# EFFECTS OF BOUNDING ON TELESCOPING IN THE NATIONAL CRIME SURVEY

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## Introduction

In a general population sample survey calling for respondent recall of events experienced prior to the interview, the type of memory error known as telescoping is of major concern. Telescoping is the tendency of the respondent to report events as occurring either earlier or later than they actually occurred. An event being reported as occurring earlier than it actually occurred is backward telescoping, whereas forward telescoping is reporting an event as occurring later. Further, both backward and forward telescoping can be either internal to the survey's reference period, or external. Internal telescoping occurs when the respondent correctly places an event within the reference period, but misinforms on the precise day, week, or month of occurrence. External telescoping occurs when the respondent erroneously places an event into the reference period. Telescoping is an important technical issue in a panel survey involving recall for two reasons. First, depending upon the magnitude, nature and direction, uncontrolled telescoping can result in serious response biases in survey estimates for a given time period; and second, various procedural efforts to control telescoping have a major impact on survey design and cost.

The National Crime Survey (NCS) is one such retrospective survey, involving individual respondents in a national, rotating sample of 72,000 housing units. The NCS rotation scheme is such that an incoming panel or rotation group of approximately 12,000 new sample units is introduced over each 6-month period, replacing about the same number of units which expire from the sample or rotate out during the 6-month period. Thus, at any given time, the NCS sample consists of a number of panels, or rotation groups, that are being interviewed for the first through the seventh time. (Due to the nature of the original introduction of the NCS sample, some small number of households may be interviewed 8 or 9 times through 1976 only, after which time the NCS rotation scheme will be fully operative.) Respondents are asked to report all incidents of specific types of criminal victimization that they have experienced during the 6 months preceding the interview month. The types of crime covered by NCS are completed and attempted assault, robbery, burglary, larceny, and auto theft. A self-respondent technique, as opposed to household respondent, is used to collect data for persons 12 and over. (Papers presented previously at ASA meetings [1, 9] explain the NCS design in greater detail.)

Developmental work during the 3-year period prior to the start of NCS in July 1972, provided some information about the nature, direction and magnitude of telescoping in victim surveys. Results of 3 reverse record check studies of known victims (4, 7, 8) indicated the presence of both forward and backward telescoping, the net effect being forward. These results also indicated the presence of both internal and external telescoping. The next stage of developmental work involved a victim survey supplement to the Quarterly Household Survey, a national probability sample of 18,000 households. Results of this effort (6) indicated that telescoping, both internal and external, was of such a magnitude as to justify incorporating special features into the NCS design and procedures that would minimize the effects of telescoping.

One NCS design feature, intended to minimize the effect of internal telescoping, both forward and backward, is the use of a rolling reference period, achieved by interviewing one-sixth of the sample each month. Data on incidents of victimization are then tabulated according to reported month of occurrence by 3-month calendar periods called Data Quarters. Data for one Data Quarter are collected over 8 months of interviewing. Because households are interviewed every 6 months, one-third of the sample is interviewed twice for the same Data Quarter. For example, a respondent interviewed in February, 1974 would be interviewed again in August, 1974; this respondent would contribute data for January, 1974 from the first interview, and for February and March 1974 from the second interview. In an effort to control forward external telescoping, an interviewing procedure called bounding was instigated for the purpose of minimizing the shifting of reports of crimes into the NCS reference period. Bounding is thus a procedure utilized to prevent the reporting of the same incidents in consecutive reference periods by eliminating reports of incidents that were also reported during the previous interview. The initial interviews at addresses in incoming rotation groups are used to bound subsequent interviews; they are not used to produce the estimates of victimizations. This is a very costly feature of the NCS design, since the data from incoming rotation groups are not used in tabulating results for publication.

The primary focus of this paper is to examine the effectiveness of the procedural and design features of NCS related to bounding, in controlling forward external telescoping, using data, for the first time, from NCS itself. This will be done by comparing estimates of victimizations based on bounded data from returning rotation groups with estimates based on unbounded data from incoming rotation groups. This estimate of the bounding effect using NCS data can provide a basis for re-evaluating the cost-effectiveness of this aspect of the survey design, though in this paper we provide only a bivariate description of the data.

A second issue addressed in this paper is variation in forward external telescoping, by means of examining defferential effects of bounding by demographic and socioeconomic characteristics of respondents, as well as by characteristics of the incidents of victimization. We believe this is a

critical issue to investigate in victimization surveys. If there are no significant differences in telescoping for either certain classes of respondents or for certain classes of events, then relationships and patterns would be unaffected by the inclusion of unbounded data in producing survey estimates, though levels of victimization would be affected. Thus it may be less crucial to maintain the bounded aspects of the NCS. On the other hand, if there are significant differences in telescoping by certain respondents or for certain incidents, then relationships and patterns would be distorted by including unbounded data in the survey estimates.

Two broad questions suggest themselves. First, do some groups of respondents telescope events more than others? Second, are some types of incidents telescoped more than other types? There are two feasible, but opposing hypotheses related to differential telescoping by type of incident. One hypothesis is that the more important, more serious, or more salient events are telescoped forward to a greater degree than the less important, etc., perhaps because the less important are more likely to be completely forgotten; the second hypothesis is that the less important, less serious, or less salient events are telescoped forward to a greater degree because the month of occurrence is less accurately recalled and therefore subject to greater recall bias.

A final aspect of the bounded NCS design discussed in this paper is the extent of actual bounding of interviews among households, and within households among persons, in repeat rotation groups.

## Comparison of Victimization Rates

We have two estimates, total personal victimizations 1/ and total property victimizations, 2/ that are of primary interest to us. For each, we are comparing the total bounded victimization rate obtained from the returning rotation groups with the unbounded victimization rate estimated from the incoming rotation groups. The rates for each sample are produced using identical processing, weighting, and tabulation procedures, with appropriate adjustments to account for the fact that the incoming rotation group is approximately one-sixth the size of the bounded sample.

The first two tables in our report show the bounded and unbounded rates for total personal and total property victimizations for Data Quarters I-74 through I-75, plus a z-test of the difference between the rates. All rates reported in the tables are victimizations per thousand people or households. Tables 1 through 7 indicate for each of the Data Quarters under analysis (column 1) the weighted population sizes being represented by the bounded and unbounded samples (columns 2 and 3), the victimization rates estimated for the population from the bounded and unbounded samples (columns 4 and 5), the difference between the unbounded and bounded rates divided by the bounded rate and expressed as a percent (column 6), the standard errors associated with the two rates (columns 7 and 8), the standard error of the absolute difference between the rates (column 9), and the z-statistic testing whether the difference between the bounded rates is significantly greater than zero. The test is calculated as the ratio of the absolute difference between the unbounded and bounded victimization rates to the square root of the sum of the squared standard errors associated with each rate (3). The standard errors used were published in the 1973 Advance Report, Criminal Victimization in the United States by the Law Enforcement Assistance Administration (LEAA). Because of the large sample sizes, the z-statistic approximates the normal distribution, and can be used in conjunction with a table of normal areas and ordinates to determine the level of significance of the test. The test being performed is a one-tailed z-test, because the procedure of bounding as applied in NCS would only eliminate reporting of victimizations in two consecutive quarters. There is never a chance that victimizations would be added to the reports because of bounding. The null hypothesis, formally stated, is "there is no difference between bounded and unbounded victimization rates." The alternative hypothesis, formally stated, is that "unbounded victimization rates are greater than bounded rates." A z-value of greater than 1.64 means that we can be sure 95 out of a hundred times that the estimated differences are greater than zero, and thus are not due to sampling variation; similarly, a value greater than 1.28 means that 90 out of a hundred times, the results will not be due to sampling variation (except in the comparison of victimizations reported to police, Table 8, which is a two-tailed test, with z-values of 1.96 and 1.64 respectively).

The z-values clearly show that there are statistically significant differences in the bounded and unbounded personal crime victimization rates for each quarter (Table 1), demonstrating that bounding does eliminate a significant number of duplicate victimization reports. The same is true for bounded and unbounded property crime victimization rates (Table 2). The unbounded personal victimization rates average 43.8 percent higher than the bounded rates, ranging from 36.4 percent to 58.6 percent. The unbounded property victimization rates average 39.9 percent higher than the bounded rates and range from 35.0 percent to 44.1 percent.

Now the question is: How does telescoping affect subgroup estimates and estimates by type of crime? The following analysis again makes use of testing the difference between bounded and unbounded rates, and represents a preliminary look at the data. A more detailed analysis of the effects of bounding on telescoping for subestimates is planned as more data are collected. The first comparisons we make are by type of crime. Rates of assaultive violence without theft are 44 percent higher on average in the unbounded sample than in the bounded sample, and rates for personal theft without assault are 51 percent higher on average in the unbounded sample (Tables 3A and 3B). But comparing the rates quarter by quarter, there is no clear-cut pattern showing that unbounded rates are uniformly higher for the one type of crime over the other. The same result can be found comparing burglaries with larcenies (Table 4A and 4B). The unbounded rates for burglaries

and larcenies are on average 40 percent higher than the bounded rates. But in some quarters the relative difference for burglaries is significantly ( $\alpha < .10$) larger than for larcenies, and in other quarters it is smaller. Telescoping doesn't seem to consistently affect rates for one major type of crimes more than another.

Looking at subgroups of burglary, however, it becomes apparent that telescoping is much more prevalent for attempted entries than for actual entries. The unbounded sample rates for burglary: actual entry are on average only 32 percent higher than the bounded rates, whereas the unbounded sample rates for burglary: attempted entry are 66 percent higher than the bounded rates on average (Tables 5A and 5B). The same is true when comparing completed and attempted larcenies. Again the relative difference is higher for attempted than completed crimes. The average relative difference for the five quarters for attempted larcenies was 50 percent, while for completed larcenies it was only 40 percent (Tables 6A and 6B). So it is apparent that telescoping does have a differential effect on the rates of various subcategories of crimes.

There also seem to be some differences in telescoping by demographic characteristics of households for property crimes. The relative difference between bounded and unbounded rates for property crimes reported by one-person households is rather low, only 20 percent higher for the unbounded sample on average. This relative difference increases as the number of persons in the household increases, rising to a 51 percent greater reporting rate in the unbounded sample for households having six or more persons (Tables 7A-7D).

Another factor which may indicate saliency or importance, and thus influence telescoping, is whether or not the victimization was reported to the police. In 4 of the 5 Quarters examined, a significantly larger proportion of the property victimizations were not reported to the police in the unbounded sample (Table 8).

Two additional factors, total loss suffered in property victimizations and for personal victimizations, whether or not the offender was a stranger to the victim, were included in our analysis as possible indicators of saliency or importance. However, we found no consistent pattern associated with either of these variables over the 5 Data Quarters examined.

The figures presented in these tables are simply a faithful reporting of the degree to which telescoping occurs. At present, it is safe to conclude that telescoping would have a significant effect on victimization rates if the interviews were not bounded. Beyond that, we can point out that some crimes are telescoped to a greater degree than others, either according to the type of crime or the circumstances, or because of the demographic characteristics of the household. We do not have valid empirical information about <u>why</u> these factors affect telescoping.

<u>Qualifications to Comparisons</u>

Three qualifications should be noted with regard to the preceding analysis comparing bounded with unbounded data from NCS. The first qualification is that since respondents are interviewed every six months, the Data Quarters are not independent from one another, as there is some overlap of respondents from one Data Quarter to the next. Secondly, all data from returning rotation groups are considered and treated as bounded for purposes of the preceding analysis in this paper. However, since NCS uses a probability sample of addresses rather than designated households or persons, not all of the interviews conducted in returning rotation groups are subject to the actual interviewing procedure of bounding. For any interviews in a household to actually be bounded, the identical household must have been interviewed the previous enumeration period. Therefore, interviews in replacement households, and households that were noninterview or not in sample the previous period, are actually unbounded. However, data from these unbounded interviews are included with data from the bounded interviews because they are in returning rotation groups, and their exclusion may bias the sample.

The unbounded households in returning rotation groups comprise a sizeable portion of the interviewed sample (Table 9), averaging 13.3 percent over the five Collection Quarters, I-74 through I-75. Of these unbounded households, an average of 9.6 percent are replacement households, and 3.7 percent were previously noninterview or not in sample. These unbounded households contribute disproportionally more victimizations than do the actually bounded households. Though bounded households make up about 86 percent of the interviews, they contribute only 76 percent of the victimizations, while unbounded households, which comprise only 13 percent of the interviews, contribute 24 percent of the victimizations. This translates into a reporting rate of about 79 percent more victimizations from unbounded households than expected from their proportion of the sample.

Even more striking in terms of contributing victimizations, is the difference between types of unbounded households. Households that were previously noninterview or not in sample, while making up 4 percent of the interviews, contribute almost 6 percent of the victimizations. But replacement households, which primarily represent movers and make up about 10 percent of the interviews, contribute an average of nearly 18 percent of the victimizations, or 92 percent more than their expected proportion. Recalling the overall difference of about a 40 percent higher victimization rate for unbounded, incoming rotation groups than for bounded, returning rotation groups, these figures appear to indicate that something more than merely the lack of bounding may be related to the disproportionate reporting of victimizations among replacement households. It is conceivable that they actually experience victimization more frequently than non-movers for reasons associated with their mobility--perhaps they move to get away from crime. This question appears to warrant further investigation.

Admittedly the set of data used in the preceding discussion of unbounded data within returning rotation groups is somewhat lacking in refinement, being based on unweighted counts. However, the stability of the patterns is apparent and provides evidence that the effect of bounding is understated in comparisons of data between incoming and returning rotation groups, since returning groups include a substantial amount of unbounded data.

The third, further qualification is that even within actually bounded households, some interviews with individual household members are unbounded, either because the person is new to the household since the prior enumeration period, or because the person was previously noninterview. A special computer match of interviewed persons in Collection Quarters I-74 through I-75 with files for previous enumeration periods was performed for the purpose of determining correspondence and bounding of individuals within bounded households. Results of that operation indicate an average of about 95 percent bounded individual interviews (Table 10). Again, this pattern is quite stable over time; and again a difference in reporting victimizations between bounded and unbounded interviews is evident. An average of 7.9 percent of the bounded persons, and 10.5 percent of the unbounded persons reported one or more victimizations. These data also appear to provide evidence that the bounding effect is understated in comparisons of incoming with returning rotation group data.

## Conclusion

The data presented in this paper strongly support the conclusion that NCS bounding procedures and design effectively reduce the memory bias of forward external telescoping. Our results, comparing bounded with unbounded sample data, are consistent with results from similar comparisons in the area of consumer expenditures (5). In that study, however, Neter and Waksberg point out that telescoping effects are compounded with conditioning effects in comparisons between unbounded data based on first interviews and bounded data based on second or later interviews. Evidence from the expenditure study (5) and also from a study of NCS panel bias (9) suggests that conditioning probably accounts for a much smaller portion of the observed differences in NCS than does telescoping.

Further, we can conclude that some variation in telescoping is associated with characteristics of victimization events. Our analysis indicated that telescoping was present for all major types of crimes, but in no discernible pattern. However, it did indicate a greater degree of telescoping for the subcategories of attempted larceny and attempted burglary than for the completed crimes. It also indicated a larger proportion of victimizations not reported to police in the unbounded sample than in the bounded. These results, considered alone, could be interpreted as evidence that the less serious, less important, or less salient events are more subject to the recall bias of forward telescoping than the more serious, etc. However, the finding of no pattern of association with total loss or victim-offender relationship, does not support this interpretation. Therefore, we can only conclude that some characteristics of events appear to be related to differential forward external telescoping. Finally, our evidence also indicates that some variation in telescoping is associated with household characteristics, but hardly any telescoping can be explained by respondent characteristics. This lack of establishing a strong relationship between respondent characteristics and the memory bias of telescoping, is consistent with findings by Gottfredson and Hindelang on total memory bias based on NCS Cities data (2). Most of the differences found in our analysis of demographic variables, including age, sex, race, education, tenure, and income, were tenuous at best.

We plan to investigate further the differential effects of bounding on telescoping by characteristics of victimization events, households, and respondents, and to test what biases would arise in the sample if unbounded households were excluded from tabulations based on returning rotation groups.

## Footnotes

[1] Personal crimes encompass completed and attempted assault, including rape, and robbery.

[2] Property crimes encompass completed and attempted burglary, larceny, and auto theft.

## References


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(2) Gottfredson, Michael R. and Hindelang, Michael J., "A Consideration of Telescoping and Memory Decay Biases in Victimization Surveys," 1976 (unpublished).

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(4) Law Enforcement Assistance Administration, The San Jose Methods Test of Known Crime Victims, Statistics Technical Report No.1, LEAA, U.S. Department of Justice, June 1972.

(5) Neter, John and Waksberg, Joseph, U.S. Bureau of the Census, "Response Errors, in the Collection of Expenditures Data by Household Interviews: An Experimental Study," Technical Paper No. 11.

(6) Turner, Anthony G., Methodological Issues in the Development of the National Crime Survey Panel: Partial Findings," LEAA, December 1970 (unpublished).

(7) U.S. Bureau of the Census, "Victim Recall Pretest (Washington, D.C.)," Demographic Surveys Division, June 10, 1970 (unpublished).

(8) U.S. Bureau of the Census, "Household Survey of Victims of Crime Second Pretest (Baltimore, Maryland)," Demographic Surveys Division, November 30, 1970 (unpublished).

(9) Woltman, Henry and Bushery, John, "A Panel Bias Study in the National Crime Survey," Paper presented at the annual meetings of the American Statistical Association, August 25-28, 1975, Atlanta, Georgia.

TABLE 1: Total Personal Victimization Rates for Bounded and Unbounded Samples (Rates per 1000 persons)

| Data | Population | | Victimization Rate | | | Standard Errors | | | z of |
|---|---|---|---|---|---|---|---|---|---|
| Quarter | Bounded | Unbounded | Bounded | Unbounded | % Difference | Bounded | Unbounded | Difference | Difference |
| I/74 | 163799000 | 27299833 | 7.89 | 11.30 | 43.219 | .268 | 0.834 | 0.876 | 3.892 |
| II/74 | 164244000 | 27374000 | 8.90 | 12.31 | 38.315 | .285 | 0.871 | 0.916 | 3.721 |
| III/74 | 164861000 | 27476833 | 9.38 | 14.88 | 58.635 | .292 | 0.958 | 1.002 | 5.491 |
| IV/74 | 165344000 | 27557333 | 9.74 | 13.29 | 36.448 | .297 | 0.904 | 0.952 | 3.731 |
| I/75 | 165874000 | 27645666 | 8.55 | 12.17 | 42.339 | .275 | 0.864 | 0.906 | 3.994 |

TABLE 2: Total Property Victimization Rates for Bounded and Unbounded Samples (Rates per 1000 households)

| Data | Households | | Victimization Rate | | | Standard Errors | | | z of |
|---|---|---|---|---|---|---|---|---|---|
| Quarter | Bounded | Unbounded | Bounded | Unbounded | % Difference | Bounded | Unbounded | Difference | Difference |
| I/74 | 71118300 | 11853050 | 102.77 | 138.75 | 35.010 | 1.247 | 3.407 | 3.628 | 9.917 |
| II/74 | 71489200 | 11914866 | 104.09 | 149.97 | 44.077 | 1.249 | 3.502 | 3.718 | 12.339 |
| III/74 | 72163700 | 12027283 | 114.99 | 156.65 | 36.229 | 1.292 | 3.548 | 3.776 | 11.033 |
| IV/74 | 72565900 | 12094316 | 119.80 | 168.96 | 41.035 | 1.308 | 3.647 | 3.874 | 12.689 |
| I/75 | 72686500 | 12114416 | 102.75 | 147.16 | 43.221 | 1.225 | 3.453 | 3.664 | 12.120 |

TABLE 3: Comparison of Bounded and Unbounded Personal Victimization Rates for Various Types of Crimes (Rates per 1000 persons)

3A - Type of Crime: Assaultive Violence--Without Theft

| Data | Population | | Victimization Rate | | | Standard Errors | | | z of |
|---|---|---|---|---|---|---|---|---|---|
| Quarter | Bounded | Unbounded | Bounded | Unbounded | % Difference | Bounded | Unbounded | Difference | Difference |
| I/74 | 163799000 | 27299833 | 5.36 | 8.12 | 51.493 | .215 | .689 | .721 | 3.827 |
| II/74 | 164244000 | 27374000 | 6.67 | 9.09 | 36.282 | .243 | .739 | .778 | 3.112 |
| III/74 | 164861000 | 27476833 | 6.67 | 10.75 | 61.169 | .241 | .812 | .847 | 4.817 |
| IV/74 | 165344000 | 27557333 | 6.76 | 8.83 | 30.621 | .242 | .724 | .764 | 2.711 |
| I/75 | 165874000 | 27645666 | 5.93 | 8.36 | 40.978 | .222 | .699 | .734 | 3.311 |

3B - Type of Crime: Personal Theft--Without Assault

| Data | Population | | Victimization Rate | | | Standard Errors | | | z of |
|---|---|---|---|---|---|---|---|---|---|
| Quarter | Bounded | Unbounded | Bounded | Unbounded | % Difference | Bounded | Unbounded | Difference | Difference |
| I/74 | 163799000 | 27299833 | 1.91 | 2.48 | 29.843 | .107 | .391 | .405 | 1.407 |
| II/74 | 164244000 | 27374000 | 1.63 | 2.62 | 60.736 | .106 | .397 | .411 | 2.410 |
| III/74 | 164861000 | 27476833 | 2.04 | 3.44 | 68.627 | .104 | .431 | .444 | 3.155 |
| IV/74 | 165344000 | 27557333 | 2.33 | 3.52 | 51.073 | .102 | .434 | .446 | 2.667 |
| I/75 | 165874000 | 27645666 | 2.01 | 2.88 | 43.284 | .100 | .407 | .420 | 2.074 |

TABLE 4: Comparison of Bounded and Unbounded Property Victimization Rates for Various Types of Crimes (Rates per 1000 households)

4A - Type of Crime: Burglary

| Data | Households | | Victimization Rate | | | Standard Errors | | | z of |
|---|---|---|---|---|---|---|---|---|---|
| Quarter | Bounded | Unbounded | Bounded | Unbounded | % Difference | Bounded | Unbounded | Difference | Difference |
| I/74 | 71118300 | 11853050 | 19.23 | 27.38 | 42.382 | .567 | 1.598 | 1.696 | 4.807 |
| II/74 | 71489200 | 11914866 | 22.60 | 33.34 | 47.522 | .612 | 1.760 | 1.864 | 5.763 |
| III/74 | 72163700 | 12027283 | 26.85 | 36.62 | 36.387 | .664 | 1.837 | 1.954 | 5.001 |
| IV/74 | 72565900 | 12094316 | 23.89 | 31.40 | 31.436 | .625 | 1.698 | 1.809 | 4.152 |
| I/75 | 72686500 | 12114416 | 20.65 | 29.13 | 41.065 | .581 | 1.634 | 1.734 | 4.890 |

4B - Type of Crime: Larceny

| Data | Households | | Victimization Rate | | | Standard Errors | | | z of |
|---|---|---|---|---|---|---|---|---|---|
| Quarter | Bounded | Unbounded | Bounded | Unbounded | % Difference | Bounded | Unbounded | Difference | Difference |
| I/74 | 71118300 | 11853050 | 79.15 | 105.34 | 33.089 | 1.110 | 3.080 | 3.274 | 7.999 |
| II/74 | 71489200 | 11914866 | 77.13 | 111.93 | 45.119 | 1.094 | 3.141 | 3.326 | 10.464 |
| III/74 | 72163700 | 12027283 | 83.34 | 113.44 | 36.117 | 1.124 | 3.144 | 3.339 | 9.014 |
| IV/74 | 72565900 | 12094316 | 90.78 | 129.74 | 42.917 | 1.163 | 3.295 | 3.494 | 11.151 |
| I/75 | 72686500 | 12114416 | 77.60 | 112.30 | 44.716 | 1.085 | 3.124 | 3.307 | 10.494 |

TABLE 5: Comparison of Bounded and Unbounded Property Victimization Rates for Various Types of Crimes (Rates per 1000 households)

5A - Type of Crime: Burglary--Entry

| Data | Households | | Victimization Rate | | | Standard Errors | | | z of |
|---|---|---|---|---|---|---|---|---|---|
| Quarter | Bounded | Unbounded | Bounded | Unbounded | % Difference | Bounded | Unbounded | Difference | Difference |
| I/74 | 71118300 | 11853050 | 15.08 | 19.89 | 31.897 | .503 | 1.362 | 1.451 | 3.314 |
| II/74 | 71489200 | 11914866 | 17.85 | 25.28 | 41.625 | .545 | 1.532 | 1.626 | 4.569 |
| III/74 | 72163700 | 12027283 | 20.94 | 27.71 | 32.330 | .587 | 1.598 | 1.703 | 3.976 |
| IV/74 | 72565900 | 12094316 | 18.77 | 24.25 | 29.196 | .554 | 1.492 | 1.592 | 3.443 |
| I/75 | 72686500 | 12114416 | 16.09 | 20.43 | 26.973 | .512 | 1.369 | 1.462 | 2.969 |

5B - Type of Crime: Burglary--Attempted Entry

| Data | Households | | Victimization Rate | | | Standard Errors | | | z of |
|---|---|---|---|---|---|---|---|---|---|
| Quarter | Bounded | Unbounded | Bounded | Unbounded | % Difference | Bounded | Unbounded | Difference | Difference |
| I/74 | 71118300 | 11853050 | 4.15 | 7.49 | 80.482 | .282 | .856 | .901 | 3.708 |
| II/74 | 71489200 | 11914866 | 4.75 | 8.06 | 69.684 | .301 | .880 | .930 | 3.559 |
| III/74 | 72163700 | 12027283 | 5.92 | 8.91 | 50.507 | .327 | .915 | .971 | 3.078 |
| IV/74 | 72565900 | 12094316 | 5.12 | 7.15 | 39.648 | .307 | .833 | .888 | 2.287 |
| I/75 | 72686500 | 12114416 | 4.56 | 8.70 | 90.789 | .289 | .903 | .949 | 4.365 |

TABLE 6: Comparison of Bounded and Unbounded Property Victimization Rates for Various Types of Crimes (Rates per 1000 households)

6A - Type of Crime: Larceny--Completed

| Data Quarter | Households | | Victimization Rate | | | Standard Errors | | | z of Difference |
|---|---|---|---|---|---|---|---|---|---|
| | Bounded | Unbounded | Bounded | Unbounded | % Difference | Bounded | Unbounded | Difference | |
| I/74 | 71118300 | 11853050 | 73.98 | 97.72 | 32.090 | 1.078 | 2.990 | 3.179 | 7.469 |
| II/74 | 71489200 | 11914866 | 72.57 | 105.22 | 44.991 | 1.065 | 3.072 | 3.252 | 10.040 |
| III/74 | 72163700 | 12027283 | 77.50 | 104.87 | 35.316 | 1.089 | 3.058 | 3.246 | 8.432 |
| IV/74 | 72565900 | 12094316 | 85.42 | 121.49 | 42.227 | 1.132 | 3.216 | 3.409 | 10.580 |
| I/75 | 72686500 | 12114416 | 72.48 | 104.34 | 43.957 | 1.053 | 3.043 | 3.221 | 9.893 |

6B - Type of Crime: Larceny--Attempted

| Data Quarter | Households | | Victimization Rate | | | Standard Errors | | | z of Difference |
|---|---|---|---|---|---|---|---|---|---|
| | Bounded | Unbounded | Bounded | Unbounded | % Difference | Bounded | Unbounded | Difference | |
| I/74 | 71118300 | 11853050 | 5.17 | 7.62 | 47.389 | .316 | .862 | .918 | 2.670 |
| II/74 | 71489200 | 11914866 | 4.56 | 6.71 | 47.149 | .295 | .817 | .869 | 2.475 |
| III/74 | 72163700 | 12027283 | 5.84 | 8.57 | 46.747 | .325 | .900 | .957 | 2.853 |
| IV/74 | 72565900 | 12094316 | 5.36 | 8.25 | 53.918 | .313 | .884 | .938 | 3.082 |
| I/75 | 72686500 | 12114416 | 5.12 | 7.96 | 55.469 | .307 | .870 | .923 | 3.078 |

TABLE 7: Comparison of Bounded and Unbounded Property Victimization Rates by Number of Persons in Household (Rates per 1000 households)

7A - Number of Persons in Household: 1 Person

| Data Quarter | Households | | Victimization Rate | | | Standard Errors | | | z of Difference |
|---|---|---|---|---|---|---|---|---|---|
| | Bounded | Unbounded | Bounded | Unbounded | % Difference | Bounded | Unbounded | Difference | |
| I/74 | 14402200 | 2400366 | 58.87 | 64.93 | 10.294 | 2.119 | 6.003 | 6.366 | .952 |
| II/74 | 14537300 | 2422883 | 62.20 | 76.44 | 22.894 | 2.166 | 6.397 | 6.754 | 2.108 |
| III/74 | 14818900 | 2469816 | 66.58 | 82.49 | 23.896 | 2.215 | 6.515 | 6.881 | 2.312 |
| IV/74 | 14924200 | 2487366 | 69.15 | 83.62 | 20.926 | 2.248 | 6.516 | 6.893 | 2.099 |
| I/75 | 14939600 | 2489933 | 58.37 | 72.01 | 23.368 | 2.061 | 6.099 | 6.438 | 2.119 |

7B - Number of Persons in Household: 2 to 3 Persons

| Data Quarter | Households | | Victimization Rate | | | Standard Errors | | | z of Difference |
|---|---|---|---|---|---|---|---|---|---|
| | Bounded | Unbounded | Bounded | Unbounded | % Difference | Bounded | Unbounded | Difference | |
| I/74 | 34497900 | 5749650 | 89.09 | 121.93 | 36.862 | 1.621 | 4.783 | 5.050 | 6.503 |
| II/74 | 34711200 | 5785200 | 91.72 | 131.01 | 42.837 | 1.637 | 4.916 | 5.182 | 7.582 |
| III/74 | 35137300 | 5856216 | 103.25 | 133.00 | 28.814 | 1.720 | 4.927 | 5.218 | 5.701 |
| IV/74 | 35417700 | 5902950 | 104.40 | 134.25 | 28.592 | 1.723 | 4.933 | 5.225 | 5.713 |
| I/75 | 35436800 | 5906133 | 87.89 | 125.66 | 42.974 | 1.588 | 4.799 | 5.055 | 7.472 |

7C - Number of Persons in Household: 4 to 5 Persons

| Data Quarter | Households | | Victimization Rate | | | Standard Errors | | | z of Difference |
|---|---|---|---|---|---|---|---|---|---|
| | Bounded | Unbounded | Bounded | Unbounded | % Difference | Bounded | Unbounded | Difference | |
| I/74 | 16814600 | 2802433 | 140.65 | 186.12 | 32.328 | 2.845 | 8.067 | 8.555 | 5.315 |
| II/74 | 16890500 | 2815083 | 138.87 | 200.83 | 44.617 | 2.825 | 8.289 | 8.757 | 7.075 |
| III/74 | 16939100 | 2823183 | 156.23 | 217.86 | 39.448 | 2.945 | 8.557 | 9.049 | 6.811 |
| IV/74 | 16974400 | 2829066 | 163.47 | 268.20 | 64.067 | 2.992 | 9.271 | 9.742 | 10.750 |
| I/75 | 17052000 | 2842000 | 141.86 | 219.25 | 54.554 | 2.830 | 8.504 | 8.963 | 8.635 |

7D - Number of Persons in Household: 6 or More Persons

| Data Quarter | Households | | Victimization Rate | | | Standard Errors | | | z of Difference |
|---|---|---|---|---|---|---|---|---|---|
| | Bounded | Unbounded | Bounded | Unbounded | % Difference | Bounded | Unbounded | Difference | |
| I/74 | 5386200 | 897700 | 189.88 | 282.68 | 48.873 | 5.890 | 16.658 | 17.669 | 5.252 |
| II/74 | 5336300 | 889383 | 188.39 | 303.44 | 61.070 | 5.886 | 17.084 | 18.070 | 6.367 |
| III/74 | 5262600 | 877100 | 197.13 | 320.15 | 62.406 | 6.029 | 17.512 | 18.520 | 6.642 |
| IV/74 | 5242700 | 873783 | 226.72 | 324.76 | 43.243 | 6.422 | 17.631 | 18.764 | 5.225 |
| I/75 | 5245700 | 874283 | 202.24 | 281.56 | 39.221 | 6.103 | 16.928 | 17.995 | 4.408 |

TABLE 8: Comparison of Total Property Victimizations Not reported to Police for Bounded and Unbounded Samples

| Data Quarter | Number of Victimizations | | Percent Victimizations Not Reported to Police | | | Standard Errors | | | z of Difference |
|---|---|---|---|---|---|---|---|---|---|
| | Bounded | Unbounded | Bounded | Unbounded | Difference | Bounded | Unbounded | Difference | |
| I/74 | 7309020 | 9870360 | 70.672 | 72.575 | 1.903 | .6000 | .4778 | .7671 | 2.481 |
| II/74 | 7441220 | 10767050 | 66.954 | 68.769 | 1.815 | .6128 | .4779 | .7771 | 2.335 |
| III/74 | 8298200 | 11316020 | 65.504 | 65.132 | -0.372 | .5824 | .4857 | .7583 | -0.491 |
| IV/74 | 8693250 | 12271730 | 68.884 | 71.546 | 2.662 | .5481 | .4394 | .7024 | 3.790 |
| I/75 | 7468410 | 10728060 | 67.343 | 71.339 | 3.996 | .6097 | .4661 | .7674 | 5.207 |