 14: Q-Q Plot of Net Worth, Full Sample vs. Full Sample with Only 1 Contact

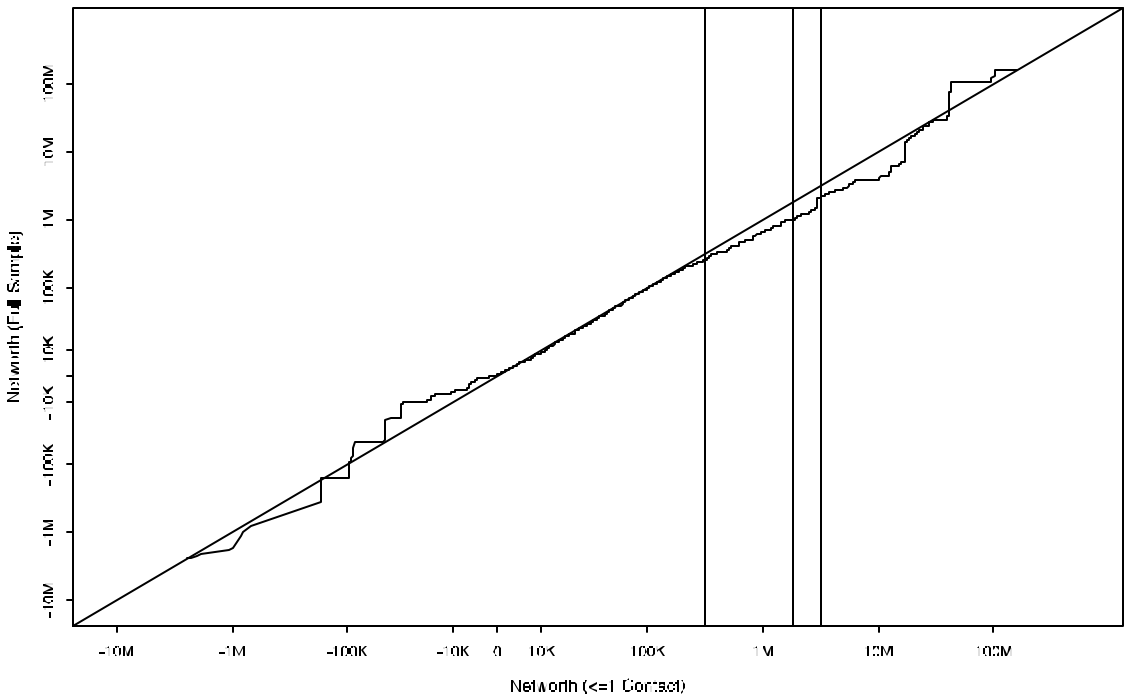


Figure 15: Q-Q Plot of Net Worth, Full AP Sample vs. AP Sample with 8 or Fewer Contacts

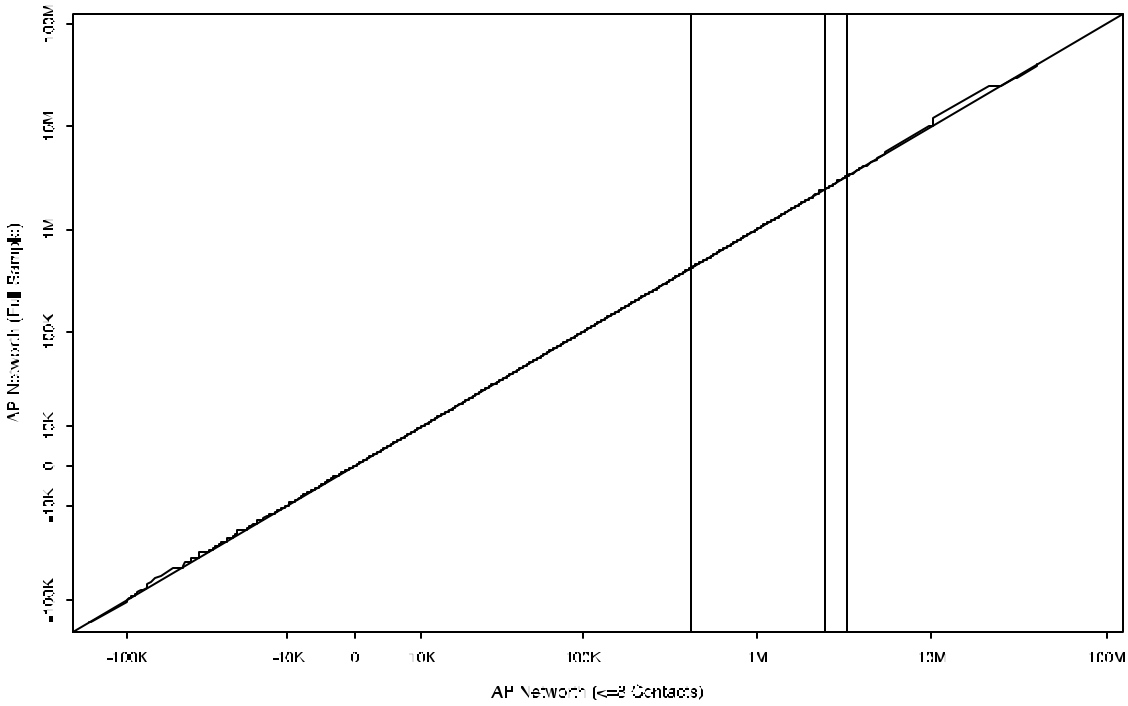


Figure 16: Q-Q Plot of Net Worth, Full AP Sample vs. AP Sample with 5 or Fewer Contacts

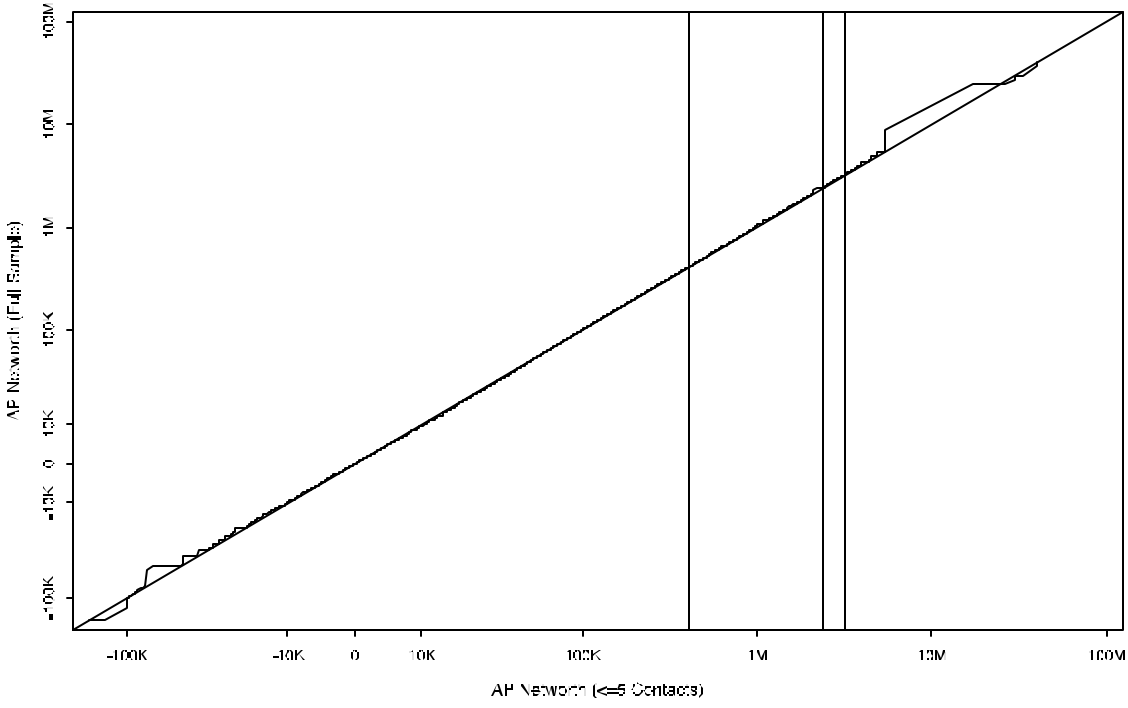


Figure 17: Q-Q Plot of Net Worth, Full AP Sample vs. AP Sample with 4 or Fewer Contacts

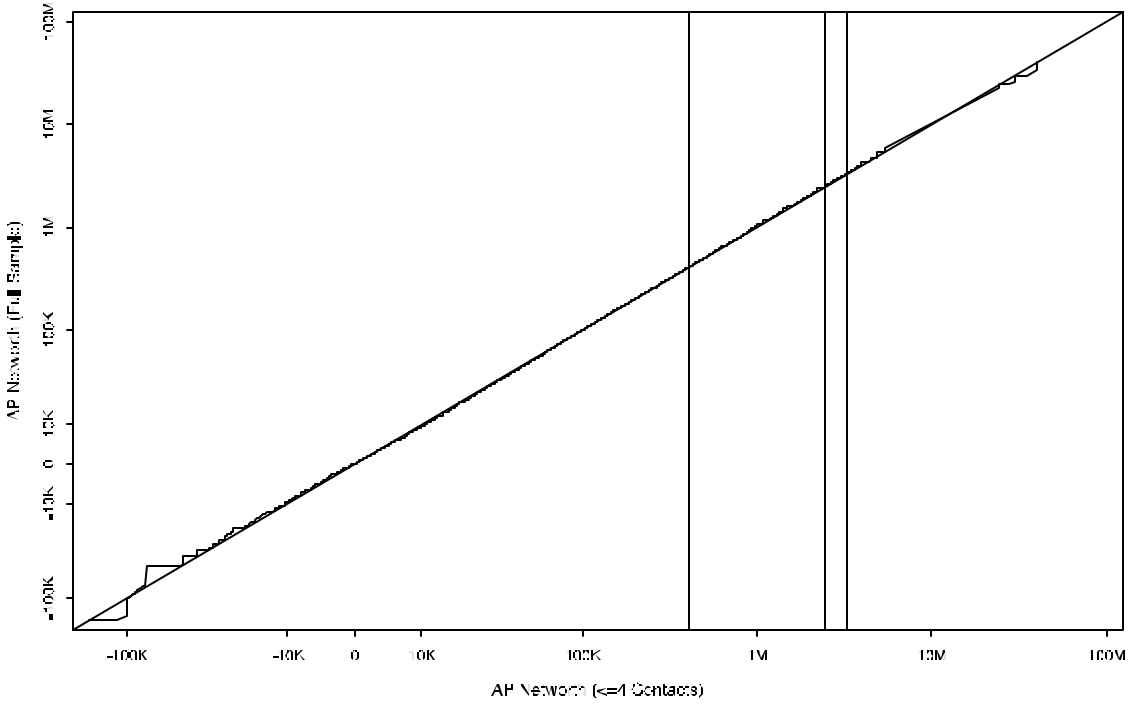


Figure 18: Q-Q Plot of Net Worth, Full AP Sample vs. AP Sample with 3 or Fewer Contacts

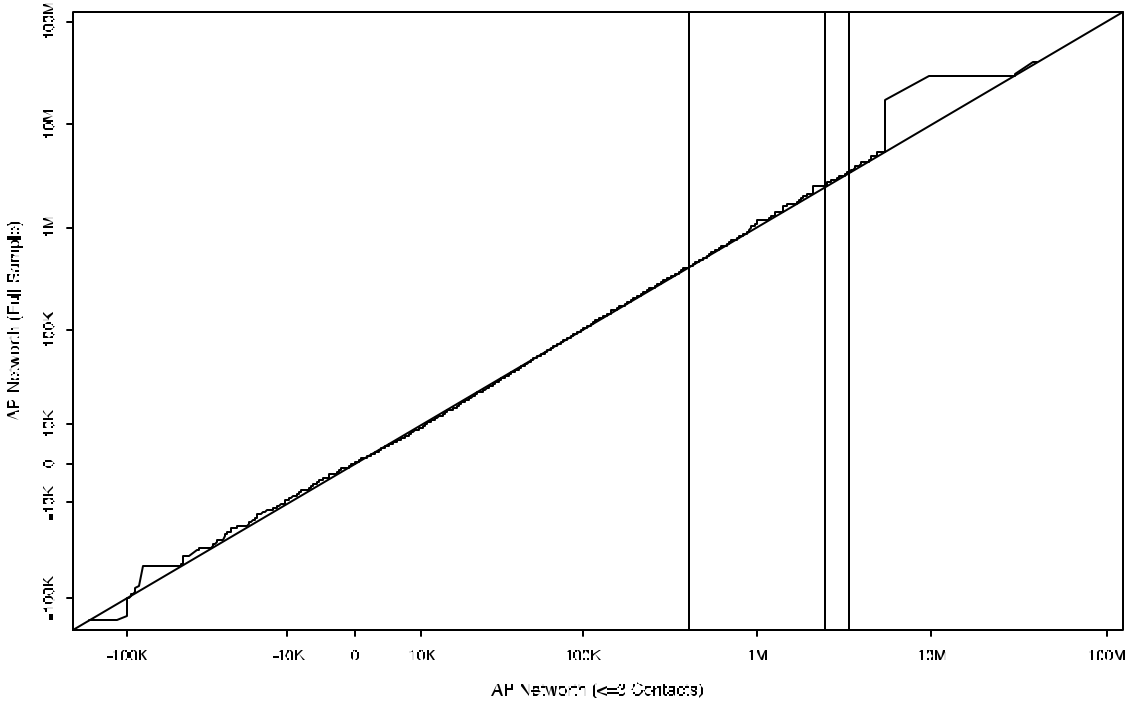


Figure 19: Q-Q Plot of Net Worth, Full AP Sample vs. AP Sample with 2 or Fewer Contacts

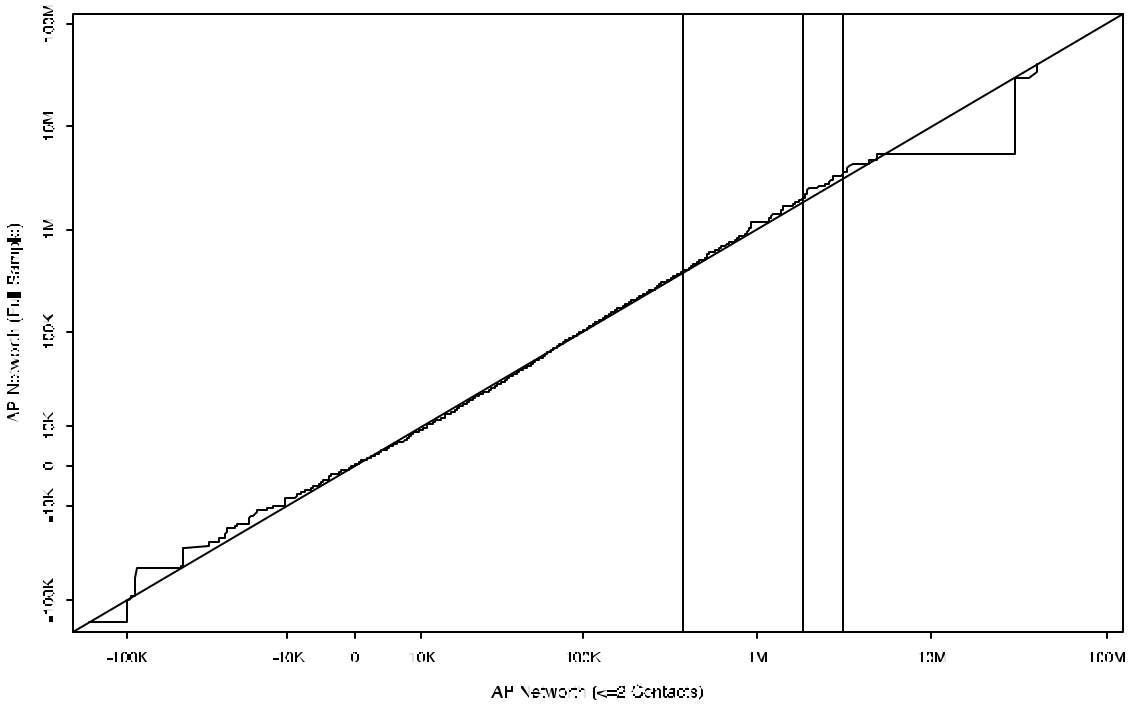


Figure 20: Q-Q Plot of Net Worth, Full AP Sample vs. AP Sample with Only 1 Contact

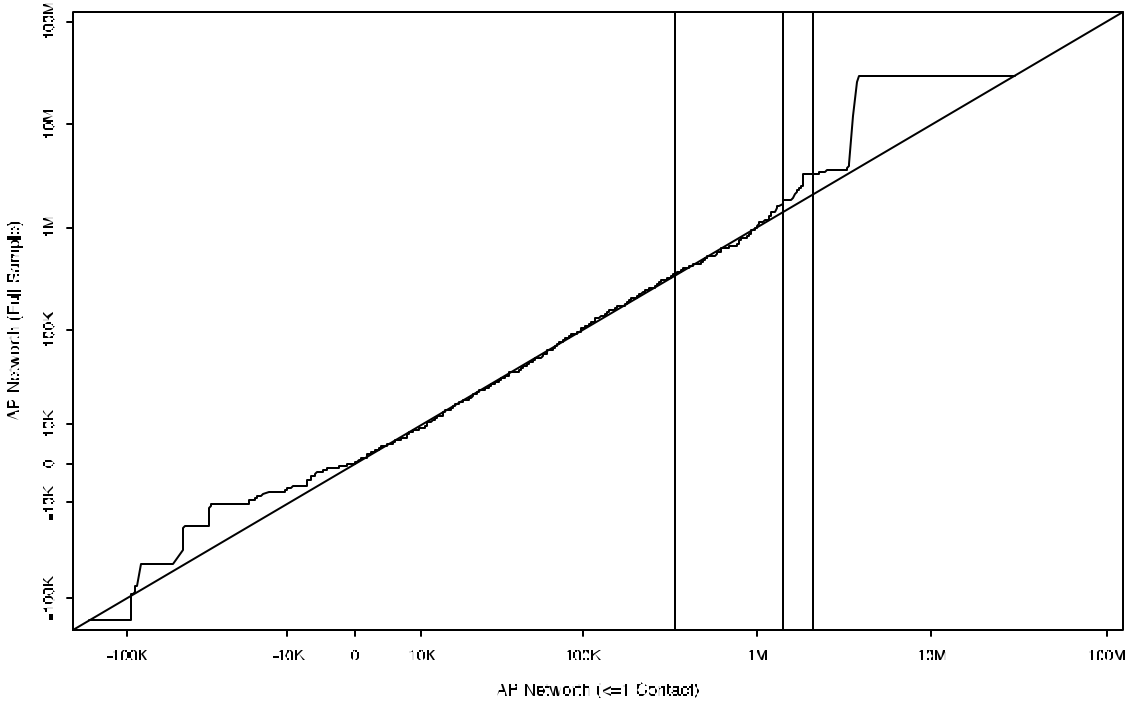


Figure 21: Q-Q Plot of Net Worth, Cases with more than 3 Contacts vs. Cases with 3 or Fewer Contacts, AP and List Samples Pooled

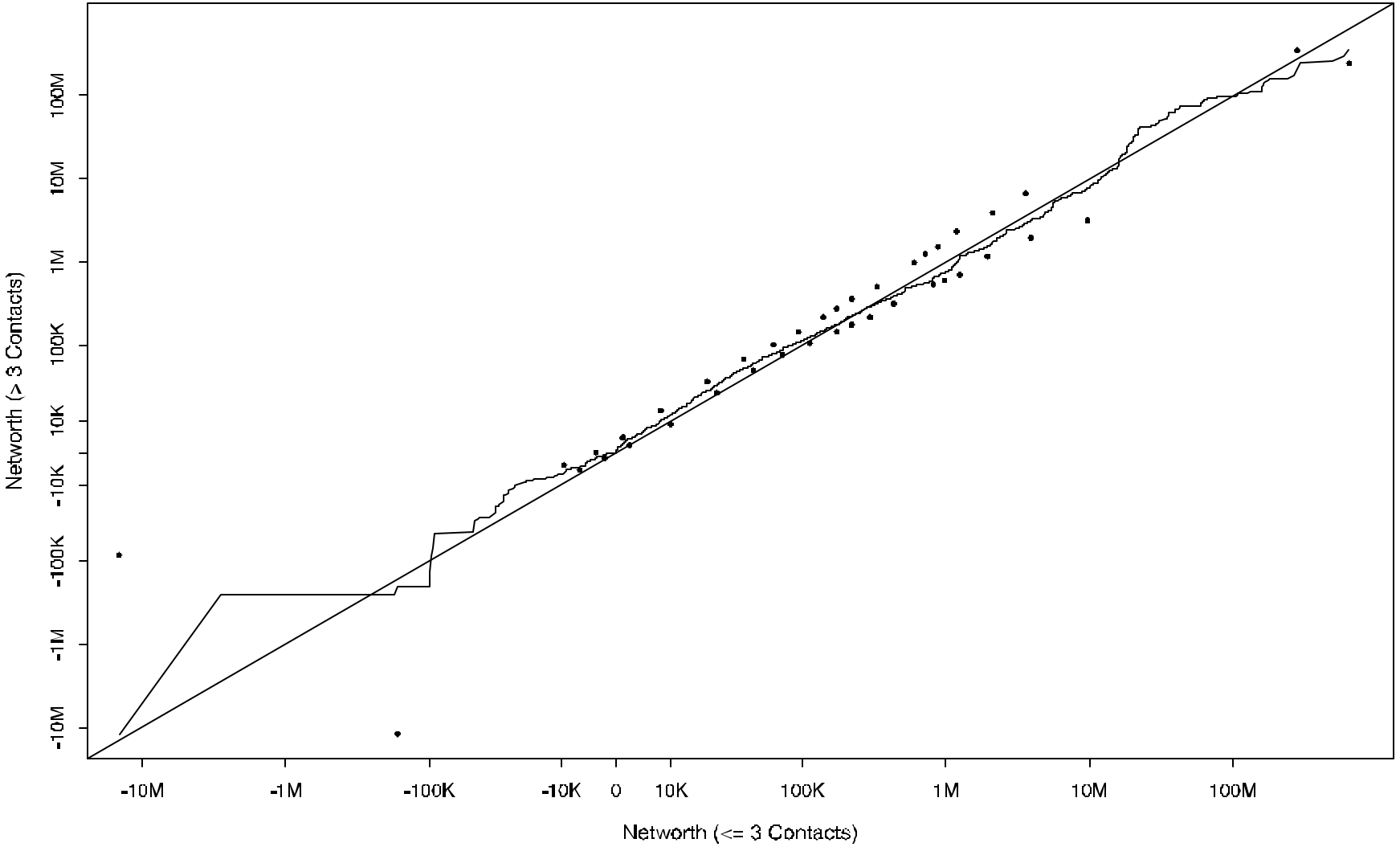
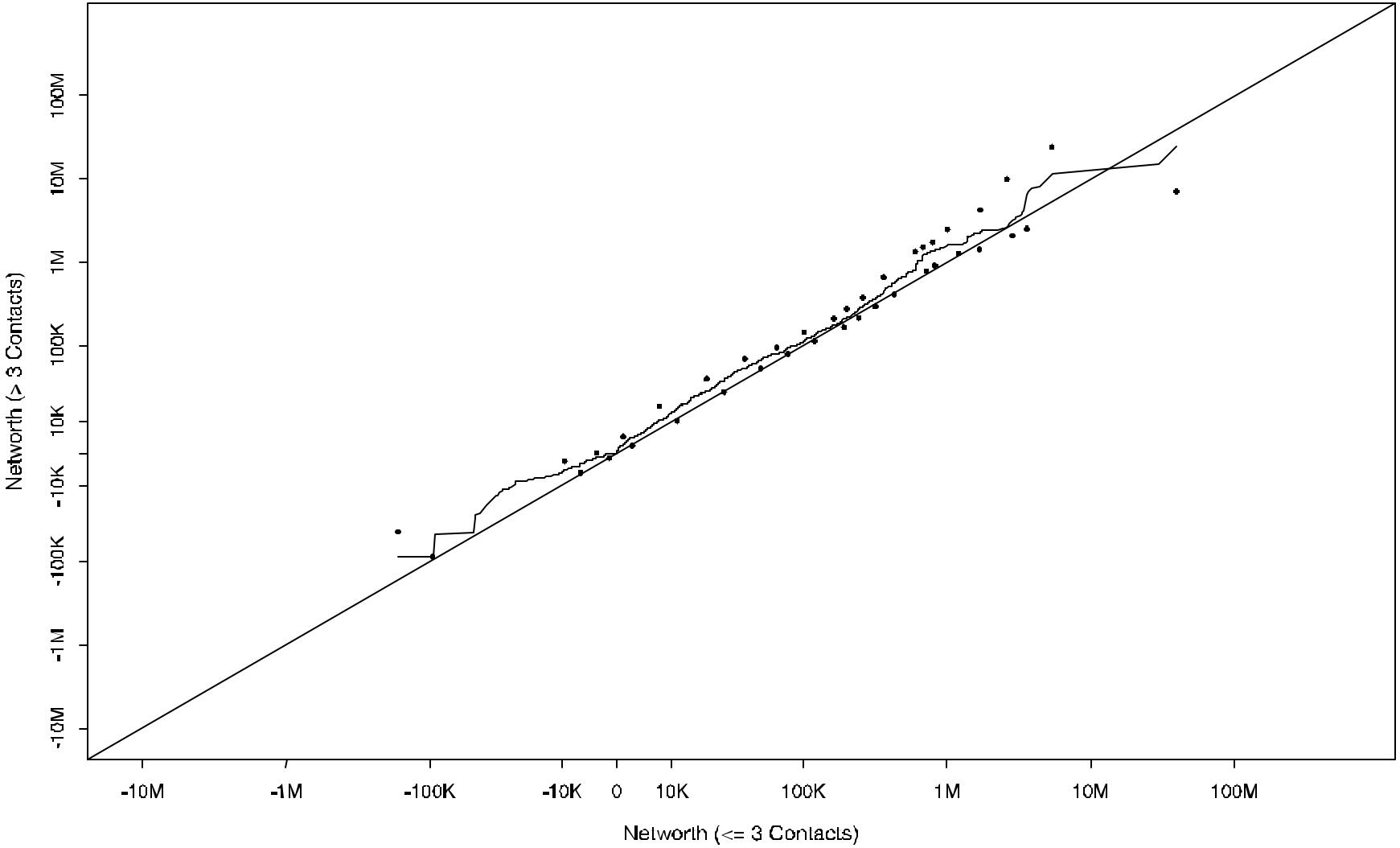


Figure 22: Q-Q Plot of Net Worth, Cases with More than 3 Contacts vs. Cases with 3 or Fewer Contacts, AP Sample Only



Because the truncated samples are also a part of the reference sample in all these Q-Q plot comparisons, the effects of differences in the different contact groups may appear understated. Focusing more directly on the truncated sample and its complement would highlight the differences btween the two groups, but sample size differences are extreme in some cases. Figure 22 compares the net worth of completed cases requiring more than three contacts with that of the complementary set of completed cases. Because the median number of contacts is three, here the numbers of cases in the two groups are roughly balanced. Weights were constructed separately for both the groups and a full set of replicate weights was computed for use in estimating confidence intervals.[25]

Clearly, the information in the figure is the same as in figure 12, but the differences are visually amplified. The distribution of wealth for the cases with more than three contacts lies a bit above that for the complementary group in the range below about $100,000, and for most of the range above about $10 million. To gauge the importance of these differences, the dotted lines show the boundaries of an estimate of the pointwise 95 percent confidence interval around the central Q-Q plot.[26] To my knowledge, this is the first instance of a confidence interval in a Q-Q plot. The results underscore the impression generated by the earlier Q-Q plots: The bounds clearly encompass the 45 degree line over

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[25]See Kennickell and Woodburn [1997] for a description of the replicate weight procedure used for variance estimation in the SCF.

[26]Because of the complexity of this calculation some simplifications are invoked in computing the confidence intervals. The Q-Q plot shows the wealth points corresponding to the same percentile points of the wealth distributions referenced on the two axes. For this figure, the 95 percent confidence bounds are computed as pointwise bounds in wealth-wealth space for a selection of percentile points. At each selected percentile of the net worth distribution (0, 2.5, 5, 15, 25, 35, 45, 55, 65, 75, 8-, 85, 90, 95, 96, 97, 98, 99, 99.5, 100), a distribution of wealth values associated with that point over the sample replicates (see Kennickell and Woodburn [1997]) is computed. This estimate is made separately for the populations on the two axes of the Q-Q plot. Given the independence of the two samples, the coordinates of the upper bound of the confidence interval for a selected percentile point are given by the wealth value corresponding to the 97.5th percentile of the distribution of wealth estimates at that point for the population on the vertical axis, and the value associated with the 2.5th percentile of the wealth estimates at that point for the population on the horizontal axis. The lower bound is defined similarly.

the range of the distribution above $100,000, and the 45 degree line is on the edge of the confidence interval below that point.

Figure 23 provides the corresponding information for the AP sample alone. In this case, virtually the entire distribution for the group with more than three contacts lies above the distribution for the complementary set of cases, suggesting that the later cases in the AP sample were broadly more wealthy than the early cases. However, the 45 degree line is contained in the 95 percent confidence interval for most of the range of the distribution. Nonetheless, it is at least visually suggestive that the lower bound of the interval straddles the 45 degree line.

Although these results are highly suggestive, for several reasons they cannot be definitive. We do not know the characteristics of the true nonrespondents, only those of the "late" respondents. One might also argue this type of pointwise confidence interval is not the most appropriate choice: The path outlined by the points of the confidence intervals may not correspond to any actual Q-Q plot.[27] Further, variations over contacts for variables other than net worth may be quite different. Nonetheless, the results suggest we should at least think carefully about expending very large efforts to secure extremely difficult cases.

Even if we could take these results literally, if we applied them in a strict way — say, by designing an optimal cost-variance-bias tradeoff — we would almost certainly induce new problems. Had interviewers been told about a protocol involving a ceiling on the number of contacts or attempts, it is likely their behavior would have changed from what we observe in the 1995 data. Some interviewers might have been "too careful" budgeting the number of attempts on difficult cases so as not to risk losing the case; others might have moved to fill the requirement with relatively empty gestures for particularly difficult cases. Some degree of monitoring would be very important to ensure consistency. In the past, the costs of monitoring at this level of detail would have been prohibitive. Perhaps automation of case control records at the interviewer level will allow a more systematic treatment of effort.

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[27]However, it is difficult to define a clear alternative because the underlying replicated Q-Q plots intersect and there is not a simple criterion to rank order the individual plots to determine a confidence interval.

Variations in Item Nonresponse Over Contacts

While the estimated distribution of net worth does not vary dramatically with the number of contacts required to complete the cases, it could still be that there is a larger fraction of missing information in the cases that required more contacts and that imputation is driving the distributional

Table 4: Distribution of Values Reported as Complete, Range Responses, Don't Know, Refused, or Blank, by Number of Contacts

Variable	*Type of response:*					*Memo item:*
Num. contacts	*Complete*	*Range*	*DK*	*Refuse*	*Blank*	*Inapplicable*
Credit card balances						
1-3	93.1	5.3	0.1	1.2	0.3	27.1
4-7	92.9	4.0	0.0	2.2	0.9	19.7
>=8	94.1	2.9	0.0	2.9	0.0	22.5
House value						
1-3	89.0	9.3	0.0	1.8	0.0	36.0
4-7	88.9	9.4	0.0	1.7	0.0	28.0
>=8	88.9	8.7	0.0	2.4	0.0	27.0
Mortgage payment						
1-3	91.8	5.3	0.1	2.3	0.5	61.5
4-7	92.6	4.4	0.0	2.5	0.6	52.3
>=8	90.9	2.1	0.0	4.8	2.1	52.8
Business value						
1-3	59.8	27.5	0.2	11.6	1.0	76.0
4-7	60.9	24.4	0.2	13.0	1.5	69.5
>=8	66.9	16.2	0.0	14.7	2.2	65.7
Checking account balance						
1-3	80.2	13.3	0.0	6.2	0.3	12.4
4-7	79.3	11.9	0.0	8.2	0.5	8.9
>=8	78.3	12.5	0.0	8.9	0.3	9.1
Stock mutual fund balance						
1-3	58.8	19.9	0.5	13.3	7.5	82.9
4-7	56.5	19.5	0.3	13.3	10.4	79.3
>=8	58.6	17.2	0.0	8.1	16.2	75.0
Head's salary/wages						
1-3	82.5	11.6	0.1	5.1	0.7	30.9
4-7	79.0	12.1	0.0	7.9	1.0	19.7
>=8	79.0	10.7	0.0	8.5	1.9	19.4
Total income						
1-3	70.7	18.2	0.0	11.0	0.1	0.0
4-7	67.0	18.2	0.1	14.7	0.0	0.0
>=8	64.7	16.9	0.0	18.4	0.0	0.0

Memo items:
Percent of completed cases with various numbers of contacts:
1 to 3 contacts: 56.1; 4 to 7 contacts: 34.7; 8 or more contacts: 9.2
Total number of complete cases: 4,299

results. If later cases have higher fractions of missing data and the imputation models are estimated disproportionately using data from earlier cases, then imputation might tend to even out net worth over contacts.

As noted earlier, it is not possible in this paper to deal completely with the role of imputation. However, it is straightforward to examine patterns of missing data. Table 4 presents information for a set of variables on the proportion applicable values that were complete answers, range responses, "don't know" (DK) responses, refusals, and blanks (indicating that there was a missing value for some higher-level variable that determined whether the question was applicable).[28] The variables included — balances on bank-type credit cards (Visa, Mastercard and Discover), value of most types of owned residences, amount of regular mortgage payments, amount in the main checking account, holdings of stock mutual funds, the wages and salary earnings of the head of the household, total income — are intended to span a wide set of potential response problems. The data apparently exhibit no consistent variation in the proportion of complete responses over contacts. However, the fraction of refusals generally rises, largely at the expense of a decline in the proportion of range responses. More detailed investigation shows no strong differences in the AP and list samples.

Interestingly, the fraction of responses indicating that the question did not apply to the respondent ("inapplicable") declines for all the variables except total income (which is always applicable) after 3 contacts, suggesting that the relatively small shifts in the overall distribution of wealth over contacts may mask an increase in the complexity of the interviews. The fact the decline in the fraction of inapplicable values does not change notably in the group of cases completed after 7 contacts suggests that a cutoff at a level of, say, 8 on the allowed number of contacts would avoid most information loss. However, other evidence suggests caution. More complex interviews should require more time, and convincing a respondent to give sufficient time for an interview might require more contacts.[29] Figures 23 and 24 provide information on the length of completed cases by the number of

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[28]See Kennickell [1996] for a discussion of item nonresponse in the 1995 SCF and the use of range responses.

[29]Interview time could also increase if cases that required large numbers of contacts were also cases that were completed in many sessions. Although the administrative data in the HEF indicate that

Figure 23: Length of Interview in Minutes by Number of Contacts to Completion, AP Sample; Median and Inter-quartlie Range

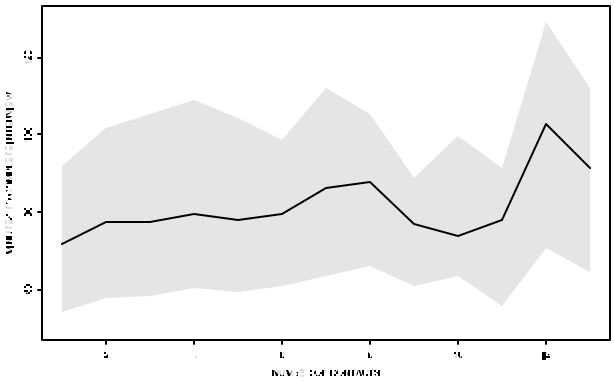
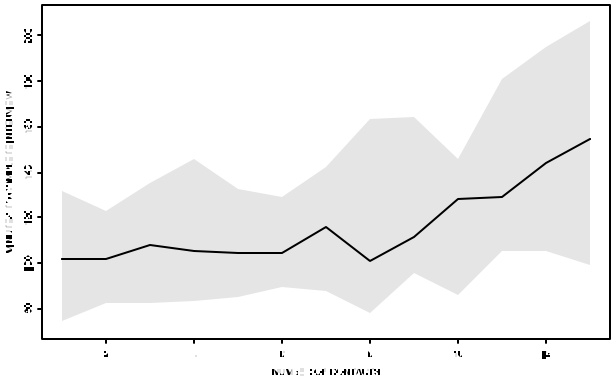


Figure 24: Length of Interview in Minutes by Number of Contacts to Completion, List Sample; Median and Inter-quartile Range



contacts required to complete AP and list cases, respectively. For the list sample, the median interview length (the dark central line in the plot) rises strongly with contacts; the trend shown in figure 24 for the AP sample is less strong and actually reverses between about 9 and 11 contacts. The inter-quartile ranges (indicated by the shaded areas) moves in about the same way as the medians. The lengthening of interviews even after 8 contacts suggests that other aspects of complexity may be missed in the patterns of the inapplicable responses shown in table 4. These item response issues deserve further investigation than is possible in this paper.

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only 257 cases required more than one session to complete, the figure could be understated because of noise in the event-level disposition codes at this level of detail.

IV. Summary and Future Research

The discrete time hazard model developed in this paper suggests that there are previously undetected dimensions of differential nonresponse in the SCF. At the least, the distinct response patterns at the level of characteristics from Census data suggest that the design of nonresponse adjustments should consider variation across sampled areas in factors such as house values, commuting time, and the proportion of older people.

The results confirm the intuitive proposition that more effort leads to a greater likelihood that a case is resolved. Perhaps more importantly, the data suggest that the greater personalization of the relationship that comes with repeated contacts between respondents and interviewers lessens the probability that a case will refuse.

Older interviewers are less likely to obtain outright refusals, though they are no different in their propensity to gain complete interviews. Similarly, interviewers who view themselves as being somewhat like an actor are less likely to obtain refusals. Some other interviewer effects are paradoxical. For example, experience as an interviewer seems to increase the likelihood that a case will resolve as a refusal. This result may reflect the assignment of more difficult cases to such interviewers.

Although there are substantial problems of missing data at the level of information about the initial contact with the respondent, there are some interesting findings. Barriers to entry, such as doormen or locked gates, do not appear to have a direct effect on the resolution of a case, perhaps because these obstacles make contact of any sort difficult. Contrary to normal survey folklore, male respondents are less likely to refuse participation. Among statements made by respondents on the initial contact, two sorts seem to have a persistent effect over future contacts. Those who made negative comments were, in fact, more likely to refuse. Those who made comments to delay the interview were less likely to resolve as either a refusal or a complete.

Partially in response to the data problems encountered in this paper, the collection of the ancillary data has been redesigned in electronic form, and the use of parts of the information collected for administrative purposes has dramatically raised the incentives to record correct and complete data.

Because of the growing importance of nonresponse, I hope to continue this line of research with the new data. I also hope that others will conduct similar work with other surveys to explore the generality of the findings.

The data suggest that the gains from devoting substantial resources to pursuing the completion of difficult cases through many contacts may have been overstated in the past. Nonetheless, we are still far from an "optimal rule." Based on the body of completed cases, there are not strong indications of differences in the distribution of net worth for cases completed after a large number of contacts compared with those requiring fewer contacts. However, the data also show signs that the cases that required more contacts were more complex cases. Future work should consider further the relationships between contacts and variables other than net worth. A related topic is the effects of variations in item response patterns over contacts on final estimates; one approach might be to work with reweighted subsamples as was done in the Q-Q plots presented in this paper.

A point outside the general discussion of this paper deserves emphasis. I believe strongly that to improve response on surveys (or even to maintain current levels), we must account for the humanity of both respondents and interviewers. Respondents are not filing cabinets to be rifled at will, but people who face conflicting demands on their time. It is generous of respondents to share their time with survey takers, and this fact should never be forgotten or taken for granted. Interviewers are paid for their work. Nonetheless, in almost every area of work, other factors than money appear to be important determinants of superior performance. It is a wasted opportunity when survey managers fail to engage interviewers' interest beyond the level of pure production. If interviewers fail to communicate a compelling vision of a survey and a deep respect for respondents' generosity, response rates will suffer. We are fortunate in having been so successful with the dedicated SCF interviewers.

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APPENDIX:
LOGIT MODELS OF NONRESPONSE AT THE CONTACT LEVEL

Table A1: Logit Model of Response by Contact with Census Variables, 1995 SCF

		Contact number:			
	Overall	*1*	*2*	*3*	*4*
INTRCPT	6.95+	3.90+	3.54+	1.88	3.41*
	0.73	1.25	1.06	1.20	1.40
CCCMSA	-0.24+	0.15	0.23	0.03	-0.06
	0.08	0.16	0.13	0.15	0.18
OCMSA	-0.15*	-0.05	-0.02	-0.20	-0.12
	0.07	0.15	0.11	0.12	0.14
MSA	-0.51+	-0.32+	-0.48+	-0.52+	-0.16
	0.09	0.12	0.11	0.13	0.16
PWHITE	-0.01	0.30	0.50*	0.24	0.78*
	0.17	0.28	0.24	0.28	0.34
PGT65	-1.93+	-0.92	-1.80	-1.92	-3.86*
	0.74	1.33	1.17	1.28	1.52
AHHSZ	0.21*	-0.04	0.18	0.20	0.17
	0.11	0.19	0.16	0.18	0.22
PCOLL	0.95+	0.25	-0.17	0.41	1.15
	0.34	0.63	0.52	0.59	0.69
PMWK	-0.41	-1.90	-2.46+	-2.04*	-1.52
	0.63	1.01	0.88	1.01	1.20
PFWK	-0.13	1.39	2.03*	1.24	-0.10
	0.62	1.09	0.92	1.03	1.21
ATRAV	-7.57+	-1.09	-6.66+	-3.73	-2.19
	1.42	2.37	1.96	2.28	2.82
MHVAL	-0.32+	-0.43+	-0.22*	-0.14	-0.34+
	0.06	0.12	0.09	0.10	0.12
LSSTGE4	-0.97+	-1.00+	-0.68+	-0.36+	-0.37+
	0.06	0.14	0.10	0.11	0.13
LSSTLT4	-0.41+	-0.21	-0.01	0.01	-0.10
	0.08	0.14	0.11	0.13	0.15
N_CASES	7524	7339	6405	4935	3666

Table A2: Logit Model of Response by Contact with Census Variables and Interviewer Variables, 1995 SCF

		Contact number:						*Contact number:*				
	Overall	*1*	*2*	*3*	*4*		*Overall*	*1*	*2*	*3*	*4*
INTRCPT	4.82+	1.59	3.05*	-2.09	0.86	ICOMEX	-0.08	-0.17	0.03	0.31+	0.25
	1.02	1.73	1.45	1.68	1.99		0.07	0.12	0.10	0.12	0.13
CCCMSA	-0.25+	0.03	0.15	0.11	-0.07	ICOLL	-0.10	-0.25	-0.22*	-0.12	-0.36*
	0.09	0.18	0.14	0.16	0.19		0.08	0.13	0.11	0.14	0.15
OCMSA	-0.20+	0.01	0.05	-0.20	-0.10	IAGE	0.32*	0.30	0.19	0.61+	0.33
	0.08	0.16	0.12	0.13	0.15		0.14	0.26	0.21	0.23	0.28
MSA	-0.49+	-0.12	-0.52+	-0.59+	-0.11	ICONV	0.02	0.01	-0.01	-0.03	-0.06
	0.1	0.13	0.11	0.14	0.18		0.03	0.05	0.04	0.05	0.06
PWHITE	-0.10	0.23	0.33	0.22	0.76*	IOUTGO	0.10*	0.09	-0.02	0.06	0.04
	0.19	0.30	0.26	0.31	0.38		0.04	0.07	0.06	0.07	0.08
PGT65	-2.17+	-0.83	-1.99	-0.90	-4.44+	ICURIO	-0.07	-0.23+	0.02	0.04	-0.10
	0.81	1.43	1.26	1.37	1.64		0.03	0.05	0.05	0.06	0.06
AHHSZ	0.10	0.03	0.20	0.21	0.12	INEIGH	0.04	0.15*	0.09	-0.06	0.06
	0.12	0.21	0.18	0.20	0.24		0.04	0.07	0.06	0.07	0.08
PCOLL	0.76*	0.14	0.09	0.06	0.75	IRES	0.03	0.01	-0.12	0.00	0.04
	0.37	0.68	0.57	0.63	0.75		0.06	0.09	0.08	0.09	0.11
PMWK	-0.10	-1.73	-2.49+	-2.32*	-1.82	IHAM	0.00	0.00	-0.02	0.03	0.06
	0.68	1.08	0.94	1.10	1.29		0.03	0.05	0.04	0.04	0.05
PFWK	-0.44	0.66	1.71	1.77	-0.34	ITALK1	0.03	0.07	0.05	0.03	0.08
	0.67	1.15	0.98	1.11	1.30		0.02	0.04	0.03	0.04	0.05
ATRAV	-6.46+	-0.37	-5.49+	-2.75	-0.88	LSSTGE4	-0.98+	-0.97+	-0.73+	-0.38+	-0.30*
	1.55	2.58	2.10	2.45	3.03		0.07	0.15	0.11	0.12	0.14
MHVAL	-0.27+	-0.35+	-0.23*	-0.07	-0.24	LSSTLT4	-0.36+	-0.23	0.07	0.06	-0.13
	0.07	0.13	0.10	0.11	0.13		0.09	0.15	0.12	0.14	0.17
IEXP	0.01	0.00	0.00	0.02	0.01						
	0.01	0.02	0.02	0.02	0.03	N_CASES	6443	6289	5463	4174	3070

Table A3: Logit Model of Response by Contact with Census Variables, Interviewer Variables, and Contact-Level Variables, 1995 SCF

	Overall	*Contact number: 1*	*2*	*3*	*4*
INTRCPT	14.04+	3.72	6.82+	-0.99	6.66*
	3.27	2.78	2.43	2.71	3.39
CCCMSA	0.00	0.22	0.20	0.55*	0.91+
	0.28	0.27	0.23	0.26	0.34
OCMSA	-0.15	0.12	0.34	-0.10	-0.30
	0.21	0.24	0.19	0.21	0.26
MSA	-0.46	0.03	-0.44+	-0.69+	-0.52
	0.26	0.19	0.17	0.2	0.27
PWHITE	-0.47	0.40	0.73	0.63	1.21
	0.54	0.48	0.42	0.47	0.63
PGT65	-6.30*	-2.45	-2.18	-4.04	-2.80
	2.68	2.43	2.09	2.40	2.98
AHHSZ	-0.38	0.43	-0.15	0.09	0.33
	0.37	0.34	0.30	0.33	0.43
PCOLL	1.94	0.00	0.69	-0.53	2.75*
	1.09	1.08	0.89	1.01	1.27
PMWK	0.36	-1.47	-1.94	-3.32	0.26
	2.15	1.70	1.52	1.73	2.25
PFWK	-2.39	0.13	0.52	-0.18	-4.17
	1.98	1.67	1.45	1.65	2.14
ATRAV	-4.79	-3.21	-1.38	0.03	4.94
	4.53	3.81	3.23	3.71	5.04
MHVAL	-0.43*	-0.34	-0.23	0.13	-0.49*
	0.20	0.20	0.16	0.18	0.23
IEXP	-0.07	-0.03	0.04	0.03	0.06
	0.04	0.04	0.03	0.04	0.04
ICOMEX	0.19	-0.22	-0.07	0.18	0.42
	0.19	0.18	0.15	0.18	0.22
ICOLL	0.10	-0.10	-0.09	0.24	-0.18
	0.22	0.20	0.16	0.21	0.24
IAGE	-0.64	-0.05	-0.60	0.06	-0.71
	0.44	0.40	0.34	0.36	0.44
ICONV	-0.16	0.02	0.05	-0.10	-0.24*
	0.09	0.08	0.07	0.08	0.09
IOUTGO	-0.08	0.08	-0.17*	0.05	-0.13
	0.11	0.10	0.08	0.10	0.12
ICURIO	-0.09	-0.23+	-0.06	0.10	-0.06
	0.09	0.07	0.07	0.08	0.09
INEIGH	0.13	0.04	0.17	-0.20	0.04
	0.11	0.11	0.10	0.11	0.13
IRES	0.21	0.06	-0.03	0.09	0.04
	0.15	0.14	0.12	0.15	0.17
IHAM	0.11	-0.02	-0.03	0.12	0.16*
	0.07	0.07	0.06	0.07	0.08
ITALK1	0.00	0.08	0.01	0.14*	0.11
	0.07	0.07	0.05	0.06	0.07
BARR	0.07	0.15	0.18	-0.42	-0.17
	0.3	0.30	0.23	0.28	0.32
RHRES	-0.21	0.06	-0.18	0.37	-0.27
	0.35	0.28	0.24	0.31	0.35
POOR	0.23	0.39	0.16	0.38	0.07
	0.29	0.21	0.19	0.23	0.31
RICH	0.21	0.09	-0.31	-0.28	-0.64*
	0.23	0.23	0.19	0.21	0.25
MALE	0.21	0.08	-0.13	0.16	0.23
	0.15	0.14	0.12	0.14	0.17
ALE30	0.15	-0.58+	-0.51+	-0.27	-0.35
	0.26	0.21	0.18	0.21	0.27
A31_40	-0.20	-0.81+	-0.36*	-0.41*	-0.06
	0.21	0.20	0.16	0.19	0.23
A41_50	-0.18	-0.55+	-0.42+	-0.09	0.09
	0.19	0.19	0.16	0.18	0.21
ONEP	-2.26+	-0.77	-0.87*	-0.87*	-1.33+
	0.23	0.41	0.34	0.36	0.49
INFOQ	0.03	-0.17	0.13	0.07	-0.05
	0.17	0.15	0.13	0.15	0.18
TIMEQ	0.34*	-0.27	0.14	0.07	-0.02
	0.17	0.16	0.13	0.15	0.18
INCENQ	0.73	-0.10	0.51	0.80*	0.12
	0.62	0.46	0.33	0.38	0.55
RNEG	-1.06+	-0.56+	-0.81+	-0.52+	-0.82+
	0.16	0.20	0.16	0.16	0.20
RDELAY	-0.51+	-1.10+	-1.18+	-0.44+	-0.49+
	0.16	0.22	0.16	0.15	0.18
LSSTGE 4	-0.49*	-0.47	-0.32	0.22	0.61*
	0.24	0.26	0.21	0.22	0.27
LSSTLT4	-0.22	0.08	0.19	-0.10	0.45
	0.24	0.22	0.19	0.23	0.26
N_CASES	2111	2109	1830	1340	951

Table A4: Logit Model of Response by Contact with Census Variables, Interviewer Variables, and Contact-Level Variables, 1995 SCF

		Contact number:		
	Overall	*2*	*3*	*4*
INTRCPT	13.82+	6.24*	-1.00	6.75*
	3.47	2.44	2.72	3.40
CCCMSA	-0.01	0.21	0.55*	0.90+
	0.29	0.23	0.26	0.34
OCMSA	-0.08	0.35	-0.10	-0.30
	0.22	0.19	0.21	0.26
MSA	-0.61*	-0.45+	-0.69+	-0.52
	0.28	0.17	0.2	0.27
PWHITE	-0.45	0.71	0.63	1.20
	0.56	0.42	0.47	0.63
PGT65	-6.79*	-2.08	-4.04	-2.85
	2.83	2.09	2.4	2.98
AHHSZ	-0.58	-0.14	0.09	0.32
	0.39	0.30	0.33	0.43
PCOLL	1.84	0.58	-0.53	2.75*
	1.13	0.89	1.01	1.27
PMWK	0.92	-1.92	-3.32	0.26
	2.23	1.52	1.73	2.25
PFWK	-3.00	0.55	-0.18	-4.18*
	2.07	1.45	1.65	2.13
ATRAV	-4.46	-1.61	0.03	4.96
	4.70	3.25	3.71	5.04
MHVAL	-0.43*	-0.21	0.13	-0.48*
	0.21	0.16	0.18	0.23
IEXP	-0.06	0.04	0.03	0.06
	0.05	0.03	0.04	0.04
ICOMEX	0.21	-0.08	0.18	0.42
	0.20	0.15	0.18	0.22
ICOLL	0.10	-0.11	0.24	-0.18
	0.23	0.17	0.21	0.24
IAGE	-0.63	-0.59	0.06	-0.72
	0.46	0.34	0.36	0.44
ICONV	-0.17	0.06	-0.10	-0.24*
	0.10	0.07	0.08	0.09
IOUTGO	-0.12	-0.18*	0.05	-0.12
	0.12	0.08	0.10	0.12
ICURIO	-0.03	-0.06	0.10	-0.06
	0.09	0.07	0.08	0.10
INEIGH	0.14	0.17	-0.20	0.04
	0.12	0.1	0.11	0.13
IRES	0.17	-0.04	0.09	0.04
	0.15	0.12	0.15	0.17
IHAM	0.12	-0.02	0.12	0.16*
	0.08	0.06	0.07	0.08
ITALK1	0.01	0.00	0.14*	0.11
	0.07	0.05	0.06	0.07
BARR	0.08	0.18	-0.42	-0.17
	0.31	0.23	0.28	0.32
RHRES	-0.06	-0.15	0.37	-0.27
	0.35	0.24	0.31	0.35
POOR	0.29	0.15	0.38	0.07
	0.32	0.19	0.23	0.31
RICH	0.17	-0.31	-0.28	-0.64*
	0.23	0.19	0.21	0.25
MALE	0.20	-0.13	0.16	0.23
	0.16	0.12	0.14	0.17
ALE30	0.17	-0.5+	-0.27	-0.35
	0.28	0.19	0.21	0.27
A31_40	-0.2	-0.36*	-0.41*	-0.06
	0.22	0.16	0.19	0.23
A41_50	-0.24	-0.42+	-0.09	0.09
	0.20	0.16	0.18	0.21
ONEP	-2.13+	-0.87*	-0.87*	-1.33+
	0.25	0.34	0.36	0.49
INFOQ	0.02	0.13	0.07	-0.05
	0.18	0.13	0.15	0.18
TIMEQ	0.33	0.13	0.07	-0.02
	0.17	0.13	0.15	0.18
INCENQ	1.14	0.51	0.80*	0.13
	0.74	0.33	0.38	0.55
RNEG	-0.99+	-0.75+	-0.52+	-0.83+
	0.17	0.17	0.17	0.20
RDELAY	-0.45+	-1.19+	-0.44+	-0.49+
	0.17	0.16	0.15	0.18
NOREF	0.54*	0.47*	0.01	-0.09
	0.21	0.21	0.21	0.25
LSSTGE4	-0.51*	-0.34	0.22	0.62*
	0.25	0.21	0.22	0.27
LSSTLT4	-0.33	0.18	-0.10	0.46
	0.25	0.19	0.23	0.26
N_CASES	2111	1830	1340	951