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I Hear You Knocking But You Can't Come In

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This article examines the effects of refusers and reluctant respondents to a general population survey on estimates of population distributions and multivariate relationships. Our data indicate that refusers do not constitute a simple random sample of respondents, and that efforts to correct for nonresponse by positing a "continuum of resistance" with refusers assumed to resemble reluctant respondents is not justified. Furthermore, we find that refusers and reluctant respondents can have serious effects on estimates of the relationship among variables. We do this by using analysis of covariance to assess the effect of callbacks on sample estimates and an imputation technique that can be used routinely in personal interview surveys to detect the magnitude and significance of refusal bias on final sample estimates.

I Hear You Knocking But You Can't Come In

The Effects of Reluctant Respondents and Refusers on Sample Survey Estimates

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Response rates to surveys of all kinds have been declining steadily and dramatically since the 1950s despite more elaborate and extensive callback policies (Benus and Ackerman, 1971: 28; Hawkins, 1975: 461; American Statistical Association Conference, 1974; Steeh, 1981).[1] This disheartening trend makes it all the more urgent that survey researchers carefully investigate the ways in which nonresponse bias can affect their conclusions.

Though it spans four decades, the literature on nonresponse provides few replicated findings. There are a number of reasons for this. First and most obvious, information about people who

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do not respond to surveys is, by definition, always very limited. Second, most published studies are based on data from a single survey; few attempts have been made to compare the findings of different studies. Third, summary articles often neglect to differentiate between nonresponse to mailed questionnaires and personal interviews. There is evidence that people who refuse to answer mailed questionnaires are not the same kinds of people who refuse to answer questions posed by an interviewer. Response rates, for example, to mailed questionnaires tend to be much lower than those to personal interview questionnaires (Benus and Ackerman, 1971: 27). In addition, the strongest and most consistent demographic correlate of refusal rate in the personal interview situation is central-city residence. We suggest in a later paper that this is related to heightened fear of crime in central-city areas, an explanation that is not relevant to mailed questionnaires. For these reasons, we compare our findings only to those studies dealing with nonresponse to personal interviews. Fourth and finally, the term "nonrespondent" has been used loosely in the literature. At times it includes varying combinations of designated respondents who refused to be interviewed, who were never at home, who were absent for varying periods of time, or who were reluctant to be interviewed. As the American Statistical Conference Survey of Human Populations concluded, comparisons of different studies have thus been very difficult to make (American Statistical Association Conference, 1974: 30-31). In this article, only people who clearly refused to be interviewed are treated as *refusers*.[2] We made this decision for two reasons. First, in our data there were many more refusals than not-at-homes (19.0% of attempted interviews [n = 259] versus 4.1% [n = 56]). Second, designated respondents who were never at home did not resemble refusers demographically (see Kish, 1965: 537; Bebbington, 1970: 179).[3] The term *reluctant* or *difficult-to-reach* respondent will be used to refer to designated respondents who required an extensive number of callbacks before agreeing to be interviewed.

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DATA AND APPROACH

Our sample consists of 1365 interviews attempted between August 1977 and February 1978 in northern California: The principal purpose of the survey was to investigate the association between community characteristics and social networks. Topics covered by the Northern California Community Survey included: (1) questions designed to produce descriptions of respondents' personal social networks; (2) items concerning respondents' geographic mobility, attitudes about their communities, and opinions on larger social issues such as abortion and women's rights; (3) measures of psychological well-being; and (4) standard demographic information. A stratified random sample was drawn from residents in a region demarcated by an arc running 200 miles north to 200 miles east of San Francisco.[4] The strata were:

(1) *Central cities:* census tracts in San Francisco, Oakland, and Sacramento.
(2) *Inner suburbs:* census tracts within about fifteen miles of downtown San Francisco, or ten miles of Oakland or Sacramento
(3) *Outer suburbs:* other tracts in the San Francisco-Oakland SMSA or in the Sacramento urbanized area
(4) *Small cities:* of the remaining communities in the region, tracts in cities over 10,000 in population in 1970
(5) *Small towns:* incorporated or unincorporated "places" between 2500 and 9999 population in 1970

Communities were selected from within each stratum to ensure a representative distribution of income. Next, two neighborhoods within each community were randomly chosen, and interviews were conducted in selected households within these small areas.[5] The selection of households and interviewing was conducted by the Survey Research Center, University of California, Berkeley.[6]

Survey interviewers systematically gathered information on the following items for people who refused to be interviewed: urbanization of the community (henceforth called stratum), age, marital status, dwelling type, race, sex, and labor force status.[7]

TABLE 1
Data Availability (refusers) and Number of Callbacks (respondents and refusers)

Panel A: Percentage of Refusers for Whom Data Available*

Strata	Age**	Marital Status	Dwelling Type	Race	Sex	Labor Force Status
100.0	60.2	47.9	95.0	91.1	98.8	37.1

Panel B: Percentage of Respondents and Refusers by Number of Callbacks

	1***	2-3	4-6	7-9	10-25	
Respondents	99.6	97.2	74.7	60.9	48.5	(1050)
Refusers	0.0	12.8	25.3	39.1	51.5	(259)
N per call	(236)	(460)	(383)	(133)	(97)	(1309)

*N = 259.
**Age was approximated by the interviewer in around one quarter of these cases.
***See note 7.

They obtained these data in several ways. The most important source of information was the household enumeration form that interviewers completed for each household contacted. The enumeration form contains questions on each household member including marital status (relationship to head), age, and labor force status. Interviewers' personal observations gave race and housing type. Thus, completed enumerations supplied us with necessary data on the designated respondent, even if that person refused to be interviewed. Generally, the form was completed with the first adult who answered the door. In some cases this person turned out to be the designated respondent, and terminated the initial contact before supplying all the information. In those cases we relied on interviewer observation for items such as age. Other types of information, such as labor force status, unless supplied by another household member, could not be obtained. Finally, for some designated respondents there was neither enumeration data nor direct visual contact. In these cases we can only report housing type and stratum. Fortunately, these cases are relatively few. Panel A of Table 1 summarizes the amount of information we were able to obtain about refusers. Panel B of Table 1 shows the distribution of responses and refusals by the number of callbacks.

We organize our discussion of the effects of callback procedures and refusals on survey estimates using a classification scheme developed by Hawkins (1977). *Terminal distributional bias* refers to the difference between the final sample and the group of refusers on selected demographic characteristics, while *interim distributional bias* refers to the demographic difference between the sample at various callback periods (e.g., after one, two, three, or four callbacks) and the final sample. Substantive bias affects the estimates of mean values of particular variables and estimates of relationships among variables. *Interim substantive bias* involves comparisons among respondents at different callback periods; *terminal substantive bias* makes comparisons between respondents and refusers.

TERMINAL DISTRIBUTIONAL BIAS

Most studies that have attempted to discover how nonrespondents differ from respondents have necessarily been case-specific, i.e., nonresponse has been considered only in relation to a particular sample drawn at a particular time in a particular place in order to investigate a particular problem.[8] Given this limitation, it is not surprising that demographic descriptions of nonrespondents have varied from one study to the next. This variation has led some authors to conclude that trying to identify "the nonrespondent" is futile and that survey researchers would learn more about nonresponse by focusing on such things as the substance of the survey and interviewer characteristics (Palmer, 1968: 73; Hawkins, 1977: 22 citing Cannell, 1964). We disagree. In many instances we discovered that our conclusions regarding the demographic correlates of refusal were supported by other investigators whose data came from surveys of different topics conducted at different times and in different places.

We found refusers to be significantly different, with some important qualifications, from those who completed the survey in strata, age, marital status, and dwelling type. Table 2 summarizes this information.

Table 2 indicates that:

(1) Refusers were far more likely to live in central cities as opposed to inner suburbs or outer suburbs of central cities, small cities, or small towns. The link between nonresponse and urban residence is nearly unanimous in the literature, although urbanization is defined differently by various authors. Mayer (1964: 29-30) found that refusals were more frequent in urban as opposed to rural areas. Kish (1965: 538) reports that large-city dwellers comprise a higher proportion of both not-at-homes and refusals. Hawkins (1977: 99) found that nonrespondents tended to live in Detroit rather than its suburbs, although in some surveys this trend appeared to be due largely to not-at-homes rather than refusals. Benus and Ackerman (1971: 28, 31) report that response rates are roughly inverse to size of place. In addition, they report that although the response rate has been dropping for all strata since the 1950s (in large part, they contend, because of increasing refusals), the gap has also been widening between larger and smaller areas (see also, House and Wolf, 1978; DeMaio, 1980: 228; Steeh, 1981: 45-46). Why city dwellers are consistently more prone to refuse than people living in less urban areas is an open question and one that we intend to investigate in a subsequent paper.[9]

(2) People who were 50 years or older were more likely to refuse than were those between 18 and 49. Within strata, this relationship remained significant in central cities ($\chi^2 = 48.9$; $p < .001$), inner suburbs ($\chi^2 = 41.1$; $p < .001$); small cities ($\chi^2 = 39.2$; $p < .001$), and small towns ($\chi^2 = 5.5$; $p < .02$). In the outer suburbs, the relationship was close to significance when we controlled for strata ($\chi^2 = 3.6$; $p < .06$). Not surprisingly, it is difficult to get information on the age of nonrespondents, and so there is little information published on the relationship between age and nonresponse. While we, too, had difficulty in this regard and thus report a great deal of missing information on the age of refusers, information that has been reported on age and nonresponse reinforces our own findings (Hawkins, 1977: 107; Hawkins, 1975: 478; Palmer, 1968; Kish, 1965: 537; Benus and Ackerman, 1971: 31; DeMaio, 1980: 227).[10]

TABLE 2

Comparison of Respondents and Refusers on Selected Demographic Characteristics

STRATA	CENTRAL CITY	OTHER					
respondents	71.1%	82.9%	X^2=19.9				
refusers	28.9%	17.1%	p .001				
TOTAL	(294)	(1015)					
AGE	18-21	22-29	30-39	40-49	50-59	60+	
respondents	86.9%	92.7%	88.1%	89.5%	83.6%	80.5%	X^2=20.3
refusers	13.1%	7.3%	11.9%	10.5%	16.4%	19.5%	p .01
TOTAL	(84)	(275)	(278)	(152)	(165)	(251)	
MARITAL STATUS	MARRIED	SEPARATED	WIDOWED	DIVORCED	NEVER MARRIED		
respondents	85.6%	84.7%	92.1%	96.6%	95.7%	X^2=29.5	
refusers	14.4%	15.3%	7.9%	3.4%	4.3%	p .001	
TOTAL	(647)	(59)	(89)	(148)	(230)		
DWELLING TYPE	SINGLE FAMILY	DUPLEX/TOWN/ROW	LOWRISE	HIGHRISE	MISCELLANEOUS		
respondents	81.7%	77.9%	84.4%	70.3%	84.2%	X^2=10.6	
refusers	18.3%	22.1%	15.6%	29.7%	15.8%	p .05	
TOTAL	(775)	(145)	(231)	(101)	(38)		
RACE	WHITE	NOT WHITE					
respondents	81.4%	83.3%	X^2=.35				
refusers	18.6%	16.7%	p .6				
TOTAL	(1124)	(183)					
SEX	MALE	FEMALE					
respondents	82.2%	79.0%	X^2=2.11				
refusers	17.8%	21.0%	p .15				
TOTAL	(568)	(738)					
LABOR FORCE STATUS	FULL TIME	PART TIME	LOOKING/LAID OFF	RETIRED	NOT WORKING		
respondents	90.3%	95.0%	96.2%	90.2%	93.4%	X^2=5.8	
refusers	9.7%	5.0%	3.8%	9.8%	6.6%	p .3	
TOTAL	(609)	(119)	(52)	(153)	(212)		

(3) Married and separated people were more likely to refuse than people who were widowed, divorced, or had never married. When we controlled for strata, this relationship remained significant in central cities, inner suburbs, and small towns.[11] Although the chi-square for the small-city stratum was not significant, the percentage refusing who were either married or separated was much greater than the percentage refusing in the other three categories. Hawkins (1977: 104) finds a similar relationship in seven of the eight surveys he analyzed. Because of the amount of missing data we report on refusers' marital status, our finding is tentative.

(4) People in either high-rise apartments or in duplex, town, or row houses were more likely to refuse than those living in either single-family houses or low-rise apartments. However, since all of our refusals who lived in high-rise apartments also lived in the central city, it was impossible to tell whether the higher refusal rate for high-rise dwellers was related to dwelling type or merely reflected strata. Furthermore, when we controlled for strata, the relationship between refusal rate and dwelling type disappeared. Little has been reported on the relationship between refusal rate and dwelling type, and comparisons are often difficult because dwelling types have been classified in various ways (see Lagay, 1969). However, our findings differ from those of Hawkins (1975: 479, 1977: 101), who reports that residents of single-family dwellings comprise a larger proportion of refusers than respondents.[12]

(5) We found no significant difference in race between respondents and refusers.[13] This is consistent with the reports of other researchers who have also either found no relationship between refusal or nonresponse and race, or whose findings on the matter have been contradictory (Schuman and Gruenberg, 1970: 258; Lagay, 1969: 617; Benus and Ackerman, 1971: 31; Hawkins, 1975: 478; Hawkins, 1977: 95; De Maio, 1980: 227). Sex was not significantly associated with refusal in this study, a finding that is again consistent with the contradictory and weak relationships reported in the literature (Hawkins, 1975: 478; 1977: 16, 106; De Maio, 1980: 228). Finally, labor force status was not associated

with refusal to be interviewed, although as in the case of both age and marital status, the amount of missing data on this variable precludes any firm conclusion.

INTERIM DISTRIBUTIONAL BIAS

We investigated the interim distributional bias in our sample for three reasons. First, this is useful to survey researchers in deciding how many attempts they should make to contact respondents. Second, identifying particular callback periods when interim distributional bias was most pronounced was a first step in our attempt to examine the relationship between interim *distributional* and interim *substantive* bias. *Direct* examination of the relationship between the two types of bias is not possible between respondents and refusers; although we have demographic information on refusers, we do not know their answers to the survey items that are our dependent variables.

Third, despite the obvious fact that most interim nonrespondents ultimately become respondents and not refusers, researchers have often assumed that the more difficult to reach or reluctant the respondent, the more closely he or she approximates the people who never respond. That is, designated respondents are assumed to fall along a "continuum of resistance" that ranges from easy to interview, to hesitant—requiring a good deal of effort on the part of the interviewer before cooperating—to actual refusers. The continuum of resistance notion has a certain logical appeal. As a result, the technique of weighting refusals in accordance with information available on hesitant or hard-to-reach respondents has periodically been suggested as one of the most practical ways of correcting for nonresponse bias in surveys (Platek, Singh, and Tremblay, 1978: 158, 159).[14] For this reason we felt it important to investigate carefully the assumption behind the continuum of resistance—that the more difficult it is to secure the cooperation of a designated respondent, the more he or she resembles the person who actually refuses to be interviewed.

Interviewers made up to 24 calls to designated respondents in our sample. Of the final sample, 72% had been interviewed by the

fourth call and 96% by the tenth call. In order to examine interim distributional bias, we compared respondents accumulated through various number of callbacks with the final sample of respondents on seven demographic characteristics: age, strata, dwelling type, labor force status, marital status, race, and sex, and on three substantive variables: number in household, number of kin in network, and attitude toward women with young children working outside the home. Table 3 summarizes this information. The following are some of the patterns that emerged:

(1) People over 60 years old were noticeably overrepresented, particularly after the first call but continuing through the fourth, while people between 30 and 39 were underrepresented through the fourth call. Dunkelberg and Day (1973: 162) also report that those over 60 were more likely than any other age group to be contacted on the first call. Hawkins (1977: 48), examining six general population surveys conducted between 1956 and 1973, notes the same general trends we did for both the elderly and middle-aged groups.[15] Our data indicate that the continuum of resistance hypothesis is untenable with respect to age. While people over 60 were initially easier to interview, overall they were more likely to refuse. People between 30 and 39, on the other hand, took more effort to contact, yet were ultimately less likely than elderly people to refuse.[16]

(2) Central-city and inner suburb residents were initially underrepresented in the sample. This underrepresentation persisted through the fourth call for central-city residents. On the first call, and to some degree on the second, small-town residents were overrepresented. Several other studies also report later response by city residents (Mayer, 1964: 29; Dunkelberg and Day, 1973; Robins, 1963: 280). Hawkins (1977: 53, 65), however, found that Detroit city residents were easier to contact in six of eight samples, while those who lived in suburban areas were harder to contact. Yet his analysis of two restricted population surveys in the same area—one of whites, the other of employed women—showed the opposite pattern. From our data it is apparent that central-city residents are either more hesitant respondents or more difficult to find than people living in less urban areas, and

TABLE 3
Change in Distributions by Number of Contacts Required to Complete Interview

	1*	1+2	1+2+3	1-4	1-9	FINAL
AGE						
18-21	6.8%**	6.9%	6.6%	6.7%	7.1%	7.0%
22-29	21.3	22.7	24.3	24.7	24.2	24.3
30-39	18.7	19.7	19.5	20.2	23.0	23.4
40-49	11.1	11.4	12.8	12.4	13.0	13.0
50-59	12.3	14.9	13.1	13.6	13.3	13.2
60+	29.7	24.4	23.8	22.8	19.6	19.3
cumulative N	(235)	(463)	(635)	(757)	(1002)	(1049)
N per call	[235]	[228]	[172]	[122]	[245]	[47]
STRATA						
CENTRAL CITY	17.0%	16.0%	16.5%	17.0%	19.3%	19.9%
INNER SUBURB	18.3	17.7	19.7	19.8	21.0	20.8
OUTER SUBURB	19.6	22.5	20.8	21.2	20.2	19.7
SMALL CITY	20.4	22.2	22.2	21.8	20.1	20.1
SMALL TOWN	24.7	21.6	20.9	20.2	19.2	19.5
cumulative N	(235)	(463)	(636)	(758)	(1003)	(1050)
N per call	[235]	[228]	[173]	[122]	[245]	[47]
DWELLING TYPE						
SINGLE FAMILY	67.1%	67.3%	64.8%	62.8%	61.3%	60.6%
DUPLEX, TOWN, ROW	6.8	8.7	10.1	10.9	10.8	10.8
LOWRISE APARTMENT	16.7	15.8	15.3	16.8	18.1	18.7
HIGHRISE APARTMENT	6.8	6.3	6.6	6.4	6.8	6.8
OTHER	2.6	1.9	3.2	3.2	3.0	3.1
cumulative N	(234)	(462)	(634)	(755)	(997)	(1044)
N per call	[234]	[228]	[172]	[121]	[242]	[47]
LABOR FORCE STATUS						
FULL TIME	28.6%	40.5%	43.6%	47.2%	51.5%	52.4%
PART TIME	10.3	10.6	11.3	11.4	11.0	10.8
LOOKING/LAID OFF	5.1	5.6	5.7	4.9	4.9	4.8
RETIRED	22.6	17.7	16.2	15.2	13.6	13.2
NOT WORKING	33.3	25.5	23.1	21.4	19.1	18.9
cumulative N	(234)	(462)	(635)	(757)	(1002)	(1049)
N per call	[234]	[228]	[173]	[122]	[245]	[47]
MARITAL STATUS						
MARRIED	63.0%	61.6%	58.6%	56.1%	54.0%	52.8%
SEPARATED	3.4	3.2	3.5	4.0	4.8	4.8
WIDOWED	8.1	8.9	9.0	9.2	8.1	7.8
DIVORCED	8.1	8.4	10.7	11.8	12.8	13.6
NEVER MARRIED	17.4	17.9	18.2	18.9	20.4	21.0
cumulative N	(235)	(463)	(636)	(757)	(1002)	(1049)
N per call	[235]	[228]	[173]	[121]	[245]	[47]

(continued)

TABLE 3 (Continued)

RACE						
WHITE	89.8%	89.6%	88.4%	88.4%	87.0%	87.1%
NOT WHITE	10.2	10.4	11.6	11.6	13.0	12.9
cumulative N	(235)	(463)	(636)	(758)	(1003)	(1050)
N per call	[235]	[228]	[173]	[122]	[245]	[47]
SEX						
MALE	40.0%	43.2%	44.5%	44.6%	44.6%	44.5%
FEMALE	60.0	56.8	55.5	55.4	55.4	55.5
cumulative N	(235)	(463)	(636)	(758)	(1003)	(1050)
N per call	[235]	[228]	[173]	[122]	[245]	[47]
TOTAL NUMBER IN HOUSEHOLD						
1	17.4%	17.3%	18.2%	19.1%	21.2%	22.3%
2	39.6	38.0	38.7	38.4	36.8	36.6
3	14.9	16.0	16.5	17.3	16.8	16.8
4	13.6	17.1	15.9	15.3	15.6	15.1
5+	14.5	11.7	10.7	9.9	9.6	9.2
cumulative N	(235)	(463)	(636)	(758)	(1003)	(1050)
N per call	[235]	[228]	[173]	[122]	[245]	[47]
# OF KIN IN NETWORK						
0	2.6%	2.4%	2.0%	2.1%	2.1%	2.1%
1-3	11.9	13.8	15.3	16.1	18.1	18.4
4-5	17.0	17.1	17.8	18.1	18.9	18.9
6-7	22.1	19.4	18.7	18.3	17.6	17.8
8-10	23.8	22.2	20.0	20.3	18.4	18.4
11-16	16.2	17.7	18.7	18.7	18.3	18.3
17-44	6.4	7.3	7.5	6.3	6.4	6.1
cumulative N	(235)	(463)	(636)	(758)	(1003)	(1050)
N per call	[235]	[228]	[173]	[122]	[245]	[47]
SHOULD , WITH YOUNG KIDS WORK?						
GENERALLY OKAY	31.5%	34.6%	35.9%	35.5%	36.1%	36.1%
IT DEPENDS	9.4	10.0	9.1	9.8	9.0	9.0
GENERALLY BAD IDEA	59.1	55.4	55.0	54.7	54.9	55.0
cumulative N	(235)	(462)	(635)	(757)	(1001)	(1048)
N per call	[235]	[227]	[173]	[122]	[244]	[47]

*Column headings refer to all those interviewed on the first attempted contact, all those interviewed on the first and second attempted contacts, all those interviewed on the first, the second, and the third attempted contacts, and so on.
**All percentages based on cumulative N.

that they are more likely than anyone else to refuse to be interviewed.

(3) People who lived in single-family houses were somewhat overrepresented through the fourth call. Hawkins (1975: 471)

reports the same pattern through the third call, though he later reports (1977: 53) somewhat mixed results from two other surveys. Though there was a slight tendency for people living in single-family houses to be less likely to refuse, this tendency did not remain after we controlled for strata. Thus, no evidence can be found to support the notion of a continuum of resistance on the basis of dwelling type.

(4) The sample continued to underrepresent full-time workers through the fifth callback. The distortion was particularly severe through the third callback. People who were not working were overrepresented through the fourth callback, and retired people were overrepresented through the third. Mayer's (1964: 28) findings appear to corroborate our own. He found that through the third callback, the percentage of completed interviews was ordinarily smaller during working hours. Since we discovered no significant differences between the final sample of respondents and refusers in labor force status, it appears that this interim distributional bias does not characterize the final sample, and is, again, inconsistent with the continuum hypothesis.

(5) Through the first four callbacks, our sample showed a noticeable overrepresentation of married people, while divorced people and people who had never been married were initially underrepresented in the sample. Interestingly, the distribution of widowed people remains consistent regardless of the number of callbacks, even though retired people and people over 60 were at first overrepresented. Hawkins (1977: 49) reports a different pattern. In five out of six samples, married people were underrepresented, though only slightly and usually only on the first callback. Since our data on terminal distributional bias indicate that married people are more likely to refuse than other marital groups, no continuum of resistance is discernible insofar as marital status is concerned.

(6) There was little over or underrepresentation of any racial group at any callback period. Hawkins (1977: 38) reports very mixed trends in the relation of race to interim distributional bias. Since the literature on this point has failed to identify any clear-cut trends, we suspect that race is not a critical variable for distinguishing reluctant respondents or refusers.

(7) On the first call women were somewhat overrepresented. After the first call, however, this overrepresentation virtually disappeared. Mayer (1964: 26) found that on the first call the percentage of males interviewed was usually lower on Monday through Friday before 6:00 p.m. and Saturday and Sunday, controlling for degree of urbanization. Hawkins (1977: 35) found males to be consistently underrepresented in the six general population surveys he analyzed, especially after the first call. However, while men tend to be underrepresented in the beginning stages of interviewing, there is no clear evidence that they are more likely to refuse. Again, it does not appear that a continuum of resistance exists with regard to sex.

(8) Table 3 indicates that at some callback periods, single-person households and people with relatively few kin (one to three) in their networks were underrepresented through the fourth call. On the other hand, respondents living in households of more than five people were overrepresented through the second call, as were respondents who listed between eight and ten kin in their personal networks. And on the first call, people who felt that it was generally okay for women with young children to work outside the home were underrepresented, while those who felt it was generally a bad idea for these women to work were overrepresented.

In conclusion, these data do not support the hypothesis that those who are difficult to reach are most like final refusers. We rejected the continuum-of-resistance notion for age, housing type, labor force status, marital status, and sex. Our evidence suggests that assessing or correcting for nonresponse bias on the assumption that a continuum of resistance exists is unwarranted.[17]

INTERIM SUBSTANTIVE BIAS

We were concerned with more than the effect of completion rates on sample representativeness and univariate distributions. We wanted to examine how callback procedures used to obtain our sample may have affected what we found out about

substantive relationships among variables. To address this issue we divided respondents into two categories. Easy-to-contact respondents were interviewed within four callbacks, while difficult-to-contact respondents required five or more callbacks.

There were two parts to our analysis. First, we estimated separate regression equations for easy- and difficult-to-contact respondents, and, using analysis of covariance techniques, determined whether the partial slopes differed between the two groups on each independent variable used in the regressions. This allowed us to determine whether or not the effects of each independent variable were the same for easy-to-contact and difficult-to-contact respondents. Next, we computed an "average gap" between the two groups for each dependent variable used in the regressions (see Wright, 1979). The average gap represents the difference on the dependent variables between respondents who share the same personal characteristics, but differ in how difficult it was to reach them. The average gap is calculated for a hypothetical respondent whose values on the independent variables fall midway between the means of those variables for each type of respondent. (For example, if AGE_e represents the mean age of respondents who were easy to contact, and AGE_d the mean age of difficult-to-reach respondents, our hypothetical respondent has a mean age equal to $[AGE_e + AGE_d/2]$.[18] Each group's deviation from this "average" age is then inserted into the regression equations used to estimate the dependent variables for each group. This shifts the intercepts from the y-axis to a point where the independent variables all have the average values of the two groups. We then evaluated the difference between these "adjusted" constant terms for the regression equations calculated within each group of respondents. The adjusted constant term in each regression represents the predicted value of our hypothetical respondent, and the difference in constant terms—the average gap—represents the difference between easy- and difficult-to-contact respondents that cannot be accounted for by differences on the independent variables. The average gap is tested for statistical significance using standard covariance techniques.[19]

Our dependent variables represent a range of different types of measures from demographics (household income and size of

household) to a social network variable (total number of kin named by respondents in their personal networks), and an attitudinal item about the role of women: "How do you feel about a woman with young children working, even if the family doesn't need the money?"[20] We expect that all but the social network variable will be familiar to readers. Since Tonnies (1957), social theorists have speculated about the effect of increased urbanization on interaction patterns. The shift from *gemeinschaft* to *gesellschaft* allegedly led to more impersonal relations and the decline of extended family ties. The number of kin in a respondent's social network is a measure of one kind of change that is supposed to accompany increased urbanization (see Wirth, 1939). The independent variables included respondents' age, sex, race, marital status, housing type, and strata.

FINDINGS: DIFFERENCES IN PARTIAL REGRESSION COEFFICIENTS

Table 4 presents the unstandardized regression coefficients and R^2 for each dependent variable by our two respondent groups. Table 5 presents the differences in partial slopes for the two groups.

For three of the four comparisons, the regression equations demonstrate substantially better fit for difficult-to-reach respondents compared to the easy-to-reach respondents. The improvement in explained variance ranges from a low of 26% for household income, to a high of over 100% for number of kin named. There is no improvement in explained variance for the attitudinal item.

The improvements in R^2 are not due to simple sampling fluctuations. We tested the significance of the difference in multiple R using a test for the difference of simple correlations, adjusting for degrees of freedom (Blalock, 1972: 406, 467). All differences in multiple R are significant at $p < .05$. These differences in R^2 *are* due to the partial slope differences. We respecified each regression equation, eliminating those independent variables that had significant differences in Table 5. This reduced the differences in multiple R to nonsignificance.

TABLE 4

Unstandardized Regression Coefficients for Interim Substantive Bias by Difficulty in Contacting

DEPENDENT VARIABLE	ADJUSTED CONSTANT[1]	SEX[2]	AGE	MARITAL STATUS[3]	RACE[4]	DWELLING TYPE[5]	INNER SUBURB	OUTER SUBURB	SMALL CITY	SMALL TOWN	R^2 [a]
Total Number of Kin in Network											
EASY (N=729)	7.83	.759	-.028	1.54	-.203	1.60	.294	1.70	2.21	1.66	.098
(Standard Error)	(.196)	(.392)	(.011)	(.347)	(.564)	(.47)	(.675)	(.70)	(.653)	(.70)	
DIFFICULT (N=275)	7.55	1.94	-.002	3.25	.313	2.29	-.083	.751	.663	.386	.210
	(.292)	(.584)	(.021)	(.631)	(.768)	(.693)	(.847)	(1.00)	(.958)	(.986)	
Household Income											
EASY	8.0	-.624	-.032	1.55	.705	1.96	-.811	-.950	-2.50	-2.69	.240
	(.114)	(.228)	(.007)	(.202)	(.328)	(.273)	(.393)	(.407)	(.38)	(.407)	
DIFFICULT	8.43	-1.66	-.024	2.35	.247	2.19	-.349	-.756	-3.09	-2.20	.302
	(.188)	(.377)	(.013)	(.407)	(.496)	(.447)	(.546)	(.645)	(.618)	(.636)	
Household Size											
EASY	2.62	.111	-.030	.590	-.254	.632	.481	.539	.467	.333	.299
	(.044)	(.088)	(.002)	(.078)	(.127)	(.105)	(.151)	(.157)	(.147)	(.157)	
DIFFICULT	2.47	.192	-.025	1.34	-.48	.632	-.058	.333	.407	.209	.431
	(.064)	(.128)	(.005)	(.138)	(.168)	(.152)	(.185)	(.219)	(.210)	(.216)	
Women With Young Kids Work											
EASY	2.12	.042	.018	.025	.073	.078	.144	.248	.463	.298	.166
	(.032)	(.064)	(.002)	(.057)	(.092)	(.077)	(.110)	(.114)	(.107)	(.115)	
DIFFICULT	2.23	-.137	.021	.090	-.361	.076	.318	.573	.441	.470	.168
	(.054)	(.107)	(.004)	(.116)	(.141)	(.127)	(.156)	(.184)	(.176)	(.181)	

(1) Adjusted according to discussion in text on average gap.
(2) 1 = Male; 2 = Female.
(3) 1 = Married; 2 = Other.
(4) 1 = White; 2 = Other.
(5) 1 = Single Family; 2 = Other.
(a) Adjusted for degrees of freedom.

TABLE 5

Difference in Partial Regression Slopes Between Easy-to-Contact and Hard-to-Contact Respondents

DIFFERENCE IN UNSTANDARDIZED PARTIAL SLOPES

DEPENDENT VARIABLE	SEX	AGE	MARITAL STATUS	RACE	DWELLING TYPE	INNER SUBURB	OUTER SUBURB	SMALL CITY	SMALL TOWN
TOTAL NUMBER OF KIN IN NETWORK									
(Easy v. Difficult)	-1.18	-.026	-1.71	-.516	-.69	.377	.949	1.55	1.27
T-Value	(-1.68)	(-1.1)	(-2.37)	(.54)	(-.82)	(.348)	(.78)	(1.33)	(1.05)
Significance of T	*		**						
HOUSEHOLD INCOME	1.04	-.008	-.80	.458	-.23	-.462	-.194	.59	-.49
	(2.35)	(-.54)	(-1.76)	(.77)	(-.44)	(-.69)	(-.25)	(.81)	(-.65)
	**		*						
HOUSEHOLD SIZE	-.081	-.005	-.75	.226	0.0	.539	.206	.06	.124
	(-.52)	(-.93)	(-4.73)	(1.07)		(2.26)	(.76)	(.23)	(.46)
			***			**			
WOMEN WITH YOUNG KIDS WORK	.179	-.003	-.065	.434	.002	-.174	-.325	.022	-.172
	(1.44)	(-.67)	(-.50)	(2.58)	(.013)	(-.91)	(-1.5)	(.11)	(-.80)
				***					

***p < .01.
**p < .05.
*p < .10.

The data on slope differences indicate that there are one or two coefficients that differ significantly in each equation. For example, marriage supplements the number of kin named by all respondents, but this effect is twice as large for difficult-to-contact respondents compared to easy-to-contact respondents (3.25 versus 1.54). Similarly, women tended to name more kin in their social networks than did men, but the magnitude of this difference is again twice as large among difficult-to-contact respondents compared to easy-to-contact respondents (1.94 versus .759).

The significance that should be attributed to these findings in formulating a callback policy raises a contentious issue. We compared the regressions for easy-to-contact respondents (N = 757) to regressions for the entire sample (easy- + difficult-to-contact respondents, N = 1050). The differences in partial regression coefficients between easy- and difficult-to-contact respondents reported in Table 5 washed out in the easy versus total comparisons. Overall, therefore, there is no difference between our estimates of relationships for the entire sample and the estimates we would have generated had we stopped interviewing after four callbacks. This lack of significant differences is a consequence of both the size of the easy-to-contact group compared to the total sample and the magnitude of the effects differences between the easy- and hard-to-contact respondents. Some researchers may argue that the lack of significant differences between the easy-to-contact group and the final sample indicates that additional callbacks are not worth the extra time and effort. We believe, however, that because of the slope differences and differences in explained variance between easy- and difficult-to-contact respondents, these data indicate the importance of pursuing designated respondents. There are several arguments we find persuasive.

First, some variables may be central to a particular investigation for theoretical reasons. Significant differences in the magnitude—and perhaps the direction—of relationships in even one such variable may alter the conclusions of a study. Second, while we found only one or two significant differences in partial slopes, selection of other dependent variables might reveal three, four, or even all coefficients to be significantly different between groups.

TABLE 6
Average Gap Between Easy-to-Contact and Difficult-to-Contact Respondents

	Number of Kin Named in Network	Household Income	Household Size	Women With Young Children Work
(EASY vs. HARD)	.28	.43	.15	-.11
T-VALUE	(.796)	(-1.96) **	(1.93) *	(-1.75) *

**p < .05.
*p < .10.
Two-tailed t test.

The point is that we do not know beforehand whether each type of respondent is likely to have peculiarities that may affect sample estimates of important relationships. This ambiguity in our knowledge argues for an extensive callback policy. Third, while only two coefficients at most differed significantly in our examples, the differences in explained variance between each group of respondents were substantial for three of the four comparisons. While we did not find significantly different estimates of relationships between easy-to-contact respondents and the final sample for four variables, there could well be differences on other variables.

In general, depending on the size of the two groups and the strength of any differences between them, we might well discover that stopping data collection after four callbacks could lead to incorrect inferences. For these reasons we believe the extra effort and expense necessary for multiple contacts are worth the serious consideration of every survey researcher.

AVERAGE GAPS

Our data on average gaps—the difference between hypothetical persons who share the same values on the independent variables but who differ in how difficult it was to interview them—are presented in Table 6. In three of the four comparisons (household income and response to the attitudinal item on women working) there are statistically significant differences

between each group of respondents. This means that differences remain between easy- and difficult-to-reach respondents that cannot be accounted for by each group's differing scores on the independent variables included in the regression equations.

This residual difference is not due to sampling fluctuations. It may be a consequence of different processes at work within each group that determine scores on the dependent variable, e.g., there may be some variable not included in the equation for one or the other group that is also an important predictor of income or attitudes about women working (specification error). Alternatively, significant differences in the average gap can reflect systematic measurement error. For example, easy-to-contact respondents may have been interviewed relatively early in the interviewing process, when interviewers were less experienced and difficult-to-contact respondents may have been interviewed relatively late, when interviewers were more experienced and accurate.

Thus our data on the average gap between the two types of respondents support the conclusion we drew from the analysis of differences in partial regression coefficients: Whenever possible, investigators should actively pursue difficult-to-reach or reluctant respondents. Failing this, researchers face the possibility of incorrect estimates of substantive relationships that are critical to a study. Although we cannot specify beforehand the extent of this distortion or even its occurrence in a particular investigation, we feel that this uncertainty itself the strongest argument for an active follow-up procedure.

TERMINAL SUBSTANTIVE BIAS

Ultimately, we study nonresponse to find out if excluding some portion of a designated sample affects either population estimates of some attribute or the relationships among a set of variables. In the latter case, the observed relationship may be attenuated, or even worse, may be an artifact if respondent cooperation is related to either or both the independent and dependent variables.

There are two major contexts in which the effects of nonresponse need to be estimated; associated with each context is a set of strategies that can be applied to estimate the effect of refusals. The first context in which weighting adjustments are made is where population estimates of particular characteristics are known. Given missing data on a set of items, researchers have employed techniques that iteratively adjust sample distributions to conform to known population distributions, as in the raking of ratio estimates (Bailar, Bailey, and Corby, 1978: 193-195). Some strategies simply extrapolate from the responses of those respondents who most closely resemble the demographic characteristics of refusers (Fuller, 1974: 241-245; O'Neill, 1979: 219). For example, if a sample is known to underrepresent older persons, one may weight up older respondents in the sample to approximate the known distribution.

While differing in technical complexity, these methods share the assumption that the relationship among the variables that are weighted and dependent variables of interest is the same for refusers as it is for respondents. This assumption is rarely tested, however. We believe that this assumption is often inappropriate, and that it does not correct for the biasing effects of refusals on the relationship *between* variables. Only if the dependent variable is strongly correlated with the particular demographic variables about which one has information will the estimates correctly approximate the true population parameter.

The second context in which weighting is employed—when there is no known population distribution of characteristics—creates even greater difficulties. When the dependent variable is a response to an attitudinal item or some behavioral attribute that can only be elicited in the interview itself, researchers have been able to do little more than speculate about the potential bias introduced by refusals.

Among the strategies that have been proposed for this type of situation has been the development of weights on the basis of a selected subset of respondents (and not a known population parameter). In this case, refusers are treated like difficult-to-reach respondents (Goudy, 1976; O'Neill, 1979; Platek, Singh, and

Tremblay, 1978), and appropriate adjustments are made. Our results with respect to terminal and interim distributional bias suggest that this, too, may be an inappropriate technique in some cases.

Although researchers have devoted substantial effort to resolving the issue of which one of the many correction strategies is the most appropriate, little attention has been devoted to thoroughly exploring the prior issue: How can we estimate the likelihood that the refusal rate has distorted the observed relationships? It may well be that there is no need to develop extensive corrections in some cases, and it is to this issue that we now turn.

Unlike our discussion of interim bias, which relied upon interviewer reports of refusers' demographic characteristics, here we need to fall back on an estimation procedure to compare respondents with refusers. Using a procedure suggested by Hawkins (1977) we estimated regression equations for several network and demographic variables employing information about respondents only. The dependent variables used were the total number of network members living in a respondent's household, household income, and the total number of kin named by respondents in their network (in and out of the immediate household). The choice of predictors was limited to those variables for which we had sufficient information on both respondents and refusers. We projected group means on the dependent variables for the refusers by multiplying the partial regression coefficients for respondents by the refusers' means on the independent variables. This is analogous to Duncan's substitution of Black means into income regressions for whites (Duncan, 1968) to assess the effects of racial discrimination on earnings. A t test was performed on the difference between this imputed mean for refusers, and the respondents' actual mean. This procedure assumes that the partial slopes are the same for both respondents and refusers. While we obviously cannot compute the covariances between the independent variables and the dependent variables for refusers, we can perform a partial test of the hypothesis of equal slopes. If the slopes are equal, we would expect the covariances among the independent variables to be the same for

respondents and refusers. We tested this assumption by comparing the covariance matrices of the independent variables for respondents and refusers using a likelihood test suggested by Morrison (1976: 309). The matrices were not significantly different. However, because listwise deletion dramatically reduced the number of cases used in the comparison, we emphasize caution in evaluating these findings. The results of the t tests appear in Table 7.

For each of the three comparisons the differences in means are statistically significant. Respondents, for example, named almost one more kin member in their networks than we projected for refusers. Respondents were also more likely to have higher incomes than refusers. Refusers, on the other hand, were more likely than respondents to include household members in their social networks. These findings should be treated with caution, since the assumption of equal slopes for refusers and respondents relies on comparisons of covariance matrices that were generated by a reduced number of cases. Obviously, where researchers find the covariance matrices to be equivalent and where these matrices use significant quantities of information on both groups, greater trust can be placed in those differences that are found. In the case where there are significantly different covariances among the independent variables, the projected means for refusers are less reliable, and a t test may lead to incorrect inferences. Different covariance matrices do suggest, however, that the process linking variables is likely to differ among refusers compared to respondents, itself an important finding.

These findings tentatively suggest that researchers should gather demographic data about refusers and should pursue the type of analysis reported here before weighting their data. This analysis prevents the indiscriminate application of weighting adjustments for refusals in situations where they may not, in fact, be necessary. Also, when adjustments are necessary and when the number of cases used to test the significance of differences for respondents' and refusers' covariance matrices is large, this procedure appears to us more reasonable than weighting diffi-

Terminal Substantive Bias: Mean Differences Between Respondents and Refusers

DEPENDENT VARIABLE	ACTUAL RESPONDENT MEAN	ESTIMATED REFUSER MEAN	T-VALUE
Number of Household Members Named in Social Network	1.19	2.60	-17.11***
Total Number of Kin in Social Network	7.77	6.88	2.97**
Total Household Income (a)	8.13	6.91	4.06***

(a) Grouped categories.
***p < .001.
**p < .01.

cult-to-contact respondents or respondents who resemble refusers demographically.

CONCLUSION

Our investigation of refusers in the Northern California Community Study and our review of past studies suggest a few critical points about this variety of nonresponse. First, it is apparent that refusers do not ordinarily constitute random samples of respondents; consequently, refusal is a potentially serious type of bias in surveys. Refusers come disproportionately from particular subgroups of the population. In this regard we urge researchers to be mindful of higher refusal rates among central-city residents and older persons. Although we are not sure why these two groups seem particularly prone to refuse (though we suspect fear of crime plays a part), answering this question will enable researchers to develop special strategies aimed at increasing the response rate of these groups.

Second, we found the assumption that the more callbacks it takes to reach a designated respondent, the more likely that person resembles a refuser, is not warranted. For only one out of seven variables considered did respondents who were difficult to reach appear similar to those who ultimately refused to be interviewed. In addition, although interim distributional bias was not particularly severe in the case of some demographic variables, we demonstrated that the magnitude of relationships between particular variables was significantly different at different callback periods. For this reason, and also because processes linking variables differ in some cases for easy- and difficult-to-reach respondents, we stress the importance of pursuing reluctant respondents.

Finally, our study suggests that not only are respondent and refusal groups different demographically, but that these differences may, in some cases, translate into significantly different estimates of dependent variables for the refusal and respondent

groups. Although the extent of terminal substantive bias depends on a variety of factors other than the number who refused, the refusal rate can be an important determinant of this bias.

NOTES

1. Hawkins (1975: 483) reports that the Detroit Area Survey response rate dwindled from 85% or more in the 1950s to current rates of 70% or less.

2. Steeh (1981: 51-52) has recently demonstrated that increases in nonresponse rates since the 1950s are primarily due to increased refusals, compared to other types of designated respondents who are not interviewed.

3. Wilcox (1977) makes an interesting case for considering the potential for interaction between these two sources of nonresponse bias.

4. The sampling design for the NCCS was a disproportionately stratified, multistage cluster design. A complete description of the sample can be found in the methodological appendix to Fischer (1982). Since cluster sampling usually generates larger standard errors than a simple random sample of the same size, we were concerned that our estimates of standard errors generated by SPSS were too small, causing us to overestimate the significance of differences in our data. Fischer examined this issue for the study as a whole. He computed jackknife estimates for the confidence intervals of correlation and partial regression coefficients (see Mosteller and Tukey, 1968: 133-160). He found that for several variables the jackknife estimates were smaller than the estimates generated by SPSS, which assumes simple random sampling. In general, he suggests inflating the standard errors by approximately 10% for the entire data set.

5. Places that were predominantly Black (i.e., more than 40% in the 1970 Census) were excluded from this sample.

6. The interviewing staff was kept purposefully small to ensure adquate quality control over the fieldwork, and because of the complexity of the survey instrument. Most of the interviewers had experience in other surveys. Prior to entering the field, all interviewers were trained in the administration of the instrument. Their work was closely scrutinized by both the SRC field staff and the NCCS principal investigator (Claude Fischer) and project director (M. Sue Gerson). To minimize interviewer effects, all interviewers were rotated across all five strata, and each community was covered by at least two different interviewers. For a complete description of the fieldwork and interviewer training, see Fischer (1982).

7. In most cases, designated respondents were not classified as refusers until they had refused to be interviewed three times.

8. O'Neil (1979: 220, 231) points to the difficulties of generalizing about nonresponse on the basis of highly specialized versus general population samples.

9. Among the reasons suggested by the American Statistical Conferences Survey on Human Populations (1974: 32) are fear of crime, suspicion of strangers, miscellaneous life-style changes, and a need or desire to avoid unusual involvement. DeMaio (1980) explores the effect of the Privacy Act on refusal rates and concludes that it does not have the feared detrimental effects on survey response.

10. Hawkins analyzed survey response over a twenty-year period. He finds that "there has been a shift over time from a terminal non-response group with especially large proportions of older (over sixty-five) persons to one with a large proportion of middle-aged respondents" (1977: 97). His age classifications, however, are somewhat different from our own and he includes in his "middle-aged" category (35-64) many people who are over 50. DeMaio (1980) reports that people 30-49 are also more likely to refuse.

11. In the inner suburbs, however, no separated people refused to be interviewed.

12. We calculated the chi square for Hawkins's data. The chi square was not statistically significant. We also calculated the Hawkins "percent difference" measure for our housing type data, after regrouping it to conform to his categories. We found that for two comparisons (single-family dwellings and two- to four-unit apartment and row houses) the differences in our data are opposite in sign compared to Hawkins's findings; and for a third comparison (apartments, trailers, other), we found no difference between respondents and refusers, though Hawkins reports a +17-point difference.

13. The fact that places where the population was over 40% Black were excluded from the Northern California Community Study sample makes this conclusion tentative. In addition, the sample contains only people who spoke English.

14. On this subject, see Ellis et al. (1970) and Filion's (1976) reply to them. Robins (1963) suggests refining the continuum of resistance concept by distinguishing between late respondents and potential nonrespondents.

15. Hawkins's results, however, are not easily comparable to our own since we use different age classifications.

16. In our sample, thirteen people between 30 and 39 refused to be interviewed. Assuming statistical independence, we would expect 23.

17. According to Ellis et al. (1970: 104), this is "clearly the most common approach" in estimating bias due to nonresponse in surveys. Another way of analyzing the data that does not, however, address the issue of the validity of the continuum-of-resistance notion, is to compare difficult-to-reach respondents with refusers. Using this approach, chi squares were found to be significant in the cases of marital status (42.7; $p < .001$) and age (35.5; $p < .001$). Difficult-to-reach respondents were defined as those who required more than four contacts before an interview was completed.

18. The differences between the two groups could also be evaluated at the means on each variable, as in discriminant analysis. Significant differences are found when the two groups are included in a discriminant function analysis, where the differences are measured at the groups centroids (the means on each independent variable included in the discriminant function). We chose to measure differences between the two groups at the "average gap" rather than at some other location because of the average gap's intuitive simplicity.

19. Both the partial regression coefficients and the adjusted constant terms are evaluated by the following equation:

$$t = \frac{b_1 - b_2}{\sqrt{SE_{b_1}^2 + SE_{b_2}^2}}$$

where the b_1 and b_2 are the coefficients, and SE_{b1} and SE_{b2} are the respective standard errors. Wright (1979) shows that this test is equivalent to Malinvaud's (1970) corrected covariance test.

20. The answer categories were (1) generally alright, (2) it depends, and (3) generally not a good thing.

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