

A RECORD CHECK EXPERIMENT IN THE MEASUREMENT OF VICTIMIZATION

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As part of the redesign project of the National Crime Survey (NCS), involving researchers and survey organizations throughout the country, the Survey Research Center at The University of Michigan (SRC) has been investigating the effects of alternative questionnaire designs on victimization reporting. In this paper, we discuss preliminary findings from a telephone survey experiment comparing two victimization questionnaires: the NCS instrument used currently by the Bureau of the Census and an experimental version developed by SRC in collaboration with NCS redesign consortium members.

Background

Current NCS Instrument: The current NCS questionnaire consists of a "control card," a set of "screener questions" and an "incident report." The control card is used to gather demographic and recontact information. The screener questions seek to identify victims of various types of crime. Seven of the 20 screener items address "household crimes"--events which may be said to victimize the entire household rather than any one household member (e.g., theft of lawn furniture). The rest of the screener items concern crimes against individuals ranging from larceny and robbery to various sorts of assault. All of the screener items refer to events occurring in the six months prior to the interview. The incident report contains questions on details of the victimization events which are reported in response to screener questions.

All people 14 years old and older in sample households are asked to respond to the individual screener questions and to complete any associated incident reports. A single adult in each household is designated to serve as a "household respondent" and answers the questions on the control card and the "household" screener questions. If the household contains people 12 or 13 years of age, the "individual" screener questions and necessary incident reports for these sample members are answered by proxy respondent.

Experimental Instrument: The alternative questionnaire designed for this experiment seeks to increase reports of crime incidents from respondents through a number of significant modifications to the current NCS instrument. First, "warmup" and "lifestyle" questions have been added to the control card section. The warmup questions ask respondents to report feelings of fear or anger associated with potential victimization situations in the previous six months. The notion behind these items is that the recollection of emotional responses which may have been associated with crimes will trigger the memory of the crimes themselves in the subsequent screener section. The lifestyle questions (about routine activities and living circumstances) are designed to give more insight into correlates of victimization than is provided by the current NCS. These questions might also stimulate recall of crimes by reminding respondents of activities which were associated with the events.

In addition to these changes in the control card section, we modified the screener questions substantially. The experimental screener section contains 45 items rather than the current twenty, and includes questions on vandalism, intimidation, and neighborhood disturbances as well as on the types of crime covered in the current NCS. Besides measuring more kinds of crime, increasing the number of screener questions allows for an "overlapping" focus. The "experimental" instrument has built-in redundancy: two or three items are devoted to measuring certain types of crime, each item using a slightly different approach. For example, there are questions about vandalism to property in general and also about damage done to motor vehicles in particular. We ask about thefts of personal property and also about loss of property which cannot be readily explained and might have been theft. Such multiple cues about individual types of crime are intended to spur recall of events which may be stored in memory in a variety of ways. By offering the respondent a range of memory cues, we hope to aid in the process of remembering and reporting victimization events.

In the field experiment described here, both the current NCS instrument and the experimental version were administered using a computer-assisted telephone interviewing system (CATI). For the NCS version, we adapted the current "hard copy" personal interview questionnaire to the SRC CATI system. The experimental questionnaire, however, took greater advantage of the complex branching feature offered by the CATI system. This feature allows questions to be individually "tailored" to a respondent's previous answer much more readily than is the case with a paper questionnaire which is limited by size and by the interviewer's ability to follow complex skip patterns. The computerized system can store many more "pages" of questions and can be programmed to follow skip patterns which an interviewer would find difficult or impossible to negotiate.

To illustrate, respondents who report that they feel afraid about potential crime situations in the "warmup" section of the experimental questionnaire are branched sometime later to special screener questions which contain a preface reminding them of their earlier recollection. Similarly, those who report living in multiple family dwellings receive questions on thefts from laundry rooms, hallways and building storage areas, while single family dwelling inhabitants are asked about thefts from lawns, storage sheds, etc. "Individualizing" questions in this way is another attempt to facilitate recall of victimization experiences.

Respondent Rules: These experimental questionnaire alterations are accompanied by respondent rules which differ in several ways from those used in the current NCS. Each respondent interviewed with the experimental screener answers all of the questions in that section: no one person serves as a "household respondent." The

rationale behind this innovation is that a designated household respondent may not report some incidents which are classifiable as "household crimes" but which are only salient for certain household members. For example, the theft of sporting goods may not be recalled by the household respondent, but may be particularly memorable for another person in the family. Therefore, at the risk that some household crimes might be reported more than once (requiring adjustment of the count during analysis), we opted to have each respondent report on all types of crime--household and individual--in the screener. In addition, we took only self-reports for people 12 and 13, rather than allowing an adult to respond for them. We reasoned that the adolescents would be more able to recall their own victimization experiences than would their parents.

Enumerating Incidents: The current NCS questionnaire and the experimental instrument also differ in their procedures for enumerating separate occurrences of particular types of crime. The present questionnaire simply asks respondents "how many times" a crime has occurred after the respondent initially reports that he or she was victimized. A separate incident report is completed for each of the occurrences of that crime. In addition, once any victimization incident has been reported in the screener section, all subsequent screener items are prefaced by the interviewer with the words, "Other than things you've already mentioned..." in order to prevent respondents from reporting the same incident more than once.

We felt that these procedures might lead to errors in enumerating incidents. For example, the "how many times" question may produce ranges (e.g. "3-4 times") and "series" mentions (reports that a type of crime happens to the respondent "all the time"). The "other than things already mentioned..." preface might lead respondents not only to avoid repeating prior incidents but also to fail to mention events which are related to earlier mentions. Victimization events sometimes consist of a set of interrelated actions, each of which may be treated as an independent event for survey purposes, but which may be encoded by the respondent as a single incident. For example, a person may be assaulted and his property may be vandalized by a neighbor as the result of a local dispute. These two crimes may be treated by the respondent as a single event since they stem from the same cause and involve the same set of actors. If this is the case, the respondent may report one of the events to the first appropriate screener question and omit the other event because of the instruction in the current NCS not to report "things already mentioned."

The experimental alternative to these procedures involves what we call a "dating sequence" of questions. Rather than asking respondents "how many times" a particular kind of victimization has occurred, we ask him or her to report the dates on which each incident happened. This procedure should help to distinguish each occurrence and to insure that remembered events actually happened during the six-month reference period.

In addition, rather than prefacing screener questions with "other than things already

mentioned..." we attempt to disentangle redundant mentions after a "yes" response is given to a screener question. We then ask respondents if the current "yes" response refers to a crime which was also reported earlier, and if so, we omit the current mention from further consideration. Thus, we tolerate potential redundancy in screener responses in order to make certain that related victimizations are not underreported due to the questionnaire design.

Questionnaire Differences Summary: The current NCS questionnaire streamlines the victimization reporting task in several ways: the use of a single household respondent who answers particular screener questions, the shorter list of screener questions, the simpler method for enumerating incidents, and the method for limiting redundancy in crime reports. The experimental questionnaire is rather administratively messy by contrast. We have built in a variety of redundancies and removed the streamlining on the assumption that this more complicated procedure will help to increase the reporting of victimization incidents. The common assumption in victimization reporting research is that underreporting of crime is the dominant response error. The experimental instrument is designed to reduce this error at the price of greatly complicating the questionnaire administration and data analysis steps in the survey. The test of the utility of the experimental instrument is how much non-redundant, codable crime it produces in comparison to the current questionnaire. In the analysis to follow, we present some comparisons which show preliminary findings in the determination of the experimental approach.

Study Design

The data presented here were collected as part of a reverse record-check study of victimization reporting in Peoria, Illinois. Some 1300 police records for January-September 1981 were sampled from the computerized files of the Peoria Police Department and recorded telephone numbers were called during October and November of 1981 in an attempt to obtain interviews with police documented crime victims. The phone numbers were randomly assigned to one of the two data collection procedures--the current NCS questionnaire or the experimental instrument. In addition, some 570 telephone numbers in the Peoria area were generated by RDD (Random Digit Dial) methods and called to obtain interviews. Interviews taken with the RDD sample numbers (randomly assigned to one of the two questionnaires) were taken to provide a comparison to the reporting of police record respondents.

The sampled telephone numbers (including both the police record and RDD numbers) yielded a total of 2077 interviews in 1034 households with 982 interviews taken with the experimental instrument and 1095 taken with the control or current NCS questionnaire. We estimate person-level response rates at 80 percent for the experimental group and 86 percent for the control group. As noted above, interviews were taken with all persons 12 years of age and older in each household.

The police record sample permits external validation of victimization reports by matching the survey answers with the records. Our match analysis is currently under way; in this paper we content ourselves with an examination of the

relative levels of crime reporting in the two questionnaire groups. Following common practice, we will view more crime reporting as more valid crime reporting.

Findings

Our first analysis focuses on the frequency of "yes" responses to screener questions in each of the questionnaires and the extent to which these responses were translated into completed incident reports. Tables 1 and 2 present the percentage of people saying "yes" to individual screener questions in the current NCS and in the experimental instruments, respectively. The second column of each table shows for each screener item the percentage of people who completed one or more incident reports for a crime. A difference between the percentage of people saying "yes" to a screener question and the percentage completing an incident report is due to "yes" reports which are redundant with other mentions, "yes" reports which concern out-of-reference period incidents, and other mistaken "yes" responses. For example, respondents may report that they experienced a household burglary when asked question 30 in the current NCS screener (Table 1). In most cases, this "yes" response would lead to a completed incident report but if it were discovered that the "yes" response referred to a crime which occurred outside the six-month reference period, an incident report would not be completed. Similarly, if two different screener items elicited reports of the same crime, an incident report would not be completed for one of the two "yes" reports.

As one can observe in Table 1, we found a rather close match between "yes" responses and incident reports completed in the current NCS instrument. Across all of the screener items there is an average of about .5 percent more "yes"s than completed incident reports. There is a slightly higher mismatch for screener questions 40 and 44 which deal with items stolen from outside the house and stolen motor vehicle parts. These screener questions elicited more redundant crime mentions than did other screener items. On the whole, however, one can see that there is relatively little redundancy among reports to the various screener questions. It is reasonable to attribute this finding to the "streamlined" features of the current NCS questionnaire which were noted above.

Table 2 tells a substantially different story about the experimental victimization questionnaire. Looking at the longer list of screener questions, we note a larger average difference between the "yes" responses and incident reports completed. The difference appears largely attributable to more redundant mentions being elicited by separate screener questions. This finding is to be expected, since the experimental screener items were not prefaced by the phrase, "Other than things you have already mentioned..." To understand better the response process to these screener questions, we took a close look at responses to questions B2 and B23 (Table 2). Ninety-nine people said "yes" when asked question B2, but 40 of them were reporting the same crime they mentioned in response to questions B1. Fifteen others mentioned a second incident (having mentioned another one on B1) and forty-four people said "yes" for the first time.

Thus, we can see both substantial redundancy in answers to B2 and substantial gain in reporting of new incidents. The findings for questions B23 are quite similar. A slight majority of those (118) people who said "yes" to this question mentioned something which was already mentioned previously. Some 30 percent of those who said "yes" mentioned a new (2+) incident, while 15 percent said "yes" for the first time.

To summarize the differences between screener responses and incident reports completed in the experimental questionnaire, it is clear that the screener questions--which tolerate potential redundancy in the hope of obtaining more new mentions of victimization experience--do gather a substantial amount of "chaff" in their search for "wheat." Respondents who were questioned with the experimental form were much more likely than their counterparts in the control group to mention a single crime more than once during the screener section. These redundant reports had to be excised during the incident enumerating process. At the same time, it also appears that the experimental approach produced many more non-redundant incidents than did the current NCS form. Thus, the "unstreamlined" approach may have benefits which compensate for the additional administrative difficulty it presents. We examine the benefits in more detail in Table 3.

This table displays the number of incidents reported, by questionnaire form and type of crime (TOC) code. We have weighted the incidents by the reciprocal of the number of people in each household who reported them in order to remove multiple within-household reports of the same crimes. We expected this adjustment to have more effect on the number of incidents reported in the experimental group, since that data collection procedure did not have a "household respondent" who could report about crimes against the household. Rather, each respondent was asked to report on all types of crime covered in the screener section, whether "household" or "individual." Thus, we expected to find more duplication of incidents reported in those households interviewed with the experimental questionnaire. As it turned out, the weighted incident count was about 13 percent lower than the unadjusted count in the experimental group, and about 10 percent lower in the control (current NCS) group. Thus, while there were more multiple within-household reports of crime in the experimental group, the difference between the two questionnaire forms in this respect was not large.

Table 3 presents two TOC codes--the code presently employed in the NCS and a revised code which we created to accommodate the increased scope of the experimental screener. The expanded code contains categories for arson, vandalism, disturbance of the peace, the phone threats and harassment, automobile hit-and-run, indecent exposure, and three categories of "borderline" incidents: apparent non-crimes, automobile accidents, and incidents occurring to someone other than the respondent. There is also a category for incidents which do not fit in any of the other codes.

Looking first at the incident totals for the two questionnaire forms in columns 1 and 2, we see that the experimental form produced about 1.6 times as many weighted reports as did the control (current NCS) form. The average number of incidents is 1.47 in the experimental group and .84 in the control. As was expected, a large number of the incidents elicited by the experimental screener (about 49 percent) were not codable in the current NCS type of crime code. A smaller percentage of incidents elicited by the current NCS screener were uncodable (28.5 percent), but the number of "uncodables" was larger than one might have expected given the fact that the TOC categories were developed for that questionnaire.

The revised TOC code (right half of the table) accommodates most of the "uncodable" mentions, leaving about 1.5 percent unclassifiable mentions for each questionnaire form. The majority of previously uncodable mentions fall into the vandalism, disturbance of the peace, apparent non-crime and incidents involving someone other than respondent categories of the revised code.

To construct a more stringent comparison between the two questionnaire procedures, we subtracted from the total number of incidents mentioned the uncodable incidents, incidents involving someone else, accidents and apparent non-crimes. Even with these mentions excluded, the experimental questionnaire produced 1.4 times as many mentions as the current NCS form (an average of 1.22 incidents mentioned in the experimental group and .79 mentioned in the control). Thus, the difference in reporting between the two groups does not appear to be entirely due to more mentions of "borderline" incidents in the experimental group.

An even more stringent comparison can be made by examining the productivity of the two questionnaires only for those incidents codable in the original Census TOC code. This comparison eliminates the experimental screener's larger type of crime scope from consideration. Under these conditions, the experimental form still produced 1.13 times as many incident mentions as the current NCS form--an average of .75 incidents per respondent vs. .60 in the control group. Thus, reducing the effect of the longer, more varied list of screener questions in the experimental form does not totally explain the difference in reporting levels produced by the experimental and control procedures. Other features of the experimental screener described above appear to have produced more reporting of even those incidents which are regularly covered by the current NCS.

Conclusion

We have examined some differences in levels of reporting of victimization elicited by two questionnaires--the current NCS instrument and an experimental questionnaire which was designed to increase the amount of victimization reporting. The data presented were collected as part of a reverse record check experiment in Peoria, Illinois. The preliminary findings reported here suggest that the experimental instrument, which introduces a number of administrative complexities in gathering victimization reports, does notably increase the amount of crime reported over that produced by the current NCS questionnaire. Our next step in this research is to analyze survey report-police record match rates for the different questionnaires to see if there are differences in the external validation of victimization reports for the two instruments.

TABLE 1
Comparison of "Yes" Responses and Valid Incident Reports for Each Question in Control (Census) Screener

| Question | % Yes | % + Incident Reports | N |
|----------------------------|-------|----------------------|------|
| 38 Breakin | 13.1 | 12.5 | 518 |
| 39 Attempted breakin | 4.8 | 4.6 | 517 |
| 40 Stolen outside | 17.8 | 16.8 | 517 |
| 41 Other things taken | 2.3 | 2.1 | 518 |
| 43 Steal MV | 2.9 | 2.7 | 471 |
| 44 Steal MV parts | 17.2 | 15.5 | 470 |
| 45 Pocket/Purse | 2.9 | 2.8 | 1073 |
| 46 Mugging | 1.1 | .9 | 1074 |
| 47 Try mugging | 1.1 | 1.1 | 1074 |
| 48 Beat up | 3.1 | 2.6 | 1073 |
| 49 Knife/shot | 1.0 | .8 | 1074 |
| 50 Threat/weapon | 5.0 | 4.7 | 1074 |
| 51 Try attack | 2.3 | 1.7 | 1072 |
| 52 Take from car | 6.9 | 6.7 | 1073 |
| 53 Stolen away from home | 8.0 | 7.6 | 1074 |
| 54 Stolen anything | 8.5 | 7.7 | 1073 |
| 55 Steal attempt | 4.6 | 4.3 | 1074 |
| 56 Call police | 11.3 | 10.4 | 1073 |
| 57 Consider calling police | 3.3 | 2.5 | 1073 |

TABLE 2

Comparison of "Yes" Responses and Valid Incident Reports for Each Question in the Experimental Screener

| Question | % Yes | % + Incident Reports | N |
|------------------------------|-------|----------------------|-----|
| B1 Taken from home | 26.7 | 25.7 | 936 |
| B2 Break in | 10.2 | 5.1 | 937 |
| B3 Evidence try | 7.1 | 4.5 | 935 |
| B4 Vacation home | 1.3 | .9 | 937 |
| B5 Pets | 5.6 | 4.1 | 932 |
| B6 Take MV | 1.8 | 1.5 | 936 |
| B7 Use vehicle | 1.9 | 1.1 | 790 |
| B8 Steal MV parts | 16.6 | 13.9 | 790 |
| B9 Bicycle | 14.4 | 5.2 | 534 |
| B9b Motorcycle | 5.2 | 1.7 | 116 |
| B10 MV vandalism | 12.7 | 10.6 | 936 |
| B11 Take inside MV | 4.8 | 3.1 | 937 |
| B12 No one got in | .5 | .5 | 424 |
| B13 Taken from place staying | 2.2 | 2.1 | 937 |
| B14 Pocket/purse | 1.9 | 1.4 | 936 |
| B15 Taken while out | 2.4 | 1.6 | 936 |
| B16 Miss anything | 4.3 | 3.6 | 937 |
| B17 Happen at work | 5.9 | 5.2 | 542 |
| B18 Try take at work | .6 | 0.0 | 539 |
| B19 Happen at school | 11.5 | 7.1 | 182 |
| B20 Try take at school | 4.9 | 2.7 | 183 |
| B21 Taken by force | .7 | .2 | 937 |
| B22 Try take by force | 2.4 | 1.2 | 937 |
| B23 Vandalism | 12.5 | 5.3 | 936 |
| B25 Nothing happened | 1.3 | 1.1 | 370 |
| B26 Attack with weapon | 2.1 | 1.5 | 937 |
| B27 Any weapon used | 1.3 | .7 | 937 |
| B28 Punched/kicked | 8.8 | 6.3 | 937 |
| B29 Objects thrown | 3.5 | 2.3 | 936 |
| B30 Attempted attack | 3.2 | 1.1 | 937 |
| B31 Known attacker | 1.5 | .3 | 937 |
| B32 Threats | 11.2 | 5.3 | 937 |
| B33 Force/threat abduction | .9 | .1 | 937 |
| B34 Intimidation | 3.7 | 1.3 | 936 |
| B35 Sexual attack | 1.3 | .4 | 936 |
| B36 Attack/threat at work | 1.6 | .9 | 543 |
| B37 Attack/threat at school | 2.1 | .5 | 183 |
| B38 Shot at vehicle | 2.9 | 2.2 | 937 |
| B39 Attack with car | 4.5 | 3.6 | 937 |
| B40 No one hurt you | .6 | 0.0 | 317 |
| B41 Neighborhood disturbance | 14.6 | 10.9 | 936 |
| B43 Call police | 20.0 | 11.9 | 935 |
| B44 Consider calling police | 10.2 | 5.9 | 937 |

Table 3

Assigned Type of Crime (TOC) Category for Current Census TOC and Revised TOC by Type of Screener Form*

| Assigned Type of Crime | NCS Type of Crime | | | | Type of Crime - Revised | | | |
|--|--------------------------|----------------|--------------------------|----------------|--------------------------|----------------|--------------------------|----------------|
| | N | | % | | N | | % | |
| | Experi- mental NCS | Current NCS | Experi- mental NCS | Current NCS | Experi- mental NCS | Current NCS | Experi- mental NCS | Current NCS |
| 1. Completed Rape | 4 | 1 | 0.3 | 0.1 | 4 | 1 | 0.3 | 0.1 |
| 2. Attempted Rape | 1 | 3 | 0.1 | 0.3 | 1 | 3 | 0.1 | 0.3 |
| 3. Completed Robbery w/Serious Injury | 1 | 0 | 0.1 | 0.0 | 1 | 0 | 0.1 | 0.0 |
| 4. Completed Robbery w/Minor Injury | 2 | .5 | 0.1 | 0.1 | 2 | .5 | 0.1 | 0.1 |
| 5. Completed Robbery No Injury | 9 | 12 | 0.6 | 1.3 | 10 | 13 | 0.7 | 1.4 |
| 6. Attempted Robbery w/Serious Injury | 2 | 1 | 0.1 | 0.1 | 2 | 1 | 0.1 | 0.1 |
| 7. Attempted Robbery w/Minor Injury | - | - | - | - | - | - | - | - |
| 8. Attempted Robbery No Injury | 9 | 12 | 0.6 | 1.3 | 9 | 14 | 0.6 | 1.5 |
| 9. Assault w/Serious Injury | - | - | - | - | 0 | 1 | 0.0 | 0.1 |
| 10. Attempted Assault w/Weapon | 20.5 | 31 | 1.4 | 3.4 | 30.5 | 37 | 2.1 | 4 |
| 11. Simple Assault w/Injury | 27 | 15 | 1.9 | 1.6 | 28 | 16 | 1.9 | 1.7 |
| 12. Attempted Assault, No Weapon | 38 | 14 | 2.6 | 1.5 | 60 | 23 | 4.2 | 2.5 |
| 21. Completed Purse Snatch/Pocket Picked | 7.5 | 3 | 0.5 | 0.3 | 10.5 | 10 | 0.7 | 1.1 |
| 22. Attempted Purse Snatch/Pocket Picked | 1.5 | 1 | 0.1 | 0.1 | 1.5 | 1 | 0.1 | 0.1 |
| 24. Completed Larceny No Contact | 200.31 | 154.64 | 13.9 | 16.8 | 217.84 | 183.14 | 15.1 | 20.0 |
| 25. Attempted Larceny No Contact | 15 | 11 | 1.0 | 1.2 | 16 | 11 | 1.1 | 1.2 |
| 31. Completed Burglary Forced Entry | 48.29 | 52.82 | 3.3 | 5.8 | 49.29 | 53.82 | 3.4 | 5.9 |
| 32. Completed Burglary No Force | 98.22 | 66.64 | 6.8 | 7.3 | 95.22 | 65.64 | 6.6 | 7.2 |
| 33. Attempted Forced Entry | 33.69 | 37 | 2.3 | 4.0 | 39.69 | 49.5 | 2.7 | 5.4 |
| 34. Completed HH. Larceny | 187.21 | 198.48 | 13 | 21.6 | 195.87 | 207.8 | 13.6 | 22.6 |
| 35. Attempted HH. Larceny | 14 | 23 | 1.0 | 2.5 | 16.5 | 25 | 1.1 | 2.7 |
| 36. Completed Vehicle Theft | 13 | 13.5 | 0.9 | 1.5 | 20.5 | 12 | 1.4 | 1.3 |
| 37. Attempted Vehicle Theft | 6.5 | 5 | 0.4 | 0.5 | 8.5 | 5.5 | 0.6 | 0.6 |
| 89. Arson | | | | | 4 | 2 | .3 | .2 |
| 91. Vandalism of Property | | | | | 170.55 | 47.5 | 11.8 | 5.2 |
| 92. Verbal Threat (other than phone) | | | | | 49.5 | 33 | 3.4 | 3.6 |
| 93. Disturbance of Peace | | | | | 120.98 | 21 | 8.4 | 2.3 |
| 94. Phone Threat, harass | | | | | 26 | 19.5 | 1.8 | 2.1 |
| 95. Hit and Run | | | | | 5 | 2.5 | 0.3 | 0.3 |
| 98. Indecent Exposure | | | | | 3 | 2 | 0.2 | 0.2 |
| 90. Apparent Non-Crime | | | | | 76.5 | 10 | 5.3 | 1.1 |
| 96. Accident | | | | | 27.5 | 2.5 | 1.9 | 0.3 |
| 97. Incident Involving Someone Other than R | | | | | 120.83 | 30 | 8.4 | 3.3 |
| Uncodable Incidents | 706.55 | 262.32 | 48.9 | 28.6 | 22.5 | 14 | 1.6 | 1.5 |
| Total Incidents | 1445.27 | 917.9 | 100.0 | 100.0 | 1445.27 | 917.9 | 100.0 | 100.0 |
| Total Respondents | 982 | 1,095 | | | 982 | 1,095 | | |

*Case counts include partial interviews. Incidents are weighted to remove multiple within-household reports of the same crimes.