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The data presented in this paper derive from the central cities sample of the LEAA/Census National Crime Panel victimization surveys. More specifically, the results presented herein are from interviews conducted with a probability sample of 165,000 persons in Atlanta, Baltimore, Cleveland, Dallas, Denver, Newark, St. Louis, and Portland, (Ore.).<sup>1</sup>

For purposes of this paper, three broad categories of personal victimization will be considered. The first is assaultive violence without theft; it includes crimes such as simple and aggravated assault and attempted and completed rape, in which no theft or attempted theft was involved. The second is assaultive violence with theft which includes crimes such as robbery and assault or rape in which a theft was completed or attempted. The third broad category of personal victimizations is personal larceny without injury which includes pocket picking and purse snatching without force or threat of force directed at the victim.

The survey results from these eight cities suggest that rates of victimization are closely linked to the characteristics of victims -- such as age, sex, marital status, family income, and race.<sup>2</sup>

One way of viewing the recent victimization survey results is to ask the extent to which they are congruent with earlier victimization survey results, studies using police statistics on reported crime, and current criminological theory. Overall, it can be said that the results for these variables are generally consistent with prior research.<sup>3</sup> The major exception to this generalization is the finding that whites have a rate of assaultive violence without theft victimization that is half again greater than that for black/others.<sup>4</sup> This finding is at odds with an impressive constellation of research using police data (Mulvihill et al., 1969:209; Pittman and Handy, 1964:468; Reiss, 1967:34; President's Commission on Crime in the District of Columbia, 1966:78; Pokorny, 1965:495, Amir, 1971:40; MacDonald, 1971:76; Zimring, 1971; Table 1) which strongly suggests that rates of victimization for black/others are far in excess of those for whites in assault as well as rape.<sup>5</sup>

Because of the magnitude of the discrepancy between these victim survey results and prior research regarding racial differences in the rate of assaults not involving theft, the purposes of this paper will be to examine various possible explanations of this anomalous finding. Problems in the Measurement of Assault

In the course of developing the instruments and procedures to be used in the National Crime Panel, LEAA, in conjunction with the Bureau of the Census, undertook a series of reverse record checks designed to ascertain the extent to which known crime victims selected from police files would report to interviewers that they had been victimized within the reference period. In three separate reverse record check studies conducted in Washington, D.C., Baltimore, Md., and San Jose, Calif., assault victims consistently had the poorest "recall" rate -- i.e., the smallest proportion of known victims who reported to the interviewers that they had been victims of a crime during the reference period. For example, across all three cities, while 88 percent of the burglary victims, 80 percent of the robbery victims, and 67 percent of the rape victims reported the crime selected from police files to the interviewers, only 47 percent of the assault victims did so.<sup>6</sup>

In addition, these reverse record checks suggested that when the offender was known to the victim -- especially when the offender was related to the victim -- the rate of "recall" was smaller than when the offender was a stranger. In assaults, more than in other personal crimes, the offender was known to the victim. How might these data be related to our incongruous finding regarding race and assaultive violence without theft victimizations? Perhaps one source of bias is that black/other victims of assaultive violence without theft are more often victimized by persons known to them. Our data are consistent with this hypothesis: 25 percent of the white males and 35 percent of the black/other males who were victims of assaultive violence without theft were attacked by persons known to them; for white and black/other females the respective percentages are 34 percent and 47 percent. Because both male and female black/others appear to be assaulted by known offenders more often than do whites -- and because reverse record checks have shown that victims of known offenders (especially related offenders) are less likely than victims of stranger offenders to "recall" victimizations to survey interviewers -- this systematic bias may help to account for our results.

Another possible explanation for this finding is that the assaultive violence without theft victimizations reported by whites and black/ others are qualitatively different. The category "assaultive violence without theft" contains numerous subcategories: rape, attempted rape, serious assault, attempted assault with a weapon, attempted assault without a weapon, and minor assault. For the least serious crime in this category, attempted assault, the rate for whites was more than twice that of black/others (15 vs. 7). For a more serious victimization, attempted assault with a weapon, whites had a rate that was about ten percent higher than that of black/ others (97 vs. 87). However, for the most serious victimizations, black/others had either higher rates of victimization than did whites (as in rape 4 vs. 2) or rates of the same magnitude (as in serious assault: 5 vs. 5).

The fact that such sharp differences occur when assaultive violence without theft victimizations are differentiated by seriousness may be indicative of a measurement problem in connection with assaultive crimes which do not involve theft. The dramatic shift in racial differences when rates of serious assaultive violence without theft are considered may reflect; in part, differential reporting by race to survey interviewers. Such differences could spring from a variety of sources, including differential memory effects.

The NCP central cities samples use a rather lengthy reference period -- one year. The length of this reference period suggests that the smaller rate of less serious victimizations reported by black/others to the survey interviewers may be a product of differential recall of less serious events, with black/others "forgetting" -- or, at least, failing to report to interviewers -- greater proportions of minor events. A number of possible hypotheses could be advanced as explanations for such differential forgetting. For example, for persons suffering a large number of serious victimizations, these less serious events may tend to lose their saliency to the victims, with the result that only the serious events are recalled. Hence, were black/others to suffer more serious victimizations relative to whites, the disproportionately low proportions of less serious events reported by black/ others noted above, could be a function of this loss of saliency for minor events. Similarly, perhaps, because they may experience fewer serious victimizations, whites may tend to telescope events into the reference period to a greater extent than do black/others. Were the effect large enough, either situation -- black/others forgetting events or whites telescoping events forward into the reference period -- could account for the finding that in these eight cities assaultive victimizations reported by blacks are disproportionately serious.

Prior research has indicated that both forgetting and telescoping may have dramatic effects on victimization estimates. For example, the work undertaken by the Bureau of Social Science Research (Bidermann, et al., 1967) indicated that respondents reported many more recent than temporally distant victimizations. Additionally, the BSSR researchers recognized the phenomena of forward telescoping, which results in respondents' bringing into the reference period victimizations that actually occurred prior to the reference period.

An informative analysis of the relative magnitudes of the telescoping and forgetting effects was carried out by researchers from the National Opinion Research Center (NORC) under the sponsorship of the President's Crime Commission (Ennis, 1967: Figure 2). They reasoned that if there were no seasonal variation in the occurrence of crime, then the crimes occurring in a twelvemonth period would be spread evenly among the four quarters of the year. Thus, if there were no seasonal variation in the occurrence of crime measured by the survey, and if there were no telescoping or memory effects, then 25 percent of the yearly crimes would be reported in each quarter of the year. Telescoping into the reference period would presumably have the greatest effect on the earliest months of the recall period and the effect would diminish in later months. On the other hand, memory decay is likely to be more serious for the earliest months and less of a problem in later months. The NORC study found that 22% of the victimizations were reported to have occurred in the first quarter, 15% in the second quarter, 23% in the

third quarter, and 40 percent in the fourth quarter. These results suggest that forgetting may be more dominant, however, some evidence of teleis also apparent.<sup>7</sup> The assumption of no seasonal variation in the occurrence of crime during the period studied was strengthened by the NORC finding that the Uniform Crime Reports' seasonal variation for the period covered by the survey was slight. The UCR, which are based primarily on crimes reported to the police, can be expected not to be greatly influenced by either telescoping or memory decay.<sup>8</sup>

In order to examine the hypotheses that differential telescoping or memory decay produced the racial differences in the proportions of minor victimizations reported in the survey, victimizations were examined according to the months in which they were reported to have occurred.<sup>9</sup> However, our analysis of the relative contributions of telescoping and forgetting is complicated by two factors: 1) the exact month of the interview was not available: although the date of the interview was recorded by the interviewer, this information was not keyed onto the machine-readable data record, and 2) the interviews were spread out over a period of slightly more than two months. Thus, although all respondents had a one year reference period, it is impossible to know exactly the reference period for any specific respondent. However, about 95 percent of the interviewing took place during the months of September and October, 1972. Persons interviewed in September fell into the reference period from September 1, 1971 through August 31, 1972, while those interviewed in October had a reference period from October 1, 1971 through September 30, 1972. Because all interviews were not completed in a single month, simply grouping victimizations according to the month in which they were reported to have occurred would confound different reference periods. For example, for those persons interviewed during September, the month of September as the time of the victimization refers to the earliest part of the reference period -- one year back -while for those interviewed in October the month of September refers to the latest part of the reference period -- one month back. Due to this ambiguity, it is necessary to exclude from consideration those victimizations reported to have occurred in the months of September and October. When only those victimizations reported to have occurred from November through August are considered, it is known that those occurring in November were in the earliest part of the reference period and those occurring in August were in the latest part of the reference period, for all respondents.<sup>10</sup> Using this approach, the monthly categories refer to two-month sliding intervals. That is, the month of August refers to one month back (from the interview) for those interviewed in September and two months back for those interviewed in October. Clearly, this procedure represents a crude approximation, but it is the best available until the month of interview is keyed onto the data records.

The NORC analysis relied on the assumption that there was no marked seasonal variation in crime rates. Our analysis relies upon two other assumptions: There are no seasonal diffences in crime rates between black/others and whites, and there are no seasonal differences related to the seriousness of the victimization. Any differences, including those between racial groups and among seriousness categories, in the proportion of victimizations reported to have occurred each month will be assumed to be a function of the method rather than a function of actual crime rate differences by month.

The distribution of all personal crimes reported in the survey for the ten-month period was examined by race of respondent and month of reported occurrence. Although there is substantial deviation between these curves and that which would be expected if crimes were evenly spread over the ten-month period, the similarity between the black and white curves is striking. Among white victims, the curve peaks in July (two and three months back) where 15.4 percent of all personal victimizations were reported to have occurred, and is least in December (nine and ten months back) where only 6.5 percent were reported to have occurred. Among blacks, the greatest proportion of victimizations (15.7 percent) was reported to have occurred in June (three and four months back), while the smallest proportion (5.7 percent) were reported to have occurred in January (8 and 9 months back).<sup>11</sup>

The similarity between the curves for black/ others and whites casts some doubt on the differential forgetting and telescoping hypotheses. While the peak and trough months are slightly different for blacks and whites, the overall similarity of the curves is consistent with the hypothesis that memory decay and telescoping effects are similar for whites and black/others.

While these curves for all personal victimizations considered simutaneously are informative, this aggregation may mask important differences between the races for victimizations that vary in seriousness. Because black/others report a greater proportion of serious victimizations than do whites, and since the seriousness of the victimization may affect memory decay and/or telescoping, it is essential to examine the distribution of victimizations throughout the reference period, while controlling for the seriousness of the victimization.

Sellin and Wolfgang (1964) have developed a scaling technique which may be used to assess the seriousness of victimizations. The weights used in their scheme were developed from subjective assessments of the seriousness of various elements of victimizations. The elements taken into account in their system include the nature and extent of physical injury and property loss, as well as weapon use. Figure 1 presents the distribution of personal crimes reported in the survey for the ten month period by race of respondent and month of reported occurrence, for various seriousness levels. The Sellin-Wolfgang seriousness scores, which have a possible range from one to 26, have been categorized into four seriousness level groups. The least serious victimizations (seriousness scores 1-3: low seriousness) constitute 28 percent of all victimizations reported to survey interviewers. The next two categories of seriousness, 4-5 and 6-7, respectively contain 31 percent and 33 percent of the victimizations. The most serious victimizations (seriousness scores of 8 or more: high seriousness)constitute 8 percent of all victimizations reported to the interviewers.

If the hypotheses that black/others were forgetting a disproportionate number of less serious victimizations relative to whites were true, it would be expected that the curves representing the less serious victimizations (Figure 1A) would show a much steeper slope towards the latter part of the reference period for black/others than for whites, while the curves representing more serious victimizations (Figure 1B) would not. Similarly, if the hypothesis that whites were disproportionately telescoping victimizations into the reference period were true, it would be expected to be reflected in higher proportions of victimizations for whites, than for black/others, occurring early in the reference period.

Examination of the figures reveals that neither of these hypotheses are borne out. While there is some variation in both the black/other and white curves as seriousness category varies, as was found for total personal victimizations, there is remarkable similarity between the black/ other and white curves for each category of seriousness.<sup>12</sup>

Perhaps the most important observation from Figure 1 is that for neither racial group does there appear to be a dramatic seriousness effect. That is, the curves at each seriousness score level are strikingly similar. <u>A priori</u>, it seems reasonable to have expected that very few of the most serious victimizations would have been forgotten, although some might have been expected to be telescoped forward in the reference periods. This would result in a relatively flat curve with a bit of an up-turn at the beginning of the reference period. The expectation for the least serious victimizations might have been that many of the victimizations occurring early in the reference period would have been forgotten, resulting in a curve with a steep positive slope. Figure 1 does not demonstrate such dramatic differences by seriousness level.

The results discussed so far have examined differential telescoping and forgetting by seriousness, where seriousness has been operationally defined by the Sellin-Wolfgang scores.<sup>13</sup> Under this scheme, victimizations are categorized according to objective elements of the crime. It may be, however, that telescoping and forgetting would be better addressed by using a measure of the victim's own subjective assessment of seriousness. Such a measure is, of course, difficult to construct. However, there is substantial rationale for considering those victimizations which are reported to the police as subjectively more serious to the victim than are those not reported to the police. Such an assumption finds support in the reasons given by victims who fail to report victimizations to the police -- the reasons "nothing could be done" and "victimization not important enough" were found in this data set to be the reasons most commonly given for not reporting the victimization to the police.<sup>14</sup> Clearly, a number of factors -- in addition to subjective seriousness -- are involved.<sup>15</sup> However, it may be assumed that, in general, the more greviously injured the victim felt himself to be as a result of the crime, the more likely he would be to report it to the authorities. The importance of examining some measure of personally-defined seriousness is, perhaps, most obviously demonstrated by differential subjective impact of theft offenses -- a hundred dollar loss may be much more "serious" to a lower income individual than to a higher income individual.

Figure 2 presents the curves by month of reported occurrence, for all personal victimization by race of victim and whether or not the crime was reported to the police (our indicant of subjective seriousness). The similarity among these curves, not only along the reported/not reported dimension but also between races, is striking. The curves most specifically germane to the hypotheses under question -- that black/ others are forgetting a greater number of less serious victimizations and that whites are telescoping a greater proportion of less serious events into the reference period -- are the curves representing victimizations not reported to the police (subjectively less serious). Contrary to our hypotheses, the month of occurrence curves are very similar for whites and blacks. The black/other curve peaks in June where 16.2 percent were reported to have occurred, whereas the white curve peaks in July were 15.6 percent were reported to have occurred. The rate for black/ others is least in January where 4.8 percent were reported to have occurred while for whites it is least in December with 5.9 percent. Contrary to the second hypothesis, there is no large telescoping effect in evidence for whites, relative to black/others, for victimizations not reported to the police and hence taken as subjectively perceived as less serious.

It must be concluded from this analysis, then, that neither differential memory decay nor differential telescoping by race seems to be able to account for the finding that black/others -- in comparison to whites -- report a disproportionately small number of less serious victimizations to the survey interviewers.

The data from the eight cities victimization surveys are generally consistent with prior criminological research. The anomalous finding that black/others, in relation to whites, reported a disproportionately small number of less serious assaultive violence without theft victimizations, was investigated in order to ascertain whether this finding could be accounted for by telescoping or forgetting. To the extent that these phenomena can be examined by studying curves for reported month of occurrence, our analyses suggest that the race finding is question is not attributable to either telescoping or forgetting.

Of more general importance, however, is the finding that the month of occurrence curves appear to be unrelated to characteristics of the victims<sup>16</sup> and to objective and subjective indicators of the seriousness of the victimization. Had either characteristics of the victim or the nature of the victimization event been found to be related to the month of occurrence curves this would have suggested that victimization surveying may be a differentially valid method of estimating the nature and the extent of criminal victimization -- a finding that would have re-

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duced the utility of victim survey results.<sup>17</sup>

The National Crime Panel central cities samples have provided a good deal of extensive and systematic data about victims of personal crime. These data can serve as a welcome complement to existing data sources about victims of criminal behavior. Careful review of the difference and similarities between victim survey findings and other empirical research, and the ways in which these findings impinge on theory, can greatly enhance our understanding of criminal victimization. As this paper has attempted to show, such reviews must attend carefully to the method by which statistics about crime are generated, and where data from two or more methods are disparate, consideration must be given to method-linked biases that may account for the inconsistencies. It is only after intensive study of the methodological adequacy of the procedures that some degree of confidence may be placed in the substantive results. The National Crime Panel method of generating data is significant in the field of criminal statistics for its history of valuable methods development and the constant impetus for improvement. However, as this innovative method generates findings over the next several years they must be systematically examined for congruence with results generated by other methods. Lack of congruence between data sets should serve as an impetus for continued assessments of the methods and methodological innovations required for continued improvements in victimization surveying techniques.

## Footnotes

<sup>1</sup>For a detailed discussion of the sampling procedures and interview techniques, see Hindelang, M. (1975). <u>Criminal Victimization in Eight</u> <u>American Cities: A Descriptive Analysis of Com-</u> mon Theft and Assault.

<sup>2</sup>See Hindelang supra note 1.

<sup>3</sup>The finding that age is inversely related to victimization is congruent with studies of assault (e.g., Mulvihill, Tumin, and Curtis, 1969: 211; Pittman and Handy, 1964: Table 3.13; President's Commission on Crime in the District of Columbia, 1966:7; Pokorny, 1965:493; Johnson, Kerper, Hayes, and Killinger, 1973:38), rape (e.g., MacDonald, 1971:77; Amir, 1971:52; Mulvihill et al., 1969:445; President's Commission on Crime in the District of Columbia, 1966:49) and robbery (e.g., Conklin, 1972:90; Mulvihill, et al., 1969:214; President's Commission on Crime in the District of Columbia, 1966:59). The victim survey data reported by Ennis (1967:34-35) show that rates of personal victimization by homicide, forcible rape, robbery, and aggravated assault for those 20 years of age or older are inversely related to age. That males are more often victimized than females is supported by studies which examined both armed and unarmed robbery (e.g., Reiss, 1967:34; Mulvihill et al., 1969:280, 284), robbery studies in which the type of force was not differentiated (e.g., Normandeau, 1968:150; President's Commission on Crime in the District of Columbia, 1966:59-65) and studies of assault (e.g., Mulvihill et al., 1969:78; President's Commission on Crime in the District of Columbia, 1966:272; Johnson et al., 1973:40; Porkorny, 1965:496). Ennis (1967) found, overall, rates

of victimization were inversely related to income.

<sup>4</sup>Since races other than black and white constitute too small a proportion of the total population in the Impact Cities to permit separate analyses, blacks and "other" races will be combined for analytical purposes and will be referred to as black/others, for ease in communication.

<sup>5</sup>Some influential criminological theory has been grounded in these strong racial differences for assaultive crimes, e.g., Wolfgang and Ferracuti, The Subculture of Violence (1967).

<sup>6</sup>These percentages are based only on those victims who were interviewed; those who could not be located or who refused to be interviewed have been excluded from these figures.

<sup>7</sup>Note that these results could also be explained by within reference period forward telescoping. Evidence available from the San Jose reverse record check (LEAA, 1972, Table 4) shows, however, that the within reference period net telescoping effects (i.e., the number of within reference period events that are telescoped forward minus the number of within reference period events that are telescoped backward) are negligible.

<sup>8</sup>Several empirical analyses of telescoping and memory decay have been performed using the reverse record check technique, each of which have found indications of considerable memory decay and telescoping. See, Law Enforcement Assistance Administration, U.S. Department of Justice, National Institute of Law Enforcement and Criminal Justice, Statistics Division. San Jose Methods Test of Known Crime Victims, 1972; U.S. Bureau of the Census. "Victim Recall Pretest (Washington, D.C.): Household Survey of Victims of Crime." Mimeo. 1970; and U.S. Bureau of the Census. "Household Survey of Victims of Crime: Second Pretest (Baltimore, Maryland)." Mimeo. 1970.

 $9_{A11}$  victims were asked to report the month in which the incident took place, if they were unsure, they were asked to give their best estimate.

10In the eight cities, 95 percent of the interviews were conducted during September and October.

11Due to space limitations, these curves are not presented here. However, they can be derived from Figure 1.

<sup>12</sup>Analysis performed on the cities separately while not shown here, are consistent with the data described herein. However, when seriousness and race are controlled, the small number of cases which results from viewing each city separately produces some fluctuation in the findings:\_

ings. <sup>13</sup>Although the relevant data are not shown here, this analysis has been replicated using other crime classification techniques. For example, the Uniform Crime Reporting Categories of rape, robbery, aggravated assault, simple assault, and personal larceny were also used. The results in terms of the hypotheses under study here were virtually identical. In particular, these very different victimizations showed very similar month-of-occurrence distributions.

<sup>14</sup>See Hindelang, M. and M. Gottfredson, (1976) "The Victim's Decision Not To Invoke The Criminal Justice Process." <sup>15</sup> In some cases a co-victim who believed the event to be serious enough to call the police, may have reported it to authorities. Alternatively, a family member other than the victim may have called the police. For a discussion of factors associated with reporting victimizations to the police, see source in note 16 supra.

lice, see source in note 16 supra. <sup>16</sup>Although only victim's race was examined with the data reported here, other analyses show that sex, age, income, and education of the victim are similarly unrelated to the month of occurrence distribution. Additionally, this analysis was repeated using data from the five largest American cities -- New York, Detroit, Los Angeles, Philadelphia, and Chicago -- with similar results. <sup>17</sup>If our assumption of no seasonal effects

<sup>17</sup>If our assumption of no seasonal effects is warranted, the results presented here suggest that substantial telescoping and/or memory decay does occur. However, because neither the characteristics of the victims nor the characteristics of the victimizations appear to be systematically associated with these phenomena, they are, <u>ipso facto</u>, less problematic.

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FIGURE IN MONTH OF REPORTED OCCURRENCE BY RACE OF VICTIN AND SERIOUSNESS LEVELS LEVELS LON AND MEDIUM LON



