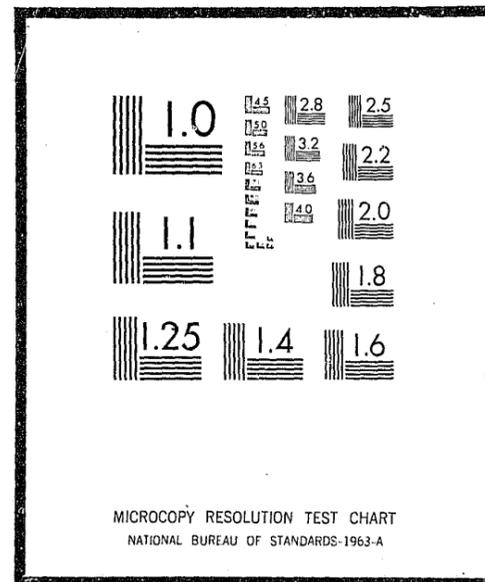


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THE 1974 PORTLAND VICTIMIZATION SURVEY:
REPORT ON PROCEDURES

by
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PREFACE

This report contains detailed information on the methods and procedures used in the 1974 Portland Victimization Survey. It is one of several reports being prepared in conjunction with a general research effort toward developing more reliable methods of measuring crime and victimization and, from these, developing better procedures for evaluating crime reduction and prevention programs. Other reports scheduled for immediate release include:

"Crime and Victimization in Portland: A Preliminary Analysis of Trends, 1971-1974."

"Methodological Approaches for the Analysis of Short-Term Victimization Trends."

"Victimization Rates and Probabilities in the Portland Metropolitan Area." *25890 (Prelim. Report)*

Additional reports are currently in preparation and scheduled for publication by March or April, 1975.

THE 1974 VICTIMIZATION SURVEY: REPORT ON PROCEDURES

INTRODUCTION

The 1974 victimization survey was designed to accomplish several somewhat diverse objectives.

1. The survey data will assist in the development of more reliable procedures for measuring the change in total crime (including crimes not reported to the police). Accurate estimates of change in crime are needed in order to provide an adequate and reliable evaluation of the federally-funded Impact programs in the city. The crimes of major interest are rape, robbery, assault, and burglary.

2. Surveys of households in the metropolitan area around the city of Portland were undertaken in order to examine whether the implementation of the Impact programs reduced crime for the entire metropolitan area, or whether the programs reduced crime in the city but inadvertently displaced crime into the adjacent areas.

3. Extensive information was collected from the survey to assess the effectiveness of two area-based crime prevention programs within the city. One of these is a street lighting program in the ghetto section of northeast Portland. The other is an anti-burglary program administered by the Portland Crime Prevention Bureau. Questions were included to assess not only the change in victimization patterns, but to examine whether programs of these types result in reduced fear of crime, improved attitudes toward the police and other law enforcement officials, increased reporting of incidents to the police, and increased participation in the criminal justice process.

In addition to these direct objectives, another major purpose of undertaking the survey work was to update the 1970 census information in order to assist in the development of area-based crime prediction models.

The 1974 survey victimization data is to be used as followup information for a victimization study conducted in 1972 by the Census Bureau under authorization from the federal Law Enforcement Assistance Administration. The 1972 survey was confined to the city of Portland, and information about victimization patterns within small areas of the city cannot be obtained from it. Thus, the use of the 1974 survey for followup studies is limited to

an analysis of victimization trends for the city as a whole. If additional surveys are undertaken in the future, the 1974 information will provide baseline data on victimization rates, reporting of crimes to the police, attitudes toward law enforcement officials, and the extent of citizen participation and involvement in the crime-reduction efforts. The information will be available for small areas of the city, some small areas outside the city limits, the city as a whole, and the entire metropolitan area (excluding Vancouver, Washington).

In order to accomplish the purposes of the research set forth above, it was necessary to develop a multi-purpose sample design which could accommodate the need for small-area analysis and, at the same time, provide reliable information on victimization for the city as a whole, and for the metropolitan area. In addition, the sample and questionnaire had to be comparable with the 1972 survey, and at the same time be sufficiently flexible to achieve as much comparability with official crime statistics as possible.

THE SAMPLE DESIGN

In order to evaluate the street lighting program and the Crime Prevention Bureau's anti-burglary program, it was necessary to draw a larger number of households within these areas than would have been selected from a perfectly random sample of the city. The analysis of whether these programs have displaced crime into nearby adjacent areas also required that a larger number of households be selected from sections of Portland adjacent to the experimental areas. Oversampling was also required within some of the towns and cities in the metropolitan area in order to accurately describe the victimization patterns within the areas.

To accomplish the objects of small-area analysis, the sample was designed so that enough households were drawn within each area of interest to provide a reasonably reliable estimate of victimization patterns and census data. The number selected was not as great as desired, but some balance had to be maintained between costs and coverage.

A description of the areas selected for special analysis, and the number of interviews in each, is contained in Table 1.

Weighting the Interviews

When information from two or more of the areas is pooled (such as when computing the victimization rates for the city as a whole) the oversampling is corrected by assigning weights to the interviews within each area so that the total number of interviews from the area is equal to the number that would have been selected from a perfectly random sampling procedure. Within each area, the sample was drawn from a block-probability sample belonging to the Bardsley and Haslacher interviewing firm in Portland. This sample had been updated in late 1972. The geographic weights produce a sample which is representative of the city as a whole, and of the metropolitan area as a whole.

Although the households are a random sample within each area, the respondent who was interviewed was not randomly selected. Again, this is not the best procedure, but some balance had to be maintained between the cost of the surveys and perfection. Interviewers were instructed to do as much interviewing in the evenings and on the weekends as they did during the day. This procedure worked fairly well, but the number of women in

Table 1

GEOGRAPHIC LOCATION OF SAMPLE

	<u>No.</u>	<u>Description</u>
Street Lighting Program Area	311	Area bounded by Fremont Street on the South, Union on the East, Interstate 5 on the West, and Ainsworth on the North except that Peninsula Park is included. This area lies mainly in census tracts 34.01 and 34.02. A second noncontiguous area which is included in the SLP is bounded by Fremont on the North, 21st Street on the East, 7th Street on the West and Broadway on the South. This area is mainly in Tract 24.01.
Area Adjacent to the SLP	430	The area from which these interviews were drawn lies entirely within the following census tracts: 22.01, 23.01, 23.02, 24.02, 25.01, 25.02, 33.02, 32, 36.01, 37.02, 38.03, 35.01, 35.02
Crime Prevention Bureau Participants	87	These addresses were drawn randomly from a list of CPB participants.
CPB Primary area	115	From tracts 36.02 and 19, 115 households were selected, in addition to those above.
Remainder of City	1015	From the remaining area within the city limits, 1015 households were selected.
Oregon City	211	
Milwaukee	196	
Gresham	205	
Hillsboro	212	
Lake Oswego	189	
Beaverton	216	
Multnomah County	300	(Unincorporated areas)
Clackamas County	205	(Unincorporated areas)
Washington County	224	(Unincorporated areas)

the sample was about 4% greater than the estimated number of women in the population. This bias could produce distorted estimates of some crimes, since men are more apt to be the victims of assaults and robberies than women.

Another source of bias which exists in the unweighted data is that the number of respondents aged 16-20 is slightly less than the proportion of this age group in the population (8% rather than 12%). To correct for these slight biases in age and sex, one could simply compute all of the crime rates separately for men and women within each age group, and then calculate the combined crime rate by projecting the results in accordance with the relative percent of the population in each age group who are men and women. This, however, is a cumbersome procedure, and increases the cost and difficulty of the data analysis. A simpler procedure which produces exactly the same result is to weight each individual interview so that the characteristics of sample respondents, as a whole, represent the characteristics of the population as a whole.

The age and sex biases in the original data were corrected by calculating the correct percentage of men and women for the age group 16-20 and over 20 within each of the experimental areas. Weights were then assigned to the interviews within each segment.

Weighting procedures of this type were used by the Census Bureau in analyzing the data from the 1972 victimization survey, and such procedures are commonly employed by all reputable research organizations, since they result in considerably less error than unweighted interviews. Without the age and sex weights in the 1974 data, the number of victimizations would be slightly underestimated.

SAMPLING ERROR

All surveys are subject to sampling error, but a randomly selected or a weighted random sample has a measurable amount of error in the estimates. In Table 2 are the .05 confidence intervals for samples of various sizes when the proportion responding in a particular way varies from 1% to 50%.

The confidence interval does not change much between a sample of size 10,000 or 20,000 (the approximate size of the 1972 LEAA survey) and 1900 (the sample size within the city of Portland in 1974). For example, if 5% report that they have been the victims of an assault, and the sample contains 20,000 respondents, the confidence interval is $\pm .004$. The upper limits would be $.05 + .004 = .054$, and the lower limit is $.046$. If 5% reported being victims of assaults, and the sample contained 1900 persons, the lower confidence interval is $.04$ and the upper interval is $.06$.

Surveys may contain error other than the easily measured random sampling error, however. The 1974 survey was conducted under the general philosophy that any citizen has a right to express an opinion to an interviewer, if he/she wishes to do so, but every citizen has the right to refuse to be interviewed and to refuse to divulge information. Interviewers were instructed not to hassle the citizens, and not to force themselves upon persons who were refusing to be interviewed, or to divulge information as a matter of principle. When interviews could not be completed at the assigned household, the interviewers were given a "random walk" pattern based on a previously assigned digit and on whether the street address of the house ended with an odd or even number. Of the original assignments made to interviewers, 67% were completed at the assigned address. Of the remaining 33%, most were due to vacant houses or to persons not being at home after two call-backs by the interviewers. Some persons did refuse to be interviewed, however. This was particularly true in the ghetto area of Portland (in which the street lighting project is located).

The problem persisted in spite of the fact that black interviewers (at least one of whom was known to the local residents) were conducting the interviews. Interviewing in the area was temporarily halted in order to better explain the survey, its purposes, and potential value to the residents. (Information from the area was that on some blocks the neighborhood groups agreed to not submit to any more interviews. This is an

Table 2

CONFIDENCE INTERVALS¹

Size of Sample	Proportion with Characteristic						
	.01,.99	.05,.95	.10,.90	.20,.80	.30,.70	.40,.60	.50,.50
20,000	.0013	.003	.0041	.0055	.006	.0067	.007
10,000	.0019	.004	.0058	.008	.009	.0096	.01
3,950	.003	.007	.0093	.012	.014	.015	.015
2,500	.0038	.008	.012	.016	.018	.019	.02
1,900	.004	.009	.013	.018	.02	.022	.022
1,400	.005	.011	.016	.02	.024	.026	.026
1,000	.006	.0135	.0185	.025	.028	.03	.03
750	.007	.015	.02	.028	.033	.035	.035
500	.009	.019	.026	.035	.04	.043	.043
300	.011	.025	.034	.045	.052	.055	.055
100	.018	.04	.059	.078	.089	.096	.096
50	-	.06	.083	.11	.127	.13	.13
20	-	.09	.13	.17	.20	.21	.22

¹ Formula:

$$CI = P \pm 1.96 \sqrt{\frac{P(1-P)}{N}}$$

over-interviewed area of the city.) Interviews were completed in 311 households in this area, and 100 of these were substitute interviews. Thus, in spite of the reluctance encountered in the area, the completion rate was approximately the same as for the city as a whole.

When interviewers have been permitted to select substitute households with a randomized walk pattern, the substituted households should be compared to those in the original sample in order to determine whether the substitution interviews actually were selected randomly.

The characteristics of heads of households from the sample which was drawn randomly were compared with the characteristics of household heads in that portion of the sample which presumably was selected randomly by the interviewers. There were no significant differences with regard to race, education, income, home ownership, or any one of the variables examined--including the number of victimizations per household.

THE QUESTIONNAIRE

Considerable research and pre-testing has been done by the LEAA in preparation of a set of screening questions designed to elicit from the respondent a recollection of all the incidents committed against the person during a particular time period. This research indicates that a three to six month recall period is the best, but that memory lapses for a period of 12 months are not much greater, and the degree of coverage from the interview is considerably increased. We used the 12-month recall period, which is the same time-span used in the LEAA surveys.

The screening questions developed for and used in the surveys conducted by the Census Bureau are designed so that each separate question fits a legal definition of a different crime. The questions are worded in normal language, using examples, but become quite repetitive and redundant. There is one question each for a robbery, attempted robbery, attack with a weapon other than a gun or knife, attack with a gun or knife, threatened with a gun or knife, and attacked in any other way. The correspondence between questions and legal categories was used originally so that the crime codes could be directly assigned from the screening questions. This procedure was criticized in subsequent research, since respondents generally do not hear or notice the subtle distinctions, and tend to answer "yes" to the first question which bears some resemblance to the incident committed against them (if any). Likewise, the procedure was criticized by law enforcement officials for not being comparable to the methods used by the police for classifying crimes. In normal police procedure, considerable attention is paid to the actual details of an incident before the proper code is selected.

In the 1974 survey, some of the screening questions were combined in such a way that respondents would not have to make subtle distinctions between crime types, but the wording and examples used in the 1972 survey were retained in their exact form.

If the respondent had been the victim of a crime, a detailed incident report was completed by the interviewer (after finishing with all of the screening questions). The questions in the 1974 interview schedule are virtually identical to those in the 1972 survey, and are designed to provide information very similar to that collected by the police from a victim.

If the respondent reported two crimes, one incident report was filled out for each. The only exception to this procedure was when the respondent reported that there had been a series of three or more incidents of the same type, committed in basically the same way and (if known) by the same persons. If these conditions were met, all of the incidents were summarized in one incident report, and the total number of incidents recorded. This procedure is the same one used by Census Bureau interviewers.

INTERVIEWING AND QUALITY CONTROL

Local interviewers, from the Bardsley and Haslacher firm in Portland, were trained on the questionnaire and incident report by Dr. L. Harmon Ziegler, John and Patricia Stryker, and the professional training staff at Bardsley and Haslacher. Each interviewer practiced on the form, and was required to turn in the first five interviews which were conducted. (New interviewers--those without previous interviewing experience--were trained more extensively in interviewing techniques, and were required to turn in the first two interviews.) When the interviews were turned in, they were immediately read and evaluated in order to correct errors made by the interviewers. If some of the interviews were not usable, they were excluded from the final sample.

In addition to daily monitoring, reading, and evaluating the questionnaires, a 10% sample of interviews each day was selected for telephone verification. This verification was done by members of the research team, not by the interviewing firm, although the firm conducted its own verification of about 10% of the interviews. As a supplement to the verification of a 10% random sample, telephone call-backs were made to an additional 20% of the persons interviewed. Some of these were to check on interviewers when an overly suspicious coder believed that the responses on questionnaires turned in by one interviewer were too similar. None of these suspicions were confirmed. Most of the additional call-backs were to locate the scene of the victimization with enough precision to permit a census-tract coding of the crimes, or to obtain additional information about the crime so that the classification would be more accurate. We permitted and asked the respondent to comment on the interviewers during these telephone conversations, and the reports were all either neutral ("she just did it the way she was supposed to") or positive. There were negative reactions to the survey, but these persons apparently called the interviewing firm or the police (to check the authenticity of the interviewer) rather than Oregon Research Institute.

Keypunching and Editing the Machine Readable Data

Errors can also be introduced into survey data during the keypunching phase of the operation. To avoid this, 100% verification of all keypunching was required.

As a final check to avoid errors in the data, a comprehensive editing program was prepared to use on the data after it had been written onto magnetic tape. Codes which were "impossible" were identified for each question, inconsistent codes were identified for some portions of the survey, and for questions on the incident report that were critically important in classification or counting incidents, a missing data check was developed. The editing program listed the interview number and the questions which failed the editing test. One of the coding supervisors (a University of Oregon law student) checked the original interview form for each questionnaire which had (or possibly had) an error. Errors which were important for the analysis of the data were corrected.

One of the consistency checks used was to compare the number of victimizations in the household which the interviewer recorded on the front cover with the number of "yes" responses on the screening question, and with the actual number of incident reports. If a discrepancy existed, the original questionnaire was checked to correct the problem and to insure that all incident reports were accounted for, but the number of victimizations on the front was not changed and the number of yes responses was not corrected. Corrections were not made on the number of yes responses because we hope to conduct a simple methodological study of the amount of bias introduced if crime classification is based on the screening questions, or if the count is based on interviewer reports of the number of incidents.

CODING, CLASSIFICATION AND COUNTING INCIDENTS

The first LEAA surveys were criticized by many law enforcement officials because of the methods used to classify and count incidents. As noted above, the crime classifications in some interviews were developed directly from answers to the screening questions rather than from the detailed information in the incident report. This procedure was not used in the 1974 survey.

Each questionnaire was first read by a trained coder who coded answers to open-ended questions, checked for inconsistencies or missing data within the questionnaire, and prepared the form for keypunching. Following the first reading, the questionnaires were read by one of three persons specially trained to code the crimes into the proper classifications. The classification scheme was developed so that it would be comparable to the Oregon Statutes and comparable to the UCR produced from Oregon.

The crimes were not classified from the screening questions. Rather, the detailed report of each incident was read and evaluated, the interviewer's report on the authenticity of the incident was read, and the crime was coded from the details of the incident report. After the classification, the entire incident report section was read by Richard Frey, LLB, to check the classification code. If his judgment differed from the original coding, the coders reviewed the statutes in an effort to reach agreement on the code. Several incidents were reviewed with personnel in the records department in Eugene or Lane County to obtain a professional opinion. In some instances, the information on the incident reports was not sufficient to determine exactly what code should be used. In these instances (and there were not many) the most probable code was assigned to the incident, but an additional digit was used to indicate that some doubt existed about the accuracy of the classification.

Coding Reliability

The usual tests of inter-coder reliability are inappropriate for determining the reliability of the crime classification codes because at least two persons participated in selecting the code. This procedure definitely will reduce the amount of simple error (such as transposing two numbers), and should increase the overall accuracy of the coding if the coders themselves were using the same rules and making the same types of judgments used by coders in police departments.

A preliminary reliability check was made on the coding by randomly selecting 30 incidents which had been coded, and then selecting an additional 23 incidents which typified the major problems and most likely sources of error. Short descriptions of these incidents were prepared. A crime classifier in the Lane County Police Department consented to code the short description of the incidents, and her codes were then compared to the ones which had been placed on the incident report. Of the 30 randomly selected incidents (10 burglaries, 8 larcenies, 1 malicious mischief, 1 robbery, 1 disorderly conduct, 4 attempted assaults, and 5 completed assaults) there were no differences in the expert's coding and the codes which had been used for the survey.

Twelve incidents were selected in which an assault with some type of a weapon other than a gun or knife had been attempted or completed. The ORI coders had classified nine of these as simple assaults and three as aggravated assaults. The expert classified all of them as simple assaults. The correct code is mainly a matter of judgment, or at least is subject to internal policies of police departments. This problem is illustrative of the commonly held belief that the distinction between simple and aggravated assault is highly judgmental. It was for this reason that we combined simple and aggravated assaults in the analysis of the data. The term simple assault is somewhat misleading in that these are serious incidents, involving the use or attempted use of force and the intent to injure someone. There is really nothing simple or trivial about a "simple" assault.

Two purse snatchings were selected in which the woman was knocked to the ground and suffered injuries. The ORI coders classified these as robberies, whereas the Lane County coder classified both as larcenies. The UCR rules clearly specify that such incidents should be considered robberies, not larcenies, and we believe our coding is correct. Again, however, the example illustrates the variability in crime coding. (The reliability check of the Portland police department original incidents did not include larcenies, so there is no way to know definitely whether some forcible purse snatches with injury are coded as larcenies in Portland. The original documents selected in the sample from Portland, however, do contain several robberies which were forcible purse snatches.)

Four incidents were selected in which the crime code could have ranged from forcible rape to minor sex offense to no crime at all. These were all coded in the same way by the expert and the ORI coders (two were rapes, one was a minor sex offense, and one was not a criminal incident).

Three incidents which the ORI coders had judged not to be a crime of any type (e.g., some type of report other than a crime report would have been filled out) were selected, and the expert coder also coded these as being non-criminal incidents.

Two incidents which might have been coded as attempted robbery vs larceny vs simple assault were selected. The coding on these was identical.

Counting the Number of Victimizations

Several decisions must be made when attempting to count the number of incidents which were committed against a respondent or which were committed within a specific area. Detailed information is not available from the Census Bureau about the counting methods which they used, and in the absence of this the counting procedures used for the 1974 survey were designed to be comparable to official police practices.

When counting the number of household crimes, one crime was counted for each incident even if the incident involved taking several items (some form within and some outside of the home, for example). This procedure is comparable to the method of counting used in the UCR and in Oregon police departments. If a larceny (theft without entrance of a structure and without force) and a burglary were committed at the same time, we coded the more serious offense and only counted it as one crime. The report prepared by the Portland Impact Planning Office on the Census Bureau procedures says that the 1972 survey counted both offenses. If this is true for household crimes, then their data probably includes more larcenies than the 1974 survey would.

For personal crimes, the police procedures are to count one incident for each victim (even though there is only one assailant) and to classify the crime into the more serious of the categories. This was the procedure used in the 1974 survey. However, the Impact office document about the 1972 survey says that double-counting was used in the 1972 LEAA survey. If both a rape and robbery occurred, both were apparently counted in the 1972 data. It is unfortunate that information has not been supplied about the amount of double-counting (e.g., how many of the rapes were also counted as robberies). And, it is not clear whether an incident which was a rape would also be counted as an assault, although this is a doubtful and questionable procedure, since by definition a forcible rape is an assault. These differences in counting could have produced considerable noncomparability

between the surveys except for the fact that only four combination offenses were encountered in the 1974 data (offenses in which the definition of the more serious crime does not include the less serious).

The police statistics include crimes committed against all persons, regardless of age, and crimes against children under 12 were counted in the 1974 data. In the 1972 survey, only the crimes against persons aged 12 and over were included. This does not constitute a comparability problem, because very few incidents against children under 12 were reported in the 1974 survey, and none were among the target offenses (most were larcenies).

RESPONDENT RECALL FOR OTHERS

There are two interviewing procedures which have been used in victimization surveys. In one procedure (used by LEAA) every person in the household aged 14 and over is questioned individually by the interviewer. In the other procedure, only one person in the household is interviewed, and is asked about crimes committed against them personally and against other members of the household. In the analysis phase for the latter procedure, some surveys have based the victimization rate estimates on all of the incidents reported by the respondent. In other surveys, only the crimes committed against the respondent and against minor children (aged 15 and below) are used to calculate the victimization rates.

The major advantage of the first procedure is that some incidents committed against other persons will not be known or remembered by just one respondent. To count all of the crimes in the household in the most accurate fashion, every person should be questioned. The disadvantage of this method is that the interview takes considerably more time per household, and the cost is much greater in comparison with the extent of areal coverage.

The respondent-only procedure (designed by the National Opinion Research Center) provides more areal coverage for a smaller cost. Some persons, however, question the accuracy of the victimization rates, since respondents may not know about crimes against others, or may not remember them as well as they remember crimes committed against themselves. Thus, the victimization rates produced by the latter procedure may be lower than they should be.

An analysis of the problem by the NORC led to the conclusion that the victimization rates for respondents were not significantly different than for other adults (with similar characteristics) whose incidents had been recalled by the respondent and reported to the interviewer.

The ORI survey used the respondent-only method, because it does not take much more time to ask the respondent about crimes committed against others, and because we hoped that the NORC results could be replicated. During the first phase of the analysis we examined the recall bias in the 1974 data. The test of whether there is recall bias using the respondent-only method is to measure the victimization rates for respondents, and to

compare these rates to the ones for other persons in the households who had not been interviewed personally. If respondents are just as able to remember crimes against others as against themselves, the victimization rates for the metropolitan area which are based only on respondents should not be significantly different than the victimization rates based only on non-respondent incidents. The assumption underlying this test is that the actual victimization rates for respondents is the same as for non-respondents. Any difference is due to respondents being less able to remember incidents against others.

The information in Table 3 reveals that the victimization percentage for non-respondents is significantly lower for the crimes of rape, assault, and personal larcenies. For robberies, the percentage for non-respondents is lower, but not significantly so.

The percentages in this table cannot be converted into victimization rates for the metropolitan area, and are different from the actual rates because incidents in months outside the one-year time span were included, series of incidents were only counted as one incident, crimes against children under 15 years of age were excluded entirely because there were no respondents under the age of 15, and multiple-victim incidents were only counted as one incident. These counting procedures were used because we believe they constitute the best test of the hypothesis, even though they are not the best way of counting incidents to compute a victimization rate comparable to official police statistics.

There is no way to know why our results differ from the test conducted by the NORC, but results of the test clearly indicate that if crimes against non-respondents (recalled by the respondent) are included, and the base population is increased by the size of the non-respondents, our estimates of the victimization rate for Portland would be biased downward. For this reason, we excluded incidents recalled against other adults from all of the analyses. Incidents against children were included, however. The LEAA surveys also include incidents against children recalled by other adults in the household.

Table 3

RESPONDENT RECALL ABILITY FOR OTHER ADULTS¹

	Respondent Victim	Respondent Recalled for other Adult	
No. of Persons	3950	4711	
	<u>%</u>	<u>%</u>	<u>Z</u>
Rape	.001*	.0003	1.48
Robbery	.054	.046	.47
Assault	.234*	.1698	2.14
Personal Larceny	.628*	.407	4.7

* Statistically significant difference

Formula:

$$Z = \frac{\bar{X}_1 - \bar{X}_2}{p(1-p) \left(\frac{1}{N_1} + \frac{1}{N_2} \right)} \quad p = \frac{Nc_1 + Nc_2}{Np_1 + Np_2}$$

¹ The method of counting incidents for this comparison differs substantially from the procedures used to count incidents and convert them to victimization rates. These figures are not to be used for calculating victimization rates.

END