Nonresponse Error in Injury-Risk Surveys

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Background: Nonresponse is a potentially serious source of error in epidemiologic surveys concerned with injury control and risk. This study presents the findings of a records-matching approach to investigating the degree to which survey nonresponse may bias indicators of violence-related and unintentional injuries in a random-digit-dialed (RDD) telephone survey.

Methods: Data from a statewide RDD survey of 4155 individuals aged 16 years and older conducted in Illinois in 2003 were merged with ZIP code–level data from the 2000 Census. Using hierarchical linear models, ZIP code–level indicators were used to predict survey response propensity at the individual level. Additional models used the same ZIP code measures to predict a set of injury-risk indicators.

Results: Several ZIP code measures were found to be predictive of both response propensity and the likelihood of reporting partner violence. For example, people residing in high-income areas were less likely to participate in the survey and less likely to report forced sex by partner, processes that suggest an over-estimation of this form of violence. In contrast, estimates of partner isolation may be under-estimated, as those residing in geographic areas with smaller-sized housing were less likely to participate in the survey but more likely to report partner isolation. No ZIP code–level correlates of survey response propensity, however, were found also to be associated with driving-under-the-influence (DUI) indicators.

Conclusions: There is evidence of a linkage between survey response propensity and one variety of injury prevention measure (partner violence) but not another (DUI). The approach described in this paper provides an effective and inexpensive tool for evaluating nonresponse error in surveys of injury prevention and other health-related conditions.

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Introduction

Nonresponse is one of several important sources of error that can seriously bias survey estimates.[1] With response rates continuing to decline,[2–4] it is increasingly imperative that researchers investigate the potential effects of unit nonresponse on their research. Unit nonresponse poses a problem to the degree that estimates based on survey respondents only vary appreciably from the estimates that would be derived from the full sample.[5] Typically, however, no information is available regarding nonrespondents, making such assessments impossible. In the absence of direct information regarding potential nonresponse bias, response rates are commonly interpreted as an indicator of the potential for nonresponse error, with lower response rates possibly leading to less-representative samples. Recent empirical evidence brings this assumption into question, as several studies have reported few differences in substantive measures across surveys with varying response rates.[6–7]

In contrast to absolutist arguments that low response rates are always, or are never, an indicator of response bias, a more thoughtful perspective suggests that the effects of nonresponse on survey quality are variable and dependent on the degree to which the measures being estimated are associated with a given survey's nonresponse processes, which distinguish respondents from nonrespondents.[8] Nonresponse processes are typically a function of survey design features such as data collection mode, field procedures, survey sponsor, incentives, topic saliency, perceived burden, and personal and interviewer characteristics.[3] Consequently, the effects of nonresponse are likely to be a function of the association among these factors and the specific variables being estimated by a given survey.

Confronting Survey Nonresponse

A variety of approaches have been developed to investigate the effects of nonresponse. These include fol-

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low-up surveys, comparisons of early versus late respondents, use of respondent scenarios, and comparisons of respondents and nonrespondents using information obtained from other sources, an approach we refer to here as a records-match study. Each of these approaches has its own advantages and disadvantages, which we briefly explore below.

Follow-up surveys collect data from persons classified as nonrespondents to an original survey. Follow-up surveys often use multiple and/or more intensive methods to contact and complete interviews with earlier nonrespondents. In an effort to decrease costs and increase benefits of participation, these potential respondents are often asked only to complete a brief subset of the original questions, and larger incentives are offered in exchange for cooperation. Data collected from these respondents are then compared with main survey findings in order to identify similarities and differences between the two groups in order to understand potential nonresponse effects on survey estimates. Limitations of this approach often include low response rates to the follow-up survey, which can also call into question the validity of this technique for understanding nonrespondents. Differences in data collection mode, compared to the initial survey, are also common. Mode effects in these cases may be mistaken for nonresponse effects.

Examining late respondents under the assumption that they are most similar to nonrespondents is another common approach to evaluating survey nonresponse. In many cases, it is unclear, however, whether nonrespondents are indeed more similar to late respondents or if they differ from both early and late respondents equally. Also, comparing the responses of easy versus difficult to reach respondents makes the assumption that there is an underlying continuum of nonresponse, an assumption that has not been empirically supported.[9] Of the methods being reviewed here, this is the only approach that does not introduce additional information into the nonresponse analysis beyond what is already available from the initial survey.

It is also possible to evaluate the potential effects of nonresponse by comparing the answers of survey respondents who do and do not indicate a willingness to participate in hypothetical interviews about specific topics.[10] This approach also has a serious limitation in that it relies on reports of hypothetical rather than actual behavior.

A final approach to investigating nonresponse is via a records-match study. This approach employs auxiliary data from other sources that offer complete coverage of the population being surveyed. Using this information, the characteristics of responding versus nonresponding persons or households can be compared. A good example of this approach is the study by Groves and Couper[3] in which they obtained and matched decennial Census data for respondents' and nonrespondents' households to several government surveys. Of the four methods reviewed here, only the records-match approach has access to information regarding both respondents and nonrespondents for the survey being evaluated. We note that applications of this methodology typically are concerned with identifying correlates of nonresponse and do not directly consider how these nonresponse processes influence substantive survey estimates.[3,11,12] That is, records-match studies usually concern themselves with the predictors of nonresponse, not the predictors of nonresponse error. Recent research by Johnson and Cho[13] and Holbrook et al.[14] have extended the records-match approach to also investigate the degree to which correlates of response propensity also are associated with outcome measures, an approach that permits a more direct assessment of the effects of nonresponse error on survey prevalence estimates.

Nonresponse in Injury-Risk Surveys

Surveys concerned with estimating injury-control risk factors may be particularly vulnerable to nonresponse error because they ask sensitive questions, and the measurement errors associated with such questions are well known.[15] The Injury, Control, and Risk Surveys, sponsored by the Centers for Disease Control and Prevention, for example, request information regarding a variety of highly sensitive topics such as interpersonal violence, suicide ideation, the use and storage of firearms, and driving while under the influence (DUI) of alcohol and other substances. Less information is available regarding the degree to which survey nonresponse may influence estimates of these risk factors. The purpose of this study is to employ a records-matching approach to investigate the degree to which survey nonresponse may bias indicators of both violence-related and unintentional injuries in a random-digit-dialed (RDD) telephone survey. Telephone surveys are a common approach to developing population-based estimates of injury-risk factors. The specific measures to be examined are survey questions concerned with partner violence and DUI-related behaviors. We begin by reviewing available research that may be relevant to the associations between each of these risk measures and survey nonresponse.

Estimating Partner Violence

There is a general belief that intimate partner violence (IPV) is under-estimated in behavioral surveys due to higher rates of nonresponse among victims.[16,17] Respondent scenario studies that ask respondents about their willingness to participate in hypothetical survey situations have in fact reported a potential for biased estimates associated with differential rates of participation among those who have been victims of violence or when a victim's partner is present in the same room

during questioning. In an assessment of the impact of IPV on the decision to participate in a survey, Waltermaurer et al.[18] found that women, regardless of abuse status, were generally willing to participate when their partner was in another room. However, Waltermaurer et al.[18] also reported a dose–response relationship between willingness to participate and the frequency and severity of reported abuse, with decreased participation associated with reports of more severe or frequent abuse and when their partner was in the room during questioning (regardless of frequency or severity). In another study of the influence of nonresponse bias on estimates of IPV, McNutt and Lee[19] found that victims of IPV were significantly more likely than nonvictims to express willingness to participate in a survey regardless of the level of violence. However, this study also reported a decreased willingness to participate when a male intimate partner was living in the same household. A reduced willingness to participate when a male partner is nearby supports previous theoretically-based concerns that the presence of others may bias estimates of IPV by limiting participation of victims of violence or abuse.[16] Beyond the use of these scenarios, however, there is little information available regarding nonresponse error in measures of IPV.

Estimating Driving-Under-the-Influence Behaviors

Although no studies have directly examined the effects of nonresponse on reports of DUI, several have examined the effects of nonresponse on more general measures of alcohol use and abuse. Follow-up surveys concerned with substance use have produced mixed findings, with some indicating less use among nonrespondents[20–22] and others indicating no relationship between substance use and survey nonresponse.[23–25] This research could be interpreted as evidence that persons with fewer substance use experiences, for whom the topic of the survey may be of less interest or saliency, may be less likely or willing to participate.

In contrast, three studies that compared early versus late respondents found that late respondents consume more alcohol, compared to less difficult-to-reach respondents,[26–28] a finding that would suggest that substance users are less willing to participate. Two similar studies, however, reported no differences in alcohol consumption among early versus late respondents.[29–30]

Two examples also are available of the use of auxiliary records to compare respondents with nonrespondents in substance use studies. Needle, McCubbin, and Lorence[31] examined the service utilization records of a health maintenance organization and found no differences between households that did and did not respond to a survey in terms of alcohol and drug treatment service utilization. More recently, Johnson and Cho[19] conducted a records-match study to assess potential nonresponse effects on estimates of substance abuse in an RDD telephone survey. Although ZIP code–level predictors of survey response propensity were associated with substance abuse treatment needs, these effects were eliminated when controlling for individual-level characteristics known to be strongly associated with substance abuse treatment needs.

Overall, these studies provide a mixed picture of the potential effects of survey nonresponse on substance use measures. It should be noted that much of this research has been concerned with self-reports of alcohol consumption, a risk behavior less likely to be interpreted as sensitive by many respondents, compared to driving while under the influence. We next examine survey data that will permit a more direct assessment of the potential effects of nonresponse error on injury prevention measurement.

Methods

The data to be analyzed come from the 2003 Illinois Household Treatment Needs Assessment Survey.[13] From January to August 2003, a total of 4155 random telephone interviews were completed by interviewers at the University of Illinois at Chicago Survey Research Laboratory. Interviews averaged 29.8 minutes in length (standard deviation, 12.7). Wherever possible, sampled households were sent an advance letter introducing the survey. All potential respondents were informed that the survey was part of a "study of Illinois residents regarding alcohol and other drug use," and that it was concerned with better understanding "the alcohol and drug treatment needs in the state of Illinois." Within each sampled household, one person aged ≥16 was randomly selected to be interviewed. Where persons aged 16 to 17 were selected, parental consent was obtained in advance. The parental approval rate for participation by their under-age children was 68%. Using the American Association for Public Opinion Research[32] response rate formula RR 3, the survey's overall response rate was estimated to be 32.7%. No incentives were paid to respondents participating in this survey.

The survey instrument was primarily concerned with issues of substance use involvement, severity, and treatment needs. The injury-risk measures of interest in this analysis were concerned with partner violence and DUI-related behaviors. The specific partner violence measures examined included four items that asked about a range of potential lifetime experiences, including a partner threatening to hit the respondent, actually hitting the respondent, a partner forcing sexual activity, and a partner trying to control or isolate the respondent. The DUI measures included questions that asked about driving under the influence of alcohol and under the influence of drugs during the past year. The specific wording of these items can be found in Table 1.

To examine the potential effects of nonresponse error in this study, two indicators of survey response were constructed using the final sample dispositions assigned to each of the 20,774 telephone numbers included in the survey's sample frame. The final dispositions for each of these numbers are presented in Table 2. Two alternative indicators of response were developed in order to contrast those households completing telephone interviews with (1) all households identi-

Table 1. Wording of injury-risk questions

Partner violence questions

1. Did a spouse or other partner ever threaten to hit you with their fist or anything else that could hurt you? (yes/no)
2. Did a spouse or other partner ever hit you or smack you with their fist or anything else that could hurt you? (yes/no)
3. Did a spouse or other partner ever force you into any sexual activity when you did not want to? (yes/no)
4. Has a spouse or other partner ever tried to control what you do or isolate you from other people? (yes/no)

Driving-under-the-influence questions

5. In the past 12 months, did you drive at all after drinking or using drugs? (yes/no)
6. How many of these involved you drinking alcohol?
7. How many involved you using drugs?

fied as eligible but that did not participate, regardless of reason, and (2) all households identified as eligible plus all unscreened telephone numbers that likely included some eligible households.

The specific disposition codes (from Table 2) employed to develop these measures were as follows. For the first general nonresponse indicator, households completing interviews included disposition codes 01 to 08, and identified households not completing interviews included codes 31 to 60. The second general nonresponse indicator was identical to the first, with the exception of also including among the nonresponding cases those telephone numbers coded as having never been answered (disposition code 30). Households not speaking English or Spanish (disposition code 88) were excluded from these analyses.

Data from several other sources were merged with this sample frame file for analysis. First, survey responses for the measures of interest were appended to those phone numbers that yielded completed interviews. The weighted and variance-adjusted prevalence estimates of past-year DUI-risk measures were as follows: DUI-alcohol (15.9, standard error [SE]=0.37), DUI-drugs (1.2, SE=0.3), any DUI (16.9, SE=1.1). The prevalence estimates of lifetime partner violence measures were partner threatened to hit (10.5, SE=0.9), hit by partner (9.3, SE=0.8), forced into sexual activity by partner (1.4, SE=0.3), partner controlled or isolated (15.4, SE=1.0), and any partner abuse (21.2, SE=1.2).

Second, each telephone exchange/area code combination included in the survey's sample frame was linked with the predominant ZIP code that they served using a proprietary database.[33] Data were available for a sample of 779 ZIP codes in Illinois. Next, a set of 69 measures available from the 2000 Census[34] were aggregated at the ZIP code–level and merged with the sample frame information. The Census variables included a variety of housing, income, occupation, and sociodemographic dimensions that collectively represented broad social conditions within each local geographic area.

Using this merged data file, an exploratory ecologic factor analysis was conducted to determine if a smaller set of underlying dimensions could be identified to represent these Census data. For this analysis, principal components with varimax rotation were used. Ten orthogonal (i.e., uncorrelated) dimensions were identified. The Census measures associated with each dimension are presented in Table 3,

Table 2. Final disposition of the sample

Code	Disposition	Number	Percent
(01)	Completed interview—English, aged 16–17	237	1.14
(02)	Completed interview—Spanish, aged 16–17	4	0.02
(03)	Completed interview—English, aged 18–24	541	2.60
(04)	Completed interview—Spanish, aged 18–24	32	0.15
(05)	Completed interview—English, aged ≥25	3082	14.84
(06)	Completed interview—Spanish, aged ≥25	83	0.40
(07)	Completed interview—aged ≥18	1	0.00
(08)	Partial completed interview	175	0.84
(30)	No answer	2278	10.97
(31)	Answering machine/answering service	1024	4.93
(32)	Eligible respondent not available	940	4.52
(33)	Unscreened respondent not available	632	3.04
(40)	Final refusal before screener completed	2304	11.09
(41)	Final refusal English interview	1855	8.93
(42)	Final refusal Spanish interview	34	0.16
(47)	Unscreened refusal—privacy manager	98	0.47
(48)	English screened refusal—privacy manager	49	0.24
(55)	Not able to interview during survey period	44	0.21
(56)	Never able to interview	204	0.98
(60)	Other eligible	2	0.01
(70)	Ineligible, age	18	0.09
(85)	Deceased	3	0.01
(86)	Nonworking	4161	20.03
(87)	Nonresidential	2795	13.45
(88)	Ineligible, foreign language	177	0.85
(90)	Other ineligible	1	0.00
	Total	20,774	100.00

along with summary descriptive labels that were assigned to each. Factor scores were constructed for each ZIP code using the factor loadings associated with each.

Hierarchical linear modeling (HLM) was used to examine potential associations between each of the 10 ZIP code–level ecologic factors and the two household nonresponse measures.[35,36] Since the outcomes are binary (coded 0 for no and 1 for yes), the first-level equations for these outcomes use a logit link function[37] to estimate the log-odds of a positive response. Each model was adjusted for state geographic region, which was included as a fixed effect covariate. All estimates presented are population-averaged with robust standard errors. Subsequent HLM models examined the effects of each ZIP code measure on the partner violence and DUI measures. These models included additional adjustments for age, gender, and race/ethnicity, which are known to be associated with both substance use and partner violence.[38–40]

Results

Table 4 presents HLM models that examine the independent effects of the 10 ZIP code–level variables on the two survey response propensity measures. The first equation examined the predictors of the response propensity measure that includes cases of known eligibility only. In this model, households residing in high-income ZIP codes were less likely to participate. Conversely, households in high-poverty ZIP codes were also

Table 3. Census variables associated with each factor at the ZIP code level

Factor 1: High-income areas
Median income in 1999 ($), households
Median income in 1999 ($), families
Per capita income in 1999 ($)
Median earnings in 1999 of full-time, year-round workers ($), male
Median earnings in 1999 of full-time, year-round workers ($), female
Factor 2: High-poverty areas
Percent with no vehicle available
Percent with no telephone service available
Income in 1999 below poverty level: percent of population for whom poverty status is determined, all ages
Income in 1999 below poverty level: percent of population for whom poverty status is determined, related children aged <18 years
Income in 1999 below poverty level: percent of population for whom poverty status is determined, aged >65 years
Income in 1999 below poverty level, percent of families
Factor 3: Immigrant populations
Total population, percent foreign born
Foreign-born population, number
Population aged ≥5 years: percent who speak a language other than English at home, total
Population aged ≥5 years: percent who speak a language other than English at home, and speak English less than "very well"
Factor 4: Smaller housing units
Median rooms
Percent in one unit, detached or attached
Percent in buildings with ≥10 units
Percent with three or more bedrooms
Factor 5: Urbanicity
Total housing units
Occupied housing units
Total population, number
Factor 6: Working families
Population aged ≥16 years: percent in labor force, female, with own children aged <6 years
Own children: percent with all parents in family in labor force, aged <6 years
Own children: percent with all parents in family in labor force, aged 6 to 17 years
Factor 7: Poor housing
Percent lacking complete plumbing facilities
Percent lacking complete kitchen facilities
Factor 8: Government employees
Percent distribution by occupation: production, transportation, and material‡ moving occupations
Percent in selected industries: manufacturing
Percent government workers (local, state, or federal)
Factor 9: Whitecollar employees
Percent distribution by occupation: sales and office occupations
Percent distribution by occupation: farming, fishing and forestry occupations
Percent in selected industries: agriculture, forestry, fishing, and hunting
Factor 10: Newer housing units
Percent: year structure built, 1990 to March 2000
Percent: householder moved into unit 1999 to March 2000
Population aged ≥5 years: percent who lived in different house in 1995, and lived in same state

Table 4. Hierarchical models of the effects of ZIP code–level indicators on survey response measures: coefficients (standard errors)

	Survey response 1 (1 = yes)	Survey response 2 (1 = yes)
Level 1 *n*	**10,997**	**13,102**
Level 2 *n*	**779**	**796**
Intercept	−0.44 (0.06)***	−0.64 (0.06)***
ZIP code–level variables		
High income areas	−0.06 (0.02)*	−0.10 (0.03)***
High poverty areas	−0.05 (0.02)**	−0.03 (0.02)
Immigrant populations	−0.03 (0.02)	−0.04 (0.02)
Smaller housing units	−0.04 (0.02)*	−0.08 (0.02)**
Urbanicity	−0.03 (0.02)	−0.02 (0.02)
Working families	0.08 (0.04)*	0.07 (0.04)
Poor housing	0.09 (0.06)	0.06 (0.07)
Government employees	0.01 (0.03)	0.02 (0.03)
Whitecollar employees	0.07 (0.04)	−0.11 (0.04)*
New housing	−0.02 (0.02)	−0.03 (0.02)

Notes: Survey response 1 excludes unknown eligibility cases, and Survey Response 2 includes unknown eligibility cases.
*$p < 0.05$;
**$p < 0.01$;
***$p < 0.001$.
All estimates were adjusted for geographic region within Illinois.

less likely to participate, as were those in ZIP codes with smaller-sized housing. Households in ZIP codes with more working families were more likely to participate. The second model in Table 4 examined the predictors of the response propensity measure that includes phone numbers with unknown eligibility status. In this model, households in high-income ZIP codes again were found to be less likely to be classified as survey respondents. Households in ZIP codes with smaller-sized housing were also again found to have a lower response propensity. Those in ZIP codes with greater proportions employed in whitecollar occupations were additionally found to be less likely to participate. These nonresponse correlates are consistent with previous research that suggests higher levels of nonresponse are typically found in densely populated urban areas where restricted-access housing units, fear of crime, and social disorganization converge to decrease contact and increase refusal rates.[3] Findings of declining response with increasing economic prosperity are also consistent with exchange models of survey participation.[41]

Having identified ZIP code–level predictors of response propensity, we next examined the degree to which any of these variables were also predictive of injury-risk measures. In Table 5, a series of HLM models are presented that examine the effects of ZIP code characteristics on partner violence measures. In these models, two measures found to be predictive of the response propensity measures—areas with high incomes and areas with smaller housing units—were also associated with partner violence indicators. Specifically, households in high-income areas were found to be less likely to report three partner abuse measures: threats from partner, being hit by partner, and being forced to have sex by partner. In addition, households in ZIP codes with smaller housing units were more likely to report three of the partner violence measures, as well as the composite measure.

ZIP codes containing more poor housing, and those with fewer government employees, were associated with greater reporting of any partner abuse and being hit by a partner, respectively. Neither of these ZIP code measures was associated with response propensity, however. In addition, both age and age-squared were associated with each of the violence measures. The directions of these coefficients suggest that younger and older respondents were less likely to report partner violence. Next, females were more likely to report threats of and actual physical violence (i.e., hitting) and forced sex. Some ethnic differences in violence reporting were also observed. Latinos were less likely than whites to report forced sex. African Americans were more likely than whites to report being isolated by their partner, and persons from other ethnic groups were less likely to report being isolated, in contrast to whites. African Americans were also more likely than whites to report any form of partner abuse.

Table 6 presents similar analyses of the effects of ZIP code and individual level predictors on response propensity. In these models, we again see strong effects of the individual level measures. In all three models, age-squared was significant, indicating that DUI-related behaviors were less common among the very young and the very old in the sample. Males also were more likely to report each of the three DUI measures. Latinos were less likely than white respondents to report each DUI behavior. African Americans and persons of other races/ethnicities were also less likely to report alcohol and any form of DUI-related behavior, compared to whites. After controlling for these individual level measures, two ZIP code–level indicators were found to be associated with drug-related DUI. Persons residing in

Table 5. Hierarchical models of the effects of ZIP code–level and individual-level indicators on lifetime partner violence measures (n = 4038 respondents, n = 627 ZIP codes): coefficients (standard errors)

	Threats from partner (1 = yes)	Hit by partner (1 = yes)	Forced sex by partner (1 = yes)	Isolated by partner (1 = yes)	Any partner abuse (1 = yes)
Intercept	−4.57 (0.38)***	−3.77 (0.36)***	−8.36 (0.93)***	−3.53 (0.32)***	−3.22 (0.27)***
ZIP code–level variables					
High-income areas	−0.18 (0.07)**	−0.12 (0.07)*	−0.58 (0.18)**	−0.10 (0.06)	−0.10 (0.06)
High-poverty areas	0.02 (0.05)	−0.06 (0.05)	0.14 (0.10)	−0.07 (0.04)	−0.04 (0.03)
Immigrant populations	−0.06 (0.05)	0.03 (0.05)	−0.03 (0.11)	0.02 (0.05)	0.02 (0.04)
Smaller housing units	0.10 (0.04)**	0.09 (0.04)**	0.12 (0.12)	0.09 (0.04)*	0.07 (0.03)*
Urbanicity	−0.01 (0.04)	0.04 (0.04)	−0.04 (0.11)	0.01 (0.04)	0.01 (0.04)
Working families	0.02 (0.11)	0.10 (0.11)	0.19 (0.22)	0.04 (0.10)	0.03 (0.08)
Poor housing	0.30 (0.16)	0.20 (0.15)	0.17 (0.27)	0.25 (0.13)	0.27 (0.12)*
Government employees	−0.09 (0.07)	−0.14 (0.06)*	−0.10 (0.14)	−0.09 (0.06)	−0.09 (0.05)
Whitecollar employees	0.03 (0.11)	0.02 (0.11)	0.32 (0.27)	0.13 (0.11)	0.09 (0.09)
New housing	0.05 (0.04)	0.04 (0.04)	0.15 (0.11)	0.01 (0.05)	0.002 (0.04)
Individual-level variables					
Age (in years)	0.14 (0.02)***	0.11 (0.02)***	0.23 (0.04)***	0.11 (0.01)***	0.11 (0.01)***
Age-squared	−0.002 (0.0002)***	−0.001 (0.0002)***	−0.002 (0.0004)***	−0.001 (0.0002)***	−0.001 (0.0001)***
Male (1 = yes)	−0.57 (0.11)***	−0.50 (0.12)***	−3.10 (0.48)***	−0.16 (0.09)	−0.14 (0.08)
African American (1 = yes)	0.14 (0.19)	0.31 (0.19)	0.16 (0.36)	0.39 (0.18)*	0.42 (0.17)*
Latino (1 = yes)	−0.29 (0.24)	−0.30 (0.23)	−1.84 (0.72)**	−0.29 (0.19)	−0.29 (0.18)
Other race/ethnicity (1 = yes)	−0.25 (0.33)	−0.09 (0.31)	−0.13 (0.62)	−0.57 (0.28)*	−0.41 (0.25)

*$p < 0.05$;
**$p < 0.01$;
***$p < 0.001$ (all bolded).
Note: All estimates were adjusted for geographic region within Illinois.

urban areas and those residing in areas with many government employees were more likely to report drug-related DUI. None of the ZIP code indicators were predictive of alcohol-related DUI or the combined DUI variable. Overall, none of the ZIP code indicators associated with response propensity were found to have an independent association with any of the DUI measures.

Table 6. Hierarchical models of the effects of ZIP code–level and individual-level indicators on past-year DUI measures (n = 4038 respondents, n = 627 ZIP codes): coefficients (standard errors)

	DUI-alcohol (1 = yes)	DUI-drugs (1 = yes)	DUI-either (1 = yes)
Intercept	−3.29 (0.32)***	−5.53 (0.98)***	−3.15 (0.32)***
ZIP code–level variables			
High-income areas	0.06 (0.06)	0.11 (0.19)	0.05 (0.06)
High-poverty areas	−0.03 (0.04)	−0.09 (0.13)	−0.03 (0.04)
Immigrant populations	0.03 (0.05)	0.25 (0.16)	0.02 (0.05)
Smaller housing units	0.01 (0.05)	0.03 (0.11)	0.01 (0.06)
Urbanicity	0.06 (0.05)	0.31 (0.12)**	0.05 (0.04)
Working families	0.08 (0.10)	0.19 (0.30)	0.10 (0.10)
Poor housing	−0.02 (0.16)	0.31 (0.30)	0.04 (0.14)
Government employees	0.09 (0.07)	0.45 (0.18)**	0.11 (0.06)
Whitecollar employees	0.13 (0.11)	0.24 (0.35)	0.12 (0.10)
New housing	0.06 (0.04)	0.12 (0.11)	0.05 (0.04)
Individual-level variables			
Age (in years)	0.07 (0.01)***	0.04 (0.05)	0.07 (0.01)***
Age-squared	−0.001 (0.0002)***	−0.002 (0.0007)*	−0.0009 (0.0001)***
Male (1 = yes)	0.94 (0.09)***	0.96 (0.31)**	0.96 (0.09)***
African American (1 = yes)	−1.00 (0.18)***	−0.69 (0.60)	−0.98 (0.17)***
Latino (1 = yes)	−0.84 (0.17)***	−1.26 (0.65)*	−0.74 (0.17)***
Other race/ethnicity (1 = yes)	−1.13 (0.27)***	−0.52 (0.76)	−1.19 (0.28)***

Note: All estimates were adjusted for geographic region within Illinois.
*$p < 0.05$;
**$p < 0.01$;
***$p < 0.001$ (all bolded).
DUI, driving under the influence.

Discussion and Conclusion

Our analyses provide evidence of a consistent linkage between survey response propensity and one variety of injury-risk measure (partner violence) but not another (DUI). The models presented in Tables 4 and 5 suggest decreased likelihood of participation among households in high-income areas, and decreased likelihood of reporting threats of physical violence (i.e., hitting), actual physical violence, and forced sex in these same geographic areas. In contrast, households in ZIP code areas with smaller housing units are also less likely to participate, but more likely (when responding) to report four of the five violence measures, suggesting that these experiences may be under-estimated in this survey. Because we have some evidence that threats of violence, and actual violence, may be over-estimated due to the under-representation of respondents residing in one type of geographic area, and under-estimated due to over-representation in another, it is possible that these nonresponse biases may effectively cancel each other out. If this assumption is correct, we might conclude that nonresponse error is having only a minimal net effect on the partner physical violence measures examined in this study.

These results also suggest that nonresponse error may be contributing to an over-estimation of forced sex in this survey. Persons residing in areas with lower response propensities (high-income areas) were less likely to report being forced to have sex by a partner, processes that suggest an over-estimation of this form of violence. In contrast, estimates of partner isolation and any form of partner abuse may be under-estimated. Persons residing in areas characterized by smaller-sized housing appear less likely to have participated in the survey, and more likely to report partner isolation and any form of partner abuse, patterns that suggest these experiences may have been under-estimated. Consequently, we conclude that these partner violence measures may have been influenced by nonresponse error, but not in a simple manner. Rather, some measures may be over-estimated, others under-estimated, and some may be influenced by multiple processes that work in opposing directions.

It is unclear why potential evidence of nonresponse error was associated with most of the partner violence measures examined, but with none of the DUI measures. It may be that the DUI behaviors are more randomly distributed geographically, while the partner violence measures are more socially distributed in a manner that can be captured at the level of aggregation employed in this analysis. We note, for example, the known household-level relationship between intimate partner violence and income. Rennison and Planty[39] found that homes with combined incomes over $50,000 a year had the lowest risk for intimate partner violence among all other income groupings. Measures of assault and violence have also been associated with measures of low SES at the ZIP code level.[42,43] In contrast, driving while intoxicated has not been found to be associated with ZIP code–income level.[44] This suggests that the records-match approach employed in this study may only be useful in detecting nonresponse effects to the degree that the phenomenon being estimated is geographically clustered. It would also have been reasonable to expect to be able to more easily identify a pattern in the geographic distribution of recent (i.e., past year) behaviors, relative to more distant (i.e., lifetime) ones. The fact that nonresponse error was in fact associated with lifetime IPV but not past-year DUI further suggests that the social distribution of these risk behaviors may be associated with the ability of this approach to detect the effects of nonresponse patterns. Future research will need to systematically consider this proposition.

Several other study limitations should also be considered. Most importantly, the analysis employed here assumes that relationships between risk behaviors and ZIP code characteristics found among respondents (in Tables 5 and 6) are the same as for nonrespondents, for whom no direct risk behavior information is available. We note, however, that a similar unproven assumption underlies the concepts of post-stratification and nonresponse weighting that is routinely used in survey research.

In addition, other sources of survey error, measurement error in particular, were not assessed in this study. Based on available validation studies,[40,45] we believe that under-reporting of both DUI and partner violence measures in telephone surveys is likely as a result of social desirability–response bias. It is unclear what the magnitude of this error may be, relative to nonresponse error. Comparative assessments of various sources of survey error would appear to be an important direction for further investigation.

Another assumption of this analysis that can be questioned is the degree to which telephone area code and prefix combinations are in fact geographically fixed. Telephone number portability is a feature of the current technological environment, giving us less confidence that all numbers included in this analysis have been correctly associated with the ZIP code in which the respondent currently resides. There is as of yet, however, little empirical evidence regarding the degree to which this relatively new phenomenon may now be influencing survey data.

An additional problem is the lack of complete correspondence between the geographic areas covered by ZIP codes and telephone exchanges. To address this problem, respondent answers to a self-reported ZIP code question were compared with the ZIP code–matching data. This comparison revealed a perfect match for only 60% of the ZIP codes included in this study. Yet, a 97% concordance rate was found when

matching was limited to the first three ZIP code digits. For those cases where five-digit ZIP codes were not precisely matched, these analyses revealed that the unmatched ZIP codes introduced random, rather than systematic error, a process that seems likely to minimize chances of finding relationships between ZIP code measures and nonresponse. Consequently, although there is certainly some error introduced via the processes of matching ZIP codes with telephone exchanges, this error would tend to favor the null hypothesis. Finally, we also acknowledge that ZIP codes may not be the optimal unit of aggregation for conducting these analyses.[46] Future research should address this issue.

The approach to assessing nonresponse error demonstrated in this paper nonetheless appears to be one of the few available approaches to this problem that can be effectively and inexpensively applied to RDD telephone surveys. Using this mode of data collection, the only information commonly available with which to assess nonresponse is the telephone number itself. By attaching community-level information to these phone numbers, this approach enables direct comparisons of known respondent households with known (and estimated) nonresponding households. While knowledge of similarities and differences between respondents and nonrespondents is by itself valuable, what we consider most important is determining the degree to which those differences can be linked, or not, to the substantive goals of the survey. Absence of associations among nonresponse propensity, ZIP code measures, and the variables being estimated can provide some, although not definitive, evidence that nonresponse error is not seriously damaging key survey estimates. Evidence of associations among these estimates of key measures and nonresponse processes, in contrast, would seem sufficient to justify adjusting for one or more of these ZIP code–level measures. We believe that this methodology provides a valuable tool for evaluating nonresponse error in surveys of injury prevention and other health-related conditions, which have important policy implications and hence require careful measurement.

The findings and conclusions in this report are those of the authors and do not necessarily represent the views of the Centers for Disease Control and Prevention.

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