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Introduction

The growing use of sample surveys to measure the volume and distribution of crime in the United States will provide social scientists and public administrators with valuable new data with which to test their theories and plan crime-reduction programs. In particular, the National Crime panel and city-level samples currently being monitored by the Bureau of the Census should produce a rich body of information on aspects of criminal and victim behavior which previously have escaped systematic analysis. A host of research problems for which current statistics are unsuitable may be confronted with this emerging data base.

The most immediate use of survey estimates of crime rates, however, has been to compare them to official statistics. Reports released by the Law Enforcement Assistance Administration have stirred public interest by their contrast with police figures on crime of the type summarized in the F.B.I.'s yearly Uniform Crime Report. 1 Such comparisons inevitably reveal wide gaps between rates registered by the two sources. National or city-level survey-generated figures usually overshadow official police statistics by a substantial margin. This type of analysis has been encouraged by the government's decision to calculate U.C.R.compatible figures from citizen surveys, although this is perhaps the least useful application of the data. The observation that there are varying discrepancies between official and survey crime estimates does not tell us where the error lies. Every statistic is shaped by the process which operationally defines it, the procedures which capture it, and the organization which processes and interprets it. Survey and police crimemeasurement procedures produce different figures, but the reasons for this and its implication require analysis. A discussion of how survey and official crime statistics differ and why we obtain these discrepancies may clarify both their comparability and their individual interpretation, and it may speak to their improvement in the future.

Measurement Error and Official Crime Statistics

The presence of error terms of considerable magnitude is not unique to measures of crime, although a half-century of continuous criticism has focused more widespread attention upon the errorful nature of crime measures than enjoyed by most social statistics. Measurement is the process of mapping an empirical system into a numerical system. It involves the application of definitions to delineate aspects of the empirical system which are of interest, and a series of "If...Then..." rules matching selected attributes of those phenomena to numbers. The resulting figures always map the richness of the referent system simplistically and inexactly.

In measurement terms, all observed scores are composed of two elements: they are partially "true score" (reflecting what we wish to observe) and partially error. Even rapidly repeated, identical measurements of the same phenomenon will produce different numerical readings. The degree to which they are similar -- our ability to reproduce our findings -- is referred to as the "reliability" of a measurement process. Reliability tests, for example, would gauge the ability of various police patrol teams to classify the same set of events in the same manner. While the ability to examine events twice and find the same thing is the sine qua non of good measurement, even reliable measures may not be useful. A researcher's procedures may not be measuring the object of interest, or the resulting figures may be artifacts of the measurement process. This is a validity problem. Police districts with ambitious commanders may consistently produce low crime totals. In order to obtain valid, nonartifactual measures we must employ multiple and differing techniques, cross-checking our findings at every turn."

Disciplines with well-developed measurement traditions have evolved routine procedures for coping with these problems. Economists have stressed reliability; they require measures which are stable and comparable across time.³ Psychologists emphasize validity. The intangibility of the psychological domain heightens concern that its apparent orderliness may be an artifact of specific methods of investigation. Sophisticated psychological measurement combines the fruits of interviews, projective evaluations, and physical observations.⁴

The measurement of crime is a substantive and methodological problem of interest to researchers in a variety of disciplines. Perhaps as a result, most of the effort expended upon measurement problems has been conducted outside of any coherent measurement model. Scattered validation studies of official statistics have been reported. Price compared state-level property-crime totals with insurance rates and uncovered only moderate cor-relations.⁵ But such criterion validation requires a dependent measure which is relatively error-free, and in this case crimes known to the police are probably a better indicator of the underlying true distribution of events than the independent validator. A better example of criterion validation is the California Criminal Statistics Bureau's comparison of police and American Bankers Association's figures on bank robbery. The latter measure appears to be clearly defined and exhaustively enumerated, and it proved to be reflected quite accurately in official statistics.6

Validity studies of official measures of more typical events, those which are less clear-cut and involve more discretion on the part of police officers and administrators, have been less hopeful. Comparisons between official records and self-reports of delinquency or informal police "contact" reports indicate that official figures greatly underestimate the volume of events which might be uncovered in other ways.⁷

The development of our current system of gathering and publishing official statistics on crime was a response to these problems. The invalidity of local department's efforts at data collection and the limited reliability of the reported figures led to the development of the Uniform Crime Reporting system in the late 1920's. This system improved reliability and sacrificed validity. Standardized definitions, data-collection forms, and data-gathering techniques produced city-level crime totals which were usually comparable from year to year, and inter-city comparisons undoubtedly are vastly improved by the U.C.R. system. But several important compromises were made in the formulation of this statistical system. The data are still gathered by local authorities, participation in the network is not mandatory, and the F.B.I.'s only option in the face of fraud is not to publish the reported figures.⁸ As early as 1931 the Wickersham Commission called for the creation of a centralized data-collection service and rigorous data-quality control.⁹ The misreporting and underreporting apparently endemic in current official statistics has led to their widespread devaluation.

Survey Measures of Crime

Continuing dissatisfaction with official measures led to the development of alternative techniques to gauge the scope and distribution of crime in American society. The most important of these is the population survey, a measuring device (with its own characteristic reliability and validity problems) which has yielded striking new pictures of the crime problem.

The use of the sample survey to study crime reflects dissatisfaction both with the apparent accuracy of official figures and the paucity of information they purport to reveal. The yearly <u>Uniform Crime Report</u> does not speak to questions about the characteristics of victims of crime. Offender data is available only on arrestees, although victim testimony might shed some light on the characteristics of successful criminals. Finally, little data is reported on the physical and social circumstances under which most crimes occur, even though this has tremendous implications for their solution and deterrence.

It was apparent to the President's Crime Commission that population surveys potentially could speak to all of these inadequacies, and in the mid-1960's the Commission funded several pilot projects and a national sample survey to test their utility.¹⁰ Since then, the federal government has inaugurated a regular surveying program on a national scale and has funded several local and state-level investigations.¹¹

It was inevitable that the victim-based data

gathered by these large-scale surveys would be used to gauge police-reported crime statistics. Suspicion of official statistics has become widespread and appreciation of the errors in crime data particularly well-known, much more so than the enormous insecurity felt by researchers who regularly employ attitude measures and selfreports of behavior. The latter deal skeptically with data and demand elaborately scaled, multiple item indicators of concepts before they test theories with any confidence. The items in the Crime Panel surveys elicited a much larger volume of events than reported by police, so it is widely assumed that they are "more accurate" measures of the true volume of crime in society. But such gaps are inevitable. Despite the surface similarity of the resulting figures, the measurement operations and their errors differ greatly when we compare police and survey procedures for estimating crime rates. The social and organizational processes which stand between events occurring in the world and our survey or official maps of them produce quite different kinds of crime statistics.

Sources of Measurement Error

In the course of mapping crime events into a numerical system, both official and survey measurement procedures generate considerable error. If we think of error as the gap between a true score and an observed score for an event, Figure 1 may be a useful summary of what we know about its sources. On the survey side, measurement error has been investigated intensively in a

Figure 1 goes here

series of pilot studies which began in 1966. Our knowledge of error-generative processes on the police side is older, but has been considerably enhanced by studies of victim behavior and systematic observations of police work during the past decade.

Ironically, the first stages in the official measurement process lie largely in the hands of civilians: the victims of crime, their relatives, neighbors, and bystanders. The first public filter through which events must pass is perceptual: someone must know that a crime has taken place. This is in part an information problem. For example, a great deal of larceny from commercial establishments (shoplifting and employee theft) is discovered only in the form of inventory shrinkage.¹² In this case we know that crime is taking place, but criminal events remain unknown and uncountable. The general difficulty is that discreet events may escape detection, while continuous indicators of their occurrence --like dollar losses per quarter or shortages at audit--cannot be enumerated under our current system of social accounts. The problem is also conceptual: people must define an event as falling into the domain of events about which "something must be done." This appears to inhibit the reporting of much consumer fraud, and it is the difference between crime and "ripping-off." Attitudinal studies of the ligitimacy of theft or fraud upon large private and governmental

bureaucracies indicate that there is far from universal agreement about the labeling of some behaviors in our society. The problem of who does the perceiving is also of interest. Pilot surveys in Dayton and San Jose revealed that a surprising 25 percent of all personal crime and 20 percent of all property crime is reported by someone other than the victim.¹⁴ Sample survey, victim based studies of reporting and non-reporting are not designed to cope with this.

The decision to call the police has been the focus of considerable research, for it is probably the most important factor shaping official statistics on crime. In the Dayton-San Jose pilot surveys conducted in 1972 respondents recalled that about 60 percent of all robbery, 56 percent of all larceny, and 40 percent of all household burglary was not reported to the police. Their reasons for failing to do so were numerous: the largest categories chosen were "not serious enough" (25-30 percent), "nothing can be done" (25 percent) or that the harm or loss was slight (10 percent).¹⁵ Other analyses of the reporting problem have focused upon race, class, or even personality characteristics of victims rather than their manifest responses, although the utility of this approach is not particularly clear. It appears that the characteristics of the event are controlling: who did it (relative or stranger), why it was done (economics or passion), what was the damage to person, property, or propriety, and what were the participants' estimates of the burdens and benefits of evoking the police. Only a portion of the latter calculation--that involving the victim's fear of the police-would appear to be a straightforward race-andclass problem. Despite much discussion of this factor, neither Ennis' national survey nor the Dayton and San Jose studies revealed more than 2 percent giving that response.¹⁶

Observational studies of police behavior indicate that even after the police are called the outcome of the crime-measurement process remains problematic. Crime recording becomes a social and organizational activity. Reiss and Black's descriptions of police-citizen encounters in Chicago, Boston, and Washington, D.C. indicate that extra-legal factors greatly influence the decision to write a formal report.¹⁹ The police are loath to file a report when the relational distance between the participants in a dispute is small, in part because they know that it is very unlikely that the case will be pursued in the courts. They tend to defer to the dispositional preferences of the complainant, who often mobilizes the police only to warn or threaten another party. Both complainants who are deferential to the police and higher-status victims are more likely to be successful in persuading the police to file a report. The police also act upon their own assessment of the complainant's culpability. Often responsibility for personal crimes or their outcomes may be apportioned among the parties, and police respond to the division of blame. Finally, in cases where juveniles are parties to a dispute the police tend to defer to the dispositional preferences of adults at the scene.

These observations suggest another reason why official statistics on crime should be lower than survey estimates. Unlike survey enumerations where the victim's claim ultimately must be recorded on his terms, police "measurement" takes place within the context of the event. Complainants are often surrounded by witnesses and bystanders who contribute their interpretations of events, and--surprisingly often--suspects themselves are present to offer countercharges and alternative explanations. The decision to file a formal report is almost "judicial" in the sense that an officer weighs claims and counter-claims before making a disposition in a case. Patrol officers quickly learn to be suspicious of the motives of complainants, for their authority is often evoked for private purposes.¹⁸ Claims of victimization are not taken on face value. As the Uniform Crime Report does not present predisposition case totals, but only "founded" complaints for each city, we have no idea of the dimensions of this process. Scattered reports of large departments on hand indicate that the effect of "unfounding" be considerable: approximately 25 percent of rapes, 13 percent of robberies, and 19 percent of gun assaults reported to the police were discounted in these cities. They probably would generate self-reports of victimization, but they did not enter our social accounts.

Technical considerations, including difficulties with the classification scheme employed in gathering official statistics, may introduce measurement errors on the police side as well. The uniform crime reporting system imposes a set of definitions which usually do not match the criminal-code pigeonholes into which the police must daily sort events. The definitions are also neither mutually exclusive nor exhaustive. The translation from local to national terminology appears to vary from jurisdiction to jurisdiction, enhanced by local differences in training and data quality control.¹⁹ Errors of this sort will shift over time within cities as well. The tremendous variation and apparently random distribution of "manslaughter by negligence" totals reported in the Uniform Crime Report, for example, appears to be a function largely of variations in local practice.²⁰ Survey studies of crime, on the other hand, utilize measurement operations which may vary considerably across cities. Because these error terms differ, further "gaps" will appear between figures from the two sources.

The final source of error on the police side is organizational and political. The ability of official records systems to "retain" information once it has been entered is problematic. In 1966 a department audit of stationhouses in New York City revealed 20-90 percent underreporting of events in their files.²¹ These and other discoveries suggest that crime is an organizational problem in police departments. Especially in cities with a strong "stationhouse culture" or where district commanders are evaluated on their ability to reduce crime, we should observe a consistent tendency toward underreporting by police departments. Events also disappear individually in response to political influence or bribes, but this is less likely to skew totals in common crime categories.

The dramatic impact of variations in police record keeping procedures upon crime statistics is illustrated by "before-and-after" studies of cities which have overhauled their systems. Many of these were noted by researchers for the Crime Commission in their discussion of crime statistics.²² New York City's 1950 reorganization, for example, boosted that department's robbery totals by 400 percent, larceny 700 percent, and assault with a weapon 200 percent.²³ The Commission correctly perceived such overhauls as part of a more general phenomenon: the increasing professionalism of big-city police departments. A working hypothesis would be that as departments centralize their administration, automate their information systems, and encourage more legalistic behavior on the part of beat patrolmen, error in the official measurement of crime may be significantly reduced.

The sources of measurement error on the survey side have been investigated in a series of national and city-level pilot studies. In some, alternative techniques are employed in different random samples of a population and the results are compared. In others, police records are sampled to locate respondents who are known to have been victimized. They are interviewed and their recall patterns analyzed. Each method gives us a different check of the reliability and validity of survey measures of crime.

These investigations suggest that the first question we must ask is, "Will the victim be interviewed?" This raises both data collection and sampling problems. In early pilot studies a randomly selected adult was used an an informant for his entire household. Interviewers quizzed a single respondent about the victimization experiences of each family member. In the Dayton-San Jose surveys, a random half of the sample in each city was completely enumerated; interviewers questioned every household member over the age of thirteen to elicit self-reports of victimization. Apparently informant fatigue or lack of information about other household members is a substantial problem, for individual questioning elicited significantly more events. The differences were so marked that future government surveys will employ only complete household enumerations despite their increased cost.

Sampling deficiencies, on the other hand, have not been remedied. In the city-level studies conducted by the Bureau of the Census the sampling frame is bounded by the territorial limits of the central city. But an average of thirteen percent of the daytime population of the nation's core cities are commuters.²⁴ In Chicago, for example, over 400,000 workers leave the city at sundown. Tourists and other transients account for another fraction. Although they may be victimized and can report their experiences to the police, they are not eligible for interviewing under current procedures.

Even if they enter the sample, victims of crime may not successfully recall the event. As Albert Biderman has noted, one of the most striking findings of the victimization pretests was the relatively low salience of many crime events. "In practice, most respondents seemed to find it difficult to remember incidents of victimization other than recent cases.²⁵ The problem of memory fade has been investigated in two ways. First, known victims have been selected from police reports and interviewed. Their recall rates have climbed from 62 percent (Washington) to 74 percent (San Jose), reflecting successive improvements in the Census Bureau's questionnaire. Second, respondents have been required to recall known events within time frames ranging from three months to one year. These tests reveal a sharply decreasing recall rate for temporally distant events. The same phenomenon may be observed by plotting the date of occurrence of each event recalled by randomly selected respondents. Monthly crime rates estimated from survey responses drop sharply as an inverse function of time.²⁶ The effect is so striking that accurate survey measurements require brief recall periods. This means that very large samples are required to provide yearly crime estimates. The current compromise for the National Crime Panel is six months; respondents in the city studies are asked to recall events for an entire year. Police estimates, of course, are subject of few of these difficulties.

Reverse checks of police records also indicate that recall rates in an interview setting are sensitive to variations among the events themselves. They suggest that responses may not be forthcoming even if an event is recalled. Victims appear to be unwilling to report clashes with friends or relatives, for example. In San Jose, those who the police noted had been victimized by strangers recalled the event 75 percent of the time; only 22 percent of the cases where the police recorded that the offender was a relative were recalled, and 58 percent of those cases involving an acquaintance. In general, property crime was much more fully recalled than personal crime. Rapes were revealed only tentatively; in the San Jose pilot survey all recalled rapes were described as "attempted." It should be noted that these variations are similar to those which appear to affect the willingness of victims to relate their experiences to the police as well. Disputes within families and rapes are both highly underreported. And, as it was noted above, the police appear to be less willing to file formal reports when disputants are acquainted. In this case, survey and official measures both systematically undercount the same classes of events.

As noted in Figure 1, the final step in the survey measurement of crime involves the coding and classification of reported victimizations. It is difficult to judge how successfully this process reflects the event. In his report to the Crime Commission, Ennis related a modest test of the inter-coder reliability of his classification scheme. Teams of lawyers and detectives were successful in classifying citizen-reported victimizations in the same U.C.R. categories as his research staff about 65 percent of the time. In a validity test of the more advanced San Jose Survey instrument, Census personnel classified 259 of 292 recalled victimizations into the same categories as the local police who initially recorded them. Since we have no confidence that police and the interviewer were told exactly the same story, it is a remarkable correspondence. This, coupled with the face validity of the current survey instrument--the items are drawn to tap the dimensions which define Part I offenses in the <u>Uniform Crime Report</u>--suggests that the classification stage of the process is probably less troublesome than most.

A final and potentially important source of error in both survey and official measures is the intrusion of other events into the observed score for a city or household. On the police side, fraudulent claims may be registered. People may misuse the police in personal vendettas, they may invent crimes to disguise their own culpability, or they may attempt to register excessive insurance claims. In addition, actual events which lie outside the domain of interest may be misclassified as falling within it. The most serious problem on the survey side is telescoping. Method checks of all kinds indicate that the tendency of respondents to "telescope in" events which occurred outside of the reference period of the survey and to claim that they occurred within the specified interval is quite strong. For example, known victims telescope events which police files place firmly beyond the reference period. Experiments with the Census' Quarterly Household Survey panels indicate that bounded interviews may avoid distortions of this kind. Respondents who are asked to recall events which have occurred since an interviewer's last visit report as few as one-half the number of victimizations recalled by those who are quizzed about the same period but who have not been previously ques-tioned.²⁷ Given the low salience of most crim Given the low salience of most crime events and their steep forgetting curve, victims require signposts to guide their recall.

Estimating Error Magnitude

Like any measure, estimates of crime rates contain error. Given the magnitude of the sources of error discussed here, it is remarkable that official and survey measures of crime covary as closely as they do. The existence of these multiple measures enables us to estimate in rough fashion the magnitude of the error in each, and to generate some simple correction factors which may make them more useful. Additional methods, tests and analyses of existing data may contribute further to our understanding of the dimensions of error.

Crosschecks of recall errors in the survey measurement process indicate that the rate at which interviews "recover" events is fairly high. In the San Jose pilot survey of 1971, of the 394 known victims who were located for questioning, 292 recalled the event in some form. Table 1-A presents the recall rate for various sub-categories of events. Note that rates for frequent

_Table_1_goes_here____

crimes, larceny and burglary, were higher than those for less frequent events. Table 1-A also presents the total number of personal and household victimizations recalled by the residents of San Jose proper in the standard population survey phase of the pilot study. These are then projected into "corrected" totals which take into account patterns of non-recall. As the column totals indicate, the San Jose survey may have recovered approximately 75 percent of the five classes of events of interest. This is a very rough indicator of the recovery power of the victimization survey instrument, one that requires further refinement.

Table 1-B examines the respondent's contributions to errors in survey measures of crime. The forgetting curve plotted in Table 1-B indicates that recall periods exceeding three months may lead to the substantial undercounting of offenses in the population. A test of the ability of those recalling events to place them in the proper month --an essential check of the ability of surveys to provide time-series crime estimates of the type anticipated--indicates that recall accuracy degrades sharply after about three months as well.28 These curves, which were computed from data in the report of the San Jose pilot study, suggest that the six-month recall period used in the National Crime Panel and the twelve-month period bounding the city-level samples may contribute significantly to the error components of those measures. Because these curves were calculated from the same data used to estimate survey recovery rates in Table 1-A, it is impossible to untangle here the distinct contributions to error of the salience of events and their temporal distribution, however. The estimates of the magnitude of forward telescoping error presented in Table 1-B are based upon the Washington, D.C. pilot survey. There, seventeen percent of the victimizations recalled by selected respondents occurred before the indicated cut-off point when a six-month limit was specified, and 21 percent telescoped in events which occurred before a twelve-month limit. As I noted before, telescoping effects--which lead to an overcounting of events--can be controlled by "bounding" the recall period with a salient event. The National Crime Panel utilizes the previous visit of an interviewer, while city-level interviews must rely upon verbal instruction. The latter measures are much more likely to overestimate crime rates due to telescoping errors.

Error introduced in stages preliminary to the interview are more difficult to estimate. Sampling errors for individual cities are introduced by the systematic elimination of commuters, conventioners, and tourists from the sampling frame. The effect of this loss upon one crime statistic, motor vehicle theft, is very roughly estimated in Table 1-C, where motor vehicle statistics for the city of Chicago are presented. The recent victimization survey of Chicago estimated that residents there suffered about 38,700 vehicle thefts in 1972, or a loss probability of .03 per motor vehicle. Projecting commuter vehicle losses at only one half of the rate for city residents it appears that excluding commuters from estimates of city crime probably undercounts victimization by about 8 percent for this offense. Other crimes dealt with in the victimization surveys cannot be so easily projected. Commuters are susceptible to personal larceny, robbery, assault, and rape,

probably in that order; conventioners and tourists may be more at risk than commuters in all of these categories. This systematic elimination of potential victims of crime is redoubled when we consider the distribution of known victimizations. The victimization surveys indicate that many offenses, most notably assault and robbery, disproportionately victimize young males; they are also a demographic group which is most difficult to enumerate in a population survey. A summary estimate is that we undercount by 5 percent or more due to sampling limitations. Together, sources of error on the survey side of Figure 1 probably accumulate to undercount events by 30 percent.

Reversing the analysis enables us to probe the magnitude of error terms on the police side as well. The most satisfactory test would reverse the record-check procedures utilized in the pilot surveys: follow-up studies of offenses which were apparently reported to the police would be conducted to determine which could be found in police records.²⁹ None of the police departments which have extended their cooperation to researchers has granted access to their records on this scale. A simple analysis of the marginal frequencies of reported and officially recorded events in some of the sample cities suggests that the gap between the two sources would be considerable. Many events which occur and are said to have been turned over to the police do not appear to survive police processing.

Without further cooperation on the part of city police departments, errors in the official measurement of crime must be estimated from aggregate totals. Officials and survey estimates of city crime rates consistently differ. The ratio of robberies recalled in interviews to robberies known to the police in Portland--one of the smallest cities analyzed here--was greater than 3-to-1 in 1971-72; in New York City, more than 2½ robberies were recalled to interviewers for every event recorded by the police. Table 2 presents official and survey robbery estimates for five cities surveyed in 1972 as part of the Bureau of the Census' Large City study.

Table 2 goes here

The victimization surveys also asked each victim of a crime whether the event was reported to the police. This reporting rate can be used to "correct" survey estimates of the crime rate in each of the five cities for citizen-induced errors in police figures. While it is socially desirable to respond under questioning that one reported a crime, which will inflate this figure somewhat, it is clear from Table 2 that substantial distance remains between citizen "reported" and police recorded crime. The gap was smallest in Detroit, where police accounts of robbery added up to 73 percent of the "reported" (by recall) by city residents. Philadelphia's extremely low robbery count, which amounted to only 37 percent of what her residents claim to have reported to the police, may be related to numerous charges that police there cheat on their statistics.

The figures presented in Table 2 suggest that official "crimes known to the police" are probably not very accurate indicators of the true volume of crime in a community. In the case of robbery, official totals accounted for an average of only 38 percent of the victimizations recalled by citizens of these five communities. That figure varied considerably across cities: official robbery totals added up to 48 percent of the survey figure in Detroit, and only 22 percent in Philadelphia. If the interview recovery rate for individual and commercial robbery in these cities approximated the individual rate in San Jose (76 percent - see Table 1), official robbery totals might amount to an average of only 28 percent of the true total, and that figure might drop to only 17 percent in Philadelphia. The correction for error induced by citizen reporting practices improves this picture somewhat--official figures may reach 63 percent of "reported" robbery--but it appears that organizational processes contribute considerably to error in policerecorded crime statistics.

NOTES

1. The <u>New York Times</u> featured survey and U.C.R. incident totals in Census surveys.

2. See G.W. Bohrnstedt, "Reliability and Validity Assessment in Attitude Measurement," in G.F. Summers (ed.) <u>Attitude Measurement</u>. Chicago: Rand McNally, 1970, 80-99.

3. O. Morgenstern, <u>On the Accuracy of Economic</u> <u>Observations</u>. Princeton: Princeton Univ. Press, 1963 (2nd ed.)

4. S.W. Cook & C. Selltiz, "A Multiple-Indicator Approach to Attitude Measurement," <u>Psychological</u> <u>Bulletin</u>, 62(1964), 36-55; D.T. Campbell & D.W. Fiske, "Convergent and Discriminant Validation by the Multitrait-Multimethod Matrix," <u>Psychological</u> Bulletin, 56(1959), 81-105.

5. J.E. Price, "A Test of the Accuracy of Crime Statistics," <u>Social Problems</u> 14(Fall, 1966), 214-221.

6.Bank Robbery in California: A 35-Year Comparison of California with the Rest of the United States and an Intensive Study of 1965 Offenses. Sacramento: California Criminal Statistics Bureau, 1967.

7. See: W. Chambliss & R.H. Nagasawa, "On the Validity of Official Statistics," J. <u>Research in</u> <u>Crime and Delinquency</u>, 6 (1969),71-77; R. Quinney <u>The Social Reality of Crime</u>. Boston: Little Brown, 1970.

8. D.J. Pittman & W.F. Handy, "Uniform Crime Reporting: Suggested Improvements," in A.W. Gouldner & S.M. Miller (eds), <u>Applied Sociology</u>. N.Y.: The Free Press, 1965, 180-188; NIMH. <u>Criminal Statistics</u>. Rockville, Md.² Center for Studies of Crime and Delinquency, 1972.

9. U.S. National Commission on Law Observance and Enforcement. <u>Report on Criminal Statistics</u>. Washington: U.S. Gov't. Printing Office, 1931, 75. 10. These included: Washington, D.C.; Chicago, Washington, & Boston; and a national sample. All published by U.S. Gov't Printing Office, 1967.

11. Pilot studies included: "Washington Pretest;"
"Baltimore Pretest," 'San Jose Methods Test."
Full scale survey of two cities followed: "DaytonSan Jose Pilot Survey." Since then two reports of
multi-city studies: Chicago, New York, Philadelphia, Los Angeles, & Detroit; and Atlanta, Baltimore, Cleveland, Dallas, Denver, Newark, Portland,
St. Louis.

12. See: R.W. Dodge & A.G. Turner, "Methodological Foundations for Establishing a National Survey of Victimization," American Statistical Association, August, 1971.

13. E.O. Smigel & H.L. Ross, <u>Crimes Against Bur-</u> eaucracy. New York: Van Nostrand, 1970.

14. "Dayton-San Jose Pilot Survey," Table 23.

15. Ibid,, 24.

16. Ibid.; Ennis, op. cit., 44-45.

17. D.J. Black, "The Production of Crime Rates," <u>American Sociological Review</u>, 35 (August, 1970), 733-748; D.J. Black and A.J. Reiss, Jr., "Police Control of Juveniles," <u>American Sociological Review</u>, 35 (Feb. 1970), 63-77.

18. J. Rubenstein. <u>City Police</u>. N.Y.: Farrar, Strauss, Giroux, 1973, Chapter 5.

19. F.B.I. Uniform Crime Report, 1972, 56.

20. New York City, for example, has twice the population of Chicago and reports $3\frac{1}{2}$ times as much

crime, but Chicago in 1972 recorded 260 negligent manslaughter cases while New York recorded 66. In general, manslaughter rates do not correlate with anything.

21. M.E. Wolfgang, "Urban Crime," in J.Q. Wilson (ed.). <u>The Metropolitan Enigma</u>. Cambridge: Harvard Unviersity Press, 1968, 254.

22.President's Commission on Law Enforcement and Administration of Justice. Task Force Report: Crime and Its Impact--An Assessment. Washington: U.S. Gov't Printing Office, 1967, 22-24.

23. Wolfgang, op. cit.

24. J.D. Kasarda, "The Impact of Suburban Population Growth on Central City Service Functions," American Journal of Sociology, 77 (May, 1972), 1122.

25. Biderman, op. cit., 31.

26. Ennis, op. cit., 97.

27. A.G. Turner, "Methodological Issues in the Development of the National Crime Survey Panel: Partial Findings," National Criminal Justice Information and Statistics Service, Statistics Division, Law Enforcement Assistance Administration, December, 1972, 8-9..

28. Three months also appeared to be the optimal recall period in the Crime Commission's national survey. Ennis, <u>op</u>. <u>cit.</u>, 95-98.

29. The magnitude of this task is discussed in "San Jose Methods Test...," <u>op. cit</u>., 10.

Table 1

FIGURE 1



Estimates of Survey Measurement Error



Estimates of Official Measurement Error

City	Total ¹ Official Robbery	Total ² Survey Robbery	Survey ³ Measure of Reporting Rate	Estimated "Reported" Robbery	Official As Percentage of Survey "Reported"
Chicago	23531	64100	57.5	36881	64
Detroit	17170	36100	65.4	23638	73
Los Angeles	14241	36400	55.1	20064	71
New York City	78202	191400	59.5	113863	69
Philadelphia	9710	44000	58.9	25914	37
Average	28571	74400	59.3	44072	63

SOURCE: ¹Uniform Crime Report, 1972.

2"Crime in the Nation's Five Largest Cities," Washington: National Criminal Justice Information and Statistics Service, Law Enforcement Assistance Administration, April, 1974, Table 1. This includes both individual and commercial offenses.

³<u>Ibid.</u>, recomputed from Table 8. This rate includes both individual and commercial reports.

Table 1-C Estimates of Survey Measurement Error (continued)

1-C: Sampling Frame Loss Estimate: Motor Vehicle Theft in Chicago, Illinois, 1972

Vehicle Registration	1,260,000 ⁴
Survey Theft Estimate	38,700 ⁵
loss probability	.03
Total Commuting Autos Daily Entering City	206,000 ⁶
Commuter Vehicle Loss at One-Half City Rate	3,090

SOURCE: ¹Tables 12 and 39, Crime and Victims: A Report on the Dayton-San Jose Pilot Survey of Victimization. Washington: Law Enforcement Assistance Administration, Department of Justice, June, 1974. The San Jose robbery total presented in Table 12 of that source is clearly incorrect--this is my estimate from the robbery sub-totals. Rape totals are not reported in raw form; 100 is estimated from percentages elsewhere in the report.

> ²"San Jose Methods Test of Known Crime Victims," Statistics Technical Report No. 1. Washington: National Institute of Law Enforcement and Criminal Justice Statistics Division, Law Enforcement Assistance Administration, June, 1972, Table C.

³Accuracy and memory fade figures were calculated from Table 4, "San Jose Methods Test...," <u>op. cit.</u>, 14; telescoping data were reported in: "Victim Recall, Pretest (Washington, D.C.)," Demographic Surveys Division, Bureau of the Census, June 10, 1970, Table G.

⁴Annual Report, Illinois Secretary of State, 1972.

⁵"Crime in the Nation's Five Largest Cities," Washington: National Criminal Justice Information and Statistics Service, Law Enforcement Assistance Administration, April, 1974, Table 1.

⁶U.S. Bureau of the Census. Journey to Work. Washington: U.S. Covernment Printing Office, June 1973, Table 2.