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THE HIDDEN 25 PERCENT:

AN ANALYSIS OF NONRESPONSE ON THE 1980 GENERAL SOCIAL SURVEY

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GSS Technical Report No. 25

April, 1981

This research was done for the General Social Survey Project directed by James A. Davis. The project is supported by the National Science Foundation, Grant No. SOC77-03279.

Nonresponse can seriously bias survey estimates and distort inferences. The relationship of nonresponse to survey estimates is simple and well-defined, but the actual impact is ill known. For the sample mean of a particular variable (Y) the association is

$$\bar{Y} = W_1\bar{Y}_1 + W_2\bar{Y}_2 \tag{1}$$

where W_1 and W_2 are the proportion respondents and nonrespondents. The relative bias (RB) of using the response mean to equal the sample means is

$$RB(\bar{Y}_1) = W_2 \frac{(\bar{Y}_1 - \bar{Y}_2)}{\bar{Y}} \tag{2}$$

We can see that the relative bias is serious only when the nonresponse rate (W_2)is large and the difference in the means is great. Given this simple formula we can easily measure the magnitude of the nonresponse bias. The problem is that while we know the nonresponse rate we do not know the nonresponse mean for the simple reason that because of nonresponse we have no measure of Y among nonrespondents. Because of this it is typically difficult to measure the bias caused by nonresponse. We could circumvent this problem if the nonresponse was small. With a small nonresponse rate we could simply state that nonresponse bias could not be appreciable or, if we wish to be more rigorous, we could calculate maximum biases given the largest possible difference in the means (Cochran, 1963 and Kish, 1965). Unfortunately for all but the smallest nonresponse rate such estimates of relative bias are so large as to be meaningless. The typical nongovernmental, face-to-face, full probability survey of a random adult in a household has a nonresponse rate of .25 (Smith, 1977; Davis, Smith, and Stephenson, 1980; and Groves and Kahn, 1979). Given a $\overline{Y}_1$ estimate of the proportion female as .5 the maximum and minimum estimate of $\overline{Y}$ would be .375-.625 (if $\overline{Y}_2 = 0$ or 1.0). Such a range, which comes on top of sampling variance, is too wide for most useful purposes. (In addition the stipulation of the maximum bias tells little about the actual or even probable bias.) In brief, nonresponse bias is a potentially serious problem, but without some estimate of the nonresponse sample it is impossible to assess its impact or to make meaningful adjustments for the bias. As a result, we need procedures to estimate the characteristics of the nonrespondents.

Two alternatives are usually presented in discussing nonresponse, how to minimize nonresponse and how to estimate and correct for differences between the respondents and nonrespondents. In this paper we ignore the first alternative, accepting that a nonresponse rate of .25 is typical for good, state-of-the-art surveys. Instead we will review the various existing approaches to studying nonresponse bias and then examine nonresponse on the 1980 GSS by applying several of the proposed approaches.

Measuring Nonrespondents and Assessing Nonresponse Bias

Numerous methods have been proposed over the years to estimate the attributes of nonrespondents. Some are appropriate for certain types of surveys (e.g., list samples only) while others can be used with modification across various methods of administration with various sample frames (e.g., from mail lists to RDD telephone). Attention will focus primarily on methods that are appropriate, or at least have been offered as appropriate, for face-to-face, national surveys. Among others this eliminates list samples where information about the respondent is known prior to the survey. Our review of nonresponse studies found nine major approaches to assess and adjust for nonresponse:

1) External population checks
2) Geographic/aggregate level data
3) Interviewer estimates
4) Interviewing nonrespondents about nonresponse
5) Subsampling of nonrespondents
6) Substitution of nonrespondents
7) Politz-Simmons adjustment
8) Extrapolation based on difficulty
9) Conversion adjustments

Probably the simplest check is to compare sample estimates (usually distributions) to some universe figures or preferred sample estimates such as the U.S. Census or the Current Population Survey (Smith, 1979 and Presser, 1981). Strictly speaking when using such a criterion comparison one is not checking how much difference comes from nonresponse but how much comes from nonresponse <u>and</u> all other sources (item unreliability, interviewer error, etc.) If one shows that differences are within sampling error then either no noticable nonresponse bias exists on the variable being compared or nonresponse bias is being offset by other countervailing errors. Similarly a large difference does not specify nonresponse as the cause. This imprecision is of course undesirable from the perspective of studying nonresponse per se, but since one typically wants to know primarily whether the survey is reliable and representative such general checks often serve the ultimate purpose satisfactorily. Superior estimates unfortunately are often unavailable and at best are usually limited to a few demographics. Since the representativeness on one variable is not generalizable to all variables or relationships in question, this limits the usefulness of this approach. For example, one may have the correct regional distribution but may seriously overrepresent the percent college educated within each region. In addition even if one gets the correct distributions for two variables, region and education, that is no assurance that the association between the variables was representative. One might have too many college educated from the South and too few from the North and thereby have the right univariate distributions, but the wrong bivariate association. Because of this, the representativeness of a few demographics does not insure representativeness in general and using post-stratification to bring errant surveys into line with the superior estimates does not assurdedly eliminate nonresponse and other biases.

Two related means of assessing bias are the geographic/aggregate level approach (Hawkins, 1975; DeMaio, 1980; House and Wolf, 1978). Since the geographic location of the sample household is known, one can code for all cases certain aggregate level data such as 1) region and city type, 2) figures for census tract or other units, 3) interviewer description of neighborhood, and 4) interviewer description of dwelling unit. These can be recorded for all households and they can include many contextual variables that are commonly used in survey analysis. They still cover only a small fraction of the total variables of interest on a typical survey and suffer from the problem of the nongeneralizability of representativeness across unexamined variables however. In particular they do not apply to individual level attributes. Thus we might get all the right distributions on region, tract characteristics, and dwelling type, but still not correctly enumerate some important group such as getting too few men. On the other hand, since we have complete and accurate observations, we have a precise measure of nonresponse bias for the covered variables.

Going a step beyond the geographic/aggregate level approach, interviewers can make observations and estimates about households and individual respondents (DeMaio, 1980; Moser and Kalton, 1972; Lansing etal, 1971). The advantage is that one can expand the range of comparable variables and that one can include individual level variables. One problem is that it is not possible to get complete information. On the 1980 GSS and the 1968 Michigan election survey no estimates were possible on race for respectively .248 and .215 of nonrespondents and on income the figures were .364 and .294. This means that one does not have even interviewer estimates for an appreciable share of nonrespondents and that one has to use some additional procedure or assumptions (such as the unestimated nonrespondents being like the estimated nonrespondents) to cover the unestimated nonrespondents. In addition the estimates by interviewers are usually more error prone than the directly acquired data. Finally, there is a clear limit to what variables can be checked. While it is probably reasonably reliable to get interviewer estimates of race, whether a married couple lives there, whether they own a car, and other basic attributes that are visible or determinable from household members other than the respondent or from neighbors,[1] it is questionable

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[1] In a household/informant survey such as the CPS reports are, of course given on all household members by a responsible member of the household. In a random respondent survey only the preselected respondent gives personal data, but this person may give some limited data about other family members (e.g., spouse's education). It might be possible to adopt a hybrid system for nonrespondents. If a selected respondent refuses or is unreachable then perhaps another household member could answer the basic demographic and behavioral questions. While these data are usually not quite as accurate as personally given data, the Census-CPS experience shows that informant information about many items is very reliable. Another variation might be to more systematically tap neighbors. Interviewers now pick-up much information from neighbors and others. In Bulgaria, certain surveys as standard practice ask neighbors certain basic facts about the nonrespondent households (Dobrianov, 1980). One would have to seriously consider both the measurement error and ethical implications of using neighbors as formal informants.

whether many personal details, attitudes, behaviors, or psychological states could be estimated.[2] And once again knowing the fit of certain variables, doesn't necessarily indicate the fit of unestimated variables.

To get into the nonrespondents' minds special field procedures are sometimes used, interviewing nonrespondents, intensive follow-ups on a subsample of respondents, and substituting for nonrespondents. Under the nonrespondent interviewing approach refusals are asked to answer a few questions about why they refused to participate. This approach rests on the shakey premise that those unwilling to cooperate with one interview will nevertheless agree to another interview. The difficulty of this approach is highlighted by the fact that according to one study two-thirds of all refusals came before even an introduction could be read (Singer, 1978). The Bureau of Social Science Research (1981) was able in a recent study to get a noninterview interview with 53 percent of refusers however. The problem of this approach is that it applies only to refusers rather than not-at-homes and others,[3] that information is available for only some refusers, and that it is difficult to pick up much substantive information pertinent to a study in a necessarily short nonrespondent interview.

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[2]One can however impute attitudes and the like from demographics (Hawkins, 1975). This assumes that the association among respondents and nonrespondents are the same and unless a extraordinarily strong predictive equation was available assignment to individual cases would not be possible.

[3]It might be possible to reach the not-at-homes with a similar nonresponse interview by leaving a mail back version.

The subsampling of nonrespondents is used to get an estimate of nonrespondents by taking extraordinary efforts to interview a representative sample of nonrespondents (Lundberg and Larsen, 1949 and Lagay, 1969-70). This method is often used successfully in mail surveys. After several mailings and reminders, a sample of nonrespondents is drawn and this group is approached via some more persuasive medium, such as telephone calls or personal visits (Hansen and Hurvitz, 1946 and Kish, 1965:556). It is difficult to use and not particularly cost efficient in a first rate, face-to-face full probability sample. A substantial effort is usually made to get all cases including typically a minimum of three or four calls and such location and conversion efforts as sending letters, making telephone calls, talking to neighbors about how to reach the respondents, and changing interviewers. The returns from extraordinary efforts beyond the standard procedures are likely to be small and it is often more efficient to keep after all nonrespondents in an area rather than to concentrate on a subsample. In addition it is clearly not desirable to use the subsampling approach instead of making a vigorous attempt to get all respondents. If however, a good effort is made to get all respondents and a subsample is very successful at interviewing the temporary nonrespondents then this group should give a accurate profile of all nonrespondents (subject of course to sampling error). However, usually only a fraction of the targeted nonrespondents will become respondents and the subsample respondents are not necessarily representative of either the subsample members who remain nonrespondents or the nonrespondents they were sampled from.

Another field procedure that has been proposed is substitution. Under this method alternative households are added to the sample to replace nonrespondent households. This method is generally not useful since it tends to replace nonrespondents with people who resemble respondents rather than nonrespondents and this approach does not appear to be widely used. However, there are related techniques that do somewhat the same thing. Block quota samples require interviewers to fill a certain quota, for example so many employed/unemployed females and so many young/old males from a given block. If no one is home, a household refuses, or no one in the household fits the remaining quota slots, then the interviewer proceeds to the next house. In effect the block quota sampling uses a substitution procedure passing over unavailable households and substituting available ones instead. The quotas are designed to insure that the hard-to-get groups are represented so in effect one does not substitute easy for hard households/respondents, but merely gets those easy and hard households/respondents that are available at the moment. On its face this sampling approach seems likely to increase nonresponse error, but controlled experiments between full probability and block quota surveys show few differences (Stephenson, 1979).

Another superficially related approach is Kish's replacement procedure (Kish and Hess, 1959 and Kish, 1965; 560-562). The procedure is essentially to substitute previous nonrespondent households (from an earlier and similar survey) for nonrespondents to the current survey. It assumes that the past nonresponse households are reasonable replacements for the current nonresponse households and that it will be possible to secure interviews in a high percent of these recycled households. Administrative problems and the difficulty of getting high response rate from the replacement households have severely limited the actual use of this approach. The Current Population Survey does use a related technique however. The CPS households are interviewed eight times, for four consecutive months of interviewing. Unlike most panel survey the CPS does not exclude nonrespondents from subsequent waves. Not-at-homes are continued in the sample and an attempt is made to reach them on all waves despite absences on earlier waves. Refusers are approached a second month and only after repeated refusals are households dropped from subsequent waves. Thus each CPS cross-section includes "replacement" households that were nonrespondents on earlier surveys.

Finally, there are several methods fom estimating the effects of nonresponse from respondents, by the Politz-Simmons approach, by extrapolation based on difficulty, and by convertability. In the times-at-home or Politz-Simmons approach (Politz and Simmons, 1949; Kish, 1965; 559-560; and Moser and Kalton, 1972; pp. 178-181) respondents are asked how many times they were at home at the time of interview during the last 'x' number of days. This is taken as representing their probability of availability. Respondents are then weighted according to the inverse of the number of days they were home. This method assumes that nonresponse is basically a funtion of availability (as the quota samples do) and that this adequately adjusts for the probabilities of availability. In two empirical tests (Durbin and Stuart, 1954 and Simmons, 1954) this was not found to perform as well as call backs, however. The technique is still commonly employed, Gallup, for example, included a times-at-home weight as part of its surveys from 1960 to 1967 and since then has included a weight factor that apparently combines a times-at-home weight with a post stratification weight (Gaertner, 1976).

The difficulty method uses some measure of how hard it was to get an interview from a respondent. This might be the number of mailings, visits, or telephone calls, how longit took to get a response, or some measure derived from these indicators of difficulty. While there are numerous variations, the basic approach determines whether a particular variable was related to difficulty (e.g., the proportion employed rising with the number of attempts). If a linear or some other regular relationship is found, then this association is used to impute the distribution of the variable among the nonrespondents. This approach has the advantage of allowing an estimate for every variable contained in the survey and thus avoids the problem of nongeneralizability of representativeness that plagues several of the methods we considered above. It rests on the premise that difficulty is related to final nonresponse. If the final nonrespondents differ from the merely difficult or if the degree of difficulty does not relate to the final nonrespondents, then this procedure will obviously misestimate the attributes of the final nonrespondents. This approach is probably the most frequently employed in estimating nonresponse and has shown some impressive results especially in mail surveys where known attributes from a list sample could be compared to estimates from the difficulty extrapolations (e.g., Crossley and Fink, 1951; Hendrick, 1956; Mayer, 1964; Dunkelberg and Day, 1973; Granberg, 1975; Filion, 1975-76; and Armstrong and Overton, 1977). At the same time, however, there have been a number of criticisms of the method and cautions about its general application. Stephan and McCarthy warn that "great care would have to be exercised in carrying out this extrapolation, and its use is not recommended except under exceptional circumstances" (1958; p. 257). (See also criticism and subsequent rebuttal by Ellis, et. al., 1970 and Filion, 1976.)

Unlike the difficulty approach which is aimed at all nonrespondents, the convertibility approach uses converted refusals as estimates for final refusals. Usually the converted are seen as substitutes for the final refusals although it is possible to do an extrapolation with the first group being the respondents who never refused, the converts making the second point and the final refusers as the last group[4] (Benson, Booman, and Clark, 1951; Stinchcombe, Jones, and Sheatsley, 1980; Robbins, 1963; O'Neil, 1979; DeMaio, 1980; and Andersen, 1978). As in the difficulty approach, all variables can be studied and the appropriateness of the technique rests on the supposition that final refusers are more like temporary refusers (in case of the substitution approach) or at least more like temporary refusers than cooperative respondents (in case of extrapolation).

In brief, a number of procedures have been proposed to assess the impact of nonresponse. Some methods such as using geographic/aggregate level data allows a complete assessment of nonresponse bias for a limited number of variables, other techniques such as difficulty extrapolation permit estimates of nonresponse bias for all variables. None of the methods permit the complete measurement of nonresponse bias for all variables.

Analysis of Nonresponse on 1980 General Social Survey

To assess the impact of nonresponse on the 1980 GSS we selected four of the more promising and widely applied techniques, 1) geographic/aggregate level analysis, 2) interviewer estimates, 3) extrapolation for difficulty, and 4) convertibility. The 1980 GSS was a multi-stage, full probability sample of the contiguous United States. Households were sampled according to NORC's equal probability selection procedures and a Kish table was used to chose a respondent from the designated households (King and Richards, 1972 and GSS, 1980). Interviewers kept a record-of-calls,

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[4] So far this approach uses only two types of respondents, cooperators and converts. This means that there is either no significant difference between the two groups or a linear relationship to extrapolate to final refusers. If temporary refusers were further subdivided into easy or hard to convert, or some other refinement, such as number of attempts needed to convert, then it would be possible for other relationships to emerge. Apparently no one has attempted such a refinement and the usually small number of total converts would make such refined analysis difficult.

recording each attempt to contact the household or respondent. This form recorded the date and time of the attempt, method of contact, (personal/telephone), and the outcome (not-at-home, temporary refusal, interview, etc.). If a contact could be made, a household enumeration folder was filled out listing all household members along with their relationship to head of household, age, sex, marital status, and location (staying at household/staying elseqwhere). If an interview was secured, a questionnaire was completed. For nonrespondents a noninterview report form was completed. This recorded the reason for nonresponse, descriptions of why the nonresponse occurred (e.g., why a person was never found at home) and interviewer estimates of the family income, race of household, number of adults, number of adult males, presence of married couple, and age of head of household.

The 1980 GSS had a net sample of 1,931. There were 1,468 completed cases, 315 refusals, 66 not-at-homes, 78 others (mostly not mentally or physically capable of participating, but also including administrative errors) and four lost documents.[5] This gives a response rate of .760, a refusal rate of .163, a not-at-home rate of .034, and an other rate of .042 (including the four unclassified cases).

In the subsequent analysis we will not only examine nonresponse in general, but will also examine the main types of nonresponse-refusals, not-at-homes, and others. Both past research and findings from this

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[5]These figures differ slightly from those presented in Davis, Smith, and Stephenson, 1980. The difference come from internal inconsistencies between the record-of-calls and the noninterview report forms. The difference usually results from discrepancies between the status of the case at the last recorded call and that entered in the noninterview report form that records the final status of the case.

study suggest that these groups are quite different in their motivations for nonresponding, in their demographic profile, and in other notable ways (Stinchcombe, Jones, and Sheatsley, 1980; Kish, 1965; O'Neil, 1979; Bebbington, 1970).

The geographic/aggregate level analysis was restricted to measures of city type (SRCBELT,SIZE,XNORCSIZ) and region (REGION). As Table 1 shows, there are large differences in the response rates across city types and regions. Response rates are lowest in central cities, rise moderately in suburbs and exurbia within metropolitan areas and increase substantially in rural areas. This urban-rural difference replicates similar finding from numerous other studies (Lansing, et al., 1971; Moser and Kalton, 1972; DeMaio, 1980; House and Wolf, 1978; Groves and Kahn, 1979). Most of the difference comes from variation in the refusal rate. The not-at-homes also roughly follow the same pattern as refusals (more clearly on SRCBELT than on XNORCSIZ) and the others appear to be scattered across city types. Regional response rates tend to be highest in the Northeast and lowest in the South although the pattern is not completely uniform. Most previous studies have found some regional differences (except House and Wolf, 1978), but there is disagreement on where the nonresponse is highest. Love and Turner (1975) find response rates lowest in the Northeast and results from Schuman and Gruenberg (1970) and Dunkelberg and Day (1973), suggest a similar conclusion, but DeMaio (1980) finds that refusals were lowest in the Northeast and highest in the West. As with city size most variation is in the level of refusals.

Both city type and region exercised independent effects on response rates. Controlling for city type the South had a response rate 8.5 percentage points below the Northeast, while with region controlled for suburbs

TABLE 1

RESPONSE/NONRESPONSE BY CITY TYPE AND REGION

Response/Nonresponse	Completed Case	Refusal	Not-at-Home	Other	
SRCBELT					
Central City of 12 Largest SMSAs	.665	.216	.065	.054	(185)
Central City 13-100 Largest SMSAs	.713	.212	.039	.036	(307)
Suburb of 12 Largest SMSAs	.697	.211	.038	.054	(185)
Suburb of 13-100 Largest SMSAs	.751	.190	.027	.032	(221)
Other Urban	.791	.144	.031	.034	(731)
Other Rural	.849	.081	.020	.050	(298)
	χ^2 = 44.6 prob. = .0001				
XNORCSIZ					
Central City 250,000+ .	.691	.216	.046	.046	(431)
Central City 50-250,000	.711	.229	.040	.020	(201)
Suburb, cc 250,000+ ...	.746	.183	.031	.041	(295)
Suburb, cc 50-250,000 .	.790	.160	.017	.034	(119)
Exurbia, cc 250,000+ ..	.752	.172	.032	.045	(157)
Exurbia, cc 50-250,000	.797	.116	.043	.043	(138)
City, 10-50,000	.794	.127	.039	.039	(102)
City, 2,500-9,999	.775	.135	.079	.011	(89)
City, less than 2,500 .	.860	.070	.023	.047	(86)
Rural	.845	.094	.010	.052	(309)
	χ^2 = 58.3 prob. = .0004				
REGION					
New England	.716	.230	.041	.014	(74)
Midatlantic	.691	.234	.038	.038	(346)
East North Central ...	.760	.160	.053	.028	(400)
West North Central ...	.819	.118	.024	.039	(127)
South Atlantic	.767	.124	.046	.063	(348)
East South Central ...	.854	.131	.000	.015	(130)
West South Central ...	.789	.132	.026	.053	(152)
Mountain	.806	.153	.020	.020	(98)
Pacific	.758	.171	.016	.056	(252)
	χ^2 = 50.5 prob. = .0012				

and exurbia had a response rate 6.3 percentage points above central cities and rural areas had a response rate 11.9 percentage points above central cities. (James A. Davis' d-systems were used to test for this. All differences significant at .05 are reported. Davis, 1975.) In brief, there are moderate nonresponse bias on both of the geographic variables investigated.

Next, nonresponse bias was examined by having interviewers estimate the following characteristics of the nonresponding households: race, family income, number of adult males, number of adults, presence of married couple, and age of head of household. Estimates were possible in two-thirds to three-quarters of households. Estimates were more available for refusers than for others or not-at-homes (see Table 2).

TABLE 2

ITEM NONRESPONSE AMONG COMPLETED CASES AND RESPONDENTS
(proportion missing)

Variables	Completed Cases	Nonrespondents			
		All	Refusals	Not-at-Homes	Others
Race	.000	.248	.203	.348	.346
Income	.075	.364	.314	.500	.449
Number of adult males	.000	.296	.273	.379	.321
Number of adults ...	.000	.303	.283	.379	.321
Married couple	.000	.320	.305	.424	.295
Age of head	.007	.285	.232	.439	.372

The absence of estimates from a substantial minority of nonrespondents as well as the probable unreliability of some of the estimates necessarily hampers the use of these interviewed estimates to study nonresponse bias. On the other hand, the proportion of data missing among all eligible cases is sufficiently small (from .059 for race to .144

for income which suffers from relatively high item nonresponse among the completed cases) to greatly reduce the relative response bias and to permit using maximum and minimum estimates for the remaining cases. Looking at first among those nonrespondents with available information (if we assume that the unestimated resemble the estimated we can talk about differences between completed cases and nonresponse. Alternatively we can think about these comparisons as between the completed cases and the estimated nonresponse cases only), we find no significant differences between complete cases and nonrespondents on race, marital status, and number of adult males (see Table 3). Nonrespondents are older, have fewer adults, and less middle income and poor.

We also discover that the profile of each type of nonrespondent is quite different. Refusals are somewhat more likely to be married, have a middle income, and be over 30 years old than respondents. Not-at-homes tend to be isolated individuals, less likely to be married and more likely to live alone. The others are also isolated individuals, but in addition they are typically old and poor as well. These differences generally follow those in earlier studies.

The finding that heads of nonresponse households tend to be older than among responding households contradicts the stereotype of "the young and the restless" nonrespondent, but actually agrees with most previous research which finds final nonresponse to be highest among the older ages (Lowe and McCormick, 1955; Lansing, et al., 1971; Weaver, Holmes, and Glenn, 1975; Hawkins, 1975; and DeMaio, 1980). Nonresponse was found higher for whites in three studies (Schuman and Gruenberg, 1970; Weaver, Holmes, and Glenn, 1975; and Hawkins, 1975) and not significantly different across races in two studies (DeMaio, 1980 and Lansing, et al., 1971). Middle income groups are usually found to have the highest

TABLE 3

SELECTED CHARACTERISTICS AMONG COMPLETED CASES AND ESTIMABLE NONRESPONSE CASES

Variables	Completed Cases	Nonresponse Cases				Probability	
		All	Refusals	Not-at-Homes	Others	Completed Cases versus Nonresponse	Completed Cases Refusals, Not-at-Homes, Others
<u>Race</u>:							
White	.898	.910	.928	.837	.882		
Black	.095	.084	.064	.163	.118	.789	.411
Other	.007	.006	.008	.000	.000		
	(1,468)	(345)	(251)	(43)	(51)		
<u>Income</u>:							
Less than $7,000 ..	.204	.226	.153	.212	.605		
$7,000-19,000	.412	.541	.597	.455	.326	.000	.000
$20,000+ ...	.384	.233	.250	.333	.070		
<u>Number of Adult Males</u>:							
None	.193	.251	.223	.244	.377		
One	.695	.684	.725	.634	.547	.066	.167
Two+	.113	.065	.052	.122	.066		
<u>Married Couple</u>:							
Yes	.606	.641	.721	.447	.455	.243	.000
No	.394	.359	.279	.553	.545		
<u>Number of Adults</u>:							
One	.252	.359	.305	.488	.491		
Two	.598	.509	.575	.341	.358	.008	.016
Three+	.151	.131	.119	.170	.151		
<u>Age of Head</u>:							
Under 30 ...	.200	.076	.087	.081	.020		
30 - 64	.607	.591	.649	.703	.224	.000	.000
Over 65	.193	.332	.264	.216	.755		

nonresponse (Lansing, et al., 1971; DeMaio, 1980), but these may well be a function of estimating error since there is probably a tendency to place people in the middle category and since the nonestimates are usually relatively high for this variable. Studies are also divided as to whether nonrespondents vary by sex. Crossley and Fink (1951), Hawkins (1975), and DeMaio (1980) found no difference, but Bartholomew (1961), Lowe and McCormick (1955); and Smith (1979) found an underrepresentation of men.

We looked at the sex distribution of the adults in the completed cases and found that the proportion of males was higher than among the actual respondents (46.7 percent versus 43.7 percent). Assuming that the listings and Kish table actually give each adult an equal probability of selection, this suggests that those households with a male respondent selected were less likely to yield a respondent than households with female respondents. This would suggest that among the nonresponse cases there should be more male nonrespondents than female. Among the 32 percent of nonrespondent households for whom the sex on the nonrespondent was determinable the opposite proved to be true, only 40.8 percent were male. Since the sex distribution should come out to be near the true population figure of approximately 47.7 percent and the combined estimate of completed cases and known nonresponse cases yield an estimate of only 43.9 percent, this suggests that male respondents must be especially high (almost 70 percent) among the unidentified nonresponse households.[6] If these figures and reasonings are correct

[6] It might be by chance we sampled households with fewer males in them. But random respondent, full probability estimates consistently yield a lower proportion male than Census estimates of the population (Smith, 1979).

they not only suggest that nonresponse is higher among males, but also that the unknown nonrespondents might differ from the known nonrespondents and that therefore nonresponse bias can not be precisely measured from the estimated proportion of nonresponse cases.

Further evidence of the possible unrepresentativeness of the nonrespondents with estimates comes from the comparison of the presence of an estimate by place of residence. The proportion of nonrespondents with estimates is lowest in large central cities and their suburbs and in rural areas, with smaller central cities, their suburbs, and small towns having estimates for a significantly higher proportion of cases. For example, the percent of nonrespondents with no estimates for any of the six variables was 38.7 percent in the central cities of the twelve largest SMSAs, 33.3 percent in rural area, 32.1 percent in suburbs of the twelve largest SMSAs, 12.5 percent in central cities of smaller SMSAs, 9.1 percent in their suburbs, and 7.2 percent in other urban areas. Missing estimates also appear to be highest in the west. In addition we looked at those nonresponse cases with estimates on age and race and found that nonestimates were typically higher for nonwhite households and on marital status, percent of males, and percent of adults nonestimates were more common among the middle aged (30-64). These results suggest that the estimated nonresponse households are not generally typical of all nonresponse cases and thus do not give an unbiased estimate of all nonrespondents.

In sum, comparisons between respondents and interviewer estimates of nonrespondents shows various differences. Nonrespondents are older and from smaller households, but do not differ on race, marital status, and number of males. In general, these findings agree with most previous research although the literature is not unanimous in its findings.

In addition, we find that nonrespondents differ amongst themselves as much or more than they differ from respondents. Of course, in both instances these conclusions rest on the questionable assumption that the one-quarter to one-third unestimated nonresponse cases resemble the estimated cases.

Next, we examined the association between difficulty and respondents. Difficulty was measured by the number of attempts it took to complete an interview. We believed that difficulty resulted from three factors, availability (essentially the probability of a respondents being home at a given time), contactability (the probability of some other responsible household member being home at a given time), and reluctance (respondent's and/or informants willingness to cooperate). The following groups were anticipated to have high availability: nonmembers of the labor force (especially homemakers), women, members of the labor force working few hours, not travelling, and not self-employed, widowed people, older people, infirm and physically restricted, and low socioeconomic status people. Households with high contactability were anticipated to include: married couples and households with more than one adult, young children, and a spouse not in the labor force. Reluctant households were presumed to be urban, fearful of crime, and mistrustful of people. In addition we included race because of its close association to several of the preceding variables.

We tested the relationship between these independent variables and difficulty by using one-way analysis of variance. As Table 4 indicates, the chosen indicators of availability were related to difficulty as anticipated.

TABLE 4

ANALYSIS OF VARIANCE OF SELECTED VARIABLES
BY NUMBER OF ATTEMPTS

Variable	Probabilities		Deviation from Linearity
	Between Groups	Linearity	
Labor force	.0000	.0000	.0044
Hours	.0000	.0000	.4541
Self-employed	.2345	--	--
Marital status	.0069	.0012	.1774
Age	.0000	.0000	.1569
Health	.0000	.0000	.7900
Education	.0000	.0000	.0300
Prestige	.0000	.0000	.0510
Income	.0000	.0000	.0002
Sex	.0024	.0001	.2876
Adults	.0356	.2475	.0332
Children under 18	.0691	--	--
Number children over 18	.0009	.0000	.2569
Spouse working	.2371	--	--
SRCBELT	.0031	.0000	.7117
SIZE	.9822	--	--
XNORCSIZE	.0402	.0009	.7209
FEAR	.2214	--	--
TRUST	.4500	--	--

Availability had a basically linear relationship with labor force participation, socioeconomic status, life stage (age and marital status), health, and sex. It was unrelated to race and self-employment. Too few occupations were identifiable asinvolving extensive travelling away from home to permit analysis of this factor. Contactability on the other hand did not show the anticipated relationships. The presence of a spouse and/or children at home were unrelated to difficulty and the number of adults had a weak and uninterpretable nonlinear association. Reluctance showed intermediate results. SRCBELT and XNORCSIZ both showed the expected associations between urbaness and difficulty, but neither fear of crime nor mistrust of people were related.

We worked with these variables, trying various combinations and introducing certain other related variables. We finally settled on the variables in Table 5 for multivariate analysis. In this table, the number of attempts becomes the dependent variable and step-wise regression is used to test the independent predictive power of the selected variables. In general the multiple regression analysis clarified relationships, eliminating some zero-order association that had been unanticipated (number of siblings) and revealing anticipated associations that had been suppressed (children at home). Labor force participation is the strongest correlate of difficulty. High socioeconomic status also meant more difficulty, probably because of the more active social and occupational activities outside the homes. Some of this might result however from the growing proportion of people in the upper social rankings "protected" from interviewers by doormen, security systems, and other barriers. The young also proved to be more difficult to reach probably because of more socializing outside of the home. Urban dwellers were also harder to reach. Part of these seems to result from more calls need to persuade reluctant respondents, but there may also be a greater tendency for urbanites to spend more time away from their homes. Finally, we find that people with dependent children were easier to reach, probably because there is usually someone at home to contact.

TABLE 5

MULTIPLE STEPWISE REGRESSION ANALYSIS OF AVAILABILITY

Variables	Standardized Coefficient	F
Variables in Equation:		
Hours (0 if not in labor force)	.217	53.00
SRCBELT (Low-urban)	-.084	9.46
Education (Years of Schooling)	.078	7.20
Age	-.095	8.84
Children under 18 at home	-.082	8.03
Variables not in equation:		
Prestige (Hodge-Siegel-Rossi Scale) ..	.062	3.75
Health (self rating)	-.052	3.22
Number of siblings	-.049	2.94
Family income	.039	1.59
Number of income earners	-.011	0.14
Number of adults	-.005	0.04
Number of children ever	-.003	0.01
Marital status	-.001	0.00

In order to evaluate the effect of nonresponse on nondemographics we ran attempts by a wide range of attitudes, behaviors, and socio-psychological scales. Only 27.4 percent showed significant variation with number of attempts. Of the significant relationships most were linear (67.4 percent linear, no significant deviation; 15.2 percent linear, with significant deviation; and 17.4 percent nonlinear). The hard-to-gets appeared to have three main characteristics--liberal political views (e.g., pro-abortion, civil rights, tolerance) high socioeconomic status (e.g., members of professional groups, never ~~received~~ governmental aid), and active and youthful life style (having received ticket, members of youth groups, watch less television, favor legalization of marijuana).

To tell whether any of these associations represented independent effects or whether they were merely reflecting associations with the demographics we entered the basic demographic model (hours, education, SRCBELT, age, number of children) into a multi-regression analysis then entered those variables that preliminary analysis suggested most strongly represented the three factors noted above and some other unclassified variables. We tried various scales and combinations of variables and found that none of the liberalism or socioeconomic variables added independent effects. Several life style variables added explanatory power however. Favoring the legalization of marijuana, and having received a traffic ticket were significantly related and drinking just missed the cut-off. (In addition those disagreeing that people shouldn't have children given the state of the world were harder to reach. It is unclear whether this relationship has any substantive meaning.)

While this means that very few nondemographics are independently related to difficulty many variables are closely enough related to the independent variables to vary notably with number of attempts. This means that not only will variables directly related to availability be affected but many attitudinal and behavioral variables will also be affected.

We also took a purposive sampling of twenty-one bivariate relationships and examined whether they varied by number of attempts. We found few significant differences and no clearly discernible pattern. Five correlations tended to decline as difficulty increased, three tended to rise, and the remaining thirteen showed no net tendencies. Low correlations did tend to cluster among those interviewed on the second or third attempt but they were quite similar among the easier and harder-to-get (mean correlations: 1 call = .157; 2 calls = .108;

4 to 5 calls = .151; 6+ calls = .149). Since two to three calls represents the modal number of attempts this may suggest that the "average" respondent has lower correlations than those at either extreme. Unfortunately the theoretical justification for this interpretation is uncertain and the empirical evidence is mixed enough to suggest caution over such a generalization. It is clear however, in terms of correlations there is not significant and clear differences between the pattern exhibited on the first call and on the last calls.

Normally the next step in using difficulty extrapolation would be to apply the extrapolations to the nonrespondents to estimate their attributes and subsequently to calculate nonresponse bias. An examination of the content of nonresponse and of the hard-to-get revealed however that difficulty could not be used to impute nonresponse attributes and an analysis attempting to use difficulty estimates found that in fact the extrapolations were usually further from the mark than assuming no difference between completed cases and nonresponses.

The reason why extrapolation according to difficulty does not work is that the number of attempts basically measures how accessible a person is while the final nonrespondents are made up primarily of refusals not inaccessibles. Call backs quite successfully reduce the number of inaccessibles and the correlations between variables and number of attempts reflect the fact that different groups of people have differing mean probabilities of being at home. Among final nonrespondents however the not-at-homes are reduced to a small minority (or to put it another way the call back procedure is so successful that it almost eliminates the not-at-homes from final nonrespondents).

TABLE 6

FINAL STATUS BY NUMBER OF ATTEMPTS

Number of Attempts	Completed Case	Refusal	Not-at Home	Other	Miscel-laneous
"Final" status after . . .					
2 calls	628	191	970	118	24
3 calls	892	204	702	109	24
4 calls	1,079	242	488	98	24
5 calls	1,207	257	351	93	23
6 calls	1,296	268	315	85	23
7 calls	1,361	289	185	73	23
8+ calls (Final) ...	1,468	315	66	78	4

As Table 6 shows the proportion of refusals steadily rises with calls as the proportion of not-at-homes falls. After two calls completed cases make up 32.5 percent, refusals 9.9 percent, not-at-homes 50.2 percent, others 6.1 percent, and miscellaneous 1.2 percent. After the final attempt completed cases have risen to 76.0 percent and refusals to 16.3 percent, while not-at-homes fell to 3.4 percent, others to 4.0 percent, and miscellaneous to 0.2 percent. In brief, repeated call backs nearly eliminated the not-at-home problem while both in relative and absolute terms refusals increased. This relationship is also evident in the conversion rates. Only 35 percent of temporary refusals are converted to respondents while 91 percent of not-at-homes (excluding those who were both not-at-homes and temporary refusals) were eventually "converted" to respondents.

The lack of association between difficulty and the final non-respondents who are mostly refusals is also suggested by the differences between temporary refusals and temporary not-at-homes. Temporary not-at-homes have the same relationship with the variables that fit in our difficulty model as number of calls and these two measures of difficulty are correlated r = .592. Temporary refusals on the other hand are only correlated to SRCBELT and have a much lower association (r .255) with attempts than temporary not-at-homes. (And if we restrict our temporary refusals to those who have never been found absent from home the insignificant associations are even smaller between temporary refusals and number of calls.) In sum, if we accept temporary refusals as a tracer of final refusals then the low association between temporary refusals and number of calls suggests that difficulty can not be used as another tracer of nonresponse when most nonresponse is final refusals. To use our difficulty measure to impute the attributes of final refusals is thus essentially to use the correlates of inaccessibility to predict the correlates of refusals. Given the differences in the nonresponse motivations and known demographic profile this is obviously an improper imputation procedure.

Of course even if difficulty can not be used to estimate the values of nonrespondents as a whole, it might be possible that this procedure would yield accurate estimates of the final not-at-homes. We were able to test this by looking at the difference between completed cases and final not-at-homes for those variables with either aggregate level data or interviewer estimates. For example, on SRCBELT urbaness was correlated with final not-at-homes and with difficulty (although probability = .058). This means that using attempts does help to predict the distribution of final not-at-homes. Among the completed cases the proportion rural was .172. If we had assumed that the not-at-homes were similar to the respondents, we would have been off by a considerable margin since the actual proportion rural among the final not-at-homes was .091. By using a linear extrapolation based on difficulty we were able to come up with much improved estimates of .060 (using number of calls to extrapolate and assuming the final not-at-homes were at a ninth call) to .090 (using cumulative proportion of cases after each call with final not-at-homes at 1.00).[7] Using difficulty to estimate final not-at-homes also works for region, race, and number of adult males. In these instances there is no association between the variables and difficulty and no association between respondents and the final not-at-homes. On the presence of a married couple the predictive ability of difficulty is questionable. Difficulty has a nonlinear association with the proportion married first declining and then rising over the record of calls while the proportion married tends to be lower among the not-at-homes, although the difference is not significant (prob. = .07). If we did use the insignificant linear association between difficulty and the presence of a married couple to estimate not-at-homes, we do get a figure that is closer to that among the not-at-homes with an interviewer's estimate than if we assumed that respondent households were the same as not-at-homes (difference between proportion with married couple among not-at-homes with interviewer estimates and extrapolation is -.112 and with completed cases is -.159). Difficulty proves to be a poor indicator for number of adults and age of head of household. Difficulty is associated with younger heads (which agrees with the associations with age of respondent and number of attempts we discussed earlier), but final not-at-homes with age estimates tend to be slightly, although not significantly, older than completed cases. As a result the two difficulty extrapolations of the proportion over 65 years old (.027

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[7] Even the assumption that the proportion rural was the same among the not-at-homes as among the completed cases would not put the estimate of the total completed cases and not-at-homes off by much since there are so few final not-at-homes. Under the erroneous same distribution assumption, the proportion rural for completed cases and not-at-homes would be .172 compared to the actual .169.

and .066) differ markedly from the proportion estimated by interviewers (.216) while this figure is quite close to that among completed cases (.193). This failure could result from unestimated not-at-homes being overwhelmingly young, but this explanation, while perhaps preserving the use of diffuluty extrapolations, would, as a result, question the useability of interviewer estimates. In this case the failure is probably due to difficulty. As our previous analysis indicated, the young are difficult to reach while the old in general and retured in particular as very accessible. But the final not-at-homes contain many people with zero probability of being at home. The Census (Palmer, 1967) finds that these "temporarily absent households" have a very high proportion of retired people (and thus older people). The high proportion of these impossible to reach older cases among the final not-at-homes means that this group is not representative of hard-to-get people in general as measured by number of calls and that difficulty extrapolation is not appropriate. In brief, we find that in four of six variables the use of simple difficulty extrapolations did give us improved estimates of not-at-homes. In two cases the procedure proved inappropriate, however. In the age of head estimate this was apparently due to the impossible to reach group differing notably from the merely hard-to-get. This underscores the problem of trying to make estimates of the unknown based on the known and shows that procedures that work well in some situations are inappropriate in other instances.

Finally, temporary refusals were used as an indicator of final refusals. Based on our review of this technique and reasons for refusals in general, we related temporary refusals to 1) mistrust and fear, 2) apathy towards social and political issues (replying DK to questions, not voting, no party identification), 3) negative psychological feelings (unhappiness, dissatisfaction, high anomia), 4) deviate behavior (having been arrested, receiving ticket), 5) attitudes towards science, 6) illness, 7) being too busy (long hours, labor force status), 8) uncooperativeness (interviewer rating and refusal to give family income, 9) place of residence, 10) conservatism, 11) socioeconomic status (education, income, occupational prestige), and 12) standard demographics (age, sex, race).

Only urbaness had a strong relationship to temporary refusals. In the twelve largest central cities 29 percent of the cases were temporary refusals while in rural counties only 11 percent of cases were temporary refusals. Of all other items only refusing to give family income had a significant (prob. = .035) association with reluctance. Of the other variables only being cooperative and being being fearful approached significance (prob. .10). On the twenty-one liberalism items (four on race relations, three on spending priorities, three on morality/personal life style, four on tolerance of Communists and atheists, and a seven items abortion scale) there was not a single significant relationship between conservatism and refusing and in the majority of cases the associations were not even in the hypothesized direction. This refutes evidence from Hawkins, 1975; Schuman and Gruenberg, 1970; and Benson, et al., 1951 but agrees with Brannon, 1973. In general these results show fewer and more modest assocations than most previous studies. We find no association with low socioeconomic status as Benson, Booman, and Clark (1951) and O'Neil (1979) found, no tendency to reply "don't know" (Stinchcombe, Jones, and Sheatsley, 1980), and no association with race, or number of children (O'Neil, 1979). Our lack of a difference between temporary refusals and age confirms Benson, Booman, but contradicts O'Neil who finds the elderly refusing more. In addition our one notable association, between urbaness and refusals differs from DeMaio (1980), who found no variation between rural and urban. The results do follow Robbins' findings (1963) that there were no significant differences. In general, the discrepancy of results suggest that findings depend on the specialized populations sampled, the survey procedures used, or other variable conditions.

On one hand the lack of associations between the hypothesized variables and temporary refusals is encouraging. If we accept temporary refusals as indicators of final refusals then the lack of significant associations suggests that except for city type final refusals are not significantly different from completed cases and therefore little bias is introduced. Yet it is somewhat surprising that temporary refusals had such consistently low correlations with variables that might have been expected on theoretical grounds to have shown more substantial relationships. One can hypothesize variables (e.g., willingness to be interviewed) that would have large associations with refusing. In addition it is reasonable to suppose that other variables touching on privacy, misanthrophy, paranoia, and fear, and other sociopsychological attitudes that should be closely related to willingness to be interviewed would show substantial associations with refusing. The fact that we were largely unable to find any of these associates may simply mean that we do not have the right variables, that indicators more closely related to refusing are needed before the anticipated relationships could be detected. Alternatively it might be that refusing is really more of a random occurance like a transitory mood and therefore there are not other related variables. The difficulty of converting temporary refusals and evidence from other studies (Stinchcombe, Jones, and Sheatsley, 1980) suggest that this is not the case however. Another alternative is that temporary refusals do not adequately indicate attributes of final refusals. Perhaps many of the temporary refusals, but not the final refusals, really represent transitory states. Or perhaps for other reasons temporary refusals are not reliable indicators of final refusals. Unfortunately, we cannot fully test these alternatives. We were able however, to carry out a more general, less focused comparison between completed cases without temporary refusals (non-refusals), temporary refusals, and final refusals on the nine variables for which we had aggregate level data or interviewer estimates--SRCBELT, XNORCSIZ, region, age of head of household, race, presence of married couple, income, number of adults, and number of adult males. On the three geographic variables temporary refusals performed well. Temporary refusals and final refusals were significantly different from non-refusals, but close to each other. Estimates using substitution or extrapolation were closer to the true distribution than assuming no difference (i.e., completed cases equal all cases). On three of the interviewer estimates (number of males, number of adults, race) there are no significant differences between the three groups. This means that temporary refusals are not really needed for estimating distribution of final refusals, but also means that they correctly predict the characteristics of final refusals. it (In two cases estimates assuming the final refusals equal completed cases would have been more accurate, while in the third cases an extrapolation using temporary refusals yields the closest fit.) For presence of married couple and age of household temporary refusals are not significantly different than non-refusals while final refusals do vary from non-refusals. For marital status the temporary refusals are in the right direction and provide a better estimate than assuming no difference between non-refusals and final refusals. On age however, temporary refusals point in the wrong direction for the old while in the correct direction for the young. Finally, on income temporary refusals do not differ from non-refusals while non-refusals significantly differ from final refusals and temporary refusals differ from final refusals. We have noted above however, that there are sufficient reservations about the reliability of the income estimates so little weight should be given to this comparison. Overall, the evidence is mixed about the appropriateness of using temporary refusals as indicators of final refusals (Table 7). On six variables the models indicated that temporary refusals were indicators of final refusals, although in only five cases were the best temporary refusal estimates closer than assuming no difference between completed cases and final refusals. The fact that temporary refusals perform well on the geographic variables is encouraging both because there are no complications from missing values in these cases and these variables have the strongest theoretical connection with refusals.[8] The evidence is further mixed on whether temporary refusals can best be substituted for final refusals or used to extrapolate to them. In three cases the extrapolation provides the best estimate while in two instances simple substitution comes closer to the mark. In general, the performance of temporary refusals is satisfactory enough to merit further investigation and selective application, but it is clear that neither substitution nor extrapolation of temporary refusals can be used routinely as a sure adjustment for final refusals.

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[8] The rank order correlation between final and temporary refusals on the city type variables is also very high.

TABLE 7

COMPARISON OF NONREFUSALS, TEMPORARY REFUSALS, AND FINAL REFUSALS

Variables	Nonrefusal Versus Temporary	Nonrefusal Vs. Final Refusal	Temporary Refusal Vs. Final Refusal	Estimates			Criterion Estimate
				No Difference	Substitution	Extra-polation	
SRCBELT (central cities) ...	.0000	.0000	.133	.2330	.257	.284	.251
XNORCSIZ (rural)	.0000	.0000	.876	.2282	.2086	.1879	.2075
Region (Northeast)	.0008	.0016	.0391	.1989	.2007	.2028	.2187
Age of Head (65+/under 30)	.134	.0000	.063	.1934/.1996	.1905/.1914	.1874/.1823	.2035/.1835
Marital (not married)	.654	.0111	.0090	.3944	.3915	.3883	.3794
Race (not white) ...	.604	.2865	.268	.1022	.1020	.1018	.0977
Number of Males (1).	.655	.1363	.0789	.6948	.6900	.6847	.6989
Income (High)	.200	.0000	.0000	.3844	.3948	.4061	.3659
Number of Adults (1)	.268	.758	.1011	.2520	.2443	.2011	.2591

Conclusion and Discussion

We come close to the conclusion that nothing works in estimating nonresponse bias. Each of the methods we examined proved to be of limited usefulness. The geographic/aggregate level approach allows definitive measurement of nonresponse bias, but it is limited to readily observable data or data linked from other sources such as the Census. The limitation is that usually only a few variables of interest are available and results from them are not necessarily generalizable to other variables (Lagay, 1969/70). Interviewer estimates help to expand the range of variables that can be checked, but 1) missing estimates (typically 25 to 35 percent of cases) prevent complete coverage, 2) the estimated portion of nonrespondents may not be representative of all nonrespondents, 3) some estimates probably have low reliability (e.g., income), and 4) once again the range of checkable variables is limited. Difficulty extrapolation was found to be inappropriate for nonresponse in general because of the high proportion of refusals among the nonrespondents. While probably usually useful for imputing to the not-at-homes, evidence in the case of age (and labor force participation) indicates that final not-at-homes are not always extensions of the hard-to-get. Temporary refusals were also found to have a mixed performance in estimating final refusals. Even when temporary refusals are indicative of final refusals the evidence is unclear whether substitution or extrapolation would be most appropriate. In sum, our analysis of nonresponse on the 1980 GSS suggests that there is no simple, general, accurate way of measuring nonresponse bias.

To further understanding of nonresponse bias more methodoligical study is urgently needed. There are very few studies of nonresponse bias on face-to-face interviews of general populations and few of these include checks of the nonresponse procedures used to estimate characteristics of nonrespondents. Most existing studies are on mail surveys of special populations (fruit tree growers, newspaper editors, hunters, etc.) and these results can not be automatically transferred to personal interview surveys.[9] In addition, while a few consistent findings on the characteristics of nonresponse bases have materialized there is considerable diversity in the research findings. Finally, the problem of nonresponse bias has probably been increasing over the last 20 years and may continue to do so in the near future. Response rate have generally been falling (Marquis, 1977; Love and Turner, 1975; Steeh, 1981) and at least on city size the difference between respondents and nonrespondents has been increasing (House and Wolf, 1978). The switch to telephone interviewing may also exacerbate the problem since response rates are generally lower than on personal interviewing (Groves and Kahn, 1979 and Jordon, Marcus, and Reeder, 1980). (It is unclear however whether differences tend to be similar.)

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[9]There is little empirical evidence that results from mail surveys are applicable to personal interviews and presumptive evidence that this may not be the case. First, it is obvious that the method of approach is quite different. A mail survey presumably reaches virtually all targets so there is essentially no not-at-homes problem. Second, since no physical intrusion into the household is needed it is likely that fear of crime, mistrust of strangers, and related factors are not related to nonresponse as strongly in mail surveys. Interest in the survey subject matter is however more likely to be related to refusals in mail surveys. Presumably most recipients glance at a mail questionnaire at least long enough to catch its substance and sponsorship. Eleanor Singer found on the other hand that 65 percent of the refusals in a national, face-to-face survey came before any part of the introduction had been read (1977;p. 150). Finally, the common use of incentives in mail surveys introduces yet another difference related to nonresponse patterns. In brief, there is sufficient evidence that various differences between mail and personal surveys could cause considerable differences in the characteristics of nonrespondents and the magnitude and direction of nonresponse bias.

To deal with nonresponse bias we need intensive methodological investigation. Such an investigation might include, among other things, a) the use of family members or neighbors as informants on a systematic basis,bB) experiments involving various types of incentive (e.g., Gunn and Rhodes, 1981), c) inclusion of refusal related variables based on expressed reasons for refusals (e.g., attitudes towards strangers, surveys, privacy, etc.) to test appropriateness of using temporary refusals, and d) short surveys of nonrespondents.

In addition we need a multiple faceted approach to nonresponse in which two or three techniques would be used to measure nonresponse bias for a variable (e.g., Andersen, et al., 1979). When definitive analysis such as is possible with the geographic/aggregate approach is not possible, cross-validating estimates by using several independent methods would be a good alternative.

By adopting a series of intensive and innovative methodological studies and by using a multiple indicators approach an assessment of the attributes of the hidden 25 percent and reliable estimates of nonresponse bias should be possible.

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